ABSTRACT

Dissertation Title: THE EFFECT OF FIREARM RELINQUISHMENT LAWS ON DOMESTIC GUN VIOLENCE

Thomas L Scott, Doctor of Philosophy, 2022

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Multiple studies have found that an abuser's access to firearms increases the likelihood that the abuser will use a firearm to shoot and kill a partner during an act of domestic abuse. This finding suggests that removing that access could be a promising method for preventing domestic gun violence. Although certain domestic abusers are prohibited from purchasing or possessing firearms under federal law, there is no mechanism for the courts and law enforcement to ensure that offenders get rid of any guns in their possession. This fact has led some states to enact gun relinquishment laws that define both a legal process for prohibited abusers to surrender any firearms in their possession and sanctions for not complying with the law. Evidence suggests that gun relinquishment laws are an effective method of preventing intimate partner homicide and may decrease the likelihood that domestic abusers are rearrested. This research is promising, but there are key gaps that remain in our understanding of the effectiveness of gun relinquishment laws for preventing gun violence. First, despite that nonfatal gun violence 1) occurs more frequently than fatal gun violence, 2) precedes fatal violence, and 3) results in substantial costs to victims, their families, and society, prior studies have focused on homicide rates as an outcome.

Second, gun relinquishment laws often extend to domestic relationships other than intimate partners, yet most studies focus on intimate partner violence. Third, because domestic abusers commit not-domestic forms of violence, research should address whether these laws prevent domestic and not-domestic forms of gun violence.

To address these gaps, in this dissertation I use crime victimization data from the National Incident-Based Reporting System (NIBRS) and the synthetic control method (SCM) to test for a relationship between gun relinquishment laws for domestic violence offenses and levels and characteristics of domestic and not-domestic gun violence. After identifying 17 states with adequate NIBRS coverage between 2005-14, I reviewed the laws in each state and determined that 2 states enacted gun relinquishment laws for domestic violence offenses during this time and could be evaluated: Iowa and Tennessee. Using the SCM, for both domestic and not-domestic violence, I test whether these states experienced a change in a) the rate of gun violence, b) the proportion of violent acts that involved a gun, or c) the lethality of severe assaults following their gun relinquishment law going into effect. The findings were often in the expected direction, though none were statistically significant. Although the lack of statistically significant findings could be a function of the study's design, the results show much uncertainty in the estimated relationships. In addition, supplemental analyses with greater statistical power support these results. Future research should replicate this dissertation's design as NIBRS data continue to improve and should pursue other study designs like individual-level analyses.

THE EFFECT OF FIREARM RELINQUISHMENT LAWS ON DOMESTIC GUN VIOLENCE

by

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Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2022

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1: INTRODUCTION

Intimate partner homicide (IPH) is a concerning public health issue, especially for women. In fact, over 1,000 women are murdered by their intimate partners each year, with a gender ratio of 3 women murdered to every 1 man murdered by an intimate partner (Fridel & Fox, 2019). Between 2010 and 2017, 44% of murdered women were killed by an intimate partner as opposed to 5% of murdered men (Fridel & Fox, 2019). Most women killed by an intimate partner are murdered with a firearm (Addington & Perumean-Chaney, 2014; Adhia, Kernic, Hemenway, Vavilala, & Rivara, 2019; Fridel & Fox, 2019). Among high-income countries, female firearm murder is a rare phenomenon, except in the U.S. In 2010, the ratio of the U.S. female firearm homicide rate to the female firearm homicide rate in 22 other high-income countries was 15.7. This ratio was greatest for women aged 15-24 at 37.6 (Grinshteyn & Hemenway, 2016). The fact that firearms contribute to the majority of IPHs makes firearm access an attractive target for prevention efforts.

Research has found that in intimate partner relationships with domestic violence (DV), firearm possession or access greatly increases the likelihood of IPH (Campbell, Messing, & Williams, 2017). For example, in an 11-city case-control study of female IPH victims and abused control women, researchers found that an abuser's access to a firearm increased a victim's likelihood of death by 860% (Campbell et al., 2003). In another study, researchers sampled all homicides that occurred in the homes of female victims in 3 metropolitan counties and then randomly selected matched control subjects. The researchers then interviewed a proxy for each homicide victim 3 to 6 weeks following the homicide and interviewed each control subject or their proxy. The study's analysis showed that the presence of one or more guns in the home increased the odds of homicide by 3.4, especially among the subsample of homicide victims killed by a spouse, lover, or close relative, where the odds ratio was 7.2 (Bailey, Kellerman, Somes, Banton, Rivara, & Rushforth, 1997).

In one recent study of 17.6 million California residents, researchers found that residents who became cohabitants of handgun owners during the study's observation period were over twice as likely to die of homicide and close to three times as likely to die of homicide by firearm compared to cohabitants of residents not owning a handgun (Studdert et al., 2022). Moreover, cohabitants who were intimate partners of the handgun owners were especially at risk. Specifically, spouses and intimate partners of handgun owners were 7.2 times as likely to die in their home from homicide by firearm compared to spouses and intimate partners of cohabitants who did not own a handgun during the study period. The figures for family members, friends, or acquaintances and strangers were 2.9 and 3.6, respectively (Studdert et al., 2022).

IPH and gun use in these homicides receive much academic attention due to the severity of murder and the availability of data on this crime type. Yet, many more women are threatened with and physically assaulted by a firearm than are killed by one each year, and an abuser's access to a firearm also increases the likelihood of nonfatal forms of intimate partner violence (IPV) (Sorenson & Schut, 2018; Sorenson & Wiebe, 2004). For example, based on data on IPV incidents from the National Incident-Based Reporting System (NIBRS) for the year 2008, Addington and Perumean-Chaney (2014) found that there were 20 times as many aggravated gun assaults as gun homicides. Wiebe (2003) analyzed records from patients who visited emergency departments for nonfatal firearm-related injuries between 1993 and 1999 and discovered that 2,500 women suffered a gunshot injury and an additional 1,300 women suffered a nonpenetrating gun-related injury from a current or former spouse. The figures for men were 1,900 and 200, respectively, making women nearly 4 times more likely than men to be nonfatally injured with a firearm by a current or former spouse compared to a stranger (Wiebe, 2003). Using data on police calls for service to the Philadelphia Police Department in 2013, Sorenson (2017) found that among all IPV incidents that involved a firearm, firearms were most often used to threaten victims (69.1%). Beyond threats, firearms were used to strike victims in 5.7% of cases, and victims were shot at in 9.9% of cases, with victims hit by a projectile in 3.0% of cases (Sorenson, 2017; see also Adhia, Lyons, Moe, Rowhani-Rahbar, & Rivara, 2021). These figures demonstrate that gun assaults occur much more frequently than gun homicides, yet relatively few studies examine nonfatal gun use in IPV (Sorenson & Schut, 2018).

In addition to its prevalence, the harm caused by nonfatal gun-involved IPV makes the issue an important topic of study. In her sample of IPV incidents known to the Philadelphia Police Department, Sorenson (2017) found that victims of IPV that involved a gun were visibly injured 17.2% of the time, shaking 33.9% of the time, and frightened 57.4% of the time. Using data from a survey of over 500 women who contacted the National Domestic Violence Hotline, Logan and Lynch (2018) found that IPV victims endure chronic fear and stress caused by the knowledge that their abuser could use his firearm to kill them or their children at any moment. Based on its frequency and the harm it causes, there is a need for greater academic attention to nonfatal uses of firearms in DV incidents.

Additionally, it is important to study IPH and IPV together because evidence suggests IPV often precedes IPH. In fact, prior IPV is the most common risk factor for IPH, being present in approximately 70% of IPH cases (Campbell, Glass, Sharps, Laughon, & Bloom, 2007). In Campbell et al.'s (2003) 11-city study of female IPH victims and matched controls, the authors found that an abuser's previous threats with a weapon and threats to kill increased the odds of IPH by 4.1 and 2.6, respectively. Additionally, an abuser's use of a gun in the worst incident of nonfatal abuse increased the odds of IPH by 41.4 (Campbell et al., 2003). In another study on stalking and IPH in 10 cities, McFarlane, Campbell, Wilt, Sachs, Ulrich, and Xu (1999) surveyed victim proxies to find that 67% of IPH victims were assaulted and 76% were stalked by an intimate partner in the year prior to their murder, while 71% of attempted IPH victims were assaulted and 85% were stalked by an intimate partner in the year prior to the attempted IPH. Therefore, a greater focus on preventing acts of nonfatal IPV that involve a firearm should reduce the likelihood and associated harms of both these acts and acts of IPH.

Like with IPH, the relatively few studies on nonfatal IPV have demonstrated a relationship between an abuser's access to firearms and the likelihood that a victim is nonfatally assaulted with a gun. Studies of battered women (Logan & Lynch, 2018; Lynch & Logan, 2018; McFarlane, Soeken, Campbell, Parker, Reel, & Silva, 1998; Sorenson & Wiebe, 2004) and male batterers (Rothman, Hemenway, Miller, & Azrael, 2005) have found that an abuser's ownership of or access to firearms increases the severity of IPV, including the likelihood that a gun is used to threaten one's partner.

Putting these findings together provides evidence for a potentially powerful IPV prevention strategy. First, the finding that nonfatal IPV typically precedes fatal IPV suggests that all forms of IPV share a common etiology and that prevention efforts aimed at reducing IPV should reduce both fatal and nonfatal forms. Second, the findings that 1) offenders commit the majority of IPHs using a firearm, 2) abusers use firearms much more often to threaten and assault victims than to kill them, and 3) an abuser's access to firearms increases the likelihood that an abuser will use a firearm to both assault and kill an intimate partner imply that removing an abuser's access to firearms will decrease the proportion of DV incidents that result in fatal and nonfatal gun violence (GV).

In addition to decreasing the prevalence of domestic gun violence (DGV), removing abusers' access to guns may decrease the frequency of not-domestic GV. Evidence on how often criminal offenders specialize in a single type of crime like robbery, as opposed to engaging in multiple types of crime such as robbery, drug distribution, assault, and burglary is mixed (Deane, Armstrong, & Felson, 2005; Osgood & Schreck, 2007; Piquero, 2000; Sullivan, McGloin, Ray, & Caudy, 2009). However, research on crime specialization among domestic abusers comes to the more consistent conclusion that abusers often commit acts of both DV and not-domestic violence over the life course (Bennett, Stoops, Call, & Flett, 2007; Bouffard & Zedaker, 2016; Klein & Tobin, 2008; Moffitt, Krueger, Caspi, & Fagan, 2000). Therefore, by restricting gun access among this group, it is likely that communities will experience a decline in rates of both DGV and not-domestic GV.

Multiple interventions exist that are aimed at restricting firearm access among DV offenders. This dissertation tests how effective gun relinquishment laws are at preventing DGV and not-domestic GV. Before describing these laws and the mechanisms that could link them to a reduction in GV, I explain how the prevalence and costs of DV create a dire need for effective interventions to reduce DV in the U.S. After providing evidence to show that DV, which includes both IPV and family violence (FV)[1], is a serious public health issue in Chapter 2, I describe gun relinquishment laws in greater detail in Chapter 3, including how they differ between states. In Chapter 4, I specify the theoretical mechanisms linking gun relinquishment laws to a reduction in state rates of GV. Then, I summarize empirical evidence on the effectiveness of gun relinquishment laws in reducing the frequency of GV in Chapter 5. I review

[1] I define FV as violence that occurs between current or former blood relatives or relatives-in-law, including children, parents, siblings, grandparents, stepfamily members, or other relatives.

my methods in Chapter 6 before presenting my main results in Chapter 7. I provide two sets of supplemental analyses in Chapters 8 and 9 that address some of the limitations of my primary analyses. In Chapter 10, I review this dissertation's findings in the context of its data and analytic limitations and discuss ways to advance the evidence base on the impact of gun relinquishment laws for DV offenses on the prevalence and characteristics of domestic and not-domestic GV.

CHAPTER 2: PREVALENCE AND COSTS OF DOMESTIC VIOLENCE

Both the prevalence and the cost to victims and society make DV a major public health issue. Nationally representative surveys show that DV occurs at an alarming rate in the U.S., with women being disproportionately affected by these violent acts. For example, using data from the National Crime Victimization Survey (NCVS) and defining DV as nonfatal violent acts committed by current or former spouses, dating partners, immediate family members, and other relatives, Truman and Morgan (2014) report that between 2003 and 2012, an average of 21% of all nonfatal violent victimizations in the U.S. were DV victimizations. Of the 1,411,330 annual DV offenses, the victim and offender were most often intimate partners (69%). Females accounted for 76% of DV victims, and guns were used in 4% of nonfatal DV victimizations (Truman & Morgan, 2014). In 2012, IPV occurred 2.5 times as often as FV, at a rate of 3.2 per 1,000 persons aged 12 or older compared to a rate of 1.3 per 1,000 persons aged 12 or older (Truman & Morgan, 2014). According to the NCVS, in 2012 the prevalence of DV among the general population was 5.0 per 1,000 persons aged 12 and older, and the prevalence of serious IPV including rape, sexual assault, robbery, and aggravated assault was 1.0 (Truman & Morgan, 2014). Importantly, these estimates represent the prevalence of these crimes in one year. Two large surveys described below provide lifetime prevalence estimates of nonfatal IPV. Each show a staggering commonness of IPV in the U.S. Notably, prevalence rates vary according to the source of data and how the target population is defined.

According to the National Intimate Partner and Sexual Violence Survey, in 2015, 21% of women and 15% of men reported experiencing severe physical violence and 18% of women and 8% of men reported experiencing contact sexual violence that was committed by an intimate partner (Smith, Zhang, Basile, Merrick, Wang, et al., 2018). These percentages resemble the percentages found in the same survey conducted from 2010 to 2012 (Smith, Chen, Basile, Gilbert, Merrick, et al., 2017). The earlier surveys also revealed that the negative impacts of IPV-related violence or stalking are greater for women than men: women were 2 times more likely than men to report a negative impact to their physical or mental health or financial wellbeing from IPV (73% to 36%, respectively). Both sexes reported the 3 most common impacts as being fearful, being concerned for their safety, and experiencing PTSD symptoms (Smith et al., 2017).

Although more dated, findings from the National Violence Against Women Survey provide more information on the nature of IPV (Tjaden & Thoennes, 2000). Of the 8,000 women and 8,000 men surveyed, 22% of women reported being physically assaulted by an intimate partner in their lifetime compared to 7% of men. For rape and/or physical assault, these percentages increased to 25% for women and 8% for men. This survey provided details about the type of assaults that occurred among intimate partners over the life course. Among the women sampled, most physical assaults involved being grabbed or pushed, slapped or hit, beat up, kicked or bit, and having hair pulled. Assaults with a gun were rarer—4% of women were threatened with a gun while 1% were assaulted with a gun. Compared to men, women were more likely to experience every type of assault except for having a gun or knife used against them ($p < 0.001$) (Tjaden & Thoennes, 2000). Disturbingly, a recent national community survey found that approximately 20% of adult nonfatal firearm abuse victims were threatened with a firearm by more than one romantic partner (Adhia, et al., 2021).

Regarding fatal violence, IPV contributed to 22% of the homicides that occurred in 27 states in 2015 where the victim-suspect relationship was known (Jack, Petrosky, Lyons, Blair, Ertl, et al., 2018). The population rate of IPH was 0.5 per 100,000 persons during that year. IPV contributed to a greater proportion of homicides of female victims (51%) than male victims (8%), and the population rate of IPH was over twice as high for females (0.7 per 100,000) compared to males (0.3 per 100,000). Family members were responsible for fewer homicides—parents killed their children in 5% of homicides, children killed their parents in 7% of homicides, and a relative killed a relative in 7% of homicides (Jack et al., 2018).

Of all fatal and nonfatal IPV and FV incidents in the US each year, there are corollary victims who are not the initial target of the DV-related attack but are harmed because of the incident. These victims can be children who intervene in an IPV-related incident to protect their mother and are subsequently assaulted or roommates who are killed simply because they are in the home during the commission of a femicide of familicide. Jack and colleagues (2018) reported that in 2015, corollary victims of IPV constituted 3% of all homicides with a known victim-suspect relationship, and 60% of these victims were males. Using the same dataset but for 16 states and the years 2003-2009, Smith, Fowler, and Niolon (2014) found that corollary victims represented 20% of all IPV-related murders. Of this group, the victims were family members (49%), new intimate partners (27%), friends or acquaintances (20%), strangers (4%), and police officers (1%). One quarter of all corollary victims and almost one half of corollary victims who were family members of the killer were under the age of 18 (Smith et al., 2014). Interventions to reduce domestic homicide should consider these victims as well.

The prevalence of fatal and nonfatal DV in the U.S. results in great financial costs. The National Center for Injury Prevention and Control (NCIPC) of the Centers for Disease Control and Prevention (CDC) estimated the financial cost of IPV-related injuries and deaths among women ages 18 and older in the U.S. for the year 1995. Based on data from a national survey, the study found that of the nearly 2.0 million IPV-related injuries, more than 0.5 million required medical attention. In total, IPV resulted in 18.5 million mental health care visits, and women lost 8.0 million days of paid work, and 5.6 million days of lost household chores. Additionally, roughly 1,252 women died from IPV, which led to a loss of $893 million in lifetime earnings. The 95% confidence interval for the study's total cost estimate of IPV among adult women in 1995 was $3.9 billion to $7.6 billion (NCIPC, 2003). There is reason to believe that this is an underestimate since the total does not include many types of costs like those related to criminal justice system expenditures, physical and mental health consequences, and the effects of IPV on victims' family members and friends.

In a 2012 study of the financial costs of IPV for both female and male adults, researchers estimated a national cost of $3.6 trillion (Peterson, Kearns, McIntosh, Estefan, Nicolaidis, et al., 2018). This study accounted for criminal justice expenditures but is likely an overestimate based on how the authors attributed costs to IPV, which discounted preexisting differences among IPV victims compared to other residents. It is challenging to estimate accurate financial costs of IPV incidents, but even conservative estimates of $713,000 per fatality and $800 in medical and mental health costs per nonfatal physical assault (NCIPC, 2003) demonstrate the large economic cost of IPV in the U.S. The addition of costs generated from assaults to family members and corollary victims adds to the immediate need for effective interventions to reduce the prevalence of DV in the U.S.

Although all DV incidents are abhorrent, DV takes many forms that vary by severity, and that severity is linked to the individual and societal costs of DV. One can think of DV incidents as falling on a severity continuum with non-physical and non-sexual abuse constituting the least severe end and murder constituting the most severe end. In the middle there are different forms of abuse with varying levels of severity. The National Survey of Crime Severity provided evidence that U.S. residents do think criminal acts vary in severity (Wolfgang, Figlio, Tracy, & Singer, 1985). Wolfgang and colleagues showed, for example, that U.S. residents rate a man stabbing his wife to death as being twice as severe as a man beating his wife with his fists to the point that she requires hospitalization, which was rated as twelve times more severe as a person intentionally shoving or pushing a victim where no medical treatment is required. Based on her research and practice addressing DV, Jacquelyn Campbell developed the widely-adopted Danger Assessment, which ranks the severity of physical domestic violence according to the following scale: 1. slapping, pushing; no injuries and/or lasting pain; 2. punching, kicking; bruises, cuts, and/or lasting pain; 3. “beating up”; severe contusions, burns, broken bones; 4. threat to use weapon; head injury, internal injury, permanent injury; 5. use of weapon; wounds from weapon (Campbell, et al., 2003b). The Danger Assessment is empirically validated and provides evidence that an increase in the severity of abuse is directly linked to the likelihood that a victim is killed by their partner (Campbell, Webster, & Glass, 2009).

Within each type of domestic abuse, there can be threats of abuse or committed abuse, with committed abuse typically being more severe than threatened abuse. The type of weapon also varies in severity, with a firearm being the most severe type of weapon given its greater risk of lethality compared to other weapon types.[2] Finally, the type of injury incurred by the victim is related to the severity of abuse, with fatal and major injuries being more severe than no or minimal injuries. This dissertation is focused on understanding how to prevent the most severe forms of DV, which include threatened and committed fatal and nonfatal violent acts with a firearm.

Based on the research reviewed so far showing an association between offender gun access and both the likelihood and lethality of DGV, it seems likely that a reduction in the availability of guns to DV abusers would cause a reduction in both the frequency and severity of DGV and possibly also not-domestic GV. Gun relinquishment laws appear to be the best mechanism currently available for reducing the number of abusers who have access to firearms.

CHAPTER 3: GUN RELINQUISHMENT LAWS FOR DOMESTIC VIOLENCE OFFENSES

Gun relinquishment laws, also called gun surrender laws, are state laws that are intended to reduce the prevalence of GV by expanding the criminal justice system's ability to enforce existing Federal, state, and local gun laws.[3] Although a few states apply these laws to a broad set of criminal offenses, most states with gun relinquishment laws apply these laws to one of two types of domestic abusers—persons with a DV misdemeanor conviction (DVMC) and DV restraining order (DVRO) respondents, which are also called protective orders. Gun relinquishment laws for a DV offense require DV offenders—either convicted misdemeanants or restraining order respondents—to relinquish (surrender) any firearms in their possession. Although Federal law prohibits certain DV offenders from purchasing or possessing firearms,

[2] For example, firearms are used in approximately 70% of all homicides (Jack et al., 2018; Planty & Truman, 2013)

[3] These laws could also prevent DV not involving a gun, but most of their effect is likely to be on GV for reasons discussed in Chapter 4.

without state gun relinquishment laws in place, law enforcement agencies (LEAs) have limited authority to search for and confiscate guns possessed by prohibited DV offenders. Specifically, the Violent Crime Control and Law Enforcement Act of 1994 prohibited persons subject to certain DVROs[4] from purchasing or possessing a firearm, and, in 1996, the Lautenberg Amendment to the Gun Control Act of 1968 prohibited DV misdemeanants from purchasing or possessing a firearm (Cook & Goss, 2014). State gun relinquishment laws extend these Federal laws by 1) creating legal procedures for the removal of firearms from prohibited persons, in this case DVRO respondents and DV misdemeanants, and 2) instructing courts to inform prohibited offenders of a) their firearm prohibition, b) the procedures for firearm relinquishment, and c) the penalties for failing to relinquish all firearms in their possession. These laws also give courts the authority to issue warrants authorizing law enforcement to retrieve any firearms not surrendered and to punish violators of the relinquishment order with additional sanctions, such as contempt of court. In doing so, gun relinquishment laws fill a gap in the enforcement of Federal and state prohibitions on firearm possession.

To demonstrate what a gun relinquishment law for DV offenses looks like, I use elements of Rhode Island's General Law § 11-47-5.3 – Surrender of firearms by persons convicted of domestic violence offenses – as an example:

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[4] Federal laws only prohibits the purchase or possession of a firearm by a DV restraining order respondent "who is subject to a court order that was issued after a hearing of which such person received notice and had the opportunity to participate in court that restrains such person from harassing, stalking, threatening, or otherwise placing in reasonable fear of bodily injury an intimate partner or child of an intimate partner and that includes a finding that such person represents a credible threat to the physical safety of the intimate partner or child and by its terms explicitly prohibits the use, attempted use, or threatened use of physical force against the intimate partner or child that would reasonably be expected to cause bodily injury" (18 U.S. Code § 922(g)(8)).

(a) … the court shall issue an order declaring that the defendant surrender all firearm(s) owned by the defendant, or in the defendant's possession, care, custody, or control as described in this section.

(1) Surrender shall be made within twenty-four (24) hours of prohibition to a law-enforcement agency or to a federally licensed firearms dealer. …

(2) The defendant may transport their firearm(s) during the twenty-four hour (24) surrender period directly to the law-enforcement agency or federally licensed firearms dealer …

(3) The defendant shall, within forty-eight (48) hours after being served with the order, either:

(i) File a copy of proof of surrender with the court and attest that all firearm(s) owned by the defendant, or in the defendant's possession, care, custody, or control at the time of the plea or conviction, have been surrendered in accordance with this section and that the defendant currently owns no firearm(s) or has any firearm(s) in their care, custody, or control; or

(ii) Attest that, at the time of the plea or conviction, the defendant owned no firearm(s) and had no firearm(s) in their care, custody, or control, and that the defendant currently owns no firearm(s) and has no firearm(s) in their possession, care, custody, or control.

Importantly, although all state firearm relinquishment laws share a common framework like the example provided above, multiple details of these laws differ across states. Key differences center around 1) who the law applies to and 2) the extent of discretion that judges have in the application of the law. Other differences include the time limit given to offenders to surrender their firearms, the type of proof of surrender that is required by the court, the persons to whom offenders must surrender their firearms, the penalties for failure to surrender or lying about the surrender of all firearms, and more.

One of the most important differences across states that have firearm relinquishment laws for DV offenses has to do with the types of persons who are subject to the firearm relinquishment law. For firearm relinquishment laws that are specific to DV offenses, who is subject to the law often depends on the state's definition of DV. For example, California and Rhode Island both have firearm relinquishment laws in place for DVRO respondents. However, who qualifies for a DVRO differs across the states. California defines DV as abuse perpetrated against a current or former spouse, cohabitant, dating or engaged partner, parent of a shared child, a shared child, and any other person related by consanguinity or affinity within the second degree (California Family Code Section 6211; California Penal Code Section 13700). Rhode Island, on the other hand, defines DV as a set of crimes committed by "…spouses, former spouses, adult persons related by blood or marriage, adult persons who are presently residing together or who have resided together in the past three years, and persons who have a child in common regardless of whether they have been married or have lived together, or if persons who are or have been in a substantive dating or engagement relationship within the past one year…" (RI Gen L § 12-29-2). Differences in definitions are important because they limit who is protected by the law. In California, an individual who is assaulted by an ex-dating partner from five years prior is protected by a firearm relinquishment law, while in Rhode Island they are not.

Another way that firearm relinquishment laws differ by who the law applies to relates to the events or crimes that can subject a defendant to these laws. For example, Nevada requires the courts to order DV misdemeanants to surrender any firearms in their possession (Nev. Rev. Stat. Ann. § 202.361) but does not require the courts to order the surrender of firearms following the issuance of a protection (restraining) order (Nev. Rev. Stat. Ann. § 33.301). In contrast, in Massachusetts, the courts are required to order a protection order defendant to surrender his firearms if he is determined to pose an immediate danger of abuse (Mass. Gen. Laws Ch. 209A, § 3B). Additionally, within the class of firearm relinquishment laws that are specific to DVRO cases, firearm relinquishment laws can apply only to permanent DVROs or to permanent and temporary DVROs. When a victim of DV applies for a DVRO, the victim receives a court date, and the abuser is notified of the hearing. At the hearing, if a judge finds cause for the DVRO, the judge will issue a permanent DVRO that typically lasts 2 years. Importantly, the date that the victim files for a permanent DVRO and the date of the hearing could be weeks apart. Therefore, in some states, judges can issue a temporary (also called 'emergency' or 'ex parte') DVRO to protect the victim until the date of the hearing (Vittes & Sorenson, 2006). In a few states such as New Jersey, judges can order that all firearms be seized from a defendant following the issuance of a temporary DVRO (N.J. Stat. Ann. § 2C:25-28). In other states, judges can only include a firearm relinquishment order after issuing a permanent DVRO (Giffords Law Center to Prevent Gun Violence, n.d.).

Finally, state firearm relinquishment laws differ in the amount of discretion given to judges in deciding when to apply the firearm relinquishment order (Zeoli, Frattaroli, Roskam, & Herrera, 2019). Some laws require judges to order certain DV offenders to surrender their firearms, while other laws give judges the ability to order the surrender of firearms, but do not mandate that judges do so. In some states, such as New Hampshire, judges have discretion in ordering the relinquishment of firearms as part of a temporary DVRO (N.H. Rev. Stat. § 173-B:4) but are required to include a relinquishment order as part of a permanent DVRO (N.H. Rev. Stat. § 173-B:5). Even in states that attempt to limit judicial discretion, courts can vary in the degree to which they apply the law (Everytown for Gun Safety, 2019; Fleury-Steiner, Miller, & Carcirieri, 2017).

Differences in the persons to whom a firearm relinquishment law applies and the amount of discretion judges have in applying the law likely impact how effective a firearm relinquishment law is at preventing levels and characteristics of violent crime. In fact, the theoretical and practical considerations behind these laws predicts this based on the premise that removing guns and applying added costs for gun possession or use to a greater number of potential re-offenders will reduce the proportion of these individuals who decide to commit GV.[5] In the Methods Chapter, I examine these characteristics when identifying states that have enacted a gun relinquishment law.

CHAPTER 4: THEORETICAL MECHANISMS LINKING GUN RELINQUISHMENT LAWS TO REDUCTIONS IN GUN VIOLENCE

The theoretical basis for predicting a negative relationship between gun relinquishment laws and GV includes 1) practical implications of the law and 2) the application of additional sanctions for possessing a gun while being subject to a gun relinquishment order. In practice, these laws likely operate multiple ways to reduce GV. First, individuals who possess one or more guns and are ordered to relinquish them may comply with the law for the duration of the order. In this situation, gun violence declines because individuals who otherwise would have used a gun to commit a violent crime do not do so because they do not have access to the gun(s). This is a practical implication of

[5] Because gun relinquishment laws can be applied in DVRO and DVMC cases involving acts of DV without the use of a gun, they may prevent DV incidents not involving a gun as well as incidents that involve the use of a gun. However, I contend that most of their effect will be on acts of violence committed with a gun based on extant evidence on general and specific deterrence, which is discussed in the next chapter.

the law. Rational Choice Theory explains why abusers in this scenario comply with the gun relinquishment order as well as why gun relinquishment orders may prevent future gun violence in other scenarios, like when abusers continue to possess one or more guns while subject to a gun relinquishment order.

Rational Choice Theory and its component Deterrence Theory, which is specific to sanctions within the criminal justice system, explain why crime prevention measures are more effective if they impose certain, severe, and swift costs for committing crime (Clarke & Cornish, 1985; Loughran, Piquero, Fagan, & Mulvey, 2012; Nagin & Pogarksy, 2001; Wright, Caspi, Moffitt, & Paternoster, 2004). According to this theory, individuals consider the perceived costs and benefits of a crime before committing it and are less likely to commit a crime when the costs outweigh the benefits (McCarthy, 2002). Recently, researchers have incorporated complex findings from the field of behavioral economics about how humans make decisions into this theory (c.f., Pogarksy, Roche, & Pickett, 2017; 2018; Wilson, 2019). This has led to a more complicated theory, but one where an increase in the expected certainty, celerity, and severity of sanctions is still contended to decrease the probability of criminal offending at some level, on average (Pickett, 2018; Loughran, Paternoster, Chalfin, & Wilson, 2016).

Importantly, perceptions about the costs and benefits associated with a crime can be learned through either direct or indirect knowledge. In this case, indirect knowledge would be obtained if a person learned about their state's gun relinquishment law and its details without being directly exposed to it. This could include learning about the law through the news, social media, or peers. Alternatively, direct knowledge would be gained if a person was subject to the law by being a DVRO respondent or having a DVMC and personally ordered by a judge to relinquish their firearm(s).

In deterrence theory, general deterrence describes the deterrent effect of the threat of punishment while specific deterrence describes the deterrent effect of the experience of punishment (Chalfin & McCrary, 2017; Stafford & Warr, 1993). Since gun relinquishment laws apply the threat of a unique sanction for failing to relinquish one's gun(s), any effect they have on reoffending should take the form of general deterrence. Importantly, gun relinquishment orders often apply to all DVRO respondents and individuals with a DVMC, not just those who used a gun in the crime(s) that led to one of these outcomes. To the extent that would-be abusers a) know about these laws through indirect knowledge and b) are gun owners who do not want to lose their firearm(s), gun relinquishment laws should have a general deterrent effect on all forms of DV in the state (Pickett, Loughran, & Bushway, 2016). On the other hand, if would-be abusers seldom obtain indirect knowledge about the enactment of these laws or do not factor that knowledge into their actions, these laws may exert a deterrent effect primarily through direct knowledge of the laws among abusers who are subject to them.

In fact, research shows that residents are often unaware of many criminal laws or their details (Barragan et al., 2017; Kleck, Sever, Li, & Gertz, 2005; MacCoun, Pacula, Chriqui, Harris, & Reuter, 2009). For instance, MacCoun et al. (2009) used an item from a national survey of adults that asked about maximum legal penalties for possession of a small amount of marijuana in their state to test resident knowledge of applicable marijuana decriminalization laws. They found that residents living in states where marijuana was decriminalized were just as likely to believe that jail was the maximum penalty as residents living in states where marijuana remained criminalized (31% to 33%, respectively). Moreover, 32% of the sample did not know the maximum penalty, and this proportion did not differ by criminalization status. In another study, Kleck et al. (2005) interviewed over 1,000 U.S. residents about their perceptions of punishment levels and combined these data with official crime and court data for their county. The authors found "… no significant association between perceptions of punishment levels and actual levels…, implying that increases in punishment levels do not routinely reduce crime through general deterrence mechanisms" (Kleck et al., 2005: 653).

On the other hand, there is evidence that individuals who have previously been sanctioned for breaking the law and are directly told that they will face enhanced sanctions for future crimes are less likely to reoffend compared to similar individuals who do not face the potential of enhanced sanctions for reoffending. For example, Helland and Tabarrok (2007) tested the deterrent effect of facing a third strike under California's three-strikes law by comparing the frequency of post-release felony arrests between individuals charged with and convicted of a second strikable offense to individuals charged with a second strikable offense but convicted of a lesser charge and who therefore did not face the potential of a third strike upon reconviction. The authors found that facing the potential of a third strike for reoffending reduced felony rearrest rates by around 17% over the three-year observation period. In a different study, researchers examined the effect of an Italian clemency bill that released some prisoners before their sentence expired and applied their remaining sentence to the sentence they would serve if convicted of a future crime. They found a negative relationship between the length of the residual sentence to be applied upon reconviction and the likelihood of reoffending over a 7-month observation period (Drago, Galbiati, & Vertova, 2009). Unfortunately, the authors did not define their measure of reoffending.

Notably, if gun relinquishment laws exert a general deterrent effect mostly through direct and not indirect knowledge of the laws and their penalties, they should primarily reduce the frequency in which a gun is used in acts of DV as compared to the frequency of DV in general. Moreover, because domestic abusers often commit violent crimes not related to domestic relationships (Bennett et al., 2007; Bouffard & Zedaker, 2016; Klein & Tobin, 2008; Moffitt et al., 2000), and these crimes would violate the gun relinquishment order if they came to the attention of law enforcement, the deterrent effect of gun relinquishment laws should extend to all types of GV for individuals who are subjected to these laws, not just DGV. In addition to the research findings discussed above on the lack of indirect knowledge of some criminal laws, the belief that these laws will largely impact GV as opposed to overall violence is based on a reasoning that if an offender never possesses a gun while being prohibited from doing so, these laws do not produce additional sanctions for him.

More fully, gun relinquishment laws should reduce future GV by increasing the severity and certainty of punishment among abusers who are subject to these laws. Specifically, these laws increase the severity of reoffending with a gun by applying unique sanctions for the violation of a gun relinquishment order. For example, Tennessee's gun relinquishment law states that violating the order will result in a Class A misdemeanor and each violation will constitute a separate offense (Tenn. Code Ann. §§ 36-3-625). These laws likely also increase the certainty of punishment by increasing the frequency in which victims of the abuse are informed that their abusers are prohibited from possessing a gun. For example, Fleury-Steiner, et al. (2017) showed that judicial officers in family court protection order trials often failed to mention that DVRO respondents were prohibited from possessing a firearm. Gun relinquishment laws direct judges to inform parties to the protective order of the respondent's prohibition against possessing a gun and their responsibility to relinquish any guns in their possession. It is possible that by directing judges to inform DVRO petitioners of this prohibition, they become more likely to report gun possession by their abuser, thereby increasing the certainty of legal sanctions for gun possession when subject to a gun relinquishment order.

In sum, based on Rational Choice Theory, abusers who are subject to these laws are unlikely to a) retain a firearm, b) become in possession of a firearm while subject to the gun relinquishment law or c) allow their unrelinquished firearm to be discovered by using it in a crime. Either way, this should result in a decrease in the frequency of GV in states that enact these laws. However, research on the crime reducing effect of deterrence-based laws suggests that gun relinquishment laws might not have a large effect on levels of GV or the frequency in which guns are used in acts of violence.

For example, an expert review of the literature on the crime-reducing effect of enhanced sanctions for the criminal use of firearms found mixed evidence (National Research Council, 2005, pp. 223-30). Some studies find that the threat of certain, severe, and swiftly occurring sanctions changes the behavior of potential repeat offenders (Hawken et al., 2016; Papachristos, Meares, & Fagan, 2007; Weisburd, Einat, & Kowalski, 2008), while other studies do not find this effect (Lattimore et al., 2016; Raphael & Ludwig, 2003). Despite the mixed findings in this body of research, as reviewed in the next section, the few studies on gun relinquishment laws and DGV find these laws lead to a reduction in that outcome. Therefore, more research is needed to determine whether these laws deter acts of DGV.

In addition to predicting that gun relinquishment laws will reduce rates of domestic and not-domestic GV, there is a basis in the practical implications of the law as well as in Rational Choice Theory for predicting that these laws will impact the type of weapon an abuser subjected to the law uses to commit an act of violence. Specifically, according to this theory, these laws should increase the likelihood that a would-be GV offender substitutes a different, less lethal weapon for a gun before committing an act of violence. Again, this is because they have either relinquished their guns or are deterred from using them to avoid facing additional sanctions for violating the gun relinquishment order. As stated, because abusers often commit GV not related to DV, these mechanisms should translate into a reduction in gun use in both domestic and not-domestic violent incidents.

Importantly, even if these laws do not reduce overall levels of violence, the effect on weapon substitution would be positive since evidence suggests that in a scenario where an offender is planning to seriously harm or kill a victim, the victim will be less seriously injured and more likely to survive the assault if the offender does not use a gun in the assault (Braga, Griffiths, Sheppard, & Douglas, 2021). For example, in a study on family and intimate partner assault, researchers found that the use of a firearm increased the likelihood of victim death by three times compared to cutting instruments and 23 times compared to other weapons or bodily force (Saltzman, Mercy, O'Carroll, Rosenberg, & Rhodes, 1992). Additionally, two studies that combine data from the NCVS and the Supplementary Homicide Reports (SHR) found a strong association between gun use in an assault and lethality (Apel, Dugan, & Powers, 2013; Felson & Messner, 1996).

Moreover, Zimring (1968) used crime data from the Chicago Police Department to show that "[t]he rate of knife deaths per 100 reported knife attacks was less than 1/5 the rate of gun deaths per 100 reported gun attacks" (p. 728). To provide support for his assumption that knife attackers do not have less lethal intentions in mind than gun attackers, Zimring (1968) showed that attacks to non-vital areas like a person's extremities occurred at near equal rates for knife and gun attacks, whereas attacks to the chest, abdomen, head, face, back, and neck occurred at a greater rate for knife attacks than for gun attacks (p. 731).

Lastly, studies of weapon use and lethality in suicide attempts replicate this finding: if an individual uses a firearm as opposed to a different method of suicide, he/she is more likely to die following the suicide attempt (Barber & Miller, 2014; Dahlberg, Ikeda, & Kresnow, 2004; Studdert et al., 2020). Even among incidents of violent gun crime, research has found a positive relationship between the size of a gun's caliber and the severity of a victim's injuries (Braga & Cook, 2018; Zimring, 1972). Clearly, the type of weapon used in an act of violence relates to the severity of the injury incurred by the target of the violence. Therefore, even if gun relinquishment laws do not prevent DVRO respondents or individuals with a DVMC from committing violence, they should prevent gun use in these crimes and thereby limit the severity and costs of violent victimizations.

CHAPTER 5: RESEARCH ON THE RELATIONSHIP BETWEEN GUN RELINQUISHMENT LAWS AND DOMESTIC GUN VIOLENCE

Research shows that gun relinquishment laws do appear to reduce DGV, although the evidence is far from conclusive. In one study, Zeoli, McCourt, Buggs, Frattaroli, Lilley, and Webster (2018) used a pooled, cross-sectional time-series design to analyze the relationship between multiple firearm provisions in state DV laws and the frequency of IPH for the years 1980 to 2013. Although the original article was retracted due to errors in the implementation dates of some of the included laws, the retraction with corrected data provided evidence of a negative association between DVRO firearm relinquishment laws and both overall IPH and IPH caused by a firearm (Zeoli et al., 2018b). In another panel study of state firearm laws and IPH, researchers examined laws prohibiting firearm possession by DV misdemeanants and DVRO respondents and state rates of IPH while distinguishing whether the laws included a provision requiring the relinquishment of firearms (Diez, Kurland, Rothman, Bair-Merritt, Fleegler, et al., 2017). The authors found that the passage of laws requiring DVRO respondents to relinquish their firearms was associated with a decrease in total IPH rates of 10% and a decrease in firearm related IPH rates of 14%, while the passage of laws prohibiting DVRO respondents from possessing firearms that did not require the surrender of firearms was not significantly associated with a change in IPH rates (Diez et al., 2017). Surprisingly, the authors found no association between IPH rates and laws prohibiting firearm possession by DV misdemeanants, regardless of firearm relinquishment status (Diez et al., 2017).

Although not specific to DV, in a working paper, Ben-Michael, Feller, and Raphael (2021) used the synthetic control method to test whether the 2006 implementation of the Armed and Prohibited Persons System in California led to a reduction in the state's murder rate. This system monitors firearm owners who become prohibited from possessing a firearm so that state law enforcement officers can retrieve and store their gun(s) until they are no longer prohibited from possessing one. The authors found that this program reduced murders by an average of 1.64 per 100,000 persons between 2007 and 2017; an effect driven entirely by a reduction in gun-involved murders as opposed to murders committed without a gun.

Wintemute, Frattaroli, Wright, Claire, Vittes, and Webster (2015) advanced this research by examining the relationship between firearm relinquishment and reoffending at the individual level. Using a sample of DVRO respondents in California, the authors tested whether firearm relinquishment was negatively associated with the likelihood of future arrest. Of the 361 respondents linked to firearms whose orders were served, 119 (33%) surrendered firearms. Although the 119 DVRO respondents who surrendered their firearms had a lower incidence of arrest (14%) compared to the 242 DVRO respondents who did not (59%), this relationship was not statistically significant after controlling for individual characteristics (OR = 0.50, $p = 0.12$).

Another study examined the effect of firearm relinquishment laws on nonfatal acts of DV. In a state-level analysis, Dugan (2003) analyzed NCVS data to understand whether several DV state statues pertaining to civil protection orders are related to DV. She found a statistically significant negative relationship between the presence of a state statute directing individuals to relinquish their firearms after being served a protection order and the likelihood of family violence and dating violence, but, surprisingly, not spousal violence. In review, although there are some inconsistent findings in the literature, most studies find a negative effect of gun relinquishment laws on DGV. To advance this evidence base, this dissertation examines the effect of gun relinquishment laws on DGV using a broader set of crime and relationship types than what is currently found in the literature and expands analyses to also test for an effect on not-domestic GV.

