State Differences in Handgun Permit-to-Purchase Responses: Criminal Usage and Purchasing

Brendan Cirillo

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Permit-to-purchase (PTP) is a state requirement to obtain a license prior to purchasing a gun. This paper uses synthetic control methods to estimate the effects of PTP laws in three states on gun purchasing behavior and usage for crime, as well as downstream effects on homicides and suicides. In Missouri, repealing PTP led to a 12.9% rise in background checks, a 22 percentage point increase in the self-sourced share of crime guns, and a 2.4-year drop in time-to-crime, alongside a 37.9% increase in firearm homicides and a delayed increase in suicide. Consistent with Missouri, Maryland's adoption of PTP reduced background checks by 23.4%, lowered the self-sourced ratio by 9.4 points, and increased time-to-crime by 3.3 years, though no short-run mortality effects were detected. Across Connecticut, Maryland, and Missouri, metropolitan areas respond immediately to PTP changes, while effects in rural areas emerge more gradually, if at all.

1. Introduction

The gun debate in the United States has a long and contentious history, in part due to the constitutional protection to have firearms. Preserving this constitutional right pushes the debate to regulation, which is, ultimately, controlling who can buy guns and how those guns can be carried or used. For 'how', there is a wide variety of laws dictating carrying in public and use on private property. While there are states that have permitless concealed carry laws, generally, concealed carry laws require gun owners to get a license, in effect registering with the state, before they are permitted to conceal and carry a gun in public. There are also Stand Your Ground and self-defense laws, which dictate under what circumstances a gun owner can defend themselves and their property. For 'who', these laws are about controlling guns before they are purchased by requiring background checks and limiting purchases by certain individuals (i.e., minors, felons) or implementing a waiting period to limit impulse purchases that might be used in a crime. This paper studies the effect of limiting access to guns (the 'who' can buy guns).

While some purchasing laws directly prevent people from purchasing a firearm, often, the main mechanism of limiting access is through creating additional hurdles and raising the cost of obtaining a gun. A cooling-off period, or waiting period, increases the cost by adding time. Background checks can help reveal criminal history and prohibit certain people from purchasing a gun altogether. A background check can create an additional cost of time, but in most cases, the background checks are instantaneous, thus only adding a cost to those with a criminal history. Background checks can be circumvented by 'straw purchases', where criminals obtain a gun via someone with a clean background check. However, if purchasing requires registration with the state, it would increase the cost for purchasers who have a clean background check by adding risk through a public record of gun ownership. If their state-registered gun ended up being used in a crime, there could be legal blowback on the purchaser. Additionally, if there were safety requirements and licensing paperwork, this would increase the cost by adding time and the literal cost of the license. Permit-to-purchase laws often create these hurdles. Permit-to-purchase laws, at a minimum, require citizens to take the extra step of obtaining a license before purchasing a gun. This licensing requirement affects first-time buyers but also existing owners if there is an expiration for the permit. It also creates an additional barrier to straw purchases compared to comprehensive background checks by adding a record with the state.

To explore the effects of permit-to-purchase laws, I focus on three different permit-to-purchase laws in Connecticut, Maryland, and Missouri. These three states are the states that have made permit-to-purchase law changes within the same time frame that the data

are available[1]. Connecticut and Maryland passed permit-to-purchase laws in 1995 and 2002, while Missouri repealed its permit-to-purchase law in 2007. Given the gaps in passage date, the complex tapestry of gun laws in each state, and the difference between the permit-to-purchase laws, I use a synthetic control method. Across states and outcomes, I use a unified framework to better synthesize the core results, which are often disparate in the gun control literature. First, I check the first-order effects on purchasing directly using background checks and data on criminal purchasing patterns. Next, I look at the impact on second-order outcomes: crime rates, firearm homicides, and firearm suicides. For the mortality outcomes, the data allow me to look at differential effects between metropolitan areas and rural areas, as well as the spillovers to neighboring states.

For purchasing behavior, using background checks as a proxy for purchases, I find complementary results in Maryland and Missouri. In Missouri, there is suggestive evidence of an increase (12.9%) in Missouri despite the state repealing state background checks along with permit-to-purchase. Therefore, this estimate is a lower bound as a proxy for purchases. Any purchase on the used marketplace or purchases from non-federally licensed dealers would not be counted in this number. In Maryland, where permit-to-purchase was passed, there is a 23.4% decrease in background checks with a p-value of .107. Both of these results suggest an increase in cost for purchasing, affecting purchasing behavior.[2]

Criminal purchasing behavior is examined using FBI trace data, which tracks the purchase data for guns used in crime and reports time-to-crime and self-sourced ratio. Time-to-crime is the time since a gun used in a crime was purchased. In Missouri, I find suggestive evidence of a decrease in time-to-crime, suggesting that guns make their way from purchase to being used in crime more quickly. Consistent with Missouri, Maryland sees a 3.3-year longer time-to-crime compared to their synthetic control (significant at conventional levels). Trace data also tracks the purchase location. The second measure, self-sourced ratio, tracks the share of guns used in a crime in Missouri that were also purchased in Missouri. There is suggestive evidence of a 22 percentage point increase in the self-sourced ratio. An increase in the self-sourced ratio indicates that guns used and recovered in crime are increasingly purchased in Missouri relative to other states. Maryland has a 9.4 percentage point decrease in the self-sourced ratio compared to the control (significant at conventional levels).[3][4]

[1]Iowa and Oregon have recently made changes (2021 and 2022, respectively), but data are not available to analyze the impact of those changes. California made changes to licensing as well, but made frequent changes in the years leading up to and after the licensing changes, making it difficult to disentangle the causal impact of licensing changes.

[2]Background check data are not available for Connecticut during the policy change.

[3]Trace data are not available for Connecticut during the policy change.

[4]Trace data is only guns that are recovered by the state, and not all guns in circulation.

For second-order effects, I look at homicides, suicides, spillovers into neighboring states, and the impact on crime rates. In Missouri and Connecticut, there is evidence that rural areas are affected, but only after a delay. In Maryland, there are relatively few rural areas, and there is no evidence of an impact in those areas, which is consistent with the main results. In urban areas in Missouri and Connecticut, the effects are immediate and reflect the main results. In Maryland, the urban areas show evidence of a decrease in suicides but largely reflect the main result. For spillovers, I find suggestive evidence of impacts in both Missouri and Maryland's border counties. Lastly, I look at the impact on both non-homicide violent crime and property crime rates. This examines the deterrent effect of the increased likelihood that a potential victim is carrying a firearm. In Connecticut and Maryland, the cost of crime, as it relates to the risk of a victim carrying a gun, should have decreased. In Missouri, the permit-to-purchase repeal should have the opposite effect on the cost of crime. In these three states, the non-homicide violent crime rates and property crime rates are unaffected.

I contribute to the literature in the following ways. First, I am the first paper, to my knowledge, that looks at the relationship between permit-to-purchase and background checks using synthetic control. Others have looked at comprehensive background check laws (CBC), which require background checks for all purchases. Castillo-Carniglia et al. (2017) and Castillo-Carniglia et al. (2019a) find limited or no impact on background checks, with Delaware's CBC law being the only law that impacted background checks. Permit-to-purchase laws increase the cost of purchasing a gun, often including a background check, and might have a differential impact compared to CBC laws because of the additional requirements, time commitment, and additional cost for the license.

Second, I look at criminal usage and purchasing. Trace data has been used by Coates and Pearson-Merkowitzz (2017) and Collins et al. (2018), who find that stronger gun laws are associated with fewer in-state purchases and fewer exports to neighboring states. Li et al. (2023) find that repeal of permit-to-purchase in Missouri led to an increase in guns purchased in Missouri that were later used in crime, compared to out-of-state, using border states in a difference-in-differences framework. I add to this literature by looking at Missouri and Maryland using synthetic control to get causal estimates for the impact on time-to-crime and where guns are sourced when used in crime.

Third, I look at permit-to-purchase laws through the lens of deterrence and examine the impact on crime rates. I examine violent non-homicide crime, property crime, and expand on the homicide and suicide literature by looking at spillovers and the breakdown between metropolitan and rural areas, all using synthetic control. Others have looked at spillovers for homicides (e.g. Liu et al. (2020), Liu et al. (2022), etc.) and crime rates (e.g. Shi and

Lee (2018), Laplana (2018), etc.). The literature in this area is consistent in finding an impact on crime rates in neighboring states as a result of strengthening gun laws. However, Laplana (2018) finds an increase in crime rates from the right-to-carry laws in both the state where the law was passed and the neighboring states. Shi and Lee (2018) finds a general increase in crimes where gun laws are weakened, and that gun laws have an impact across borders. As for homicides, Liu et al. (2020) and Liu et al. (2022) both find that neighbors matter and that permit-to-purchase could have an impact on homicides both in and out of state. To my knowledge, no other paper uses synthetic control to examine the impact of permit-to-purchase on crime rates, rural and metropolitan breakdown, and spillovers.

My paper creates a unified framework to look at the impact of permit-to-purchase laws across three states, using the same methodology for each state. It confirms earlier results that find an increase in homicides in Missouri and a decrease in homicides in Connecticut, although with less statistical significance (Rudolph et al. (2015), Crifasi et al. (2015), and Webster et al. (2014)). The consistent methodology and new outcomes related to purchasing behavior and criminal usage harmonize the literature under a consistent synthetic control matching methodology.

The rest of the paper is organized as follows. Section 2.1 provides more information on the permit-to-purchase laws, the literature in this area, the public health debate, and a description of the data and empirical methodology. Section 3 shows the results for each state. Within Section 3, Section 3.1 uses trace data to look at time-to-crime and the sourcing of guns used in crime. Section 3.2.1 summarizes the analysis that overlaps with earlier work, namely, whole state homicides and suicides. Section 3.2.2 breaks down the impact on suicides and homicides between metropolitan and rural areas. Section 3.3 looks at non-homicide violent and property crime rates. Section 4 concludes.

2. Background

At the center of the gun debate in the United States is the balance between the Second Amendment rights and public health. The debate over these two tenets has a long and contentious history. The issue of gun regulation is important now. From a public health perspective, gun-related deaths are on the rise. Since 2015, there has been a rise in gun-related homicides and a steady growth in mass shootings.[5] The public health debate should focus on the balance between the direct effects on homicides and suicides and the impact on

[5]According to Gun Violence Archive, which categorizes mass shootings as having four or more victims injured or killed, not including the shooter. This differs from other definitions that remove certain victims based on the circumstances of the shooting.

crime rates. Gun-carrying law-abiding citizens could provide a deterrent effect on crime. The deterrence effect would come from the perceived increase in the cost of crime—the increased risk of death or serious injury. If this deterrence effect is happening, then the change in homicides and suicides should be weighed against the benefit from the change in crime rates. A third consideration should be how criminal behavior changes from the direct impact of gun laws, not just the perceived changes in the cost of crime.

Some might wonder about the efficacy of gun regulations. The United States has more guns, owned by private citizens, than people, according to estimates from the Small Arms Survey. Despite the large number of guns, only 40% of households own a gun, according to Pew Research. While the cost of crime can still increase from a deterrence perspective, by increasing the number of households with guns, the access to guns is likely a function of both the number of guns and households with guns. These stats help assess if criminal behavior might change, as it relates to gun purchasing and usage, if guns are already readily available. To that end, I focus on the restrictions for purchasing handguns through permit-to-purchase laws. Handguns account for 90% of all firearm homicides (FBI, 2019).[6] For suicides, firearms, in general, account for over 50% of all suicides, handguns account for roughly 70% of firearm suicides and that number continues to grow (Wintemute et al. (1988), Hanlon et al. (2019)). Handguns play a crucial role in the public health debate because, while handguns are the most common firearm used in homicides and suicides, handguns are also the easiest to carry and deter crime. Regulation targeting the access and use of handguns could be highly effective at impacting crime as well as homicides and suicides.

