

What Is the Impact of Gun Laws on Firearm Homicides?

Investigating the Plausibility of Obtaining Valid Causal Effects

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Abstract

There exists a large body of literature exploring the effects of various gun laws on firearm homicides. However, most of these existing studies use some form of two-way fixed-effects specification without assessing the validity of the assumptions needed for this specification to yield viable causal estimates. In this paper, I use the latest difference-in-differences tools to revisit the effects of three types of gun laws – stand-your-ground laws, “shall issue” and permitless concealed carry laws, and extreme risk protection order laws. More specifically, I employ tests of the parallel trends and overlap assumptions, as well as tools robust to treatment effect heterogeneity, to explicitly test whether the assumptions required for interpreting these models’ estimated effects as causal hold in regard to the implementation of these three gun laws. While the estimated effects of enacting an SYG law or a “shall issue” or permitless concealed carry law are positive, I also find evidence that key assumptions are likely violated. In particular, there appears to be violations of parallel trends and limited overlap between states that do and those that do not enact these policies. These results suggest that caution is necessary in interpreting existing estimates of the impacts of gun-control laws as causal.

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1 Introduction

As of June, 2022, six in ten American adults believe that gun violence is a “very big problem” in the US today, and another 23% say gun violence is a “moderately big problem” (Pew Research Center, 2013). Furthermore, 62% of Americans say they expect the level of gun violence to increase over the next five years. However, there is much less of a consensus regarding the best way to combat this issue. According to Gallup polling from October, 2023, 56% of Americans favor stricter gun laws, 32% believe they should be kept as they are now, and 12% of Americans support having fewer restrictions on gun ownership (Gallup, 2023).

While there is widespread disagreement regarding the efficacy of different gun policies, it is indisputable that the number of gun deaths in the US has been steadily increasing each year. According to the CDC, 48,830 Americans died of gun-related injuries in 2021, a higher number than any other year on record. This represents an 8% increase from the number of deaths in 2020, continuing the steady increase in gun deaths per year that has been occurring since 2000. It also brings the number of gun deaths since 2000 to a total of 756,935 Americans. Therefore, it is important to understand the effect that different types of regulations, or the lack thereof, have on gun deaths.

There exists a wide body of literature regarding the effect of various gun regulations on outcomes, including violent crime, the frequency of mass shootings, and firearms regulations. However, as discussed in a comprehensive literature review conducted by the RAND institute in 2023, many studies investigating the same gun law reach vastly different conclusions, including a large number that yield inconclusive results. The report also notes that many of the studies investigating the existing studies face methodological issues, such as including too many parameters given the number of observations and failing to adjust standard errors for potential serial correlation in the outcome variable, which lead to potentially biased estimates (RAND, 2023d).

One category of methodological issues the RAND report specifically does not comment

on are those related to certain gun laws potentially having heterogeneous effects over time. Recent literature has shown that, when units become treated at different times, two-way fixed effects (TWFE) models can produce biased estimates (Goodman-Bacon, 2021), yet there is little research evaluating whether this issue exists when estimating the effects of gun policies (RAND, 2023d). Furthermore, when employing difference-in-differences techniques, interpreting the results as causal relies on a number of assumptions. Despite this, many of the existing papers that use this empirical approach do not provide evidence to support the validity of these assumptions, calling into question whether the results they obtain can be truly regarded as the causal effect of enacting specific gun laws. A full summary of which papers do and do not check these assumptions can be found in Table 6 in Section 8.

Furthermore, when employing traditional difference-in-differences techniques, interpreting the results as causal relies on a number of assumptions. Despite this, many of the existing papers that use this empirical approach do not provide evidence to support the validity of these assumptions, calling into question whether the results they obtain can be truly regarded as the causal effect of enacting specific gun laws.

In contrast, this paper uses the latest difference-in-differences tools to revisit the effects of three types of gun laws – stand-your-ground (SYG) laws, “shall issue” and permitless concealed carry laws, and extreme risk protection order (ERPO) laws. I chose these three categories of laws because they were all implemented by at least 10 states between 2001-2020 and because a large number of previous studies investigating the effects of these policies on firearm homicides have yielded inconclusive results. For each policy, I estimate the effects said law has on the age-adjusted firearm homicide rate using two different methods that are robust to treatment effect heterogeneity. The first method is a dynamic difference-in-differences model with two-way fixed effects, thus allowing for heterogeneous treatment effects over time. The second approach I use is the Sun-Abraham (2021) method, which also accounts for potentially heterogeneous effects across cohorts of states that enact a given law at different times.

Using these methods, I focus explicitly on testing whether the assumptions required for interpreting the effects estimated from difference-in-differences regressions as causal hold in regard to the implementation of these three gun laws. Additionally, given the wide variety of results in the current literature, this paper details a number of omitted variables and other confounds that potentially inhibit researchers’ ability to accurately estimate the causal effects of said policies.

I find that both methods yield similar results regarding the effect of each type of firearm policy on firearm homicides, namely that enacting an SYG law or a “shall issue” or permitless concealed carry law has a positive effect on the age-adjusted firearm homicide rate. Both the dynamic TWFE model and Sun-Abraham (2021) method also yield estimates providing inconclusive evidence regarding the effect of instituting an ERPO law on the firearm homicide rate. However, I also find evidence that the assumptions to interpret these effects as causal are likely to be violated. In particular, there appear to be violations of parallel trends and limited overlap between states that do and those that do not enact these policies. These results suggest that we should be cautious when interpreting the estimates I obtain for the effects of these three types of gun laws, as well as those from existing studies as causal.

The remainder of the paper is organized as follows: Section 2 presents a deeper review of the current literature regarding SYG, “shall issue” and permitless concealed carry laws, and ERPO laws; Section 3 provides background regarding these three firearm policies; Section 4 furthers details the data used; Section 5 outlines my empirical strategy, Section 6 presents and discusses the results, and Section 7 offers a conclusion.

2 Literature Review

The first type of laws that I investigate are commonly known as “stand-your-ground” (SYG) laws. These laws allow the use of deadly force in public if a person feels a sufficient threat of death or physical violence. In their summary of research related to the effect

of these laws on firearm homicides, RAND found eleven methodologically valid studies¹ conducted from 2014 to 2020 (RAND, 2023c). Of these, seven (Gius (2016); Munasib, Kostandini and Jordan (2018); Webster, Crifasi and Vernick (2014); Siegel, Pahn, Xuan, Fleegler and Hemenway (2019); Siegel, Solomon, Knopov, Rothman, Cronin, Xuan and Hemenway (2020); Knopov, Siegel, Xuan, Rothman, Cronin and Hemenway (2019); Schell, Cefalu, Griffin, Smart and Morral (2020a)) yielded inconclusive results regarding the effect of SYG laws on firearm homicides.

The remaining four studies (Crifasi, Merrill-Francis, McCourt, Vernick, Wintemute and Webster (2018); Humphreys, Gasparrini and Wiebe (2017); Guettabi and Munasib (2018); McClellan and Tekin (2017)) found that SYG laws led to a significant increase in firearm homicides in certain geographic or demographic subsets of the population. Crifasi et al. (2018) studied the effect of SYG laws in only urban counties. Humphreys et al. (2017) focused on Florida, which was the first state to enact an SYG law, finding that this SYG law led to a significant increase in firearm homicides in this state specifically. However, this study notes that the model does not include covariates to account for pre-treatment differences between Florida’s homicide rates and those of the control states, meaning that the effect they found could in part be due to other unobserved differences. Guettabi and Munasib (2018) looked at the state-specific effect of SYG laws on homicides in 14 states, finding that the implementation of this type of law had a significant effect on firearm homicides in only three of these states and uncertain effects in the other eleven. The authors of this paper also did not aggregate their results into an overall estimated treatment effect for said laws. Finally, McClellan and Tekin (2017) found that SYG laws led to a significant increase in firearm homicides among white males, but had uncertain effects on other demographic groups.

¹RAND determined that a study was methodologically invalid if it had issues including, but not limited to: (1) the model included too many parameters given the number of observations; (2) the model failed to adjust standard errors for potential serial correlation in the outcome variable; (3) the dependent variable appeared to violate model assumptions; (4) the study used multiple models and the magnitude and direction of the estimated effects differed widely across models; (5) the study focused on a policy that was implemented by too few localities (and thus there were not enough “treated” observations) (RAND, 2023d)

Among these eleven papers yielding a wide variety of results, six of these studies employ a traditional static difference-in-differences strategy with TWFE. However, of these six, only one mentions and provides support for the validity of the parallel trends assumption in regards to the states and time frame it focuses on.

The second category of laws are those relating to concealed carry, including “shall issue” vs. “may issue” laws and policies that allow people to conceal carry without a permit. In a review similar to that of the literature regarding SYG laws, RAND first notes that many of the studies conducted pre-2004 had methodological issues, including models with too many parameters, misclassification of which states had which types of laws, incorrect data, and the inclusion of covariates that led to certain locations being mistakenly dropped from the analysis (RAND, 2023a). As for research conducted after 2004, RAND found nine studies investigating the effects of concealed carry laws on firearm homicides in the post-2000 period. Similar again to the investigations of SYG laws, multiple of these yielded uncertain results (Donohue, Aneja and Weber (2019); Luca, Malhotra and Poliquin (2017); Schell et al. (2020a)). An additional group of studies (Knopov et al. (2019); Fridel (2021); Siegel et al. (2019)) found that more-permissive concealed-carry regulations (i.e., “shall issue” laws and policies allowing for permitless concealed carry), led to an increase in firearm homicides. Similar to the research regarding SYG laws, five of these studies utilize static difference-in-differences with TWFE approaches and yet only one discusses the assumptions required for the estimates obtained from this type of model to be deemed causal.

Therefore, this paper expands upon the existing literature by by using more modern difference-in-differences tools, providing a more thorough evaluation of necessary assumptions, and using this, along with a discussion of other potential confounding factors, to attempt to explain the discrepancies across results in previous analyses.

The final category of laws that this paper investigates are ERPO laws, also known as “red flag” laws. ERPO laws allow temporary orders to prohibit individuals deemed to be a risk to themselves or others from owning firearms. While there are now 21 states with ERPO

laws (Everytown Research & Policy, 2024), only five states had such laws prior to 2018. Hence, the effects of these policies on various gun use outcomes, and on firearm homicides in particular, has not been as extensively studied. In their literature review, RAND identified one study investigating the effects of ERPO laws (RAND, 2023b). However, this study, Gius (2020), looked at two treated states – Connecticut and Indiana – separately, meaning that each analysis was based on only one treated state. They also found conflicting effects as the passage of ERPO laws was associated with an increase in firearm homicides in Indiana but a decrease in Connecticut. Another study, Delafave (2021), found that, while ERPO laws led to a statistically significant decrease in firearm suicides, they had no effect on firearm homicides. Additionally, Delafave (2021) used a static difference-in-differences specification with TWFE and did not discuss the corresponding required assumptions. This study also only uses data from 1990 to 2018, which excludes evidence from the additional 16 states that passed ERPO laws during or after 2018. As such, this paper adds to the literature regarding ERPO laws by using more recent data and newer empirical techniques to further investigate the effects of said policies and evaluate the assumptions needed to regard these effects as causal.

3 Background

3.1 Stand-Your-Ground Laws:

Since the founding of the United States, it has been the rule of law that, if an individual feels threatened, they have a duty to retreat before resorting to force. The exception to this is the “Castle Doctrine,” a common law principle that gives individuals the right to use force, including deadly force, against an intruder in their home and that has been fully or partially codified by most states. In 2005, Florida became the first state to pass a “stand-your-ground” law, which expands the “Castle Doctrine” by removing the duty to retreat and allowing the use of force in response to the threat of death or “great bodily harm” in

public places when (1) an individual has a legal right to be present and (2) the individual is themselves not engaged in any illegal activity. Since 2005, an additional 28 states have passed similar laws.

3.2 Concealed Carry laws:

Concealed carry is the right to carry a handgun on one's person such that it is hidden from others. All states allow concealed carry to some extent; however, the practice of receiving a permit to do so differs from state to state. The strictest form of issuing concealed carry permits are what are known as “may issue” laws. These laws give law officials the discretion to not grant an individual a concealed carry permit. On the other hand, “shall issue” laws remove this discretion, stating that any individual may be granted a concealed carry permit unless they are explicitly prohibited by some other state regulation. Finally, some states also allow for permitless carry, abolishing the need for permits altogether.

3.3 Extreme Risk Protection Orders:

ERPO laws give courts the authority to temporarily revoke an individual's right to purchase and possess firearms if said individual is deemed to pose a serious risk to themselves or others (Brady, 2023). The process of procuring such orders is similar to that of obtaining restraining orders due to domestic violence and common factors that are typically considered in such cases include recurrent violent threats or actions, a history of dangerous behavior with guns, substance abuse, and a recent purchasing of abnormal amounts of firearms or ammunition. While ERPOs are temporary, they can be extended at further hearings where both parties are again allowed to submit evidence in favor or against the issuing of said order.

ERPO laws also differ slightly by state in terms of who is allowed to petition the court for the issuance of said order, how long the orders last, and what type of evidence must be presented in order to obtain an ERPO. For example, many states allow “final orders” of

up to a year, as well as “temporary orders” spanning anywhere from one to two weeks to almost two months. Additionally, whereas most states with these laws allow family members, mental health professionals, or law enforcement officials to initiate the process of obtaining an ERPO, others only allow law enforcement officials, or even only state attorneys, to petition the courts. (CITE)

4 Data

4.1 Gun Law Data:

I obtained a novel dataset compiled by Siegel et al. (2019) that details the presence or absence of 134 laws for each state and year from 1991-2016. They assembled this dataset using data from the State Firearm Law Database, the Westlaw database of state statutes and session laws, and Everytown for Gun Safety to determine the presence or absence of 134 laws for each state and year from 1991-2016. More specifically, laws are encoded as the presence of the restrictive version of a given law. Thus, if a state enacts an expansive law, such as legalizing the right to carry concealed handguns without a permit, the indicator for the presence of a law that does require an individual to have a permit in order to concealed carry would change from 0 to 1. Therefore, I was able to create indicators for *treatment_expansive* and *treatment_restrictive* that are 1 if a state has implemented any expansive or restrictive law, respectively, in a given year and 0 otherwise without having to manually classify each law type as belonging to one of these two categories.

Next, since “may issue” vs. “shall issue” laws and requiring vs. not requiring a permit in order to concealed carry a firearm are also encoded separately in the original dataset, I added another indicator that is 1 if a state has a “shall issue” and permitless carry laws and 0 otherwise.

For ERPOs, there was no column in the Siegel dataset directly corresponding to this category of regulations. Therefore, I created my own, using data from the Giffords Law

Center and various state legislatures.

Finally, I added variables encoding the year when a given state passed an SYG law, a “shall issue” or permitless carry law, or ERPO.

