Do Red Flag Laws Save Lives?

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Abstract

Seventeen states have passed Red Flag or Extreme Risk Protection Order (ERPO) laws which allow police, family members, individuals living in the same residence, and others to file a petition for a court order temporarily seizing the firearms of persons accused to be a danger to themselves or others. The theory is that some individuals could pose a danger to themselves and others that could be made worse by the presence of firearms. Therefore, a policy that denied the individual access to their firearms, if only temporarily, might indeed save lives. However, it is possible that these laws could increase homicide or suicide. If a troubled person is aware of the existence of a Red Flag law, he or she may well not seek help because of the threat of an ERPO. Also, the enforcement of the orders could also have perverse consequences.

Two states have considerable experience with ERPO's: Connecticut, since1999, and Indiana since 2005. We use synthetic controls and difference in differences methods to evaluate these laws. The experience in both Connecticut and Indiana is that red flag laws have had no significant effect on either homicide or suicide. We also find that ERPO laws have had no significant effect on deaths or injuries from mass public shootings.

1. Introduction

Seventeen states have passed Red Flag or Extreme Risk Protection Order (ERPO) laws which allow police, family members, individuals living in the same residence, and others to file a petition for a court order temporarily seizing the firearms of persons accused to be a danger to themselves or others (Devos et al., 2018, pp. 89-95). Thirteen of them since March 2018. The theory is that some individuals could pose a danger to themselves and others that could be made worse by the presence of firearms. Therefore, a policy that denied the individual access to their firearms, if only temporarily, might indeed save lives.

However, it is possible that these laws could increase homicide or suicide. In the absence of such a law, a person contemplating homicide or suicide might speak to a family member in confidence and, as a result, be dissuaded from that course of action. If the same person is aware of the existence of a Red Flag law, he or she may well not approach a family member or anyone else who might initiate an ERPO. The result could be that such individuals go on to kill themselves or others. The actual enforcement of the warrants might also have perverse consequences. "If you have an individual who's angry, bitter, threatening other people, [and] owns a gun the attempt to take that gun away can actually precipitate the very violent act that you're trying to prevent," according to James Alan Fox (Sullum 2019 p.51). Accidents can also happen when police try to serve warrants on unsuspecting people at 5 AM. In fact, Gary Willis was killed by police in Glen Burnie, Maryland who were attempting to serve an extreme risk protective order on him.[1]

Another consideration is that there are already civil commitment options in all the states for those posing a danger to themselves or others, and thus Red Flag laws may make little difference in stopping those who are dangerous. While the focus of Red Flag laws is primarily people who are suicidal and considered mentally ill, unlike civil commitment options, none of the Red Flag laws actually require mental health care professional be involved in any part of the evaluation process. As opposed to Red Flag laws where the only option is to take away a person's guns, civil commitment allows judges a wide array of options. Involuntarily civil committing a person is obviously an extreme option, but judges can also people to first try voluntary therapy or even have the individual give up their weapons in exchange for not involuntarily committing them.

Given the potential for saving lives and the potential for abusing a protected constitutional right, an examination of the evidence concerning the effectiveness of these laws might be useful. Since the relevant data extend only to 2016 and most of the adopting states passed their ERPO's in 2016 or later, there is not much evidence as to their effectiveness. However, there are two states with considerable experience with ERPO's, Connecticut which passed its law in 1999 and Indiana which passed its law in 2005. Focusing on these two states, we use synthetic control analyses and difference in differences analyses to determine if these laws reduced murder, homicide, firearm homicide, non-firearm homicide, deaths and injuries from multiple victim public shootings, suicide, firearm suicide, or non-firearm suicide.

[1] https://www.baltimoresun.com/news/crime/bs-md-aa-shooting-20181105-story.html

2. Methods

We use two methods of policy analysis. The synthetic control (Abadie, et al. 2010) method has become popular in recent years, and it is particularly useful in cases where there are not many experiments. It uses panel data across states and years and a sophisticated matching algorithm to generate a weighted average of the outcome variable (e.g., murder, suicide, etc.) from a list of control states that do not have red flag laws. The behavior of this weighted average is the counterfactual, i.e. what the murder rate would have been in the treatment state (Connecticut or Indiana) if the treatment state had not passed an ERPO. The difference between the counterfactual (e.g. synthetic Connecticut) and the actual value of the outcome is the gap (Y_treated-Y_synthetic). If this gap is positive (negative), then the outcome in the treated state is greater (less) than that in the synthetic control state, indicating that the policy is associated with an increase (decrease) in the outcome, e.g. the murder rate.

This is an elegant methodology and produces graphical evidence that is easily understood. However, it has two serious problems. It does not control for unobserved heterogeneity, the effect of unobserved permanent factors that might be correlated with both the outcome and the treatment, creating omitted variable bias. For example, it fails to control for the differences in culture, climate, geography, history, attitudes toward crime, attitudes towards firearms, etc. between Connecticut (or Indiana) and Louisana, Utah, Florida, etc., so that the control states could have had historically higher (or lower) murder, homicide, or suicide rates than either of the treated states before and after the treatment. (See Angrist and Pischke 2009, p.244, fn.9.) The method also fails to control for changes in the exogenous variables in the treatment period, which could explain some of the gap.