As for these regulations, gun control laws can be broken down into 2 categories: restricting how people buy guns and restricting how people can carry guns. This paper focuses on restricting how people buy guns. To that end, there are two main laws: comprehensive background checks and permit-to-purchase. As such, it is useful to distinguish permit-to-purchase laws from background check laws. Comprehensive background check laws require a background check for all purchases and not just for federally licensed dealers. Permit-to-purchase is, fundamentally, a licensing requirement for the buyer, but it can include background checks, registration of sales with the state, safety training requirements, and a minimum age to purchase. Everytown, a pro-gun control research and policy group, scores the strength of the laws and reports a score of a state's holistic gun control approach. They assign each law a score between 1 and 6 and score the entire state out of 100; the higher the score, the stronger the gun control laws. Permit-to-purchase and comprehensive background check laws are both considered 6-point laws (Everytown, 2023).

Gun control literature often looks at comprehensive background checks (CBC), which add

[6]Once removing 'firearm, type not stated' which accounts for roughly 30% of reported firearm homicides.

the requirement for background checks for private sellers. This is above and beyond the requirement for federally licensed dealers. Federally licensed sellers must always perform a background check, but state laws differ for private sellers. The literature on CBC laws, which frequently overlaps with permit-to-purchase laws, shows mixed results for downstream outcomes. Kalesan et al. (2016) and Siegel et al. (2019) find that background checks are associated with lower mortality. Disentangling permit-to-purchase from CBC laws, Kagawa et al. (2018) and Castillo-Carniglia et al. (2019b) add to the CBC literature showing that CBC repeal and passage do not impact homicides and only Castillo-Carniglia et al. (2019b) find an impact on suicides. This result even holds when CBC is paired with misdemeanor violence prohibition policies, which restrict purchasing for some misdemeanors. McCourt et al. (2020) find that CBC laws that are paired with licensing laws (like permit-to-purchase) are consistently associated with lower suicide and homicide rates. McCourt et al. (2020) also find that CBC laws alone were not associated with decreases.

While CBC literature shows mixed, weak, or no results on suicides and homicides, the literature shows permit-to-purchase laws are associated with a decrease in firearm suicides and homicides (e.g. Rudolph et al. (2015) and Crifasi et al. (2015)). These results extend to the younger demographic, with **?** showing increased suicides among the young.

The mechanism behind these differences in results is likely the impact on the cost of purchasing associated with permit-to-purchase and CBC laws. CBC laws are designed to restrict the purchasing for people with criminal backgrounds. CBC laws do not provide any tangible increase in cost for people without criminal backgrounds. This is because, while the FBI has 3 days to respond to a background check, most background checks are instantaneous (National Rifle Association Institute for Legislative Action (2019)). On the other hand, going through the state licensing requirements increases the cost associated with purchasing a weapon for all first-time buyers (or renewers). The prospective purchaser must complete all of the requirements before purchasing, instead of showing up and waiting for an often instantaneous background check. Li et al. (2023) provides evidence for the mechanism showing that purchases are affected for criminals as the proportion of guns being purchased in the state increases among guns recovered from crime.

Maryland, Missouri, and Connecticut passed permit-to-purchase laws and are the focus of this paper. In 1995, Connecticut passed a permit-to-purchase law. The law required background checks and safety training and raised the minimum age to get a permit and purchase a handgun. The passage was part of a widespread effort to increase gun control in a state that had minimal gun control laws that pertained to handguns. While the law is permit-to-purchase, it highlights the complexity of categorizing laws within each category. Missouri repealed permit-to-purchase in late 2007. This repealed the entire process of ob-

taining the permit: background checks, minimum age, and registration. I refer to these inclusions (background checks, minimum age, and registration) in the permit-to-purchase law as 'comprehensive permit-to-purchase' to distinguish from the licensing requirement. In Missouri, the law also extended the existing Castle Doctrine. Missouri's repeal was passed as a crime prevention bill, based on the deterrence effect of gun-carrying law-abiding citizens theory to lower crime rates, and was passed with other crime prevention laws.

Sweeping gun control, that is going from very few (lenient) restrictions to many (strict) or vice versa, is rare. This is especially true since more data have been available to study the impact of the laws. Other states have added or modified such laws similarly to a step function, adding or removing laws from their existing gun control laws one or two at a time. An example of this is Maryland, which, in late 2013, passed permit-to-purchase. In this case, Maryland added the licensing portion of the law for handgun private sales, which requires the completion of safety training. Maryland already had background checks, minimum age, and registration of the sale with the state. This law added the requirement for the purchaser to obtain a license before a purchase could be made. In contrast to Missouri and Connecticut, this is an incremental change within the context of the gun laws within the state.

2.1. Data and Method

To analyze the impact of permit-to-purchase, I look at the following primary outcomes: firearm homicides, firearm deaths, all homicides, and violent-non-homicide crime rate, as these outcomes are available for all states in the necessary years. All of the variables will be at the state-by-year level. Firearm suicides and homicides are from the Centers for Disease Control and Prevention's Restricted-Use Vital Statistics data for all years. The violent-non-homicides crime rate is the sum of robbery, assault, and rape. This measure removes property crime to track the crime that involves a victim who could be carrying a gun. Following early papers by Lott, which sparked the idea that fewer restrictions on guns could decrease crime, I look at crime that involves the potential cost increase/decrease of guns being present/absent during a crime. For example, in Missouri, allowing guns to be purchased more easily allows for a greater chance that citizens have guns and increases the potential cost of crime to criminals.

Secondary outcomes are time-to-crime, self-sourced ratio, and federal background check data. Background check data comes from the FBI's National Instant Criminal Background Check System (NICS). Time to crime reports the average length of time in months/days until the gun is used in a crime from the purchase date. For a gun to be included in this statistic,

the gun has to have a traceable purchase date and be recovered by law enforcement.[7] Self-source recovery ratio is the proportion of guns recovered by the state that were purchased within the state. The gun does not necessarily need to be used in a crime to be included in this summary, but this is the most common form of recovery. There are guns included that do not have a purchase date but can be traced back to a state of origin. All trace data, used for time-to-crime and self-sourced ratio, are from the Bureau of Alcohol, Tobacco, Firearms and Explosives and are limited to 2006-2020. These outcomes will be at the state-by-year level.

State-by-year level demographic variables consist of the proportion of the population in the following categories: ages 15-24, Black, Hispanic, and living in a metropolitan statistical area. Economic controls are the proportion of the population living in poverty, the state-level Gini coefficient, and jobs per adult. Lastly, crime controls are the rates of the following crimes: robbery, larceny, property crime, motor vehicle theft, assault, and rape. The proportion aged 15 to 24 years, the proportion Black, the proportion Hispanic, and the proportion of the population living at or below the poverty line are gathered from the United States Census Bureau. A state-level Gini coefficient is from Frank (2009). Jobs per adult is from the Bureau of Economic Analysis. Lastly, the proportion of the population that lives in an MSA as well as the rates for larceny, robbery, assault, rape, motor vehicle theft, and property crime are from the Federal Bureau of Investigation's Uniform Crime Reporting.

To measure heterogeneous effects, I define counties as metropolitan or rural based on the National Center for Health Statistics (NCHS) classification. There are three classifications over time (1990, 2006, 2013), and I use the classification system published closest in time to the date of the policy for the entirety of that state's analysis. This way, the counties do not change in the pre- and post-intervention periods. For Connecticut, Missouri, and Maryland, the specifications used to define metropolitan and rural were 1990, 2006, and 2013, respectively.

Descriptive statistics for the three treated states and overall at the national level are presented in Table A.1, for 2020.[8]. Relative to the national average, Connecticut has a high gun control score, low crime, and a low firearm homicide rate. Missouri has a low gun

[7] This system is used to track weapons to purchase date or a report submitted by local, federal, or international law enforcement. This is done with serial numbers, which can be traced to the purchase date if the purchase was recorded. The serial number can also be used to locate the manufacturer, place, and date of manufacture. The serial number is also tracked with the type of crime, date of recovery, and names of individuals involved. When tracked to a sale, the data are usually tracked to the first sale. Furthermore, the recoveries are not always crime-related; however, time-to-crime uses recoveries used in a crime, only.

[8] These are 2020 descriptive statistics but highlight that motivation could differ among states and the impact of the laws is susceptible to this endogeneity.

control score, high crime rates, and a high firearm homicide rate. Maryland has a high gun control score and a high firearm homicide rate. The crime rates, income per capita, and demographics that make up the state all differ from each other. Given the differences in the environment in which PTP is passed (or repealed) and the nature of the laws themselves, I construct a synthetic control state to provide the proper counterfactual of what would have happened in the treated state had the law not been passed.

2.2. Method

To estimate the causal effect of each of the three law changes, I construct a synthetic control using the same framework for the three states' analyses. This allows for the comparison of the differences in pre- and post-intervention for three permit-to-purchase law changes relative to a control that takes into account treatment over time and improves on a traditional pre/post design. This controls for the shocks over time and the heterogeneous treatment effects between states. Connecticut and Maryland passed a permit-to-purchase law in late 1995 and 2013, respectively. Missouri repealed permit-to-purchase in 2007. When data are available, 10 years before the policy change are used to match the treated state with the synthetic control.

To facilitate a better synthetic control match, all demographic, economic, and crime controls are included in the matrix used to match. A three-year rolling average is used for outcomes, which facilitates a better match and is common for examining firearm-related deaths (Rudolph et al. (2015) and Crifasi et al. (2015)). The main results are presented as every year matching. However, the results are not sensitive to this decision, and every other year matching is presented alongside every year. Additionally, I do robustness checks by matching only in the first 5 years to allow a five-year validation period prior to the policy change.

Statistical inference is based on p-values produced from placebo tests following Abadie et al. (2010). The placebo tests treat each state in the donor pool as treated, i.e., they are coded as the treatment state in the same year as the actual treated state being considered. The results of these placebo tests are compared to the actual treatment state to gauge the relative size of a state's response relative to other states whose change is clearly not due to a policy but rather captures the likelihood that states spuriously diverge from their synthetic control in the treated year. To control for the relative fit of the synthetic controls, states are removed from the comparison group if their relative mean squared prediction error (MSPE) in the pre-intervention period is not close to the MSPE of the treatment state in the pre-intervention period. I present 20, but included 5 and 2 times the treatment state's MSPE.

Throughout the paper, I report the 'p-values', the fraction of states with more extreme results than the treatment, for the restriction based on 20 times the MSPE, but for both every-year and every-other-year match. This leaves the number of control states relatively large and only removes states where the procedure does not find a reasonable match (assuming the procedure finds a good match for the treatment state). If the treatment state is the most extreme state, the fraction will be one (treatment state) over the number of control states plus one for the treatment state. This can make the p-value large if the number of control states is small (which is the case when MSPE limits are lowered). For that reason, I also include the fraction that determines the p-values and make a note when the p-value is large but the state is the first or second most extreme response overall.

Following Abadie et al. (2010), the synthetic control is created using matrix minimization methods in the pre-intervention period. The synthetic control is a weighted average of control pool states and is created using the pre-intervention period, but applies to the post-intervention period as well:

$$\hat{Y}_{t,Synth} = Y_{t,CP}W. \tag{1}$$

Here, CP stands for Control Pool, Y is the outcome variable, and t is any year pre- or post-intervention. To get W, which is a matrix of weights, minimize the following equation for the designated time before the policy intervention:

$$(X_{Target} - X_{CP}W)'V(X_{Target} - X_{CP}W) \tag{2}$$

where X is a matrix of variables for each state, including the outcome, and V is the relative importance of each variable in predicting the outcome variable for the target state in the pre-intervention period. The designated time will depend on data availability and the matching procedure, but will usually be 10 years before the law changes. This determines the relative importance of control variables in predicting the outcome and then finds the appropriate weighted average of control states that minimizes the gap between the synthetic control and the treated state.

The synthetic control for each state is designed to match corresponding outcome variables in the pre-intervention period. States can still differ from their synthetic control on other characteristics, such as the proportion of the population in various minority categories or the proportion of the population living in the MSA. The key assumption behind this approach is that states would have tracked their synthetic control on the outcome of interest in the post-intervention period had the policy not been enacted. These characteristic differences do not affect that assumption. Also, while this assumption can not be directly tested, I present empirical evidence in support of this assumption in two ways. First, I show that the synthetic control and the treated state track one another prior to the policy implementation.