4.2 Homicide Data:

I obtained the homicide data from the WISQARS Fatal and Non-Fatal Injury Reports, which is an online database maintained by the CDC. This data includes the homicide rate, the age-adjusted firearm homicide rate, which takes into account the age distribution of a given state, and the raw number of homicides for all 50 states for each year from 2001-2016. The WISQARS reports also include versions of the homicide data broken down by race, gender, and whether or not the incident occurred in a metro area. Given that younger people are more likely to be murdered than the elderly, and thus having a younger population could affect a state’s homicide rate, I use the age-adjusted rate as opposed to the raw rate as the outcome variable of interest. Additionally, since this variable is not normally distributed, I follow the common practice in the literature of using a log-transformed version of the age-adjusted rate in my analysis. Because three states – North Dakota, Vermont, and Wyoming – did not report homicides in every year within the 2001-2016 timespan, I drop these states from the analysis.

4.3 Controls Data:

As is common in the existing literature regarding firearm regulations, my analysis also controls for a number of pre-treatment covariates. The full Panel Contains the following eleven factors:

1. the percent of the population that is black
2. the percent of the population that is male and aged 15-29
3. the number of law enforcement officers per 1,000 people
4. the number of violent crimes excluding homicide per 100,000 people

5. the unemployment rate
6. the poverty rate
7. per capita alcohol consumption
8. the incarceration rate
9. population density
10. the log of population
11. the proportion of adults living in a household with a firearm

I obtain state level demographic data from the ACS 1-year survey data available on IPUMS and sum the PERWT variable, which denotes how many people are represented by a given person in the data, to calculate the percent of the population that is black, the percent that is male and aged 15-29, and the log of the population for each state and year. I also obtained state areas (measured in square miles and excluding bodies of water) from the census bureau and used this to calculate the population density for each state. I used the same state geographic areas across all years because no state's area changed significantly between 2001-2016.

Data detailing the number of law enforcement officers per 1,000 people and the number of violent crimes excluding homicide per 100,000 people comes from the FBI's crime data explorer. It is important to note that not every law enforcement agency in each state reports these statistics; however, the FBI takes this into account when calculating the per capita values by only including the population served by agencies that did report.

I acquire poverty rate data from the Bureau of Labor Statistics and unemployment data from the Census Bureau. Per capita alcohol consumption comes from a dataset available on OpenISPCR that contains per capita (for people aged 14+) consumption of ethanol measured in gallons for each state and year. For the incarceration rate, I obtain the number of prisoners at the end of the year for each state and year from the Bureau of Justice Statistics Corrections Statistical Analysis Tool and then divide this by the state's population in a given year to obtain the rate.

Finally, the data detailing the proportion of adults living in a household with a firearm comes from “State-Level Estimates of Household Firearm Ownership,” a dataset compiled by researchers at the RAND Corporation. RAND constructed these state level estimates by combining the results of 51 national surveys of household gun ownership with administrative data regarding firearm suicides, hunting licenses, subscriptions to *Guns & Ammo* magazine, and background checks.

Due to the differing ranges of years available in the homicides data and laws data, I restrict my analysis to the years for which I had all the necessary data, namely 2001-2020.

4.4 Summary Statistics:

In order to investigate the effect of SYG, expansive concealed carry laws, and ERPO laws on firearm homicides, this study exploits the variation in the timing of when different states enacted these laws. Figure 1 illustrates, for each law, which states are “always treated” and passed said law before 2001, which states “become treated” and enacted the given law between 2001 and 2020, and which states are “never treated.”

Table 1 displays similar information as Figure 1 in table form. Note that for the “expansive concealed carry” category, treatment is defined as passing either a “shall issue” law or a permitless carry law.

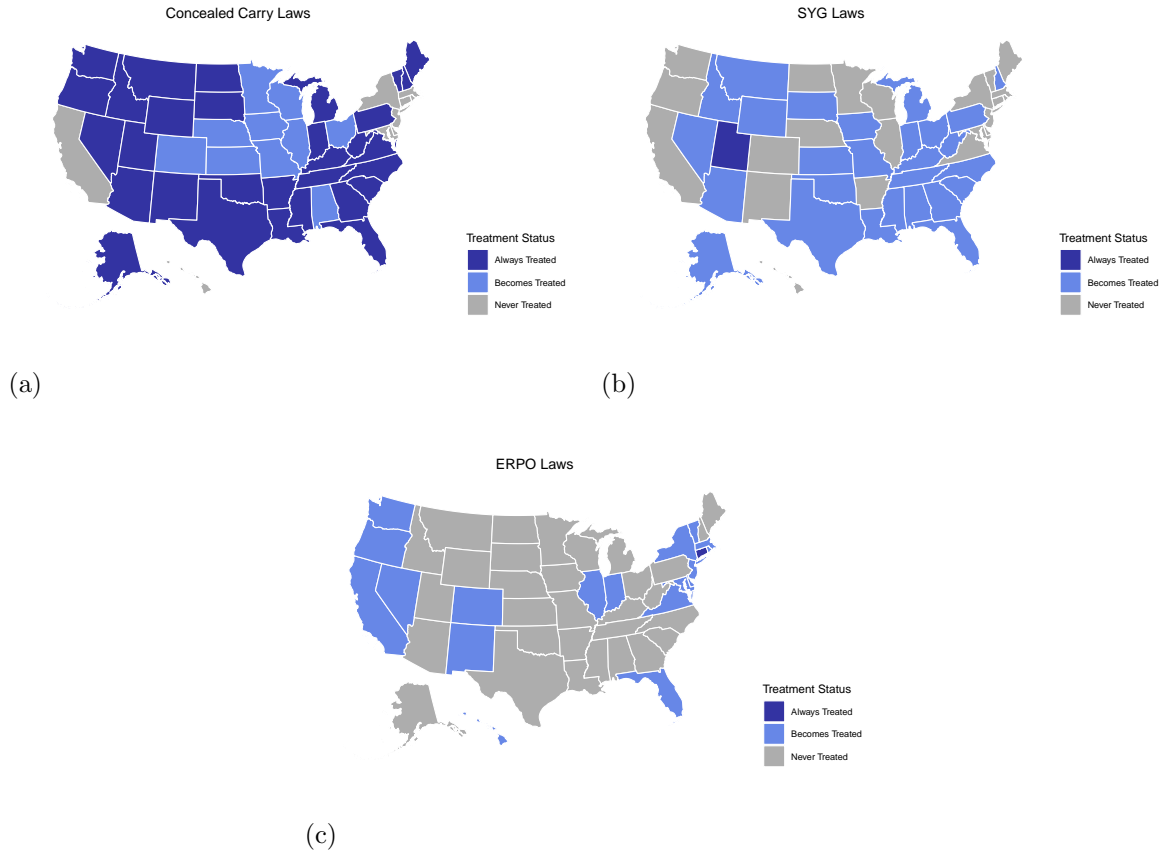
Table 1: State Treatment Status Summary

Law Type	Always Treated States	Never Treated States	States that Become Treated
SYG	1	22	27
Expansive Concealed Carry	31	9	10
ERPO	1	31	18

Note: This Table displays, for each law type, the number of states that are “always treated” (i.e., they passed the given law before 2001), the number that are “never treated” (i.e., they never enacted the given law), and the number that “become treated” and enact the given law between 2001-2020.

Figure 2 plots the average age-adjusted firearm homicide rate for different groups of states with different concealed carry laws, the presence or absence of an SYG law, and the presence or absence of an ERPO law. The average for each group for each year is calculated

Figure 1: Treatment Status by State and Law Type



Note: Each panel shows states' treatment status for a given law. In Panel A treatment is defined as passing a "shall issue" or permitless concealed carry laws. In Panel B treatment is defined as enacting an SYG law. In Panel C treatment is defined as passing an ERPO law.

using the rates from all states with that law in that specific year. For example, Tennessee passed its SYG law in 2007. As such, Tennessee's age-adjusted homicide rate is included in the group of states with no SYG law pre-2007 and then the group of states with an SYG law post-2007. Because WISQARS has no homicide data from North Dakota, Vermont, and Wyoming, these states are excluded.

Panel A shows that, except for 2011-2012, states with a "may issue" concealed carry law had a lower average age-adjusted firearm homicide rate than those with a "shall issue" or permitless carry law. Additionally, outside of 2009-2012 and 2016, the average age-adjusted firearm homicide rates for both groups of states appears to follow a similar general trend of increases and decreases.

Figure 2: Age-Adjusted Firearm Homicide Rate Over Time



Note: Panel A shows the average age-adjusted firearm homicide rates over time for states with “may issue” concealed carry laws as opposed to states with “shall issue” laws or permitless carry. Panel B shows the average age-adjusted firearm homicide rates over time for states with ERPO laws as compared to those without. Panel C shows the average age-adjusted firearm homicide rates over time for states with stand-your-ground laws in comparison to those without one.

Panel B shows that states with an ERPO law have a lower average age-adjusted homicide rate than states without one.

Panel C shows that the average age-adjusted firearm homicide rate for states without an SYG law is more stable than that of states with an SYG law. Prior to 2005, there was only one state (Utah) with a version of an SYG law. In 2005, Florida passed a more updated form of said law, popularizing this policy and leading to eleven more states enacting a similar version in 2006. One of these eleven states was Louisiana, which consistently has a significantly higher age-adjusted firearm homicide rate than all other states and could thus account for part of the dramatic increase in the average age-adjusted firearm homicide rate for SYG states between 2004 to 2006. Similarly, Louisiana also has a “shall issue” concealed carry law and no ERPO law, which be a large contributor to why “shall issue” and permitless

carry states have higher age-adjusted homicide rates compared to “may issue” states and why ERPO states have a lower rates compared to states without an ERPO law.

5 Empirical Strategy

5.1 Models:

Many studies in the current literature investigating the effects of gun laws employ traditional two-period two-way fixed effects models, regressing the outcome variable on an indicator of whether a state was treated in a given year, along with state and year fixed effects. As shown in Goodman-Bacon (2021), this approach yields an estimate that is a weighted average of difference-in-differences estimates obtained by comparing all possible pairs of units and time periods where one unit’s treatment status changes and the other unit’s does not. Therefore, if units first receive treatment at different times, some of these comparisons will use an already-treated unit as the control for a newly treated unit. However, this can lead to the treatment effect for these “forbidden controls” receiving a negative weight in the overall estimated treatment effect. For example, in the case of gun laws, a state that first enacted an SYG law in 2006 would be included in the control group for a state that first enacted said law in 2011. This then leads to treatment effect dynamics being inaccurately incorporated into the regression coefficient. In the case of gun laws, the treatment times, namely when a given state implements a specific type of law, are staggered. Thus, using the canonical two-period difference-in-differences approach could yield inaccurate estimates of the effect of passing certain types of gun laws.

In order to try accounting for the staggered implementation of a gun law, I will use two slightly different dynamic difference-in-differences models. I will first use a dynamic TWFE model, which looks at the effect of treatment relative to when a state first became treated. More specifically, I run three regressions comparing the age-adjusted firearm homicide rates before and after treatment, where treatment is defined as enacting an SYG law, “may issue”

or permitless carry law, or ERPO law. These regressions take the following form:

$$homicides_{it} = \sum_{s=-T, s \neq -1}^T (\beta_s \times treatment_{i,t-s}) + \phi_i + \gamma_t + \delta X_i \times t + \epsilon_{it}$$

where $homicides_{it}$ is the log of the age-adjusted firearm homicide rate for state i in year t and $treatment_{it}$ is an indicator for whether or not state i is “treated” in year t where “treatment” is defined as enacting an SYG law, “shall issue” or permitless concealed carry law, or ERPO law for each respective model

The vector X_i contains state i ’s pre-treatment covariates as measured in 2001. This vector is then interacted with t to allow for state-specific linear trends of these characteristics.

The coefficients of interest are the β_s ’s where each β_s denotes the effect of passing a “shall issue” or permitless carry law (for model 1), an SYG law (for model 2), or an ERPO law (for model 3) for each year relative to the year before the law was implemented (i.e., to relative year $t - s = -1$). Finally, this regression also includes fixed effects for state (ϕ_i) and year (γ_t), and an error term u_{it} .

The above dynamic TWFE specification is an improvement on the static TWFE model because it allows for heterogeneity in time since treatment by estimating the treatment effect on the treated for each year relative to when a state first becomes treated, assuming that this effect is on average the same regardless of the actual year when a state becomes treated. However, as shown by Sun and Abraham (2021), if there are also heterogeneous treatment effects across adoption cohorts, the coefficients obtained from using a dynamic TWFE model can still be biased. For example, if the effects of implementing an SYG law one year after the law goes into place are different for the group of states who first enacted an SYG law in 2006 and the group that passed said law in 2013, then this can lead to similar issues as using a static TWFE where the presence of “forbidden comparisons” between newly treated states and already treated states biases the estimates.

Therefore, to attempt to correct for this issue, I also run each regression using the Sun and Abraham (2021) method of estimation where, rather than interacting relative time

period with an indicator of treatment, the relative time period is instead interacted with an indicator of the cohort that a given unit belongs to. In the case of this study, the “cohort that a given unit belongs to” is essentially the year that state implemented a specific type of gun law.

5.2 Assumptions:

Interpreting the β_s coefficients from these regression as causal estimates for the effect of passing a given law $t - s$ years after said law was enacted relies on a number of assumptions. To explain these assumptions, let us denote t as a specific year where $t = 1, \dots, 16$ are all the years from 2001-2016, inclusive. Next, let $D_{i,t}$ be an indicator of state i 's treatment status in year t with $D_{i,t} = 1$ if state i has a specific gun law (i.e., state i is treated) in year t . Then, let $G_i = g$ be the year that state i enacts a given law where, like t , $g \in [1, 20]$. As such, $G_i = g$ indicates the year that state i becomes treated. $Y_{i,t}$ will denote state i 's age-adjusted firearm homicide rate in year t . Additionally, let \bar{g} be the last year that some state becomes treated – i.e., the last year that any state passes a specific law. If state i never becomes treated, then let $C_i = 1$. Finally, let X_i represent a vector of pre-treatment covariates for state i .

Assumption 1: *Conditional Parallel Trends Assumption:* For all $t = 2, \dots, 20$, $g = 1, \dots, \bar{g}$ such that $t \geq g$:

$$\mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, G = g] = \mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, C = 1]$$

This assumption states that, conditional on the covariates in X , the path of potential outcomes that states first enacting a law in year g would have experienced if they had not passed said law is parallel to the path of potential outcomes for states that never enact said law. However, I will show in section 6.1 that the pre-treatment analog to the parallel trends assumption does not appear to hold for most of my specifications, even when conditioning on a large panel of covariates.

Assumption 2: *No Anticipation Assumption:* For all $t = 2, \dots, 20$, $g = 1, \dots, \bar{g}$ such that $t < g$:

$$\mathbb{E}[Y_t(g)|G = g] = \mathbb{E}[Y_t(0)|G = g]$$

In regards to gun laws, this assumption means that, in each year t , if a state has not passed a given gun law (i.e., it is untreated), its firearm homicide rate does not depend on whether or not it will pass said law a future year (i.e., whether or not it becomes treated in some year g). While it seems that the anticipation of a certain gun law being implemented would not affect the number of firearm homicides, section 6.1 will discuss why this assumption in fact does not appear to hold for the enactment of SYG laws in particular.

