Investigating the Relationship Between Gun Control Laws and the Gun Death Rate Using Machine Learning Methods: Laws Relating to Background Checks, Criminal History, and Domestic Violence Matter

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Abstract:

This paper analyzes the relationship between the gun death rate and a set of 102 gun control laws in the United States implemented at the state level between 1991-2018. The literature studying the effects of gun control laws on firearm mortality is broad, but the associations found are largely between the gun death rate and a single law (or a small set of related laws). Conversely, my paper makes no assumptions about which types of gun control laws may be predictors of firearm mortality; I instead compile a high-dimensional dataset and let the data decide which laws (if any) matter. I first estimate a fixed effects model – controlling for state-specific time trends in addition to the typical state- and time-fixed effects – and find that ten law covariates are significant at the 5% level, namely laws related to criminal history or domestic violence. These effects have a causal interpretation assuming that all potential confounders are time-invariant or captured in either a national trend or a state-specific trend.

Dimension reduction, or "regularization," is necessary, however, to avoid overfitting and to create meaningful out-of-sample forecasting properties, allowing any apparent associations to be generalized outside this framework. I use a variant of the Least Absolute Shrinkage and Selection Operator (Lasso) estimator, a method known as cross-fit partialing out (or double machine learning), to first penalize overfitting by selecting only a subset of potential covariates, and then to estimate the relationship between the gun death rate and a given law indicator in the approximately sparse setting. To be clear, instead of estimating a single model, I estimate 102 models where the regressor of interest is one of the law indicators, and the other covariates are chosen by Lasso from the following: the other law indicators, the state-fixed effects, the year-fixed effects, and the state-specific time trends. This optimizes predictive performance and inference outside of the studied sample, as the set of chosen controls changes depending on the regressor of interest. I show that ten law covariates are significant at the 5% level and are either related to background checks, criminal history, or domestic violence. Continuing with the theme of regularization, I lastly design an implementation of the Random Forests machine learning algorithm to measure which law "features" (covariates) best predict the gun death rate. Again, I find that the most important categories of gun control laws are background checks, criminal history, and domestic violence.

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1. Introduction

Gun violence claimed 39,426 lives in the United States in 2019.[1] For context, 38,800 Americans lost their lives to automobile accidents in 2019, and another 34,200 were killed by influenza during the 2018-19 flu season.[2] Further, over the past several decades, over 60% of all firearm-related deaths in the U.S.[3] have been suicides – in 2019, 24,090 Americans committed suicide with a firearm. Additionally, roughly half of all suicides[4] committed in the US are committed with a firearm.[5]

These figures are even more disturbing when compared with other countries. In 2010, the U.S. had roughly 102 gun deaths per 1,000,000 residents, compared to 23 in Canada, 11 in Germany, and just two in the United Kingdom.[6] (See **Figure 1.0.1** in the ***Appendix***). Perhaps more surprising is the rate of violent gun deaths (excluding suicides) in the US compared to countries in the Middle East, where the U.S. rate of 4.43 deaths per 100,000 residents is higher than Afghanistan's rate of 3.96, which tops the region.[7] (See **Figure 1.0.2).** A clear reason for the difference in gun violence derives from the Second Amendment to the U.S. Constitution[8] and the country's robust history with firearms. Thus, the historically limited role in countering gun violence – *Heller v. District of Columbia*, 554 U.S. 570 (2008), and *McDonald v. City of Chicago*, 561 U.S. 742 (2010), recently articulate the Supreme Court's interpretation of the Second

[1] Gun Violence Archive
[2] https://www.cdc.gov/flu/about/burden/2018-2019.html
[3] Centers for Disease Control and Prevention (CDC), National Vital Statistics Report (2017)
[4] Intentional self-harm (suicide) was the 9th leading cause of death in the US in 2017 according to the CDC.
[5] https://everytownresearch.org/firearm-suicide/
[6] https://www.cnn.com/2017/10/03/americas/us-gun-statistics/index.html.
[7] https://www.npr.org/sections/goatsandsoda/2019/08/05/743579605/how-the-u-s-compares-to-other-countries-in-deaths-from-gun-violence
[8] "A well-regulated Militia, being necessary to the security of a free State, the right of the people to keep and bear Arms, shall not be infringed."

Amendment[9] – has concentrated on preventing at-risk or dangerous individuals from having firearms (Swanson et al., 2017).

In 1999, after a nearly decade-long decline, the national gun death rate (GDR)[10] plateaued at about 10.3 – it fluctuated between 10.0 and 10.4 through 2014. Yet, in 2015, the GDR rose to 11.1, to 11.8 the next year, and then to 12.0 the year after that. (See **Figure 1.0.3).** What can explain the fall in gun violence during the late 1990s, and what has caused the recent rise? The passage of the Brady Handgun Violence Prevention Act of 1994 (commonly known as the Brady Law)[11] mandated a five-day waiting period on the purchase of handguns, established a dealer-conducted national criminal background check, and expanded concealed carry laws. The waiting-period provision expired in 1998 after the launch of the National Instant Criminal Background Check System,[12] although nine states plus Washington, D.C. still mandate a waiting period on firearm purchases.[13] Still, the passage of the act is correlated with a decline in the GDR. In 1991, states, on average, enforced 24.65% of a set of 102 gun control laws,[14] where, in 1994, 37.52% of these laws were observed. A similar association is not found when investigating the GDR increase beginning five years ago (see **Figure 1.0.4**). The percentage of gun control laws[15] followed (in this

[9] Footnote 97 of (Swanson et al., 2017).
[10] For the rest of the paper, the gun death rate will be for 100,000 residents.
[11] "On November 30, 1993, the Brady Handgun Violence Prevention Act was enacted, amending the Gun Control Act of 1968. The Brady Law imposed as an interim measure a waiting period of 5 days before a licensed importer, manufacturer, or dealer may sell, deliver, or transfer a handgun to an unlicensed individual. The waiting period applies only in states without an acceptable alternate system of conducting background checks on handgun purchasers. The interim provisions of the Brady Law became effective on February 28, 1994 and ceased to apply on November 30, 1998. While the interim provisions of the Brady Law apply only to handguns, the permanent provisions of the Brady Law apply to all firearms" (Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF) https://www.atf.gov/rules-and-regulations/brady-law
[12] Edwards et al. (2015)
[13] https://lawcenter.giffords.org/gun-laws/policy-areas/gun-sales/waiting-periods/
[14] These are the set of gun laws that are tracked by Everytown Research from 1991-2019 for each state. I will discuss these laws more in subsequent sections.
[15] Throughout this paper, gun control laws will often be referred to as "gun laws" or simply "laws." The names are synonymous.

paper) per year generally increases over time, but there is not a clear visual indication that laws are being widely repealed, although total laws enforced peaked in 2016.

The introduction has so far discussed national statistics, but the focus of the paper will be on state gun control laws. Specifically, many of the laws relevant to the paper are federal laws, but state and local officials typically cannot enforce the prohibitions without analogous state laws.[16] Gun policy in the U.S. is generally decentralized – there were over 300 unique state laws in the years following the Brady Act (Ludwig and Cook, 2003). Consequently, there is significant state-to-state variation in total gun control laws enforced. For example, California followed 58.3% of available gun control laws in 1994, while Montana followed just 9.7% of the laws. This is a reflection of the heterogeneity in preferences for varying degrees of gun regulations across the country. The differences in both gun policy and gun violence across states presents an opportunity to track laws state-by-state, but it complicates analysis at the same time. Thus, the goal of this paper is to study which gun control laws are meaningfully related to changes in the GDR and to provide potential evidence for appropriate changes to gun policy.

To give a sense for the type of laws discussed in the paper, each law falls under one of the following categories: background checks, criminal history, domestic violence, drugs and alcohol, mental illness, minimum age, permitting process, and miscellaneous. Specifically, an example of a criminal-history related law would be: "*Does state law disqualify people from getting concealed carry permits based on other criminal history?"* Often, laws will be slight variants of one another – a different law asks the same question except replacing "*carrying concealed weapons"* with "*getting concealed carry permits."* An example of a domestic-violence related law would be: "*Does state law require all people under final domestic violence restraining orders to turn in their*

[16] Everytown Research

firearms when they become prohibited from having them?" To be clear, the law variables are binary – a state either enforces a given law in a given year, or it does not.

Specifically, I consider 1,428 observations, each pertaining to a distinct state and year[17] along with 102 law indicator variables. A complication is that many of the pertinent laws are passed in packages, with respect to both time and entity. Additionally, a given state might pass a package of laws in the same year following a mass shooting or other high-profile, gun-related tragedy (Luca et al., 2019). The implication is that the regressors are all correlated in some way or another, and in varying degrees.

For example, in 1994, Connecticut passed 22 laws tracked by Everytown Research (ETR), a not-for-profit support fund for gun research and safety (see **Figures 1.0.5a and 1.0.5b**).[18] Even if there was a significant shift in the GDR in Connecticut circa 1994, it is difficult to extract the causal effect of a single law change given that 21 other laws were changed in the same year. California passed eight domestic-violence related laws in 2000, so exploiting across-state variation is the key to understanding the relationship between the GDR and gun control laws when the regressors are highly correlated within entity. (See **Figures 1.0.6a and 1.0.6b** for the difference in California's followed laws between 1999 and 2000, specifically noting the domestic violence subset). A significant portion of this paper is dedicated to handling these confounding effects.

I begin with an Ordinary Least Squares (OLS) specification and a series of six Fixed Effects (FE) models. In each specification, the outcome of interest is the GDR and the regressors of interest are the set of law indicators. The most-sophisticated FE model includes state- and year-fixed effects, a state-specific linear time trend, and a state-specific non-linear time trend. The time trends are created by interacting the state-fixed effects with a linear, and then non-linear, time trend. The

[17] For example, Arizona 1997 or Texas 2006
[18] In these figures, ETR displays only 67 of the 102 total laws relevant to this paper.

fixed-effects control for some confounding effects, namely fixed differences between states and national trends over time. This entity and time demeaning are done by differencing two equations, where each has a fixed error term at the state level along with a time-varying error term. The inclusion of the trend terms allows omitted variables within states to vary at different rates relative to other states. Under specifications with at least one time-trend interaction, 16 of the 102 law indicators are significantly related to gun deaths at the 5% level, and 15 of the 16 relate to background checks, criminal history, or domestic violence. In other words, laws related to drugs and alcohol, mental illness, minimum age requirements, and the permitting process do not seem to have a meaningful relationship with firearm mortality.

Finally, I employ two "big data" solutions: Least Absolute Shrinkage and Selection Operator (Lasso) with cross-fit partialing-out (XPO) and Random Forests (RF), the first designed for inference and the second for prediction. Both are tailored for analysis with high-dimensional data – settings where the number of covariates is "large" relative to the number of observations – so "regularization" is employed to reduce the number of variables in the model. In the XPO Lasso implementation, I estimate 102 models, where the covariate of interest is a different law indicator for each model. The Lasso estimator selects from the set of possible controls (the fixed effects and the other 101 law indicators), and then the XPO algorithm gives an unbiased estimate of the coefficient of interest. RF lend additional insight into which covariates are important for predicting changes in the outcome. Second, RF estimates an alternative, out-of-sample measure of GDR.

The results from XPO Lasso and RF are similar to the FE results, in that laws relating to background checks, criminal history, and domestic violence are the best predictors of changes in the GDR. Under the XPO Lasso specification, 26 of the 102 law indicators are significantly related to gun deaths at the 5% level, and 12 of the 26 are related to domestic violence. The RF predictions corroborate these results; of the 27 laws that best predict the GDR, all relate to either background checks, criminal history, or domestic violence. Importantly, law 19 and law 24 produce the most consistently significant coefficients across models – the coefficients from the XPO Lasso model, -1.620 ($p<0.001$) and -2.408 ($p<0.01$), respectively, may be taken as causal under the assumption that the included fixed-effects and time trends adequately control for confounding variables. Overall, the passage of laws pertaining to background checks, criminal history, and domestic violence widely corresponds to declines in the GDR.

Finally, I will note that the GDR is not the only outcome variable of interest in this paper. I will also regress the suicide rate, by state, on the gun-law indicator variables. As mentioned above, over 60% of gun deaths in the U.S. are from suicides (which have risen in the U.S. since 2001),[19] so finding evidence of laws associated with changes in the suicide rate is of importance to policy makers. Note that the overall suicide rate, not suicides by firearms, is the outcome of interest so to control for substitution effects. A model considering only suicides by firearms could not control for individuals who commit suicide with alternative means when they do not have access to guns.

The FE, Lasso, and RF models generally produce the same results with the suicide rate substituted for the GDR as the outcome variable of interest – the majority of significant coefficients (or important predictors) belong to background check, criminal history, or domestic violence laws in each specification.

This paper proceeds as follows. Section 2 presents background information on relevant gun control laws and general trends. Section 3 discusses the relevant literature surrounding the relationship between firearm mortality and gun control laws, and additionally explains this paper's

[19] See **Figure 1.0.4b**. Note, however, that this rate is not the national rate, but instead an average across all states, giving equal weight to all states rather than by population.

contributions to the subject. Section 4 provides and describes data on the GDR and the set of gun control laws. Section 5 explains the employed methodology. Section 6 outlines how the methodology differs with the alternative specifications. Section 7 details the machine learning algorithms, XPO Lasso and Random Forests. Section 8 interprets and contextualizes the findings. Section 9 examines caveats and directions of future research. Section 10 concludes.

2. Gun Law Background

The first piece of national gun control legislation, the National Firearms Act (NFA), was passed in 1934 as a part of the "New Deal for Crime,"[20] which levied a tax on the transportation and selling of long-guns.[21] The next significant legislation, the Gun Control Act of 1968 (GCA), largely a response to the assassinations of President John Kennedy, Attorney General Robert Kennedy, and Dr. Martin Luther King Jr., imposed "stricter licensing and regulation on the firearms industry, established new categories of firearms offenses, and prohibited the sale of firearms and ammunition to felons and certain other prohibited persons."[22] The Firearms Owners' Protection Act of 1986 sought to protect the rights of gun owners by reversing many of the provisions of the GCA. Yet, it amended the GCA to "prohibit the transfer or possession of machineguns. Exceptions were made for transfers of machineguns to, or possession of machineguns by, government agencies, and those lawfully possessed before the effective date of the prohibition, May 19, 1986."[23] The Brady Law of 1994[24] mandated a five-day waiting period for the sale of firearms to unlicensed individuals and authorized dealers to conduct background

[20] https://time.com/5169210/us-gun-control-laws-history-timeline/

[21] "Long-guns" are the category of guns meant to be shot with two hands and include rifles, some shotguns, and machine guns.

[22] Bureau of Alcohol, Tobacco, and Firearms (ATV)

[23] Id.

[24] See 6.

checks. National and state legislation has trended toward greater gun control, with the exception of concealed carry laws, which were expanded under the Brady legislation.

These packages of laws apply federally, and the law may not be properly enforced without a mirroring law in a given state. For example, federal law prohibits convicted felons from possessing firearms, as they would fail a background check if they attempted to purchase from a legal dealer. Yet, according to ETR, 14 states do not "generally"[25] prohibit all people convicted of felonies from having firearms. The federal law applies in every state, but state and local officials cannot enforce it, thus preventing the law from having the desired scope. ETR highlights the importance of state laws and their ability, in addition to mirroring federal law, to close federal loopholes. Specifically, federal law prohibits domestic abusers from having guns, but often only if they are married to the victim. States like New Jersey have closed this loophole by writing a law to apply to abusive boyfriends and girlfriends.

Overall, ETR surveys hundreds of gun control laws in all 50 states plus D.C. from 1991-2019. Thus, the data behind its user-friendly interface can be concentrated into observations consisting of a unique state-year combination, and whether or not a given law was followed in the observation. This paper will analyze the 102 primary gun control laws tracked by ETR,[26] all of which fall into eight categories: background checks, criminal history, domestic violence, drugs and alcohol, mental illness, minimum age, permitting process, and miscellaneous. (See **Table 2.0.0** for a description of each law).

To provide a sense for the changes in gun control laws over time, see **Figures 2.0.1-2.0.8.** Each of these time-series tracks a law in a unique category (typically the most-general law of each category), mapping the difference between states following the law in 1991 and states following

[25] Language used by ETR
[26] Many of these primary gun control laws tracked by ETR have corresponding sub-questions.

the law 2020.[27] Lines between nodes demonstrate states that either passed or repealed the law between 1991 and 2020. Put simply, looking at the differences between "Yes" answers between 1991 and 2020 reveals the overall trend with respect to that class of law. States with answers of "N/A" in **Figures 2.0.2, 2.0.3** classify as having answered "No" because the effect is the same. For example, in **Figure 2.0.3**, which displays a time series of law 24,[28] a state is classified as "N/A" by ETR if it does not prohibit all people under final domestic violence restraining orders from having firearms. Yet, this means that these states will *not* require all people under final domestic violence restraining orders to turn in their firearms when they become prohibited from having them. For time-series of every law tracked by ETR, along with an interactive interface that allows the user to observe trends in every year (not just between 1991-2020) please visit its website.[29] Again, with the exception of concealed-carry laws, the general trend is a pro-gun-control one.

3. Literature Review

3.1 Firearm Mortality

The literature largely focuses on the relationship between GDR (or an equivalent metric) and a specific gun control law (or class of laws), or, more broadly, associations between the GDR and changes in firearm ownership. Lemieux (2014) examines both sides of the gun control debate, empirically evaluating the merits of the argument that lax regulations increase the prevalence of mass shootings and all firearm mortality. In a cross-state analysis, he finds that "restrictive regulations" on guns is the best predictor for total death by firearms ($p<0.001$), implementing the

[27] Note: formal analysis in this paper stops in 2018 (the year of the most recent GDR data released by the CDC), not 2020.

[28] Law_24: "*Does state law require all people under final domestic violence restraining orders to turn in their firearms when they become prohibited from having them?"*

[29] https://everytownresearch.org/navigator/trends.html?dataset=background_checks (last visited April 27, 2020).

Gifford Law Center's Legal Community Against Violence Report (LCAV), which ranks states based on 25 approaches to regulating firearms. These restrictive regulations might require gun dealers to obtain a license and pass a background check or require a permit to openly carry handguns. Sumner, et al. (2008) find that firearm background checks administered by local-level agencies were associated with 22% decrease in the gun-homicide rate and a 27% decrease in the gun-suicide rate.

The general consensus in the literature finds that increases in gun exposure – whether it be through ownership or laws facilitating gun use – are associated with increases in the GDR. Cook and Ludwig (2004) evaluate the relationship between household gun prevalence and gun homicides rates, using the percentage of suicides committed with a gun as a proxy for gun ownership. They estimate that the marginal social cost of gun ownership ranges from $100-$600 annually per household. Siegel et al. (2013) contribute by studying the relationship between firearm homicide rates, utilizing the same proxy as Cook and Ludwig. The authors find that a 1% increase in firearm ownership was associated with a 0.9% increase in the gun homicide rate.

Cheng and Hoekstra (2013) exploit state-to-state variation in self-defense law to estimate rates of violent crime with guns and estimate that the presence of "stand-your-ground" laws are associated with an 8% increase in firearm mortality. McClellan and Tekin (2017) expand on and validate this finding. The authors show that at least 30 individuals were killed each month in the United States as a result of stand-your-ground laws. Donohue et al. (2017) employ state panel data to show that right-to-carry (RTC) laws are associated with a statistically significant 13-15% increase in the overall rates violent crime (lagged ten years).

I build upon this literature in several ways. Primarily, I take a distinct approach from the literature by not disciplining my question from the beginning. These studies establish associations between firearm mortality and specific gun control laws, but generally they do not look at the big picture – there are hundreds of laws at the national and state level, each related in some way or another, either from the region of effect, the time of passage, or in some other manner. Performing such analysis under the scope of a single law or class of laws is far too narrow in breadth, given that we know state laws are often passed in packages. Further, the estimated coefficients of these studies typically do not have causal interpretations because they do not account for the fact that the passage of one law is typically correlated with the passage of another. Therefore, the relationship between GDR and a law or group of laws should be estimated in the context of all other laws. Additionally, this study extends analysis to areas of gun control seldom covered in the literature, like domestic-violence related laws. Overall, this study's minimal-hypothesis approach allows the data to determine which laws are the best predictors of gun violence without any pre-conceived biases of their predicted importance.

3.2 Suicides

As stated above, over 60% of annual firearm deaths in the US are suicides, so a considerable and appropriate portion of the literature is devoted to studying this problem. Suicides are distinct from all other firearm violence, so a separate analysis is logical. For example, the passage or repeal of criminal-related gun control laws could presumably have a stronger association with changes in the homicide rate than suicide rate, and vice versa for changes in mental-illness-related laws.

Much of the suicide-focused gun control literature concerns "red-flag" laws, "laws that seek to prohibit high risk individuals from owning firearms"[30] by empowering those close to the high-risk individual to petition a court for the temporary removal of firearms from the individual.[31]

[30] Rodríguez-Andrés & Hempstead (2011)
[31] ETR's law 93

This is often referred to as an “extreme risk protection order.” Rodríguez-Andrés & Hempstead (2011) showed that laws aimed at reducing gun availability have a more-significant deterrent effect on male suicide than red-flag laws. Swanson et al. (2017), however, estimate that Connecticut’s red-flag law has the effect of preventing one suicide for every ten to twenty gun seizures. The authors do not argue for or against this tradeoff between public health and gun-ownership rights, and instead leave the answer to legislators.

The effects of background checks and purchase delays, much like in the gun-homicide-related literature, is extensively explored with respect to suicides as well. Chapman et al. (2006) find that, among Australian men between 1997-2005, non-gun suicides declined by 24.5% where gun suicides declined by 59%, evidence of a minimal substitution. Edwards et al. (2015) show that purchase delays are associated with a 3% decrease in firearm-related suicides. These authors analyze states that passed mandatory delay laws both before, during, and after the Brady Law’s effective years (1994-1998). On the other hand, Lang (2013) shows that overall suicide rates are not statistically related to background checks and establishes the same insignificance with regard to youth suicides.

This paper builds upon the firearm-related suicides literature in two ways. First, similarly to the above section, I analyze the relationship between suicides and gun control laws in the context of all gun control laws. Second, my outcome variable is the suicide rate by state by year – not just the firearm-related suicide rate – to control for substitution effects. If the passage of a law decreases the firearm-related suicide rate by 10% but fails to affect the actual suicide rate, did it really have an impact?

4. Data

4.1 Gun Law Statistics

This study's data comes from two primary sources: Every Town Research (ETR) and the Centers for Disease Control and Prevention (CDC). Most relevant, ETR tracks 102 major gun control laws across all 50 states and D.C. since 1991. There is one dataset for each law, and I removed all variables besides *state*, *year*, and *response* (renamed *law_#*). The *law_#* variables are binary regressors and have a simple interpretation: *law_#* = 1 if a given state followed a given law in a given year, and *law_#* = 0 otherwise. Therefore, each observation consists of *state*, *year*, the set of 102 law indicators, and outcomes of interest.

ETR included *applies_to* to track whether a certain law (when applicable) applied to two sets of conditions: (i) handguns, long-guns, or both; and (ii) purchase, possession, or both. This complicated the analysis, however, because it caused states with laws that varied by condition (gun type, purchase vs. possession, or both) to have multiple observations for a given year. To have a balanced panel dataset, I desire only one observation per state, per year – 51 states x 28 years = 1,428 unique observations. To fix this, I counted the state as following the law (*law_#* = 1) if the condition applied to either handguns or long-guns, *or* if it applied to either purchase or possession. A caveat of this approach is that my analysis does not consider the degree of restriction, only whether or not *any* restriction was present.[32]

Overall, the cleaning and compiling of ETR data was a significant time commitment and important process. An extra observation was added to the data (i.e. multiple observations for the same state-year combination) for each condition in *applies_to* that the state followed. Connecticut, for example, often had four times the necessary observations because it had conditions on handgun purchase, handgun possession, long-gun purchase, and long-gun possession. This had the effect of offsetting the correct *law_#* value for observations within the same state. To fix this, I went through

[32] See **Section 9** for a more-detailed discussion of this caveat.

each state for each law variable on ETR's online interface to verify that the dataset (with *applies_to* removed) was correct.

Beyond removing additional observations that were conditional on gun type, purchase vs. possession, or both, many of the 102 law datasets contained errors (clear inconsistencies with ETR's online interface). For example, instead of a state having one observation per year, it was common for there to be, for example, over 100 observations of "Iowa 2012" in a row. Manually inputting the correct value of *law_#* for each state-year observation was the only solution to this problem.

Next, I will discuss ETR's *response* variable (which I renamed *law_#*) in detail. The variable took on values of either "Yes", "No", or "N/A". "Yes" was assigned a dummy value equal to 1, "No" was assigned a dummy value equal to 0, and "N/A" was assigned a dummy value equal to 0 in nearly all cases. The exceptions occurred in questions regarded concealed carry, where the questions were framed in a way where "Yes" and "N/A" effectively meant the same thing. For example, law 19 asks: "*Does state law disqualify people from getting concealed carry permits based on other criminal history?*" An answer of "N/A" means that the state does not offer concealed carry permits. Therefore, states that do not offer concealed carry permits effectively "*disqualify people from getting concealed carry permits based on other criminal history,*" because the entire state population is disqualified, including those with "other" criminal history. To be clear, a value equal to 1 means stricter on guns and a value equal to 0 means less strict on guns. Thus, the general rule is that if an answer of "N/A" indicates being effectively stricter on guns, it would be assigned a value equal to 1 (and 0 otherwise). In other words, law indicators equal 1 if the restriction is present, and 0 if the restriction is not present.

To emphasize the importance of correctly classifying law responses, I will discuss the only non-concealed-carry law in the dataset where I make "N/A" = 1. Law 5 asks: "*Are background checks required for all sales by unlicensed sellers doing business at gun shows?*" (also known as the "Gun Show Loophole"). The original question on ETR asks: "*For states that don't require background checks for all sales by unlicensed sellers, are background checks required for unlicensed sellers doing business at gun shows?"* where an answer of "N/A" means that the state requires background checks for all sales by unlicensed sellers. My alternate question groups together all observations that require background checks for all sales by unlicensed sellers doing business at gun shows, regardless of whether the state requires background checks for *all* (as opposed to just gun show) sales by unlicensed sellers. This captures the effects of the gun show loophole on outcomes of interest.

As discussed in previous sections, each variable in the dataset relates to one of the following: background checks, criminal history, domestic violence, drugs and alcohol, mental illness, minimum age, permitting process, or miscellaneous. Often, laws are conditional on the main law within its category. For example, many concealed carry laws are conditional on states issuing permits, causing answers of "N/A" to be counted differently in different contexts (as discussed above). Also, several domestic-violence related laws are conditional on whether the state prohibits firearm possession by all people under final (or temporary) domestic violence restraining orders. Answers of "N/A" are still changed appropriately, but a side-effect is that some laws are highly correlated with one another. The correlation coefficients between law 0[33] and other

[33] *Does state law require criminal background checks for gun sales by unlicensed sellers?*

"important"[34] background check laws – law 4,[35] law 5,[36] and law 8[37] – equal 0.9892, 0.9226, and 0.9862, respectively. Therefore, a caveat is that any significant differences in the coefficients on these background check law indicators will be driven by relatively small subsamples of observations. In other words, each variable could be significantly related to a decrease in the GDR, but in truth, only one of the indicators could be driving that change. I will discuss this in greater detail in **Section 6: Results and Discussion**. Luckily, this problem does not persist throughout the dataset, as there are few instances of high correlations, especially in the other important categories – criminal history and domestic violence.[38]

4.2 Outcomes of Interest: The Gun Death Rate and the Suicide Rate

The outcome variable of interest, Gun Death Rate (*gdr*) was obtained from the CDC. The *gdr* is the death rate by firearm per 100,000 residents in a given state (including D.C.) in a given year (1991-2018). The figure is age-adjusted because death rates are affected by the population composition of a given state – the mean age is not constant across all states, for example. Overall, this data was straightforward.

The alternative outcome variable used in this paper is the suicide rate (*suicide_r*) and was also obtained by the CDC. The *suicide_r* is the rate of suicide – suicides by any method, not just

[34] Laws 4, 5, and 8 are significant at the 5% level or higher in the XPO Lasso specification.
[35] *Do any exceptions apply to the background check requirement?*
[36] *Are background checks required for all sales by unlicensed sellers doing business at gun shows?*
[37] *Are there penalties for a buyer who fails to follow the background check law?*
[38] See **Figures 4.1.1-4.1.2**. Note: Correlation coefficients between law indicators that ask identical concealed-carry questions but for the difference between "*carrying concealed guns in public*" and "*getting concealed carry permits*" are indicated by ^. These questions elicit highly correlated responses across the dataset. Further, note that all other correlations in these figures are relatively low. Further, "row" covariates that are conditional on "column" covariates are indicated by *. Finally, note that some of these laws may ask similar questions (differences between final and temporary restraining orders, for example) while not being conditional on one another or as similar as in the concealed-carry example. These correlation coefficients are not marked.

by firearms – per 100,000 residents in a given state (including D.C.) in a given year (2001-2018). Like with *gdr*, the figure is age-adjusted. Recall that this study measures the overall suicide rate rather than the suicide rate with a firearm to control for substitution effects, an approach consistent with Lang (2013). This data was also straightforward.

To perform analysis, I added a *gdr* variable and a *suicide_r* variable to my cleaned dataset of the variables *state*, *year*, and *law_0, …, law_102.* Each unique state-year pairing was assigned the appropriate *gdr* and *suicide_r* values.

4.3 Summary Statistics

Next, I will briefly discuss general patterns seen in the data (see **Figure 4.3.1** for a table of summary statistics). The outcome variables of interest, *gdr* and *suicide_r,* have mean values of 12.30 and 13.76, respectively – the average number of gun deaths or suicides per 100,000 residents across all state-year observations (New Jersey 2013, for example). The lowest and highest values of *gdr* in a single observation are 2.1 (Hawaii 2005) and 60.7 (D.C. 1996), respectively. The lowest and highest values of *suicide_r* in a single observation are 4.6 (D.C. 2009) and 29.8 (Wyoming 2012), respectively.

The interpretation of the mean value of the *law_#* variables is the percentage of state-year observations that follow the given law. Markedly, 94.0% of observations have a minimum age requirement for purchasing a handgun from a federally licensed dealer (*law_72),* the most common law in the dataset. Two laws that I show to have a significant and negative relationship with decreases in the GDR, *law_19* and *law_24,* have follow rates of 48.0% and 19.6%, respectively – only 22 laws are followed less often than Law 24.

The set of variables "*category"_p* aggregate these means by category. For example, the mean value of *bchecks_p* of 0.220 shows that background check laws were followed in 22.0% of all possible observations, the lowest follow-rate of any category. Minimum age laws were followed in 70.8% of observations, the highest follow-rate of any category. The variable *lawtotal_p* tracks the percentage of laws followed in a given observation out of all 102 laws; the mean value of *lawtotal_p* is the percentage of total laws followed across the dataset (38.1%). Colorado (1993, 1997, and 1998) followed 7.07% of possible laws (the lowest percent in the dataset) and Massachusetts (2005) followed 73.8% of possible laws (the highest percent in the dataset).

5. Methodology

5.1 Fixed Effects Specifications

I exploit my balanced panel dataset by utilizing a fixed effects (FE) framework to connect changes in state guns laws to changes in the GDR within states and over time. I estimate the relationship with the following specification:

$$gdr_{it} = \alpha + \beta * law_{k_{it}} + \gamma_t + \tau_i + \varepsilon_{it} \quad \text{(Model 4, FE)}$$

where gdr_{it} is the gun death rate (per 100,000 residents) of a given state in a given year; $\beta * law\_k_{it}$ is the vector of law indicator variables, where it takes on a value of 1 if a given state-year observation follows law *k* and zero otherwise; γ is the set of year-fixed effects; τ is the set of state-fixed effects; and ε_{it} is the error in the regression (standard errors are clustered at the state level).

I expand the above specification to consider within-state time trends:

$$gdr_{it} = \alpha + \beta * law_{k_{it}} + \gamma_t + \tau_i + (\tau_i * t) + \varepsilon_{it} \quad \text{(Model 5, FE linear trend)}$$

where $\tau_i * t$ are state-specific time trends. Simply, this is an interaction between a linear trend and each state-fixed effect. This is an important addition because there is a difference between national trends (which is controlled for by the year-fixed effects, γ_t) and trends of the same variable but specific to a state. Allowing states to have this pattern of change over time – specifically, allowing them to change every year – takes away any variation in *gdr* that is caused by state-specific trends. For analysis, I impose that they change in a linear fashion. Thus, this interaction allows states to trend at different rates relative to other states.

Next, I discuss further motivation for interacting the state-fixed effects with a linear-time trend. Consider Alabama and California, or any two states that are considered relatively divergent. If you look at both states in 1990, they will have some of the same differing features they have today – California has always had a larger Hispanic population, where Alabama has always had a larger African American population – but they also change in different ways over time. Crime rates have fallen across the country since the 1990s, but at different rates in different states; states adopt progressive (or conservative) legislation at varying degrees; and states differ in unobserved variables that may be associated with *gdr* – Florida has an increasingly aging population, and retirees are less likely to commit violent crime.

As discussed, many law indicators are highly correlated with each other. Yet, central to my analysis is the study of the relationship between a given law indicator and the GDR, in the context of all other law indicators. Hypothetically, it is plausible that both closing the gun-show loophole and passing "red-flag" laws are both meaningfully associated with decreases in the GDR, but passing them together uncovers an additional layer. The ideal specification would include an interaction between every possible pair of law variable. This is impractical, however, as the fixed-effects model is already strained by the large number of regressors.

I build on the above specification in two ways. First, I add a state-specific, non-linear time trend to control for unobserved variables that both vary at different rates across different states and that have a non-linear relationship with the outcome. Several of these non-linear interaction terms are significant at the 1% level, thus validating this approach.

Second, I add four interaction terms. From the previous specification, every coefficient that was significant at the 5% level or higher corresponded to a law relating to either background checks, criminal history, or domestic violence. A question to ask is whether these laws matter totally independently of one another, or are, for example, the effects of domestic-violence related laws strengthened by the presence of stringent background checks? Does prohibiting concealed carry augment the enforcement of robust criminal history laws? For each observation, I measure the percentage of laws the state followed for each category in that year, denoted *category_p*. I then create four interaction terms to add to the model: (1) *bchecks_p * domesticv_p * criminals_p*; (2) *bchecks_p * domesticv_p*; (3) * *domesticv_p * criminals_p*; and (4) *bchecks_p * criminals_p*.