Specifically, to this author's knowledge, empirical studies have neither examined the effect of firearm relinquishment laws on non-fatal forms of DGV like aggravated assault, robbery, and kidnapping, nor accounted for their effect on all domestic relationships that are covered by gun relinquishment laws, such as family members. Instead, the focus of most prior research has been on change in the frequency of murder committed by an intimate partner. These are important omissions, because firearm relinquishment laws are written to prevent each of these forms of violence among multiple types of domestic relationships. Additionally, acts of nonfatal DGV occur much more frequently than acts of fatal DGV, increase the risk of fatal DGV, and come with great costs to victims and society.

Furthermore, studies show that domestic abusers often engage in forms of both domestic and not-domestic violence (Bennett et al., 2007; Bouffard & Zedaker, 2016; Klein & Tobin, 2008; Moffitt et al., 2000), yet research has not explored whether gun relinquishment laws prevent not-domestic GV. According to Rational Choice Theory, gun relinquishment laws should reduce all forms of GV through the threat of increased sanctions for possessing/using a gun following a relinquishment order. Additionally, by removing guns from domestic abusers, there should be a reduction in opportunities for them to use a gun in the commission of future domestic and not-domestic violent offenses. This dissertation addresses these research gaps by estimating the relationship between gun relinquishment laws and change in multiple forms of domestic and not-domestic GV, while accounting for multiple relationship types that are covered by these laws.

Lastly, even though gun relinquishment laws vary substantially across states in the groups that they protect and the strength of their provisions, much prior research combines states to estimate an average effect of these laws on DGV. A more suitable approach is a state-by-state analysis because it allows one to disentangle differences in the details of gun relinquishment laws across states when estimating relationships between these laws and GV outcomes. For this reason, this dissertation identifies states that enacted gun relinquishment laws, examines key differences in the laws across the sampled states, and uses state crime data to examine the effect of gun relinquishment laws on multiple GV outcomes within each state. In addition to better accounting for state differences in gun relinquishment laws, this approach allows for the establishment of valid and transparent control groups in which to compare outcome trajectories before and after gun relinquishment laws go into effect. Before discussing these design considerations, I list each of my research questions with my related hypotheses below.

5.1 Research questions and hypotheses

- Research question 1: Do gun relinquishment laws prevent DGV?
  - Hypothesis 1: There will be a negative association between the enactment of gun relinquishment laws for DV offenses and a state's population rate of DGV victimizations.
- Research question 2: Do gun relinquishment laws prevent not-domestic GV?
  - Hypothesis 2: There will be a negative association between the enactment of gun relinquishment laws for DV offenses and a state's population rate of not-domestic GV victimizations.
- Research question 3: Do gun relinquishment laws cause offenders to substitute other, less lethal weapons for a gun in DV incidents?
  - Hypothesis 3: Gun relinquishment laws will be positively associated with the proportion of DV incidents that involve a weapon other than a gun.
  - Hypothesis 4: Gun relinquishment laws will be negatively associated with the likelihood that victims of severe domestic assault die from these attacks.

- Research question 4: Do gun relinquishment laws cause offenders to substitute other, less lethal weapons for a gun in not-domestic violent incidents?
  - Hypothesis 5: Gun relinquishment laws will be positively associated with the proportion of not-domestic violent incidents that involve a weapon other than a gun.
  - Hypothesis 6: Gun relinquishment laws will be negatively associated with the likelihood that victims of severe not-domestic assault die from these attacks.

CHAPTER 6: METHODS

In this dissertation, I use longitudinal data from NIBRS for the years 2005-14 and the synthetic control method (SCM) to conduct comparative case studies of the gun relinquishment laws that went into effect in Tennessee (TN) in 2009 and Iowa (IA) in 2010. As discussed in this chapter, I selected these two states because they 1) had high NIBRS coverage over this period, 2) enacted gun relinquishment laws during this period, 3) did not put into effect laws around the time that their gun relinquishment laws went into effect that are likely to confound the impact of these laws, and 4) had enough years to measure pre- and post-intervention changes in the outcomes. My analytic approach relies on variation over time and space, while the estimation procedure relies on variation between a treated and synthetic control state. The unit of analysis is the state, and each measure is observed repeatedly at the yearly level for each state.

To answer my first and second research questions—do gun relinquishment laws prevent DGV or not-domestic GV, respectively—I calculate state rates of GV committed by offenders 1) in a domestic relationship with the victim and 2) not in a domestic relationship with the victim. I define domestic relationship status based on the definition used in the gun relinquishment law in each treatment state. To answer my third and fourth research questions—do gun relinquishment laws cause domestic or not-domestic offenders, respectively, to substitute other, less lethal weapons for a gun in violent crime incidents—I create two additional measures.

The first measure is the proportion of violent crimes that involved a weapon other than a gun and the second measure is the ratio of murders to aggravated assaults. If the proportion of violent crimes that involve a weapon other than a gun increases in the period following the enactment of a gun relinquishment law, it implies that more offenders are switching from using guns to using other weapons like their hands and feet, knives, or blunt objects. If the ratio of murders to aggravated assaults decreases following the enactment of a gun relinquishment law, it suggests violent assaults are becoming less lethal in the state. This is because elements of an aggravated assault include inflicting severe or aggravated bodily injury, and the Federal Bureau of Investigation (FBI) directs LEAs to code attempted murders as aggravated assaults in NIBRS (FBI, 2021:8). Therefore, in this dissertation, aggravated assaults are viewed as one step down from murder and used with murder to indicate how lethal violent attacks are in a state.

Before providing a more detailed description of my measures and data analysis procedure, I further discuss my data and explain 1) which states consistently reported complete or nearly complete crime incident data to NIBRS from 2005-14 and 2) my method of identifying states that enacted a gun relinquishment law for DV offenses during this time.

6.1 Data

To answer each of my research questions, I use annual crime incident data recorded in NIBRS. NIBRS is a national crime reporting system that was founded by the FBI in 1989 to collect more detailed information on a greater number of crimes than the preceding crime reporting system, the Summary Reporting System (SRS) (Strom & Smith, 2017). Among local and state LEAs, NIBRS collects information on criminal incidents for 52 offense types, with information on just arrests for an additional 10 offense types, which LEAs voluntarily submit to the FBI each year. The FBI defines a criminal incident as "one or more offenses committed by the same offender, or group of offenders acting in concert, at the same time and place" (FBI, 2021: 5).

NIBRS captures more information than SRS by recording detailed characteristics of each offense, victim, piece of property, offender, and arrestee involved in a criminal incident. This information includes characteristics like the weapon(s) used, any known motivations for the incident, the relationship between the victim and offender, the type of location in which the incident occurred, the type of injury caused to the victim, and more. This additional detail allows for an examination of whether changes in crime depend on both the type of offense and characteristics of the offense like the relationship between the offender and victim or the type of weapon used to commit the crime. The fact that NIBRS includes the date in which the incident occurred allows for an examination of change in crime over time. In this dissertation, crime incidents are aggregated to the year. Because violent crime is relatively rare and has seasonal fluctuations (Carbone-Lopez & Lauritsen, 2013), a clearer analysis of violent crime occurs at the yearly level as opposed to a smaller period like months or days. Because gun relinquishment laws take effect at the state level, I examine change in annual crime counts at the state level.

In this dissertation, I use the NIBRS victim-level extract files created by the Inter-university Consortium for Political and Social Research (ICPSR) for the years 2005-14. The unit of observation in these datafiles is the year, and my measures are constructed from each state's number of violent crime victims per year. I measure the number of crime victims as opposed to the number of crime incidents because the former measure describes the number of persons who are protected each year by gun relinquishment laws and will capture any change in the magnitude of GV incidents caused by the enactment of these laws, with magnitude measured as the number of victims per incident.

The outcomes used in this dissertation are based off a yearly summed count of murders, kidnappings, robberies, and aggravated assaults (henceforth, "violent crimes"), disaggregated by whether an offender possessed or used a gun during the commission of the crime and whether the offender and victim were in a current or former domestic relationship. Because the FBI changed its definition of rape in 2013 and not all LEAs immediately adhered to the new definition (Kaplan, 2021:3.4.1), I do not include this crime type in my measure of violent crime. In this dissertation, I examine the effect of gun relinquishment laws on my outcomes in two states that enacted gun relinquishment laws for DV offenses between 2005-14. Before describing my measures, I explain my state selection process in the next two sections.

6.2 State coverage in NIBRS

Because providing detailed crime incident data to the FBI is time intensive and costly, LEAs have been slow to transition from SRS to NIBRS. In 2017, only 43% of law enforcement agencies across the country that submitted crime data to the FBI did so through NIBRS (FBI, n.d.). The large amount of unit missingness in NIBRS limits its usefulness for understanding many important issues, and likely explains why IPH is so heavily studied in comparison to other forms of DV like aggravated assault (Thompson, Saltzman, & Bibel, 1999). Despite low agency coverage nationwide, some states have had consistently high coverage throughout the 21st century, providing nearly complete longitudinal data over this time.

I calculated LEA coverage rates and population coverage rates for states in NIBRS with high coverage for the years 2005-2014. Few states had adequate coverage in NIBRS prior to 2005, and 2014 is the last year of data used in this dissertation's analyses. For this dissertation, I define high coverage as an average of at least 80% coverage in either LEA coverage or population coverage over the period of observation. LEA coverage rates provide information on the extent to which all LEAs with crime reporting responsibility in the state report crime incident data to NIBRS. Population coverage rates provide information on the proportion of each state's population that reside in the jurisdictions of these LEAs. These rates might differ, since a state could have many rural LEAs that do not submit data to NIBRS, but, because these LEAs provide services to such a small fraction of the state's population, the low LEA coverage rate has little impact on the population coverage rate. Although both coverage rates are important to consider, I prioritize population coverage rates because my analyses estimate changes in crime among the population, not among LEAs.

To calculate the LEA coverage rate, I used LEA participation data from the FBI's Crime Data Explorer (CDE) website and NIBRS victim-level extract files that were downloaded from ICPSR. The CDE data provide the universe of LEAs that could have reported crime data to NIBRS, by state and year, which serves as the denominator. The NIBRS data provide the number of unique LEAs that submitted crime data to NIBRS for a given year and state, which serves as the numerator. To calculate the population coverage rate, I used the NIBRS data to sum the number of persons in a state who reside in each LEA's jurisdiction in NIBRS for each state and year, which serves as the numerator. For the denominator, I used data from the U.S. Census to measure each state's annual population.

Below, I show LEA and population coverage rates for each state in NIBRS with 80% or more coverage in either LEA coverage or population coverage over the 10 years of data. Table 1 shows state averages for each coverage rate for the years 2005-14 and

Table 2 shows state coverage rates for each individual year. In both tables, states are sorted by their overall average population coverage rate rather than their average LEA coverage rate. I prioritize the population coverage rate as a measure of coverage over the LEA coverage rate for two reasons. First, my analyses are on change in crime among a state's resident population not among a state's LEA population. Second, the FBI's measure of the number of active LEAs with crime reporting responsibilities is less accurate than the Census Bureau's estimate of resident populations. For example, the average LEA coverage rate for SC was 65%, but the average population coverage rate was 100%. This suggests that the FBI's number of LEAs in SC with crime reporting responsibilities is much too high.

Table 1. Average population and LEA coverage rates for states with an average coverage ≥ 80%, 2005-2014

State	**Population Coverage**	**LEA Coverage**
ID	100%	94%
TN	100%	94%
DE	100%	93%
VA	100%	88%
SC	100%	65%
MI	99%	85%
RI	98%	95%
VT	98%	83%
MT	97%	79%
IA	97%	83%
ND	94%	75%

WV	91%	53%
AR	86%	81%
NH	86%	74%
SD	85%	59%
CO	84%	68%
MA	83%	69%

Table 2. Yearly population and LEA coverage rates for states with an average coverage ≥ 80%, 2005-2014

	2005		**2006**		**2007**		**2008**	
State	**Population**	**LEA**	**Population**	**LEA**	**Population**	**LEA**	**Population**	**LEA**
ID	100%	92%	100%	95%	100%	96%	99%	96%
TN	99%	92%	99%	95%	100%	92%	99%	94%
DE	100%	90%	99%	93%	99%	93%	99%	96%
VA	99%	90%	99%	82%	99%	85%	99%	92%
SC	100%	60%	99%	74%	99%	80%	99%	76%
MI	100%	85%	100%	86%	100%	83%	99%	83%
RI	84%	92%	100%	96%	100%	94%	100%	96%
VT	97%	76%	98%	90%	98%	90%	97%	83%
MT	90%	70%	96%	68%	97%	79%	98%	76%
IA	98%	83%	97%	84%	97%	83%	97%	84%
ND	88%	65%	88%	67%	89%	63%	92%	69%
WV	93%	66%	92%	54%	92%	59%	92%	49%
AR	61%	65%	68%	77%	72%	78%	89%	82%

NH	80%	70%	77%	71%	78%	73%	88%	74%
SD	67%	40%	76%	42%	85%	56%	85%	55%
CO	72%	60%	77%	61%	77%	61%	78%	62%
MA	75%	61%	76%	62%	84%	66%	84%	69%

Table 2. Continued.

	2009		2010		2011		2012	
State	**Population**	**LEA**	**Population**	**LEA**	**Population**	**LEA**	**Population**	**LEA**
ID	99%	95%	99%	90%	100%	93%	100%	93%
TN	100%	96%	100%	94%	100%	93%	100%	95%
DE	99%	95%	100%	95%	100%	96%	100%	95%
VA	99%	94%	100%	87%	100%	82%	100%	95%
SC	99%	67%	100%	63%	100%	58%	100%	67%
MI	100%	88%	99%	85%	100%	87%	99%	86%
RI	100%	96%	100%	96%	100%	96%	99%	94%
VT	98%	83%	98%	78%	99%	80%	99%	93%
MT	98%	84%	99%	82%	99%	83%	99%	85%
IA	96%	82%	96%	81%	96%	86%	96%	83%
ND	92%	75%	98%	76%	97%	72%	98%	88%
WV	90%	49%	93%	44%	92%	55%	93%	56%
AR	94%	85%	96%	78%	98%	85%	98%	87%

NH	88%	76%	90%	76%	90%	74%	90%	74%
SD	88%	64%	87%	59%	88%	66%	90%	68%
CO	79%	63%	80%	57%	84%	64%	98%	83%
MA	86%	70%	85%	72%	85%	73%	86%	75%

Table 2. Continued.

	2013		2014	
State	**Population**	**LEA**	**Population**	**LEA**
ID	100%	96%	100%	95%
TN	100%	95%	100%	96%
DE	100%	90%	100%	84%
VA	100%	87%	100%	87%
SC	100%	51%	100%	50%
MI	98%	84%	99%	86%
RI	100%	96%	100%	96%
VT	100%	82%	100%	75%
MT	99%	80%	97%	79%
IA	95%	81%	97%	82%
ND	99%	88%	100%	85%
WV	91%	52%	86%	47%
AR	95%	85%	91%	88%

NH	89%	71%	94%	79%
SD	90%	68%	91%	70%
CO	98%	83%	98%	84%
MA	87%	75%	86%	71%

As seen in Tables 1 and 2, despite the low national NIBRS participation rate by LEAs, multiple states have had high LEA and/or population coverage in NIBRS over much of the observation period. If one can find one or more states among this list that enacted a gun relinquishment law over this time and one or more comparable states that did not, a quality test of the effect of gun relinquishment laws on multiple forms of GV is possible with NIBRS. To do so, I conducted an online search to determine whether any of these states enacted a gun relinquishment law, and, if so, when the law went into effect.

6.3 Firearm relinquishment laws in high coverage states

I conducted an online search of multiple websites including Giffords Law Center to Prevent Gun Violence, Everytown for Gun Safety's Gun Law Navigator, Justia US Law, state government websites, and more and examined state statutes, session laws, and codes, news articles, research articles and reports, governor statements, and more. I began this search with the Giffords Law Center to Prevent Gun Violence's website, where a multitude of experts have compiled and continually update information on state gun laws. After identifying all relevant state laws pertaining to gun relinquishment for DV offenses, I confirmed that I did not miss any state laws by checking a similar website, Everytown for Gun Safety's Gun Law Navigator, as well as local news articles, research publications such as Zeoli and colleagues (2019, Table 1), and the RAND State Firearm Law Database version 3.0 (Cherney, Morral, Schell, & Smucker, 2020).

After compiling a comprehensive list of state laws for the states that had gun relinquishment laws, I read the state codes and session laws that resulted in or were related to those state codes on Justia US Law, a website that offers free access to state codes and regulations, and state legislature websites. Through this search, I identified the years that gun relinquishment laws went into effect, whether the laws applied to DVMCs, DVROs, or both, and the amount of discretion given to judges in applying the laws. Table 3 documents the findings from this search. States are presented in the same order as in Tables 1 and 2. If a state did not enact a gun relinquishment law, each column is marked as "NA," meaning not applicable because there is no gun relinquishment law in effect. If multiple gun relinquishment laws were enacted during different years in a state, the laws are presented chronologically in vertical order across the table within the state row.

Table 3. Gun relinquishment law presence and characteristics, by state

State	Year law went into effect	Types of offenses to which law applies	Judicial discretion in applying law
ID	NA	NA	NA
TN	2009[a]	DVMC and permanent DVRO	No
DE	1999[b] 2017[c] 2018[d]	Permanent DVRO Temporary DVRO DVRO laws strengthened	Yes Yes NA
VA	2021[e]	DVRO	No
SC	NA	NA	NA
MI	NA	NA	NA
RI	2005[f] 2016[g] 2017[h]	Permanent DVRO DVMC	Yes No Unclear[i]

		Relinquishment laws strengthened	
VT	2018[j]	DVRO	Yes
MT	NA	NA	NA
IA	2010[k]	DVMC and permanent DVRO	No
ND	1997[l]	Permanent DVRO	Yes
WV	NA	NA	NA
AR	NA	NA	NA
NH	2000[m]	DVRO	No for permanent DVRO, Yes for temporary DVRO
SD	1989[n]	DVRO	Yes
CO	2013[o]	DVMC and permanent DVRO	No
MA	1994[p]	Temporary DVRO	No

a. Tenn. Code Ann. §§ 36-3-625
b. 11 Del. Code § 1448
c. Del. Code Ann. tit. 10, § 1045
d. Del. Code Ann. tit. 10, § 7703-04
e. Va. Code Ann. § 18.2-308.1:4
f. R.I. Gen. Laws §§ 8-8.1-3; 15-15-3
g. R.I. Gen. Laws §§ 11-47-5.4
h. R.I. Gen. Laws §§ 8-8.1-3; 15-15-3
i. Everytown for Gun Safety (2019)
j. Vt. Stat. Ann. tit. 13, § 4053-54
k. Iowa Code §§ 724.26
l. N.D. Cent. Code § 14-07.1-02
m. N.H. Rev. Stat. Ann. § 173-B:4; B:5
n. S.D. Codified Laws §§ 25-10-24
o. Colo. Rev. Stat. § 13-14-105.5
p. Mass. Gen. Laws chapter 209A, §§ 3B

Based on these results, I selected two states to use as treatment states in my analyses: IA and TN. Each of these states enacted, on the same date and during my observation period, gun relinquishment laws that applied to both DVMCs and permanent DVROs, did not allow for judicial discretion in ordering gun relinquishment, and had high population coverage in NIBRS in the years surrounding the year their law went into effect. Despite CO sharing these characteristics, CO's population coverage rate in NIBRS before 2013 is low at around 80% during years 2009-11. More importantly, in 2013 several major laws related to gun control, DV, and protection orders took effect in CO, including one that established a universal background check system (Cherney et al., 2020).[6] The enactment of these laws at the same time as the gun relinquishment law would make it impossible to identify the effect of CO's gun relinquishment law on my outcomes. Although a gun relinquishment law went into effect in RI in 2005, I cannot test the impact of this law due to a lack of pre-intervention data. However, because there were 4-5 years separating RI's gun relinquishment law effect date from the effect dates of TN's and IA's laws, I retain RI as a possible control state. Additional states that passed a gun relinquishment law were ineligible because their gun relinquishment laws went into effect either prior to or after my observation period (DE; MA; ND; NH; RI; SD; VA; VT). Since CO's gun relinquishment law went into effect so shortly after TN and IA's laws went into effect, I exclude CO as a potential comparison state in all analyses.

Next, I considered the persons who are subject to IA and TN's gun relinquishment laws. As previously discussed, most research on the effect of gun relinquishment laws on DGV

[6] In the *Appendix*, I describe a search of state legislature websites for IA and TN to identify any laws that went into effect during the same year, the year prior, and the year after the gun relinquishment laws took effect that might confound the relationships between the enactment of gun relinquishment laws and my outcomes.

examines rates of IPH. This is a limitation, because these laws can apply to a broader set of domestic relationships than intimate partners. To identify the "treatment group" of domestic relationships in each treatment state, I searched state statutes, session laws, and codes, as previously discussed. Table 4 indicates the domestic relationships covered (not covered) by IA and TN's gun relinquishment laws with a check mark (x).

Table 4. Domestic relationships covered by gun relinquishment laws in each treatment state

	IA		**TN**	
Abusers to whom gun relinquishment law applies	**DVMC[a]**	**DVRO[a, b]**	**DVMC[c]**	**DVRO[b, d]**
Current spouse of victim	✓	✓	✓	✓
Former spouse of victim	✓	✓	✓	✓
Current cohabitant of victim	✗	✓	✓	✓
Former cohabitant of victim	✗	✓	✓	✓
Current dating partner of victim	✗	✗	✓	✗
Former dating partner of victim	✗	✗	✓	✗
Parent of victim	✓	✗	✓	✗
Guardian of victim	✓	✗	✓	✗

Person whom which the victim shares a child in common	✓	✓	✓	✓
Person related to the victim by blood, adoption, or a current or former marriage	✗	✗	✓	✗
Children of a person in one of these relationships	✗	✓	✓	✓
Person similarly situated to a spouse, parent, or guardian to the victim	✓	✗	✗	✗

a. Iowa Code § 236.4(2)
b. 18 U.S.C. § 922(g)(8)
c. Tenn. Code Ann. § 39-13-111
d. Tenn. Code Ann. § 36-3-625

As shown in Table 4, IA and TN apply their DVRO gun relinquishment laws to the same set of relationship types, while TN applies its DVMC gun relinquishment law to a much broader set of relationships than IA does. Specifically, each state has at least one gun relinquishment law that protects spouses, cohabitants, children, wards, and persons sharing a child. Even though these laws apply to a broader group than just intimate partners, most studies on the effect of gun relinquishment laws on DGV focus on IPH. Limiting analyses of the effectiveness of these laws to this one relationship type results in the loss of data and may lead to false conclusions about the effect of these laws on DGV. Therefore, in this dissertation, I create treatment state-specific measures of DV that include as many relationship types covered by the respective state's gun relinquishment law as possible. As stated, this provides for a more complete test of the effect of gun relinquishment laws on DGV.

6.4 Measures

This dissertation uses 3 outcomes that are each disaggregated by whether the victimization is related to DV or not. First, I examine annual state population rates of GV victimizations, with GV defined as murders, kidnappings, aggravated assaults, and robberies where a gun was used in the crime. To account for different levels of NIBRS coverage across states and years, I use the summed annual population of all persons residing in the jurisdictions of NIBRS participating LEAs in the state to calculate state rates instead of using the annual state population from the U.S. Census Bureau. For each year, I calculate state rates by dividing the summed number of GV victimizations in the state by the total number of residents covered by NIBRS participating agencies in the state, and then multiply by 100,000.

Second, I examine the proportion of each state-year's violent victimizations that are committed with a weapon other than a gun. For this measure, I divide the number of violent victimizations committed without a gun by the total number of violent victimizations for each state and year. Third, I generate a measure of the lethality of severe assaults in each state-year. This outcome is measured as the ratio of murders to aggravated assaults. Together, I consider murders and aggravated assaults as severe forms of assault. Elements of aggravated assault include inflicting severe or aggravated bodily injury, and the FBI directs LEAs to code attempted murders as aggravated assaults in NIBRS (FBI, 2021:8). Therefore, I view aggravated assault as one step down from murder and use the offense with murder to indicate how lethal severe assaults are in a state.

Each of my 3 outcomes are disaggregated by whether the victimizations were related to DV or not. My definition of DV differs by treatment state, since IA and TN's gun relinquishment laws apply to different types of domestic relationships (see Table 4). Specifically, DV is defined as a violent victimization involving a victim and offender that are involved in a domestic relationship that is subject to the treatment state's gun relinquishment law. Not-domestic violence is defined as a violent victimization involving a victim and offender not in such a relationship.

For IA, the domestic relationship group includes the following NIBRS victim-offender relationship classifications: victims who are a former or current spouse of the offender, victims who are a child or stepchild of the offender, victims who are a grandchild of the offender since grandparents are likely seen as or similarly situated to a guardian, and victims who are a child of a boyfriend or girlfriend of the offender. Although IA's laws do not explicitly protect children of dating partners, they do protect children of current or former cohabitants, and it seems likely that in many of these DV instances the offender cohabitated with the victim's parent at least briefly. For TN, the domestic relationship type includes the following NIBRS victim-offender relationship classifications: victims who are a former or current spouse of the offender, victims who are a boyfriend or girlfriend to the offender or in a homosexual relationship with the offender, victims who are a child or stepchild of the offender, victims who are a child of a boyfriend or girlfriend of the offender, and victims who are related to the offender through blood, adoption, or law including parents, siblings, grandparents, grandchildren, stepparents, stepsiblings, in-laws, or other family members.

A limitation of NIBRS that is also shared by SHR is that it does not measure some relevant relationship types over my observation period, like victims who are an ex-dating partner of the offender or victims who are a current or past cohabitant of the offender.[7] It is unclear how agencies enter data in these cases. Using ex-dating partners as an example, some agencies might report these relationship types as missing while others report them as current dating partners and others report them as "otherwise known." Therefore, it is difficult to know how these missing response options could bias my measures of DV and not-domestic violence. Still, NIBRS provides a much more diverse set of relationship types than are included in previous analyses of gun relinquishment laws and DV and allows me to include most of the relationship types covered by the gun relinquishment laws in IA and TN.

The offense types included in my definition of violent crime are murder and non-negligent manslaughter, kidnapping/abduction, robbery, and aggravated assault. According to the NIBRS user manual (FBI, 2021), the crime of murder and non-negligent manslaughter is defined as the willful (non-negligent) killing of one human being by another. Kidnapping/abduction is defined as the unlawful seizure, transportation, and/or detention of a person against his/her will or of a minor without the consent of his/her custodial parent(s) or legal guardian. Robbery is defined as taking or attempting to take anything of value under confrontational circumstances from the control, custody, or care of another person by force or threat of force or violence and/or by putting the victim in fear of immediate harm. Finally, aggravated assault is defined as an unlawful attack by one person upon another wherein the offender uses a weapon or displays it in a threatening manner, or the victim suffers obvious severe or aggravated bodily injury involving apparent broken bones, loss of teeth, possible internal injury, severe laceration, or loss of consciousness (FBI, 2021).

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[7] Another limitation of SHR but not NIBRS is that it only measures the victim-offender relationship type for one victim per incident. NIBRS measures the victim-offender relationship type for each victim in the incident.

In addition to these outcomes, this dissertation includes a set of state-level covariates that are related to GV. First is a factor score of household gun ownership that was created and validated by RAND Corporation (Schell et al., 2020). Research has shown that household gun ownership predicts GV generally (Miller, Hemenway, & Azrael, 2007; Siegel, Ross, & King III, 2013; Studdert et al., 2022) and DGV specifically (Campbell et al., 2003; 2017; Studdert et al., 2022). Next are measures of state economic conditions including the percent of state residents below the poverty line from the American Community Survey and the percent of state residents who are unemployed from the Bureau of Labor Statistics. Research shows that levels of economic disadvantage like these are positively associated with crime rates (Pratt & Cullen, 2005). Next is a measure of the percent of each state's population that is composed of active duty military personnel. Studies have demonstrated a positive association between being in the military and committing severe forms of DV (Heyman & Neidig, 1999). Last are measures of state population demographics from the Decennial Census including the percent of residents that are black, white, and another race, and the percent of state residents that are between the ages of 1 and 9, 10 and 19, 20 and 29, 30 and 49, and 50 and above. Evidence shows that both race and age predict violent crime (Snyder, 2011). Except for the population demographic covariates that were measured in 2010, each covariate was measured in 2008 prior to IA or TN's gun relinquishment laws going into effect. Given how slowly each covariate changes over time, these cross-sectional measures satisfactorily demarcate differences between states in the pre-treatment period.

In this dissertation, I examine each outcome over the 4 years prior to the year that the gun relinquishment law went into effect and the 4 years after the year that the gun relinquishment law went into effect. For IA whose gun relinquishment law went into effect in 2010, the pre-intervention years include 2006-09 and the post-intervention years include 2011-14. For TN whose gun relinquishment law went into effect in 2009, the pre-intervention years include 2005-08 and the post-intervention years include 2010-13. I selected this observation period to balance the need to have enough years to capture pre- and post-intervention trends in my outcomes with the need to ensure that post-intervention changes in these trends were caused by the gun relinquishment laws going into effect and not by other state interventions in either my treatment and/or control units in following years. Because of a lack of state coverage in NIBRS prior to 2005, I could not include longer pre-intervention trends. Future studies like this one will benefit from increased state population coverage in NIBRS. For example, in 2018 there were 22 states with more than 80% population coverage in NIBRS and in 2019 there were 25 (FBI, n.d.).

6.5 Missing item-level data

Between 2005-14 for the states included in this analysis, weapon type is missing in at least 2.0% and at most 2.5% of the violent victimizations recorded in NIBRS. I proceed with the assumption that if the weapon type is missing, whatever the weapon was, it was not a gun. I argue that this is a safe assumption for several reasons. First, for GV involving a discharged gun, gun use should come to the attention of law enforcement because bullet wounds and holes in the case of missed shots are distinctive, nearby persons are likely to hear a gunshot, and guns can leave physical evidence like shells and shell casings. Second, for cases where a gun is used in a crime but not discharged, it seems likely that a victim would alert dispatchers or law enforcement to the fact that a gun was used given how dangerous and distinctive they are, even in cases where the victim later decides not to cooperate in an investigation. Third, because the percent of missingness in weapon type does not change over time, the fact that I treat these cases the same way for every year of the analysis means that even if a sizable number of these missing cases involve a gun, this bias would not influence my analysis of change in GV over time.

Compared to weapon type, the relationship between the victim and offender is missing more often. Between 2005-14 for the states included in this analysis, victim-offender relationship type is missing in at least 25.5% and at most 26.9% of the violent victimizations recorded in NIBRS. The victim-offender relationship might be missing in violent victimizations because the victim was killed and could not identify the offender, the offender wore a mask or was not seen by the victim, the victim knew the offender's identity but did not want to cooperate with law enforcement, the victim did not remember the incident due to memory loss or intoxication, or other reasons. Unfortunately, research has yet to explain the various causes of missing victim-offender relationship information in police-reported violent crime incidents, which are likely both specific to offense types and diverse.

I handle missing information on the victim-offender relationship by conducting a complete case analysis (also known as listwise deletion). Although this method of only analyzing victimizations that are not missing information on the victim-offender relationship assumes that victim-offender relationship status is missing at random, Allison (2000) explains that "Although it is possible to formulate and estimate models for data that are not missing at random, such models are complex, untestable, and require specialized software. Hence, any general-purpose method will necessarily invoke the missing at random assumption" (p. 302). Furthermore, even if it was practicable to estimate a model based on data that are missing not at random, doing so requires knowing the various causes of missingness and having the data to model them, which is not the case here.

In this application, complete case analysis has strengths and weaknesses. One weakness is that it can lead to biased results if the data are not missing completely at random (Allison, 2009). A strength is that analyses done with complete case analysis are easier to translate to policymakers and governing authorities and may be better received because they are based on real data. In fact, Allison (2009) refers to complete cases analysis as a more "honest" approach to missing data compared to other common methods (p. 76). Additionally, unlike some other methods for handling missing data like multiple imputation, analyses based on complete case analysis can be reproduced by others. Importantly, as with missing weapon types, the percent of violent victimizations that are missing information on the victim-offender relationship type is remarkably stable from 2005-14 for the states used in this dissertation. Therefore, any bias caused by a complete case analysis should remain stable over time and therefore not distort findings generated from analyses of change over time.

6.6 Analytic Plan

To test whether the enactment of a gun relinquishment law affects GV in the state, it is necessary to identify a comparison unit(s) that can serve as a counterfactual for each treated unit. In this dissertation, I apply the synthetic control method (SCM) for this purpose. The SCM was developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) to improve upon alternative comparative case study designs. It does so by 1) applying a more reliable and accurate statistical method for selecting a comparison unit for each treated unit and 2) providing for the inclusion of multiple comparison units per treated unit, which can improve the equivalency between treated and comparison units in quasi-experimental studies (Abadie, 2021). Simply, because no single state is likely to serve as a perfect counterfactual for either Iowa or Tennessee, I use the SCM to identify a weighted combination of untreated states (i.e., a synthetic control) that closely resemble each of these states. If one can estimate a synthetic control in a model with good fit to the data, one can use the difference between units in the post-intervention period as the estimated effect of the intervention on an outcome.[8] Moreover, as discussed below, one can conduct a set of permutation tests using the untreated states to test the statistical significance of this estimated effect. For this reason, unlike other popular analytic methods, statistical power is a function of the number of untreated units available to the analysis, which in this case are states. Next, I describe the data-driven process used in the SCM to construct an equivalent synthetic control unit.

To generate a synthetic control unit, the SCM uses all possible untreated comparison units—called the "donor pool"—to identify a weighted average of comparison units that best matches the treated unit based on values of the outcome and any included observed covariates for periods prior to the intervention (Abadie et al., 2010). Although it is possible to include predictors other than the preintervention values of the outcome, scholars have noted that their inclusion seldom impacts the results of an SCM analysis (Abadie et al., 2010; Doudchenko & Imbens, 2016). In this dissertation, I conduct each analysis first using only the pre-intervention values of the outcome and then using both those values and pre-intervention values of the set of covariates described in the Measures section. In the Results Chapter, I present results for the better fitting model based on values of the root mean squared prediction error (RMSPE) (Abaide et al., 2010; 2015). Once a synthetic control has been identified, the SCM calculates the difference or "gap" in post-intervention values of the outcome between the treated unit and the synthetic control unit. It is this difference that is attributed to the impact of the intervention on

[8] Importantly, this interpretation requires that the treatment does not "spill over" into a control unit. This "no interference" assumption could be violated if, for example, the National Rifle Association shifted moneys away from other states and directed them to either TN or IA in response to the states enacting their gun relinquishment laws. This seems unlikely given the high level of support for prohibiting persons subject to a DVRO or who have a DVMC from possessing a gun (Barry et al., 2018; 2019).

the outcome. Before discussing technical aspects of the SCM, I briefly discuss the method of inference used in the SCM.

The SCM differs from common methods of testing intervention effects with longitudinal data like the interrupted time series, difference-in-differences (DiD), and panel regression designs in multiple ways. One way includes how it measures uncertainty around estimated effects and tests for statistical significance. Unlike these other methods, the SCM applies randomization inference and uses permutation tests to measure uncertainty in and test for statistical significance of estimated effects (see Cunningham, 2021: Chapters 4 and 10). In an informative article, Athey and Imbens (2017) explain that unlike sampling-based approaches to causal inference that consider treatment assignment as fixed and outcomes as being random, randomization-based inference considers treatment assignment as random and potential outcomes as fixed. Here, statistical significance is tested using exact p-values for sharp hypotheses (Cunningham, 2021: Chapters 4 and 10; Fisher, 1935).

In detail, Fisher's sharp null hypothesis states that a treatment will have no causal effect on any unit (e.g., individual; firm; country) exposed to it. The alternative hypothesis is that the treatment will have a causal effect on at least one unit that is exposed to it. Under the null hypothesis, one can use observed outcomes to make inferences about missing potential outcomes by iteratively reassigning treatment status to the untreated ("placebo") units. Then, one can calculate Fisher's exact p-value as the probability over the distribution of these effects that the observed value is larger, which one can use to test the null hypothesis of no treatment effect (Athey & Imbens, 2017). Importantly, because exact p-values are based on the size of the permutation/placebo distribution and model fit, statistical power depends more on the number of untreated units than the length of the time series, as compared to sampling-based statistical tests

(Abadie, 2021; Firpo & Possebom, 2018). Next, I discuss the technical aspects of the SCM in detail.

Following Abadie et al. (2010), suppose one has a sample of $J+1$ states that are observed over some number of periods from $t = 1$ to $t = T$, where $j = 1$ experiences an intervention at $t = T_0$ (e.g., Iowa in 2010), and $j = 2$ through $j = J + 1$ states (the donor pool) do not. Here, T_0 occurs at some time between $t = 1$ (2006 for Iowa) and $t = T$ (2014 for Iowa). The SCM estimates the impact of the intervention on $j = 1$ at $T_0 + 1$ to T by estimating the value of $Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$ where t is an element of all time points after T_0, w is a weight between 0 and 1 that sums to 1 across the donor pool, and Y is the observed outcome. To assign w_j to all j states in the donor pool, Abadie et al. (2010) suggest using those weights that minimize the mean squared prediction error (MSPE) of Y during $t = 1$ to T_0.

Using the SCM, one can assign weights to both Y_{jt} and Z_{jt}, with Z being a r x 1 vector of covariates that influence Y. Abadie, Diamond, and Hainmueller (2015) explain that if the number of periods between $t = 1$ and T_0 is sufficiently large, it is unnecessary to estimate $w_j^* Z_{jt}$ because matching on a lengthy trajectory of preintervention Y will account for the influences of Z on Y. Others have noted that the inclusion of Z seldom impacts the results after accounting for Y (Abadie et al., 2010; Doudchenko & Imbens, 2016). Because t is relatively small in this analysis, I test whether including multiple state-level covariates that theoretically predict state levels of GV results in different findings than a model that is based only on preintervention vales of Y. When differences do occur, I select a preferred model based on the RMSPE values.

Importantly, Abadie et al. (2010) state that if the SCM cannot achieve a match between the treated and synthetic control units in their pre-intervention outcome trajectories then one should not use the SCM to test for a treatment effect across units. If one can find a synthetic control unit that is equivalent to the treated unit, one can proceed with calculating the impact of the intervention on an outcome by subtracting the post-intervention values of that outcome for the synthetic control unit from that of the treated unit. Importantly, although this result will describe the magnitude of the intervention's impact, it does not describe the amount of uncertainty in the estimate, which exists due to uncertainty about how well the synthetic control reproduces the unobserved post-intervention outcome trend of the treated unit had it not experienced the intervention (i.e., its counterfactual).

To measure uncertainty in the estimated difference in post-intervention trends between the treated and synthetic control units, Abadie et al. (2010) propose a set of in-space and in-time permutation tests. In the in-space version, the SCM is applied to every j in the donor pool, thereby treating each one as if it had experienced the intervention at T_0. If the intervention has an impact on Y, one should see an impact when the treated unit, but not a comparison unit, is modeled as experiencing the intervention at T_0. In addition to this "in-space" placebo test, with a large enough t, one can conduct an "in-time" placebo test by assigning the intervention to one or more pre-intervention values of t other than T_0 (Abadie et al., 2010; 2015). If the intervention has an impact on Y, one should find this effect at the date of the intervention and not some other date. Given the short length of t used in this dissertation, I apply the in-space placebo test and not the in-time placebo test to measure uncertainty in the impact of gun relinquishment laws on my outcomes and test for statistical significance.

In the following analyses, I consider 2010 as the treatment year for both IA and TN. Although IA's gun relinquishment law went into effect on 3/22/2010, only a small proportion of IA's 7,391 violent victimizations occurred between 1/1/2010 and 3/22/2010. Moreover, because the post-intervention period includes the years 2010-14, this small percent of victimizations should not impact the substantive findings. Because TN’s gun relinquishment law went into effect on 7/1/2009 and because 51% of TN’s violent crimes in 2009 occurred in January-June, I error on the conservative side by treating 2009 as a pre-intervention year when estimating each synthetic TN. When interpreting results from this state’s SCM analyses, I compare outcome values in 2005-8 to outcomes values in 2010-13, ignoring the year 2009.

CHAPTER 7: RESULTS

This chapter is organized as follows. First, I present descriptive results and then I present explanatory results based on a series of comparative case studies using the SCM. Within each section, I present results for each of my outcomes in the following order: 1) the DGV victimization rate, 2) the not-domestic GV victimization rate, 3) the proportion of DV victimizations committed without a gun, 4) the proportion of not-domestic violent victimizations committed without a gun, 5) the lethality of severe DV assaults, and 6) the lethality of severe not-domestic violent assaults. For each outcome, I first show the results for TN and then show the results for IA.

7.1 Descriptive results

7.1.1 GV victimization rate

Figures 1-4 show the DGV and not-domestic GV victimization rate trends for TN and IA during their observation periods. A vertical line at the year 2010 indicates the year that the state’s gun relinquishment law went into effect (IA) or is being treated as going into effect for analytic purposes (TN). One can see that each state experienced a declining trend in both forms of GV during the 9-year period surrounding the date its gun relinquishment laws went into effect. Descriptively, it appears that rates of DGV dropped in both TN and IA after 2009 when their gun relinquishment laws were in effect. For not-domestic GV, the rates in TN and IA began declining several years prior to their gun relinquishment laws going into effect.

