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Impact of Stand Your Ground, Background Checks and Conceal and Carry Laws on Homicide Rates in the U.S

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In recent years, the number of gun related killings appear to be on the rise. In fact, data show that gun related murders rose 32% between 2014 and 2017 (Gramlich 2019). While the second amendment to the U.S. Constitution allows citizens to bear weapons, many states have passed additional laws regulating the industry. These include restrictive and prohibitive laws. The goal of this paper is to assess the impact of changes in hand gun related legislation on firearm homicide rates in the United States for the period 1999-2015. More specifically, we focus on the impact of stand your ground, right to carry and background checks laws and how they impact changes in homicide rates. Using a unique data set, we created a change point model and used regression models to show that changes to handgun laws do in fact impact homicide rates in many states.

Key Words: Handgun laws, stand your ground, background checks, right to carry.

Guns have been a part of the American culture since the first colonist arrived in the 15th century and gained significant popularity in the 19th century during and after the Civil War (Hofstadter 1970). More recently, the Centers for Disease Control and Prevention (Giffords Law Center 2021), indicated that over 45,000 Americans were killed due to gun violence in 2020. Mass killings have also increased. In 2020, the rate of mass killings reached an all-time high of 611, surpassing the 417 mass killings in 2019 (Gun Violence Archive 2021). Recent random mass public shootings in Colorado Springs, CO (7 dead); Indianapolis, IN (9 killed,

7 injured); Rock Hill, SC (7 killed); Boulder, CO (10 killed and 1 injured); and Sunrise, FL (3 killed and 3 injured), all highlight the need to investigate and study gun legislation and its impact on homicide rates (Gun Violence 2021).

These seemingly senseless acts are important to examine closely because it has brought the issue of gun violence to the forefront for law enforcement agencies and policy makers. In fact, these tragic acts have sparked a national debate about federal and state policies to reduce firearm violence. While some state policymakers are considering solutions to reduce the ability of citizens to purchase guns (universal background checks, mandatory gun safety courses, bans on assault weapons and stricter regulation of semiautomatic weapons to name a few), other states are considering laws that make it easier to carry and use firearms. These include conceal and carry, stand your ground laws and so on. In addition, other states are contemplating legislation that seeks to change gun culture by banning all gun related activities such as shooting clubs or other public facilities. Clearly, there is no universal agreement on how to manage this issue.

Previous literature notes several gaps in research examining hand gun homicides. These include, gender differences, intimate partner violence, risks from the commencement of gun ownership, previous criminal records, and so on. Our research contributes to the literature by adding using a change-point model to examine the incidence of hand gun homicides. By analyzing the efficacy of gun laws, research can inform policymakers, thereby yielding gun laws that reduce gun violence while maintaining the sanctity of the second amendment in the Constitution. Our research addresses one key question: Do changes in state gun laws affect homicide rates? Using unique, state level data from 1999-2015, we assess the impact of: right to carry or conceal and carry (RTC); criminal background checks (CBC); and stand your ground laws (SYG) on homicide rates in the U.S. Our overall goal is to determine if any change in the law, restrictive or facilitative, is associated with a change in homicide rates in the U.S. We hypothesize, based on the literature, that the addition of any restrictive hand gun law will lower the rate of hand gun homicides, and any facilitative law will increase homicide rates. We also include additional independent variables to tease out the impact of the gun laws. Finally, we provide more in-depth analysis for five states that had significant changes in homicide rates during the period under investigation (Arizona, California, Florida, Missouri, and Texas).[1]

Literature Review

Gun violence in the United States has reached epidemic proportions and ascended high into public consciousness. Public health experts, policymakers, and researchers have recognized the multifaceted nature of gun violence and its consequences, which deeply affect individuals, families and society at large (Sanchez et al., 2020). In our search of the literature, we located only one study that examined the impact of gun legislation on hand gun homicides in a holistic fashion. Ik-Whan et al. (2005) found that states that had multiple gun control laws were more likely to see lower gun related fatalities. A wider range of studies employing different design models and data sets have been used to evaluate the effects of particular firearm policies on the incidences and burdens of mortality and morbidity, including homicide rates, even though measuring the extent to which individual laws are enforced is difficult (Siegel et al., 2019; Smart et al., 2020; Webster & Wintemute, 2015). Another limitation noted in most studies is the difficulty of demonstrating causal relationships between adopted laws and the prevalence of gun violence (Siegel et al., 2019). Notwithstanding, numerous studies indicate that a

[1] Seven of the fifty states were omitted due to incomplete data.

greater number of gun policies reduce firearm-related homicides and suicides (Crifasi et al., 2018a; Fleegler et al.,2013; Siegel et al., 2017). Still, more research is needed to study the various facets of gun policies and associated impacts on homicide rates. Fortunately, evaluating the effects of gun laws has advanced over time due to fewer methodological concerns, availability of data, and improved covariate selection.

The next section provides a description/definition of Stand Your Ground (SYG), Background Checks, and Right to Carry as well as a summary of the literature for each set of laws and its impact on homicide rates.

Stand Your Ground Research

Stand Your Ground (SYG) laws expand the right of self-defense outside the home (Castle Doctrine) and into the public space, thus allowing individuals to use deadly force without a "duty to retreat" from a location they reasonably believe they should be present, and without fear of imminent danger. Essentially, the legal provisions of SYG laws minimize criminal penalties and eliminate civil liabilities for persons who claim self-defense even in deadly encounters (Cheng & Hoekstra, 2013; McClellan & Tekin, 2017) although the specifics of what constitutes lethal self-defense, on the presumption of fear, vary by jurisdictions (Dirlam et al., 2020). The removal of the "duty to retreat" rules have prompted opponents to label SYG laws as "Shoot First" laws and stirred arguments for and against SYG laws. Supporters assert that these laws have deterrent effects on criminal behavior and violence, but critics contend that these provisions may potentially escalate situations of violent confrontations (McClellan & Tekin, 2017).

The state of Utah passed the first SYG law in 1994, but it was not until Florida introduced its statute in 2005 that widespread legislation modeled after the two states occurred (Rand, 2021). As of 2020, 34 states had adopted SYG laws or expanded the Castle Doctrine outside the residence. Initial scholarship on SYG research can be traced to law journals that examined the effects of these laws from a legal standpoint (Catalfamo, 2007; Ross, 2007; Weaver, 2008). However, it would appear that the emergence of empirical studies coincided with the raucous national debate that ensued, following the acquittal of George Zimmerman, the Florida neighborhood watch volunteer, who claimed self-defense in the fatal shooting of Trayvon Martin, an unarmed black teenager, in 2012.

