The Impact of Extreme Risk Protection Orders on Homicide and Suicide: Are there Any Red Flags?

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Abstract: Nineteen states have instituted Extreme Risk Protection Order (ERPO) laws, also known as red flag laws. These laws allow law enforcement, family members, and others to petition courts to temporarily remove firearms from an individual who is believed to be a danger to themselves or others. These laws have been passed with the goal of reducing firearm homicide and firearm suicide. This paper tests whether these laws save lives using a generalized synthetic control model. This preliminary study suggests ERPOs as currently written do not significantly reduce total homicide or total suicide nor firearm homicide or firearm suicide.

Conflict of Interest: None

Introduction

Extreme Risk Protection Orders (ERPOs), colloquially known as "red flag laws," are preemptive and temporary measures which authorize the removal of firearms from individuals deemed at risk of harming themselves—through mechanisms such as suicidal ideation—or others, such as individuals showing violent tendencies, making threats, domestic violence, et cetera. ERPOs are part of a broader effort by policymakers and activists to reduce the number of people killed each year by firearms. These court orders are civil, and not criminal, and have been framed as a way to protect gun owners from themselves during times of crisis (Kapoor et al. 2018).

Two states have extensive experience with ERPOs: Indiana and Connecticut. Connecticut adopted the law first, in 1999, after a mass shooting where an assailant stabbed and shot one of his bosses and three other executives before turning the gun on himself. In 2005, Indiana adopted an ERPO law in response to the shooting of five police officers, one of whom was killed, by a mentally disturbed individual (Ward 2015). As of early 2022, nineteen states have adopted ERPO laws, and thirteen of these allow family members (as well as law enforcement) to file firearm removal orders (Giffords Law Center 2021). One state, Oklahoma, has adopted an anti-red flag law, which specifically prohibits the state or any county, city, or other political subdivision from adopting red flag laws.

[TABLE 1]

The rapid adoption of red flag laws has led to considerable political and policy debate. On the one hand, thousands of Americans die yearly from both gun violence and firearm suicide. Because of this, policymakers have been pondering ways to disarm individuals who are at a high risk of committing acts of violence or self-harm while minimizing the number of low-risk individuals disarmed. In theory, policies such as this would have the largest impact on gun violence and suicide without significant political pushback. Further, by minimizing the number of law-abiding low risk individuals disarmed, the potential positive effects of gun ownership—such as criminal deterrence and self-protection—can remain in place while only high-risk individuals are impacted.

On the other hand, there are concerns over how red flag laws are currently written. Conservative legal academics have criticized ERPOs over issues surrounding due process and high rates of potential error—that is, a warrant impacting an innocent law-abiding citizen who is not a danger to himself or others. If these arguments are true, it may be possible that ERPOs are not written in a way conducive to reducing gun violence (Kopel 2021). Some policymakers may choose to be skeptical of these laws even if they do reduce violence given some of these drawbacks.

Given that thousands of Americans die yearly from both gun homicide and firearm suicide, and one of the main benefits of a red flag law would be a reduction in firearm deaths, it is important to test the impact of ERPOs empirically. This paper tests the impact of ERPO laws on homicide and suicide using panel data at the state level between 1980-2018. This paper contributes to the literature by testing a unique form of the synthetic control model not used in the previous firearms literature, as well as utilizing a large dataset in order to complete one of the most comprehensive preliminary analyses of the criminological impacts of red flag laws to date.

Previous Research

The previous research on ERPOs and their impact on homicide and suicide is limited. This may be due to, in part, data limitations: only two states, Indiana and Connecticut, have had these laws for an extended period of time, which makes testing the effects difficult. Further, the last wave of states to adopt red flag laws has been relatively recent and public controversy over these laws is a recent phenomenon (Kopel 2021; Giffords Law Center 2021; Ward 2015).

The effect of these laws is theoretically ambiguous. Firearms are an exceedingly efficient killing tool, and scholars generally acknowledge that the introduction of a firearm into any violent encounter increases the possibility of a fatality (National Research Council 2005). For example, economist and gun violence scholar Phillip Cook has noted that "[i]f you introduce a gun into a violent encounter, it increases the chance that someone will die" (Cook 1982). Temporarily removing firearms from at-risk individuals in a time of crisis may reduce violence by limiting the ability of an assailant to have access to a firearm during a violent encounter. In the case of mass public shootings, where shooters often display one or more observable behaviors before committing their crime, ERPOs may allow law enforcement to intervene before the act is committed (Silver, Simons, and Craun 2018). ERPOs, which allow law enforcement to intervene among a broader swath of the population than currently fall under Federal prohibitions on gun ownership, could save lives by preventing homicide.

The same principle applies to suicide. In fact, in Connecticut, 61% of ERPOs are issued in response to concerns about self-harm alone, only 32% being issued due to concerns about harms to others. The remainder, about 9%, were issued because the individual was believed to be a risk to both himself and/or others (Kapoor et al. 2018). This is similar to gun deaths as a whole, where suicide comprised 61.5% of all firearm deaths in 2018 (Murphy et al. 2021). Public health scholars have long argued for handgun purchase waiting periods because causing a delay may cause "suicidal impulses to pass or diminish" (Lewiecki and Miller 2013, p.29). ERPOs, being temporary orders to remove firearms from the domiciles of citizens at risk of self-harm, may have the same effect.

On the other hand, it is possible that, in practice, these laws do not do a good job identifying individuals they need to target to have a positive impact on homicide and suicide rates. In the case of homicide, evidence suggests psychiatrists and other mental health professionals cannot accurately identify individuals who pose a threat to others because individuals with psychiatric diagnoses who fit into many mass-shooter demographic profiles usually commit no crimes at all (Metzl and MacLeish 2015). In fact, individuals with mental illness are often more likely to be victims of violent crime than perpetrators of it: individuals diagnosed with schizophrenia, for example, have victimization rates 60-130% higher than the general population (Brekke et al. 2001).

This poses two issues. First, it means that red flag laws may not target at-risk populations prone to committing violence, as the law enforcement and family members filing ERPOs are not equipped to diagnose violent tendencies any more than mental health professionals are, whose ability to accurately predict the violent tendencies of their patients is debated. Second, many individuals profiled as potentially violent themselves may not only be perpetrators but also *victims* of violence, and removing their firearms may remove an effective tool of self-defense, increasing their risk of victimization (Southwick 2000; Tark and Kleck 2004; Guerette and Santana 2008). Others have argued that these laws as currently written can be used by criminals to disarm their victims or harass innocent individuals (Kopel 2021).