Figure 1. DGV victimizations per 100,000 persons covered in NIBRS, by year, TN

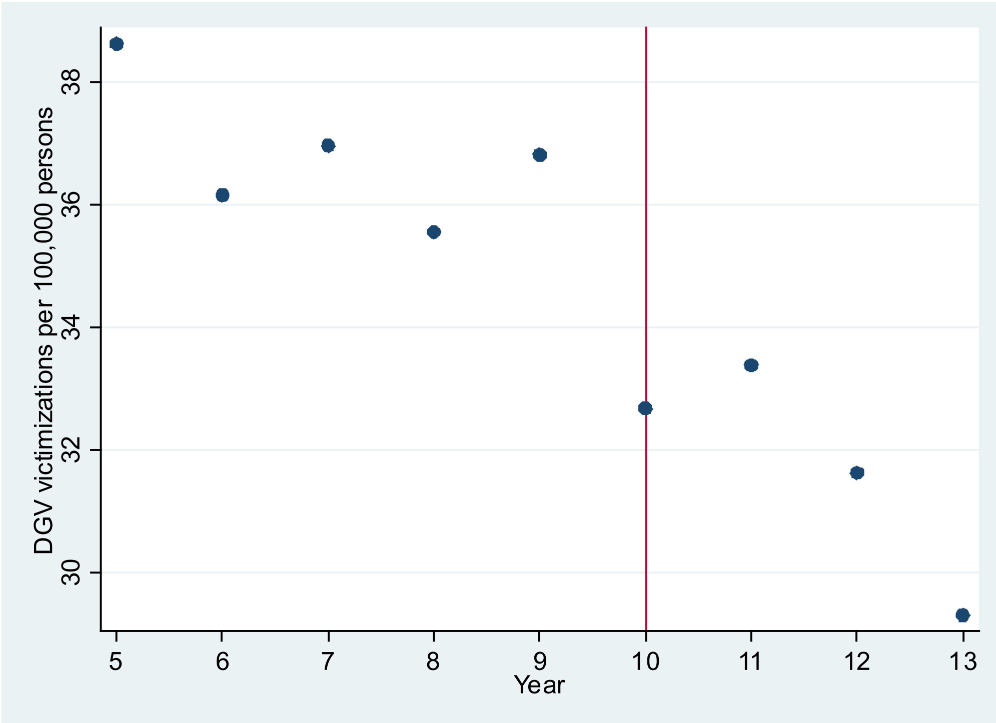


Figure 2. DGV victimizations per 100,000 persons covered in NIBRS, by year, IA

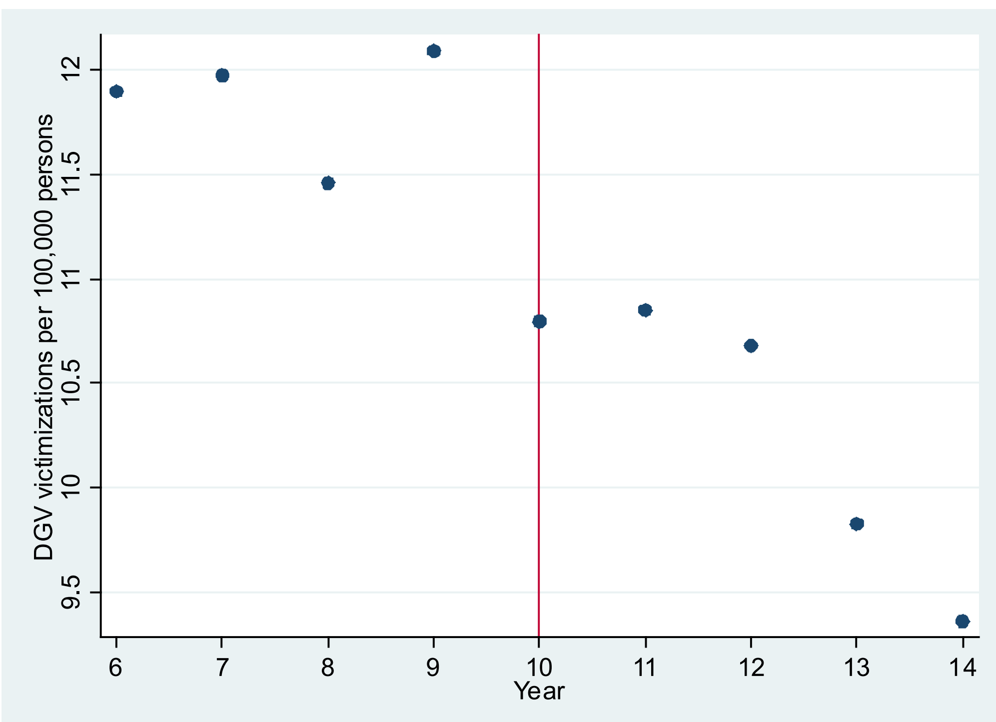


Figure 3. Not-domestic GV victimizations per 100,000 persons covered in NIBRS, by year, TN

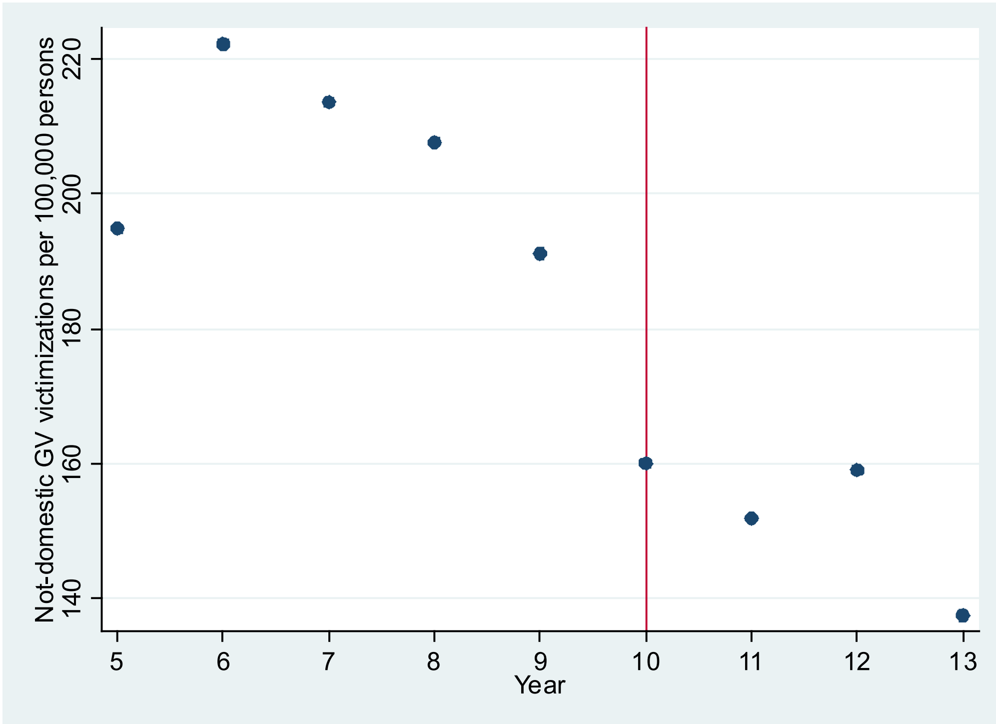
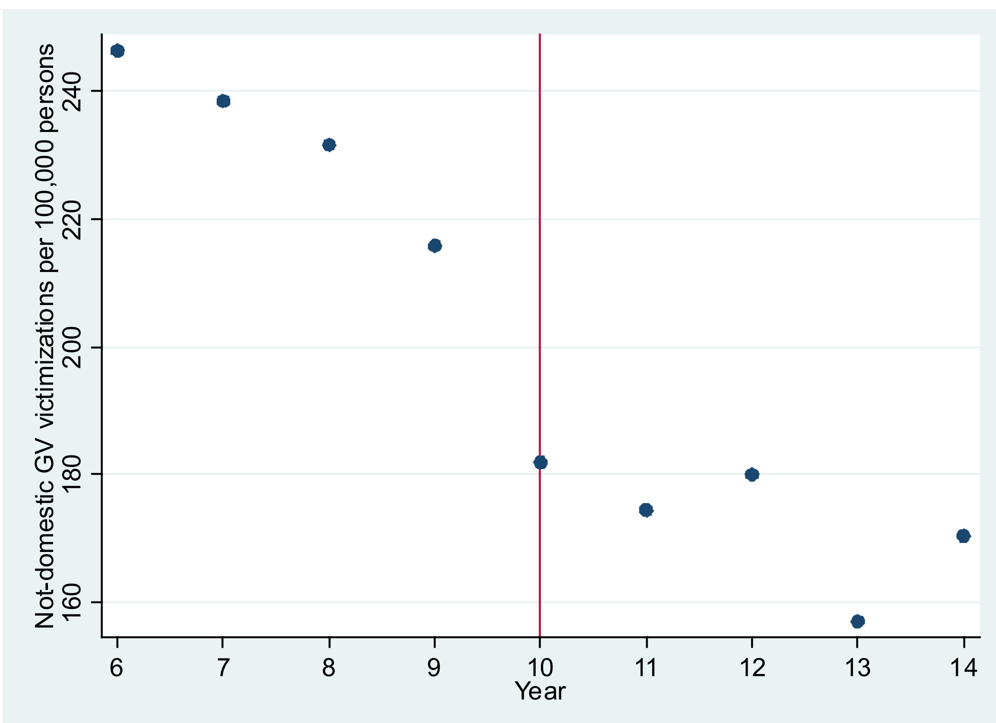


Figure 4. Not-domestic GV victimizations per 100,000 persons covered in NIBRS, by year, IA



7.1.2 Weapon substitution in violent victimizations

In addition to examining changes in rates of GV, I explore whether the proportion of domestic and not-domestic violent victimizations that did not involve a gun changed following each state's gun relinquishment law going into effect. Figures 5-8 show that for both domestic and not-domestic violence, the proportion of violent victimizations that did not involve a gun increased following TN and IA's gun relinquishment laws going into effect. However, these increases were small.

Figure 5. Proportion of DV victimizations committed without a gun, by year, TN

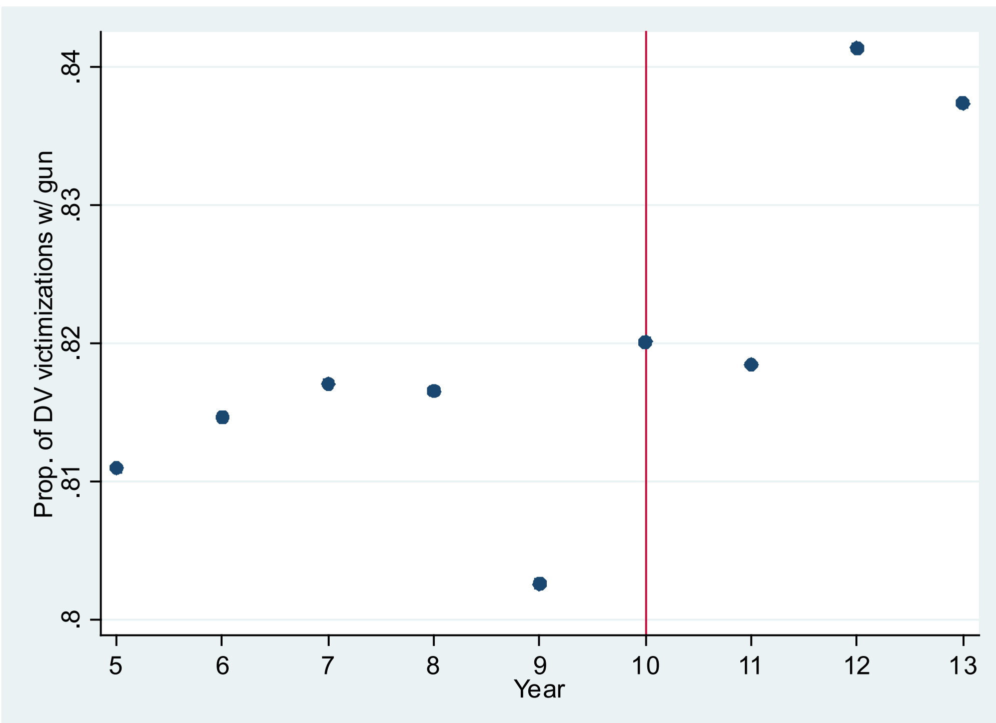


Figure 6. Proportion of DV victimizations committed without a gun, by year, IA

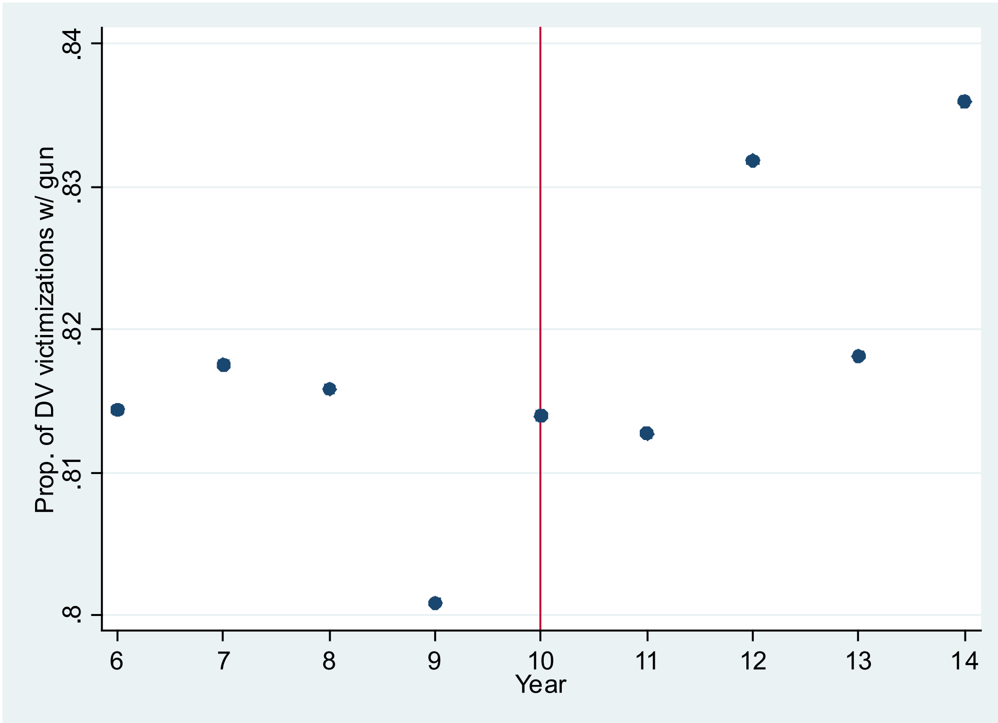


Figure 7. Proportion of not-domestic violent victimizations committed without a gun, by year, TN

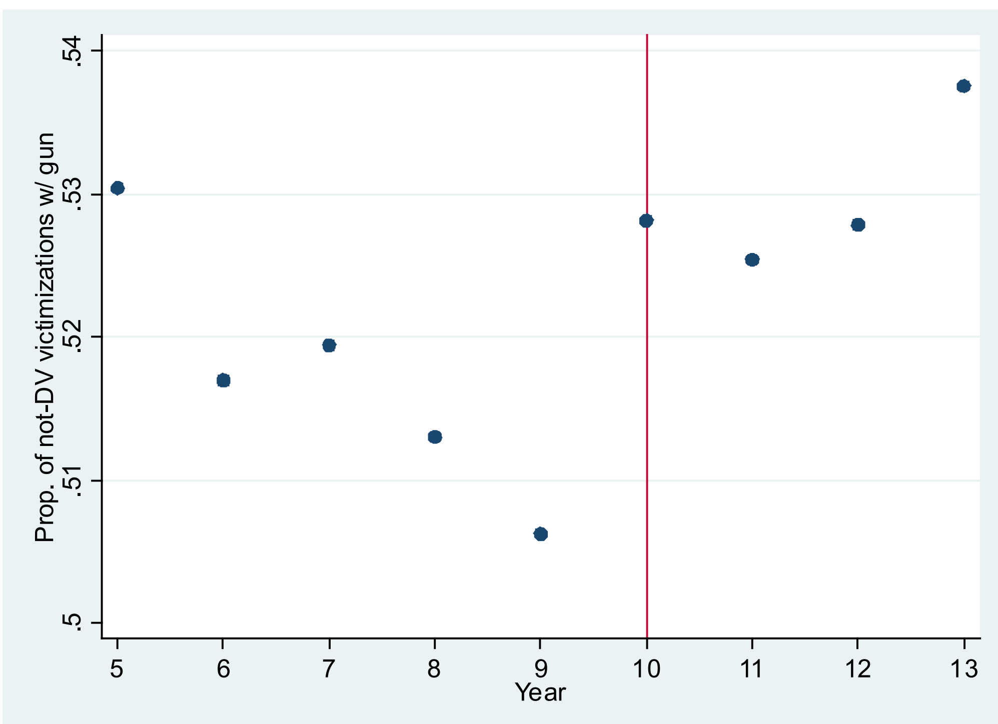
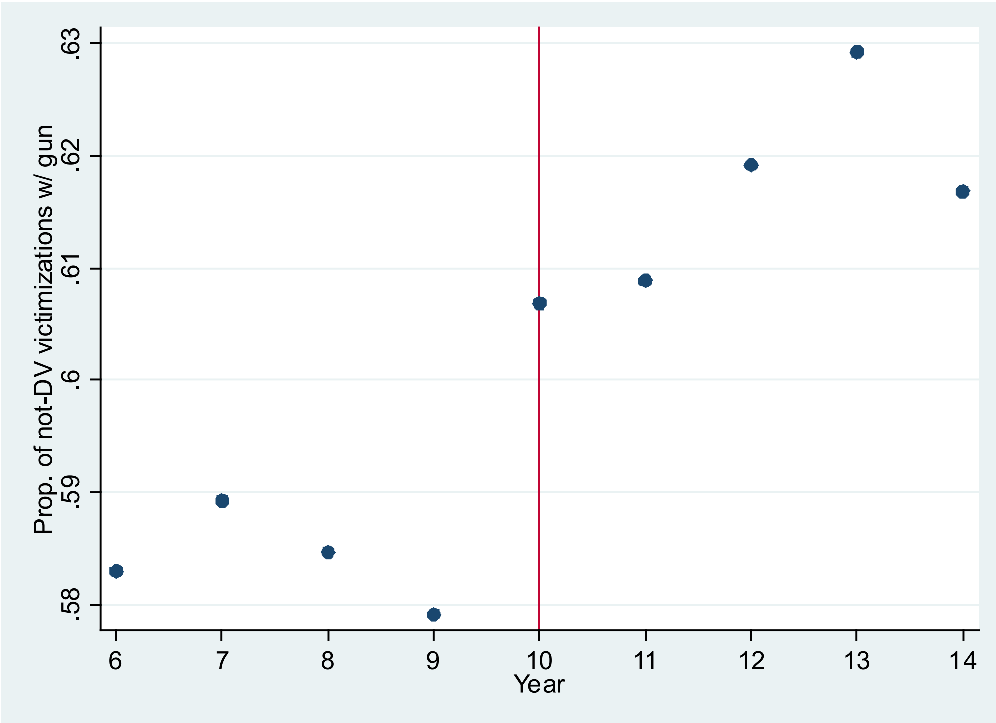


Figure 8. Proportion of not-domestic violent victimizations committed without a gun, by year, IA



7.1.3 Lethality of severe assaults

Lastly, I examine trends in the lethality of severe domestic and not-domestic assaults, measured as the ratio of murders to aggravated assaults. Figures 9-12 show that this ratio stayed relatively stable for both forms of violence in TN and IA during the observation period.

Figure 9. Lethality of severe domestic assaults, by year, TN

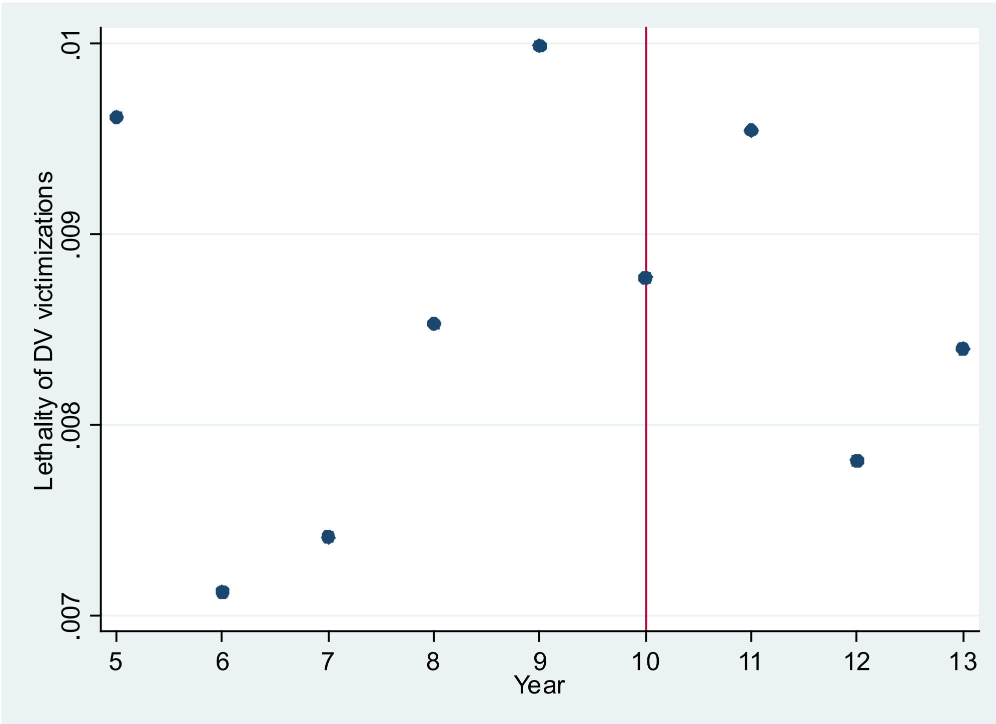


Figure 10. Lethality of severe domestic assaults, by year, IA

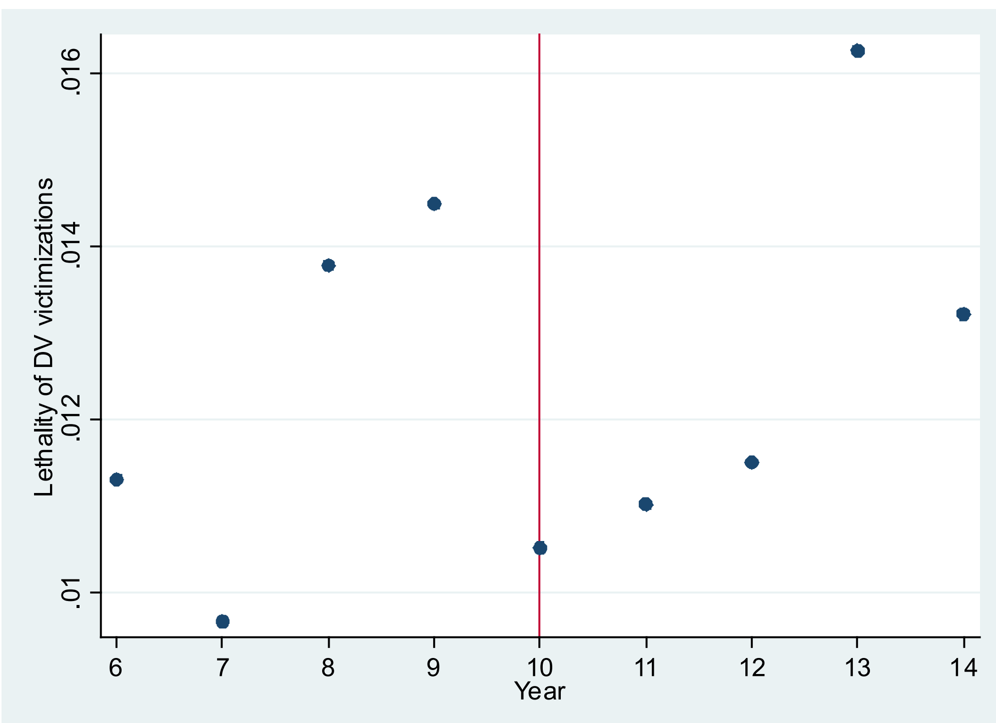


Figure 11. Lethality of severe not-domestic assaults, by year, TN

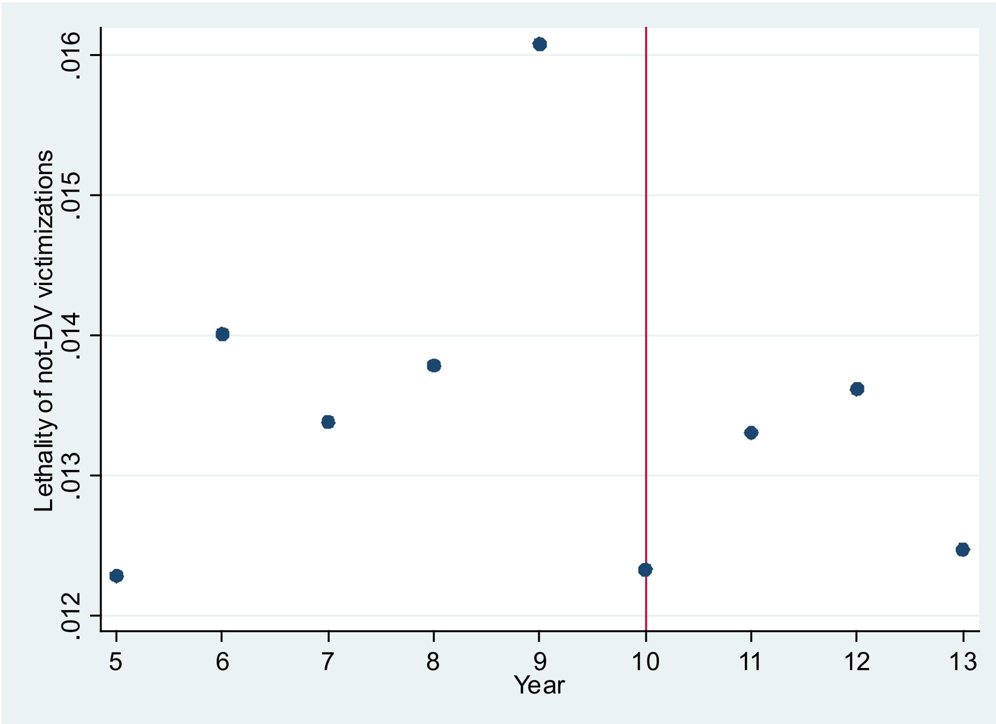
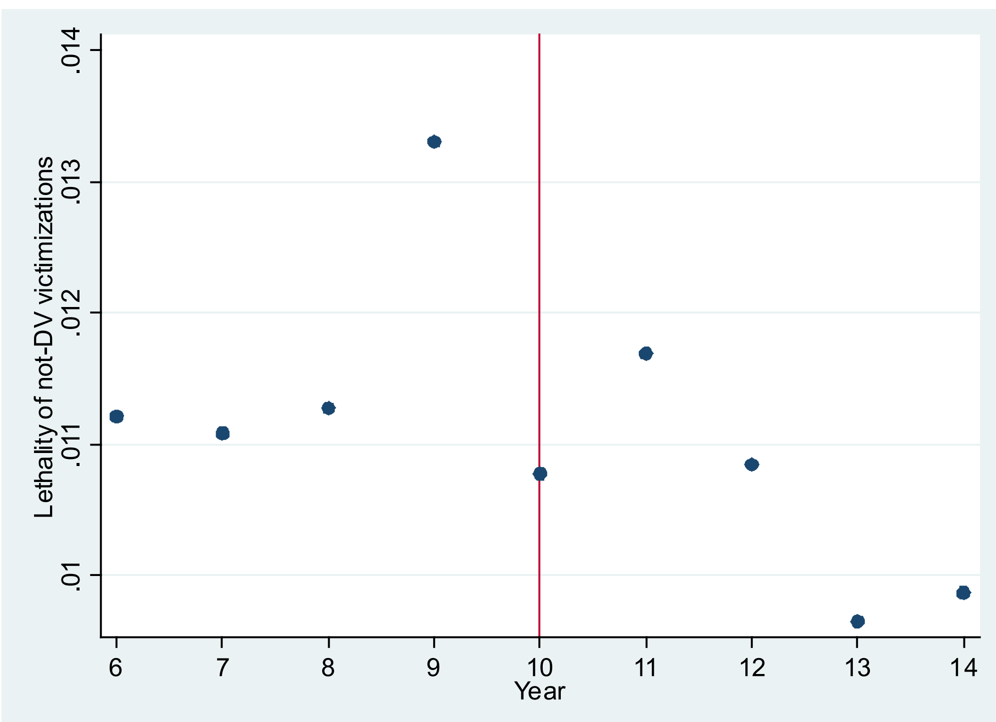


Figure 12. Lethality of severe not-domestic assaults, by year, IA



In the next section, I use the SCM to conduct a series of comparative case studies to test whether TN and IA's gun relinquishment laws altered post-intervention outcome trends compared to what the states' trends would have looked like had they not enacted their gun relinquishment laws.

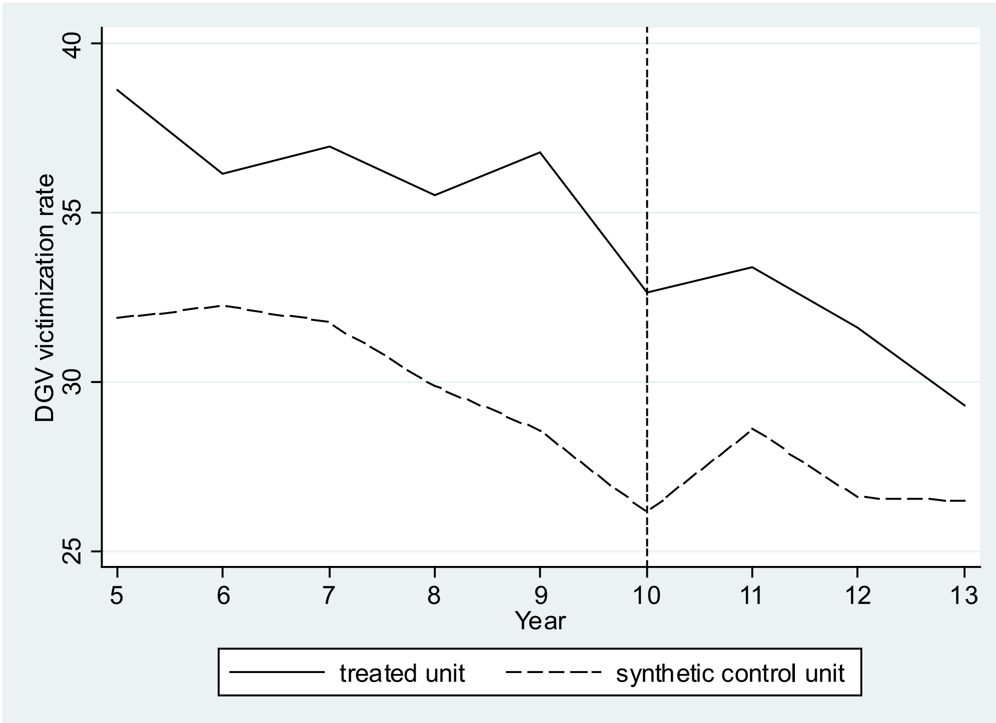
7.2 Explanatory results

7.2.1 DGV victimization rate

The first analysis examines how the rate of DGV changed following TN's gun relinquishment law going into effect. Figure 13 shows the results of a SCM where the dependent variable is the yearly number of DGV victimizations that occurred in the state divided by the total population covered by agencies who reported to NIBRS in the state that year, multiplied by 100,000. The model was the same regardless of whether the predictors included the pre-intervention values of *Y* or those values plus the additional covariates. Synthetic TN is composed of only one state, SC, and doesn't align with the trajectory of TN. The reason for this match appears to be the large level difference in DGV between TN and SC and the remaining states. For the years 2005-13, TN has the highest average rate of DGV at 34.6 followed by SC at 29.1. The next highest value is AR at 14.7. Because the SCM matches trajectories on levels and slopes, SC is the only close match. Wide variation in the level of an outcome is common in practice, which has led statisticians to develop adaptations to the SCM that account for it (c.f., Ben-Michael, Feller, & Rothstein, 2021; Doudchenko & Imbens, 2016; Ferman & Pinto, 2021). In the next analysis, I apply Ferman and Pinto's (F&P) (2021) adaptation of the SCM to account for differences in pre-intervention levels of DGV between states.[9]

Figure 13. SCM of DGV victimizations per 100,000 persons covered in NIBRS, TN

[9] Although I considered conducting a 2 x 2 difference-in-difference analysis (Card & Krueger, 1994) using data from TN and SC, the states did not have parallel pre-intervention DGV rate trends. Specifically, the 2005-9 gaps in values were 6.7, 3.9, 5.2, 5.6, and 8.2, respectively.



The F&P SCM constructs the synthetic control estimator using an outcome that is demeaned based on the average value of the outcome over the pre-intervention period. Specifically, the value of the outcome at *t* is replaced with the value of the outcome at *t* minus the pre-treatment average of the outcome. This relaxes the no intercept constraint, which is not evoked in other comparative case study methods like DiD and comparative interrupted time series designs (Doudchenko & Imbens, 2016). In fact, the primary difference between the F&P SCM and the DiD method is that the former estimates unique weights for each donor unit while the latter assigns the same weight to each donor unit (Doudchenko & Imbens, 2016).

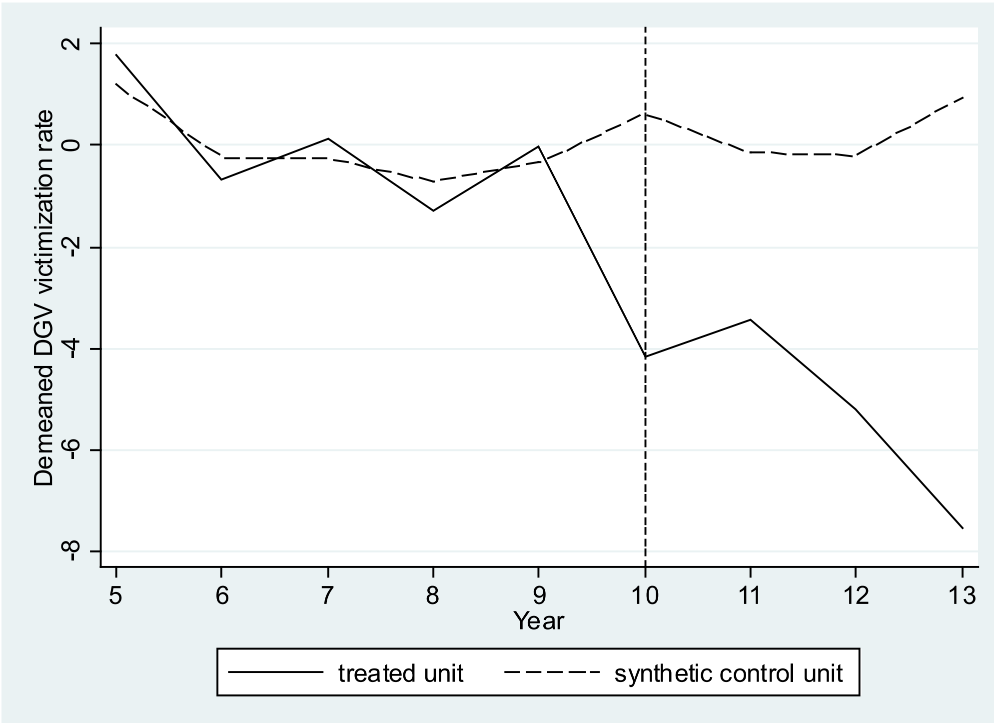
Importantly, relaxing the no-intercept constraint allows the treated and synthetic control units to differ greatly on levels of the outcome as is the case here given how much higher TN's DGV victimization rate is to every other state in the donor pool besides SC. If levels of DGV either directly or indirectly influence changes in DGV victimization rate trends over time, this would likely mean that results derived from this application of the F&P SCM are invalid.

Although there are not strong reasons to think that this is the case, the findings from this analysis should be interpreted with caution. This issue is discussed further in the Discussion Chapter.

The result of this F&P SCM is shown below in Figure 14. Again, the results were the same regardless of the set of covariates used. TN's pre-intervention trajectory of demeaned DGV was closely matched by a weighted combination of ID's (W = 82%), MT's (W = 12%), and DE's (W = 7%) DGV trends. TN and Synthetic TN's trends then diverged after the gun relinquishment law went into effect. Specifically, TN had an average DGV victimization rate of 36.8 between 2005-8 and 31.7 between 2010-13, which is a difference of -5.1 DGV victimizations per 100,000 persons.[10] The figures for synthetic TN were 7.5, 7.8, and 0.3, respectively. This suggests that TN's gun relinquishment law was responsible for reducing the state's DGV victimization rate by an average of 4.8 during the 4-year period after it went into effect.

Figure 14. F&P SCM of DGV victimizations per 100,000 persons covered in NIBRS, TN

[10] Again, because TN's gun relinquishment law went into effect on 7/1/2009, after 51% of its violent victimizations had already occurred that year, I error on the conservative side by treating 2009 as a pre-intervention year when estimating the SCM and ignoring the year 2009 when interpreting findings.



Importantly, this analysis provides an estimated effect size but does not measure uncertainty in that estimate and does not provide a test of statistical significance. To gauge how uncertain this estimate is, I follow Abadie et al.'s (2010; 2015) recommendation and conduct an in-space placebo test by iteratively treating each state in the donor pool as if they had experienced the treatment in 2010. Then, I calculate the post- to pre-treatment RMSPE ratio and compare TN's value to values from the placebo tests.[11] This is a form of randomization inference for measuring uncertainty in relationships and testing hypotheses about causal effects (Cunningham, 2021: Chapter 4). Abadie et al. (2010; 2015) explain that if the effect estimated following a SCM analysis is large relative to effects estimated for units that were not exposed to the treatment, this provides confidence that the treated unit experienced a unique change following the treatment date. Alternatively, if the treated unit's effect is similar to or less than

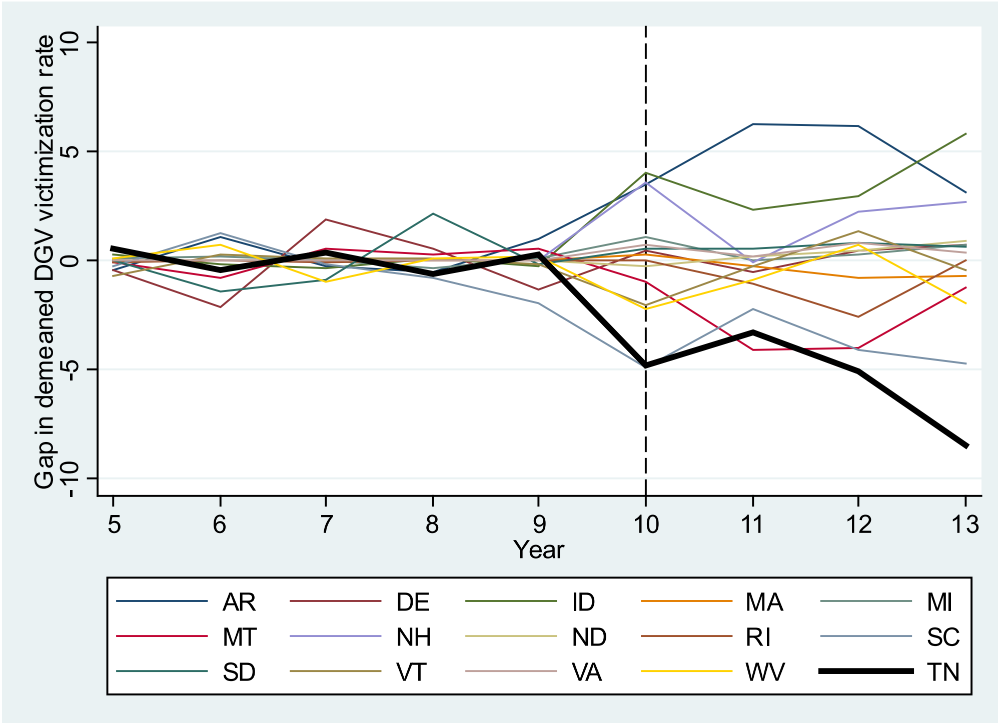
[11] Thank you to Scott Cunningham whose free online version of *Causal Inference: The Mixtape* (2021) provided useful Stata code for this analysis.

effects estimated for multiple untreated units, one would have less confidence that the treatment caused a change in the outcome.

Figure 15 shows the distribution of gaps or differences in outcome values across the observation period between the treated unit and synthetic control unit after assigning the treatment to each of the donor states and TN. TN's trend is bolded to show how its estimated effect compares to the distribution of placebo test estimated effects. From this figure, one can see that multiple control states as well as TN diverged greatly in the post-intervention period compared to their synthetic controls. This suggests that there is a fair amount of uncertainty in the effect estimated for TN. To test the null hypothesis that there was no effect for any units, I calculate an exact p-value (Fisher, 1935) using information on how TN ranks in its ratio of post- to pre-treatment RMSPE compared to the 14 donor states used in the SCM analysis (Abadie et al., 2015). Under this test, a statistically significant finding would be one where the effect estimated for the treated unit is larger than the effects estimated when the intervention is reassigned to each control unit (Abadie et al., 2015). I find that TN ranks 7 out of 15 in this ratio ($p = 0.47$). Therefore, I consider this finding to not be statistically significant.[12]

Figure 15. Distribution of gaps in demeaned DGV victimization rate between iteratively assigned treated units and estimated synthetic control units, TN

[12] From Figure 15, this may seem like an odd finding given that TN stands out as having the largest decline in its demeaned DGV victimization rate over the post-intervention period compared to its synthetic control. The reason for this finding is that multiple placebo tests had excellent fit over the pre-intervention period, resulting in pre-intervention RMSPE values approximately equal to zero. In this case, the differences between the treated and synthetic control units in the post intervention period compared to the pre-intervention period are larger than TN's difference even though TN's difference in the post-intervention period is larger.



The next analysis examines how the rate of DGV victimizations changed following IA's gun relinquishment law going into effect in 2010. Figure 16 shows the results of a SCM that was estimated using only the pre-intervention values of the outcome. This model did not differ from a model estimated using these values and the additional set of covariates. Synthetic IA is a weighted combination of MA (*W* = 54%), ND (*W* = 24%), and AR (*W* = 22%). IA and Synthetic IA's trends matched closely during the pre-intervention years. Figure 17 shows this using a gap plot of the differences in values between IA and Synthetic IA in their DGV victimization rate for each year in the pre-intervention period. In the post-intervention period, IA experienced a lower rate of DGV compared to Synthetic IA. Specifically, IA's average DGV victimization rate was 1.3 in the pre-intervention period and 1.5 in the post-intervention period, for a difference of 0.2. For Synthetic IA, the values were 1.3, 1.9, and 0.6, respectively. Thus, IA's gun relinquishment law reduced the average DGV victimization rate IA experienced between 2010-14 by 0.4

compared to what it would have experienced had the gun relinquishment law not gone into effect.