Second, when I only match the first half of the pre-intervention period, I show that the synthetic control and treatment state track until the policy is passed and diverges only after the implementation or repeal.

2.2.1. Spillover Analysis

To check for any spillover effects as a result of a policy change, I look at counties that border the treatment states. If guns in driving range (i.e., border counties in neighboring states) become easier to purchase, then there could be an increase in firearm homicides and suicides, and vice versa. The change in purchase difficulty could also affect the flow of guns across the border. For the flow of guns, I present the change in guns flowing from treatment states to border states. For firearm homicides and suicides, I repeat the synthetic control method with a few modifications. The specification used for the three primary states is applied to border counties. The border counties are grouped together, border-county-state (BCS), to detect changes in the BCS's outcome relative to their synthetic control during the time of the neighboring state's law change. There are some challenges with matching as a result of the border counties having unique demographic details or unusually low firearm homicide or suicide rates.

3. Results

Results are separated into three sections: purchasing and usage behavior, crime, and homicide and suicide outcomes. The direction of expected results should be different depending on the state. Maryland and Connecticut add restrictions to the existing gun laws, while Missouri repeals restrictions. For each state and outcome, I will show the synthetic control graphs plotting the state average along with two synthetic controls implementing the two matching procedures discussed above. The main results are firearm homicides, firearm suicides, and violent non-homicide crimes, all per 100,000 persons. For each state, when permitted by the data, the death-related outcomes are broken down between rural and metropolitan areas. The last homicide and suicide results are spillovers, with suicides being based on residence, not occurrence. Purchasing and usage behavior includes background checks and trace data. Background checks are in checks per 100,000. Trace outcomes will be in either years since the purchase or the proportion of guns purchased in a state. Connecticut is sometimes omitted due to data limitations

3.1. Background Checks and Trace Data

To check the impact on purchasing behavior, this section looks at background checks and trace data. Since background checks are required for purchases from federally licensed dealers, background checks are a good proxy for purchases. This is not a perfect proxy since this number includes rejections and background checks that do not lead to purchases. Additionally, there are other ways to buy guns in states with fewer restrictions (e.g. used guns and private non-federally-licensed dealers).

Trace data consists of self-sourced ratio and time-to-crime. Self-sourced ratio, which is the number of guns used in crime that are purchased in the state where the crime occurred, measures where guns are being purchased for crime. Time-to-crime, which is the crime date minus the purchase date, measures how quickly guns are being used in crime. These three outcomes are a crucial aspect of the public health debate surrounding gun laws. Generally, if permit-to-purchase makes guns more difficult to purchase, then the time-to-crime should grow, the self-sourced ratio should decrease, and background checks should decrease. Conversely, for the repeal of permit-to-purchase, one might expect the opposite to happen: an increase in the self-sourced ratio, a decrease in time-to-crime, and an increase in background checks. Missouri and Maryland can offer some insight into the impact permit-to-purchase on these statistics.[9]

For background checks, Missouri and Missouri's synthetic control stay relatively close and do not deviate in the pre-intervention period. This tracking provides evidence that Missouri and Missouri's synthetic control would have continued to match without treatment. The synthetic control tracks across the two matching procedures of every year and every other year. Following the repeal, Missouri deviates from synthetic Missouri in the post-intervention period (Figure B.1a). This deviation is an average, per year, of 902 more background checks per 100,000 people. This is a 12.9% increase above the synthetic control.

To assess the significance, Missouri's deviation is compared to the other states in the control group. Each control state goes through the synthetic control procedure, and the deviations are compared. Missouri is never the most extreme response. This is reflected in the p-values. The differences between Missouri's observed number of background checks and those in the synthetic Missouri aren't statistically significant at conventional levels (0.172 and 0.375). Missouri's repeal included the repeal of state background check laws. This allowed for sales by private dealers without background checks, unless they were licensed. Licensed dealers would still be covered by the Brady Law, which requires federally licensed dealers to perform background checks. The background check data only captures an increase in

[9]Connecticut does not have data available for this subsection due to the earlier passage of its law.

background checks from licensed dealers and not any increases in sales from private dealers. The increase in federal background checks could be an indication of a broader increase in gun purchases throughout the state that are not captured here. However, trace data can help further illustrate the pattern for purchasing and usage in Missouri.

For Missouri, the pre-intervention period for trace data is only 2 years. Figure B.2 shows the self-sourced ratio and time-to-crime synthetic control results. For the self-sourced ratio, the values do not match in the pre-intervention period. Following the policy intervention, there is a clear increase in Missouri's self-sourced ratio compared to the synthetic control. This gap widens over the second half of the post-intervention period. Due to the poor match, the causal interpretation is not possible, but there is suggestive evidence that the changes led to more guns being purchased in Missouri, which were later recovered in crimes. For time-to-crime, the match shows an early deviation but is close in value. Following the repeal of permit-to-purchase, time-to-crime for guns purchased in Missouri falls, and the gap between Missouri and the synthetic control widens. The placebo graphs, Figure B.2, show that Missouri's gap is among the most extreme values for most of the post-intervention period. This gap is a 28.9% decrease in time-to-crime over the entire period, which is an average of 2.4 years sooner for a gun to be used in a crime, but not significant at conventional levels. From 2006 to 2018, Missouri had a 5-year decrease in the average time-to-crime. The causal interpretation is difficult with only two years to match on in the pre-intervention period, but there is suggestive evidence that the repeal of the permit-to-purchase changed the purchasing behavior and usage of guns used for crime in Missouri.

Maryland's background check data tells the opposite story compared to Missouri, as expected given the passage of the policy instead of a repeal. First, Maryland tracks with the synthetic control in the pre-intervention period before a deviation after the intervention; a large and sudden drop in background checks compared to the synthetic control (Figure B.1c). This decrease is an average of 765 background checks per 100,000 people, per year, compared to the synthetic controls. Maryland is 23.4% lower than the synthetic control. The placebo graph, in Figure B.1d, shows that Maryland is among the most extreme responses. Maryland is always between the 2nd and 4th most extreme response across all specifications, but not statistically significant at conventional levels (0.107 and 0.167)

For the self-sourced ratio, Maryland's ratio of self-sourced guns falls relative to synthetic control (Figure B.2e). The average gap in the post-intervention period is a 9.4 percentage point decrease in the self-sourced ratio. Maryland is the most extreme result in all placebo tests, making it statistically significant at conventional levels (0.034 - 0.050). Moreover, when matching only in the first half of the pre-intervention period, Maryland continues to match prior to the intervention before the large decrease following the intervention (see

Online Appendix). Maryland behaves the same as it does in the main specification with the deviation happening before returning to a normal level.

For time-to-crime, as seen in Figure B.2g, Maryland tracks well with the synthetic control methods in the pre-intervention period. Following the passage of permit-to-purchase, Maryland maintained a similar time-to-crime (12.1-year average from 2014-2020). The synthetic control sees a decrease in time-to-crime over the same time frame. The gap widens over the first few years and then levels off from 2016 to 2020. The average difference between Maryland and the synthetic control from 2016-2020 is 3.3 years. Maryland is the most extreme response for each year in the post-intervention period compared to the donor pool(Figure B.2h). The p-values for the time-to-crime range are 0.057 and 0.059, suggesting significance at conventional levels. Lastly, matching in the first half of the pre-intervention period, the deviation from the synthetic control does not happen until the implementation of the permit-to-purchase law. This provides evidence that the identifying assumption is plausible and that the synthetic control and Maryland would have tracked in the post-intervention period. These results are also consistent with the observation from Missouri.

3.2. Homicides and Suicides

The most often considered area of focus when analyzing the impact of gun control laws is deaths. Firearm homicides and firearm suicides are an important aspect of the public health debate. To assess the impact, I replicate earlier work on whole-state results for homicides and suicides. This sets a foundation for the analysis of the breakdown between rural and metropolitan areas. For the last part of the analysis on homicides and suicides, I then treat the counties that border the treatment state as a collective area and check for spillovers.

3.2.1. Whole State Replication

Before looking at the breakdown between metropolitan and rural areas, I will go over the homicide and suicide results for each state as a whole. This section is largely a replication of earlier research, which is why the visual results are relegated to the Online Appendix. While this is a replication, the results for the whole state are necessary to understand the context for the breakdown between metropolitan and rural areas. For the most part, the replication finds similar size effects for Missouri and Connecticut. The p-values differ slightly, which is likely the result of more data and a different synthetic control matching procedure.

In Connecticut and Missouri, the results largely follow earlier work. In Connecticut, there is a decrease in both homicides and suicides. For homicides, most results are significant at conventional levels, similar to earlier work. However, in contrast to earlier results, the suicide

results are only suggestive. This could be due to extra data availability in small states or different procedures for synthetic control. All other results are similar. In Missouri, the results follow earlier works showing an increase in suicides and homicides. Homicides are the stronger result and statistically significant, while suicides are not significant but follow earlier results for suggestive increases in suicides. The results of Maryland's licensing change in 2013 are new. For both homicides and suicides, there is no evidence of an impact from the licensing change. Results for statewide analysis for all three states are relegated to the Online Appendix.

3.2.2. Metropolitan and Rural Breakdown

[HERE] The breakdown between metropolitan and rural areas can help identify which areas are being impacted and if there is an impact in one area that is not being seen in the whole state analysis from Section 3.2.1. Generally, the suicide rate per capita is higher in rural areas, while the homicide rate per capita is higher in metropolitan areas. When aggregating the results to the whole state, increases in rural areas might be suppressed due to the relative population sizes. This breakdown should identify heterogeneous effects between the two areas.

Starting with Missouri, the whole state results show an increase in homicides and a suggestive increase in suicides near the end of the post-intervention period. The breakdown between the metropolitan and rural can be seen in Figure B.3. Across both matching procedures, matching every year and matching every other year, the synthetic control tracks with Missouri in all four outcomes (rural and metropolitan for suicides and homicides). Following the repeal of the permit-to-purchase, the gap for firearm homicides in metropolitan areas is immediate. The gap is steady, depending on the matching procedure, until it jumps in 2015. The 2015 jump happened in both rural and urban areas. This indicates that the jump in the second half of the post-intervention period in Missouri's main results is due to an increase in both metropolitan deaths and rural deaths. For suicides, the pattern is similar but less pronounced. There is a small gap immediately in metropolitan areas that jumped in 2014. The jump is also seen in rural areas. Together, these two results show a delayed response to the policy in rural areas. The initial jump in the main results is due to the metropolitan areas, while the jump in the second half of the post-intervention period is from both areas.

In Connecticut, there is a similar pattern for suicides. There is some noise in the matching procedure, but the synthetic controls largely track with Connecticut (Figure B.4). Following the implementation of permit-to-purchase, there is an immediate decrease in metropolitan areas. In the second half of the post-intervention period, the gap in rural areas increases.

Again, there is a pattern of delayed impact in rural areas and immediate impact in urban areas.

For Maryland, the main results indicate that there was not a significant impact on firearm suicides or firearm homicides. The purpose of this breakdown is the see if there is some evidence of an increase that was not picked up in the aggregate. This differs slightly from Missouri and Connecticut which had strong or suggestive evidence of an increase in this replication or earlier research. For the synthetic control match, the results are noisy for rural areas but both areas and both outcomes track fairly well. Following the implementation of the permit-to-purchase licensing requirement, rural homicide, rural suicides, and metropolitan homicides all mirror the aggregate results. Metropolitan suicides show some evidence of an immediate decrease that levels off in the second half of the post-intervention period.

The three states together have a consistent pattern for metropolitan areas. If there is an impact, the metropolitan areas show it immediately. Both the rural and metropolitan areas also see delayed results. Both areas can be affected by these policies, and individual areas may be affected without being captured in aggregate analysis.