The earliest empirical analysis that we located was conducted by McClellan and Tekin (2012) whose findings contested the notion that SYG gun policies helped to enhance public safety. Using state-level data from the U.S. Vital Statistics for 2000-2010 to investigate the impact of SYG laws on homicides and firearm-related injuries, they found that states with SYG laws had significant increases in total firearm homicides. In particular, these provisions increased homicide rates for white males, but not for black males, and further led to an upsurge of emergency room visits and hospital discharges for firearm related injuries. In a related study, Cheng and Hoekstra (2013), using data from 2000-2010, found that SYG statutes led to an increase in criminal homicides by 8% and did not deter violent crimes such as aggravated assault, burglary, or robbery. For the same data period (2009–2010), but showing a contrary result, Webster et al. (2014) assessed the impact of SYG laws on age-adjusted homicide rates across states, and found no significant relationship between SYG laws and total homicide rates. Chamlin (2014), utilized monthly crime data from 2002–2011 to show that SYG laws had substantial effects on homicides rates in Arizona overall.

Some studies specifically noted appreciable increases in firearm homicides after the implementation of SYG laws in a number of states. For instance, Humphreys et al. (2017a) analyzed the relationship between SYG law and patterns of homicide rates in Florida, using

an interrupted times series method, with data collected from 1999 to 2014. By comparing homicide trends in four states without the law and controlling for suicide outcomes and other underlying trends, findings showed not only an increase, but an abrupt and steady rise in homicide rates from 2005 forward when Florida amended its law. They further found that a causal relationship was associated with 20 more homicides per month, or an additional 2,229 homicides after the law took effect. By contrast, there were no significant differences in homicide patterns in comparison states as well as suicide outcomes before and after the law. The authors concluded that changes in Florida's SYG law may have proved more harmful by escalating violent altercations rather than deterring them.

Guettabi and Munasib (2018) employed the synthetic control method to estimate the impacts of SYG laws on homicides in fourteen comparative states, using data from 1991 to 2012. Overall, they found inconsistent results across states, but observed substantial surges in homicide rates in Alabama, Florida and Michigan. They concluded that homicides and gun deaths were markedly reduced in the absence of SYG policies in the named states, which otherwise had "duty to retreat" requirements prior to implementing SYG. It is worth mentioning that not all states with duty to retreat policies had uniform effects. Crifasi et al. (2018a) found that urban counties in states with SYG laws experienced an additional 8% (see also Crifasi et al. (2018b).

Still, other studies found uncertain association between SYG laws and firearm homicides. Munasib et al. (2018) examined the effect of SYG laws on gun deaths in rural and urban locations using a difference-in-difference model and data from 1999-2013. They found that the adoption of SYG laws had no impact on net gun deaths statewide, while holding a number of control factors constant. However, significant increases were noted in core cities and the suburbs. Siegel et al. (2019) analyzed the impact of various state-level gun laws on homicide and suicide rates in a longitudinal study from 1991 to 2016, and found after controlling for a large number of factors that SYG laws had no effect on homicides rates. Universal background checks, however, reduced the firearm homicide rates by nearly 15%. Finally, a systematic review of existing literature in a RAND report concluded that there was "moderate evidence that stand-your-ground laws may increase total homicide rates, supportive evidence that stand-your-ground laws may increase firearm homicides, but inconclusive evidence for the effect of stand-your ground laws on other types of violent crime" (Smart et al., 2020, p. 245).

Right to Carry or Concealed Carrying Research

Concealed carry are state laws that outline procedures individuals must follow to obtain permits to carry concealed weapons. These are usually handguns, although some states may include Billy clubs and knives in this provision. Individuals can carry concealed firearms in public, on their person, or in close proximity. Since firearm carriage are mainly regulated by states, federal law does not govern the issuance of concealed-carry permits or licenses, except for certain active-duty and retired law enforcement officers, who can carry concealed weapons interstate regardless of state laws (18 U.S.C. 926).

There are three types of concealed carry laws: 1) "shall issue", whereby permits or licenses are issued to all eligible applicants, 2) "may issue", whereby state officials exercise a greater degree of discretion in granting permits, and 3) "no issue" or "permitless carry", which does not require permits to carry concealed handguns. Currently, thirty-two states and the District of Columbia have adopted the more permissive "shall issue" statutes, whereas nine states have adopted the restrictive "may issue" statues. Nine other states have "permitless carry" provisions. In 2015, there were thirty-seven states with "shall issue" laws (Steidley,

2019), indicating the shift from "no issue" or "may issue" to "shall-issue" concealed carry handgun laws. The absence of federal regulations means that state statutes concerning concealed carry permits vary tremendously. For example, the average number of provisions per state in 2016 was 4, out of the 7 possible provisions (McClenathan, Pahn, & Siegel, 2016).

An estimated 14 million people had concealed carry permits in the United States in 2015 (Rowhani-Rahbar et al., 2017) compared to more than 17 million permit holders in 2018 (Lott, 2018). The rise was likely due to factors ranging from permit fees, level of discretionary authority of local officials (may-issue or shall issue provision) and length of time the law was passed (Smart, et al., 2020). The implication of concealed carry laws is that expanding ownership of firearms could potentially increase crime levels on the one hand, or deter violent crime because possessors are likely to defend themselves using force if faced with the prospects of bodily harm, on the other hand. Scholars are split on opposing sides of this debate (Donohue, 1999).

Expectedly, the impact of right-to-carry concealed weapons on crime has received tremendous scrutiny than most gun policies (Smart et al., 2020), thus there is a large reservoir of empirical evidence to draw upon assessing the effects of concealed-carry on homicides. In a pioneering research project, Lott and Mustard (1997) examined the deterrent effects of "shall issue" right-to-carry concealed handgun laws on crime, using cross-sectional time-series county-level data from 1977–1992. After controlling for a large number of covariates, including the type of crimes, arrest rate, demographic characteristics, and yearly and county fixed-effect dummy variables, they found that states with shall issue laws had significantly lower rates of murders, rapes and aggravated assaults, but increased larceny and theft. Overall, the net effect was a sharp decrease in crime. The authors concluded that the implementation of concealed handguns laws prevented homicides and other violent crimes, thereby saving lives. The conclusion that more guns led to drastic declines in crime sparked great interest among scholars and more robust empirical studies ensued.

