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Essays on Public Policy

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Abstract

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The first chapter examines the impact of Red Flag Laws on homicide rates and suicide rates. Red Flag Laws seek to implement gun control measures by allowing the removal of firearms from individuals who pose a danger to themselves or others. Using a two-way fixed effects (TWFE) difference-in-differences (DiD) estimations, I demonstrate a negative and plausibly causal relationship between a state implementing a Red Flag Law and homicide rates. While there is also a reduction in suicide rates, I am unable to make causal claims. This study is the first to empirically examine Red Flag Laws, with an eye towards casual inference. These effects are primarily driven by states that permit both family members and law enforcement to petition a state court for the removal of firearms.

The second chapter adds to the existing literature on capital punishment. However, I focus on state-level execution moratoriums and their impact on homicide rates. Currently, there are five states with such moratoriums: Oregon (since 2011), Colorado (since 2013; the death penalty was abolished in 2020), Washington (since 2014; the death penalty was abolished in 2018), Pennsylvania (since 2015), and California (since 2019). Using the synthetic control method (SCM) I fail to find a statistical significance on homicide rates for the states who adopt such a policy. Robustness checks and supplementary analyses were conducted to ensure the validity of the primary SCM results. Furthermore, additional analyses incorporating control variables, such as population, race, income, and unemployment rate, were performed using a two-way fixed effects (TWFE) difference-in-differences (DiD) model. The findings consistently show no statistically significant effect of the policy on homicide rates.

The third chapter is joint work with Dr. Bryan McCannon. In it, we exploit a novel Covid-19 policy where the state of California mandated a state-wide zero dollar bail for all misdemeanor and non-violent felony offenses. This policy was in affect for three months, from April 2020 through June 2020. After the state-wide mandate was lifted, individual counties were able to continue the zero dollar bail policy or revert back to pre-Covid bail schedules. We investigate whether the elimination of cash bail promotes crime. Our empirical evidence suggests that this policy had a statistical significant impact on violent crimes, specifically assaults. Property crime results tend to unreliable, depending on the specification used. We further show that it did not affect law enforcement's clearance rate and, therefore, is likely to be a direct effect from eroding deterrence. Using a leave-one-out process, which allows us to assess how sensitive our result is to crime and policy in any one particular jurisdiction. Doing so, we find little difference in the coefficient estimate. Each of the 58 regressions produces an estimated effect that positive and statistically different from zero at the 5% level.

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1 Are Red Flag Laws a green light to save lives?

1.1 Introduction

The link between mental health and mass shootings has attracted growing public attention in recent years. Policymakers have responded by enacting extreme risk protective orders (ERPO), commonly known as Red Flag Laws. The first Red Flag Law was passed by Connecticut as a direct response to a mass shooting that occurred on March 6, 1998 at the Connecticut Lottery headquarters. Between 1999 and 2021, across all states who have passed and implemented Red Flag Laws, at least 18,383 petitions were filed (Research and Policy, 2023).

A Red Flag Law is a form of gun control that allows law enforcement officers or family members to petition a civil court to temporarily remove firearms from an individual who they believe is a danger to themselves or others. The logic behind Red Flag Laws is that many shooters display warning signs before a shooting or tragic event takes place. Extreme risk protection orders bypass the criminal court system, giving family members or law enforcement officers a way to intervene before things escalate even further. If the civil court comes to the conclusion that an individual does indeed pose a serious threat to others or themselves, then that individual is temporarily barred from not only possessing a firearm but is also prevented from purchasing new firearms while the order is in place. It is important to note that the burden to prove that firearm removal is necessary is on the petitioner, meaning they must provide sufficient evidence that the individual in question does pose a danger to themselves or others. The individual in question does have the opportunity to refute the evidence presented as well as present their own evidence [(Research and Policy, 2023). The duration of the order varies among states; some can last up to 180 days, while others up to one year. Indiana, on the other hand, which implemented their Red Flag Law in 2005, has a duration that extends until it is terminated by the court.[1]

[1] The appendix contains more details on who can initiate an extreme risk order and how long the

According to a study conducted by the Federal Bureau of Investigation (FBI) that examined pre-attack behaviors of active shooters in the United States between 2000 and 2013, it was found that, on average, each active shooter exhibited four to five observable concerning behaviors over time Silver et al. (2018). These behaviors were noticeable to individuals in the shooter's vicinity. The study identified mental health issues, problematic interpersonal interactions, and leakage of violent intent as some of the most frequent concerning behaviors.

The study also investigated who noticed these concerning behaviors before the attacks took place. The results indicated that 87% of spouses or domestic partners, 68% of family members, and 25% of law enforcement personnel reported observing such behaviors prior to the attacks. To date, there has been no formal investigation conducted to assess the impact of Red Flag Laws on mitigating these concerning behaviors and preventing acts of violence.

To address this question, I use a panel dataset and employ a two-way fixed effects (TWFE) difference-in-differences model looking at the staggered adoption of Red Flag Laws across the United States. I use homicide rate data from the FBI's Uniform Crime Reporting (UCR) and suicide rate data from the Center of Disease Control and Prevention (CDC) as dependent variables.

I find that for states that have enacted a Red Flag Law, the implementation corresponds to a 6.26% reduction in suicide rates and a 10.96% reduction in homicide rates. Furthermore, I find that states that allow both family members and law enforcement to petition a state court for a ERPO tend to be the driving force in the reduction of suicide rates as well as homicide rates when compared to states that only allow law enforcement to petition for a ERPO.

orders can last.

1.2 Literature Review

While Red Flag Laws have not been empirically explored in the economics literature, there are other fields that have explicitly examined them. For the most part, Red Flag Laws have been studied within the context of legal scholarship that debate their constitutionality and whether it infringes on the second amendment (Johnson, 2021), (Gay, 2020). Another field that has explored Red Flag Laws is psychiatry.

Within the psychiatry field, the studies on Red Flag Laws focus on case studies to extrapolate the effects of the policy, they do not use any formal econometric models. They are not able to assess the external validity of the intervention policymaking across the country. For example, Swanson et al. (2019) evaluate Indiana's Red Flaw Law by examining 395 gun-removals in Marion County, Indiana, which includes Indianapolis. They extrapolate that one life was saved for every ten gun-removals. Swanson et al. (2017) investigates 762 gun-removal cases in Connecticut between October 1999, and June 2013. They found a reduction in firearm suicide rates among individuals subjected to firearm seizures. Kivisto and Phalen (2018) evaluate whether the Red Flag Laws in Connecticut and Indiana affect suicide rates. Overall, they find that risk-based firearm seizure laws corresponded with a reduction in population-level firearm suicide rates for both states examined. This present study is the first comprehensive empirical study on the effectiveness of Red Flag Laws on homicide and suicide rates.[2] To date, no one has empirically, with an eye towards causal inference, studied whether Red Flag Laws have had the intended impact on reducing violence.

Other gun laws have received attention and have been studied using various empirical approaches. There are three main themes within the gun law economics literature: Right-to-Carry (RTC) laws, mandatory waiting periods between the purchase of a firearm and its delivery to the final consumer, and Permit-to-Purchase (PTP) laws.

The seminal paper on RTC laws starts with Lott and Mustard (1997) where they find that

[2] Dalafave (2020) uses a difference-in-differences approach to evaluate Red Flag Laws in 5 states (Connecticut, Indiana, California, Washington, and Oregon). She finds a statistically significant reduction in suicide rates, but not in homicide rates.

RTC laws reduced crimes rates in the United States, without an increase in accidental deaths. More recent, Moody and Lott (2022) investigated whether RTC laws still reduce crime. They conclude that states with a RTC law have a much lower murder rate than those states without a RTC law, while not increasing other crime such as violent or property crime. Another recent RTC law paper examines the impact of when a RTC law was banned in Brazil (Schneider, 2021). Schneider finds that after the RTC law was banned, Brazil experienced a reduction in gun-related homicides by 12.2% as well as a reduction in gunshot wounds that were 'intended to kill' by 16.3% in the year after the ban was implemented. Others have contended the deterrence effect of concealed weapons (Aneja et al., 2014). They find that RTC laws increase aggravated assault, rape, robbery, and murder.

Mandated delays between the purchase and delivery of a handgun, also referred to as a waiting period, have also been explored to measure if they have had any impact on outcomes such as suicides, homicides, along with other crimes. Edwards et al. (2018) examines how waiting period laws have an impact on firearm-related homicides and suicides. They find a reduction of 3% in firearm-related suicides, with no evidence of a substitution effect towards non-firearm related suicides. They also conclude that waiting periods do not appear to have any impact on homicide rates. Koenig and Schindler (2021) examines a six-month period post the 2012 Presidential election and Sandy Hook shooting to see if handgun purchase delays had any impact on homicide rates. They found that states with a handgun purchase delay experienced a 2% lower homicide rate during that six-month period compared to states without such a law. Luca et al. (2017) explores the impact of handgun waiting periods on gun deaths, specifically homicides and suicides. They find that waiting periods significantly reduce homicides by 17% and suicides by 7-11%.

Permit-to-purchase laws have also been studied within economics to measure the impact on outcomes such as homicide rates. Rudolph et al. (2015) examine Connecticut's 1995 PTP law and find that it reduced homicide rates. More specifically, they find a 40% reduction in firearm homicide rates during the first decade post-implementation. Looking at it from the opposite direction, Webster et al. (2014) examines Missouri's repeal of their PTP law in 2007. They find that Missouri's 2007 PTP law repeal was associated with an annual increase in homicide rates of 23%, when they use UCR data they find that murder rates increased 16%.

Other papers within the economics gun law literature include gun law changes in a single state or public access to a handgun carry permit database. A recent paper by Kahane and Sannicandro (2019) examine gun law changes in Massachusetts using a synthetic control approach. In 1998, Massachusetts enacted 23 gun laws, Kahane and Sannicandro find a statistically significant reduction in suicide rates but the effects abate by 2005. Acquisti and Tucker (2022) examine crime and handgun carry permit data for the city of Memphis to estimate the effect of publicly available handgun carry permit database on burglaries. Unsurprisingly, they find that burglaries increased in zip codes with fewer gun permits and decreased in zip codes with more gun permits, after the database became publicly available.

My contribution is to empirically test the effectiveness of Red Flag Laws by measuring if there has been a reduction in suicide rates as well as homicide rates in states that have implemented a Red Flag Law.

1.3 Data

1.3.1 Data Source

I construct a state by year panel with data collected from a variety of sources. These sources include the FBI's UCR database, CDC, Bureau of Economic Analysis (BEA), and Bureau of Labor Statistics (BLS). I collected homicide rates from the FBI's UCR database at the state level. Data are available from 1990 to 2020.[3] I collected suicide rate data from the CDC. That data is available from 1990 to 2020. Both variables are measured per 100,000 people.

[3] The FBI does not have homicide rate data on Mississippi from 1990-94.

For falsification purposes I collected data on total property crime at the state level from the FBI's UCR database. I was able to gather on property crime from 1990 to 2020. It is also measured per 100,000 people.

Other variables employed include population data, male and female data, as well as race/ethnicity data provided by the CDC for the years 1990 to 2020. The income data was extracted from the BEA for the years 1990-2020, more specifically the median annual income. Seasonally adjusted annual state-level unemployment rate data was collected from the BLS also for the years 1990 to 2020. I simply took the first month of each year for each state and used that unemployment rate for the entire year. For example, Alabama's monthly unemployment rate in January 1990, was 6.7% so I used that for Alabama's 1990, unemployment rate in my data-set.

1.3.2 Policy Implementation

Below is a table of the states that currently have a Red Flag Law implemented. The vast majority of states that have adopted a Red Flag Law have done so within the past few years. Despite the District of Columbia adopting a Red Flag Law in 2019, it is excluded from the analysis due to its unique status as a federal district, rather than a state.

States	Year Implemented
Connecticut	1999
Indiana	2005
California	2016
Washington	2016
Oregon	2018
Florida	2018
Vermont	2018
Maryland	2018
Rhode Island	2018
Delaware	2018
Massachusetts	2018
New Jersey	2019
Illinois	2019
New York	2019
Colorado	2020
Nevada	2020
Hawaii	2020
New Mexico	2020
Virginia	2020

There are potentially several reasons for the recent increase in Red Flag Laws. One significant factor is the rise in mass shootings that have occurred in recent years.

Early implementations of Red Flag Laws, such as those in Connecticut in 1999 and Indiana in 2005, were responses to mass shootings or other forms of gun violence committed by individuals with mental health issues. Although the states themselves voluntarily adopted these policies, the events leading to their implementation were random, meaning they were unrelated to the states' levels of homicide or suicide rates. Late adopters of Red Flag Laws often acted in response to mass shootings that occurred in neighboring states, which can be considered as random events.

For example, Connecticut cited a mass shooting at the Connecticut Lottery Corporation headquarters, where an employee killed four bosses before taking his own life, as a reason for implementing their Red Flag Law (Foley and Thompson, 2018). Indiana named their Red Flag Law after a police officer who was killed by a mentally ill man, who had also killed his own mother, after stopping his prescribed medication for schizophrenia (Indiana Law Enforcement Memorial 2022). California referenced a tragic event near the University of California, Santa Barbara, where a mentally ill man killed six people and injured thirteen others before committing suicide (Foley and Thompson, 2018). The University of California, Santa Barbara incident was also mentioned as a reason for implementing the Red Flag Law in the state of Washington since one of the victims was a Washington native.

In recent years, mass shootings have either increased or gained more coverage in the news cycle. Notable examples include the Orlando nightclub shooting in 2016, the Las Vegas mass shooting in 2017, the Southern Baptist Church shooting in Sutherland Springs, TX in 2017, the Parkland, FL high school mass shooting in 2018, the Santa Fe, TX high school mass shooting in 2018, and the El Paso, TX Wal-Mart shooting in 2019.

A turning point in the number of states with Red Flag Laws occurred after the Parkland, FL high school mass shooting in 2018, as the number of states passing such laws more than doubled following that tragic event (Wing and Jeltsen, 2018, Livingston, 2018). For example, Delaware, which had previously failed to pass a Red Flag Law in 2013, unanimously passed the law after the Parkland, FL shooting (Livingston, 2018). Other states, like New York and Colorado, also cited specific incidents, such as the Dayton, OH shooting and the death of Deputy Zackari Parrish, respectively, as reasons for implementing their Red Flag Laws. Nevada referred to the Las Vegas strip mass shooting, Hawaii mentioned a 1999 shooting at Xerox Corp, and New Mexico cited the El Paso, TX Wal-Mart mass shooting as contributing factors for their respective Red Flag Laws (Arnold, 2019, Phillips, 2018, Apgar, 2019, Dayton, 2019, Boyd, 2020). Virginia, on the other hand, referenced the Virginia Beach mass shooting as a contributing factor for their Red Flag Law (Duster, 2019).

Most of the states where these shootings occurred went on to pass and implement Red Flag Laws in an effort to prevent future incidents or at least reduce the likelihood of their occurrence. While Massachusetts did not specify a particular tragic event that led to the passing and implementation of their Red Flag Law, they cited the national increase in mass shootings as a contributing factor (Miller, 2018). Other states, neighboring those where mass shootings took place, took a proactive approach in hopes of preventing similar incidents within their borders. State Red Flag Laws are therefore a response to high-profile events, which are essentially random. Importantly, they are not driven by the states' underlying aggregate levels of homicides or suicides, and they do not reflect time trends in those levels.