Assumption 3: *Irreversibility of Treatment:* For all $t = 2, \dots, 20$:

$$D_{i,t-1} = 1 \implies D_{i,t} = 1$$

In the context of this paper, this assumption means that once a state has passed a given law, that law remains in place for the duration of the window of investigation. Although states sometimes do repeal laws, there are no instances of this occurring within the window of investigation for the specific laws I am studying. Thus this assumption holds.

As discussed in Callaway and Sant’Anna (2021), non-parametric identification also depends on the strong overlap assumption.

Assumption 4: *Strong Overlap Assumption:* For all $t = 2, \dots, 20$, $g = 1, \dots, \bar{g}$, there exists an $\epsilon > 0$ such that:

$$P(G = g) > \epsilon \text{ and } P_g(X) < 1 - \epsilon$$

where P_g denotes the probability of becoming treated at time g , conditional on the values of X , as opposed to being in the never-treated group.

Essentially, this means that, at each period g , a positive fraction of states becomes treated and that, for each possible set of covariates X , there is a positive probability that a state with these values is not treated. However, section 6.3 will discuss how it is likely that this

assumption also does not hold due to the significant differences in observable characteristics between states that do and do not pass specific firearm regulations.

5.3 Covariates:

Another potential issue when estimating the effect of gun laws is reverse causality, namely that a state will pass a given policy as a response to rising rates of firearm homicides or violent crime in general. This concern is especially germane when looking at states that switch from “may issue” to “shall issue” concealed carry laws because, as shown in Grossman and Lee (2008), there is evidence that increasing rates of violent crime accelerated the nationwide shift towards “shall issue” laws. However, including lagged outcome variables in regression specifications can also potentially lead to biased coefficients.

Similarly, there may be concern about including covariates that are affected by the passage of a given gun law. For example, two common and important pre-treatment covariates in studies investigating the impact of firearm regulations on firearm homicides are non-homicide violent crime levels and the proportion of adults living in a household with a gun, as higher non-homicide violent crime and gun ownership rates is likely correlated with higher firearm homicide rates. Furthermore, higher violent crime rates can lead to states adopting “tough on crime” policies that also make them more likely to adopt certain gun laws. Similarly, states with higher rates of gun ownership are often more likely to pass additional expansive gun laws. On the other hand, especially in the case of “may issue” vs. “shall issue” or permitless concealed carry laws and SYG laws, the implementation of a more expansive concealed carry policy or an SYG law has also been found to be correlated with an increase in overall violent crime and the proportion of adults living in a household with a gun. However, when including pre-treatment covariates, it is assumed that these characteristics are unaffected by the treatment. If this is not the case, as might be true with violent crime levels and the proportion of adults living in a household with a gun, then conditioning on these values can introduce more bias. To explore robustness to these issues, I will run each

regression with four slightly different panels of covariates:

1. the full panel of covariates layed out in Section 4.2
2. the full panel of covariates layed out in Section 4.2, excluding lagged age-adjusted firearm homicide rates
3. a partial set of covariates, excluding violent crime per capita and the proportion of adults living in a household with a gun, but including lagged age-adjusted firearm homicide rates
4. a partial set of covariates, excluding violent crime per capita, the proportion of adults living in a household with a gun and the lagged age-adjusted firearm homicide rates

Each panel contains the values for each characteristic for each state as of 2001 (excluding the lagged firearm homicide rate, which is the rate from 2000 as this is the year directly prior to my window of investigation). I will also run each regression including only the state and year fixed effects.

6 Results and Discussion

6.1 Estimated Effects:

Tables 7, 8, and 9 in section 8 show the β_s coefficients from the event-study regression where treatment is defined as the enactment of an SYG law, the passage of a “shall issue” or permitless carry law, and the enactment of an additional mental-health related law, respectively. Each row in these tables presents the difference-in-differences estimate of that year as compared to the year before the treatment year. Ideally, there should be no difference in trends between the treated and untreated states during the pre-treatment years and thus the confidence intervals for each β_s during the pre-treatment period should be relatively small and should encompass 0. If this is the case, then we cannot reject the validity of parallel trends assumption in the pre-treatment period, thus increasing the plausibility that these coefficients are the true causal effects of passing given gun laws.

Additionally, for each definition of treatment, I ran the given regression four times, controlling for four slightly different sets of pre-treatment covariates, and then again including only the state and year fixed effects.² However, in each case, altering these sets of covariates had little effect on the resulting estimated effects.

In order to be able to visually assess the trends in coefficients, Figures 9, 10, and 11 in section 8 plot these β_s coefficients and their corresponding 95% confidence intervals for Tables 7, 8, and 9, respectively. Because the different panels of pre-treatment covariates did not lead to significant differences in the estimated effects, I will only show the coefficient plots for the regressions using the full panel of covariates. In each of the following figures, Panel A displays the estimates from the model using the dynamic TWFE approach and Panel B displays the estimates from the model using the Sun-Abraham method. Because the outcome variable is the log of the age-adjusted firearm homicide rate, a point estimate of 0.1 corresponds to an approximately 10% change in the firearm homicide rate.

6.1.1 Effect of Enacting an SYG Law on Firearm Homicides

Figure 9 displays the estimated effects of passing an SYG law. Looking first at the dynamic TWFE estimates in Panel A shows that most of the relevant post-treatment coefficients from this model are approximately 0.1.³ However, most of the pre-treatment coefficients from this model are also around 0.1, and the confidence intervals of all of said estimates encompass this value. Therefore, there does not appear to be evidence of enacting an SYG law having any significant effect on the age-adjusted firearm homicide rate. To further illustrate this, Figure 15 in Section 8 shows the estimates obtained from a similar dynamic TWFE model when the reference year is two years prior to treatment as opposed to one year prior. When

²See Figures 12, 13, and 14 in Section 8 for coefficient plots comparing the point estimates and confidence intervals for the models with the full panel of covariates to those of the models including only state and year FEs.

³The estimated effects for ten or more years post treatment are not as close to 0.1. However, due to when most of the treated states enacted their specific SYG laws, these estimates were calculated using very few treated observations, have confidence intervals encompassing both positive and negative values, and are thus less relevant.

using this regression specification, most of the relevant pre- and post-treatment estimated effects, excluding the one corresponding to the year directly before a state’s SYG law goes into effect, are close to zero, and all of the confidence intervals encompass zero. As such, the parallel trends assumption appears to hold until the year directly prior to treatment when the treated states experience a differential drop in firearm homicides, providing evidence of potential anticipation of or selection into treatment based on some shock at the end of the pre-treatment period.

The estimates from the Sun-Abraham model displayed in Panel B show a similar phenomenon where most of the relevant post-treatment coefficients are again around 0.1, and there is a significant drop in homicides in the year directly prior to treatment. Furthermore, the pre-treatment estimates from the Sun-Abraham are more varied and further from both 0 and 0.1, particularly in the three years leading up to the year a state enacts an SYG law where there appears to be a more consistent differential decreasing trend in the age-adjusted firearm homicide rate for treated states. Therefore, if we believe that this method is indeed more accurate than the dynamic TWFE approach, this provides further evidence that there are likely violations of the parallel trends assumption close to the time of treatment, and it would thus be misleading to interpret some of the significant positive point estimates in the post-treatment period as the causal effect of enacting an SYG law.

6.1.2 Effect of Enacting a “Shall Issue” or Permitless Concealed Carry Law on Firearm Homicides

Next, looking at Figure 10, which shows the estimated effects of a “shall issue” or permitless concealed carry law on firearm homicides, it appears that the point estimates and corresponding confidence intervals obtained from the dynamic TWFE model (shown in Panel A) and those obtained from the Sun-Abraham approach (shown in Panel B) are almost exactly the same. This is especially true in the post-treatment period, where all of the estimates are positive and increasing. (This fact can be further illustrated by looking at the exact es-

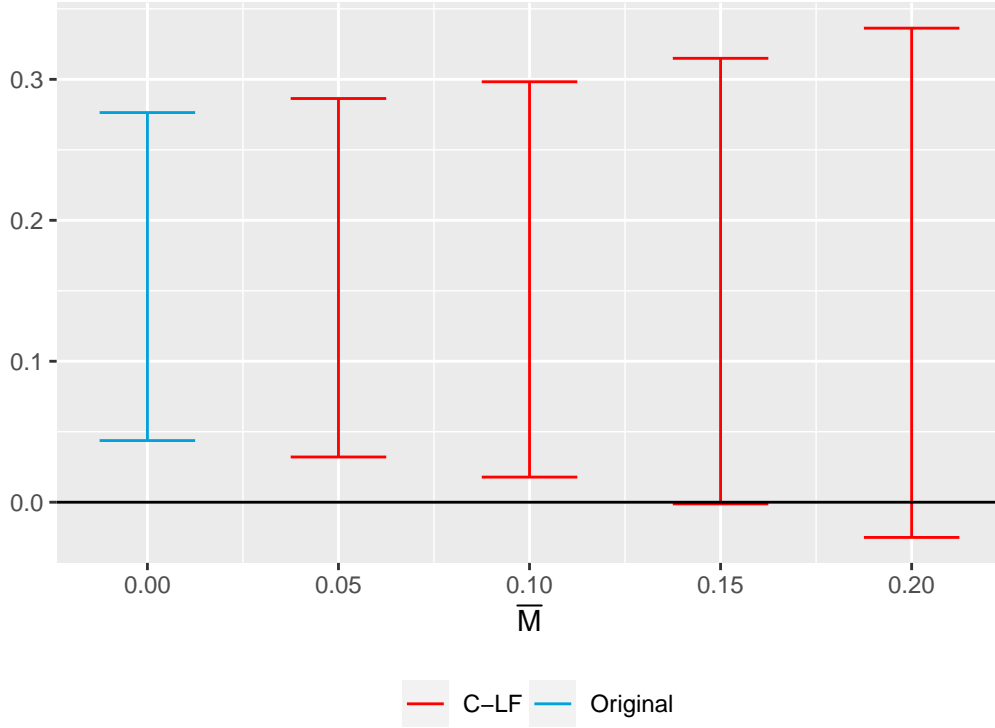
estimates shown in Columns (1) and (5) of Table 8). As such, if one had simply assumed that the parallel trends assumption holds, it would appear that enacting a “shall issue” or permitless concealed carry law does have a significant positive effect on the age-adjusted firearm homicide rate. However, while the pre-treatment coefficients obtained from the Sun-Abraham method are slightly closer to zero than those from the dynamic TWFE model, the estimates from both models show a clear trend that continues throughout the pre- and post-treatment periods. Therefore, the patterns in the post-treatment period may be due to violations of the parallel trends assumption as opposed to effects of passing this type of gun law.

However, as discussed in Rambachan and Roth (2023), in the case where there is a violation of the parallel trends assumption in the pre-treatment period, we can obtain bounds on the treatment effect if we assume that the violations of parallel trends in the post-treatment period are no larger than some constant \bar{M} times the maximum violation of parallel trends in the pre-treatment period.

To determine if allowing for violations of parallel trends yields significant results in regards to the effect of enacting a “shall issue” or permitless concealed carry law on firearm homicides, I used the HonestDiD package developed by Rambachan and Roth to run a sensitivity analysis on the coefficients obtained from the dynamic TWFE regression. Since the coefficient for relative year 0 was insignificant, I ran this analysis on the estimate for relative year 1. Since almost all of the coefficients from the Sun-Abraham model up until relative year 8 are insignificant, I did not run a similar analysis on the estimates from this model. Figure 3 plots the confidence intervals for different values of \bar{M} for the coefficients from the dynamic TWFE model.

This figure shows that the significant positive effect is robust only to allowing for violations 15% the size of those in the pre-period. Therefore, if we were willing to allow for violations of parallel trends as large as those in the pre-treatment period, the estimated effects of enacting a “shall issue” or permitless concealed carry law on the firearm homicide

Figure 3: Bounding the Relative Magnitude of Parallel Trends Violations



Note: This figure plots the confidence intervals associated with different values of \bar{M} .

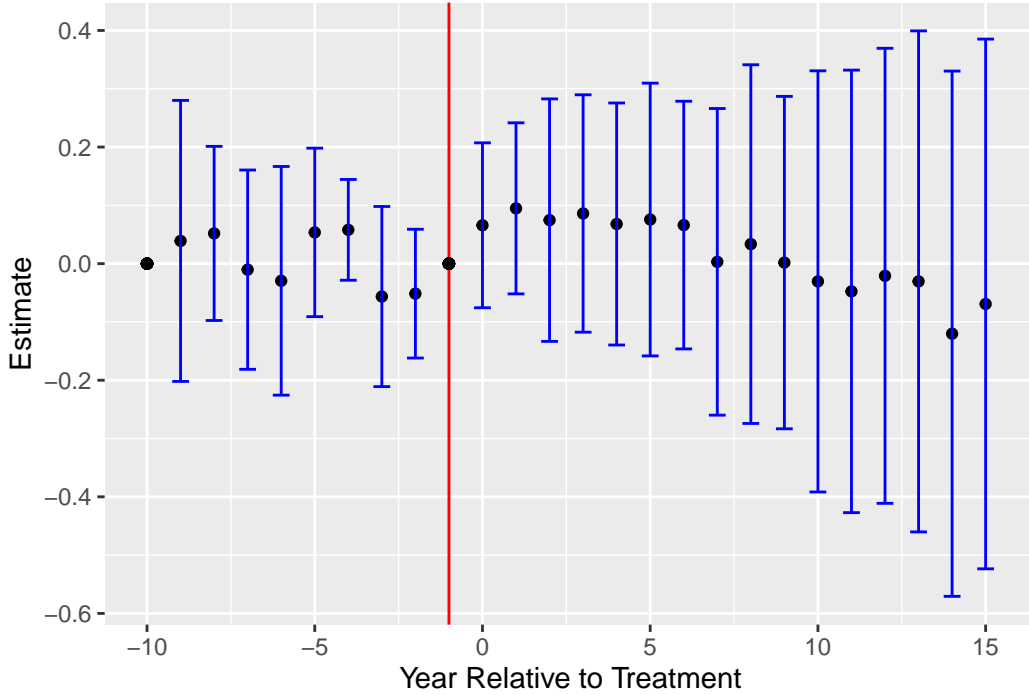
rate would not be significant.

We can also consider what happens if we allow for linear violations of parallel trends by including a relative linear time trend. Figure 4 displays the point estimates from a modified version of the dynamic TWFE model that extrapolates the trend between the first and last pre-treatment periods.

As shown, when we allow for the continuation linear pre-trend, the estimated effects from the post-treatment period that were previously positive and significant become smaller in magnitude and are no longer significant. Furthermore, approximately five years after a law is enacted, these effects reverse from positive to negative, although the confidence intervals surrounding the point estimates also increase dramatically.

In summary, the results from the analysis using HonestDiD shows that the violations of parallel trends exhibited in Figure 10 can be explained by a close to linear pre-trend.

Figure 4: Estimated Effects of Expansive Concealed Carry Laws, Allowing for Linear Trends



Note: This figure plots the difference-in-differences estimates from the dynamic TWFE regression where treatment is defined as enacting a “shall issue” or permitless concealed carry law and an additional relative time term is added to the specification to allow for a continuation of the pre-trend observed in the original model.