The final fixed-effects specification follows:

$$gdr_{it} = \alpha + \beta * law_{k_{it}} + \gamma_t + \tau_i + (\tau_i * t) + (\tau_i * t^2) + (\pi * W_{it}) + \varepsilon_{it} \qquad \text{(Model 7, FE non-linear trend)}$$

where $(\tau_i * t^2)$ is the state-specific, non-linear time trend and W_{it} is the vector of the four law-category-percentage interaction terms.

It is important to clearly address the decision to exclude control variables from model specifications. This paper argues that state-fixed effects, a national trend, and a state-specific linear trend will adequately control for confounding variables that may be associated with the outcome of interest. Typical controls are excluded for two reasons. First, many of these variables are controlled for in the fixed effects. Second, traditional control selection (choosing variables that are presumably related to outcomes of interest like income, education, poverty, crime rates, or population density) would be inconsistent with this paper's data-driven approach. Such selection risks biasing the estimated relationship between the GDR and the set of gun control laws.

Although I discuss just three specifications in this section, I run seven variants of Ordinary Least Squares (OLS) and FEs models, which are displayed in **Table 6.1.1**. Model (1) is a basic OLS model, a regression of the GDR on the set of law indicators. Model (2) is a FE regression of the GDR on the law indicators that includes only the state-fixed effects. Model (3) adds year-fixed effects. Model (4) expands on the third model by clustering the standard errors at the state level, a necessary assumption given that observations within each state are not i.i.d. (independently and identically distributed). Model (5) adds the state-specific linear time trend to the fourth model. Model (6) adds a non-linear state-specific time trend. Model (7) adds W_{it}, the four interaction terms discussed above.

5.2 Machine Learning Estimation

5.2.1 Selecting an Approach

This paper analyzes high-dimensional data, where the number of variables is large relative to the number of observations – there are 1,429 observations and 102 law variables, plus the fixed-effects terms – where "large" does not require the number of covariates to be greater than or equal to the number of observations. Athey et al. (2019) provide motivation for regularization techniques: "Even with K [the K-component vector of covariates] modest in magnitude, the predictive properties of the least squares estimator may be inferior to those of estimators that use some amount of regularization." This may be true with $K \geq 3$. Thus, the fixed-effects specifications used above overfit the model. Alternatively, Belloni et al. (2014) argue that ML regularization, "guards against false discovery and overfitting, does not erroneously equate in-sample fit to out-of-sample predictive ability, and accurately accounts for using the same data to examine many different hypotheses or models."

Suppose I wanted to estimate the effect of the passage of law *k* on the GDR. Concerns of overfitting prevent the inclusion of every law indicator as a control. One approach would involve selecting controls through the traditional, intuitive approach: selecting laws (or other variables) that could reasonably be associated with the outcome or regressor of interest. Alternatively, to avoid both doubting having selected the correct control variables and imposing subjective biases onto the model, I take a data-driven approach to regularization. This strategy, consistent with modern ML techniques, is to perform dimension reduction via out-of-sample cross-validation, rather than a Bayesian approach which selects parameters *a priori* (Athey at al., 2019).

An underlying assumption is that of *approximate sparsity* of the high-dimensional linear model, where "s variables among all of the $x_{i,j}$, where s is much smaller than n, have associated coefficients β_j that are different from 0, while permitting a nonzero approximation error $r_{p,i}$" (Belloni et al., 2014). For example, the cross-fit partialing-out sparsity requirement is that $s / (N / \ln p)$ is small (Chernozhukov et al., 2018). I will discuss this methodology in more detail in **Section 5.2.2**.

In this section I use ML techniques to offer additional insight into the relationship between the GDR and gun control laws, considering both inference- and prediction-minded approaches. The goal of the paper is to provide meaningful insight to the gun control debate, which means that the estimates of gun-law parameters must have application out-of-sample. First, I estimate a Least Absolute Shrinkage and Selection Operator (Lasso) model that is designed for causal inference. Second, I implement the Random Forests algorithm to determine which law indicators are most important for predicting changes in firearm mortality, using mean decrease in node impurity, and then predict out-of-sample values of the GDR based on the trained in-sample data.

5.2.2 XPO Lasso

Lasso (Tibshirani, 1996) estimates the parameters of sparse and high-dimensional linear models by choosing coefficients to minimize the sum of the squared residuals and penalizing the regression by a factor proportional to the sum of the absolute values of its coefficients. Belloni et al. (2012) define the Lasso estimator as:

$$\beta = \arg \min_{b} \sum_{i=1}^{n} (y_i - b^T x_i)^2 + \lambda(|b|_q)^{1/q} \text{ (Lasso)}$$

where $|b|_q = \sum_{k=1}^{K} |b_k|^q$, $q = 1$, and $\lambda > 0$ is the penalty term. Intuitively, λ selection is a tradeoff between accuracy and simplicity. $\lambda = 0$ would correspond to OLS, where there is no penalization, which is optimal for in-sample accuracy. The degree of regularization is increasing in λ, thus higher values correspond to simpler models, but do not predict the data as well. This paper selects the optimal λ through a plug-in iterative formula, which was designed for inference methods and also developed in Belloni et al. (2012). Plug-in is preferred to a common alternative of λ selection, cross-validation (CV), because CV does not necessarily have good performance when prediction is not the goal (Belloni et al., 2014), as CV produces models with far more selected covariates than plug-in produces,[39] and is thus subject to a lesser degree of regularization.

This paper employs the cross-fit partialing-out (XPO) variant of Lasso for inference, developed by Chernozhukov et al. (2018), to estimate the effects of a given law indicator on the outcome, the GDR. This is also known as Double Machine Learning 2 (DML2). Therefore, unlike the fixed-effects specifications which estimated a single model with all 102 law indicators as regressors of interest, I estimate 102 unique XPO models, each with a different law indicator as the parameter of interest, and a set of possible controls which includes the rest of the law indicators

[39] In my framework, during early-stage analysis, I found that selecting λ via cv produced models which regularly selected greater than 99% of possible covariates, where plug-in produced models which selected less than 50% of possible covariates on average.

and the fixed-effects variables (although I force the inclusion of the state-specific time trend). The regression model follows:

$$E[y_{it}|\, d_{it}, X_{it}] = \alpha + \beta * d_{it} + \theta * X_{it} + \varepsilon_{it} \qquad \text{(XPO/DML2)}$$

where $E[\varepsilon_{it} \,|\, d_{it}, W_{it}] = 0$; the number of non-zero elements in α must not be too large to satisfy the sparsity requirement; y_{it} is the GDR in a given state in a year; d_{it} is the law indicator of interest (one of a possible 102) and the state-specific linear time trend, $\tau_i * t$; and X_{it} is the set of possible controls[40] – every law indicator minus the chosen regressor of interest, along with the state-fixed effects (τ_i) and year-fixed effects (γ_t). Note that I exclude a state-specific, non-linear time trend ($\tau_i * t_2$) seen in the FE specifications because the algorithm failed to converge on the optimal λ during the variable selection phase, for each of the 102 models.[41] Beyond this, I am comfortable with dropping the non-linear trend because the between R-squared value does not meaningfully change in the FE models when the non-linear trend is added, increasing from 0.81 to 0.84. Also, I exclude the interaction terms of "important" law categories, W_{it}, to avoid the assumption that the same laws will be significant in under the XPO specification.

Doing inference on β requires the implementation of the DML2 algorithm (Chernozhukov et al., 2018). The data is randomly split into K parts, where $K = 10$ (the standard in the literature).[42] Lassos of both the covariates of interest and the outcome are separately run to select controls from the set of all controls, X_{it}. Once controls are selected via the plug-in method, alternate measures of d_{it} and y_{it} are constructed with respect to the selected controls and its estimated coefficients, within both the training sample (with 9/10 parts) and the validation sample (the 10th part). This sample splitting unbiases the data because out-of-sample comparisons are made rather than tests of in-

[40] XPO Lasso does not display the coefficients on the controls selected by the model.
[41] The Stata error states the following: "Convergence for the lasso penalty = # not reached after 100000 iterations."
[42] State XPO Lasso manual citing Chernozhukov et al. (2018).

sample goodness-of-fit (Athey et al., 2019). The algorithm is repeated $K = 10$ times for each law covariate of interest in this 10-fold method, where each split of the data is used as the validation sample in one iteration. I discuss the algorithm in more detail in **Section 7.1**.

5.2.3 Random Forests

Where the Lasso techniques allow inference on specific model parameters (the set of gun control law variables), Random Forests (Breiman, 2001), an extension of regression or decision trees (Breiman et al., 1984), are a popular method in regression problems when prediction is the primary goal, where this study has focused on inference so far.

The Random Forests (RF) algorithm broadly involves sequentially splitting the data into subsamples, where the splits consider a single covariate or "feature" (as denoted in the RF literature), *k* after time threshold *c*. Athey et al. (2019) define the RF methodology used in this paper as follows: Consider a split on feature *k* and threshold *c* in a full training sample. The sum of in-sample squared errors before the split:

$$Q = \sum_{i=1}^{N} (Y_i - \overline{Y})^2 , \qquad \text{where } \overline{Y} = \frac{1}{N} \sum_{i=1}^{N} Y_i .$$

After a split based in feature *k* and threshold *c* the sum of in-sample squared errors is:

$$Q(k,c) = \sum_{i:X_{ik} \leq c} (Y_i - \overline{Y}_{k,c,l})^2 + \sum_{i:X_{ik} > c} (Y_i - \overline{Y}_{k,c,r})^2$$

Where the following are the average outcomes in the two subsamples with "l" and "r" denoting "left" and "right":

$$Y_{k,c,l} = \sum_{i:X_{ik} \leq c} Yi \bigg/ \sum_{i:X_{ik} \leq c} 1 , \qquad \text{and } Y_{k,c,r} = \sum_{i:X_{ik} > c} Yi \bigg/ \sum_{i:X_{ik} > c} 1$$

are the average outcomes in the two subsamples. This is repeated over all subsamples while optimizing each split with respect to minimizing the residual squared error, $Q(k, c)$. This study gains two key insights from the implementation of the RF algorithm.

First, I measure the importance of each law feature by the total decrease in node impurity (the residual sum of squares). This is obtained by splitting on feature k and is averaged across all decision trees in the forest. It is important to note that selected variables may not have a causal relationship with the outcome but instead may be highly correlated with another variable in the model which does have a causal relationship with the outcome – this is a common feature of the dataset given that many related laws are passed at the same time. Additionally, take two law indicators that are 100% correlated with each another. It is impossible to isolate the individual, causal effects of effects of each law on the outcome. Athey et al. (2019) suggest that RF will split on features highly associated with the outcome but could consequently ignore features correlated with the selected feature. This makes sense given that the goal is prediction, not inference. The takeaway is that features which RF does not select but are highly correlated with important features should be taken as equally important in this prediction-oriented setting.

Second, I use the model's estimates to build a prediction of the outcome variable in out-of-bag (oob) observations (this is the validation sample). The oob data consists of every observation from 2014-2018. That way, the predictions have some greater relevance to the present, and are not just random observations independent of state and year. These estimates are unbiased because the data was trained on the other subsample of data. Comparing the set of estimated values of the GDR with the actual GDR will offer insight into the degree to which GDR can be predicted by using primarily the selected features. To emphasize, this approach is designed for prediction – causality cannot be inferred – but any similarities between the significant Lasso covariates and features with the highest node impurities in RF will lend credence to both methods.

6. Results and Discussion

6.1 Fixed Effects Results

The FE specifications generally indicate that laws specifically relating to background checks, criminal history, or domestic violence have a higher association with changes in the GDR than do laws falling in any of the other five categories. Another trend is that the number of coefficients statistically significant at the 5% level or higher decreases with the complexity of the model. For brevity, I cannot discuss all 102 laws, so I instead focus on laws with coefficients significant at the 5% level or higher in the more-sophisticated models (models 5, 6, or 7). For example, Model 4 suffers from omitted variable bias in the form of variables related to the outcome that trend at different rates across different states.

Coefficient interpretation should be viewed under the lens of the average treatment effect (ATE), where the value is the expected change in the GDR (on average and in a given state) as a result of passing *law_k* in the state. The perfect experiment would involve observing the GDR in state *x* over a period of time *t* where the state does not follow *law_k*. Then, *law_k* would be passed, holding all else equal, and the GDR would be measured *t* later. The difference in GDR before and after the passage would be the ATE. Actual coefficients in this paper are the on-average (with respect to state and year) estimated effects – actual effects vary across state.[43] In many observational settings where treatment cannot be forced by the researcher, the ATE can be

[43] Suppose the coefficient on *law_k* in this perfect experiment across all 51 “states” equals -5.00. States with lower starting GDR’s will usually experience a smaller effect from passing the law – if the GDR in Massachusetts had a starting value of 4.50, it could not possibly have a negative GDR. Similarly, a state with a much higher starting GDR, like Louisiana, could expect the decrease in GDR to be larger in magnitude than the model’s coefficient.

measured by observing the differences in outcomes between observations which follow *law_k* and observations that do not. This paper's specifications attempt to control for confounding effects that contribute to differences in these outcomes not driven by the covariate of interest, so that treatment is effectively as good as randomly assigned. During the discussion of results throughout this section and the next, I say that the coefficient value x *corresponds* to x-point change in the GDR. This correspondence may only be taken as causal under the assumption that the models adequately control for confounding effects.

Next, I transition to a detailed discussion of covariates with coefficients significant at the 5% level or higher in either Model 5, 6, or 7. Below is the abridged table of this output. (See **Table 6.1.1** for a complete display of OLS and FEs Models 1-7).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	FE	FE	FE	FE	FE	FE
VARIABLES		state-fixed only	state- and year-fixed	clustered std. errors	state-specific linear time trend	non-linear time trend	non-linear trend, law category interaction
Outcome variable of interest:	*gdr*	*gdr*	*gdr*	*gdr*	*gdr*	*gdr*	*gdr*
law_1	0.0650	0.344	-0.591	-0.591	**-1.940*** **	**-1.048** **	-0.984*
	(0.679)	(0.846)	(0.811)	(1.233)	**(0.550)**	**(0.489)**	(0.520)
law_4	0.942	0.0623	0.973	0.973	0.823	**1.838** **	**1.841** **
	(1.043)	(1.713)	(1.618)	(1.676)	(0.650)	**(0.818)**	**(0.820)**
law_7	1.152*	0.867	-0.00985	-0.00985	**-0.759** **	0.0762	0.0768
	(0.676)	(0.818)	(0.776)	(0.569)	**(0.361)**	(0.220)	(0.220)
law_8	-3.298***	-2.392*	-2.396*	-2.396*	0.0301	**-1.350** **	**-1.384** **
	(0.873)	(1.302)	(1.229)	(1.366)	(0.909)	**(0.602)**	**(0.608)**
law_15	-3.931***	1.343*	2.208***	2.208***	**1.403** **	0.403	0.374
	(0.441)	(0.706)	(0.672)	(0.629)	**(0.561)**	(0.565)	(0.559)
law_19	-0.114	-1.690***	-1.904***	-1.904**	**-1.831*** **	**-1.639*** **	**-1.649*** **
	(0.306)	(0.437)	(0.416)	(0.884)	**(0.545)**	**(0.472)**	**(0.476)**

law_22	-1.044**	-0.907*	0.0212	0.0212	**1.251*** **	0.202	0.169
	(0.472)	(0.522)	(0.502)	(0.833)	**(0.421)**	(0.319)	(0.315)
law_24	-5.584***	-2.690***	-2.958***	-2.958	**-1.404*** **	**-1.171** **	**-1.127** **
	(0.798)	(0.603)	(0.576)	(2.090)	**(0.364)**	**(0.518)**	**(0.511)**
law_31	5.981***	0.104	-0.393	-0.393	-2.255*	**-4.515*** **	**-4.737*** **
	(1.354)	(1.945)	(1.839)	(1.401)	(1.306)	**(1.435)**	**(1.436)**
law_32	-6.478***	0.582	0.765	0.765	2.182	**4.337*** **	**4.572*** **
	(1.373)	(1.973)	(1.867)	(1.377)	(1.399)	**(1.447)**	**(1.451)**
law_33	2.063*	-3.521	-3.037	-3.037***	**-4.110*** **	-1.183	-1.286
	(1.212)	(2.162)	(2.033)	(0.832)	**(0.768)**	(1.114)	(1.102)
law_34	-1.483	2.687	2.154	2.154*	**4.641*** **	1.706	1.770
	(1.215)	(2.177)	(2.048)	(1.075)	**(0.995)**	(1.169)	(1.157)
law_35	3.072**	-0.853	-0.0666	-0.0666	**3.922*** **	0.512	0.570
	(1.342)	(2.224)	(2.115)	(1.171)	**(1.119)**	(1.462)	(1.445)
law_36	-2.341*	0.897	0.331	0.331	**-3.163** **	-0.0210	-0.0633
	(1.344)	(2.267)	(2.155)	(1.302)	**(1.398)**	(1.503)	(1.481)
law_89	-0.490	-0.778***	-0.223	-0.223	0.191	**0.294** **	0.298*
	(0.347)	(0.246)	(0.308)	(0.351)	(0.123)	**(0.144)**	(0.150)
bchecks_criminals_domesticv							-1.827
							(1.299)
bchecks_criminals							0.159
							(0.526)
bchecks_domesticv							0.473
							(0.720)
criminals_domesticv							1.016**
							(0.501)
Constant	12.22***	10.37***	15.75***	15.75***	354.0***	8,530	8,917
	(1.648)	(1.259)	(1.443)	(1.362)	(61.09)	(11,586)	(11,214)
Observations	1,428	1,428	1,428	1,428	1,428	1,428	1,428
R-squared	0.466	0.327	0.420	0.420	0.807	0.841	0.841
State FE		YES	YES	YES	YES	YES	YES

Year FE			YES	YES	YES	YES	YES
Clustered Standard Errors				YES	YES	YES	YES
Time Trend Interaction					YES	YES	YES
Non-linear Trend						YES	YES
Significant-law-category Interaction							YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The section continues with a law-by-law examination of significant covariates in detail, with respect to interpretation, variation across models, the number of states that enforced the law in 2020,[44] and potential directions for future research. I start with four laws – laws 1, 19, 24, and 31 – that have consistently significant effects (**bolded** in above table and below discussion) across model specifications, particularly the sophisticated specifications. These four laws are broad in scope and none are highly correlated with the laws they are conditional on.[45] Additionally, note that a handful of laws have coefficients that are significant in the positive direction (the passage of a law corresponds to an increase in the GDR) in one or more sophisticated specifications. Sometimes this can be explained by high correlation with another law, but other times possible explanations are outside this paper's scope.

Law_1*: Is a criminal background check required for the sale of all firearms (as opposed to only the sale of all handguns)?*

[44] Reminder: analysis is based on a sample from 1991-2018. I chose to use 2020 as a reference point instead of 2018 because (1) it puts interpretation in present terms, and (2) the regularization techniques (employed in the Lasso specification in the next section) are designed to facilitate out-of-sample (in this case, post-2018) predictive and inferential performance.
[45] See **Tables 4.1.1 and 4.1.2**.

To be clear, states that do not require criminal background checks for gun sales by unlicensed sellers, and states that require a criminal background check, but for only the sale of handguns, are counted as not following this law (i.e., are counted as "no"). Consider Model 5. The interpretation on the coefficient of -1.940 is that these states would, on average, see its GDR fall by 1.940 points if it passed law 1. When the state-specific, non-linear time trend is added, the estimated coefficient corresponds to a 1.048-point decrease in the GDR. The estimates evidence that the law is working as intended. As of 2020, 16 states plus D.C. follow this law.

***Law_19**: Does state law disqualify people from getting concealed carry permits based on other [non-misdemeanor convictions] criminal history?*

The coefficient on law 19 ($p<0.01$) corresponds to a 1.831-point decrease in the GDR in Model 5 and a roughly 1.64-point decrease in the GDR in Models 6-7. This is the first concealed carry law discussed, so it is important to reemphasize that states that do not issue concealed carry permits answer "yes" to disqualification questions, given that the state disqualifies all people from getting concealed carry permits. This interpretation can be taken as causal under the assumption that the fixed-effects, linear trend, and non-linear trend capture any confounding effects. Twenty-five states followed this law in 2020.

***Law_24**: Does state law require all people under final domestic violence restraining orders to turn in their firearms when they become prohibited from having them?*

The coefficients on law 24 are significant in Models 5 ($p<0.01$) and 6-7 ($p<0.05$), where the effect of enforcing this law corresponds with the following respective decreases in the GDR: 1.404, 1.171, and 1.127. Given the consistency across significant models, the interpretations are causal if the different fixed-effects and trends subdue omitted variable bias. Twenty-one states plus D.C. enforced this law in 2020.

Law_31*: Does state law disqualify people with convictions for abusing their boyfriends and girlfriends from carrying concealed guns in public?*

The coefficients on law 31 are significant at the 1% level in Models 6-7, where the effect of enforcing this law corresponds with a 4.515- and 4.737-point decrease, respectively, in the GDR, the strongest ATE thus far. Interestingly, the coefficients are only significant when the FE specification includes a state-specific, non-linear time trend (the coefficient on law 31 in the OLS specification is also significant, but in the opposite direction) – it is possible that non-linear omitted variables were suppressing the effects of law 31 pre-Model 6. Twenty-three states plus D.C. enforced this law in 2020.

Next, I discuss laws that are less-consistently significant, including laws that have positive coefficients, but in less detail and not necessarily law-by-law. Like most concealed-carry laws in the dataset, law 32[46] is distinct from law 31 only in that it disqualifies the restricted group from "getting concealed carry permits" rather than from "carrying concealed guns in public."[47] The coefficients on law 32 are also significant at the 1% level in Models 6-7 but both are positive – the estimated *increases* in the GDR after passing this law are 4.337 and 4.572, respectively. This result raises a red flag with respect to laws 31 and 32. The correlation between the two laws equals 0.965, so this substantial difference in coefficients (both are significantly different from zero, but in opposite directions) is driven by relatively small subsamples of the data. Rather surprisingly, given the similarity between the two laws, there is essentially zero correlation (-0.02) between states (in a given year) that *allow the concealed carry of handguns in public* and *issue concealed carry permits* across the dataset.

[46] See **Table 2.0.0** for a comprehensive table of gun law descriptions.
[47] There are several of these pairs of laws throughout the set, so I will at times refer to any given pair as a "concealed carry pair."

An initial explanation for the discrepancy between coefficients is that disqualifying abusers from carrying concealed guns in public has the effect of decreasing the GDR, regardless of the specific law or class of dangerous/at-risk individuals (felons, domestic abusers, minors, etc.), and that disqualifying abusers from getting permits has the effect of increasing the GDR, again regardless of the law itself. The coefficients on law 33 ($p<0.01$) and law 34 ($p<0.01$) in Model 5 (-4.110 and 4.641, respectively) are consistent with this explanation: disqualifying abusers from carrying in public corresponds with a decrease in the GDR, where prohibiting abusers from getting concealed carry permits corresponds with an increase in the GDR. Also, similarly to laws 31 and 32 the correlation between law 33 and 34 equals 0.956. This suggests that, of the relatively small number of observations that differ with respect to enforcing laws 33 and 34, those observations also differ with respect to allowing the concealed carry of guns and issuing concealed carry permits.

The coefficients on law 35 ($p<0.01$) and law 36 ($p<0.05$) in Model 5, however, invalidate this hypothesis. The correlation between the two laws is also high (0.953), but the coefficient on law 35 corresponds to an estimated 3.922-point increase in the GDR and the coefficient on law 36 corresponds to an estimated 3.163-point decrease in the GDR. In the previous two pairs of laws discussed, disqualifying abusers from carrying concealed guns in public was associated with a *decrease* in the GDR and disqualifying abusers from getting concealed carry permits was associated with an *increase* in the GDR. The effects are reversed with respect to laws 35 and 36. Overall, inferences on laws 31-36 are difficult because there is no clear explanation for the discrepancy between coefficients within these three pairs of domestic-violence related concealed-carry laws, other than the understanding that small subsamples of the data drive the difference.

Further analysis is outside of this paper's scope and reserved for a potential direction of future research.

Four other laws – laws 4, 15, 22 and 89 – correspond to estimated increases in the GDR in at least one of the sophisticated FE specifications, but are not consistent across the majority of specifications like laws 1, 19, 24, and 31. In Models 6-7, the effect of having exceptions to the background check requirement (law 4 ($p<0.05$)) corresponds to an expected 1.84-point increase in the GDR. Again, states that do not require criminal background checks for gun sales by unlicensed sellers are counted as "no." Only one state (Indiana 1991-1997) did not have exceptions to the requirement, so this law is perfectly correlated with law 0 (and law 9)[48] for the exception of Indiana (1991-97), a law that with non-significant coefficients in each sophisticated model. I would also note that the coefficient only becomes significant when the non-linear trend is added to the specification. Thus, an explanation for this counter-intuitive direction of significance may relate to omitted variables with a non-linear association with the GDR in Indiana circa 1991-97, but a closer analysis is required.

Law 15 ($p<0.05$) and law 22 ($p<0.01$) have significant coefficients in Model 5 only and correspond with a 1.403-point and 1.251-point increases, respectively, in the GDR. The coefficient on law 89 is significant at the 5% level but only in Model 6, where the effect of enforcing this law corresponds to a 0.294-point increase in the GDR. In this context, states that either do not issue concealed carry permits or do not require a permit to carry a concealed firearm in public are grouped with states that require a revocation. This estimate evidences the notion that stronger concealed carry laws deter crime. Additionally, note the coefficients on laws 15, 22, and 89 are

[48] Law 0 and law 9 are perfectly correlated.

not statistically significant under the XPO Lasso specification (and the coefficient on law 4 is significant but negative).

Law 7 and law 8 are background-check laws that are significant at the 5% level, the former in Model 5 and the latter in Models 6-7. The interpretation on law 7's coefficient follows: mandating a waiting period corresponds with a 0.759-point decrease in the GDR. The variation in this law offers ample opportunity for future research. For instance, ETR additionally tracks the event that triggered the waiting period and differences in days of the waiting period. The interpretations on law 8's coefficients follow: having penalties for a buyer who fails to follow background check law corresponds to 1.350-point and 1.384-point decreases, respectively, in the GDR. Note, however, that the correlation between law 8 and law 0 equals 0.986 (and the coefficient on law 0 is not statistically different from zero), so a relatively small number of observations seem to be causing the significance.

Overall, the FE specifications offer traditional econometric insight into the analysis, but the employed ML specifications – XPO Lasso and RF – are designed for high-dimensional data that limit overfitting, thus facilitating out-of-sample predictive power. For reasons discussed in **Section 5.2**, evaluating 102 independent models (one for each law indicator) and optimally selecting controls with respect to the indicator of interest is preferred to analysis under the FE lens, which instead forces every indicator into one model and implements no regularization.

6.2 XPO Lasso Results

In the interest of studying the relationship between the GDR and a given law indicator in the context of all other gun control laws, the FE specifications include every law indicator as a covariate of interest within the same model, but this approach does not allow for proper regularization. The XPO Lasso specification is really 102 separate models, each with a different law indicator as the covariate of interest. This method allows each law covariate to be considered in the context of the other laws (by including every law variable and fixed effect in the set of possible controls) while performing regularization. In fact, individualizing each model to the covariate of interest optimizes variable selection. The set of optimal controls varies across the covariates of interest – what may be important to law 8 may not be important to law 24. Again, it should be emphasized that the coefficients produced by XPO Lasso should be taken with greater confidence than those produced by FE specifications.

Below is the abridged table, showing only the law indicators with coefficients significant at the 5% level or higher in either Model 5, 6, or 8 (XPO Lasso).[49] In regard to Model 8, recall that each coefficient was derived from a separate model – the coefficients are displayed under the model heading of "XPO Lasso" for brevity. This allows clear comparison to the coefficients on the same law but of the sophisticated FE models. (See **Table 6.2.1** for a complete display of the XPO Lasso results).

	(5)	(6)	**(8)**
	FE	FE	**XPO Lasso**
VARIABLES	state-specific linear time trend	State-specific non-linear time trend	**Linear-time trend only**
Outcome variable of interest:	*gdr*	*gdr*	*gdr*
law_1	-1.940***	-1.048**	**0.224**
	(0.550)	(0.489)	**(0.499)**
law_2	0.634	0.412	**2.081*** **
	(0.491)	(0.394)	**(0.663)**
law_4	0.823	1.838**	**-2.426*** **
	(0.650)	(0.818)	**(1.059)**
law_5	0.531	0.780*	**-1.929******

[49] Law indicators that have significant coefficients under XPO Lasso specification are **bolded**.

	(0.462)	(0.403)	**(0.442)**
law_7	-0.759**	0.0762	**-0.0337**
	(0.361)	(0.220)	**(0.546)**
law_8	0.0301	-1.350**	**-4.314****
	(0.909)	(0.602)	**(1.060)**
law_10	0.924*	0.775	**1.680***
	(0.505)	(0.498)	**(0.562)**
law_14	-0.502	-0.625	**3.372****
	(0.617)	(0.701)	**(0.745)**
law_15	1.403**	0.403	**-0.553**
	(0.561)	(0.565)	**0.617**
law_16	-0.433	-0.406	**-1.643**
	(0.595)	(1.042)	**(0.744)**
law_19	-1.831***	-1.639***	**-1.620****
	(0.545)	(0.472)	**(0.393)**
law_21	0.207	0.388	**-1.041**
	(0.358)	(0.405)	**(0.501)**
law_22	1.251***	0.202	**-0.751**
	(0.421)	(0.319)	**(0.401)**
law_24	-1.404***	-1.171**	**-2.408***
	(0.364)	(0.518)	**(0.912)**
law_27	-0.307	-0.388	**-1.867***
	(0.419)	(0.551)	**(0.572)**
law_28	0.503	-0.775	**-3.199****
	(0.628)	(0.785)	**(0.838)**
law_29	0.635	0.0565	**-2.307***
	(0.558)	(0.654)	**(0.786)**
law_31	-2.255*	-4.515***	**8.939****
	(1.306)	(1.435)	**(2.683)**
law_32	2.182	4.337***	**-7.362***
	(1.399)	(1.447)	**(2.516)**
law_33	-4.110***	-1.183	**-6.012***
	(0.768)	(1.114)	**(1.904)**
law_34	4.641***	1.706	**3.458**
	(0.995)	(1.169)	**(2.567)**
law_35	3.922***	0.512	**1.649**
	(1.119)	(1.462)	**(1.928)**
law_36	-3.163**	-0.0210	**-4.285***
	(1.398)	(1.503)	**(1.313)**
law_38	0.0931	0.0497	**-1.305****
	(0.259)	(0.379)	**(0.326)**
law_39	0.308	0.127	**-1.442****
	(0.347)	(0.355)	**(0.416)**
law_40	0.858*	-0.0790	**1.068**
	(0.461)	(0.396)	**(0.446)**

law_42	-0.111	0.001	**-1.195*** **
	(0.326)	(0.305)	**(0.440)**
law_46	0.260	0.284	**1.297**** **
	(0.305)	(0.321)	**(0.375)**
law_53	-1.188*	-0.747	**-1.915** **
	(0.670)	(0.611)	**(0.845)**
law_62	0.212	0.294*	**-1.094*** **
	(0.177)	(0.168)	**(0.367)**
law_87	0.0307	-0.0994	**-0.905*** **
	(0.192)	(0.183)	**(0.296)**
law_89	0.191	0.294**	**-0.220**
	(0.123)	(0.144)	**(0.267)**
law_97	0.838	0.664	**-1.887**** **
	(0.577)	(0.451)	**(0.515)**
Constant	354.0***	8,530	
	(61.09)	(11,586)	
Observations	1,428	1,428	
R-squared	0.807	0.841	

Robust standard errors in parentheses
**** p<0.001 *** p<0.01, ** p<0.05, * p<0.1
Note: **** not available to Model (5)-(6) output.

Note that the standard errors of the coefficients from the XPO Lasso model are generally higher than the standard errors from the FE models. When inference is the goal in high-dimensional settings, there is a tradeoff between in-sample fit and out-of-sample flexibility – the regularization via control selection employed in the XPO Lasso model makes inferences more flexible but lowers the accuracy within the model.

This section continues with a general discussion of the similarities and differences between the FE and XPO Lasso output – both the number and the percentage of significant laws with negative coefficients is higher under the XPO Lasso model (20 out of 26) than under Model 5 (6 out of 10) or Model 6 (5 out of 8) – and emphasizes gun control laws that either have been consistently significant across specifications or produce significant coefficients for the first time.

I begin with an analysis of the four laws emphasized in **Section 6.1** (laws 1, 19, 24, and 31[50]) and continue with a category-by-category evaluation of general trends seen in the coefficients produced by the XPO Lasso model.

The coefficient on law 1 is not statistically significant in the XPO Lasso model, suggesting that the strong relationship between law 1 and the GDR seen in the FE models was at least partially a result of overfitting and that the inferences drawn in FE models may not extrapolate to the future. On the other hand, XPO Lasso output validates the inferences drawn on laws 19 ($p<0.001$) and 24 ($p<0.01$); the estimated effect of passing the law corresponds to 1.620-point and 2.408-point decreases, respectively, in the GDR. Inference on law coefficients under the XPO Lasso specification should generally be done with confidence, and especially so when the results mirror those from previous models. This paper produces strong evidence that the passage of both laws 19 and law 24 correspond to substantial declines in the GDR – these relationships are causal if the fixed-effects and trends effectively control for confounding variables.

The coefficient on law 31 is highly significant under the XPO Lasso specification and corresponds to an 8.939-point increase in the GDR. The coefficient on this law in Model 6 corresponds to a 4.515-point *decrease* in the GDR. This flip from a positive value to a negative value, especially of such a substantial magnitude, raises questions. Further, the interpretation of law 31's XPO Lasso coefficient is again complicated by the coefficient on law 32. Recall, the coefficient on law 32 in Model 6 corresponding to a 4.337-point *increase* in the GDR, despite the indicators being 96.5% correlated. Under the XPO Lasso specification, the coefficient on law 32 also flips (-7.362 significant at the 1% level). Laws 33 and 36's XPO Lasso results mirror those from Model 5, with coefficients that correspond to 6.012-point and 4.285-point decreases,

[50] Recall, inference on law 31 is difficult because of the strange results seen in FE discussion.

respectively, in the GDR. The coefficient on law 38 is newly significant ($p<0.001$) and corresponds to a 1.305-point decrease in the GDR under the XPO Lasso specification. Overall, however, this set of domestic violence, concealed-carry laws (laws 31-38) produce inconsistent results that cannot be clearly explained by anything in the data. Therefore, inference on this set should be done cautiously, despite the majority of the coefficients being negative and statistically significant under the XPO Lasso specification.