Table 1 below provides the summary statistics for this project. In addition to the variables mentioned above I for simplicity I created a population ratio of males to females labeled "Male Ratio" and a race ratio variable of whites to blacks labeled "White Ratio". The panel data-set covers all fifty states over a thirty-one year period from 1990 to 2020.

Table 1.1: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Homicide Rate	1,545	5.24	2.97	0.2	20.3
Suicide Rate	1,550	13.84	4.01	1.5	31.4
Property Crime	1,550	3,364.21	1,139.05	1,053.2	7,566.5
Population	1,550	5,861,491	6,497,797	453,690	39,512,223
Male Ratio	1,550	0.492	0.008	0.479	0.527
White Ratio	1,550	0.886	0.098	0.608	0.997
Median Household Income	1,550	35,331.58	12,467.3	13,356	78,609
Unemployment Rate	1,550	5.4	1.87	2	13.7

The male variable statistic indicates the ratio among males and females. The white variable statistic indicates a race ratio of whites to blacks. 2020 suicide rate data was not available at the time of this writing. The homicide and suicide rates are calculated by dividing the number of murders (suicides) by the total population then multiplying the result by 100,000 to give the figure as the number of murders (suicides) per 100,000 people. The FBI did not have homicide rate data on Mississippi from 1990-94.

1.4 Identification Strategy

I investigate empirically the impact that Red Flag Laws have had on homicide rates and suicide rates in the states that have a Red Flag Law on the books. I formalize this relationship with the following regression model:

$$Y_{st} = \alpha RedFlagLaw_{st} + \beta X_{st} + \sigma_s + \tau_t + \epsilon_{st}. \tag{1.1}$$

The variable Y_{st} represents an outcome for state s and year t. I will use both homicide rate and suicide rate as dependent variables. The model includes state fixed effects, notated by σ, year fixed effects, τ, and an error term, ϵ. I also include time-varying state level controls, which is notated by X. The coefficient of primary interest is α which is the difference-in-differences (DiD) estimate of the effect of Red Flag Laws on homicide rate or suicide rate in states that have passed a Red Flag Law. Difference-in-differences attempts to identify a causal effect by comparing the changes in outcomes over time between a group that has received the treatment/adopted a policy to a group that did not receive the treatment/adopt the policy.

1.5 Results & Discussion

1.5.1 Results

Table 2 below presents the main results for this paper, demonstrating the impact of Red Flag Laws on both homicide rates and suicide rates. The table includes models with and without control variables. The variable of interest in Table 2 is the effect of Red Flag Laws on suicide rates and homicide rates from 1990 to 2020, denoted by the Red Flag Law variable. Models (A) and (C) incorporates the full set of control variables. Models (B) and (D) exclude all control variables.

Table 1.2: TWFE DiD Results

Dependent Variable:	Suicide Rate		Homicide Rate	
Years:	1990-2020	1990-2020	1990-2020	1990-2020
Model:	(A)	(B)	(C)	(D)
Variables				
Red Flag Law	-0.8699**	-1.381***	-0.5744***	-0.7086**
	(0.3515)	(0.4328)	(0.1870)	(0.2680)
Median Household Income	0.2489		0.5259	
	(1.169)		(1.1163)	
White Ratio	57.47***		-3.680	
	(13.46)		(13.56)	
Male Ratio	84.41		64.21*	
	(83.12)		(37.90)	
Population	−0.08121***		−0.08915***	
	(0.01927)		(0.01315)	
Unemployment Rate	0.0190		-0.0395	
	(0.0556)		(0.0553)	
Fixed-effects				
States	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,550	1,550	1,545	1,545
R^2	0.91055	0.88976	0.88228	0.85930

These are the DiD regression coefficients from equation 1 when the dependent variable is suicide rates for columns (A) and (B) and when the dependent variable is homicide rates for columns (C) and (D). Suicide rate and homicide rate data is at the state level and runs from 1990-2020. All variable data is at the state level on an annual basis. All models include both state and year fixed effects. The coefficients and standard errors for Median Household Income and Population have been re-scaled to be in the thousands. Standard errors are clustered at the state level in parentheses.
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 2 shows that there are varying levels of statistical significance for states that have implemented a Red Flag Law compared to states without such a law. The difference-in-differences estimator provides evidence that the treatment (implementing a Red Flag Law) did indeed correspond with a movement in the expected direction. There is a reduction in the number of suicides in the treated group that have a Red Flag Law when compared to states in the control group. For model (A) this is just over a 6% decrease in suicide rate.[4]

[4] The 6% comes from taking the coefficient of 0.8669 from the Red Flag Law variable in Model (A) to

For model (B), where no control variables are included I find a reduction of 10% in the suicide rate for states that have implemented a Red Flag Law.

The standard errors are clustered at the state level. The reason for clustering at the state level is to account for within state heterogeneity, which can potentially make the standard errors too small.[5] Since it is a state-level policy this is the appropriate clustering level (Abadie et al., 2023).

Models (C) and (D) in Table 2 provides the results of the impact of implementing a Red Flag Law on homicide rates. The variable of interest in Table 2 is the impact Red Flag Laws had on homicide rates from 1990-2020, notated by the Red Flag Law variable. Model (C) includes control variables such as median income, race ratio, gender ratio, Population, as well as Unemployment Rate. Model (D) drops all control variables.

Implementation of a Red Flag Law corresponds to a decrease of homicide rates ranging from just under 11% to just over 13.5%. In both homicide rate specifications, standard errors are clustered at the state level. There is a reduction in the homicide rate across both models, similar to what I find on suicide rates. Model (C) shows the results including all the control variables. The result of interest is the negative coefficient on the Red Flag Law, which translates to a reduction in homicide rates by 10.96% in states that have adopted a Red Flag Law.[6] Model (D) drops all control variables and I find an even greater reduction in homicide rates, which translates to a reduction in homicide rates by 13.52%. It appears that Red Flag Laws are working as policymakers intended: deaths from firearms via a reduction in homicides and suicides.

1.5.2 Parallel Trends

The primary identifying assumption of the difference-in-differences method is that of parallel trends in homicide rates and suicide rates. Causal identification requires that

the suicide rate mean of 13.84, $(\frac{0.8669}{13.84}) = 0.06263$.

[5]Each model presented in this paper has the standard errors clustered at the state-level to account that the errors might be related within each state over time, which could lead to smaller standard errors.

[6]The 10.96% comes from taking the coefficient of 0.5744 from the Red Flag Law variable in Model (C) to the homicide rate mean of 5.24, $(\frac{0.5744}{5.24}) = 0.1096$.

treated states follow time trends in homicides and suicides that run parallel to the time trends of the untreated states in pre-treatment periods. While I obviously cannot observe these rates for the treated states had they not been treated, I can assess the common trends prior to treatment. To test for differential trends, I conduct an event study and check for pre-existing trends in homicide rate and suicide rate. The t=-5 corresponds to Connecticut (who implemented their Red Flag Law in 1999) having implemented it in 1994 instead. For Indiana, the t=-5 corresponds to the year 2000. For California and Washington it would be 2011, and so on and so forth. It works in the same idea when the after treatment period begins. For the t=+2, states like Florida and Vermont (who both implemented their Red Flag Law in 2018) would correspond to 2020. Whereas for Connecticut, the t=+2 corresponds to 2001. The results are presented below, Figure 1 shows the event study for homicide rate and Figure 2 shows the event study for suicide rate.

These estimates ask whether homicide rate patterns were changing in the time period leading up to or after a Red Flag Law adoption. It's important to note, that with the staggered adoption, the post-treatment period is limited in the number of observations since most states have passed a Red Flag Law in recent years. For example, there are five states (Colorado, Nevada, Hawaii, New Mexico, and Virginia) that implemented a Red Flag Law in 2020, meaning there are no after treatment observations in the data. Something similar can be said for the states that implemented a Red Flag Law in 2019 (New Jersey, Illinois, and New York) there is only one observation for each state for the plus one (after treatment) period.

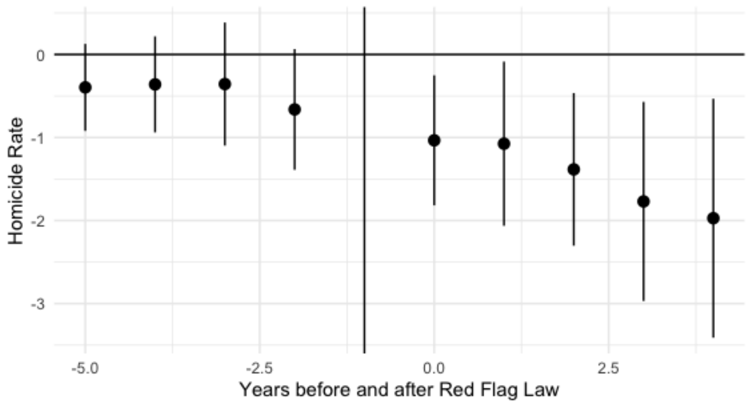
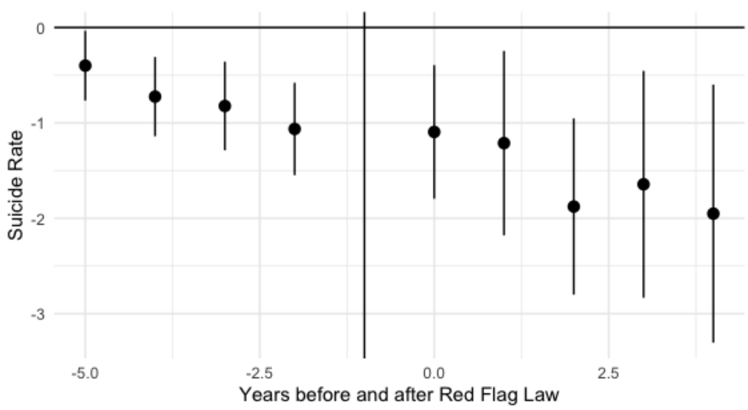


Figure 1.1: Homicide Rate Event Study

It is important to note the fact that zero is within every confidence interval for the entire pre-treatment period. In addition to conducting an event study on homicide rates, I also performed a χ^2 test to assess the joint significance, which resulted in a p-value of 0.2. This finding suggests that there is evidence to support making causal claims regarding the impact of Red Flag Laws implementation on the reduction of homicide rates.

Figure 2 shows difference-in-differences event study results for suicide rates. These estimates ask whether suicide rate patterns were changing in the time period leading up to or after a Red Flag Law adoption. There are similar issues with the data observations for suicide rates as there were with homicide rates, more specifically during the after treatment period.

Figure 1.2: Suicide Rate Event Study



The specification above shows less clear evidence of parallel trends for suicide rates. A

χ^2 test on the joint significance has a p-value of less than 0.01. This implies a violation of the parallel trends assumption in the difference-in-differences identification strategy. Consequently, it becomes challenging to establish causal claims regarding the impact of Red Flag Laws implementation on the reduction of suicide rates. Based on the event study presented in Figure 2, suicide rates were already declining prior to the implementation of a Red Flag Law. Therefore, while I expect the Red Flag Laws to have a causal effect on suicides, some of the estimated reductions can be coming from the pre-existing upward time trend.

1.5.3 Heterogeneous Effects

Table 3 below presents the results when the Red Flag Law variable is divided into two categories: Law Enforcement Only and Law Enforcement & Family. In states that have implemented a Red Flag Law, there are two variations: some states allow only law enforcement to request or invoke the Red Flag Law, while other states permit both law enforcement and family members to do so. Out of the 19 states that currently have a Red Flag Law, seven states exclusively authorize law enforcement to enact the law, while the remaining 12 states allow both law enforcement and family or household members to utilize it. For a detailed breakdown of which states fall into each category, please refer to Table 9 in the supplemental appendix.

Table 1.3: Results for Red Flag Law Decomposed

Dependent Variables:	Suicide Rate		Homicide Rate	
Years:	1990-2020		1990-2020	
Model:	(E)	(F)	(G)	(H)
Variables				
Law Enforcement Only	-0.4801	-0.8250*	-0.4207	-0.3970*
	(0.3171)	(0.4364)	(0.2603)	(0.2118)
Law Enforcement and Family	-1.348***	-2.005***	-0.7630**	-1.059**
	(0.4814)	(0.5538)	(0.3135)	(0.4473)
Median Household Income	0.3756		0.5773	
	(1.1571)		(1.1214)	
White Ratio	57.90***		-3.509	
	(13.42)		(13.74)	
Male Ratio	79.08		62.08	
	(83.10)		(37.10)	
Population	−0.08083***		-0.08881***	
	(0.001839)		(0.01303)	
Unemployment Rate	0.0147		-0.0412	
	(0.0562)		(0.0552)	
Fixed-effects				
States	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,550	1,550	1,545	1,545
R^2	0.91090	0.89042	0.88238	0.85968

These are the DiD regression coefficients when the indicator variable "Red Flag Law" from equation 1 is decomposed into one of two variables: "Law Enforcement Only" or "Law Enforcement and Family." Suicide rate is the dependent variable for models (E) and (F); homicide rate is the dependent variable for models (G) and (H). Of the 19 states that have a Red Flag Law on the books, 7 states fall under the "Law Enforcement Only" variable with the remaining 12 falling under the "Law Enforcement and Family" variable. For which states fall under which of the two variables, see Table A.1 in the Supplemental Appendix. The coefficients and standard errors for Median Household Income and Population have been re-scaled to be in the thousands. Standard errors are clustered at the state level in parentheses. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

In Table 3, the Red Flag Law variable is divided into two separate variables: Law Enforcement Only and Law Enforcement & Family. The states that allow either law enforcement or family and household members to request an ERPO demonstrate statistical significance concerning both suicide rates and homicide rates across all models. However, it appears the effectiveness of Red Flag Laws are less impactful when only law enforcement are allowed to implement the policy. This observation is sensible and highly plausible due to a compelling reason. When a greater number of individuals, including family members who often possess more knowledge, have the ability to take action or voice concerns upon noticing something unusual, the effectiveness of the law is likely to increase.

The results from Table 3 indicate statistical significance for both dependent variables, both with and without control variables, in regards to the Law Enforcement & Family variable. Models (E) and (F), where the dependent variable is suicide rate, the statistical significance is at the 1% level for the Law Enforcement & Family variable. Models (G) and (H), where the dependent variable is homicide rate, the statistical significance is observed at a 5% level for the Law Enforcement & Family variable.

The findings from Table 3 suggest that the involvement of family members in the implementation of Red Flag Laws has significant policy implications. When family and household members are included in the process of requesting or invoking Red Flag Laws, the impact on reducing suicide rates and homicide rates becomes more apparent. These results further corroborate the findings reported by the FBI regarding active shooters. The FBI's research indicated that in cases involving active shooters, family or household members often noticed four to five concerning behaviors. Spouses or domestic partners reported noticing 87% of these behaviors, while family members noticed 68% (Silver et al. 2018).

By allowing family members to participate, these laws enable those who are closest to individuals at risk of harming themselves or others to take proactive measures. More often than not, family members have personal knowledge of the individual's behavior, mental health, as well as potential warning signs. This makes them valuable sources of information for identifying and addressing potential risks before a tragic event takes place.