The only method that controls for unobserved heterogeneity is the fixed effects model which is also estimated on panel data. It also controls for exogenous variables in the treatment period. However, it also has its drawbacks. The first is that, while it controls for unobserved time-invariant (fixed) effects, it does not control for omitted time-variant effects. We can control for these unobserved time variant factors by adding lags of the dependent variable to the list of variables in the fixed effects model. This raises another issue. The lagged dependent variable is correlated with the error term in the regression, so the estimates are biased (Nickell 1981). The bias is of the order of 1/T, where T is the number of time periods in the sample, so that the bias goes to zero as T goes to infinity. The bias directly affects only the coefficient on the lagged dependent variables and only affects the coefficient on the policy variable through its correlation with the lagged dependent variables. Also, Judson and Owen (1999) show that Nickell bias is only a problem for samples of $T<30$. Since we have 47 years of data, we are comfortable using lags of the dependent variable to control for unobserved time variant effects.

The fixed effects panel data model does have one serious drawback. The fixed effect model is a pure time series model, so serial correlation in the error term is almost always a problem. This problem is compounded by the fact that the difference in differences dummy variable consists of a series of zeroes followed by a series of ones, implying that it is very likely correlated with the serially correlated error term. This correlation causes the estimates to be biased and inconsistent and the standard errors to be seriously underestimated. Bertrand, Duflo and Mullinathan (2002,

2004) suggest that the standard errors can be corrected using clustered standard errors. This has become the industry standard. However, Conley and Taber (2011) find that clustered standard errors are themselves seriously underestimated if there are few states that adopt the policy being investigated. That is exactly the situation here. Only two states adopted red flag laws during our sample period, causing the resulting t-statistics to be seriously overestimated.

Interestingly, Bertrand, Duflo, and Mullinathan (2002), in the same paper that suggested using clustered standard errors, also suggested a cure for this problem, namely placebo law simulation. In this approach we estimate the fixed effects model using clustered standard errors as usual. We then engage in a placebo law simulation by replacing the states adopting the policy with states that did not adopt red flag laws (control states). We the re-estimate the regression, but this time under the null hypothesis that the law had no effect (i.e., on a state that did not adopt it). We repeat this process 1000 times, generating a sampling distribution of the t-statistics centered on the true null hypothesis of no effect. We sort the resulting t-ratios (in absolute value) from smallest to largest and count the number that exceed the t-statistic from the original regression. This number, divided by 1000, is the corrected p-value for the statistic from the original regression. A nice feature of this approach, compared to a Monte Carlo approach, is that it is robust to problems with the sample because it estimates the same regression over the same data. The Monte Carlo method would require generating data under specific conditions, that may or may not reflect whatever problems exist with the actual data.

For the analysis of deaths and injuries due to mass shootings, we use the negative binomial fixed effects model, since these are rare events.

3. Data

The data come from public sources for all 50 states from 1970-2016. The murder rate is taken from Uniform Crime Reports. Homicide, firearm homicide, suicide and firearm suicide are taken from the CDC Wonder website. All three homicide measures have missing values in several small states, which causes them to be dropped from the list of potential control states in the synthetic control algorithm. As a result, we interpolated the missing values using the murder rate as the interpolating variable. We estimate models for both murder and homicide because homicide includes justifiable homicides and executions, which may be socially beneficial. On the other hand, some violent deaths are not counted as murder, but are categorized as homicide.

Deaths and injuries from multiple victim public shootings, 1982-2016, are taken from the Mother Jones database.[2] The control variables used in the regression analyses and the synthetic control matching program for murder, mass shooting deaths, and homicide are: a measure of the crack epidemic from 1985-1991constructed from Fryer's (2013) crack cocaine index, beer consumption per capita, prison population per capita, sworn police officers per capita, real per capita personal income, real welfare payments per capita, the poverty rate, the percent of the population that is black, the unemployment rate, total employment per capita, military employment per capita, construction employment per capita, the percent of the population 15-34 and the percent of the

[2] https://www.bjs.gov/ucrdata/Search/Crime/State/TrendsInOneVar.cfm, https://wonder.cdc.gov/mortSQL.html, https://www.motherjones.com/politics/2012/12/mass-shootings-mother-jones-full-data/

population 65 and over. The control variables used in the suicide models are a subset of these, plus population as an exposure variable. We also use individual state trends and year dummies in the fixed effects models.[3]

4. Synthetic control results

4.1 Murder and homicide

The results of the analysis using synthetic control are presented in Figures 1-4 and Table 1. For both Connecticut and Indiana, the murder rate started falling several years before the red flag law was passed. Also, Indiana has historically had a higher murder rate than the synthetic control state which persisted into the treatment period. As a result, there is a large positive gap between the actual and synthetic murder rate. However, if we adjust the actual rate to be equal to the synthetic rate in the treatment year, the gap becomes much smaller. Synthetic Connecticut matches actual Connecticut very closely both before and after the passage of the red flag law. As a result, the adjusted and unadjusted gaps are small for Connecticut. The red flag laws appear to have had little effect on the murder rate for either state.

Table 1 about here.

This impression is confirmed (Table 1) by the fact that neither the unadjusted nor the adjusted mean gap for Connecticut, nor the adjusted mean gap for Indiana is significantly different from zero using a robust t-test.

Figure 1 about here.

The results for homicide are shown in Table 1 and Figure 2. The graphs are similar to those for murder. Homicide began declining several years before the ERPO laws were passed in both states. However, the decline was reversed in both states a few years after the law's passage. While there appears to be a large and significant decline in Connecticut using the unadjusted gap, the adjusted gap, shows a very small and insignificant difference between the experience of Connecticut and its synthetic control state. The situation is similar for Indiana, except that the unadjusted gap is positive and significant. However, the adjusted gap is small and not significantly different from zero. Both Connecticut and Indiana have had historically higher rates of murder and homicide than their synthetic control states, consequently the adjusted gaps, which control for this fact, are more relevant to the issue of effect of the ERPO law.