In terms of suicide, similar problems arise: the laws may not be targeted enough to accurately identify individuals at risk of suicide, and thus may not have the desired effect on levels of suicide. Red flag laws may also cause individuals at risk of suicide not to seek out the care they need: in Connecticut, an average of seven guns were seized each warrant, and in Indiana an average of 2.7 guns were seized in the execution of each warrant (Kapoor et al. 2018). Firearms can be procured for a wide range of prices, but it is not uncommon to see firearms sold for hundreds or even thousands of dollars. Associated accessories such as magazines, ammunition, and permitting—none of which can be used if the firearms are seized—adds to the total cost. As a result, an individual with suicidal ideation who may not want their highly valuable firearms taken for a whole host of legitimate reasons (personal protection, hunting, sport) may be reticent to seek help out of fear of themselves being reported and having their valuables taken. Even if such orders are temporary, in Indiana it can often take over 140 days for a hearing to convene, and the duration of the order itself can last up to a year (Kopel 2021).

It is possible that all the potential impacts of red flag laws listed above operate simultaneously, or it is possible that some of these effects do not exist at all. In any case, the potential effects of these laws are ambiguous enough to warrant empirical investigation. Currently, the empirical research on red flag laws is mixed. Using a synthetic control model, Kivisto and Phaelen (2018) estimated the effect of Indiana and Connecticut's ERPO laws on suicide. The authors note that the level of enforcement of red flag warrants in Connecticut increased in 2007, and they therefore tested models which code the beginning of the law in 1999 but also in 2007. The authors find that Indiana's red flag law was associated with a 7.5% reduction in firearm suicides and Connecticut's law with a reduction of 1.6%. Using the post-2007 enforcement of red flag laws as the beginning of the period, Kivisto and Phaelen find that firearm suicides fell by 13.7% (Kivisto and Phaelen 2018).

While ERPOs appear to reduce firearm suicides in Kivisto and Phaelen's research, the question of substitutability is an important one. Even if a red flag warrant removes firearms from a suicidal individual's home, the individual may go on to commit suicide using another method. Because of this, when studying the effects of gun control laws, it is important to look not only at firearm suicide—which may be directly impacted by gun laws—but also total suicide. It is possible to theorize that any substitution effect may not override any positive effect on firearm suicide since firearm suicide since has been argued to be the most lethal method (Connor, Azrael, and Miller 2019). Indeed, public health researchers contend that decreases in firearm access impact both firearm and total suicide rates (Miller et al. 2006; Lubin et al. 2010). If this is the case, individuals substituting to hanging or other forms of suicide over firearms will still lead to a net drop in total suicides. However, there exist at least ten studies which show that non-firearm suicide methods, such as hanging, can be as likely as firearms to lead to suicide completion (Kleck 2020). Other studies suggest the substitution effect is a serious confounding factor, since research suggests gun ownership levels may influence firearm suicide but do not always have an impact on total suicides (Kleck 2019). Kivisto and Phaelen (2018) test this hypothesis and find that, in Indiana, the decline in firearm suicides is in fact offset by an increase in non-firearm suicides, but that this was not the case in Connecticut, which also saw a decline in total suicides as well. The evidence from Kivisto and Phaelen (2018) seems to suggest these laws may reduce firearm suicides, but the impact on total suicides is unclear.

Some research has focused on the impact of ERPOs on specific demographics. For example, Saadi et al. (2020) looked at the impact ERPO legislation had on firearm suicide among older adults (ages 55-64 and 65+). Using a fixed effects regression model, the authors conclude that ERPOs were associated with a 2.5% decrease in firearm suicide among adults aged 65+ and 2.4% among adults aged 55-64. Although there was no correlation between ERPOs and non-firearm suicide, as expected, there was a significant relationship between ERPO laws and total suicide.

A paper by Swanson et al. (2017) attempted to estimate the number of suicides prevented by that Connecticut's ERPO law. The authors analyzed the mortality information of over 700 red flag warrant subjects, 21 of whom died from suicide (29% suicide by firearm). Using known suicide method effectiveness measures, the proportion of firearm suicide to non-firearm suicide in the state population of similar demographics as red-flag warrant subjects, and other correlates, the authors were able to create a counterfactual where they estimated that 72 more deaths would have occurred in the absence of the red flag law. The researchers replicated their analysis in Indiana and came to similar conclusions, suggesting these laws may prevent suicide (Swanson et al. 2019).

While Swanson et al.'s methods are novel and interesting, there are many issues which give pause. First, as the papers had no formal control group, the results rest solely on the assumptions of the authors in creating their counterfactual. Second, one of their model assumptions—that firearms are consistently the most effective suicide method—is not universally accepted (Kleck 2020). Finally, there are no observations in the Swanson studies prior to the law's enactment, so it is difficult to ascertain causation. Despite these limitations, the results from Swanson et al. (2017) and (2019) suggest that these laws might potentially reduce suicide.

Lott and Moody (2019) in a currently unpublished paper have tested the impact of red flag laws on suicide and homicide. While the papers above focus on suicide—as the effects of ERPOs are likely to be strongest for suicide rather than homicide—given that red flag laws are often passed in response to instances of violence, testing the impact on murder is important. The authors use both a fixed effects approach and synthetic control approach to test the impact of red flag laws. While using a synthetic control model themselves—just as Kivisto and Phaelen (2018) did—the authors critique the method as it fails to control for unobserved heterogeneity. Given that the synthetic control method functions as a cross section, they argue "unobserved permanent factors that might be correlated with both the outcome and the treatment, creating omitted variable bias"(p.2). To remedy this, they utilize a fixed effects model. Fixed effects models, the authors note, are a time series model, which means serial correlation is almost always a problem. While clustering the standard errors is the standard way to correct for this problem in criminological research, clustered standard may be underestimated if the number of states in the panel adopting the policy is low (Conley and Taber 2011). Lott and Moody use placebo law simulation to overcome this problem and find—both in their fixed effects and synthetic control models—that red flag laws do not reduce homicide or suicide.