The statistical significance observed in the results highlights the effectiveness of including family members in the implementation of Red Flag Laws. This indicates that their involvement can contribute to more successful interventions and prevention efforts. With a broader network of individuals empowered to act when they observe concerning behaviors or indicators of potential harm, the likelihood of detecting and addressing risks increases.

From a policy perspective, these findings suggest that expanding the scope of Red Flag Laws to include family members as stakeholders is beneficial. Policymakers should give careful consideration to including family members and household members, who share a close relationship with individual who is suspected of being a danger to themselves or others, to play a role in the implementation and enforcement of these laws. One way this could be achieved is by providing training, resources, and support to help family members identify warning signs, communicate concerning behavior to authorities, and help them steer through the legal process.

Moreover, these results further highlight the importance of public engagement and partnership in addressing mental health along with public safety concerns. By recognizing the crucial role that family and household members can play, policymakers can strengthen the effectiveness of Red Flag Laws and improve overall outcomes in preventing suicides and homicides.

Moreover, these results emphasize the importance of community involvement and collaboration in addressing mental health and public safety concerns. By recognizing the valuable role that family members can play, policymakers can enhance the effectiveness of Red Flag Laws and improve overall outcomes in preventing suicides and homicides.

1.6 Robustness Checks

One potential limitation of this model is the staggered treatment effects, which occur when Red Flag Laws are implemented at different times across states. For example, some states like Connecticut adopted the policy early on, while others such as Nevada, Hawaii, New Mexico, and Virginia implemented it at a later stage. When using a TWFE DiD model to analyze this staggered policy adoption, there is a possibility of bias due to the heterogeneity of treatment effects. Recent advancements in econometric theory suggest that staggered difference-in-differences identification strategies might not yield accurate estimates of the Average Treatment Effect (ATE) or the Average Treatment Effect on the Treated (ATT).

To address this limitation, I employ Goodman-Bacon (2021) decomposition that breaks down TWFE models into all two-by-two estimates and their corresponding weights. The staggered treatment effect raises the question of which group is primarily influencing the coefficient of interest. For instance, is it the early adopters or the later adopters driving the negative coefficient? Goodman-Bacon's decomposition enables applied research to determine which specific group is truly responsible for driving the coefficient of interest.

A basic DiD estimator is a weighted average of all two-by-two estimators in the data. Those weights come from the size of each subgroup (within the context of this project the number of states at a given time that are in the treatment group relative to the number of states in the control group) and the variance of treatment (when the treatment turns on in terms of how close to the beginning/end of the subsample). The estimates can change due to the weights changing, the two-by-two DiD terms changing, or in some cases it can be a combination of both (Goodman-Bacon, 2021). The vast majority of states that have adopted a Red Flag Law have done so within the past few years, meaning there are several states that turned the treatment on near the end of the subsample. This could potentially bias the DiD estimator I get when I run my TWFE DiD model. The Bacon Decomposition separates the four two-by-two DiD estimates where the weights are based on group sizes as well as variance in treatment.

1.6.1 Bacon Decomposition

Table 4 below shows the results for the Bacon decomposition for suicide rate. The tables below (Tables 4 & 5) do not include any control variables in the specification such as median income, population, unemployment rate, race, or gender. It is important to note that since my suicide rate data starts in 1990 and the first state to implement a Red Flag Law was in 1999, there is no "always treated" group.

Table 1.4: Suicide Rate

	1990-2020	
Type	Weight	Avg. Estimate
Earlier vs. Later Treated	0.16806	0.07662
Later vs. Earlier Treated	0.02563	-0.05828
Treated vs. Untreated	0.80631	-1.72635

From Table 4 the driving force for the negative coefficient that is presented in Table 2 for the Red Flag Law variable largely comes from the "Treated vs. Untreated" type (weight > 0.80). This is a good indication that the results in Table 2 from the TWFE DiD model is driven by the states that implemented a Red Flag Law compared to states that do not. It appears the source of bias is small, with the "Later vs. Earlier Treated" group contributing only 0.02563 of the weight towards the coefficient of interest.

Table 5 below shows the results for the Bacon decomposition for homicide rate.

Table 1.5: Homicide Rate

	1990-2020	
Type	Weight	Avg. Estimate
Earlier vs. Later Treated	0.17240	-0.05663
Later vs. Earlier Treated	0.02629	-0.50424
Treated vs. Untreated	0.80130	-0.86267

In order to balance the panel, which is required to run the Bacon decomposition, Mississippi had to be dropped since I am missing Mississippi's homicide rate data from 1990-1994

Similar to Table 4, Table 5 shows the "Treated vs. Untreated" with a weight of 80% is the driving the coefficient of interest. This is a good indication that the results in Table 2 from the TWFE DiD model is driven by the states that implemented a Red Flag Law compared to states that never do. Again, it apepars the source of bias is minor, with the "Later vs. Earlier Treated" group contributing only 0.02629 of the weight towards the coefficient of interest. Another important point to highlight is that all the estimates in Table 5 are negative.

1.6.2 Callaway & Sant'Anna

To further assess the sensitivity of my results presented in Table 2 using a doubly-robust estimator. My goal is to estimate average treatment effects of Red Flag Laws on treated states. One estimator I use to do so is (Callaway and Sant'Anna, 2021). This approach first estimates group/cohort and time specific average treatment effects on the treated (ATT), using two-period/two-group DiD estimators and then aggregates them, weighting them with respect by the size of the treatment group/cohort, to produce summary treatment effect estimates. The primary concept in Callaway and Sant'Anna (2021) is the group-time ATT. The group-time ATT is a different ATT for a cohort of treated units at the same point in time. For example, in this paper, California and Washington both implemented their Red Flag Law in 2016, then they are referred to as the 2016 group, or cohort. If seven more states implemented their Red Flag Law in 2018, then they would be referred to as the 2018 group. And so forth. For each cohort/group Callaway and Sant'Anna (2021) calculates a group's ATT. For instance, the 2016 group, this estimator allows me to see their group's ATT in 2017 and 2018. Essentially, as far out as the data-set goes I can see a group's ATT and in the context of this paper I can see each group's ATT through 2020. Upon calculating each group's ATT Callaway and Sant'Anna (2021) aggregate all of them into fewer simpler parameters.

Table 6 below presents the results for suicide rates and homicide rates when using Callaway and Sant'Anna's estimator.

Table 1.6: Callaway & Sant'Anna Results

Dependent Variable:	Suicide Rate	Homicide Rate
Years:	1990-2020	1990-2020
Model:	(I)	(J)
Variables		
Red Flag Law	-0.0763	-0.4741***
	(0.2496)	(0.1503)
Control Group		
Never Treated	X	X

These are the coefficients when using Callaway and Sant'Anna's estimator, where the dependent variable is suicide rate for column (I) and homicide rate for column (J). Standard errors are clustered at the state level in parentheses. The control group consists of units that never receive the treatment *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

In Table 6, the Red Flag Law variable represents the overall ATT. The overall ATT calculates the average treatment effect across all groups that received the treatment. It represents the average effect of the policy (Red Flag Laws) across all states that implemented this policy in any given year.

Columns (I) represents the results when the dependent variable is suicide rate. Despite the negative coefficient of interest (Red Flag Law), it is not statistically significant. This suggest the TWFE DiD results on suicide rates do not hold up when using a doubly robust estimator.

Columns (J) represents the results when the dependent variable is homicide rate. The coefficient of interest exhibits the expected sign and is statistically significant at the 1%-level. This further supports the initial TWFE DiD findings, indicating that the implementation of Red Flag Laws has a significant impact on reducing homicide rates in states that have passed and implemented such laws.

While Table 6 displays slightly smaller effects compared to Table 2, specifically in column (D) where no control variables are used and the dependent variable is the homicide rate, a reduction of 13.52% is observed. On the other hand, in column (J) of Table 6, applying the coefficient to the mean homicide rate results in a 9.05% reduction.

1.6.3 Falsification Test

For a robustness check, I conducted a falsification test using property crime data to determine if Red Flag Laws have had any effect on such crimes. The rationale behind this test is that the implementation of a Red Flag Law should not lead to a substitution effect where individuals, instead of engaging in violent acts, opt to commit property crimes. It is reasonable to assume that a policy aimed at reducing violence would not influence someone contemplating suicide or homicide to suddenly shift their actions towards property crime.

The purpose of conducting a falsification test is to further validate my findings by examining whether the implementation of Red Flag Laws has had a significant impact on unrelated crimes, such as property crime. If the adoption of Red Flag Laws coincides with notable declines in unrelated crimes, it raises concerns about the validity of my results.

Table 8 below displays the result of the falsification test conducted using property crime as the dependent variable. The TWFE DiD model from equation (1) is employed, incorporating both year fixed effects and state fixed effects. The falsification test is executed with control variables.

Table 1.7: Falsification Test

Dependent Variable:	Property Crime	
Years:	1990-2020	
Model:	(M)	(N)
Variables		
Red Flag Law	16.04	-156.5
	(114.4)	(148.6)
Median Household Income	-0.0102	
	(0.0146)	
White Ratio	3,836.2	
	(4,475.3)	
Male Ratio	81,962.4***	
	(16,411.3)	
Population	-0.01706**	
	(0.01055)	
Unemployment Rate	35.10	
	(27.10)	
Fixed-effects		
States	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	1,550	1,550
R^2	0.92273	0.88969

These are the DiD regression coefficients from equation 1 when the dependent variable is property crime. The purpose of these results is to show that Red Flag Laws did not impact something they were not targeted to have an impact on. The model includes state and year fixed effects as well as include all control variables involved in this project. The coefficients and standard errors for Median Household Income and Population have been re-scaled to be in the thousands. Standard errors are clustered at the state level in parentheses.
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The findings in Table 8 indicate that the implementation of Red Flag Laws did not have a significant impact on property crime. The coefficients obtained from the analysis are not statistically significant, indicating that there is no meaningful relationship between the enactment of Red Flag Laws and changes in property crime rates.

The lack of statistical significance suggests that the introduction of Red Flag Laws did not cause a substitution effect where individuals inclined towards violent acts shifted their behavior towards property crime instead. This reinforces the notion that the primary objective of Red Flag Laws, which is to reduce violence and prevent harm to oneself or others, did not inadvertently lead to an increase in property crimes. The results of the falsification test provide additional support for the validity of the findings regarding the impact of Red Flag Laws on suicide rates and homicide rates.

1.7 Conclusion

Overall, this study provides evidence supporting the effectiveness of Red Flag Laws in reducing both homicide and suicide rates. The difference-in-differences estimates suggest a significant reduction in suicide rates, ranging from a 6% decrease to a 10% reduction. Similarly, the study finds a larger reduction in homicide rates, ranging from 10.96% to 13.52%.

Recent advancements in the difference-in-differences literature have raised questions regarding which specific group is driving these results, particularly when the treatment is implemented over time. To address this concern, the Bacon decomposition method was employed, revealing that the vast majority of the results are primarily driven by the treated vs. untreated group. Furthermore, utilizing the estimation method developed by Callaway and Sant'Anna yields consistent findings with the original specification for homicide rates.

Importantly, the study highlights that the reductions in both suicide and homicide rates are primarily driven by states that allow family members, in addition to law enforcement, to petition a state court for the removal of firearms from a family member whom they perceive as a threat to themselves or others. This inclusion of more individuals in the process increases the efficacy of the policy, potentially leading to saving lives. In conclusion, this study provides compelling evidence in support of Red Flag Laws as an effective policy measure.

Some potential issues or limitations with this study include the limited time that has elapsed since the implementation of Red Flag Laws in most states. Given their relatively recent adoption, it may be necessary to reevaluate the effectiveness of these policies as more time passes and additional data becomes available. Furthermore, the availability of more data on Extreme Risk Protective Orders would allow for a more comprehensive examination of their true effectiveness.

Another potential limitation is the possibility that other forms of gun control measures implemented during the study period could have influenced the presented results. Considering the decline in mental health in the United States, it becomes crucial to have policies in place aimed at protecting individuals from themselves and potentially safeguarding others as well.

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1.9 Appendix

Table 1.8: Red Flag Law State Categories

State	Law Enforcement	Family Member	Duration of the Final Order
California	**X**	**X**	1 year
Colorado	**X**	**X**	6 months
Connecticut	**X**		Up to 1 year
Delaware	**X**	**X**	Up to 1 year
Florida	**X**		Up to 1 year
Hawaii	**X**	**X**	1 year
Illinois	**X**	**X**	6 months
Indiana	**X**		Until terminated by the court
Maryland	**X**	**X**	Up to 1 year
Massachusetts	**X**	**X**	Up to 1 year
Nevada	**X**	**X**	Up to 1 year
New Jersey	**X**	**X**	Until terminated by the court
New Mexico	**X**		Up to 1 year
New York	**X**	**X**	Up to 1 year
Oregon	**X**	**X**	1 year
Rhode Island	**X**		1 year
Vermont	**X**		Up to 6 months
Virginia	**X**		Up to 180 days
Washington	**X**	**X**	1 year

2 The Impact of Execution Moratoriums on Homicide Rates

2.1 Introduction

One of the basic core principles in the field of economics is that humans respond to incentives, whether they are positive or negative. This principle is based on the assumption that rational human beings will attempt to avoid endeavors that cause them aches and pains (disutility) and instead engage in activities that bring them joy and happiness (utility). In the context of criminal justice policy, negative incentives are commonly referred to as deterrence. Deterrence theory posits that threats of punishment, and more importantly, the actual implementation of punishment, can dissuade individuals from engaging in certain actions. Deterrence is generally more effective when the actor believes that the probability of facing the threat is real and outweighs the potential benefits they would gain from such behavior.

Our entire criminal justice system is built on the idea of deterrence: commit a crime, such as theft or assault, and face the associated punishment. Depending on the offense, this could involve a monetary fine and/or some form of incarceration. In more extreme cases, such as capital punishment, the convicted individual may face death as their punishment. The assumption behind capital punishment is that not only are human beings rational actors capable of weighing the costs and benefits of their actions, but also that the potential consequences defined in criminal justice laws are common knowledge. Deterrence has three key components: severity, certainty, and speed. The severity of the punishment, the likelihood of being caught and punished, and the speed of the punishment's execution all play a role. In the case of capital punishment, deterrence largely relies on the severity component, which is rarely imposed and often prolonged over several years before being carried out.

Capital punishment has been extensively studied over the past five decades. The literature on the deterrent effect of capital punishment is inconclusive at best. Recently, several states have implemented moratoriums on executions. These moratoriums involve the governors of these states halting capital punishment, although under the moratorium, a defendant can still be tried and sentenced to death. However, the execution will not be carried out as long as the moratorium remains in effect.

Capital punishment as a deterrent has been studied in various settings using different methods. Currently, there are five states with such moratoriums: Oregon (since 2011), Colorado (since 2013; the death penalty was abolished in 2020), Washington (since 2014; the death penalty was abolished in 2018), Pennsylvania (since 2015), and California (since 2019).

These recent moratoriums have various underlying reasons. One is the financial burden imposed on taxpayers due to the extensive costs associated with repeated court proceedings. Another significant factor is the perceived discrimination against mentally ill or black defendants, as well as individuals unable to afford high-priced legal representation. Moreover, the irreversibility of capital punishment raises concerns, as there is no way to undo an execution once it has been carried out. Additionally, the notable number of death penalty sentences being reversed raises questions about whether true justice is being served through capital punishment.