In a critical fashion, Ludwig (1998) contradicted the Lott and Mustard (1997) results with evidence derived from state panel data, additional variables and a more rigorous methodological design disaggregating adult and juvenile homicide rates. He found that shall issue laws *in fact* increased adult homicide rates. Other studies supported Ludwig's conclusion with evidence showing that "shall issue" laws significantly increased firearm and total homicides (French & Heagerty, 2008) compared to "no issue" laws (LaValle & Glover, 2012). LaValle & Glover (2012) also found additional evidence, associating "may issue" laws with lower homicides. In a follow up study, LaValle (2013) found that "shall issue" or "may issue" statutes led to a reduction in total homicide rates compared to "no issue" laws. In general, a significant number of studies found that concealed-carry laws either significantly increase or had uncertain effects on firearm homicides and other crimes (Ayres & Donohue, 1999, 2003a, 2003b; Crifasi, Pollack, & Webster, 2016; Donohue, Aneja, & Weber, 2019; Hamill et al., 2019; Helland & Tabarrok, 2004; Hepburn et al., 2004; Luca, Malhotra, Poliquin, 2017).

Hepburn et al. (2004) analyzed the impact of shall-issue laws on homicide rates, using panel data from 1979–1998 using a negative binomial regression model with two-way fixed effects. Their results showed no changes in homicide rates in states with shall-issue laws. Another panel study from the same period (1980–2000), using city-level data, found no evidence that shall-issue laws decreased or increased violent crime rates (Kovandzic, Marvell, & Vieraitis, 2005). Moody et al. (2014) found suggestive evidence that "shall issue" laws caused fewer homicide rates, however, an improved study evaluating four additional years of data found no changes in the law in seven out of eight years of implementation, albeit a

significant negative impact was observed in the seventh year (Moody & Marvell, 2018). Two recent studies applying extensive data periods (1977–2014) similarly found that shall issue laws have uncertain effects on firearm homicides among adults (Donohue, Aneja, & Weber, 2019; Luca, Malhotra, & Poliquin, 2017).

By contrast, Siegel et al. (2017) found that "shall issue" laws caused significantly higher firearm and total homicide rates compared to "may issue" laws. Specifically, states with "may issue" laws had a 6.5 percent higher total homicide rate, an 8.6 percent higher firearm homicide rate, and 10.6 percent greater handgun-specific homicides than "may issue" states. More recently, a panel study showed that "shall issue" statutes increased total homicide rates by 9 percent (Siegel et al., 2019). Donohue, Aneja and Weber (2019) showed that concealed carry laws contributed to an overall increase in crime. They found that violent crimes were thirteen to fifteen percent greater after a ten-year adoption period of concealed carry laws compared to states with "no issue" laws.

It would appear that an appropriate description of the broad research examining the impact of concealed-carry laws on homicides and violent crime rates is one of conflicting evidence. According to Smart et al. (2020), "shall issue" laws may increase violent crime, but there is little evidence for the relationship. The research suggests that "shall issue" laws have uncertain impacts on firearm homicides, total homicides, and other crimes like assaults, robberies, and rapes. Hence, the evidence remains inconclusive for this relationship.

Criminal Background Checks Research

Background checks are gun policies that regulate the sale and transfer of firearms to eligible purchasers. Legal provisions require licensed dealers, known as a Federal Firearms Licensees (FFLs), to initiate background checks on prospective buyers to prevent firearm access by people with either criminal records, or those who are otherwise disqualified from buying or owning guns (FBI, 2021). Individuals prohibited from possessing firearms include: minors, particular convicted felons, fugitives from the law, substance users, and domestic violence offenders. Others include: those with dishonorable military discharges, mental illness histories, restraining orders, and illegal residents of United States (18 U.S.C. 922).

The 1993 Brady Handgun Violence Prevention Act (called the Brady Act), which amended the Gun Control Act of 1968, established the federal requirements and procedures on all FFLs, according to rules and regulations set by the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) (18 U.S.C. 922). While private sales and transfers (e.g., gun show sales, gifts) are exempt from federal regulations, at least twenty-two states and the District of Columbia have broadened federal requirements and mandates, including background checks for unlicensed, private parties, and imposing stricter rules for audits and recordkeeping to reduce firearm diversions from rightful owners to prohibited possessors (Smart et al., 2020). Such expanded laws are known as universal background check laws. In 2016, the average number of background check provisions per state was 2.6, with California and Washington having the maximum possible number of provisions of eleven. Thirty-two states had no provisions (McClenathan et al., 2016).

In one of the earliest empirical studies examining the effects of background checks on homicide rates, Ludwig and Cook (2000) compared pre-Brady Act (including background checks and waiting periods) and post-Brady (with broader changes to the law) homicide rates using state-level data, and found no significant differences in homicide rates in adults aged 21 years of age or older. Similarly, using a difference-in-difference-in-difference design to evaluate the differential effects of the Brady Act across states and over time, Monroe (2008) showed that the law had no impact on firearm and total homicide rates, although there were

significant reductions in firearm homicides involving guns which were not handguns. La Valle (2013) examined the impact of pre-Brady background checks policies on firearm homicides and total homicides using data from large U.S cities from 1980–2010. While accounting for time-varying factors, including other state gun laws, the results showed no significant effects on firearm homicides and overall homicide rates.

Sen and Panjamapirom (2012) analyzed the effects of different types of post-Brady background checks performed by states on firearm and total homicides while observing great variations, both in the classification of checks (restraining orders, fugitive status, misdemeanors) and information sharing with the National Instant Criminal Background Check System (NICS) across states and local jurisdictions. Using state-level data from 1996–2005, they found that background checks for fugitive standing, mental illness and restraining orders were associated with substantially lower firearm and total homicides, unlike those checking for criminal history alone. Specifically, background checks for fugitive standing reduced firearm and total firearm rates by twenty-one percent and twenty-three percent, respectively. Background checks for mental illnesses decreased both firearm and total homicides by seven percent, while those that included restraining orders had a 13 percent reduction in firearm homicides and other violent crimes.

These results were consistent with several studies that found that the inclusion of wide-ranging background checks may help to reduce firearm homicides (Crifasi et al., 2018; Kalesan et al., 2016; Kaufman et al., 2020; Ruddell & Mays, 2005; Rudolph et al., 2015; Sen and Panjamapirom, 2012; Sumner, Layde, & Guse, 2008; Webster, Crifasi, & Vernick, 2014). Moreover, findings from a recent longitudinal study indicated that universal background checks decreased total homicide rates by about fifteen percent (Siegel et al., 2019).

