Detecting Effective Firearm Policies in The United States

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Contents

1 Introduction

The United States faces a unique and persistent firearm mortality crisis. Since 1968, firearm suicides in the United States have hovered around 6.3 to 7.7 per 100,000 people, and firearm homicides have remained around 5.2 to 7.2 per 100,000 people. The data shows that these rates have remained remarkably constant, with no significant overall trend in the data from 1968 to 2013 signifying that the issue does not appear to be improved. There are two notable periods of time that have seen substantial decreases in firearm homicides. From 1992 to around 2013 firearm homicides dropped from 7 per 100,000 in 1992 to a low of 3.5 per 100,000 people in 2013. Recently, there has been a substantial 22% decrease in firearm homicides from 2024 to 2025, with firearm homicides forecasted to drop to 4 per 100,000 in 2026 (Boxerman & Lopez, 2026). Despite these periods of reductions in mortality, the rates of firearm homicides and suicides quickly recover back to their typical levels. Firearms have remained the leading cause of death in the youth population in the U.S. since 2020, and have consistently ranked in the top five causes of death in those under 45 (Centers for Disease Control and Prevention, 2026). Firearm suicides accounted for close to 60% of all firearm related mortalities in the United States in 2023, firearm homicides accounting for 38% (Gramlich, 2025). This motivates the choice of examining the effectiveness of firearm policies on the reduction in firearm homicides and suicides, since these account for the vast majority of firearm mortalities.

The scale of this issue is best understood through a comparative lens. Globally, the U.S. is in the 93rd percentile for overall firearm mortalities, and in the 92nd percentile for youth firearm mortalities (Gumas et al., 2024). Firearm mortalities are substantially higher in the U.S. when compared specifically to other high income nations, with firearm homicide mortalities being 20 - 25 times higher and firearm suicide mortalities being 8 times higher than these comparable countries (Grinshteyn & Hemenway, 2019). Notably, non-fatal crime rates are very similar to those of other high income countries, highlighting that the

American issue is specifically one of lethality, likely driven by the prevalence and accessibility of firearms. For example, the U.S. has very comparable rates of assault of other high income countries but is a significant outlier when it comes to the lethality of these encounters, of which predominantly involve firearms (Hawkins & Zimring, 1997).

Now that the issue of firearm violence has been established as a significant issue in the U.S. the question then becomes a matter of if there exists effective policies to address this issue. An effective policy in this context means a policy that leads to a decrease in firearm homicides, firearm suicides, or both. It is clear by the constancy of the rates of aggregate firearm violence in the face of enacted firearm policies that this issue is not quickly solved.

The primary goal of this research is to contribute to the literature on firearm policies by not just assessing the effect of a specific, pre-defined intervention, but to rather ask the reverse causal question of "What firearm policies have led to a reduction in firearm mortality in the United States?". This question is addressed by applying the novel structural break detection approach by (Pretis & Schwarz, 2026) to state level homicide and suicide data. This methodology utilizes machine learning to identify structural breaks in a Two Way Fixed Effects difference in differences (DID) framework. These detected breaks are subsequently evaluated for their association with specific policy mixes that were implemented during the identified treatment timing that were most likely responsible for the detected breaks. The benefit of this approach is that instead of examining every single policy, of which many have unclear treatment timing, one is able to detect real changes in firearm mortalities without imposing known treatment time and then associate policies which occurred around this time. Unknown or misaligned treatment time is often observed in the firearm policy literature and in part leads to differing observed effects in these studies, since incorrect treatment time biases the estimated treatment effect. The break detection method has been shown to correctly detect the magnitude and treatment timing of enacted policies without prior knowledge of treatment timing.

The break detection method used in this paper has been applied successfully to several uses in climate econometrics to find effective policy mixes that have, for example, led to major emission reductions (Stechemesser et al., 2024), or to assess the effectiveness of specific policies such as the provincial BC carbon tax (Pretis, 2022). This method finds particular success compared to other methodologies when evaluating policies in combinatory regimes, where there is not just one single treatment imposed but several treatments are imposed within a short span of time. In Stechemesser et al., 2024, their findings reinforced the conclusions of earlier literature, demonstrating that single policies rarely achieve significant reductions in isolation and that successful interventions almost exclusively involve a mix of complementary policy instruments. This global evidence further validates the "reverse-causal" framework as a robust tool for identifying effective treatments in policy environments where traditional "effects-of-causes" models often fail to find a clear signal. In the context of their study, almost all OECD countries have some form of carbon emission reduction policies in place, which makes the identification of policy evaluation of particular policies difficult due to having a limited to non-existent control group of untreated units. The ability to only focus on the significant real breaks in carbon emissions allows for data driven hypothesis generation that allows the researcher to focus on what actually matters, and then attribute policies to those identified treatments.

This paper, to the authors knowledge, is the first application of this break detection method to firearm policies. One of the primary motivations for using this method for the analysis of firearm policy is for the aforementioned problem of known treatment timing and the amount of treatments. The firearm policy landscape in the U.S. can be considered similar to that of the carbon emission policies in OECD countries. There are few global policies that affect all countries at the same time, and the identification relies on the country level variation in policy implementation. In the same way, there are very few federal level firearm policies that affect all states at the same time. Rather, there is significant state level variation in firearm policies of which can be exploited by this method. If we were to control every firearm policy that is enacted in the states, we would face the same issue as analyzing carbon reduction policies. We would be left with almost no statistical power due to each state having some level of treatment imposed at every time period. Additionally, when using the traditional difference in differences framework, the untreated group is often not completely untreated. States that adopt one policy, say a mandatory background check for the purchase of a firearm with the intention of barring certain individuals that are considered high risk to perpetrate violence with a firearm to themselves or others, may already have a mix of other policies such as a mandatory waiting period for the purchase of the firearm. We may be comparing states that are both effectively treated and thus not identifying the true effect of policy in question. Furthermore, a common critique of firearm policy related literature is the results are very sensitive to the assumptions made and treatment timing. Often, these assumptions are left untested, and when not included, the results are found to be statistically insignificant (Manski & Pepper, 2018). The break detection method avoids the common pitfall of uncertain treatment timing by restricting the attention to agnostically detected changes in firearm mortalities.

The following section will outline the firearm policy environment in the United States. In the discussion of the firearm policy landscape, policies that have the most evidence for being successful will be discussed, while many policies with inconclusive evidence will be omitted. This is due to the sheer number of policies that have been put in place across the U.S. as well as the nature of this essay being about finding policies that have led to reductions in firearm mortalities. It is important to understand the state level variation in firearm policy implementation, as well as the policies that would be expected to surface as detected breaks in firearm mortalities.

2 Effectiveness of Firearm Policies in the United States

The evidence for firearm policies that achieve a reduction in firearm mortalities is rather inconclusive due to limited and sometimes contradictory evidence concerning specific firearm policies. There are several reasons why this may be the case, without suggesting that firearm policies are actually ineffective at reducing firearm mortalities. The first reason is treatment compliance. When a policy is put into place, the ability of the state to actually enforce it depends substantially on the ability of registration and reporting of not only firearms themselves, but criminal and mental health records. There is limited federal legislation that mandates the reporting of firearm possession and mental health records. In fact, there is no federal or centralized state level firearm registry, in part due to the fact that federal law prohibits the creation of a national registry for firearms. Consequently, only a few states have an official firearm registry and the true number of firearms in circulation relies mainly on surveys and proxies of ownership, such as accidental firearm incidents. While there are federal restrictions on certain mental health events and firearm ownership, the actual reporting of mental health to the Federal Bureau of Investigation's National Instant Criminal Background check (NICS) is voluntary at the state level. Even if reported, there is extreme variation in what or when states report mental health records. Therefore, this creates a disconnect where a person may be legally prohibited but still able to pass a background check because the record never reached the database (Bowen et al., 2021)).

Not only does the difficulty in proper implementation of firearm policies lead to treatment non compliance, another consistent barrier to establishing the true effect of firearm policies is due to the illegal firearm market and the ease of access to firearms through illegal channels. Even if the legislation correctly bars those that are more likely to commit violence with a firearm, the vast majority of firearm homicides are not committed with a legally purchased firearm. In fact, illegally possessed firearms account for roughly 80% of firearm homicides (Braga et al., 2002; Webster et al., 2013). Often, these high risk individuals acquire firearms through their social network, rather than a professional black market. If one with a violent criminal history was looking to purchase a firearm, and the state had a background check in place, why would this individual look to the legal channel for purchasing a firearm? Why would the individual not just have a friend of a friend who meets the requirements purchase the firearm and then buy it off of them. This exact phenomenon has been documented, and it is found that for firearms used in homicides roughly 30% were obtained from a family member or friend (Cook & Pollack, 2017; Webster et al., 2013). This results in the severe undermining of the effectiveness of these “point of sale” policies.

Additionally, spillover between states with weaker gun laws and stricter gun laws is well documented. Knight, 2013 details the flow of firearms across state borders, showing that firearms flow from states with weak gun laws to states with stricter gun laws. As a consequence, it was shown that the effectiveness of state background checks depended on the proximity to a state that did not have similar requirements.

Despite the challenges that exist for the proper implementation of firearm policies, there is promising evidence that certain firearm policies achieve a reduction in firearm mortalities. Among the most successful policies are background checks, mandatory waiting periods between purchase and ownership of a firearm, minimum age requirements for ownership of a firearm, and Child Access Prevention laws (CAP). There is significant variation between states on the policies they chose to impose, which gives way to being able to estimate the state level effects of specific firearm policies and firearm policy mixes.

Background checks are the foundational regulatory mechanism in the United States designed to prevent high risk individuals, such as convicted felons or those with specific violent misdemeanors, from purchasing firearms. At the federal level, the Brady Act required background checks on purchases from licensed firearm dealers. However, many state-level universal background check laws expand this requirement to private sales and transfers, which represent a significant portion of the secondary firearm market. An understanding of the administrative side of the background check is essential to the aforementioned treatment non-compliance. The background check system works by states contributing to the federal NICS database, and then screening individuals at the point of purchase of a firearm. The evidence for the effectiveness of these policies is stronger for reducing firearm homicides than firearm suicides.

The efficacy of background checks is highly dependent on the type of check and how well implemented it is. Background checks that included restraining orders, mental illness, and violent misdemeanors have been found to have significant reductions in both firearm homicides and suicides, though there is significant variation in the size of the results. For the effects on firearm homicides, the effects of strict background checks that included restraining orders and mental illness, range from a 7% to 21% decrease (Sen & Panjamapirom, 2012; Swanson et al., 2016).