In this paper, my research objective is to evaluate the impact of state-level moratorium, on homicide rates. To accomplish this, I employ the Synthetic Control Method (SCM) developed by (Abadie and Gardeazabal, 2003). After conducting the analysis, I find no statistically significant impact on homicide rates.

2.2 Literature Review

Capital punishment, also known as the death penalty, has been extensively examined through an economic lens in numerous studies published. One influential work by Becker (1968) presents an economic model that suggests the death penalty can act as a deterrent to crime. In this seminal work, Becker presents an economic model that considers the deterrence effect of capital punishment on potential offenders. He argues that individuals weigh the costs and benefits of committing a crime, including the risk of punishment. Becker suggests that the death penalty can act as a deterrent to crime.

Research on the deterrent impact of capital punishment has yielded mixed results, with several factors contributing to this variability. These factors include methods adopted, time periods examined, observational units, and the nature of the dataset (such as time-series or cross-sectional). Additionally, the selection and treatment of variables play a crucial role. For instance, researchers must decide whether to transform variables (e.g., using logarithmic or square root functions), handle outliers, or employ order differences instead of absolute values. These methodological differences make it challenging to reach a consensus on the deterrent effect of capital punishment (Yang and Lester, 2008).

Some early studies found a deterrent effect, while others failed to find such an effect. Studies that reported a deterrent effect likely played a role in the reinstatement of capital punishment in 1976 after it was deemed unconstitutional by the Supreme Court in 1972. One notable study by Ehrlich (1975) examines the relationship between capital punishment and its potential deterrent effect on crime rates. Using time-series analysis covering the period from 1933 to 1969, the study supports the deterrence hypothesis. Specifically, Ehrlich's findings suggested that one execution could potentially prevent 7-8 murders per year during the examined time period.

However, Passell and Taylor (1977) revisited Ehrlich's data, focusing on the years 1935 to 1964, and found that Ehrlich's results were highly sensitive to the choice of independent variables and model specifications. When using alternative models, Ehrlich's original findings failed to generate a deterrent effect for executions. In response to these criticisms, Ehrlich (1977) expands on his previous work, this time using a cross-sectional analysis for 1940 and 1950, refining his models, and incorporating updated data. The paper presents additional analysis and evidence supporting the deterrence hypothesis, suggesting that an increase in the probability of execution is associated with a decrease in homicide rates.

Cloninger (1977) examines the relationship between the death penalty and crime deterrence using a cross-sectional analysis, focusing solely on year 1960. By analyzing data from multiple states, the study finds significant evidence to support the deterrence hypothesis. Layson (1985) reevaluates the evidence on the relationship between homicide rates and deterrence in the United States, with a specific focus on the potential impact of capital punishment. Using time-series data, Layson employs a variety of specifications, including varying lengths of examined time periods, different sets of independent variables, and alternative functional forms. Overall, the findings indicate that there is evidence to support the claim that capital punishment serves as a deterrent to homicide.

Shepherd (2005) investigates the heterogeneity in the effects of capital punishment across different states in the United States. Using a data-set composed of U.S. counties from 1977 to 1996, the study examines whether capital punishment has a differing impact on murder rates among states. The analysis reveals that while some states experience a deterrent effect, others exhibit a brutalization effect, wherein executions lead to an increase in homicide rates. Mocan and Gittings (2003) contribute to the discussion by demonstrating that commutations to life imprisonment, rather than executions, have a significant deterrent effect. Their research explores the idea that the possibility of being removed from death row may weaken the perceived severity of the punishment, potentially diminishing the deterrent effect. The findings indicate that commuted sentences have a substantial impact on reducing future homicides, highlighting the role of the fear of execution in reducing violent crime rates. Using panel data, Dezhbakhsh et al. (2003) analyze the impact of capital punishment on murder rates. Their results indicate that capital punishment has a deterrent effect, particularly in states with a high execution rate. The authors suggest that a well-designed and consistently implemented death penalty system can contribute to reducing homicides.

On the other side of the debate, there is a growing body of literature suggesting that capital punishment does not have a deterrent effect. Cheatwood (1993), using a matching process, identified 293 pairs of counties in the U.S. that share 45% or more of their borders across a state line. The study used data from the 1988 *County and City Data Book* to examine the difference in the violent crime rate in each pair. By comparing counties within the same state that differ in their application of the death penalty, the study finds no significant influence of capital punishment on violent crime rates. Grogger (1990) takes a unique approach by analyzing daily time-series data in California from 1960 to 1963 to examine the immediate and short-term effects of capital punishment on homicide rates. Using two-week and four-week windows surrounding the dates on which an execution was carried out, he examines for potential deterrent effects. The study does not find significant evidence to support the deterrence hypothesis in the short term.

Furthermore, Donohue and Wolfers (2006) conducted an investigation into the empirical evidence concerning the impact of the deterrent effect of capital punishment. Their analysis concludes that studies supporting the deterrence hypothesis often rely on flawed methodologies, leading to an overstatement of the deterrent effect. Additionally, in another study, Donohue III and Wolfers (2009) examined panel data from all 50 U.S. states and found no evidence to support the notion that the death penalty has a significant deterrent effect on murder rates.

Similarly, Zimmerman (2004) examined the relationship between state executions and homicide rates to assess the potential deterrent effect of capital punishment. The study did not find any evidence to suggest that executions have a significant negative impact on murder rates. Another example is the study by Kovandzic et al. (2009), which evaluated panel data from 1977 to 2006. Their results provided no empirical support for the argument that the death penalty has a deterrent effect. Parker (2021), using the SCM, examines seven states that abolished capital punishment between 1995 and 2018 and its impact on deterring murders. The findings of the study suggest that the presence of capital punishment on the books in a state is not sufficient to deter murders. Overall, Parker's research challenges the belief that the mere presence of capital punishment legislation is effective in deterring murders.

The present study shares similarities with Oliphant (2022) research as both studies examines recent moratoriums on executions. However, there are some distinctions between the papers. For instance, I study the following states: Oregon, Washington, Colorado, and Pennsylvania whereas Oliphant studies Illinois, New Jersey, Washington, and Pennsylvania.

Additionally, in my study, all untreated states serve as units in the donor pool, in contrast to Oliphant's approach of including only states that currently have the death penalty in the donor pool. I also use a TWFE DiD model to estimate the impact of this policy on homicide rates.

The current findings align closely with those of Oliphant (2022), as both studies fail to observe a deterrent effect of the death penalty. These findings suggest that recent moratoriums on executions have not resulted in a significant impact on reducing homicide rates.

2.3 Data

2.3.1 Data Source

I constructed a state-by-year panel using data collected from various sources, including the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) database, the Center for Disease Control and Prevention (CDC), the Bureau of Economic Analysis (BEA), and the Bureau of Labor Statistics (BLS). Homicide rates were collected from the FBI's UCR database for the years 2000 to 2020. Suicide rate data was obtained from the CDC, available from 2000 to 2019. Additionally, total property crime data was collected from the FBI's UCR database for the years 2000 to 2020. Homicide rates, suicide rates, and property crime rates are measured per 100,000 people.

Based on their previous inclusion in death penalty studies, twelve variables were included in this study (Kovandzic et al., 2009, Oliphant, 2022). These variables are at the state-level. The following variables were used in the present study: population data, gender data (male and female), and race/ethnicity data provided by the CDC for the period spanning 2000 to 2020. Income data was extracted from the BEA, specifically the median household annual income, covering the years 2000 to 2020. Seasonally adjusted annual state-level unemployment rate data was collected from the BLS for the same time frame. I selected the unemployment rate from the first month (January) of each year for each state and used it as the rate for the entire year.

Education attainment variables, such as high school attainment and college attainment, were obtained from Mark W. Frank's website[7] and cover the years 2000 to 2015. These education variables are expressed as percentages by dividing the total number of graduates by the total state population. Prison population data, measured per 100,000 adults, was acquired from Jacob Kaplan's website[8] and encompasses the years 2000 to 2016.

Table 1 below presents the summary statistics of the dataset used in this project. The panel dataset includes data from all fifty states spanning a twenty-one-year period, from 2000 to 2020.

Table 2.1: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Homicide Rate (per 100,000)	1,050	4.672	2.402	0.60	15.80
Suicide Rate (per 100,000)	1,000	14.153	4.174	1.50	29.70
Property Crime Rate	1,050	2,900.81	846.84	1,053.20	5,849.80
Prison Population Black Male	850	3,449	1,396	1,030	19,208
Prison Population White Male	850	516	197	136	1,060
Population	1,050	6,138,838	6,809,090	494,300	39,512,223
Male (gender ratio)	1,050	0.498	0.137	0.480	0.762
White (proportion of population)	1,050	0.827	0.264	0.296	0.978
Black (proportion of population)	1,050	0.113	0.102	0.004	0.385
Median Household Income	1,050	41,387.96	10,341.43	21,640	78,609
Unemployment Rate	1,050	5.398	2.009	2.00	13.70
High School Attainment	800	0.646	0.039	0.526	0.748
College Attainment	800	0.195	0.042	0.107	0.306

Suicide rate data runs from 2000 to 2019. The prison population variables run from 2000 to 2016 and is measured per 100,000 adults. The male variable measures the ratio among males and females. The race variables measure the proportion of population. The educational attainment variables run from 2000 to 2015.

2.4 Policy Implementation

Currently, there are five states that have implemented a moratorium on executions: Oregon in 2011, Colorado in 2013, Washington in 2014, Pennsylvania in 2015, and California in

[7] Accessed at https://www.shsu.edu/eco_mwf/inequality.html
[8] Assessed at https://jacobdkaplan.com/index.html

2019. Currently, in the United States, there are 27 states that have the death penalty and 23 states that do not.

The reason behind these moratoriums is the claim that capital punishment has resulted in discrimination against mentally ill, black and brown defendants, or individuals who cannot afford expensive legal representation (Center, n.d.). Additionally, it has been argued and debated that capital punishment provides no public safety benefits or value as a deterrent and has wasted billions of taxpayer dollars.[9] Pennsylvania Governor Tom Wolf, in 2015, stated that the decision for a moratorium was based on a flawed system characterized by endless court proceedings, inefficiency, injustice, and high costs (Center, n.d.). Oregon Governor John Kitzhaber, in 2011, expressed his belief that executions did not contribute to public safety nor did they elevate our society morally. He further stated that the death penalty, as implemented in Oregon, lacked fairness and justice, and lacked the attributes of swiftness, certainty, and equal application for all individuals involved. Similarly, Washington Governor Jay Inslee, in 2014, expressed concerns about flaws within the system, emphasizing that when the ultimate decision is death, the stakes are too high to accept an imperfect process. He also noted that the majority of death penalty sentences lead to reversals, raising doubts about the entire system (Center, n.d.).

2.5 Method

I empirically test whether the implementation of moratorium policies has had an impact on homicide rates using the SCM approach developed by (Abadie and Gardeazabal, 2003). The logic behind the SCM involves selecting a group of comparison units from the pool of untreated units, known as the donor pool. These comparison units are assigned weights in a way that ensures the synthetic control closely resembles the treated unit before the implementation of the treatment. Typically many untreated units receive a weight of zero, resulting in the synthetic control being a weighted average of only a subset of the donor pool.

[9] California Governor Gavin Newsom March 13, 2019 (Center, n.d.)

To formalize this model, let's suppose there are S control units available, which have not implemented the moratorium on executions, forming what is referred to as the "donor pool." Let T_0 be the final time period before the policy's implementation. Thus, the periods before the treatment are denoted as $t = 1, ..., T_0$, while the treated periods are represented by $t = T_0 + 1, ..., \overline{T}$.

Let $\mathbf{W} = (w_1, w_2, ..., w_S)$ be a $S \times 1$ vector, where each w_s is a non-negative weight assigned to an individual control unit from the donor pool of S units. The weights in the vector sum up to one. There are K predictor variables. Define $\mathbf{X}_1$ as a $K \times 1$ vector that contains the values of these predictor variables for the treated unit. Similarly, $\mathbf{X}_0$ is the $K \times S$ matrix that contains the same predictor variables for the S control units. The optimal counterfactual is determined by the vector of weights, $\mathbf{W}^*$, which minimizes the distance between $||\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}||$. Where $||\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}|| = \sqrt{(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})' V (\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})}$ with V being a diagonal matrix. Let X_{sm} be the value of the mth covariates for unit s. Then the synthetic control weights minimize:

$$\sum_{m=1}^{K} v_m \left(X_{1m} - \sum_{s=1}^{S} w_s X_{sm} \right)^2$$

where v_m is a weight that represents the relative importance assigned to the mth variable (Cunningham, 2021). The goal of constructing the synthetic control is to estimate the counterfactual time path of the outcome variable for the treated unit in the absence of the policy adoption.

I apply the SCM independently to Colorado, Oregon, Washington, and Pennsylvania, which currently have these policies, while using the remaining 45 states as the donor pool.[10] When applying the SCM to a single treated state, the other treated states are excluded from the donor pool because they implemented the policy at a later stage. The post-treated period for each state is initiated in the year when the moratorium on executions was implemented.

[10]One reason for including states that do not have capital punishment is to have a larger pool of potential comparison units. This is particularly important because, with the synthetic control method, it is possible for states in the donor pool to receive a weight of zero.

Predictor variables used in this project include the outcome variable (homicide rates) as well as suicide rates, property crime, prison population (separately for black males and white males), total population, gender (male-to-female ratio), race (percentage of white and black populations), median household income, unemployment rate, and educational attainment (separately for high school and college degree). Following Abadie et al. (2015), in addition to optimizing over the donor pool, I also optimize the weights of the predictor variables.

The purpose of the synthetic controls is to provide the best possible representation of what the homicide rates would have been in the treated states if the moratorium on executions had not been implemented. Any differences observed between the actual homicide rates in Oregon, Colorado, Washington, and Pennsylvania after 2011, 2013, 2014, and 2015, respectively, and the synthetic counterparts can be attributed to the respective moratoriums that were put in place.

2.6 Results

Using the techniques described in the previous section, I create a synthetic control for each treated unit (Oregon, Colorado, Washington, and Pennsylvania). Table 2 below presents the weights assigned to the donor states that make up each synthetic control.

Table 2.2: State Weights in each Synthetic

States	Weight	States	Weight
Oregon		*Colorado*	
Alaska	0.135	Alaska	0.158
Hawaii	0.008	Maryland	0.173
Idaho	0.040	New Hampshire	0.397
Maine	.671	New Mexico	0.042
Rhode Island	0.135	Vermont	0.229
Utah	0.050		
Washington		*Pennsylvania*	
Hawaii	0.157	Maryland	0.067
Maine	0.056	Michigan	0.418
Minnesota	0.273	Missouri	0.098
Ohio	0.322	New Jersey	0.117
Rhode Island	0.118	New York	0.058
Utah	0.074	South Dakota	0.169
		Virginia	0.047
		Wyoming	0.023

All other states in the donor pool obtain zero weights for each synthetic. In each case the synthetic state weights sum to one.