Figure 2 about here.

The results for firearm homicide are shown in Table 1 and Figure 3. Again, both these states experienced declines in firearm homicide well before the passage of the ERPO law and the declines were reversed in the post-law period. The means of both the adjusted and unadjusted

[3] All data, programs and results may be downloaded from the lead author's website.

gaps are significantly negative for Connecticut. For Indiana, the mean unadjusted gap is significantly positive, while the mean adjusted gap is small and insignificantly negative. The adjusted gap is more relevant in Indiana because its homicide rate has exceeded that for its synthetic control state since 1990. Overall, it appears that the red flag law significantly reduced firearm homicide in Connecticut, relative to its synthetic control state, while having no significant effect in Indiana.

Figure 3 about here.

The results for non-firearm homicide are presented in Table 1 and Figure 4. In Connecticut, the mean unadjusted gap is negative, but small and not significant. However, the adjusted gap is large and significantly positive.[4] This confirms the murder and homicide results, namely that the overall rates are not significantly affected in the treatment period, apparently because of substitution. For Indiana, the unadjusted gap is significantly positive while the adjusted gap is negative and marginally significant. These results also confirm the murder and homicide results.

Figure 4 about here.

Overall, the synthetic control results indicate substitution between firearm and non-firearm homicide for Connecticut and no significant effect for either in Indiana, leading to the result of no significant effect on the overall murder or homicide rates for either state.

4.2 Suicide

The synthetic control results for suicide are presented in Figure 5 and in the second section of Table 1. The control variables used in the synthetic effects matching algorithm are: population, beer consumption per capita, real per capita income, real per capita welfare payments, poverty rate, percent black, unemployment rate, employment per capita, percent of the population 15-34, and percent of the population 65 and older. The suicide rate in Connecticut declined substantially in 1996, before the implementation of the ERPO law and rebounded shortly after. The suicide rate in Indiana has been rising since 1999. There is little difference between the outcomes for the actual and synthetic states, so the adjusted and unadjusted gaps are very close. There is no significant difference between the actual suicide rate and the synthetic control rate for either state.

Figure 5 about here.

The results for firearm suicide are shown in Figure 6 and Table 1. Both states show a significant reduction in firearm suicide in the treatment period for both the unadjusted and adjusted gaps.

[4] The results are the same when the spike in non-firearm homicides in 2001 in Connecticut is dropped from the sample.

Figure 6 about here.

The results for non-firearm suicide is shown in Figure 7 and Table 1. There is no significant difference between synthetic and actual Connecticut, but there is a significant increase in non-firearm suicide for the adjusted gap in Indiana.

Figure 7 about here.

Overall, the results for Connecticut are inconsistent. There is a significant reduction in firearm suicide and no increase in non-firearm suicide. This should imply a reduction in overall suicide. However, there is no significant change in overall suicide. The results for Indiana are more consistent. For the adjusted gaps, there is a significant reduction in firearm suicide, but a corresponding increase in non-firearm suicide, resulting in no significant change in overall suicide. There is apparently substitution between firearm and non-firearm suicide methods in Indiana, and possibly in Connecticut. Alternatively, these apparently significant differences could be false positives.

5. Difference in differences results

The fixed effects results are presented in Table 2. Although not reported to conserve space, all regressions include the control variables listed in Section 3 above, two lags of the dependent variable, state fixed effects, year fixed effects, and individual state trends.[5]

Table 2 about here.

As Table 2 shows, there appears to be a significant negative effect of red flag laws on murder. However, according to Conley and Taber (2011) the standard errors are seriously underestimated when there are only two states with policy changes in the sample period. Accordingly, the placebo law simulation shows that the t-statistic is not significant at customary levels. With respect to the effect of the ERPO law on homicide, the coefficient on the policy dummy variable is not significant using the placebo law p-values. Similarly, neither the negative coefficient on firearm homicide nor the positive coefficient on non-firearm homicide are significantly different from zero using the placebo law p-values.

The policy dummy is not significant in the suicide regression but appears to be significantly negative with respect to firearm suicide, and significantly positive in the non-firearm suicide regression. However, neither of these coefficients is significant using the placebo law simulations.

Red flag laws have also been proposed as a potential policy response to mass public shootings. We estimated the effect of ERPO laws on deaths and injuries incurred in multiple victim public shooting using the fixed effect negative binomial model. The control variables are the same as those used in the murder regression except that we dropped the state trends and lagged dependent variables because neither was significant as a group using standard F-tests. The estimates of the coefficient on the policy dummy variable is shown in the last two rows of Table 2. Neither coefficient is significant using conventional z-statistics implying that red flag laws have no

[5] The lagged dependent variables, year fixed effects, and state trends are all highly significant in all regressions. Complete results may be downloaded from the lead author's website.

significant effect on deaths or injuries from mass public shootings. Since the t-ratios are already insignificant and Conley and Taber (2011) find that t-ratios are underestimated with only two policy changes, we used the conventional p-values instead of placebo law simulations.

Connecticut increased the number of gun seizures tenfold in 2007 and by over 700 by 2013 (Swanson et al. 2016, p.8). Consequently, we re-estimated the models using 2007 as the implementation date for Connecticut. The results were qualitatively unchanged. All results are available on the lead author's website.

