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ABSTRACT: One in two women in the U.S. report experiencing physical violence, sexual violence, or stalking by an intimate partner in their lifetime, and gun access is a central channel through which abusers can harm and control partners. I study whether state reforms that restrict domestic abusers' access to firearms reduce violence against women. Leveraging variation in law changes across time and states, I find significant declines in reported violence after changes in the law with spillovers beyond intimate-partner incidents. The pattern is consistent with changes in coercive control and deterrence mechanisms. The results indicate that carefully scoped firearm prohibitions can reduce violence against women and these findings are relevant to inform policy discussions on gun laws and women's safety.

JEL Codes: H0, I18, K0
Keywords: Laws, firearms, violence against women, domestic violence, gun violence

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1 Introduction

Firearm violence is increasingly recognized as a public health epidemic in the United States. In 2022, nearly 48,000 people died from firearm-related injuries, with many more suffering nonfatal harm. Beyond these immediate losses, gun incidents generate far-reaching social and psychological costs, including trauma, fear, and long-term impacts on community well-being (Currie et al., 2023). Gun access is also intrinsically linked to another pervasive societal issue - gender-based violence: one in two women in the U.S. report experiencing physical violence, sexual violence, or stalking by an intimate partner in their lifetime, with firearms playing an outsized role in such violence (Leemis et al., 2022). Abusers with firearms are five times more likely to kill their female victims (Campbell et al., 2017), and the risk of violence involving a firearm also increases the likelihood of non-fatal incidents. Approximately 3.4 % of IPV cases in the U.S. involve nonfatal firearm use (or 32,900 events per year) (Zeoli et al., 2022). Firearms play a critical role in shaping patterns of gender-based violence, yet the mitigating effects of policy in this area are not yet understood. This paper unites these two issues by analyzing how firearm regulations aimed at domestic violence offenders affect rates of violence against women, a salient issue as federal and state policies that disrupt abusers' access to guns can save lives (Everytown for Gun Safety, 2020).

I study the effect of stricter state-level firearms laws pertaining to domestic abusers, specifically analyzing the impact of closure of the *boyfriend loophole* on reported rates of violence against women in the United States. While federal law restricts firearm access for some domestic abusers, it historically excluded many dating partners who did not meet the legal definition of a spouse or cohabitant. This gap is crucial, since gun violence affects women usually at the hands of non-marital partners, as dating partners are the most common IPV offender (Sorenson and Spear, 2018). In recent decades, a growing number of states have acted to close the *boyfriend loophole* by expanding firearm purchase and/or possession prohibitions to cover dating partners subject to protective orders or convicted of certain misdemeanors.[1] These policy changes provide a unique setting for evaluating the causal effect of targeted firearm restrictions on gender-based violence.[2] Research consistently shows that prior violent behavior is one of the strongest predictors of future violence (Aizer and Dal Bó, 2009; Aizer and Doyle Jr, 2015; Chalfin and McCrary, 2017; Zeoli et al., 2022). My analysis leverages state-level variation in boyfriend-loophole closures to test this prediction: does restrict-

[1] Although the definition of protective order and misdemeanor crimes changes state-to-state, under federal law, a "protection order" (also called a protective order) means any injunction, restraining order, or other court order (civil or criminal) that is issued for the purpose of: preventing violent or threatening acts, harassment, sexual violence, or contact or communication (or physical proximity) with another person (18 USC § 2266(5)). A misdemeanor crime of domestic violence is defined as a misdemeanor under federal, state, or tribal law; and has, as an element, the use or attempted use of physical force, or the threatened use of a deadly weapon, committed by a current or former spouse, intimate partner, parent of the victim's child, or a person similarly situated to a spouse, intimate partner, or parent of the child (18 U.S.C. § 922(g)(9)).

[2] Men are also victims of domestic abuse, however this study focuses on women, who face a markedly higher risk of intimate partner violence (Cunningham et al., 2023; Tjaden and Thoennes, 1998). In the U.S., an estimated 20% of women compared to 7.6% of men experienced contact sexual violence by a partner in their lifetime (Leemis et al., 2022). In addition, in the U.S., men are especially likely to be gun owners: approximately 40% say they personally own a gun, compared with 22% of women - these numbers are also higher for White men when compared with White women and nonwhite men (Pew Research Center, 2017). Therefore, this gendered analysis is consistent with both the empirical evidence and the policy context underlying these laws.

ing firearm access for previously abusive partners–regardless of marital status–reduce both intimate partner violence and broader violence against women?

Using panel data on reported incidents from the National Incident-Based Reporting System (NIBRS) from 2013–2023, and combining it with legal archival research on state statutes, legislative histories, and implementation timelines, I exploit variation in the timing of state-level policy changes to estimate their causal impact on rates of violence against women. I identify the effects using a staggered difference-in-differences design, implemented through the Callaway and Sant'Anna (2021) estimator, which allows treatment effects to vary across groups and over time in a setting with staggered policy adoption. The results indicate that closing the *boyfriend loophole* significantly reduced violence against women, with clear spillovers onto non-intimate partner gender-based violence. States closing the loophole for protective orders or jointly for both protective orders and misdemeanor domestic violence crimes saw roughly a 13% decline, with more recent evidence suggesting even larger reductions—around 30%—robust across specifications, controls for county characteristics and gun prevalence, and tests accounting for COVID-19 and unrelated crimes. Decreases in cases of violence were slightly more pronounced for White non-Hispanic women and older women - this is perhaps due to the demographics of gun ownership, as White, older males are considered the largest demographic of gun owners in the U.S. (Pew Research Center, 2017). I also document significant declines in severe crimes, as well as stronger effects in counties with greater shares of female police officers - indicating that there are actual declines in the incidence of violence, rather than changes in reporting behavior.

Stricter regulations on firearm access may influence intimate partner violence (IPV) and broader violence against women (VAW) through two main channels. First, firearms play a broader role in facilitating not only lethal outcomes but also nonfatal injury, intimidation, and coercive control. Abusers may escalate abuse by directly brandishing or threatening with a firearm, or indirectly by leveraging access to a gun to instill fear. These actions contribute to a pattern of coercive control that restricts victims' freedom and sustains abusive relationships. Recognizing the role of firearms in this dynamic underscores the urgency of regulating access for known abusers to mitigate both fatal and nonfatal harm (Campbell et al., 2017; Sorenson, 2017; Sorenson and Schut, 2018). Second, beyond coercion, another channel is deterrence: boyfriend-loophole closures change the legal status of firearm purchase and possession for abusers[3], raising the expected costs of keeping, purchasing, or using a gun. The effectiveness of deterrence may depend on how much abusers value firearms, but also on the credibility of enforcement and the way firearm prohibitions alter the strategic use of violence and control (Adams et al., 2024). I find suggestive evidence that deterrence operates less through attachment to firearms per se and more through how legal restrictions reshape abuser incentives and weaken coercive control, due to stronger effects in contexts where gun prevalence is lower.

A growing literature examines the determinants of firearm violence and the effects of gun control policies

[3]In many cases, violation of these laws are considered a felony offense.

(Anderson et al., 2021; Duggan, 2001; Edwards et al., 2018), yet most work centers on general crime or homicide rather than gender-based violence. In economics, research on intimate partner violence (IPV) has traditionally relied on household bargaining and resource-based frameworks, emphasizing women's economic status as the main determinant of violence (Aizer, 2011; Bloch and Rao, 2002; Bobonis et al., 2013; Farmer and Tiefenthaler, 1997; Tauchen et al., 1991). Less is known about how institutional and legal interventions—particularly firearm-specific restrictions—affect IPV and broader violence against women. My paper addresses this gap by evaluating firearm prohibitions for domestic abusers as a policy lever that alters both the incentives and coercive power of abusers, linking the economics of deterrence and bargaining to contemporary firearm policy.

This paper makes several contributions to the literature. First, it provides the first causal evidence on the effects of firearm laws on broader rates of violence against women. Prior public health and sociology work has documented strong associations between domestic violence firearm laws and intimate partner homicide (Rochford et al., 2022; Sorenson, 2006; Vigdor and Mercy, 2003, 2006), but most studies are either descriptive, focus narrowly on fatal outcomes, and do not assess spillover effects onto other forms of violence. By exploiting staggered adoption of state-level closures of the *boyfriend loophole*, I am able to identify causal impacts of these laws on both fatal and nonfatal intimate partner violence, as well as on non-intimate partner violence against women. This broader scope offers a more comprehensive picture of how firearm policy shapes women's safety.

Second, this study contributes to the literature on the risk factors of intimate partner violence. A large body of research has examined how economic conditions, labor market shocks, and substance use affect IPV risk (Aizer, 2010; Bobonis et al., 2013; Castilla and Murphy, 2023; Erten and Keskin, 2018). I extend this literature by showing that firearm access itself is a critical determinant of IPV incidence and severity. Guns play a role not only in increasing lethality but also in sustaining coercive control - abusers use firearms to intimidate, threaten, and constrain victims even when they are not discharged (Adams et al., 2024; Sorenson and Schut, 2018). By examining how firearm prohibitions reshape both fatal and nonfatal outcomes, I highlight the central role of guns as a risk factor for IPV.

Finally, the paper contributes to the policy analysis of targeted gun laws and women's well-being. A growing literature examines how changes in public policy shape the prevalence of intimate partner violence, with evidence from mandatory arrest laws (Chin and Cunningham, 2019; Iyengar, 2009), unilateral divorce reforms (Stevenson and Wolfers, 2006), prescription opioid regulations (Dave et al., 2023), conditional cash transfers (Bobonis et al., 2013), and educational reforms (Erten and Keskin, 2018). My paper adds to this body of work by analyzing firearm-specific restrictions, showing that closing the boyfriend loophole yields significant protective effects. The results reinforce the importance of aligning firearm policy with contemporary relationship patterns and highlight the potential of targeted legal interventions to reduce both fatal and nonfatal violence against women.

This paper is organized as follows: Section 2 revises the policy relevant information on the subject, Section 3 and Section 4 describe the data and methodology I am using, Section 5 presents the causal estimates of firearms laws on VAW rates while Section 6 explains the theoretical framework, Section 7 provides a series of robustness checks, and finally Section 8 will conclude with principal findings and policy implications.

2 Institutional Background

2.1 Federal policy landscape

Federal law, codified under Title 18, Section 922 of the U.S. Code (18 U.S.C. § 922), establishes critical prohibitions on the purchase and possession of firearms and ammunition for individuals convicted of misdemeanor crimes of domestic violence or subject to specific domestic violence protective orders[4].

The Violence Against Women Act (VAWA) of 1994 marked a transformative shift in federal policy by introducing measures to prevent domestic abusers from accessing firearms. Specifically, it prohibited individuals subject to final protective (or restraining) orders from possessing, receiving, transporting, or shipping firearms or ammunition, provided the order followed a hearing where the defendant had an opportunity to be heard[5]. These restrictions lapsed once the restraining order expired. In 1996, the Lautenberg Amendment expanded these restrictions to include individuals convicted of certain domestic violence misdemeanors, building on the prohibitions for felons established by the 1968 Gun Control Act (Small et al., 2019).

Despite these federal provisions, implementation varied widely across states. Some states had adopted similar laws even before VAWA, while others added new regulations afterward. Variability extended to definitions of who qualified as an "intimate partner", including whether same-sex partners were eligible for restraining orders. This patchwork of regulation revealed critical gaps, most notably the so-called *boyfriend loophole* which excluded many dating partners who did not meet the federal criteria of spouse or cohabitant. Most recently, the Bipartisan Safer Communities Act of 2022 amended federal law to extend firearm prohibitions to current or former dating partners convicted of misdemeanor crimes of domestic violence. However, this prohibition expires after five years unless the individual is convicted of multiple offenses[6]

[4]Federal law defines a misdemeanor crime of domestic violence as a misdemeanor offense under federal, state, or tribal law that has, as an element, the use or attempted use of physical force, or the threatened use of a deadly weapon, committed by a current or former spouse, parent, or guardian of the victim, by a person with whom the victim shares a child in common, by a person who is cohabiting with or has cohabited with the victim as a spouse, parent, or guardian, by a person similarly situated to a spouse, parent, or guardian of the victim, or by a person who has a current or recent former dating relationship with the victim (18 U.S.C. 921(a)(33)).

[5]Previously, the Gun Control Act of 1968 had made it illegal only for those convicted of domestic violence **felonies** to possess a firearm.

[6]If a person is convicted more than once of a misdemeanor crime of domestic violence against a person with whom they have or had a dating relationship, the prohibition on gun purchases and possession lasts indefinitely, i.e., it does not automatically expire after five years, although the person can seek restoration of their firearms rights in the same manner as people convicted of felonies or other misdemeanor crimes of domestic violence (18 U.S.C. § 921(a)(33)).

(Giffords Law Center, 2024; RAND, 2024). These legislative milestones underscore the evolving yet incomplete nature of federal efforts to address firearm-related domestic violence, highlighting the need for further research and policy refinement. It is important to note that at the federal level, violation of the prohibition on receiving or possessing firearms if convincted of a misdemeanor crime of domestic violence is a federal offense punishable by up to 15 years imprisonment and/or a fine of up to $250,000.[7]

2.2 State policy landscape

The regulation of firearms in the United States involves a complex interplay between federal and state laws. While the federal government sets minimum standards (e.g., background checks for licensed dealers, bans on certain weapons), states retain significant authority to enact stricter regulations under their police powers, which allow them to address local public safety concerns. This is possible because Congress has not fully preempted firearm regulation, meaning states can impose additional measures (e.g., safe storage laws, expanded background checks) as long as they do not conflict with federal law. Federal agencies like the Bureau of Alcohol, Tobacco, and Firearms (ATF) have limited resources and jurisdiction, making it difficult to enforce federal laws uniformly across all states. In contrast, state and local law enforcement agencies are better positioned to enforce firearm regulations but may lack the specialized resources needed to address complex issues like gun violence prevention. This cooperative federal-state approach enables states to experiment with policies tailored to their specific needs, while the federal government ensures a baseline level of regulation across the country (Harwood, 2002). At the state level, the divide on gun policy and safety is enormous. On one end, there are states like Missouri which have enacted laws such as the Second Amendment Protection Law[8], although this is not necessarily favoured by conservative police and prosecutors. At the other end of the spectrum, there are several states which have pursued various gun safety measures and because of this experience lower levels of gun violence (Stanford Law School Blogs, 2021).

There is significant heterogeneity across states regarding firearm access laws for individuals with misdemeanor domestic violence convictions or those subject to domestic violence protective orders. States vary widely during the last decades in whether they enacted intimate partner violence (IPV)-related firearm restrictions, the specific conditions under which these prohibitions apply (e.g., during the enforcement of a protective order or following an IPV misdemeanor conviction), and the categories of individuals eligible for such protections (Giffords Law Center, 2024; Rochford et al., 2022). Below, in Figure 1 through to Figure 3, three different maps display the variability in firearms laws pertaining to domestic violence offenders during my study period - a 10-year pooled analysis from 2013-2023, and a more recent analysis from 2021 to 2023 as data coverage significantly improves after 2021[9]. In addition to federal firearms restrictions, states have

[7] 18 U.S.C. §§ 922(g)(9), 921(a)(33).

[8] The Second Amendment Preservation Act (SAPA) prohibits state agencies from helping the federal government enforce any law, rule or regulation which Missouri considers an infringement on the right to bear arms. Each violation can carry a $50,000 penalty.

[9] Due to NIBRS data availability, even though legal research can be updated to 2024, my study period ends in December 2023.

also enacted laws which: 1) prohibit domestic violence misdemeanants not covered by federal law from buying or possessing guns and/or ammunition, 2) authorize or require courts to order people convicted of domestic violence misdemeanors to relinquish their guns and/or ammunition (referred to as relinquishment laws), and 3) require officials to submit records regarding domestic violence offenses to databases used for firearm purchaser background checks (Giffords Law Center, 2024).