A recent case study published by Pear et al. (2022) investigated the impact of ERPOs on gun violence in San Diego County. Like past research, the authors opted to utilize a synthetic control method on a sample of 28 California counties. San Diego was used as the treated unit because of the county's prolific use of red flag warrants—the 27 counties chosen as controls were selected because they issued no or very few warrants. The authors focused on fatal and nonfatal firearm assault injuries and self-harm injuries and conclude that ERPOs were not associated with population-level rates of firearm violence in San Diego County (Pear et al. 2022).

This paper contributes to the existing literature in multiple ways. First, it uses a new form of synthetic control model, the Generalized Synthetic Control Model (GSCM), which improves both upon the synthetic control models used in past papers but also avoids the pitfalls of difference-in-difference models used by other researchers. Second, the dataset for this paper ranges between 1980-2018, making it one of the largest datasets used to test the effect of red flag laws to date. And third, this paper directly tests whether or not the substitution effect is at play regarding the impact of ERPOs on suicide rates.

Methods

Differences-in-differences (DID) statistical techniques are perhaps the most used and well understood empirical methods in the social sciences generally. A key assumption of DID models is the parallel trends assumption, which states that, in the absence of policy intervention, the trends in both treated and non-treated units would have followed parallel paths (Angrist and Pischke 2009). Unfortunately, it is often impossible to test the parallel trends assumption directly, so scholars must rely on data prior to the treatment in order to have any confidence in the assumption. Typically, if the trends between the treatment and controls are similar prior to intervention, many scholars accept that the trends would be the same after intervention, ensuring that their model assumptions hold.

Unfortunately, if there exist unobserved time-varying confounders, the assumption can fail (Xu 2017). There are a few ways to attempt to control for this problem. The first is to use matching techniques which matches both pretreatment covariates and outcomes between treated units and control units to ensure that the control group and the treatment group are comparable. These matching processes allow scholars to create a synthetic control unit as the counterfactual to the treatment by weighting the matched control units (Abadie, Diamond, and Hainmueller 2010; Xu 2017). In the firearms literature, this method has come into prominence especially over the past few years. Indeed, the previous papers written on ERPO laws which have attempted to measure the impact of ERPOs on homicide and suicide have chosen to utilize synthetic control models (Lott and Moody 2019; Kivisto and Phaelen 2018; Pear et al. 2022). There are many benefits to using synthetic control models. Synthetic control models produce easily interpretable graphical evidence, which is especially useful when attempting to engage the public and legislators in academic research findings. The models are elegant and intuitive. Unfortunately, it is difficult to engage in hypothesis testing with synthetic control models, making it difficult to report uncertainty, and they only apply to one treated unit—in this case of most criminological studies, this means one state at a time. It makes it difficult, therefore, to test what the average treatment effect of any given intervention is.

Other scholars have tried to model time-varying heterogeneities explicitly through the use of quadratic or unit-specific linear time trends. Indeed, time trends have been used in the criminological literature to model changes such as increased 9-11 coverage, the advent of the cell phone, the internet, improved forensic techniques, improved medical procedures, changing police training and standards, and other time-varying factors (Black and Nagin 1998; Ayres and Donahue 2003; Moody and Marvell 2010). Scholars can also control for these confounders using lagged dependent variables (Lott and Moody 2019). A major drawback to correcting for time-varying confounders in this manner, however, is it reduces the degrees of freedom in a model and does not always completely solve the problem (Xu 2017). The introduction of lagged dependent variables to control for time-variant factors may lead to other concerns. For example, a lagged dependent variable is correlated with the error term in a regression, which may bias one's estimates especially when datasets are smaller (Nickell 1981).

A relatively recent literature has attempted to model these unobserved time-varying confounders semiparametrically. Bai (2009) created an interactive fixed effects (IFE) model, which uses unit-specific intercepts interacted with time-varying coefficients (also known as latent factors). This paper uses a generalized synthetic control method (GSCM) proposed by Xu (2017) which links the recent IFE literature with the synthetic control method. By unifying these two approaches, the drawbacks of using a synthetic control model can be minimized and the benefits maximized.

The GSCM first estimates an IFE model using only data from the control states, which obtains a fixed number of latent factors. The algorithm selects the number of latent factors which minimizes the mean squared prediction error. The algorithm then estimates the number of factor loadings for each treated state. Finally, the model imputes treated counterfactuals based on the number of factors and the factor loadings to each treated unit. The standard errors are calculated using bootstrapping techniques (Xu 2017). This model has many advantages over the standard synthetic control models used in past criminological studies.

First, this method allows us to *generalize* the synthetic control method across multiple treated units. Since the IFE model is estimated once, the user of the algorithm needs not find adequate matches for each treated unit one by one. Second, the GSCM calculates frequentist uncertainty estimates which makes interpretation of the uncertainty estimates straightforward. Finally, as the model can automatically calculate the number of factors with a high degree of accuracy, it makes the model less susceptible to overfitting and allows the method to be used in a very straightforward manner (Xu 2017).

Dependent Variables

The dependent variables in this study were total homicide, total suicide, firearm homicide, and firearm suicide. These variables were chosen because they are the variables most likely to be influenced by the passage of ERPO laws. The passage of ERPOs in Connecticut and Indiana was directly due to prominent homicides (Kivisto and Phaelen 2018), and, as noted above, these laws are triggered to prevent self-harm at least 60% of the time. These data were obtained from the CDC's online WONDER database. They were extracted from the National Center for Health Statistics (NCHS) and provide annual estimates for the number of homicides, suicides, and firearm homicide and suicides in all 50 states and D.C.

Choice of Control Variables

The choice of control variables in the crime equation is exceedingly important in criminological studies. Studies which include few or no statistically significant control variables have been shown to come to opposite conclusions as papers which include at least a handful of significant control variables (Moody and Marvell 2010; Kleck 2015; Kleck 2019). In order to avoid the problem of too few control variables, which would lead to omitted variable bias, it is possible to be tempted to include too many control variables as well. Too many control variables lead to over-parameterization and increases the standard errors, falsely rendering some variables insignificant which were, in fact, significant. To avoid the temptation of including both too few and too many control variables, this paper's choice of control variables was determined by (a) variables that have been used in the past firearms control literature and (b) variables which criminological theory suggests should be included.