6. Summary and conclusion

We have conducted a policy analysis on red flag or extreme risk protection order laws. There are two states with considerable experience with the consequences of this policy. Connecticut passed its law in 1999 yielding 17 observations in the treatment period. Indiana passed its law in 2005 yielding 11 more years of experience that could help us determine whether these laws reduce gun violence. We use two techniques to evaluate this policy, the relatively new synthetic control method (Abadie 2010) and the widely used difference in differences method.

With respect to the overall murder, homicide, and suicide rates, the synthetic control results indicate that red flag laws have had no significant effect, especially if we constrain the synthetic and actual levels to be equal in the treatment year. There two reasonable explanations for this result. Either there is a significant reduction in the use of firearms and a corresponding increase in the use of other methods, or there is no significant effect on either. There appears to be substitution between firearm and non-firearm homicide in Connecticut, leading to no significant net effect on murder and homicide, and no significant effect at all in Indiana. With respect to suicide, there is evidence of substitution between firearm and non-firearm suicide in Indiana. The results with respect to Connecticut are inconsistent in that the policy apparently had no significant effect on overall suicide but a significantly negative effect on firearm suicide, while having only a very small and insignificantly positive effect on non-firearm suicide.

Using the fixed effects model, the synthetic control results for the overall murder and homicide rates are confirmed by the difference in differences analysis using placebo law p-values. However, we also find no significant effect on firearm homicide and non-firearm homicide. Similarly, we confirm the synthetic control results of no significant effect on overall suicide. However, we also find that the red flag laws have had no significant effect on firearm suicide and non-firearm suicide. Finally, red flag laws in Connecticut and Indiana have had no significant effect on deaths and injuries from mass public shootings.

Overall, the experience in Connecticut and Indiana is that red flag laws have had no significant effect on either homicide or suicide because there was substitution between firearms and other methods or because there was no significant effect on either of these modalities. We also find that ERPO laws have had no significant effect on deaths or injuries from mass public shootings.

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Table 1: Mean gap of outcomes

Outcome	Connecticut		Indiana	
	Unadjusted	Adjusted	Unadjusted	Adjusted
Murder	-0.064	-0.029	0.315***	0.056
	(-1.52)	(-0.73)	(15.10)	(1.50)
Homicide	-0.105**	-0.020	0.180***	-0.027
	(-2.44)	(-0.52)	(9.23)	(-0.88)
Firearm homicide	-0.223***	-0.307***	0.159***	-0.043
	(-3.12)	(-3.83)	(6.90)	(-1.19)
Non-firearm homicide	-0.030	0.158***	0.197***	-0.084*
	(-0.87)	(6.06)	(7.30)	(-1.85)
Suicide	0.020	-0.010	0.004	-0.023
	(1.35)	(-0.64)	(0.29)	(-1.56)
Firearm suicide	-0.86***	-0.960***	-0.149***	-0.259***
	(-2.76)	(-3.00)	(-3.91)	(-5.59)
Non-firearm suicide	0.004	0.024	0.010	0.053***
	(0.28)	(1.56)	(0.54)	(3.10)

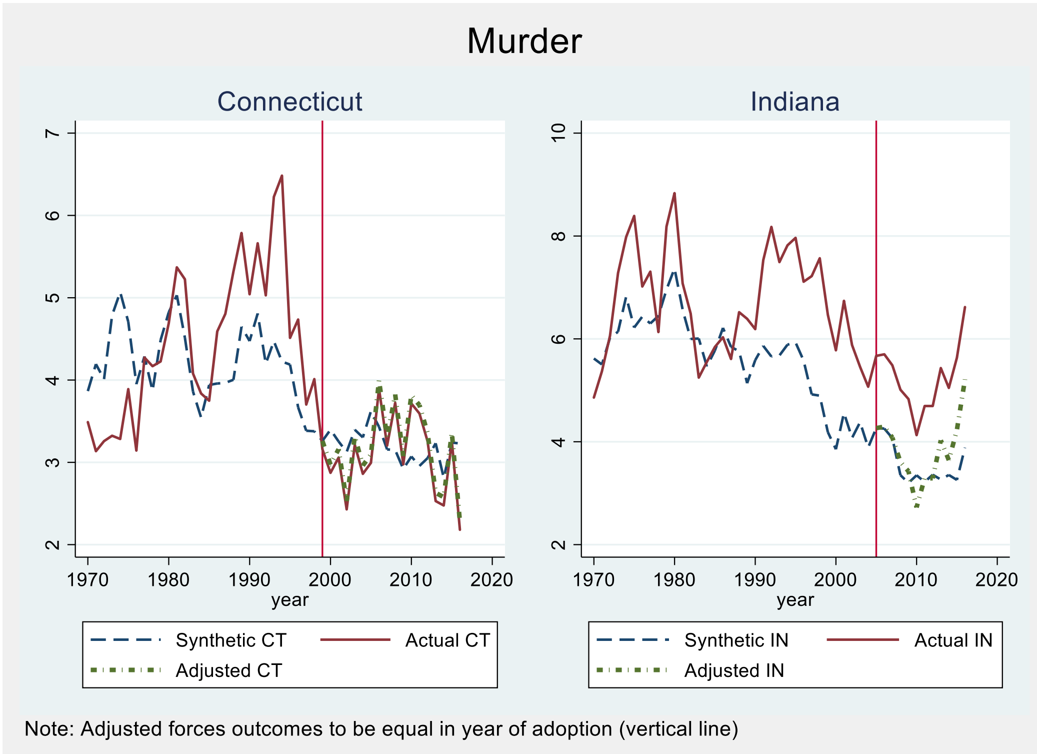
Notes: * <.10, **<.05, ***<.01; gap= (Y_actual – Y_synthetic); H0 mean gap equals zero, robust standard errors; t-ratios in parentheses.