Figure 1: States with a change (closure) of loophole (2013-2023)

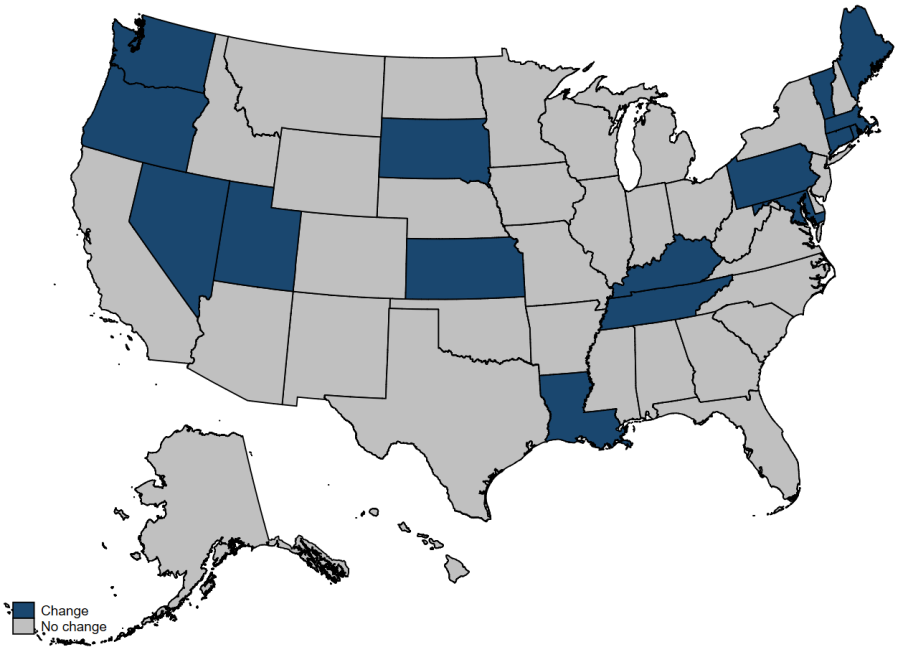


Figure 1 shows the states which had a change in the loophole during the years of 2013 to 2023, with 17 states having closed the boyfriend loophole in some capacity within state firearm access laws pertaining to perpetrators of domestic violence. Figure 2 displays the states with a closure of the loophole during the later study period of 2021-2023, including 7 states which had a closure of the loophole. My baseline analysis uses both the pooled sample and this second reduced sample, as NIBRS participation and data quality improve substantially from 2021 onwards. Figure 3 displays a heatmap in 2023 of the loophole closure, showing the variability in firearms access laws across the U.S.[10]

[10]The data used for the maps is legal archival research using resources from the websites of Gifford's Law Center, Everytown on Gun Safety, and information on the law statutes from Justia Law.

Figure 2: States with a change (closure) of loophole (2021-2023)

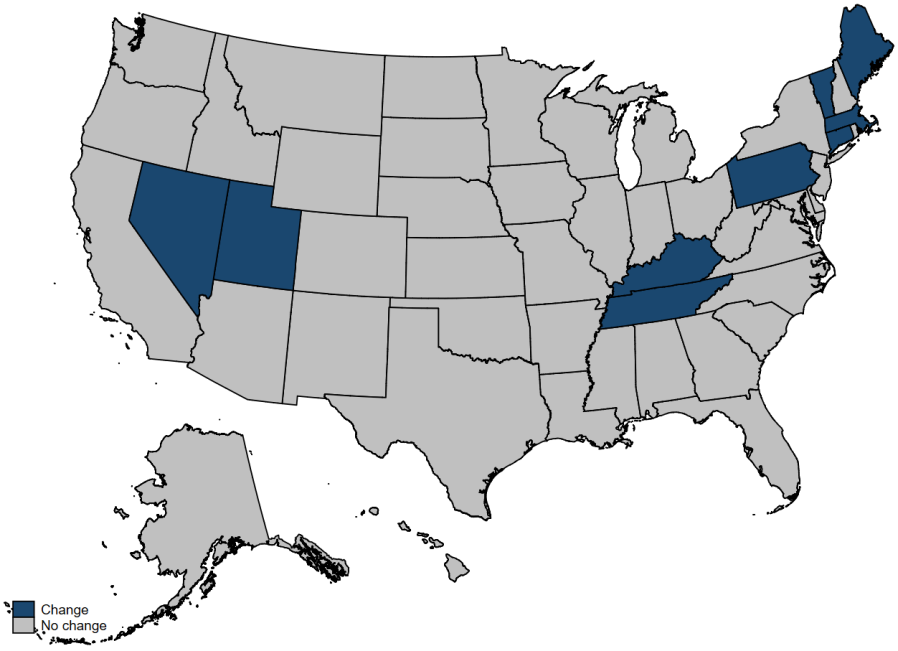
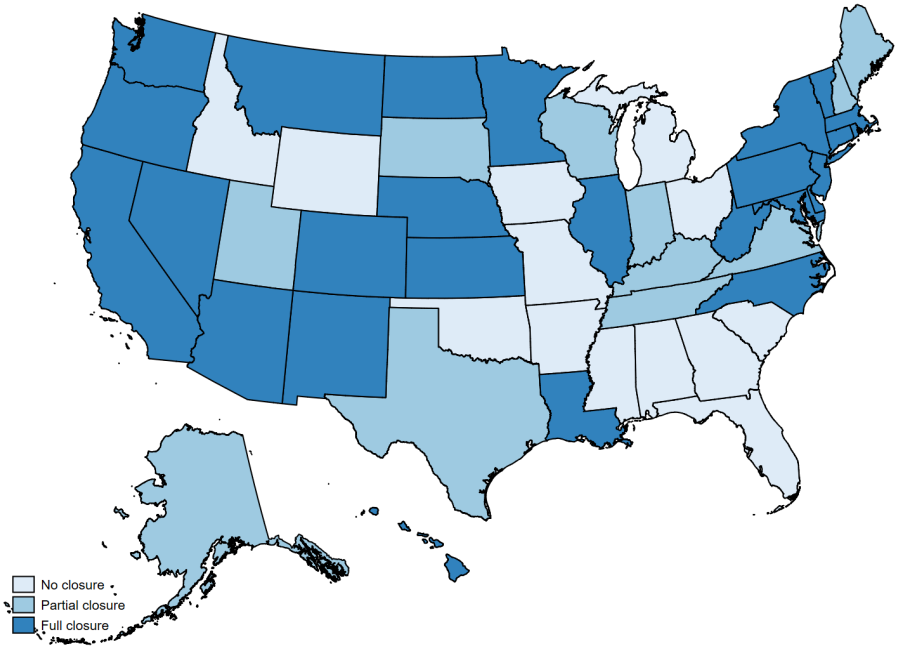


Figure 3: Heatmap of boyfriend loophole closure in 2023

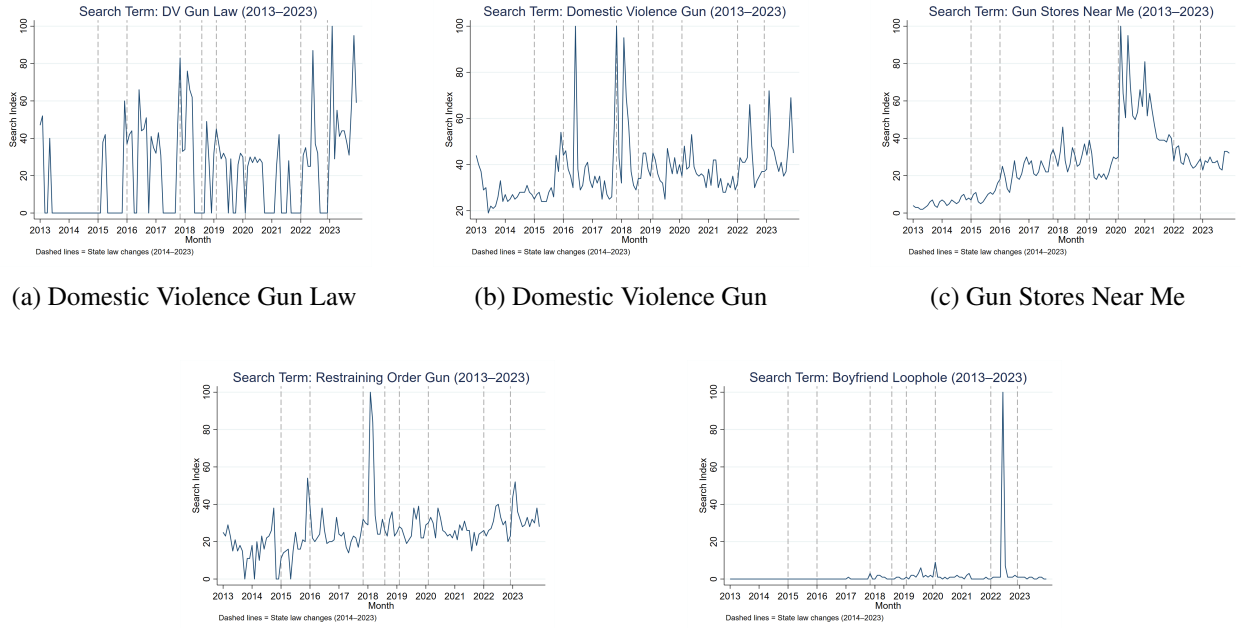


2.3 Salience of the law

I use Google Trends data to capture public interest in topics related to gun laws and domestic violence. It is useful in examining the salience of the law. The Google Trends search index reflects the relative popularity of a search term over time within a given region. It is scaled from 0 to 100, where 100 represents the peak level of search interest during the period observed, and all other values are indexed proportionally. A value of 50, for example, indicates that the term was half as popular as at its peak. A value of 0 does not imply zero searches, but rather very low search volume relative to the peak. This measure allows us to track shifts in public attention to key topics around the timing of legal changes (Choi and Varian, 2012; Colagrossi et al., 2023; Google, 2024).

Google search activity around domestic-violence firearm terms rises precisely in months when states change these laws, indicating that the reforms were salient to the public and likely triggered information-seeking about who is covered, how restraining orders interact with gun possession, and whether the boyfriend loophole still applies. The spikes are particularly visible for searches such as "domestic violence gun law" and "restraining order gun," suggesting that awareness and public attention increased around the timing of legal changes. In contrast, the line for "gun stores near me" remains relatively stable (apart from a spike due to the COVID-19 pandemic), indicating that these fluctuations are not simply driven by general interest in firearms but by attention to the legal and safety dimensions of gun access in the context of domestic violence.[11] It is important to note that there is a broader temporal pattern in intimate partner violence, which is known to exhibit strong seasonal and cyclical variation, often peaking during warmer months (Koutaniemi and Einiö, 2021; Santonja et al., 2025).

[11] For example, WomensLaw.org is a detailed informational legal website for victims of abuse in which each state's laws in relation to guns, domestic violence, and victim's rights are detailed in plain-language.



(a) Domestic Violence Gun Law

(b) Domestic Violence Gun

(c) Gun Stores Near Me

(d) Restraining Order Gun

(e) Boyfriend Loophole

Figure 4: Google Trends search indices for five firearm-related search terms (2013–2023).

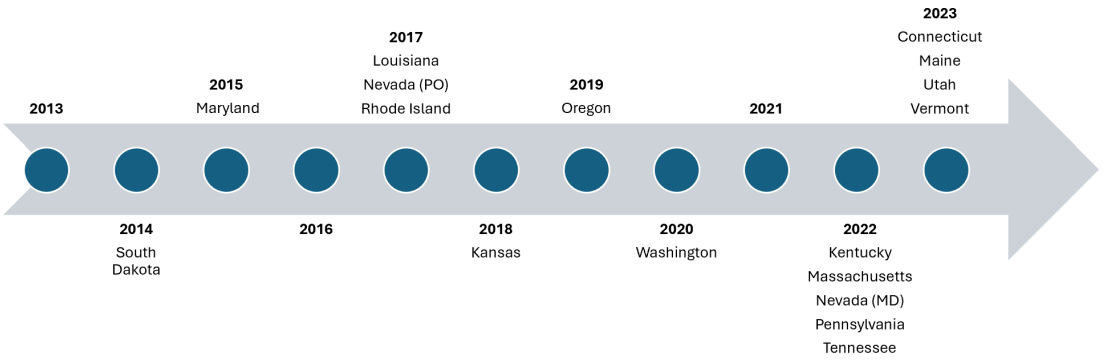
(a) Notes: Values range from 0 to 100 and are scaled relative to each term's peak interest. Dashed vertical lines mark months when key state-level firearm laws changed.

3 Data

To examine the impact of stricter state-level firearm restrictions on reported cases of violence against women, I construct two samples by merging an indicator of state-level legal changes with county-level reports of violence against women. The first spans 2013–2023, while the second, covering 2021–2023, captures a period of improved data quality and expanded NIBRS coverage.

Policy data

Figure 6: Timeline of state-level law changes



Throughout the study period, state-level firearm access laws for individuals subject to protective orders or with misdemeanor domestic violence convictions have varied widely. These laws differ in several ways: whether they exist at all, the circumstances under which firearm prohibitions apply (e.g., due to a protection order or misdemeanor crime of domestic violence (MCDV)), and the individuals covered by these protections (Giffords Law Center, 2024). Some states narrowly define IPV to include only violence between married or cohabiting partners, excluding dating partners from firearm-related restrictions. This gap, discussed as the "boyfriend loophole" highlights a critical area for policy evaluation (Rochford et al., 2022; Sorenson and Spear, 2018). Figure 6 shows a timeline of the policy changes across my study period.[12] The key treatment variable in this analysis is a state-level indicator capturing the timing and scope of *boyfriend loophole closures*—laws extending firearm prohibitions to dating partners or to those convicted of misdemeanor domestic violence. A closure is coded as occurring when a state enacted legislation that (i) expanded its *protective-order* prohibitions to include dating partners, (ii) extended its *misdemeanor domestic violence* prohibitions to include dating partners, or (iii) adopted both provisions. These three categories—protective order (PO) only, misdemeanor (MCDV) only, and both—form the basis for the heterogeneity analysis discussed in Section 5.

The policy framework allows me to identify the states that are treated during the sample period. Effective dates correspond to the statutory implementation date. States that had already closed the loophole prior to

[12] It is important to note that although 17 states had a change in the law during the decade, due to data limitations and missing observations, my treatment group for the full sample consists of 13 states from the larger sample. These include Connecticut, Kansas, Kentucky, Louisiana, Maine, Massachusetts, Oregon, Rhode Island, South Dakota, Tennessee, Utah, Vermont, and Washington.

2013 are coded as "always-treated" and excluded from the estimation sample. For all other states, the treatment variable is defined as a once-and-for-all indicator that switches from 0 to 1 in the quarter of adoption and remains 1 thereafter. This coding structure accommodates staggered adoption across states and aligns with the Callaway and Sant'Anna (2021) difference-in-differences estimator used in the analysis.

Table A1 in Appendix A provides the full list of statutes, years of adoption, and coverage type for each state.[13]

Violence against women

This analysis utilizes data from the National Incident-Based Reporting System (NIBRS) provided by the Federal Bureau of Investigation (FBI) to estimate the incidence of violence against women (VAW) that is known to law enforcement[14]. The NIBRS offers detailed information on reported incidents, including specifics about the incident's location, and the identities of both the perpetrator and victim. As of January 2021, the FBI has been shifting its data collection from the Uniform Crime Report (UCR) to NIBRS, which allows for more precise identification of the type of incident and the relationship between the victim and the perpetrator. This makes NIBRS a crucial source of national administrative data for assessing VAW. It is essential to acknowledge, however, that the data likely represent an underestimation of the actual prevalence of VAW, as not all instances are reported or recorded accurately in the NIBRS. This analysis should therefore be interpreted as a possible lower bound of the true effect. Each report in the NIBRS contains detailed information about the victim (age, gender, race, ethnicity, and relationship to the offender) and the offender (age, gender, race, ethnicity). NIBRS improves on survey-based crime measures by reducing underreporting associated with recall and social desirability bias, particularly for gender-based violence. I examine violence experienced by female victims, not restrictive to any type of relationship. However, in heterogeneity analysis, I examine IPV (restricting offender to boyfriends/ex-boyfriends, spouses/ex-spouses, or common-law spouses) as well as spillovers to non-IPV violence (excluding these groups of offenders). The primary indicator is the quarterly VAW rate per 100,000 population at the county level. This is a victimization rate - as what I am interested in is if these laws have a causal impact on the number of female victims, not just the number of cases of violence[15]. A distribution of offenses can be seen in Figure A3; as well as a visual breakdown of the main perpetrator's relationship to victim in Figure A4.