Webster et al. (2014a) studied the effect of the repeal to Missouri's permit-to-purchase (PTP) law on state homicides and found that the change obligating gun purchasers to a pass background check was associated with an increase in gun homicide rates. An erratum to the study estimated that 168 firearm homicides occurred in Missouri each year between 2008–2012 (Webster et al., 2014b). Using the synthetic control method to compare Connecticut's handgun PTP law on homicides before implementation (1984–1994) and after implementation (1995–2005), Rudolph et al. (2015) estimated that the law led to a forty percent reduction in homicide rates during the first decade that it was adopted. By contrast, no evidence was associated with a drop in non-firearm homicide rates. The counterfactual was estimated using panel data weighted from a combination of comparison states without PTP law change based on prelaw homicide patterns and annually measured state-level covariates.

One of the more recent studies by Lee et al. (2017) found, using a content analysis of peer-reviewed articles from 1970-2016, that stronger state gun laws decreased firearm homicide rates. In addition, they also found that back ground checks and permit-to-purchase laws decreased homicide rates from firearms. Similarly, Santaella-Tenorio et al. (2016) collected data from 130 studies in 10 countries, dating from 1950-2014, and found that background checks and access to firearms were associated with lower homicide rates and unintentional deaths in children.

It is possible due to variations in policy implementation, methodological weaknesses, and challenges associate with measuring individual policy component, that the overall effects of background checks are uncertain. According to a notable RAND research synthesis of gun policies, evidence is inclusive on the relationship between background checks and violent

crime and total homicide rates; and the association between private-seller background checks and firearm homicides. Although, there is moderate evidence of reduced firearm homicides for dealer background checks (Smart et al., 2020, pp. 139–140).

Data and Model

We posit that a change in the adoption or repeal of state firearm laws may have an impact on homicide rates. Thus, our dependent variable is firearm homicide rates. We collected the data for this variable from the Centers for Disease Control and Prevention Web-based Injury Statistics Query and Reporting System (CDC 2018). There were eight independent variables utilized in our study. This included three firearm laws where data were available: stand your ground and right to carry (Boston University School of Public Health 2020) and criminal background checks (Cherney et al. 2020).

Our data analysis is split into two sections. First, we assess the extent to which changes in hand gun laws impact homicide rates in the U.S. using a change point model. Second, we focused our attention on five states that had significant changes in homicide rates with the goal of determining if a change in hand gun law affected the homicide rates. The data for criminal background checks was consistent for each state during the period of our study. That is, each state either had a criminal background check law (coded as 1) for entire duration of the study or they did not have a law (coded as 0). As a result, the variable was not included in our individual state focused regression models. Also, the data for the two remaining firearm laws were also consistent for several of the states that we examined closely and as a result were left out of the analysis. Finally, we included poverty rates (U.S. Census Bureau 2020), unemployment rates (Bureau of Labor Statistics 2020), burglary rates (Uniform Crime Reports 2020a), incarceration rates (Bureau of Justice Statistics 2020) and law enforcement officers per capita (Uniform Crime Reports 2020b). In order to maintain the statistical power of our model and focus on our chief independent variables, which would have led to over-fitting, we limited the number of independent variables that were available to us.

Methodology

We used a change point analysis model to determine if a "change-point" occurred for homicide rates in any of the states using a Bayesian Change Point model (BCP) (Barry and Hartigan 1993). Change-point analysis allowed us to detect whether any changes occurred in our time ordered data. Our change-point analysis model was designed to detect small subtle changes in hand-gun related homicide rates for any given state over the sixteen-year period of our study (1999-2015). More specifically, our model was designed to detect specific years where the model found a "change point(s)" (shifts/changes in firearm related homicide rates), the extent of the change, and the level of "certainty" associated with the change points.

In this model, we assumed that there is a time series process $X_1, X_2, \dots, X_n$ occurring, where X_i is the firearm related death in year i (1999-2015) for a particular state. Next, we assumed that the data points are coming from an underlined distribution parametrized by some unknown parameter θ_i (such as $N(\mu_i, \sigma_i^2)$). In addition to that, we denoted the probability of a specific time point *i* (here year *i*) will be a change point by p_i, and the change points are independent of each other. In our change point model, we assumed that there exists an unknown partition of the observed data into mutually exclusive blocks such that the unknown parameters $\theta_1, \theta_2, \dots, \theta_n$ are consistent within the blocks. Our goal was to examine these blocks and thereby determine the change points. The BCP model allowed us to detect those change points or the years where we saw a significant shift in firearm related homicide

rates.

To determine the blocks, we introduced indicator variables $\rho = (U_1, U_2, \ldots, U_n)$ where $U_i = 1$ indicated a change at time $t = (i + 1)$. We initialized U_i to *0* for all $i < n$, with $U_n = 1$. Then, the odds of a change point at a particular position $(i + 1)$ in the partition (given the data *X* and the current partition) can be obtained from the ratio $\frac{p_i}{1-p_i} = \frac{P(U_i=1\,|U_j, j\neq i)}{P(U_i=0\,|U_j, j\neq i)}$, which can be expressed using 4 sum of squares; W_0, W_1, B_0, B_1 which were the within and between block sums of squares obtained when U_i = 0 and U_i = 1 respectively and 2 tuning parameters γ and λ, which takes a value between 0 and 1. The tuning parameters controlled the effectiveness of our BCP model so that our method was effective in situations where there were not too many changes (γ small), and where the changes that did occur were of a reasonable size (λ small).

The mean of a block that began at position $(i+1)$ and ended at position *j*, is denoted by the parameter μ_{ij}. In our model (BCP), we specified a prior distribution on μ_{ij} as N(μ_0, $\frac{\sigma_0^2}{\mathrm{j}-\mathrm{i}}$). According to Barry and Hartigan (1993), this choice of prior distribution allowed weak signals, provided that there were sufficient data to estimate them. Since this is a Bayesian methodology, the posterior means of the odds of change probabilities were updated after every iteration, where the posterior samples were generated from a Markov Chain Monte Carlo algorithm proposed by the above-mentioned paper.