Table 2: Coefficient on policy dummy variable

Outcomes	Coefficient	T-statistic	P-value
Murder	-0.149	-2.64[a]	0.213[b]
Homicide	-0.095	-1.82[a]	0.305[b]
Firearm homicide	-0.192	-1.74[a]	0.334[b]
Non-firearm homicide	0.045	0.77[a]	0.926[b]
Suicide	-0.004	-0.19[a]	0.914[b]
Firearm suicide	-0.083	-2.65[a]	0.215[b]
Non-firearm suicide	0.075	2.70[a]	0.754[b]
Mass shooting deaths	0.592	0.42[c]	0.676
Mass shooting injuries	1.163	0.66[c]	0.507

Notes: fixed effects used in all regressions. Control variables, state and year dummies, lags of the dependent variable, and state trends are included in all regressions. Complete results may be downloaded from the lead author's website. [a] T-ratios using clustered standard errors. [b] P-values taken from placebo law simulations. [c] Z-statistics.

Figure 1


Murder
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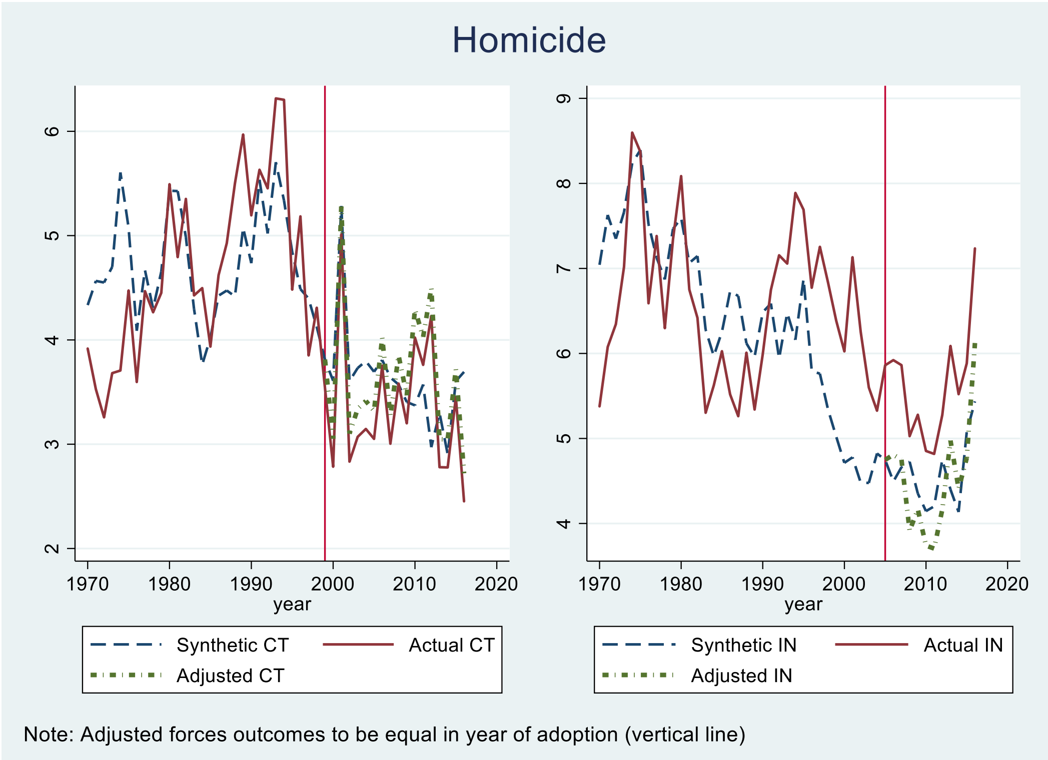
Synthetic IN
Actual IN
Adjusted IN
Note: Adjusted forces outcomes to be equal in year of adoption (vertical line)