Reporting within NIBRS is conducted at the incident level by various law enforcement agencies, although participation is voluntary. By 2019, approximately 46% of all U.S. law enforcement agencies across 42 states were contributing to NIBRS. Coverage has expanded substantially over time, with 79% of U.S. law

[13] In the table body, Y refers to loophole is closed, N signifies not closed, and if the date of closure is outside of my study period, it has a Y but will not have a date next to it.

[14] Details on the types of offenses included in the definition of VAW are available in the appendix in Table 5.

[15] The most policy relevant outcome is the number of victims of violence. This means that there may be one incident with several victims, and I am counting them as different "cases".

enforcement agencies across the country participating in 2023 (Dave et al., 2025). A graph displaying the agencies covered in 2022 is visible in Figure A1. Due to the voluntary nature and uneven participation rates among agencies, the dataset can be unbalanced. To address this, I do not include states in which the number of agencies reporting is zero in any given year[16]. Also, given the prevalence of small agencies reporting zero incidents, my primary analysis is restricted to counties with populations exceeding 10,000 to ensure a more consistent sample (Churchill et al., 2022). In robustness checks, I also run analysis in which only counties which can be observed across the entire panel are included, as well as include a control accounting for number of agencies per county. Although NIBRS has these coverage flaws, it is still a widely used dataset in the U.S. for examining crime outcomes and has many benefits as it provides detailed crime reports, including information on offenders, victims, and circumstances of a crime (Dave et al., 2025, 2023; Ferrazares et al., 2025; Fone et al., 2019). The average evolution of cases reported of VAW for both treatment and control groups can be viewed in Figure A2 of the Appendix. A sharp spike in cases can be seen after 2020, which is largely because the FBI retired the older Supplementary Reporting System from January 1, 2021, and required all participating agencies to transition to NIBRS. The FBI backed this change with funding, training and technical support. As more law-enforcement agencies (and states) become certified and begin reporting incident-based data, the proportion of the U.S. population captured by NIBRS increases, improving the quality and completeness of data from around 2021 onwards (Federal Bureau of Investigation, 2021).[17]

Control variables

I draw from multiple data sources to account for time-varying county-level characteristics that may influence the outcomes of interest. Demographic data, including racial and age composition of women, are sourced from the Surveillance, Epidemiology, and End Results (SEER) Program, which aggregates information from the U.S. Census Bureau. These data capture the percentage of each county's Black, White, and Hispanic populations, along with the distribution of residents across age groups. I specifically focus on the proportion of Hispanic and Black women in each county, as my outcome variable is weighted by the population of women in each county over the age of 14.[18] Additionally, I account for socioeconomic conditions using the annual unemployment rate from the Bureau of Labor Statistics (BLS). In robustness checks, I also include a proxy for firearm prevalence[19], which is a 5-year rolling average of the proportion of suicides committed

[16] Excluding Vermont - there are 0 observations in 2015 for Vermont, however as the law change in this state is not until 2023 I keep it in the analysis.

[17] However, there still exist gaps in the data - the two largest police agencies in the U.S., the New York Police Department and the Los Angeles Police Department, are still missing in the federal data. My analysis does not include either of these states since the boyfriend loophole in both CA and NY was closed before 2013.

[18] I weight all regressions by the county population of women aged 14 and older, as this group represents the population at risk of intimate partner and gender-based violence. Weighting by the at-risk female population ensures that counties contribute proportionally to their exposure and that the estimates reflect effects on the average woman rather than the average county. This approach also mitigates the influence of sparsely populated counties with volatile reporting rates and aligns with the construction of the outcome variable, which is normalized by the same population base.

[19] I use the within-state, demeaned five-year firearm rate, which removes persistent differences across states and retains only within-state variation over time. This avoids collinearity with state fixed effects and isolates changes in the local firearm environment

using a firearm in each state-year (Cook and Ludwig, 2006; Rochford et al., 2022), as well as age composition controls from the SEER program. When I examine results by race and age, I adjust the denominators accordingly to reflect the population at risk using data from SEER.

4 Methodology

The causal impact of changes in closure of the boyfriend loophole on violence against women is estimated using the difference-in-differences with multiple time periods strategy (Callaway and Sant'Anna, 2021). I take advantage of the staggered timing of state closures of the boyfriend loophole by comparing trends in violence-against-women (VAW) before and after each law's effective date in treated states against contemporaneous trends in states without the law. While it remains possible that other factors are at play, leveraging the variation in enactment dates helps mitigate the risk that a single unobserved shock is driving changes in VAW rates across all states, as well as the fact that the Callaway and Sant'Anna (2021) estimator does not suffer from bias due to time-varying or cohort-specific treatment effects. For expositional clarity, I first present a simplified reduced-form equation, though the CS estimator is used for estimation.

$$Y_{ct} = \alpha_{s(c)} + \beta D_{s(c)t} + \mathbf{X}_{cy} + \varepsilon_{ct} \tag{1}$$

where Y_{cst} represents the number of reported cases of VAW per 100,000 women in county c, state s, at time period t (3-month year-quarterly periods). $\alpha_{s(c)}$ are state fixed effects. $D_{s(c)t} = \mathbb{1}(t \geq G_g)$ indicates whether the treatment (law) is active in county c at quarterly period t in year y (G_g is the period in which a county c adopts the law). $\mathbf{X}_{cy}$ is a vector of controls at the county-year level - I include percent Black and Hispanic female populations; unemployment rate; and the 5-year rolling firearms suicide rate by state. Regressions are weighted by county female population.[20] Standard errors are clustered at the state level to account for serial correlation in the error term within a state, as the treatment is at the state level. This measures the average impact of the law change on the treated counties by comparing the outcome of interest before and after the law enactment. More specifically, the parameter of interest is:

$$\theta(e) = \sum_{g \in \mathcal{G}} 1\{g + e \leq \mathcal{J}\} P(G = g \mid G + e \leq \mathcal{J}) ATT(g, g + e) \tag{2}$$

This is the average effect of participating in the treatment e time periods after the treatment was adopted

from fixed state-level characteristics.

[20] Following standard practice in county-level analyses of intimate partner violence (Dave et al., 2025, 2023), I weight regressions by the female population aged 14 and older in each county. This approach ensures that estimates are representative of the population at risk and mitigates the influence of small counties with high sampling variance. In contrast to total population weights, weighting by the relevant female population aligns more closely with the exposure group for VAW.

across all groups that are ever observed to have participated in the treatment for exactly e time periods. In other words, it captures the average treatment effect at different event times e (with e = $t - g$, where t is the current time and g is the treatment adoption year). Finally, the ATT is defined as:

$$\text{ATT}(g,t) = E\left[Y_t - Y_{g-1} \mid G_g = 1\right] - E\left[Y_t - Y_{g-1} \mid C = 1\right] \tag{3}$$

where C is the control group (counties that were *never treated*[21]), G_g is the group of counties treated in year g, Y_{g-1} represents the outcome for the treated counties in the period before treatment, and Y_t is the outcome for both the treated and control counties in the post-treatment period. After estimating the group-time effects $ATT(g,t)$, these estimates are combined into overall treatment effect parameters that are more economically meaningful by taking weighted averages across groups and time periods. Unlike standard two-way fixed effects (TWFE) estimators or other recent approaches that account for heterogeneous treatment effects—including the estimators proposed by de Chaisemartin and D'Haultfœuille (2020) and the stacked difference-in-differences framework—the method developed by Callaway and Sant'Anna (2021) provides flexibility in how these weights are defined and aggregated, especially in the presence of time-varying covariates. This feature allows researchers to tailor the estimation to specific policy questions and data structures.

For the main analyses, I present two different aggregations of the individual dynamic treatment effects $ATT(g,t)$. First, I follow Callaway and Sant'Anna (2021) in presenting aggregations analogous to classic event-study parameters, reporting the weighted average of all cohorts' treatment effects k quarters relative to policy adoption, for $k \in \{-4, \ldots, -1, 0, \ldots, 5\}$:

$$ATT_k^{CS,ES} = \sum_g w(g,k)\, ATT^{CS}(g, g+k), \tag{4}$$

where $w(g,k)$ is a weight that depends on the relative size of group g among all groups that are ever observed to participate in treatment for k periods. This aggregation allows me to examine the dynamic effects of closing the boyfriend loophole and to assess the extent to which pre-treatment trends in violence-against-women (VAW) rates were evolving similarly in treated and control states prior to policy adoption.

To summarize the dynamic treatment effects $ATT(g,t)$ into a single, policy-relevant parameter, I report the simple average of the post-treatment effects through five quarters following policy adoption. This average provides an estimate of the medium-term impact of closing the boyfriend loophole on violence against women. The choice of a five-quarter window is guided by the distribution of treated state-quarters that contribute to identification in the data. As a robustness check, I also report estimates using alternative aggregation windows of three and six quarters after adoption, presented in Table A15. As in Dave et al. (2025), the coefficients capture the *Intention-to-Treat* (ITT) effect—representing the average impact of be-

[21] In robustness checks I also use the *not-yet treated* counties.

ing exposed to the law, regardless of whether an individual would personally be prohibited from firearm possession, which cannot be observed in the data. This parameter captures both direct deterrence among potential offenders and broader preventive effects that may operate through changes in perceived risk or enforcement intensity.

To estimate treatment effects, I employ the *csdid* command[22] in Stata, which implements the difference-in-differences estimator for settings with variation in treatment timing, following Callaway and Sant'Anna (2021). Identification in this framework relies on two key assumptions. First, in the absence of treatment, outcomes in treated and control states would have followed parallel trends during the post-treatment period. Second, the policy change is assumed not to generate anticipation effects, meaning it has no causal impact prior to implementation. To assess the validity of these assumptions, I examine pre-treatment dynamics using event-study plots, which provide visual evidence on whether treated and control states exhibit similar trends before adoption.

5 Results

This section presents the estimated effects of closing the *boyfriend loophole* on violence against women (VAW). The analysis is structured around two main samples and several complementary specifications. The first sample covers the full period from 2013 to 2023, while the second focuses on a shorter and more recent window from 2021 to 2023, when NIBRS coverage improved substantially. In both cases, I compare treated states to a consistent control group of states that kept the boyfriend loophole fully open throughout the study period.[23] I begin by examining the overall effects on reported cases of violence against women for each sample. Given the broader policy variation in the full sample, I then distinguish between closures related to *protective orders* (PO), *misdemeanor crimes of domestic violence* (MCDV), or both. Subsequent sections investigate whether the effects extend to non-intimate-partner forms of violence, and explore heterogeneity across offense severity, firearm prevalence, race, and age. Summary statistics for both samples are presented in Table 1. From the descriptive statistics, it is clear that for most of the covariates the rates are stable between treatment and control groups, however on average, the control group of states has a higher number of cases of violence consistently for both samples.

[22] For all specifications, I use the *reg* option in `csdid`, which estimates the effect of treatment through outcome regression with ordinary least squares (OLS). This is based on Sant'Anna and Zhao (2020). I also employ the *long2* aggregation procedure, which recovers group-time average treatment effects and then averages them across cohorts and time periods using appropriate weights. The *reg* option implements OLS outcome regression for each group-time cell, while *long2* is an aggregation method in `csdid` that summarizes group-time average treatment effects into overall estimates following the approach of Callaway and Sant'Anna (2021).

[23] Due to data limitations, the control group for the full sample consists of Arkansas, Idaho, Iowa, Michigan, Mississippi, Missouri, Ohio, Oklahoma, and South Carolina. For the reduced sample, the control group includes Alabama, Arkansas, Georgia, Idaho, Iowa, Michigan, Mississippi, Missouri, Ohio, Oklahoma, South Carolina, and Wyoming.

Table 1: Summary statistics: full vs. reduced sample

Panel A. 2013–2023 sample				
	Treatment		**Control**	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Cases of VAW (per 100,000 women)	365.968	260.243	415.172	280.410
Cases of IPV (per 100,000 women)	142.970	104.712	140.720	102.912
Cases of non-IPV VAW (per 100,000 women)	230.828	169.235	281.337	200.835
Share of Black females (mean)	0.064	0.107	0.093	0.149
Share of Hispanic females (mean)	0.057	0.077	0.044	0.048
Unemployment rate (percentage mean)	5.192	2.059	4.946	1.977
Firearm suicide rate (5-year rolling mean)	0.564	0.104	0.573	0.065
Total population of women per county (mean)	47,138.160	92,627.142	36,890.561	72,221.661
Observations	17,091		21,230	
Panel B. Reduced sample 2021–2023				
	Treatment		**Control**	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Cases of VAW (per 100,000 women)	312.087	240.317	427.143	275.467
Cases of IPV (per 100,000 women)	122.804	98.191	149.391	103.260
Cases of non-IPV VAW (per 100,000 women)	193.435	151.618	284.221	195.098
Share of Black females (mean)	0.051	0.074	0.143	0.178
Share of Hispanic females (mean)	0.047	0.050	0.048	0.047
Unemployment rate (percentage mean)	4.199	1.178	3.815	1.160
Firearm suicide rate (5-year rolling mean)	0.578	0.101	0.615	0.064
Total population of women per county (mean)	56,374.728	109,293.006	34,922.838	67,414.293
Observations	3,887		9,084	

5.1 Main Effects on Violence Against Women

I begin by estimating the overall effect of closing the boyfriend loophole on reported cases of violence against women (VAW). The analysis is carried out separately for two samples: the full period from 2013 to 2023, and a reduced sample covering 2021 to 2023. The first allows for examination of longer-run policy variation across a wide set of states (although with lower coverage), while the second leverages the sharp improvement in NIBRS coverage and data quality after 2021. In both cases, the estimates are based on the Callaway and Sant'Anna (2021) difference-in-differences framework, with time aggregated to the quarterly level.

The results are presented in two parts: Section 5.1.1 discusses findings for the 2013–2023 sample, focusing on heterogeneity by type of law enacted (protective-order, misdemeanor, or joint closures). Section 5.1.2 then turns to the 2021–2023 subsample, which provides a more precise picture of recent policy changes in a period of near-complete reporting coverage.

5.1.1 Full Sample: 2013–2023

In the full sample covering 2013–2023, I find no significant aggregate effect of loophole closure on overall cases of violence against women, as shown in Figure A5. However, this average masks meaningful heterogeneity across the types of legal reforms enacted.

Following Vigdor and Mercy (2006), who find that firearm restrictions tied to restraining orders have a stronger preventive impact than those limited to misdemeanor convictions, I separate the treatment group by the scope of legal coverage. Specifically, I distinguish between states that (1) implemented closures applying to individuals under *protective orders* or that adopted *joint closures* covering both protective orders and misdemeanor crimes of domestic violence,[24] and (2) states that closed the loophole only for *misdemeanor crimes of domestic violence*.[25] These two categories capture the key distinction emphasized in the literature between preventive firearm restrictions that apply at the point of separation and those that activate only after conviction.

The results, shown in Table 2, confirm that the scope of coverage matters substantially. States adopting protective-order or joint closures experienced a statistically significant decline of approximately 13.1% in reported cases of VAW (significant at the 5% level and visible in Figure 7), while states that enacted misdemeanor-only closures saw a small, statistically insignificant increase of roughly 6%. This pattern aligns closely with the earlier findings of Vigdor and Mercy (2006), suggesting that firearm restrictions tied to restraining orders reach a broader population at risk and thus have a greater preventive effect, whereas reforms limited to convicted offenders are often too narrow to meaningfully reduce overall violence.

[24] There is not enough variation to further separate states that closed only the protective-order loophole and those that closed both simultaneously. This group includes Louisiana, Oregon, Rhode Island, Vermont, and Washington.