The coefficients on the background check laws (laws 2, 4, 5, and 8) generally[51] suggest that passing laws of this type corresponds to declines in the GDR. The "gun show loophole" law, law 5, is for the first time significant ($p<0.001$) and corresponds to a 1.929-point decrease in the GDR. Although its coefficients are not consistently significant like those on laws 19 and 24, the XPO Lasso model is the best lens of analysis and it produces strong evidence that closing the gun show loophole is associated with a decline in the GDR. The results are similarly promising with regard to laws 4 and 8. Note that laws 0, 4, and 8 are highly correlated (97.5%+), so differences in coefficients are driven by small variations in the data. Therefore, it may be more prudent to expect the enforcement of a package of broad background checks to have a greater impact on the GDR than to take the coefficients on law 4 or 8 as independently causal.

The coefficients on criminal-history related laws (laws 10, 14, 16, and 19) are split between positive and negative values, but none are consistent across specifications but for law 19. Nonetheless, interpretations of the coefficients follow: law 10 corresponds to a 1.680-point increase in the GDR; law 14 corresponds to a 3.372-point increase in the GDR; and law 16 corresponds to a 1.643-point decrease in the GDR. Again, in the absence of strong correlations

[51] The coefficient on law 2 corresponds to a 2.081-point *increase* in the GDR but is a very specific law (see **Table 2.0.0**) and its coefficient does not have the same significance in earlier specifications.

and inconsistent results that plagued the interpretation of laws 31-36, providing an explanation for the positive significance seen here is outside of this paper's scope.

The XPO Lasso model produces strong evidence that passing domestic-violence related gun control laws[52] (above all other category of laws) is most important to the aim of lowering the GDR. Specifically, passing these laws is associated with declines in the GDR.[53] The following law indicators' coefficients correspond to *x*-point decreases in the GDR: law 21 ($p<0.05$) to a 1.041-point decrease; law 27 ($p<0.01$) to a 1.867-point decrease; law 28 ($p<0.001$) to a 3.199-point decrease; law 29 ($p<0.01$) to a 2.307-point decrease; and law 39 to a 1.442-point decrease. Also, note that the coefficient on law 22, which was positive Model 5, is not significant. Inference can be confidently drawn on each coefficient given that this group of laws is generally weakly correlated,[54] unlike the case with background check laws.

It is also important to briefly discuss the six laws pertaining to other categories that have (newly) significant coefficients under the XPO Lasso specification. Law 46 corresponds to an estimated 1.297-point increase in the GDR. Laws 53, 62, 87, and 97 correspond to the following estimated point decreases in the GDR: law 53 ($p<0.05$), 1.915; law 62 ($p<0.01$), 1.094; law 87 ($p<0.01$), 0.905; and law 97 ($p<0.001$), 1.887. This is strong evidence of a potential causal relationship with the GDR, but the evidence is lacking in two main respects. One, this is the first (albeit the most sophisticated) specification in which these laws have statistically significant coefficients. Two, the laws are spread out across categories – if one of these laws had a causal relationship with the GDR, it seems plausible to expect more related laws to have significant

[52] Possession-, not concealed carry, related.
[53] See **Table 2.0.0** for a description of each relevant law.
[54] See **Table 6.2.2** for the correlation matrix.

associations with the GDR. Notwithstanding, it is entirely possible that one of these laws has a causal relationship with the GDR in truth, but more analysis is needed to draw valid inferences.

The drawn inferences assume that the state-fixed effects, national time trend, and state-specific time trends control for a substantial portion of the effects of confounding variables – if these assumptions are correct, the estimated relationships are causal. Nonetheless, this paper produces strong evidence that the passage of laws related to background checks, criminal history (partially), and domestic violence (especially) will correspond to declines in the GDR. Regularization techniques – dimension reduction, namely – increase model flexibility and thus out-of-sample (years following the 1991-2018 sample) application.

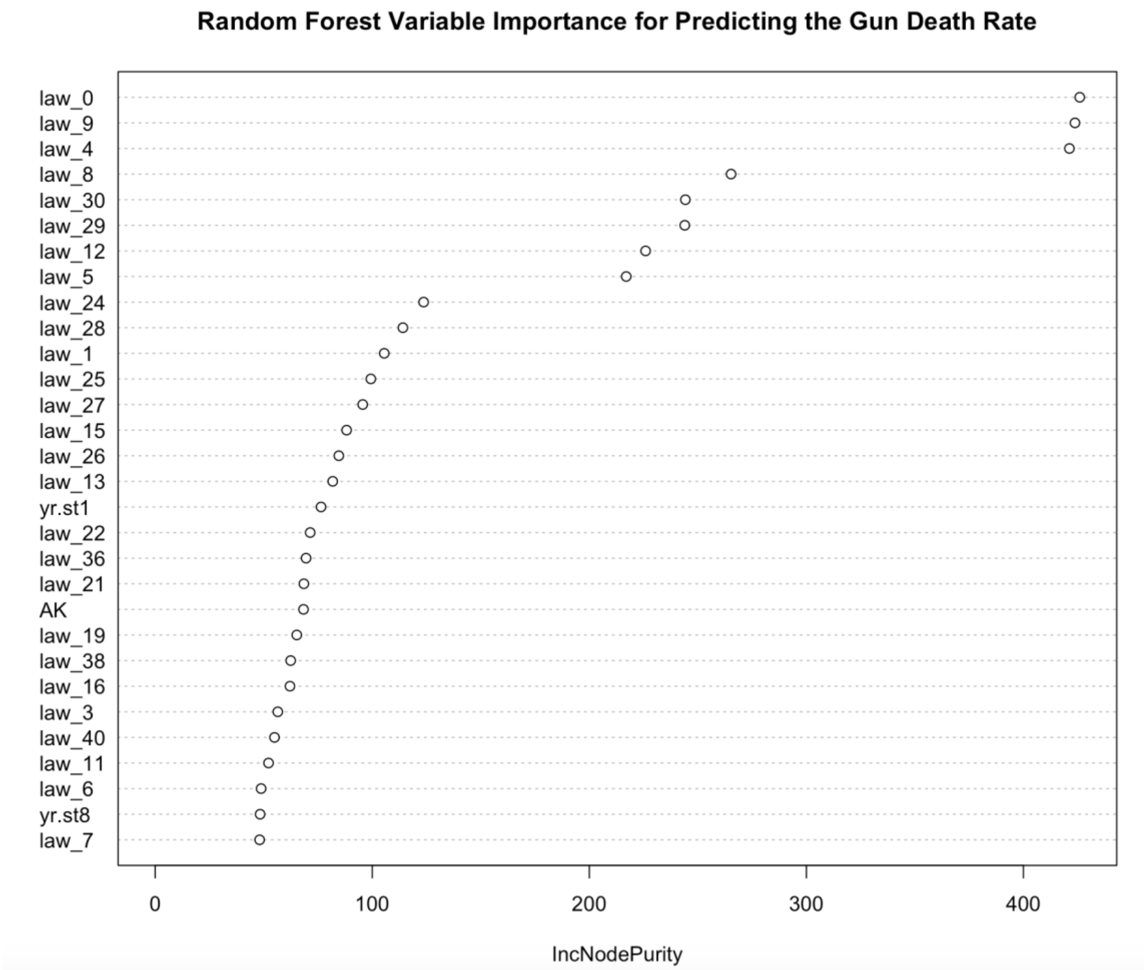
6.3 Random Forests Results

The interpretation of the Random Forests results are straightforward. RF is designed for prediction, so standard errors are not produced, and inferences cannot be made. The algorithm, however, is optimized for estimating the associations between law features[55] and the GDR. First, features are ranked in descending order of importance in predicting changes in the GDR. XPO Lasso results will be further validated if the law indicators at the top of this list generally mirror the significant indicators under the XPO specification. Second, I use the model's estimates to build a prediction of the outcome variable in oob observations (2014-2018, in this case). Comparing the set of estimated values of the GDR with the actual GDR will offer insight into the degree to which GDR can be predicted by using primarily the selected features.

The below table[56] illustrates variable importance for predicting the GDR with respect to total decrease in node impurity. Potential features include: the set of law indicators, *K;* the state-

[55] Covariates are called "features" in the RF literature.
[56] Only the 30 most important features displayed.

fixed effects, γ_t; the year-fixed effects, τ_i; the state-specific linear time trends, $(\tau_i * t)$; and the state-specific, non-linear time trends, $(\tau_i * t^2)$. Variables are displayed in descending order of importance. (See **Table 6.3.1** for the total decrease in node impurities of each law feature).



There is some discrepancy between the features deemed most important by RF and the covariates with the most-significant coefficients in the XPO Lasso regressions. For example, the two laws most consistently significant in FE and XPO Lasso models – law 19 and law 24 – have total-decrease-in-node-impurities values of 58.7 and 127.3, respectively. Law 0, on the other hand, is deemed most important with a value of 430.0. It is critical to remember that RF will often exclude features that are highly correlated with features it splits on. Thus, law indicators that are highly correlated with those displayed in the above table (but which are not included) are also predictors of the GDR. Overall, there are three key takeaways.

First, only two of the above features (law 36 and law 38) belong to the subset of domestic violence, concealed carry laws. The coefficients on these concealed carry pairs (notably, laws 31-36) are inconsistent across FE and XPO Lasso specifications and within pair. Thus, the paper concludes that causal inference on any one of the coefficients should be avoided. The results here support that conclusion: domestic violence, concealed carry pairs are not among the most important predictors of the GDR.

Second, only one state and only two time-trends are present in this set of important features. This suggests that laws themselves are the most important drivers of the GDR, despite significant differences in the GDR across states. The features *yr.st1* and *yr.st8* refer to the state-specific time trends of Alaska and D.C., respectively. Intuitively, these inclusions are expected given how dramatically DC's GDR trended throughout the observation period (see **Figure 6.3.2**).

Third, and most important, every law feature in the above table relates to either background checks, criminal history, or domestic violence. These three categories have consistently been the most important across all specifications. This is further evidence that policy makers should look first to background-check, criminal-history, or domestic-violence related laws if the goal is to change the GDR.

This section also uses the RF results to predict an alternate metric of the GDR (for each state in years 2014-2018), as detailed in **Section 5.2.3**. To reiterate, the difference between the actual GDR and the predicted GDR will offer additional insight into the selected features' (see above figure) predictive power. Specifically, the mean of this difference (if significantly different from zero) will reveal the presence of bias, and the standard deviation of this difference will reveal the general accuracy of the alternate GDR metric. (See **Table 6.3.3** for a complete display of GDR predicted values on the oob sample).

Figure 6.3.4: Summary Statistics and T-test of the (Predicted – Actual) GDR

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
diff_~dr	**255**	**.0584314**	**.0935094**	**1.493226**	**-.1257212**	**.2425839**

```
    mean = mean(diff_rf_gdr)                                  t =   0.6249
Ho: mean = 0                                  degrees of freedom =      254

    Ha: mean < 0                 Ha: mean != 0                 Ha: mean > 0
 Pr(T < t) = 0.7337         Pr(|T| > |t|) = 0.5326          Pr(T > t) = 0.2663
```

Given that the *p-value* in Pr.(|T| > |t|) > 0.05, the null hypothesis (the mean value of 0.058 is not statistically significantly different from zero) is accepted. This confirms that the alternate metric of the GDR is unbiased. Further, the standard deviation of 1.493 is relatively low given the significant variation in values of the actual GDR across states and years. Overall, these reflect that the GDR can be predicted accurately and without bias using chiefly the laws given the most weight – laws related to background checks, criminal history, and domestic violence – which are the law indicators with the highest measured total decreases in node impurity from splitting on the feature.

7. Details of Machine Learning Estimation

7.1 XPO Lasso Insights

This section is devoted to increasing an understanding of the XPO Lasso (DML2) algorithm as an alternate lens into scrutinizing the XPO Lasso results. First, I outline the algorithm, developed by Chernozhukov et al. (2018). Next, I explain what is actually happening in each step. Lastly, I put the insights in the context of the results above.

As an aside, it is helpful to restate the XPO Lasso regression model employed by this paper:

$$E[y_{it}|\, d_{it}, X_{it}] = \alpha + \beta * d_{it} + \theta * X_{it} + \varepsilon_{it} \qquad \text{(XPO/DML2)}$$

where E[ε_{it} | d_{it}, W_{it}] = 0; the number of non-zero elements in α must not be too large to satisfy the sparsity requirement; y_{it} is the GDR in a given state in a year; d_{it} is the law indicator of interest (one of a possible 102) and the state-specific linear time trend, $\tau_i * t$; and X_{it} is the set of possible controls – every law indicator minus the chosen regressor of interest, along with the state-fixed effects (τ_i) and year-fixed effects (γ_t).

The following breakdown of the XPO Lasso algorithm is found in Stata's Lasso for Inference manual:

1. *Randomly split the data into K sections, where K = 10 (the standard in the literature)*
2. *In Sample 1 (the sample with K – 1 of the K splits):*
    a. *Run a Lasso of d on X. Let $\widetilde{X_{d1}}$ be the covariates.*
    b. *Regress d on $\tilde{x}_{d1}$. Let $\widehat{\beta_1}$ be the estimated coefficients.*
    c. *Run a Lasso of y on X. Let $\widetilde{X_{y1}}$ be the covariates.*
    d. *Regress y on $\widetilde{X_{y1}}$. Let $\widehat{\gamma_1}$ be the estimated coefficients.*
3. *In Sample 2 (the K_{th} split):*
    a. *Fill in $\tilde{d}$= d – $\widetilde{X_{d1}}\ \widehat{\beta_1}$.*
    b. *Fill in $\tilde{y}$ = y – $\widetilde{X_{y1}}\widehat{\gamma_1}$.*
4. *Remaining in Sample 2:*
    a. *Run a Lasso of d on X. Let $\widetilde{X_{d2}}$ be the covariates.*
    b. *Regress d on $\tilde{x}_{d2}$. Let $\widehat{\beta_2}$ be the estimated coefficients.*
    c. *Run a Lasso of y on X. Let $\widetilde{X_{y2}}$ be the covariates.*
    d. *Regress y on $\widetilde{X_{y2}}$. Let $\widehat{\gamma_2}$ be the estimated coefficients.*
5. *In Sample 1:*
    a. *Fill in $\tilde{d}$= d – $X_{d2}\widehat{\beta_2}$.*
    b. *Fill in $\tilde{y}$ = y – $\widetilde{X_{y2}}\widehat{\gamma_2}$.*
6. *In the full sample: Regress $\tilde{y}$ on $\tilde{d}$.*

Step 1: Sample 1 is the "training" sample and Sample 2 is the "validation" sample. The sample is split to optimize out-of-sample performance. The training sample is used to fit the model and the validation sample measures the performance of the model out-of-sample, with the goal of minimizing the out-of-sample mean squared error (MSE). This is where the issue of overfitting arises. The main idea is to build a model that is not too specific to the training data because, again, the goal is to maximize the performance on the validation sample. Building a generalized model is critical; the aim is to capture broad patterns likely to arise in similar (but different) data while avoiding fine-tuning. Given K = 10, this entire algorithm has ten iterations, where a different partition of data is the validation sample in one iteration. Results are averaged out to control for outliers, a technique central to the RF algorithm.

Step 2: Running Lassos and OLS regressions within the training sample is the first step following the partition of the data into *K* parts. When a Lasso of the law indicator of interest, d, is run on the set of possible controls, X, the optimal λ is selected via the plug-in method, which determines the optimal balance between generalizability and accuracy. In other words, it chooses the controls most-relevant to d. The question the Lasso aims to answer is how to best predict d with the fewest number of controls. The selected controls are then regressed on d. The same process is then repeated, except with the outcome of interest, y, in place of the indicator of interest, d.

Step 3: The algorithm now moves to the validation sample (the tenth part of the data). An alternate metric of d is estimated, where the selected controls and the covariates on these controls from the previous regression (and the training sample) is subtracted from the true treatment value in the validation sample. Essentially, this tunes the validation d to the *selected* controls in the training sample. Again, the same is done for outcome of interest in place of the law indicator of interest.

Steps 4-6: The algorithm first repeats Step 2, but this time on the validation sample. Next, Step 3 is repeated, but this time on the training sample. The two alternate measures of d (and the two alternate measures of y), each built by out-of-sample estimates are combined – the constructed values of treatment, d, are affixed so that there is a measure for the entire sample. Finally, this new outcome is regressed on the new treatment, where both are functions of the selected controls discussed in previous steps. A consequence of this procedure, however, is that XPO Lasso does not produce coefficients on controls; the list of selected controls for each model is known, but the relative importance of each is not.

Overall, XPO Lasso selects controls that are strong predictors of the law indicator of interest or the outcome of interest. The idea is to maximize the predictive power of the model with as few controls as possible. Not only is some form of regularization (like control selection) necessary in this high-dimensional framework, but it also narrows the focus of analysis within the contexts of controls that are either associated with the outcome or law indicator.

7.2 Random Forests Insights

Where XPO Lasso is tailored for inference, RF is tailored for prediction. The incorporation of RF contributed to this paper in a number of ways. First, and most broadly, it provides an additional lens through which to view the relationship between the GDR (or suicide rate) and the set of gun control laws. Even given the difference between inference and prediction, the exploration of two distinct methodologies (XPO Lasso and RF) offers the opportunity for corroborating results – there should be overlap between law indicators with strong relationships with the outcome. Second, the RF model makes an out-of-sample prediction of the GDR (and suicide rate). These predictions test the tradeoff between in-sample fit and out-of-sample generalizability. If the outcome is estimated accurately and without bias (with primarily the features with the highest total-decrease-in-node-impurities values), we know that a small subset of the 102 gun control laws drive the majority of the variation in the outcome. It is helpful to review the RF algorithm implemented in this paper as defined in Athey et al. (2019). The sum of in-sample squared errors before the split:

$$Q = \sum_{i=1}^{N}(Y_i - \overline{Y})^2, \qquad \text{where } \overline{Y} = \frac{1}{N}\sum_{i=1}^{N} Y_i.$$

After a split based in feature k and threshold c the sum of in-sample squared errors is:

$$Q(k,c) = \sum_{i:X_{ik}\leq c} (Y_i - \overline{Y}_{k,c,l})^2 + \sum_{i:X_{ik}>c} (Y_i - \overline{Y}_{k,c,r})^2$$

Where the following are the average outcomes in the two subsamples with "l" and "r" denoting "left" and "right":

$$Y_{k,c,l} = \sum_{i:X_{ik}\leq c} Yi \Big/ \sum_{i:X_{ik}\leq c} 1\,, \qquad \text{and } Y_{k,c,r} = \sum_{i:X_{ik}>c} Yi \Big/ \sum_{i:X_{ik}>c} 1$$

are the average outcomes in the two subsamples. This is repeated over all subsamples while optimizing each split with respect to minimizing the residual squared error, $Q(k,c)$.

Broadly, Random Forests are comprised of many decision trees, where the results of RF derive from the average across all trees. Within a single tree, at each decision the data is split on the node (feature) that minimizes the average squared error, so that this error never increases. Then, it is split on the next node with the same optimization strategy. The idea is to split on features in descending order of importance to predicting the outcome. Suppose law k was enforced in the observations with the lowest levels of GDR, and not enforced in the highest levels of GDR. This would be the first node to be split on because it most differentiates the data.

It is important to reiterate, however, that RF has the tendency to split on one feature in a set of highly correlated features (Athey et al., 2019). The effect is that many features highly correlated with the outcome may not be split on (and thus selected as important predictors). Interestingly, however, a large number of highly correlated background check laws (laws 0, 4, 5, 6, 8, and 9) show up in splits.

Generally, RF operates similarly to XPO Lasso in that the model is fit on the in-sample (training) data, and then tested on the out-of-sample (validation) data. A key difference with RF, however, is that, within each decision tree in the forest, only a subset of possible features is available to be split on.

8. Alternative Outcome: The Suicide Rate

Firearms are used in roughly half of all suicides (and over 60% of gun deaths are suicides), so a study of the relationship between gun control laws and the GDR would be incomplete without particular attention given to suicides. Given this, it is important to highlight that the GDR and suicide rate are inherently correlated. This paper does *not* attempt to isolate the effects of law indicators with respect to the type of gun death or method of suicide. For example, if law 24 has x effect on the GDR, it is unknown whether the effect is on primarily gun suicides, primarily gun homicides, or something in between. The overall suicide rate, rather than the suicide-by-firearm rate, is used to control for the substitution of non-firearm suicides for firearm suicides when laws are enacted.

This section employs the same methodology to estimate the relationship between the set of guns laws and the suicide rate as was used in GDR analysis – results and discussion largely mirrors that from **Section 6**. There is one expectation: the non-linear time trend was dropped from (across FE and XPO Lasso only) because this trend's covariates were automatically omitted by Stata. Also, recall that the sample is drawn instead from 2001-2018, so the notable 1990s' decline in gun violence and rise in laws enforced is not considered in the following analysis. For each lens of analysis (FE, XPO Lasso, and RF), I briefly recap the methodology before then focusing on important results along with particular attention to trends that endure from results discussed in **Section 6**.

8.1 Suicides: Fixed Effects Methodology, Results, and Discussion

I estimate the relationship between the suicide rate and the set of 102 gun control laws under the same FE specification seen in Model (5) in **Section 5.1**:

$$suicide\_r_{it} = \alpha + \beta * law_{k\,it} + \gamma_t + \tau_i + (\tau_i * t) + \varepsilon_{it} \qquad \text{(Model 5, FE)}$$

where $suicide\_r_{it}$ is the suicide rate (per 100,000 residents) of a given state in a given year; β * law_k_{it} is the vector of law indicator variables, where it takes on a value of 1 if a given state-year observation follows law k and zero otherwise; γ is the set of year-fixed effects; τ is the set of state-fixed effects; $\tau_i * t$ are state-specific time trends; and ε_{it} is the error in the regression (standard errors are clustered at the state level).

I run five variants of OLS and FE models, the most sophisticated of which (Model 5), is described above. Model (1) is a basic OLS model, a regression of the suicide rate on the set of law indicators. Model (2) is a FE regression of the suicide rate on the law indicators that includes only the state-fixed effects. Model (3) adds year-fixed effects. Model (4) expands on the third model by clustering the standard errors at the state level, a necessary assumption given that observations within each state are not i.i.d. (independently and identically distributed). And Model (5) adds the state-specific linear time trend to the fourth model.

Next, I transition to a detailed discussion of covariates with coefficients significant at the 5% level or higher in Model (5). Below is the abridged table of this output. (See **Table 8.1.1** for a complete display of OLS and FEs Models 1-5).

	(1)	(2)	(3)	(4)	**(5)**
	OLS	FE	FE	FE	**FE**
VARIABLES		state-fixed only	state- and year-fixed	clustered std. errors	**state-specific linear time trend**
Outcome variable of interest:	*suicide_r*	*suicide_r*	*suicide_r*	*suicide_r*	*suicide_r*
law_0	0.0600	2.833*	4.242***	4.242***	**2.636*** **
	(1.106)	(1.457)	(1.260)	(1.115)	**(0.691)**
law_1	0.218	0.199	-0.639	-0.639	**-0.921*** **
	(0.440)	(0.638)	(0.556)	(0.452)	**(0.303)**
law_5	2.510***	-2.301***	-2.581***	-2.581***	**-1.043** **
	(0.460)	(0.855)	(0.738)	(0.494)	**(0.491)**
law_6	- 1.911***	-1.022	-1.309**	-1.309***	**-0.489** **

	(0.394)	(0.728)	(0.627)	(0.289)	**(0.234)**
law_16^	-5.427***	-3.139***	-2.635***	-2.635***	**-2.403*** **
	(0.714)	(0.653)	(0.569)	(0.351)	**(0.285)**
law_17^	3.844***	4.471***	3.272***	3.272***	**3.420*** **
	(0.897)	(0.984)	(0.856)	(0.532)	**(0.504)**
law_26	-0.847*	-0.262	-0.777*	-0.777*	**-0.687** **
	(0.472)	(0.509)	(0.442)	(0.400)	**(0.337)**
law_27	2.758***	-0.603	-0.534	-0.534	**1.668*** **
	(0.434)	(0.813)	(0.704)	(0.562)	**(0.456)**
law_29	-2.811***	0.799	0.292	0.292	**-2.008*** **
	(0.507)	(1.051)	(0.912)	(0.802)	**(0.560)**
law_33^	4.082***	1.890	1.336	1.336***	**2.206*** **
	(0.984)	(1.178)	(1.015)	(0.461)	**(0.355)**
law_34^	-4.664***	-1.845	-1.356	-1.356**	**-1.346** **
	(0.972)	(1.212)	(1.043)	(0.517)	**(0.522)**
law_40	-0.793***	-0.656	-0.235	-0.235	**0.859** **
	(0.303)	(0.761)	(0.661)	(0.797)	**(0.360)**
law_47^	-0.484	0.457	-0.759	-0.759	**-1.042** **
	(1.030)	(1.041)	(0.975)	(0.651)	**(0.479)**
law_48^	0.649	0.0743	0.757	0.757	**1.075** **
	(0.976)	(1.025)	(0.941)	(0.628)	**(0.476)**
law_49^	-1.584	-2.630	-1.307	-1.307	**-2.189** **
	(1.190)	(1.632)	(1.479)	(1.222)	**(0.915)**
law_50^	-1.701	-0.499	1.623	1.623*	**2.073** **
	(2.234)	(1.412)	(1.284)	(0.903)	**(0.786)**
law_52	0.667	-0.280	0.938	0.938	**1.421** **
	(1.299)	(1.128)	(1.040)	(0.783)	**(0.640)**
law_68^	0.0226	1.140	2.665*	2.665***	**3.165*** **
	(0.489)	(1.589)	(1.418)	(0.786)	**(0.774)**
law_69^		-1.375	-2.622*	-2.622***	**-2.812*** **
		(1.573)	(1.442)	(0.892)	**(0.855)**
law_73	-0.140	-0.441	-0.589*	-0.589*	**-0.853*** **
	(0.453)	(0.281)	(0.317)	(0.304)	**(0.273)**
law_83	-6.753***	-0.508	0.413	0.413	**0.741** **
	(1.836)	(0.366)	(0.378)	(0.421)	**(0.347)**
law_92	1.157***	0.777***	0.455*	0.455*	**0.476** **
	(0.378)	(0.220)	(0.233)	(0.243)	**(0.229)**
law_100	4.709***	1.105	0.289	0.289	**0.893** **
	(0.964)	(1.037)	(0.942)	(0.485)	**(0.424)**
	(0.986)	(1.049)	(0.956)	(0.544)	**(0.512)**

Constant	14.61***	9.955***	10.71***	10.71***	**102.2*** **
	(1.456)	(1.182)	(1.176)	(1.056)	**(38.11)**
Observations	918	918	918	918	**918**
R-squared	0.738	0.601	0.712	0.712	**0.809**
Number of state_id		51	51	51	**51**
State FE		YES	YES	YES	**YES**
Year FE			YES	YES	**YES**
Clustered Standard Errors				YES	**YES**
Time Trend Interaction					**YES**

Robust standard errors in parentheses

**** p<0.001 *** p<0.01, ** p<0.05, * p<0.1

Note: **** not available to Model (5) output. And ^ is used to label "concealed carry pairs" discussed below.

Ten law indicators have significant coefficients at the 5% level or higher (six of which are negative) in Model (5) of the GDR analysis. Here, 23 are significant at the 5% level or higher (11 of which are negative). It is striking that nearly a quarter of all law indicators are significantly associated with the suicide rate – 12 of which have a positive relationship – in a framework that controls for state-fixed effects, a national time trend, and a state-specific time trend. Further, just 12 of the 23 indicators relate to either background checks, criminal history, or domestic violence – recall that all but one of the law indicators that was significant under sophisticated FE specifications in GDR analysis fell under one of these categories.

This section continues with a discussion of "concealed carry pairs." Five concealed carry pairs have significant coefficients under Model (5): laws 16/17, 33/34, 47/48, 49/50, and 68/69, where each pair has a correlation of at least 91.4%. Nonetheless, no law's coefficient is the same sign as the coefficient of its pair. Furthermore, consistent with the finding in **Section 6.1**, the distinction between "*allowing the concealed carry of handguns in public*" and "*issuing concealed* *carry permits*” is not associated with the sign of the coefficient. Inference on these laws' coefficients should not be drawn with confidence because there is no clear explanation for the discrepancy between coefficients, other than the understanding that small subsamples of the data drive the difference (as evidenced by the high correlations within pairs). As concluded earlier, further analysis is outside of this paper's scope and reserved for a potential direction of future research.

The coefficients on background-check related laws (laws 0, 1, 5, and 6) generally suggest that passing laws of this type corresponds to declines in the suicide rate. The passage of law 0 corresponds to a 2.636-point *increase* in the suicide rate. Laws 1, 5, and 6 correspond to the following estimated point decreases in the suicide rate: law 1 ($p<0.01$), 0.921; law 5 ($p<0.05$), 1.043; and law 6 ($p<0.05$), 0.489. With GDR as the outcome of interest, the coefficient on law 1 is significant ($p<0.01$) in Model (5) and corresponded to a 1.940-point decrease in the GDR. Under the XPO Lasso specification, passing law 5 (“closing the gun show loophole”), corresponds to an estimated 1.929-point decrease in the GDR. Additionally, the coefficients on laws 5 and 6 are consistent across FE specifications. In 2020, 22 states plus D.C. enforce law 5 and 18 states plus DC enforce law 6.

The evidence in support of passing domestic-violence related laws as a method of lowering the outcome of interest is not as robust with respect to suicide rate. The enforcement of laws 26 ($p<0.05$) and 29 ($p<0.01$) correlate with a decrease in the suicide rate (coefficients of -0687 and -2.008, respectively), while the enforcement of laws 27 ($p<0.01$) and 40 ($p<0.05$) correlate with an increase in the suicide rate (coefficients of 1.668 and 0.859, respectively).

Although the remaining five laws (52, 73, 83, 92, and 100) have significant coefficients in the most sophisticated FE specification, the evidence of a causal relationship with the suicide rate here is weaker than the evidence for a causal relationship with respect to the background check laws discussed above. There are two reasons. The coefficients on these laws are not significant across the majority of FE specifications, and these laws are unrelated to each other – it is reasonable to expect laws similar to these five to also be associated with the suicide rate if any of these five are causal in truth.

As maintained in **Section 6.1**, analysis under XPO Lasso and RF specifications are preferred to a FE framework because the ML approaches are designed for high-dimensional data and thus limit overfitting. Under XPO Lasso, 102 unique regression equations are estimated, each with a different law indicator as the covariate of interest, where controls (fixed-effects and the other 101 indicators) are selected with respect to each indicator, thus optimizing the degree of regularization within each model.

8.2 Suicides: XPO Lasso Methodology, Results, and Discussion

This paper employs the cross-fit partialing-out (XPO) variant of Lasso for inference (discussed at length in **Section 5.2.2**) to model the relationship between the suicide rate and the set of 102 gun control laws. To emphasize, the XPO model is actually 102 unique models, each with a different law indicator as the covariate of interest. The regression specification follows:

$$E[y_{it} | d_{it}, X_{it}] = \alpha + \beta * d_{it} + \theta * X_{it} + \varepsilon_{it} \qquad \text{(XPO } \textit{suicide\_r}\text{)}$$

where $E[\varepsilon_{it} | d_{it}, W_{it}] = 0$; the number of non-zero elements in α must not be too large to satisfy the sparsity requirement; y_{it} is the suicide rate in a given state in a year; d_{it} is the law indicator of interest (one of a possible 102) and the state-specific linear time trend, $\tau_i * t$; and X_{it} is the set of possible controls[57] – every law indicator minus the chosen regressor of interest, along with the

[57] XPO Lasso does not display the coefficients on the controls selected by the model.

state-fixed effects (τ_i) and year-fixed effects (γ_t). Again, note that I exclude a state-specific, non-linear time trend ($\tau_i * t_2$), like in the FE suicides models and the XPO Lasso GDR model.

Next, I discuss regressors with coefficients significant at the 5% level or higher in the XPO Lasso model and observe noteworthy deviations from the FE output. Below is the abridged table, showing only the law indicators with coefficients significant at the 5% level or higher in either Model 5 or 6 (XPO Lasso).[58] In regard to Model (6), recall that each coefficient was derived from a separate model – the coefficients are displayed under the model heading of "XPO Lasso" for brevity. This allows clear comparison to the coefficients on the same law but of the sophisticated FE models. (See **Table 8.2.1** for a complete display of the XPO Lasso results).