3.2.3. Spillovers

The mechanism for spillover effects is driven by the relative ease or difficulty of purchasing in border states. To check this mechanism for guns used in crime, using trace data, I look at how the supply of guns from the treatment states (Maryland and Missouri) to neighboring states changes. Figure B.6a shows the ratio of guns from Missouri that are recovered in Kansas or Illinois, relative to all other guns sourced from outside states recovered in Kansas or Illinois[10]. Missouri's ratio shows a steady increase in the number of guns supplied, relative to all other outside states, that are later recovered in Illinois and Kansas. Relative to all other outside states, Maryland has a similar outcome, in the opposite direction as Missouri (Figure B.6b). The ratio of guns supplied to neighboring states with shared metropolitan areas decreases following the implementation of the licensing requirement. This is suggestive of the mechanism for spillovers being present, at least for guns used in crime.

To follow up on the suggestive evidence that the mechanism for spillovers is present, I look at how permit-to-purchase affects firearm suicides and homicides in border counties. I treat all of the counties that border a treatment state as a single state and repeat the primary synthetic control analysis for these border counties. I remove the rest of the counties from

[10]e.g. Kansas Ratio = (Missouri Guns Recovered in Kansas) / (All other out of state guns recovered in Kansas

neighboring states from the donor pool and also remove the original treatment state. I repeat the synthetic control procedure from the original analysis on the border county state (BCS) while removing all border states from the control pool. Crime rates have dropped from controls due to data limitations at the county level for treatment states. Additional age groups are added to improve the match since crime rates are not used.

The BCS for Missouri tracks their synthetic control well for suicides prior to the intervention (Figure B.7). For homicides, there is a deviation before the repeal, but the deviation disappears before the treatment. This should be kept in mind when interpreting the post-intervention results. Following the treatment, homicides in Missouri track with the synthetic control. Suicides show an immediate increase over the synthetic control (Figure B.7c), and the placebo graphs suggest that Missouri's BCS is among the most extreme responses (Figure B.7d). This gap between Missouri's BCS and the synthetic control for the first half is a 6.1% increase in suicides. Over the second half of the post-intervention period, the gap widens to 9.7%. For the entire post-intervention period, there is an 8.2% increase in suicides, making Missouri the second most extreme response (0.125 and 0.154).

For Maryland, the BCS tracks well with the synthetic control before the intervention (Figures B.8). Following the implementation of the licensing requirement, there is evidence of a drop in firearm homicides compared to the synthetic control. For the first half of the post-intervention period, Maryland's BCS is the most extreme result. Overall, there is a 25.7% decrease compared to the synthetic control from 2014 to 2018 and a 14.1% decrease over the entire post-intervention period. This is suggestive of a spillover effect, especially early on in the post-intervention period but it is not significant at conventional levels. For firearm suicides, there is no evidence of deviation from the synthetic control in any specification.

For Connecticut's BCS, there is not a good synthetic control match. This could be due to the unique demographics in the combined border counties. Depending on how those demographics contribute to predicting firearm homicides and suicides during the fitting procedure, there could be difficulty matching. Furthermore, the low suicide and homicide rate in the spillover BCS also creates difficulty in finding similar matches. Many of the border states that are removed from the control pool have similarly low firearm homicide and suicide rates (e.g., Rhode Island). While the match is not good, visually, there is a pattern of the rates remaining unaffected during the policy intervention, maintaining the gap in the post-intervention period for both outcomes (Figure B.9). Notwithstanding problems with obtaining estimates in Connecticut, Maryland, and Missouri's spillover analysis of suicides and homicides presents evidence of spillovers in border counties.

3.3. Crime Rates

A second aspect of the public health debate is the impact on crime. Gun laws that make guns more difficult to buy might lower the probability of a citizen having a gun and thus make the cost of crime lower. For this reason, I break crime into two categories: those involving people and those involving property. For Missouri, this section is relevant to the political environment when this bill was passed. Missouri's repeal was passed as a crime bill, relying on the deterrence effect of gun-carrying citizens. Connecticut's and Maryland's laws were passed as gun control bills.

For violent non-homicide crime, the story for all three states is similar (Figure B.10). Each state has a good synthetic control match pre-intervention, followed by no deviation after the law change. These results suggest that there was no evidence of a decrease in non-homicide violent crime rates. The placebo graphs show that all results are near the middle, indicating a precise null, and none of the p-values suggest statistical significance at conventional levels. The robustness check, which is only matching in the first 5 years and checking for early deviation, is again used to provide support for the identifying assumption. In Maryland and Missouri, there is support for the identifying assumption from this reobustness test, but there is an early deviation in Connecticut (see the Online Appendix). However, in Connecticut, the synthetic control in the post-intervention period remains parallel to Connecticut.

For property crime, there are two possible effects. In Missouri, the repeal might cause a substitution to or an increase in crime that doesn't involve people. The other effect could be the perceived increase in risk—targeting property could still result in facing an armed citizen. For Connecticut and Maryland, the opposite effect is expected. The synthetic control match is good for Connecticut and Maryland. In Missouri, the control and treatment are parallel, heading into treatment. In all three states, there is no evidence of a change in property crime. In Missouri, the gap remains the same, but the poor synthetic control match makes it hard to make a definitive claim. Maryland and Connecticut continue to track with the control after treatment. There is a lack of evidence that the change in gun laws had any impact on crime involving other people or property.

4. Discussion

This paper evaluates the consequences of adopting or repealing permit-to-purchase firearm laws, some of the strictest regulatory mechanisms available for purchasing handguns. While much of the existing debate focuses on whether criminals bypass such laws, my findings suggest that PTP policies meaningfully alter both legal purchasing behavior and the use

of guns in crime, with downstream effects on firearm mortality. First, there is no evidence that PTP laws increase non-homicide violent crime or property crime. This finding offers evidence against the "more guns, less crime" hypothesis that underpins many permissive gun policy arguments. Despite claims that armed victims deter crime, the repeal of PTP laws did not reduce crime. Nor did adoption embolden criminals. Changing the legal barriers to gun purchasing does not appear to shift deterrence dynamics.

Second, the impact of PTP laws on firearm deaths is substantial. I find consistent evidence that repealing a PTP law leads to increases in firearm homicides and suicides, while adopting one reduces them. These effects are concentrated in metropolitan areas but also spill over to rural counties and sometimes neighboring states. Importantly, these patterns persist across a range of robustness checks, indicating that these are not one-off results.

Third, PTP laws affect how guns are purchased and how quickly they are used in crimes. In Missouri, repeal led to more guns being purchased (as measured by background checks), a higher proportion of crime guns being sourced in-state, and a faster time-to-crime. In contrast, Maryland's adoption of PTP led to fewer background checks, fewer crime guns sourced in-state, and longer delays between purchase and criminal use. These behavioral shifts show that even when guns are plentiful, the policy environment influences both buyers and offenders.

Together, these findings suggest that PTP laws can be effective tools for reducing firearm violence and altering the supply chain of crime guns, without evidence of unintended consequences in crime rates. While the abundance of firearms and informal markets might be expected to weaken regulatory impact, the data show that targeted policies like PTP still shape gun purchasing in meaningful ways. The broader implication is that while no single law can eliminate gun violence, policies that raise the cost threshold for legal gun purchasing, especially when tied to background checks and training, may provide public health benefits. Policymakers considering reforms should weigh these benefits against claims that such laws are ineffective or impose unjustified burdens. The evidence here suggests that permit-to-purchase policies have real potential to reduce firearm harm without negatively impacting public safety through crime rate changes.

Appendices

A. Tables

Table A.1: Descriptive Statistics for the Three Treatment States and National Average: 2020

	National Average	Connecticut	Maryland	Missouri
All Firearm Deaths Rate	15.24	6.15	13.26	23.18
Firearm Homicide Rate	5.30	2.83	8.68	11.10
Firearm Suicide Rate	9.33	3.06	4.40	11.44
Homicide Rate	6.91	4.27	10.71	13.05
Proportion Black	0.101	0.105	0.301	0.115
Proportion Hispanic	0.125	0.172	0.108	0.0450
Proportion in MSA	0.761	0.950	0.975	0.751
Per Capita Income in $1,000	56.95	78.46	65.68	52.11
Robbery Rate	55.53	57.15	118.46	74.37
Rape Rate per 100,000	44.61	16.70	28.61	43.26
Assault Rate	278.59	103.79	243.66	413.33
Motor Vehicle Theft Rate	230.65	236.77	172.88	393.22
Percent in Poverty	11.14	11.20	9.20	10.60
Jobs per Adult	0.793	0.810	0.7920	0.798
Self-Sourced Ratio	0.706	0.417	0.448	0.801
Average Time-to-Crime	6.58	10.71	10.91	4.27
Gun Strength Score	32	78.5	72.5	9

All rates are annual averages per 100,000 people. Homicide and Suicide rates come from the CDC's Restricted-Use Vital Statistics. The demographic data comes from the United States Census Bureau. Other crime rates come from the FBI's Uniform Crime Reporting. Jobs and Poverty data comes from the Bureau of Economic Analysis. Gun strength comes from the Everytown research group. Self-sourced ratio and average time-to-crime come from the Bureau of Alcohol, Tobacco, Firearms, and Explosives.

Table A.2: Missouri's p-values for Each Outcome and Matching Procedure

Outcome	Control States Included	Every Year	Every Other Year
Firearm Homicides	<20 MSPE	0.024 (1/41)	0.023 (1/43)
	<5 MSPE	0.053 (1/19)	0.042 (1/24)
	<2 MSPE	0.200 (1/5)	0.111 (1/9)
Firearm Suicides	<20 MSPE	0.370 (17/46)	0.391 (18/46)
	<5 MSPE	0.447 (17/38)	0.450 (18/40)
	<2 MSPE	0.500 (17/34)	0.500 (17/34)
Violent Non-homicide Crime	<20 MSPE	0.657 (23/35)	0.571 (16/28)
	<5 MSPE	0.842 (16/19)	0.813 (13/16)
	<2 MSPE	0.929 (13/14)	0.900 (9/10)
Property Crime	<20 MSPE	0.792 (38/48)	0.688 (33/48)
	<5 MSPE	0.792 (38/48)	0.688 (33/48)
	<2 MSPE	0.804 (37/46)	0.733 (33/45)
Firearm Homicides - Metro	<20 MSPE	0.022 (1/45)	0.044 (2/45)
	<5 MSPE	0.023 (1/43)	0.044 (2/45)
	<2 MSPE	0.027 (1/37)	0.047 (2/43)
Firearm Homicides - Rural	<20 MSPE	0.229 (8/35)	0.286 (10/35)
	<5 MSPE	0.350 (7/20)	0.364 (8/22)
	<2 MSPE	0.364 (4/11)	0.545 (6/11)
Firearm Suicide - Metro	<20 MSPE	0.083 (3/36)	0.028 (1/36)
	<5 MSPE	0.100 (3/30)	0.037 (1/27)
	<2 MSPE	0.188 (3/16)	0.056 (1/18)
Firearm Suicide - Rural	<20 MSPE	0.467 (21/45)	0.457 (21/46)
	<5 MSPE	0.488 (21/43)	0.488 (21/43)
	<2 MSPE	0.525 (21/40)	0.525 (21/40)
Background Checks	<20 MSPE	0.172 (5/29)	0.375 (12/32)
	<5 MSPE	0.235 (4/17)	0.450 (9/20)
	<2 MSPE	0.500 (4/8)	0.438 (7/16)
Self Sourced Ratio*	<20 MSPE	0.854 (41/48)	
	<5 MSPE	0.867 (39/45)	
	<2 MSPE	0.872 (34/39)	
Time-to-Crime*	<20 MSPE	0.455 (20/44)	
	<5 MSPE	0.588 (20/34)	
	<2 MSPE	0.667 (20/30)	

The p-values in this table are the ratio of donor pool states with a more extreme MSPE in the post-intervention period that qualify by MSPE size in the pre-intervention period and come from the placebo procedure listed in the Method section. 'Every Year' and 'Every Other Year' refer to the matching procedure used for the synthetic control.
*Pre-period does not allow for every other year matching.