However, if we perform a reasonable extrapolation of said trend by including a linear time adjustment, the resulting estimates are no longer significant. Thus the initial positive estimates for “shall issue” or permitless carry laws could be explained by violations of parallel trends rather than a treatment effect.

6.1.3 Effect of Enacting an ERPO Law on Firearm Homicides

Finally, looking at Figure 11, which presents the estimated effects of passing an ERPO law, the estimates obtained from the dynamic TWFE model (shown in Panel A) and those from the Sun-Abraham method (shown in Panel B) are again extremely similar. In both plots, the point estimates from the pre-treatment period are very close to zero and their respective confidence intervals all encompass zero.

However, running a chi-square test on the pre-treatment coefficients from the dynamic

TWFE model yields an F-statistic of 18.035 and p-value of 0.0348, rejecting the null hypothesis that these estimates are jointly zero. Additionally, performing a chi-square test on the pre-treatment estimates from the Sun-Abraham model yields an F-statistic of 22.106 and p-value of 0.009. Therefore, regardless of which method we use, it turns out that we can actually reject the null hypothesis that all of the pre-treatment coefficients are zero, which casts doubt on the validity of the parallel trends assumption in the case where treatment is defined as enacting an ERPO law. Thus we may worry that the estimated effects are biased for the true causal effect of these laws.

As for the post-treatment estimated effects, the values obtained from both models are relatively close to zero until eight years post-treatment when there is a large jump to more positive effects and then another to more negative effects. However, all of these point estimates have large confidence intervals that include both positive and negative values. Therefore, even if we were confident in the validity of the parallel trends assumption, it would still be hard to say anything definitive about the true effect of ERPO laws on firearm homicides.

6.2 Further Discussions of Parallel Trends:

The presence of parallel trends violations in the case of all three categories of laws I investigate is unfortunate as it casts doubt on the degree to which any estimated effects of enacting an SYG law, “shall issue” or permitless concealed carry law or ERPO law on firearm homicides is actually caused by the passage of said law. However, given the significant differences between the states that do and do not pass a given law, these violations are not unexpected. Tables 2, 3, and 4 display summary statistics for the full panel of covariates, comparing treated states to untreated states. In Table 2, treatment is defined as passing an SYG law. In Table 3 a treated state is one that enacts a “shall issue” or permitless carry law and in Table 4 treatment is the passing of an ERPO law. In addition to the means and standard errors, each table displays the difference in means between the two groups and the corresponding t-statistic.

Table 2: Summary Statistics by Presence or Absence of an SYG Law

Variables	SYG Law mean (s.e.)	No SYG Law mean (s.e.)	Diff.	t-stat
% Black	0.12 (0.11)	0.083 (0.081)	0.0412	5.98
% Male + Age 15-29	0.1 (0.01)	0.099 (0.0068)	0.00419	6.77
Law Enforcement per Capita	2.2 (0.46)	2.3 (0.52)	-0.0526	-1.63
Poverty Rate	12.4 (3.3)	10.7 (3.1)	1.73	8
Unemployment Rate	4.7 (0.74)	4.4 (0.93)	0.303	5.58
Alcohol Consumption per Capita	2.2 (0.54)	2.3 (0.39)	-0.148	-4.43
Incarceration Rate	0.0048 (0.0018)	0.004 (0.0017)	0.000851	7.24
Log of Population	15.1 (0.9)	15 (1.1)	0.152	2.36
Population Density	38.1 (29.7)	84.4 (110.1)	-46.3	-10.2
Violent Crime per Capita	456.5 (186.5)	395.5 (185.4)	61	4.87
Gun Ownership	0.44 (0.087)	0.36 (0.14)	0.0858	12
Age-Adjusted Firearm Homicide Rate (2001)	3.9 (2.2)	2.8 (1.9)	1.09	7.73
Observations	330	670		

Note: This table displays the mean and standard error for each control for states with an SYG law as compared to those without one, as well as the difference and t-statistic obtained from comparing said means. The values for the percent of the population that is Black (% Black), the percent of the population that is male between the ages of 15-29 (% Male + Age 15-19) are displayed as decimals. Per capita alcohol consumption is measured in gallons per person. Incarceration rate is calculated as the number of incarcerated persons for each person in a given state. Population density is measured as persons per square mile.

As shown, for all three laws, the t-statistic is greater than 1.96 for almost all of these observed characteristics, which means that there is a statistically significant difference between the mean for the states that enact a given law and the mean for those that do not. Although I condition on these controls in my regressions, the apparent high degree of selection into specific treatment groups based on these observable characteristics leads to the concern that there is also selection on unobservable characteristics, which casts doubt on the conditional parallel trends assumption. Furthermore, the fact that the states that pass a given gun law and those that do not are significantly different among a number of observable covariates is indicative of the firearm homicide rates of said groups of states trending on different paths from the beginning. These differences thus provide a potential explanation for why the parallel trends assumption may be violated in the case of all three categories of gun laws

6.3 Violations of the Strong Overlap Assumption:

In addition to the parallel trends assumption being violated in the case of all three types of laws, when looking at the effects of passing an expansive concealed carry law or ERPO

Table 3: Summary Statistics by Type of Concealed Carry Law

Variables	"May Issue" Law	"Shall Issue" or Permitless Carry Law	Diff.	t-stat
	mean (s.e.)	mean (s.e.)		
% Black	0.092 (0.098)	0.11 (0.079)	-0.0183	-2.96
% Male + Age 15-29	0.1 (0.0084)	0.096 (0.0062)	0.00581	11.6
Law Enforcement per Capita	2.2 (0.43)	2.6 (0.53)	-0.452	-12.1
Poverty Rate	11.7 (3.3)	9.7 (2.6)	1.98	9.6
Unemployment Rate	4.6 (0.91)	4.3 (0.74)	0.3	5.19
Alcohol Consumption per Capita	2.3 (0.49)	2.3 (0.29)	-0.00378	-0.147
Incarceration Rate	0.0042 (0.0017)	0.0044 (0.0018)	-0.000124	-0.938
Log of Population	15 (0.97)	15.3 (1.1)	-0.339	-4.3
Population Density	33.8 (29.5)	178 (133.8)	-144.2	-16.7
Violent Crime per Capita	404.7 (195)	449.3 (159.9)	-44.6	-3.59
Gun Ownership	0.44 (0.089)	0.24 (0.12)	0.199	24.1
Age-Adjusted Firearm Homicide Rate (2001)	3.3 (2.1)	2.9 (1.9)	0.403	2.79
Observations	755	245		

Note: This table displays the mean and standard error for each control for states with an “may issue” law as compared to those with a “shall issue” or permitless carry law, as well as the difference and t-statistic obtained from comparing said means. The values for the percent of the population that is Black (% Black), the percent of the population that is male between the ages of 15-29 (% Male + Age 15-19) are displayed as decimals. Per capita alcohol consumption is measured in gallons per person. Incarceration rate is calculated as the number of incarcerated persons for each person in a given state. Population density is measured as persons per square mile.

law, there is also evidence that the strong overlap assumption may be violated. For each definition treatment, I used a logit regression propensity score model to estimate $\hat{p}_C(X_i)$, namely the probability of being treated at some point (i.e., $C = 0$) given a set of covariates X_i . Figure 5 displays the distributions of these propensity scores where treatment is defined as enacting an expansive concealed carry law, an SYG law, and ERPO law in Panels A, B, and C, respectively.

As can be seen in Panels A and C, the distributions of propensity scores where treatment is defined as enacting a “shall issue” or permitless concealed carry law (Panel A) and an ERPO law (Panel C) are heavily skewed away from the middle. To illustrate this further, Table 5 presents, for each definition of treatment, the percent of propensity scores between 0 and 0.1 and the percent of scores between 0 and 0.9.

In order for the strong overlap assumption to hold, the probability that a state becomes treated given its pre-treatment characteristics should be bounded away from 1. However, as shown, when treatment is defined as enacting a “shall issue” or permitless concealed carry law, 18% of states have a probability less than 0.1 of being treated and 82% of states have

Table 4: Summary Statistics by Presence or Absence of an ERPO Law

Variables	ERPO Law mean (s.e.)	No ERPO Law mean (s.e.)	Diff.	t-stat
% Black	0.087 (0.057)	0.097 (0.097)	-0.0106	-1.48
% Male + Age 15-29	0.095 (0.0075)	0.1 (0.0082)	-0.00539	-6.06
Law Enforcement per Capita	2.3 (0.48)	2.3 (0.5)	-0.00077	-0.0136
Poverty Rate	9.2 (2.2)	11.4 (3.3)	-2.19	-8.01
Unemployment Rate	4.2 (1.1)	4.5 (0.86)	-0.333	-2.7
Alcohol Consumption per Capita	2.2 (0.28)	2.3 (0.46)	-0.0275	-0.797
Encarceration Rate	0.0043 (0.0015)	0.0043 (0.0018)	8.51e-05	0.476
Log of Population	15.3 (0.93)	15 (1)	0.321	2.92
Population Density	152.2 (117.7)	62 (88.5)	90.2	6.66
Violent Crime per Capita	416.9 (160.6)	415.5 (190.1)	1.36	0.071
Gun Ownership	0.27 (0.1)	0.4 (0.13)	-0.123	-10.1
Age-Adjusted Firearm Homicide Rate (2001)	2.7 (1.4)	3.2 (2.1)	-0.504	-2.95
Observations	79	921		

Note: This table displays the mean and standard error for each control for states with ERPO laws as compared to those without, as well as the difference and t-statistic obtained from comparing said means. The values for the percent of the population that is Black (% Black), the percent of the population that is male between the ages of 15-29 (% Male + Age 15-19) are displayed as decimals. Per capita alcohol consumption is measured in gallons per person. Incarceration rate is calculated as the number of incarcerated persons for each person in a given state. Population density is measured as persons per square mile.

Table 5: Summary of Propensity Score Distributions

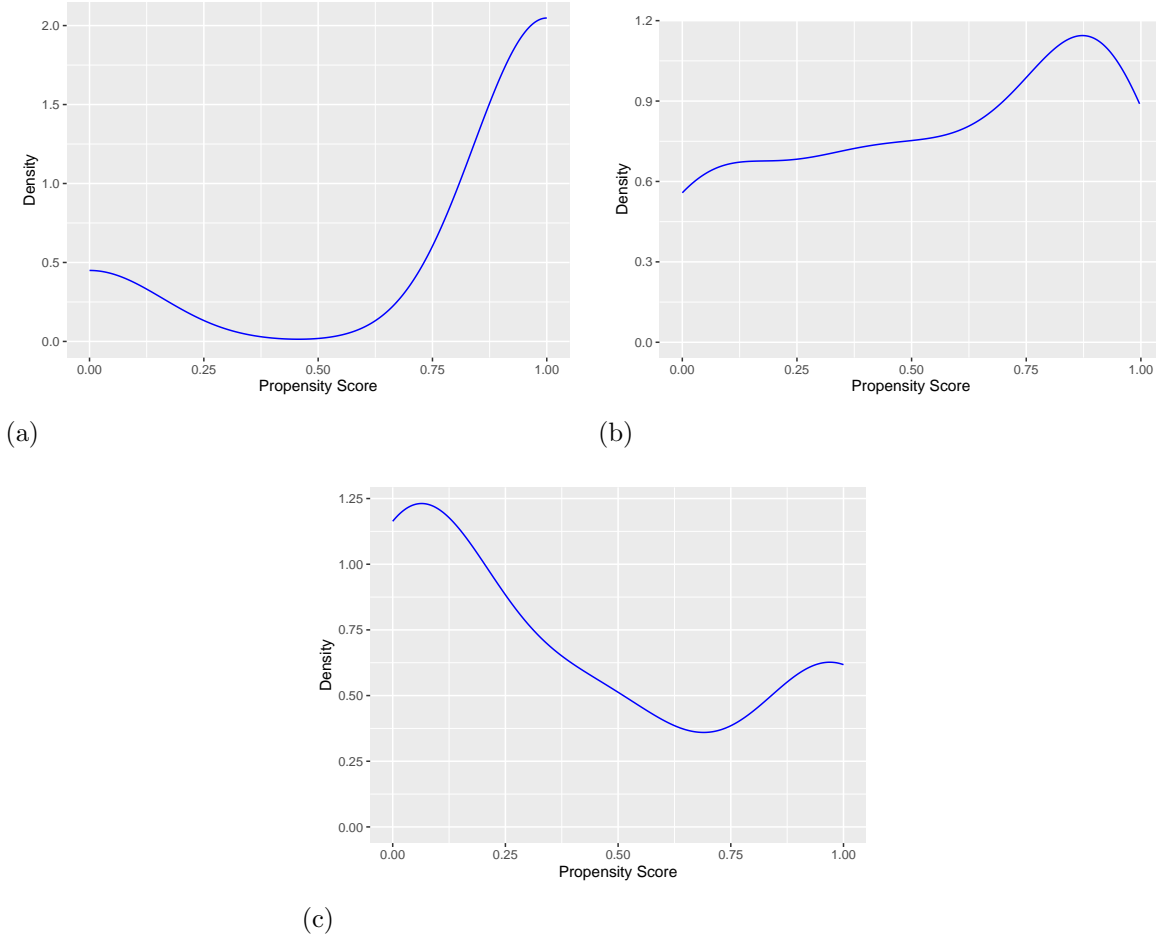
Law Type	% of Propensity Score ≤ 0.1	% of Propensity Score ≥ 0.9
SYG	0.16	0.24
Expansive Concealed Carry	0.18	0.82
ERPO	0.36	0.24

Note: This table displays, for each definition of treatment, the percent of states with a propensity score less than 0.1 and the percent with a score greater than 0.9.

a probability greater than 0.9. Therefore, for this definition of treatment, there is relatively weak overlap between states that do and those that pass this type of firearm legislation. Similarly, when treatment is defined as enacting an ERPO law, a total of 60% of states have a probability of being treated that is either less than 0.1 or greater than 0.9. As such, for this definition of treatment, there is also a lack of overlap between treated and untreated states.

As such, the lack of strong overlap between states that do and do not enact an expansive concealed carry law or ERPO law presents yet another reason why causal interpretation of the estimated effects of these three types of gun laws should be taken with caution

Figure 5: Distribution of Propensity Scores for Each Definition of Treatment



Note: Each panel shows the distribution of propensity scores for a given definition of treatment. In Panel A, treatment is defined as enacting a “shall issue” or permitless concealed carry law. In Panel B, it is defined as passing an SYG law. In Panel C, treatment is defined as enacting an ERPO law.