Figure 16. SCM of DGV victimizations per 100,000 persons covered in NIBRS, IA

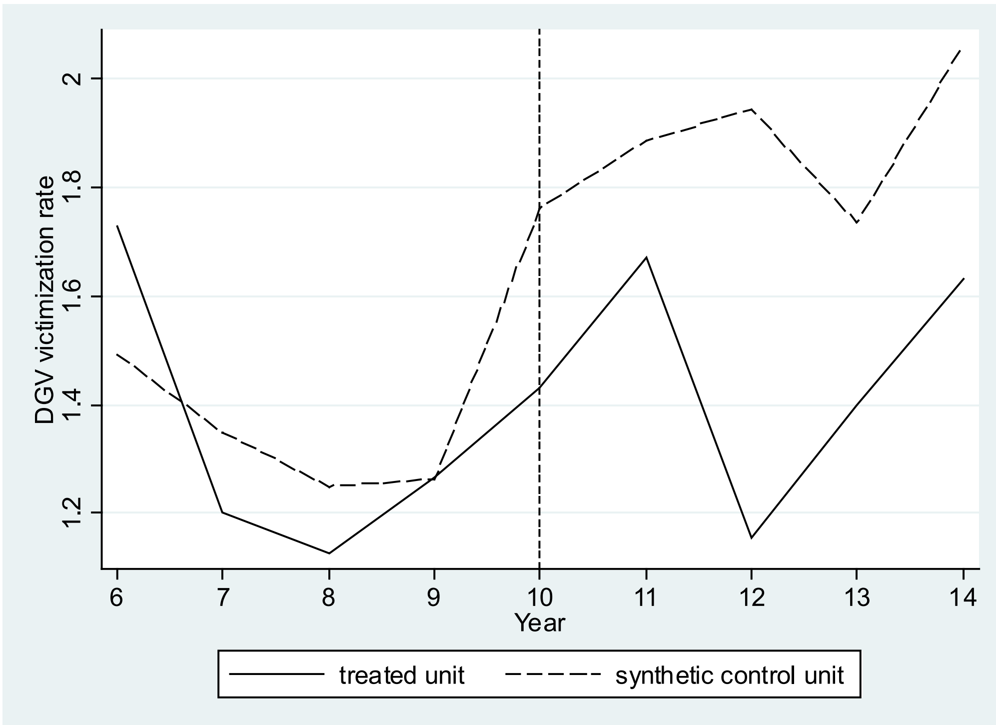
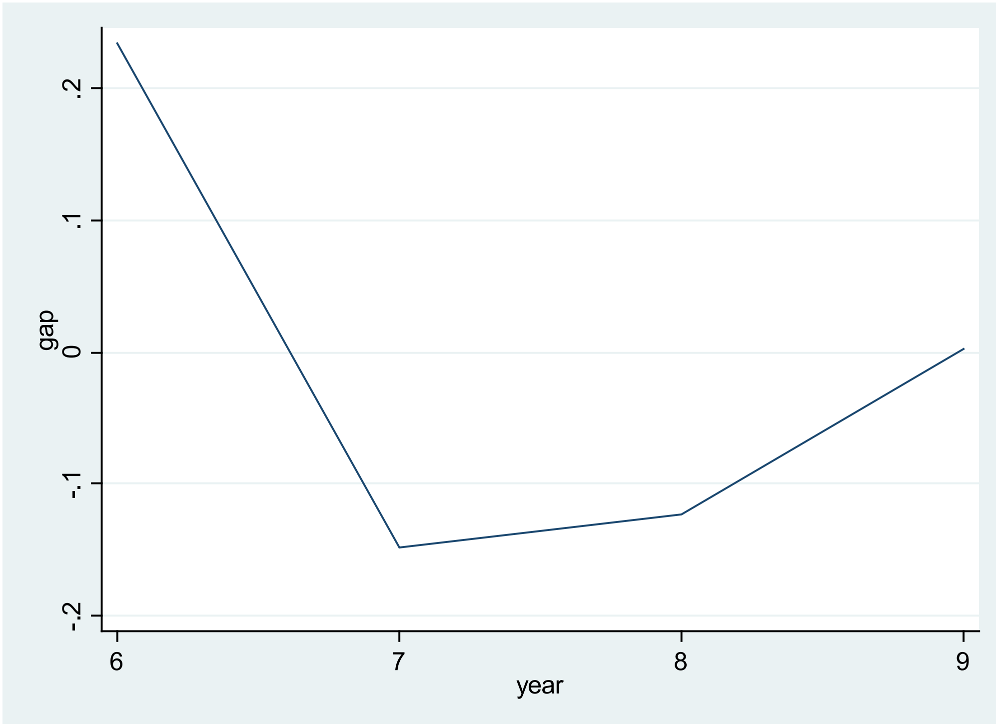
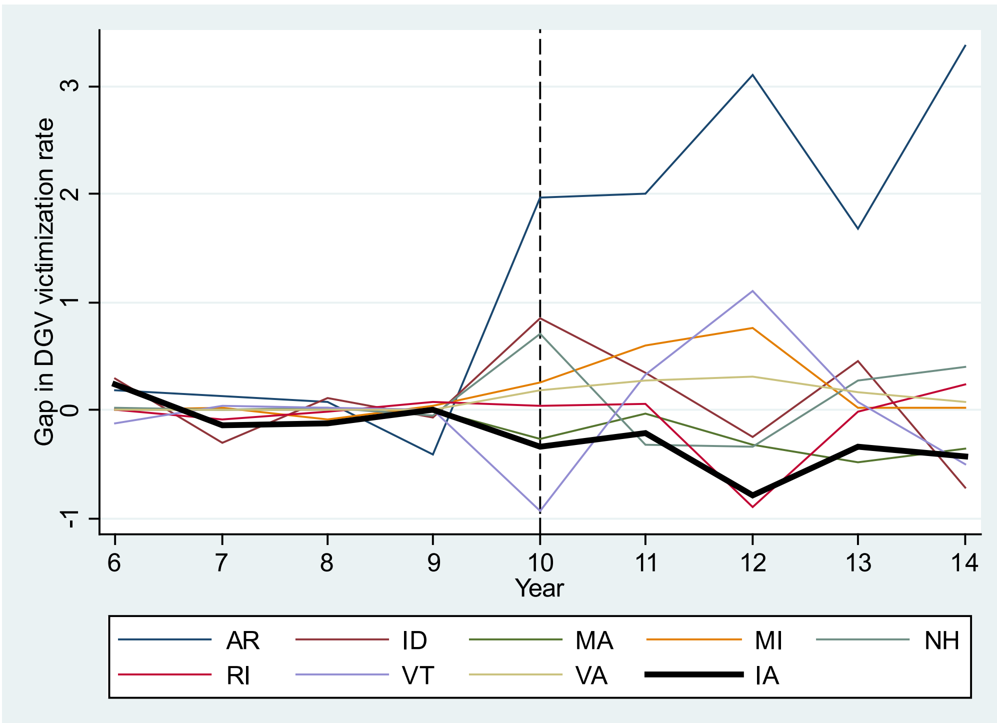


Figure 17. Gap plot of difference in DGV victimization rate between IA and Synthetic IA over the pre-intervention period



Again, I conduct an in-space placebo test to measure the uncertainty in this estimated effect. Figure 18 shows the distribution of gaps or differences in outcome values across the observation period between the treated unit and synthetic control unit after assigning the treatment to each of the donor states and IA. Although a placebo test was not conducted for SC because the fully nested optimization procedure encountered a flat or discontinuous region, the state could still contribute to the synthetic control units for the other placebo tests. IA's trend is bolded to show how its estimated effect compares to the distribution of placebo effects. From this figure, one can see that there was a noticeable divergence in the trends of multiple placebo tests as well as IA's trend at the time of the intervention. This suggests that there is a fair amount of uncertainty in IA's estimated effect. In fact, I find that IA ranked 8 out of 14 in its ratio of post- to pre-treatment RMSPE. Importantly, Abadie et al. (2010: 502) explain that placebo tests with poor fit between the "treated" and synthetic control unit should not inform an evaluation of uncertainty for a SCM analysis with good fit. Figure 18 includes only those placebo tests with a pre-intervention RMSPE value less than two times the size of IA's value. Here, IA still ranked 8 out of 9 in its RMSPE ratio (p = 0.89). Thus, IA's estimated effect should not be considered statistically significant.

Figure 18. Distribution of gaps in DGV victimization rate between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, IA



7.2.2 Not-domestic GV victimization rate

Because research suggests that domestic abusers engage in a variety of criminal offenses as compared to only committing DV offenses (Bennett et al., 2007; Bouffard & Zedaker, 2016; Klein & Tobin, 2008; Moffitt et al., 2000), it's possible that TN and IA's gun relinquishment laws also impacted not-domestic GV victimization rates in these states. To test for this effect, next I conducted the same set of analyses using the not-domestic GV victimization rate as my outcome.

For TN, I again use the F&P SCM to estimate a weighted combination of states whose demeaned pre-intervention not-domestic GV victimization rate trend matched TN's. Because the RMSPE is lower when the full set of covariates are included in addition to pre-intervention values of the outcome, I present results from that model. The result is shown below in Figure 19. Synthetic TN is comprised of SC (W = 83%) and DE (W = 17%). One can see that even after demeaning the outcome, TN's pre-intervention trend does not closely align with Synthetic TN's trend. In Figure 20, I present a gap plot that shows the large differences between TN and Synthetic TN in the demeaned not-DGV victimization rate over the pre-intervention period. Because the SCM loses its ability to provide a causal estimate when the pre-intervention fit between the treated and synthetic control unit is poor (Abadie et al., 2010), I do not interpret the findings of this analysis.

Figure 19. F&P SCM of demeaned not-domestic GV victimizations per 100,000 persons covered in NIBRS, TN

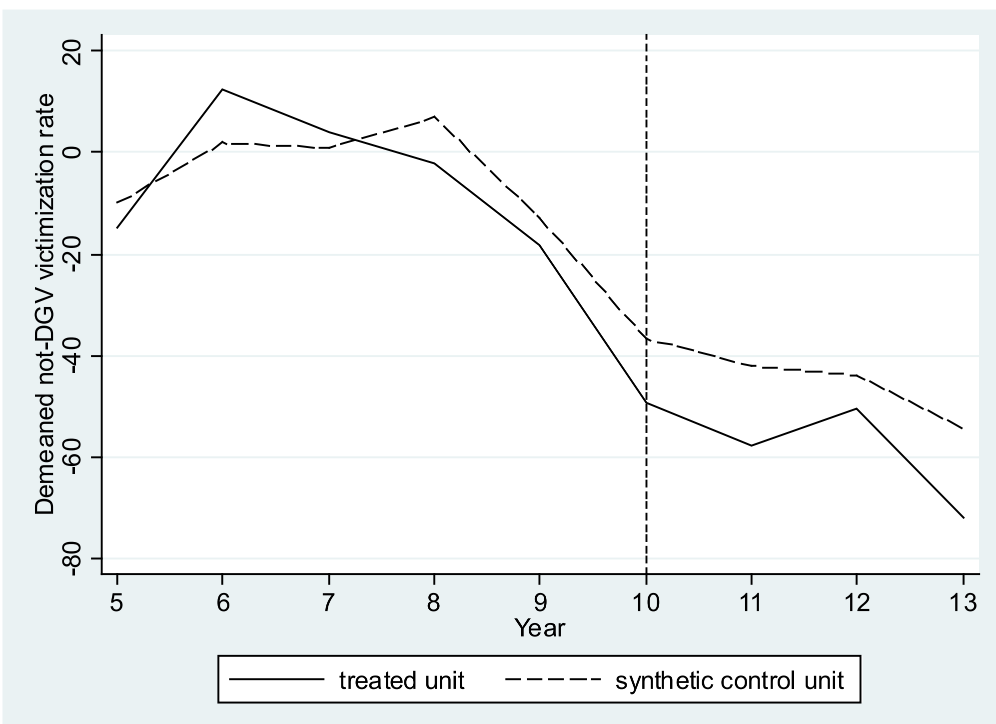
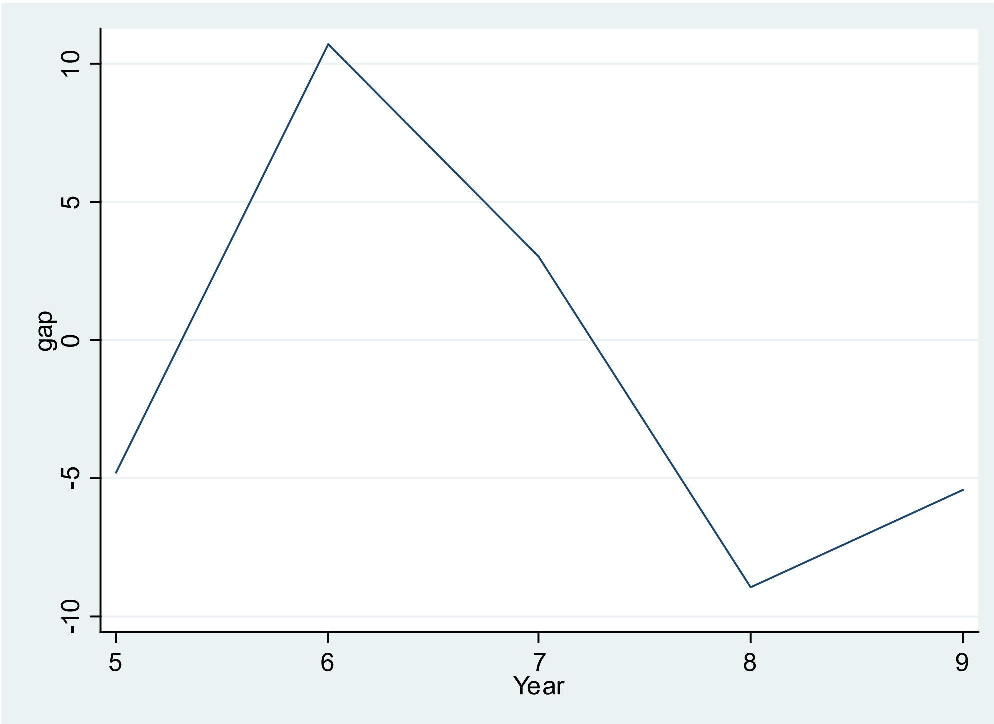


Figure 20. Gap plot of difference in demeaned not-domestic GV victimization rate between TN and Synthetic TN over the pre-intervention period



Next, Figure 21 shows the result of a SCM analysis with IA as the treatment state and the not-domestic GV victimization rate as the outcome. Based on a lower RMSPE, I present results using only pre-intervention values of the outcome as covariates but note that the results were practically identical when the larger set of covariates were used. Table 5 shows the weights assigned to each donor state. One can see both that Synthetic IA is composed of a weighted combination of all 14 states in the donor pool and that the pre-intervention trends of IA and Synthetic IA closely align. Starting in 2010 when IA's gun relinquishment law went into effect, the two units' not-domestic GV victimization rate trends diverged. Surprisingly, the analysis suggests that IA's gun relinquishment law resulted in an increase in its not-domestic GV victimization rate. Specifically, in 2010, IA's not-domestic GV rate was 19.5 compared to 19.1 in Synthetic IA, and, in 2014, IA's rate was 23.9 compared to 19.8 in Synthetic IA. The average difference in the post-intervention period was 2.8. Next, I conduct an in-space placebo test to measure uncertainty in this estimated effect.

Figure 21. SCM of not-domestic GV victimizations per 100,000 persons covered in NIBRS, IA

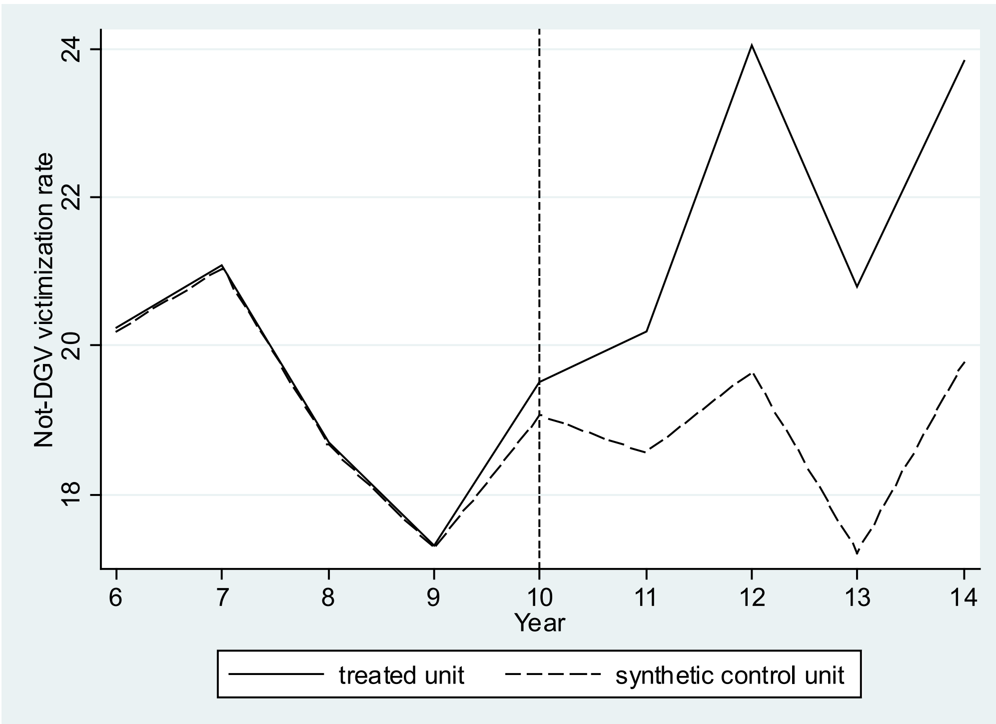


Table 5. Weights assigned to each donor state in Synthetic IA, not-domestic GV victimization rate

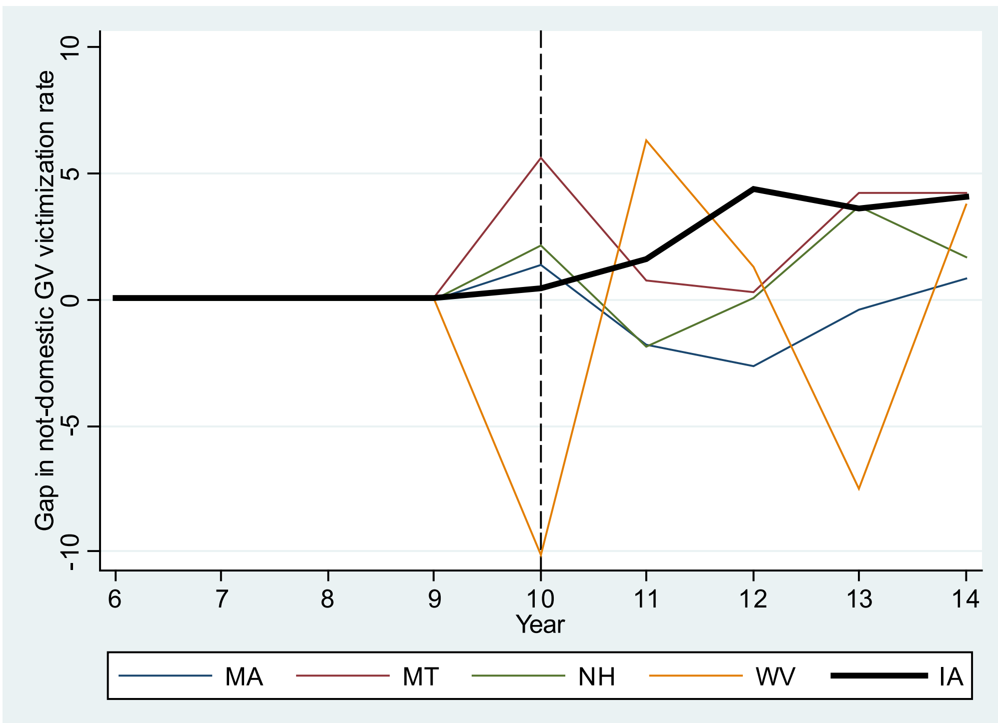
State	Weight
SD	0.439
ID	0.281
ND	0.119
VT	0.028

MT	0.024
NH	0.019
VA	0.018
MA	0.015
RI	0.014
WV	0.012
DE	0.010
MI	0.008
AR	0.007
SC	0.005

Placebo tests were not conducted for ND or SC because the fully nested optimization procedure encountered a flat or discontinuous region, but these states could still contribute to the synthetic control unit for the other tests. Based on this analysis, I find that IA was not unique among the included states in showing a sizable change in its not-domestic GV victimization rate between the pre- and post-intervention years. The fact that IA's post-intervention effect was not unique or extreme relative to the placebo tests suggests that there is a fair amount of uncertainty in the relationship shown in Figure 21. Specifically, I find that IA ranks 4 out of 13 in its ratio of post- to pre-treatment RMSPE ($p = 0.31$). A more accurate measure of uncertainty, albeit limited by the small number of observations, results from dropping placebo tests with a pre-intervention

RMSPE equal to more than two times that of IA's value (Abadie et al., 2010; Cunningham, 2021: Chapter 10). Here, still, IA ranked 4 out of 5 based on its ratio of post- to pre-treatment RMSPE (p = 0.8). This suggests that there is much uncertainty in the estimated effect for IA and it should not be considered statistically significant. Figure 22 displays the distribution of gaps between the "treated" and synthetic control units among the good-fitting placebo tests and IA.

Figure 22. Distribution of gaps in not-domestic GV victimization rate between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, IA

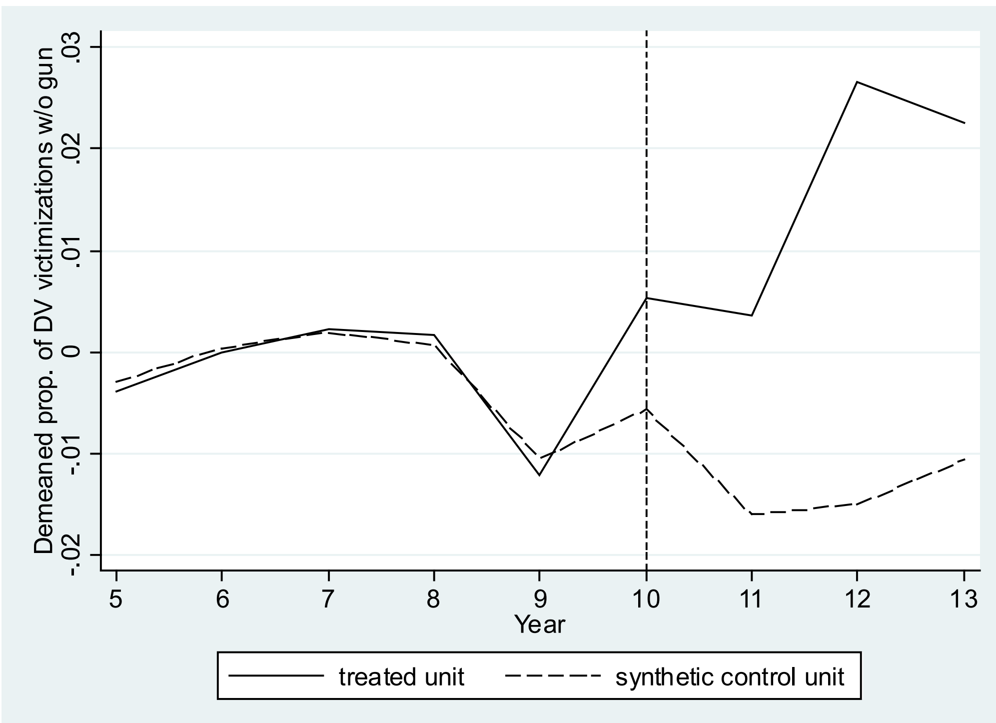


7.2.3 Weapon substitution in DV victimizations

Starting with TN, I conducted a SCM analysis using the proportion of DV victimizations that did not involve a gun as the outcome. It was again necessary to conduct an F&P SCM given differences in pre-intervention levels of the outcome between TN and Synthetic TN. I estimated one model using only the pre-intervention values of the outcome and one model using those values plus the set of covariates. The results were practically identical. Since the outcome-only model had a slightly lower RMSPE value, I present results from that model in Figure 23.

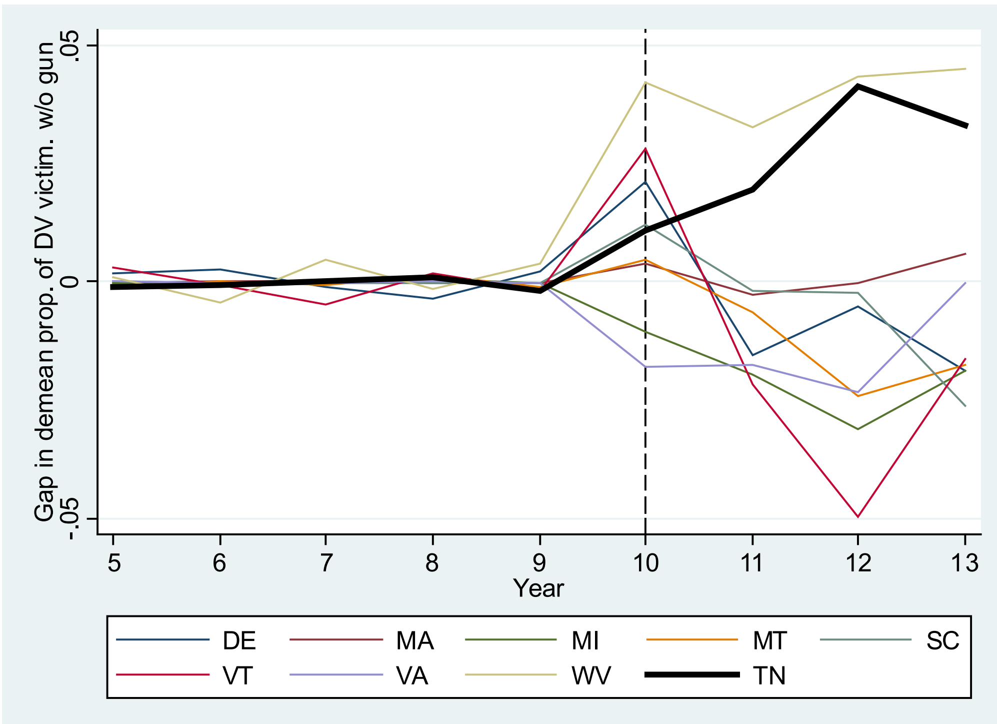
Synthetic TN is a weighted combination of MI (77%), MT (15%), SD (5%), and RI (4%). One can see that TN and Synthetic TN followed similar trajectories of their demeaned proportion of DV victimizations that did not involve a gun throughout the pre-intervention period. Then, their trajectories split with TN's moving upward and Synthetic TN's moving downward in the post-intervention period.

Figure 23. F&P SCM of demeaned proportion of DV victimizations that did not involve a gun, TN



To measure the amount of uncertainty around this effect, I next conducted an in-space placebo test. This test showed that the finding for TN should not be considered statistically significant, since TN ranked 5 out of 15 in its RMSPE ratio (p = 0.3). Figure 24 shows the distribution of placebo tests after removing the 6 placebo tests with poor fit, as measured by having a pre-intervention RMSPE value that was more than two times greater than the value for TN (Abadie et al., 2010; Cunningham, 2021). This figure demonstrates the great amount of uncertainty in this estimate, given that multiple states in addition to TN evidenced a large change in this outcome during the post-intervention period.

Figure 24. Distribution of gaps in demeaned proportion of DV victimization that did not involve a gun between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, TN



Next, I tested whether the proportion of DV victimizations that did not involve a gun changed in IA following its gun relinquishment law going into effect. I conducted a SCM using only the pre-intervention outcome values and then using both the pre-intervention outcome and pre-intervention covariate values, and the results were identical. Synthetic IA was a weighted combination of MA (W = 68%), ID (W = 18%), and AR (W = 14%). Figure 25 shows that both IA and Synthetic IA saw their proportion of DV victimizations that did not involve a gun increase from 2006 to 2007 and then remain relatively flat between 2007 and 2009. The values for IA and Synthetic IA were identical in 2010 when IA's gun relinquishment law went into effect and in 2011. Then, Synthetic IA's trend remained flat while IA's trend diverged upward.

Although there are some differences between IA and Synthetic IA in pre-intervention values of the outcome, these differences are extremely small, as shown in the gap plot in Figure 26. Even though the SCM analysis suggests a positive relationship between IA's gun relinquishment law going into effect and the proportion of DV victimizations in the state that did not involve a gun, the differences in post-intervention values of the outcome across IA and Synthetic IA are miniscule. For example, IA and Synthetic IA's average values over the post-intervention period were 0.946 and 0.938, respectively. Furthermore, Synthetic IA's difference between pre-intervention and post-intervention average values was less than a percent lower than IA's value.

Figure 25. SCM of proportion of DV victimizations that did not involve a gun, IA

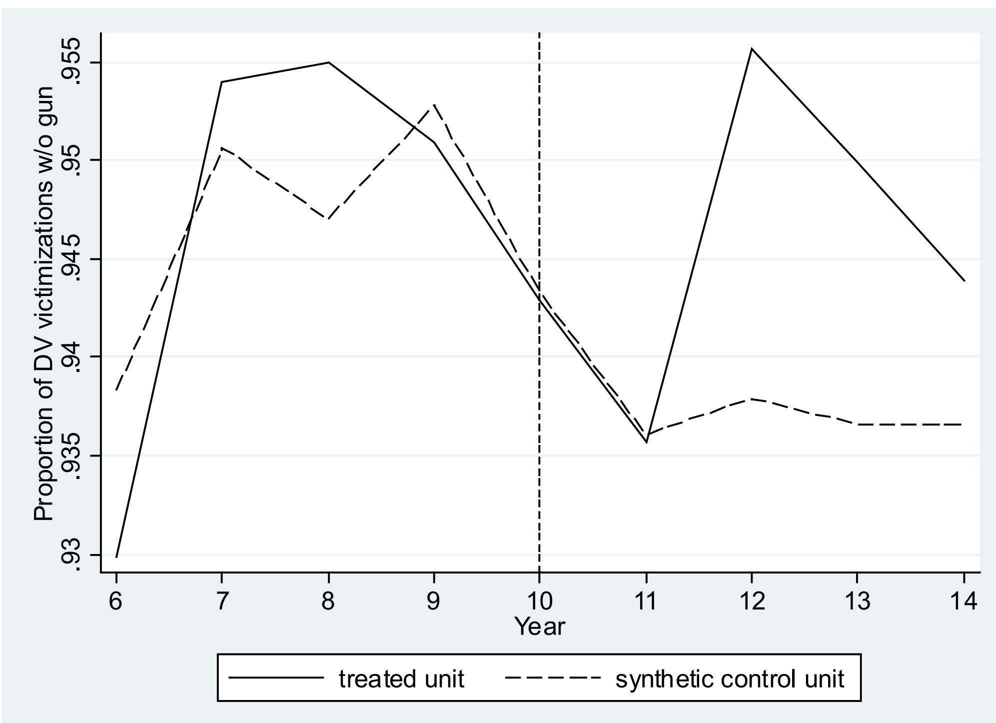
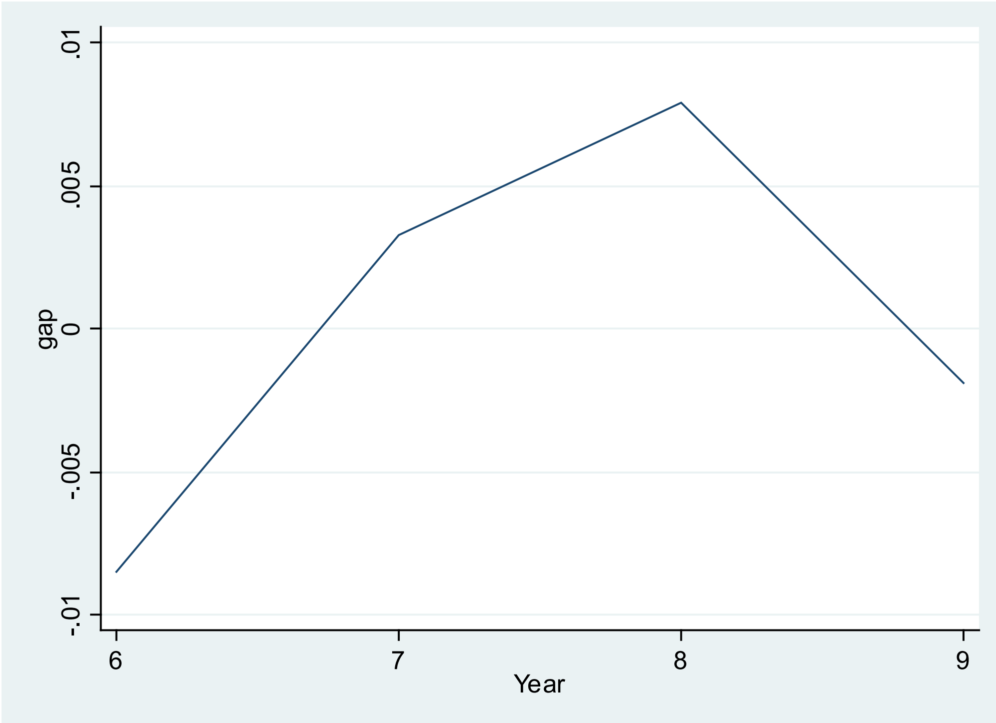
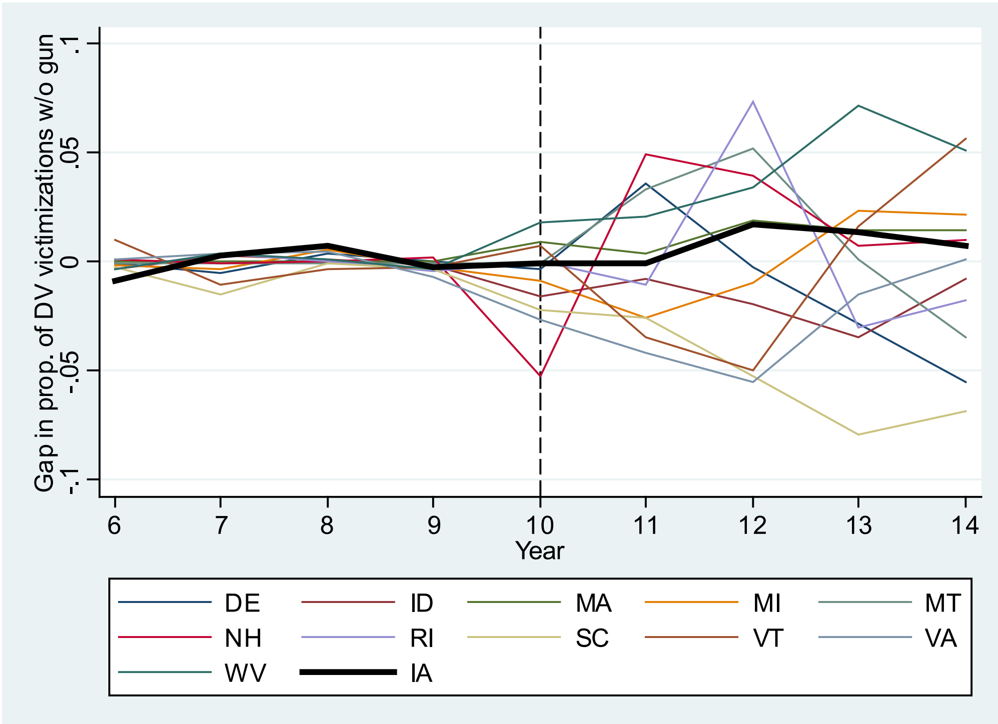


Figure 26. Gap plot of difference in proportion of DV victimizations that did not involve a gun between IA and Synthetic IA over the pre-intervention period



Next, I measure how much uncertainty there is in this effect. I did not include a placebo test for ND because the Hessian was found to be unstable/asymmetric in the nested optimization procedure. Additionally, two placebo tests were dropped because of poor fit during the pre-intervention period. The results are shown in Figure 27. They reveal much uncertainty in the estimated effect of IA's gun relinquishment law going into effect on the proportion of DV victimizations that did not involve a gun in the state. Based on the RMSPE ratio, IA ranked last out of the 12 placebo tests with good pre-intervention fit (p = 1.00).

Figure 27. Distribution of gaps in proportion of DV victimization that did not involve a gun between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, IA



7.2.4 Weapon substitution in not-domestic violent victimizations

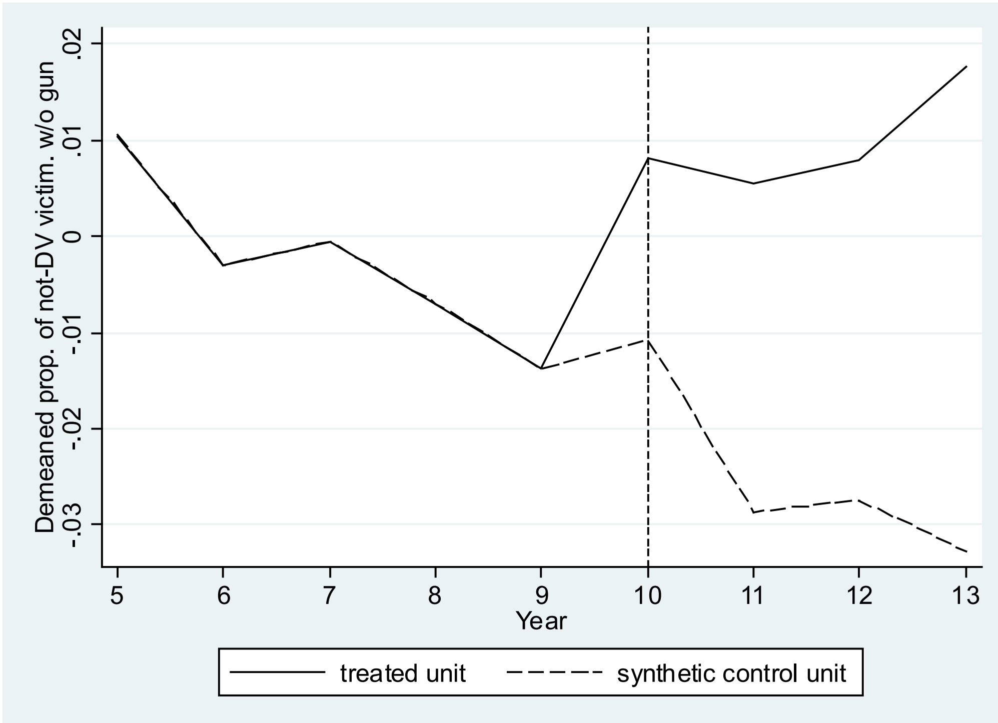
Because gun relinquishment laws remove firearms from individuals who also commit violent crimes not related to DV, I next examine whether TN's law led to an increase in the proportion of not-DV violent victimizations that did not involve a gun. The best fitting model based on the RMSPE was an F&P SCM that included pre-intervention annual values of the outcome as the only covariates. As shown in Table 6, this analysis applied weights to each of the 14 donor states, and, as shown in Figure 28, the pre-intervention trends of TN and Synthetic TN were almost perfectly aligned. During the post-intervention period, TN's trend increased while Synthetic TN's trend decreased. To measure the likelihood that this finding is caused by chance, I next conduct an in-space placebo test.

Table 6. Weights assigned to each donor state in Synthetic TN, proportion of not-domestic violent victimizations that did not involve a gun

State	**Weight**

VA	0.258
SC	0.152
RI	0.137
AR	0.113
ND	0.057
VT	0.046
SD	0.043
ID	0.043
MI	0.035
NH	0.033
MT	0.026
WV	0.024
MA	0.024
DE	0.009

Figure 28. F&P SCM of demeaned proportion of not-domestic violent victimizations that did not involve a gun, TN



Results from this in-space placebo test show that TN ranked 1 out of 15 ($p = 0.07$) on its post- to pre-treatment RMSPE ratio and had the lowest pre-intervention RMSPE ratio of any test. However, of the 14 donor state placebo tests, a total of 9 had a pre-intervention RMSPE value that was more than two times that of TN's value. This leaves an insufficient number of placebo tests to measure uncertainty around this estimated effect. Even with a rank of 1, the finding is not statistically significant ($p = 0.17$). Because it's not appropriate to compare TN's effect to effects estimated from models with poor fit, I treat the finding as not statistically significant. Since removing the poor-fitting placebo tests leaves only 6 placebo tests in which to compare TN's effect, I present results from each set of placebo tests in Figures 29 and 30 so that the reader may get a better sense of variation in effects across tests. One can see that even among the small number of placebo tests with a similar quality of pre-intervention fit as TN (Figure 30), there were similarly sized estimated effects to TN's, which suggests a fair amount of uncertainty around TN's estimated effect despite it having the lowest RMSPE ratio.