Table 3 presents the predictor variables values for each treated state and its corresponding synthetic. This allows us to evaluate the quality of the synthetic by comparing the predictor variables between the actual treated states (Oregon, Colorado, Washington, and Pennsylvania) and their respective synthetics. Essentially, the table compares the pre-treatment characteristics of the treated states with those of the created synthetics.[11]

Table 2.3: Homicide Rate Predictor Means

State	Oregon		Colorado		Washington		Pennsylvania	
Variable	Treated	Synthetic	Treated	Synthetic	Treated	Synthetic	Treated	Synthetic
Homicide Rate	2.33	2.36	3.54	3.59	2.92	3.02	5.50	5.38
Suicide Rate	16.5	15.6	18.4	15.5	14.4	12.4	12.9	12.5
Property Crime	3,530	2,407	3,082	2,427	3,893	2,975	2,121	2,459
Prison Pop. Black Male	3,909	2,590	4,038	3,606	2,796	2,967	3,804	3,399
Prison Pop. White Male	647	375	490	477	407	405	333	472
Total Population	3,833,168	1,277,607	5,057,360	1,856,077	6,474,998	5,776,368	12,620,622	7,798,563
Male Population	0.495	0.493	0.502	0.496	0.499	0.493	0.488	0.512
White Population	0.907	0.912	0.90	0.858	0.843	0.787	0.846	0.838
Black Population	0.025	0.028	0.048	0.070	0.046	0.074	0.131	0.142
Median HH Income	39,094	40,589	45,198	46,804	46,450	41,506	43,650	42,763
Unemployment Rate	6.56	5.42	5.03	4.65	6.02	5.21	5.67	5.67
High School Attainment	0.675	0.675	0.659	0.682	0.665	0.663	0.671	0.661
College Attainment	0.206	0.201	0.266	0.238	0.209	0.201	0.199	0.199

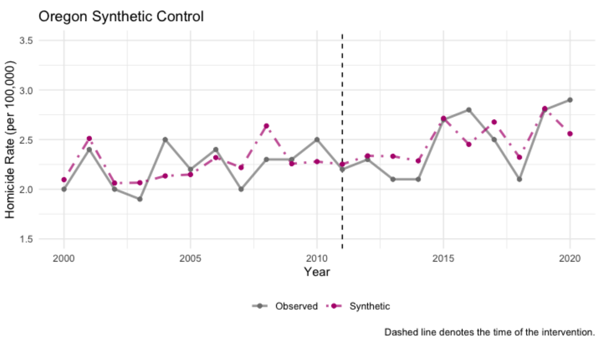
[11]The predictor variable weights table can be found in the Appendix. For example, the four most important predictors for synthetic Colorado (in order from highest to lowest weight) are homicide rate (.442), college attainment (.199), suicide rate (.103), and male population (.069).

The key observation in Table 3 is that the states comprising the respective synthetics closely track the treated states in terms of the outcome variable, which in this case is homicide rates. Across all four synthetics, the outcome variable shows a strong similarity. In most cases, the synthetics closely match the values observed in the treated states. Therefore, the SCM does a commendable job of replicating the treated states' conditions prior to the policy implementation.

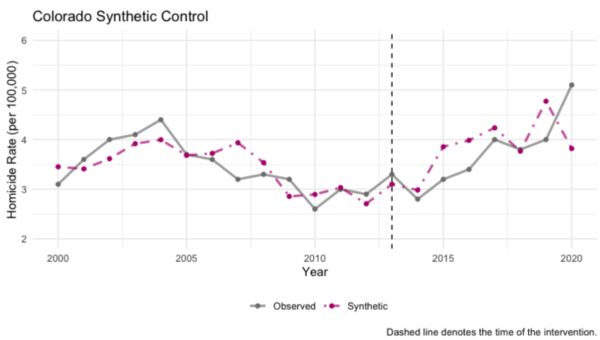
2.6.1 Treatment Effect

Figure 1 visually represents the two time series for each of the four synthetics. The dashed vertical lines indicate when the treated state implemented its moratorium on executions.

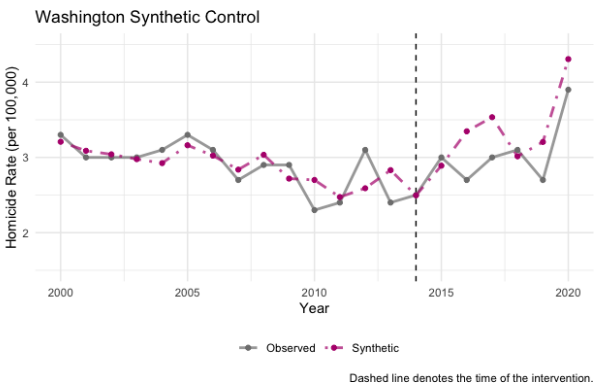
Figure 2.1: Synthetic Control Plots



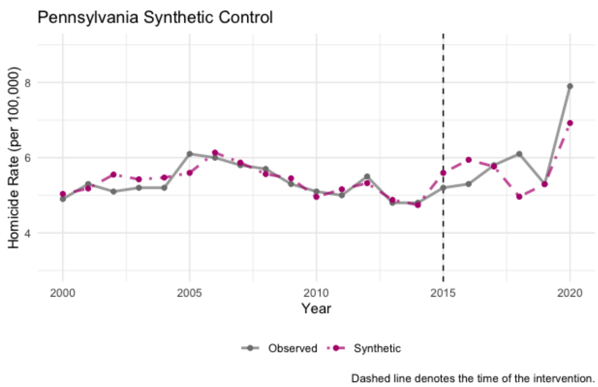
((a)) Actual & Synthetic OR Homicide Rates



((b)) Actual & Synthetic CO Homicide Rates



((c)) Actual & Synthetic WA Homicide Rates



((d)) Actual & Synthetic PA Homicide Rates

Panel (a) of Figure 1 displays the results for Oregon, with the synthetic control represented by the dashed line and the observed data represented by the solid line. The post-treatment period shows no outstanding divergence between the synthetic and observed time series, suggesting that the policy had no discernible effect.

Panel (b) of Figure 1 illustrates the results for Colorado. The pre-treatment fit demonstrates a reasonably close match between the synthetic Colorado and the actual observed data. Similarly, in the post-treatment period, the synthetic Colorado and observed data remain relatively close to each other, indicating that the policy implementation did not have a noticeable impact on homicide rates.

Panel (c) of Figure 1 presents the results for Washington. The pre-treatment match shows a fairly good alignment between the synthetic Washington and the actual observed data. Although there is a slight divergence between the two series around 2016/2017, they converge again in 2018, suggesting no substantial impact of the policy on homicide rates.

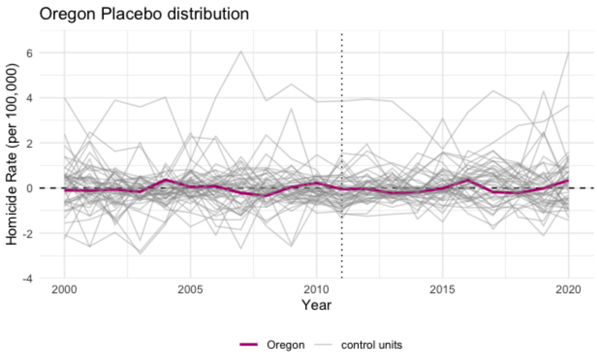
Panel (d) of Figure 1 illustrates the results for Pennsylvania. The pre-treatment fit appears to be quite accurate. Furthermore, there is no noticeable difference in homicide rates between the synthetic Pennsylvania and the actual Pennsylvania after the treatment is implemented.

2.7 Hypothesis Testing

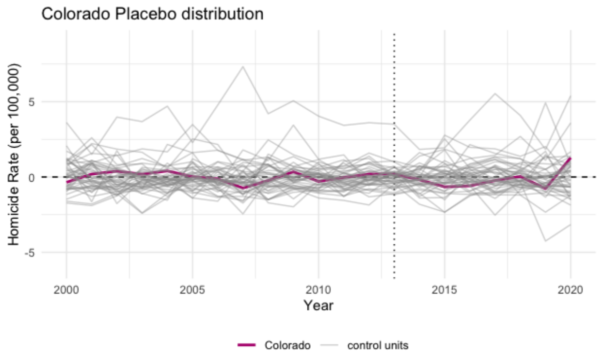
The most common approach to conducting statistical inference with synthetic control is to perform multiple placebo tests, following the methodology outlined in (Abadie et al., 2010). In these placebo tests, each unit in the donor pool is treated as if it had adopted or received the policy in the same year it was actually implemented for the treated state. For each state in the donor pool, I assume that it had the treatment in the same year as the treated states (2011, 2013, 2014, and 2015). Consequently, a synthetic control is constructed, and the divergence observed with this “counterfeit" treatment date is calculated.

By comparing the computed divergence for each treated state (Oregon, Colorado, Washington, and Pennsylvania) to the distribution of divergences, I can draw conclusions about the impact of the policy treatment (moratorium on executions). If the computed divergence for a state falls within the middle of the distribution, it suggests that the policy had little impact. Conversely, if only a few states show significant divergence, it indicates a non-zero treatment effect.

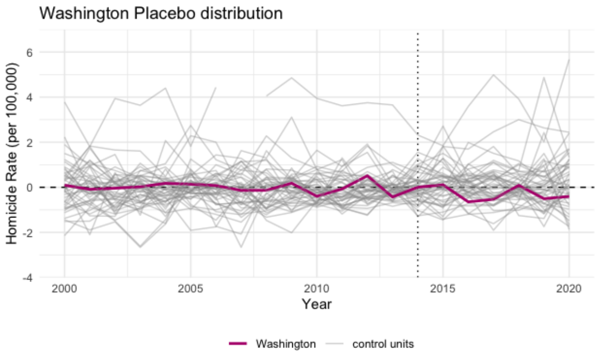
Figure 2.2: Placebo Tests



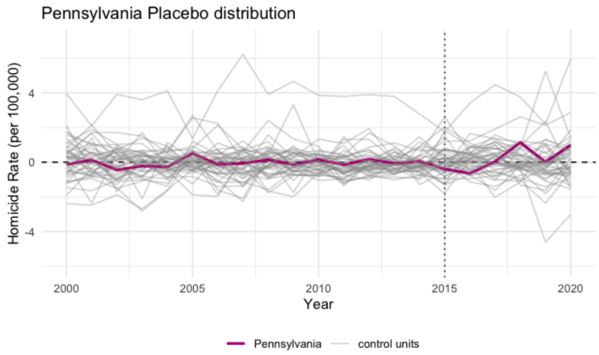
(**(a)**) Oregon Placebo Tests



(**(b)**) Colorado Placebo Tests



(**(c)**) Washington Placebo Tests



(**(d)**) Pennsylvania Placebo Tests

Rather than using the traditional approach of hypothesis testing, which involves examining the ratio of pre-MSPE and post-MSPE, I chose to employ a one-tailed test for the post-treatment periods. This decision was made to consider the direction and actual difference between the synthetic and treated time series, rather than solely looking at the squared distance. In the one-tailed test, I analyzed the distribution of placebo homicide rates in the post-treatment periods that were greater than or equal to the homicide rates of the treated units.

To conduct this test, I subtracted the synthetic homicide rate from the actual treated state homicide rate for each year in the post-treatment period, as well as for each placebo case. Then, I calculated the average of these differences to create a distribution. The same procedure was applied to all placebo cases, resulting in a distribution of average differences. This entire process was separately repeated for each treated state. Table 4 below presents the results from the one-tailed test.

Table 2.4: Post-treatment P-value

State	P-value
Oregon	0.44
Colorado	0.54
Washington	0.74
Pennsylvania	0.20

In the one-tailed test for the post-treatment period, it was observed that 20 states had a greater or equal average post-treatment value compared to Oregon's, $(\frac{20}{46}) = 0.4348$. For Colorado, 25 states had a greater or equal average post-treatment value, $(\frac{25}{46}) = 0.5434$. For Washington, 34 states had a greater or equal average post-treatment value, $(\frac{34}{46}) = 0.7391$. For Pennsylvania, 9 states had a greater or equal average post-treatment value, $(\frac{9}{46}) = 0.1956$.

Based on the p-values presented in Table 4, these findings suggest that the policy did not have a statistically significant effect on homicide rates.

2.8 Robustness Checks

While the SCM is currently considered the most transparent method and does not require parallel trends for casual identification, for the sake of providing a more widely understood approach, I also chose to investigate this policy using the difference-in-differences (DiD) method. This also allows me to test the null results using a different method and see if they change when employing DiD.

2.8.1 Difference-in-Differences

In addition to examining the impact of the moratoriums on executions on homicide rates using the SCM, I employ a two-way fixed effects (TWFE) DiD model to further evaluate their effect on homicide rates:

$$Y_{st} = \alpha Intervention_{st} + \beta X_{st} + \sigma_s + \tau_t + \epsilon_{st}. \tag{2.1}$$

The variable Y_{st} represents an outcome for state s and year t. I will use homicide rate as the dependent variables. The model includes state fixed effects, notated by σ, year fixed effects, τ, and an error term, ϵ. I also include time-varying state level controls, which is notated by X. The coefficient of primary interest is α which is the DiD estimate of the effect of Moratorium on Executions on homicide rates in states that have passed this policy. DiD attempts to identify a causal effect by comparing the changes in outcomes over time between a group that has received the treatment/adopted a policy to a group that did not receive the treatment/adopt the policy.

Table 5 below displays the results on the impact of a moratorium on executions on homicide rates. In addition to the original four states (Oregon, Colorado, Washington, and Pennsylvania), this specification includes California as a treated state since it implemented a moratorium on executions in 2019. Consequently, there are a total of five treated states considered in the DiD analysis.

Table 2.5: DiD Homicide Rate Results

Dependent Variable:	Homicide Rate	
Model:	(A)	(B)
Variables		
Intervention	-0.0736	0.0657
	(0.1847)	(0.1688)
Property Crime		0.0007***
		(0.0002)
Population		-0.0386**
		(0.01635)
Male		-29.64
		(24.72)
White		15.64
		(12.16)
Black		11.51
		(19.46)
Income		-0.8192
		(1.1738)
Unemployment		-0.1606***
		(0.0452)
Fixed-effects		
States	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	1,050	1,050
R^2	0.89872	0.91220

These are DiD regression coefficients. Homicide Rate data is at the state level on an annual basis. All data included in the models are at the state level. Both models include both state and year fixed effects. The coefficients and standard errors for Income and Population have been re-scaled to be in the thousands. Standard errors are clustered at the state level in parentheses.
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Model (A) in Table 5 shows the results of the TWFE DiD analysis without control variables. The coefficient of interest, representing the moratorium on executions, is negative but not statistically significant. Model (B) includes control variables, and the coefficient of interest reverses, suggesting that the removal of capital punishment is associated with an increase in homicide rates. However, this coefficient is also not statistically significant. These findings align with previous DiD and synthetic control estimates, indicating no statistically significant impact of the policy on homicide rates, regardless of the inclusion of control variables.

Following McCannon (2022), I pooled the data from the four treated states with the four synthetics to calculate the average treatment effects. This pooling resulted in a panel data set with 168 observations (8 observational units $\times$ 21 years = 168 observations). Using this dataset, I estimate a DiD specification:

$$Y_{sty} = \delta Synthetic_t + \gamma Intervention_{sy} + \omega Synthetic_t \times Intervention_{sy} + \epsilon_{sty}. \tag{2.2}$$

The dummy variable $Synthetic_t$ is equal to one if the observation comes from a synthetically created observation, and zero if it represents the actual state's value. The dummy variable $Intervention_{sy}$ equals one if the observation occurs in a year, y, after state s has implemented its moratorium on executions. The final term represents the DiD component as it identifies whether the gap between the synthetic and actual treated state widens or narrows in the years following the moratorium on executions.