Figure 2


Homicide
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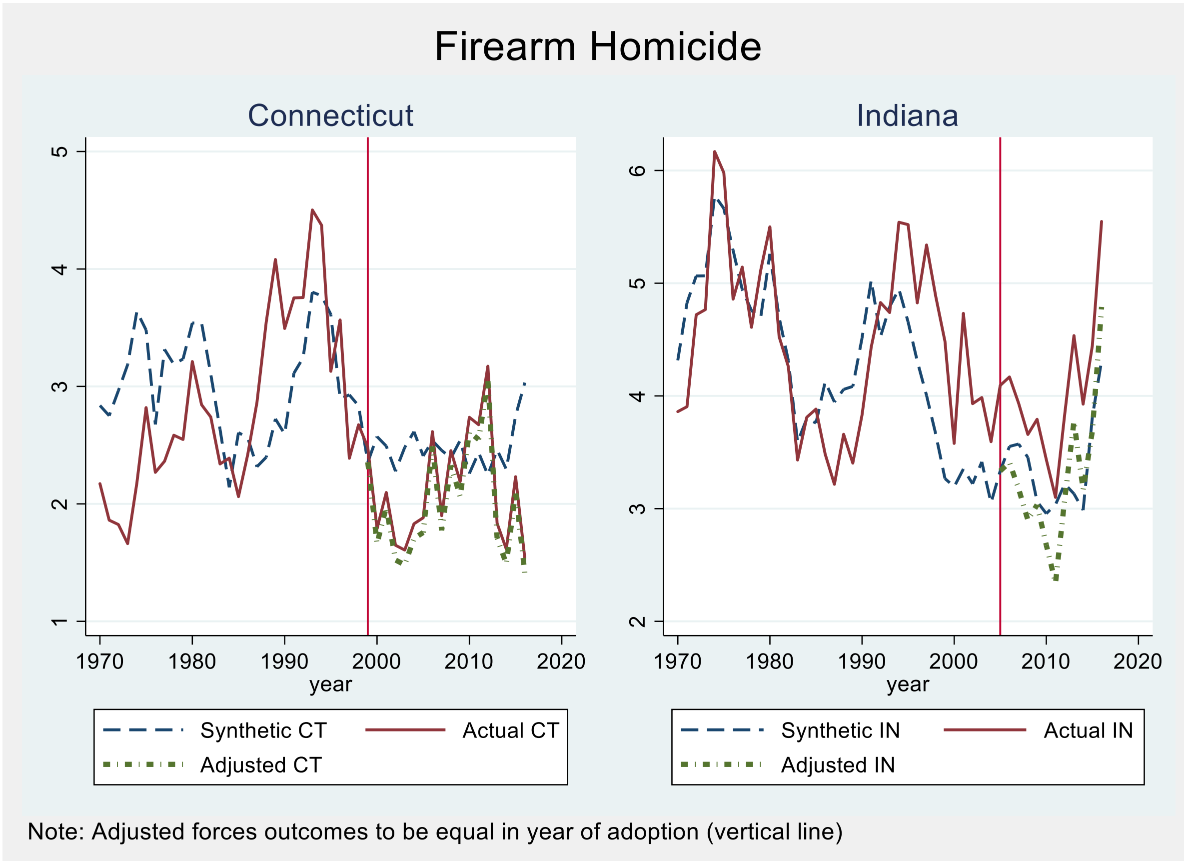
Note: Adjusted forces outcomes to be equal in year of adoption (vertical line)

Figure 3



Firearm Homicide
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Synthetic IN
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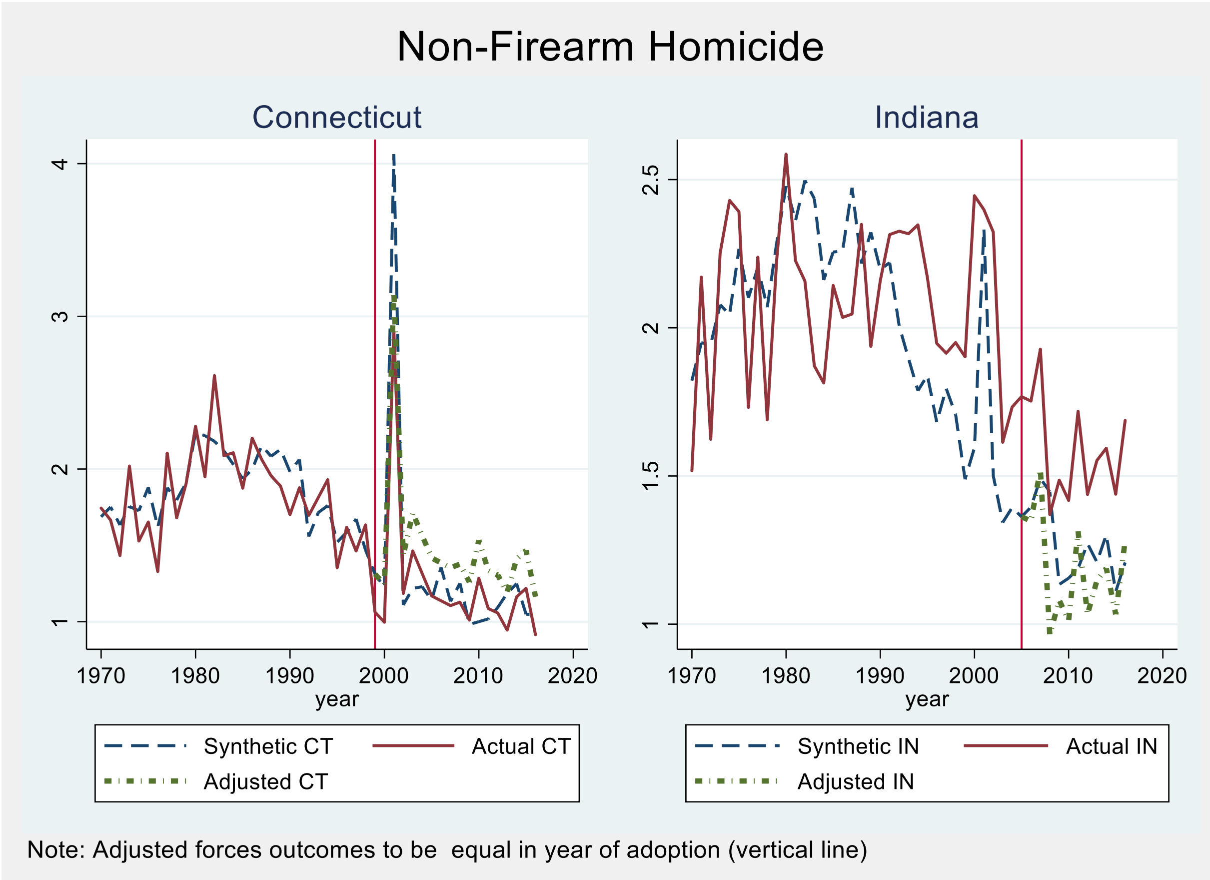
Note: Adjusted forces outcomes to be equal in year of adoption (vertical line)