Figure 29. Distribution of gaps in demeaned proportion of not-domestic violent victimization that did not involve a gun between iteratively assigned treated units and estimated synthetic control units, all tests, TN

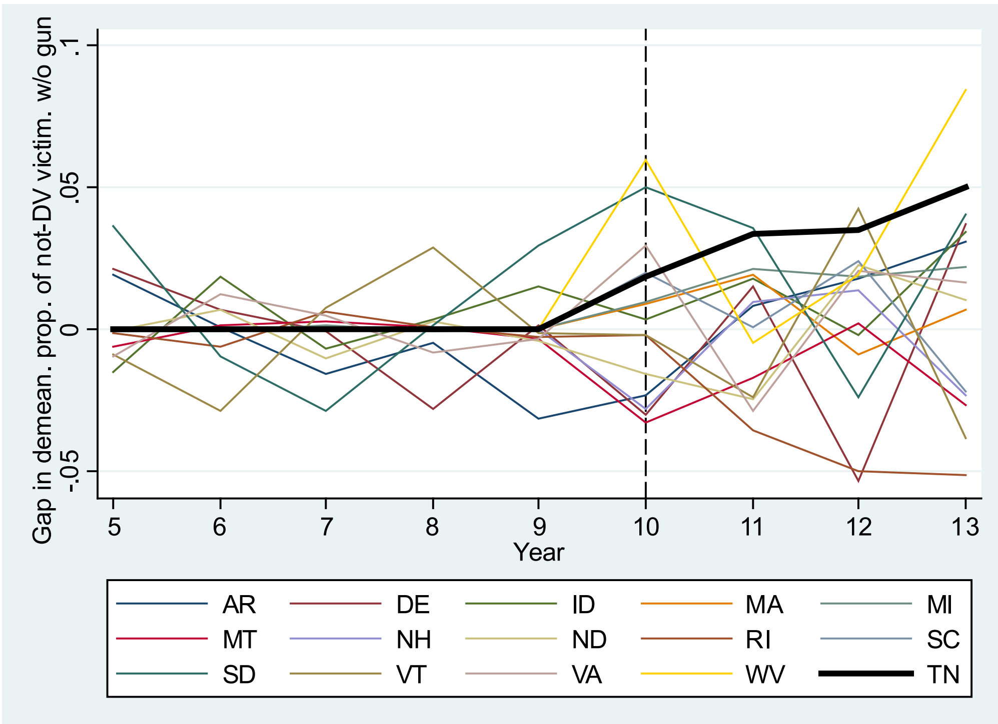
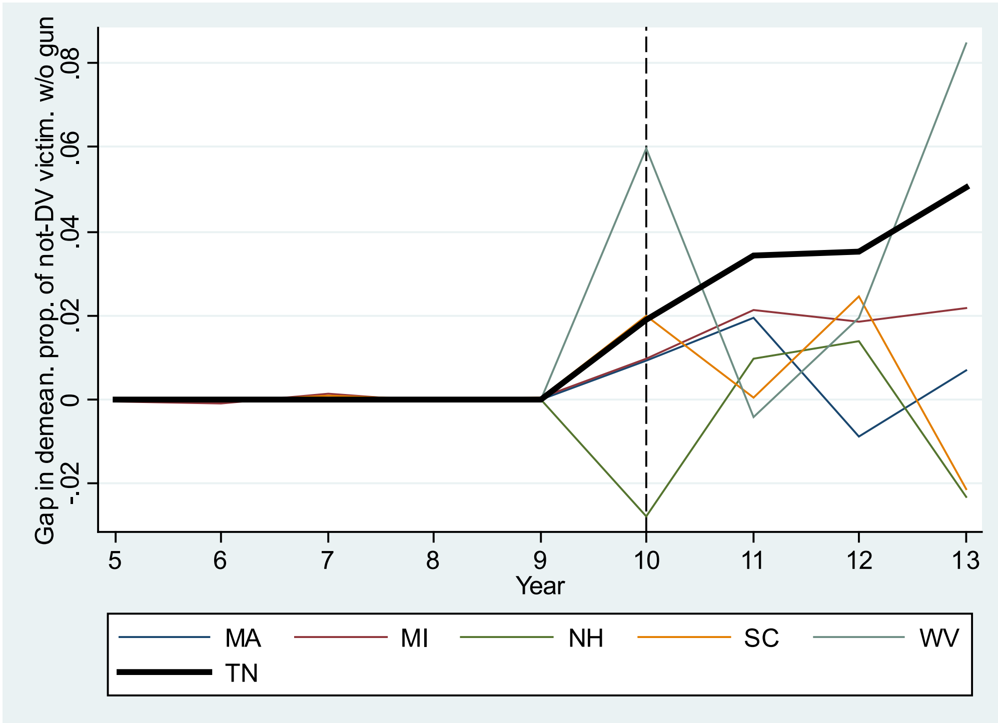
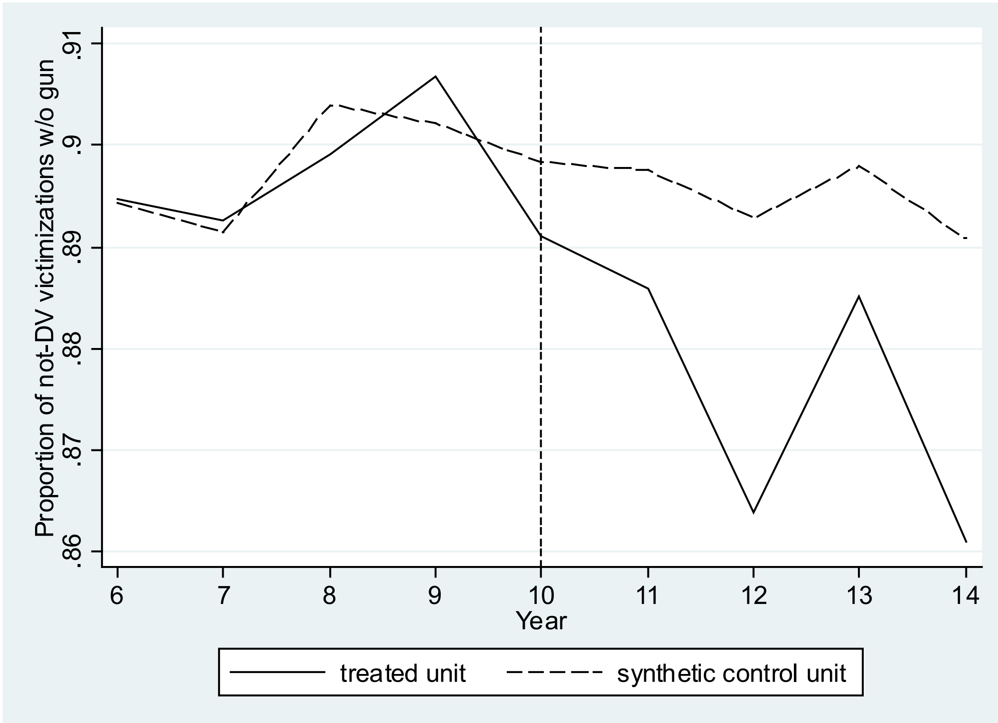


Figure 30. Distribution of gaps in demeaned proportion of not-domestic violent victimization that did not involve a gun between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, TN



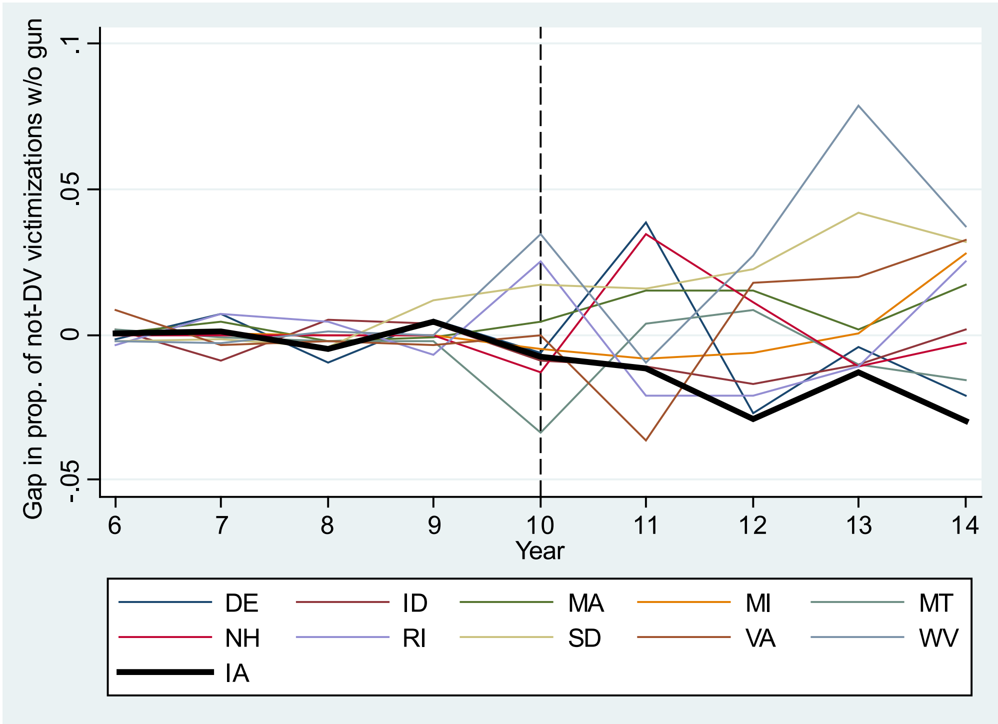
Next, I conduct the same set of analyses with IA. A SCM with just pre-intervention values of the outcome and one with those values plus the set of pre-intervention covariates generated the same results. Synthetic IA was a weighted combination of MA (62%), ND (19%), SD (15%), and DE (5%). Figure 31 shows that IA and Synthetic IA followed a similar trend prior to the gun relinquishment law going into effect in 2010. Thereafter, the proportion of not-domestic violent victimizations that did not involve a gun declined slightly in IA and remained flat in Synthetic IA. Specifically, in IA (Synthetic IA), 91% (90%) of not-domestic violent victimizations were committed with a gun in 2009. The average value over the post intervention period was 88% (90%).

Figure 31. SCM of proportion of not-domestic violent victimizations that did not involve a gun, IA



This is a small decline but one that is not meaningless (assuming there is a low probability that this finding is the result of chance) given the extreme harm that guns cause when used in acts of violence. To measure the level of uncertainty in this effect, I conduct an in-space placebo test. The results with 3 states removed due to poor fit are shown in Figure 32. Additionally, I did not include a placebo test for ND because the Hessian was found to be unstable/asymmetric in the nested optimization procedure. This figure shows that the proportion of not-domestic violent victimizations that did not involve a gun changed noticeably in many states during the post-intervention period, suggesting a good amount of uncertainty in IA's estimated effect. In fact, IA ranked 5th out of 14 in its RMSPE ratio with each lower ranked state having a lower pre-intervention RMSPE value. Therefore, this effect is not considered a statistically significant finding (p = .36)

Figure 32. Distribution of gaps in proportion of not-domestic violent victimization that did not involve a gun between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, IA



7.2.5 Lethality of severe domestic assaults

Next, I examine how the lethality of severe domestic assaults—measured as the ratio of murders to aggravated assaults—changed in TN and IA following their gun relinquishment laws going into effect and whether any changes can reasonably be attributed to these laws. Starting with TN, a SCM model with only pre-intervention values of the outcome and a model with both pre-intervention values of the outcome and the set of covariates generated the same results. The results are shown in Figure 33. Based on this figure and the gap in the pre-intervention trends shown in Figure 34, one can see that the pre-intervention trend of TN and Synthetic TN, which is a weighted combination of DE (45%), MI (45%), RI (5%), VA (3%), and SD (1%), tracked each other closely. Between 2010-13, TN and Synthetic TN's trends diverged but only to a slight degree. The average post-intervention gap between the two is equal to -0.0007.

Figure 33. SCM of lethality of severe domestic assaults, TN

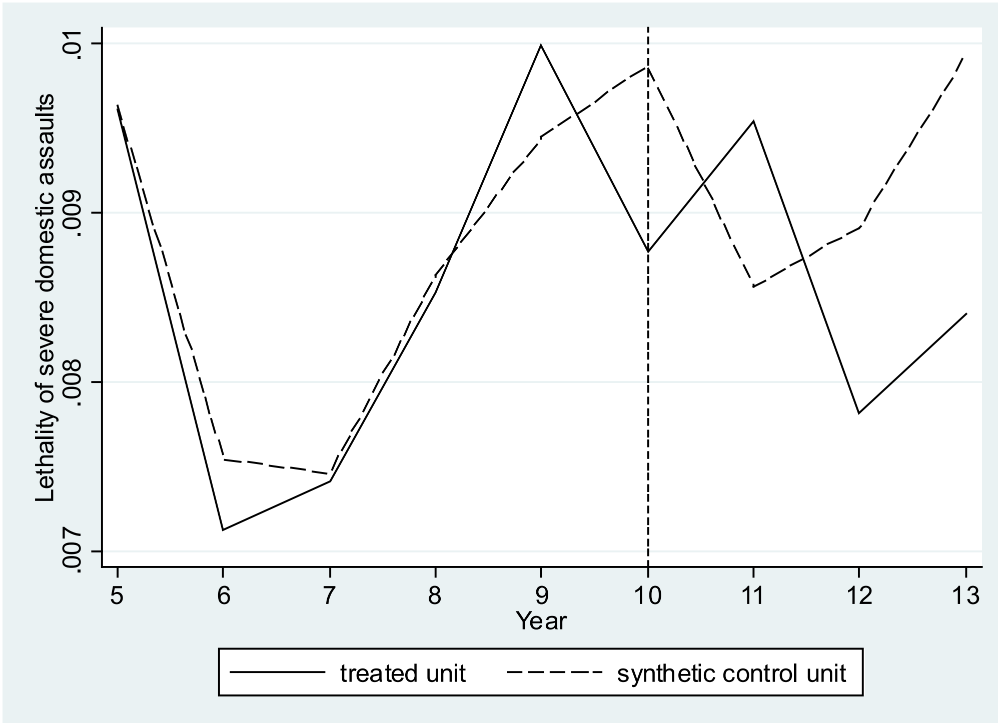
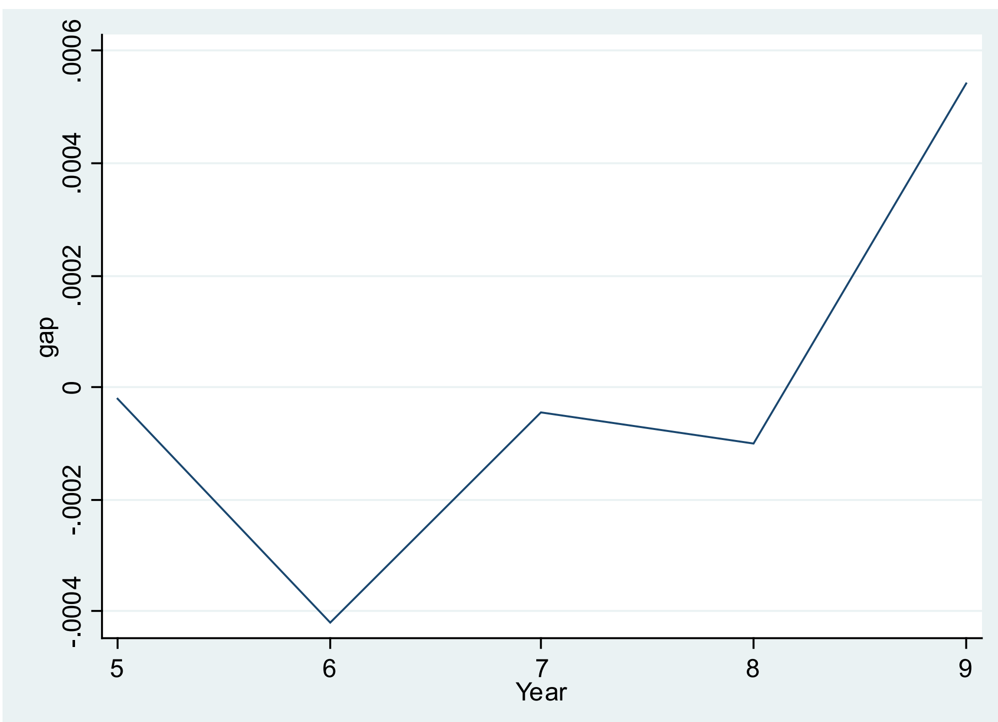
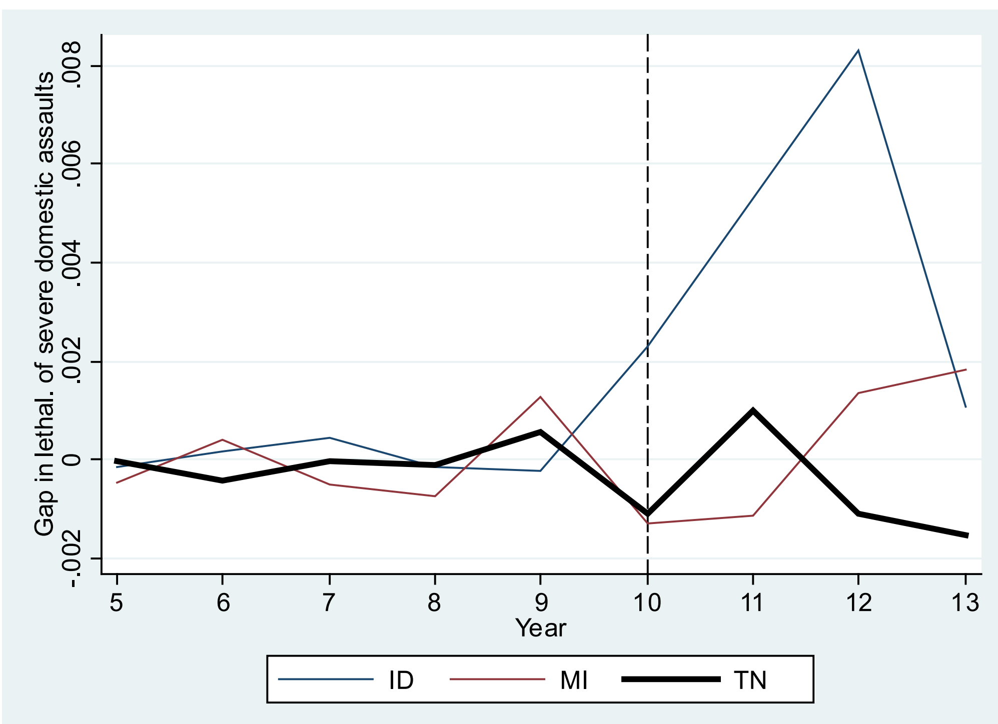


Figure 34. Gap plot of difference in lethality of severe domestic assaults between TN and Synthetic TN over the pre-intervention period



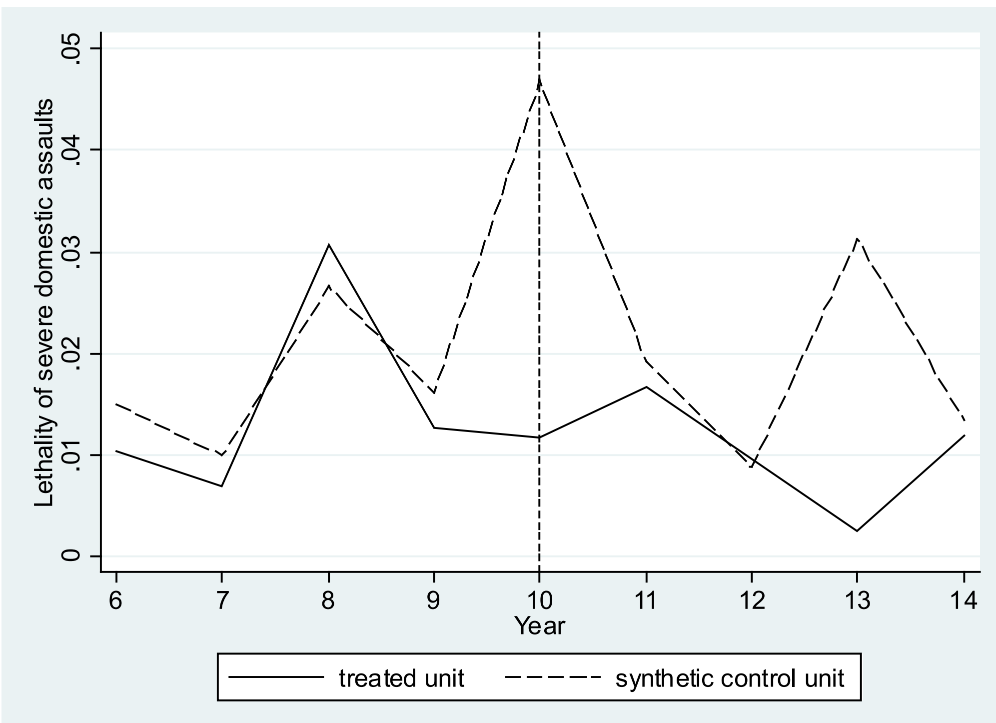
Results from an in-space placebo test show that TN ranked 3 out of 15 in its RMSPE ratio. However, one of the placebo tests that ranked higher than TN had a pre-intervention RMSPE value that was 5.5 times greater than TN's value, which was not an adequate level of pre-intervention fit. In fact, there were only 2 placebo tests that had a pre-intervention RMSPE value within two times of TN's value, and TN ranked second among them in its RMSPE ratio. This is not considered a statistically significant finding, regardless of whether one considers TN's rank out of all 15 placebo tests or out of the 3 tests with adequate pre-intervention fit. Figure 35 shows the results of the placebo test gap distribution for those placebo tests with good fit. One can see that TN's level of change at the time of the intervention was not unique, and, in fact, ID experienced relatively large changes in the post-intervention period compared to its placebo synthetic control unit. This demonstrates the uncertainty around TN's estimated effect.

Figure 35. Distribution of gaps in the lethality of severe domestic assaults between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, TN



For IA, a SCM with just pre-intervention values of the outcome and one with those values plus the set of pre-intervention covariates produced the same results. Synthetic IA was a weighted combination of RI (57%), VT (27%), MT (9%), and NH (7%). Figure 36 shows that IA and Synthetic IA followed a similar trend in their ratio of murders to aggravated assaults between 2006-2009 and had similar levels for 3 of the 5 post-intervention years. Although Synthetic IA experienced 2 years of increased lethality in the post-intervention period, these spikes may just represent natural noise. To test this, I next conducted an in-space placebo test.

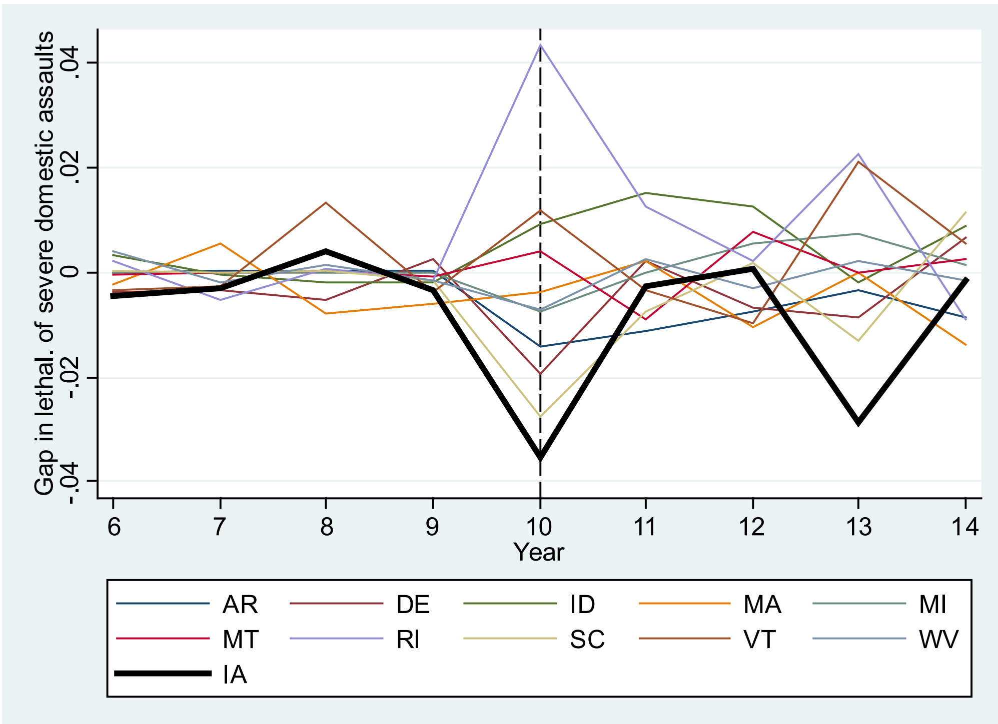
Figure 36. SCM of lethality of severe domestic assaults, IA



The in-space placebo test using all 14 donor states revealed much uncertainty in the SCM finding reported above. A Fisher's exact test results in a p-value of 0.4. However, there were multiple placebo tests with poor pre-intervention fit. Figure 37 shows the results of only the placebo tests with a pre-intervention RMSPE value that is no greater than two times the size of

IA's (Abadie et al., 2010; Cunningham, 2021). IA still ranked sixth in its post- to pre-treatment RMSPE ratio. Therefore, IA's effect is not considered statistically significant.

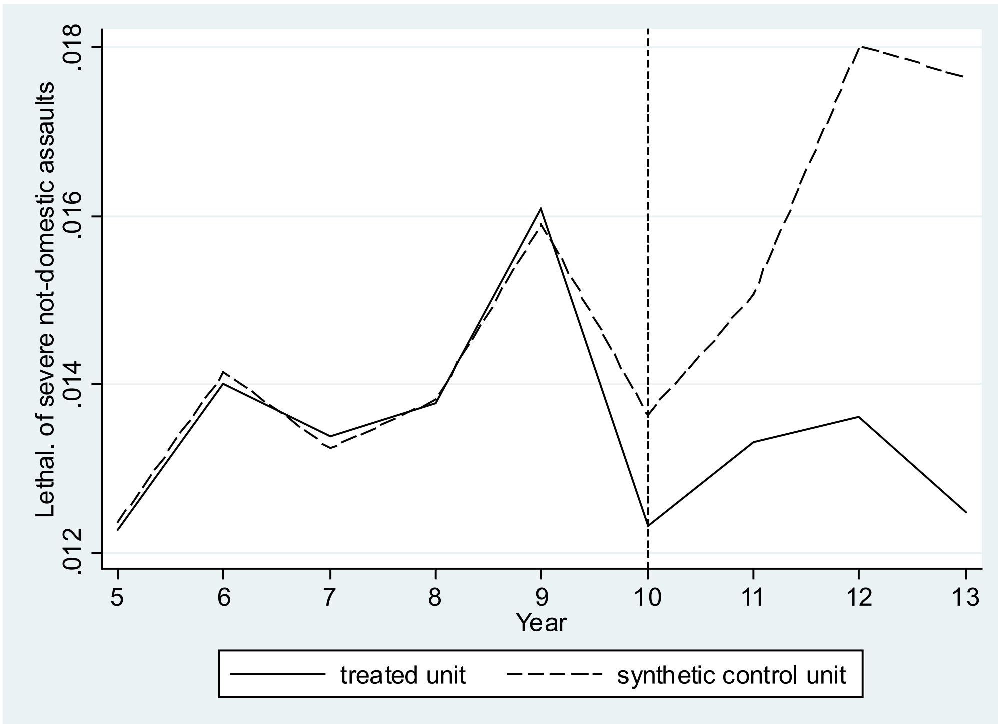
Figure 37. Distribution of gaps in the lethality of severe domestic assaults between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, IA



7.2.6 Lethality of severe not-domestic assaults

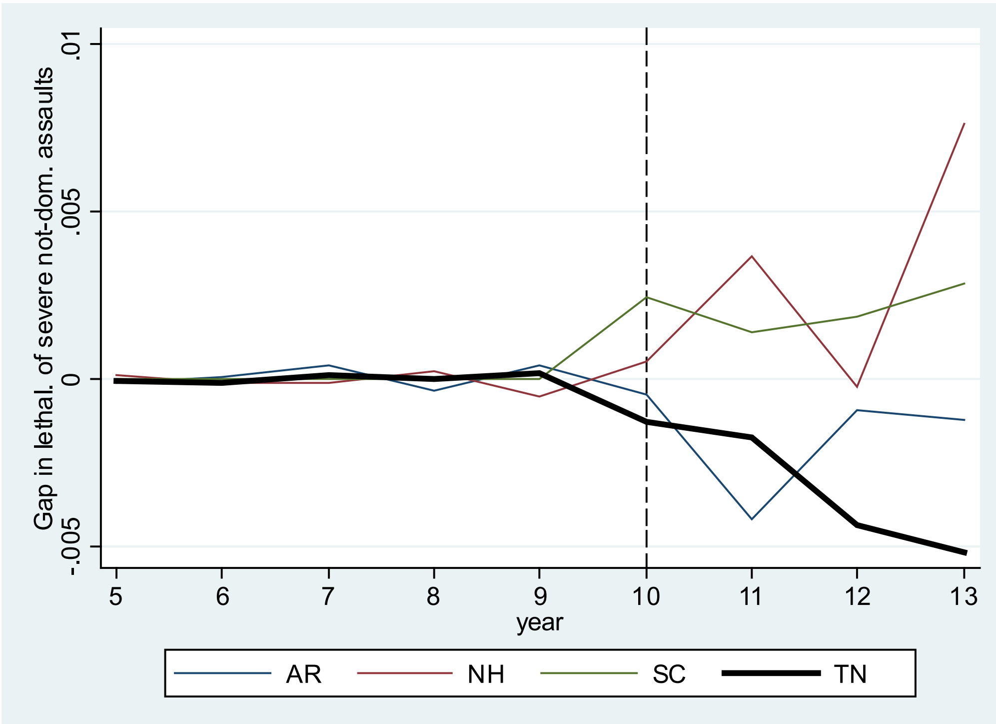
To understand whether gun relinquishment laws reduce the lethality of severe not-domestic assaults, I next conducted SCM analyses using this outcome. Starting with TN, I found that a SCM using only the pre-intervention outcome and one that also included a set of pre-intervention covariates estimated the same model. Synthetic TN was a weighted combination of MI (33%), VA (19%), RI (18%), AR (16%), and MT (14%). Figure 38 shows that TN's gun relinquishment law seemed to have a negative effect on the lethality of severe not-domestic assaults in the state. One can see that TN's trend remained relatively flat in the post-intervention period while Synthetic TN's trend increased. Specifically, between 2010-13, Synthetic TN's average ratio of not-domestic murders to not-domestic aggravated assaults was 1.24 times larger than TN's average value over this time. Next, I measure the amount of uncertainty around this estimated effect.

Figure 38. SCM of lethality of severe not-domestic assaults, TN



Based on the results of an in-space placebo test, TN ranked 2 out of 15 in its RMSPE ratio (p = .13). However, most of the placebo tests had poor pre-intervention fit. There were only 3 placebo tests with a pre-intervention RMSPE value within two times of TN's. Figure 39 shows the gap between the "treated" and synthetic control unit for each of these 3 placebo tests and TN. In addition to SC having a larger RMSPE ratio, one can see from this figure that NH experienced a post-intervention change that was similar in size to TN's. In fact, the average gap between NH and Synthetic NH during the post-intervention period was equal to 0.003 and for TN it was equal to -0.003. Together, these findings suggest that there is a good degree of uncertainty around TN's estimated effect and that it should not be considered a statistically significant finding.

Figure 39. Distribution of gaps in the lethality of severe not-domestic assaults between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, TN



Next, I conducted a SCM analysis with IA. Like with TN, the inclusion of an additional set of covariates did not change the results compared to a model using only pre-intervention values of the outcome. Here, Synthetic IA was a weighted combination of DE (52%), MA (43%), and NH (6%), whose pre-intervention trend followed TN's trend closely (see Figures 40 and 41). Their trends diverged considerably in 2010 when IA's gun relinquishment law went into effect but then slowly converged until being approximately equal in 2014. The average gap in their post-intervention values is equal to only -0.002, which is a miniscule negative effect. To measure the level of uncertainty around this estimate and to test whether this finding is statistically significant, I conducted an in-space placebo test.

Figure 40. SCM of lethality of severe not-domestic assaults, IA

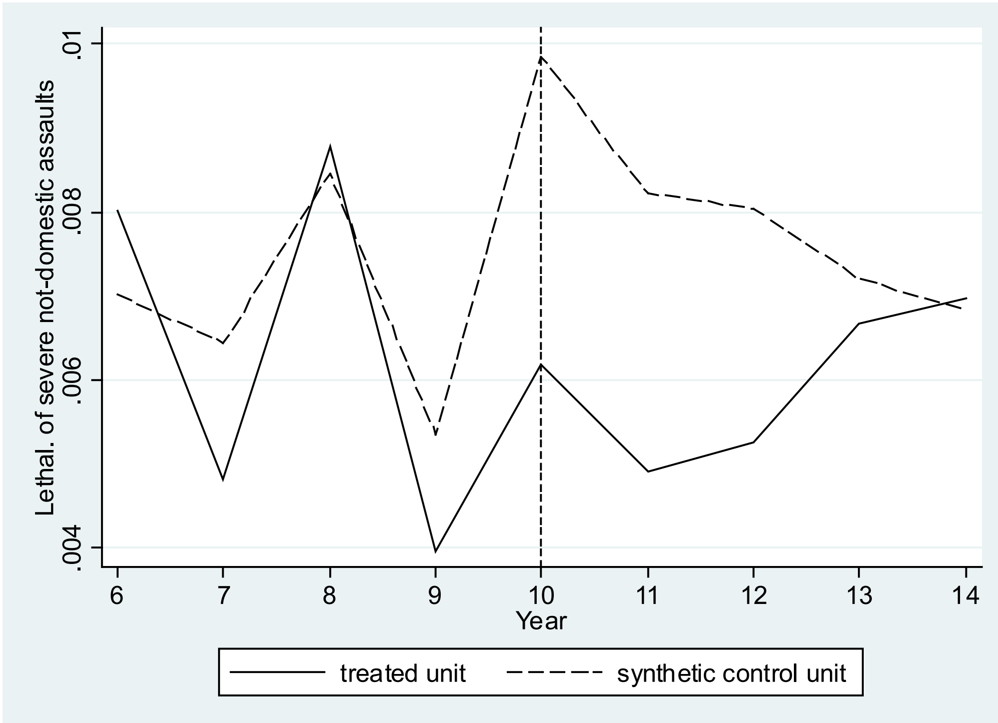
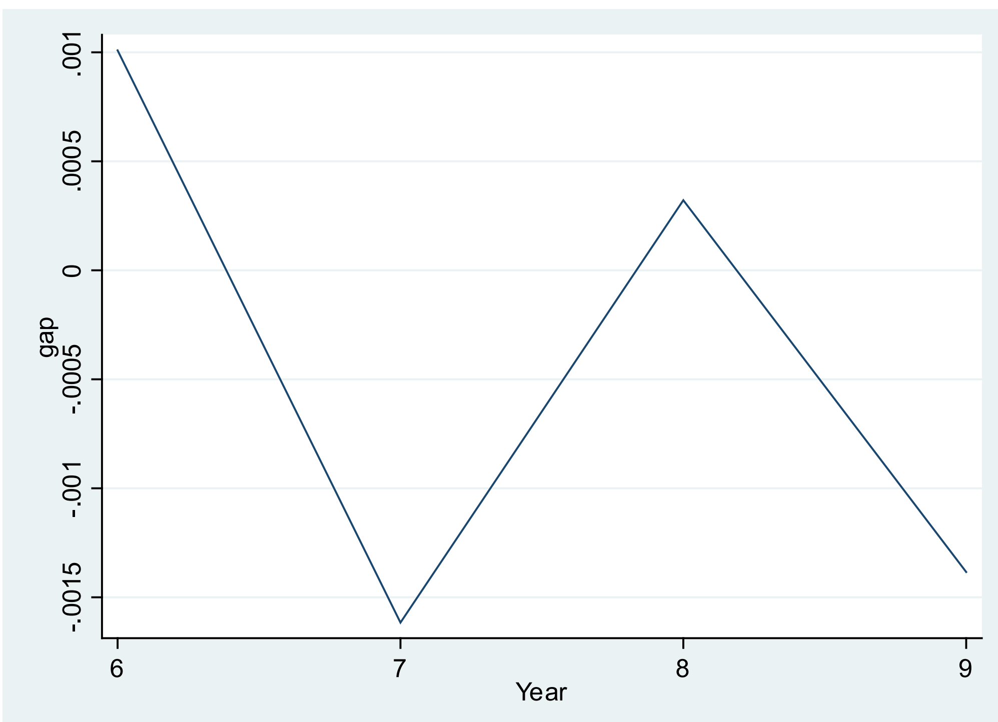
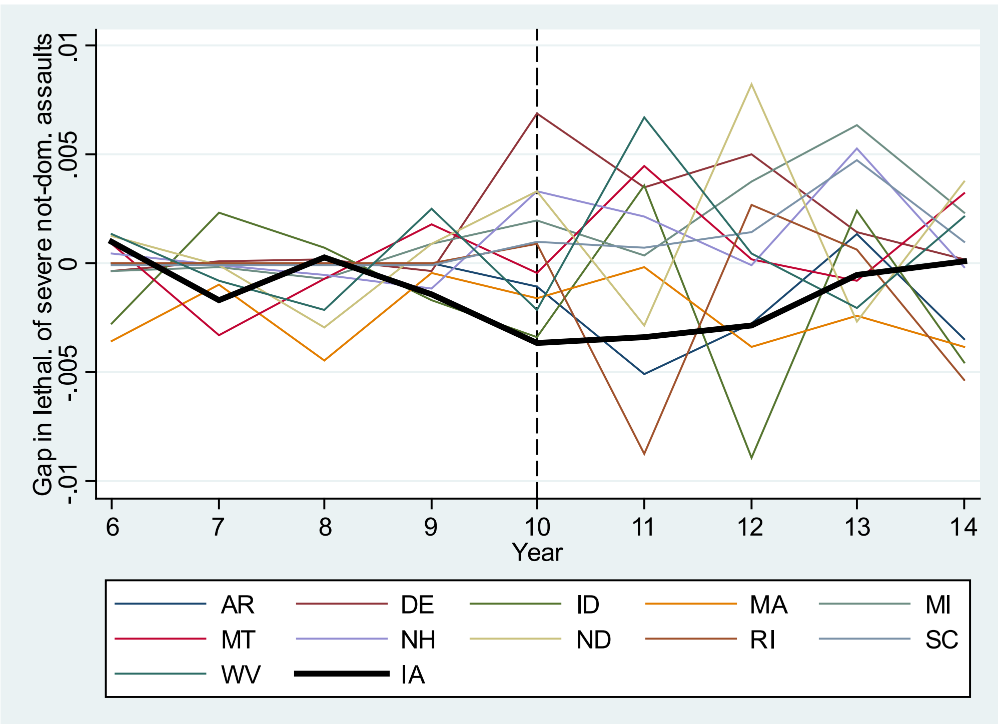


Figure 41. Gap plot of difference in lethality of severe not-domestic assaults between IA and Synthetic IA over the pre-intervention period



The result of this test shows that IA ranked ninth out of the 15 placebo tests in its RMSPE ratio (p = .6). Figure 42 shows the gaps in this outcome for each placebo test excluding 3 that had poor pre-intervention fit. One can see that the effect for IA is not unique compared to the effects experienced by placebo test states. In other words, IA's effect is estimated with a low degree of certainty and is not statistically significant.

Figure 42. Distribution of gaps in the lethality of severe not-domestic assaults between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, IA



CHAPTER 8: SUPPLEMENTAL ANALYSES USING LAW ENFORCEMENT AGENCIES AS THE UNIT OF ANALYSIS

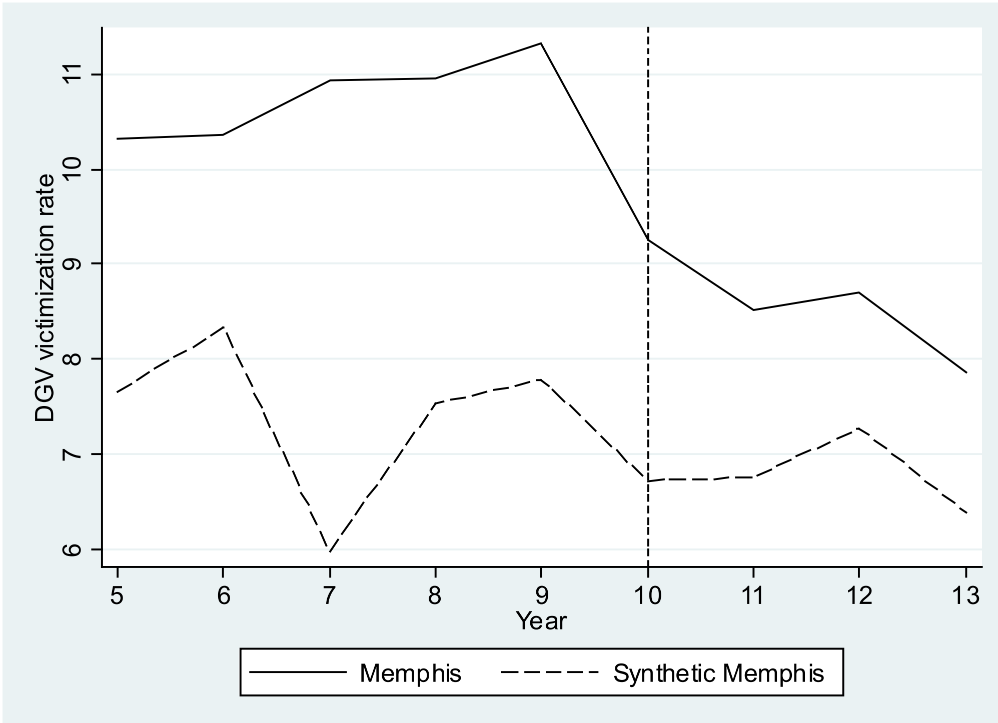
The analyses presented in the previous chapter use the state as the unit of analysis because gun relinquishment laws go into effect at the state level. Although it is beneficial to know the effects of gun relinquishment laws at this level, using the state as the unit of analysis presented problems including incomplete agency coverage in NIBRS and poor statistical power due to the small number of states in NIBRS over the observation period. To address these limitations, for both TN and IA I conducted the same analyses using the largest local law enforcement agency in the state with complete participation in NIBRS over the observation period as the treated unit and include every local law enforcement agency with complete participation in NIBRS over the observation period in the donor pool. I focus on the largest local law enforcement agency in each treatment state because violent crime is rare among many small-to-medium sized agencies and conducting a separate analysis for each agency in the state would be unmanageable. Given that the law enforcement role of state police agencies differs across states, I focus on municipal and county agencies. Although this analysis is limited in that it only estimates an effect for one large agency per treatment state, it addresses the limitations of low statistical power and incomplete agency participation/population coverage in my state-level analyses.

After removing agencies that did not report their crime data to NIBRS each year between 2005-14 or were not county or city agencies, I was left with a sample of 2,278 agencies operating in 21 states. In addition to the 17 states used in my primary analyses, this list also included agencies located in the states of Connecticut, Kansas, Oregon, and Washington. Because Colorado and Connecticut both had gun relinquishment laws go into effect in 2013 and Washington put a new gun relinquishment law into effect in 2014 (Cherney et al., 2020; Giffords Law Center to Prevent Gun Violence, n.d.), I excluded agencies in these states from the donor pool. The resulting sample includes 2,141 municipal and county law enforcement agencies from 18 states. Within this sample, the largest agency in IA is the Des Moines Police Department, which had a population of 195,000 in 2005 that increased to 208,000 in 2014. The largest agency in TN is the Memphis Police Department, which had a population of 679,000 in 2005 that decreased to 655,000 in 2014. Given the size of these agencies, I further restrict my sample to agencies with a jurisdictional population of at least 50,000 and no more than 1,000,000 people on average over the observation period. In addition to decreasing the likelihood of comparing the treated agencies to dissimilar control agencies, this addresses requirements of the SCM like that the panel is balanced and the size of the donor pool does not exceed an upper limit. These inclusion criteria limit my sample to 152 medium-to-large local law enforcement agencies. I use the same measures as those used in my primary analyses except that 1) crime rates are now measured per 10,000 persons instead of per 100,000 persons and 2) I do not test for an effect of gun relinquishment laws on the lethality of severe assaults given the rarity of murder at the agency level. I follow the order used in the Results Chapter when presenting findings from these supplemental analyses.

8.1 DGV victimization rate

Starting with the DGV victimization rate and Memphis (TN) Police Department, a SCM showed that there was no suitable synthetic control unit for Memphis. As shown in Figure 43, the intercept and the year-to-year changes of the outcome over the pre-intervention period differed between Memphis and Synthetic Memphis, which was a weighted combination of Saginaw (MI) Police Department (W = 50%) and Detroit (MI) Police Department (W = 50%). Therefore, as in my primary analyses, I next conduct an F&P SCM that relaxes the no-intercept constraint to identify a synthetic control unit that matches the treatment unit on pre-intervention changes in the outcome but not intercept values.

Figure 43. SCM of DGV victimizations per 10,000 persons, Memphis



Results from this analysis reveal that Synthetic Memphis is comprised of a weighted combination of almost every agency in the donor pool. Agencies making up 5% or more of its weight include North Little Rock (AR) Police Department (*W* = 26%), Orangeburg County (SC) Sheriff's Office (*W* = 20%), Charleston (SC) Police Department (*W* = 9%), and Horry County (SC) Police Department (*W* = 7%). Figure 44 shows that Memphis and Synthetic Memphis had nearly identical trajectories for the outcome over the pre-intervention period. Importantly, one of the strengths of the SCM is transparency about how the control group is comprised, and the fact that nearly 150 agencies contributed to Synthetic Memphis makes it difficult to judge how valid this comparison is. Additionally, the two agencies that comprised 46% of the weight of Synthetic Memphis each have jurisdiction sizes of around 65,000 persons, which is about 10 times less than Memphis's population. Therefore, I do not judge this to be a valid comparison group. However, because some readers might disagree, I next conduct in-space placebo tests to measure uncertainty around this effect and to calculate an exact p-value.