Table 2.6: Average Treatment Effects

Dependent Variable:	Homicide Rate
Model:	(C)
Variables	
Synthetic $\times$ Intervention	0.0244
	(0.2811)
Synthetic	0.0175
	(0.2415)
Intervention	-0.0077
	(0.2811)
Fit statistics	
Observations	168
R^2	0.00019

These are the DiD regression coefficients from the four treated states along with the four synthetics. Standard errors are presented in parentheses. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The results presented in Table 6 above indicate that the implementation of moratoriums on executions does not yield statistically significant effects on homicide rates. These findings reinforce the previous conclusions drawn from the synthetic control method along with the TWFE DiD, which also demonstrated statistically insignificant results.

2.9 Conclusion

In conclusion, I investigate the impact of moratoriums on executions on homicide rates. The SCM is employed to construct synthetic control groups for each of the four states that implemented the moratorium. Overall, the results presented in this study indicate that for the states who have a moratorium on executions there is no statistically significant deterrent effect on homicide rates. To ensure the validity of the primary method, the study conducted robustness checks and supplementary analyses.

However, I would be remiss if I did not also raise the point that this paper's findings suggest no clear evidence that homicide rates increased due to moratoriums on executions. Given that the death penalty is final and cannot be reversed, and its significant implications, policymakers should carefully consider the evidence regarding its deterrent effect, while also considering other factors such as fairness, justice, and equity in their decision-making process regarding capital punishment policy.

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2.11 Appendix

Table 2.7: Variable Weights in each Synthetic

Variables	Oregon	Colorado	Washington	Pennsylvania
Homicide Rate	.861	.442	.747	.453
Suicide Rate	.01	.103	0	.074
Property Crime	.001	.044	.017	.046
Prison Population Black Male	.005	.07	.024	.004
Prison Population White Male	.087	.002	.065	.001
Total Population	.005	.035	.018	0
Male Population	.017	.069	.01	0
White Population	.014	0	.019	.114
Black Population	.023	0	.001	0
Median HH Income	.002	0	.002	.044
Unemployment Rate	.004	.016	.019	.1
High School Attainment	.048	0	.072	.008
College Attainment	.014	.199	.001	.153

3 The Impact of California's COVID-19 Zero Dollar Bail Policy on Crime

3.1 Introduction

Heightened attention has been placed on criminal justice policy recently. Advocates for change have put forth a number of policies aimed at addressing disparate treatment and overcriminalization. Collectively, these policies are referred to as *progressive prosecution* (Bellin, 2020). A central policy is the elimination of cash bail.

When arrested for a crime, the accused goes before a magistrate where pretrial detention is determined. The accused can be released, detained, or released conditional on providing a monetary guarantee. If the individual putting up this money returns to court for the trial, then the money is returned. If the defendant flees, then the money is forfeited. The logic is that cash bail provides additional incentive to appear in court.

Concerns have been raised regarding this institutional feature of U.S. criminal justice. For one, many individuals do not have the funds to pay the bail. This means that they are incarcerated without being convicted. Hence, pretrial detention is determined in large part by wealth, which is correlated with race and ethnicity. Further, the use of bail bond services to finance the cash bail comes with substantial fees. Thus, cash bail often creates a punishment without, again, having been convicted.[12]

On the other hand, it is argued that it can be an effective mechanism at inducing individuals to appear in court without having to expend the public's resources on jails. Without cash bail and with crowded jails, a concern is that the accused will simply be released on their own recognizance. This can put potentially dangerous individuals back on the streets increasing future crime. Further, if individuals know that an arrest will not likely lead to pretrial detention, then the expected sanction to criminal activity is

[12]See www.brennancenter.org/our-work/research-reports/how-cash-bail-works for an example of these arguments put forth by the Brennan Center for Justice at NYU.

reduced, which may erode deterrence.

Here, we take this concern seriously and ask whether the policy of eliminating cash bail has an effect on the crime rate. To do this, we explore a novel policy experiment. During early months of the Covid-19 pandemic, concerns arose over disease in jails. Steps were taken to reduce the jail populations. In April 2020 California instituted a statewide mandate eliminating cash bail across the state following the logic that the accused would be released and there would be fewer individuals in jail. The statewide mandate ended at the end of June 2020 but, importantly, the state's policy allowed for individual counties to continue with the zero cash bail policy. While a majority of the counties in California re-instated cash bail in July 2020, many did not. Some counties continued the policy until the summer of 2021, while others have maintained it into 2022. This provides us with the opportunity to assess whether the elimination of cash bail has a measurable effect on crime.

We collect monthly, county-level crime from California's Attorney General's office over the 2015-21 time period. We ask whether crime in the months without cash bail in the counties that have continued with the policy differs from crime in months with cash bail allowed. Our primary result is that we do record a small increase in violent crimes and fail to find a change in property crimes. We show that the violent crime increase is approximately $\frac{1}{20}^{th}$ of a standard deviation increase, and that it is concentrated in assaults. Thus, the effect while statistically distinguishable from zero is modest and that its effect arises in the subcategory of crime in which the policy applies and not in the more-violent felonies that were exempt from the zero cash bail policy. Further, we show that the policy is unrelated to crime's clearance rate. This suggests that it is not an alteration in law enforcement's effectiveness that is driving crime but, rather, we are measuring a direct effect of reduced expected sanctions on crime.

Much of the past research on pre-trial detention focuses on its direct effect on case outcomes. For example, Gupta et al. (2016) and Leslie and Pope (2017) examine the impact of defendants not able to make bail on the likelihood of being convicted and the length of sentence that is handed down using data from Pennsylvania and New York City, respectively. They find that defendants who are not able to post bail are more likely to be convicted. Similarly, Stevenson (2020) using data from Philadelphia shows that pretrial detention affects the probability of being convicted and the length of incarceration. She argues that pretrial detention incentivizes plea bargaining, which causes the increase in conviction rates. Dobbie et al. (2020) goes further and shows that pretrial detention, while not affecting post-release crime rates, does affect employment opportunities.

Some have investigated the effectiveness of cash bail. For example, Helland and Tabarrok (2004) compare failure-to-appear in jurisdictions which use public bonds versus private bond dealer. Fishbane et al. (2020) reports on RCTs aimed at 'nudging' defendants to appear in court. Their interventions included text message reminders and redesigning the summons form. Finally, Abrams and Rohlfs (2011) engages in a cost-benefit analysis to estimate a representative defendant's willingness to pay for freedom.

Another important area of research is the relationship between magistrate's pretrial incarceration decisions and race. For example, McIntyre and Baradaran (2013) first document that there are large disparities in the rate at which minority defendants are granted pretrial release, but then go on to show that much of this gap can be explained by differences in recidivism while released and discrepancies in the probability of flight. An early example is Demuth (2003) who studies the influence of race and ethnicity on pretrial release. They report that Hispanic defendants were more likely to be denied bail, had a higher likelihood of having to pay bail to secure their release, and when bail was required were subjected to higher bail amounts. Demuth and Steffensmeier (2004) extend this analysis to the intersection of pretrial incarceration, race, and gender. Ayres and Waldfogel (1994) uses market prices of bail bonds as evidence of racial disparities. Arnold et al. (2018) provides evidence of important race-related disparities that non-White defendants often face higher bail requirements compared to their White counterparts.

Moreover, a growing body of literature delves into alternatives to cash bail, with a particular focus on the utilization of pretrial risk assessments. These instruments are designed to reduce the adverse impacts associated with cash bail. Operating on an actuarial basis, they employ a range of factors including prior instances of non-appearance, substance abuse history, and past convictions to generate a risk score. Zottola and Desmarais (2022) show that the bail amounts were not significantly linked to either failure to appear or re-arrest. Interestingly, individuals who did fail to appear or were re-arrested tended to have higher average bail amounts compared to those who did not. They argue that employing pretrial risk assessments could lead to more accurate release decisions. Further, Desmarais et al. (2021) conduct a meta-analysis of 11 studies that examined the predictive validity of pretrial risk assessment instruments. The findings revealed fair-to-excellent predictive validity for these instruments, both overall and when analyzed within subgroups based on race. Across all groups, higher risk scores were consistently associated with a higher probability of failure to appear, re-arrest (regardless of the type), and re-arrest specifically for violent crimes.

Relatively little investigation has been conducted on the elimination of cash bail even though it is a keystone policy in the progressive prosecution platform. Other popular criminal justice policies, such as diversion programs, have received attention. Mueller-Smith and Schnepel (2021), for example, considers such a program in Harris County, Texas and shows that it reduces re-offending rates and promotes employment. The work closest to ours is Ouss and Stevenson (2022). They consider a policy change in Philadelphia's prosecutor's office eliminating requests for cash bail in 2018. First, they provide empirical evidence that this policy leads to more individuals being released on their own recognizance. Second, while we consider total crime, they consider offending by those currently released and their failure to appear in court. Ouss and Stevenson (2022) finds that there is no change in their criminal behavior. We ask a different question by recognizing that expected sanctions to criminal activity are reduced when cash bail is eliminated, which can erode deterrence. We find that it can.[13]

[13] It is also important to point out the differing settings. Their policy was created by the head, elected prosecutor in a jurisdiction where magistrates have the ultimate decision on pretrial detention. Thus, they consider changes in the requests for cash bail. In our setting, cash bail was eliminated by the judicial system leadership.

3.2 Methods

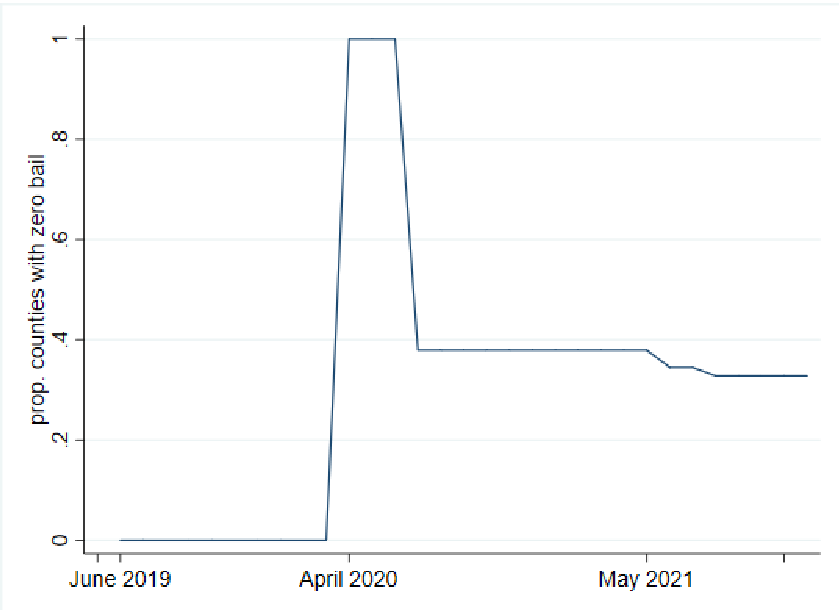
3.2.1 California's Emergency Bail Schedule

Amid growing concerns of Covid-19's spread in California's jails, the California Judicial Council imposed the Emergency Bail Policy. Enacted on April 6, 2020, the Council lowered the cash bail amount to $0 for misdemeanors and non-violent felonies. This mandate applied to all jurisdictions across the state.

The policy was set to expire on June 30, 2020. After that date, counties were free to either continue with the policy or return to the previous bail schedules in use prior to the pandemic.

This policy created heterogeneity in the use of cash bail across the state. A majority of counties re-instituted cash bail when the state mandate was lifted. Some counties, on the other hand, did not remove the policy until 2022 or still have them in effect. While most counties who re-instated cash bail did so as soon as the state mandate was lifted, a few held on to the zero cash bail policy until the middle of 2021. Figure 3.1 depicts the policy's adoption over time.

Figure 3.1: Prevalence of Zero Cash Bail in California



The figure depicts the proportion of California counties who do not have cash bail each month.

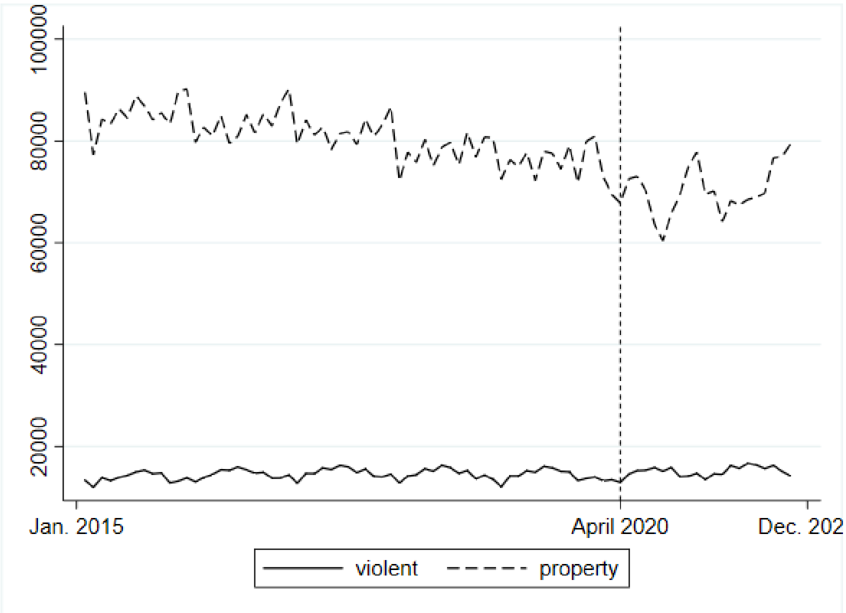
We exploit this policy variation to assess cash bail's impact on crime.

3.2.2 Data

We collect crime data from the Open Justice project. Open Justice is provided by the California Attorney General's office and provides detailed data on criminal justice outcomes across the state.[14]

From this portal, we collect both reported and cleared crime data over the 2015 to 2021 time period.[15] Monthly, law enforcement-jurisdiction level data is available. We obtain total violent and total property crime, along with their breakdowns by major subcategory of crime. Figure 3.2 depicts the volume of violent crime and property crime in the state over time.

Figure 3.2: Prevalence of Zero Cash Bail in California



The figure depicts the total violent crime (solid line) and property crime (dashed line) in California over 2015-21. The vertical line denotes the beginnning of the pandemic policies (April 2020).

While, obviously, the level of property crime is substantially higher than that of violent crime, the time series differ in their trends. There is a reduction in property crimes during the pandemic. The downward trend, though, starts years before the pandemic and

[14] https://openjustice.doj.ca.gov

[15] We choose to limit our analysis by not including older crime data as we expect it to be less representative.

continues for a few months after. Thus, it is not clear that property crime deviated from its time trend with the policy. Violent crime, on the other hand, is rather flat over time. There is an increase, though, in the months following the start of the pandemic. We ask, though, whether the crime patterns differ between counties with and without cash bail.

3.2.3 Identification Strategy

The policy variable of interest is the elimination of cash bail in a county. Therefore, our treatment variable will be equal to one for those counties for those months with the zero cash bail policy in place.