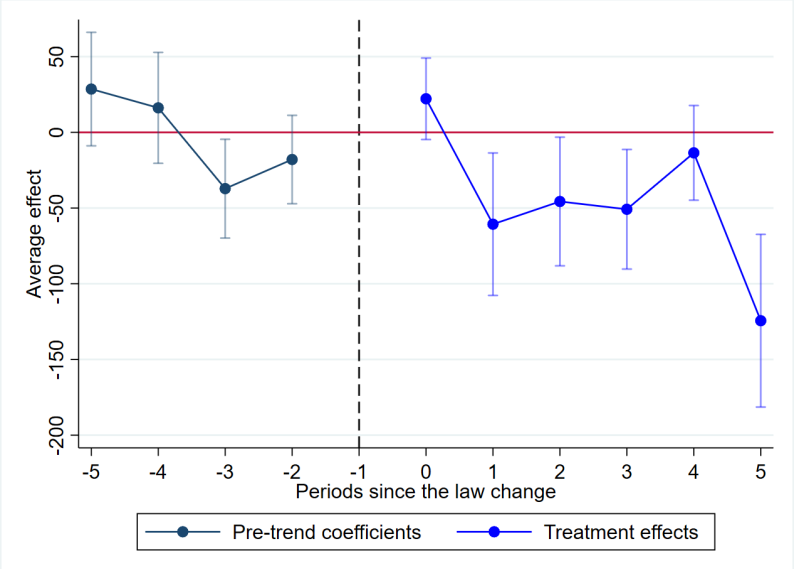
[25] States that closed the loophole only for misdemeanor crimes are Connecticut, Kansas, Kentucky, Maine, Massachusetts, South Dakota, Tennessee, and Utah. Although Table A1 lists Kansas as closing both the PO and misdemeanor loopholes, the PO closure is narrowly defined—it applies only to cohabiting dating partners—and therefore is not counted here as a substantive closure.

Table 2: Effect of loophole closure on VAW, by type of closure (2013–2023)

	(1)	(2)	(3)
Protective-order or joint closures			
Pre_avg	-6.494	0.386	-7.029
	(13.043)	(13.596)	(13.253)
ATT	-39.222**	-38.610**	-35.260**
	(16.023)	(15.581)	(15.361)
Pre-treatment mean	298.374	298.374	298.374
Observations	25,375	25,375	25,375
Misdemeanor-only closures			
Pre_avg	-15.677	-3.076	-15.525
	(18.901)	(19.292)	(18.827)
ATT	23.867	15.857	24.174
	(18.555)	(18.550)	(18.426)
Pre-treatment mean	374.829	374.829	374.829
Observations	32,761	32,761	33,120
Controls	Yes	Yes	Yes
Firearm suicide rate		Yes	
Not-yet-treated			Yes

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Standard errors are wild bootstrap (499 replications) clustered at the state level. ***p<0.01, **p<0.05, *p<0.1.

Figure 7: Effect of loophole closure on violence against women (PO or both)

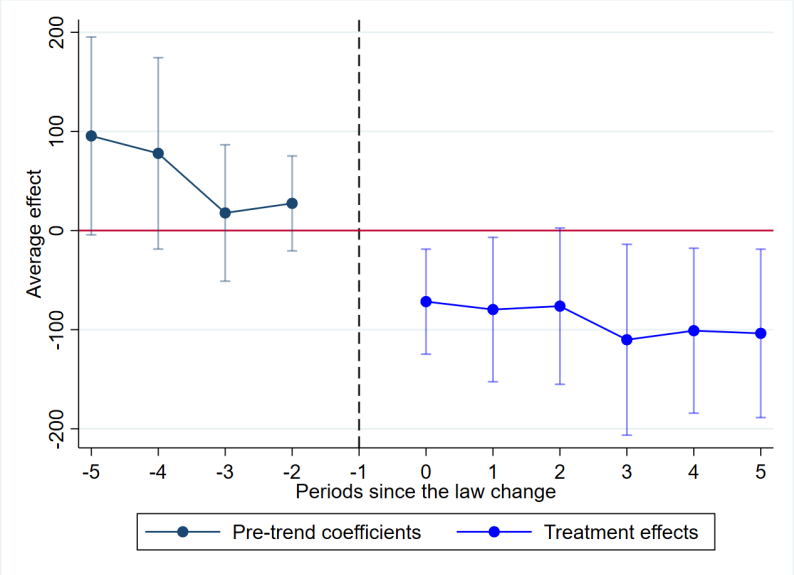


All specifications include state-level fixed effects, and county-level covariates (unemployment rate, and share of Black and Hispanic females per county). Bands represent 95% confidence intervals. Source: NIBRS 2013–2023.

5.1.2 Reduced Sample (2021–2023)

To take advantage of a period with improved data quality, I re-estimate the model for the reduced sample covering 2021–2023. This period benefits from substantially greater NIBRS coverage and uniformity in reporting across states, allowing for more precise measurement of county-level outcomes. The analysis focuses on nine states that implemented relevant legislative changes during this time.[26] As shown in Figure 8, some pre-treatment coefficients differ from zero, though none are statistically distinguishable from it at conventional levels. The main results indicate a statistically significant decline of approximately 29.4% in reported cases of violence against women following loophole closure, robust to additional controls and other checks. As can be observed in Table A4, the estimates remain consistent with the identifying assumption of parallel trends in every specification except for the addition of within-state firearms prevalence changes as accounted for by the *firearms suicide rate* control as well as the estimation for *not-yet-treated*, where the average of point estimates in the pre-law change period is marginally significant at the 10% level.

Figure 8: Effect of loophole closure on violence against women (2021–2023)



Notes: All specifications include state-level fixed effects, and county-level covariates (unemployment rate, and share of Black and Hispanic females per county). Bands represent 95% confidence intervals. Source: NIBRS 2021–2023.

Because eight of these nine states enacted *misdemeanor-only* closures, the strong negative estimate in this period likely reflects the combined influence of improved data quality and a changing policy environment in which most states had already implemented protective-order prohibitions. In other words, these later reforms operate within a legal context that already offers broader protections, so the additional closures—though technically misdemeanor-specific—likely function as incremental policy improvements in a more compre-

[26] Kentucky, Massachusetts, Nevada, Pennsylvania, and Tennessee in 2022; Connecticut, Maine, Utah, and Vermont in 2023.

hensive framework of firearm restrictions.

Together, the results from both samples suggest that the largest and most consistent reductions in violence occur in states that extend firearm prohibitions to individuals under protective orders. By contrast, reforms limited to misdemeanor convictions show no measurable impact on overall VAW rates. The stronger reductions observed in the reduced sample likely reflect both improved data quality and the cumulative policy environment of the 2020s, when protective-order restrictions were already widespread.

5.2 Heterogeneity Analysis

In this section, I extend the analysis by seeing whether the effects differ across groups or different dimensions. I will look at effects on IPV vs. non-IPV violence against women, severe crimes, effects by a proxy of gun prevalence, as well as a race and age analysis. Lastly, I examine results by female empowerment proxies.

5.2.1 IPV vs. Non-IPV Violence Against Women

Because the main outcome variable aggregates all reported cases of violence against women (VAW), I next examine whether the estimated effects are concentrated among intimate-partner cases or extend to broader forms of gender-based violence. Table A5 and Table A6 present these estimates. As the laws I am examining specifically pertain to domestic abusers, it may be probable to assume that the majority of the effect size on reductions in violence against women is coming from the closure of the loophole on IPV. To further investigate this, I re-estimate the model excluding cases of IPV. This allows me to assess whether firearm restrictions targeted at domestic abusers also have spillover effects on other forms of violence against women, as perpetrators of intimate partner violence often commit broader acts of violence: Geller et al. (2021) find that nearly 60% of mass shootings in the U.S. between 2014 and 2019 involved domestic violence, and that many offenders with histories of abuse also killed non-intimate victims.

Full sample (2013–2023). For states adopting protective-order or joint closures, the results indicate that the reductions in overall VAW are not driven solely by intimate-partner incidents, which can be seen in Table A5. When IPV cases are excluded, the estimated decline remains statistically significant and at a slightly stronger magnitude (of 14.9% decline in comparison to 10% for IPV), suggesting that the policy's effects extend beyond the domestic sphere. This pattern aligns with evidence that perpetrators of intimate partner violence often form part of a broader violent offending trajectory: many IPV offenders also commit non-intimate partner crimes or other violent offences outside the relationship context, and restricting firearm access among this group generates broader spillovers that can enhance public safety (Ouellet, 2022; Piquero et al., 2006; Zhang and Howard, 2020).

Reduced sample (2021–2023). For the reduced sample of 2021-2023, results presented in Table A6 of the Appendix indicate that the policy continues to exert an impact even when IPV cases are excluded, at a similar magnitude to the results on VAW (approximately 28.6% decrease in cases). However, the pre-average of the event-study coefficients for intimate-partner violence (IPV) show statistically significant pre-treatment differences, likely reflecting short pre-periods. For this reason, the IPV-specific effects in the reduced sample should be interpreted cautiously.

It is also important to recognize the limitations of the underlying relationship-to-victim variable in NIBRS. Roughly 15% of cases have missing information on the victim–offender relationship, and about 50.5% of recorded perpetrators are categorized as other types of relationships (including but not limited to acquaintances, friends, strangers). The remainder are identified as spouses, ex-spouses, or dating partners. This likely understates the true incidence of IPV, implying that any estimated differences between IPV and non-IPV cases should be viewed as conservative.

5.2.2 Effect on Severe Crimes

Drawing on the FBI's Uniform Crime Reporting (UCR) categories (Federal Bureau of Investigation, 2010), I classify "severe" violence against women (VAW) as incidents involving homicide, rape, or aggravated assault.[27] I re-estimate the model using this restricted outcome to test whether the policy effects observed in the aggregate results also extend to the most serious forms of violence.

Full sample (2013–2023). For the pooled sample, I focus on states that closed the loophole for individuals under protective orders or for both protective orders and misdemeanor crimes. Because misdemeanor-only closures show no statistically significant effects in the baseline analysis, I do not conduct additional heterogeneity analyses for that group. The estimates, shown in Table A7, indicate a measurable decline in severe crimes following protective-order or joint closures - 33% at the 1% significance level. Because these results are based on the more credible identification sample (with stable pre-trends), they provide strong evidence that the observed reductions reflect genuine declines in the intensity of violence, rather than shifts in reporting behavior - however, this must be taken cautiously, as with NIBRS I cannot say anything definitive about reporting behaviour.

Reduced sample (2021–2023). Turning to the shorter 2021–2023 window, I estimate the effect of loophole closures on severe cases for all treated states combined, without distinguishing by closure type due to limited policy variation in this period. As shown in Table A8, the estimated coefficients are negative across all specifications, indicating declines in severe violence against women; however, these effects are

[27] This narrower outcome substantially lowers baseline frequencies: the pre-law mean is approximately 66 cases per 100,000 women versus 311 in the unrestricted 2021–2023 sample.

not statistically significant except for when accounting for within-state fluctuations of firearm prevalence. When I further disaggregate by relationship context in Table A9, the declines are concentrated among severe intimate-partner violence (IPV) cases, which show approximately 20% reductions relative to pre-law averages, marginally significant at the 10% level. Effects for non-IPV severe violence are small and not statistically significant. These patterns suggest that the deterrent impact of firearm prohibitions is strongest within intimate-partner contexts—where access to a gun is most directly linked to escalation risk—but directionally consistent across broader categories of violence.

Because severe offenses are far less susceptible to underreporting, the persistence of these effects is *less consistent* with a mechanism driven solely by changes in reporting behavior (e.g., fear of retaliation) and *more consistent* with a real reduction in the incidence or lethality of violence. While I cannot directly test reporting dynamics, the fact that reductions persist among severe crimes supports the interpretation that firearm prohibitions reduce both the frequency and the severity of violent incidents.

Overall, these results are consistent with the conceptual framework outlined in Table 3. When firearm prohibitions extend to individuals under protective orders, they operate at a particularly high-risk stage—when victims are attempting separation and the potential for escalation is greatest. Restricting access to firearms during this period weakens abusers' capacity to use or threaten lethal force, thereby reducing both the incidence and severity of violence (Sullivan and Weiss, 2017; Vigdor and Mercy, 2003; Zeoli et al., 2019).[28] The declines observed in severe crimes suggest that these laws not only deter violence but also limit its potential lethality. The results therefore indicate that comprehensive protective-order provisions play a critical role in preventing the most severe forms of gender-based violence.

5.2.3 Effect by Tercile of Firearms Suicide Rate

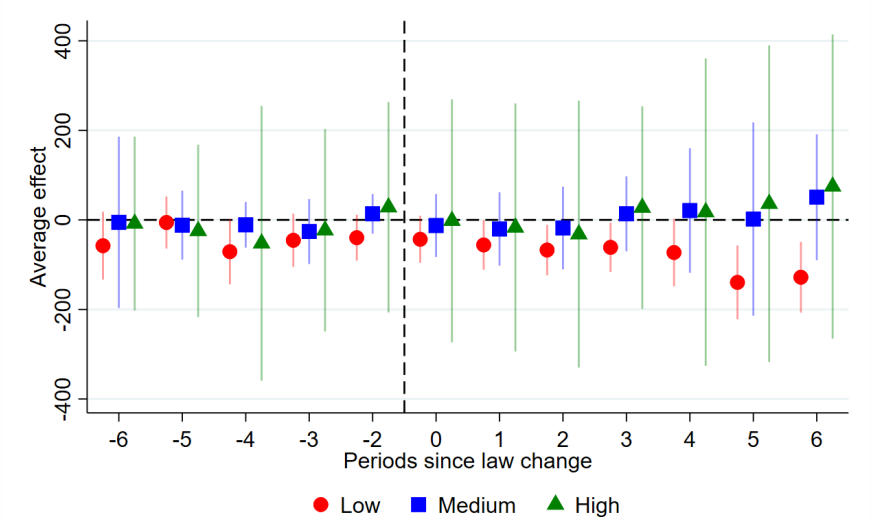
To assess whether the effectiveness of firearm restrictions depends on the broader prevalence of guns within a state, I next examine heterogeneity by firearm suicide rate (FSS), which serves as a widely used proxy for household firearm ownership (Cook and Ludwig, 2006). This analysis also relates to one of the mechanisms discussed earlier: if U.S. men place a high value on firearm ownership, then laws that risk removing that access could, in principle, deter violent behavior—since committing a disqualifying offense would mean losing a valued possession. However, the results point in the opposite direction.

Using FSS data from the CDC (Centers for Disease Control and Prevention (CDC), 2024), I classify states into terciles of low, medium, and high firearm prevalence based on their average five-year rolling mean over

[28] This reasoning is supported by public-health research showing that firearm prohibitions linked to protective orders reduce the likelihood of injury and homicide: for example, Willie et al. (2021) find lower non-fatal IPV injuries in states that require firearm relinquishment for PO respondents, and Vigdor and Mercy (2006) show associations between DV-firearm laws and reduced intimate partner homicides.

the full 2013–2023 sample. I then re-estimate the models separately for each tercile. Since firearm availability and related reporting practices may follow different trajectories across states, all specifications in this section include state linear time trends to account for differential underlying trends in gun ownership. Consistent with Edwards et al. (2018), the results (Figure 9 and Figure 10) indicate that the effects of loophole closures are strongest in states with the lowest firearm prevalence and attenuate in states with the highest prevalence. This pattern suggests that when firearm ownership is highly prevalent and culturally embedded, restrictions targeting specific groups—such as domestic abusers—become less binding, as alternative access channels (private transfers, secondary markets, or family-held firearms) remain open. Conversely, in lower-prevalence states, where gun access is more limited, the same legal reform likely raises the real and perceived cost of firearm-enabled violence, strengthening deterrence. In such contexts, perpetrators subject to prohibitions are also more likely to encounter background check requirements when attempting to acquire a firearm, as noted by Edwards et al. (2018), further amplifying the policy's effectiveness.[29]

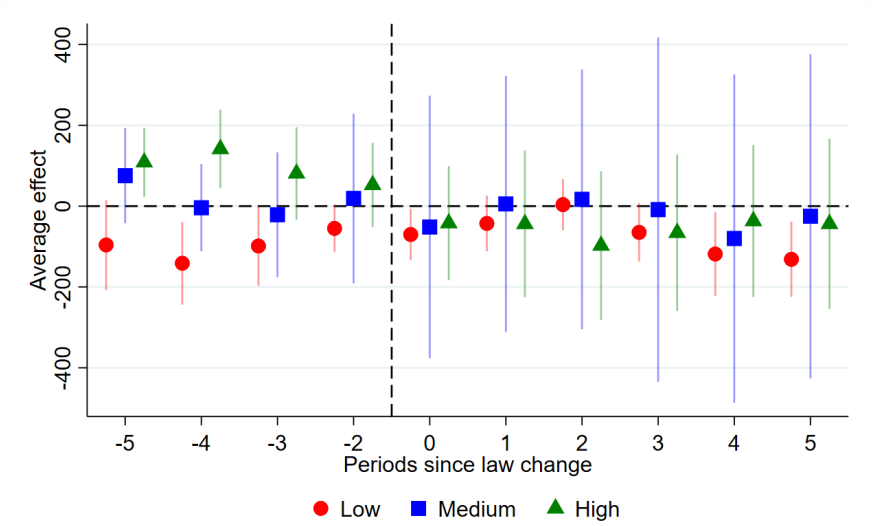
Figure 9: Heterogeneous loophole closure effects by tercile of FSS (2013 to 2023)



Notes: All specifications include state linear time trends, state-level fixed effects, county-level covariates: unemployment rate, and share of Black and Hispanic females per county. Bands represent 95% confidence intervals. Associated tables are available upon request. Source: NIBRS 2013-2023.