Figure 44. F&P SCM of DGV victimizations per 10,000 persons, Memphis

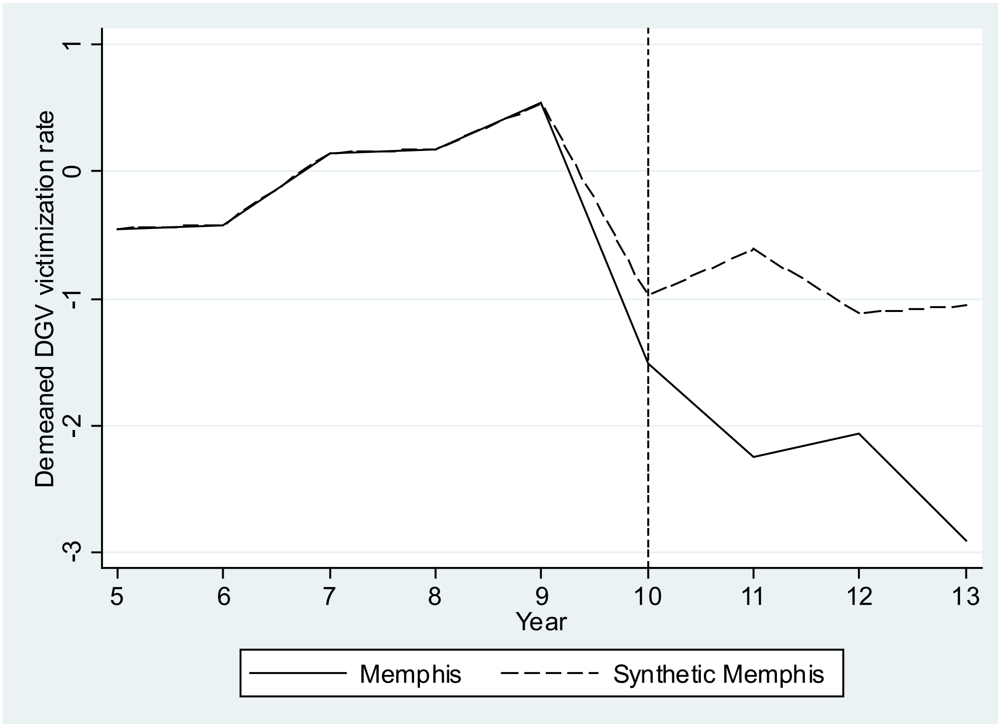
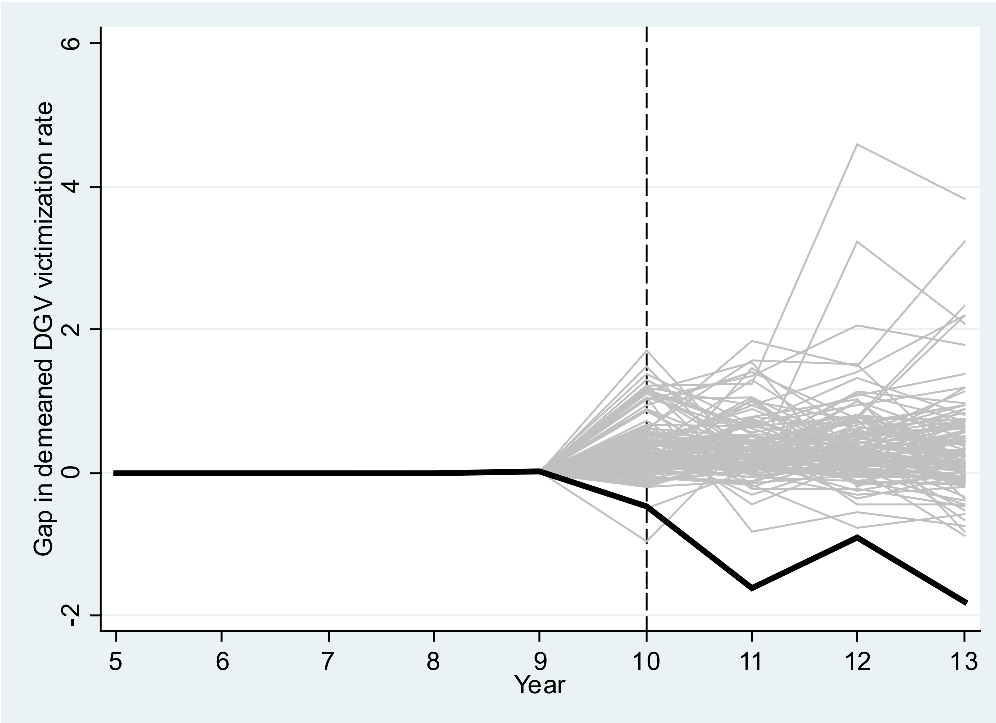


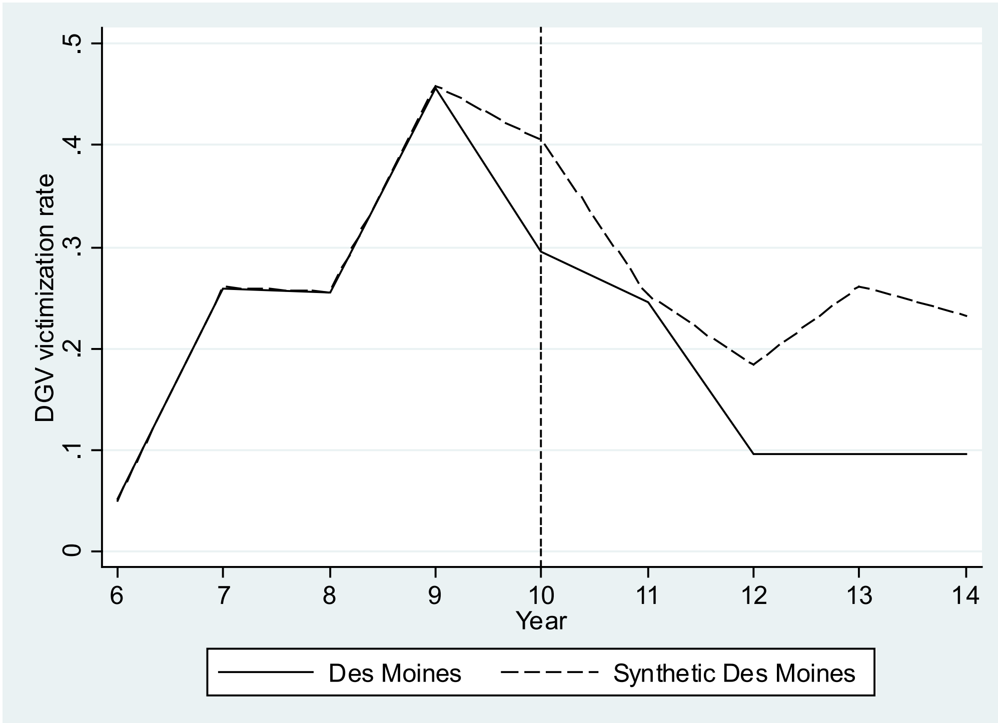
Figure 45 shows that the estimated effect for Memphis (bold, black line) was one of the largest estimated across the distribution of 121 placebo tests with good model fit (dim grey lines). Memphis's post-intervention to pre-intervention RMSPE ratio ranked 10th of out 122, which is an exact p-value equal to 0.08. This effect is not statistically significant according to the traditional benchmark of 0.05.

Figure 45. Distribution of gaps in demeaned DGV victimization rate between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, Memphis



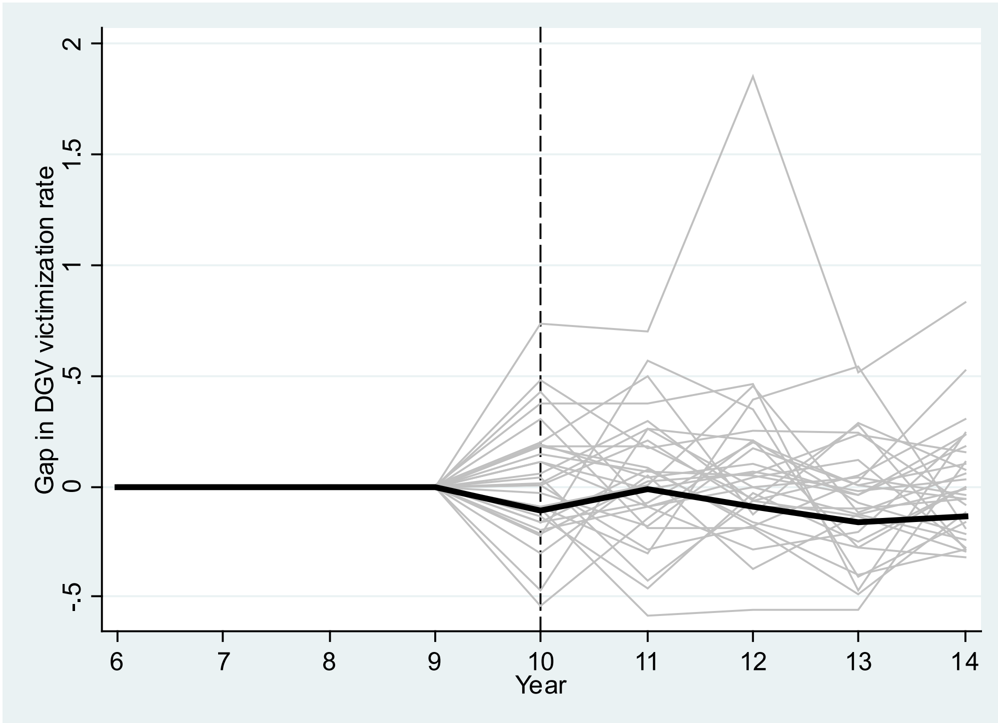
Next, I test whether IA's gun relinquishment law impacted the DGV victimization rate in Des Moines, IA. The result of a SCM analysis is shown in figure 46. In this model, Synthetic Des Moines (IA) Police Department is a weighted combination of almost every agency in the donor pool, with 3 agencies comprising more than 5% of its weight: Raleigh County (WV) Sheriff's Office (*W* = 22%), York County (VA) Sheriff's Office (*W* = 18%), and Wyoming (MI) Police Department (*W* = 10%). This figure shows that IA's gun relinquishment law appeared to reduce the DGV victimization rate in Des Moines. Compared to the pre-intervention period, Des Moines's DGV victimization rate declined by 0.09 victimizations per 10,000 persons after 2010. On the other hand, Synthetic Des Moines's DGV victimization rate increased by 0.01 victimizations per 10,000 persons for an overall effect size equal to 0.1 fewer victimizations per 10,000 persons. Next, I evaluate whether this is a statistically significant finding by conducting a series of in-space placebo tests.

Figure 46. SCM of DGV victimizations per 10,000 persons, Des Moines



Because domestic violence was defined more narrowly in the Des Moines (IA) Police Department analysis compared to the Memphis (TN) Police Department analysis given the differences across states in who is protected by the law, some agencies had very low DGV victimization rates over the pre-intervention period in the Des Moines (IA) Police Department analysis. For this reason, 66 of the 152 placebo tests failed to estimate a synthetic control unit using agencies in the donor pool. Of the 85 successful placebo tests not counting the Des Moines test, 55 tests had poor model fit over the pre-intervention fit. Of the 31 tests with good model fit, Des Moines ranked 21st in its RMSPE ratio (p = 0.68). The distribution of estimated effects for the 31 placebo tests is shown in figure 47 with Des Moines (IA) Police Department's effect bolded and in black. This figure shows that IA's effect was not large compared to the distribution of estimated good-fitting placebo test effects.

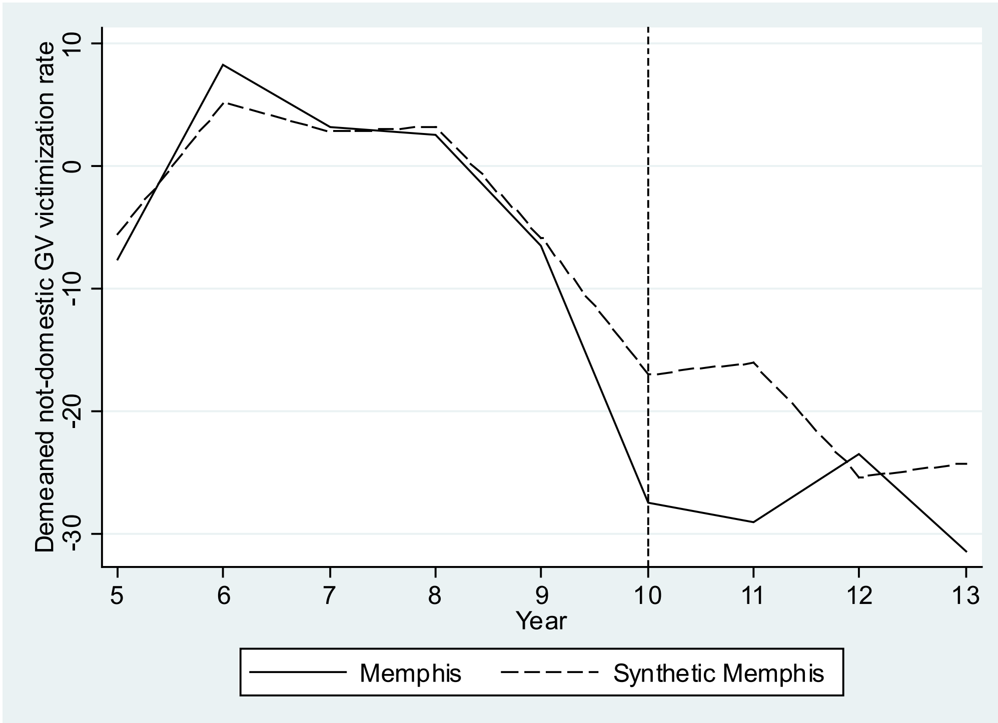
Figure 47. Distribution of gaps in DGV victimization rate between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, Des Moines



8.2 Not-domestic GV victimization rate

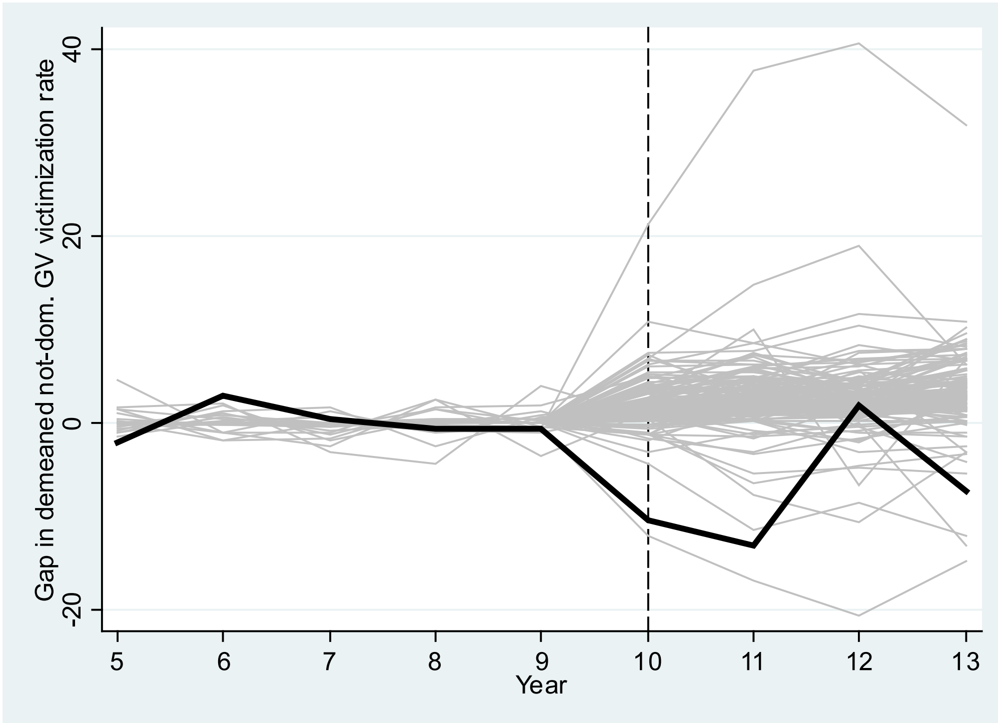
Moving to the not-domestic GV victimization rate in Memphis, TN, I again conducted the F&P SCM due to imperfect pre-intervention fit between Memphis (TN) Police Department and its estimated synthetic control unit. The result of this model is shown in figure 48. In this model, Synthetic Memphis was a weighted combination of Charleston County (SC) Sheriff's Office (*W* = 67%) and North Charleston (SC) Police Department (*W* = 33%). This figure shows that Memphis and Synthetic Memphis had similar trajectories of the demeaned outcome over the pre-intervention period and Memphis had lower values for 3 of the 4 post-intervention periods. Although this finding suggests a negative effect of TN's gun relinquishment law on its rate of not-domestic gun violence, this effect is not estimated with certainty. To understand the level of uncertainty around this estimated effect and to test if the effect is statistically significant, I next conduct a series of in-space placebo tests.

Figure 48. F&P SCM of not-domestic GV victimizations per 10,000 persons, Memphis



After removing 2 placebo tests with a pre-intervention RMSPE value that was more than 2 times the value of Memphis's test, the effect for Memphis (TN) Police Department ranked 145 out of 150 according to its RMSPE ratio ($p = 0.97$). Figure 49 shows that although Memphis had one of the larger post-intervention effect sizes, it had relatively poor model fit based on the pre-intervention period. This explains why it's exact p-value has a value close to 1.0. According to this test, the finding show in Figure 48 is not statistically significant.

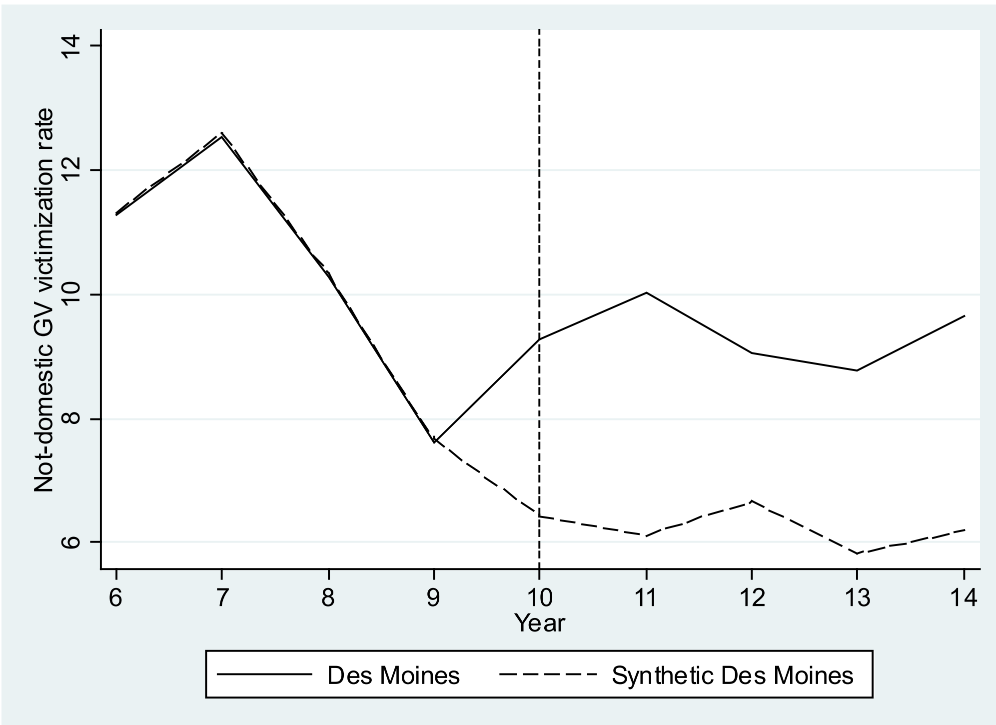
Figure 49. Distribution of gaps in demeaned not-domestic GV victimization rate between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, Memphis



Next, I test whether IA's gun relinquishment law impacted Des Moines's not-domestic GV victimization rate. The result of a SCM analysis is shown in figure 50. Synthetic Des Moines was a weighted combination of almost every agency in the donor pool with the following agencies comprising more than 5% of its weight: Haverhill (MA) Police Department (*W* = 12%), Sumter County (SC) Sheriff's Office (*W* = 11%), Berrien County (MI) Sheriff's Office (*W* = 8%), and North Charleston (SC) Police Department (*W* = 5%). The remaining 147 agencies with a non-zero weight each comprised less than 5% of the weight of Synthetic Des Moines. One can see from figure 50 that IA's gun relinquishment law was associated with an increase in Des Moines's not-domestic GV victimization rate compared to its synthetic control unit. Although they followed a similar declining trajectory over the pre-intervention period, Des Moines's and Synthetic Des Moines's trajectories diverged in 2010 when IA's gun relinquishment law took effect. Des Moines's not-domestic GV victimization rate reversed its trend and hovered around

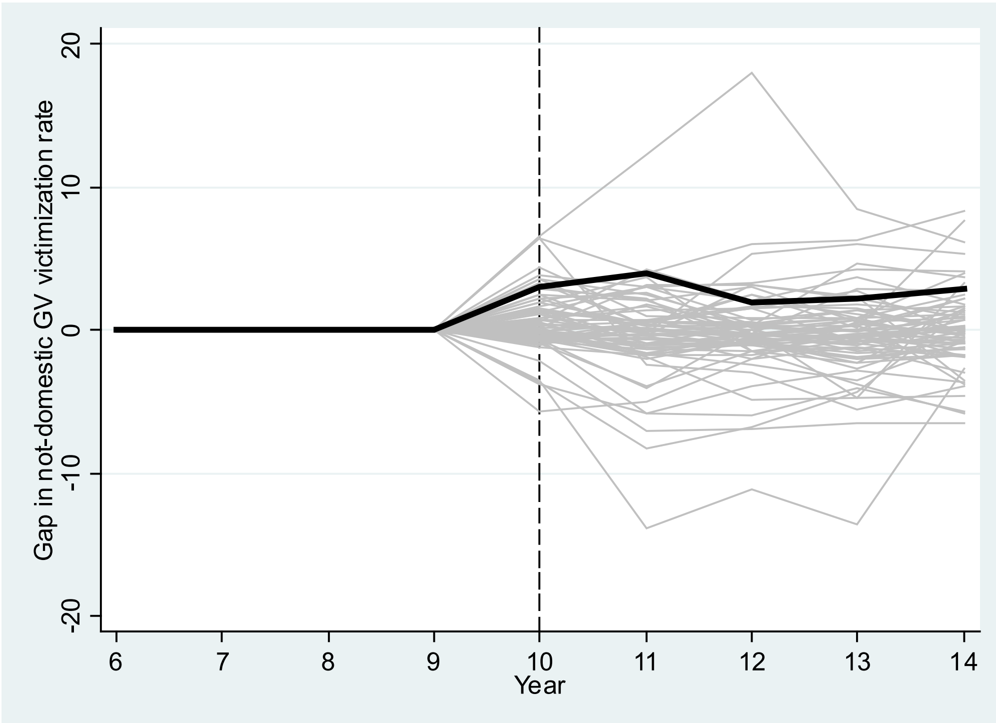
9.0 victimizations per 10,000 persons while Synthetic Des Moines continued its trend and hovered around a rate of 6.0 victimizations per 10,000 persons in the post-intervention period.

Figure 50. SCM of not-domestic GV victimizations per 10,000 persons, Des Moines



Although this is a surprising finding, the effect is not estimated with certainty. Therefore, it is important to measure uncertainty around the estimated effect and test whether it is a statistically significant finding. Results from a series of in-space placebo tests reveal that of the 71 placebo tests with good model fit, Des Moines (IA) Police Department's effect ranked 33 in the RMSPE ratio, which is an exact p-value equal to 0.46. Therefore, the finding shown in figure 50 is not statistically significant. Figure 51 shows the distribution of placebo test effects with Des Moines (IA) Police Department's effect bolded in black. From this figure, one can see that multiple placebo tests had similarly sized or larger differences in the post-intervention period compared to their synthetic control units than Des Moines (IA) Police Department did.

Figure 51. Distribution of gaps in not-domestic GV victimization rate between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, Des Moines



8.3 Weapon substitution in DV victimizations

Next, I estimate whether TN's gun relinquishment law impacted the proportion of DV victimizations that involved a weapon other than a gun in Memphis, TN. Since the SCM requires a balanced panel to estimate a synthetic control unit, and in cases where an agency recorded no instances of GV in a year the denominator will equal zero, I dropped 3 agencies from the analysis where this occurred. The result of this SCM analysis is presented in figure 52. Synthetic Memphis (TN) Police Department was a weighted combination of 130 of the 148 agencies in the donor pool, with the following agencies comprising more than 5% of its weight: Henry County (VA) Sheriff's Office (W = 56%) and Lancaster County (SC) Sheriff's Office (W = 25%). One can see from the figure that Memphis and Synthetic Memphis followed nearly identical trajectories during the pre-intervention period. After an upward spike in 2010, Synthetic Memphis's post-intervention trajectory declined while Memphis's trajectory increased and then stayed roughly level during the post-intervention period. This finding suggests that TN's gun relinquishment law prevented Memphis from experiencing an increase in the proportion of DV victimizations that involved a gun over the post-intervention period. Next, I measure uncertainty around this effect and test whether it is a statistically significant finding.

Figure 52. SCM of proportion of DV victimizations that did not involve a gun, Memphis

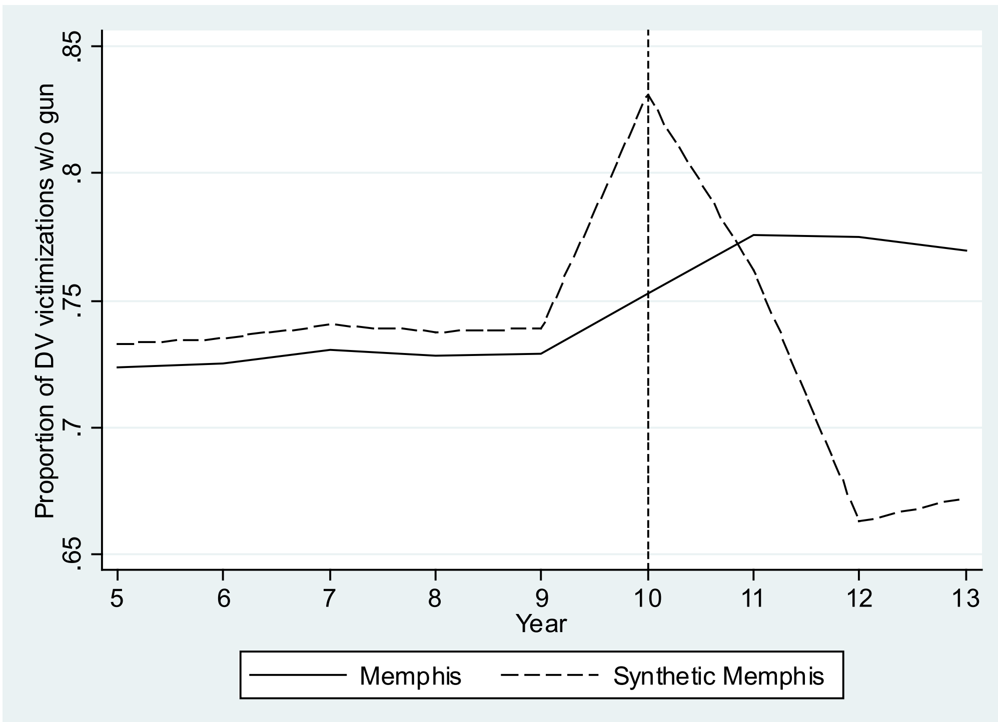
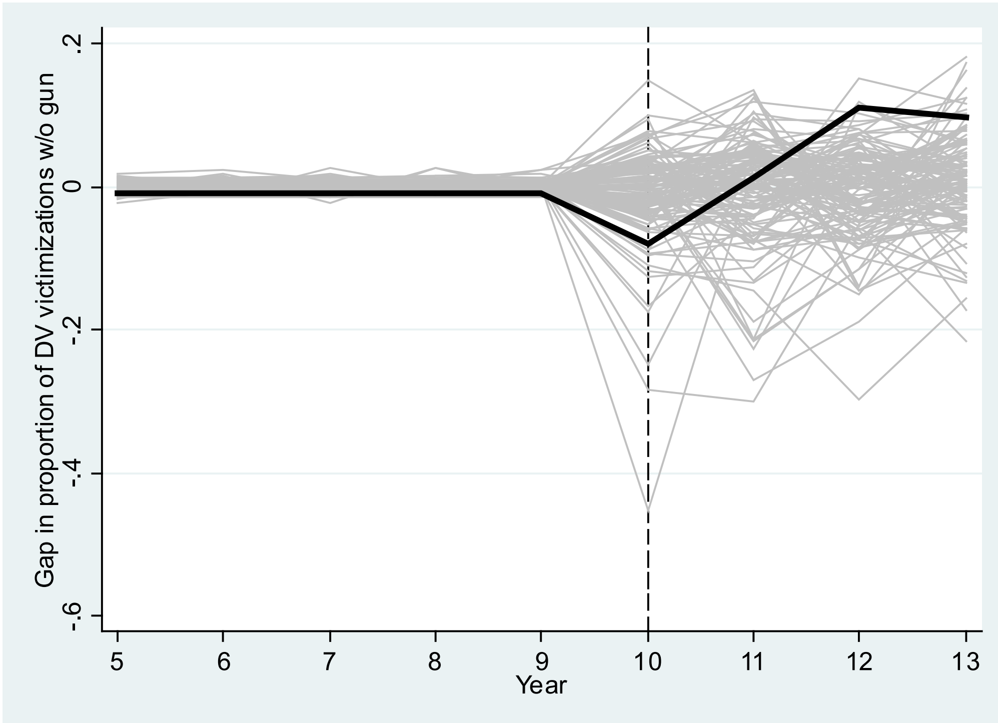


Figure 53 shows the distribution of placebo test effects along with Memphis's effect (bold and in black) for the 128 tests with good model fit. Of these estimated effects, Memphis (TN) Police Department's RMSPE ratio ranked 95th (p = 0.74), meaning it is not a statistically significant effect.

Figure 53. Distribution of gaps in proportion of DV victimizations that did not involve a gun between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, Memphis



Next, I conducted the same analyses using Des Moines (IA) Police Department as the treated unit. I dropped 7 agencies from the analysis that did not record an instance of GV for at least one year during the observation period. The result of a SCM analysis is presented in figure 54. Synthetic Des Moines is a weighted combination of nearly every agency in the donor pool with only 1 agency representing more than 5% of its weight: York County (VA) Sheriff's Office (*W* = 12%). With so many agencies making up Des Moines (IA) Police Department's synthetic control unit and no agencies making up a large share, it is impossible to determine the face validity of this control group. Of course, if the estimated effect is not statistically significant, how equivalent Des Moines (IA) Police Department is to its estimated synthetic control unit is a moot issue. Therefore, I proceed with estimating the uncertainty around this effect to determine if a more comprehensive analysis of comparability is warranted.

Figure 54. SCM of proportion of DV victimizations that did not involve a gun, Des Moines

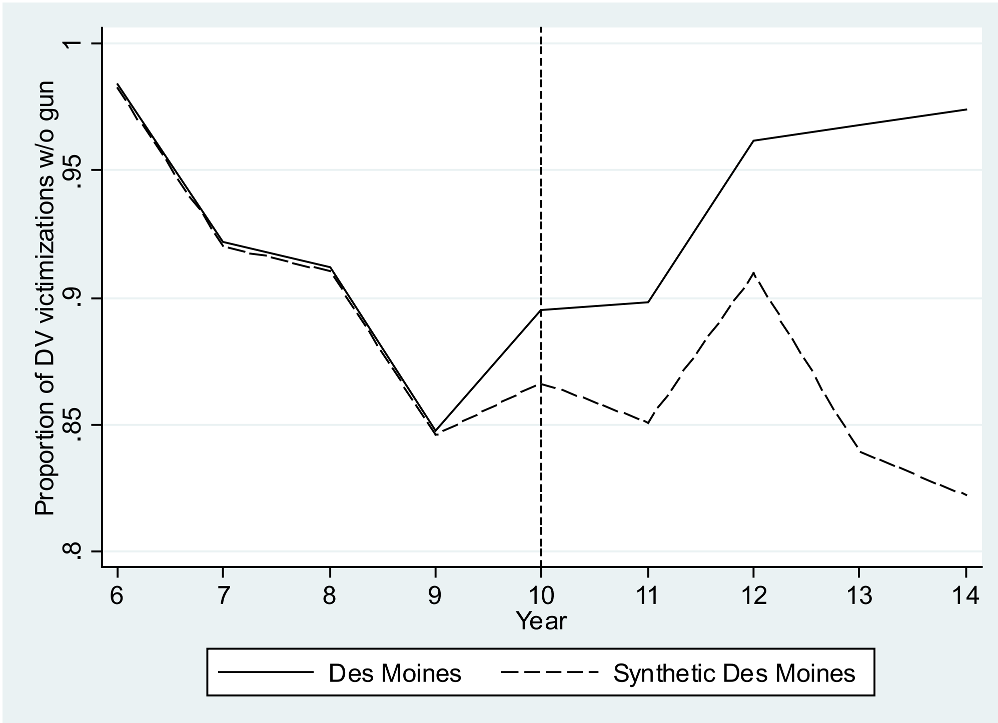
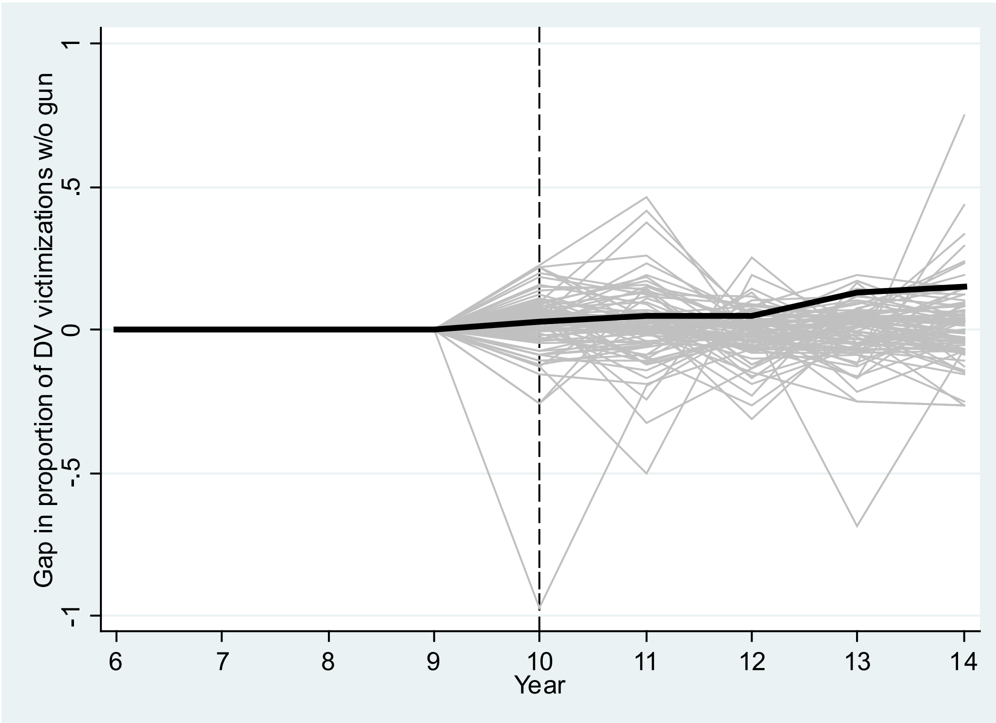


Figure 55 presents the result of a series of in-space placebo tests. One can see from this figure that the estimated effect for the Des Moines (IA) Police Department looks similar in size to many estimated effects among the placebo test distribution. In fact, among the placebo tests with good model fit, Des Moines ranked 50 out of 84 (p = 0.60). Therefore, the effect shown in figure 54 should not be considered a statistically significant finding.

Figure 55. Distribution of gaps in proportion of DV victimizations that did not involve a gun between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, Des Moines



8.4 Weapon substitution in not-domestic violent victimizations

Next, I estimate the impact of gun relinquishment laws on weapon substitution in not-domestic violent victimizations. Beginning with Memphis (TN) Police Department, I remove 2 agencies from the analysis because they did not record at least one not-domestic GV victimization for each year of the observation period. An initial SCM analysis resulted in a poor-fitting model (figure 56). Therefore, I conducted an F&P SCM analysis (figure 57). In the latter model, Synthetic Memphis is a weighted combination of every agency in the donor pool with the largest individual weight equal to 3%. Given that this model both relaxes the no-intercept constraint and no single agency contributes a meaningful percentage of the synthetic control unit's total weight, I do not treat the estimated effect as valid and therefore I do not test whether it is statistically significant.

Figure 56. SCM of proportion of not-domestic violent victimizations that did not involve a gun, Memphis

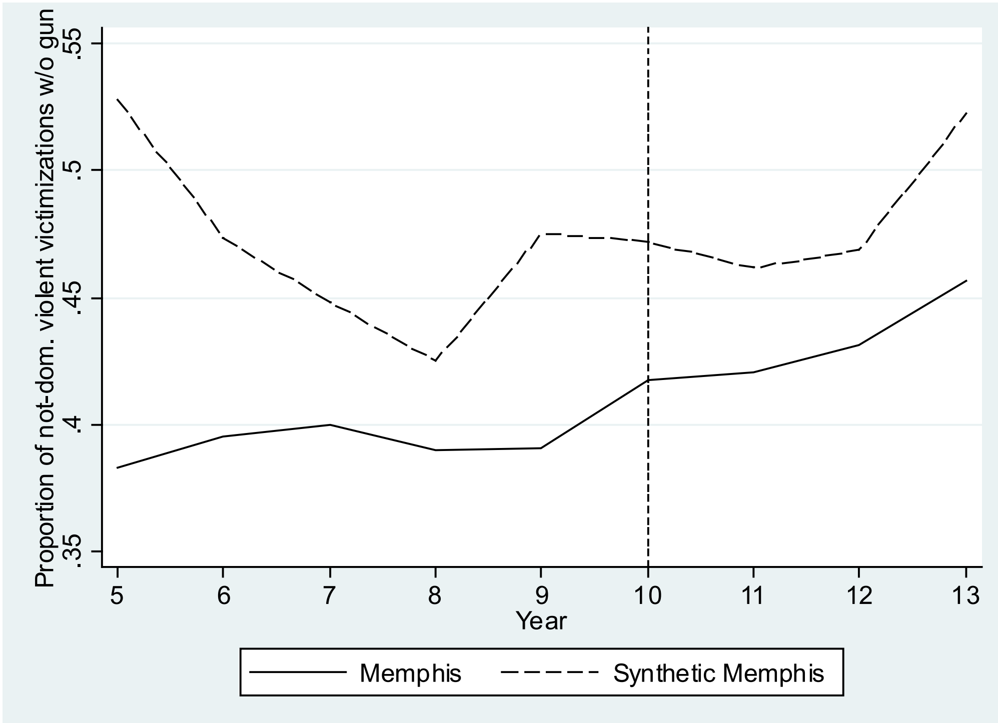
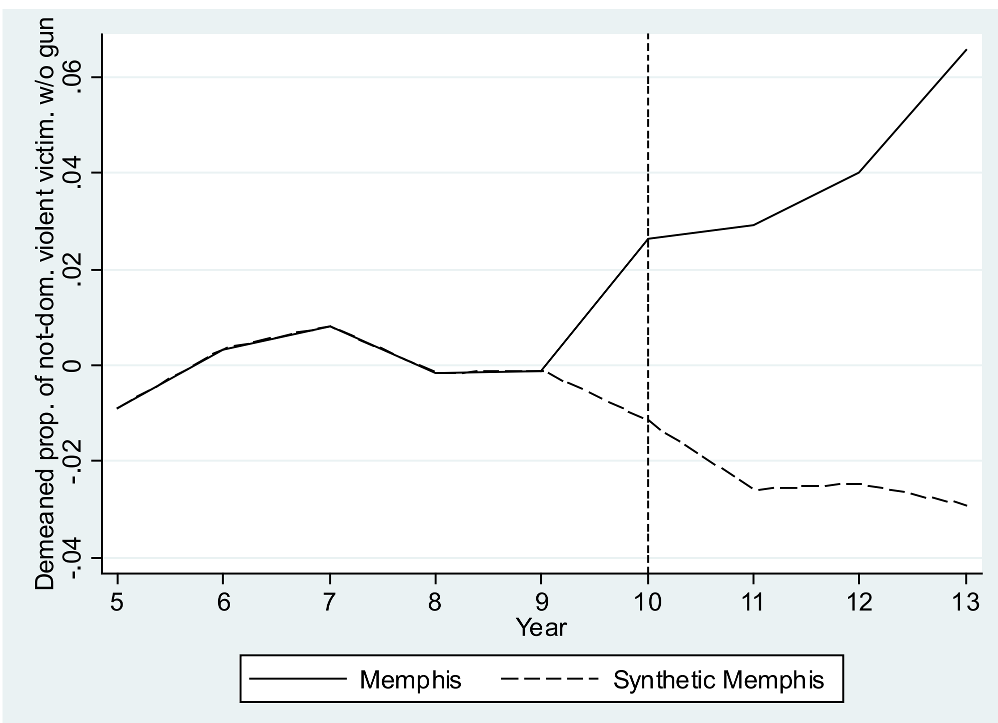
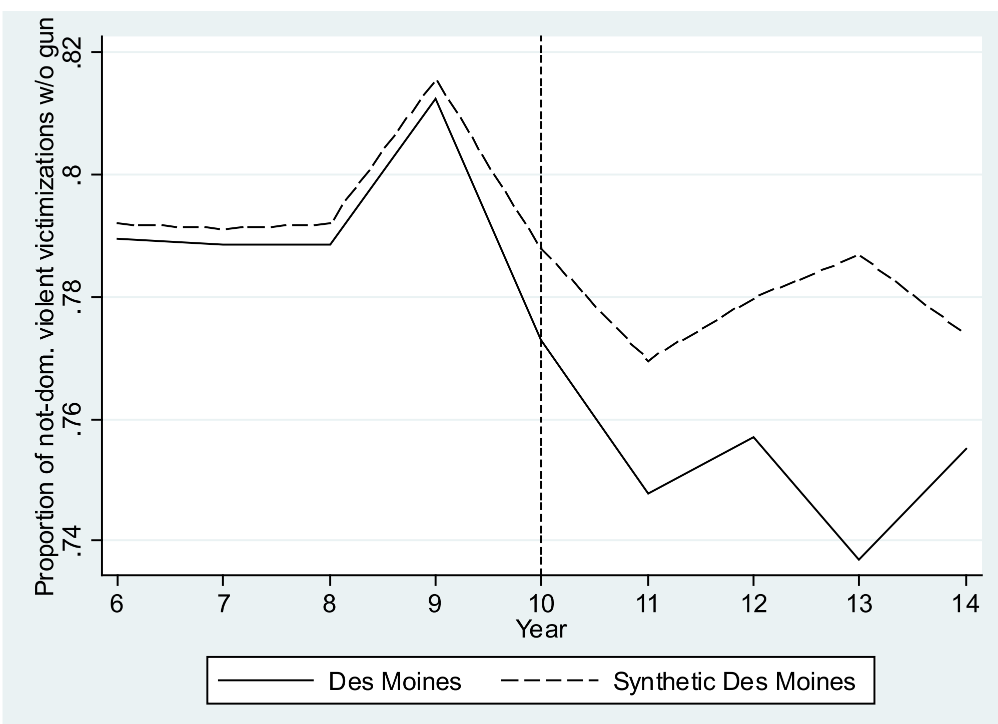


Figure 57. F&P SCM of proportion of not-domestic violent victimizations that did not involve a gun, Memphis



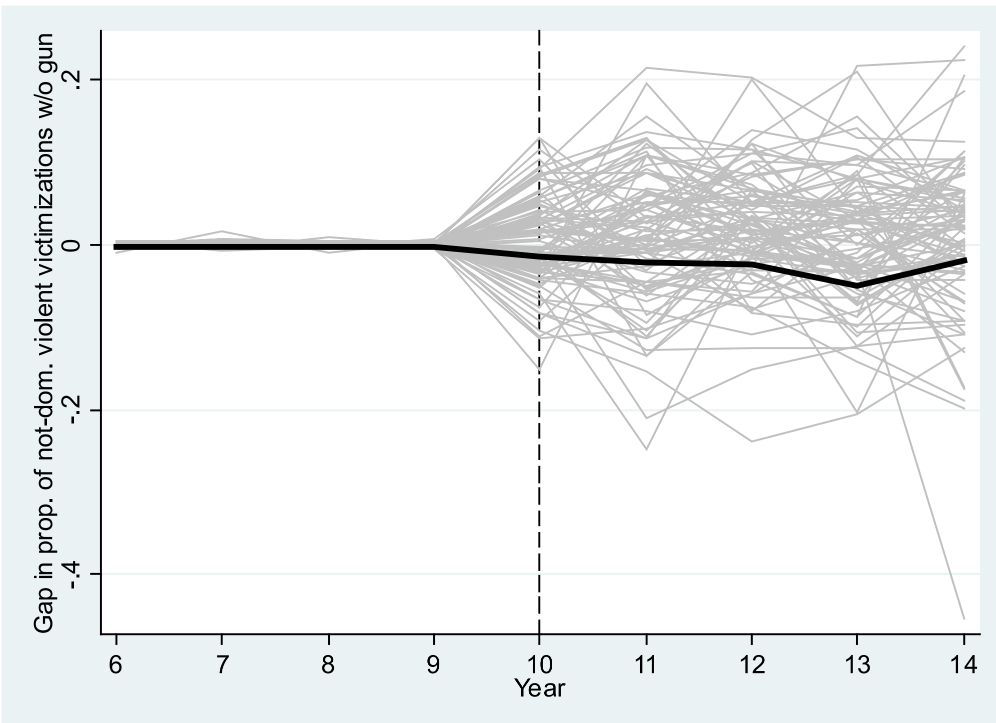
Moving to the Des Moines (IA) Police Department analysis, I remove 1 agency for not recording a not-domestic GV victimization each year of the observation period. Then, I estimate a SCM using the proportion of not-domestic violent victimizations that did not involve a gun as the outcome. The result of this analysis is presented in figure 58, which shows an effect in the opposite direction of what I predicted. As with the Memphis analysis above, the synthetic control unit was comprised of every agency in the donor pool, with only 1 agency contributing a weight that was greater than or equal to 5%: Canyon County (ID) Sheriff's Office (W = 9%). Despite the difficulty in interpreting the validity of Synthetic Des Moines based on its composition, I (cautiously) treat it as a real effect because it aligns closely with both Des Moines (IA) Police Department's intercept and year-to-year changes of the outcome over the pre-intervention period. Therefore, I next conduct a set of in-space placebo tests to measure the amount of uncertainty around and the statistical significance of this estimated effect.

