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Abstract

Non-fatal strangulation (NFS) is a common and dangerous form of intimate partner violence (IPV) and a predictor of homicide, yet it was historically neglected by the criminal justice system. Since the year 2000, most U.S. states have enacted laws enlisting NFS as a standalone criminal offense. We compile a novel dataset on state NFS statutes and link it to the FBI Supplementary Homicide Reports from 1990 to 2019 to estimate the causal effects of these laws on intimate partner homicide rates. Using a difference-in-differences strategy, and an estimator that accounts for staggered adoption and treatment heterogeneity, we find that NFS laws led to significant reductions in intimate partner homicides (IPH). We estimate that these laws reduce female-victim IPH by 14% and male-victim IPH by 36%, among victims 18-34. No significant effects are observed for victims 50 and above or for homicides committed by strangers. Event-study estimates support the parallel trends assumption. Our findings suggest that NFS laws can effectively disrupt the escalation of IPV and reduce lethal outcomes.

Keywords: Intimate Partner Violence; Non-Fatal Strangulation; Homicide; Difference-in-Differences; Criminal Justice Policy.

JEL codes: C21; I18; J1; J16; K14; K42.

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"If you prosecute a strangler, you can prevent a homicide."
— Casey Gwinn, former City Attorney of San Diego

1 Introduction

Intimate partner violence (IPV) is a pervasive and devastating social problem (Adams-Prassl et al., 2023; Adams et al., 2024). In the United States, one-third of murdered women are killed by an intimate partner (Smith, 2022; Black et al., 2023). A particularly severe but historically overlooked form of IPV is non-fatal strangulation (NFS), a gendered form of abuse commonly inflicted by men. An estimated 68% of domestic violence victims have been strangled by their partners at some point (Stellpflug et al., 2022). Strangulation is a strong predictor of subsequent intimate partner homicide (IPH) and signals an escalation of violence and control within the relationship (Glass et al., 2008).

Despite the severity of this form of abuse (McGowan, 2024), U.S. state legislatures only began criminalizing NFS as a standalone offense in the year 2000. These statutes defined NFS, elevated its legal status, and aimed to increase recognition among law enforcement of its life-threatening nature (Alliance for Hope International, ND). By 2019, 47 states had adopted such laws; by 2025, all but one (South Carolina) had done so. Yet no systematic analysis has examined whether these laws reduce IPV outcomes.

We address this gap by making two contributions. First, we compile a comprehensive dataset documenting the timing of NFS statute adoption across U.S. states. Second, we merge this dataset with the FBI Supplementary Homicide Reports (1990–2019), which record the relationship between victim and perpetrator, to estimate the causal effect of NFS laws on intimate partner homicides of men and women.

Before these statutes, NFS was often misclassified as simple assault or not recorded at all by law enforcement, owing to a lack of visible injuries and limited awareness of its near-lethality. As Gael Strack, a leading U.S. expert on NFS, notes:

> "Most states treated strangulation about as seriously as if the victim was slapped in the face. The lack of physical evidence was causing the criminal justice system to treat many choking cases as minor incidents, when in fact these were the most lethal and violent cases in the system."

Our empirical strategy exploits the staggered adoption of NFS laws across states. Our preferred approach is the two-stage difference-in-differences (2SDID) estimator proposed by Gardner et al. (2025), which addresses concerns about bias arising from the interaction of staggered treatment timing and heterogeneous treatment effects. This imputation-based method has been effectively applied to other staggered policy reforms—for instance, to study the impact of prescription authority laws on naloxone dispensing rates (Smart et al., 2024).

We find that the introduction of NFS laws led to substantial reductions in intimate partner homicides (IPH) among young adults. In states that enacted NFS laws, male-victim IPH rates for individuals aged 18-34 declined by 36% (from 0.332 to 0.212 per 100,000), female-victim IPH rates in the same age group fell by 14% (from 1.205 to 1.032), and female-victim IPH rates for ages 35-49 decreased by 13% (from 1.241 to 1.076). These effects remain robust after adjusting for state-level baseline covariates interacted with linear time trends. Estimated effects for older age groups (50-70) are smaller and not statistically significant. Event-study results based on 2SDID estimates support the parallel trends assumption and reinforce the interpretation of our results as (overall) average treatment effects on the treated. As a falsification test, we find no evidence that NFS laws affected homicides committed by strangers.