For background checks overall, Gius, 2015 and Schell et al., 2024 found that universal background checks are associated with a statistically significant increase in firearm related mortalities in some model specifications, but also found the opposite results once in other model specifications, indicating significant sensitivity to model specification. Schell et al., 2024 found that firearm suicides reduced by 4%. Siegel et al., 2019 found a statistically significant 12% reduction in firearm homicides in urban areas, but uncertain results in rural areas. Similarly, Fridel, 2021 found a significant 20% reduction in firearm homicides after universal background check being implemented.

The primary theoretical reasoning for the effects on firearm suicides being lower than that for firearm homicides is that background checks are able to prohibit those with a registered history of violent offenses, but does not prevent those with clean records and an intent of personal harm from purchasing firearms. Background checks that include a mandated check of state mental health database do see reductions in firearm suicides, but these are quite rare due to limited state reporting to mental health registries. Additionally, these background checks are often implemented as part of a broader policy mix, where identifying the independent effect of the background check in traditional DID methods is difficult. Again, this motivates the use of agnostic break detection, which does not depend on known treatment timing and can attribute the policy mix being enacted to the break.

A mandatory waiting period for the purchase of a firearm has supportive evidence for reducing firearm suicides. These policies enforce a purchase to possession delay that is designed to introduce a cooling off period to prevent impulsive acts of firearm violence. At the federal level, the 1994 Brady Act established a mandatory five day waiting period for all states, but was repealed in 1999. Before the Brady Act, many states already had waiting periods in place with variation in the length of the waiting period, the shortest being a two day waiting period in Wisconsin (which later repealed this in 2015) and the longest being a 15 day waiting period in California, Oregon, and Tennessee. When the Brady Act expired, most states reverted back to their previous waiting periods. While some states continued to impose waiting periods, as of 2013, which is the last year in the dataset used in this paper, 38 states have a waiting period in place.

Edwards et al., 2018, utilize a DID TWFE model to estimate the effect of purchase delays. They find that an implmentation of a purchase delay of three days or longer are associated with a 2% to 5% reduction in firearm suicides, while not finding significant effects on total suicides or firearm homicides. The results from this paper replicate this finding and find the same overall effect of purchase delays on suicides and homicides. Luca et al., 2017 use a very similar methodology and model specification and find similar results, with a 7% to 11% reduction in firearm suicides. A statistically significant reduction in firearm homicides was also found of 18%, which is significantly different than that found in Edwards et al., 2018. This was potentially caused by the differing treatment timing used. Several other modern papers find similar results, despite using increasingly complex methods. Schell et al., 2024 use a Bayesian autoregressive time series model, analyzing purchase delays of seven days or longer, and while the results are not directly comparable to Edwards or Luca, the study suggests that waiting periods in combination with other firearm policies greatly increased the reductions in firearm homicides and suicides. Two studies looked at the repeal of mandatory waiting periods and found that the repeal led to a 3.3% increase in firearm suicides (Donohue et al., 2023; Oliphant, 2022). Oliphant, 2022 used a synthetic control to evaluate the impact of Wisconsin's 2015 repeal, though the construction of the synthetic control group was built using only three control states. Additionally, the firearm suicide to homicide ratio was used as a proxy for gun ownership which essentially uses the outcome to predict the outcome, so the causal validity of this result is uncertain. Overall, a reduction in firearm suicides was robust to several model specifications and datasets, giving supportive evidence of waiting periods being effective in reducing firearm suicides, though the conflicting results on firearm homicides do not fully support waiting periods being effective in reducing firearm homicides.

Other notable firearm policies that have been associated with an effect on firearm mortalities are minimum age requirements, concealed carry laws, and stand your ground laws. Minimum age requirements show statistically significant reductions in firearm suicide rates amoung the 18 - 20 year age group (Andrés & Hempstead, 2011; Webster et al., 2004) and in the 15 - 44 year age group (Kappelman & Fording, 2021). Since these policies affect a rather small subset of the population, it is unlikely that the minimum age requirement will manifest in a detected break. Stand your ground laws are associated with a statistically significant increase in firearm homicides, with no effect seen on firearm suicides (Miller & Pepper, 2020; Schell et al., 2024). Similar effects were found for concealed carry laws. Interestingly, a lot of research on firearm policy is concentrated in concealed carry laws, due to the work of Lott and Mustard, 1997 in which they found that concealed carry laws reduce the number of firearm homicides. However when other researchers replicated the research, the opposite effects were found (Ayres & Donohue, 2003; Black & Nagin, 1998). The bulk of the research is very sensitive to model specification, and inconclusive evidence of the effects of concealed carry laws persist in much of the previous literature. Modern studies have created a substantial amount of evidence that concealed carry laws increase firearm homicides and the type of concealed carry law determines the effect on firearm suicides (Siegel et al., 2019). For example, permit-less carry laws are associated with an 11.1% increase in firearm homicides and a 5% increase in firearm suicides. These effects on firearm suicides became inconclusive when looking at shall issue concealed carry laws, which require a permit to be able to have a concealed firearm.

3 Data

The data used in this paper is a longitudinal dataset sourced from Edwards et al., 2018. The included control variables are common in the literature, which serve to capture the underlying socioeconomic and demographic factors driving firearm related fatalities. These are listed and explained below. Statistical evidence suggests these covariates are robust predictors of both firearm homicide and suicide incidence. The time period consists of 24 years from 1990 to 2013, and includes annual state level observations of total homicides per 100,000 people, firearm homicides per 100,000 people, non-firearm homicides per 100,000 people, total suicides per 100,000 people, firearm suicides per 100,000 people, and non-firearm homicides per 100,000 people. These are the dependent variables of interest, and these variables are log-transformed to be able to interpret the coefficients as percentage changes as is standard across the literature. These data were sourced from the National Centre for Health Statistics (NCHS). The inclusion of non firearm homicides and suicides will be used as a sort of placebo check to ensure that any detected break are specific to firearm regulation rather than general shifts in firearm related mortalities. While it is true that an effective firearm policy can affect non firearm homicides and suicides in addition to non firearm homicides and suicides, extra caution is required in associating these breaks with firearm policies compared to general overall shifts in firearm mortalities.

Control variables for socioeconomic and demographics are included; unemployment rate, which is sourced from the Bureau of Labour Statistics, and real per capita income sourced from the Bureau of Economic Analysis. The demographic variables are sourced from the US census, including the percent of the population that is in an urban area, fraction of the population that is African American, the fraction of the population that are males aged 45-64. Infant mortality rate was sourced from the NCHS. Per capita ethanol consumption sourced from the National Institute on Alcohol Abuse and Alcoholism (NIAAA) to control for the role of alcohol in homicides and suicides, and the proportion of each state's house and senate that is democrat is included to control for the potential political differences in the passage of firearm legislation. Real per capita mental health expenditures are included to control for the relationship between mental health and suicide and firearm violence Ross et al., 2012.

Importantly, a dummy variable for if a mandatory purchase delay is in place for this state in this year is included as a policy control variable. It is coded as treated at the first full year of enactment. Results have been shown to be insensitive to coding them to have the first partial year of enactment considered treated, as well as using the first year as a proportion of the year treated (for example, a law passed in June 2001 would be coded as 0.5 for 2001) (Edwards et al., 2018).

As mentioned previously, there is no comprehensive federal registry of who owns which guns, nor are there common standardized state registries of firearms that are used to measure the stock, flow, or usage of firearms. Therefore, accidental firearm deaths from the NCHS multiple cause of death files are included as a proxy for the stock, flow, and usage of firearms. This is a good proxy for the stock, flow, and usage since accidental firearm deaths are likely a percentage of firearm use and would increase proportionally to usage of firearms. Accidental firearm deaths should not be correlated with firearm homicides or suicides. As noted in both Cook et al., 2009 and Edwards et al., 2018, the measurement of accidental gun deaths can depend on local standards for what constitutes a purposeful firearm death to an accidental one. Under the assumption that these judgements are time invariant, where one state is more likely than another to consider a death an accident, the state fixed effects absorb the difference. If there was a national change that affected how all states classify accidents, then the year fixed effects would absorb this. Most importantly, under the reasonable assumption these judgement calls are not systematically related to the timing of firearm specific legislative shifts, the potential bias would be absorbed with the inclusion of the state and year fixed effects.

The time period these data consists of is over one of the few periods that firearm homicides

National Firearm Suicides and Homicides (1990–2013)

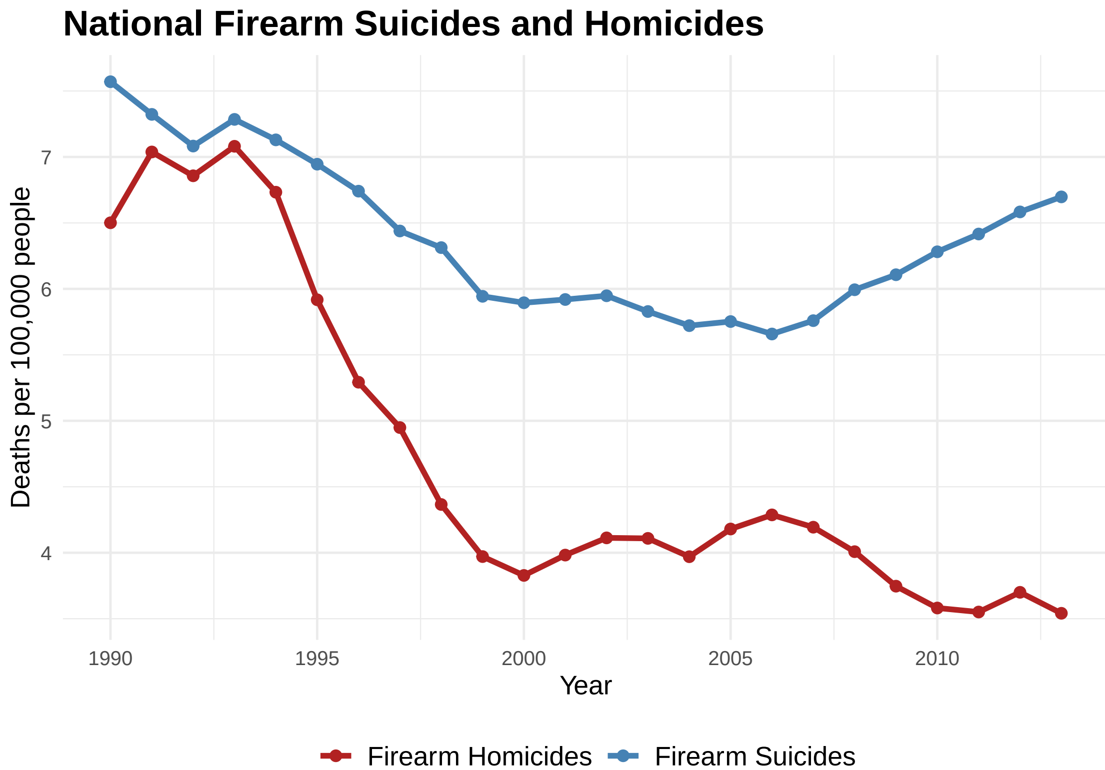

Figure 1: Trends in U.S. firearm mortalities per 100,000 people by type (1990–2013)