Table A.3: Connecticut's p-values for Each Outcome and Matching Procedure

Outcome	Control States Included	Every Year	Every Other Year
Firearm Homicides	<20 MSPE	0.104 (5/48)	0.043 (2/47)
	<5 MSPE	0.139 (5/36)	0.053 (2/38)
	<2 MSPE	0.200 (5/25)	0.077 (2/26)
Firearm Suicides	<20 MSPE	0.171 (7/41)	0.216 (8/37)
	<5 MSPE	0.292 (7/24)	0.320 (8/25)
	<2 MSPE	0.300 (6/20)	0.353 (6/17)
Violent Non-homicide Crime	<20 MSPE	0.979 (46/47)	0.978 (44/45)
	<5 MSPE	0.973 (36/37)	0.971 (33/34)
	<2 MSPE	0.963 (26/27)	0.958 (23/24)
Property Crime	<20 MSPE	0.730 (27/37)	0.706 (24/34)
	<5 MSPE	0.840 (21/25)	0.704 (19/27)
	<2 MSPE	0.800 (12/15)	0.714 (10/14)
Firearm Suicide - Metro	<20 MSPE	0.273 (12/44)	0.318 (14/44)
	<5 MSPE	0.294 (10/34)	0.400 (14/35)
	<2 MSPE	0.417 (10/24)	0.429 (12/28)
Firearm Suicide - Rural	<20 MSPE	0.244 (11/45)	0.333 (15/45)
	<5 MSPE	0.244 (11/45)	0.333 (15/45)
	<2 MSPE	0.263 (10/38)	0.368 (14/38)

For all homicides and firearm homicides, Connecticut is the 2nd most extreme or most extreme response in all cases.
The p-values in this table are the ratio of donor pool states with a more extreme MSPE in the post-intervention period that qualify by MSPE size in the pre-intervention period and come from the placebo procedure listed in the Method section. 'Every Year' and 'Every Other Year' refer to the matching procedure used for the synthetic control.

Table A.4: Maryland's p-values for Each Outcome and Matching Procedure

Outcome	Control States Included	Every Year	Every Other Year
Firearm Homicides	<20 MSPE	0.717 (33/46)	0.804 (37/46)
	<5 MSPE	0.717 (33/46)	0.804 (37/46)
	<2 MSPE	0.717 (33/46)	0.804 (37/46)
Firearm Suicides	<20 MSPE	0.333 (8/24)	0.412 (14/34)
	<5 MSPE	0.375 (3/8)	0.519 (14/27)
	<2 MSPE	0.286 (2/7)	0.722 (13/18)
Violent Non-homicide Crime	<20 MSPE	0.936 (43/47)	0.957 (45/47)
	<5 MSPE	0.955 (42/44)	0.956 (43/45)
	<2 MSPE	0.957 (41/43)	0.977 (43/44)
Property Crime	<20 MSPE	0.429 (18/42)	0.429 (18/42)
	<5 MSPE	0.600 (18/30)	0.548 (17/31)
	<2 MSPE	0.667 (14/21)	0.667 (14/21)
Firearm Homicides - Metro	<20 MSPE	0.614 (28/45)	0.682 (31/45)
	<5 MSPE	0.625 (26/41)	0.705 (31/44)
	<2 MSPE	0.622 (24/38)	0.711 (27/38)
Firearm Homicides - Rural	<20 MSPE	0.178 (8/45)	0.154 (4/26)
	<5 MSPE	0.147 (5/34)	0.174 (4/23)
	<2 MSPE	0.185 (5/27)	0.250 (4/16)
Firearm Suicide - Metro	<20 MSPE	0.103 (3/29)	0.200 (3/15)
	<5 MSPE	0.143 (2/14)	0.357 (3/8)
	<2 MSPE	0.286 (2/7)	0.400 (2/5)
Firearm Suicide - Rural	<20 MSPE	0.244 (11/45)	0.190 (8/42)
	<5 MSPE	0.300 (9/30)	0.296 (8/27)
	<2 MSPE	0.500 (9/18)	0.467 (7/15)
Background Checks	<20 MSPE	0.167 (5/30)	0.107 (3/28)
	<5 MSPE	0.214 (3/14)	0.200 (3/15)
	<2 MSPE	0.375 (3/8)	0.375 (3/8)
Self-Sourced Ratio	<20 MSPE	0.034 (1/29)	0.050 (1/20)
	<5 MSPE	0.043 (1/23)	0.111 (1/9)
	<2 MSPE	0.063 (1/16)	0.200 (1/5)
Time-to-Crime	<20 MSPE	0.059 (2/34)	0.057 (2/35)
	<5 MSPE	0.095 (2/21)	0.091 (2/22)
	<2 MSPE	0.154 (2/13)	0.133 (2/15)

The p-values in this table are the ratio of donor pool states with a more extreme MSPE in the post-intervention period that qualify by MSPE size in the pre-intervention period and come from the placebo procedure listed in the Method section. 'Every Year' and 'Every Other Year' refer to the matching procedure used for the synthetic control.

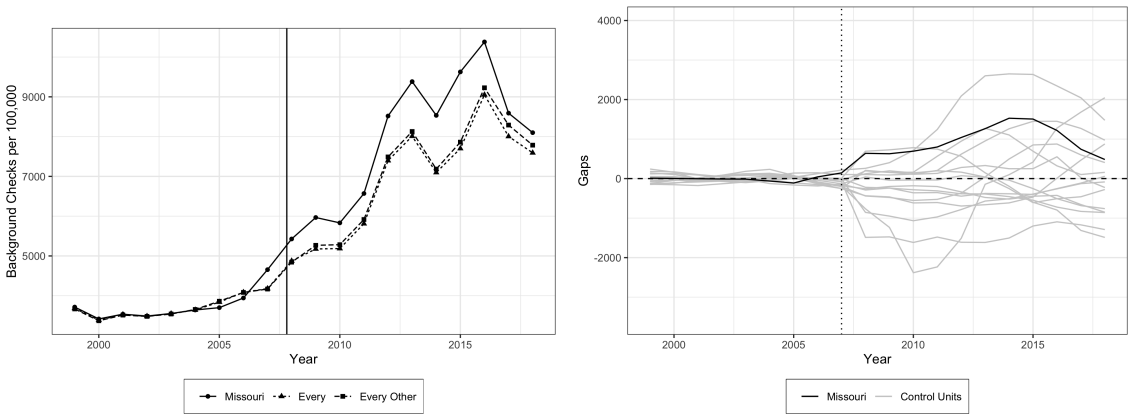
Table A.5: Spillover p-values for Each Outcome and Matching Procedure

Outcome	Control States Included	Every Year	Every Other Year
	Missouri's Spillover County State		
Firearm Homicides	<20 MSPE	0.923 (36/39)	0.949 (37/39)
	<5 MSPE	0.946 (35/37)	0.974 (37/38)
	<2 MSPE	0.941 (32/34)	0.972 (35/36)
Firearm Suicides	<20 MSPE	0.125 (2/16)	0.154 (2/13)
	<5 MSPE	0.250 (2/8)	0.333 (2/6)
	<2 MSPE	0.667 (2/3)	0.667 (2/3)
	Connecticut's Spillover County State		
Firearm Homicides	<20 MSPE	0.283 (13/46)	0.200 (9/45)
	<5 MSPE	0.295 (13/44)	0.214 (9/42)
	<2 MSPE	0.333 (13/39)	0.265 (9/34)
Firearm Suicides	<20 MSPE	0.565 (26/46)	0.543 (25/46)
	<5 MSPE	0.565 (26/46)	0.543 (25/46)
	<2 MSPE	0.591 (26/44)	0.568 (25/44)
	Maryland's Spillover County State		
Firearm Homicides	<20 MSPE	0.256 (10/39)	0.237 (9/38)
	<5 MSPE	0.333 (10/30)	0.290 (9/31)
	<2 MSPE	0.381 (8/21)	0.368 (7/19)
Firearm Suicides	<20 MSPE	0.824 (14/17)	0.769 (10/13)
	<5 MSPE	0.833 (5/6)	0.600 (3/5)
	<2 MSPE	0.600 (3/5)	0.667 (2/3)

The p-values in this table are the ratio of donor pool states with a more extreme MSPE in the post-intervention period that qualify by MSPE size in the pre-intervention period and come from the placebo procedure listed in the Method section. 'Every Year' and 'Every Other Year' refer to the matching procedure used for the synthetic control.

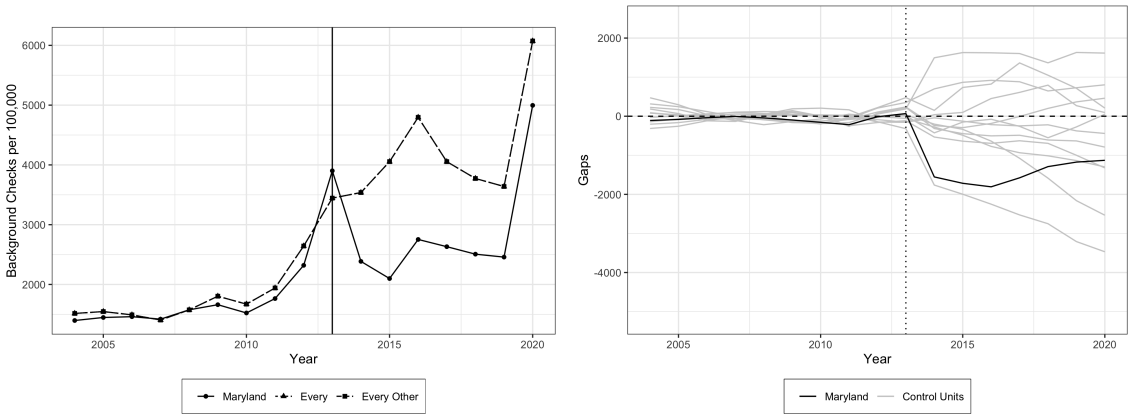
B. Figures

B.1. Background Checks, Time-to-Crime, and Self-Sourced Ratio



(a) Missouri Background Checks

(b) Missouri Background Checks: Placebo

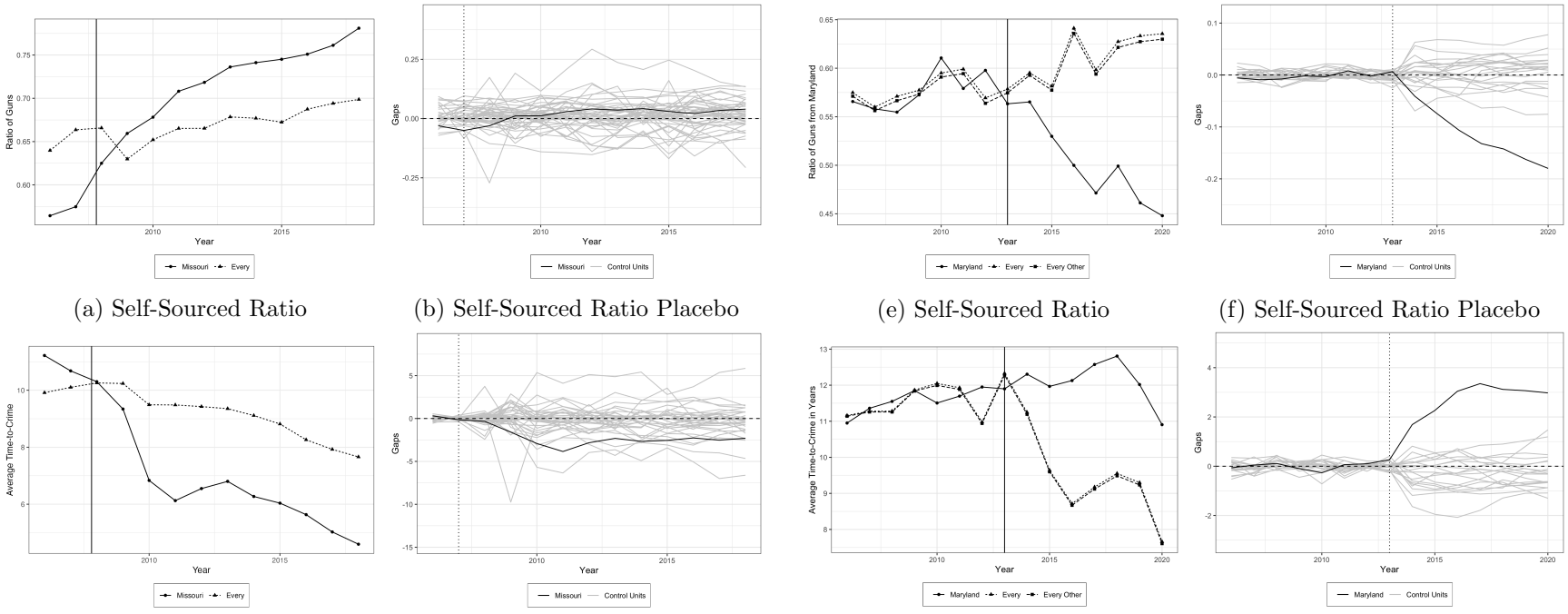


(c) Maryland Background Checks

(d) Maryland Background Checks: Placebo

Figure B.1: Background Checks for Missouri and Maryland

These figures are the visual synthetic control results for background checks for Missouri and Maryland. Background checks are from the FBI's NICS. The x-axis is the 10 years before and 10 years after the policy intervention, except in Maryland which only has 7 years after. The vertical lines represent the policy intervention date. Panels (a) and (c) are a comparison of the two synthetic control procedures to the treatment state and the y-axis is the background checks per 100,000 (outcome variable is not rolling average). Panels (b) and (d) are the graphical representations of the placebo testing that determines the p-values in table Table A.2-A.4 and the y-axis is the gap between the synthetic control and treatment state for the every year match (outcome variable is rolling average). In all graphs, the treatment state is the solid black line. The synthetic controls are the dashed lines and the placebo states are grey.