In total, the models in this paper include up to 31 control variables. This includes various measures of law enforcement such as per capita incarceration rates, police officers per capita, and execution rates. These law enforcement variables are not included in the suicide model. Other demographic variables, such as percent white and black, are included in the model, as these have been shown to impact homicide rates (Worrall 2008). Other gun laws, such as concealed carry laws, waiting periods for handgun purchases, and minimum age requirements are also included in both the suicide and homicide models, as these may potentially impact crime and/or suicide by increasing or decreasing firearm availability. In our homicide model, the inclusion of the Fryer et al. (2013) crack-cocaine index is extremely important, as the crack-cocaine epidemic exists during the pretreatment phase of our dataset. The age structure is also included in this dataset, as crime is often considered a "young man's game," and the age structure has been shown to impact suicide rates as well (Cutright and Fernquist 2001). The specific age brackets used were percent aged 15-19, percent aged 20-24, percent aged 25-29, percent aged 30-34, percent aged 35-39, percent aged 40-44, percent aged 45-49, percent aged 50-54, percent aged 55-59, percent aged 60-64, and percent aged 65+. Economic factors such as unemployment rates, poverty rates, and military employment per capita are also included. These economic data were obtained from public datasets from the University of Kentucky Center for Poverty Research and Crime Research Prevention Center. While the effect of economic variables on crime is not always straightforward, interesting case studies, such as regional fracking booms, justify the inclusion of these variables in the crime equation (Worrall 2008; Street 2019).

Alcohol consumption, obtained from Kaplan (2019), is also included, as alcohol consumption is related not only to various forms of deaths of despair, but also aggression (Sher 2005; Heinz et al. 2011).

One economic variable included in this analysis but not many other analyses evaluating firearm polices is a state-level inequality measure. Income inequality has been increasing in the United States during the sample period (1980-2018), and many have convincingly argued that income inequality may be associated with homicide in the United States (Rowhani-Rahbar et al. 2019). The relationship between income inequality and homicide may be tempered somewhat by income. Indeed, it is possible for income inequality to increase while income increases across the board. In this situation, an increase in inequality may not precipitate a homicide increase (Daly et al. 2001). Income inequality may also be tied to suicide: research on "deaths of despair" in the United States has hypothesized that increasing income inequality due to economic deracination may be a contributing factor to increased suicide rates especially among middle aged Americans (Case and Deaton 2015). This paper uses state-level Gini coefficients to control for changes in income inequality across the study period as well as differences in inequality between states to control for various economic changes which may influence homicide and suicide.

Results

The GSCM allows us to look at the average treatment effect of states that adopted ERPO laws compared to the counterfactual of those which did not. In other words, the method estimates the average treatment effect on the treated states. This result will be abbreviated henceforth as

ATT. In this sample, the states which have adopted ERPOs during the study period are Indiana, Connecticut, California, and Washington. The ATT can be read as the mean difference between treatment states and the generated counterfactuals calculated by the synthetic controls. If an ATT is negative—below the X-axis of the graph—that means the homicide (or suicide) rate is *lower* than would be expected in the absence of a red flag law; in other words, the homicide (suicide) rate in the treated state is lower than it is for the control states. If the ATT is above positive, or above the X-axis of the graph, that means the homicide (or suicide) rate is *higher* than would be expected in the absence of a red flag law. In the pre-treatment period, the ATT should fluctuate around 0 and the CIs should comfortably cross the X axis; it is in the post-treatment period that we would expect to an ATT fluctuation. The ATT for homicide rates can be seen in Figure 1.

[FIGURE 1]

The shaded areas in Figure 1 represent the 95% confidence intervals. As can be seen, the ATT for homicide across Indiana, Connecticut, California (3 years), and Washington (1 year), is negative but not significant at any point during the study period. That is, we cannot be certain the effect is significantly different from zero. The results in Figure 1 can also be see in tabular form below (Table 2), with the ATT, 95% confidence intervals, and associated p-values for each year ten years before and after implementation of ERPOs.

[TABLE 2]

It is important to investigate the impact of ERPOs on firearm homicides as well. Indeed, if ERPOs were to reduce homicide, it would primarily be through the reduction of firearm homicides, as ERPOs are unlikely to reduce homicide rates unrelated to firearms. The ATT for firearm homicides can be seen in Figure 2.

[FIGURE 2]

Just like in Figure 1, the ATT is slightly negative, but the confidence intervals suggest the result is insignificant. Interestingly, the ATT for firearm homicide is *less* negative than it is for overall homicide: ten years after the passage of ERPOs in Indiana and Connecticut, total homicide rates were lower than would be expected by 2.1 per hundred thousand, whereas firearm homicide was lower by only 0.54 per hundred thousand. In other words, the effect is 3.8 times larger for total homicide than firearm homicide. This is the opposite of what we would expect if ERPOs were reducing homicide rates and that the reduction was causal, as it is unlikely that ERPOs would influence non-firearm homicide more than firearm homicide. Overall, the results from the GSCM in Figures 1 and 2 suggest ERPOs likely have no significant impact on homicide. The results in table form can be seen below.

[TABLE 3]

The effect of ERPOs on suicide may be more significant than the effect on homicide, as over 60% of total gun deaths are suicide, and over 60% of red flag warrants are issued due to concerns over self-harm (Kapoor et al. 2018; Murphy et al. 2021). We begin by analyzing changes in the total suicide rate. As noted earlier, it is possible for ERPOs to reduce firearm suicide rates but for there to be a substitution effect towards non-firearm suicide methods. As Kivisto and Phaelen (2018) noted, in Indiana, there was an increase in non-firearm suicides which offset the decrease in firearm suicides. Our total suicide results can be seen below.

[FIGURE 3]

As can be seen, in our GSCM results, ERPOs have no impact on total suicide rates. This comports with the results in Lott and Moody (2019) and the results from Kivisto and Phaelen (2018) in Indiana. The ATT averages around 0, with small fluctuations above and below the X-axis. The confidence intervals overlap 0 the entire period before and after ERPOs were passed, providing further evidence that ERPOs have no impact on total suicide rates in this sample. The results of the above figure are presented in tabular form below.