and suicides see a decline nationally. Figure 1 shows the national level of firearm homicides and suicides per 100,000 people over the time period used in this paper. The mid to late 1990s to 2013 saw a rapid and persistent decline in firearm mortalities, with firearm homicides falling by roughly 44% by 2000 and continuing a slight negative trend until 2013. This period of time consisted of two major federal legislations of the 1994 Brady Act and the Federal Assault Weapons Ban, as well as broader socioeconomic shifts including the improvement of the crack epidemic, which has was well documented in the literature to drive a significant increase in firearm violence Cook and Laub, 1998; Evans et al., 2022.

As evident in Figure 2, the 1988 to 1996 period saw a significant deviation from a general decline in firearm homicides (Pew Research Center, 2025). The significant time trends seen in the data motivate the use of year fixed effects in our model specification. The omission of the year fixed effects would lead to falsely attributing the drop in firearm homicides to any state that passed a law in this time frame.

National Firearm Suicides and Homicides (1968–2023)

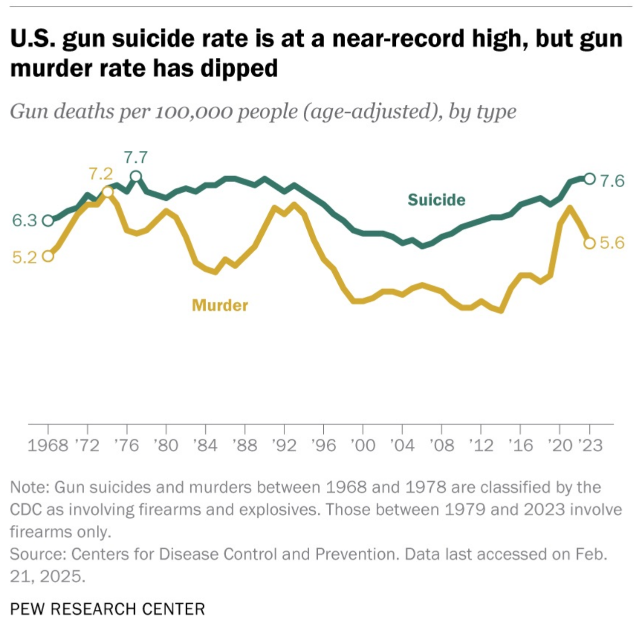

Figure 2: Trends in U.S. firearm mortalities per 100,000 people by type (1968–2023)

4 Methodology

The structural break detection methodology employed in this paper has been developed by Pretis and Schwarz, 2026, and used predominantly in climate econometrics applications such as detecting large reductions in road transport emissions in Koch et al., 2022, detecting effective carbon emissions policies globally in Stechemesser et al., 2024, and analyzing the effectiveness of the BC carbon tax in Pretis, 2022 in both the aggregate and transport sector, among others. In this section, an overview of the econometric foundation of the structural break detection in the panel data setting to detect unknown treatment and to associate these breaks to policies will be provided, using the work of Pretis and Schwarz, 2026. This will be followed by a brief overview of a few notable applications of this method, and then finally the discussion of how this method is applied to the firearm policy landscape and why it is a suitable and promising endeavor.

4.1 Detecting Unknown Treatment as Breaks in Panel Models

The search for structural changes in a variable of interest has evolved from testing pre-specified dates to using a selection method of all dates for discovering these breaks. Using break detection to associate breaks with treatment has been in common use for several decades. In the time series context, methods that test for a break in the data date back to the Chow test in 1960, where the use case was to pre-specify the date of a suspected break and to test for a shift coefficient at that single point. This framework was expanded by Bai and Perron, 1998 which developed a global optimization method to detect multiple breaks at unknown dates in a single time series. An issue that was faced in this methodology was how computationally expensive it was. The computational requirements grew at a rate much quicker than the number of control variables included, and did not handle high dimension data. These issues were eased with the indicator saturations (Hendry et al., 2008). Rather than searching for a few specific breaks at specific points in time, the model is saturated with an indicator for every single observation. These indicators are then selected over with some selection algorithm to keep only the indicators within some statistically significant bound. This has then been extended into the panel data sphere, which allows for the detection of idiosyncratic breaks that are specific to one individual of observation, such as a state or country.

Pretis and Schwarz, 2026 provide the theoretical foundation for the application of the structural break detection method. The main difference between previous work in break detection in time series and panel data and the work of Pretis and Schwarz, 2026 is the ability to identify treatment effects from the detected breaks. In time series, the link to causality is difficult to obtain since there is no control group to be able to compare against. This is fundamental to the idea of recovering treatment effects in the TWFE and DID frameworks. This lack of control group requires “super exogeneity” to maintain causality, which is a strong assumption that is often difficult to establish. Pretis and Schwarz build on the expanding DID with staggered adoption and heterogeneous treatment effect literature, to show that in TWFE settings treatment effects can be consistently estimated by using interactions between treatment times and unit dummies. Therefore, when estimating a TWFE model and searching over the indicators, we are effectively searching for unknown treatment effects. Here, the control group are the units where no structural break has been detected, which means that the detected break can be interpreted as a treatment effect in a well specified model (i.e. that the break was in fact in the variable of interest, not in some omitted variable). In the detection of the treatment effect, the method does not say what the treatment was, only that there was a treatment at that point. This is to say that the detected breaks are merely a call to search for what the treatment was that caused the detected break, a reverse causal question asking: "what treatment could have caused this detected structural break". This framework is fundamentally different from the traditional econometric focus of asking what the effect of a known treatment was. The process of searching for the treatment that caused this effect is essentially the process of finding previously unknown treatments, and to understand and improve the model as a whole (Gelman & Imbens, 2013).

4.2 Agnostic Break Detection: In Search of Effective Firearm Policies

The structural break methodology is primarily hypothesis generating, the search for effective treatments. This lends itself to policy areas in which there is much uncertainty about what treatments have been effective in their goals. As previously discussed, there is little consensus on which firearm policies have been effective in reducing firearm mortalities. The primary motivation of the use of structural break detection in firearm policy analysis is to search for treatments that have potentially not been considered, or work as some verification of the policies that have proven to be effective. Essentially, it is clear that the search for effective firearm policies is not complete and the use of hypothesis generating methods could be very useful. The second reason is that the previous literature on firearm policy commonly utilizes the TWFE framework, which the novel structural break detection method is an extension of. To that end, this essay extends the TWFE model employed by Edwards et al., 2018 to the break detection methodology of Pretis and Schwarz, 2026. This paper has two primary variables of interest, firearm homicides and firearm suicides. Also included are total homicides, total suicides, non-firearm homicides, and non-firearm suicides. The log of each variable will be used as the dependent variable for their own estimation within the break detection framework. Here, model specification will concern firearm suicides, but the model specification is the same for each dependent variable listed above.

The baseline TWFE model as specified in Edwards et al., 2018:

$$\ln(y)_{i,t} = \theta W_{i,t} + \beta \mathbf{X}_{i,t} + g_t + c_i + \varepsilon_{it} \tag{1}$$

Where X is a vector of control variables as specified in the data section of this paper, W is the indicator if a purchase delay policy is in place in state i at time t, g_t represents year fixed effects, and c_i are the state fixed effects. The model is weighted by the population of the state. The $\ln(y)$ variable in this notation is a placeholder for the log-transformed dependent variables of state level firearm homicides per 100,000 people, firearm suicides per 100,000 people, total homicides per 100,000 people, total suicides per 100,000 people, non-firearm homicides per 100,000 people, and non-firearm suicides per 100,000 people are to be included in their respective estimations.

To extend this to the break detection method, the TWFE model is saturated with a full set of step shifts:

$$\ln{(y)}_{i,t} = c_i + g_t + \sum_{j=1}^{N} \sum_{s=2}^{T} \tau_{j,s} \, 1_{\{i=j,\, t \geq s\}} + \beta X_{i,t} + \varepsilon_{i,t} \tag{2}$$

Where $\tau_{j,s}$ are the treatment coefficients, and are under the assumption of sparse treatment, meaning that only the treated states at treatment times have a non zero treatment effect. To put plainly, the sparse treatment assumption is that there are available states to be in the control group.

These models are estimated using the R package “getspanel” (essentially an extension of the “gets” R package to panel data) which utilizes the machine learning General-to-Specific (GETS) block search algorithm to select the breaks. The GETS method and implementation is overviewed in Pretis et al., 2018. The primary advantage of the GETS search algorithm is the ability to specify a level of significance and thereby controlling the false positive rate of detected breaks. The breaks that are then retained by the GETS algorithm are the breaks that are the potential unknown treatment effects. Letting $\hat{H}$ represent the treated units, and $\hat{R}$ as the detected treatment times corresponding to each respective element of treated units $\hat{H}$, the selected breaks post selection is represented as:

$$\ln{(\hat{y})}_{i,t} = \hat{c}_i + \hat{g}_t + \sum_{j \in \hat{H}} \sum_{s \in \hat{R}_j} \hat{\tau}_{j,s} \, 1_{\{i=j,\, t \geq s\}} + \hat{\beta} X_{i,t} \tag{3}$$

Where $\hat{\tau}_{j,s}$ is the estimated step-shift (treatment effect) in the dependent variable for state j at time s. These detected breaks are identified when $\ln{(y)}_{i,t}$ deviates from its predicted counterfactual path (defined by state fixed effects, common time effects, and exogenous control variables in X) by a margin that is statistically significant according to the GETS selection criterion. These shifts can represent either sustained structural changes or, if followed by offsetting breaks, temporary idiosyncratic shocks. Therefore, these breaks are state-specific idiosyncratic breaks, rather than a common break across all states. In essence, federal policies that apply to all states would not be detected unless a specific state has a heterogeneous response to the policy relative to all other states to such a statistically significant degree that it is detected. Additionally, smaller and more gradual policy changes effects are often absorbed rather than being shown as a detected break at each off/on treatment time, unless the effect of the treatment is large enough to be detected.