(a) Self-Sourced Ratio (b) Self-Sourced Ratio Placebo (e) Self-Sourced Ratio (f) Self-Sourced Ratio Placebo

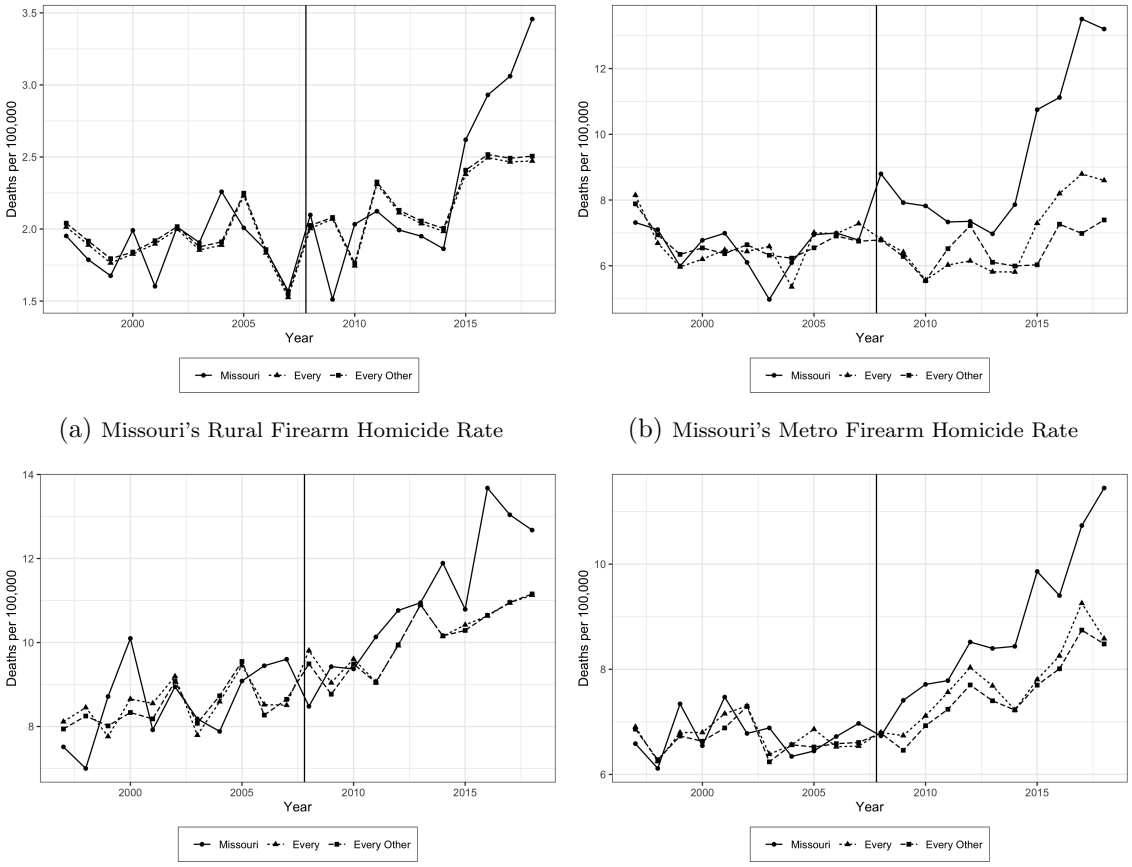
(c) Time-to-Crime (d) Time-to-Crime Placebo (g) Time-to-Crime (h) Time-to-Crime Placebo

Missouri Maryland

Figure B.2: Missouri and Maryland's Synthetic Control For Recovery Self-Sourced Ratio and Time-to- Crime

These figures are the visual synthetic control results for trace data (self-sourced ratio and time-to-crime) for Missouri and Maryland. Trace data comes from the FBI's ATF. For Missouri, the x-axis is 2 years before and 10 years after the policy change. For Maryland, The x-axis is the 7 years before and 7 years after the policy intervention. The vertical lines represent the policy intervention date. Panels (a), (c), (e), and (f) are a comparison of the synthetic control procedures to the treatment state (one for Missouri and two for Maryland) and the y-axis is the ratio of guns from the treatment state in panel (a) and (e) and average time-to-crime in years for panel (c) and (g) (outcome variables are not rolling average). Panels (b), (d), (f), and (h) are the graphical representations of the placebo testing that determines the p-values in table Table A.4 and the y-axis is the gap between the synthetic control and treatment state for the every year match (outcome variables are rolling average). In all graphs, the treatment state is the solid black line. The synthetic controls are dashed and the placebo states are grey.

B.2. Metropolitan and Rural Breakdown



(a) Missouri's Rural Firearm Homicide Rate

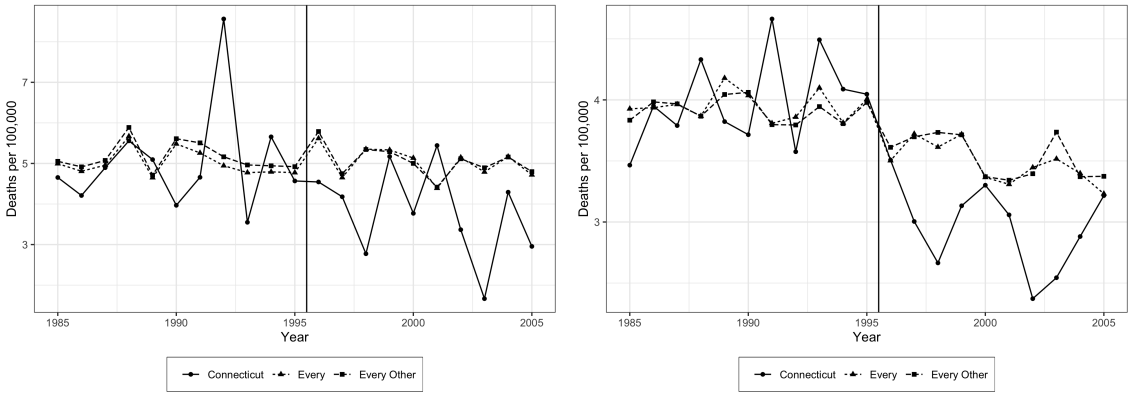
(b) Missouri's Metro Firearm Homicide Rate

(c) Missouri's Rural Firearm Suicide Rate

(d) Missouri's Metro Firearm Suicide Rate

Figure B.3: Missouri's Rural and Metropolitan Breakdown

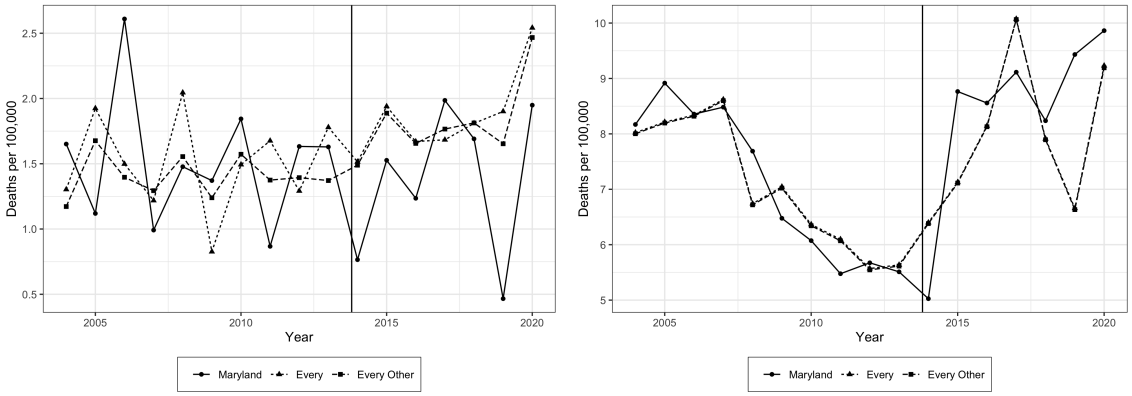
This mirrors Missouri's synthetic control results for firearm homicides and suicides above except that the rates are broken down between rural and urban areas, as determined by the NCHS.The deaths are from the Restricted-Use Vital Statistics from the CDC. The y-axis is deaths per 100,000 and the x-axis is the 10 years before and after the policy intervention. The vertical lines represent the policy intervention date. Panels (a) and (b) are homicides while (c) and (d) are suicides. In all graphs, Missouri is the solid black line, and the synthetic controls are dashed.



(a) Connecticut's Rural Firearm Suicide Rate (b) Connecticut's Metro Firearm Suicide Rate

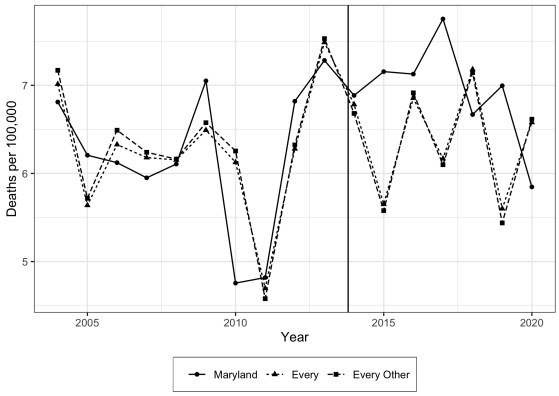
Figure B.4: Connecticut's Rural and Metropolitan Breakdown

This mirrors Connecticut's synthetic control results for firearm suicides above except that the rates are broken down between rural and urban areas, as determined by the NCHS. The deaths are from the Restricted-Use Vital Statistics from the CDC. The y-axis is deaths per 100,000 and the x-axis is the 10 years before and after the policy intervention. The vertical lines represent the policy intervention date. In both graphs, Connecticut is the solid black line, and the synthetic controls are dashed.