6.4 Examples of Potential Confounding Variables:

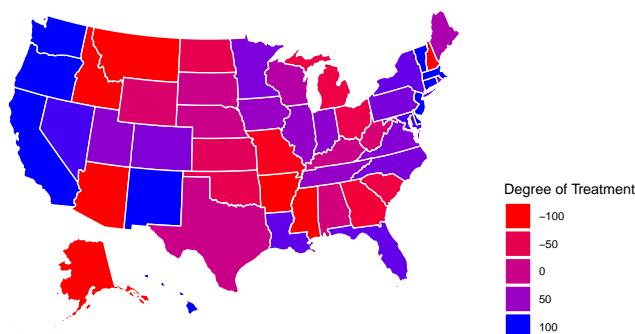
If, prior to the enactment of a given gun law, the average homicide rates among states that do and do not eventually implement said law had moved in parallel and there was evidence of overlap between these groups of states, we would be able to obtain causal estimates of the effects of these specific policies even with the presence of potentially omitted variables. However, given the concern that these assumptions are not valid when treatment is defined as enacting an SYG, expansive concealed carry, or ERPO law, it is likely that there are confounding variables biasing the estimated effects of these laws. Two specific potential

confounders, which many papers in the current literature do not discuss, are states' political leanings and the fact that states often pass numerous gun laws simultaneously or within the span of a few years.

6.4.1 States' Political Climates

One specific potential confounding variable is the political leaning of states that do or do not pass specific types of gun laws. Figure 6 displays the number of restrictive as compared to expansive firearm policies enacted by each state as a percent of the total number of regulations said state enacted between 2001 to 2020.

Figure 6: "Expansive" and "Restrictive" Laws by State



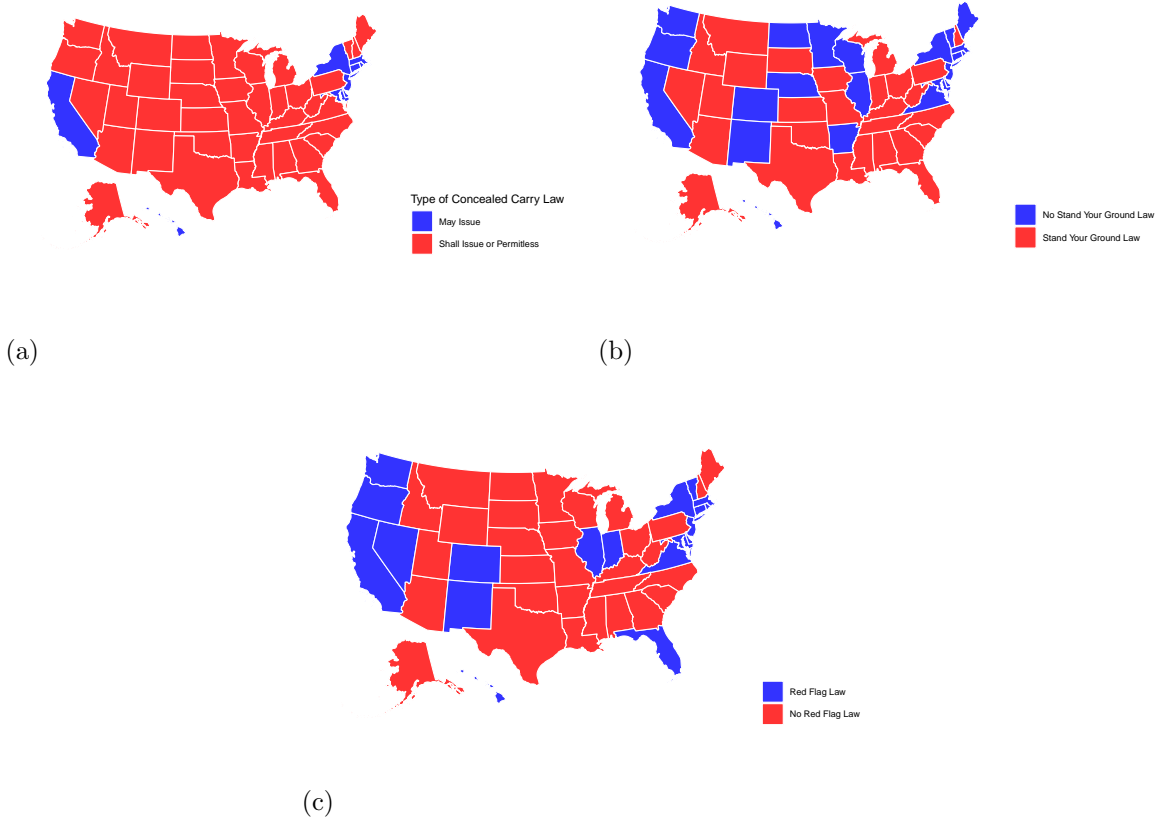
Note: This figure color codes each state by the number of restrictive laws compared to expansive laws as a percentage of the total number of laws it enacted from 2001 to 2020 (i.e., states with a "treatment score" of -100 implemented only expansive laws, whereas states with a "treatment score" of 100 implemented only restrictive laws).

This map shows that states that traditionally lean Democrat passed a higher percentage of restrictive as opposed to expansive laws, and states that traditionally lean Republican passed a higher percentage of expansive laws.

Figure 7 shows similar maps, specifically depicting which states have enacted SYG laws and which have "shall issue" or permitless concealed carry laws.

Again, many of the states that pass SYG laws and more expansive concealed carry laws are ones that tend to lean Republican, and many of the states with ERPO laws are ones that

Figure 7: SYG, Concealed Carry, and ERPO Laws by State in 2020



Note: Panel A shows which states have “may issue” as compared to “shall issue” or permitless concealed carry laws as of 2020. Panel B illustrates which states do and do not have SYG laws as of 2020. Panel C depicts which states do and do not have ERPO laws as of 2020.

lean Democrat, showing that the political preferences of a state likely impact said state’s selection into treatment – namely, whether or not it ends up enacting a given gun law. However, a state’s political leaning is not accounted for in many of the papers studying gun laws, showing this is potentially an omitted variable biasing the estimated effects of specific firearm regulations.

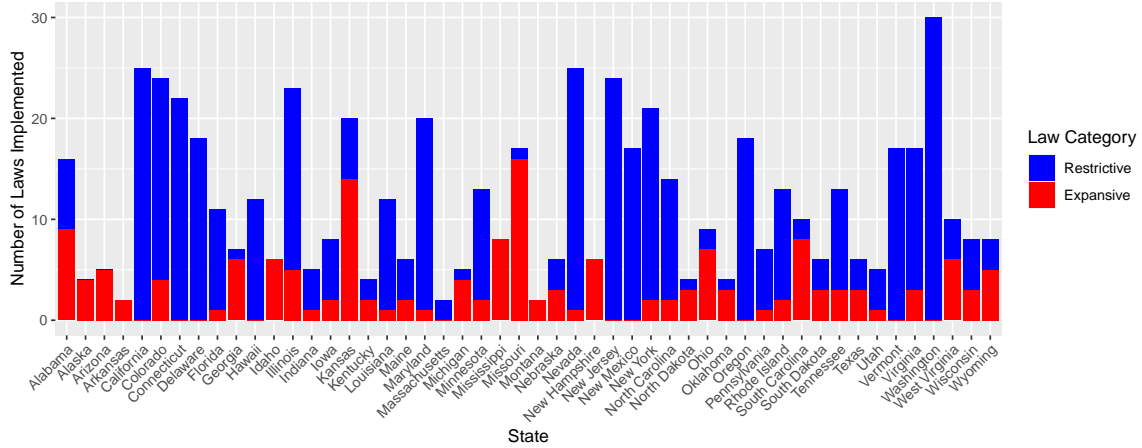
6.4.2 Enacting Multiple Gun Laws

Another reason why the causal effect of a given gun law on firearm homicides is difficult to estimate is that, in any given span of years, many states pass multiple firearm regulations. Furthermore, oftentimes some of these laws are expansive and make it easier for citizens to

obtain guns whereas others are restrictive and increase regulations surrounding the purchase of possession of certain types of firearms.

Figure 8 shows, for each state, the number of expansive and restrictive laws implemented between 2001 to 2020, inclusive.

Figure 8: Degree of Treatment by State



Note: This figure displays, for each state, the number of restrictive and expansive gun laws enacted over the entire 2001-2020 year range.

Looking at this graph, we can see that 19 states enacted 10 or more laws within this time window and 30 states passed both expansive and restrictive policies. The number of states that implement multiple gun laws within this range of years, as well as the fact that a majority of states pass laws with potentially opposing effects on gun homicides and other gun-death related outcomes, illustrates another potential reason why papers studying the effects of one type of law in isolation struggle to find significant effects or effects that can feasibly be interpreted as causal.

Furthermore, even in the hypothetical case where there was evidence of parallel trends in the pre-treatment period, because the presence of additional gun laws is an omitted variable that is likely correlated with states' firearm homicide rates, we would still be worried that this confounder is the biasing estimated effects of a specific firearm regulation. To combat this issue, we would ideally want to control for the degree to which a state is treated, and more specifically, which other policies it has passed. Another potential approach to handling this

problem would be to design a multivariable model that looks at the effects of different laws simultaneously and the interactions between said laws. However, as per the comprehensive database of gun laws used in this paper and others in the literature, there are at least 135 different firearm regulations. Therefore, it is practically impossible to control for the presence or lack thereof of all other possible gun laws while also having enough data to make viable comparisons between states with different combinations of laws.

Another issue with controlling for the presence of other gun laws is that the specifics of many of these laws often differ on a state-by-state basis. For example, as discussed in section 3.3, while there are 18 states that passed an ERPO law between 2001-2020, different states' regulations vary in terms of the specifics of who can petition for the issuance of an ERPO and the process that the petitioner follows. However, if one were to divide these states into separate treatment groups based on the nuances of their individual ERPO law, there would likely not be enough treated units in each group to accurately estimate the effects of a specific type of ERPO law. In this way, variations between states with what is categorized as the same law make it even harder to investigate the effects of specific policies.

7 Conclusion

This paper focuses on three types of gun laws – SYG laws, “shall issue” and permitless carry laws, and ERPO laws – and explores the plausibility of estimating the causal effect that enacting these laws has on firearm homicides. Recent research regarding the commonly used static difference-in-differences identification strategy has shown that staggered treatment timing can bias the estimates obtained from this type of model. I employ the newest difference-in-differences methods that were designed specifically to correct for these issues, finding that enacting an expansive concealed carry law has a statistically significant positive effect on the firearm homicide rate and that enacting an SYG or ERPO law has no statistically significant effect. However, the use of these methods also shows that numerous

identifying assumptions that are critical to the interpretation of the estimates obtained from these models as causal (but are not discussed in many studies in the existing literature) may not be valid when treatment is defined as enacting one of these three types of firearm policies. Therefore, the estimated effects I obtain, as well as those in much of the existing literature, may be explained by the presence of pre-existing differences in the firearm homicide rate trends in states that do and those that do not eventually institute one of these laws. The violation of key assumptions also means that any estimated effects are potentially biased by omitted variables, such as states' political preferences, which is potentially correlated with the chance that a specific state will pass a certain type of law, as well as the enactment of additional gun laws beyond the policy of interest, which could lead to confounding shocks to states' firearm homicide rates.

This paper provides a template for evaluating difference-in-differences designs and identifying assumptions that can be applied to studies of other types of firearm regulations. In my research, I chose to only look into three specific gun laws, as well as solely examine the effect of these policies on firearm homicides. Therefore, one avenue for future research is to similarly investigate whether these assumptions hold when looking at other types of firearm policies or other outcomes, such as firearm suicides, overall violent crime, and mass shootings.

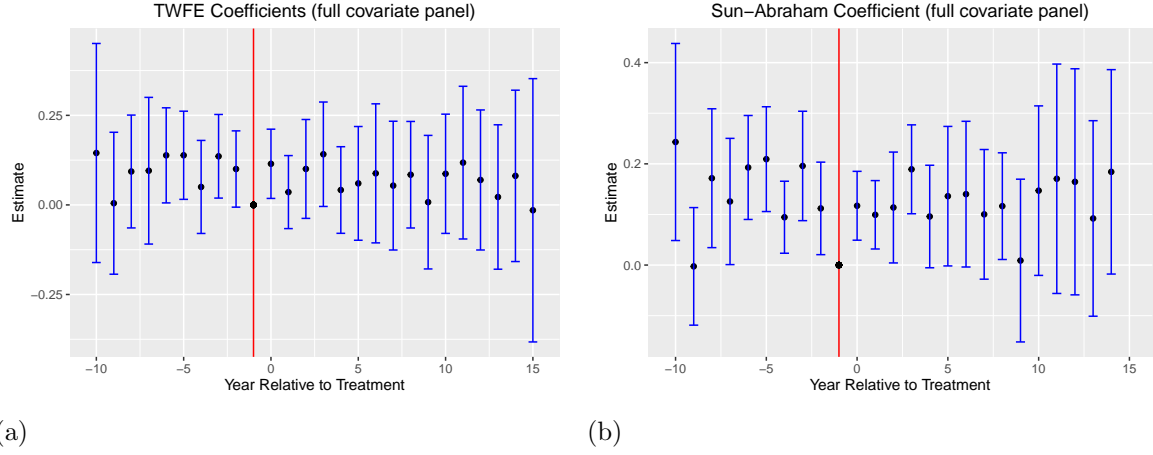
Another potential direction for further research is to explore aspects of SYG, expansive concealed carry laws, and ERPO laws that this paper does not focus on. For example, as is common in the literature, when looking at the effect of expansive concealed carry laws, I divided states into two groups – those with “may issue” laws and those with “shall issue” or permitless concealed carry laws. However, it is possible that it is not the difference between these two categories but the difference between having a “may” or “shall issue” law as opposed to allowing permitless concealed carry that has an impact on firearm homicides. Most ERPOs only last a few weeks to months before the restrained party is allowed to purpose and possess firearms again. Therefore, it would be interesting to further consider

the effect of these laws on firearm homicides using more granular (e.g., month-level) homicide data.

Finally, this paper provides evidence that differences-in-differences might not be the best causal inference design to use to learn the effects of gun laws on firearm homicides. Therefore, it would also be useful to investigate whether other types of models are more suited towards determining the effects of these policies.