[29]The heterogeneity analysis is informative even though the full-sample average effect is not significant, as it helps identify contextual factors that may obscure aggregate patterns. In addition, results for the sample of 2021-2023 should be interpreted cautiously due to some significant pre-trends.

Figure 10: Heterogeneous loophole closure effects by quartile of FSS (2021 to 2023)



Notes: All specifications include state linear time trends, state-level fixed effects, county-level covariates: unemployment rate, and share of Black and Hispanic females per county. Bands represent 95% confidence intervals. Associated tables are available upon request. Source: NIBRS 2021-2023.

5.2.4 Effects by Race and Age

In Table A10, I document that pre-treatment levels of violence against women are highest for Black women, followed by Hispanic and then non-Hispanic White women. After loophole closure, however, the percentage decline is slightly larger for non-Hispanic White women (about 16.8%) than for Black women (about 14%). This pattern is consistent with group differences in firearm ownership. Survey evidence from the Pew Research Center (2017) shows that gun ownership in the United States is disproportionately concentrated among White, non-Hispanic men—particularly those living in rural areas—while ownership rates are considerably lower among Black and Hispanic adults. This pattern may also help explain the results found by (Wallin et al., 2022), in which passage of domestic violence restraining order laws has significant effects for White women, but not for Black women. Meanwhile, the much higher pre-treatment levels among Black women underscore that they continue to face greater overall risk, even if the percentage decline is somewhat smaller. Interestingly, controlling for within-state gun-environment variation (through my proxy for firearm prevalence) yields a stronger effect - approximately a 25% decrease in cases of violence for Black women after the law is passed. Thus, when I adjust for within-state short-run fluctuations in gun environments, the estimated effect of the loophole closure becomes stronger for Black women — consistent with them being more exposed to—and more affected by—such fluctuations.[30]

[30] Black women face disproportionately high exposure to firearm violence compared to White women (Lanfear et al., 2023). However, there still is not a large enough body of evidence explaining how effects of gun policies differ across racial and ethnic

In addition, reporting behavior with respect to IPV varies systematically across racial and ethnic groups, partly shaped by cultural and community norms. Conditional on experiencing IPV, Black women have been found to be more likely to report incidents to police—particularly when the episode involves injury—while Hispanic women are somewhat less likely to report, potentially due to immigration concerns and linguistic barriers (Dave et al., 2025; Erten and Keskin, 2018; Holliday et al., 2020). These reporting differences may help explain some of the variation observed across groups, as underreporting can attenuate measured treatment effects even when true reductions in violence occur. It is also important to note that race and ethnicity are imperfectly measured in NIBRS: in 2023, approximately 46.4% of observations are missing ethnicity and 34.3% are missing race, suggesting that the estimates for Hispanic women in particular should be interpreted as lower bounds.

Turning to age heterogeneity, as can be seen in Table A11, I find statistically significant reductions in VAW for both groups, but the effects are clearly larger for women over 30—approximately a 34% decline compared to 21.8% for younger women—even though the pre-treatment mean is higher for the under-30 group. One plausible explanation is that firearm ownership is more common among older, non-Hispanic men—especially White, middle-aged men—so firearm-based prohibitions bind more in these relationships.

5.2.5 Female Empowerment

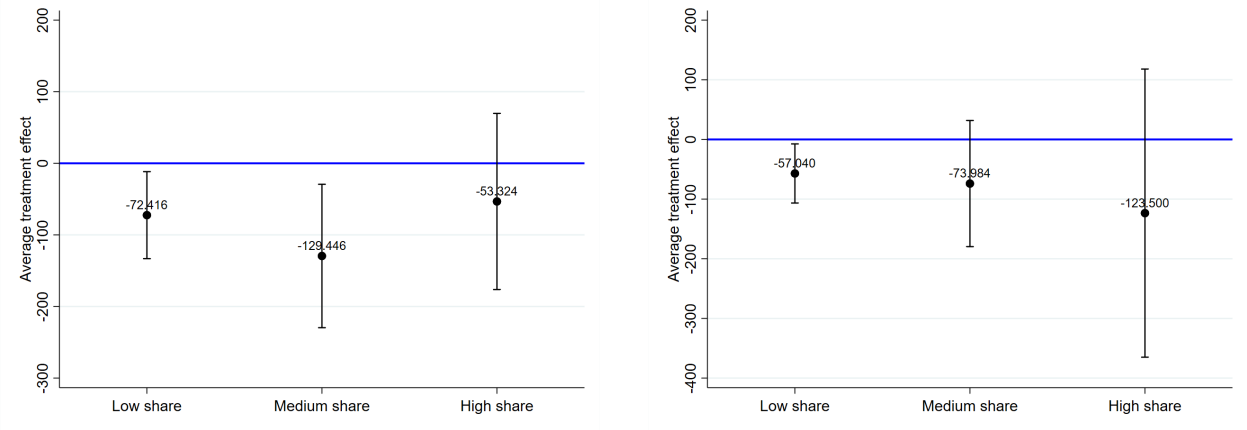
Figure 11 presents heterogeneity in treatment effects by two proxies for female empowerment. The left panel groups counties by terciles of the share of full-time sworn female officers[31], and the right panel by the share of agencies in each county with any domestic-violence (DV) personnel. Across both measures, estimated effects of closing the boyfriend loophole are consistently negative, indicating declines in violence against women following the policy change. The magnitude of these reductions generally increases with higher levels of empowerment—particularly where a greater share of agencies employ DV staff—although confidence intervals are wide and differences across groups are not statistically significant.

Consistent with evidence that greater female representation in policing increases reporting of violent crime and domestic violence (Miller and Segal, 2019), these patterns suggest that the law's effectiveness is enhanced in contexts where victims may feel safer engaging with formal systems. In counties with higher female representation and more DV staffing, greater trust and responsiveness may have strengthened enforcement of firearm restrictions and facilitated reporting. The Law Enforcement Management and Administrative Statistics (LEMAS) data from 2013 and 2020 capture these institutional characteristics, though a proportion of agencies do not report information on DV units.

groups.

[31] According to the Census of State and Local Law Enforcement Agencies from 2018, women made up about 12% of the full-time sworn personnel employed by state and local law enforcement agencies (Gardner and Scott, 2022).

Figure 11: Female empowerment heterogeneity (2021–2023)



Share of full-time female police officers

Share of agencies with any DV personnel

Source: NIBRS. *Notes:* Each panel shows the simple average of the event-time effects over event periods 0–5. Each share is by county. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females).

5.3 Interpretation

The results reveal meaningful heterogeneity across types of firearm prohibitions, clarifying the mechanisms through which these laws influence violence against women. The statistically significant declines following the closure of the protective-order loophole—approximately a 13% reduction in reported cases—suggest that these laws deter violence at a critical moment of risk: when victims initiate separation. Prior research shows that women typically seek restraining orders after severe abuse or stalking, often while leaving their abuser—a period associated with heightened homicide risk (Campbell et al., 2007; Logan et al., 2008). At this stage, firearm restrictions reduce the credibility of lethal threats, weaken abusers' ability to exert coercive control, and diminish the likelihood of escalation into severe violence.

By contrast, misdemeanor-only closures are associated with small, statistically insignificant changes in reported violence. The coefficients are positive but close to zero in magnitude, consistent with the idea that such narrowly defined prohibitions—limited to those convicted of misdemeanor domestic violence—do not substantially alter the overall risk environment. However, in the reduced sample (2021–2023), where nearly all states had already implemented protective-order restrictions, new misdemeanor closures coincide with additional reductions in violence, particularly for non-intimate partner gender-based violence. This pattern suggests that the effectiveness of such laws depends on the broader legal environment: when protective-order coverage is already in place, incremental extensions to misdemeanor offenders may strengthen deterrence by closing residual gaps in enforcement.

Importantly, the analysis of severe crimes supports this interpretation. Reporting does not decline following

reform, indicating that victims are not silenced but may instead feel safer engaging with law enforcement. This is additionally supported by the female empowerment results - which shows that firearm restrictions may not only deter potential offenders but also may further empower victims by improving their perceived safety and outside options.

Finally, evidence on non-intimate-partner violence against women (non-IPV VAW) indicates that men with histories of domestic abuse are also more likely to commit violent acts outside the relationship. There are many cases in the U.S. in which a perpetrator with a violent past committed violence outside of a relationship (Burkett, 2023; Lawson, 2024; Staff, 2025). This spillover reinforces the interpretation that boyfriend loophole closures reduce violence not only by altering dynamics within abusive partnerships, but also by constraining individuals who are generally prone to violent behavior. In this way, the laws address both relationship-specific coercive control and the broader propensity for violence, improving public safety.

6 Mechanisms

This section discusses the mechanisms and theoretical framework through which boyfriend-loophole closures may reduce violence against women. Two channels are especially salient. First, firearm restrictions can disrupt patterns of coercive control that sustain abusive relationships. Guns are not only tools of physical harm but also instruments of intimidation and dominance. Removing legal access to firearms limits an abuser's ability to exert "silent" control through threats, thereby weakening the broader system of coercion that underlies intimate partner violence (Lynch and Logan, 2018; Sorenson and Schut, 2018). As Small et al. (2019) note, victims may feel safer and more willing to seek police assistance once a firearm is removed from a home, while abusers deprived of a gun may resort to more overt—and less effective—forms of intimidation. This interpretation aligns with sociological and psychological theories emphasizing coercive control as a mechanism of power and domination within abusive relationships.

Second, these laws may operate through deterrence. By criminalizing firearm purchase or possession among individuals subject to qualifying misdemeanors or protective orders, boyfriend-loophole closures increase the expected cost of domestic violence. In economic terms, they alter the incentive structure facing potential offenders, consistent with models of deterrence and household bargaining in which behavior responds to changes in the expected benefits and costs of violence (Aizer, 2011; Farmer and Tiefenthaler, 1997; Tauchen et al., 1991). Deterrence is often assumed to be strongest where gun ownership is widespread and highly valued, but it may also depend on the credibility of enforcement and the extent to which abusers perceive legal access to firearms as important.

Finally, these mechanisms may also operate through the victim's side. By reducing the perceived risk of lethal retaliation, firearm prohibitions expand victims' outside options and may increase reporting or facilitate exit from violent relationships (Aizer and Dal Bó, 2009). Together, these channels link the economics

of deterrence and bargaining with sociological theories of coercive control, offering a multidimensional understanding of how firearm policy can alter both offender behavior and victim responses.

6.1 Coercive control

Beyond lethal outcomes, firearms play a central role in coercive control within abusive relationships. Nonfatal firearm use—including brandishing a weapon, threatening with a gun, or discharging it without intent to kill—serves as a powerful means of intimidation and control. Even when not fired, the presence of a firearm communicates a credible threat that can instill fear, compel compliance, and restrict a victim's ability to seek help or leave the relationship. Administrative data on police-attended incidents confirm that weapons are frequently used in this way across both marital and dating relationships, with boyfriends and girlfriends responsible for a substantial share of violent episodes (Sorenson and Spear, 2018). Descriptive analyses of weapon use in IPV further show that, although guns are used in a minority of incidents, they are primarily employed to threaten and intimidate rather than to inflict immediate physical injury (Sorenson, 2017), reinforcing the idea that firearms amplify coercive control by enhancing the credibility of threats of lethal force, and raising the perceived cost of exit. In a dynamic model of abusive relationships, abusers may strategically shift between overt violence and coercive control to suppress women's outside options and maintain dominance (Adams et al., 2024). When access to firearms is restricted—such as through protective-order or joint loophole closures—these threats become less credible, weakening abusers' control and improving victims' perceived safety.

Although coercive control is difficult to observe directly, the patterns in my data are consistent with this mechanism: significant reductions in violence, stronger effects for severe offenses when following protective order or joint closures, and no evidence of reporting decline all point toward an underlying empowerment channel. By limiting the abuser's capacity to credibly threaten lethal harm, these laws can reduce both overt violence and the coercive power that sustains abusive relationships.

6.2 Deterrence

Deterrence operates through changes in the incentives facing abusers. State laws that extend firearm prohibitions to dating partners raise the "price" of using or keeping a gun in the context of domestic violence—through mandatory purchase bans, permit loss, and enforced relinquishment.[32] Consistent with this mechanism, jurisdictions with restraining-order firearm prohibitions observe significant reductions in intimate partner homicide, with larger effects where statutes cover dating partners, apply at the ex parte stage,

[32] Firearm relinquishment is difficult to enforce. For example, 19 states explicitly require all people subject to final domestic violence restraining orders to relinquish their firearms for the duration of the court order - however, only 11 require proof of compliance. Similarly, 16 states require individuals convicted of domestic violence misdemeanors to relinquish their firearms after conviction, however only 10 require proof of compliance (Giffords Law Center, 2024).

and require firearm relinquishment (e.g., Vigdor and Mercy, 2003, 2006; Zeoli et al., 2017; Zeoli and Webster, 2010). Further, if an abuser values gun ownership, the threat of losing access increases the expected cost of firearm-involved IPV.

Given the unusually high salience of gun ownership in the United States—an estimated 120.5 guns per 100 people (World Population Review, 2025)—this channel is especially relevant. If abusers place high value on legal access to firearms, then restrictions that criminalize possession should meaningfully alter behavior. This motivates my empirical test of whether deterrence effects are stronger in states with higher gun prevalence. In a dynamic framework, credible and enforceable firearm prohibitions increase the expected costs to abusers—both by heightening legal risks and by removing a powerful tool of threat—thereby reducing the use of firearms and limiting the effectiveness of coercive control (Adams et al., 2024). A key assumption in this channel is that abusers value gun ownership; if losing access to firearms is costly, restrictions should deter violence more strongly in contexts where gun ownership is common.

To test this, I compare effects across states with higher versus lower levels of gun prevalence, using the FSS as a proxy.[33] If abusers in high-ownership states are more motivated to avoid losing access, deterrence effects should be larger there. Instead, I find the opposite: the reductions in VAW are stronger in states with lower levels of gun ownership. This suggests that deterrence may operate less through attachment to firearms per se, and more through broader enforcement credibility or through reductions in coercive control when firearms are less prevalent. This contrast highlights that deterrence may not hinge solely on abusers' attachment to firearms, but on how much legal prohibitions shift the risks of possession and the credibility of sanctions. Where gun ownership is less prevalent, firearm prohibitions may more sharply disrupt coercive control by criminalizing possession, whereas in high-ownership states abusers may find ways to retain access despite the law. This also helps explain why protective-order closures, which apply at the critical moment of separation, are associated with strong reductions of severe IPV (33%).