Both positive and negative detected breaks will be considered since states commonly repeal firearm legislation as well as add to their firearm legislation. This means that the repeal of an effective firearm policy is the same as that state removing its treatment and so the treatment effect to some degree. Additionally, some firearm policies have been found to have a possible increase in firearm mortalities, so both directions of breaks should be considered as treatment dates. Again, effective firearm policies are considered as those that lead to a reduction in firearm mortalities. If a firearm policy leads to an increase in firearm mortalities, it can be considered ineffective.

The policy attribution step will be conducted with the longitudinal database of state firearm laws that is maintained by RAND. The database contains all firearm policies that were enforced at the state level. This includes federal mandates that states enacted, such as the mandatory background checks and purchase delays imposed by the Brady Act. This dataset includes the years from 1979 to 2023, which well encompasses the time period in this paper's data. All state specific firearm legislation that occurs with a three year window around detected break will be considered. All break models have been estimated with a variety of unique sets of control variables. The non-offsetting breaks that are discussed are common across the robustness check specifications, and so are considered sufficiently robust for the purposes of this paper. Again, the main aim of this paper is to apply the novel break detection method from Pretis and Schwarz, 2026 in a different context than it has been applied before, and to be able to go through the hypothesis generating process of attempting to attribute policies to the detected breaks in a reasonable manner.

5 Results

First, the baseline TWFE results that replicate the Edwards et al., 2018 specification for both firearm suicides and firearm homicides are included. This model specification includes all states and all control variables. In Edwards et al., 2018, results were robust across several model specifications, including both linear and quadratic state time trends. State time trends aren't included here. All states and the District of Columbia (the entire sample of this dataset) were included as the baseline TWFE specification. As will be discussed, the baseline specification for the break detection method estimation will contain the 48 contiguous states, and omit the District of Columbia. The primary reason for this is these three states are outliers when it comes to the ration of firearm mortalities between suicides and homicides, geographical differences, and the fact that D.C. is governed essentially by the federal legislative branch, opposed to the state legislative branch.

From Table 1, the coefficient on purchase delays in both model specifications are consistent with that found in Edwards et al., 2018, which shows a 2.9% decrease in firearm suicides significant at the 5% level and no statistically significant coefficient on purchase delays in firearm homicides. This gives confidence that the baseline model specification is consistent with that of previous literature, and will now be extended to the break detection specification.

Table 1: Firearm Suicides and Firearm Homicides (Edwards et al. specification)

Variable	Firearm Suicides	Firearm Homicides
Accidental Firearm Death Rate	0.029***	-0.005
	(0.010)	(0.023)
Accidental Poisoning Rate	0.004***	0.008***
	(0.001)	(0.003)
Brady Act Background Check	0.019	-0.049
	(0.015)	(0.035)
Fraction Black	0.802***	0.861**
	(0.143)	(0.336)
Fraction Male (45-64)	-0.006	0.043***
	(0.004)	(0.009)
Fraction Urban	-0.589***	-0.328
	(0.121)	(0.293)
Infant Mortality Rate	0.000	0.000**
	(0.000)	(0.000)
Log Per Capita Ethanol Consump	0.517***	0.481***
	(0.075)	(0.172)
Purchase Delay (Any)	-0.029**	-0.013
	(0.012)	(0.028)
Real Per Capita Income	-0.007***	0.001
	(0.002)	(0.005)
Real Per Capita Mental Health Exp.	0.000	-0.001**
	(0.000)	(0.000)
State House (% Democrat)	-0.036	0.002
	(0.070)	(0.161)
State Senate (% Democrat)	-0.068	-0.206
	(0.061)	(0.142)
Unemployment Rate	-0.004	0.014*
	(0.004)	(0.008)
Observations	1224	1224

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A few interesting results from Table 1 include consistency with theoretical models of firearm mortalities. Firearm suicides appear to be much more of an issue in more rural areas, with a one percentage point increase in the fraction of a state's population being urban associated with a 0.589% decrease in firearm suicides, which has been documented in the literature. It does not appear that there is much difference between states with a higher percentage of the House or Senate being Democrat in firearm suicides or firearm homicides since the coefficients on the percentage of state house and senate that is democrat are statistically insignificant. Interestingly, the unemployment rate is not statistically significant on firearm suicides, but is positive and weakly statistically significant on firearm homicides, suggesting a 1.4% increase in firearm homicides being associated with a 1% increase in the unemployment rate, holding all else equal. A potential reason for this is that unemployment rates may be more volatile within urban areas, and since firearm suicides are not as common within urban areas, there may be an effect on total suicides but not on firearm homicides. If there was some increase in theft following a rise in unemployment, the lethality in crime from firearm use would also see an uptick (Hawkins, 1997).

The following is the presentation of the empirical results from the structural break detection and discussion. First, the detected breaks in firearm suicides will be discussed and compared against breaks in total suicides and non firearm suicides. The results from the different chosen significance levels will also be presented. The policy attribution step will follow. Then the process will be repeated for firearm homicides.

The breaks that are detected are robust across multiple model specifications and significance levels. The states that are included in the main included tables are the select contiguous states that have a good model fit, as defined by the R^2 for each individual states fitted model. This within R^2 provides a foundational assessment of the proportion of variance in firearm-related outcomes explained by the model. Another potential measure would be the relative RMSE, which would serve as a scale-independent measure of model fit to compare across states. By normalizing the RMSE by the mean of the dependent variable in each state, relative RMSE allows for a consistent comparison of model performance across different states where the firearm homicides and firearm suicides often vary significantly. Together, these metrics provide a measure of the descriptive power of the baseline specification, and to give confidence that detected structural breaks are embedded within a model that has relatively high descriptive power and are not merely spurious breaks. For future analysis and broader application of this structural break detection method, this paper proposes that some form of model fit diagnostic testing is employed to provide further confidence and justification of detected breaks.

5.1 Step Breaks: Firearm Suicides

From Table 2, we see a potential issue in the fact that there are many offsetting breaks within specific states that are detected. The offsetting breaks are usually year to year, such as in District of Columbia where the 1998 to 2002 breaks are yearly. These offsetting breaks can be interpreted as impulses rather than true step breaks. As noted in Pretis and Schwarz, 2026, model fit is extremely important to be able to distinguish between true structural breaks and spurious breaks. This motivates the analysis of the model fit, and whether or not this model specification is truly capturing the individual state's data. One thing to note is that the states that have these multiple offsetting breaks have low populations, which may explain why these states have much larger volatility than states with larger populations. These detected breaks then, are detecting the year to year volatility in impulses, which is why they offset.

The coefficient on the known policy now suggests a purchase delay policy is associated with a 3.3% reduction in firearm suicides, and is significant at the 1% significance level. This is very similar to the baseline model, which suggested a 2.9% decrease in firearm suicides, statistically significant at the 5% significance level.

First, the sample is restricted to the 48 contiguous states, without the District of Columbia. This improves model fit measured by R^2 across all states (as seen in Table 8), and other individual non-offsetting breaks in other states are discovered. The large volatility in the District of Columbia seems to have crowded out the power to detect these other breaks. The exclusion of Alaska, Hawaii, and the District of Columbia is three fold. First, their divergent firearm mortality profiles, specifically the extreme variance in suicide to homicide ratios compared to the national average. For instance, Alaska's firearm mortalities are almost entirely consist of suicides. Second, the geographic isolation of Alaska and Hawaii, which effectively makes these states immune from spillover effects from other states. Thirdly, the unique legal

Table 2: Step Breaks: Firearm Suicides (All States)

Variable	Policy Timing Included	No Policy Timing Included
Alaska (1993, Step)	0.399***	0.387***
	(0.061)	(0.062)
Alaska (2000, Step)	0.141***	0.151***
	(0.045)	(0.045)
Arkansas (1994, Step)	0.199***	
	(0.048)	
Arkansas (2000, Step)		0.147***
		(0.038)
Delaware (1999, Step)	0.172***	0.166***
	(0.041)	(0.041)
Delaware (2008, Step)	-0.169***	-0.166***
	(0.049)	(0.049)
DistrictofColumbia (1998, Step)	-0.330***	-0.333***
	(0.097)	(0.097)
DistrictofColumbia (1999, Step)	0.601***	0.588***
	(0.120)	(0.120)
DistrictofColumbia (2000, Step)	-0.636***	-0.632***
	(0.120)	(0.120)
DistrictofColumbia (2001, Step)	1.071***	1.070***
	(0.119)	(0.120)
DistrictofColumbia (2002, Step)	-0.563***	-0.561***
	(0.104)	(0.104)
DistrictofColumbia (2004, Step)	-0.766***	-0.763***
	(0.104)	(0.104)
DistrictofColumbia (2005, Step)	0.801***	0.808***
	(0.096)	(0.096)
DistrictofColumbia (2009, Step)	-0.698***	-0.691***
	(0.096)	(0.096)
DistrictofColumbia (2010, Step)	0.839***	0.839***
	(0.122)	(0.123)
DistrictofColumbia (2011, Step)	-0.456***	-0.456***
	(0.105)	(0.105)
DistrictofColumbia (2013, Step)	-0.420***	-0.420***
	(0.103)	(0.104)
Hawaii (1994, Step)	0.382***	0.380***
	(0.074)	(0.075)
Hawaii (1996, Step)	-0.320***	-0.318***
	(0.071)	(0.071)
Hawaii (2002, Step)	-0.192***	-0.198***
	(0.049)	(0.049)
Hawaii (2009, Step)	0.357***	0.356***
	(0.054)	(0.054)
Hawaii (2013, Step)	-0.454***	-0.454***
	(0.095)	(0.095)
Maine (2001, Step)	-0.116***	-0.118***
	(0.042)	(0.042)
Nevada (2000, Step)	-0.179***	-0.172***
	(0.039)	(0.039)
NorthDakota (1993, Step)	0.277***	0.272***
	(0.054)	(0.054)
RhodeIsland (2003, Step)	-0.809***	-0.815***
	(0.088)	(0.088)
RhodeIsland (2004, Step)	0.648***	0.646***
	(0.093)	(0.093)
RhodeIsland (2009, Step)	0.525***	0.525***
	(0.093)	(0.093)
RhodeIsland (2010, Step)	-0.507***	-0.508***
	(0.095)	(0.095)
SouthDakota (1993, Step)	0.254***	0.259***
	(0.059)	(0.059)
SouthDakota (2000, Step)	-0.306***	-0.313***
	(0.059)	(0.059)
SouthDakota (2003, Step)	0.288***	0.287***
	(0.065)	(0.065)
SouthDakota (2007, Step)	-0.248***	-0.230***
	(0.054)	(0.054)
Texas (1995, Step)	-0.177***	-0.181***
	(0.045)	(0.045)
Vermont (1994, Step)	-0.459***	-0.465***
	(0.095)	(0.096)
Vermont (1995, Step)	0.313***	0.306***
	(0.089)	(0.090)
Vermont (2009, Step)	0.250***	0.254***
	(0.055)	(0.056)
Vermont (2012, Step)	-0.309***	-0.310***
	(0.077)	(0.078)
Purchase Delay (Any)	-0.033***	
	(0.011)	
Observations	1224	1224