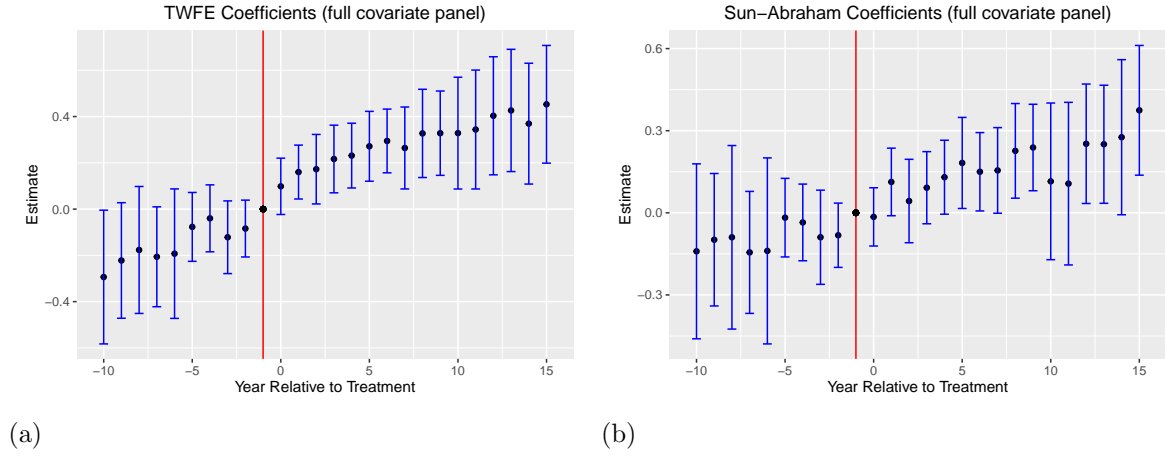
8 Additional Figures

Figure 9: Effect of SYG Laws on Firearm Homicide Rate



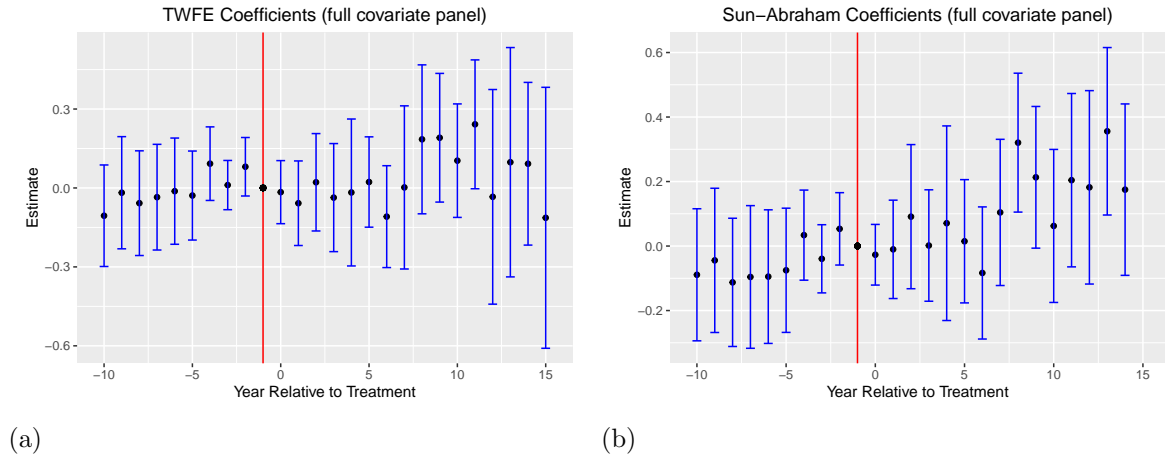
Note: These plots present the difference-in-differences estimates from the event-study regression where treatment is defined as passing an SYG law. Panel A displays the estimates from the dynamic TWFE model and Panel B the estimates from the model using the Sun-Abraham estimation method. Each point represents the difference-in-differences estimate for that year as compared to the pre-treatment year.

Figure 10: Effect of Expansive Concealed Carry Laws on Firearm Homicide Rate



Note: These plots present the difference-in-differences estimates from the event-study regression where treatment is defined as enacting a “shall issue” or permitless concealed carry law. Panel A displays the estimates from the dynamic TWFE model and Panel B the estimates from the model using the Sun-Abraham estimation method. Each point represents the difference-in-differences estimate for that year as compared to the pre-treatment year.

Figure 11: Effect of ERPO Laws on Firearm Homicide Rate



Note: These plots present the difference-in-differences estimates from the event-study regression where treatment is defined as enacting an ERPO law. Panel A displays the estimates from the dynamic TWFE model and Panel B the estimates from the model using the Sun-Abraham estimation method. Each point represents the difference-in-differences estimate for that year as compared to the pre-treatment year.

Table 6: Literature Review Overview

Paper	Law	Window of Investigation	Empirical Strategy	Discussion of Assumptions	Results
Gius (2016)	SYG	1980-2011	static DiD with TWFE	no	significant effect of SYG laws on overall homicides; uncertain effects
Munasib et al. (2018)	SYG	1999-2013	static DiD with TWFE	yes	uncertain effects of SYG laws on firearm homicides at state level; significant positive effect when disaggregate by level of urbanization as opposed to by state
Webster et al. (2014)	SYG	1999-2013	static DiD with TWFE	no	uncertain effects
Siegel et al. (2019)	SYG	1991-2016	static DiD with TWFE	no	uncertain effects
Siegel et al. (2020)	SYG	1991-2016	static DiD with TWFE	no	uncertain effects
Knopov et al. (2019)	SYG	1991-2016	static DiD with TWFE	no	no effect
Schell et al. (2020a)	SYG	1980-2016	autoregressive negative binomial regression model	N/A	uncertain effects
Humphreys et al. (2017)	SYG	2005-2014	interrupted time series design	N/A (but model had no covariates)	significant positive effect in FL
Guettabi and Munasib (2018)	SYG	1991-2011	synthetic control matching	N/A	positive effect in 3 states; uncertain effects in 11 states
Crifasi et al. (2018)	SYG	1984-2015	Poisson regression with random intercepts for counties and FEs for year	no	positive effect
McClellan and Tekin (2017)	SYG	2000-2010	static DiD with TWFE	blah	positive effect for white males; uncertain effects for black males
Hamill et al. (2019)	expansive concealed carry	1986-2015	blah	N/A	no effect
Knopov et al. (2019)	expansive concealed carry	1991-2016	static DiD with TWFE	no	significant positive effect
Fridel (2021)	expansive concealed carry	1991-2016	GEE with TWFE	no	positive effect
Siegel et al. (2019)	expansive concealed carry	1991-2016	static DiD with TWFE	no	significant positive effect
Donohue et al. (2019)	expansive concealed carry	1977-2014	static DiD with TWFE	yes	PTA not satisfied when outcome is firearm homicides; uncertain effects
Luca et al. (2017)	expansive concealed carry	1977-2014	static DiD with TWFE	no	uncertain effects
Schell et al. (2020a)	expansive concealed carry	1980-2016	autoregressive negative binomial regression model	N/A	uncertain effects
Gius (2020)	ERPO	990-2017	synthetic control matching	N/A	negative effects in CT; positive effects in IN
Delafave (2021)	ERPO	1990-2018	static DiD with TWFE	no	no effect

Table 7: Effect of Enacting an SYG Law on Age-Adjusted Firearm Homicide Rate in a Given Year

Dependent Variable:	Log of Age-Adjusted Rate				Sun-Abraham			
	Dynamic TWFE		(4)		(6)		(7)	
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Variables</i>								
Year: -18	-0.1798 (0.1428)	-0.2287 (0.1425)	-0.2060 (0.1428)	-0.2524 (0.1305)	-0.5215 (0.1451)	-0.5729 (0.1466)	-0.5386 (0.1696)	-0.5484 (0.1496)
Year: -17	0.2877 (0.3747)	0.2463 (0.3667)	0.2479 (0.3485)	0.2080 (0.3411)	0.2797 (0.1393)	0.2271 (0.1269)	0.0924 (0.1480)	0.0824 (0.1219)
Year: -16	-0.1323 (0.1401)	-0.1703 (0.1405)	-0.1925 (0.1271)	-0.2266 (0.1202)	-0.3245 (0.1037)	-0.3713 (0.0992)	-0.4255 (0.1131)	-0.4446 (0.1006)
Year: -15	-0.0496 (0.1389)	-0.0448 (0.1442)	-0.0893 (0.1325)	-0.0809 (0.1323)	-0.1140 (0.1066)	-0.1098 (0.1235)	-0.2391 (0.1116)	-0.2211 (0.1171)
Year: -14	-0.0293 (0.1557)	-0.0100 (0.1567)	-0.0968 (0.1447)	-0.0699 (0.1402)	-0.2937 (0.0904)	-0.2756 (0.0936)	-0.3827 (0.0883)	-0.3530 (0.0868)
Year: -13	0.0664 (0.2165)	0.1053 (0.2060)	0.0365 (0.2252)	0.0774 (0.2134)	-0.1107 (0.0775)	-0.0612 (0.0882)	-0.1722 (0.0761)	-0.1194 (0.0829)
Year: -12	0.1386 (0.2330)	0.1497 (0.2301)	0.0663 (0.2196)	0.0796 (0.2161)	0.3107 (0.1370)	0.3035 (0.1524)	0.1544 (0.1298)	0.1703 (0.1357)
Year: -11	0.2016 (0.1979)	0.2031 (0.1945)	0.1593 (0.1911)	0.1647 (0.1888)	0.2800 (0.0822)	0.2762 (0.0917)	0.1820 (0.0769)	0.1948 (0.0830)
Year: -10	0.1449 (0.1560)	0.1643 (0.1642)	0.1098 (0.1496)	0.1277 (0.1595)	0.2432 (0.0993)	0.2647 (0.1324)	0.1600 (0.1027)	0.1875 (0.1248)
Year: -9	0.0047 (0.1011)	0.0505 (0.1165)	-0.0204 (0.0943)	0.0186 (0.1063)	-0.0026 (0.0593)	0.0427 (0.0632)	-0.0457 (0.0563)	-0.0077 (0.0602)
Year: -8	0.0933 (0.0804)	0.1381 (0.0980)	0.0794 (0.0804)	0.1184 (0.0931)	0.1716 (0.0700)	0.2118 (0.0877)	0.1297 (0.0660)	0.1693 (0.0785)
Year: -7	0.0955 (0.1044)	0.1278 (0.1123)	0.0747 (0.1018)	0.1034 (0.1042)	0.1256 (0.0636)	0.1564 (0.0632)	0.0756 (0.0600)	0.1076 (0.0571)
Year: -6	0.1384 (0.0677)	0.1451 (0.0727)	0.1237 (0.0738)	0.1333 (0.0756)	0.1928 (0.0524)	0.1910 (0.0582)	0.1415 (0.0506)	0.1530 (0.0485)
Year: -5	0.1386 (0.0628)	0.1505 (0.0658)	0.1180 (0.0583)	0.1296 (0.0607)	0.2093 (0.0528)	0.2197 (0.0601)	0.1448 (0.0582)	0.1586 (0.0627)
Year: -4	0.0502 (0.0663)	0.0586 (0.0645)	0.0377 (0.0575)	0.0486 (0.0573)	0.0945 (0.0363)	0.0995 (0.0453)	0.0427 (0.0420)	0.0572 (0.0448)
Year: -3	0.1357 (0.0595)	0.1507 (0.0612)	0.1276 (0.0621)	0.1381 (0.0642)	0.1958 (0.0551)	0.2101 (0.0638)	0.1561 (0.0568)	0.1723 (0.0642)
Year: -2	0.1002 (0.0543)	0.1012 (0.0543)	0.0925 (0.0526)	0.0925 (0.0528)	0.1120 (0.0466)	0.1061 (0.0437)	0.0731 (0.0338)	0.0732 (0.0330)
Year: 0	0.1148 (0.0493)	0.1156 (0.0478)	0.1099 (0.0566)	0.1084 (0.0553)	0.1172 (0.0347)	0.1177 (0.0338)	0.0883 (0.0351)	0.0887 (0.0343)
Year: 1	0.0358 (0.0520)	0.0451 (0.0505)	0.0562 (0.0525)	0.0587 (0.0526)	0.0993 (0.0345)	0.1063 (0.0320)	0.0865 (0.0336)	0.0919 (0.0314)
Year: 2	0.1004 (0.0704)	0.1070 (0.0707)	0.1118 (0.0739)	0.1136 (0.0724)	0.1137 (0.0559)	0.1173 (0.0532)	0.1003 (0.0527)	0.1049 (0.0505)
Year: 3	0.1415 (0.0743)	0.1419 (0.0711)	0.1564 (0.0714)	0.1576 (0.0672)	0.1892 (0.0448)	0.1932 (0.0435)	0.1705 (0.0463)	0.1736 (0.0452)
Year: 4	0.0416 (0.0617)	0.0352 (0.0570)	0.0556 (0.0625)	0.0514 (0.0576)	0.0959 (0.0517)	0.0910 (0.0496)	0.0970 (0.0537)	0.0917 (0.0494)
Year: 5	0.0601 (0.0810)	0.0682 (0.0762)	0.0796 (0.0795)	0.0849 (0.0736)	0.1361 (0.0703)	0.1400 (0.0665)	0.1297 (0.0714)	0.1323 (0.0667)
Year: 6	0.0881 (0.0990)	0.0940 (0.0916)	0.1168 (0.0953)	0.1247 (0.0872)	0.1401 (0.0734)	0.1470 (0.0691)	0.1642 (0.0760)	0.1719 (0.0704)
Year: 7	0.0539 (0.0917)	0.0585 (0.0853)	0.0733 (0.0871)	0.0805 (0.0796)	0.1002 (0.0654)	0.1070 (0.0635)	0.1174 (0.0670)	0.1264 (0.0650)
Year: 8	0.0843 (0.0759)	0.0814 (0.0694)	0.1155 (0.0820)	0.1219 (0.0738)	0.1164 (0.0538)	0.1186 (0.0522)	0.1325 (0.0590)	0.1377 (0.0550)
Year: 9	0.0077 (0.0951)	0.0125 (0.0958)	0.0439 (0.0922)	0.0557 (0.0891)	0.0089 (0.0820)	0.0111 (0.0800)	0.0446 (0.0819)	0.0541 (0.0798)
Year: 10	0.0869 (0.0849)	0.0965 (0.0856)	0.1479 (0.0934)	0.1558 (0.0926)	0.1470 (0.0855)	0.1527 (0.0870)	0.1607 (0.0870)	0.1693 (0.0893)
Year: 11	0.1181 (0.1087)	0.1237 (0.1083)	0.1979 (0.1204)	0.2038 (0.1190)	0.1705 (0.1156)	0.1709 (0.1144)	0.2264 (0.1124)	0.2348 (0.1151)
Year: 12	0.0696 (0.0997)	0.0775 (0.0985)	0.1494 (0.1021)	0.1550 (0.0994)	0.1645 (0.1140)	0.1666 (0.1127)	0.2023 (0.1080)	0.2118 (0.1105)
Year: 13	0.0221 (0.1028)	0.0359 (0.1034)	0.0868 (0.1025)	0.0956 (0.1019)	0.0921 (0.0986)	0.0936 (0.0957)	0.1212 (0.0972)	0.1294 (0.0973)
Year: 14	0.0811 (0.1220)	0.0932 (0.1225)	0.1707 (0.1196)	0.1774 (0.1185)	0.1842 (0.1030)	0.1838 (0.1009)	0.2662 (0.1152)	0.2677 (0.1119)
Year: 15	-0.0150 (0.1875)	0.0300 (0.1843)	0.1168 (0.1850)	0.1265 (0.1846)	0.1842 (0.1030)	0.1838 (0.1009)	0.2662 (0.1152)	0.2677 (0.1119)
Year: 16	0.1896 (0.3646)	0.2367 (0.3373)	0.2233 (0.3877)	0.2082 (0.3541)	0.2883 (0.1590)	0.3120 (0.1659)	0.3607 (0.1617)	0.3554 (0.1620)
Year: 17	-0.2018 (0.3714)	-0.1531 (0.3603)	-0.1270 (0.3754)	-0.1350 (0.3602)				
Year: 18	0.0050 (0.3436)	0.0823 (0.2923)	0.0498 (0.3671)	0.0303 (0.3250)				
Year: 19	-0.3386 (0.4992)	-0.2694 (0.4762)	-0.3407 (0.4839)	-0.3085 (0.4641)				
<i>Fixed-effects</i>								
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Varying Slopes</i>								
Adults in Household with Gun (Year)	Yes	Yes			Yes	Yes		
Violent Crime per Capita (Year)	Yes	Yes			Yes	Yes		
Lagged Firearm Homicides (Year)	Yes	Yes	Yes		Yes	Yes	Yes	
<i>Fit statistics</i>								
Observations	940	940	940	940	939	939	939	939
R ²	0.95159	0.94957	0.94668	0.94509	0.97506	0.97308	0.97129	0.97004
Within R ²	0.05000	0.05105	0.05122	0.05192	0.51089	0.49385	0.48953	0.48316
<i>Clustered (State) standard-errors in parentheses</i>								

Note: This table presents estimated treatment effects for each year relative to the year before treatment first occurred, where treatment is defined as passing an SYG law. Columns (1) and (5) displays the estimates from models with the full panel of covariates, including lagged firearm homicide rates. Columns (2) and (6) reports the estimates from models with the full panel of covariates, but excluding lagged firearm homicide rates. The estimates shown in Column (3) and (7) are from models using the partial panel of covariates and including lagged firearm homicide rates and those in column (4) and (8) are from models including partial panel of covariates but excluding lagged firearm homicide rates. Additionally, Columns (1)-(4) use a standard dynamic TWFE model and Columns (5)-(8) use the Sun-Abraham method. Standard errors are robust and clustered at the state level.