Figure 4



Non-Firearm Homicide
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Adjusted CT
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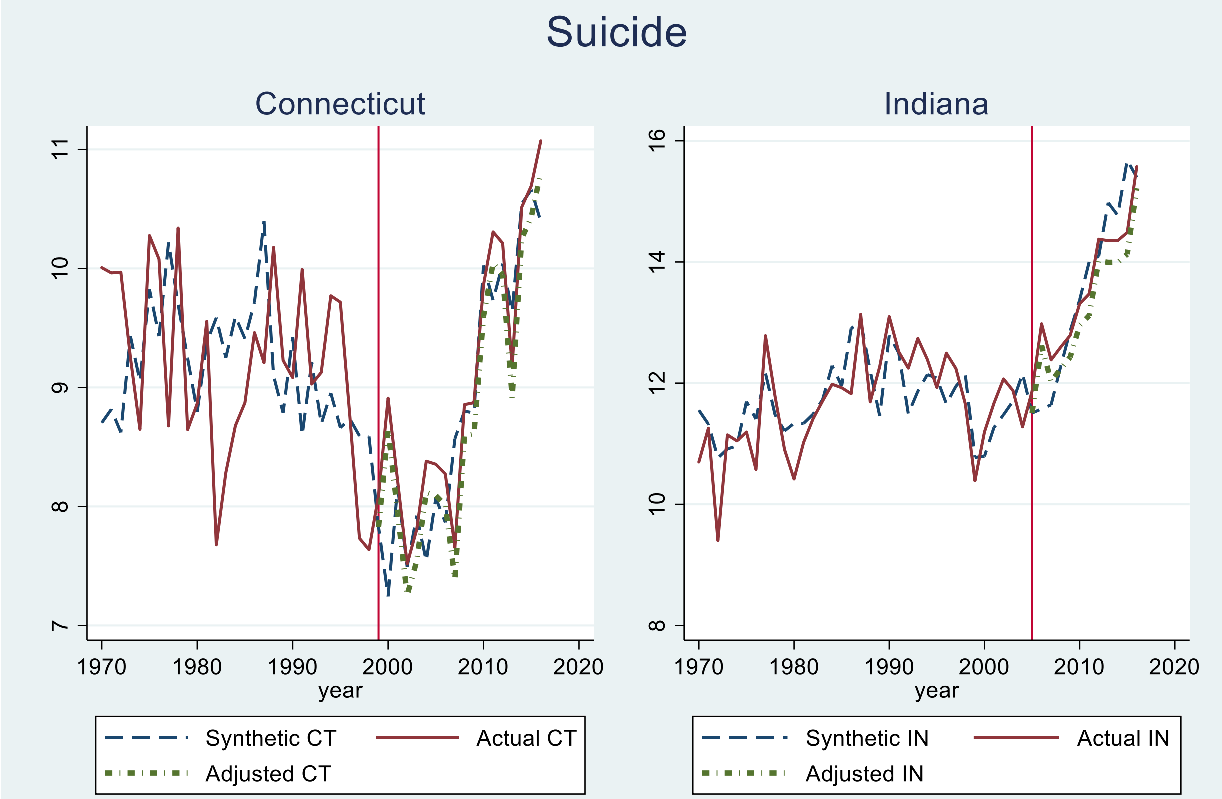
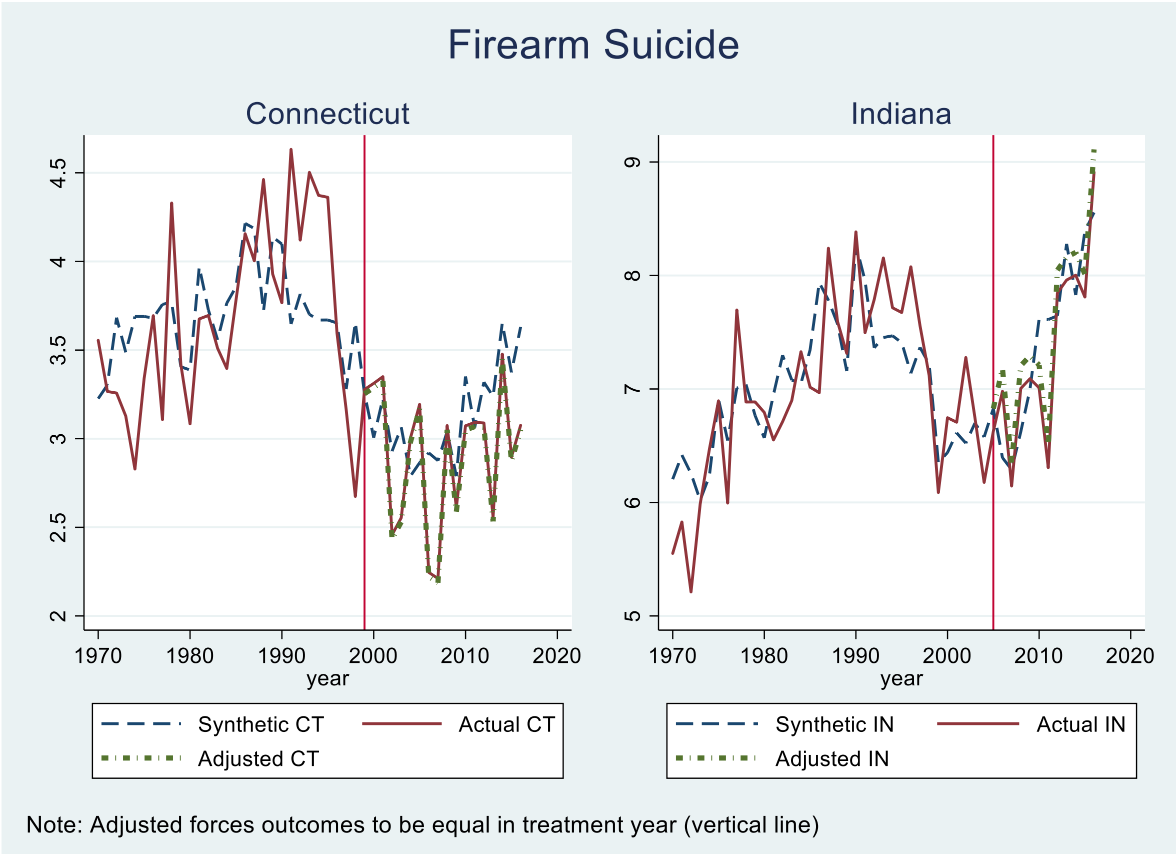
Note: Adjusted forces outcomes to be equal in year of adoption (vertical line)