We used the R package *bcp* proposed by Erdman and Emerson (2007) to fit our BCP model. We maintained the default value of the tuning parameters γ and λ, which were fixed to be 0.2. A conservative change point that was recommended by Barry and Hartigan (1993). In our Markov Chain Monte Carlo method, we generated 5,000 posterior samples for each state and then discarded the first 500 as burn-in (which is required for convergence of the algorithm purpose). Then, using the post burn-in posterior samples, we estimated the posterior means of changes for all the time points in every state. Finally, for every year using our BCP model, we computed the posterior probability or chance that the year was a change point or that something out of ordinary happened and the gun related homicide rate significantly changed. We repeated this analysis for each of the states individually. We used a cutoff of 25% after examining the posterior probability from all of the states to label a year to be a "change-point." Hence, if the calculated posterior probability of change-point is 25% and above, during those years the gun related homicide rates breakaway from the past trends, it suggested that something significant influenced gun related homicides beside randomness.

Further, we investigated our thesis by creating a unique data set that included each state by year (1999-2015). For 43 states (7 states omitted due to the incompleteness of data), our dependent variable was gun related homicide rate per thousand and our independent variables were: poverty rate (percentage of population under poverty), burglary rate, law enforcement officers per capita, incarceration rate, unemployment rate, and stand your ground laws. Overall, our goal was to understand how these socio-economic variables and firearm laws influenced the firearm homicide rate in any particular state. Therefore, our regression model for a specific state was:

> Firearm Homicide Rate =β_0 + β_1 × percentage of population under poverty + β_2 × burglary rate + β_3 × incarceration rate + β_4 × unemployment rate + β_5 × law enforcement officers per capita + β_6 × stand your ground laws + error

In our original analysis, we created a model for each state using 1999-2015 data. Hence, we had seventeen data points for every state, which was akin to a regression model with seventeen samples. Similarly, Webster et al. (2014a) used 12 years of data with 10 covariates in their study. In our model, the error degrees of freedom was ten degrees in comparison to Webster et al., which had only two degrees of freedom. We closely monitored all standard errors of the coefficient estimates and we did not encounter any large standard errors in our models. However, since all of the seventeen data points were over seventeen years from the same states, there may be some dependence among those. Therefore, we had to be careful with the type of error distributions that we wanted to assume in our regression model. In this paper, we fitted 3 different types of regression models that have different types of random error assumptions.[2]

In *Model 1,* we fitted our regression model under the independent error assumption. In so doing, we assumed from years 1999 to 2015 that the data were independent of each other, year wise. In *Model 2,* we fitted our regression model under AR(1) (auto regressive error of lag 1 time series) error assumption. The AR(1) did not consider independence among the years, but placed a correlation structure among the seventeen years of data. The correlation structure was created such that the correlation diminished as two years spread further. For example, ρ^d, where ρ is the correlation and d was the separation between 2 years. If $\rho = 0.5$ (we considered 1999 and 2000 which is of separation of 1 year), then the correlation between them would be 0.5, whereas if we considered 1999 and 2010 (which is of separation 11 years) the correlation between them would be $0.5^{11} = 0.0005$. In *Model 3,* we fitted our regression model under MA(1) (moving average error of lag 1 time series) error assumption. MA(1) placed a correlation structure as $\rho(d) = \theta/(1+\theta^2)$ for $d = 1$ and $\rho(d) = 0$ for $d > 1$.

Findings

Before we examine the data for our regression model, we first provide a full country-wide analysis using our BCP model. The data in Table 1 indicates where there was a change in the homicide rate using a 25% (moderate change) and a 50% (significant change) cutoff. Data for this period were only available for 43 of the 50 U.S. states.[3] In the table, we note overall that very few states have "significant change points" during this period. In general, there is a very slight increase in "change-points" as we move to the latter years. The data essentially ebbs and flows with random increases in the "change-points." The year 2015 shows the most activity with eleven states showing "moderate change points" and four states showing "significant change points."

The data in Table 2 provides a summary of each state that has significant changes in the firearm homicide rates, the year(s) that it occurs, and any laws that could affect the change. With respect to the three laws that we examined in this paper, nine of the twenty-one states listed passed a background check, stand your ground or a concealed weapons law during the period 1999-2015. Six states passed other gun related laws, and six states did not pass any gun related laws during that period.

The analysis in the paragraphs below show the change-points for five specific states:

[2] We also included an independent variable for the right to carry (RTC) in the Missouri model. In California, the stand your ground (SYG) law variables were all zero for the years 1999-2015. Thereby making it useless in our analysis. So, we substituted the Saturday Night Special ban (SNSban) instead of using the SYG variable.

[3] We excluded Hawaii, New Hampshire, Maine, North Dakota, South Dakota, Vermont and Wyoming due to incompleteness in their handgun related homicide data.

Arizona, California, Florida, Missouri, and Texas (Figures 1-5). The vertical axis is the posterior probability or chance that the year is a change-point or something out of ordinary happened and the gun related homicide rate significantly changed. Whereas the horizontal axis are the years.

Table 1: Change-Points in Homicide Rates, 1999-2015

Year	No Change	Moderate Change	Significant Change
1999	N.A.	N.A.	N.A.
2000	40	3	0
2001	37	5	1
2002	40	3	0
2003	39	3	1
2004	35	6	2
2005	38	4	1
2006	40	2	1
2007	39	3	1
2008	35	6	2
2009	35	5	3
2010	40	2	1
2011	37	6	0
2012	38	4	1
2013	39	4	0
2014	40	2	1
2015	28	11	4

Table 2: States with Significant Changes in Homicide Rates

State	Change in H.R.	Year	Law Passed in Current or Previous Year
Arizona	94.6%	2009	Firearm restoration & CCW Permit concealed;[4] cc background;[5] cc renew background;[6] ccrevoke,[7]
California	58.1%, >36.5%	2002, 07-10	No
Florida	80.5%	2006	SYG[8]
Idaho	61.2%	2008	Preemption narrow;[9] Preemption broad[10]
Illinois	89.2%	2004	No
Louisiana	58.9%	2000	No

[4] Law requires a permit in order for an individual to carry a concealed weapon, or the law bans all concealed weapons.
[5] Law requires that individuals undergo a background check when applying for a concealed carry permit, or law bans all concealed weapons.
[6] Law requires individuals to undergo a background check in order to renew a concealed carry permit, or law bans all concealed weapons.
[7] Law requires authorities to revoke a concealed carry permit under certain circumstances, or law bans all concealed weapons.
[8] This provision refers to an extension of a "Castle doctrine" law.
[9] Any state law that preempts local regulation of firearms is narrow in scope (i.e., in one area of regulation).
[10] State law does not completely preempt local regulation of firearms.