Table 8: Effect of Enacting an Expansive Concealed Carry Law on Age-Adjusted Firearm Homicide Rate in a Given Year

Dependent Variable:		Log of Age-Adjusted Rate							
		Dynamic TWFE				Sun-Abraham			
Model:		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Variables</i>									
Year: -18		-0.1798 (0.1428)	-0.2287 (0.1425)	-0.2060 (0.1428)	-0.2524 (0.1305)	-0.5215 (0.1451)	-0.5729 (0.1466)	-0.5386 (0.1696)	-0.5484 (0.1496)
Year: -17		0.2877 (0.3747)	0.2463 (0.3667)	0.2479 (0.3485)	0.2080 (0.3411)	0.2797 (0.1393)	0.2271 (0.1269)	0.0924 (0.1480)	0.0824 (0.1219)
Year: -16		-0.1323 (0.1401)	-0.1703 (0.1405)	-0.1925 (0.1271)	-0.2266 (0.1202)	-0.3245 (0.1037)	-0.3713 (0.0992)	-0.4255 (0.1131)	-0.4446 (0.1006)
Year: -15		-0.0496 (0.1389)	-0.0448 (0.1442)	-0.0893 (0.1325)	-0.0809 (0.1323)	-0.1140 (0.1066)	-0.1098 (0.1235)	-0.2391 (0.1116)	-0.2211 (0.1171)
Year: -14		-0.0293 (0.1557)	-0.0100 (0.1567)	-0.0968 (0.1447)	-0.0699 (0.1402)	-0.2937 (0.0904)	-0.2756 (0.0936)	-0.3827 (0.0883)	-0.3530 (0.0868)
Year: -13		0.0664 (0.2165)	0.1053 (0.2060)	0.0365 (0.2252)	0.0774 (0.2134)	-0.1107 (0.0775)	-0.0612 (0.0882)	-0.1722 (0.0761)	-0.1194 (0.0829)
Year: -12		0.1386 (0.2330)	0.1497 (0.2301)	0.0663 (0.2196)	0.0796 (0.2161)	0.3107 (0.1370)	0.3035 (0.1524)	0.1544 (0.1298)	0.1703 (0.1357)
Year: -11		0.2016 (0.1979)	0.2031 (0.1945)	0.1595 (0.1911)	0.1647 (0.1888)	0.2800 (0.0822)	0.2762 (0.0917)	0.1820 (0.0769)	0.1948 (0.0830)
Year: -10		0.1449 (0.1560)	0.1643 (0.1642)	0.1098 (0.1496)	0.1277 (0.1595)	0.2432 (0.0993)	0.2647 (0.1324)	0.1600 (0.1027)	0.1875 (0.1248)
Year: -9		0.0047 (0.1011)	0.0505 (0.1165)	-0.0204 (0.0943)	0.0186 (0.1063)	-0.0026 (0.0593)	0.0427 (0.0632)	-0.0457 (0.0563)	-0.0077 (0.0602)
Year: -8		0.0933 (0.0804)	0.1381 (0.0980)	0.0794 (0.0804)	0.1184 (0.0931)	0.1716 (0.0700)	0.2118 (0.0877)	0.1297 (0.0660)	0.1693 (0.0785)
Year: -7		0.0955 (0.1044)	0.1278 (0.1123)	0.0747 (0.1018)	0.1034 (0.1042)	0.1256 (0.0636)	0.1564 (0.0632)	0.0756 (0.0600)	0.1076 (0.0571)
Year: -6		0.1384 (0.0677)	0.1451 (0.0727)	0.1237 (0.0738)	0.1333 (0.0756)	0.1928 (0.0524)	0.1910 (0.0582)	0.1415 (0.0506)	0.1530 (0.0485)
Year: -5		0.1386 (0.0628)	0.1505 (0.0658)	0.1180 (0.0583)	0.1296 (0.0607)	0.2093 (0.0528)	0.2197 (0.0601)	0.1448 (0.0582)	0.1586 (0.0627)
Year: -4		0.0502 (0.0663)	0.0586 (0.0645)	0.0377 (0.0575)	0.0486 (0.0573)	0.0945 (0.0363)	0.0995 (0.0453)	0.0427 (0.0420)	0.0572 (0.0448)
Year: -3		0.1357 (0.0595)	0.1507 (0.0612)	0.1276 (0.0621)	0.1381 (0.0642)	0.1958 (0.0551)	0.2101 (0.0638)	0.1561 (0.0568)	0.1723 (0.0642)
Year: -2		0.1002 (0.0543)	0.1012 (0.0543)	0.0925 (0.0526)	0.0925 (0.0528)	0.1120 (0.0466)	0.1061 (0.0437)	0.0731 (0.0338)	0.0732 (0.0330)
Year: 0		0.1148 (0.0493)	0.1156 (0.0478)	0.1099 (0.0566)	0.1084 (0.0553)	0.1172 (0.0347)	0.1177 (0.0338)	0.0883 (0.0351)	0.0887 (0.0343)
Year: 1		0.0358 (0.0520)	0.0451 (0.0505)	0.0562 (0.0525)	0.0587 (0.0526)	0.0993 (0.0345)	0.1063 (0.0320)	0.0865 (0.0336)	0.0919 (0.0314)
Year: 2		0.1004 (0.0704)	0.1070 (0.0707)	0.1118 (0.0739)	0.1136 (0.0724)	0.1137 (0.0559)	0.1173 (0.0532)	0.1003 (0.0527)	0.1049 (0.0505)
Year: 3		0.1415 (0.0743)	0.1419 (0.0711)	0.1564 (0.0714)	0.1576 (0.0672)	0.1892 (0.0448)	0.1932 (0.0435)	0.1705 (0.0463)	0.1736 (0.0452)
Year: 4		0.0416 (0.0617)	0.0352 (0.0570)	0.0556 (0.0625)	0.0514 (0.0576)	0.0959 (0.0517)	0.0910 (0.0496)	0.0970 (0.0537)	0.0917 (0.0494)
Year: 5		0.0601 (0.0810)	0.0682 (0.0762)	0.0796 (0.0795)	0.0849 (0.0736)	0.1361 (0.0703)	0.1400 (0.0665)	0.1297 (0.0714)	0.1323 (0.0667)
Year: 6		0.0881 (0.0990)	0.0940 (0.0916)	0.1168 (0.0953)	0.1247 (0.0872)	0.1401 (0.0734)	0.1470 (0.0691)	0.1642 (0.0760)	0.1719 (0.0704)
Year: 7		0.0539 (0.0917)	0.0585 (0.0853)	0.0733 (0.0871)	0.0805 (0.0796)	0.1002 (0.0654)	0.1070 (0.0635)	0.1174 (0.0670)	0.1264 (0.0650)
Year: 8		0.0843 (0.0759)	0.0814 (0.0694)	0.1155 (0.0820)	0.1219 (0.0738)	0.1164 (0.0538)	0.1186 (0.0522)	0.1325 (0.0590)	0.1377 (0.0550)
Year: 9		0.0077 (0.0951)	0.0125 (0.0958)	0.0439 (0.0922)	0.0557 (0.0891)	0.0089 (0.0820)	0.0111 (0.0800)	0.0446 (0.0819)	0.0541 (0.0798)
Year: 10		0.0869 (0.0849)	0.0965 (0.0856)	0.1479 (0.0934)	0.1558 (0.0926)	0.1470 (0.0855)	0.1527 (0.0871)	0.1607 (0.0870)	0.1693 (0.0893)
Year: 11		0.1181 (0.1087)	0.1237 (0.1083)	0.1979 (0.1204)	0.2038 (0.1190)	0.1705 (0.1156)	0.1709 (0.1144)	0.2264 (0.1124)	0.2348 (0.1151)
Year: 12		0.0696 (0.0997)	0.0775 (0.0985)	0.1494 (0.1021)	0.1550 (0.0994)	0.1645 (0.1140)	0.1666 (0.1127)	0.2023 (0.1080)	0.2118 (0.1105)
Year: 13		0.0221 (0.1028)	0.0359 (0.1034)	0.0868 (0.1025)	0.0936 (0.1019)	0.0921 (0.0936)	0.0936 (0.0957)	0.1212 (0.0972)	0.1294 (0.0973)
Year: 14		0.0811 (0.1220)	0.0932 (0.1225)	0.1707 (0.1196)	0.1774 (0.1185)	0.1842 (0.1030)	0.1838 (0.1009)	0.2662 (0.1152)	0.2677 (0.1119)
Year: 15		-0.0150 (0.1875)	0.0300 (0.1843)	0.1168 (0.1850)	0.1265 (0.1846)	0.1842 (0.1030)	0.1838 (0.1009)	0.2662 (0.1152)	0.2677 (0.1119)
Year: 16		0.1896 (0.3646)	0.2367 (0.3373)	0.2233 (0.3877)	0.2082 (0.3541)	0.2883 (0.1590)	0.3120 (0.1659)	0.3607 (0.1617)	0.3554 (0.1620)
Year: 17		-0.2018 (0.3714)	-0.1531 (0.3603)	-0.1270 (0.3754)	-0.1350 (0.3602)				
Year: 18		0.0050 (0.3436)	0.0823 (0.2923)	0.0498 (0.3671)	0.0303 (0.3250)				
Year: 19		-0.3386 (0.4992)	-0.2694 (0.4762)	-0.3407 (0.4839)	-0.3085 (0.4641)				
<i>Fixed-effects</i>									
Year		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Varying Slopes</i>									
Adults in Household with Gun (Year)		Yes	Yes			Yes	Yes		
Violent Crime per Capita (Year)		Yes	Yes			Yes	Yes		
Lagged Firearm Homicides (Year)		Yes	Yes	Yes		Yes	Yes	Yes	
<i>Fit statistics</i>									
Observations		940	940	940	940	939	939	939	939
R ²		0.95159	0.94957	0.94668	0.94509	0.97506	0.97308	0.97129	0.97004
Within R ²		0.05000	0.05105	0.05122	0.05192	0.51089	0.49385	0.48953	0.48316
<i>Clustered (State) standard-errors in parentheses</i>									

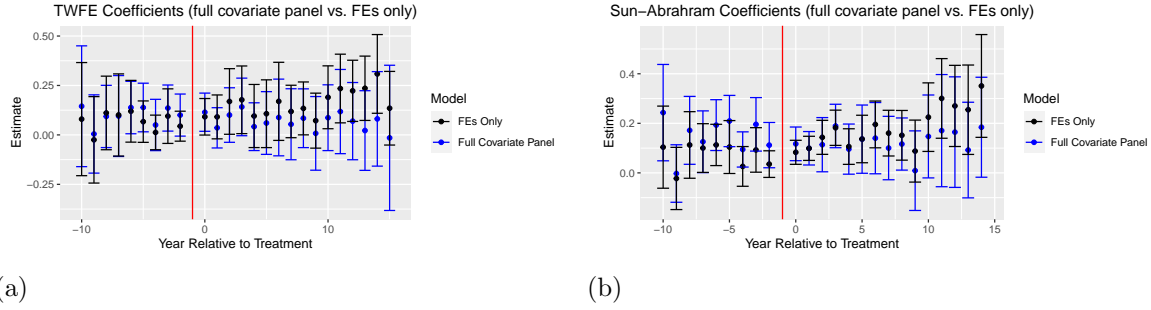
Note: This table presents estimated treatment effects for each year relative to the year before treatment first occurred, where treatment is defined as passing an expansive concealed carry law. Columns (1) and (5) displays the estimates from models with the full panel of covariates, including lagged firearm homicide rates. Columns (2) and (6) reports the estimates from models with the full panel of covariates, but excluding lagged firearm homicide rates. The estimates shown in Column (3) and (7) are from models using the partial panel of covariates and including lagged firearm homicide rates and those in column (4) and (8) are from models including partial panel of covariates but excluding lagged firearm homicide rates. Additionally, Columns (1)-(4) use a standard dynamic TWFE model and Columns (5)-(8) use the Sun-Abraham method. Standard errors are robust and clustered at the state level.