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

status of the District of Columbia. Unlike the 50 sovereign states, the District's governance is subject to federal legislative oversight under the D.C. Home Rule Act. This lack of independent legislative sovereignty creates a fundamentally different policy making mechanism than the state level processes analyzed in this study. The non-offsetting breaks at the 1% and 0.1% significance levels from using the sample of 48 contiguous states are shown below in Table 3.

Table 3: Comparison of GETS Thresholds for Step Breaks: Firearm Suicides

Variable	GETS 0.01	GETS 0.001
Arkansas (1994, Step)	0.212***	0.199***
	(0.043)	(0.046)
Colorado (2000, Step)	0.102***	
	(0.034)	
Connecticut (1996, Step)	-0.124***	
	(0.039)	
Maine (2001, Step)	-0.118***	
	(0.039)	
Massachusetts (1999, Step)	-0.138***	
	(0.035)	
Nebraska (2003, Step)	-0.098***	
	(0.032)	
Nevada (2001, Step)	-0.172***	-0.184***
	(0.035)	(0.037)
NorthDakota (1993, Step)	0.283***	0.283***
	(0.048)	(0.051)
Texas (1995, Step)	-0.164***	-0.183***
	(0.040)	(0.043)
WestVirginia (1992, Step)	0.168***	
	(0.058)	
Accidental Firearm Death Rate	0.021**	0.029***
	(0.009)	(0.009)
Accidental Poisoning Rate	0.007***	0.006***
	(0.001)	(0.001)
Brady Act Background Check	0.011	0.021
	(0.014)	(0.014)
Fraction Black	0.765***	0.725***
	(0.128)	(0.133)
Fraction Male (45-64)	0.002	-0.006
	(0.004)	(0.004)
Fraction Urban	-0.608***	-0.627***
	(0.116)	(0.112)
Infant Mortality Rate	0.000***	0.000**
	(0.000)	(0.000)
Log Per Capita Ethanol Consump	0.440***	0.480***
	(0.069)	(0.072)
Purchase Delay (Any)	-0.029***	-0.033***
	(0.010)	(0.011)
Real Per Capita Income	-0.006**	-0.006***
	(0.002)	(0.002)
Real Per Capita Mental Health Exp.	0.000	0.000*
	(0.000)	(0.000)
State House (% Democrat)	0.086	0.068
	(0.063)	(0.066)
State Senate (% Democrat)	-0.118**	-0.143**
	(0.058)	(0.059)
Unemployment Rate	-0.003	-0.004
	(0.003)	(0.004)
Observations	1152	1152

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

There are 10 non-offsetting breaks detected at the target significance level $\gamma_\alpha = 0.01$ (1%), and 4 at the stringent $\gamma_\alpha = 0.001$ (0.1%) significance level. The detected negative breaks range from (-0.172 or about 17.2% decrease in firearm suicides) in Nevada in 2001, to 0.283 (28.3% increase in firearm suicides) in Nebraska in 1993. As expected, the largest of the detected breaks remain at the 0.1% significant level. In Figure 3, the observed log-transformed firearm suicides per 100,000 people is represented by the black line, the model's fitted line is shown in blue, and the detected step breaks in the TWFE model is shown in the vertical red line.

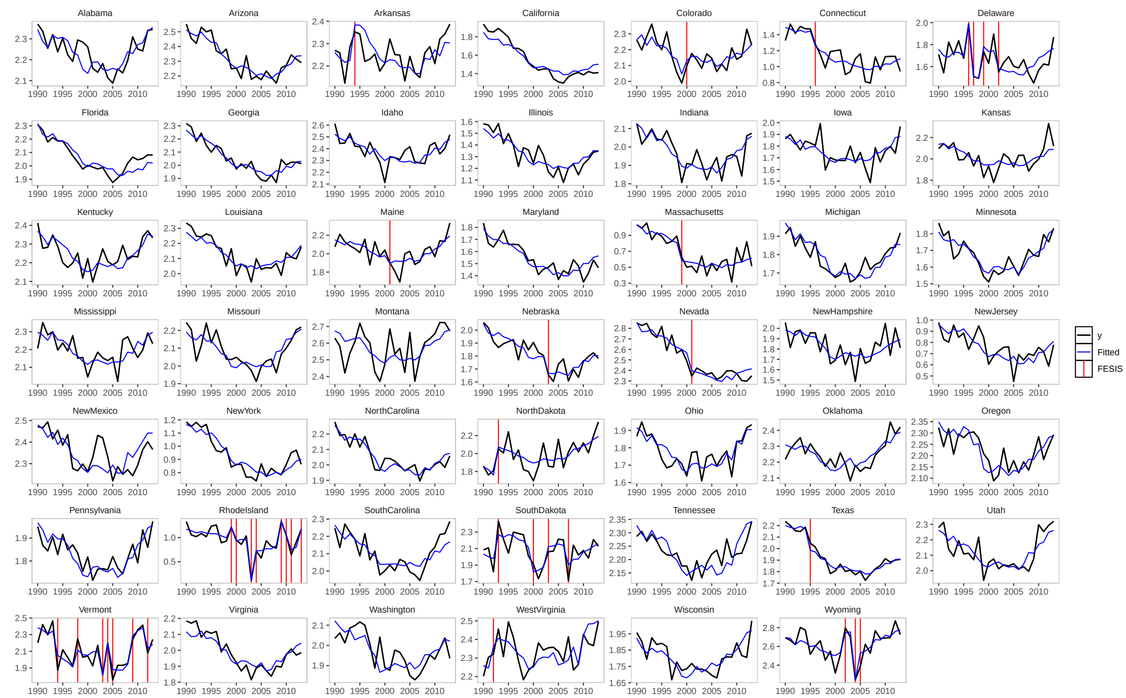

Figure 3: Model Fit and Detected Step Breaks in Firearm Suicides

Figure 3 provides a visual of the model fit for each individual state in the sample. We can see that the model successfully captures the overall trend of each states firearm suicides, but struggles with accounting for the high year to year volatility in some of the states. Interestingly, we see that in the states where several offsetting breaks are detected that the model fit is quite high. Overall, the model's R^2 ranges from a high of 0.96 in California, to a low of 0.22 in West Virginia. Approximately half of the state's fitted models have an R^2 of above 0.6 as shown in Table 8.

Table 4 shows whether or not firearm suicides change with non firearm suicides. As shown in the table, for each of the 10 non-offsetting detected step breaks in firearm suicides at the

1% significance level, none strongly coincide with a step break in non firearm suicides. This indicates that the detected non-offsetting step breaks in firearm suicides are independent and specific to firearms themselves. Breaks in firearm suicides often coincide with breaks in total suicides, which is reasonable considering that over 60% of total suicides are firearm suicides (Gramlich, 2025).

Since the step break detection approach is used, gradual changes in suicides and homicides are not explicitly captured. Policies that have some gradual effect over time can still be detected, but this "trend" break will not be consistently measured. With the stock of firearms being difficult to control due to the illegal firearm market, some firearm policies that target the supply of firearms may be overlooked by the detected step breaks. However, in the long run, the effects of these policies may lead to detected step breaks as significant changes to firearm mortalities if there is some change in the level of enforcement. To illustrate, consider the detected step break in Connecticut in 1996. The state enacted a stricter policy than the federal Brady Act in 1994 and enforced it in 1995. The Connecticut firearm statute was amended by Public Act 94-1, strictly enforcing a waiting period of two weeks (longer than the mandated 5 day waiting period), background checks for purchases of firearms, registration of firearm sales, specifically prohibiting anyone convicted of any felony and specific violent misdemeanors, and expanded requirements for obtaining permits to sell and carrying firearms (Connecticut Public Act No. 94-1, 1994). The detected step break of (-0.124) indicates a -12.4% level shift in firearm suicides relative to all other states. These sorts of background checks that have stringent rules and ability to enforce the proper record checks have been found to be associated with reductions in firearm suicides (Sen & Panjamapirom, 2012). Specifically background checks with strict requirements, such as prohibiting those with violent misdemeanors rather than only felony records, are found to have a larger reduction in firearm suicides.