6.3 Victim responses and outside options

The mechanisms discussed above emphasize the perpetrator's incentives, but laws may also operate through the victim's side of the relationship. In household bargaining models, a woman's threat point or outside option determines her bargaining power and, in turn, the level of violence she faces (Adams et al., 2024). Abusers often sustain control by suppressing these outside options through intimidation, coercion, or threats of retaliation. Firearm access plays a central role: the presence of a weapon raises the cost of exit by making retaliation potentially lethal. By contrast, restricting firearm access lowers these costs, expands outside options, and increases victims' ability to leave abusive relationships or resist coercion.

[33] Background check data measure new firearm transactions and therefore capture the flow rather than the stock of firearms (Lang, 2013). In contrast, the firearm suicide share (FSS) has been repeatedly validated as a stable proxy for household gun ownership across time and regions (Cook and Ludwig, 2006; Miller et al., 2002).

The IPV literature emphasizes that women stay in abusive relationships not only due to economic dependence, but also due to fear of retaliation, social stigma, custody concerns, and lack of protective alternatives (Heise et al., 1994). Firearm restrictions target one of the most salient barriers—fear of violent retaliation—thereby reducing entrapment and enhancing victim autonomy. This aligns with theoretical perspectives that view violence as instrumental: when guns are removed, the credibility of threats diminishes, reducing the effectiveness of coercive control as a bargaining tool (McGlynn and Johnson, 2014).

These laws may also influence reporting behavior. On the one hand, higher penalties for armed abusers could discourage reporting if victims fear escalation or backlash. On the other hand, by reducing abusers' access to lethal weapons, such laws lower the perceived risks of retaliation, potentially making victims more willing to seek help. My empirical results on severe crimes indicate that reporting does not decline following boyfriend-loophole closures, suggesting that victims are not silenced but instead may feel safer engaging with formal systems. Consistent with this interpretation, the heterogeneity analysis shows larger reductions in violence in counties with higher female representation in law enforcement and greater DV staffing—contexts where victims may face lower barriers to reporting and enforcement tends to be more responsive. Thus, firearm restrictions improve both the bargaining position of victims within relationships and their ability to mobilize external enforcement when abuse occurs.

Table 3: Conceptual framework: how firearm restrictions map to offender- and victim-side mechanisms

Legal context	**Perpetrator mechanisms (deterrence & coercive control)**	**Victim mechanisms (reporting & outside options)**
PO loophole open; misdemeanor coverage only	Weaker deterrence at separation. Abusers can still retain or access firearms under protective orders, so the cost of misuse is lower. Consistent with Adams et al. (2024), abusers may substitute toward less-severe abuse to maintain control.	Mixed reporting incentives. Some victims may feel safer reporting lower-severity incidents, while fear of lethal retaliation persists. This may present as increased total incidents without a rise in severe crimes.
PO loophole closed (with or without misdemeanor coverage)	Strong deterrence at separation. Firearm removal, inability to purchase post separation, or high cost of possessing firearm reduces the credibility of lethal threats and constrains coercive control, lowering IPV intensity and escalation.	Higher outside options and stable reporting. Reduced retaliation risk enhances exit options; evidence of no decline in severe-crime reporting supports empowerment rather than silencing.
Both PO and misdemeanor coverage	Strongest constraint. Comprehensive prohibitions raise the cost of possession/misuse and reduce scope for substitution between violence and coercive control.	Safest environment for help-seeking. Lower perceived lethality supports reporting and service use; outside options expand through reduced entrapment and improved willingness to engage with formal systems.

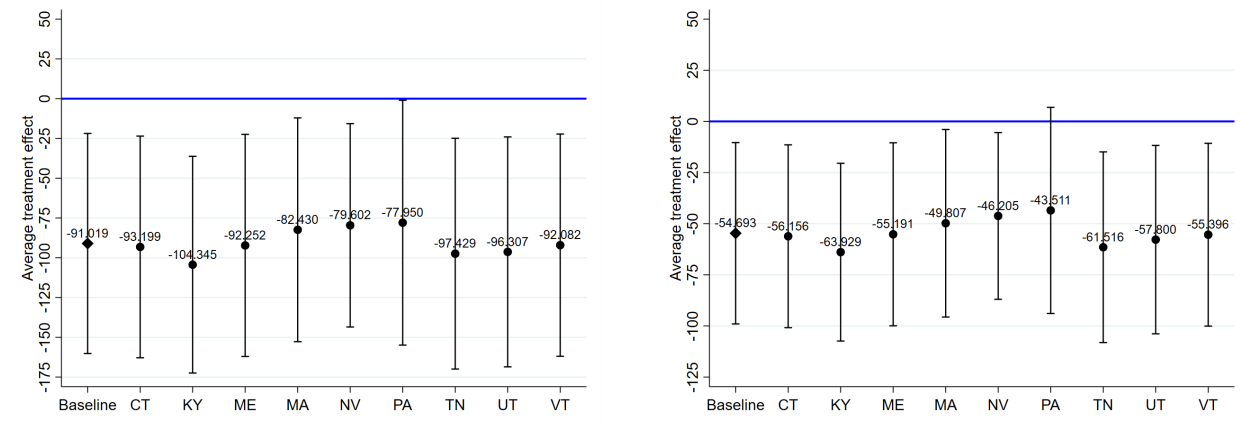
7 Robustness Checks

I conduct a number of analyses to examine whether the results (particularly for the recent sample of 2021 to 2023, as it more reliable due to data coverage) are robust to changes in specification. I perform a "leave-one-out" exercise, re-estimate the model on a balanced subsample of counties, run a number of falsification tests on other unrelated crimes, and lastly, in Table A15 show that my results are robust to 1) additional age group controls, 2) controlling for the COVID-19 public health crisis by adding the COVID mortality rate as a control, 3) alternate definition of VAW as used in Dave et al. (2023)[34], and 4) alternative aggregations of event-study effects. In Table A16 I report results with robust asymptotic normal standard errors, similar to Lawler and Skira (2025). Finally, in Table A17, I control for the number of agencies reporting VAW incidents within each county-quarter-year to ensure data quality which produces very similar estimates (as done by Dave et al. (2023)). The results are robust to each of these tests.

7.1 Leave one state out analysis

To assess whether the estimated treatment effects are driven by any single treated state, I conduct a leave-one-state-out (LOSO) analysis, shown in Figure 12. In this approach, I re-estimate the main specification repeatedly, each time excluding one treated state from the sample. This allows me to evaluate the stability of the results. The graphs suggest that no particular state has an disproportionate impact on my estimates. In addition, I perform this exercise on the heterogeneous results of non-IPV violence (as this specification showed no significant pre-trends), and find similarly stable results.[35]

Figure 12: Robustness of estimated coefficient on VAW to dropping one treated state at a time (2021–2023)



Violence against women

Non-IPV VAW

Source: NIBRS. *Notes:* Each panel shows the simple average of the event-time effects over event periods 0–5 in the baseline specification and when iteratively dropping each treated state. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females).

[34]The incidents considered are aggravated assaults, simple assaults, forced sex, and intimidation.

[35]Except for Pennsylvania - although still significant at the 10% level.

7.2 Balanced sub-sample analysis

Similar to (Muratori, 2025), I re-estimate the main analysis on a balanced subsample[36] and find no notable changes in magnitude or significance of the effects. There is a slightly stronger effect as can be seen Table A12.

7.3 Falsification test

A useful robustness check for assessing potential omitted variable bias is to test whether the laws in question are also associated with changes in other types of crime. Table A13 presents the estimated effects of the state-level law indicators on larceny, robbery, burglary, and motor vehicle theft rates.[37] Reasonably, the change in laws regarding firearm purchase and possession for dating partners should not have a significant or notable effect on these crime rates. The list of crimes included is Table A14 and as can be seen in the table, I find no robust coefficients. If the observed relationship between the boyfriend loophole closures and violence against women were driven by an unobserved factor reflecting broader trends in crime, violence, or enforcement, we would expect to see similar effects on these other crime categories as well. The falsification test follows Vigdor and Mercy (2006), and the placebo outcomes—such as theft or property crimes—are conceptually distinct from gender-based or domestic violence, typically involving different offenders, victims, and motivations. These crimes are therefore not expected to respond directly to policy changes targeting intimate partner or gender-based violence. A null finding in these categories strengthens the interpretation that the observed effects are specific to violence against women, rather than reflecting general shifts in violence or policing.

8 Conclusion

This research provides the first causal evidence on how closing the *boyfriend loophole*—by extending firearm prohibitions to dating partners—affects violence against women. Leveraging variation in the timing of these reforms, I find that extending prohibitions to individuals under protective orders generates sizable declines in reported VAW of more than 13 percent, and nearly 30 percent in later years when NIBRS coverage improves. In contrast, misdemeanor-only closures produce insignificant or inconsistent effects, underscoring that the scope and enforceability of firearm restrictions matter. I also document clear spillovers onto non-IPV violence, consistent with mechanisms of deterrence, incapacitation, and weakened coercive control—where firearm access increases the credibility of threats and the costs of exit (Vigdor and Mercy, 2006).

[36] These are the counties which can be observed throughout the entire sample period

[37] In the FBI's Uniform Crime Reporting framework, "larceny-theft" is defined broadly as the unlawful taking, carrying, leading, or riding away of property from the possession of another (excluding motor vehicle theft, embezzlement, forgery, fraud, etc.).

These results align with broader evidence that individuals with histories of domestic abuse pose heightened risks of violent offending beyond intimate partnerships. Men with domestic violence (DV) histories are disproportionately involved in non-IPV violence and mass shootings (Geller et al., 2021); firearm access further amplifies these risks (Zeoli et al., 2022). Related research shows that well-targeted firearm policies can reduce violence in high-risk groups, such as child access prevention laws lowering juvenile firearm homicides (Anderson et al., 2021). My findings complement this literature by demonstrating that domestic-violence firearm restrictions reduce violence against women both within and outside intimate relationships.

From an economic perspective, these findings reflect the framework of Edwards et al. (2018), who emphasize that firearms generate both private utility and significant externalities, and that well-designed regulations can improve welfare by reducing harms among high-risk populations. The results are also consistent with the public-health literature documenting that restricting firearm access for abusers under protective orders reduces intimate-partner homicides and that effects of misdemeanor restrictions are often limited (Rochford et al., 2022; Vigdor and Mercy, 2006). As non-fatal IPV vastly exceeds fatal cases (Rennison and Welchans, 2000), it is reasonable to expect larger policy impacts in this context.

The heterogeneity analysis reveals that the benefits of firearm prohibitions are most pronounced for older women and for White women, despite higher baseline rates of violence among Black women (Giffords Law Center, 2024). This pattern likely reflects differences in firearm access across demographic groups—gun ownership in the United States is disproportionately concentrated among White men (Pew Research Center, 2017). Effects for women over 30 may also relate to age-related patterns of firearm access and relationship stability. Additional evidence from severe crimes and local institutional characteristics suggests that the reductions in victimization observed reflect true reductions in violence rather than changes in reporting, and are amplified in counties where greater female representation and DV staffing reduce barriers to seeking help.

The primary limitation of this study is incomplete NIBRS coverage. While contributing agencies represent an increasing share of the U.S. population, heterogeneity in reporting reduces geographic representativeness. However, robustness checks adjusting for the number of contributing agencies per county and analyses restricted to later years yield consistent results. As NIBRS expands nationwide– now covering more than 80 percent of the U.S. population— it will provide an increasingly powerful tool for studying violence against women on a nationally representative scale. Future work will be able to test the durability and generalizability of these findings with more comprehensive data.

Finally, focusing on the closure of the boyfriend loophole provides important policy insight. One in three women worldwide experience physical and/or sexual violence during their lifetime, most often at the hands of an intimate partner (United Nations Women, 2024). The urgency of addressing intimate partner violence is thus both global and national in scope, with firearm access playing an especially critical role in shaping its

prevalence and lethality in the United States. The Violence Against Women Act (VAWA) sought to reduce women's risk of injury and death from armed abusers (Small et al., 2019), yet early firearm restrictions relied heavily on marriage, cohabitation, or childbearing as criteria—leaving large portions of the population unprotected (Sorenson and Spear, 2018). Given rising ages at marriage and first birth (Khandwala et al., 2017; Lee and Payne, 2010; Sorenson and Spear, 2018), these gaps have become increasingly consequential. The evidence presented in this study suggests that closing the loophole meaningfully enhances safety for vulnerable populations.

Overall, this research highlights the importance of aligning legal frameworks with empirical realities. Targeted firearm regulations—whether restricting minors' access or limiting abusers' access in domestic-violence contexts—can substantively reduce violence and improve welfare. The U.S. experience underscores both the risks of leaving legal gaps in firearm restrictions and the potential benefits of closing them. At the same time, enforcement remains a challenge - particularly around firearm relinquishment - and future research should examine how institutional capacity, victim reporting behavior, and complementary supports shape the effectiveness of domestic-violence firearm laws.

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A Appendix: Supplementary Figures and Tables

A. Data and Legal Variables

Table A1: Relevant statutes and closure of the boyfriend loophole by state

State	Protective Order (PO)	Misdemeanor (MCDV)	Closure (PO)	Closure (MCDV)
Alabama	Ala. Code § 13A-11-72(a)	Ala. Code § 13A-11-72(a)	N	N
Alaska	Alaska Statute § 18.66.100(c)(6), (c)(7)	Alaska Statute § 12.55.015(f); 18.65.515(b); Alaska Statute § 18.66.990	Y	N
Arizona	Ariz. Rev. Stat. § 13-3602(G)(4); Ariz. Rev. Stat. § 13-3601	Ariz. Rev. Stat. § 13-3101(A)(7)(d), 13-3102(A)(4)	Y	Y
Arkansas	Ark. Code § 9-15-207(b)(3); Ark. Code §§ 9-15-205; 9-15-206	Ark. Code § 5-73-308(a)(1)(A), (a)(2)	N	N
California	Cal. Penal Code § 29825(a)	Cal. Penal Code § 29805	Y;	Y;
Colorado	Colo. Rev. Stat. § 13-14-101(2); 18-6-803.5(1)(c)(I)	Colo. Rev. Stat. § 18.6.801(8)(a)I)	Y;	Y;
Connecticut[38]	Conn. Gen. Stat. § 53a-217(a)(4).	Conn. Gen. Stat. § 53a-217c(a)(D), (C), 53a-217(a)(D), (C); 46b-38h; 46b-38a	Y;	Y; 2023
DC	D.C. Code Ann. § 7-2502.03(a)(12)	D.C. Code Ann. § 7-2502.03(a)(4)(D).	Y;	Y;
Delaware	Del. Code Ann. tit. 11, § 1448(a)(6)	Del. Code Ann. Tit. 10 § 1041	Y;	Y;
Florida	F.S.A. § 790.401; F.S.A. § 790.233(1)	F.S.A. § 790.23(1)	N	N
Georgia		GA Code § 16-11-129 (b)(2-E)	N	N
Hawaii	HI Rev Stat § 134-7(f)	HI Rev Stat § 134-7	Y	Y
Idaho	ID Code § 18-3302		N	N
Illinois	725 ILCS 5/112A-14(b)(14.5)(A)	430 ILCS 65/4(a)(2) (ix); 430 ILCS 65/8(l).	Y	Y
Indiana	IC 35-31.5-2-128	IN Code § 35-47-2-1.5; IN Code 35-31.5-2-128(a)(2); IN Code 35-47-4-6	N	Y
Iowa	IA Code § 724.26; IA Code §§ 236.2, 4 ,5 IA Code §236.5(1)(b)(2), 724.26(2), (4);	IA Code § 724.26; IA Code § 236.2	N	N
Kansas	K.S.A. § 21-6301(a)(18), (m)(1)[39]	KSA § 21-6301(a)(18), 21-6301(m)(1).	Y; 2018	Y; 2018
Kentucky	KRS § 237.110(13)(k)	KRS § 237.110(4).	N	Y; 2022
Louisiana	LA R.S. 46:2135; 2017 House Bill 223 (HB 223)[40]	LA Rev Stat § 14:34.9; 2017 House Bill 223 (HB 223)	Y; 2017	Y; 2017
Maine	15 ME Rev Stat § 393; 19-A ME Rev Stat § 4110 (3)(B), (4); 19-A ME Rev Stat § 4110[41]	19-A M.R.S. § 4102(4); M.R.S. § 393(1-B); Title 15, §393. Public Law 2021 c. 647.	N	Y; 2023
Maryland	MD. Family Law Code § 4-501 (m); MD Pub Safety § 5-133(b)(1)[42]	MD Pub Safety § 5-101(b-1); MD Family Law Code Ann. § 6-233., 5-101(g)(3)	Y; 2015	Y; 2015
Massachusetts	Mass. Gen. Laws ch 209a § 3b	Mass. Gen. Laws ch. 140, § 129B(1)(i)(f); MA Gen L ch 265 § 13n	Y;	Y; 2022
Michigan	Mich. Comp. Laws Serv. §28.422(3)(a)(iii) and (iv); Effective 2024	MI Comp L § 750.224f; Am. 2023, Act 201, Eff. Feb. 13, 2024	N	N
Minnesota	Minn. Stat. §§ 609.749, subd. (8), 609.2242, subd. (3)	Minn. Stat. §§ 609.749, subd. (8), 609.2242, subd. (3)	Y;	Y;