Figure 58. SCM of proportion of not-domestic violent victimizations that did not involve a gun, Des Moines



Based on this analysis, I determine that the estimated effect shown in figure 58 is not statistically significant. Figure 59 shows the distribution of estimated effects among the placebo tests and Des Moines (IA) Police Department. This shows that Des Moines's effect is not extreme compared to the distribution of placebo effects. In fact, of the 95 tests with good model fit, Des Moines ranks 89th in its RMSPE ratio ($p = 0.94$).

Figure 59. Distribution of gaps in proportion of not-domestic violent victimizations that did not involve a gun between iteratively assigned treated units and estimated synthetic control units, good-fitting tests, Des Moines



CHAPTER 9: SUPPLEMENTAL ANALYSES USING DATA FROM THE SUPPLEMENTARY HOMICIDE REPORTS

Although the supplemental analyses presented in the prior chapter address the lack of statistical power in my primary analyses, they do not address another limitation, which is the short pre-intervention period. In the following supplemental analyses, I analyze data from the SHR to address both limitations, albeit while introducing new limitations. As previously discussed, prior state-level research on the relationship between gun relinquishment laws for DV offenses and DGV has used data from the SHR and a measure of IPH as an outcome. This data collection has some benefits over NIBRS, including much higher agency coverage over time. However, as mentioned, use of these data to study gun relinquishment laws is limited by the data's inclusion of only one rare crime type. Gun relinquishment laws are designed to prevent multiple forms of GV, and nonfatal GV occurs much more frequently than fatal GV. Therefore, the use of these data to study gun relinquishment laws may result in researchers missing important effects of these laws on nonfatal forms of GV. Additionally, unlike NIBRS which measures the relationship type between every victim and offender in a criminal incident, SHR only measures the relationship type between offenders and victims for one victim per incident. This can bias analyses based on the victim-offender relationship type if violent incidents involve multiple victims in different types of relationships with the offender(s).

Still, despite the measurement-related benefits of NIBRS over SHR, NIBRS is limited by having much less participation by law enforcement agencies compared to SHR, especially going further back in time. These characteristics of NIBRS led to this dissertation's analyses having two key weaknesses. The first is a short pre-intervention period, which can lead to biased findings since the validity of the SCM is based on the treated and synthetic control unit following a shared trajectory for an outcome prior to the intervention (Abadie, 2021). The second is having few states to use in permutation tests to calculate a Fisher's exact p-value, which is essentially an issue of low statistical power (Athey & Imbens, 2017; Fisher, 1935). With these benefits and limitations in mind, I conduct a series of supplemental analyses to test whether the findings in this dissertation differ when using SHR data to test similar relationships.

In the SHR data, there were 38 states with population coverage of 80% or more for at least 80% of the 30 years between 1985 and 2014, including during the post-intervention years. I selected 1985 as the start of the observation period to balance the desire for a long pre-intervention period with the desire to retain as many states with adequate coverage as possible for the analyses. I selected 2014 as the end of the observation period to stay consistent with my primary analyses and to better ensure that changes in outcomes during the post-intervention period are related to the gun relinquishment laws going into effect. States not included in this group were: Alaska, Florida, Illinois, Indiana, Kansas, Kentucky, Mississippi, Montana, New Hampshire, New Mexico, Ohio, and South Dakota. Although Nebraska was initially included based on these criteria, I excluded it because Omaha (NE) Police Department did not report to the SHR for the years 1993-2003, and this city makes up approximately 25% of the state's population and is one of the few major metropolitan areas in the state. Additionally, I excluded the following states because they had gun relinquishment laws go into effect in the years immediately surrounding the dates that TN and IA's laws went into effect: California (2013), Colorado (2013), Connecticut (2013), Maryland (2009), Minnesota (2014), Nevada (2007), and Pennsylvania (2006). In all, this leaves 30 states to use in my analyses, including TN and IA.

Next, I reconstructed the DV measures for each analysis based first on TN's law and then on IA's law. SHR included most of the values used to define DV in TN using NIBRS response options. These included offenders who were a current or former spouse, common-law spouse, dating partner, in a homosexual relationship, in-law, parent, child, sibling, stepparent, stepchild, and other family. It did not include victims who were a child of a boyfriend or girlfriend of the offender or offenders who were grandparents, grandchildren, or stepsiblings. However, many of these relationships are likely captured in the "other family" value in SHR. IA's SHR DV measure was identical to its NIBRS measure except for the omission of offenders who were grandchildren, which is likely to be captured as an "other family" relationship, and victims who were a child of a boyfriend or girlfriend of the offender. Like with NIBRS, these definitions do not include every relationship type covered by TN and IA's gun relinquishment laws, such as former dating partners and cohabitants. In TN, 20% of all murders and nonnegligent manslaughters that occurred between 1985-2014 were related to DV, 54% were not related to DV, and 26% were missing information on the relationship between the first victim and any offenders. For IA, these figures were 17%, 59%, and 24%, respectively.

Like with my primary analyses, if the weapon type was missing, I assumed it was not a gun. Although NIBRS provides a more detailed breakdown of firearm type, SHR includes the same broad types: handgun, shotgun, rifle, other firearm, and firearm – type not stated. In TN, 68% of murders were committed with a gun, including 57% of domestic murders and 71% of not-domestic murders. In IA, 49% of murders were committed with a gun, including 43% of domestic murders and 47% of not-domestic murders. I used these measures to construct 4 of the 6 outcomes used in my primary analyses: the domestic gun murder rate, the not-domestic gun murder rate, the proportion of domestic murders involving a weapon other than a gun, and the proportion of not-domestic murders involving a weapon other than a gun. Again, I used TN's operationalization of "domestic" in its analyses and IA's operationalization in its analyses.

The SCM analysis requires a balanced panel, which I did not initially have. Specifically, IA and WI were missing one year of SHR data (1991 and 1998, respectively) and ME was missing 2 years of data (1991 and 1992). Given this small amount of missingness and the presence of data from surrounding years, I imputed these missing years using the average of the surrounding years. Specifically, for IA (WI) for each outcome, I replaced the missing 1991

(1998) value with the average of the 1990 (1997) and 1992 (1999) values. For ME for each outcome, I replaced the missing 1991 and 1992 values with the average of the 1990 and 1993 values. Given the long time series and how far they precede the treatment dates, these imputed values should not meaningfully impact findings from the SCM analyses. Based on these data, I conduct SCM analyses to test the impact of TN and IA's gun relinquishment laws on 4 additional outcomes. In the following outcome-specific sub-chapters, I present results for TN first and then IA.

9.1 Domestic gun murder rate

The trends for TN and Synthetic TN are shown in Figure 60. Synthetic TN is a weighted combination of GA (38%), WI (12%), WY (10%), WV (10%), ND (8%), ME (7%), SC (5%), AK (4%), RI (3%), and ID (3%). This figure shows that the domestic gun murder rate followed a similar declining trend in TN and Synthetic TN over most of the pre-intervention period before flattening out in the late 2000s. In the 4-year post-intervention period between 2010-13, each unit bounced around a rate of 1.0 per 100,000 persons covered by the SHR-reporting agencies in the state. To better show the estimated effect, I present a gap plot for the post-intervention years in Figure 61, which shows the difference in values between TN and Synthetic TN over this period. These figures show that there was very little difference between TN and Synthetic TN in their rates of domestic gun murders over the post-intervention period. Because the average post-intervention difference equaled 0.03, I treat this as a null effect and do not conduct placebo tests to test for statistical significance.

Figure 60. SCM of domestic gun murders per 100,000 persons covered in SHR, TN

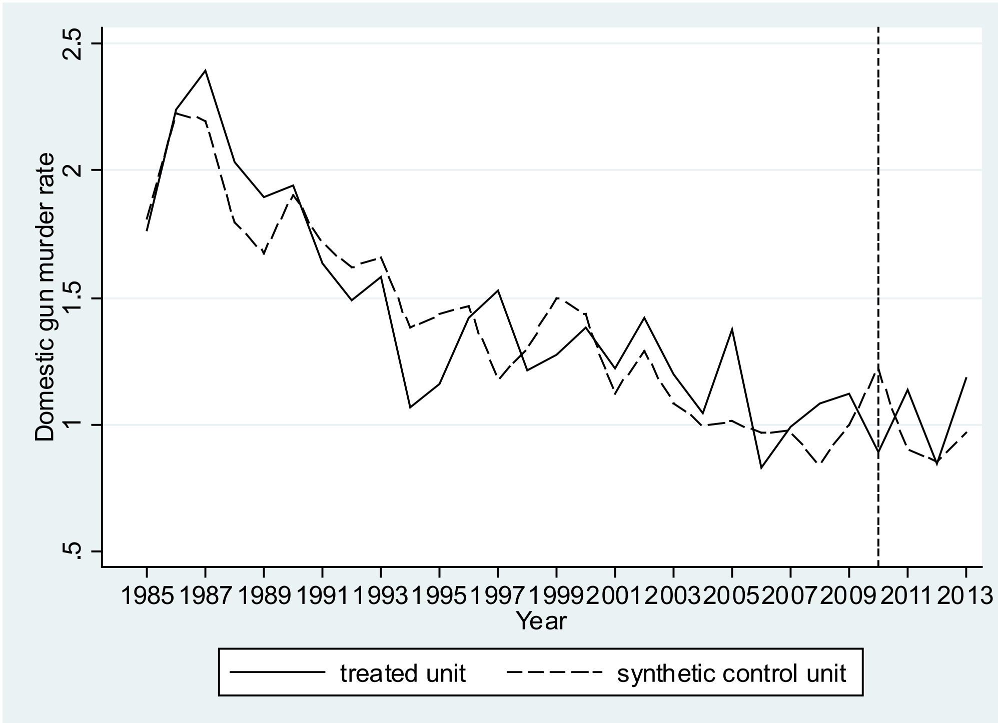
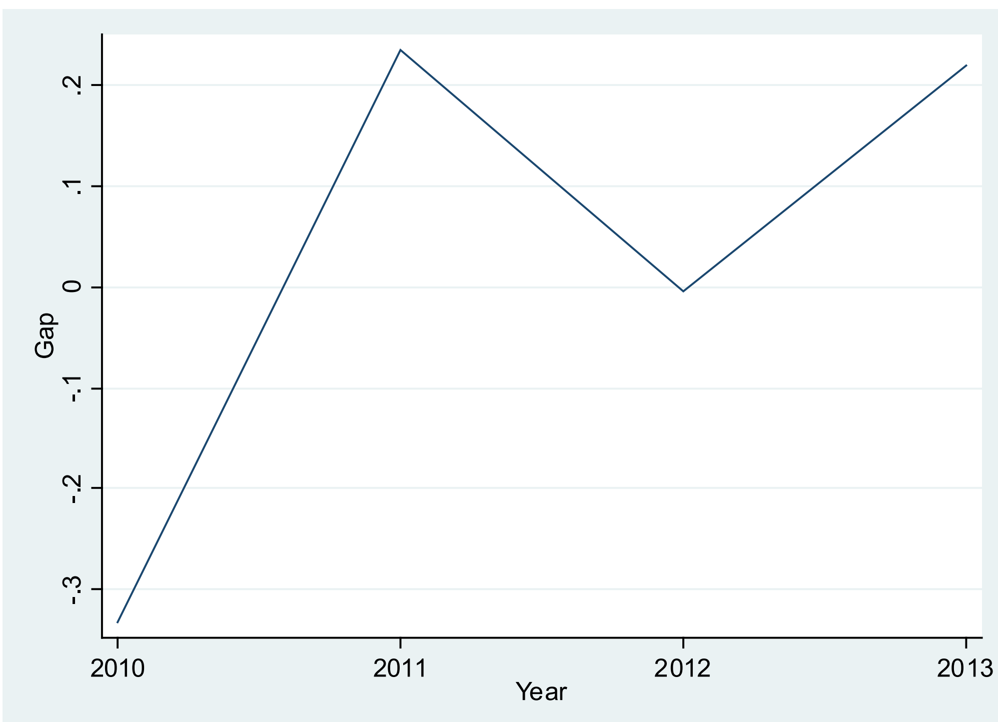


Figure 61. Gap plot of difference in domestic gun murder rate between TN and Synthetic TN over the post-intervention period



Next, I conduct the same analysis for IA. Given how much rarer murder is in IA compared to TN, one can see from Figure 62 that IA's trend is much less smooth compared to

TN's trend in Figure 60. Figure 62 also shows that IA and Synthetic IA followed similar trajectories, as they each hovered around 0.5 between 1985 and 1998 before beginning a downward trend through 2014. In this analysis, Synthetic IA is a weighted combination of HI (52%), AZ (14%), WY (10%), UT (9%), SC (7%), ME (6%), and ND (2%). Figure 63 shows the gap in values over the pre-intervention period between IA and Synthetic IA. Like with the pre-intervention period, IA and Synthetic IA followed similar trajectories over the post-intervention period. IA's domestic gun murder rate was lower than Synthetic IA's in 3 of the 4 post-intervention years. However, the average gap between IA and Synthetic IA was equal to only -0.06. Given such a small effect size, I treat this as a null finding.

Figure 62. SCM of domestic gun murders per 100,000 persons covered in SHR, IA

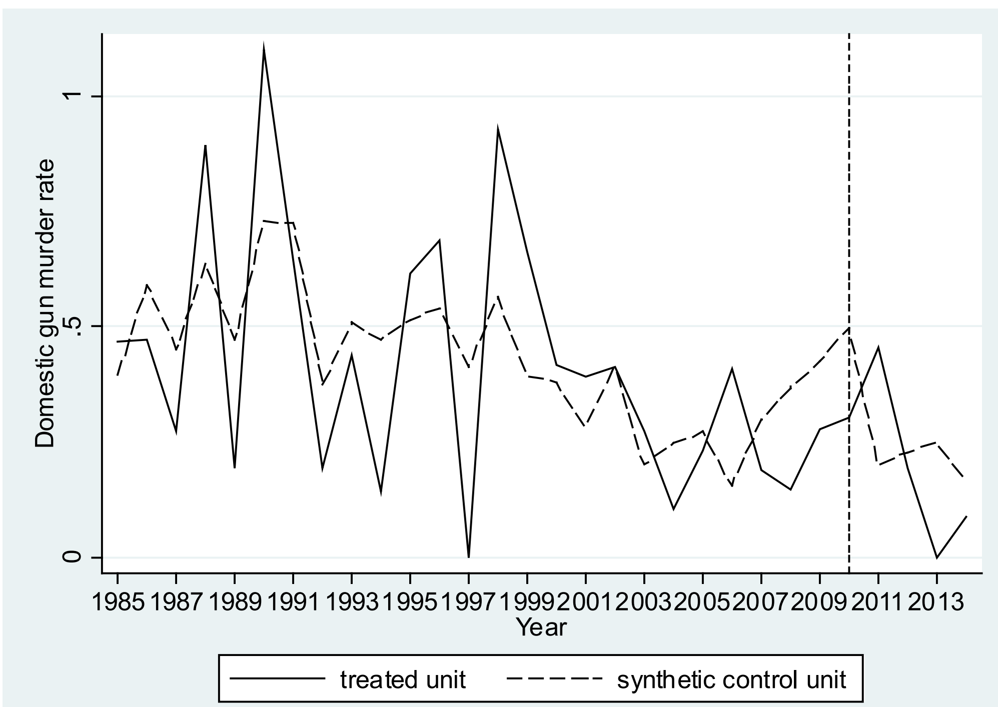
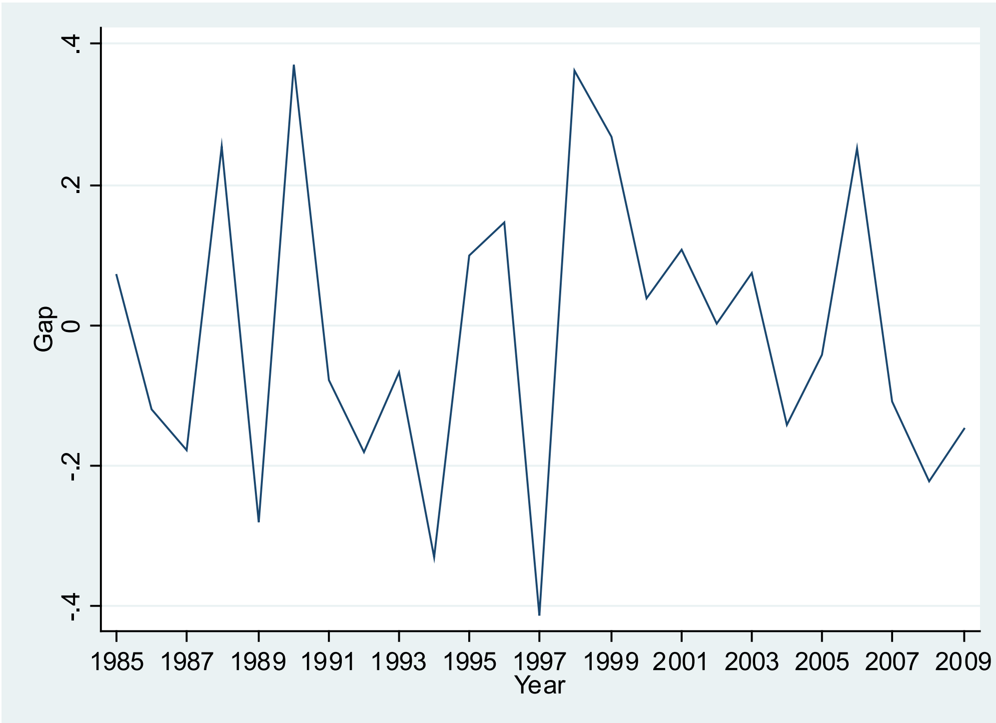


Figure 63. Gap plot of difference in domestic gun murder rate between IA and Synthetic IA over the pre-intervention period



9.2 Not-domestic gun murder rate

Next, I test for an effect on the rate of gun murders not related to domestic violence. Like with TN's results for domestic murder, the results for this outcome indicate a miniscule effect. Figure 64 shows the difference in this outcome between TN and Synthetic TN, which is a weighted combination of LA (39%), MI (35%), TX (14%), AR (11%), & WY (1%), over the observation period. One can see that although there were a few years in the pre-intervention period where TN experienced larger spikes than Synthetic TN did, the two states followed similar trajectories over this time. For the post-intervention period, TN's not-domestic gun murder rate was higher than Synthetic TN's in 2011 and 2012 and lower in 2010 and 2013. Figure 65 shows the gap in these values over this time. Based on these figures and the average estimated effect being equal to only 0.1, I treat this as a null finding.

Figure 64. SCM of not-domestic gun murders per 100,000 persons covered in SHR, TN

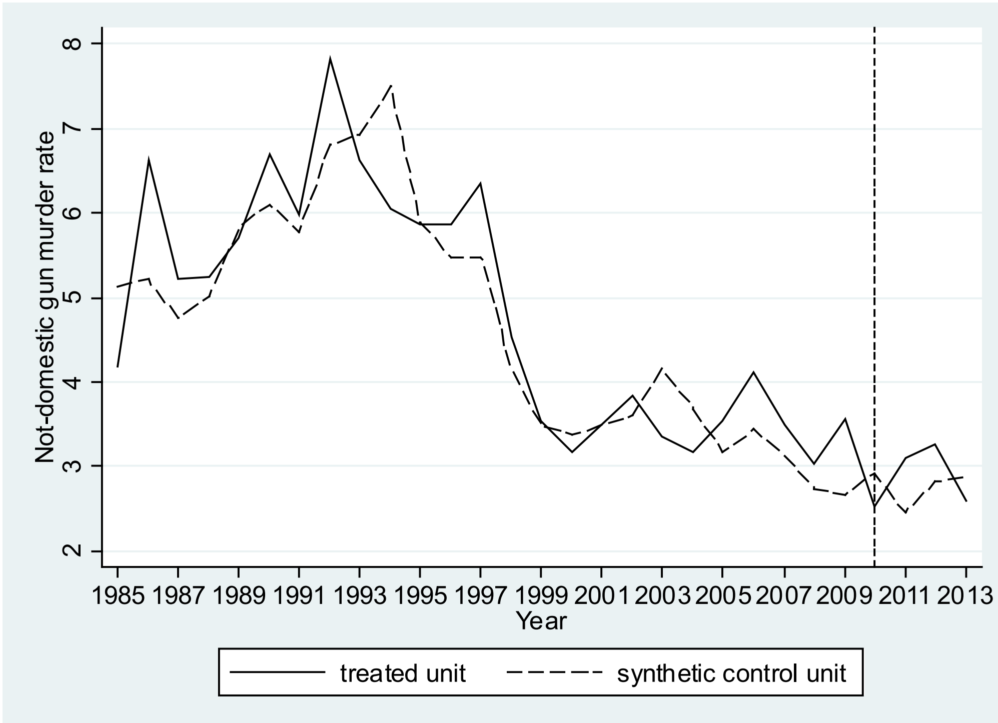
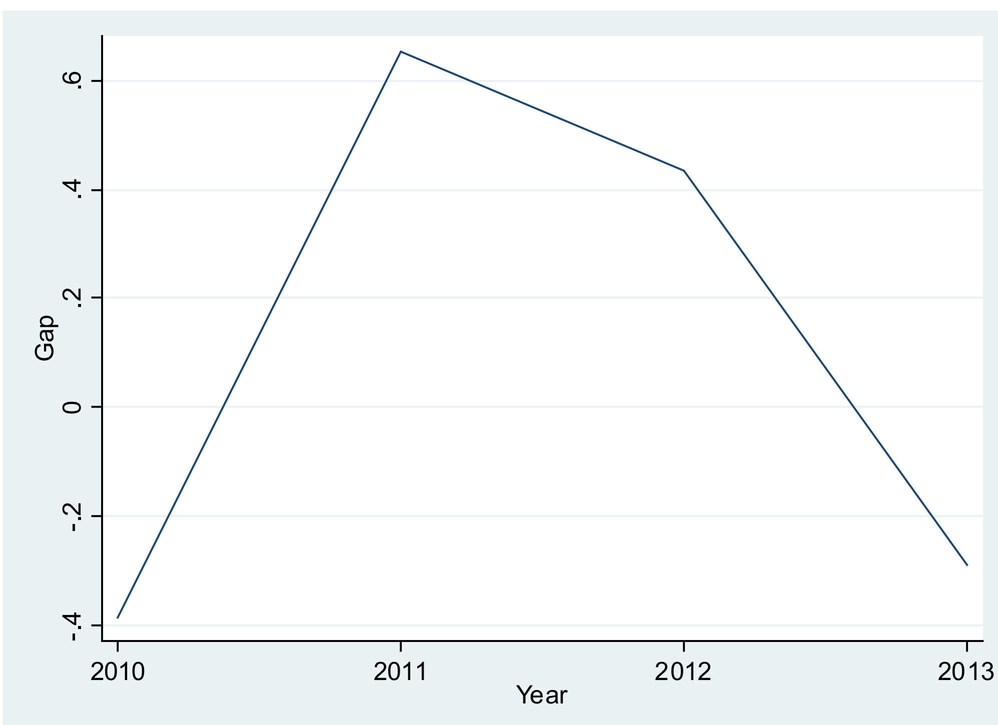


Figure 65. Gap plot of difference in not-domestic gun murder rate between TN and Synthetic TN over the post-intervention period



Next, I test this relationship with IA. As in Figure 62, Figure 66 shows that IA's not-domestic gun murder rate fluctuates widely from year to year. With Figure 67, one can see that although Synthetic IA's pre-intervention trajectory often follows IA's trajectory in direction, it differs relatively greatly in the magnitude of change over time. In this model, Synthetic IA is comprised of WA (31%), HI (31%), DE (24%), RI (8%), and WY (6%). One can compare figures 63 and 67 to see that the fit between IA and Synthetic IA is worse for the not-domestic gun murder rate compared to the domestic one. In fact, the RMSPE in this model is twice as large as the domestic gun murder model. Given these findings, I consider this SCM analysis as having too poor of fit to interpret the effect of IA's gun relinquishment law on this outcome.

Figure 66. SCM of not-domestic gun murders per 100,000 persons covered in SHR, IA

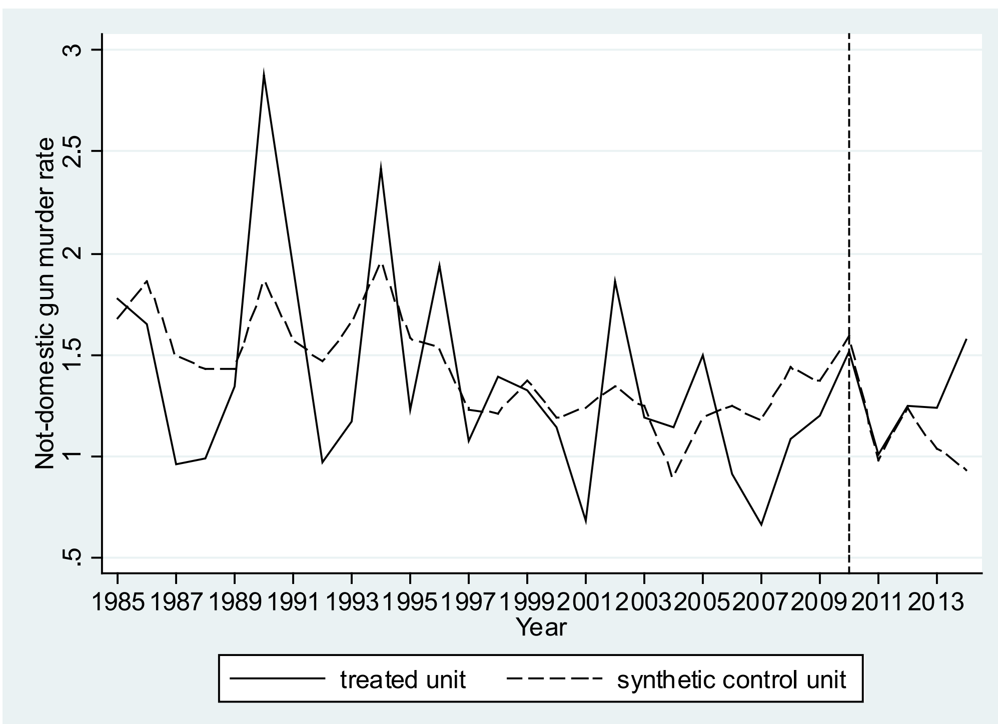
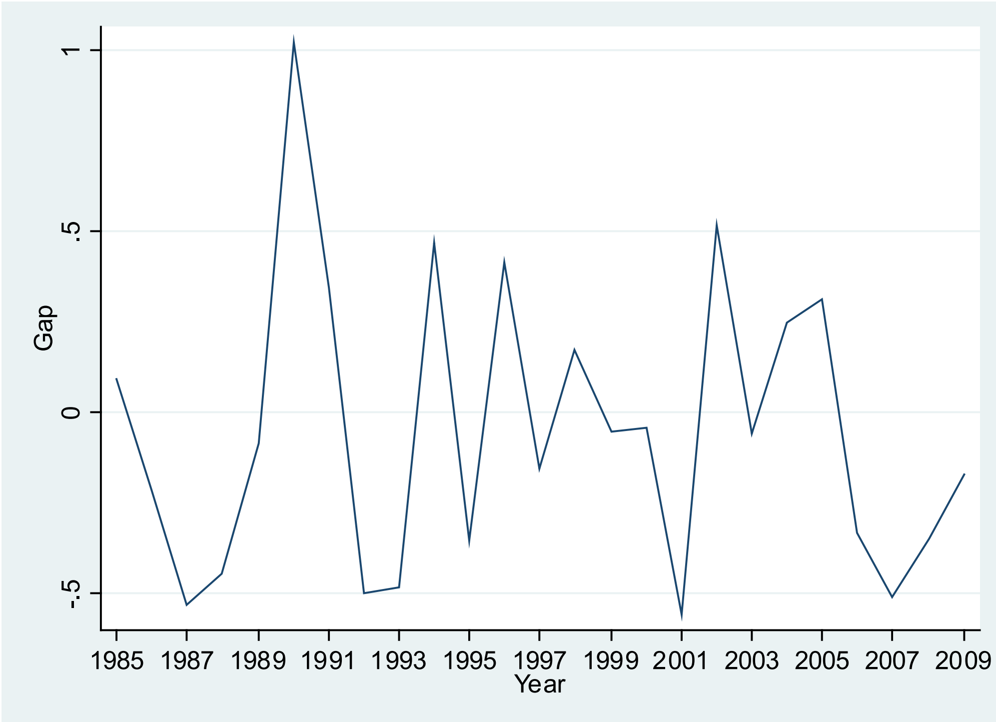


Figure 67. Gap plot of difference in not-domestic gun murder rate between IA and Synthetic IA over the pre-intervention period



9.3 Weapon substitution in domestic murder incidents

Because the SCM requires a balanced panel, missing or not applicable values present a problem. Since murder is rare, this outcome was not applicable for years where there were no domestic murders. For TN, this was the case for 3 years in ND and 1 year in VT. Because each of these not applicable values had applicable values in both the preceding and following year, and because the number of not applicable years was small, I replaced these 4 values with the average of the values in the preceding and following year. For IA, this outcome was not applicable for 21 observations. Of these observations, ND contributed 9, VT contributed 5, WY contributed 2, and AK, DE, HI, RI, and WI all contributed 1. Due to the large amount of not applicable values, I remove VT and WY from the donor pool for this analysis. For the remaining states, I replaced the not applicable value(s) with the average of the values in the preceding and following year.

Starting with TN, I conducted a SCM which estimated a Synthetic TN that was a weighted combination of SC (33%), LA (22%), NC (16%), TX (8%), AR (6%), ND (4%), ID (4%), OR (3%), VT (3%), and ME (1%). Because the fit of the model looked questionable in Figure 68, I plotted the gap in pre-intervention values, which is shown in Figure 69. These figures shows that while the differences between TN and Synthetic TN hovered around 0 over the pre-intervention period, the differences did get relatively large at times. Still, since the average gap during this period was equal to -0.01 and the RMSPE was relatively low at 0.04, I treat this model as having a good fit. Turning back to Figure 68, one can see that while TN experienced large changes during the post-intervention period, they were in both directions. In fact, the average effect was equal to only 0.03. Because of these findings, I treat this as a null effect.

Figure 68. SCM of proportion of domestic murder incidents that did not involve a gun, TN

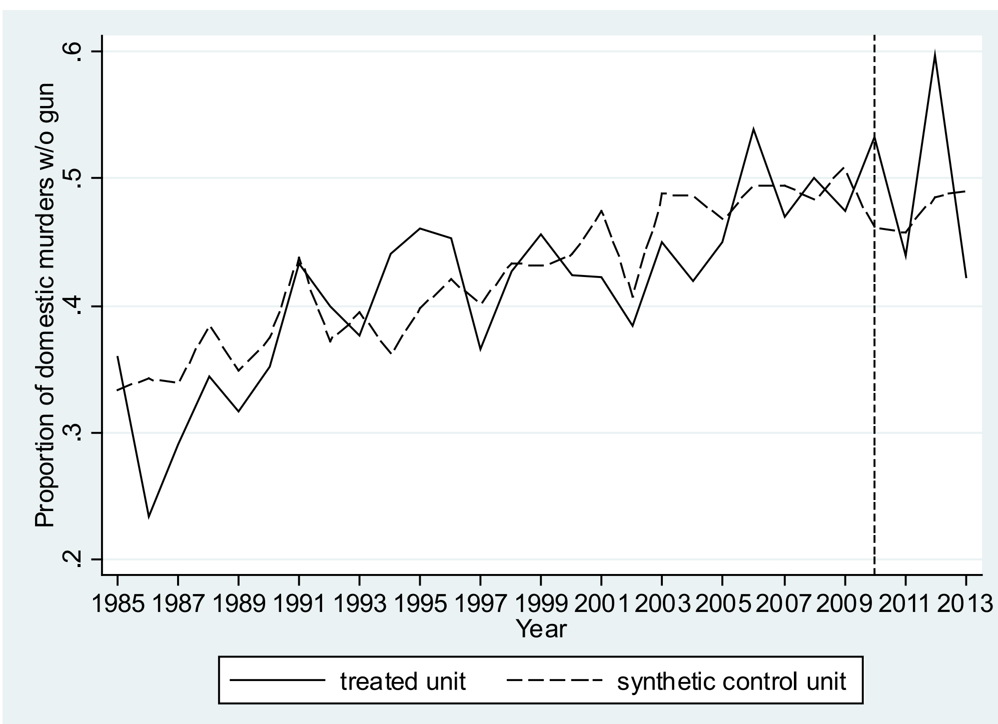
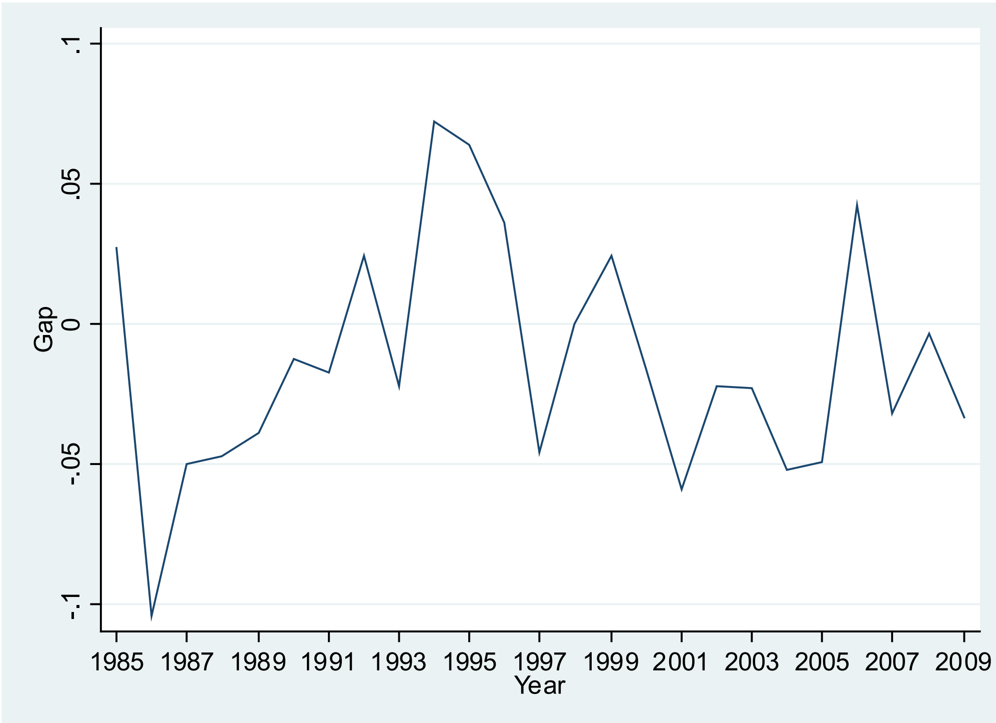


Figure 69. Gap plot of difference in proportion of domestic murder incidents that did not involve a gun between TN and Synthetic TN over the pre-intervention period



A SCM analysis with IA resulted in a poor fit between IA and Synthetic IA, which was made up of NJ (40%), WY (24%), ME (20%), AZ (12%), and MI (4%). Figure 70 shows the results of this model and Figure 71 shows the pre-intervention gap in values between IA and Synthetic IA. Together, these figures show that IA's pre-intervention values were often more than one-tenth greater or less than Synthetic IA's values, which is a large difference in the proportion of murders than did not involve a gun. To confirm this evaluation, I compared the pre-intervention RMSPE value in this model to the value from each in-space placebo test. Ranked from worse to better fitting models, IA's SCM was ranked 21 out of 27 in its pre-intervention RMSPE value, with two-thirds of the better fitting models having a value equal to half the size of IA's value. Based on this determination, I do not interpret the estimated effect of IA's gun relinquishment law on this outcome.

Figure 70. SCM of proportion of domestic murder incidents that did not involve a gun, IA

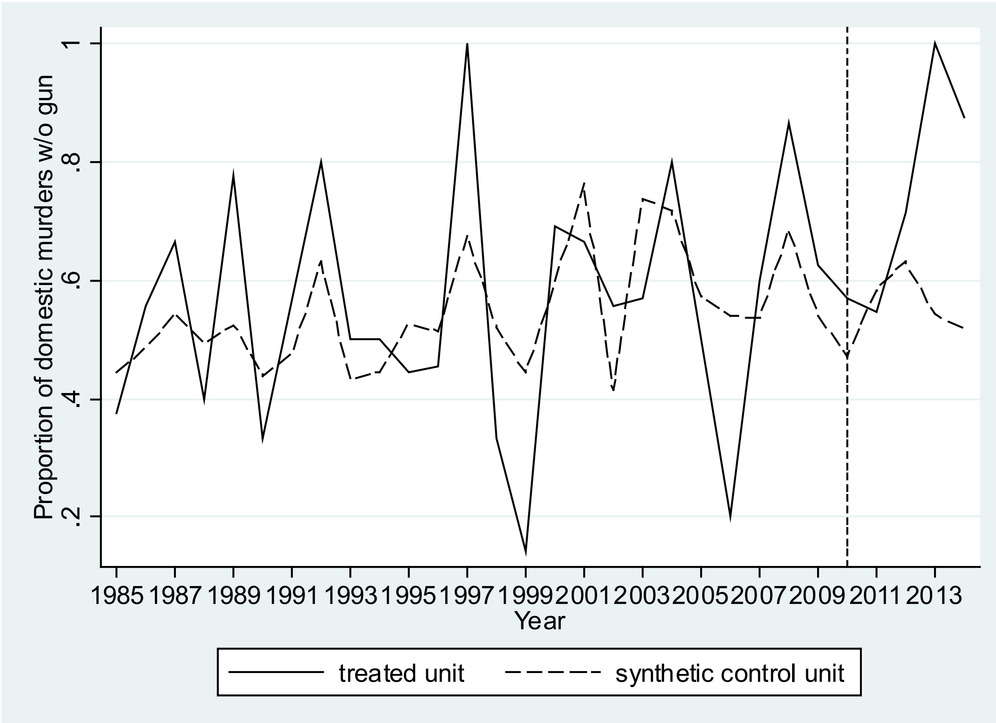
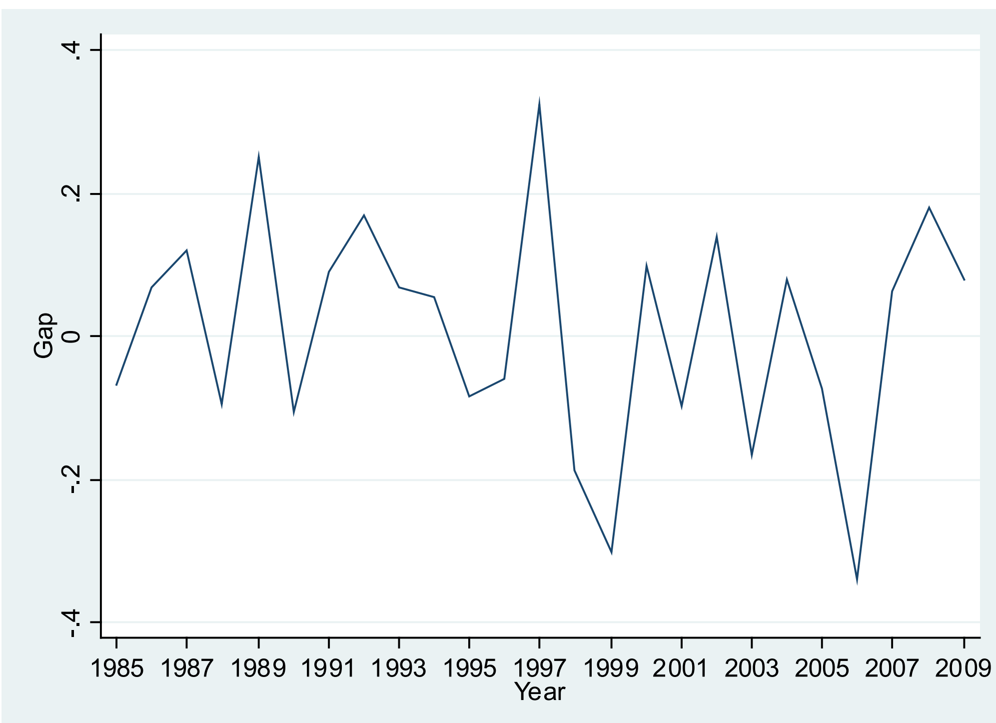


Figure 71. Gap plot of difference in proportion of domestic murder incidents that did not involve a gun between IA and Synthetic IA over the pre-intervention period



9.4 Weapon substitution in not-domestic murder incidents

Lastly, I test the effect of TN and IA's gun relinquishment laws on change in the proportion of not-domestic murder incidents that did not involve a gun. As discussed in the previous section, this outcome was not applicable in cases in which there were no not-domestic murders. For TN, this outcome was not applicable for 1 year in VT. I replaced this value with the average of the values in the preceding and following year in the state. This was not an issue for IA.