Table 4: Step breaks: Total Suicides vs Firearm Suicides vs Non-Firearm Suicides

State (year)	Total Suicide	Firearm Suicides	Non-Firearm Suicides
Alabama (1991, Step)			0.379*** (0.090)
Alabama (2001, Step)	-0.116*** (0.027)		
Arkansas (1992, Step)			0.428*** (0.067)
Arkansas (1994, Step)	0.173*** (0.033)	0.212*** (0.043)	
Arkansas (2004, Step)			0.114*** (0.039)
California (1996, Step)	-0.112*** (0.028)		
Colorado (2000, Step)		0.102*** (0.034)	
Connecticut (1996, Step)		-0.124*** (0.039)	
Idaho (2008, Step)			0.135*** (0.046)
Kansas (1996, Step)			0.168*** (0.042)
Louisiana (1998, Step)	-0.084*** (0.025)		
Maine (1998, Step)	0.167*** (0.045)		0.304*** (0.069)
Maine (2000, Step)	-0.256*** (0.045)		-0.354*** (0.069)
Maine (2001, Step)		-0.118*** (0.039)	
Maryland (1992, Step)			0.225*** (0.065)
Maryland (1993, Step)	0.096*** (0.036)		
Massachusetts (1999, Step)		-0.138*** (0.035)	
Massachusetts (2000, Step)	-0.104*** (0.029)		
Massachusetts (2007, Step)	0.131*** (0.032)		
Minnesota (1996, Step)			-0.163*** (0.044)
Mississippi (2001, Step)			0.165*** (0.036)
Nebraska (2003, Step)	-0.117*** (0.024)	-0.098*** (0.032)	
Nebraska (2009, Step)			-0.239*** (0.055)
Nebraska (2012, Step)			0.262*** (0.079)
Nevada (2001, Step)	-0.118*** (0.027)	-0.172*** (0.035)	
Nevada (2007, Step)			-0.173*** (0.044)
Nevada (2011, Step)	-0.121*** (0.038)		
NewJersey (1996, Step)	0.094*** (0.029)		
NewYork (2006, Step)	0.112*** (0.031)		
NorthDakota (1993, Step)	0.162*** (0.036)	0.283*** (0.048)	
NorthDakota (2008, Step)			-0.421*** (0.092)
NorthDakota (2009, Step)			0.364*** (0.096)
Oklahoma (1991, Step)			0.363*** (0.090)
Oklahoma (2007, Step)			-0.130*** (0.043)
Tennessee (1996, Step)			0.177*** (0.043)
Texas (1995, Step)		-0.164*** (0.040)	
Texas (1997, Step)	-0.100*** (0.027)		
Utah (2009, Step)	0.091*** (0.030)		
WestVirginia (1992, Step)		0.168*** (0.058)	
WestVirginia (1993, Step)	0.150*** (0.037)		
WestVirginia (1996, Step)			0.173*** (0.047)
Observations	1152	1152	1152

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This appears to be exactly what is happening here, where Connecticut imposed one of the strictest background checks and increased waiting period at the same time as other states had a shorter waiting period and either did not impose the strict background checks or did not have the necessary record sharing employed at that time. The fact that this break disappears at the 0.1% significance level does warrant some caution on the confidence of causality. However, the fact that this breaks is corroborated in the literature does show the hypothesis generating nature of this method where it warrants further analysis about how the stringency and ability to enforce a specific policy may determine the effectiveness of it.

Another break of interest is Massachusetts 1999 (-0.138) break which is a negative level shift of 13.8% in firearm suicides. In 1998, the Massachusetts Gun Control Act was passed. This greatly increased the overall stringency of firearm legislation in Massachusetts by overhauling the previous permitting system that was required for most firearms. It introduced a tiered permitting structure based on weapon class, allowing permits for concealed carry handguns but not for other firearms. Access to weapons not deemed for hunting or self defense became subject to strict licensing approval processes. It gave authority to local municipalities to deny firearm access to those they deemed unsuitable, without even a criminal record check. This permit system is classified as "may-issue", where the application for a permit does not have to be approved even if all requirements are met, which includes mandatory firearm training. The second big change was that it is the first state act to mandate that all firearms not under immediate control of the owner be stored in a locked container. Unlike other states Child Access Prevention (CAP) laws, which have been shown to be effective at reducing firearm suicides amoung those aged under 21 (Azad et al., 2020; DeSimone et al., 2013), the Massachusetts law was universal and applied whether or not a child was present in the home. These sorts of may-issue laws have been found to reduce firearm homicides compared to shall-issue laws (Siegel et al., 2019). Specifically, Kahane and Sannicandro, 2019 use synthetic controls and found a statistically significant reduction in firearm suicides and total suicides. Firearm suicide rates fell in 1999 and remained below

1998 trend levels, while overall suicide rates initially fell at the same time but recovered to the 1998 trend levels. This is exactly what is shown in Table 4. We see the reduction in total suicides in 2000, but then find an offsetting break in 2007. No break was detected in non-firearm suicides around this time, which gives further confidence that this break was due to a firearm specific policy and not a change in overall non firearm suicides.

5.2 Step Breaks: Firearm Homicides

Table 5 shows a similar story to the detected breaks in firearm suicides when all 50 states and the District of Columbia is included. Interestingly, the inclusion of the purchase delay timing seems to significantly reduce the number of detected step breaks, and primarily the number of detected offsetting breaks. This gives further confidence for including the purchase delay policy timing in the baseline model specification for firearm homicides. North Dakota, South Dakota, and Vermont are again the states that have the most detected offsetting breaks, just as in the firearm suicides specification. It appears that these states have something specific to them that increases their year to year volatility that the model captures in impulse breaks.

In the baseline TWFE model Table 1, the estimated effect of a purchase delay of any length was -0.013 (-1.3%) and not statistically significant at the 10% level which is similar to what was found in Edwards et al., 2018. When the TWFE is extedend to the structural break detection model and search for additional breaks, there is a nominal increase in the point estimate to -0.032 suggesting a 3.2% reduction in firearm homicides, though remains statistically insignificant.

Table 5: Step Breaks: Firearm Homicides

Variable	Policy Timing Included	No Policy Timing Included
Arizona (1994, Step)		0.428***
		(0.101)
Delaware (2002, Step)	0.659***	0.648***
	(0.093)	(0.084)
DistrictofColumbia (2010, Step)	-0.667***	-0.848***
	(0.129)	(0.116)
Hawaii (2003, Step)	-0.615***	-0.437***
	(0.088)	(0.084)
Hawaii (2011, Step)		-1.013***
		(0.188)
Hawaii (2012, Step)		0.813***
		(0.217)
Idaho (2008, Step)	-0.434***	-0.481***
	(0.103)	(0.092)
Maine (2006, Step)		0.513***
		(0.095)
Nebraska (2007, Step)	0.474***	0.463***
	(0.092)	(0.081)
NewJersey (2002, Step)		0.619***
		(0.081)
NewJersey (2003, Step)	0.607***	
	(0.091)	
NewYork (1995, Step)	-0.588***	-0.547***
	(0.108)	(0.096)
NorthDakota (1992, Step)		0.710***
		(0.178)
NorthDakota (1994, Step)		-1.183***
		(0.217)
NorthDakota (1995, Step)		0.978***
		(0.217)
NorthDakota (1997, Step)	-0.951***	-1.173***
	(0.215)	(0.217)
NorthDakota (1998, Step)	1.144***	1.258***
	(0.214)	(0.190)
NorthDakota (2005, Step)		-0.733***
		(0.190)
NorthDakota (2006, Step)	-1.771***	-1.122***
	(0.214)	(0.250)
NorthDakota (2007, Step)	1.899***	1.806***
	(0.220)	(0.195)
Ohio (2005, Step)	0.365***	0.388***
	(0.086)	(0.077)
Oklahoma (2011, Step)		0.293***
		(0.113)
Pennsylvania (1997, Step)	0.367***	0.427***
	(0.100)	(0.086)
RhodeIsland (1999, Step)		0.411***
		(0.092)
RhodeIsland (2006, Step)		-0.407***
		(0.099)
SouthDakota (1992, Step)		-0.754***
		(0.218)
SouthDakota (1993, Step)		1.396***
		(0.250)
SouthDakota (1994, Step)		-0.902***
		(0.195)
SouthDakota (1999, Step)		0.847***
		(0.194)
SouthDakota (2000, Step)	-1.466***	-2.139***
	(0.211)	(0.251)
SouthDakota (2001, Step)	1.517***	1.524***
	(0.215)	(0.190)
SouthDakota (2008, Step)	0.623***	0.613***
	(0.162)	(0.143)
SouthDakota (2010, Step)	-0.824***	-0.833***
	(0.176)	(0.154)
Texas (1995, Step)		-0.398***
		(0.094)
Vermont (1994, Step)	-1.731***	-1.733***
	(0.226)	(0.199)
Vermont (1995, Step)	1.385***	1.421***
	(0.211)	(0.189)
Vermont (2004, Step)		-0.962***
		(0.188)
Vermont (2005, Step)		1.332***
		(0.198)
Vermont (2009, Step)	-1.316***	-1.626***
	(0.155)	(0.154)
Vermont (2011, Step)	1.096***	1.082***
	(0.183)	(0.162)
Wyoming (1991, Step)		-0.787***
		(0.184)
Purchase Delay (Any)	-0.013	
	(0.028)	
Observations	1224	1224

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Comparison of GETS Thresholds for Step Breaks: Firearm Homicides

State (Year of break)	GETS 0.01	GETS 0.001
Arizona (1994, Step)	0.448***	
	(0.100)	
Delaware (2002, Step)	0.380***	0.661***
	(0.133)	(0.092)
Delaware (2004, Step)	0.393***	
	(0.139)	
Idaho (2008, Step)	-0.437***	-0.437***
	(0.092)	(0.101)
Nebraska (2007, Step)	0.469***	0.494***
	(0.080)	(0.089)
NewJersey (2002, Step)	0.623***	0.567***
	(0.080)	(0.088)
NewYork (1995, Step)	-0.567***	-0.691***
	(0.097)	(0.106)
Ohio (2002, Step)	0.397***	0.401***
	(0.072)	(0.081)
Pennsylvania (1997, Step)	0.395***	
	(0.090)	
RhodeIsland (1999, Step)	0.408***	
	(0.087)	
RhodeIsland (2007, Step)	-0.444***	
	(0.097)	
Texas (1995, Step)	-0.403***	
	(0.091)	
Wyoming (1991, Step)	-0.788***	
	(0.180)	
Accidental Firearm Death Rate	-0.011	-0.031
	(0.020)	(0.022)
Accidental Poisoning Rate	0.009***	0.010***
	(0.003)	(0.003)
Brady Act Background Check	-0.053*	-0.050
	(0.032)	(0.034)
Fraction Black	0.552*	0.802**
	(0.307)	(0.334)
Fraction Male (45-64)	0.021**	0.016
	(0.010)	(0.010)
Fraction Urban	0.021	-0.032
	(0.278)	(0.278)
Infant Mortality Rate	0.000	0.000*
	(0.000)	(0.000)
Log Per Capita Ethanol Consump	0.545***	0.576***
	(0.160)	(0.174)
Purchase Delay (Any)	-0.032	-0.034
	(0.025)	(0.026)
Real Per Capita Income	0.019***	0.008
	(0.005)	(0.005)
Real Per Capita Mental Health Exp.	-0.001***	0.000
	(0.000)	(0.000)
State House (% Democrat)	0.061	-0.002
	(0.148)	(0.164)
State Senate (% Democrat)	-0.328**	-0.308**
	(0.130)	(0.142)
Unemployment Rate	0.026***	0.019**
	(0.008)	(0.008)
Observations	1152	1152

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

There are 13 non-offsetting breaks detected at the $\gamma_\alpha = 0.01$ (1%) significance level, and 6 at the stringent $\gamma_\alpha = 0.001$ (0.1%) significance level. The magnitude of the detected breaks in firearm homicides are larger than those in firearm suicides, ranging from (-0.788 or about a 78.8% reduction in firearm homicides) in Wyoming in 1991 to a 0.623 which is about a 62.3% increase in firearm homicides New Jersey in 2002.