[TABLE 4]

Although total suicide rates are our primary metric to determine of ERPOs save lives, it is important to uncover any relation ERPOs may have with firearm suicide rates as well. If they are decreased as a result of ERPOs, it may suggest substitution is at play. If they are unchanged, it suggests ERPOs may be unsuccessful in reducing suicide rates for other reasons. The results for firearm suicide are reported below.

[FIGURE 4]

Like total suicide, the results from these tests do not show any association between the adoption of a red flag law and firearm suicide rates. If ERPOs had an impact on suicide at all, we would assume that there would be a decrease in firearm suicides. Given this result, there may not even be a substitution effect at play; current ERPOs may simply be ineffective at stopping suicide entirely.

[TABLE 5]

Discussion

ERPOs have been implemented for a variety of reasons, but they were chiefly passed to reduce gun violence and suicide. This paper is one of the first papers to analyze the impact of these laws empirically, and the first to use a generalized synthetic control model. The results of this present study suggests these laws do not reduce total homicide and total suicide. Further analysis was conducted on firearm homicide and firearm suicide, and likewise there was no effect. These results comport with past findings which suggest ERPOs do not have a strong impact on suicide or homicide (Lott and Moody 2019; Pear et al. 2022) but contradicts research by Kivisto and Phaelen (2018) and Saadi et al. (2020) which suggest these laws reduce firearm homicide. Given the results in this paper, one may conclude these laws may not be effective tools to prevent homicide or suicide.

While the results of this paper suggests ERPOs may not be effective in reducing homicide and suicide, there are important limitations to consider. First, the results of this study are dominated by just two states—Indiana and Connecticut. These are the only two states which have had these laws for at least a decade both before and after. California and Washington are included in the results, however, as noted in the results section, California and Washington only impacted the ATT for the first 3 years, in the case of California, and one year in the case of Washington. While the ATT in years 1-3 after implementation does not look radically different from the ATT in years 3-10, suggesting California and Washington's absence in the latter half of the study period probably would not have changed the results, future research after more time has passed with more data from Washington and California ought to be conducted.

Second, this paper only looks at homicide, suicide, firearm homicide, and firearm suicide. ERPOs may have an impact on other forms of violence not studied here, such as rape, assault, or robbery. It is also possible that even if ERPOs do not have a discernable impact on overall homicide rates, they may be effective for specific types of homicide, such as multiple victim public shootings. While a decline in multiple victim shootings may show up in total homicide or firearm homicide results, these changes would likely be small and difficult to detect. Future research is needed to answer these questions when more data becomes available, especially regarding these laws' impact on multiple victim public shootings.

Third, it is possible ERPOs may be rewritten in such a way to make them better targeted and more effective. So, while these laws may not prevent homicide and suicide in the states studied, it is possible that the bevy of states which have adopted these laws after the study period may have legal stipulations which cause them to be more effective than the laws in Connecticut, Indiana, California, and Washington. As more research is conducted, it may be possible to determine which ERPO systems work, and which do not.

Finally, papers such as Pear et al. (2022), which also concluded the effect of ERPOs on outcomes are null, may be better suited in some ways than papers which focus on aggregate state-level changes to test the effect of these laws. In California, only about 400 ERPO warrants were issued in the years studied (Pallin et al. 2020). This may make any impact from these laws difficult to detect at the state level due to aggregation bias, especially because warrants are not evenly distributed across the state but rather centered in specific counties (Pear et al. 2022). Studies looking at county level differences in red flag warrants, like Pear et al. (2022), may be a worthwhile endeavor in other states where longitudinal county level warrant data is available.

Despite this papers' limitations, it still forwards the literature by being one of the first papers to empirically assess the impacts of ERPOs on important criminological outcomes. Not only that, but it contributes to the study of criminology and firearms control in two key ways. First, it is the first paper to use the generalized synthetic control method in the firearms literature. This analysis demonstrates that the method developed by Xu (2017) has useful applications for criminological research. Second, this paper tests the substitution hypothesis that individuals with suicidal ideation who are barred from using firearms may switch to other non-firearm suicide methods, making efforts to control firearms an ineffective means of reducing total suicide (Kleck 1997; Kleck 2019). Within the context of red flag laws, the substitution hypothesis was not supported, as the impact of ERPOs on both total homicide and suicide firearm homicide and firearm suicide was found to be insignificant. Instead, the results suggest ERPOs are ineffective at preventing suicide period rather than preventing firearm suicide and individuals switching to other suicide methods.

Overall, the findings of this paper are null: these laws have no effect on either homicide or suicide. In spite of the null findings, the results are relevant to researchers interested in the impact of gun control on suicide substitution effects, and the null results can directly inform policymakers who see ERPOs as a method to reduce gun deaths. If other research confirms that

ERPOs are an ineffective gun control measure, other solutions to America’s gun violence problem will need to be explored.

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TABLES AND FIGURES

Table 1: Year of Adoption of State-Level ERPOs			
State	**Year**	**State**	**Year**
California	2016	Nevada	2019
Colorado	2020	New Jersey	2019
Connecticut	1999	New Mexico	2020
Delaware	2020	New York	2019
District of Columbia	2019	Oregon	2018
Florida	2018	Rhode Island	2018
Hawaii	2020	Vermont	2018
Indiana	2005	Virginia	2020
Maryland	2018	Washington	2017
Massachusetts	2018		

Data for adoption dates was taken from the Rand Corporation's Firearms Law Database Version 3.0

Table 2: Average Treatment Effect for ERPOs and Homicide					
Years Pre and Post ERPO	**ATT**	**Std. Error**	**CI Lower**	**CI Upper**	**p-value**
-10	0.3601794	0.5091738	0.6377829	1.3581417	0.4793303
-9	-0.1324016	0.461955	1.0378167	0.7730136	0.7744099
-8	0.14015757	0.5120157	0.8633749	1.14369	0.7842869
-7	-0.1263905	0.4169069	-0.943513	0.690732	0.7617661
-6	-0.189172	0.4142204	-1.001029	0.6226851	0.647891