Continued on next page

[38] See also Conn. Gen. Stat. § 53a-217c(a)(5) for similar prohibitions for "criminal possession of a pistol or revolver."
[39] Limiting because only with dating partners who have cohabited
[40] Limiting because it only applies to current partners
[41] Relief is available but not directly through firearms laws. This refers to the federal law which has not covered boyfriends.
[42] Only if relationship was within 1 year of the petition

Table A1: Relevant statutes and closure of the boyfriend loophole by state (continued)

State	Protective Order (PO)	Misdemeanor (MCDV)	Closure (PO)	Closure (MCDV)
Mississippi	Mississippi Code § 97-3-7 (3), (4)	Mississippi Code § 93-21-3 (a), (d), MS Code § 45-9-101(2)	N	N
Missouri	MO Rev Stat § 455.010; MO Rev Stat § 455.045; MO Rev Stat § 455.050	MO Rev Stat § 455.010	N	N
Montana	MT Code § 40-15-102	MT Code § 45-5-206(2)(b).	Y;	Y;
Nebraska	NE Code § 28-1206(5) ; 28-323(8).	NE Code § 28-1206(5), 28-323(8).	Y;	Y;
Nevada	NRS 202.360 (d) apply to an extended order issued pursant to NRS 33.030 on or after October 1, 2017	NV Rev Stat § 202.360	Y; 2017	Y; 2022
New Hampshire	NH Rev. Stat.§ 173-B:5(II) and B:4	N.H. Rev. Stat §§ 159:3; 3-a[43]	Y	N
New Jersey	N.J. Stat. Ann. §§ 2C:25-19d; N.J. Stat. Ann. § 2C:25-29(b)	N.J. Stat. Ann. §§ 2C:25-19d; N.J. Stat. Ann. § 2C:39-7b(1), (2).	Y	Y
New Mexico	NM Stat. Ann. § 30-3-11(A), (B).	NM Stat § 40-13-2.; NM Stat § 30-7-16(A)(3)	Y	Y
New York	NY Crim Pro L § 530.14	NY Penal L § 400.00 (1) ; NY Penal L § 265.00 (17)	Y	Y
North Carolina[44]	NC Gen Stat § 50B-1(b), § 50B-3	NC Gen Stat § 50B-1(b)	Y	Y
North Dakota	ND Cent Code § 14-07.1-02(4)(g); 62.1-02-01	ND Cent Code § 14-07.1-01; 12.1-17-01.2.; 62.1-02-01	Y	Y
Ohio[45]	Ohio Rev. Code § 3113.31(E)(1)(h)	Ohio Rev. Code § 2923.132(A)(2)	N	N
Oklahoma	22 OK Stat § 60.8	22 OK Stat § 60.8	N	N
Oregon	OR Rev. Stat. § 166.255, 135.230.	OR Rev. Stat. § 166.255, 135.230.	Y; 2019	Y; 2019
Pennsylvania	23 PA Cons. Stat. Ann. § 6102; 18 PA Cons. Stat. Ann. § 6105(c)(6)	18 Pa. Cons. Stat. Ann. § 6105(c)(9)	Y	Y; 2022
Rhode Island	R.I. Gen. Laws §§ 11-47-5(a)(4)(iii); 12-29-5, 15-15-3	R.I. Gen. Laws §§ 11-47-5(a)(4), 12-29-5	Y; 2017	Y; 2017
South Carolina	SC Code § 16-25-10; SC Code § 16-25-30	SC Code § 16-25-10; SC Code § 16-25-30	N	N
South Dakota	SD Codified L § 25-10-3.1; SD Codified L § 25-10-5	SD Codified L § 22-14-15.2	N	Y; 2014
Tennessee	TN Code § 39-17-1316(a)(1)	TN Code § 39-17-1307	N	Y; 2022
Texas	Tex. Pen. Code § 25.07(a)	Tex. Fam. Code Ann. § 71.001[46] ; Tex. Penal Code Ann. §§ 22.01, 46.04(b)	Y	N
Utah	UT Code § 76-10-503-1(b)(xi); UT Code § 78B-7-603; UT Code § 78B-7-102 (2g)	UT Code § 76-10-503; UT Code § 76-10-501(7) - 76-10-503(Subsection(1)(d))[47]	N	N
Vermont	13 V.S.A. § 4053; 13 V.S.A. § 4017a	13 V.S.A. § 4017; 5301(7); 13 V.S.A. § 1042	Y; 2023	Y
Virginia	VA Code § 18.2-308.1:4	Va. Code Ann. § 18.2-308.1:8	Y	Y

Continued on next page

[43] Does not include misdemeanor crimes of domestic violence (MCDV) in its firearm prohibitions.

[44] Not applicable to same-sex relationships.

[45] No specific laws preventing those with protective orders from possessing/purchasing firearms, however, the law does allow a judge to grant any relief that s/he believes is "equitable and fair" to protect the protected party. No specific laws preventing those with MCDV's purchasing/possessing firearms.

[46] Texas prohibits firearm possession by domestic violence misdemeanants for five years following release from confinement or community supervision.

[47] Must be one than one misdemeanor conviction

Table A1: Relevant statutes and closure of the boyfriend loophole by state (continued)

State	Protective Order (PO)	Misdemeanor (MCDV)	Closure (PO)	Closure (MCDV)
Washington	WA Rev Code §§ 9.41.010(5); 10.99.020(3); and Wash. Rev. Code Ann. § 26.50.010(6).; WA Rev Code § 7.105.010[48]	WA Rev Code § 9.41.040 (2)(a)[49]	Y; 2020	Y; 2020
West Virginia	W. Va. Code § 61-7-7(a)(7)	W. Va. Code § 61-7-7(a)(8)	Y	Y
Wisconsin	1. Wis. Stat. § 813.12(1)(am)		Y	N
Wyoming	Other injunctive relief available but WY does not have specific gun restrictions in place	Wyo. Stat. § 6-8-102, Wyo. Stat. § 6-8-404 - Firearms Freedom Act	N	N

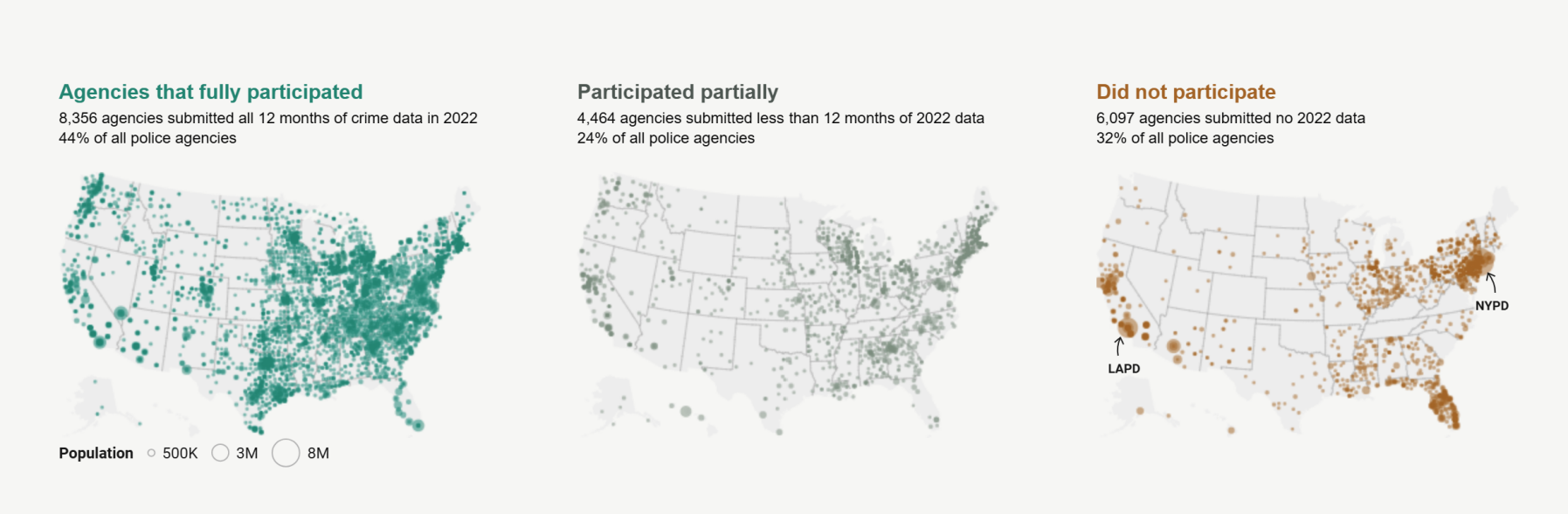
Table A2: NIBRS Crimes included in VAW

Types of offenses
Aggravated assault, Simple assault, Intimidation, Murder/non-negligent manslaughter, Negligent manslaughter, Justifiable homicide, Kidnapping/abduction, Human trafficking, Commercial sex acts, Sexual assault with an object, Forcible fondling, Statutory rape, Human trafficking–involuntary servitude, Pornography/obscene material, Prostitution, Assisting or promoting prostitution, Purchasing prostitution, Forcible rape, Forcible sodomy

[48] See note following RCW 7.105.900.

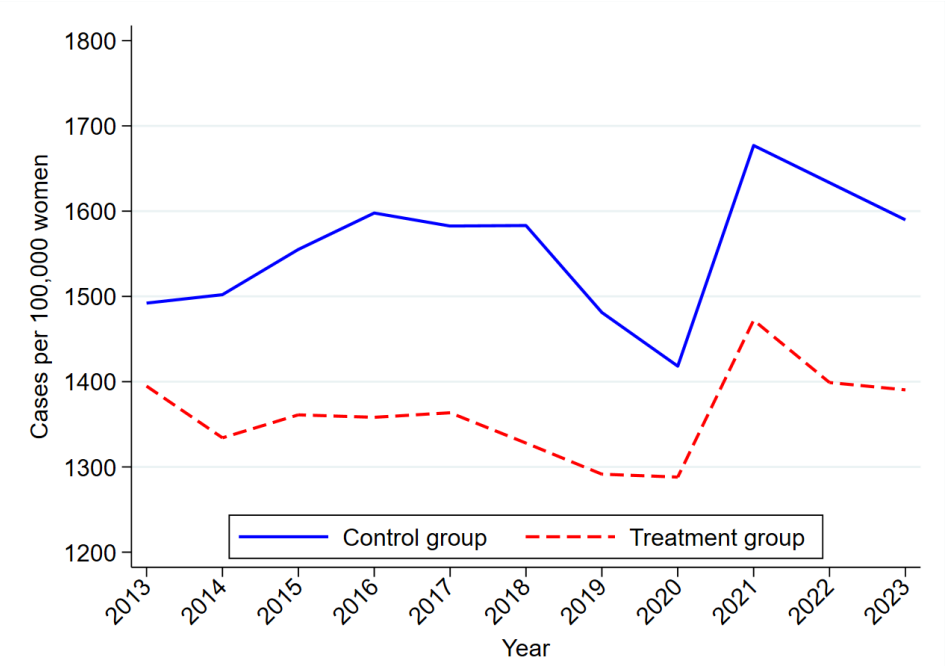
[49] Edited to include dating partners in Substitute House Bill 2473

Figure A1: NIBRS agency participation in 2022



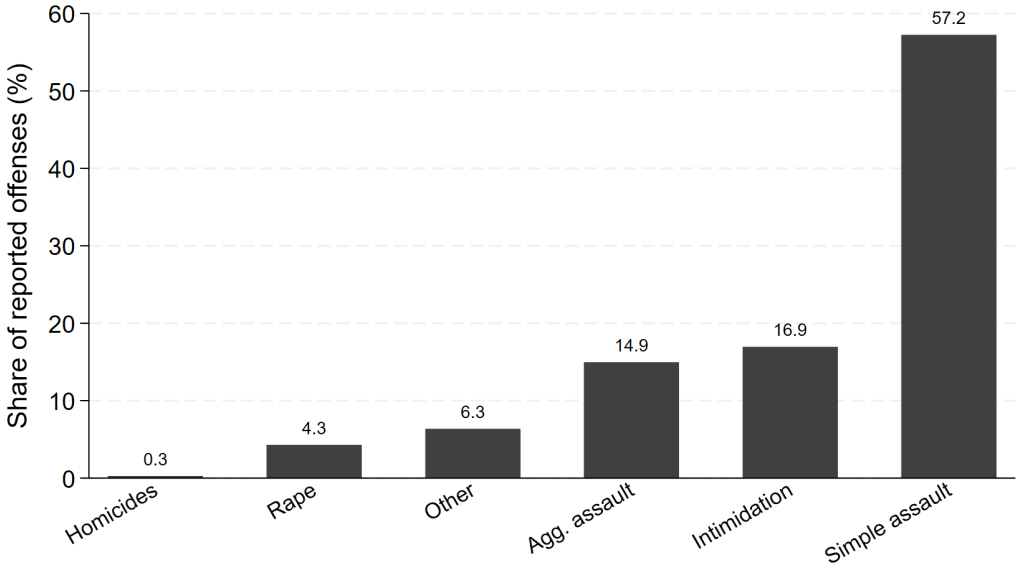
Source: The Marshall Project (Li and Ricard, 2023). Notes: Agency participation data compiled by the Federal Bureau of Investigation on Feb. 6, 2023, which was the deadline for local agencies to submit crime data for the Q4 2022 quarterly report. Local agencies had until April 3, 2023 to submit data for the FBI's 2022 national crime report, so the final participation status may change. Map only shows law enforcement agencies that police more than 5,000 people.

Figure A2: Average time trends in violence against women, 2013-2023



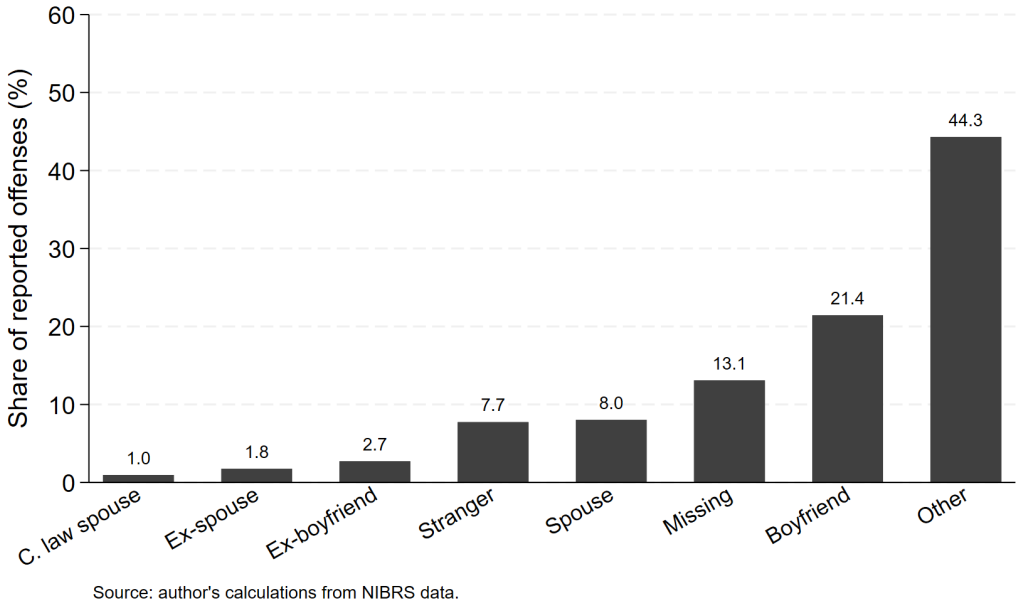
Notes: Average time trends in gender violence for the subsample of treated and untreated counties. Treated counties are the ones which experienced a closure in the loophole during the study period, and untreated counties are those which have the loophole completely open during the entire study period.