The model fit of the baseline specification, which includes the 48 contiguous states, omitting Alaska, Hawaii, and the District of Columbia is very similar to that of the model fit seen in Figure 3. The observed log of firearm homicides per 100,000 people is shown in

Table 7: Step Breaks: Total Homicides vs Firearm Homicides vs Non-Firearm Homicides

Variable	Total Homicide	Firearm Homicides	Non-Firearm Homicides
Arizona (1994, Step)		0.448***	
		(0.100)	
California (1996, Step)	-0.314***		
	(0.066)		
Delaware (1998, Step)			-0.377***
			(0.087)
Delaware (2002, Step)		0.380***	
		(0.133)	
Delaware (2004, Step)		0.393***	
		(0.139)	
Delaware (2005, Step)	0.605***		
	(0.066)		
Idaho (1992, Step)	0.574***		0.667***
	(0.103)		(0.143)
Idaho (2008, Step)	-0.230***	-0.437***	
	(0.069)	(0.092)	
Nebraska (1997, Step)			0.413***
			(0.086)
Nebraska (2007, Step)	0.345***	0.469***	
	(0.062)	(0.080)	
NewJersey (2001, Step)	1.098***		1.556***
	(0.141)		(0.192)
NewJersey (2002, Step)	-0.788***	0.623***	-1.709***
	(0.142)	(0.080)	(0.194)
NewYork (1995, Step)	-0.548***	-0.567***	
	(0.083)	(0.097)	
NewYork (2001, Step)	0.985***		1.474***
	(0.147)		(0.194)
NewYork (2002, Step)	-0.999***		-1.637***
	(0.143)		(0.195)
Ohio (1999, Step)			0.354***
			(0.080)
Ohio (2002, Step)	0.339***	0.397***	
	(0.056)	(0.072)	
Pennsylvania (1997, Step)		0.395***	
		(0.090)	
RhodeIsland (1999, Step)		0.408***	
		(0.087)	
RhodeIsland (2007, Step)		-0.444***	
		(0.097)	
Texas (1995, Step)	-0.351***	-0.403***	
	(0.070)	(0.091)	
WestVirginia (2005, Step)			0.430***
			(0.092)
Wyoming (1991, Step)		-0.788***	
		(0.180)	
Wyoming (1992, Step)			0.763***
			(0.160)
Observations	1152	1152	1152

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

black, and the fitted values are shown in blue. The detected step breaks are shown as the red vertical lines to signify the year of the break. The overall trends in firearm homicides by state is well captured by the model, and the year to year variation is well captured in several states.

Table 7 shows the detected breaks in total homicides, firearm homicides, and non-firearm homicides at the 1% significance level. As in Table 4, the detected breaks in firearm homicides typically do not coincide with that of non-firearm homicides. In New Jersey 2002, the detected break in firearm homicides is opposite that of the detected negative break (of very large magnitude) in non-firearm homicides. There are five detected negative breaks that are

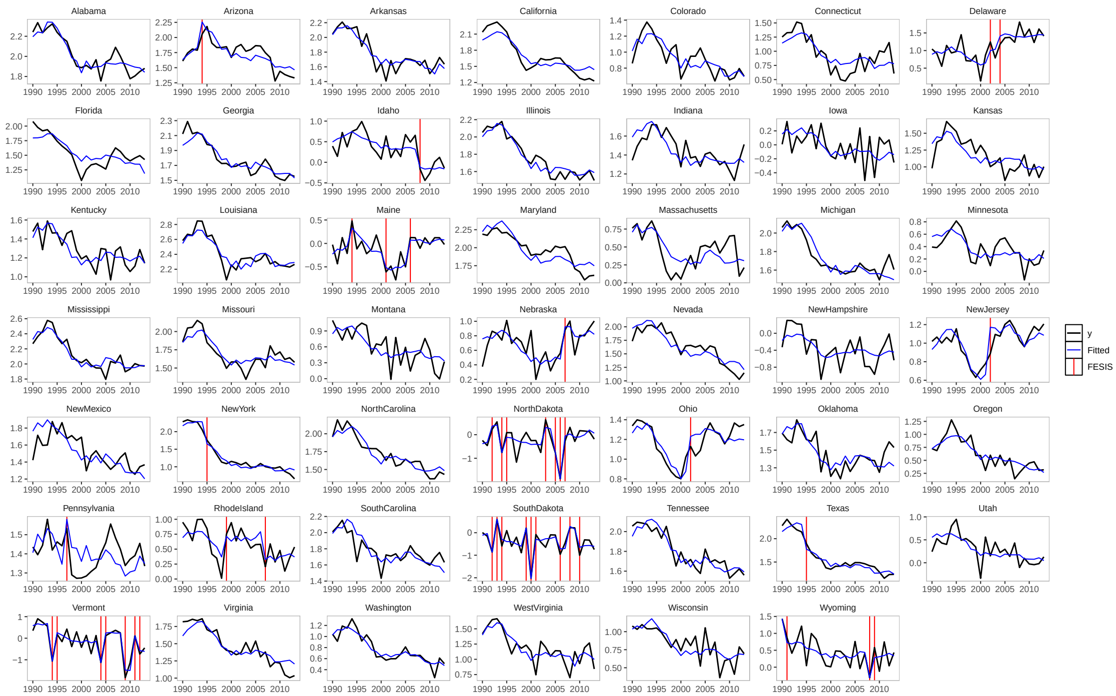

Figure 4: Model Fit and Detected Step Breaks in Firearm Homicides

negative at the 1% significance level, and eight detected positive breaks.

Texas's 1995 (-0.164) break in firearm suicides and (-0.403) break in firearm homicides perfectly correlates with the enactment of Senate Bill 60, which established the concealed handgun licensing system. Before 1995, it was illegal to carry a firearm unless on your own property, or "traveling". The legal definition prior to this legislation was quite vague in regards to what "traveling" meant. With this, there were plenty of instances of firearm homicides outside of the perpetrators residence, and whether or not it was considered to be a true concealed firearm and thus a felony offense was entirely up to the court's discretion. In effect, the enforcement of the no concealed carry was not strictly enforced. Under the new legislation, an age requirement of 21 and older, as well as mandatory training and a proficiency test was required to receive the permit. However, the actual purchase of a firearm was now regulated by the Brady Act that mandated a background check and 5 day waiting period, which Texas did not have before. The attribution of the concealed carry law to the detected decrease in firearm suicides and homicides is not very strong considering the multitude of legislative changes that occurred around 1995. Perhaps a stronger link is observed with the combination of this policy with the mandatory waiting period, background check, and age requirements that has more support from previous literature. Again, this break can be considered as a heterogeneous treatment effect specific to Texas in response to the similar treatment found in other states. This potentially arises from the fact that since firearm legislation in Texas was relatively lax compared to many states at the time, any significant increase in policy stringency may have a proportionally large increase in its effect.

Ohio changed from a prohibited concealed carry to a shall issue state in 2004. While this is two years after the detected break of about 39.7% in 2002, and also corresponds to the Cincinnati riots of 2001, and the subsequent increase in firearm homicides around that time. It is likely that this detected break is not solely from a policy change, but rather in large part to other social factors that occurred at the same time. Additionally, there was a detected increase in non-firearm homicides in 1999 of about 35.4%, similar to the sign and magnitude of the break in 2002, which in conjunction with the social changes suggest that a broader change to homicides overall. Therefore, we cannot say that this detected break is definitively from the implementation of a shall-issue concealed carry law, and this detected increase in homicides is likely from other social factors.

6 Conclusion

Firearm mortalities are a large portion of total mortalities in the U.S., and finding effective policies that regulate firearm use and lead to a reduction in firearm violence has proven difficult. Traditional forward causal models have been subject to sensitivity to model specification and known timing bias. This paper uses the novel TWFE break detection framework of Pretis and Schwarz, 2026 to examine state level firearm mortalities from a reverse causal standpoint to better understand what has lead to reductions in firearm suicides and homicides. Several detected breaks in firearm mortalities that were selected by the machine learning algorithm GETS could reasonably be associated with firearm specific policies.

Of all breaks that were detected, this paper found 3 breaks in firearm homicides and suicides that were matched with policies that came into effect within a two year window of each break. Many of these policies that were found to lead to significant reductions mirrored what has been shown in the literature, such as stringent background checks, minimum age laws, storage requirements, and purchase delays.

One of the benefits seen in this method is detailed in the way these policies are associated with the detected breaks. Groups of states often implement similar policies, though with varying degrees of stringency. In the traditional literature the specifics of each state's firearm policy, such as the policies enacted near the same time or in combination with, is often not considered. Generally, the broad policy classification is considered as the treatment, but not the combinatorial effects of a policy mix. Given that this paper's main aim was to apply the novel break detection method to a new context and to assess its ability to detect effective firearm policies, it provides promising results for the further use of this method in the analysis of firearm policies. While many firearm policies are likely to have small effects on firearm homicides and suicides, several policies idendefied as effective in the literature were detected in this paper, alongside providing a unique discussion on the specifics of firearm policies and the varying effects these policies have in different states. This highlights the effectiveness of the hypothesis generating capabilities of this method. An interesting observation from this paper is that policies that were able to be associated with the detected breaks in firearm homicides and suicides were not necessarily broad restrictions to access. Rather, these policies were similar in how and who access to firearms was prohibited. Additionally, these associated policies often consist of the proper training in the use of firearms as well as storage requirements. While there is significant correlation between rates of firearm ownership and firearm mortalities, the United States is predominantly an outlier in rates of firearm mortalities, not firearm ownership. A possible reason for this discrepancy is that countries with high rates of firearm ownership are those with mandatory military service, which includes extensive training in the use of firearms. Therefore, with the supportive evidence of the effectiveness of policies enforcing firearm training as a prerequisite for firearm ownership presented in this paper, as well as the observational evidence regarding firearm training in other countries, firearm training appears to be a strong candidate to include in effective firearm policy mixes to reduce firearm mortalities. The inclusion of idiosyncratic structural breaks does not alter the fundamental finding from the baseline TWFE specification regarding purchase delays. While the point estimate for homicides remains nominally negative, the lack of statistical significance at the 10% significance level persists. This suggests that the large magnitude reductions in firearm homicides detected in specific states are driven by factors distinct from, or much more complex than, the presence of a mandatory waiting period alone.