The implementation of this policy allows the use of a two-way, fixed-effects model (hereafter TWFE). We use a variety of crime measurements as the dependent variable. The econometric model we estimate also includes the full set of county fixed effects, κ_c, and month-by-year fixed effects, $\tau_{m,y}$. Specifically, we estimate

$$Crime_{c,m,y} = \beta_1 ZeroBail_{c,m,y} + \kappa_c + \tau_{m,y} + \epsilon_{c,m,y}. \tag{3.1}$$

Standard errors are clustered at the county level.

In addition, since the zero cash bail policy was mandated for every county in the state for three months (April - June 2020), those three month-by-year indicators will fully absorb any effect arising in those months. To appreciate the full impact of the policy, we replace the month-by-year fixed effects with both the twelve month-of-year fixed effects and year fixed effects. Rather, to ensure the robustness of our results, we will also estimate

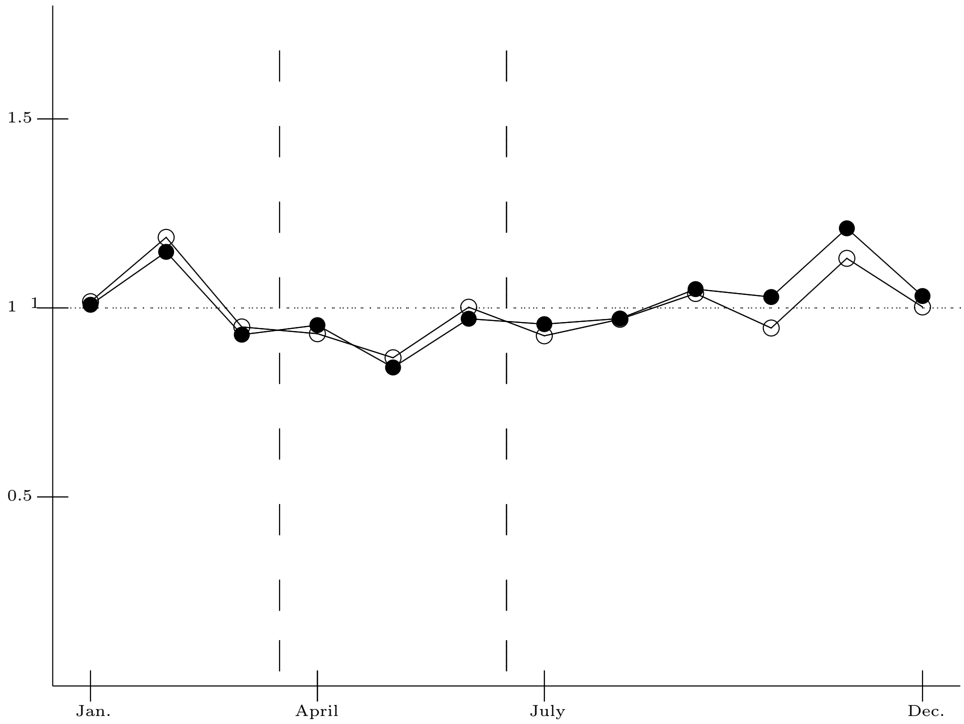
$$Crime_{c,m,y} = \beta_2 ZeroBail_{c,m,y} + \kappa_c + \mu_m + \theta_y + \epsilon_{c,m,y}. \tag{3.2}$$

Eliminating cash bail reduces deterrence promoting crime when $\widehat{\beta}_1, \widehat{\beta}_2 > 0$.

Causal identification fails if the crime rate in the counties that re-instituted cash bail differ from the counties that did not prior to the policy change. If they are trending differently, then estimated difference after the policy changed could be driven simply by the divergent trends rather than by the policy itself.

To assess this, we first consider total violent crime. Analyzing the raw data, we partition those counties that re-instated cash bail as soon as the statewide mandate ended from those counties who chose to maintain the policy into 2021 or 2022. We aggregate the total amount of violent crime for each subsample for each month in 2020. This exercise, then, does not consider the policy changes which occurred later in time. In total, 88% of the counties who re-instated cash bail did so in 2020. Thus, this exercise will capture the experience in most (but not all) jurisdictions. Further, each month's observation is normalized by that same month's value in the previous year (2019). Hence, a value greater than one indicates that crime in 2020 is greater than crime in 2019 for that same time of the year. Figure 3.3 graphically depicts the two time series.

Figure 3.3: Violent Crime in 2020

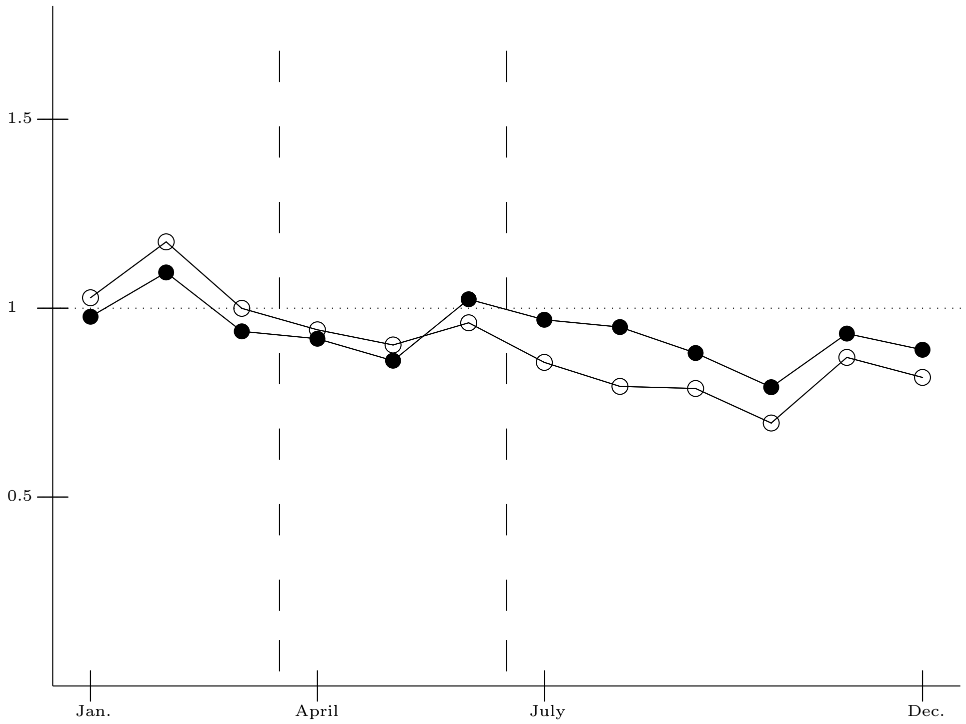


The two lines present the total amount of violent crime in California for each month of 2020. The counties are partitioned into those who kept the zero cash bail policy into 2021 and 2022 (line with filled-in circle markers) and those who re-instated cash bail in July 2020 (line with open-circle markers). The dotted lines are the period of statewide mandated zero cash bail. Each value is normalized by that month's value in 2019.

The subsample of counties who maintained the zero cash bail throughout the rest of 2020 is depicted in the line with the filled-in circle markers. The subsample of counties that re-instated cash bail is depicted with the line with the open-circle markers. As one can see, the two lines track quite closely for most of the year. Importantly, though, the two lines diverge after September 2020. Thus, violent crime was trending similarly in both the months prior to the pandemic (January - March 2020), and were trending similarly during the months of the pandemic's peak (April - June 2020).

Similarly, we consider property crime in 2020 again partitioning the counties by whether they re-instate cash bail that year. Figure 3.4 illustrates.

Figure 3.4: Property Crime in 2020



The two lines present the total amount of property crime in California for each month of 2020. The counties are partitioned into those who kept the zero cash bail policy into 2021 and 2022 (line with filled-in circle markers) and those who re-instated cash bail in July 2020 (line with open-circle markers). The dotted lines are the period of statewide mandated zero cash bail. Each value is normalized by that month's value in 2019.

A similar observation arises with property crime. Normalized by the 2019 values for each month to account for seasonal variation, the two lines co-move for both the three month period prior to the pandemic, and the three-month period making up the height of the pandemic. Only after the two subgroups begin to differ in criminal justice policy do we see a difference between the two lines. Hence, again, it seems that the time series are following common trends prior to cash bail being re-instated.[16]

While the two depictions of raw, post-treatment observations do not necessarily provide convincing evidence of a treatment effect, as it ignores 2021 data and those counties which re-instituted cash bail later in time, they do strongly suggest parallel trends prior to policy adoption. Thus, a TWFE specification can be argued to produce causal estimates.

3.3 Results

3.3.1 TWFE

We estimate the two-way, fixed-effect models as specified previously. The dependent variable is the number of crimes (either violent or property) for each county in each month. The treatment variable is equal to one if that county has zero cash bail in that month. All counties take values equal to one for April, May, and June of 2020, and take values of zero for months prior to April 2020. The counties continuing with the zero cash bail policy after the statewide mandate expired have values equal to one for their observations until the policy was removed. Table 3.1 presents the results.

The first four columns consider the 2015 to 2021 time window, while the second four columns focus in on the 2018 to 2021 time period. Regardless, a consistent result emerges. The elimination of cash bail does not have a statistically significant effect on property crime, but does lead to an increase in violent crime. Using the estimated coefficient in column (5), eliminating cash bail increases violent crime by just less than $\frac{1}{20}^{th}$ of a standard deviation. Thus, the effect is statistically different from zero but modest in size.

One should expect that the counties who maintained the zero cash bail policy after the statewide mandate was relaxed are systematically different. Importantly, they are more heavily-populated counties. Using annual American Community Survey data, the average

[16] Also, if we estimate a stacked difference-in-difference event study of the 6 months prior and four months after (the maximum post-period for the most recent policy-adopting county) for all counties adjusting cash bail (not just those who do so in 2020), then each point estimate in the pre-treatment periods are statistically indistinguishable from zero and a joint test of significance also fails to find a discrepancy in the pre-treatment periods.

Table 3.1: TWFE Results

	2015-21				*2018-21*			
	Violent (1)	Property (2)	Violent (3)	Property (4)	Violent (5)	Property (6)	Violent (7)	Property (8)
Zero Bail	33.555 ***	-152.380	20.620 **	-106.928 *	28.287 ***	-70.708	16.055 **	-43.845
	(12.635)	(90.852)	(8.101)	(62.111)	(9.825)	(57.308)	(6.128)	(38.714)
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	No	No	Yes	Yes	No	No
Month of Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
R^2	0.991	0.992	0.990	0.991	0.992	0.992	0.992	0.992
AIC	54206.2	68146.0	54188.5	68206.9	30527.3	38482.4	30508.0	38529.4
DV μ	251.923	1347.895	251.923	1347.895	254.133	1276.928	254.133	1276.928
DV σ	641.016	2846.392	641.016	2846.392	648.347	2733.567	648.347	2733.567
N	4872	4872	4872	4872	2784	2784	2784	2784

The dependent variable is the total number of reported crimes of that type for each month for all agencies located within a county. While all specifications include county fixed effects, they differ in whether they include month-by-year fixed effects or separately include month-of-year and year fixed effects. Standard errors clustered at the county level are presented in parentheses (58 clusters); *** 1%, ** 5%, * 10% level of significance.

2020 population is more than 1.34 million per county for those to maintained the zero cash bail policy, while those who immediately re-instate cash bail have an average population of 276,365.[17]

We consider two adjustments to the econometric method to assess whether population differences matter for our primary result. First, we estimate a weighted least squares regression to account for heteroskedasticity that can be expected to arise with observations coming from widely-varying population sizes. Second, we transform the dependent variables to measure the number of crimes per 100,000 residents. The annual population levels for each county, as recorded in the American Community Survey, are used in the normalization. Estimating the weighted least squares model does require a coarsening of the time fixed effects though. Table 3.2 presents the results.

Violent crime continues to show an increase when the zero cash bail policy is in effect. Using the estimate in column (3), violent crime increases by just less than 0.07 standard deviations. Accounting for differences in county-level population flips the sign on the effect of property crimes and gains statistical significance. This suggests that, indeed, the elimination of cash bail may have increased property crime. Using column (4), this

[17]Those who maintained the zero cash bail policy also had larger proportions of their population as non-White, had higher median household incomes, and had greater labor force participation rates.

Table 3.2: Accounting for Population Differences

	2015-21		*2018-21*	
	Violent	Property	Violent	Property
	(1)	(2)	(3)	(4)
Zero Bail	2.117 ***	8.386 ***	1.594 ***	8.066 ***
	(0.203))	(0.896)	(0.212)	(0.928)
County Fixed Effects?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes
χ^2	42550.9 ***	43496.0 ***	27542.3 ***	21882.9 ***
DV μ	36.998	180.464	36.970	166.015
DV σ	22.585	76.285	23.800	72.715
N	4872	4872	2784	2784

The dependent variable is the total number of reported crimes of that type for each month for all agencies located within a county per 100,000 residents. Each specification includes county and year fixed effects; *** 1%, ** 5%, * 10% level of significance.

represents a 0.11 standard deviation increase.

A variety of other sensitivity checks are conducted to assess the robustness of our result. Rather than use variance weighting in the regression, we instead estimate the primary specification with a population weighting.[18] Doing this, the coefficient estimates for violent crime expands. For example, in column (5) of Table 3.1 it grows to 80.909 and retains its significance at the 1% level. The coefficient estimates in columns (6) and (8) fall closer to zero and remain highly statistically insignificant. Similar adjustments occur with the extended time from (i.e., 2015-21). Thus, the main results are unaffected. In addition, right-skew to the total volume of crime, caused by the large variation in population, can be addressed by log transforming the dependent variable.[19] This main message is unaffected with this transformation. The estimated effects of zero cash bail is to increase violent crime and property crime by 0.07 and 0.04 standard deviations, respectively (using our primary specifications in columns (5) and (6) of Table 3.1). Finally, we engage in a leave-one-out process. Within it, we eliminate all observations from one county and re-estimate our primary result. Iterating this process for each of the 58 counties allows us to assess how sensitive our result is to crime and policy in any one particular jurisdiction. Doing so, we find little difference in the coefficient estimate. Each of the 58 regressions

[18] This allow us, for example, to continue using clustered standard errors and the full month-by-year temporal fixed effects.

[19] Since a small number of observations take zero values, we consider the transformation $Crime' = log(Crime + 1)$.

produces an estimated effect that positive and statistically different from zero at the 5% level.[20] Thus, the magnitude of the effect is insensitive to specification. Going forward we will use the 2018-21 time window with the dependent variable in levels as the baseline, preferred specification.

3.3.2 Clearance Rates

One concern is that law enforcement may be disincentivized to make arrests if they perceive that the accused will not be incarcerated. If law enforcement reduces enforcement, then a rational potential law-breaker may choose to engage in crime not because of the policy directly, but because of the policy's indirect effect on policing incentives.

To address this, we use the crime's clearance rate as the dependent variable. That is, we consider the proportion of reported crimes that result in an arrest as the outcome variable.[21] Table 3.3 presents the results.