Table 9: Effect of Enacting an ERPO Law on Age-Adjusted Firearm Homicide Rate in a Given Year

Dependent Variable:		Log of Age-Adjusted Rate							
		Dynamic TWFE				Sun-Abraham			
Model:		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Variables</i>									
Year: -19		0.0379 (0.1899)	0.0722 (0.2178)	0.0827 (0.1900)	0.1061 (0.1824)	-0.1021 (0.2022)	-0.0839 (0.2178)	0.0134 (0.2252)	0.0246 (0.2174)
Year: -18		0.0319 (0.1645)	0.0237 (0.1809)	0.1326 (0.1495)	0.1263 (0.1432)	-0.0411 (0.1735)	-0.0679 (0.1823)	0.0628 (0.1613)	0.0428 (0.1601)
Year: -17		-0.0089 (0.1733)	-0.0165 (0.1895)	0.0172 (0.1682)	0.0235 (0.1619)	-0.1098 (0.1901)	-0.1551 (0.1941)	-0.0748 (0.1668)	-0.0909 (0.1682)
Year: -16		0.0206 (0.1358)	-0.0143 (0.1556)	0.1066 (0.1353)	0.0856 (0.1401)	0.0201 (0.1460)	-0.0366 (0.1494)	0.0557 (0.1359)	0.0253 (0.1386)
Year: -15		-0.0845 (0.1603)	-0.1167 (0.1741)	-0.0524 (0.1426)	-0.0616 (0.1484)	-0.1278 (0.1688)	-0.1823 (0.1754)	-0.1192 (0.1422)	-0.1430 (0.1489)
Year: -14		-0.0798 (0.1191)	-0.1048 (0.1277)	-0.0216 (0.1124)	-0.0332 (0.1124)	-0.0725 (0.1271)	-0.1053 (0.1302)	-0.0569 (0.1123)	-0.0697 (0.1139)
Year: -13		-0.0360 (0.1366)	-0.0475 (0.1440)	0.0054 (0.1236)	0.0012 (0.1248)	-0.0569 (0.1391)	-0.0703 (0.1463)	-0.0421 (0.1189)	-0.0518 (0.1184)
Year: -12		-0.1479 (0.0953)	-0.1507 (0.1058)	-0.0227 (0.1051)	-0.0245 (0.1035)	-0.1470 (0.1143)	-0.1510 (0.1229)	-0.0525 (0.1148)	-0.0520 (0.1138)
Year: -11		-0.0273 (0.1009)	-0.0333 (0.1048)	0.0311 (0.0955)	0.0275 (0.0954)	-0.0039 (0.1132)	-0.0183 (0.1136)	0.0163 (0.0962)	0.0083 (0.0926)
Year: -10		-0.1056 (0.0984)	-0.1147 (0.0990)	-0.0122 (0.0948)	-0.0166 (0.0939)	-0.0891 (0.1045)	-0.1103 (0.1083)	-0.0239 (0.0923)	-0.0299 (0.0934)
Year: -9		-0.0181 (0.1088)	-0.0133 (0.1111)	0.0557 (0.1168)	0.0577 (0.1190)	-0.0444 (0.1141)	-0.0476 (0.1184)	0.0057 (0.1064)	0.0077 (0.1095)
Year: -8		-0.0578 (0.1015)	-0.0544 (0.1027)	0.0412 (0.1070)	0.0388 (0.1077)	-0.1126 (0.1014)	-0.1148 (0.1069)	-0.0103 (0.0982)	-0.0126 (0.1000)
Year: -7		-0.0350 (0.1025)	-0.0291 (0.1039)	0.0232 (0.1016)	0.0289 (0.1037)	-0.0958 (0.1128)	-0.1037 (0.1134)	-0.0374 (0.0961)	-0.0356 (0.0988)
Year: -6		-0.0122 (0.1030)	-0.0181 (0.1032)	0.0181 (0.0992)	0.0152 (0.1050)	-0.0948 (0.1057)	-0.1078 (0.1057)	-0.0483 (0.0878)	-0.0526 (0.0925)
Year: -5		-0.0288 (0.0863)	-0.0272 (0.0893)	0.0028 (0.0770)	0.0140 (0.0807)	-0.0752 (0.0983)	-0.0779 (0.1008)	-0.0371 (0.0773)	-0.0263 (0.0822)
Year: -4		0.0924 (0.0713)	0.1099 (0.0761)	0.1122 (0.0784)	0.1227 (0.0833)	-0.0336 (0.0713)	0.0481 (0.0775)	0.0780 (0.0716)	0.0856 (0.0795)
Year: -3		0.0110 (0.0478)	0.0226 (0.0488)	0.0261 (0.0491)	0.0363 (0.0492)	-0.0396 (0.0539)	-0.0342 (0.0534)	0.0063 (0.0465)	0.0104 (0.0458)
Year: -2		0.0806 (0.0567)	0.0926 (0.0578)	0.0802 (0.0542)	0.0904 (0.0545)	0.0533 (0.0573)	0.0546 (0.0575)	0.0628 (0.0478)	0.0661 (0.0467)
Year: 0		-0.0159 (0.0612)	-0.0134 (0.0613)	-0.0309 (0.0566)	-0.0358 (0.0565)	-0.0270 (0.0480)	-0.0247 (0.0500)	-0.0493 (0.0425)	-0.0550 (0.0440)
Year: 1		-0.0580 (0.0820)	-0.0575 (0.0828)	-0.0262 (0.0775)	-0.0363 (0.0744)	-0.1003 (0.0778)	-0.0180 (0.0851)	0.0045 (0.0606)	-0.0050 (0.0602)
Year: 2		0.0216 (0.0944)	0.0249 (0.0995)	-0.0062 (0.0919)	-0.0138 (0.0907)	0.0911 (0.1140)	0.0748 (0.0985)	-0.0121 (0.0883)	-0.0189 (0.0841)
Year: 3		-0.0366 (0.1048)	-0.0427 (0.1051)	-0.0656 (0.1013)	-0.0798 (0.0965)	0.0016 (0.0881)	-0.0064 (0.0851)	-0.0735 (0.0780)	-0.0835 (0.0672)
Year: 4		-0.0172 (0.1425)	-0.0366 (0.1365)	-0.0788 (0.1426)	-0.0997 (0.1352)	0.0708 (0.1539)	0.0437 (0.1508)	-0.0439 (0.1230)	-0.0631 (0.1145)
Year: 5		0.0226 (0.0876)	-0.0366 (0.0852)	0.0131 (0.0821)	-0.0423 (0.0724)	0.0148 (0.0975)	-0.0488 (0.0874)	0.0270 (0.0941)	-0.0219 (0.0808)
Year: 6		-0.1090 (0.0988)	-0.1460 (0.0925)	-0.1439 (0.1043)	-0.1846 (0.0920)	-0.0835 (0.1045)	-0.1178 (0.0840)	-0.0694 (0.0981)	-0.0982 (0.0769)
Year: 7		0.0021 (0.1582)	-0.0289 (0.1514)	-0.0007 (0.1517)	-0.0436 (0.1407)	0.1042 (0.1156)	0.0672 (0.0914)	0.1149 (0.1096)	0.0812 (0.0873)
Year: 8		0.1849 (0.1444)	0.1575 (0.1453)	0.1593 (0.1434)	0.1245 (0.1370)	0.3206 (0.1099)	0.2920 (0.0898)	0.3282 (0.1010)	0.3022 (0.0809)
Year: 9		0.1909 (0.1248)	0.1209 (0.1225)	0.1702 (0.1151)	0.0939 (0.1019)	0.2131 (0.1121)	0.1376 (0.0968)	0.2071 (0.1042)	0.1307 (0.0852)
Year: 10		0.1038 (0.1101)	0.0497 (0.1035)	0.0838 (0.1078)	0.0324 (0.0932)	0.0623 (0.1210)	-0.0050 (0.1108)	0.0635 (0.1233)	0.0229 (0.1131)
Year: 11		0.2419 (0.1249)	0.2072 (0.1191)	0.2155 (0.1178)	0.1757 (0.1103)	0.2042 (0.1371)	0.2098 (0.1279)	0.1871 (0.1129)	0.1470 (0.1129)
Year: 12		-0.0337 (0.2081)	-0.0647 (0.1998)	-0.0647 (0.2001)	-0.0866 (0.1963)	0.1820 (0.1530)	0.1347 (0.1432)	0.1548 (0.1516)	0.1345 (0.1338)
Year: 13		0.0978 (0.2224)	0.0521 (0.2309)	0.0476 (0.2210)	0.0098 (0.2323)	0.3558 (0.1325)	0.3053 (0.1277)	0.3330 (0.1362)	0.3030 (0.1259)
Year: 14		0.0921 (0.1578)	0.0353 (0.1542)	0.0200 (0.1696)	-0.0098 (0.1671)	0.1749 (0.1356)	0.1154 (0.1233)	0.1598 (0.1421)	0.1429 (0.1301)
Year: 15		-0.1134 (0.2530)	-0.1457 (0.2454)	-0.1809 (0.2557)	-0.2079 (0.2496)	0.1629 (0.1269)	0.1326 (0.1110)	0.1241 (0.1244)	0.1098 (0.1103)
Year: 16		-0.1008 (0.2028)	-0.1394 (0.2006)	-0.2535 (0.2061)	-0.2741 (0.1919)				
Year: 17		-0.2685 (0.1856)	-0.3035 (0.1825)	-0.4168 (0.1854)	-0.4411 (0.1751)				
Year: 18		-0.0733 (0.2221)	-0.1289 (0.2183)	-0.2276 (0.2188)	-0.2568 (0.2090)				
Year: 19		-0.0955 (0.2549)	-0.1356 (0.2499)	-0.2462 (0.2244)	-0.2809 (0.2191)				
<i>Fixed-effects</i>									
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Varying Slopes</i>									
Adults in Household with Gun (Year)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Violent Crime per Capita (Year)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Firearm Homicides (Year)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>									
Observations	940	940	940	940	940	939	939	939	939
R ²	0.95092	0.94898	0.94582	0.94416	0.95539	0.95384	0.95096	0.94982	
Within R ²	0.03678	0.03988	0.03594	0.03580	0.12367	0.13096	0.12673	0.13306	
<i>Clustered (State) standard-errors in parentheses</i>									

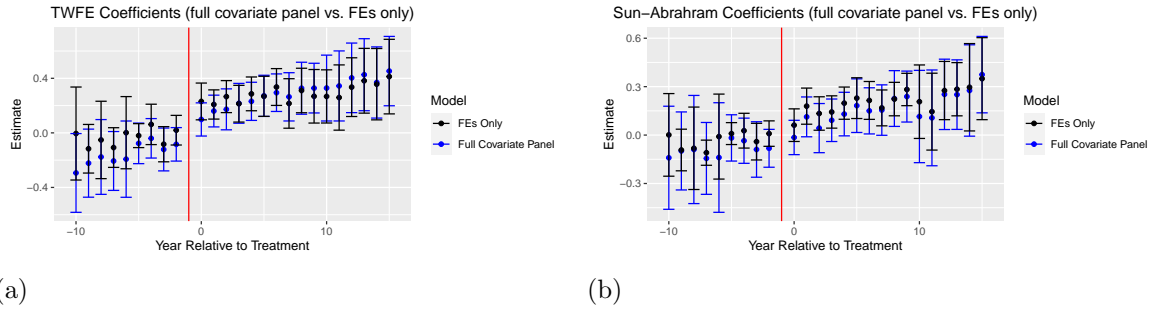
Note: This table presents estimated treatment effects for each year relative to the year before treatment first occurred, where treatment is defined as passing an additional mental health-related law. Columns (1) and (5) displays the estimates from models with the full panel of covariates, including lagged firearm homicide rates. Columns (2) and (6) reports the estimates from models with the full panel of covariates, but excluding lagged firearm homicide rates. The estimates shown in Columns (3) and (7) are from models using the partial panel of covariates and including lagged firearm homicide rates and those in column (4) and (8) are from models including partial panel of covariates but excluding lagged firearm homicide rates. Additionally, Columns (1)-(4) use a standard dynamic TWFE model and Columns (5)-(8) use the Sun-Abraham method. Standard errors are robust and clustered at the state level.

Figure 12: Effect of SYG Laws on Firearm Homicide Rate II



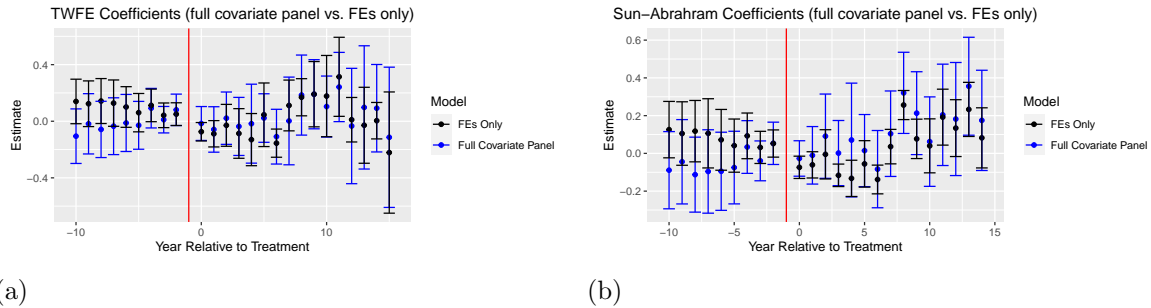
Note: These plots present the difference-in-differences estimates from two versions of the regression where treatment is defined as passing an SYG law. The first version includes the full panel of covariates, and the second includes only state and year FEs. Panel A displays the estimates from the dynamic TWFE model and Panel B the estimates from the model using the Sun-Abraham estimation method. Each point represents the difference-in-differences estimate for that year as compared to the pre-treatment year.

Figure 13: Effect of Expansive Concealed Carry Laws on Firearm Homicide Rate II



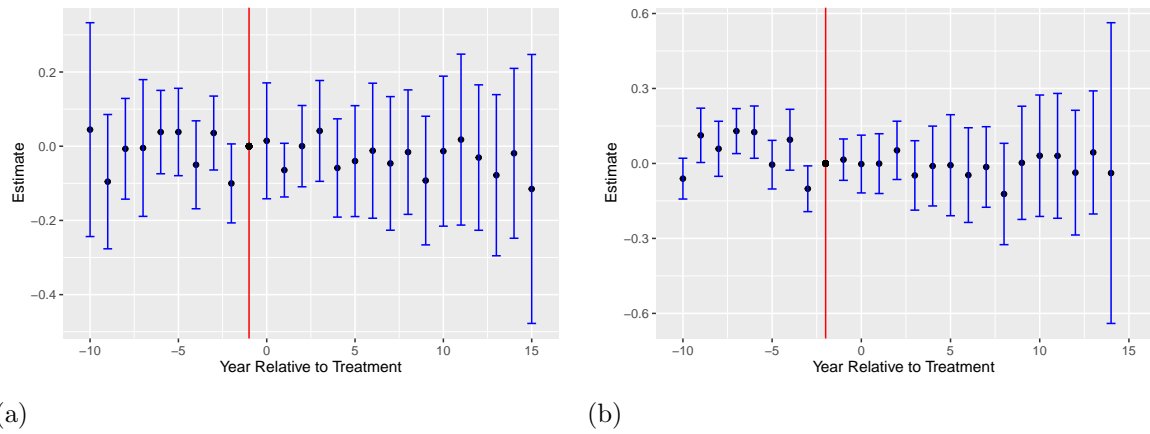
Note: These plots present the difference-in-differences estimates from two versions of the regression where treatment is enacted as a “shall issue” or permitless concealed carry law. The first version includes the full panel of covariates, and the second includes only state and year FEs. Panel A displays the estimates from the dynamic TWFE model and Panel B the estimates from the model using the Sun-Abraham estimation method. Each point represents the difference-in-differences estimate for that year as compared to the pre-treatment year.

Figure 14: Effect of ERPO Laws on Firearm Homicide Rate II



Note: These plots present the difference-in-differences estimates from two versions of the regression where treatment is defined as passing an ERPO law. The first version includes the full panel of covariates, and the second includes only state and year FEs. Panel A displays the estimates from the dynamic TWFE model and Panel B the estimates from the model using the Sun-Abraham estimation method. Each point represents the difference-in-differences estimate for that year as compared to the pre-treatment year.

Figure 15: Estimates for Effect of Enacting an SYG Law with Adjusted Reference Year



Note: These plots present the difference-in-differences estimates from the dynamic TWFE regression (Panel A) and the Sun-Abraham model (Panel B) where treatment is defined as enacting an SYG law. Each point represents the difference-in-differences estimate for that year as compared to the two years prior to treatment (i.e., relative year -2).

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