Future research using this method on firearm policies could include the use of trend breaks to potentially detect policies that do not have sharp discontinuities, but rather a gradual change in the trends of firearm mortalities. Additionally, addressing model fit and determining the statistical effect of model fit is suggested, given that this method relies on detecting changes in the fitted model. Further addressing concerns of spurious breaks with the several offsetting "impulse" breaks would also provide more confidence in the overall results.

Overall, there is growing evidence of the effectiveness of firearm policies, and this paper's conclusion is that it is unlikely that there is a single "one shot" solution to firearm mortalities. Since firearm policy implementation is staggered and subject to high levels of unobserved heterogeneity, the ability to identify idiosyncratic state level changes is indispensable. By "discovering what mattered", the agnostic break detection of structural breaks provides a partisan free approach to not prove a predetermined idea, but to identify what actually works.

A Supplemental Tables and Robustness Checks

Table 8: Firearm Suicides: Comparison of Within-State R^2: Full Sample (1) vs. Contiguous 48 (2)

State	**(1)**	**(2)**	**State**	**(1)**	**(2)**
Alabama	0.460	0.530	Montana	0.390	0.490
Alaska	0.470		Nebraska	0.580	0.740
Arizona	0.840	0.810	Nevada	0.900	0.880
Arkansas	0.290	0.450	New Hampshire	0.430	0.550
California	0.910	0.960	New Jersey	0.380	0.440
Colorado	0.260	0.580	New Mexico	0.370	0.560
Connecticut	0.670	0.720	New York	0.850	0.840
Delaware	0.300	0.410	North Carolina	0.810	0.800
District of Columbia	0.930		North Dakota	0.560	0.630
Florida	0.790	0.780	Ohio	0.690	0.670
Georgia	0.850	0.810	Oklahoma	0.540	0.640
Hawaii	0.760		Oregon	0.470	0.400
Idaho	0.470	0.520	Pennsylvania	0.530	0.510
Illinois	0.790	0.840	Rhode Island	0.750	0.830
Indiana	0.640	0.630	South Carolina	0.680	0.740
Iowa	0.430	0.470	South Dakota	0.640	0.650
Kansas	0.430	0.610	Tennessee	0.350	0.450
Kentucky	0.490	0.440	Texas	0.950	0.940
Louisiana	0.780	0.740	Utah	0.640	0.670
Maine	0.560	0.490	Vermont	0.640	0.810
Maryland	0.760	0.840	Virginia	0.790	0.800
Massachusetts	0.700	0.760	Washington	0.480	0.520
Michigan	0.680	0.720	West Virginia	−0.010	0.220
Minnesota	0.770	0.760	Wisconsin	0.560	0.650
Mississippi	0.380	0.310	Wyoming	0.410	0.730
Missouri	0.580	0.690			

Table 9: Firearm Homicides: Comparison of Within-State R^2: Full Sample (1) vs. Contiguous 48 (2)

State	**(1)**	**(2)**	**State**	**(1)**	**(2)**
Alabama	0.830	0.850	Montana	0.490	0.470
Alaska	0.140		Nebraska	0.390	0.430
Arizona	0.200	0.680	Nevada	0.760	0.760
Arkansas	0.780	0.800	New Hampshire	0.410	0.440
California	0.810	0.840	New Jersey	0.640	0.680
Colorado	0.680	0.670	New Mexico	0.380	0.260
Connecticut	0.610	0.630	New York	0.960	0.960
Delaware	0.560	0.630	North Carolina	0.860	0.840
District of Columbia	0.940		North Dakota	0.580	0.730
Florida	0.600	0.630	Ohio	0.690	0.710
Georgia	0.850	0.850	Oklahoma	0.470	0.580
Hawaii	0.720		Oregon	0.720	0.730
Idaho	0.620	0.640	Pennsylvania	0.080	−0.060
Illinois	0.870	0.910	Rhode Island	0.170	0.560
Indiana	0.350	0.440	South Carolina	0.460	0.560
Iowa	0.110	0.120	South Dakota	0.550	0.830
Kansas	0.500	0.550	Tennessee	0.810	0.820
Kentucky	0.490	0.500	Texas	0.770	0.930
Louisiana	0.780	0.790	Utah	0.460	0.480
Maine	−0.070	0.580	Vermont	0.710	0.860
Maryland	0.720	0.680	Virginia	0.860	0.830
Massachusetts	0.300	0.410	Washington	0.840	0.850
Michigan	0.860	0.800	West Virginia	0.560	0.600
Minnesota	0.450	0.510	Wisconsin	0.510	0.550
Mississippi	0.840	0.860	Wyoming	0.240	0.490
Missouri	0.710	0.770			

Table 10: Step Indicators: Firearm Suicides

State (year)	Contiguous States: Policy timing	Contiguous States: No Policy timing	All States, District: Policy	All States, District:
Arkansas (1994, Step)	0.212***	0.191***	0.199***	
	(0.043)	(0.044)	(0.048)	
Arkansas (2000, Step)				0.147***
				(0.038)
California (1996, Step)		-0.165***		
		(0.038)		
Colorado (2000, Step)	0.102***			
	(0.034)			
Connecticut (1996, Step)	-0.124***	-0.158***		
	(0.039)	(0.039)		
Delaware (1996, Step)	0.305***	0.302***		
	(0.082)	(0.082)		
Delaware (1997, Step)	-0.437***	-0.440***		
	(0.092)	(0.093)		
Delaware (1999, Step)	0.369***	0.351***	0.172***	0.166***
	(0.069)	(0.069)	(0.041)	(0.041)
Delaware (2002, Step)	-0.176***	-0.147***		
	(0.051)	(0.052)		
Delaware (2008, Step)			-0.169***	-0.166***
			(0.049)	(0.049)
Maine (2001, Step)	-0.118***		-0.116***	-0.118***
	(0.039)		(0.042)	(0.042)
Massachusetts (1999, Step)	-0.138***	-0.164***		
	(0.035)	(0.036)		
Nebraska (2003, Step)	-0.098***	-0.113***		
	(0.032)	(0.032)		
Nevada (2000, Step)			-0.179***	-0.172***
			(0.039)	(0.039)
Nevada (2001, Step)	-0.172***	-0.194***		
	(0.035)	(0.035)		
NorthDakota (1993, Step)	0.283***	0.237***	0.277***	0.272***
	(0.048)	(0.051)	(0.054)	(0.054)
NorthDakota (2002, Step)		0.125***		
		(0.039)		
SouthDakota (1993, Step)	0.269***	0.276***	0.254***	0.259***
	(0.053)	(0.053)	(0.059)	(0.059)
SouthDakota (2000, Step)	-0.295***	-0.290***	-0.306***	-0.313***
	(0.052)	(0.053)	(0.059)	(0.059)
SouthDakota (2003, Step)	0.273***	0.291***	0.288***	0.287***
	(0.058)	(0.058)	(0.065)	(0.065)
SouthDakota (2007, Step)	-0.245***	-0.208***	-0.248***	-0.230***
	(0.048)	(0.048)	(0.054)	(0.054)
Texas (1995, Step)	-0.164***	-0.167***	-0.177***	-0.181***
	(0.040)	(0.040)	(0.045)	(0.045)
Vermont (1994, Step)	-0.273***	-0.292***	-0.459***	-0.465***
	(0.055)	(0.055)	(0.095)	(0.096)
Vermont (1995, Step)			0.313***	0.306***
			(0.089)	(0.090)
Vermont (1998, Step)	0.228***	0.224***		
	(0.052)	(0.050)		
Vermont (2003, Step)	-0.285***			
	(0.083)			
Vermont (2004, Step)	0.403***			
	(0.106)			
Vermont (2005, Step)	-0.344***	-0.174***		
	(0.084)	(0.048)		
Vermont (2009, Step)	0.353***	0.367***	0.250***	0.254***
	(0.058)	(0.058)	(0.055)	(0.056)
Vermont (2012, Step)	-0.302***	-0.290***	-0.309***	-0.310***
	(0.069)	(0.069)	(0.077)	(0.078)
WestVirginia (1992, Step)	0.168***			
	(0.058)			
Wyoming (2002, Step)	0.198***	0.220***		
	(0.058)	(0.059)		
Wyoming (2004, Step)	-0.459***	-0.467***		
	(0.092)	(0.093)		
Wyoming (2005, Step)	0.332***	0.329***		
	(0.080)	(0.081)		
Accidental Firearm Death Rate	0.021**	0.024***	0.027***	0.025**
	(0.009)	(0.009)	(0.010)	(0.010)
Accidental Poisoning Rate	0.007***	0.006***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Brady Act Background Check	0.011	0.000	0.024	0.020
	(0.014)	(0.013)	(0.015)	(0.015)
Fraction Black	0.765***	0.694***	0.712***	0.782***
	(0.128)	(0.128)	(0.138)	(0.137)
Fraction Male (45-64)	0.002	-0.002	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Fraction Urban	-0.608***	-0.529***	-0.727***	-0.753***
	(0.116)	(0.110)	(0.124)	(0.124)
Log Per Capita Ethanol Consump	0.440***	0.389***	0.490***	0.490***
	(0.069)	(0.070)	(0.074)	(0.074)
Purchase Delay (Any)	-0.029***		-0.033***	
	(0.010)		(0.011)	
Real Per Capita Income	-0.006**	-0.006**	-0.007***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	1152	1152	1224	1224

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Alaska, DC, Hawaii omitted for brevity.

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