An analysis examining TN shows that estimated Synthetic TN, which is composed of LA (43%), VA (23%), WV (18%), NC (15%), and AK (2%) had adequate model fit (see Figures 72 and 73). Although there were years with relatively large differences in values, the average gap between TN and Synthetic TN during the pre-intervention period was equal to -0.01 and the RMSPE value was 0.04. Again, the estimated effect was miniscule, suggesting a null effect. The average difference between TN and Synthetic TN in the post-intervention period was equal to only 0.02.

Figure 72. SCM of proportion of not-domestic murder incidents that did not involve a gun, TN

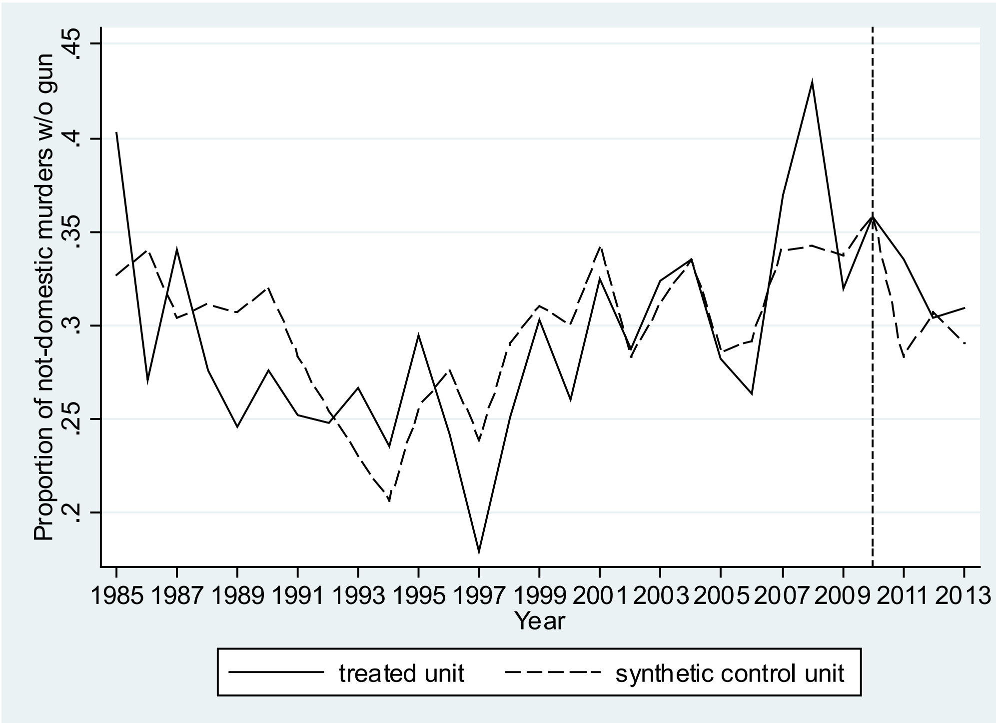
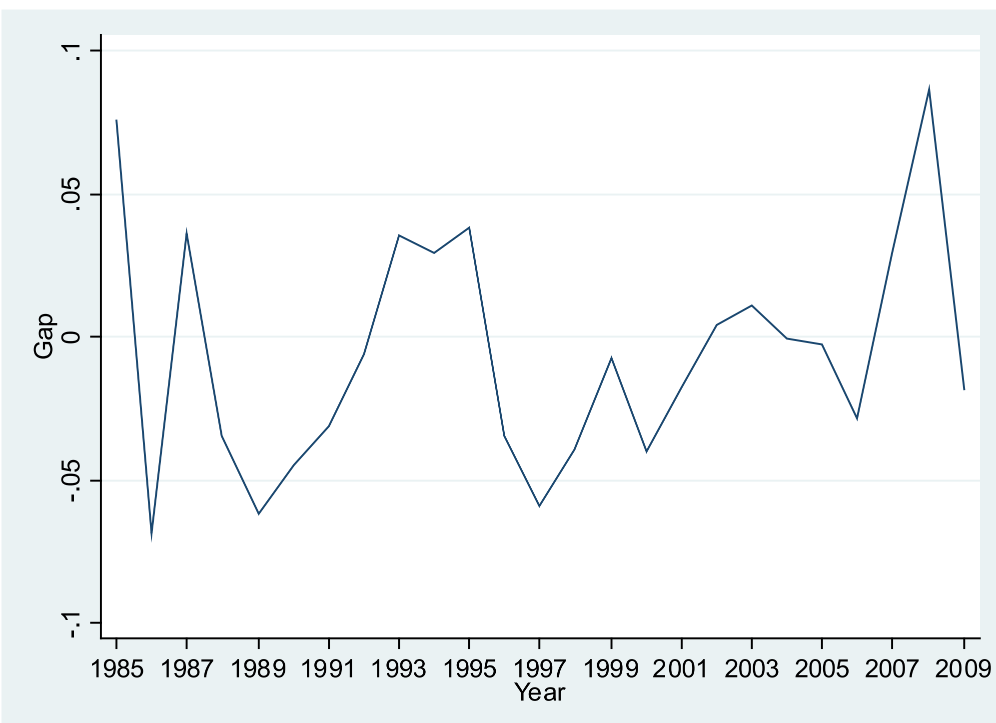
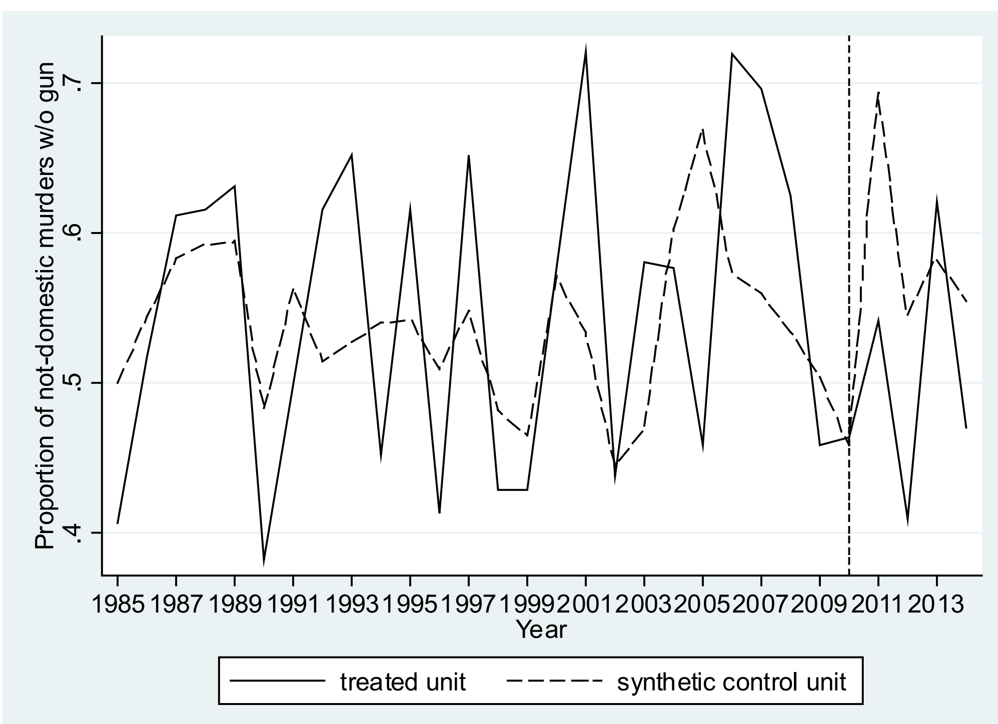


Figure 73. Gap plot of difference in proportion of not-domestic murder incidents that did not involve a gun between TN and Synthetic TN over the pre-intervention period



Next, I present results for IA. In this SCM analysis, Synthetic IA was comprised of RI (30%), DE (23%), MA (15%), HI (12%), ND (9%), WA (9%), and AK (3%). Figure 74 shows that while IA experienced much larger annual changes than Synthetic IA did, their trajectories were similar during the pre-intervention period. In the post-intervention period, IA and Synthetic IA began with similar values in 2010 but then IA's trend remained lower for 3 of the 4 years between 2011-14. On average, the proportion of not-domestic murder incidents that did not involve a gun was 0.07 less in IA than Synthetic IA in the post-intervention period. Given the danger guns present to society when used in crimes, this effect size seems consequential. To measure uncertainty around this unexpected finding and to test for statistical significance, I conducted a set of in-space placebo tests with each of the 29 states. IA ranked 23 in its RMSPE ratio with 20 states having a lower pre-intervention RMSPE value. Therefore, I find that IA's estimated effect is not statistically significant ($p = 0.79$).

Figure 74. SCM of proportion of not-domestic murder incidents that did not involve a gun, IA



CHAPTER 10: DISCUSSION

DGV is an important public health issue in the U.S. that requires evidence-based prevention methods. For instance, the ratio of the U.S. female firearm homicide rate to the female firearm homicide rate in 22 other high-income countries was 15.7 in 2010 (Grinshteyn & Hemenway, 2016). Moreover, 44% of all women murdered in the U.S. between 2010 and 2017 were killed by an intimate partner (Fridel & Fox, 2019). In fact, IPV contributed to 22% of all homicides committed in 27 states in 2015 (Jack et al., 2018). Related to its prevalence, every year DGV produces great costs to both victims and society (Logan & Lynch, 2018; NCIPC, 2003; Peterson et al., 2018; Sorenson, 2017). Due to its high prevalence and cost, there is a need to identify effective methods for preventing DGV.

One promising method for preventing both domestic and not-domestic forms of GV includes the enactment of gun relinquishment laws. Gun relinquishment laws create legal procedures for both removing firearms from prohibited possessors—in this case DVRO respondents and persons with a DVMC—and sanctioning non-compliers. These laws are based on the practical consideration that removing guns from abusers will make them unavailable to be used in future crimes and on a rational choice theory of criminal offending, which postulates that individuals consider the costs and benefits before committing a crime and are less likely to engage in criminal activity when the costs outweigh the benefits (Clarke & Cornish, 1985; Loughran et al., 2012; Nagin & Pogarksy, 2001; Wright et al., 2004). These laws are also informed by evidence that shows firearm possession or access greatly increases the likelihood of fatal and nonfatal GV victimizations, especially within domestic relationships (Bailey et al., 1997; Campbell et al., 2003; 2017; Rothman et al., 2005; Studdert et al., 2022).

The limited research on the effect of gun relinquishment laws for DV offenses on DGV suggests a preventative effect (Diez et al., 2017; Wintemute et al., 2015; Zeoli et al., 2018b). This dissertation adds to this body of research in multiple ways. First, I examine the impact of these laws on multiple forms of GV, not just murder. Second, I examine the impact of these laws on multiple forms of domestic relationships, not just intimate partner relationships. Notably, most prior studies have the restricted outcome of IPH. This is despite that 1) gun relinquishment laws protect victims in more types of domestic relationships than just intimate partner relationships and 2) non-fatal forms of DGV are much more frequent than domestic murder and carry substantial costs to victims and society.

Third, most studies only examine the impact of these laws on GV committed within domestic relationships. Importantly, research shows that domestic abusers often commit criminal offenses other than DV over the life course (Bennett et al., 2007; Bouffard & Zedaker, 2016; Klein & Tobin, 2008; Moffitt et al., 2000). It is possible that gun relinquishment laws prevent both domestic and not-domestic forms of GV. Finally, even though gun relinquishment laws vary substantially across states in the groups that they protect and the strength of their provisions, prior research has paid little attention to whether this variation moderates the effect of these laws on DGV. This dissertation applied a state-by-state analytic approach to estimate the impact of gun relinquishment laws on multiple forms of domestic and not-domestic GV that occur in a variety of victim-offender relationships. This approach builds on the existing evidence base to advance our understanding of the impact of gun relinquishment laws for DV offenses on state levels and characteristics of GV.

This dissertation sought to answer 4 research questions. One, do gun relinquishment laws reduce state rates of DGV victimizations? Two, do gun relinquish laws reduce state rates of not-domestic GV victimizations? Three, do gun relinquishment laws cause offenders to substitute other, less lethal weapons for a gun in DV victimizations? And four, do gun relinquishment laws cause offenders to substitute other, less lethal weapons for a gun in not-domestic violent victimizations? The unit of analysis in these tests was the state and estimates were based on yearly changes in several outcomes. Based on extant evidence and theory, I hypothesized that these laws would reduce both forms of GV and lead to weapon substitution for a gun, which would decrease the lethality of severe assaults.

To test these hypotheses, I used violent crime victimization data from NIBRS for states with high population coverage between 2005-14 and conducted a series of comparative case studies using the synthetic control method (Abadie, 2021; Abadie et al., 2010) and a modification of the SCM for cases with imperfect pre-intervention fit (Ferman & Pinto, 2021). Following an analysis of state laws among states with high NIBRS coverage during this time, I identified two states—Iowa and Tennessee—that enacted gun relinquishment laws during this time that applied to both DVRO respondents and persons with a DVMC and that did not allow for judicial discretion in applying the law. These states served as the treatment states while the remaining 14 states were in the donor pool and used to construct a synthetic control unit for each outcome.

Because TN had uniquely high levels of GV compared to states in its donor pool, the SCM could not identify a synthetic control unit that fit well with its pre-intervention GV trend for several outcomes. This is not uncommon in real-world applications of the SCM, which has led statisticians to develop adaptations of the original SCM for cases with imperfect fit (Ben-Michael et al., 2021; Doudchenko & Imbens, 2016; Ferman & Pinto, 2021). In this dissertation, I applied the F&P SCM in these instances, which relaxes the no-intercept constraint in the SCM and estimates a synthetic control unit using slopes but not levels of an outcome over time. To the extent that levels of GV do not impact trends, the results of an F&P SCM analysis should be no more biased than the results of a SCM analysis. However, if there are threshold effects where interventions or societal changes only impact GV trends if they occur when there is a certain level of GV, the F&P SCM results would be biased. One example could be federal assistance for crime prevention. If law enforcement agencies or governments receive a large amount of federal assistance when GV rates increase from a high but not a low baseline, comparing annual changes in GV among states with different levels of GV might not be an apples-to-apples comparison. Although there are not strong, evidence-backed reasons for believing an example like this is likely to impact these analyses, one should use caution when interpreting results from comparative case study analyses that do not ensure that treated and control units have similar intercepts for an outcome prior to the intervention.

I recognize that having only 4 full years of pre-intervention data and 14 states in the donor pool to use for estimating synthetic control units and conducting permutation tests weakens my analyses. Although the model fit appeared to be adequate for most of these analyses, the small number of years imposes a risk that the treated and synthetic control units were not equivalent (Abadie, 2021; Abadie et al., 2010). Additionally, since the SCM relies on randomization inference to test the statistical significance of findings, statistical power is a function of the number of placebo tests (Abadie, 2021). To address these limitations, in addition to the state-level NIBRS-based analyses, I conducted two sets of supplemental analyses.

In the first set of supplemental analyses, I conducted an agency-level analysis and examined the impact of the treatment states' gun relinquishment laws on the largest agencies in Iowa and Tennessee with complete participation in NIBRS over the observation period—Des

Moines Police Department and Memphis Police Department, respectively. In these analyses, every agency in NIBRS with complete participation over the observation period and that did not reside in a state that enacted a gun relinquishment law during the observation period served in the donor pool. I selected the largest agency in each treatment state to serve as the treatment unit because severe forms of violent crime are rare in smaller communities and presenting results for each agency in the state would be unmanageable. Although understanding the effect of gun relinquishment laws on GV in the jurisdictions of large law enforcement agencies in the state is less beneficial than understanding the state-wide effects, this analysis provides much more statistical power, which improves my ability to measure uncertainty around and to test for statistical significance of estimated effects. In addition, by only including agencies with complete NIBRS participation over the entire observation period, these analyses address a key limitation in my primary analyses involving incomplete agency and population coverage across state-years.

In the second set of supplemental analyses, I conducted another state-level analysis but this time used data from the SHR for the years 1985-2014 instead of data from NIBRS for the years 2005-2014. Although these data are limited by only including murder as a measure of violent crime, the increased number of years and states addresses statistical limitations in my analyses. Specifically, the longer pre-intervention period increases the likelihood of identifying an equivalent synthetic control unit and the larger sample of states increases 1) the likelihood of identifying a good fitting synthetic control unit and 2) the number of possible placebo tests, which results in increased statistical power for calculating exact p-values. Moreover, agency participation in the SHR has been much greater than agency participation in NIBRS over time. In sum, NIBRS and SHR have unique benefits and limitations for testing the relationship between gun relinquishment laws and violence. By using both, I test how consistent my findings are across these different constraints.

In Table 7, I present the findings from my main analyses and each of my supplemental analyses. This table shows findings for each outcome for each state including the composition of the synthetic control unit and the direction and statistical significance of the estimated effect. Of the primary analyses, the estimated effect was in the predicted direction for 9 outcomes including all the DV-related outcomes and in the opposite direction for 2 outcomes. One can see that in no case was the estimated effect statistically significant. Although the deck was stacked against finding a statistically significant effect for any outcome given the small number of states in the donor poll, findings in the Results Chapter show much uncertainty around each estimated effect based on the high degree of change in the outcome over the post-intervention period among several in-space placebo tests. The results from my agency-level supplemental analyses provide support for these state-level findings in that no estimated effect was statistically significant. Lastly, among the second set of supplemental analyses using the SHR data, all but one of the estimated effects were so small that they constituted null effects. No effect was statistically significant. Thus, I do not find support for any of the 6 hypotheses tested in this dissertation.

Table 7. Study findings, by outcome and state

Treatment Unit	**Synthetic Control Unit Composition**[a]	**Direction of Effect**	**Statistically Significant**
Primary analyses			
DGV victimization rate			
TN[b]	ID = 82% MT = 12% DE = 7%	Negative	No
IA	MA = 54% ND = 24% AR = 22%	Negative	No

Not-domestic GV victimization rate			
TN[b]	SC = 83% DE = 17%	Not estimated	Not estimated
IA	SD = 44% ID = 28% ND = 12% MT = 2% NH = 2% VA = 2% MA = 2% RI = 1% WV = 1% DE = 1% MI = 1% AR = 1% SC = 1%	Positive	No
Proportion of DV victimizations committed without a gun			
TN[b]	MI = 77% MT = 15% SD = 5% RI = 4%	Positive	No
IA	MA = 68% ID = 18% AR = 14%	Positive	No
Proportion of not-domestic violent victimizations committed without a gun			
TN[b]	VA = 26% SC = 15% RI = 14% AR = 11% ND = 6% VT = 5% SD = 4% ID = 4% MI = 4% NH = 3% MT = 3% WV = 2% MA = 2% DE = 1%	Positive	No
IA	MA = 62% ND = 19% SD = 15% DE = 5%	Negative	No

Lethality of severe domestic assaults			
TN	DE = 45% MI = 45% RI = 5% VA = 3% SD = 1%	Negative	No
IA	RI = 57% VT = 27% MT = 9% NH = 7%	Negative	No
Lethality of severe not-domestic assaults			
TN	MI = 33% VA = 19% RI = 18% AR = 16% MT = 14%	Negative	No
IA	DE = 52% MA = 43% NH = 6%	Negative	No
Supplemental analyses – agency level[c]			
DGV victimization rate			
Memphis (TN) Police Department[b]	North Little Rock (AR) Police Department = 26% Orangeburg County (SC) Sheriff's Office = 20% Charleston (SC) Police Department = 9% Horry County (SC) Sheriff's Office = 7%	Negative	No
Des Moines (IA) Police Department	Raleigh County (WV) Sheriff's Office = 22% York County (VA) Sheriff's Office = 18% Wyoming (MI) Police Department = 10%	Negative	No
Not-domestic GV victimization rate			
Memphis (TN) Police Department[b]	Charleston (SC) Police Department = 67% North Charleston (SC) Police Department = 33%	Negative	No

Des Moines (IA) Police Department	Haverhill (MA) Police Department = 12% Sumter County (SC) Sheriff's Office = 11% Berrien County (MI) Sheriff's Office = 8% North Charleston (SC) Police Department = 5%	Positive	No
Proportion of DV victimizations committed without a gun			
Memphis (TN) Police Department	Henry County (VA) Sheriff's Office = 56% Lancaster County (SC) Sheriff's Office = 25%	Positive	No
Des Moines (IA) Police Department	York County (VA) Sheriff's Office = 12%	Positive	No
Proportion of not-domestic violent victimizations committed without a gun			
Memphis (TN) Police Department[b]	N/A	Not estimated	Not estimated
Des Moines (IA) Police Department	Canyon County (ID) Sheriff's Office = 9%	Negative	No
Supplemental analyses – SHR data			
Domestic gun murder rate			
TN	GA = 38% WI = 12% WY = 10% WV = 10% ND = 8% ME = 7% SC = 5% AK = 4% RI = 3% ID = 3%	Positive[d]	No
IA	HI = 52% AZ = 14% WY = 10% UT = 9% SC = 7% ME = 6% ND = 2%	Negative[d]	No
Not-domestic gun murder rate			

TN	LA = 39% MI = 35% TX = 14% AR = 11% WY = 1%	Positive[d]	No
IA	WA = 31% HI = 35% DE = 24% RI = 8% WY = 6%	Not estimated	Not estimated
Proportion of domestic murder incidents committed without a gun			
TN	SC = 33% LA = 22% NC = 16% TX = 8% AR = 6% ND = 4% ID = 4% OR = 3% VT = 3% ME = 1%	Positive[d]	No
IA	NJ = 40% WY = 24% ME = 20% AZ = 12% MI = 4%	Not estimated	Not estimated
Proportion of not-domestic murder incidents committed without a gun			
TN	LA = 43% VA = 23% WV = 18% NC = 15% AK = 2%	Positive[d]	No
IA	RI = 30% DE = 23% MA = 15% HI = 12% ND = 9% WA = 9% AK = 3%	Negative	No

a. Due to rounding, weights may not sum to 100%
b. Results are from a Ferman & Pinto (2021) SCM for imperfect pre-treatment fit
c. Synthetic control unit weights are presented for only those agencies that comprised at least 5% of the total weight
d. Due to the estimated effect size, I treat this as a null finding

These findings differ from prior state-level analyses, which found close to a 15% decline in gun-involved IPH rates following a gun relinquishment law going into effect (Diez et al., 2017; Zeoli et al., 2018). There are several possible reasons for why my primary findings differ from these analyses. First, this dissertation used a measure of severe violent crime as an outcome instead of limiting the outcome to murder. As previously stated, these laws are designed to reduce all forms of DGV by applying additional sanctions for gun possession, not just murder, and non-fatal forms of GV are much more common than fatal GV. Second, this dissertation used multiple types of domestic relationships in its outcomes, not just intimate partner relationships. Gun relinquishment laws often protect victims from a variety of domestic abusers including family members. Third, this dissertation focused on fewer states and years than some prior analyses. Although NIBRS contains much richer data than alternative crime datasets like SHR, it is limited by a lack of participation by LEAs, especially as one goes further back in time. Fortunately, LEA participation in NIBRS is increasing (FBI, n.d.), so similar analyses aimed at assessing more recent gun relinquishment laws should be less limited by this factor. In fact, given the FBI's transition to collecting only NIBRS data and not SHR data starting January 1, 2021, by necessity researchers will need to use NIBRS data to evaluate more recent gun relinquishment laws (FBI, n.d.).

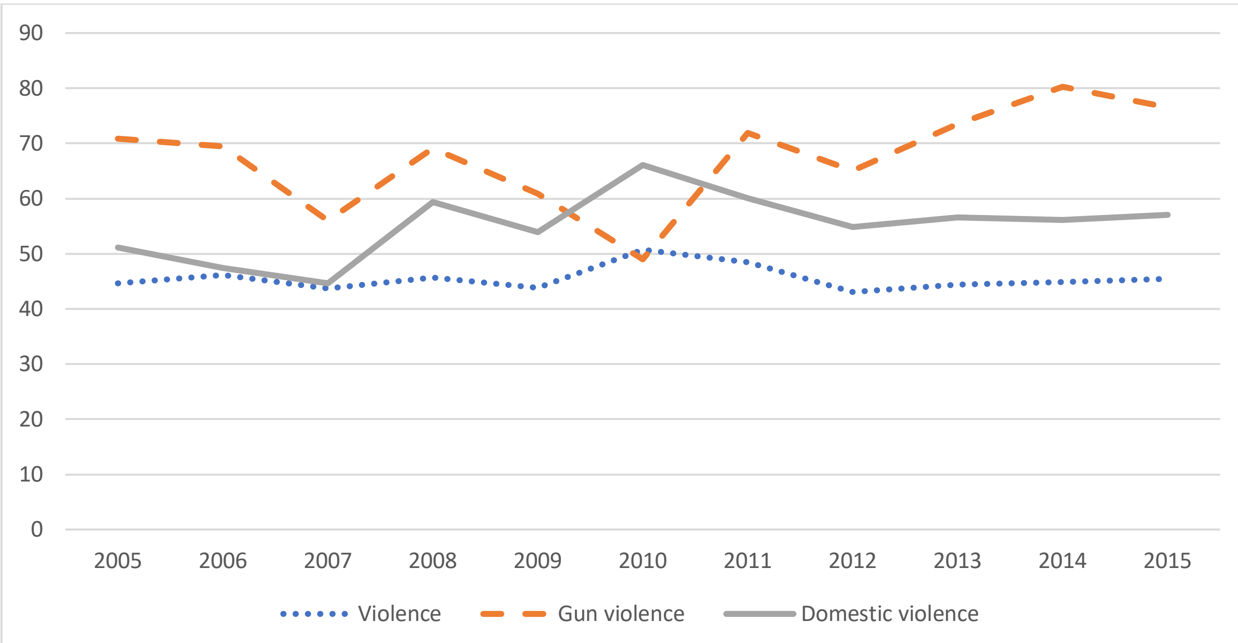
The primary reason for differences in findings is likely the difference in methods. In this dissertation, I applied the SCM to identify a weighted combination of control states that did not receive the treatment to use in a comparative case study for each treated state and outcome. Then, I conducted in-space placebo tests to measure uncertainty around those findings, which is a method of randomization inference. As mentioned, this method makes clear both the control units in which treated units are compared and the level of pre-treatment fit between the treated and synthetic control units. In comparison, prior studies (Diez et al., 2017; Zeoli et al., 2018) have compared treated to untreated states using generalized estimating equations, which is a method of estimating population-averaged effects in panel regression analysis (Hubbard et al., 2010; Zeger, Liang, & Albert, 1988). This method estimates a population average treatment effect across the treated states and time by comparing treated state effects to untreated state effects after controlling for state-level confounders. Each of these analytic methods have unique strengths and weaknesses, and it is likely that they would produce distinct findings. Future research could replicate each study with distinct methodologies to determine whether findings differ according to the analytic design.

Unfortunately, state-level data on the frequency in which judges issue gun relinquishment orders and the frequency in which individuals subjected to these orders relinquish their gun(s) are not publicly available, and likely difficult to obtain, especially when examining less recent laws. If these data were available, one could conduct simulations to estimate how large of an effect these laws are likely to have on DGV—a 15% reduction according to some analyses or a null effect according to this dissertation's analyses. Because research has shown a lack of implementation of these laws in some states (Everytown for Gun Safety, 2019; Moracco et al., 2006; Webster et al., 2010; Wintemute et al., 2014), and there are multiple steps that must all be met for offenders to lose access to guns during the duration of the order, including being ordered to relinquish the gun, having the order enforced, and complying with the order for its duration, it seems unlikely that these laws would have a large impact on state rates of DGV. Moreover, if data were available on how frequently individuals violated these orders and the median number of days between being ordered to relinquish one's gun(s) and a violation of the order, it would allow researchers to understand how immediate or gradual any effect of these laws is likely to be.

Importantly, the analyses in this dissertation are subject to several limitations that may impact the findings. First, all analyses are at the state level. Although gun relinquishment laws go into effect at the state level, they are applied at the individual level. Thus, one cannot know for certain whether the post-intervention changes in GV estimated for TN and IA were caused by changes in offending among persons subject to gun relinquishment orders. Inferences about individual behavior that are based on findings from groups to which the individuals belong (in this case states) can be inaccurate (Greenland & Robins, 1994). Second, NIBRS measures crimes reported to the police and not crimes that occur. Therefore, if gun relinquishment laws impact the proportion of DV offenses that are reported to the police, any estimated effects based on police recorded crime data will be biased since they will capture both the law's effect on crime and its effect on crime reporting.

Although not directly comparable to NIBRS, the NCVS provides nationally representative statistics on victimizations including the percent of victimizations that are reported to the police. Statisticians have used these data to show that much crime is not reported to the police including only 62% of nonfatal GV victimizations between 2007-2011 (Planty & Truman, 2013) and 55% of domestic assaults between 2003-12 (Truman & Morgan, 2014). I used data from the NCVS to calculate the percent of a) violent incidents, b) GV incidents, and c) DV incidents that were reported to the police between 2005 and 2015. These findings are reported in Figure 75. I did not calculate values for DGV incidents due to the small number of these events that were recorded in the NCVS. Unfortunately, the NCVS does not allow for state-level analyses. The results show that the proportion of DV and GV crimes that were reported to the police changed over this period. Importantly, these changes could have influenced my analysis of change in police-recorded crime data if changes in reporting behavior varied greatly across states or were related to gun relinquishment laws going into effect.

Figure 75. Percent of violent incidents that were reported to the police, NCVS, 2005-15



Third, this dissertation was limited by the small number of states available to use in donor pools to calculate the synthetic control units. After excluding states with less than an average of 80% population coverage in NIBRS between 2005-14 and the other treatment states (CO and either IA or TN) from each analysis, there were 14 states remaining to use in control groups for each analysis. In addition to reducing the likelihood of achieving good pre-intervention fit between the treated and synthetic control units when estimating effect sizes, this limited the number of in-space placebo tests available to measure uncertainty around those effects and to calculate exact p-values. Importantly, although this is a limitation, it did not appear to impact the substantive findings given the large amount of uncertainty found around these effects even among the small number of placebo tests. Fourth, several of the states used in this dissertation had incomplete population coverage in NIBRS for some years over the observation period. Although I accounted for this fact in my GV rate measure, it's possible that treatment and synthetic control unit states were not comparable if the NIBRS-reporting agencies in a state were not fully representative of the state's GV levels and characteristics.

Fifth, the analyses in this dissertation were limited by a small number of time points in which to measure the outcomes. Because of low state population coverage in NIBRS, this dissertation did not include data prior to the year 2005. Since the SCM's ability to identify causal effects is connected to how well the treated and synthetic control units match on pre-intervention values of the outcome, the rigor of the method is linked to the amount of pre-intervention data used in the analysis. Unfortunately, this dissertation used a relatively small number of pre-intervention time points, which increases the risk that the treated and synthetic control units were not comparable prior to the intervention date. Additionally, a small post-intervention period prevents one from uncovering gradual intervention effects. Unfortunately, this is necessary to increase the likelihood that changes in an outcome are due to the intervention being studied and not another intervention that occurred during the follow-up period.

Sixth, this analysis removed victimizations from analyses in which the victim-offender relationship was unknown. Although complete case analysis can bias results when data is not missing at random, as is the case here, the fact that the proportion of victimizations missing this information remained remarkably stable over the observation period suggests that this decision likely did not impact these analyses of change over time. Seventh, although the victim-offender relationship codes recorded in NIBRS are detailed, they do not include every relationship type included in gun relinquishment law eligibility specifications. For example, both IA and TN's gun relinquishment laws protect victims who are abused by current or former cohabitants, but NIBRS will not record this unique relationship type until 2023 (FBI, 2022). Similarly, although TN's DVMC gun relinquishment law protects victims of former dating partners, NIBRS did not measure a response of ex-dating partner until 2019 (FBI, 2021). Moreover, it is unknown to what extent LEAs recorded these relationships as either "otherwise known," a current dating partner, or unknown/missing prior to 2019. With 1) the inclusion of ex-dating partners in NIBRS after 2018, 2) the inclusion of cohabitants after 2022, 3) additional research on the causes of missing victim-offender relationship information in police-recorded violent crime data, and 4) new advancements in methods for imputing data that are not missing at random, future replications of this dissertation should produce more rigorous analyses.

Future research can build on this dissertation in multiple ways both by addressing these limitations and by using alternative research designs. First, more research is needed on the relationship between DGV and gun relinquishment laws for DV offenses that uses the individual as the unit of analysis. Although there could be a general deterrent effect of gun relinquishment laws, much of the impact of these laws is likely to come from preventing DGV by abusers who are subject to these laws. To this author's knowledge, only one study has tested the impact of gun relinquishment laws on the occurrence of DGV among individuals who were subjected to a gun relinquishment order (Wintemute et al., 2015). An individual-level study is a better design than a state-level analysis because it directly measures the effect of these laws on persons who are subject to them and can better identify small effects. For example, if gun relinquishment laws only save a small number of victims from being murdered by a domestic abuser, this small effect might not be noticeable in a state-level analysis. Yet, due to the large cost of DGV and murder, this effect might justify the cost of implementing a gun relinquishment law. In addition to identifying small effect sizes, individual-level studies better allow for cost-benefit analyses than state-level studies, which can advance research on the effect of gun relinquishment laws on GV.

Second, future research should replicate the design of this dissertation to test more recent gun relinquishment laws using more recent NIBRS data. One can see from Table 3 that multiple states have enacted gun relinquishment laws recently. Because LEA coverage in NIBRS continues to improve, and the FBI might provide imputed missing victim-offender relationship values at some point, future replications of this dissertation's design should be able to address many of the current limitations. Arguably, 1) the SCM is a better method for causal inference than panel regression and 2) NIBRS offers benefits over SHR for studying the effects of these laws, an, in fact, will be the only national data available to test recent and future gun relinquishment laws. Thus, I hope this dissertation can serve as a guide for more valid tests of the effect of gun relinquishment laws on GV levels and characteristics in the future.

Third, analyses of more recent laws should try to account for levels of implementation or enforcement of these laws both within and between states. Multiple studies of gun relinquishment laws have shown a lack of implementation of these laws by judges and law enforcement agencies in some states (Everytown for Gun Safety, 2019; Moracco et al., 2006; Webster et al., 2010; Wintemute et al., 2014). Because the gun relinquishment laws examined in this dissertation went into effect over a decade ago, it was impossible to explore how often they were applied and enforced in IA or TN. With recent or forthcoming gun relinquishment laws, researchers could attempt to measure and factor into their analyses any variation in the implementation of these laws between or within states. This would allow for a more refined study of the relationship between gun relinquishment law and DGV, because one could weight judicial areas, counties, or states based on the extent to which the law was applied and enforced.

A fourth challenge for future research to address involves this dissertation's reliance on police-recorded crime data to measure the impact of gun relinquishment laws on GV. It's possible that future researchers may be able to separate changes in crime from changes in crime reporting by using state-level NCVS estimates to evaluate these laws. Alternatively, researchers could consider asking representative samples of DV victims and abusers about their perceptions of how gun relinquishment laws might affect their behaviors or could replicate Wintemute et al.'s (2015) individual-level study but use victimization surveys instead of abuser arrest information as an outcome. This is a challenging but necessary issue to address in future research so that one can obtain causal estimates of the effect of these laws on actual crime rather than police-recorded crime.

In summary, despite having several of its own limitations, the research design used in this dissertation improves upon prior tests of the effect of gun relinquishment laws on DGV in multiple ways. Future studies should replicate this design and will benefit from forthcoming improvements in the data and methods used in this dissertation. Additionally, given the many time-varying factors that impact GV trends and the likely small effect size of gun relinquishment laws, researchers should explore using alternative research designs like those at the individual level of analysis to better identify any casual effect of these laws on GV and to estimate cost-benefit ratios. Based on this dissertation's findings, researchers will need to identify evaluation designs that can contend with the large amount of uncertainty around the effects of these laws to uncover any small effects that may exist. Given the importance of this issue to public health, this is a worthy challenge for future research.

APPENDIX

Before conducting my analyses, I searched state legislature websites to identify if any laws went into effect during the same year, the year prior, and the year after the gun relinquishment laws in IA and TN that might confound the effects of the gun relinquishment laws on DGV. Examples include laws that make stalking or domestic assault felony offenses, laws that expand the state's definition of domestic assault, by, for example, adding dating partners, laws that allow law enforcement officers to remove firearms at the scene of a domestic assault, and broader gun control laws like universal background check systems. Each of these laws could impact levels of gun violence and/or DV, thereby explaining any effect of gun relinquishment laws on DGV trends. The results of this search revealed that IA passed few potentially confounding laws and TN passed more. I discuss the results of each state search below and their ramifications for my analyses. For reference, IA's gun relinquishment laws went into effect on 3/22/2010 and TN's gun relinquishment laws went into effect on 7/1/2009.

I could identify no relevant state laws that went into effect in either 2009 or 2011 in IA that might confound the relationship between the enactment of gun relinquishment laws and my DGV outcomes. In 2010, along with the new gun relinquishment laws, IA made the knowing possession, shipment, transportation, or reception of a firearm, offensive weapon, or ammunition while subject to a protective order or DVMC a class D felony and required courts to inform persons subject to these DV offenses of their firearm restrictions (IA SF2357). The same law required courts to enter in DVRO respondent information into the IA criminal justice information system, which could have allowed law enforcement to enforce existing Federal law prohibiting gun possession by DVRO respondents and convicted DV misdemeanants more effectively (IA SF2357). Importantly, provisions prohibiting possession among DV offenders typically accompany new gun relinquishment laws when state law does not already match or exceed existing Federal gun prohibition provisions.[13] Therefore, these provisions could be considered an inherent component of gun relinquishment laws.

Unlike IA, TN enacted more laws that could confound the effect of its gun relinquishment laws on DGV. On 1/1/2008, multiple provisions of TN Senate Bill 1967 took effect, which established separate felony offenses for the 1) possession of a firearm with the intent to go armed and 2) the employment of a firearm while committing or attempting to commit several crimes considered dangerous felonies. The law also set mandatory minimum sentences ranging from three years for possession by a non-felon to 10 years for employment by a felon that require offenders serve at least 85% of their sentences. Although these provisions only apply to a small number of crimes, the list does include several offenses that will be examined in this dissertation including murder and kidnapping and it includes aggravated stalking. Senate Bill 1967 also made possession of a deadly weapon other than a firearm with the intent to employ it during the commission or attempted commission of a crime a Class E felony. On 7/1/2008, Senate Bill 2866 expanded the Class E felony designation of possession of a handgun by certain felons to apply to all felons and Senate Bill 0219 replaced "handgun" with "any firearm" in the original provision Senate Bill 2866 was updating that restricted handgun possession by certain felons. Importantly, Federal law, which supersedes state law, already restricts all firearm possession by all felons (18 U.S. Code § 922(g)(1)).

On 7/1/2009, House Bill 0411 created a Class A misdemeanor offense of attempting to buy a firearm by a prohibited person or attempting to sell a firearm to a known prohibited person.

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[13] This claim is based on my review of the state gun relinquishment laws shown in Table 3. To my knowledge, there is no reference that describes the similarities and differences in laws and provisions that accompany the enactment of state gun relinquishment laws.

On 6/19/2009, House Bill 1796 made the TN Firearms Freedom Act effective, making Federal laws and regulations regarding personal firearms or their accessories or ammunition inapplicable if these products were manufactured in and remain in TN. On 7/1/2009, with the new gun relinquishment laws, Senate Bill 0314 made the possession of a firearm by a DVRO respondent or a DV misdemeanant, as defined by Federal law, as well as anyone else prohibited from possessing a firearm under state or Federal law, a class A misdemeanor. As stated, gun relinquishment laws for DV offenses often accompany prohibitions on firearm possession by these groups if state law does not already match or exceed federal law.

On 7/1/2010, House Bill 2781 authorized judges to direct individuals convicted of domestic assault to a counseling program such as a batterer's intervention program, which, if not completed, could result in the revocation of an alternative sentencing program. On 7/1/2010, House Bill 2780 added "coming about the petitioner" to "telephoning, contacting, or otherwise communication with the petitioner" as a possible prohibition of a DVRO. No other laws passed between 2008-2010 are likely to confound the relationship between TN's gun relinquishment laws and DGV.

Although neither state passed its DVRO and DVMC gun relinquishment laws in a vacuum, IA passed few additional laws pertaining to DV or gun possession/use during the same year or one year preceding or following the year that the gun relinquishment laws were enacted. The laws IA did pass that made possession of firearms by prohibited DV offenders a felony offense, required courts to inform relevant DV offenders of their firearm prohibitions, and required the court's recording of DVRO respondent information in a state database, commonly cooccur with the enactment of gun relinquishment laws. Therefore, these laws can be considered a part of the intervention. This cannot be said so confidently for TN.

TN enacted major reforms to laws pertaining to gun possession and use, DV offenses, and restraining orders near the same time that it enacted its gun relinquishment laws. The sentence enhancements for gun possession or use during the commission of a dangerous felony might have reduced rates of gun violence, including DGV (c.f. Abrams, 2012; Barati & Adams, 2019; Marvell & Moody, 1995; McDowall, Loftin, & Wiersema, 1992). Fortunately, these laws went into effect 18 months before the gun relinquishment laws did, leaving time between that intervention and the gun relinquishment intervention in which to estimate independent effects. The law extending the felony offense of possession of a firearm by a felon went into effect 12 months before the gun relinquishment law did and was already prohibited by Federal law. The co-occurring law that is most concerning is the TN Firearms Freedom Act, since it went into effect within a month of the gun relinquishment laws. It is difficult to know what effect this law might have on gun violence without knowing the exact Federal laws that were nullified and every state law related to firearm manufacturing, possession, and commerce in TN. It seems likely that this law did not have a large effect on individual gun ownership and criminal use given that it applies to a small proportion of guns and TN state laws remain applicable. Still, one should consider these co-occurring factors when interpreting results from this state.

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