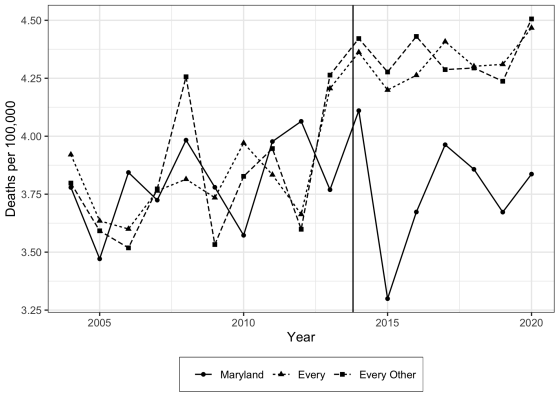


(a) Maryland's Rural Firearm Homicide Rate

(b) Maryland's Metro Firearm Homicide Rate



(c) Maryland's Rural Firearm Suicide Rate

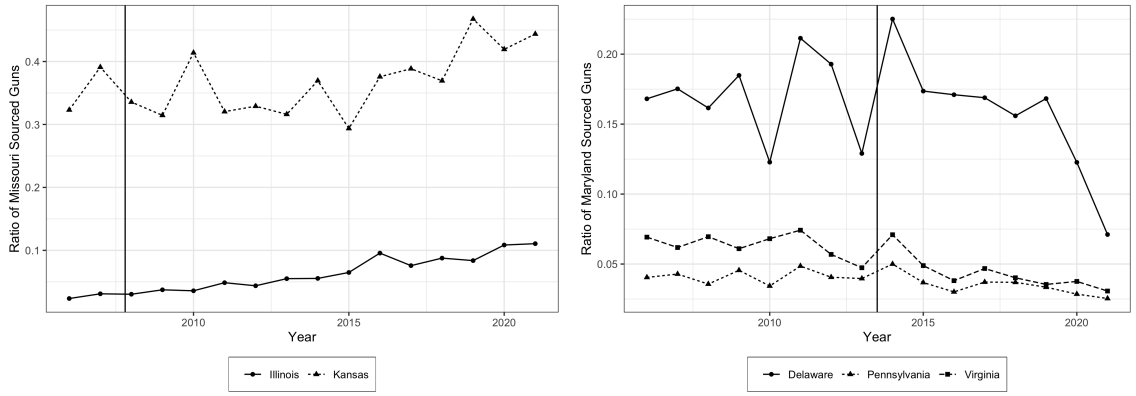


(d) Maryland's Metro Suicide Rate

Figure B.5: Rural and Metropolitan Firearm Homicide Rates

This mirrors Maryland's synthetic control results for firearm homicides and suicides above except that the rates are broken down between rural and urban areas, as determined by the NCHS.The deaths are from the Restricted-Use Vital Statistics from the CDC. The y-axis is deaths per 100,000 and the x-axis is the 10 years before and 7 years after the policy intervention. The vertical lines represent the policy intervention date. Panels (a) and (b) are homicides while (c) and (d) are suicides. In all graphs, Missouri is the solid black line, and the synthetic controls are dashed.

B.3. Spillover Analysis

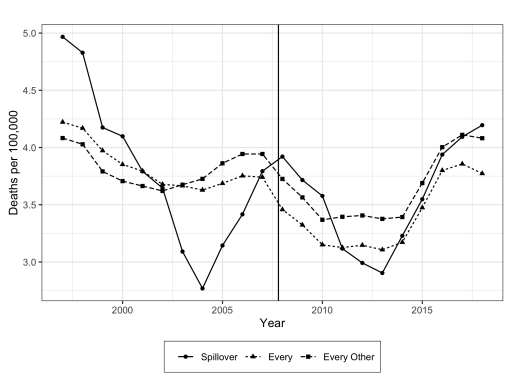


(a) Missouri Sourcing Neighbors Guns: Ratio of All Outside Guns

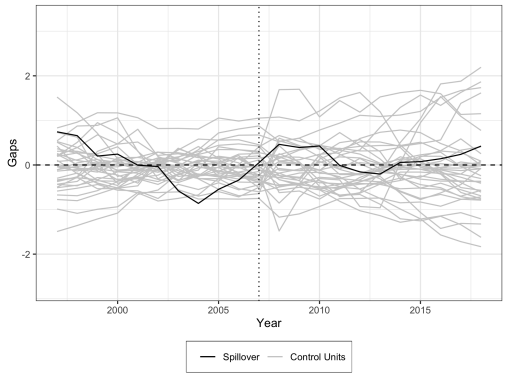
(b) Maryland Sourcing Neighbors Guns: Ratio of All Outside Guns

Figure B.6: Spillover of Purchasing Behavior

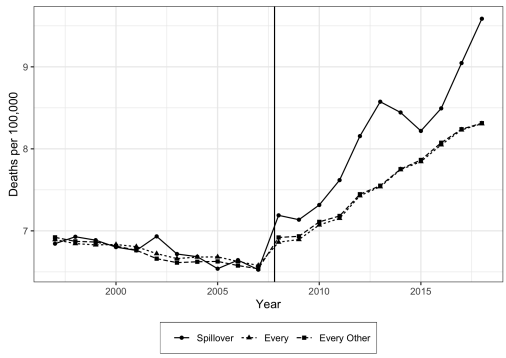
The ratio of guns purchased in Missouri or Maryland later recovered by a neighboring state (that shares a major metropolitan border) relative to all other out-of-state recovered guns in those states. This is effectively the treatment-sourced ratio as opposed to the self-sourced ratio presented earlier. The y-axis is the ratio of guns recovered from the treatment state over all guns recovered in those border states. The x-axis is time and contains two years before treatment for Missouri and 7 years before and after for Maryland. The vertical lines are the respective treatment dates in the state of interest.



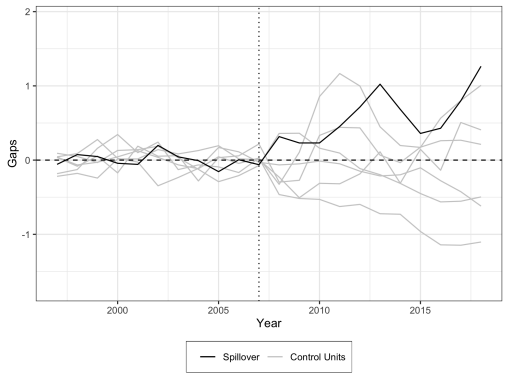
(a) All Border Counties: Homicide



(b) All Border Counties: Homicide Placebo



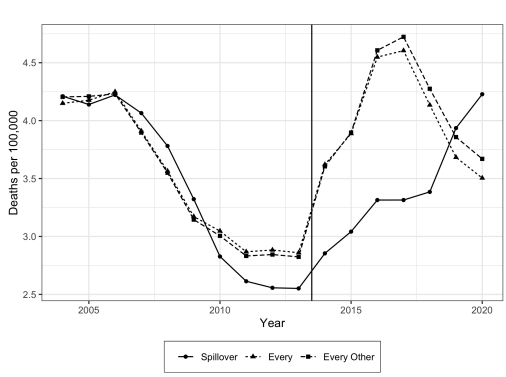
(c) All Border Counties: Suicide



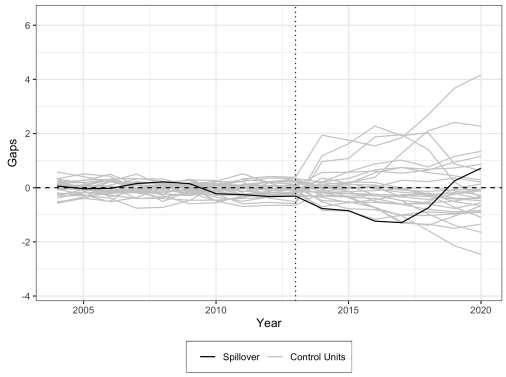
(d) All Border Counties: Suicide Placebo

Figure B.7: Missouri Spillovers for All Border Counties

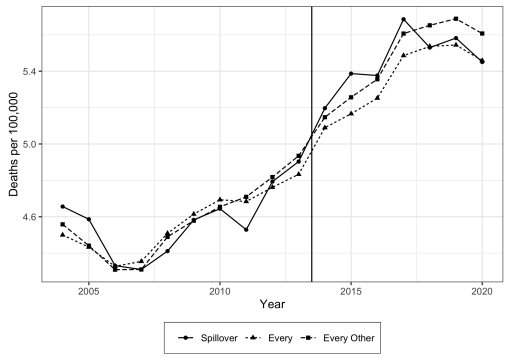
These figures are the visual synthetic control results for the border-county-state for Missouri, which is the combination of counties that border Missouri into their own 'state'. Firearm homicides and suicides are from the Restricted-Use Vital Statistics from the CDC. The x-axis is the 10 years before and after the policy intervention. The vertical lines are the date of the policy intervention in Missouri. The y-axis is the deaths per 100,000 in panels Panels (a) and (c) (outcome variable is not rolling average). Panels (a) and (c) are a comparison of the two synthetic control procedures to the treatment border-county state. Panel (b) and (d) are the graphical representations of the placebo testing that determines the p-values in table Table A.5 and the y-axis is the gap between the synthetic control and treatment border-county state for the every year match (outcome variable is rolling average). In all graphs, the treatment border-county state is the solid black line. The synthetic controls are dashed and the placebo states are grey.



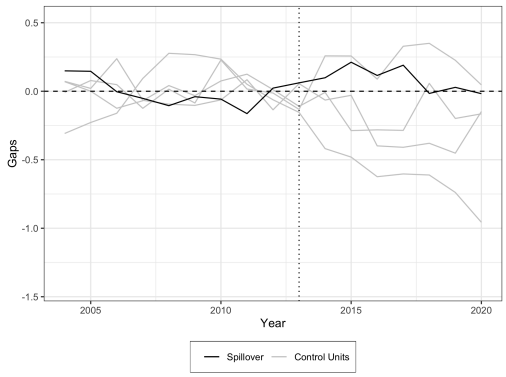
(a) All Border Counties: Homicide



(b) All Border Counties: Homicide Placebo



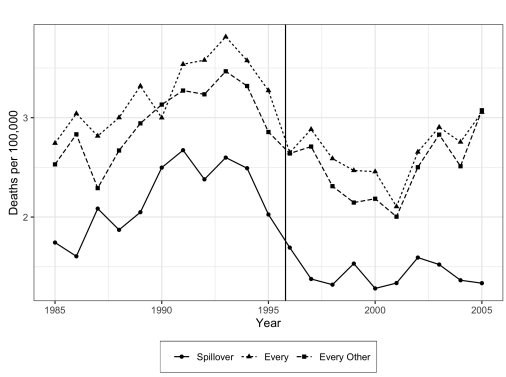
(c) All Border Counties: Suicide



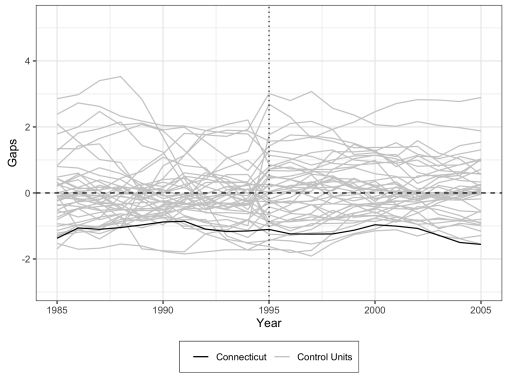
(d) All Border Counties: Suicide Placebo

Figure B.8: Maryland Spillovers for All Border Counties

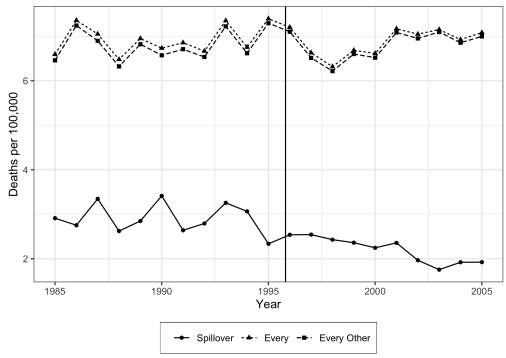
These figures are the visual synthetic control results for the border-county-state for Maryland, which is the combination of counties that border Maryland into their own 'state'. Firearm homicides and suicides are from the Restricted-Use Vital Statistics from the CDC. The x-axis is the 10 years before and after the policy intervention. The vertical lines are the date of the policy intervention in Maryland. The y-axis is the deaths per 100,000 in panels Panels (a) and (c) (outcome variable is not rolling average). Panels (a) and (c) are a comparison of the two synthetic control procedures to the treatment border-county state. Panel (b) and (d) are the graphical representations of the placebo testing that determines the p-values in table Table A.5 and the y-axis is the gap between the synthetic control and treatment border-county state for the every year match (outcome variable is rolling average). In all graphs, the treatment border-county state is the solid black line. The synthetic controls are dashed and the placebo states are grey.