How many lives do NFS laws save, and how? Our estimates suggest that these laws save up to 238 women's lives and 91 men's lives per year in the US. While we observe only reduced-form impacts on IPH, our findings are consistent with two behavioral channels: increased incapacitation of abusers, and reduced need for preemptive violence by victims. By enlisting non-fatal strangulation as a serious offense, these laws may increase the likelihood that abusive partners are incapacitated after victims reports NFS, thus preventing escalation to lethal violence. They may also reduce the need for victims to resort to lethal self-defense to protect themselves, especially in situations of repeated violent abuse (Aizer and Dal Bo, 2009). We provide a conceptual framework consistent with these behavioral responses.

Taken together, our findings indicate that NFS laws are an effective policy tool for preventing intimate partner homicides. More broadly, recognizing NFS as a serious crime may improve the safety and wellbeing of individuals trapped in violent relationships. By identifying a scalable legal intervention that addresses a common and highly predictive form of abuse (Stellpflug et al., 2022), we offer actionable guidance for policymakers seeking to reduce gender-based violence and its deadliest consequences (Bhalotra et al., 2025; Aizer, 2010). Globally, many jurisdictions still lack NFS-specific statutes. England and Wales introduced such a law only in 2022; Victoria, Australia followed in 2024; and in Scotland, legislation is under debate as of 2025. Many other countries (e.g., Singapore) have no provision at all.

Our analysis contributes to three strands of literature. First, it advances the growing body of evidence on how criminal justice interventions affect IPV. Second, it sheds new light on gendered patterns in violent crime and homicide. Third, it contributes to broader debates on gender inequality and relationship dynamics by showing that legislation targeting a gendered form of IPV, NFS, can reduce intimate partner homicides.

We complement evidence from Aizer and Dal Bo (2009) and Miller and Segal (2018), who show that criminal justice interventions can reduce IPV-related homicides. Aizer and Dal Bo (2009) show that no-drop prosecution policies in the U.S. significantly reduce male-victim IPH. Miller and Segal (2018) find that increasing the share of female police officers reduces both male- and female-victim IPH. We extend this literature by focusing on NFS, a previously overlooked IPV phenomenon.

Our findings align with research on how legal changes influence abusive relationship dynamics. Erten and Keskin (2022) examine the impact of compulsory schooling reforms on IPV in Turkey. The effects of stricter arrest policies for IPV in the US were revisited by Chin and Cunningham (2019). Dave et al. (2025) estimate that abortion restrictions increase IPV reports to law enforcement. Brassiolo (2016) shows that easing access to divorce reduces domestic violence. Similarly, Amaral et al. (2023) document both incapacitation and deterrence effects of domestic violence arrests, while Black et al. (2023) find that pressing charges reduces recidivism among abusers. In general, there remains limited empirical evidence on which policies may effectively reduce IPV, and our study contribute to fills this gap.

The paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 contains descriptive statistics. Section 6 reports the main results. Section 7 quantifies the lives saved and provides a conceptual framework for mechanisms. Section 8 concludes.

2 Institutional Background

This section explains what NFS is and describes the statutory evolution of NFS laws in the United States.[1] We also detail the construction of a new dataset on state-level NFS statutes, which underpins our empirical analysis.

2.1 Non-Fatal Strangulation

Non-fatal strangulation (NFS) is a near-lethal form of IPV that reflects both physical violence and coercive control. NFS involves intentional restriction of a victim's airway and/or blood circulation, typically using the attacker's hands. As little as 11 pounds of pressure on the carotid artery (or 4.4 pounds on the jugular vein) can induce unconsciousness in 6–8 seconds—less than the force needed to open a soda can (Strack and Gwinn, 2011). If strangulation is sustained, brain death can occur within 4–5 minutes. Even when pressure is quickly released, victims may suffer brain damage and other long-term injuries.