Figure 5



Note: Adjusted forces outcomes to be equal in treatment year (vertical line)

Figure 6



Firearm Suicide
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Adjusted CT
Synthetic IN
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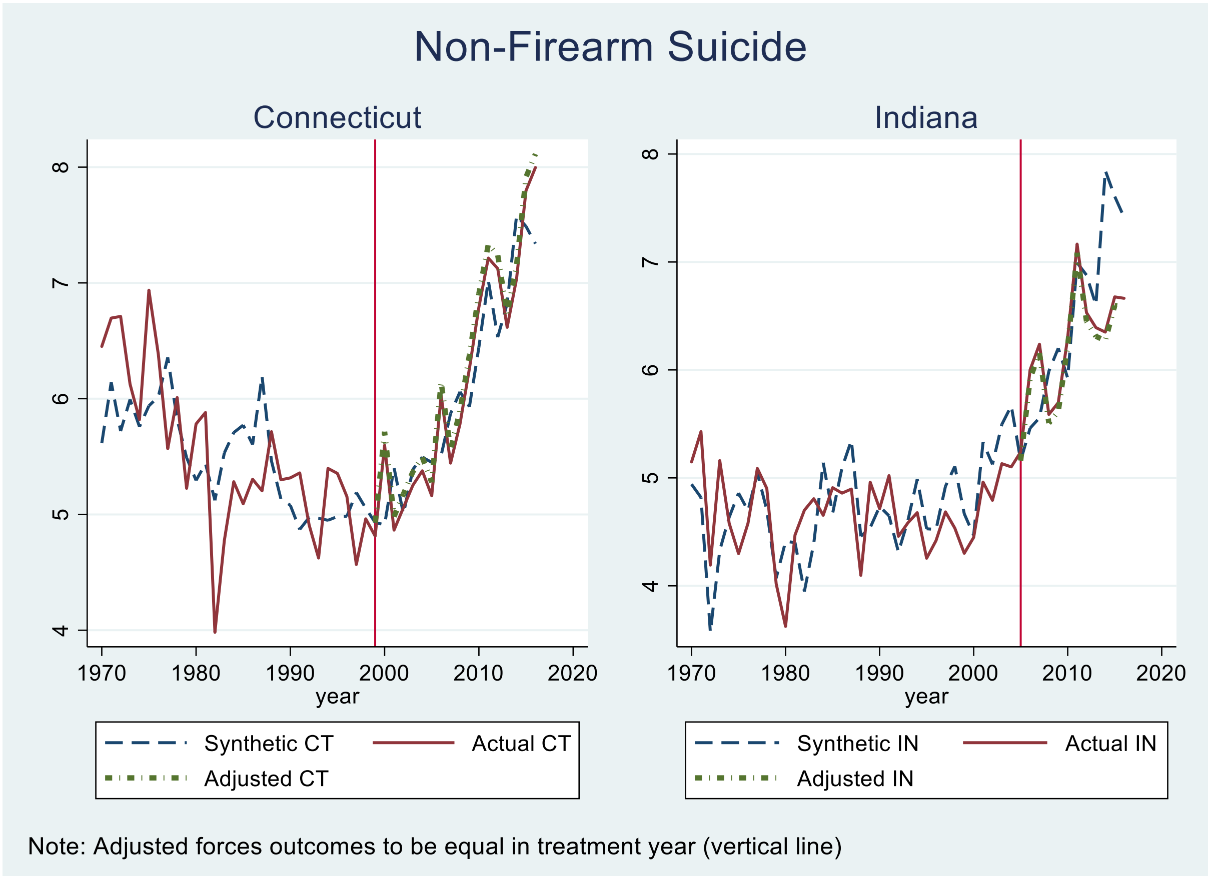
Note: Adjusted forces outcomes to be equal in treatment year (vertical line)

Figure 7



Non-Firearm Suicide
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Actual CT
Adjusted CT
Synthetic IN
Actual IN
Adjusted IN
Note: Adjusted forces outcomes to be equal in treatment year (vertical line)