Table 2 Continued

Maryland	89.2%, 97.4%	2009 & 15	2009: dvrosurrender;[11] dvrosurrendernocondit;[12] 2015: microstamp;[13] dvrodating[14]
Massachusetts	80.3% & 95.0%	2004 & 2012	No, 2004: No, 2012
Mississippi	77.5%	2004	Immunity[15]
Missouri	95.8%	2015	CCW;[16]
Nebraska	64.5%	2007	Mayissue;[17] Showing[18]
Nevada	95.4%	2008	No
New Jersey	80.6%	2003	General[19]
New Mexico	66.9%	2001	Mayissue;[20] Showing;[21] ccbackground;[22] ccbackgroundnics;[23] ccrenewbackground;[24] ccrevoke[25]
North Carolina	80.7%	2009	No
Ohio	77.8%	2005	CCW[26]
Oregon	78.9%	2015	Restrictions on possession
South Carolina	90.4%	2015	Background Checks
Texas			Submissions of data to FBI in 2009
Virginia	58.9%	2008	Concealed weapons law and forbids disclosure of gun ownership
Washington	53.5%	2015	Universal background checks required

[11] State law requires DVRO subjects to surrender their firearms.

[12] There are no additional conditions on the requirement that DVRO subjects turn in their firearms. No additional finding is necessary.

[13] All handguns sold must have either ballistic fingerprinting or microstamping so that they can be identified if used in a crime.

[14] DVROs are automatically prohibiting if the subject is a dating partner of the petitioner.

[15] No law provides blanket immunity to gun manufacturers or prohibits state or local lawsuits against gun manufacturers.

[16] As of October 11, 2014, a valid CCW overrides local laws against open carry state-wide. Missouri Statute 571.070

[17] Law provides authorities with discretion in deciding whether to grant a concealed carry permit, or the law bans all concealed weapons.

[18] Applicants are required to make a heightened showing to obtain a concealed carry permit.

[19] The *New Jersey Childproof Handgun Law*, also known as P.L.2002, c.130, was a now-repealed law that would restrict the sale of handguns in NJ to smart guns that "can only be fired by an authorized or recognized user" and would take effect three years after the technology is available for retail purposes.

[20] Law provides authorities with discretion in deciding whether to grant a concealed carry permit, or the law bans all concealed weapons.

[21] Applicants are required to make a heightened showing to obtain a concealed carry permit.

[22] Law requires that individuals undergo a background check when applying for a concealed carry permit, or law bans all concealed weapons.

[23] Law explicitly requires that individuals applying for a concealed carry permit must undergo a background check process that includes a check of the NICS database.

[24] Law requires individuals to undergo a background check in order to renew a concealed carry permit, or law bans all concealed weapons.

[25] Law requires authorities to revoke a concealed carry permit under certain circumstances, or law bans all concealed weapons.

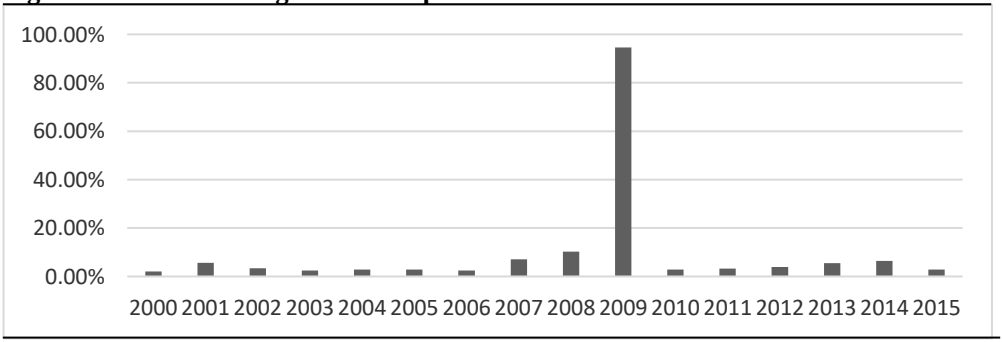
[26] H.B. 12 was signed by Gov. Taft in 2004. Ohio becomes the 46th state to legalize concealed carry. Ohio's requirements are the most restrictive of any in the nation. Ohio sheriffs begin accepting concealed handgun license applications and issuing licenses.

For example, Figure 1 shows the BCP model for Arizona from 1999-2015. The data shows a dramatic change-point in 2009 indicating that something likely occurred in that year or the prior year to affect the data. An examination of the data shows that Arizona passed several laws making it more feasible to own a gun in 2009, including a conceal and carry law. Also note that in the subsequent year, the percentage decreases. This does not denote that the rate of hand gun homicides decreased, but that there was not a significant change point from the preceding year that would cause a change in the data point.

For example, Figure 1 shows the BCP model for Arizona from 1999-2015. The data shows a dramatic change-point in 2009 indicating that something likely occurred in that year or the prior year to affect the data. An examination of the data shows that Arizona passed several laws making it more feasible to own a gun in 2009, including a conceal and carry law. Also note that in the subsequent year, the percentage decreases. This does not denote that the rate of hand gun homicides decreased, but that there was not a significant change point from the preceding year that would cause a change in the data point.

While we did not find any substantive laws in California (Figure 2) during this period, we did find several change-points in the data. The state requires background checks, has a partial open carry law, and allows handguns to be carried in vehicles. Conversely, Florida passed a stand your ground law in 2006 and we see a significant change-point in homicides in this year (Figure 3). Figure 4 shows the BCP model for Missouri from 1999-2015. The model indicates sudden strong (more than 50% chance) significant change in the years 2008 and 2015. Interestingly, in 2007 and 2014, the Missouri legislature passed a handgun background check repeal law and open carry arms law respectively (Webster, Crifasi, & Vernick 2014a). The last figure (5) provides the BCP model for Texas. As shown, the state had a significant change-point in 2009. We note that Texas passed three handgun related laws in 2009, but none of them included the three variables that we focus on in our literature review (stand your ground, right to carry, and background checks).[27] [28]

Figure: 1 Arizona Change-Point Graph



[27] Texas passed a discharge of firearms law, a law requiring the submission of firearm prohibition records, and an affirmative defense to prosecution law in 2009 (Giffords Law Center 2010).

[28] We developed an online application that can be used to analyze and visualize data for any state, chose the cut-off points and detect the handgun related homicide rate change-points over years (1999-2015). The application is located at https://sounakchakraborty.shinyapps.io/GunHomicide/.