	(5)	**(6)**
	FE	**XPO Lasso**
VARIABLES	state-specific linear time trend	**state-specific linear time trend**
Outcome variable of interest:	suicide_r	**suicide_r**
law_0	2.636***	**-**
	(0.691)	
law_1	-0.921***	**2.110*** **
	(0.303)	**(0.523)**
law_2	0.323	**-1.516*** **
	(0.274)	**(0.577)**
law_5	-1.043**	**0.620**
	(0.491)	**(0.784)**
law_6	-0.489**	**1.205** **
	(0.234)	**(0.585)**
law_7	-0.664*	**-1.612*** **
	(0.377)	**(0.600)**
law_8	-0.858*	**-2.294** **
	(0.489)	**(1.076)**
law_10	0.849*	**2.234**** **
	(0.450)	**(0.577)**
law_14	-	**-3.732*** **
		(1.350)

[58] Law indicators that I discuss at length below are **bolded**. The coefficients on these laws should be trusted above all others for reasons I outline.

law_16^	-2.403***	**-2.874******
	(0.285)	**(0.620)**
law_17^	3.420***	**5.789******
	(0.504)	**(0.864)**
law_19	-0.0746	**-2.467******
	(0.326)	**(0.428)**
law_20	0.0587	**-0.795****
	(0.221)	**(0.357)**
law_25	-0.146	**1.942******
	(0.333)	**(0.503)**
law_26	-0.687**	**0.957**
	(0.337)	**(0.987)**
law_27	1.668***	**-1.819*****
	(0.456)	**(0.637)**
law_29	-2.008***	**-1.566****
	(0.560)	**(0.746)**
law_30	-0.0440	**-1.251*****
	(0.434)	**(0.395)**
law_31	-0.674	**8.916******
	(0.870)	**(2.061)**
law_32	0.341	**-7.776*****
	(0.809)	**(2.449)**
law_33^	2.206***	**0.666**
	(0.355)	**(0.771)**
law_34^	-1.346**	**-1.584**
	(0.522)	**(0.999)**
law_35	-0.122	**2.454****
	(0.657)	**(0.995)**
law_36	0.916	**-3.896*****
	(0.643)	**(1.256)**
law_38	-0.363	**-1.601******
	(0.347)	**(0.365)**
law_39	-0.197	**2.174******
	(0.424)	**(0.519)**
law_40	0.859**	**2.036*****
	(0.360)	**(0.630)**
law_41	-0.537*	**-0.557****
	(0.318)	**(0.241)**
law_47^	-1.042**	**-0.224**
	(0.479)	**(0.816)**
law_48^	1.075**	**-0.291**
	(0.476)	**(0.926)**
law_49^	-2.189**	**-**
	(0.915)	
law_50^	2.073**	**-**

	(0.786)	
law_52	1.421**	**0.456**
	(0.640)	**(0.615)**
law_68^	3.165***	**0.793**
	(0.774)	**(0.630)**
law_69^	-2.812***	**-**
	(0.855)	
law_73	-0.853***	**-0.182**
	(0.273)	**(0.331)**
law_78	0.645	**1.986********
	(0.651)	**(0.413)**
law_80	-0.112	**-0.839****
	(0.370)	**(0.401)**
law_83	0.741**	**-**
	(0.347)	
law_91	-0.890*	**-1.142******
	(0.492)	**(0.384)**
law_92	0.476**	**-0.0471**
	(0.229)	**(0.234)**
law_97	0.138	**-1.464******
	(0.474)	**(0.492)**
law_100	0.893**	**0.206**
	(0.424)	**(0.902)**
Constant	102.2***	**-**
	(38.11)	
Observations	918	**918**
R-squared	0.809	**-**
State FE	YES	**YES**
Year FE	YES	**YES**
Clustered Standard Errors	YES	**YES**
Time Trend Interaction	YES	**YES**

Robust standard errors in parentheses
**** p<0.001 *** p<0.01, ** p<0.05, * p<0.1
Note: **** not available to Model (5) output. And ^ is used to label "concealed carry pairs."

Like in **Section 6.2**, the standard errors of the coefficients from the XPO Lasso model are generally higher than the standard errors from the FE models, evidencing the tradeoff between in-sample fit and out-of-sample flexibility. Regularization techniques increase the model's flexibility – which is necessary in this high-dimensional setting – but at the cost of in-sample accuracy. This section continues with a discussion of general trends in the data, along with similarities between these results and the results from FE analysis of the suicide rate (**Section 8.1**) and XPO Lasso analysis of the GDR (**Section 6.2**). Overall, the most significant trend in the paper persists; gun controls laws concerning background checks, criminal history, or domestic violence drive change in the GDR and suicide rate.

Seven gun control laws – laws 7, 8, 14, 19, 20, 29, and 30 – will be the focus of discussion for three reasons: (i) each falls under one of the consistently important categories of background checks, criminal history, or domestic violence; (ii) the coefficient on each of these laws is either newly significant under the XPO Lasso specification or has a similar coefficient (significance and direction) as under Model 5 – in other words, the sign of the coefficient does not flip between sophisticated specifications; and (iii) none of these laws are "concealed carry pairs," which were discussed at length in the previous section.

The background checks laws – laws 7 ($p<0.01$) and 8 ($p<0.05$) – correspond to 1.612-point and 2.294-point decreases, respectively, in the suicide rate. The correlation between laws 7 and 8 equals 57.8% (much lower than the correlation between most background-check related laws), so the indicators could affect the suicide rate largely independent of one another. On that note, it is important to remember that law 8 is highly correlated with law 0 (98.6%), law 4 (97.5%), law 5 (90.8%), and law 9 (98.6%). Thus, evidence suggests that mandating background checks in many forms corresponds to estimated declines in the suicide rate (much like the way in which these enforcements corresponded to estimated declines in the GDR).

The criminal history laws – laws 14 ($p<0.01$) and 19 ($p<0.001$) – correspond to 3.732-point and 2.467-point decreases, respectively, in the suicide rate. Recall that under the XPO Lasso GDR framework, the coefficient in law 14 ($p<0.001$) corresponds to a 3.372-point *increase* in the GDR.

The inconsistent results between outcomes of interest raises a red flag given the noted correlation between the GDR and suicide rates. Consequently, the validity of both estimates is challenged. Law 19[59] is the most-consistent law indicator in the dataset – the passage of the law corresponded to decreases in the GDR across all **Section 6** specifications. Although the coefficient was not significant under Model 5 with the suicide rate as the outcome of interest, it is highly significant under the XPO Lasso model, which, again, is designed for inference in high-dimensional settings. This relationship is viewed as causal assuming the FEs adequately control for confounders.

The domestic violence laws – laws 20 ($p<0.05$), 29 ($p<0.05$), and 30 ($p<0.01$) – correspond to 0.795-point, 1.566-point, and 1.251-point decreases, respectively, in the suicide rate. Non-concealed-carry domestic violence laws largely correspond to declines in both the GDR and suicide rate, particularly under an XPO Lasso framework. Laws 20 and 30 are newly significant and also generally lack high correlation with other domestic violence laws, so the evidence of a strong relationship between these laws and the suicide rate is not as robust as the evidence for a strong relationship between law 29 and the suicide rate. The coefficient on law 29 ($p<0.01$) in the XPO Lasso GDR model corresponded to a 2.307-point decrease in the GDR – the estimated effect on the suicide rate is similar, thus lending confidence to both inferences.

The coefficients on laws 78, 91, and 97 are also significant under the XPO Lasso framework, which is strong evidence of a potential causal relationship with the suicide rate given the sophistication of the specification. The evidence is lacking in two principal regards, however, for reasons previously discussed (see **Section 6.2**). First, these laws only have significant coefficients in one model. Second, the laws are spread out across categories – if one of these laws had a causal relationship with the suicide rate, it seems plausible to expect more related laws to

[59] Law 19 asks: "*Does state law disqualify people from getting concealed carry permits based on other criminal history?*" See **Table 2.0.0** for a complete list of law descriptions.

have significant associations with the suicide rate. Nonetheless, it is entirely possible that, in truth, one or more of these laws has a causal relationship with the suicide rate, but more analysis is needed to prove a causal relationship.

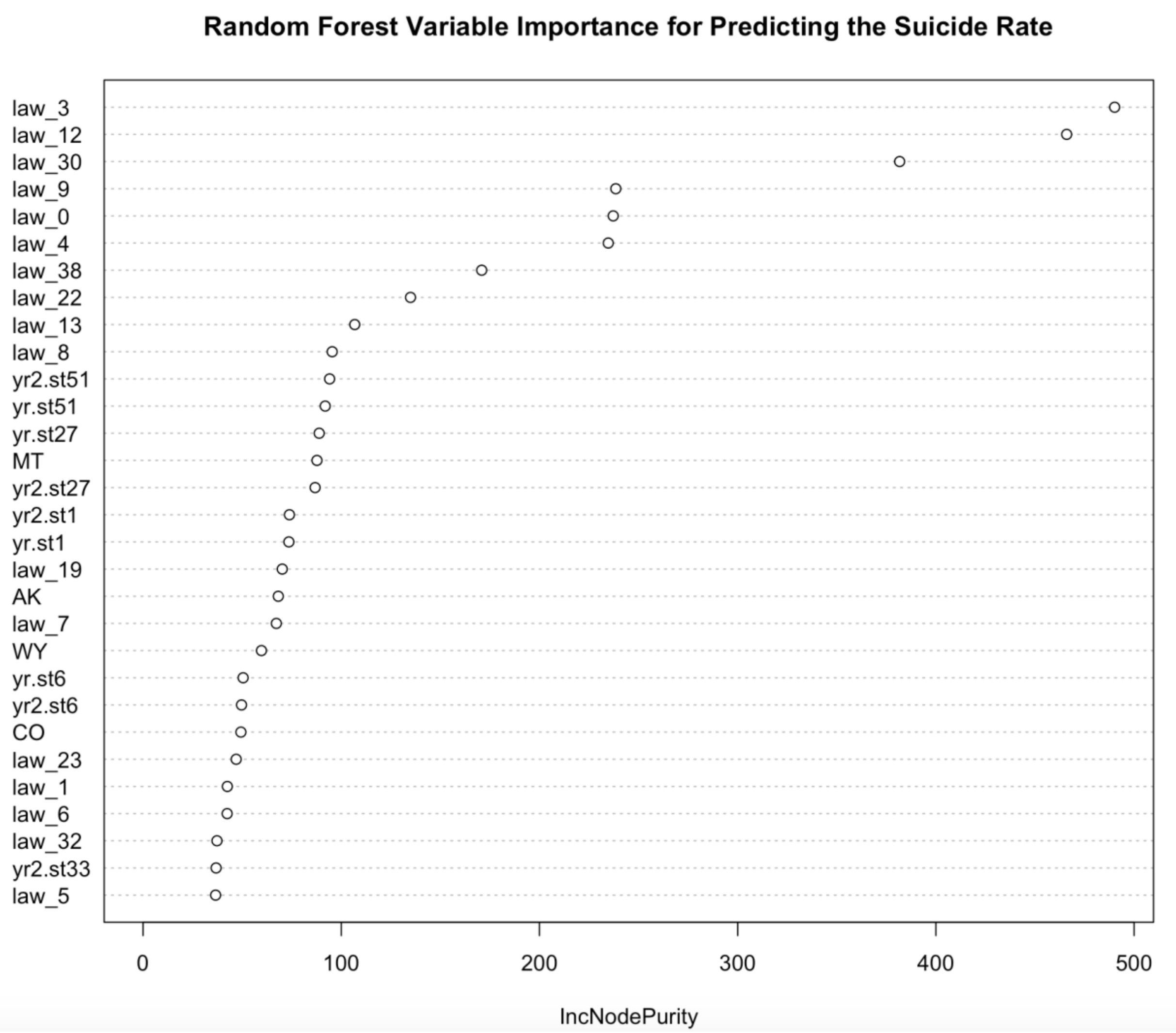
The inferences drawn assume that the state-fixed effects, national time trend, and state-specific time trends control for a substantial portion of the effects of confounding variables – if these assumptions are correct, the estimated relationships are causal. Nonetheless, this paper produces strong evidence that the passage of laws related to background checks, criminal history (partially), and domestic violence (especially) will correspond to declines in the suicide rate. Regularization techniques – dimension reduction, namely – increase model flexibility and thus out-of-sample (years following the 2001-2018 sample) application.

8.3 Suicides: Random Forests Methodology, Results, and Discussion

The interpretation of the Random Forests results are straightforward and mirror the results outlined in **Section 6.3**. First, features are ranked in descending order of importance, but here with respect to predicting changes in the suicide rate. The existence of broad similarities between selected features and law indicators with significant coefficients under the XPO Lasso specification (**Section 8.2**) will add support to both conclusions. Next, the model's estimates from the in-sample training data[60] are used to build a prediction of the suicide rate in oob observations (2014-2018). Again, comparing the set of estimated values of the suicide rate with the actual suicide rate will offer insight into the degree to which the suicide rate can be predicted by using primarily the selected features.

[60] Training data from 2001-2013.

The below table[61] illustrates variable importance to predicting the suicide rate with respect to total decrease in node impurity. Potential features include: the set of law indicators, *K;* the state-fixed effects, γ_t; the year-fixed effects, τ_i; the state-specific linear time trends, $(\tau_i * t)$; and the state-specific non-linear time trends, $(\tau_i * t^2)$. Variables are displayed in descending order of importance. (See **Table 8.3.1** for the total decrease in node impurities of each law feature).



As in **Section 6.3**, there is some discrepancy between the features deemed most important by RF and the covariates with the most-significant coefficients in the XPO Lasso regressions. For

[61] Only the 30 most important features displayed.

example, three laws consistently significant across FE Model 5 and the XPO Lasso model – laws 7, 8, and 29 – have total-decrease-in-node-impurities values of 67.2, 95.4, and 19.3, respectively. Law 3, on the other hand, does not have a statistically significant coefficient in either previous model, but is nonetheless selected as the most important predictor of the suicide rate by RF. A possible explanation is that law 3 has at least 75% correlation with the following background check laws: laws 0, 4, 5, 8, and 9. Each of these laws is selected as one of the 30 most-important features with respect to predicting the suicide rate, and, even more telling, each of these laws is selected as one of the eight most-important features with respect to predicting the GDR. The central conclusions broadly echo those from GDR analysis, in part due to the correlation between the suicide rate and the GDR.

Most importantly, every law feature in the above table relates to either background checks, criminal history, or domestic violence – a theme consistent across all methodologies and outcomes of interest. This is further evidence that policy makers should look first to background-check, criminal-history, or domestic-violence related laws if the goal is to lower the suicide rate.

Also, as was the case in the previous RF analysis, only two of the above features (law 32 and law 38) belong to the subset of domestic violence, concealed carry laws. The coefficients on concealed carry pairs[62] are inconsistent across FE and XPO Lasso specifications and within pair. The paper consequently concludes that causal inference on any one of the coefficients should be avoided. Again, it is interesting that RF does not split on these features to the extent that XPO Lasso outputted significant coefficients on the same covariates. Evidently, RF recognized a weakness in the relationship between these features and the suicide rate not realized by XPO Lasso.

[62] Laws 16/17, 33/34, 47/48, 49/50, and 68/69.

A key difference, however, is that 13 state or time-trend variables are listed in this set of important features (there were only three in the GDR RF output). This suggests that law indicators generally have more predictive power with respect to GDRs than to suicide rates. The features *yr.st51* (and *yr2.st51*) refer to Wyoming's state-specific linear (and non-linear) time trends;[63] *st27* is Montana; *st1* is Alaska; *st6* is Colorado; and *st33* is New Mexico. These states drove much of the change in the suicide rate.

This section also uses the RF results to predict an alternate metric of the suicide rate (for each state in years 2014-2018), as detailed in **Section 5.2.3**. To emphasize, the difference between the actual suicide rate and the predicted suicide rate will offer additional insight into the selected features' (see above figure) predictive power. Specifically, the mean of this difference (if significantly different from zero) will reveal the presence of bias, and the standard deviation of this difference will reveal the general accuracy of the alternate GDR metric. (See **Table 8.3.3** for a complete display of suicide rate predicted values on the oob sample).

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
diff_~sr	**255**	**-.018549**	**.0827607**	**1.321583**	**-.1815336**	**.1444356**

```
    mean = mean(diff_rf_sr)                                       t =  -0.2241
Ho: mean = 0                                     degrees of freedom =      254

    Ha: mean < 0                 Ha: mean != 0                 Ha: mean > 0
 Pr(T < t) = 0.4114         Pr(|T| > |t|) = 0.8228          Pr(T > t) = 0.5886
```

Given that the *p-value* in Pr.(|T| > |t|) > 0.05, the null hypothesis (that the mean value of -0.019 is not statistically significantly different from zero) is accepted, verifying that the RF-predicted suicide rate is unbiased. The standard deviation of 1.322 is relatively low and slightly lower than the standard deviation of 1.493 in the GDR prediction, which is telling given that the

[63] See **Figure 8.3.2** for a time-series of Wyoming's suicide rate.

suicide rate training sample is smaller. given the significant variation in values of the actual suicide rate across states and years. The suicide rate, like the GDR can be predicted accurately and without bias using mainly laws related to background checks, criminal history, and domestic violence.

9. Caveats and Directions of Future Research

This paper has a number of caveats (mentioned throughout) that transition into potential directions of future research. The foremost among them derives from Knight (2011), where the author considers the cross-state externalities associated with state gun control laws, notably discovering that illegal gun trafficking is responsive to state laws – the contraband flows from states with weak laws to those with strong laws, and trade significance is negatively related to the distance between the source and destination state. The critical insight here for this paper is that the passage of law k in state 1 may have less of an effect if the surrounding states do not follow law k. Therefore, the GDR or suicide rate in, say, New Jersey, is not only a function of its own gun control laws, but also whether the same laws are enforced in nearby states. Suppose law 5 (the "gun show loophole") is causally and negatively related to the GDR. If the gun show loophole is open in both New Jersey and Pennsylvania, the effect of closing it in New Jersey on the GDR of New Jersey would presumably be stronger if Pennsylvania also closed the loophole. This paper does not consider these cross-state effects. An area of future research could involve implementing Knight's insights into this paper's framework to see if results changed.

Furthermore, future research could consider differences in gun type and/or purchase vs. possession. (**Section 4.1** details this paper's decision to drop these conditions from analysis). Many of the gun control laws studied – in a subset of states – are enforced with respect to long-guns, but not handguns, and vice versa. Further, many states may prohibit possession, but not purchase, or vice versa. A state enforcing a law on any one of these conditions is (in the data) the same as if the enforced law applied to all conditions. Assuming law *k* has a statistically significant relationship with the GDR, the effect of enforcing law *k* with respect to all gun types, purchase, and possession would presumably be stronger than if it was enforced but applied to only the purchase of handguns. Further exploration could provide an answer.

ETR's sub-questions provide an additional direction of future research. Many of the studied 102 gun control laws have sub-questions. For example, law 24[64] has 23 sub-questions (most laws have far fewer). These sub-questions range from "*With whom can the person store the gun? (law enforcement, licensed dealer, third party)*" to "*Does state law apply not only to intimate partners under final domestic violence restraining orders, but also to abusive people restrained against their family members? (includes variations for every type of family member).*" An exhaustive analysis of the sub-questions of consistently significant laws (like laws 8, 19, 24, and 29) would lend valuable insight into the associations established in this paper.

As discussed at length in **Section 6** and **Section 8**, laws within categories, but particularly background check laws, are highly correlated. Consequently, isolating inference is difficult if multiple of these highly correlated laws (notably laws 0, 4, 5, 8, and 9) have similar and significant coefficients. Future research could include methods of isolating the effects of highly correlated laws to see if specific laws within the background checks category predict changes in GDR better than others.

10. Conclusion

[64] Law 24 asks: "*Does state law require all people under final domestic restraining orders to turn in their firearms when they become prohibited from having them?*"

The literature establishes statistically significant associations between specific gun control laws (or narrow sets of guns laws) and declines in the GDR or suicide rate. It is also confirmed that gun exposure (overall possession, concealed-carry laws, stand-your-ground laws, etc.) are positively related to outcomes of interest. This paper aims to study similar relationships, but in the context of all laws with no prior assumptions about which laws will matter most. Thus, a comprehensive and balanced panel dataset containing measures of the GDR, the suicide rate,[65] and the set of 102 gun control law indicators – for each state plus D.C. from 1991-2018 – has been compiled. For both outcomes of interest, this paper utilizes a fixed-effects specification that clusters standard errors at the state level and incorporates state-fixed effects, a national time trend, and state-specific linear time trends. The passage of laws related to background checks, criminal history, and domestic violence is significantly associated with declines in the GDR and suicide rate. Given the high-dimensional nature of the data, regularization (in the form of dimension reduction) is needed to draw meaningful inferences. Therefore, a variant of Lasso, the cross-fit partialing-out algorithm, is implemented to achieve this end. Specifically, 102 distinct models are run (each with a different law indicator as the covariate of interest), where the Lasso selects controls from the remaining law indicators, the state-fixed effects, and the year-fixed effects (the state-specific time trend is forced into the model). Modeling the relationship between the outcome of interest and the indicator of interest with fewer total covariates decreases the in-sample accuracy of the estimated coefficients but increases flexibility and thus confidence in out-of-sample inferences. With minor variation in specific results between GDR and suicide rate XPO Lasso models, the FE results are largely confirmed. Finally, the RF algorithm is employed in a prediction-minded approach to analyzing the relationship between the GDR (or suicide rate) and law indicators. The same results stand. This

[65] Observed only from 2001-2018.

study contributes to the literature by establishing a significant relationship between declines in the GDR (and suicide rate) and the passage of gun control laws pertaining to background checks, criminal history, and domestic violence. If state-fixed effects, the national time trend, and state-specific time trends control for a considerable portion of confounding effects, the estimated relationships may be taken as causal.

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B. Appendix: Supporting Figures and Tables

Figure 1.0.1:

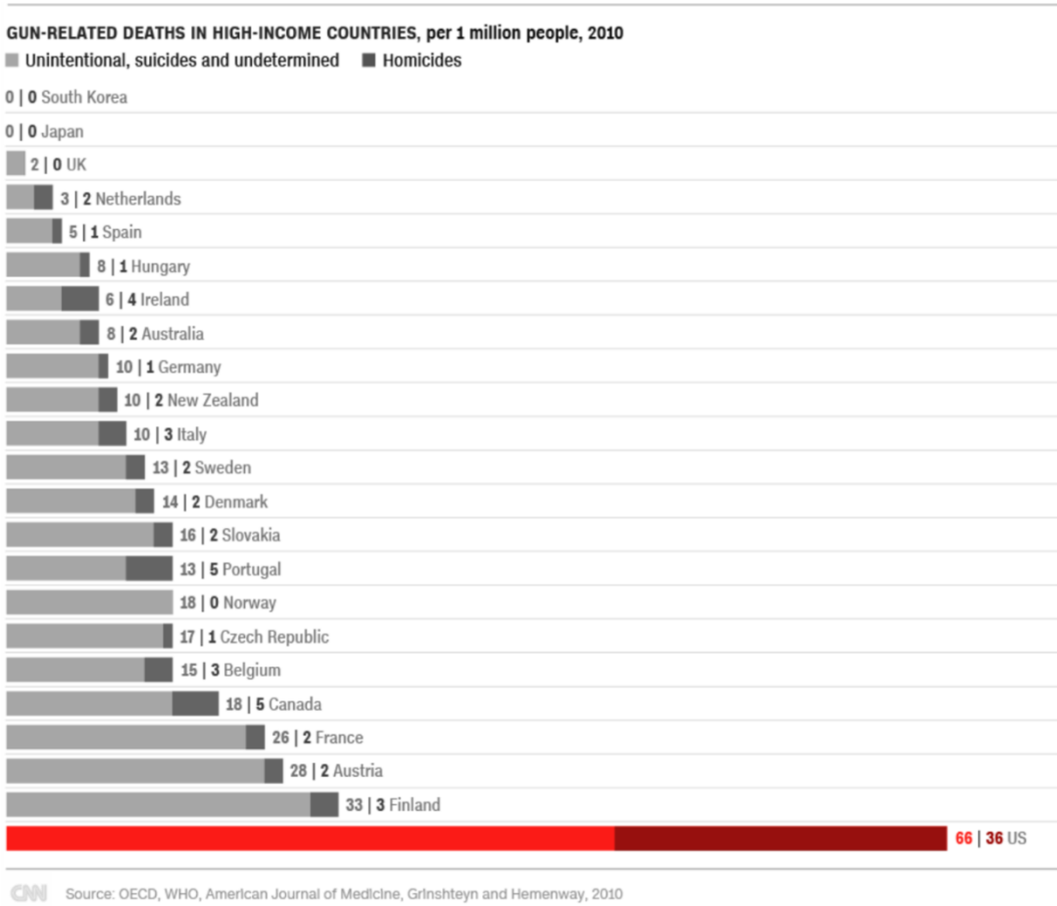


Figure 1.0.2:

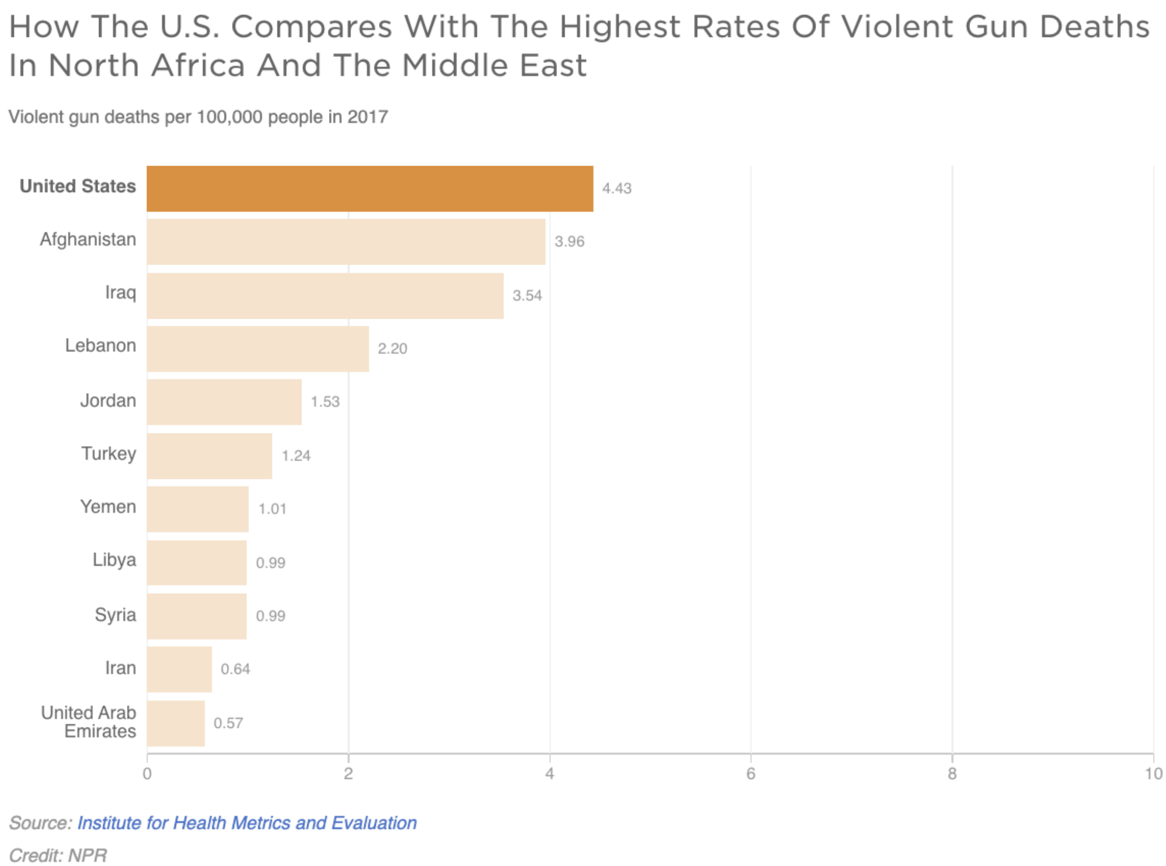


Figure 1.0.3:

US Gun Death Rate (per 100,000 residents) over time. Rate is age-adjusted.

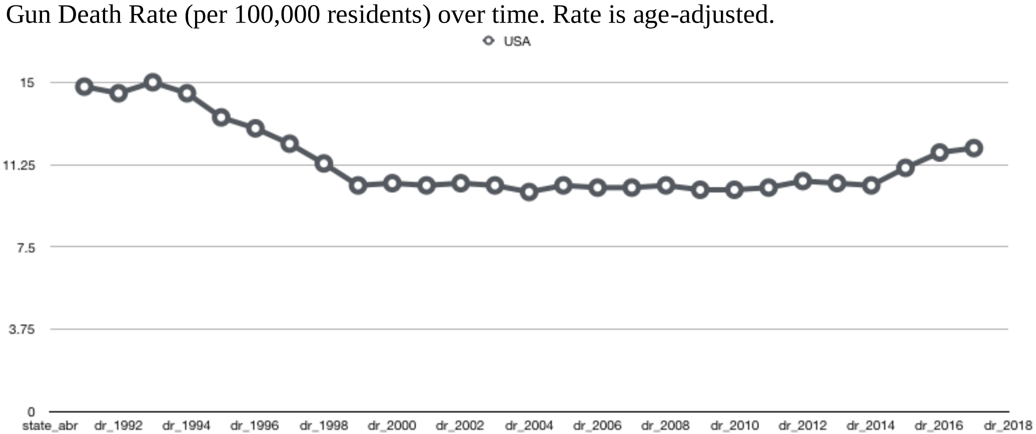


Figure 1.0.4:

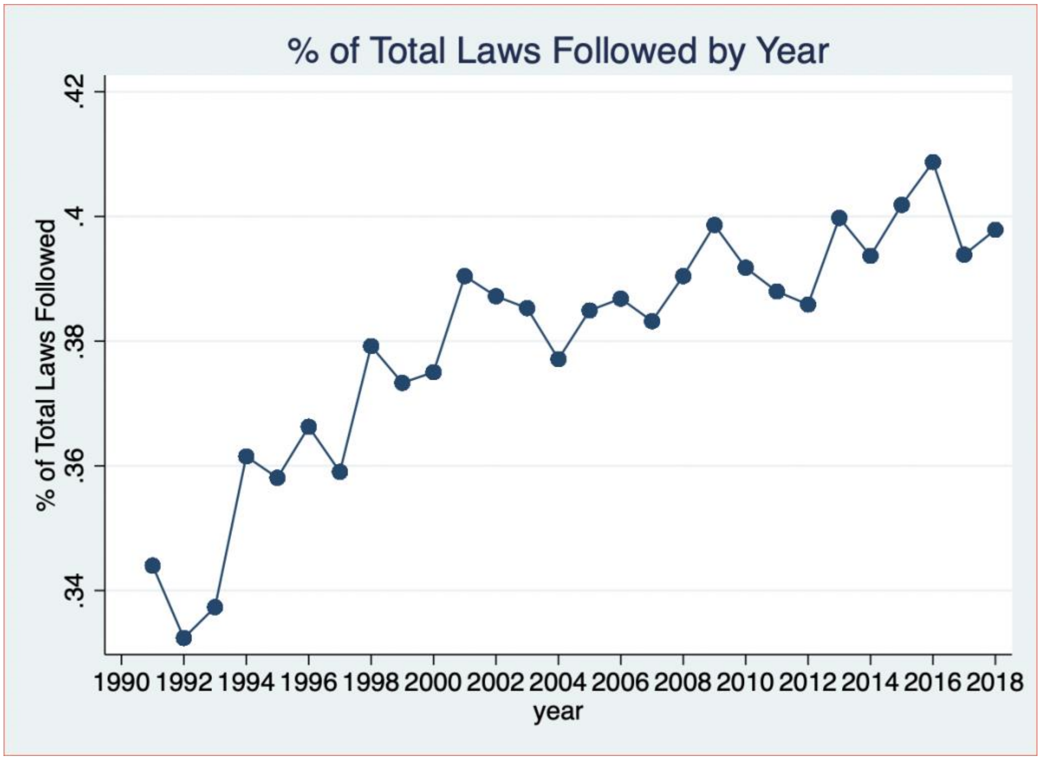


Figure 1.0.4b: US Suicide Rate Over Time (per 100,000 residents)

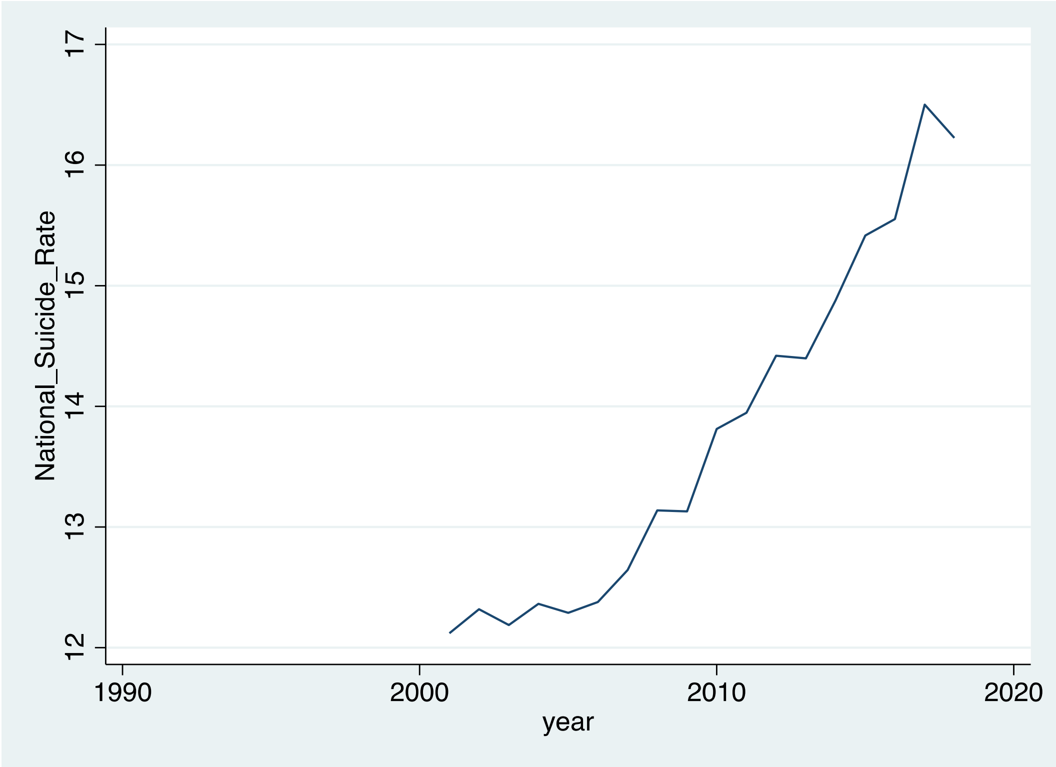
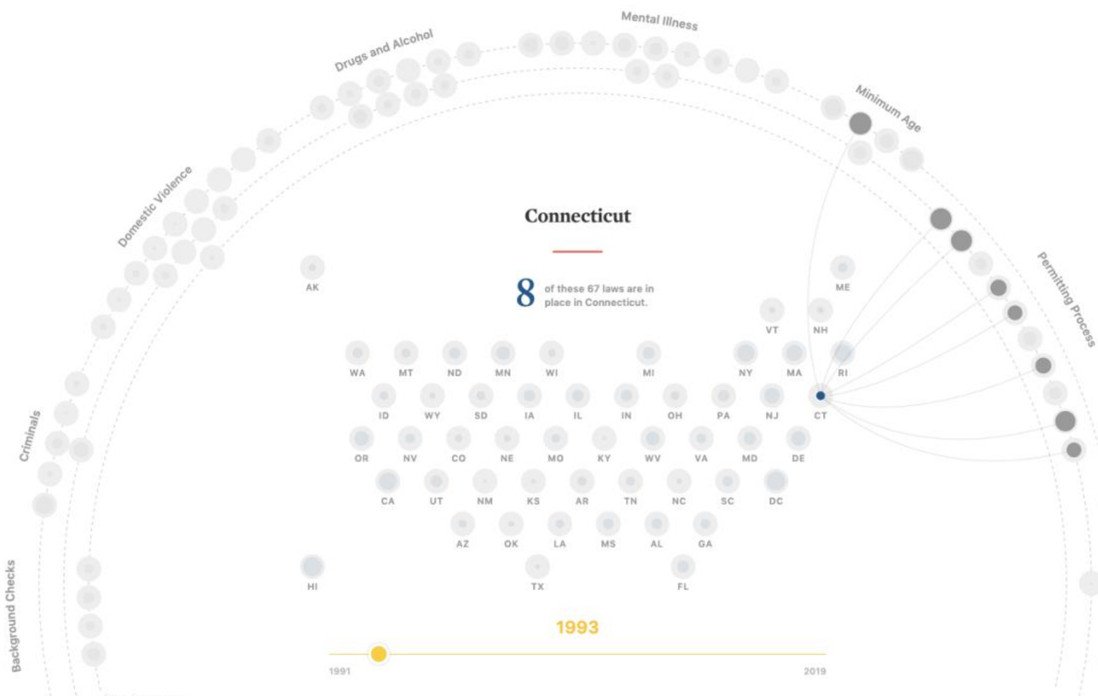