-5	0.06820085	0.4546726	-0.822941	0.9593427	0.8807647
-4	-0.1878149	0.3733217	0.9195119	0.5438821	0.6149
-3	0.44095112	0.4164254	0.3752277	1.2571299	0.2896472
-2	0.11250326	0.4131699	0.6972949	0.9223014	0.7853968
-1	-0.0574147	0.4009292	0.8432215	0.728392	0.8861289
0	-0.1412883	0.380389	0.8868371	0.6042604	0.7103164
1	-0.7193805	0.8636763	2.4121549	0.9733939	0.4048851
2	-2.3678359	2.5877275	7.4396885	2.7040168	0.3601784
3	-1.9013521	1.5128016	4.8663887	1.0636845	0.208811
4	-1.3145585	1.8096074	4.8613237	2.2322068	0.4675734
5	-1.4246788	2.2810467	5.8954483	3.0460906	0.5322517
6	-2.0183104	2.946339	7.7930288	3.756408	0.4933293
7	-1.8174813	3.1193311	7.9312578	4.2962952	0.5601283
8	-1.5011259	2.9444204	-7.272084	4.2698321	0.6101772
9	-2.0994105	3.2196372	8.4097835	4.2109626	0.5143597
10	-2.1207495	3.3571845	8.7007101	4.4592112	0.5275798

Table 3: Average Treatment Effect for ERPOs and Firearm Homicide

Years Pre and Post ERPO	ATT	Std. Error	CI Lower	CI Upper	p-value
-10	0.164344	0.4826	-0.7815	1.1102	0.7334
-9	-0.20353	0.5261	-1.2346	0.8275	0.6988
-8	0.069424	0.4422	-0.9361	0.7972	0.8752
-7	0.055242	0.3904	-0.8205	0.71	0.8875
-6	0.069089	0.3807	-0.677	0.8152	0.856
-5	0.011411	0.473	-0.9156	0.9384	0.9808
-4	0.145833	0.4063	-0.6505	0.9422	0.7196
-3	0.176627	0.4225	-0.6515	1.0047	0.6759
-2	0.052056	0.4109	-0.7534	0.8575	0.8992
-1	0.042811	0.4101	-0.8467	0.7611	0.9169
0	0.136561	0.4543	-1.0269	0.7538	0.7637
1	0.531329	0.8897	-2.2752	1.2125	0.5504
2	-0.86596	1.2699	-3.3549	1.623	0.4953

3	1.337668	1.4803	-4.239	1.5636	0.3662
4	0.641907	1.6643	-3.9038	2.62	0.6997
5	-0.69486	2.1036	-4.8178	3.428	0.7412
6	1.014163	2.3585	-5.6366	3.6083	0.6672
7	0.427197	2.4977	-5.3225	4.4681	0.8642
8	0.566235	2.4064	-5.2827	4.1502	0.814
9	0.636791	2.6975	-5.9238	4.6502	0.8134
10	0.545253	2.9391	-6.3057	5.2152	0.8528

Table 4: Average Treatment Effect for ERPOs and Suicide					
Years Pre and Post ERPO	**ATT**	**Std. Error**	**CI Lower**	**CI Upper**	**p-value**
-10	-0.1004259	0.6475106	1.3695235	1.1686716	0.8767461
-9	0.03962845	0.5996786	1.1357201	1.214977	0.9473119
-8	0.3841076	0.5733813	0.7396991	1.5079143	0.5029221
-7	-0.0259187	0.5970217	1.1960598	1.1442223	0.965372
-6	-0.2146115	0.5544565	1.3013262	0.8721033	0.6987071
-5	0.1696708	0.5989504	1.0042504	1.343592	0.776962
-4	0.00022347	0.586539	1.1493718	1.1498188	0.999696
-3	0.14297279	0.6033471	1.0395658	1.3255113	0.8126831
-2	-0.1174457	0.574806	1.2440448	1.0091533	0.8381016
-1	-0.3983302	0.7092012	-1.788339	0.9916786	0.5743474
0	0.5780388	0.7609786	0.9134518	2.0695294	0.4474941
1	0.570945	0.9004486	1.1939019	2.3357919	0.5260369
2	0.0817762	1.1503799	-2.172927	2.3364794	0.9433291
3	-0.7194067	1.0813798	2.8388721	1.4000588	0.5058795
4	-0.0363573	1.0345876	2.0641118	1.9913972	0.9719666
5	0.364144	1.1104309	1.8122605	2.5405485	0.7429643
6	0.66133764	1.181699	1.6547498	2.9774251	0.5757183
7	0.75604534	1.3759456	1.9407585	3.4528492	0.5826807
8	0.26863077	1.2864865	2.2528364	2.790098	0.8345969
9	0.33957548	1.3608554	-2.327652	3.006803	0.8029501
10	0.25166229	1.5596407	2.8051774	3.3085019	0.8718105

Table 5: Average Treatment Effect for ERPOs and Firearm Suicide					
Years Pre and Post	**ATT**	**Std. Error**	**CI Lower**	**CI Upper**	**p-value**
-10	0.230519	0.4411	-1.0951	0.6341	0.6013
-9	0.031185	0.4191	-0.8527	0.7903	0.9407
-8	0.416559	0.4195	-0.4056	1.2387	0.3207
-7	0.126994	0.4301	-0.716	0.97	0.7678
-6	-0.06307	0.4097	-0.866	0.7399	0.8777
-5	0.207332	0.4348	-0.6449	1.0595	0.6335
-4	0.153424	0.4541	-1.0435	0.7366	0.7355
-3	0.057279	0.3942	-0.7154	0.83	0.8845
-2	0.153805	0.3972	-0.9324	0.6247	0.6986
-1	0.532827	0.4543	-1.4232	0.3576	0.2408
0	0.007339	0.4672	-0.9083	0.923	0.9875
1	0.218331	0.5696	-1.3346	0.898	0.7015
2	0.446585	0.779	-1.9734	1.0802	0.5664
3	0.902028	0.6934	-2.261	0.457	0.1933
4	0.178552	0.7708	-1.3323	1.6894	0.8168
5	0.356632	0.8639	-1.3365	2.0498	0.6797
6	0.067781	0.8909	-1.6784	1.814	0.9394
7	0.372241	0.7847	-1.1656	1.9101	0.6352
8	0.168306	0.8536	-1.5047	1.8413	0.8437
9	0.421842	0.9191	-1.3795	2.2232	0.6462
10	0.027673	0.8547	-1.6475	1.7028	0.9742

Figure 1:

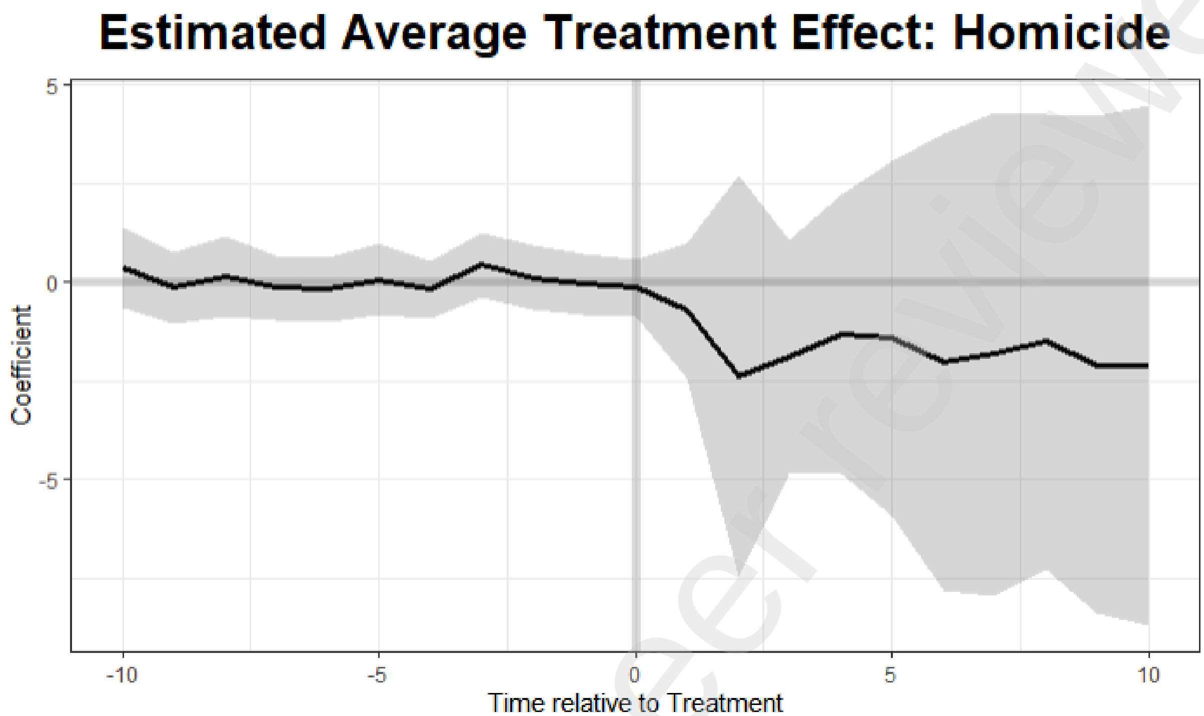

Estimated Average Treatment Effect: Homicide
5
0
-5
Coefficient
-10
-5
0
5
10
Time relative to Treatment

Figure 2:

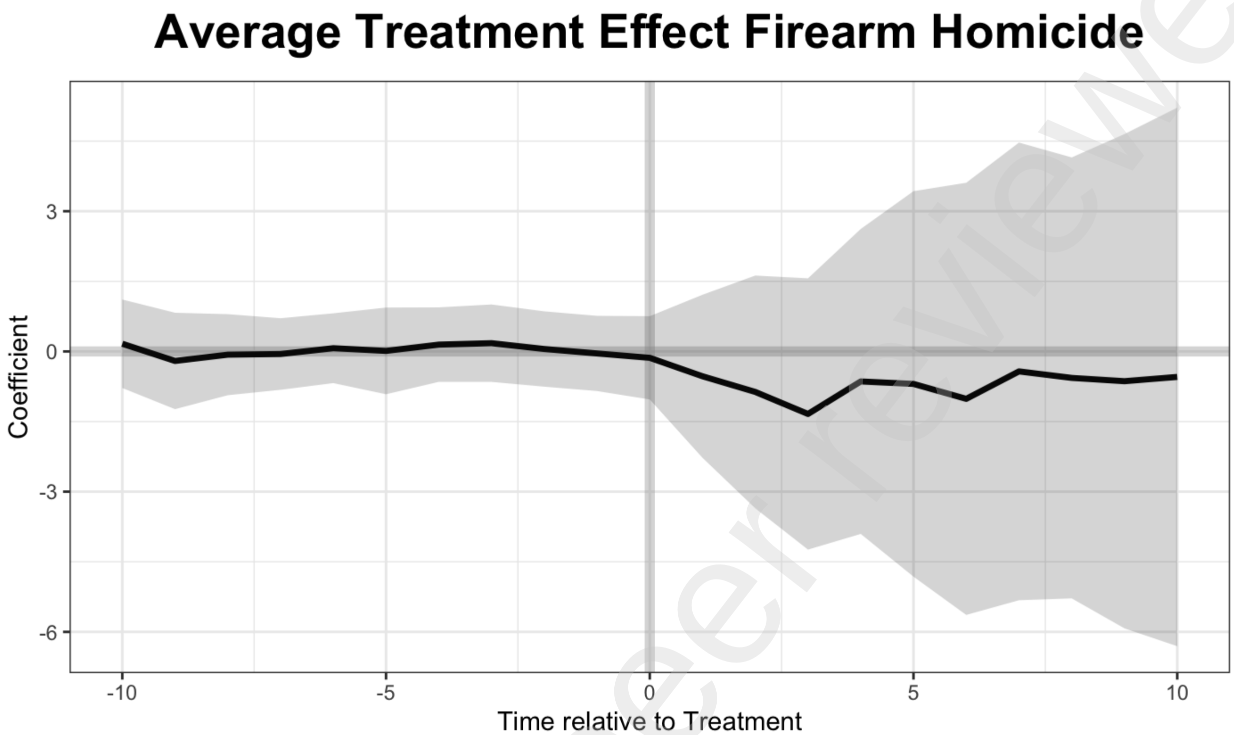

Average Treatment Effect Firearm Homicide
3
0
-3
-6
Coefficient
-10
-5
0
5
10
Time relative to Treatment

Figure 3:

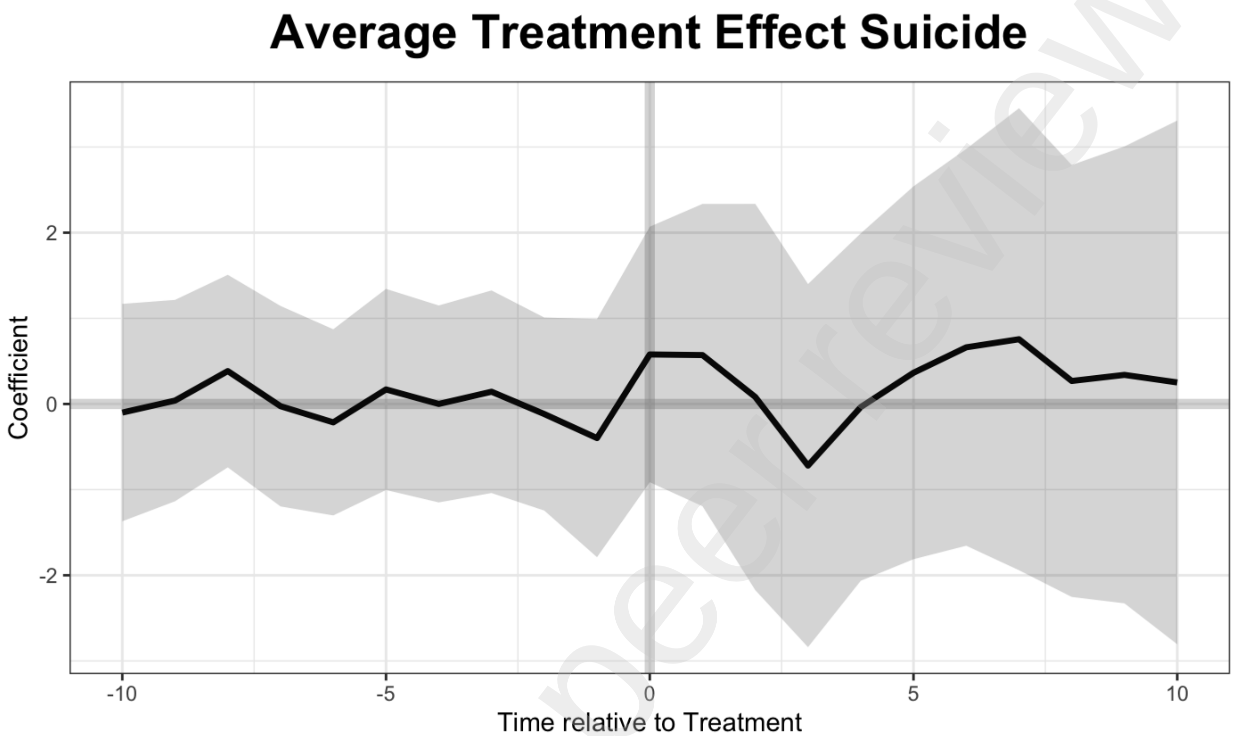
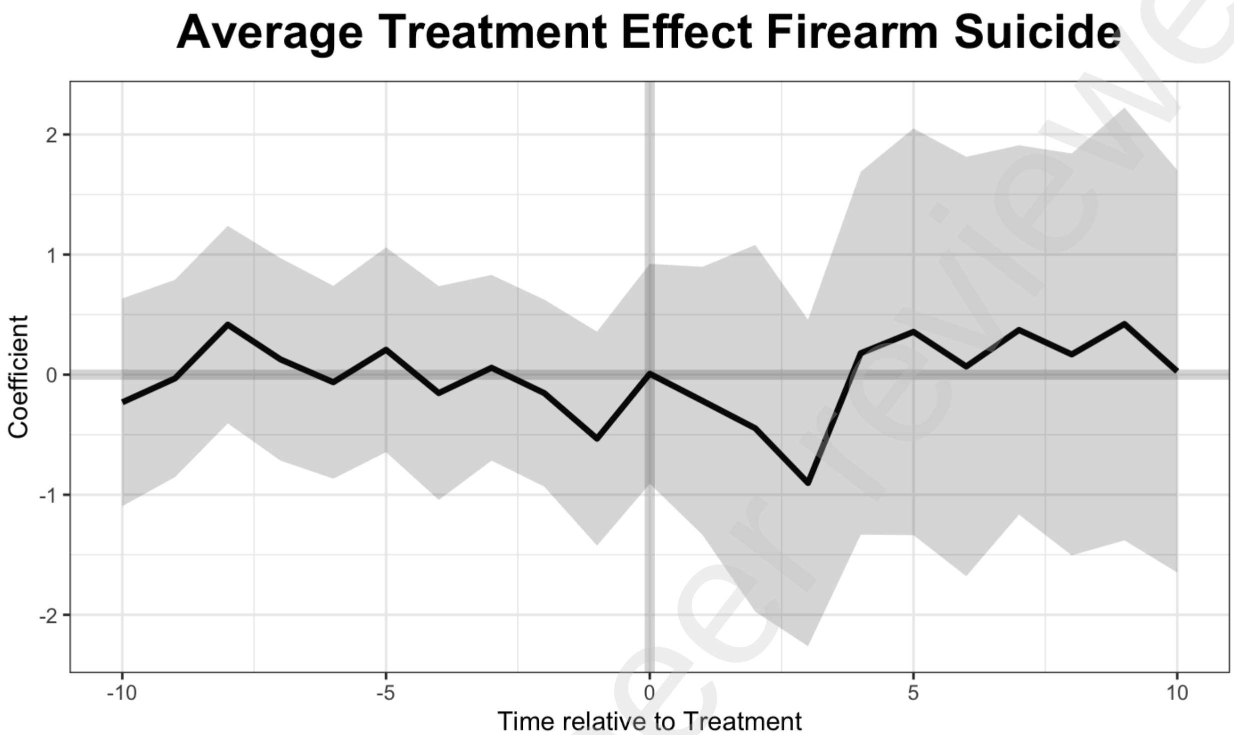

Average Treatment Effect Suicide
Coefficient
2
0
-2
-10
-5
0
5
10
Time relative to Treatment

Figure 4:



Average Treatment Effect Firearm Suicide
2
1
0
-1
-2
Coefficient
-10
-5
0
5
10
Time relative to Treatment