Figure: 2 California Change-Point Graph

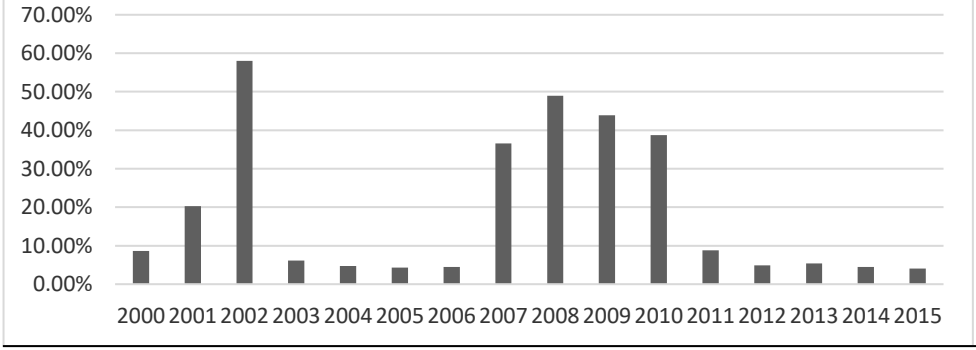


Figure 3: Florida Change-Point Graph

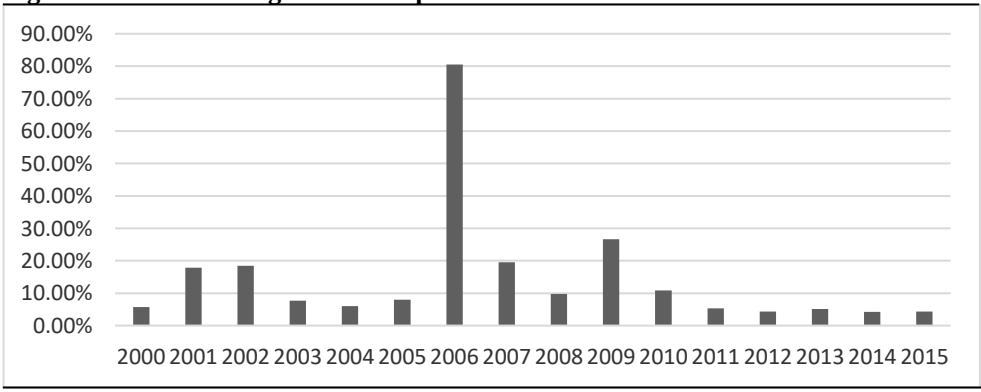


Figure 4: Missouri Change Point-Graph

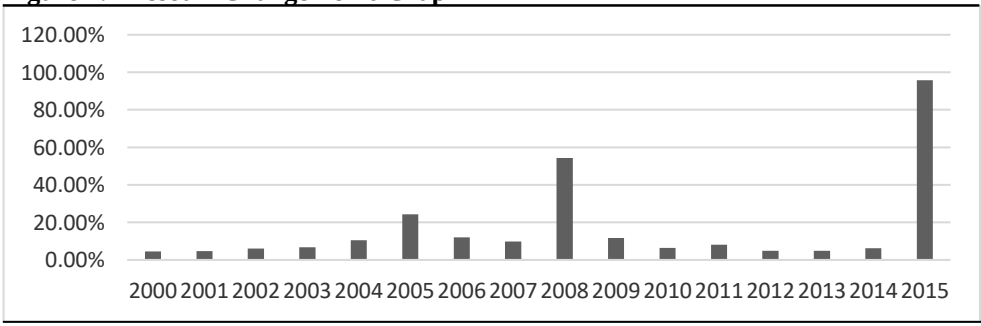
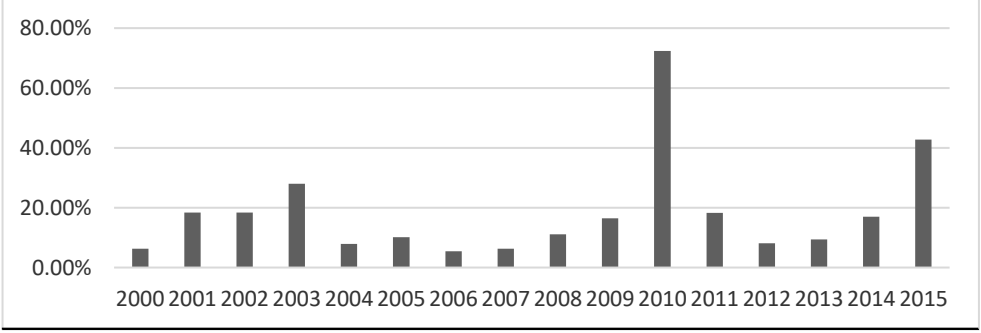


Figure 5: Texas Change-Point Graph



Regression Models

In Tables 3-7, we tabulate the fitted regression coefficient estimates and their corresponding p-values based on our all three models. The coefficients, which have a p-value of 0.10 or less in at least one of our model are counted as significant and highlighted in bold. Due to the scarcity of the data (seventeen years), a 10% p-value cutoff is very reasonable.

Table 3: Arizona Fitted Regression Models

	Model 1: Independent		Model 2: AR(1)		Model 3: MA(1)	
Coefficients	Estimate	p-value	Estimate	p-value	Estimate	p-value
Intercept	-8.361	0.407	-6.764	0.547	-9.288	0.410
Poverty Rate	-0.044	0.677	-0.053	0.585	-0.064	0.500
Burglary Rate	0.007	**0.005**	0.006	**0.024**	0.006	**0.019**
L.E.O. Per Capita	-0.002	0.950	-0.0001	0.996	0.004	0.878
Incarceration Rate	0.017	0.163	0.015	0.300	0.018	0.219
Unemployment Rate	-0.258	**0.039**	-0.222	0.111	-0.240	**0.084**
Stand Your Ground Law	-0.398	0.574	-0.497	0.551	-0.539	0.522

Table 4: California Fitted Regression Models

	Model 1: Independent		Model 2: AR(1)		Model 3: MA(1)	
Coefficients	Estimate	p-value	Estimate	p-value	Estimate	p-value
Intercept	4.869	0.110	3.750	0.221	5.567	0.094
Poverty Rate	-0.162	0.170	-0.174	**0.082**	-0.203	**0.073**
Burglary Rate	0.003	0.126	0.003	0.216	0.003	0.256
L.E.O. Per Capita	-0.013	0.296	0.003	0.831	-0.012	0.441
Incarceration Rate	0.006	**0.042**	0.004	0.290	0.005	**0.093**
Unemployment Rate	-0.043	0.435	-0.011	0.850	-0.017	0.755
Saturday Night Special Ban	0.621	**0.006**	0.250	0.336	0.404	**0.101**