Figure 1.0.5a:



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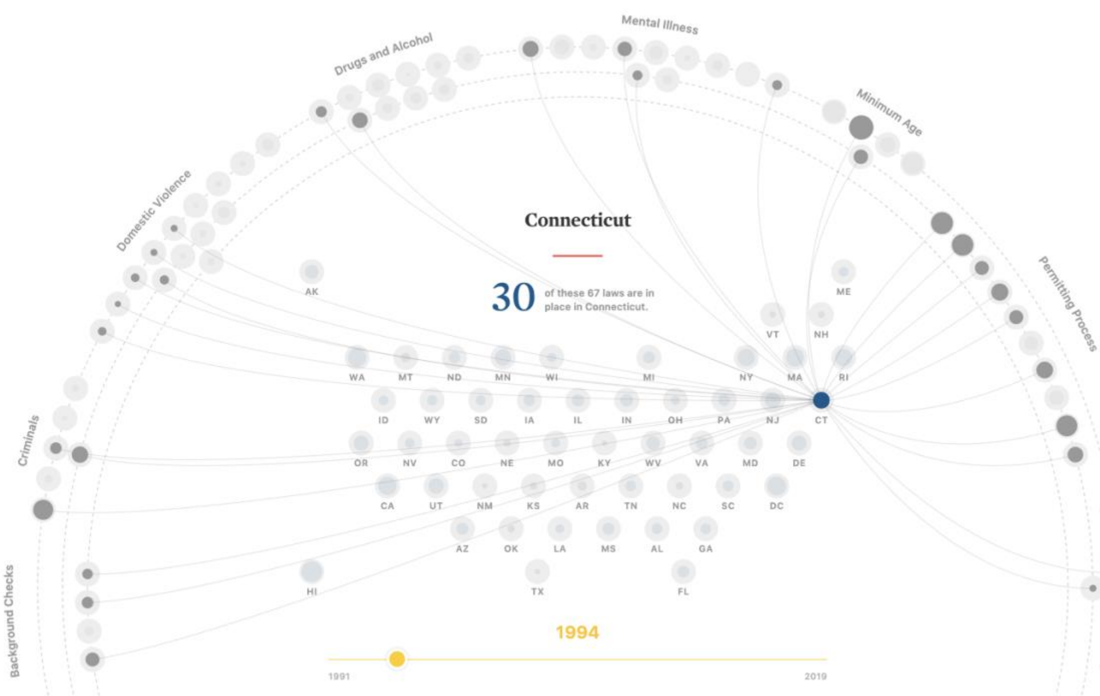

Figure 1.0.6a:

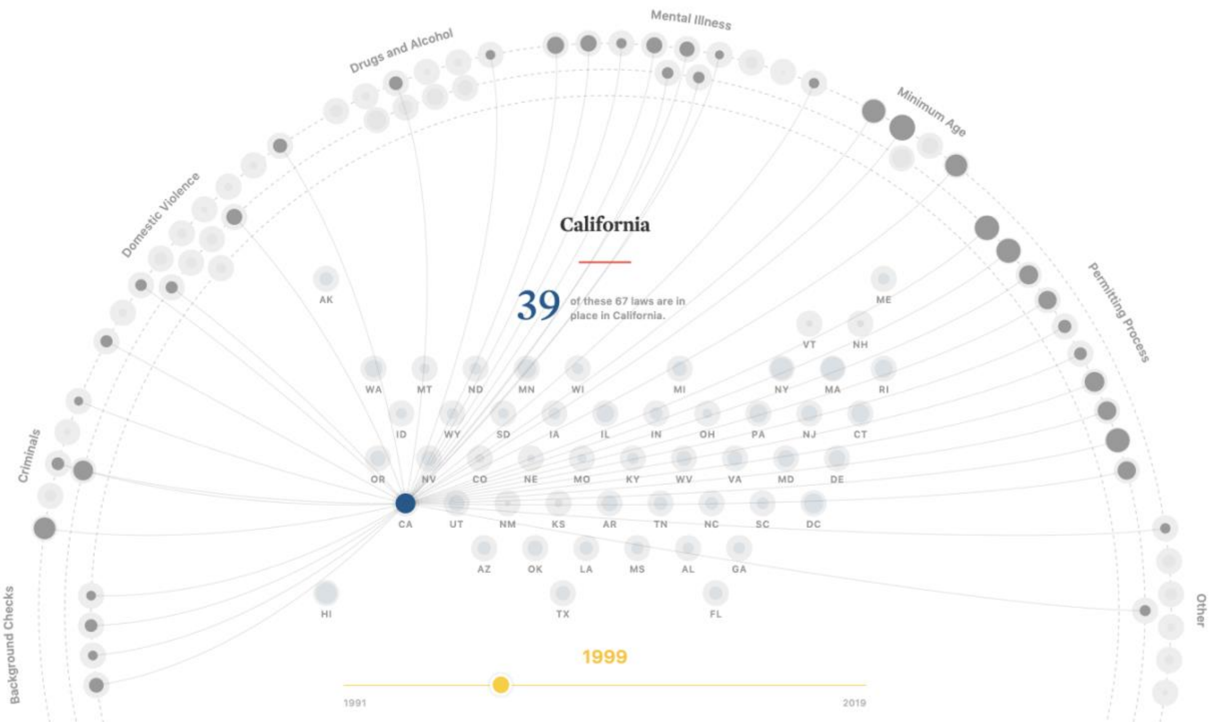

Figure 2.0.6b:

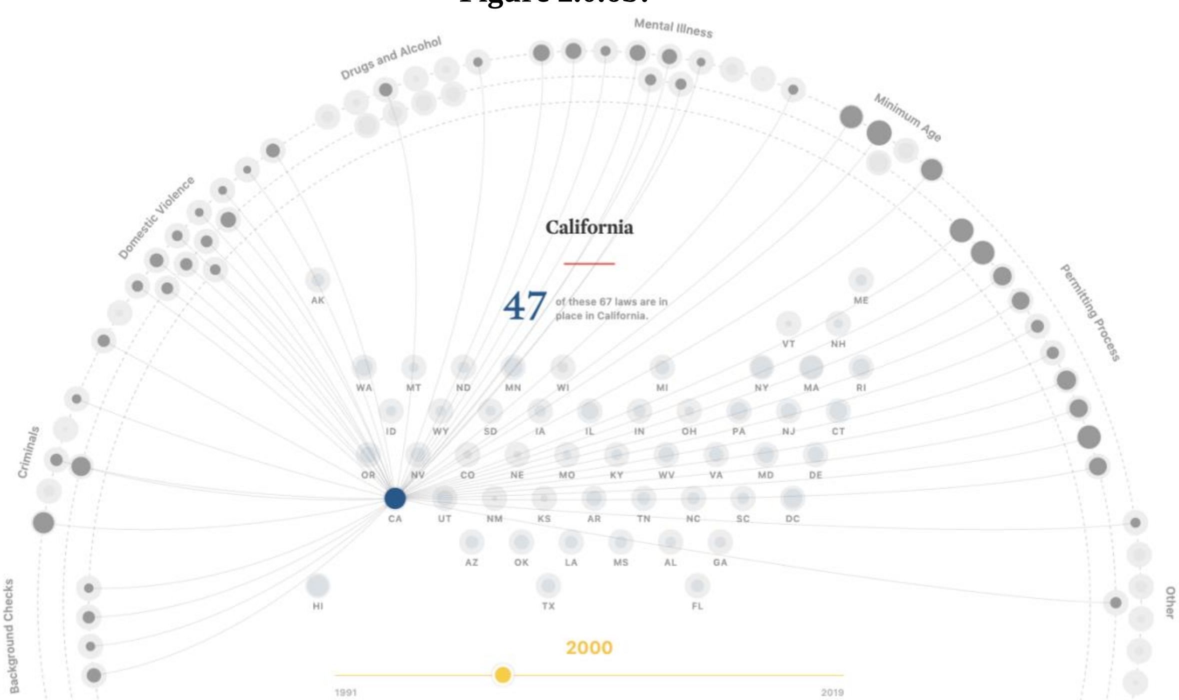

Table 2.0.0: Gun Law Description Table

law_code	law_category	law_classication	law_description	Notes
0	background_checks	other	*Does state law require criminal background checks for gun sales by unlicensed sellers?*	

1	background_checks	other	*Is a criminal background check required for the sale of all firearms (as opposed to only the sale of all handguns)?*	
2	background_checks	other	*Does the background check take place at the point of sale?*	The question on ETR was split up into two, this question and the following.
3	background_checks	other	*Does the background check take place in connection with a purchase permit required to acquire a gun?*	
4	background_checks	other	*Do any exceptions apply to the background check requirement?*	
5	background_checks	other	*Are background checks required for all sales by unlicensed sellers doing business at gun shows?*	ETR had a condition for states that don't require background checks for all sales by unlicensed sellers. I simply ask the question and include states that require background checks for all sales by unlicensed sellers. (These states were N/A but here they are "yes" = 1).
6	background_checks	other	*Is any record of the sale required to be kept?*	
7	background_checks	other	*Must the parties wait a certain period of time after the background check before completing the firearm sale?*	
8	background_checks	other	*Are there penalties for a buyer who fails to follow the background check law?*	
9	background_checks	other	*Are there penalties for a seller who fails to follow with the background check law?*	
10	criminals	possession	*Does state law generally prohibit all people convicted of felonies from having firearms?*	
11				*removed
12	criminals	possession	*Does the state prohibit people under felony indictment from having firearms?*	
13	criminals	possession	*Other than domestic violence offenders, does the state prohibit any people convicted of misdemeanors from having firearms?*	
14	criminals	possession	*Does the state prohibit people under misdemeanor indictment from having firearms?*	
15	criminals	possession	*Does state law prohibit any people from having firearms based on other criminal history?*	

16	criminals	concealed_carry	*Other than domestic violence offenders, does state law disqualify any people convicted of misdemeanors from carrying concealed guns in public?*	Given nature of the question, “yes” and “N/A” = 1. (up until this point, “no” and “N/A” have been group together — =0.
17	criminals	concealed_carry	*Other than domestic violence offenders, does state law disqualify any people convicted of misdemeanors from getting concealed carry permits?*	Given nature of the question, “yes” and “N/A” = 1. (up until this point, “no” and “N/A” have been group together — =0.
18	criminals	concealed_carry	*In states that do not disqualify people from getting concealed carry permits based on misdemeanor convictions that are not domestic violence-related, can law enforcement opt to deny concealed carry permits to these offenders?*	Given nature of the question, “yes” and “N/A” = 1. In other words, the question could state: “In addition to states where concealed carry is restricted, which additional states face similar, yet unofficial, restrictions?”
19	criminals	concealed_carry	*Does state law disqualify people from getting concealed carry permits based on other criminal history?*	Given nature of the question, “yes” and “N/A” = 1. This question shows the standard to obtain a concealed carry permit in each state -- regardless of whether the given state requires a person to obtain the permit to carry in public.
20	domestic_violence	possession	*Does state law prohibit people who have been convicted of domestic violence misdemeanors from having firearms?*	
21	domestic_violence	possession	*Does state law require abusers convicted of domestic violence misdemeanors to turn in their firearms when they become prohibited from having them?*	

22	domestic_violence	possession	*Does state law prohibit all people under final domestic violence restraining orders from having firearms?*	
23	domestic_violence	possession	*In states that do not prohibit firearm possession by all people under final domestic violence restraining orders, does state law explicitly allow judges, at their discretion, to prohibit people under these orders?*	
24	domestic_violence	possession	*Does state law require all people under final domestic violence restraining orders to turn in their firearms when they become prohibited from having them?*	
25	domestic_violence	possession	*In states that do not require all people under final domestic violence restraining orders to turn in firearms, does state law explicitly allow judges, at their discretion, to order people under these orders to turn in firearms?*	
26	domestic_violence	possession	*Does state law prohibit all people under temporary domestic violence restraining orders from having firearms?*	
27	domestic_violence	possession	*In states that do not prohibit firearm possession by all people under temporary domestic violence restraining orders, does state law explicitly allow judges, at their discretion, to prohibit people under these orders?*	
28	domestic_violence	possession	*Does state law require all people under temporary domestic violence restraining orders to turn in their firearms when they become prohibited from having them?*	
29	domestic_violence	possession	*In states that do not require all people under temporary domestic violence restraining orders to turn in firearms, does state law explicitly allow judges, at their discretion, to order these people to turn in firearms?*	
30	domestic_violence	possession	*Are all convicted stalkers in the state prohibited from having firearms?*	
31	domestic_violence	concealed_carry	*Does state law disqualify people with convictions for abusing their boyfriends and girlfriends from carrying concealed guns in public?*	Given nature of the question, “yes” and “N/A” = 1. (up until this point, “no” and “N/A” have been group together — =0. Question is concerned with disqualification. Yes means they are disqualified and N/A means that the state does not allow the carry of concealed handguns or issue permits.

32	domestic_violence	concealed_carry	*Does state law disqualify people with convictions for abusing their boyfriends and girlfriends from getting concealed carry permits?*	Id.
33	domestic_violence	concealed_carry	*Does state law disqualify boyfriends and girlfriends under final domestic violence restraining orders from carrying concealed guns in public?*	Id.
34	domestic_violence	concealed_carry	*Does state law disqualify boyfriends and girlfriends under final domestic violence restraining orders from getting concealed carry permits?*	Id.
35	domestic_violence	concealed_carry	*Does state law disqualify people under temporary domestic violence restraining orders from carrying concealed guns in public?*	Id.
36	domestic_violence	concealed_carry	*Does state law disqualify people under temporary domestic violence restraining orders from getting concealed carry permits?*	Id.
37	domestic_violence	concealed_carry	*Are all convicted stalkers disqualified from carrying concealed guns in public?*	Id.
38	domestic_violence	concealed_carry	*Are all convicted stalkers disqualified from getting concealed carry permits?*	Id.
39	domestic_violence	other	*Does state law require law enforcement to remove firearms from the scene of a domestic violence incident?*	
40	domestic_violence	other	*In states that do not require law enforcement to remove firearms from the scene of a domestic violence incident, does state law explicitly allow law enforcement to do so?*	
41	drugs_alcohol	possession	*Does state law prohibit any people convicted of drug-related misdemeanors from having firearms?*	
42	drugs_alcohol	possession	*Does state law prohibit firearm possession based on recent treatment for drug-related reasons?*	
43	drugs_alcohol	possession	*Does state law prohibit any other categories of unlawful drug users from having firearms?*	
44	drugs_alcohol	possession	*Does state law prohibit any people convicted of alcohol-related misdemeanors from having firearms?*	
45	drugs_alcohol	possession	*Does state law prohibit firearm possession based on recent treatment for alcohol-related reasons?*	
46	drugs_alcohol	possession	*Does the state prohibit firearm possession based on any other reason related to alcohol abuse?*	
47	drugs_alcohol	concealed_carry	*Does state law disqualify any people convicted of drug-related misdemeanors from carrying concealed guns in public?*	Id.
48	drugs_alcohol	concealed_carry	*Does state law disqualify any people convicted of drug-related misdemeanors from getting concealed carry permits?*	Id.
49	drugs_alcohol	concealed_carry	*Does state law disqualify any people recently treated for drug-related reasons from carrying concealed guns in public?*	Id.
50	drugs_alcohol	concealed_carry	*Does state law disqualify any people recently treated for drug-related reasons from getting concealed carry permits?*	Id.

51	drugs_alcohol	concealed_carry	*Does state law disqualify any other categories of unlawful drug users from getting concealed carry permits?*	Id.
52	drugs_alcohol	concealed_carry	*Does state law disqualify any people convicted of alcohol-related misdemeanors from carrying concealed guns in public?*	Id.
53	drugs_alcohol	concealed_carry	*Does state law disqualify any people convicted of alcohol-related misdemeanors from getting concealed carry permits?*	Id.
54	drugs_alcohol	concealed_carry	*Does state law disqualify any people recently treated for alcohol-related reasons from carrying concealed guns in public?*	Id.
55	drugs_alcohol	concealed_carry	*Does state law disqualify any people recently treated for alcohol-related reasons from getting concealed carry permits?*	Id.
56	drugs_alcohol	concealed_carry	*Does state law disqualify people from getting concealed carry permits based on any other reason related to alcohol abuse?*	Id.
57	mental_illness	possession	*Does state law prohibit people who have been involuntarily committed to a psychiatric hospital from having firearms?*	
58	mental_illness	possession	*Does state law prohibit firearm possession by people who are found to be a danger to themselves or others?*	
59	mental_illness	possession	*Does state law prohibit people from having firearms because they are found incapable of managing their affairs due to mental illness?*	
60	mental_illness	possession	*Does state law prohibit people found not guilty of a crime by reason of insanity from having firearms?*	
61	mental_illness	possession	*Does state law prohibit people found incompetent to stand trial from having firearms?*	
62	mental_illness	possession	*Does state law prohibit people who have had a guardian appointed because of mental illness from having firearms?*	
63	mental_illness	possession	*Does state law prohibit people who have been involuntarily committed to outpatient treatment from having firearms?*	
64	mental_illness	possession	*Does state law empower immediate family members or law enforcement to petition a court for temporary removal of guns from a person who poses a danger to self or others (sometimes called an "extreme risk protection order")?*	
65	mental_illness	possession	*Does state law prohibit people from having firearms for any other reason related to mental illness?*	
66	mental_illness	concealed_carry	*Does state law disqualify people who have been committed to a psychiatric hospital for emergency care from carrying concealed guns in public?*	Id.
67	mental_illness	concealed_carry	*Does state law disqualify people who have been committed to a psychiatric hospital for emergency care from getting concealed carry permits?*	Id.

68	mental_illness	concealed_carry	*Does state law disqualify people who have been voluntarily committed from carrying concealed guns in public?*	Id.
69	mental_illness	concealed_carry	*Does state law disqualify people who have been voluntarily committed from getting concealed carry permits?*	Id.
70	mental_illness	concealed_carry	*Does state law disqualify people from getting concealed carry permits for any other reason related to mental illness?*	Id.
71	minimum_age	possession	*Does the state have a minimum age requirement for possessing a handgun?*	
72	minimum_age	possession	*Does the state have a minimum age requirement for purchasing a handgun from a federally licensed dealer?*	
73	minimum_age	possession	*Does the state have a minimum age requirement for possessing a rifle or shotgun?*	
74	minimum_age	possession	*Does the state have a minimum age requirement for purchasing a rifle or shotgun from a federally licensed dealer?*	
75	minimum_age	concealed_carry	*What is the minimum age to carry a concealed gun in public?*	Is the minimum age >= 21 or is there no concealed carry?
76	minimum_age	concealed_carry	*What is the minimum age to get a concealed carry permit?*	Id.
77	permitting_process	concealed_carry	*Does the state allow the concealed carry of handguns in public?*	These are a different set of concealed carry laws — they do not ask about disqualification — where an answer of N/A will be classified as no unless otherwise stated
78	permitting_process	concealed_carry	*Does the state require a permit in order to carry a concealed handgun in public?*	
79	permitting_process	concealed_carry	*Does the state require firearm training in order to carry concealed guns in public?*	
80	permitting_process	concealed_carry	*Does the state require firearm training in order to get concealed carry permits?*	
81	permitting_process	concealed_carry	*Does law enforcement have the authority to prohibit people from carrying a concealed gun on the basis that they pose a danger or threaten public safety?*	n/a = yes
82	permitting_process	concealed_carry	*Does law enforcement have the authority to deny permits to people on the basis that they pose a danger or threaten public safety?*	n/a = yes
83	permitting_process	concealed_carry	*Does law enforcement otherwise have the authority to prohibit a person from carrying a concealed gun at their discretion?*	n/a = yes
84	permitting_process	concealed_carry	*Does law enforcement otherwise have the authority to deny a permit at their discretion?*	n/a = yes
85	permitting_process	concealed_carry	*Does the state law require a person to show good cause or a special need in order to carry a concealed gun in public?*	

86	permitting_process	concealed_carry	*Does the state law require a person to show good cause or a special need in order to get a concealed carry permit?*	
87	permitting_process	concealed_carry	*Does the state require people to pass a background check in order to get concealed carry permits (in states that require a permit to carry a concealed handgun in public)?*	
88	permitting_process	concealed_carry	*Must applicants be state residents in order to get concealed carry permits (In states that require a permit to carry a concealed handgun in public)?*	
89	permitting_process	concealed_carry	*Does the state require revocation of concealed carry permits under certain circumstances (in states that require a permit to carry a concealed handgun in public)?*	
90	permitting_process	concealed_carry	*How long does a concealed carry permit last before it must be renewed (in states that require a permit to carry a concealed handgun in public)?*	Does a concealed carry permit last fewer than 5 years? (>= 5 years or n/a means 0)
91	permitting_process	concealed_carry	*Does the state require concealed carry permit holders to apply for renewal in order for permits to be renewed (in states that require a permit to carry a concealed handgun in public)?*	
92	permitting_process	concealed_carry	*Does the state require a new background check to be conducted on the person during the renewal process (In states that require a permit to carry a concealed handgun in public)?*	
93	miscellaneous	possession	*Does state law empower immediate family members or law enforcement to petition a court for temporary removal of guns from a person who poses a danger to self or others (sometimes called an "extreme risk protection order")?*	
94	miscellaneous	possession	*Does state law prohibit people under restraining orders that are not domestic violence-related from possessing firearms?*	
95	miscellaneous	possession	*Does state law prohibit fugitives from having firearms?*	
96	miscellaneous	possession	*Does state law prohibit any people from having firearms based on immigration status?*	
97	miscellaneous	possession	*Does state law prohibit people who have renounced their United States citizenship from having firearms?*	
98	miscellaneous	possession	*Does state law prohibit people from having firearms based on certain types of discharge from the United States military?*	
99	miscellaneous	possession	*Other than for criminal history, mental illness, drug and alcohol abuse, and restraining orders, does state law prohibit any other people from having firearms?*	
100	miscellaneous	concealed_carry	*Does state law disqualify people under restraining orders that are not domestic violence-related from carrying concealed guns in public?*	Disqualification. n/a = yes

101	miscellaneous	concealed_carry	*Does state law disqualify people under restraining orders that are not domestic violence-related from getting concealed carry permits?*	Id.
102	miscellaneous	concealed_carry	*Other than for criminal history, mental illness, drug and alcohol abuse, and restraining orders, does state law disqualify any other people from getting concealed carry permits?*	Id.

Figure 2.0.1: law_0 time-series (Background Checks)

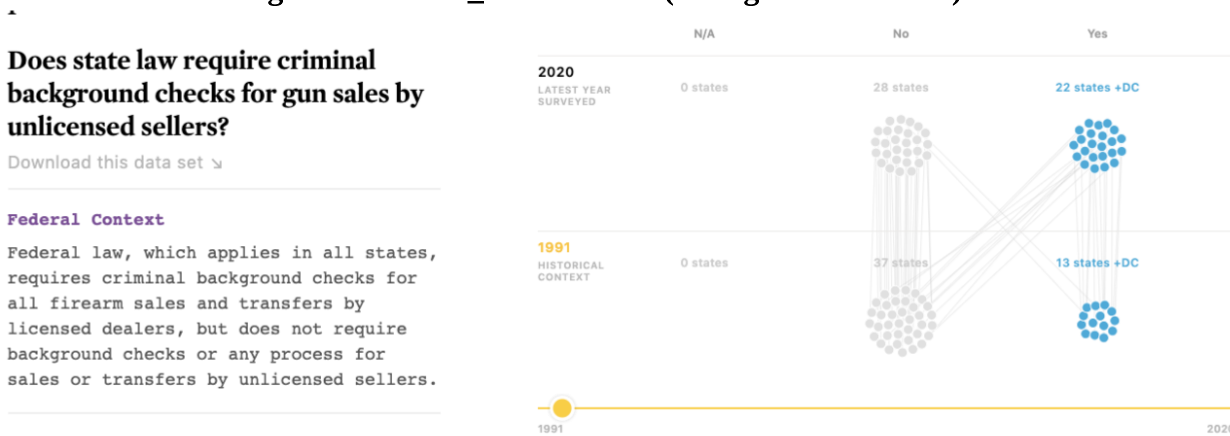

Figure 2.0.2: law_19 time-series (Criminal History)

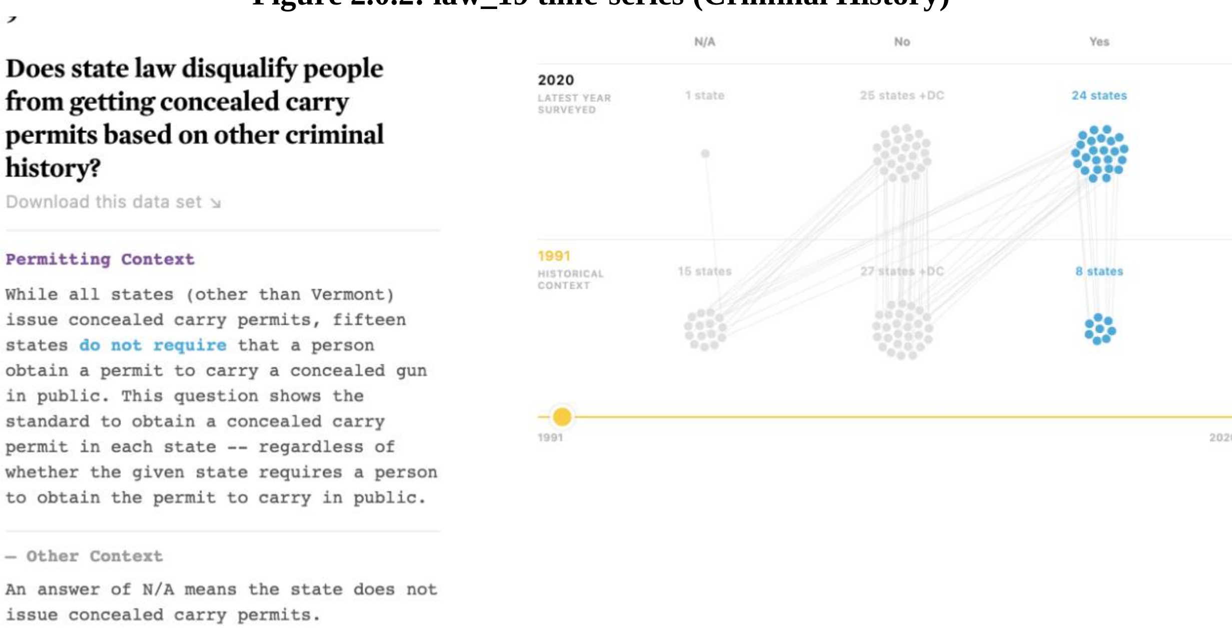

Figure 2.0.3: law_24 time-series (Domestic Violence)

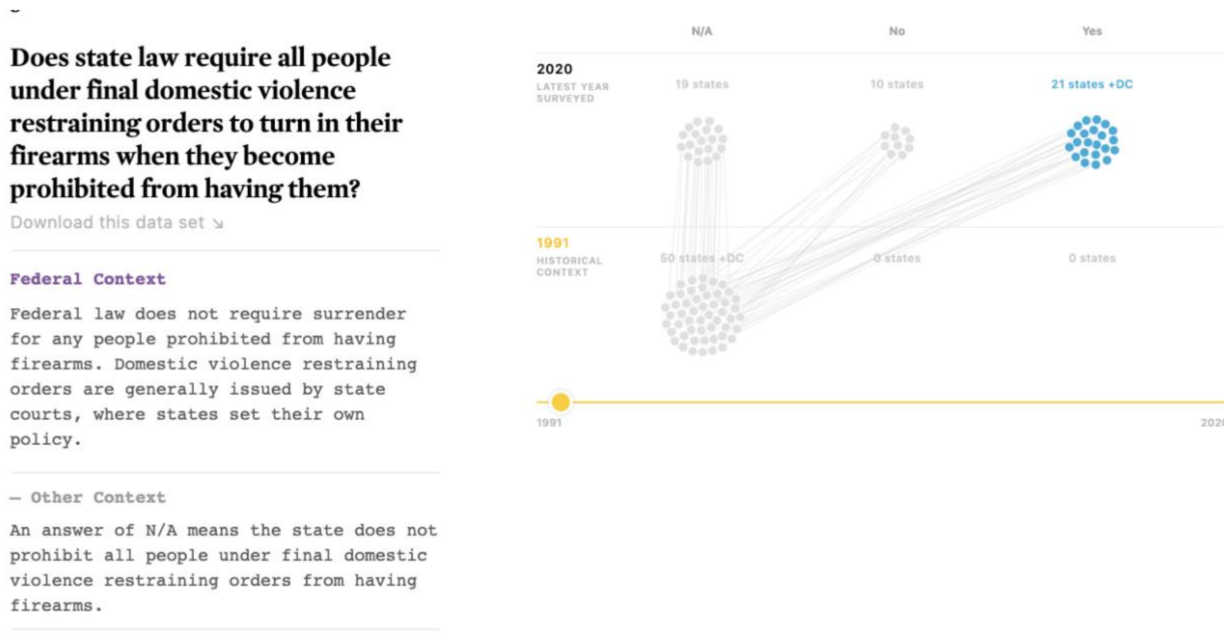

Figure 2.0.4: law_41 time-series (Drugs and Alcohol)

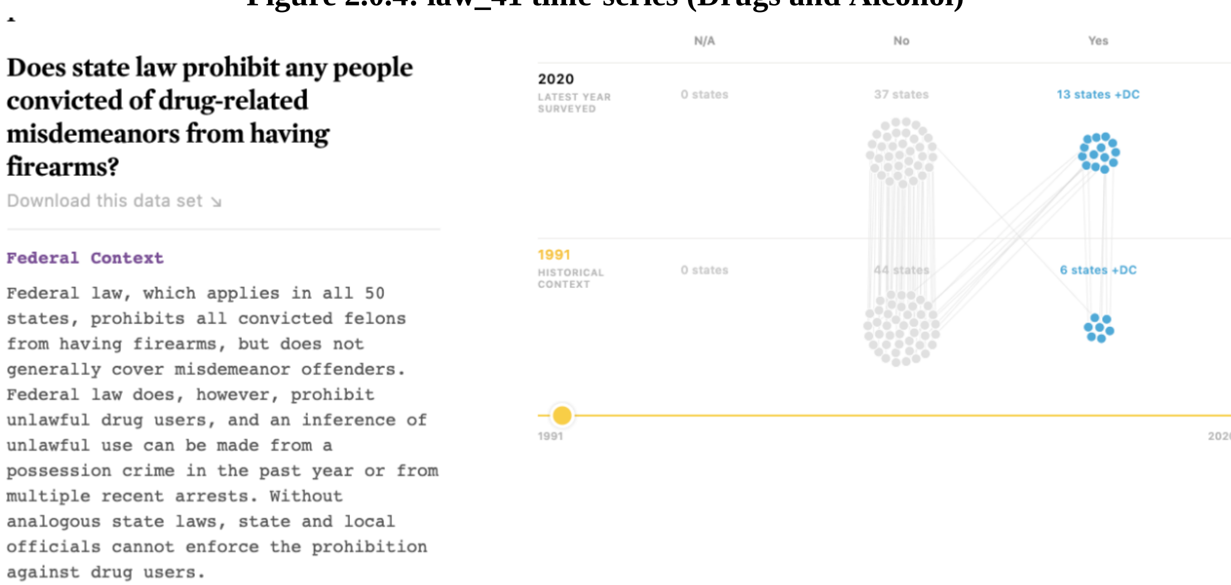

Figure 2.0.5: law_57 time-series (Mental Illness)

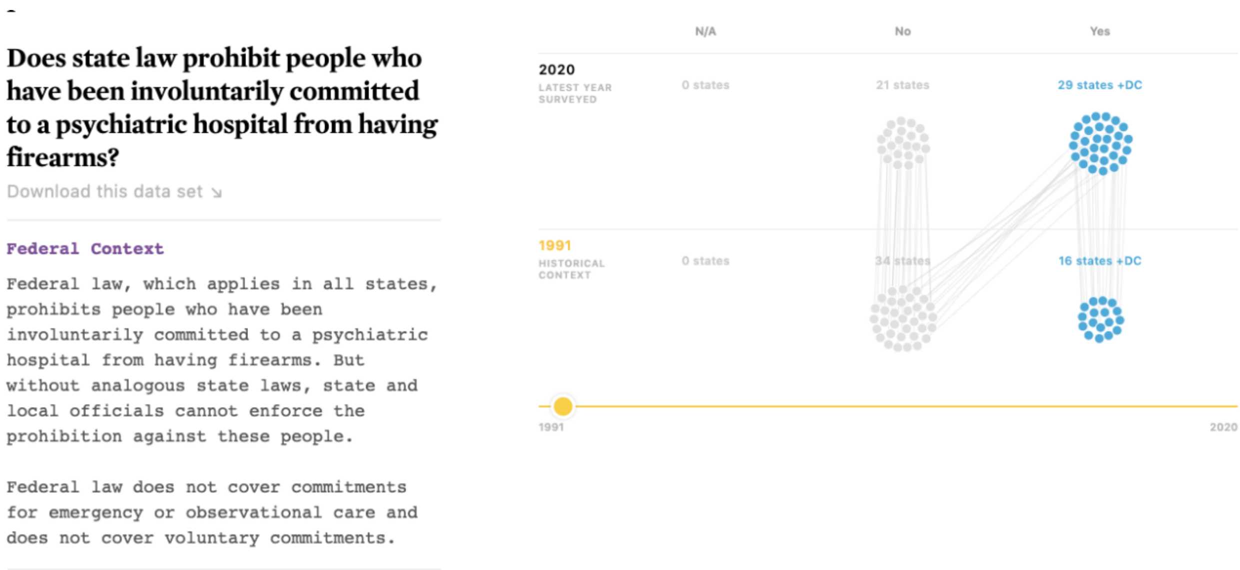

Figure 2.0.6: law_71 time-series (Minimum Age)

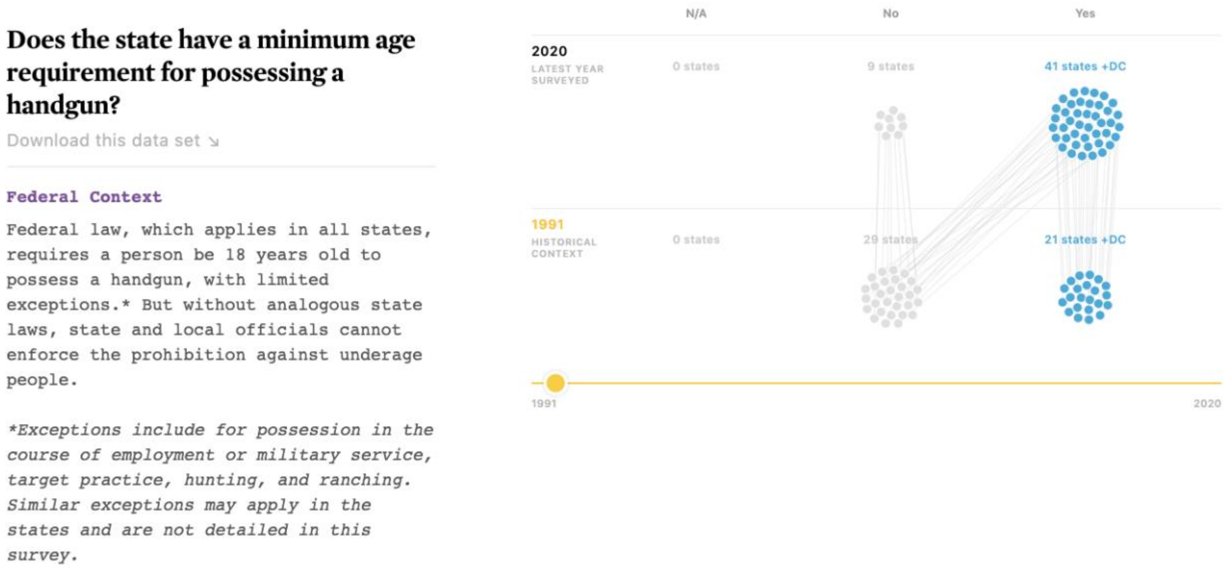

Figure 2.0.7: law_77 time-series (Permitting Process)

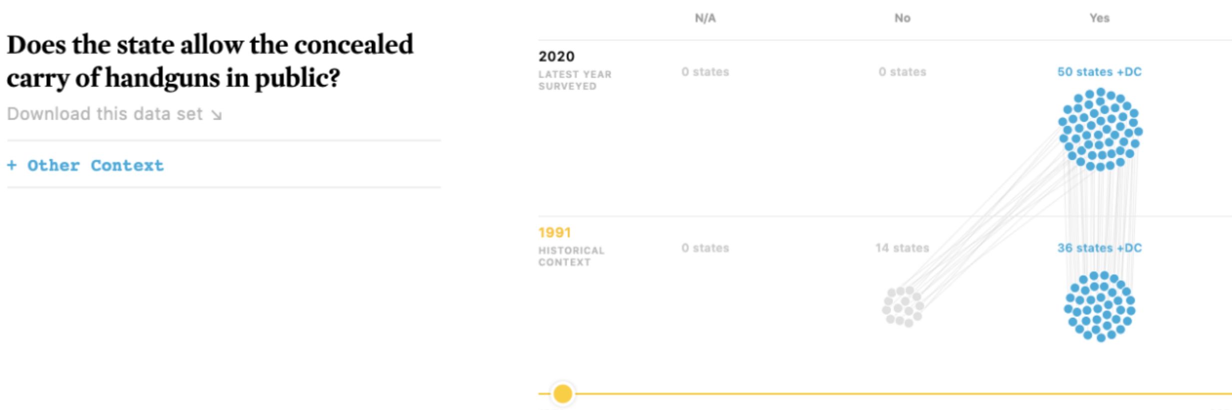

Figure 2.0.8: law_93 time-series (Miscellaneous – in this case, “Red-flag” Laws)

Does state law empower immediate family members or law enforcement to petition a court for temporary removal of guns from a person who poses a danger to self or others (sometimes called an "extreme risk protection order")?