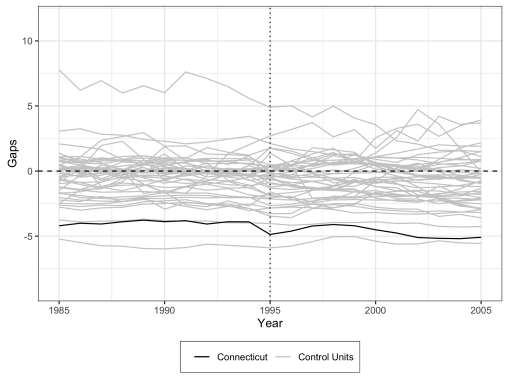
(a) All Border Counties: Homicide



(b) All Border Counties: Homicide Placebo



(c) All Border Counties: Suicide

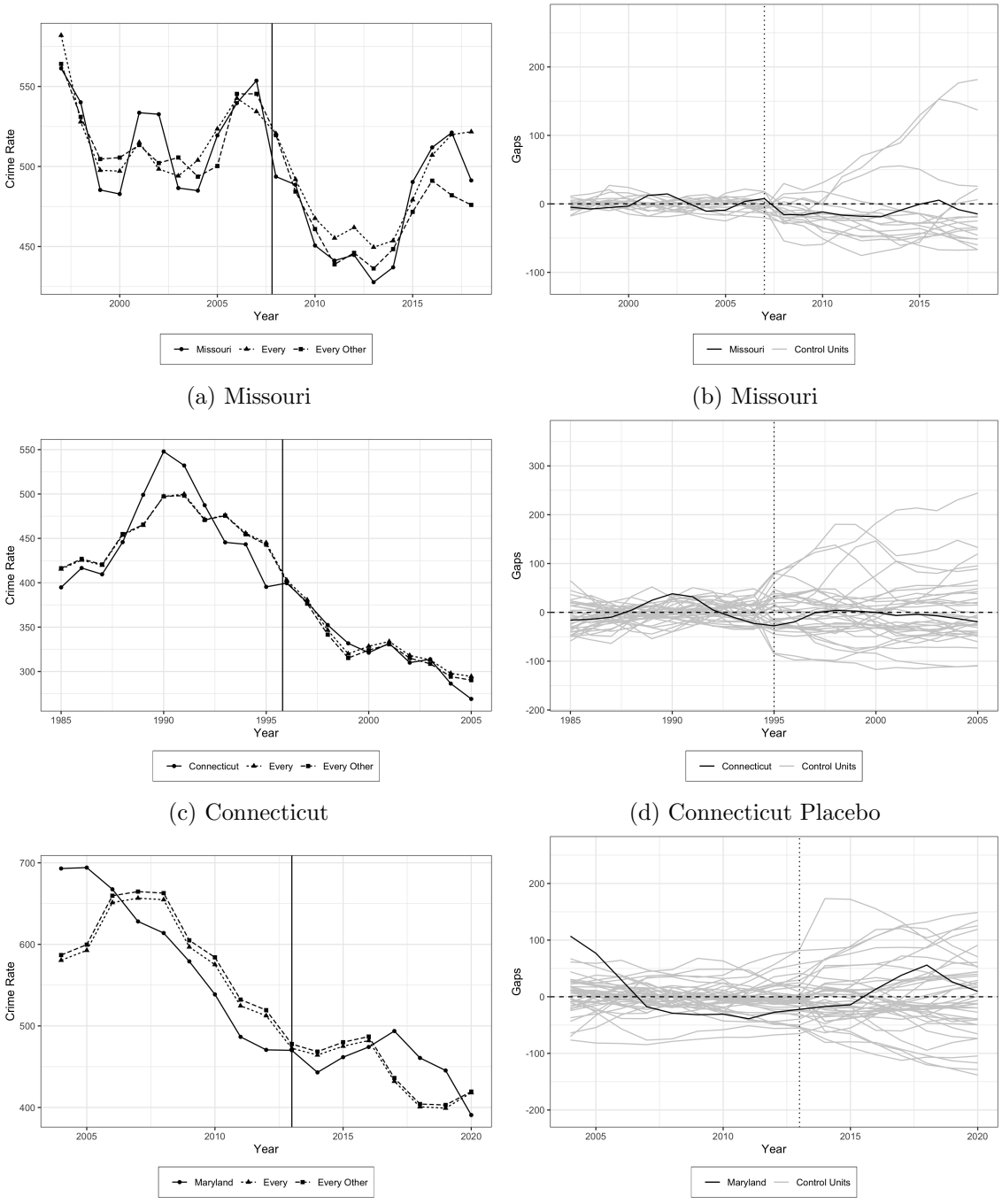


(d) All Border Counties: Suicide Placebo

Figure B.9: Connecticut Spillovers for All Border Counties

These figures are the visual synthetic control results for the border-county-state for Connecticut, which is the combination of counties that border Connecticut into their own 'state'. Firearm homicides and suicides are from the Restricted-Use Vital Statistics from the CDC. The x-axis is the 10 years before and after the policy intervention. The vertical lines are the date of the policy intervention in Connecticut. The y-axis is the deaths per 100,000 in panels Panels (a) and (c) (outcome variable is not rolling average). Panels (a) and (c) are a comparison of the two synthetic control procedures to the treatment border-county state. Panel (b) and (d) are the graphical representations of the placebo testing that determines the p-values in table Table A.5 and the y-axis is the gap between the synthetic control and treatment border-county state for the every year match (outcome variable is not rolling average). In all graphs, the treatment border-county state is the solid black line. The synthetic controls are dashed and the placebo states are grey.

B.4. Non-homicide Violent and Property Crime Rates



(a) Missouri

(b) Missouri

(c) Connecticut

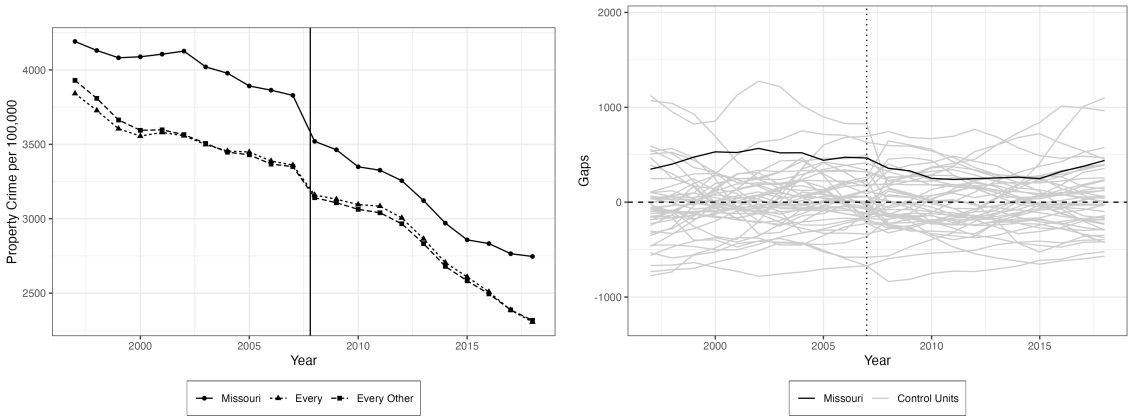
(d) Connecticut Placebo

(e) Maryland

(f) Maryland Placebo

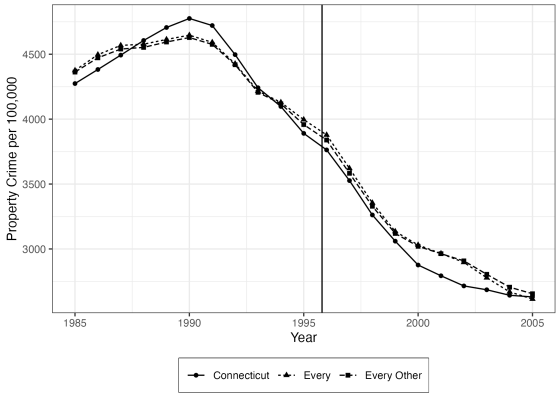
Figure B.10: Violent Non-homicide Crime Synthetic Control and Placebos

The synthetic control results for firearm homicides for each state using non-homicide violent crime rates from the FBI's UCR. The x-axis is the 10 years before and 10 years after the policy intervention, except in Maryland which only has 7 years after. The vertical lines represent the policy intervention date. Panels (a), (c), and (e) are a comparison of the two synthetic control procedures to the treatment state and the y-axis is the crime rate or crimes per 100,000 people (outcome variable is not rolling average). Panels (b), (d), and (f) are the graphical representations of the placebo testing that determines the p-values in table Table A.2-A.4 and the y-axis is the gap between the synthetic control and treatment state for the every year match (outcome variable is rolling average). In all graphs, the treatment state is the solid black line. The synthetic controls are dashed and the placebo states are grey.

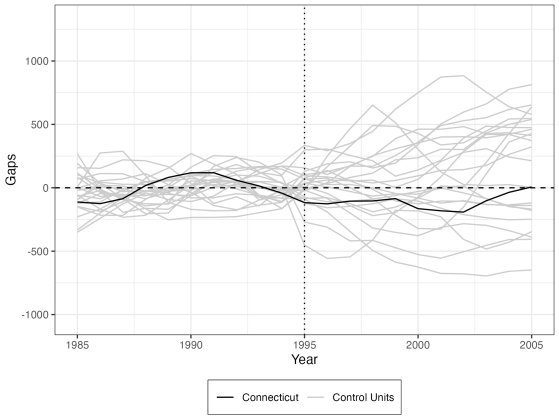


(a) Missouri

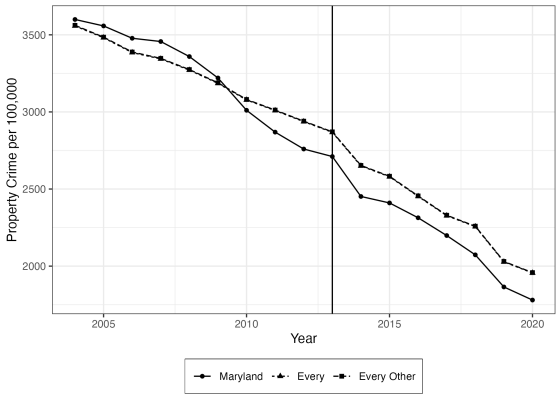
(b) Missouri Placebo



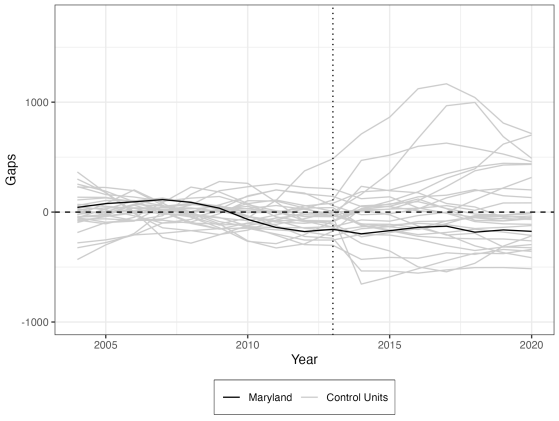
(c) Connecticut



(d) Connecticut Placebo



(e) Maryland



(f) Maryland Placebo

Figure B.11: Property Crime Rate Synthetic Control and Placebos

The synthetic control results for property crime rates for each state using the FBI's UCR. The x-axis is the 10 years before and 10 years after the policy intervention, except in Maryland which only has 7 years after. The vertical lines represent the policy intervention date. Panels (a), (c), and (e) are a comparison of the two synthetic control procedures to the treatment state and the y-axis is crimes per 100,000 people (outcome variable is not rolling average). Panels (b), (d), and (f) are the graphical representations of the placebo testing and the y-axis is the gap between the synthetic control and treatment state for the every year match (outcome variable is rolling average). In all graphs, the treatment state is the solid black line. The synthetic controls are dashed and the placebo states are grey.

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