Table 5: Florida Fitted Regression Models

	Model 1:Independent		Model 2: AR(1)		Model 3: MA(1)	
Coefficients	Estimate	p-value	Estimate	p-value	Estimate	p-value
Intercept	-10.580	0.146	-9.049	0.207	-8.113	0.224
Poverty Rate	0.226	**0.109**	0.222	**0.087**	0.185	**0.083**
Burglary Rate	0.001	0.519	0.001	0.553	0.000	0.718
L.E.O. Per Capita	0.024	0.211	0.022	0.201	0.019	0.246
Incarceration Rate	-0.012	**0.053**	0.010	0.177	0.011	**0.094**
Unemployment Rate	-0.195	**0.041**	-0.182	**0.074**	-0.155	**0.074**
Stand Your Ground Law	0.400	0.391	0.493	0.370	0.298	0.542

We find that poverty rates, incarceration rates, and unemployment rates are significant variables in three of the five states. The burglary rate and stand your ground laws are significant in two of the five states. Whereas, law enforcement officers per capita was only found to be significant in one of the five states. The Saturday Night Special ban used only in California (look into footnote 7 in page 9) came out to be highly significant for that state. Overall, this data shows us that, in general, socio-economic factors have greater explanatory power for firearm related deaths than the law enforcement officers per capita in a state. We also note that a combination of factors associated with our dependent variable are not consistent across states. Hence, there is a heterogeneity in the data and evidence that a single policy will not work for all states in reducing firearm related homicide rates.

Table 6: Missouri Fitted Regression Models

	Model 1: Independent		Model 2: AR(1)		Model 3: MA(1)	
Coefficients:	Estimate	p-value	Estimat	p-value	Estimate	p-value
Intercept	14.927	0.259	5.763	0.707	-8.902	0.446
Poverty Rate	-0.260	**0.049**	-0.121	0.292	-0.031	0.717
Burglary Rate	0.001	0.834	0.001	0.861	0.004	0.331
L.E.O. Per Capita	-0.035	0.356	-0.034	0.356	-0.006	0.835
Incarceration Rate	-0.001	0.962	0.016	0.449	0.028	0.172
Unemployment Rate	-0.056	0.780	-0.218	0.354	-0.501	**0.037**
Stand Your Ground Law	2.149	**0.024**	2.507	**0.029**	2.740	**0.008**
Right to Carry	0.982	0.147	0.491	0.634	0.315	0.738

Table 7: Texas Fitted Regression Models

	Model 1: Independent		Model 2: AR(1)		Model 3: MA(1)	
Coefficients:	Estimate	p-value	Estimate	p-value	Estimate	p-value
Intercept	5.572	0.074	6.178	0.075	5.990	0.092
Poverty Rate	0.090	0.413	0.051	0.666	0.082	0.506
Burglary Rate	0.003	0.011	0.002	0.214	0.003	0.065
L.E.O. Per Capita	0.021	0.108	0.007	0.603	0.010	0.380
Incarceration Rate	-0.013	0.005	-0.008	0.136	-0.009	0.041
Unemployment Rate	-0.234	0.010	-0.146	0.217	-0.220	0.056
Stand Your Ground Law	-0.791	0.013	-0.480	0.224	-0.588	0.100

Conclusions

We have reviewed existing research regarding the effects of three areas of firearm laws on injury, homicide, and crime related outcomes, with a focus on firearm homicide as the primary outcome of interest. An increase in homicides caused by the use of firearms and a heightened sense of urgency created by police killings in the United States has brought the issue of gun control policy back to the forefront for many persons. While this paper does not focus on gun control policy per se, we were very interested in understanding how a change in firearm/gun policy would impact homicide rates in the U.S. In order to create a narrative, we began the paper by examining research on three specific gun/firearm laws: stand your ground (SYG), criminal background checks (CBC), and right to carry (RTC).

Unlike previous research, we employed a more sophisticated Bayesian Change Point (BCP) model to assess the impact of firearm homicide rates. This model does not make any assumptions about changes in laws, it simply alerts the reader if there was a significant change point based on a times series model. In this case, we assessed changes in firearm homicide rates. Our findings were consistent with much of the previous research that showed changes in firearm laws that facilitated purchase and the right to carry guns had a negative impact on homicide rates. That is, if a state passed a facilitative SYG law, firearm homicide rates were likely to increase. If a state removed the CBC law or passed a RTC law, firearm related homicides were likely to increase. Conversely, the literature was also pretty clear with respect to the impact of implementing a restrictive law. For example, states that implemented CBC laws saw decreases in gun related homicides and suicides (Kaufman et al. 2020; Sumner, Layde, & Guse 2008).

Using the BCP model, we found evidence in both our descriptive analysis and our regression models that firearm laws do in fact affect changes in firearm homicide rates. The analysis showed that most states did not have significant changes in their homicide rates during the period 1999-2015. For the twenty states that had a significant change-point, we noted that seventy-five percent of them passed one of the three aforementioned laws or another law during the period where a change-point occurred. Since the model does not control for events or changes in law, we cannot directly contribute the change-point to the change in law. However, anecdotal evidence suggests that there is a strong relationship between the two.

We also focused our attention on five states (Arizona, California, Florida, Missouri and Texas) that had a single or multiple significant change-points during the period under

investigation. For example, we found a significant change-point (94.6%) in Arizona in 2009. When we examined their gun laws, we found that the state passed several laws during the period where the change-point occurred. One of those laws included a RTC law. We found similar findings in Florida when they passed a SYG law in 2006 and also in Missouri when they passed a conceal and carry law and repealed the background check law. The state of California had multiple change-points, but the state did not enact a new firearm law during the year of the change-point or the year before. Hence, it is reasonable to assume that other non-gun related factors also drive changes in homicide rates.

We concluded the analysis by creating regression models for five states with significant change points. As expected and consistent with previous literature, poverty rates, incarceration rates, and unemployment rates were significant variables in the model. With respect to gun laws, we found that SYG laws were significant predictors of the dependent variable (gun related homicide rates) in three of the five models. Unfortunately, we were not able to examine all three-gun law variables in each of the models due to missing data.

Although our analysis is limited with respect to available data and the ability to determine a cause and effect relationship between homicide rates and firearms laws, the policy implications of our findings are clear that changes in gun laws impact homicide rates. With respect to our hypothesis, in some cases, restrictive changes in the law do in fact have a negative impact on homicide rates. These finding are consistent with other research and reinforce the need for policymakers to give serious consideration to changing a law that essentially makes it easy to purchase and carry a gun.

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