Download this data set ↘

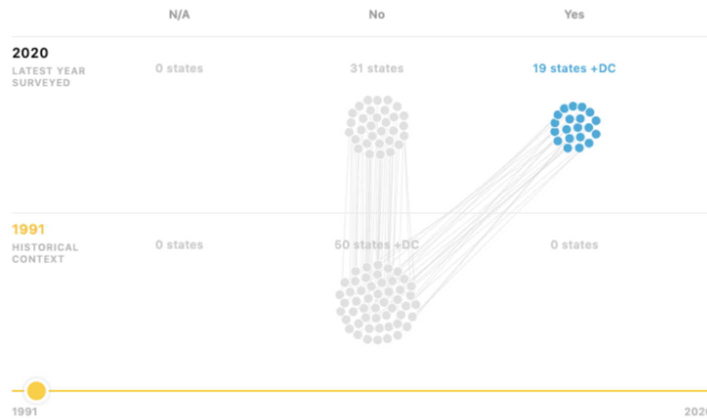

Table 4.1.1: Correlation Matrix of Criminal History Laws

|	law_10	law_12	law_13	law_14	law_15	law_16	law_17	law_18	law_19
law_10 |	1.0000								
law_12 |	0.2647	1.0000							
law_13 |	0.1288	0.2912	1.0000						
law_14 |	0.1316	0.3574	0.1046	1.0000					
law_15 |	0.1755	0.2134	0.5054	0.1883	1.0000				
law_16 |	0.1566	0.2392	0.3895	0.0944	0.2709	1.0000			
law_17 |	0.1346	0.2065	0.3600	0.0830	0.2495	**0.9141**^	1.0000		
law_18 |	0.1581	0.1345	0.2991	0.0582	0.2050	**0.7568***	**0.8309***	1.0000	
law_19 |	0.0381	0.2004	-0.0619	-0.1959	-0.0369	0.3120	0.3428	0.2209	1.0000

Table 4.1.2: Correlation Matrix of Domestic Violence Laws

|	law_20	law_21	law_22	law_23	law_24	law_25	law_26	law_27
law_20 |	1.0000							
law_21 |	**0.5818***	1.0000						
law_22 |	0.6375	0.3349	1.0000					
law_23 |	0.0183	-0.1130	**-0.1124***	1.0000				
law_24 |	0.4783	0.5653	**0.6526***	-0.2151	1.0000			
law_25 |	0.0683	-0.1333	-0.0423	0.5570	**-0.2502***	1.0000		
law_26 |	0.4091	0.2729	0.5543	-0.1568	0.5028	-0.1890	1.0000	
law_27 |	0.1486	-0.0598	0.2320	0.4440	-0.0256	0.3292	**-0.1440***	1.0000
law_28 |	0.3828	0.3912	0.4424	-0.1458	0.6779	-0.1696	**0.7981***	-0.1366
law_29 |	0.1444	-0.0330	0.1353	0.0097	0.0112	0.5257	-0.1530	0.5697
law_30 |	0.2988	0.1900	0.2263	0.2011	0.2073	0.1953	0.2528	0.1543
law_31 |	0.4985	0.3499	0.3329	-0.0991	0.3538	-0.1801	0.2689	-0.0460
law_32 |	0.4702	0.3330	0.3038	-0.0595	0.3338	-0.1473	0.2521	-0.0594
law_33 |	0.3044	0.2174	0.4549	-0.1926	0.4855	-0.2422	0.3392	0.0137
law_34 |	0.2844	0.1969	0.4161	-0.1366	0.4583	-0.1955	0.3169	-0.0040

law_35	0.1935	0.1294	0.3477	-0.1627	0.3571	-0.2928	0.5251	-0.1791
law_36	0.1821	0.1206	0.3160	-0.1047	0.3393	-0.2430	0.5093	-0.1944
law_37	-0.0115	-0.0147	-0.0078	0.0366	-0.0018	0.0354	0.0143	0.0011
law_38	0.1360	0.0970	0.1056	0.1350	0.1095	0.0317	0.0926	0.0176
law_39	0.1581	0.1091	0.0742	0.0760	0.0607	-0.0531	0.0758	0.1376
law_40	0.1447	0.0671	0.1148	0.3810	0.1470	0.2875	-0.0097	0.2613
	law_28	law_29	law_30	law_31	law_32	law_33	law_34	law_35
law_28	1.0000							
law_29	**-0.1221***	1.0000						
law_30	0.1710	0.0874	1.0000					
law_31	0.3363	-0.1293	0.0917	1.0000				
law_32	0.3220	-0.1406	0.0675	**0.9653^**	1.0000			
law_33	0.3576	-0.0307	0.0507	0.5612	0.5302	1.0000		
law_34	0.3389	-0.0461	0.0348	0.5347	0.5700	**0.9555^**	1.0000	
law_35	0.4746	-0.2499	0.0214	0.4843	0.4566	0.6473	0.6098	1.0000
law_36	0.4656	-0.2622	0.0144	0.4642	0.5046	0.6108	0.6559	**0.9530^**
law_37	-0.0071	0.0074	0.0261	-0.0224	-0.0228	0.0095	0.0061	0.0095
law_38	0.0678	-0.1027	0.4947	0.2302	0.2549	0.3007	0.3267	0.3040
law_39	0.0577	0.0014	0.0319	0.1019	0.0850	0.2052	0.1826	0.1513
law_40	-0.0050	0.1191	0.0945	-0.0019	-0.0153	-0.0358	-0.0446	-0.1824
	law_36	law_37	law_38	law_39	law_40			
law_36	1.0000							
law_37	0.0060	1.0000						
law_38	0.3378	**0.0166^**	1.0000					
law_39	0.1279	0.0409	0.0705	1.0000				
law_40	-0.1891	-0.0076	-0.0350	**-0.1831***	1.0000			

Figure 4.3.1:

Summary Statistics: (N = 1,428*)

VARIABLES	Mean	Std. Dev.	min	max
gdr	12.30	5.530	2.10	60.70
suicide_r	13.76	4.19	4.60	29.80
law_0	0.321	0.467	0	1
law_1	0.162	0.369	0	1

law_2	0.138	0.345	0	1
law_3	0.228	0.420	0	1
law_4	0.317	0.465	0	1
law_5	0.308	0.462	0	1
law_6	0.243	0.429	0	1
law_7	0.134	0.341	0	1
law_8	0.317	0.465	0	1
law_9	0.321	0.467	0	1
law_10	0.704	0.456	0	1
law_11	0.312	0.464	0	1
law_12	0.244	0.429	0	1
law_13	0.263	0.441	0	1
law_14	0.0399	0.196	0	1
law_15	0.154	0.361	0	1
law_16	0.705	0.456	0	1
law_17	0.735	0.441	0	1
law_18	0.801	0.399	0	1
law_19	0.480	0.500	0	1
law_20	0.328	0.470	0	1
law_21	0.140	0.347	0	1
law_22	0.366	0.482	0	1
law_23	0.153	0.360	0	1
law_24	0.196	0.397	0	1
law_25	0.196	0.397	0	1
law_26	0.153	0.360	0	1
law_27	0.134	0.341	0	1
law_28	0.103	0.304	0	1
law_29	0.111	0.314	0	1
law_30	0.315	0.465	0	1
law_31	0.385	0.487	0	1
law_32	0.402	0.490	0	1
law_33	0.375	0.484	0	1
law_34	0.396	0.489	0	1
law_35	0.331	0.471	0	1
law_36	0.352	0.478	0	1
law_37	0.587	0.493	0	1
law_38	0.583	0.493	0	1
law_39	0.177	0.382	0	1
law_40	0.132	0.339	0	1

law_41	0.328	0.470	0	1
law_42	0.127	0.333	0	1
law_43	0.371	0.483	0	1
law_44	0.0588	0.235	0	1
law_45	0.122	0.327	0	1
law_46	0.146	0.354	0	1
law_47	0.648	0.478	0	1
law_48	0.687	0.464	0	1
law_49	0.392	0.488	0	1
law_50	0.429	0.495	0	1
law_51	0.667	0.471	0	1
law_52	0.532	0.499	0	1
law_53	0.574	0.495	0	1
law_54	0.410	0.492	0	1
law_55	0.447	0.497	0	1
law_56	0.489	0.500	0	1
law_57	0.539	0.499	0	1
law_58	0.511	0.500	0	1
law_59	0.300	0.459	0	1
law_60	0.482	0.500	0	1
law_61	0.439	0.496	0	1
law_62	0.232	0.423	0	1
law_63	0.310	0.463	0	1
law_64	0.0469	0.212	0	1
law_65	0.210	0.408	0	1
law_66	0.295	0.456	0	1
law_67	0.315	0.465	0	1
law_68	0.300	0.459	0	1
law_69	0.324	0.468	0	1
law_70	0.352	0.478	0	1
law_71	0.770	0.421	0	1
law_72	0.958	0.201	0	1
law_73	0.389	0.488	0	1
law_74	0.721	0.449	0	1
law_75	0.693	0.462	0	1
law_76	0.716	0.451	0	1
law_77	0.898	0.302	0	1
law_78	0.838	0.368	0	1
law_79	0.552	0.497	0	1

law_80	0.583	0.493	0	1
law_81	0.633	0.482	0	1
law_82	0.666	0.472	0	1
law_83	0.338	0.473	0	1
law_84	0.359	0.480	0	1
law_85	0.216	0.412	0	1
law_86	0.216	0.412	0	1
law_87	0.709	0.455	0	1
law_88	0.546	0.498	0	1
law_89	0.511	0.500	0	1
law_90	0.436	0.496	0	1
law_91	0.826	0.379	0	1
law_92	0.571	0.495	0	1
law_93	0.0469	0.212	0	1
law_94	0.181	0.385	0	1
law_95	0.335	0.472	0	1
law_96	0.342	0.474	0	1
law_97	0.195	0.397	0	1
law_98	0.208	0.406	0	1
law_99	0.0616	0.241	0	1
law_100	0.328	0.470	0	1
law_101	0.351	0.477	0	1
law_102	0.392	0.488	0	1
lawtotal_p	0.381	0.125	0.0777	0.738
bchecks_p	0.220	0.323	0	0.900
criminals_p	0.444	0.206	0.100	0.900
domesticv_p	0.282	0.198	0	0.762
drugsalc_p	0.402	0.232	0	0.875
mental_p	0.333	0.290	0	1
minage_p	0.708	0.275	0	1
permit_p	0.556	0.208	0.0625	1
misc_p	0.244	0.225	0	0.900

Table 6.1.1:

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	FE	FE	FE	FE	FE	FE
VARIABLES		state-fixed only	state- and year-fixed	clustered std. errors	state-specific linear time trend	non-linear trend	non-linear trend

Outcome variable of interest:	*gdr*	*gdr*	*gdr*	*gdr*	*gdr*	*gdr*	*gdr*
law_0	-5.545**	3.572	2.262	2.262	-0.262	-0.924	-0.841
	(2.394)	(2.805)	(2.648)	(2.738)	(1.903)	(1.572)	(1.550)
law_1	0.0650	0.344	-0.591	-0.591	-1.940***	-1.048**	-0.984*
	(0.679)	(0.846)	(0.811)	(1.233)	(0.550)	(0.489)	(0.520)
law_2	5.476***	-0.978	-0.973	-0.973	0.634	0.412	0.395
	(1.500)	(1.064)	(1.005)	(1.240)	(0.491)	(0.394)	(0.406)
law_3	5.053***	-0.563	-0.705	-0.705	-0.0393	0.474	0.437
	(1.676)	(1.009)	(0.956)	(1.351)	(0.501)	(0.680)	(0.688)
law_4	0.942	0.0623	0.973	0.973	0.823	1.838**	1.841**
	(1.043)	(1.713)	(1.618)	(1.676)	(0.650)	(0.818)	(0.820)
law_5	0.0665	0.249	1.063	1.063	0.531	0.780*	0.752*
	(0.566)	(0.684)	(0.652)	(0.822)	(0.462)	(0.403)	(0.407)
law_6	0.564	-0.777	-0.472	-0.472	-0.718	0.0861	0.00246
	(0.564)	(1.131)	(1.065)	(1.267)	(0.713)	(0.747)	(0.736)
law_7	1.152*	0.867	-0.00985	-0.00985	-0.759**	0.0762	0.0768
	(0.676)	(0.818)	(0.776)	(0.569)	(0.361)	(0.220)	(0.220)
law_8	-3.298***	-2.392*	-2.396*	-2.396*	0.0301	-1.350**	-1.384**
	(0.873)	(1.302)	(1.229)	(1.366)	(0.909)	(0.602)	(0.608)
o.law_9	-	-	-	-	-	-	-
law_10	-0.515	0.647	1.083	1.083	0.924*	0.775	0.799
	(0.546)	(0.722)	(0.685)	(0.957)	(0.505)	(0.498)	(0.511)
o.law_11		-	-	-	-	-	-
law_12	-2.089***	-0.0179	0.309	0.309	-0.0581	-0.577	-0.542
	(0.471)	(0.495)	(0.472)	(0.547)	(0.446)	(0.606)	(0.615)
law_13	1.359***	0.0113	0.342	0.342	0.903*	0.657	0.729
	(0.425)	(0.619)	(0.585)	(0.547)	(0.517)	(0.476)	(0.481)
law_14	-3.248***	-0.370	0.445	0.445	-0.502	-0.625	-0.702
	(0.753)	(1.164)	(1.101)	(1.271)	(0.617)	(0.701)	(0.715)
law_15	-3.931***	1.343*	2.208***	2.208***	1.403**	0.403	0.374
	(0.441)	(0.706)	(0.672)	(0.629)	(0.561)	(0.565)	(0.559)
law_16	-5.374***	-1.573*	-1.830**	-1.830***	-0.433	-0.406	-0.347
	(0.793)	(0.877)	(0.833)	(0.534)	(0.595)	(1.042)	(1.008)
law_17	2.681***	2.807**	2.644**	2.644**	0.627	0.809	0.738
	(0.884)	(1.092)	(1.038)	(1.291)	(0.757)	(1.281)	(1.229)
law_18	3.319***	-1.789**	-0.886	-0.886	-0.0447	-0.124	-0.0900
	(0.453)	(0.724)	(0.693)	(0.734)	(0.625)	(0.644)	(0.609)
law_19	-0.114	-1.690***	-1.904***	-1.904**	-1.831***	-1.639***	-1.649***
	(0.306)	(0.437)	(0.416)	(0.884)	(0.545)	(0.472)	(0.476)
law_20	1.456***	1.366***	1.370***	1.370	0.153	0.533	0.552

	(0.366)	(0.423)	(0.406)	(1.023)	(0.463)	(0.403)	(0.400)
law_21	1.429***	0.502	0.420	0.420	0.207	0.388	0.358
	(0.537)	(0.510)	(0.481)	(0.982)	(0.358)	(0.405)	(0.402)
law_22	-1.044**	-0.907*	0.0212	0.0212	1.251***	0.202	0.169
	(0.472)	(0.522)	(0.502)	(0.833)	(0.421)	(0.319)	(0.315)
law_23	0.407	-0.409	-0.444	-0.444	-0.495	-0.326	-0.374
	(0.563)	(0.538)	(0.511)	(0.691)	(0.418)	(0.340)	(0.344)
law_24	-5.584***	-2.690***	-2.958***	-2.958	-1.404***	-1.171**	-1.127**
	(0.798)	(0.603)	(0.576)	(2.090)	(0.364)	(0.518)	(0.511)
law_25	-1.771***	-0.968*	-0.192	-0.192	0.513	0.259	0.293
	(0.485)	(0.516)	(0.494)	(0.825)	(0.462)	(0.450)	(0.443)
law_26	-1.729***	0.258	-0.245	-0.245	-0.865	0.148	0.0285
	(0.546)	(0.745)	(0.707)	(1.122)	(0.543)	(0.451)	(0.468)
law_27	2.329***	-1.414**	-1.564**	-1.564**	-0.307	-0.388	-0.326
	(0.506)	(0.667)	(0.633)	(0.663)	(0.419)	(0.551)	(0.538)
law_28	2.921***	0.501	1.269	1.269	0.503	-0.775	-0.700
	(0.953)	(0.897)	(0.849)	(2.264)	(0.628)	(0.785)	(0.833)
law_29	-3.372***	2.735***	3.212***	3.212**	0.635	0.0565	-0.00782
	(0.558)	(0.791)	(0.752)	(1.211)	(0.558)	(0.654)	(0.650)
law_30	-1.570***	-0.909***	-0.00198	-0.00198	0.637	0.412	0.421
	(0.372)	(0.335)	(0.330)	(0.473)	(0.605)	(0.367)	(0.361)
law_31	5.981***	0.104	-0.393	-0.393	-2.255*	-4.515***	-4.737***
	(1.354)	(1.945)	(1.839)	(1.401)	(1.306)	(1.435)	(1.436)
law_32	-6.478***	0.582	0.765	0.765	2.182	4.337***	4.572***
	(1.373)	(1.973)	(1.867)	(1.377)	(1.399)	(1.447)	(1.451)
law_33	2.063*	-3.521	-3.037	-3.037***	-4.110***	-1.183	-1.286
	(1.212)	(2.162)	(2.033)	(0.832)	(0.768)	(1.114)	(1.102)
law_34	-1.483	2.687	2.154	2.154*	4.641***	1.706	1.770
	(1.215)	(2.177)	(2.048)	(1.075)	(0.995)	(1.169)	(1.157)
law_35	3.072**	-0.853	-0.0666	-0.0666	3.922***	0.512	0.570
	(1.342)	(2.224)	(2.115)	(1.171)	(1.119)	(1.462)	(1.445)
law_36	-2.341*	0.897	0.331	0.331	-3.163**	-0.0210	-0.0633
	(1.344)	(2.267)	(2.155)	(1.302)	(1.398)	(1.503)	(1.481)
law_37	-0.0692	-0.0264	-0.276	-0.276	0.102	0.0574	0.0685
	(0.344)	(0.222)	(0.241)	(0.247)	(0.130)	(0.129)	(0.133)
law_38	0.547	0.251	-0.441	-0.441	0.0931	0.0497	0.0383
	(0.394)	(0.327)	(0.329)	(0.562)	(0.259)	(0.379)	(0.376)
law_39	0.393	-0.196	1.076**	1.076	0.308	0.127	0.148
	(0.283)	(0.452)	(0.441)	(0.865)	(0.347)	(0.355)	(0.351)
law_40	0.968**	0.870*	1.556***	1.556*	0.858*	-0.0790	-0.00860
	(0.481)	(0.509)	(0.484)	(0.903)	(0.461)	(0.396)	(0.399)
law_41	-0.495	-0.315	0.204	0.204	0.108	0.181	0.184
	(0.502)	(0.347)	(0.371)	(0.209)	(0.176)	(0.179)	(0.173)
law_42	0.688	0.635	1.071	1.071	-0.111	0.000580	0.0220
	(0.965)	(0.595)	(0.693)	(1.146)	(0.326)	(0.305)	(0.324)

law_43	-0.101	0.299	-0.533	-0.533	-0.103	-0.0937	-0.106
	(0.478)	(0.314)	(0.364)	(0.366)	(0.157)	(0.146)	(0.147)
law_44	0.785	0.779	0.181	0.181	-0.0623	0.141	0.144
	(0.717)	(0.489)	(0.648)	(0.384)	(0.391)	(0.425)	(0.445)
law_45	-0.182	-0.363	-0.321	-0.321	0.313	0.261	0.242
	(1.141)	(0.636)	(0.717)	(0.900)	(0.467)	(0.412)	(0.433)
law_46	0.569	0.0735	0.109	0.109	0.260	0.284	0.299
	(0.657)	(0.478)	(0.538)	(0.354)	(0.305)	(0.321)	(0.319)
law_47	0.962	-0.995	-0.848	-0.848	-0.648	-0.497	-0.487
	(1.536)	(1.650)	(1.644)	(0.981)	(0.684)	(0.559)	(0.572)
law_48	-0.763	0.795	0.626	0.626	0.661	0.493	0.499
	(1.447)	(1.626)	(1.615)	(0.983)	(0.661)	(0.553)	(0.557)
law_49	-2.900**	1.453	0.135	0.135	-0.582	-0.499	-0.523
	(1.275)	(1.843)	(1.898)	(1.604)	(0.689)	(0.461)	(0.472)
law_50		-3.244*	-0.726	-0.726	0.640	0.475	0.505
		(1.774)	(1.794)	(2.147)	(0.718)	(0.523)	(0.540)
law_51	-0.464	-0.732***	-0.299	-0.299	-0.0365	-0.0870	-0.0852
	(0.487)	(0.252)	(0.300)	(0.245)	(0.149)	(0.139)	(0.140)
law_52	0.601	0.106	1.533	1.533*	1.215	0.733	0.699
	(1.821)	(1.666)	(1.672)	(0.882)	(0.734)	(0.676)	(0.683)
law_53	-0.681	-0.132	-1.486	-1.486*	-1.188*	-0.747	-0.722
	(1.716)	(1.634)	(1.630)	(0.864)	(0.670)	(0.611)	(0.608)
law_54	3.621	1.660***	0.591	0.591	-0.237	-0.191	-0.198
	(3.299)	(0.635)	(0.764)	(0.634)	(0.307)	(0.348)	(0.334)
o.law_55		-	-	-	-	-	-
law_56	0.203	0.767***	0.601*	0.601	0.194	0.209	0.206
	(0.586)	(0.285)	(0.359)	(0.528)	(0.208)	(0.197)	(0.195)
law_57	-0.393	-0.107	-0.268	-0.268	0.158	0.322	0.292
	(1.007)	(0.636)	(0.686)	(0.645)	(0.363)	(0.287)	(0.286)
law_58	0.363	0.425	0.200	0.200	-0.0493	-0.165	-0.150
	(0.789)	(0.556)	(0.615)	(0.736)	(0.482)	(0.311)	(0.308)
law_59	-0.400	-0.578*	-0.230	-0.230	-0.000499	-0.0417	-0.0241
	(0.498)	(0.338)	(0.418)	(0.261)	(0.188)	(0.198)	(0.204)
law_60	0.827	1.298***	0.731	0.731	-0.187	-0.203	-0.192
	(0.647)	(0.438)	(0.525)	(0.495)	(0.285)	(0.271)	(0.278)
law_61	-0.682	-0.638	-0.283	-0.283	-0.0228	-0.0414	-0.0476
	(0.623)	(0.427)	(0.520)	(0.465)	(0.218)	(0.189)	(0.188)
law_62	-0.715	0.309	0.118	0.118	0.212	0.294*	0.273
	(0.654)	(0.450)	(0.502)	(0.231)	(0.177)	(0.168)	(0.169)
law_63	0.456	-0.427	-0.178	-0.178	-0.101	-0.0688	-0.0347
	(0.566)	(0.357)	(0.407)	(0.319)	(0.178)	(0.183)	(0.186)
law_64	0.687	0.839**	0.307	0.307	-0.0274	0.202	0.186
	(0.536)	(0.392)	(0.404)	(0.263)	(0.214)	(0.231)	(0.228)
law_65	0.297	-0.255	0.0816	0.0816	0.190	-0.181	-0.179

	(0.702)	(0.444)	(0.502)	(0.518)	(0.305)	(0.269)	(0.268)
law_66		-5.554**	0.819	0.819	1.008	-0.0402	-0.0910
		(2.313)	(2.530)	(1.296)	(1.019)	(0.862)	(0.889)
law_67	-0.534	5.771**	-1.181	-1.181	-1.288	-0.116	-0.0826
	(0.921)	(2.289)	(2.498)	(1.445)	(1.115)	(0.823)	(0.843)
law_68	0.120	-1.246	-1.831	-1.831	0.289	0.816	0.960
	(0.502)	(1.917)	(1.942)	(2.807)	(0.766)	(0.740)	(0.733)
law_69		1.052	1.611	1.611	-0.384	-0.754	-0.895
		(1.902)	(1.968)	(2.684)	(0.745)	(0.713)	(0.715)
law_70	0.184	0.196	-0.147	-0.147	0.0465	0.156	0.136
	(0.347)	(0.234)	(0.307)	(0.403)	(0.142)	(0.137)	(0.136)
law_71	0.991*	0.571*	0.150	0.150	0.145	0.0962	0.0712
	(0.510)	(0.333)	(0.378)	(0.222)	(0.212)	(0.195)	(0.189)
law_72	0.556	-0.224	-0.205	-0.205	0.304	0.352	0.378
	(0.751)	(0.540)	(0.566)	(0.371)	(0.420)	(0.461)	(0.472)
law_73	-0.786	-0.655**	0.217	0.217	0.109	0.0830	0.0965
	(0.577)	(0.307)	(0.386)	(0.319)	(0.220)	(0.238)	(0.243)
law_74	0.651	0.733**	0.466	0.466*	-0.139	-0.248	-0.255
	(0.401)	(0.284)	(0.329)	(0.274)	(0.183)	(0.200)	(0.212)
law_75	0.500	0.783***	0.195	0.195	0.130	-0.0897	-0.0829
	(3.416)	(0.291)	(0.397)	(0.451)	(0.215)	(0.146)	(0.145)
o.law_76		-	-	-	-	-	-
law_77	1.261	2.636**	-0.548	-0.548	1.035	1.052	1.066
	(1.566)	(1.263)	(1.321)	(0.778)	(0.773)	(0.880)	(0.847)
law_78	0.775	0.263	-1.021	-1.021	-0.417	-0.192	-0.204
	(1.500)	(1.234)	(1.225)	(1.320)	(0.464)	(0.420)	(0.420)
law_79	-1.118	-2.677**	-0.429	-0.429	0.390	0.0797	0.165
	(2.680)	(1.238)	(1.284)	(1.375)	(0.454)	(0.471)	(0.459)
law_80	1.641	2.668**	0.147	0.147	-0.654	-0.265	-0.359
	(2.676)	(1.236)	(1.291)	(1.337)	(0.454)	(0.453)	(0.447)
law_81	2.067	3.346**	-0.740	-0.740	-0.318	-0.200	-0.210
	(3.265)	(1.331)	(1.392)	(1.208)	(0.689)	(0.631)	(0.639)
law_82	-2.164	-3.369***	0.628	0.628	0.499	0.358	0.367
	(3.170)	(1.296)	(1.365)	(1.104)	(0.556)	(0.501)	(0.509)
law_83	-3.115	0.917**	-0.258	-0.258	0.242	0.304	0.299
	(3.170)	(0.461)	(0.512)	(0.373)	(0.284)	(0.276)	(0.274)
o.law_84		-	-	-	-	-	-
law_85	-0.785	-0.414	-0.120	-0.120	-0.360	-0.325	-0.319
	(0.633)	(0.427)	(0.523)	(0.298)	(0.338)	(0.335)	(0.337)
o.law_86	-	-	-	-	-	-	-
law_87	-0.212	-0.201	-0.529	-0.529*	0.0307	-0.0994	-0.111
	(0.505)	(0.352)	(0.384)	(0.301)	(0.192)	(0.183)	(0.180)

law_88	-0.421	-0.464*	0.546*	0.546	0.137	-0.00946	-0.0237
	(0.382)	(0.249)	(0.303)	(0.440)	(0.142)	(0.122)	(0.126)
law_89	-0.490	-0.778***	-0.223	-0.223	0.191	0.294**	0.298*
	(0.347)	(0.246)	(0.308)	(0.351)	(0.123)	(0.144)	(0.150)
law_90	-0.363	-0.445**	-0.0382	-0.0382	0.105	0.0686	0.0775
	(0.385)	(0.225)	(0.239)	(0.153)	(0.201)	(0.147)	(0.143)
law_91	-0.782	-0.0809	1.284	1.284	-0.0581	0.119	0.120
	(1.289)	(0.808)	(0.892)	(1.099)	(0.414)	(0.384)	(0.384)
law_92	-0.178	-0.206	0.0136	0.0136	-0.321	-0.278	-0.291
	(0.431)	(0.285)	(0.346)	(0.235)	(0.249)	(0.216)	(0.217)
o.law_93	-	-	-	-	-	-	-
law_94	0.249	0.802**	-0.467	-0.467	-0.242	-0.108	-0.117
	(0.507)	(0.350)	(0.451)	(0.463)	(0.250)	(0.195)	(0.191)
law_95	-0.692	-1.319***	-0.423	-0.423	-0.191	-0.140	-0.152
	(0.614)	(0.405)	(0.471)	(0.270)	(0.227)	(0.222)	(0.223)
law_96	0.414	-0.762**	-0.333	-0.333	0.0708	-0.0196	-0.0361
	(0.513)	(0.374)	(0.411)	(0.249)	(0.295)	(0.232)	(0.234)
law_97	-0.674	-1.677*	-0.528	-0.528	0.838	0.664	0.674
	(1.014)	(0.864)	(0.994)	(0.857)	(0.577)	(0.451)	(0.459)
law_98	0.508	2.038**	0.844	0.844	-0.906	-0.707	-0.692
	(1.004)	(0.857)	(1.033)	(1.006)	(0.685)	(0.529)	(0.535)
law_99	-0.299	0.175	0.954	0.954*	-0.0267	-0.0973	-0.0853
	(0.794)	(0.511)	(0.651)	(0.565)	(0.361)	(0.376)	(0.375)
law_100	-1.254	0.976	-0.000778	-0.000778	0.218	0.674	0.624
	(1.459)	(1.716)	(1.703)	(1.630)	(0.674)	(0.436)	(0.459)
law_101	1.811	-0.689	0.644	0.644	-0.0712	-0.610	-0.566
	(1.512)	(1.713)	(1.710)	(1.957)	(0.735)	(0.450)	(0.475)
law_102	0.337	-0.00157	0.134	0.134	0.0749	0.0916	0.0917
	(0.488)	(0.267)	(0.334)	(0.553)	(0.335)	(0.333)	(0.337)
bchecks_criminals_domesticv							-1.827
							(1.299)
bchecks_criminals							0.159
							(0.526)
bchecks_domesticv							0.473
							(0.720)
criminals_domesticv							1.016**
							(0.501)
Constant	12.22***	10.37***	15.75***	15.75***	354.0***	8,530	8,917