Figure A3: Distribution of offenses (violence against women), 2013-2023



Notes: Other comprises all other crimes not mentioned here, available in Table A2; homicides include justifiable homicide and manslaughter; rape includes statutory rape.

Figure A4: Distribution of main perpetrator's relationship to victim, 2013-2023

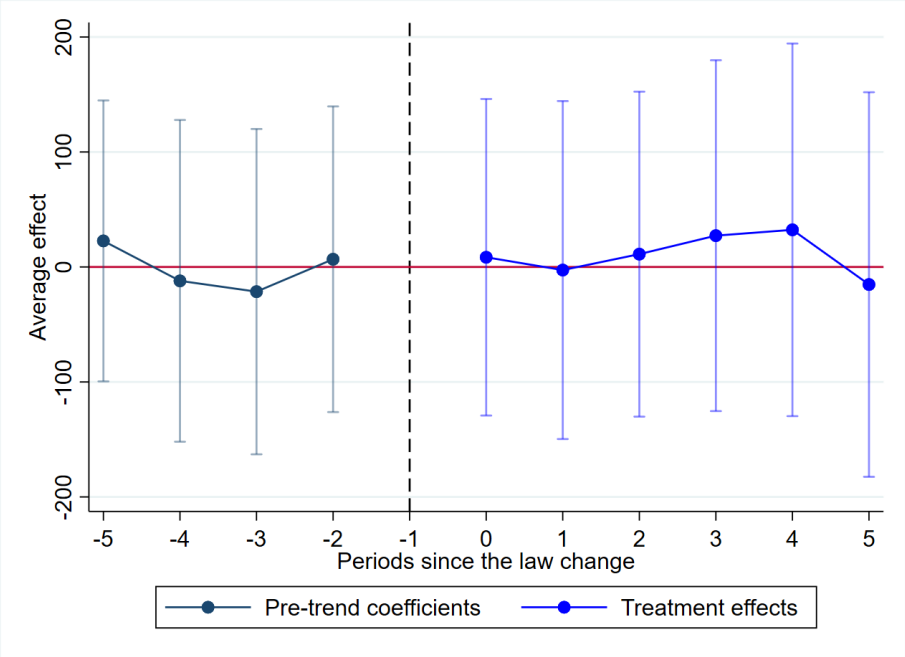


Notes: Other comprises all other relationship types, not including those listed. Categories include but are not limited to: neighbor, acquaintance, step-family members, employee, employer.

B. Baseline and Event-Study Results

B.1 Full sample (2013–2023)

Figure A5: Effect of loophole closure on violence against women (2013–2023)



Notes: All specifications include state-level fixed effects, and county-level covariates (unemployment rate, and share of Black and Hispanic females per county). Bands represent 95% confidence intervals. Source: NIBRS 2013–2023.

Table A3: Effect of loophole closure on VAW

	(1)	(2)	(3)	(4)
Pre_avg	-2.416	0.431	-5.041	-2.396
	(51.537)	(51.121)	(54.644)	(54.091)
ATT	10.210	12.734	17.616	18.382
	(57.948)	(57.357)	(58.664)	(58.138)
Controls	Yes	Yes	Yes	Yes
Age bucket controls			Yes	Yes
Not yet treated		Yes		Yes
Pre-Treatment Mean	354.879	354.879	354.879	354.879
Observations	38,321	38,321	38,257	38,257

Notes: Each cell reports the simple average of the event-time effects over $e \in [0, 5]$. Pre_avg averages $ATT(e)$ for $e < 0$. All models include state fixed effects and county covariates (unemployment rate; shares of Black and Hispanic females). Columns (3)–(4) additionally include age-bucket controls (shares of females aged 15–44, 45–64, 65+). Wild bootstrap SEs (499 reps) clustered at the state level. ***p<0.01, **p<0.05, *p<0.1.

B.2 Reduced sample (2021–2023)

Table A4: Effect of loophole closure on VAW (2021–2023)

	(1)	(2)	(3)	(4)	(5)	(6)
Pre_avg	54.990	60.585*	56.133*	41.915	46.080	44.049
	(33.627)	(34.350)	(33.088)	(36.163)	(36.618)	(35.659)
ATT	-91.019***	-100.163***	-89.641***	-90.024**	-98.514***	-89.284**
	(35.294)	(35.391)	(34.791)	(36.131)	(36.224)	(35.775)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Age bucket controls				Yes	Yes	Yes
Firearms suicide rate		Yes			Yes	
Not yet treated			Yes			Yes
Pre-Treatment Mean	310.935	310.935	310.935	310.935	310.935	310.935
Observations	12,871	12,871	12,935	12,871	12,871	12,871

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state-level fixed effects and county-level covariates: unemployment rate and shares of Black and Hispanic females per county. Wild bootstrap standard errors (499 reps) clustered at the state level, in parentheses. ***p<0.01, **p<0.05, *p<0.1.

C. Heterogeneity analysis

C.1 Effects on IPV vs. non-IPV VAW

Table A5: Effect of loophole closure on IPV and non-IPV VAW (PO estimates)

	(1) VAW baseline	(2) IPV	(3) non-IPV VAW
Pre_avg	-6.494	4.355	-11.079
	(13.043)	(5.340)	(10.497)
ATT	-39.222**	-12.807**	-28.857**
	(16.023)	(6.462)	(11.266)
Controls	Yes	Yes	Yes
Pre-Treatment Mean	298.374	114.720	187.648
Observations	25,375	24,609	25,148

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state-level fixed effects and county-level covariates: unemployment rate, and shares of Black and Hispanic females per county. Wild bootstrap standard errors (499 reps) clustered at the state level, in parentheses. ***p<0.01, **p<0.05, *p<0.1.

Table A6: Effect of loophole closure on IPV and non-IPV VAW (2021–2023)

	(1) IPV VAW	(2) Non-IPV VAW
Pre_avg	34.783**	20.287
	(15.035)	(20.503)
ATT	-36.285**	-54.693**
	(14.706)	(22.616)
Controls	Yes	Yes
Pre-Treatment Mean	124.757	189.993
Observations	12,432	12,788

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state-level fixed effects and county-level covariates: unemployment rate, and shares of Black and Hispanic females per county. Wild bootstrap standard errors (499 reps) clustered at the state level, in parentheses. ***p<0.01, **p<0.05, *p<0.1.

C.2 Effects on Severe Crimes

Table A7: Effect of loophole closure on **severe** VAW: Protective orders

	(1)	(2)	(3)
Pre_avg	1.440	2.247	1.885
	(5.077)	(5.081)	(4.897)
ATT	-18.953***	-17.907***	-18.091***
	(5.866)	(5.555)	(5.609)
Pre-Treatment Mean	56.768	56.768	56.768
Observations	24,137	24,137	24,186
Controls	Yes	Yes	Yes
Firearms suicide rate		Yes	
Not yet treated			Yes

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Wild bootstrap SEs (499 reps) clustered at the state level. ***p<0.01, **p<0.05, *p<0.1.

Table A8: Effect of loophole closure on **severe** VAW (2021–2023)

	(1)	(2)	(3)
Pre_avg	5.326	6.592	6.447
	(9.967)	(10.099)	(9.569)
ATT	-12.377	-14.029*	-11.772
	(7.795)	(7.758)	(7.587)
Controls	Yes	Yes	Yes
Firearms suicide rate		Yes	
Not yet treated			Yes
Pre-Treatment Mean	66.042	66.042	66.042
Observations	12,010	12,010	12,084

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Wild bootstrap SEs (499 reps) clustered at the state level. ***p<0.01, **p<0.05, *p<0.1.

Table A9: Effect of loophole closure on **severe** IPV and non-IPV VAW (2021–2023)

	(1) VAW	(2) IPV	(3) non-IPV VAW
Pre_avg	5.326	5.621	-1.427
	(9.967)	(3.480)	(7.853)
ATT	-12.377	-5.113*	-7.742
	(7.795)	(2.996)	(6.403)
Controls	Yes	Yes	Yes
Pre-Treatment Mean	66.042	24.995	46.614
Observations	12,010	10,063	11,309

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Wild bootstrap SEs (499 reps) clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

C.3 Race and Age Heterogeneity (Reduced Sample)

Table A10: Effect of loophole closure on VAW by race/ethnicity (2021–2023)

	(1)	(2)	(3)
Non-Hispanic Black			
Pre_avg	135.564	81.143	112.017
	(84.276)	(85.627)	(84.514)
ATT	-135.167*	-235.753***	-135.979**
	(70.076)	(84.549)	(68.183)
Pre-Treatment Mean	934.776	934.776	934.776
Observations	7,056	7,056	7,146
Non-Hispanic White			
Pre_avg	30.589	30.857	29.652
	(20.099)	(20.569)	(19.621)
ATT	-41.504**	-44.580**	-41.051**
	(20.268)	(20.305)	(19.862)
Pre-Treatment Mean	244.877	244.877	244.877
Observations	10,834	10,834	10,904
Hispanic			
Pre_avg	56.115	58.312	60.067
	(46.765)	(47.833)	(45.477)
ATT	-41.830	-45.015	-42.071
	(49.452)	(49.325)	(48.507)
Pre-Treatment Mean	480.552	480.552	480.552
Observations	6,185	6,185	6,271
Controls	Yes	Yes	Yes
Firearms suicide rate		Yes	
Not yet treated			Yes

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Wild bootstrap SEs (499 reps) clustered at the state level. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table A11: Effect of loophole closure on VAW by age group (2021–2023)

	(1)	(2)	(3)
Over 30			
Pre_avg	34.644	39.004	36.591
	(28.383)	(28.642)	(28.021)
ATT	-73.524***	-82.199***	-72.821***
	(27.130)	(27.304)	(26.884)
Pre-Treatment Mean	213.417	213.417	213.417
Observations	12,758	12,758	12,822
Under 30			
Pre_avg	62.637	64.534	65.801
	(67.536)	(68.609)	(66.516)
ATT	-148.279**	-156.748**	-147.207**
	(69.943)	(69.922)	(69.181)
Pre-Treatment Mean	678.253	678.253	678.253
Observations	12,651	12,651	12,720
Controls	Yes	Yes	Yes
Age bucket controls	Yes	Yes	Yes
Firearms suicide rate		Yes	
Not yet treated			Yes

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Wild bootstrap SEs (499 reps) clustered at the state level. ***p<0.01, **p<0.05, *p<0.1.

D. Robustness

Table A12: Effect of loophole closure on VAW (2021–2023, balanced)

	(1)	(2)	(3)
Pre_avg	57.616	64.551*	57.190
	(35.159)	(35.929)	(34.879)
ATT	-94.898***	-103.662***	-94.352***
	(36.237)	(36.355)	(36.014)
Controls	Yes	Yes	Yes
Firearms suicide rate		Yes	
Not yet treated			Yes
Pre-Treatment Mean	327.3704	327.3704	327.3704
Observations	12,372	12,372	12,372

Notes: Balanced panel restriction. Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All models include state fixed effects and county covariates (unemployment rate, share of Black and Hispanic females). Wild bootstrap SEs (499 reps) clustered at the state level. ***p<0.01, **p<0.05, *p<0.1.

Table A13: Falsification tests: other crime outcomes (2021–2023)

	Larceny	Robbery	Burglary	Property
Pre_avg	80.079*	9.094*	-9.920	-15.020
	(42.738)	(5.174)	(13.002)	(22.950)
ATT	-1.864	-0.447	2.387	7.898
	(16.811)	(2.374)	(4.255)	(9.380)
Controls	Yes	Yes	Yes	Yes
Pre-Treatment Mean	229.642	17.067	73.445	110.956
Observations	12,438	3,891	11,487	11,630

Notes: Each cell presents the simple average of the event-time effects over event periods 0 through 5 (inclusive). Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Wild bootstrap SEs (499 reps) clustered at the state level. ***p<0.01, **p<0.05, *p<0.1.

Table A14: NIBRS Crimes included in Larceny

Types of offenses
Pocket-picking, Purse-snatching, Shoplifting, Theft From Building, Theft From Coin-Operated Machine or Device, Theft From Motor Vehicle, Theft of Motor Vehicle Parts/Accessories, All Other Larceny

Table A15: Robustness checks: VAW outcomes (2021–2023)

	(1) VAW baseline	(2) + COVID	(3) + COVID + Age	(4) Alt. IPV	(5) Alt. IPV + Age	(6) Post (0–3)	(7) Post (0–6)
Pre_avg	54.990	54.990*	41.915	53.786	42.768	54.990	54.990
	(33.627)	(30.695)	(64.957)	(32.640)	(35.182)	(33.627)	(33.627)
ATT (Post_avg)	-91.019***	-91.019***	-90.023**	-89.566***	-87.944**	-84.797**	-81.954**
	(35.294)	(34.782)	(43.272)	(34.340)	(35.179)	(35.117)	(35.795)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age bucket controls	No	No	Yes	No	Yes	No	No
Post-treatment aggregation	0–5	0–5	0–5	0–5	0–5	0–3	0–6
COVID mortality rate	No	Yes	Yes	No	No	No	No
Alternate IPV definition	No	No	No	Yes	Yes	No	No
Observations	12,871	12,871	12,871	12,851	12,851	12,871	12,871

Notes: Each cell presents the simple average of the event-time effects over the post-treatment window indicated in the row "Post-treatment aggregation". Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Age bucket controls are the percentages of county female population between ages 15 and 44, 45 and 64, and 65 plus. Wild bootstrap SEs (499 reps) clustered at the state level. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table A16: Effect of loophole closure on VAW (2021–2023), with standard error adjustment

	(1)	(2)	(3)	(4)	(5)	(6)
Pre_avg	54.990*	60.585	56.133*	41.915	46.080	44.049
	(31.384)	(42.804)	(30.817)	(32.062)	(40.043)	(31.385)
ATT	-91.019***	-100.163***	-89.641***	-90.023***	-98.514***	-89.284***
	(29.418)	(32.293)	(29.093)	(27.940)	(30.746)	(27.955)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Age bucket controls				Yes	Yes	Yes
Firearms suicide rate		Yes			Yes	
Not yet treated			Yes			Yes
Observations	12,871	12,871	12,935	12,871	12,871	12,871

Notes: Each cell presents the simple average of the event-time effects over the post-treatment window indicated in the row "Post-treatment aggregation". Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Age bucket controls are the percentages of county female population between ages 15 and 44, 45 and 64, and 65 plus. Robust asymptotic normal standard errors clustered at the state level. ***p<0.01, **p<0.05, *p<0.1.

Table A17: Effect of loophole closure on VAW (2021–2023), including agency-count control

	(1)	(2)	(3)	(4)	(5)	(6)
Pre_avg	48.390	53.092	50.230	33.130	35.942	35.591
	(33.312)	(34.101)	(32.782)	(35.894)	(36.377)	(35.374)
ATT	-95.913***	-105.537***	-93.735***	-95.503***	-104.632***	-94.081***
	(36.204)	(36.235)	(36.638)	(36.793)	(36.829)	(36.403)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Age bucket controls				Yes	Yes	Yes
Firearms suicide rate		Yes			Yes	
Not yet treated			Yes			Yes
Observations	12,871	12,871	12,935	12,871	12,871	12,871

Notes: Each cell presents the simple average of the event-time effects over the post-treatment window indicated in the row "Post-treatment aggregation". Pre_avg is the average of $ATT(e)$ for $e < 0$. All specifications include state fixed effects and county-level covariates (unemployment rate; shares of Black and Hispanic females). Age bucket controls are the percentages of county female population between ages 15 and 44, 45 and 64, and 65 plus. Wild bootstrap SEs (499 reps) clustered at the state level. ***p<0.01, **p<0.05, *p<0.1.

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