Table 3.3: Clearance Rates

	2018-21			
	Violent	Property	Violent	Property
	(1)	(2)	(3)	(4)
Zero Bail	0.0010	0.0058	-0.0141	-0.0022
	(0.0282)	(0.0125)	(0.0186)	(0.0080)
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	No	No
Month of Year Fixed Effects?	No	No	Yes	Yes
Year Fixed Effects?	No	No	Yes	Yes
R^2	0.360	0.462	0.351	0.452
AIC	-1913.4	-5881.4	-1940.7	-5896.3
DV μ	0.5037	0.1376	0.5037	0.1376
DV σ	0.2091	0.1119	0.2091	0.1119
N	2716	2766	2716	2766

The dependent variable is the total number of reported crimes (either violent crimes or property crimes) for each month for all agencies located within a county. Standard errors clustered at the county level are presented in parentheses (58 clusters); *** 1%, ** 5%, * 10% level of significance.

[20]For example, using the specification in column (1) of Table 3.1, while the average treatment effect is 0.052 standard deviations, the leave-one-out process produces effects ranging from 0.042 to 0.057 standard deviations. Thus, the estimated effect is quite stable.

[21]While rare, if a county in a month does not report a value for a particular crime type, then that observation is removed. Further, as we are using aggregated data, it is possible that some crimes are reported in one year and cleared in the next. Thus, our clearance rate variable is a noisy measurement of policy effectiveness.

Interestingly, there is not a statistically significant change in crime's clearance rate with cash bail. This suggests that the policy did not necessarily erode law enforcement's incentives to make arrests. It is that the volume of violent crime increased when cash bail was eliminated that is driving our result.

3.3.3 Crime Subcategories

Up to this point, we have only considered the total number of crimes reported. There is no reason to believe that every type of crime will be affected similarly. Here, we break down violent crime into homicides, rapes, robberies, and assaults. Property crime is separated into burglaries, auto thefts, and other larcenies. Table 3.4 presents the results.

Table 3.4: Crime Subcategories

	Violent Crime				*Property Crime*		
	Homicide	Rape	Robbery	Assault	Burglary	Theft	Auto
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Zero Bail	1.718	-1.821	-17.553	45.937 ***	-28.378	-127.530	85.201
	(1.057)	(1.553)	(12.029)	(19.896)	(24.231)	(77.656)	(61.534)
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.909	0.976	0.982	0.984	0.983	0.986	0.963
AIC	12781.2	19158.0	26340.5	29882.5	30765.8	37794.0	34074.3
DV μ	2.792	20.420	73.658	155.099	238.056	871.803	238.037
DV σ	7.495	47.630	210.501	383.027	501.541	1811.603	560.380

Data covers the 2018-21 time period; $N = 2784$. The dependent variable is the total number of reported crimes of that type for each month for all agencies located within a county. While all specifications include county fixed effects, they differ in whether they include month-by-year fixed effects or separately include month of year and year fixed effects. Standard errors clustered at the county level are presented in parentheses (58 clusters); *** 1%, ** 5%, * 10% level of significance.

The fact that the violent crime effect is concentrated in assaults makes sense. California's zero bail policy excluded violent felonies. California's penal code includes murder, rape, and robbery in this category and, hence, the policy would not apply to them. Only serious assaults such as assault with a deadly weapon are included in this list of exceptions. That the elimination of cash bail only affects assaults is consistent with our hypothesis that this policy incentivizes crime. The failure to affect property crime is consistent across its subcategories.

3.3.4 Mandatory or Voluntary?

As with any policy, it is reasonable to ask whether all treated units are affected similarly. For this policy experiment, there is one important heterogeneity of interest. The initial mandate applied to all counties within a state. After it ended, counties were free to continue with a zero cash bail. Here, we ask whether the effect differs between the period where the policy was mandatory or whether it was more impactful when the policy was optional.

Table 3.5: Mandatory or Voluntary?

	2018-21	
	Violent	Property
	(1)	(2)
Statewide Mandate	-5.023	57.202 **
	(4.251)	(25.703)
County Extension	26.262 ***	-92.776
	(9.082)	(55.698)
County Fixed Effects?	Yes	Yes
Month of Year Fixed Effects?	Yes	Yes
Year Fixed Effects?	Yes	Yes
R^2	0.992	0.992
AIC	30488.1	38503.1

Data covers the 2018-21 time period; $N = 2784$. The dependent variable is the total number of reported crimes (either violent crimes or property crimes) for each month for all agencies located within a county. Standard errors clustered at the county level are presented in parentheses (58 clusters); *** 1%, ** 5%, * 10% level of significance.

The positive change in violent crime occurs during the period of voluntary elimination of cash bail. This is consistent with the narrative that it reduced deterrence. The positive and statistically significant effect of the mandatory period on property crime should be interpreted with caution. As all counties had the policy in place during this time, this coefficient is essentially a time indicator variable. Property crimes increased during the height of the pandemic (relative to the pre-pandemic levels and the post-pandemic levels when cash bail was re-installed). This could be from cash bail, or it could be coming from any number of other policy variables changing at this time.

3.4 Conclusion

This paper examines the impact of eliminating cash bail on crime rates in California, taking advantage of a Covid-19-related policy change. Initially, a statewide mandate abolished cash bail for misdemeanors and non-violent felonies, but later individual counties were given the freedom to decide whether to continue with the policy. This created variation in the re-implementation of cash bail which we exploit. We find that the elimination of cash bail leads to a modest increase in violent crime but has no effect on property crime. we also demonstrate that the policy does not affect law enforcement's clearance rate, suggesting that the rise in violent crime is likely due to a weakening of deterrence. Additionally, the increase in violent crime primarily occurs in assaults rather than more serious felonies, as the latter were exempt from the zero cash bail policy.

As the elimination of cash bail is a central platform in the progressive prosecution's platform, our work provides early, important information on its effect. We are able to do this by exploiting a novel policy. The primary limitation to the applicability of our findings is that we study Covid-era crime, which may not replicate criminal activity outside of pandemic. Further, the policy we study was a transitory intervention by the judiciary and, therefore, we are unsure whether similar results would arise with either permanent implementations or prosecutor office driven policy. Nevertheless, we expect our results will help shed light on this policy's consequences.

3.5 References

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3.6 Appendix

Additional results worth considering are reported here.

3.6.1 Robustness

We present a number of additional sensitivity checks here.

Table 3.6: Normalization

	2015-21				*2018-21*			
	Violent	Property	Violent	Property	Violent	Property	Violent	Property
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zero Bail	4.040	6.971	2.370	4.147	3.108	5.448	1.609	4.022
	(3.169)	(8.224)	(2.076)	(5.977)	(2.485)	(6.557)	(1.632)	(4.592)
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	No	No	Yes	Yes	No	No
Month of Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
R^2	0.416	0.720	0.409	0.713	0.459	0.694	0.452	0.686
AIC	4696.9	49977.8	41670.4	50009.3	23936.4	28565.3	23901.5	28571.5
DV μ	36.998	180.464	36.998	180.464	36.970	166.015	36.970	166.015
DV σ	22.585	76.285	22.585	76.285	23.800	72.715	23.800	72.715
N	4872	4872	4872	4872	2784	2784	2784	2784

Replication of Table 3.1 except the dependent variable is normalized to measure the number of crimes per 100,000 residents.

Table 3.7: Log Transformation

	2015-21				2018-21			
	Violent (1)	Property (2)	Violent (3)	Property (4)	Violent (5)	Property (6)	Violent (7)	Property (8)
Zero Bail	0.1289	0.0879	0.0850	.0590513	0.1164	0.0711	0.0786	0.0509
	(0.0908)	(0.0577)	(0.0659)	(0.0430)	(0.0844)	(0.0512)	(0.0614)	(0.0379)
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	No	No	Yes	Yes	No	No
Month of Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
R^2	0.962	0.985	0.962	0.985	0.958	0.984	0.957	0.984
AIC	3652.8	209.6	3621.7	227.6	2439.7	347.4	2396.1	341.3
DV μ	4.096	5.671	4.096	5.671	4.090	5.592	4.090	5.592
DV σ	1.783	2.001	1.783	2.001	1.788	2.011	1.788	2.011
N	4872	4872	4872	4872	2784	2784	2784	2784

Replication of Table 3.1 except the dependent variable is log transformed. To adjust for the (rare) zero values, the transformation is $Crime' = log(Crime + 1)$.

Table 3.8: Adding Controls

	2015-21				2018-21			
	Violent (1)	Property (2)	Violent (3)	Property (4)	Violent (5)	Property (6)	Violent (7)	Property (8)
Zero Bail	26.617 **	-104.427	15.318 **	-71.717	27.599 **	-59.890	15.072 **	-35.329
	(11.807)	(94.421)	(7.246)	(63.007)	(11.524)	(56.142)	(6.848)	(37.205)
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	No	No	Yes	Yes	No	No
Month of Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.991	0.992	0.991	0.992	0.992	0.992	0.992	0.992
AIC	54060.7	67934.7	54046.2	68000.9	30469.2	38483.7	30449.9	38529.5
DV μ	251.923	1347.895	251.923	1347.895	254.133	1276.928	254.133	1276.928
DV σ	641.016	2846.392	641.016	2846.392	648.347	2733.567	648.347	2733.567
N	4872	4872	4872	4872	2784	2784	2784	2784

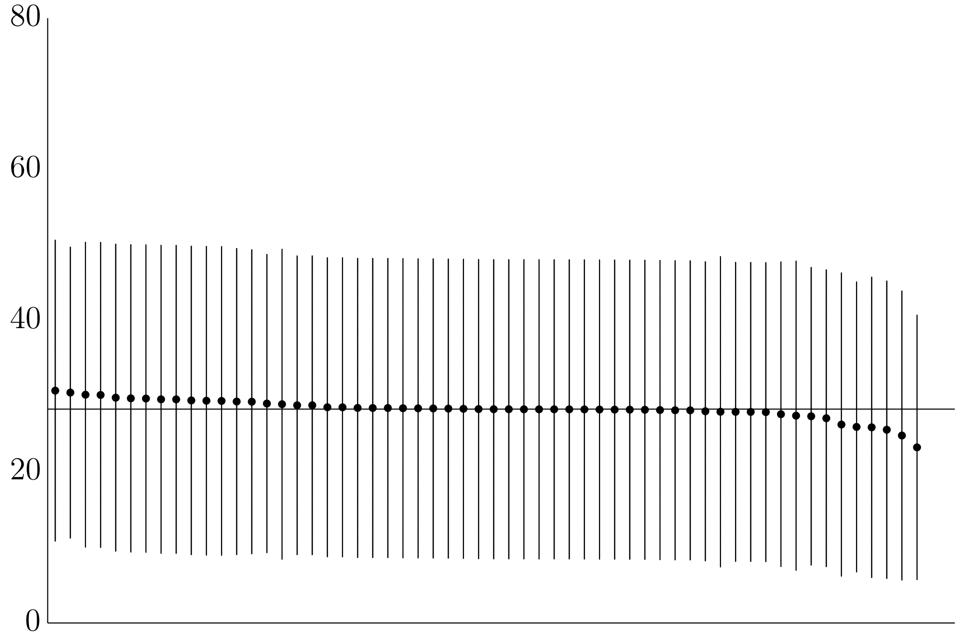
Replication of Table 3.1 except the control variables are included in each specification. The control variables used are the county's population, median household income, unemployment rate, labor force participation rate, and the proportion of the county's population that is nonwhite and the proportion that is of two or more races.

Table 3.9: Population Weighted Regression

	2015-21				2018-21			
	Violent (1)	Property (2)	Violent (3)	Property (4)	Violent (5)	Property (6)	Violent (7)	Property (8)
Zero Bail	98.744 ***	-326.649	52.994 ***	-270.599	80.909 ***	-45.693	40.066 ***	-68.288
	(30.564)	(284.280)	(13.762)	(207.964)	(23.034)	(140.024)	(10.596)	(121.514)
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	No	No	Yes	Yes	No	No
Month of Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
Year Fixed Effects?	No	No	Yes	Yes	No	No	Yes	Yes
R^2	0.991	0.994	0.991	0.993	0.993	0.995	0.993	0.993
AIC	64107.6	76131.8	64321.6	76671.9	35961.6	42884.8	36112.1	43346.7
DV μ	251.923	1347.895	251.923	1347.895	254.133	1276.928	254.133	1276.928
DV σ	641.016	2846.392	641.016	2846.392	648.347	2733.567	648.347	2733.567
N	4872	4872	4872	4872	2784	2784	2784	2784

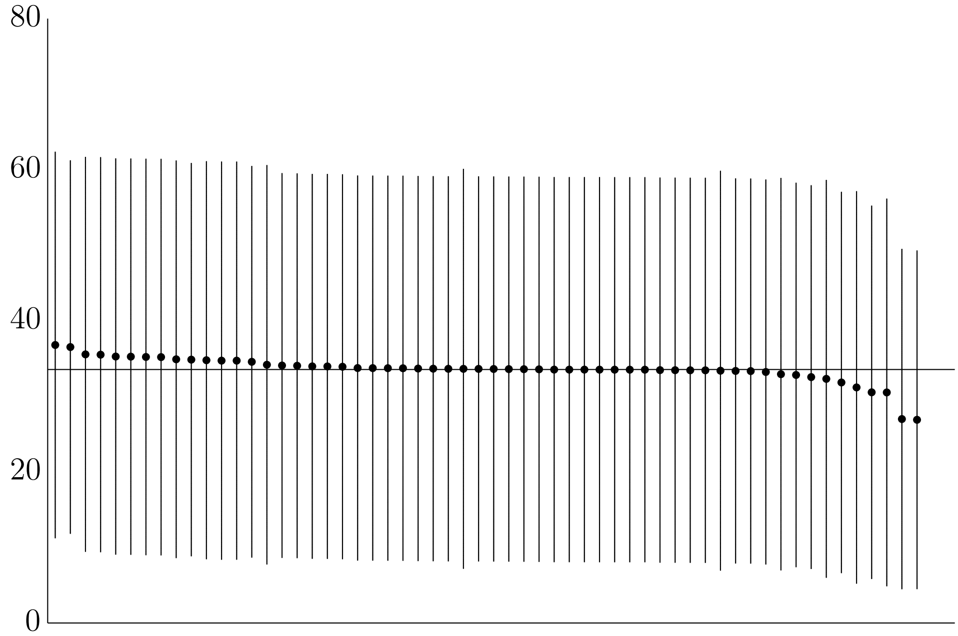
Replication of Table 3.1 except the regression is weighted by the each county's population for that year.

Figure 3.5: Leave-One-Out Analysis: Violent Crime 2018-21



Results from 58 separate regressions presented. The specification in column (1) of Table 3.1 is re-estimated. Each specification excludes all observations from one county in California. The coefficient estimate and the 95% confidence interval is depicted. Results are ordered in decreasing coefficient estimates. The first two counties eliminated (with the largest marginal effect) is Stanislaus and Kern counties The last two counties eliminated (with the smallest marginal effect) is San Bernardino and Los Angeles counties. The solid black horizontal line is the point estimate without dropping any county.

Figure 3.6: Leave-One-Out Analysis: Violent Crime 2015-21



Results from 58 separate regressions presented. The specification in column (4) of Table 3.1 is re-estimated. Each specification excludes all observations from one county in California. The coefficient estimate and the 95% confidence interval is depicted. Results are ordered in decreasing coefficient estimates. The first two counties eliminated (with the largest marginal effect) is Stanislaus and Kern counties The last two counties eliminated (with the smallest marginal effect) is San Bernardino and Los Angeles counties. The solid black horizontal line is the point estimate without dropping any county.