	(1.648)	(1.259)	(1.443)	(1.362)	(61.09)	(11,586)	(11,214)
Observations	1,428	1,428	1,428	1,428	1,428	1,428	1,428
R-squared	0.466	0.327	0.420	0.420	0.807	0.841	0.841
Number of state_id		51	51	51	51	51	51
State FE		YES	YES	YES	YES	YES	YES
Year FE			YES	YES	YES	YES	YES
Clustered Standard Errors				YES	YES	YES	YES
Time Trend Interaction					YES	YES	YES
Non-linear Trend						YES	YES
Significant Law Package Interaction							YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6.2.1:

	(5)	(6)	**(8)**
	FE	FE	**XPO Lasso**
VARIABLES	state-specific linear time trend	State-specific non-linear time trend	
Outcome variable of interest:	*gdr*	*gdr*	*gdr*
law_1	-1.940***	-1.048**	**0.224**
	(0.550)	(0.489)	**(0.499)**
law_2	0.634	0.412	**2.081*** **
	(0.491)	(0.394)	**(0.663)**
law_3	-0.0393	0.474	**0.320**
	(0.501)	(0.680)	**(0.795)**
law_4	0.823	1.838**	**-2.426** **
	(0.650)	(0.818)	**(1.059)**
law_5	0.531	0.780*	**-1.929**** **
	(0.462)	(0.403)	**(0.442)**
law_6	-0.718	0.0861	**0.365**
	(0.713)	(0.747)	**(0.613)**
law_7	-0.759**	0.0762	**-0.0337**
	(0.361)	(0.220)	**(0.546)**
law_8	0.0301	-1.350**	**-4.314**** **
	(0.909)	(0.602)	**(1.060)**

o.law_9	-	-	**-**
law_10	0.924*	0.775	**1.680*** **
	(0.505)	(0.498)	**(0.562)**
o.law_11	-	-	**-**
law_12	-0.0581	-0.577	**-0.806**
	(0.446)	(0.606)	**(0.431)**
law_13	0.903*	0.657	**-0.302**
	(0.517)	(0.476)	**(0.419)**
law_14	-0.502	-0.625	**3.372*******
	(0.617)	(0.701)	**(0.745)**
law_15	1.403**	0.403	**-0.553**
	(0.561)	(0.565)	**0.617**
law_16	-0.433	-0.406	**-1.643****
	(0.595)	(1.042)	**(0.744)**
law_17	0.627	0.809	**1.861**
	(0.757)	(1.281)	**(1.051)**
law_18	-0.0447	-0.124	**-0.640**
	(0.625)	(0.644)	**(0.635)**
law_19	-1.831***	-1.639***	**-1.620*******
	(0.545)	(0.472)	**(0.393)**
law_20	0.153	0.533	**0.608**
	(0.463)	(0.403)	**(0.393)**
law_21	0.207	0.388	**-1.041****
	(0.358)	(0.405)	**(0.501)**
law_22	1.251***	0.202	**-0.751**
	(0.421)	(0.319)	**(0.401)**
law_23	-0.495	-0.326	**-0.768**
	(0.418)	(0.340)	**(0.467)**
law_24	-1.404***	-1.171**	**-2.408*****
	(0.364)	(0.518)	**(0.912)**
law_25	0.513	0.259	**0.664**
	(0.462)	(0.450)	**(0.496)**
law_26	-0.865	0.148	**-0.441**
	(0.543)	(0.451)	**(1.010)**
law_27	-0.307	-0.388	**-1.867*****
	(0.419)	(0.551)	**(0.572)**
law_28	0.503	-0.775	**-3.199*******
	(0.628)	(0.785)	**(0.838)**
law_29	0.635	0.0565	**-2.307*****
	(0.558)	(0.654)	**(0.786)**
law_30	0.637	0.412	**-0.578**
	(0.605)	(0.367)	**(0.341)**
law_31	-2.255*	-4.515***	**8.939*******

	(1.306)	(1.435)	**(2.683)**
law_32	2.182	4.337***	**-7.362*** **
	(1.399)	(1.447)	**(2.516)**
law_33	-4.110***	-1.183	**-6.012*** **
	(0.768)	(1.114)	**(1.904)**
law_34	4.641***	1.706	**3.458**
	(0.995)	(1.169)	**(2.567)**
law_35	3.922***	0.512	**1.649**
	(1.119)	(1.462)	**(1.928)**
law_36	-3.163**	-0.0210	**-4.285*** **
	(1.398)	(1.503)	**(1.313)**
law_37	0.102	0.0574	**-0.311**
	(0.130)	(0.129)	**(0.193)**
law_38	0.0931	0.0497	**-1.305**** **
	(0.259)	(0.379)	**(0.326)**
law_39	0.308	0.127	**-1.442**** **
	(0.347)	(0.355)	**(0.416)**
law_40	0.858*	-0.0790	**1.068** **
	(0.461)	(0.396)	**(0.446)**
law_41	0.108	0.181	**-0.0109**
	(0.176)	(0.179)	**(0.273)**
law_42	-0.111	0.000580	**-1.195*** **
	(0.326)	(0.305)	**(0.440)**
law_43	-0.103	-0.0937	**-0.189**
	(0.157)	(0.146)	**(0.253)**
law_44	-0.0623	0.141	**0.329**
	(0.391)	(0.425)	**(0.381)**
law_45	0.313	0.261	**-0.572**
	(0.467)	(0.412)	**(0.447)**
law_46	0.260	0.284	**1.297**** **
	(0.305)	(0.321)	**(0.375)**
law_47	-0.648	-0.497	**-0.415**
	(0.684)	(0.559)	**(1.162)**
law_48	0.661	0.493	**0.269**
	(0.661)	(0.553)	**(1.109)**
law_49	-0.582	-0.499	**-**
	(0.689)	(0.461)	
law_50	0.640	0.475	**-**
	(0.718)	(0.523)	
law_51	-0.0365	-0.0870	**0.295**
	(0.149)	(0.139)	**(0.201)**
law_52	1.215	0.733	**1.248**
	(0.734)	(0.676)	**(0.779)**
law_53	-1.188*	-0.747	**-1.915** **
	(0.670)	(0.611)	**(0.845)**

law_54	-0.237	-0.191	**-**
	(0.307)	(0.348)	
o.law_55	-	-	**-**
law_56	0.194	0.209	**-0.411**
	(0.208)	(0.197)	**(0.234)**
law_57	0.158	0.322	**0.168**
	(0.363)	(0.287)	**(0.494)**
law_58	-0.0493	-0.165	**-0.263**
	(0.482)	(0.311)	**(0.416)**
law_59	-0.000499	-0.0417	**-0.0321**
	(0.188)	(0.198)	**(0.273)**
law_60	-0.187	-0.203	**-0.0760**
	(0.285)	(0.271)	**(0.396)**
law_61	-0.0228	-0.0414	**-0.254**
	(0.218)	(0.189)	**(0.337)**
law_62	0.212	0.294*	**-1.094*** **
	(0.177)	(0.168)	**(0.367)**
law_63	-0.101	-0.0688	**0.129**
	(0.178)	(0.183)	**(0.397)**
law_64	-0.0274	0.202	**-**
	(0.214)	(0.231)	
law_65	0.190	-0.181	**0.577**
	(0.305)	(0.269)	**(0.392)**
law_66	1.008	-0.0402	**-**
	(1.019)	(0.862)	
law_67	-1.288	-0.116	**-**
	(1.115)	(0.823)	
law_68	0.289	0.816	**1.270**
	(0.766)	(0.740)	**(1.057)**
law_69	-0.384	-0.754	**-0.0854**
	(0.745)	(0.713)	**(0.531)**
law_70	0.0465	0.156	**-0.0875**
	(0.142)	(0.137)	**(0.242)**
law_71	0.145	0.0962	**0.114**
	(0.212)	(0.195)	**(0.251)**
law_72	0.304	0.352	**0.251**
	(0.420)	(0.461)	**(0.421)**
law_73	0.109	0.0830	**-0.448**
	(0.220)	(0.238)	**(0.296)**
law_74	-0.139	-0.248	**0.285**
	(0.183)	(0.200)	**(0.258)**
law_75	0.130	-0.0897	**-**
	(0.215)	(0.146)	
o.law_76	-	-	**-**

law_77	1.035	1.052	**-0.217**
	(0.773)	(0.880)	**(0.777)**
law_78	-0.417	-0.192	**1.683**
	(0.464)	(0.420)	**(0.865)**
law_79	0.390	0.0797	**0.969**
	(0.454)	(0.471)	**(0.625)**
law_80	-0.654	-0.265	**0.257**
	(0.454)	(0.453)	**(0.713)**
law_81	-0.318	-0.200	**1.231**
	(0.689)	(0.631)	**(0.717)**
law_82	0.499	0.358	**0.196**
	(0.556)	(0.501)	**(0.650)**
law_83	0.242	0.304	**-**
	(0.284)	(0.276)	
o.law_84	-	-	**-**
law_85	-0.360	-0.325	**-**
	(0.338)	(0.335)	
o.law_86	-	-	**-**
law_87	0.0307	-0.0994	**-0.905*** **
	(0.192)	(0.183)	**(0.296)**
law_88	0.137	-0.00946	**-0.0538**
	(0.142)	(0.122)	**(0.231)**
law_89	0.191	0.294**	**-0.220**
	(0.123)	(0.144)	**(0.267)**
law_90	0.105	0.0686	**-0.204**
	(0.201)	(0.147)	**(0.216)**
law_91	-0.0581	0.119	**-0.471**
	(0.414)	(0.384)	**(0.753)**
law_92	-0.321	-0.278	**0.0146**
	(0.249)	(0.216)	**(0.249)**
o.law_93	-	-	**-**
law_94	-0.242	-0.108	**-0.328**
	(0.250)	(0.195)	**(0.378)**
law_95	-0.191	-0.140	**-0.609**
	(0.227)	(0.222)	**(0.340)**
law_96	0.0708	-0.0196	**-0.233**
	(0.295)	(0.232)	**(0.275)**
law_97	0.838	0.664	**-1.887******
	(0.577)	(0.451)	**(0.515)**
law_98	-0.906	-0.707	**0.319**
	(0.685)	(0.529)	**(0.515)**

law_99	-0.0267	-0.0973	**-0.228**
	(0.361)	(0.376)	**(0.427)**
law_100	0.218	0.674	**1.444**
	(0.674)	(0.436)	**(0.833)**
law_101	-0.0712	-0.610	**-0.731**
	(0.735)	(0.450)	**(1.061)**
law_102	0.0749	0.0916	**0.156**
	(0.335)	(0.333)	**(0.267)**
Constant	354.0***	8,530	
	(61.09)	(11,586)	
Observations	1,428	1,428	
R-squared	0.807	0.841	
Number of state_id	51	51	
State FE	YES	YES	
Year FE	YES	YES	
Clustered Standard Errors	YES	YES	
Time Trend Interaction	YES	YES	
Non-linear Trend		YES	

Robust standard errors in parentheses
**** p<0.001 *** p<0.01, ** p<0.05, * p<0.1
Note: **** not available to Model (5)-(6) output.

Table 6.2.2: Correlation Matrix of Significant Possession-Related Domestic Violence Laws under the XPO Lasso Specification

	law_21	law_24	law_27	law_28	law_29	law_39
law_21	1.0000					
law_24	0.5653	1.0000				
law_27	-0.0598	-0.0256	1.0000			
law_28	0.3912	0.6779	-0.1366	1.0000		
law_29	-0.0330	0.0112	0.5697	-0.1221	1.0000	
law_39	0.1091	0.0607	0.1376	0.0577	0.0014	1.0000

Table 6.3.1: RF Variable Importance for Predicting the GDR

Law Feature	IncNodePurity	Law Feature	IncNodePurity	Law Feature	IncNodePurity
law_0	429.96	**law_35**	35.01	**law_69**	4.57
law_1	99.11	**law_36**	65.41	**law_70**	20.37
law_2	15.01	**law_37**	5.66	**law_71**	6.02
law_3	54.23	**law_38**	57.85	**law_72**	3.05
law_4	412.25	**law_39**	28.84	**law_73**	7.67
law_5	218.31	**law_40**	47.34	**law_74**	5.90

law_6	49.88	law_41	1.81	law_75	4.28
law_7	42.24	law_42	3.90	law_76	10.53
law_8	262.99	law_43	3.88	law_77	4.73
law_9	422.28	law_44	0.17	law_78	6.79
law_10	17.63	law_45	0.72	law_79	4.50
law_11	42.01	law_46	2.42	law_80	4.76
law_12	215.30	law_47	4.09	law_81	5.33
law_13	74.66	law_48	3.05	law_82	5.75
law_14	2.82	law_49	3.65	law_83	3.45
law_15	80.67	law_50	3.87	law_84	5.87
law_16	56.03	law_51	2.12	law_85	0.58
law_17	12.57	law_52	3.45	law_86	0.64
law_18	31.30	law_53	2.82	law_87	3.14
law_19	58.67	law_54	2.70	law_88	15.40
law_20	27.00	law_55	3.79	law_89	10.84
law_21	63.56	law_56	3.47	law_90	15.00
law_22	68.75	law_57	6.44	law_91	6.23
law_23	13.27	law_58	4.59	law_92	5.24
law_24	127.27	law_59	3.11	law_93	0.69
law_25	96.74	law_60	4.29	law_94	11.38
law_26	78.14	law_61	3.55	law_95	7.19
law_27	89.72	law_62	2.47	law_96	4.32
law_28	106.78	law_63	8.13	law_97	1.91
law_29	239.68	law_64	0.76	law_98	2.63
law_30	233.99	law_65	5.66	law_99	2.43
law_31	27.77	law_66	3.91	law_100	3.01
law_32	27.68	law_67	10.72	law_101	5.10
law_33	14.79	law_68	3.12	law_102	3.30
law_34	21.01				

Figure 6.3.2: GDR (y-axis) in D.C. Time-series

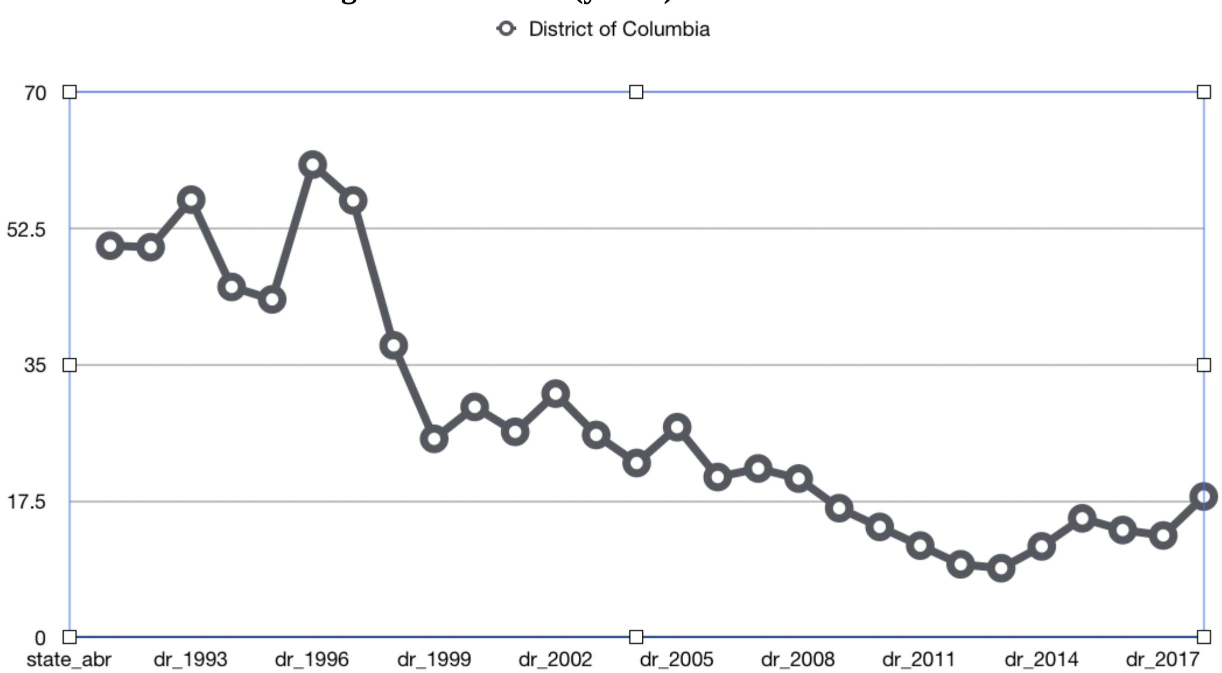

Table 6.3.3: RF Prediction of the GDR

state	year	predict_rf	gdr	diff_rf_gdr		state	year	predict_rf	gdr	diff_rf_gdr
AK	2014	20.69	19.2	1.49		MS	2016	18.27	19.9	-1.63
AK	2015	19.94	23.4	-3.46		MS	2017	19.54	21.5	-1.96
AK	2016	21.47	23.3	-1.83		MS	2018	20.05	22.9	-2.85

AK	**2017**	21.28	24.5	-3.22		MT	**2014**	16.16	16.1	0.06
AK	**2018**	21.76	21.0	0.76		MT	**2015**	18.68	19.4	-0.72
AL	**2014**	18.33	16.9	1.43		MT	**2016**	17.48	18.9	-1.42
AL	**2015**	18.88	19.7	-0.82		MT	**2017**	17.58	22.5	-4.92
AL	**2016**	19.56	21.5	-1.94		MT	**2018**	20.59	17.3	3.29
AL	**2017**	19.83	22.9	-3.07		NC	**2014**	12.52	11.8	0.72
AL	**2018**	20.03	21.8	-1.77		NC	**2015**	12.31	12.6	-0.29
AR	**2014**	16.18	16.6	-0.42		NC	**2016**	12.24	13.7	-1.46
AR	**2015**	17.34	17.0	0.34		NC	**2017**	12.38	13.7	-1.32
AR	**2016**	17.71	17.8	-0.09		NC	**2018**	12.66	13.3	-0.64
AR	**2017**	18.16	20.3	-2.14		ND	**2014**	11.63	12.3	-0.67
AR	**2018**	18.41	18.9	-0.49		ND	**2015**	12.16	12.8	-0.64
AZ	**2014**	14.32	13.5	0.82		ND	**2016**	12.19	11.9	0.29
AZ	**2015**	14.64	13.8	0.84		ND	**2017**	12.39	13.2	-0.81
AZ	**2016**	14.59	15.2	-0.61		ND	**2018**	13.16	11.5	1.66
AZ	**2017**	14.81	15.8	-0.99		NE	**2014**	8.79	9.5	-0.71
AZ	**2018**	14.84	15.3	-0.46		NE	**2015**	8.99	8.9	0.09
CA	**2014**	7.56	7.4	0.16		NE	**2016**	9.24	9.1	0.14
CA	**2015**	7.33	7.7	-0.37		NE	**2017**	9.38	8.3	1.08
CA	**2016**	7.55	7.9	-0.35		NE	**2018**	8.87	9.0	-0.13
CA	**2017**	7.60	7.9	-0.30		NH	**2014**	9.81	8.7	1.11
CA	**2018**	7.77	7.5	0.27		NH	**2015**	10.30	8.9	1.40
CO	**2014**	12.54	12.2	0.34		NH	**2016**	9.99	9.3	0.69
CO	**2015**	12.92	12.6	0.32		NH	**2017**	10.74	10.4	0.34
CO	**2016**	12.82	14.3	-1.48		NH	**2018**	10.85	10.8	0.05
CO	**2017**	13.32	13.4	-0.08		NJ	**2014**	5.85	5.3	0.55
CO	**2018**	13.11	15.2	-2.09		NJ	**2015**	5.94	5.4	0.54
CT	**2014**	7.67	5.0	2.67		NJ	**2016**	5.91	5.5	0.41
CT	**2015**	7.61	5.3	2.31		NJ	**2017**	5.94	5.3	0.64
CT	**2016**	5.43	4.6	0.83		NJ	**2018**	7.57	4.8	2.77
CT	**2017**	6.11	5.1	1.01		NM	**2014**	15.95	16.0	-0.05
CT	**2018**	5.38	4.9	0.48		NM	**2015**	16.64	18.6	-1.96
DC	**2014**	13.21	11.7	1.51		NM	**2016**	16.78	18.1	-1.32
DC	**2015**	12.10	15.3	-3.20		NM	**2017**	18.07	18.5	-0.43
DC	**2016**	13.77	13.8	-0.03		NM	**2018**	18.11	20.7	-2.59
DC	**2017**	14.36	13.1	1.26		NV	**2014**	13.94	14.8	-0.86
DC	**2018**	13.07	18.1	-5.03		NV	**2015**	14.75	15.0	-0.25
DE	**2014**	11.39	11.1	0.29		NV	**2016**	14.75	16.8	-2.05
DE	**2015**	11.28	12.1	-0.82		NV	**2017**	13.86	16.7	-2.84
DE	**2016**	11.68	11.0	0.68		NV	**2018**	14.10	17.9	-3.80
DE	**2017**	11.64	11.7	-0.06		NY	**2014**	4.57	4.2	0.37
DE	**2018**	11.87	11.6	0.27		NY	**2015**	4.57	4.2	0.37
FL	**2014**	12.93	11.5	1.43		NY	**2016**	4.63	4.4	0.23
FL	**2015**	13.00	12.0	1.00		NY	**2017**	4.96	3.7	1.26
FL	**2016**	13.06	12.6	0.46		NY	**2018**	4.43	4.1	0.33
FL	**2017**	14.60	12.4	2.20		OH	**2014**	13.23	10.3	2.93
FL	**2018**	13.66	12.9	0.76		OH	**2015**	13.28	11.9	1.38
GA	**2014**	14.98	13.7	1.28		OH	**2016**	13.13	12.9	0.23
GA	**2015**	15.67	14.1	1.57		OH	**2017**	14.00	13.7	0.30
GA	**2016**	16.09	15.0	1.09		OH	**2018**	13.55	13.1	0.45
GA	**2017**	17.84	15.4	2.44		OK	**2014**	15.82	15.7	0.12
GA	**2018**	16.33	15.7	0.63		OK	**2015**	16.76	18.0	-1.24
HI	**2014**	4.67	2.6	2.07		OK	**2016**	16.87	19.6	-2.73
HI	**2015**	3.93	3.6	0.33		OK	**2017**	17.94	17.2	0.74
HI	**2016**	4.00	4.5	-0.50		OK	**2018**	17.89	16.8	1.09
HI	**2017**	4.41	2.5	1.91		OR	**2014**	14.04	11.7	2.34
HI	**2018**	4.23	4.0	0.23		OR	**2015**	11.17	11.4	-0.23
IA	**2014**	8.82	7.5	1.32		OR	**2016**	11.86	11.9	-0.04
IA	**2015**	8.91	7.8	1.11		OR	**2017**	11.85	12.1	-0.25
IA	**2016**	8.73	9.2	-0.47		OR	**2018**	12.24	11.7	0.54
IA	**2017**	8.62	9.0	-0.38		PA	**2014**	11.16	10.5	0.66
IA	**2018**	9.09	8.7	0.39		PA	**2015**	11.25	11.4	-0.15

ID	**2014**	14.83	13.2	1.63		PA	**2016**	11.54	12.0	-0.46
ID	**2015**	14.65	14.9	-0.25		PA	**2017**	11.44	12.5	-1.06
ID	**2016**	16.40	14.6	1.80		PA	**2018**	11.16	12.5	-1.34
ID	**2017**	16.79	16.4	0.39		RI	**2014**	5.70	3.0	2.70
ID	**2018**	16.25	16.6	-0.35		RI	**2015**	5.28	4.7	0.58
IL	**2014**	10.13	9.0	1.13		RI	**2016**	6.03	4.1	1.93
IL	**2015**	9.93	9.5	0.43		RI	**2017**	5.34	3.9	1.44
IL	**2016**	9.59	11.7	-2.11		RI	**2018**	5.57	3.3	2.27
IL	**2017**	8.80	12.1	-3.30		SC	**2014**	15.83	15.5	0.33
IL	**2018**	10.11	10.9	-0.79		SC	**2015**	16.38	17.3	-0.92
IN	**2014**	13.91	12.4	1.51		SC	**2016**	16.89	17.7	-0.81
IN	**2015**	14.16	12.7	1.46		SC	**2017**	16.95	17.7	-0.75
IN	**2016**	14.06	15.0	-0.94		SC	**2018**	16.57	17.6	-1.03
IN	**2017**	14.35	15.3	-0.95		SD	**2014**	12.41	10.3	2.11
IN	**2018**	14.91	14.7	0.21		SD	**2015**	12.64	11.1	1.54
KS	**2014**	14.79	11.3	3.49		SD	**2016**	11.76	13.4	-1.64
KS	**2015**	15.26	11.4	3.86		SD	**2017**	12.64	11.9	0.74
KS	**2016**	15.67	13.4	2.27		SD	**2018**	12.17	13.6	-1.43
KS	**2017**	16.13	16.0	0.13		TN	**2014**	15.55	15.2	0.35
KS	**2018**	16.20	14.8	1.40		TN	**2015**	16.24	15.9	0.34
KY	**2014**	15.53	13.9	1.63		TN	**2016**	16.84	17.1	-0.26
KY	**2015**	16.14	15.2	0.94		TN	**2017**	16.87	18.4	-1.53
KY	**2016**	16.92	17.5	-0.58		TN	**2018**	16.62	17.8	-1.18
KY	**2017**	17.94	16.2	1.74		TX	**2014**	11.31	10.7	0.61
KY	**2018**	17.96	16.9	1.06		TX	**2015**	11.21	11.8	-0.59
LA	**2014**	18.41	19.0	-0.59		TX	**2016**	12.09	12.1	-0.01
LA	**2015**	19.43	20.5	-1.07		TX	**2017**	12.09	12.4	-0.31
LA	**2016**	19.80	21.3	-1.50		TX	**2018**	11.96	12.2	-0.24
LA	**2017**	20.22	21.7	-1.48		UT	**2014**	14.16	12.3	1.86
LA	**2018**	17.86	21.4	-3.54		UT	**2015**	14.37	12.9	1.47
MA	**2014**	4.33	3.2	1.13		UT	**2016**	14.28	12.9	1.38
MA	**2015**	4.24	3.0	1.24		UT	**2017**	13.96	14.0	-0.04
MA	**2016**	4.35	3.4	0.95		UT	**2018**	14.21	13.2	1.01
MA	**2017**	3.61	3.7	-0.09		VA	**2014**	11.31	10.3	1.01
MA	**2018**	3.93	3.5	0.43		VA	**2015**	11.63	10.9	0.73
MD	**2014**	11.08	9.0	2.08		VA	**2016**	11.84	12.1	-0.26
MD	**2015**	11.37	11.9	-0.53		VA	**2017**	12.19	11.9	0.29
MD	**2016**	11.56	11.9	-0.34		VA	**2018**	11.73	11.8	-0.07
MD	**2017**	11.17	12.3	-1.13		VT	**2014**	15.10	10.3	4.80
MD	**2018**	11.79	11.7	0.09		VT	**2015**	12.12	9.6	2.52
ME	**2014**	10.23	9.4	0.83		VT	**2016**	12.12	11.1	1.02
ME	**2015**	10.73	9.8	0.93		VT	**2017**	11.90	11.7	0.20
ME	**2016**	10.62	8.3	2.32		VT	**2018**	10.13	12.8	-2.67
ME	**2017**	10.37	11.7	-1.33		WA	**2014**	9.92	9.7	0.22
ME	**2018**	10.99	10.3	0.69		WA	**2015**	9.65	9.8	-0.15
MI	**2014**	11.19	11.1	0.09		WA	**2016**	10.05	9.0	1.05
MI	**2015**	11.59	11.7	-0.11		WA	**2017**	9.58	11.1	-1.52
MI	**2016**	11.65	12.3	-0.65		WA	**2018**	9.90	10.4	-0.50
MI	**2017**	12.09	11.3	0.79		WI	**2014**	10.73	8.2	2.53
MI	**2018**	11.76	12.9	-1.14		WI	**2015**	10.56	10.4	0.16
MN	**2014**	8.25	6.6	1.65		WI	**2016**	10.83	11.4	-0.57
MN	**2015**	8.29	7.4	0.89		WI	**2017**	11.22	10.6	0.62
MN	**2016**	8.30	7.6	0.70		WI	**2018**	10.86	10.1	0.76
MN	**2017**	8.02	8.2	-0.18		WV	**2014**	14.86	14.6	0.26
MN	**2018**	8.50	7.8	0.70		WV	**2015**	16.11	14.1	2.01
MO	**2014**	16.09	15.3	0.79		WV	**2016**	17.07	17.5	-0.43
MO	**2015**	18.92	18.2	0.72		WV	**2017**	17.10	18.6	-1.50
MO	**2016**	17.34	19.0	-1.66		WV	**2018**	16.66	18.2	-1.54
MO	**2017**	18.66	21.5	-2.84		WY	**2014**	17.01	16.2	0.81
MO	**2018**	19.86	21.5	-1.64		WY	**2015**	17.69	19.8	-2.11
MS	**2014**	16.62	18.3	-1.68		WY	**2016**	19.76	17.4	2.36
MS	**2015**	20.12	19.7	0.42		WY	**2017**	18.86	18.8	0.06

						WY	2018	17.46	21.5	-4.04

Table 8.1.1:

	(1)	(2)	(3)	(4)	(5)
	OLS	FE	FE	FE	FE
VARIABLES		state-fixed only	state- and year-fixed	clustered std. errors	state-specific linear time trend
Outcome variable of interest:	*suicide_r*	*suicide_r*	*suicide_r*	*suicide_r*	*suicide_r*
law_0	0.0600	2.833*	4.242***	4.242***	2.636***
	(1.106)	(1.457)	(1.260)	(1.115)	(0.691)
law_1	0.218	0.199	-0.639	-0.639	-0.921***
	(0.440)	(0.638)	(0.556)	(0.452)	(0.303)
law_2	-1.500***	-0.0747	-0.512	-0.512	0.323
	(0.555)	(0.825)	(0.713)	(0.478)	(0.274)
law_3	-2.144***	-0.137	-0.336	-0.336	0.808
	(0.673)	(0.771)	(0.668)	(0.716)	(0.540)
o.law_4	-	-	-	-	-
law_5	2.510***	-2.301***	-2.581***	-2.581***	-1.043**
	(0.460)	(0.855)	(0.738)	(0.494)	(0.491)
law_6	-1.911***	-1.022	-1.309**	-1.309***	-0.489**
	(0.394)	(0.728)	(0.627)	(0.289)	(0.234)
law_7	-1.097***	-0.641	-0.126	-0.126	-0.664*
	(0.425)	(0.701)	(0.610)	(0.428)	(0.377)
law_8	-1.065	-0.0520	-0.245	-0.245	-0.858*
	(1.006)	(0.779)	(0.676)	(0.688)	(0.489)
o.law_9	-	-	-	-	-
law_10	-0.155	1.662*	1.555*	1.555	0.849*
	(0.607)	(0.927)	(0.804)	(0.945)	(0.450)
o.law_11	-	-	-	-	-
law_12	-3.622***	-1.534***	-1.155***	-1.155	-0.873
	(0.348)	(0.496)	(0.430)	(0.815)	(0.561)
law_13	-1.409***	0.155	0.251	0.251	0.284
	(0.250)	(0.486)	(0.422)	(0.206)	(0.269)

o.law_14		-	-	-	-
law_15	-0.131	0.0371	-0.325	-0.325	-0.426
	(0.311)	(0.779)	(0.677)	(0.554)	(0.382)
law_16	-5.427***	-3.139***	-2.635***	-2.635***	-2.403***
	(0.714)	(0.653)	(0.569)	(0.351)	(0.285)
law_17	3.844***	4.471***	3.272***	3.272***	3.420***
	(0.897)	(0.984)	(0.856)	(0.532)	(0.504)
law_18	1.959***	-1.379*	-0.645	-0.645	-0.200
	(0.618)	(0.798)	(0.693)	(0.620)	(0.805)
law_19	1.794***	0.253	0.0139	0.0139	-0.0746
	(0.283)	(0.440)	(0.381)	(0.653)	(0.326)
law_20	0.607**	1.226***	0.599*	0.599	0.0587
	(0.253)	(0.357)	(0.312)	(0.427)	(0.221)
law_21	0.299	-0.934***	-0.305	-0.305	0.323
	(0.358)	(0.350)	(0.306)	(0.234)	(0.363)
law_22	-2.312***	-0.0177	-0.190	-0.190	-0.361
	(0.352)	(0.492)	(0.430)	(0.523)	(0.387)
law_23	-0.313	0.826**	0.473	0.473	-0.201
	(0.410)	(0.417)	(0.362)	(0.292)	(0.286)
law_24	2.573***	-0.469	-1.045**	-1.045**	-0.728*
	(0.488)	(0.489)	(0.425)	(0.406)	(0.370)
law_25	1.792***	-0.617	-0.806**	-0.806	-0.146
	(0.353)	(0.441)	(0.384)	(0.588)	(0.333)
law_26	-0.847*	-0.262	-0.777*	-0.777*	-0.687**
	(0.472)	(0.509)	(0.442)	(0.400)	(0.337)
law_27	2.758***	-0.603	-0.534	-0.534	1.668***
	(0.434)	(0.813)	(0.704)	(0.562)	(0.456)
o.law_28		-	-	-	-
law_29	-2.811***	0.799	0.292	0.292	-2.008***
	(0.507)	(1.051)	(0.912)	(0.802)	(0.560)
law_30	-1.681***	-0.529	-0.522*	-0.522	-0.0440
	(0.284)	(0.338)	(0.298)	(0.340)	(0.434)
law_31	8.421***	1.616	0.982	0.982	-0.674
	(1.320)	(1.332)	(1.154)	(1.125)	(0.870)
law_32	-7.758***	-1.912	-1.157	-1.157	0.341
	(1.310)	(1.338)	(1.163)	(1.105)	(0.809)
law_33	4.082***	1.890	1.336	1.336***	2.206***
	(0.984)	(1.178)	(1.015)	(0.461)	(0.355)

law_34	-4.664***	-1.845	-1.356	-1.356**	-1.346**
	(0.972)	(1.212)	(1.043)	(0.517)	(0.522)
law_35	-4.030***	-3.698***	-2.336**	-2.336***	-0.122
	(1.186)	(1.330)	(1.157)	(0.665)	(0.657)
law_36	3.835***	3.798***	2.742**	2.742***	0.916
	(1.198)	(1.354)	(1.176)	(0.813)	(0.643)
law_37	0.755***	0.242	-0.0830	-0.0830	0.128
	(0.292)	(0.161)	(0.162)	(0.193)	(0.168)
law_38	-1.120***	-0.282	-0.201	-0.201	-0.363
	(0.277)	(0.408)	(0.355)	(0.445)	(0.347)
law_39	1.070***	-0.422	-0.600	-0.600	-0.197
	(0.232)	(0.714)	(0.618)	(0.723)	(0.424)
law_40	-0.793***	-0.656	-0.235	-0.235	0.859**
	(0.303)	(0.761)	(0.661)	(0.797)	(0.360)
law_41	-0.537	-1.007***	-0.311	-0.311	-0.537*
	(0.437)	(0.253)	(0.258)	(0.317)	(0.318)
law_42	3.428***	3.391***	-0.289	-0.289	-0.0339
	(1.324)	(0.723)	(0.712)	(0.803)	(0.576)
law_43	0.381	0.751***	-0.372	-0.372	-0.456
	(0.495)	(0.279)	(0.312)	(0.392)	(0.372)
law_44	0.217	-0.185	0.0987	0.0987	-0.127
	(0.495)	(0.325)	(0.380)	(0.446)	(0.508)
law_45	-3.782***	-2.844***	0.521	0.521	0.352
	(1.357)	(0.759)	(0.773)	(0.799)	(0.619)
law_46	0.958	-0.217	0.237	0.237	0.221
	(0.658)	(0.378)	(0.370)	(0.438)	(0.366)
law_47	-0.484	0.457	-0.759	-0.759	-1.042**
	(1.030)	(1.041)	(0.975)	(0.651)	(0.479)
law_48	0.649	0.0743	0.757	0.757	1.075**
	(0.976)	(1.025)	(0.941)	(0.628)	(0.476)
law_49	-1.584	-2.630	-1.307	-1.307	-2.189**
	(1.190)	(1.632)	(1.479)	(1.222)	(0.915)
law_50	-1.701	-0.499	1.623	1.623*	2.073**
	(2.234)	(1.412)	(1.284)	(0.903)	(0.786)
law_51	0.348	0.0315	-0.295	-0.295	-0.244
	(0.438)	(0.249)	(0.253)	(0.335)	(0.312)
law_52	0.667	-0.280	0.938	0.938	1.421**
	(1.299)	(1.128)	(1.040)	(0.783)	(0.640)
law_53	-0.625	0.571	-0.595	-0.595	-0.927
	(1.255)	(1.113)	(1.011)	(0.745)	(0.705)

law_54		3.611***	-0.151	-0.151	-0.0335
		(0.987)	(0.936)	(1.008)	(0.720)
o.law_55		-	-	-	-
law_56	-0.615	0.108	-0.0500	-0.0500	-0.119
	(0.501)	(0.302)	(0.339)	(0.430)	(0.324)
law_57	- 3.110***	-2.491***	0.743	0.743	0.233
	(0.980)	(0.583)	(0.593)	(0.603)	(0.529)
law_58	1.308	1.097**	-0.502	-0.502	0.0932
	(0.869)	(0.476)	(0.494)	(0.513)	(0.404)
law_59	0.632	0.546**	-0.340	-0.340	-0.308
	(0.412)	(0.254)	(0.274)	(0.265)	(0.287)
law_60	-0.119	1.121***	0.363	0.363	0.346
	(0.523)	(0.281)	(0.295)	(0.239)	(0.221)
law_61	0.241	0.539*	-0.253	-0.253	-0.481
	(0.542)	(0.300)	(0.344)	(0.355)	(0.288)
law_62	-1.388**	-0.851**	0.0502	0.0502	0.0305
	(0.621)	(0.357)	(0.351)	(0.379)	(0.318)
law_63	1.682***	0.742**	-0.299	-0.299	-0.178
	(0.470)	(0.293)	(0.304)	(0.257)	(0.280)
law_64	-0.645	0.211	0.407	0.407	0.0164
	(0.511)	(0.329)	(0.291)	(0.306)	(0.319)
law_65	-0.452	-1.278***	-0.118	-0.118	-0.235
	(0.725)	(0.423)	(0.458)	(0.506)	(0.373)
law_66		-2.220	0.321	0.321	-0.681
		(1.722)	(1.745)	(1.091)	(1.095)
law_67	1.326	4.630***	0.0112	0.0112	0.580
	(0.862)	(1.666)	(1.674)	(1.057)	(1.051)
law_68	0.0226	1.140	2.665*	2.665***	3.165***
	(0.489)	(1.589)	(1.418)	(0.786)	(0.774)
law_69		-1.375	-2.622*	-2.622***	-2.812***
		(1.573)	(1.442)	(0.892)	(0.855)
law_70	-0.381	-0.0235	0.250	0.250	0.0832
	(0.367)	(0.209)	(0.249)	(0.288)	(0.289)
law_71	0.828*	0.331	0.307	0.307	0.473*
	(0.499)	(0.260)	(0.268)	(0.323)	(0.268)
law_72	-0.787	-0.204	-0.194	-0.194	-0.403
	(0.639)	(0.352)	(0.349)	(0.312)	(0.298)
law_73	-0.140	-0.441	-0.589*	-0.589*	-0.853***
	(0.453)	(0.281)	(0.317)	(0.304)	(0.273)
law_74	0.775***	0.396**	0.305	0.305	0.369
	(0.298)	(0.185)	(0.205)	(0.248)	(0.223)
law_75	4.077***	0.449**	-0.296	-0.296	-0.338*
	(1.266)	(0.206)	(0.231)	(0.214)	(0.186)

o.law_76		-	-	-	-
law_77	0.614	2.819***	0.802	0.802	1.068
	(1.307)	(1.032)	(1.037)	(0.939)	(0.834)
law_78	0.0483	0.804	0.983	0.983	0.645
	(0.849)	(1.024)	(0.934)	(0.646)	(0.651)
law_79	1.225	0.444	0.347	0.347	-0.0682
	(0.744)	(1.203)	(1.054)	(0.625)	(0.455)
law_80	-1.646**	-0.993	-0.423	-0.423	-0.112
	(0.758)	(1.214)	(1.069)	(0.570)	(0.370)
law_81	-2.695**	-0.855	-0.441	-0.441	0.0207
	(1.086)	(1.406)	(1.247)	(0.717)	(0.546)
law_82	1.973*	0.149	0.0739	0.0739	-0.135
	(1.026)	(1.388)	(1.226)	(0.759)	(0.630)
law_83	-6.753***	-0.508	0.413	0.413	0.741**
	(1.836)	(0.366)	(0.378)	(0.421)	(0.347)
o.law_84		-	-	-	-
law_85	-0.381	0.0846	-0.417	-0.417	-0.560
	(0.595)	(0.331)	(0.359)	(0.446)	(0.375)
o.law_86	-	-	-	-	-
law_87	-0.319	-0.507*	-0.508*	-0.508*	-0.351
	(0.481)	(0.272)	(0.265)	(0.266)	(0.300)
law_88	-0.481	-0.364*	-0.175	-0.175	-0.0954
	(0.368)	(0.200)	(0.218)	(0.275)	(0.238)
law_89	-0.948***	-1.312***	0.0203	0.0203	0.0732
	(0.309)	(0.169)	(0.192)	(0.176)	(0.157)
law_90	0.503*	0.330**	-0.127	-0.127	-0.0591
	(0.266)	(0.153)	(0.149)	(0.165)	(0.133)
law_91	0.331	-0.537	-0.718	-0.718	-0.890*
	(0.653)	(0.680)	(0.637)	(0.552)	(0.492)
law_92	1.157***	0.777***	0.455*	0.455*	0.476**
	(0.378)	(0.220)	(0.233)	(0.243)	(0.229)
o.law_93	-	-	-	-	-
law_94	0.610	0.231	0.287	0.287	0.224
	(0.431)	(0.244)	(0.282)	(0.234)	(0.212)
law_95	-0.937	-1.036***	0.320	0.320	0.487
	(0.595)	(0.373)	(0.389)	(0.415)	(0.426)
law_96	1.447**	0.450	-0.245	-0.245	0.282
	(0.593)	(0.328)	(0.319)	(0.294)	(0.202)
law_97	-	-2.690***	-0.323	-0.323	0.138

	2.780***				
	(0.794)	(0.505)	(0.550)	(0.537)	(0.474)
law_98	1.797**	2.061***	0.974	0.974	0.250
	(0.903)	(0.535)	(0.624)	(0.646)	(0.456)
law_99	-0.00557	1.321***	0.522	0.522	0.506
	(0.716)	(0.407)	(0.461)	(0.515)	(0.441)
law_100	4.709***	1.105	0.289	0.289	0.893**
	(0.964)	(1.037)	(0.942)	(0.485)	(0.424)
law_101	- 4.137***	-0.222	-0.243	-0.243	-0.933*
	(0.986)	(1.049)	(0.956)	(0.544)	(0.512)
law_102	-0.440	-0.821***	0.0979	0.0979	-0.0384
	(0.337)	(0.199)	(0.224)	(0.208)	(0.181)
Constant	14.61***	9.955***	10.71***	10.71***	102.2***
	(1.456)	(1.182)	(1.176)	(1.056)	(38.11)
Observations	918	918	918	918	918
R-squared	0.738	0.601	0.712	0.712	0.809
Number of state_id		51	51	51	51
State FE		YES	YES	YES	YES
Year FE			YES	YES	YES
Clustered Standard Errors				YES	YES
Time Trend Interaction					YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8.2.1:

	(5)	(6)
	FE	XPO Lasso
VARIABLES	state-specific linear time trend	state-specific linear time trend
Outcome variable of interest:	suicide_r	suicide_r
law_0	2.636***	-
	(0.691)	
law_1	-0.921***	2.110***
	(0.303)	(0.523)
law_2	0.323	-1.516***
	(0.274)	(0.577)
law_3	0.808	-1.066

	(0.540)	(1.132)
o.law_4	-	-
law_5	-1.043**	0.620
	(0.491)	(0.784)
law_6	-0.489**	1.205**
	(0.234)	(0.585)
law_7	-0.664*	-1.612***
	(0.377)	(0.600)
law_8	-0.858*	-2.294**
	(0.489)	(1.076)
o.law_9	-	-
law_10	0.849*	2.234****
	(0.450)	(0.577)
o.law_11	-	-
law_12	-0.873	-0.272
	(0.561)	(0.652)
law_13	0.284	-0.340
	(0.269)	(0.407)
law_14	-	-3.732***
		(1.350)
law_15	-0.426	-0.468
	(0.382)	(0.599)
law_16	-2.403***	-2.874****
	(0.285)	(0.620)
law_17	3.420***	5.789****
	(0.504)	(0.864)
law_18	-0.200	0.705
	(0.805)	(0.766)
law_19	-0.0746	-2.467****
	(0.326)	(0.428)
law_20	0.0587	-0.795**
	(0.221)	(0.357)
law_21	0.323	0.0882
	(0.363)	(0.420)
law_22	-0.361	-0.403
	(0.387)	(0.477)
law_23	-0.201	0.222
	(0.286)	(0.512)
law_24	-0.728*	-0.321
	(0.370)	(0.554)
law_25	-0.146	1.942****
	(0.333)	(0.503)

law_26	-0.687**	0.957
	(0.337)	(0.987)
law_27	1.668***	-1.819***
	(0.456)	(0.637)
law_28	-	-0.364
		(0.907)
law_29	-2.008***	-1.566**
	(0.560)	(0.746)
law_30	-0.0440	-1.251***
	(0.434)	(0.395)
law_31	-0.674	8.916****
	(0.870)	(2.061)
law_32	0.341	-7.776***
	(0.809)	(2.449)
law_33	2.206***	0.666
	(0.355)	(0.771)
law_34	-1.346**	-1.584
	(0.522)	(0.999)
law_35	-0.122	2.454**
	(0.657)	(0.995)
law_36	0.916	-3.896***
	(0.643)	(1.256)
law_37	0.128	0.0193
	(0.168)	(0.183)
law_38	-0.363	-1.601****
	(0.347)	(0.365)
law_39	-0.197	2.174****
	(0.424)	(0.519)
law_40	0.859**	2.036***
	(0.360)	(0.630)
law_41	-0.537*	-0.557**
	(0.318)	(0.241)
law_42	-0.0339	0.349
	(0.576)	(0.349)
law_43	-0.456	0.0599
	(0.372)	(0.289)
law_44	-0.127	-0.481
	(0.508)	(0.442)
law_45	0.352	0.325
	(0.619)	(0.407)
law_46	0.221	-0.0493
	(0.366)	(0.332)
law_47	-1.042**	-0.224
	(0.479)	(0.816)
law_48	1.075**	-0.291

	(0.476)	(0.926)
law_49	-2.189**	-
	(0.915)	
law_50	2.073**	-
	(0.786)	
law_51	-0.244	-0.421
	(0.312)	(0.240)
law_52	1.421**	0.456
	(0.640)	(0.615)
law_53	-0.927	-1.210
	(0.705)	(0.635)
law_54	-0.0335	-
	(0.720)	
o.law_55	-	-
law_56	-0.119	-0.109
	(0.324)	(0.234)
law_57	0.233	0.0894
	(0.529)	(0.374)
law_58	0.0932	0.0684
	(0.404)	(0.305)
law_59	-0.308	-0.288
	(0.287)	(0.247)
law_60	0.346	-0.459
	(0.221)	(0.301)
law_61	-0.481	-
	(0.288)	
law_62	0.0305	-0.295
	(0.318)	(0.279)
law_63	-0.178	0.394
	(0.280)	(0.301)
law_64	0.0164	-
	(0.319)	
law_65	-0.235	-0.0165
	(0.373)	(0.410)
law_66	-0.681	-
	(1.095)	
law_67	0.580	-
	(1.051)	
law_68	3.165***	0.793
	(0.774)	(0.630)
law_69	-2.812***	-
	(0.855)	
law_70	0.0832	0.0450
	(0.289)	(0.250)

law_71	0.473*	0.227
	(0.268)	(0.310)
law_72	-0.403	0.311
	(0.298)	(0.300)
law_73	-0.853***	-0.182
	(0.273)	(0.331)
law_74	0.369	0.0806
	(0.223)	(0.224)
law_75	-0.338*	-
	(0.186)	
law_76	-	0.473
		(1.067)
law_77	1.068	0.715
	(0.834)	(0.762)
law_78	0.645	1.986****
	(0.651)	(0.413)
law_79	-0.0682	-0.506
	(0.455)	(0.571)
law_80	-0.112	-0.839**
	(0.370)	(0.401)
law_81	0.0207	-0.513
	(0.546)	(0.445)
law_82	-0.135	-0.202
	(0.630)	(0.678)
law_83	0.741**	-
	(0.347)	
o.law_84	-	-
law_85	-0.560	-
	(0.375)	
o.law_86	-	-
law_87	-0.351	-0.789
	(0.300)	(0.280)
law_88	-0.0954	0.0627
	(0.238)	(0.202)
law_89	0.0732	-0.0760
	(0.157)	(0.190)
law_90	-0.0591	-0.198
	(0.133)	(0.172)
law_91	-0.890*	-1.142***
	(0.492)	(0.384)
law_92	0.476**	-0.0471
	(0.229)	(0.234)
o.law_93	-	-

law_94	0.224	0.129
	(0.212)	(0.355)
law_95	0.487	0.672
	(0.426)	(0.348)
law_96	0.282	0.383
	(0.202)	(0.229)
law_97	0.138	-1.464***
	(0.474)	(0.492)
law_98	0.250	0.819
	(0.456)	(0.524)
law_99	0.506	-0.0158
	(0.441)	(0.409)
law_100	0.893**	0.206
	(0.424)	(0.902)
law_101	-0.933*	-0.428
	(0.512)	(0.653)
law_102	-0.0384	0.0745
	(0.181)	(0.244)
Constant	102.2***	-
	(38.11)	
Observations	918	918
R-squared	0.809	-
State FE	YES	YES
Year FE	YES	YES
Clustered Standard Errors	YES	YES
Time Trend Interaction	YES	YES

Robust standard errors in parentheses
**** p<0.001 *** p<0.01, ** p<0.05, * p<0.1
Note: **** not available to Model (5) output.

Table 8.3.1: RF Variable Importance for Predicting the Suicide Rate

Law Feature	IncNodePurity	Law Feature	IncNodePurity	Law Feature	IncNodePurity
law_0	237.20	law_35	11.14	law_69	3.09
law_1	42.52	law_36	9.90	law_70	6.96
law_2	25.35	law_37	2.63	law_71	3.61
law_3	490.18	law_38	170.85	law_72	1.52
law_4	234.69	law_39	28.48	law_73	3.39
law_5	36.60	law_40	24.20	law_74	3.84
law_6	42.43	law_41	2.14	law_75	2.21
law_7	67.24	law_42	1.62	law_76	2.98
law_8	95.43	law_43	4.11	law_77	0.95
law_9	238.50	law_44	1.09	law_78	2.95
law_10	25.25	law_45	0.41	law_79	3.07
law_11	23.35	law_46	2.63	law_80	3.12

law_12	465.98	**law_47**	5.16	**law_81**	4.11
law_13	106.79	**law_48**	3.28	**law_82**	2.96
law_14	13.59	**law_49**	2.81	**law_83**	0.86
law_15	14.76	**law_50**	4.69	**law_84**	2.41
law_16	26.78	**law_51**	1.35	**law_85**	0.11
law_17	11.40	**law_52**	6.17	**law_86**	0.10
law_18	11.51	**law_53**	4.03	**law_87**	3.80
law_19	70.23	**law_54**	2.51	**law_88**	9.40
law_20	8.10	**law_55**	4.53	**law_89**	7.34
law_21	11.44	**law_56**	3.67	**law_90**	4.61
law_22	134.93	**law_57**	2.81	**law_91**	2.30
law_23	46.99	**law_58**	2.39	**law_92**	4.02
law_24	22.13	**law_59**	1.99	**law_93**	0.88
law_25	13.54	**law_60**	3.62	**law_94**	6.03
law_26	29.47	**law_61**	3.26	**law_95**	2.90
law_27	10.69	**law_62**	0.89	**law_96**	3.64
law_28	13.01	**law_63**	7.65	**law_97**	1.47
law_29	19.33	**law_64**	0.94	**law_98**	2.02
law_30	381.70	**law_65**	2.33	**law_99**	2.40
law_31	32.04	**law_66**	0.86	**law_100**	3.22
law_32	37.29	**law_67**	2.56	**law_101**	4.48
law_33	29.41	**law_68**	1.96	**law_102**	2.27
law_34	11.85				

Figure 8.3.2: Suicide Rate (y-axis) in Wyoming Time-series

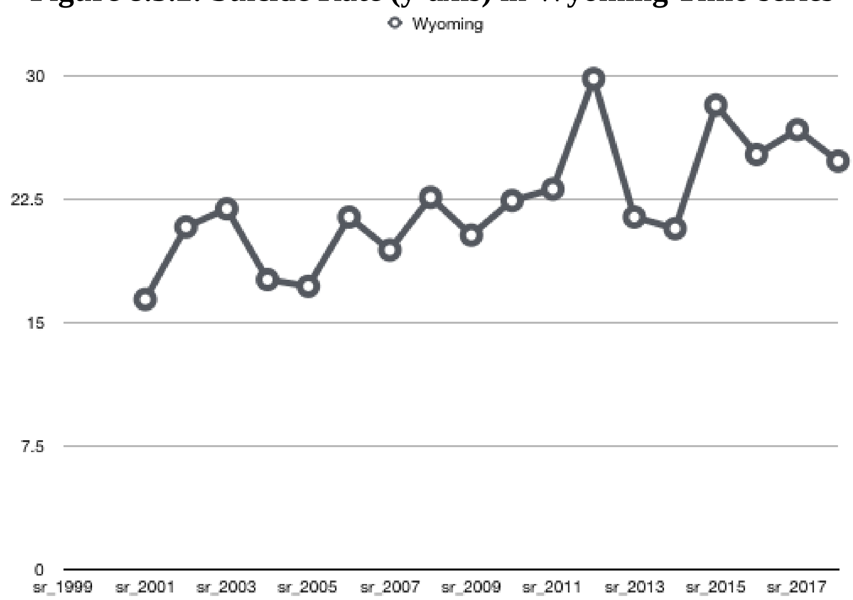

Table 8.3.3: RF Prediction of the Suicide Rate

state	year	s_predict_rf	suicide_r	diff_rf_sr	state	year	s_predict_rf	suicide_r	diff_rf_sr
AK	**2014**	25.11	22	3.11	**MS**	**2016**	14.40	12.7	1.70
AK	**2015**	23.42	26.8	-3.38	**MS**	**2017**	14.20	15	-0.80
AK	**2016**	24.93	25.4	-0.47	**MS**	**2018**	15.04	13.7	1.34
AK	**2017**	24.41	27.1	-2.69	**MT**	**2014**	25.37	23.8	1.57
AK	**2018**	25.01	24.4	0.61	**MT**	**2015**	25.38	25.3	0.08
AL	**2014**	17.38	14.5	2.88	**MT**	**2016**	24.58	26	-1.42
AL	**2015**	16.25	14.9	1.35	**MT**	**2017**	24.55	28.9	-4.35
AL	**2016**	15.88	15.6	0.28	**MT**	**2018**	25.94	24.9	1.04
AL	**2017**	17.08	16.6	0.48	**NC**	**2014**	13.25	13	0.25
AL	**2018**	16.65	16.5	0.15	**NC**	**2015**	13.05	13.4	-0.35
AR	**2014**	17.24	17.2	0.04	**NC**	**2016**	13.34	13	0.34
AR	**2015**	17.70	19.1	-1.40	**NC**	**2017**	14.05	14.3	-0.25

AR	**2016**	18.21	18.2	0.01	**NC**	**2018**	14.13	13.7	0.43
AR	**2017**	18.44	20.7	-2.26	**ND**	**2014**	17.41	17.5	-0.09
AR	**2018**	18.99	18.4	0.59	**ND**	**2015**	17.98	17.4	0.58
AZ	**2014**	17.78	18	-0.22	**ND**	**2016**	17.44	19	-1.56
AZ	**2015**	17.57	18.2	-0.63	**ND**	**2017**	18.24	20.5	-2.26
AZ	**2016**	18.14	17.6	0.54	**ND**	**2018**	19.13	18.8	0.33
AZ	**2017**	18.03	18.1	-0.07	**NE**	**2014**	12.78	13.4	-0.62
AZ	**2018**	17.79	19.2	-1.41	**NE**	**2015**	13.60	11.6	2.00
CA	**2014**	10.87	10.5	0.37	**NE**	**2016**	12.93	13	-0.07
CA	**2015**	10.76	10.2	0.56	**NE**	**2017**	13.72	14.7	-0.98
CA	**2016**	10.73	10.5	0.23	**NE**	**2018**	14.24	13.4	0.84
CA	**2017**	10.98	10.4	0.58	**NH**	**2014**	16.86	17.6	-0.74
CA	**2018**	10.96	10.8	0.16	**NH**	**2015**	17.71	16.6	1.11
CO	**2014**	20.14	19.8	0.34	**NH**	**2016**	17.63	17.3	0.33
CO	**2015**	20.01	19.5	0.51	**NH**	**2017**	18.22	18.8	-0.58
CO	**2016**	19.90	20.5	-0.60	**NH**	**2018**	18.29	19.3	-1.01
CO	**2017**	19.98	20.4	-0.42	**NJ**	**2014**	8.40	8.3	0.10
CO	**2018**	19.69	21.8	-2.11	**NJ**	**2015**	8.42	8.3	0.12
CT	**2014**	10.77	9.7	1.07	**NJ**	**2016**	8.89	7.2	1.69
CT	**2015**	9.78	9.8	-0.02	**NJ**	**2017**	8.79	8.4	0.39
CT	**2016**	9.98	10	-0.02	**NJ**	**2018**	9.13	8.3	0.83
CT	**2017**	10.85	10.5	0.35	**NM**	**2014**	22.43	21	1.43
CT	**2018**	10.12	10.5	-0.38	**NM**	**2015**	21.81	23.5	-1.69
DC	**2014**	9.31	7.7	1.61	**NM**	**2016**	22.33	22.5	-0.17
DC	**2015**	7.07	4.9	2.17	**NM**	**2017**	22.69	23.3	-0.61
DC	**2016**	6.84	5.1	1.74	**NM**	**2018**	22.20	25	-2.80
DC	**2017**	6.59	6.4	0.19	**NV**	**2014**	18.91	19.5	-0.59
DC	**2018**	6.43	7.4	-0.97	**NV**	**2015**	18.71	18.4	0.31
DE	**2014**	12.10	13.2	-1.10	**NV**	**2016**	18.24	21.4	-3.16
DE	**2015**	12.22	12.5	-0.28	**NV**	**2017**	18.38	20.3	-1.92
DE	**2016**	12.56	11.5	1.06	**NV**	**2018**	18.25	20.8	-2.55
DE	**2017**	12.47	11.6	0.87	**NY**	**2014**	8.27	8.1	0.17
DE	**2018**	12.79	11.4	1.39	**NY**	**2015**	8.33	7.8	0.53
FL	**2014**	15.14	13.8	1.34	**NY**	**2016**	8.39	8.1	0.29
FL	**2015**	14.82	14.4	0.42	**NY**	**2017**	8.67	8.1	0.57
FL	**2016**	14.88	13.9	0.98	**NY**	**2018**	8.67	8.3	0.37
FL	**2017**	16.12	13.9	2.22	**OH**	**2014**	14.49	12.6	1.89
FL	**2018**	15.46	15.1	0.36	**OH**	**2015**	14.35	13.9	0.45
GA	**2014**	14.09	12.7	1.39	**OH**	**2016**	14.63	14.1	0.53
GA	**2015**	14.29	12.7	1.59	**OH**	**2017**	14.70	14.8	-0.10
GA	**2016**	13.95	13.3	0.65	**OH**	**2018**	14.65	15.2	-0.55
GA	**2017**	14.66	13.6	1.06	**OK**	**2014**	19.23	19.1	0.13
GA	**2018**	14.46	14.5	-0.04	**OK**	**2015**	18.83	20.4	-1.57
HI	**2014**	12.64	13.7	-1.06	**OK**	**2016**	19.44	20.9	-1.46
HI	**2015**	12.91	13.5	-0.59	**OK**	**2017**	19.68	19.1	0.58
HI	**2016**	12.57	12	0.57	**OK**	**2018**	19.71	20	-0.29
HI	**2017**	11.56	15	-3.44	**OR**	**2014**	17.71	18.7	-0.99
HI	**2018**	13.04	11.8	1.24	**OR**	**2015**	18.23	17.8	0.43
IA	**2014**	14.09	12.8	1.29	**OR**	**2016**	18.50	17.8	0.70
IA	**2015**	13.56	14	-0.44	**OR**	**2017**	18.15	19	-0.85
IA	**2016**	13.56	14.5	-0.94	**OR**	**2018**	18.08	19	-0.92
IA	**2017**	14.43	15.1	-0.67	**PA**	**2014**	14.56	13.3	1.26
IA	**2018**	13.99	15.5	-1.51	**PA**	**2015**	14.25	13.9	0.35
ID	**2014**	21.12	20.1	1.02	**PA**	**2016**	14.33	14.7	-0.37
ID	**2015**	20.35	22.2	-1.85	**PA**	**2017**	14.48	15	-0.52
ID	**2016**	21.42	21.3	0.12	**PA**	**2018**	13.38	14.9	-1.52
ID	**2017**	21.08	23.2	-2.12	**RI**	**2014**	12.03	10	2.03
ID	**2018**	21.18	23.9	-2.72	**RI**	**2015**	11.51	11.2	0.31
IL	**2014**	10.56	10.4	0.16	**RI**	**2016**	11.75	11.1	0.65
IL	**2015**	10.35	10.3	0.05	**RI**	**2017**	11.10	11.8	-0.70
IL	**2016**	10.31	10.7	-0.39	**RI**	**2018**	11.92	9.6	2.32
IL	**2017**	10.14	11.2	-1.06	**SC**	**2014**	15.04	15.1	-0.06

IL	**2018**	10.57	11.3	-0.73	**SC**	**2015**	15.37	14.8	0.57
IN	**2014**	15.69	14.3	1.39	**SC**	**2016**	15.53	15.7	-0.17
IN	**2015**	15.47	14.4	1.07	**SC**	**2017**	15.71	16.2	-0.49
IN	**2016**	16.01	15.4	0.61	**SC**	**2018**	15.74	15.5	0.24
IN	**2017**	15.72	16.4	-0.68	**SD**	**2014**	19.65	17.1	2.55
IN	**2018**	15.88	16	-0.12	**SD**	**2015**	18.84	20.6	-1.76
KS	**2014**	17.09	15.7	1.39	**SD**	**2016**	19.97	20.5	-0.53
KS	**2015**	18.28	16.2	2.08	**SD**	**2017**	19.31	22.4	-3.09
KS	**2016**	17.46	17.9	-0.44	**SD**	**2018**	20.24	19.2	1.04
KS	**2017**	18.09	19	-0.91	**TN**	**2014**	16.35	14.8	1.55
KS	**2018**	17.87	19.2	-1.33	**TN**	**2015**	16.23	15.6	0.63
KY	**2014**	17.00	15.9	1.10	**TN**	**2016**	16.34	16.3	0.04
KY	**2015**	17.27	17.1	0.17	**TN**	**2017**	16.73	16.9	-0.17
KY	**2016**	17.56	16.8	0.76	**TN**	**2018**	16.54	16.6	-0.06
KY	**2017**	18.17	17	1.17	**TX**	**2014**	12.94	12.2	0.74
KY	**2018**	17.83	17.4	0.43	**TX**	**2015**	12.89	12.4	0.49
LA	**2014**	14.91	14.3	0.61	**TX**	**2016**	13.12	12.6	0.52
LA	**2015**	15.14	15.3	-0.16	**TX**	**2017**	13.36	13.3	0.06
LA	**2016**	15.48	14.1	1.38	**TX**	**2018**	13.16	13.7	-0.54
LA	**2017**	15.49	15.2	0.29	**UT**	**2014**	21.16	20.6	0.56
LA	**2018**	15.65	15.1	0.55	**UT**	**2015**	20.73	22.4	-1.67
MA	**2014**	9.66	8.3	1.36	**UT**	**2016**	21.37	21.8	-0.43
MA	**2015**	9.14	8.9	0.24	**UT**	**2017**	20.05	22.7	-2.65
MA	**2016**	9.29	8.7	0.59	**UT**	**2018**	20.12	22.1	-1.98
MA	**2017**	9.12	9.4	-0.28	**VA**	**2014**	13.23	12.9	0.33
MA	**2018**	9.00	9.9	-0.90	**VA**	**2015**	13.10	12.7	0.40
MD	**2014**	9.80	9.8	0.00	**VA**	**2016**	13.04	13.2	-0.16
MD	**2015**	9.80	8.8	1.00	**VA**	**2017**	13.67	13.3	0.37
MD	**2016**	9.61	9.3	0.31	**VA**	**2018**	13.41	14	-0.59
MD	**2017**	9.77	9.9	-0.13	**VT**	**2014**	17.93	18.6	-0.67
MD	**2018**	9.66	10.1	-0.44	**VT**	**2015**	17.36	14.8	2.56
ME	**2014**	16.63	15.7	0.93	**VT**	**2016**	16.11	17.3	-1.19
ME	**2015**	17.34	16.1	1.24	**VT**	**2017**	16.71	18.4	-1.69
ME	**2016**	17.52	15.7	1.82	**VT**	**2018**	13.61	18.7	-5.09
ME	**2017**	17.61	18.8	-1.19	**WA**	**2014**	15.44	15.2	0.24
ME	**2018**	18.14	18.4	-0.26	**WA**	**2015**	15.28	15.4	-0.12
MI	**2014**	13.34	13.2	0.14	**WA**	**2016**	15.71	14.8	0.91
MI	**2015**	13.57	13.7	-0.13	**WA**	**2017**	15.34	16.9	-1.56
MI	**2016**	13.31	13.3	0.01	**WA**	**2018**	15.63	15.9	-0.27
MI	**2017**	14.47	14.1	0.37	**WI**	**2014**	14.88	13.1	1.78
MI	**2018**	13.72	15	-1.28	**WI**	**2015**	14.14	14.6	-0.46
MN	**2014**	13.26	12.3	0.96	**WI**	**2016**	14.83	14.6	0.23
MN	**2015**	13.20	13.2	0.00	**WI**	**2017**	14.70	15.5	-0.80
MN	**2016**	13.23	13.2	0.03	**WI**	**2018**	14.85	14.8	0.05
MN	**2017**	13.23	13.9	-0.67	**WV**	**2014**	19.00	18.1	0.90
MN	**2018**	13.56	13.1	0.46	**WV**	**2015**	19.26	17.5	1.76
MO	**2014**	17.34	16.3	1.04	**WV**	**2016**	19.45	19.5	-0.05
MO	**2015**	18.25	17	1.25	**WV**	**2017**	19.50	21.2	-1.70
MO	**2016**	17.24	18.3	-1.06	**WV**	**2018**	19.06	21.1	-2.04
MO	**2017**	18.53	18.5	0.03	**WY**	**2014**	25.38	20.7	4.68
MO	**2018**	18.29	19.5	-1.21	**WY**	**2015**	21.88	28.2	-6.32
MS	**2014**	14.20	12.5	1.70	**WY**	**2016**	25.56	25.2	0.36
MS	**2015**	14.19	14	0.19	**WY**	**2017**	24.93	26.7	-1.77
					WY	**2018**	25.44	24.8	0.64

Honor Pledge:

This paper represents my own work in accordance with University regulations.
/s/ Declan Farmer