Unlike other forms of assault (e.g., stabbing), NFS frequently leaves no visible external injuries: as many as 50% of victims show no outward signs. However, internal injuries may be severe, including fractures of the larynx, or trachea; carotid tears or occlusions; blood clots; anoxic brain trauma; voice changes; and persistent cognitive and respiratory symptoms.

Historically, public and law enforcement perception of the dangers of NFS has been very limited. Victims often underreport these attacks, while police and prosecutors may consider them as minor incidents or miss them at all, due to the absence of visible injuries (McKay, 2014). Compared to stabbing, where even superficial wounds trigger serious charges, NFS cases often escaped legal scrutiny. This under-recognition left victims exposed to escalating IPV, and victims who experience NFS are more likely to later be killed by their intimate partner (Glass et al., 2008). As Strack (2011) notes:

> "When a victim is strangled, she is at the edge of a homicide."

2.2 NFS Statutory Classification in the United States

Strangulation statutes represent a relative recent development in criminal justice. The first major legal shift occurred in Missouri in 2000, when the state legislature passed a bill enlisting NFS as a standalone serious criminal offense and describing the act of NFS itself. Over the following two decades, nearly all states followed suit. By 2019, 47 U.S. states had enacted similar laws; by 2025, all but one state (South Carolina) had done so.

These statutes explicitly define NFS and elevate it to a serious offense—aggravated assault or serious bodily injury, typically a felony (or Class A misdemeanor). Explicit

[1] Our study focuses on NFS in the context of intimate partner violence (IPV), and does not address the recent emerging dating trend of "sexual choking."

recognition in statute gives NFS accountability and hold the offender accountable for this abuse, providing law enforcement with a clear tool to investigate and prosecute cases that had previously gone unnoticed or uncharged. Even severe NFS incidents used to be classified as simple assaults or not recorded at all, with deadly consequences (e.g., the case of Monica Weber-Jeter from Ohio).

Testimonies in state legislative hearings debating the NFS bill highlight the gap these statutes were designed to fill. For example, in the North Dakota's 2007 hearings on Bill SB2185, Dan Draovitch, a retired police chief, stated:

> "Please, on behalf of our law enforcement folks—please modify this law to specifically add strangulation, and strengthen our laws to better protect victims of domestic violence...."

And the State's Attorney Office stated:

> "Do you know how hard it is to explain to a victim of strangulation that the person who nearly ended their life could only be charged with simple assault because the victim only had a red mark on their neck and no other visible injury? Imagine having to explain to this person that the maximum penalty for this offense is only 30 days in jail. Does that seem like the punishment fits the crime?"

The Montana Coalition against Domestic and Sexual Violence's testimony to the Senate in 2017 debating the NFS bill SB153 clearly states:

> "Quite simply, SB153 will help to save lives"

By explicitly recognizing NFS, these statutes may have improved detection, incapacitation of abusers, and prosecution, disrupting the pathway from nonfatal strangulation to IPH. Ultimately, our goal is to investigate whether criminalizing non-fatal strangulation as a standalone offense reduces IPH and, in doing so, saves lives.

3 Data Description

3.1 NFS Laws Taxonomy: Treatment Variable

Despite the widespread adoption of NFS statutes, no systematic dataset exists documenting their passage and implementation across U.S. states. Prior literature identifies this as a key gap in IPV policy research (Pritchard et al., 2017).

We construct a new dataset following a two-step process. First, we manually review state legislative archives and proceedings. For each state in the U.S. until 2025, we identified the relevant bill introducing NFS as a standalone offense, verified its legislative passage history, and recorded both the date in which the law was signed by the Governor and the date in which the law became effective. Second, we verified these collected data with Legislative State Librarians at the Legislative Library or State Law Library of each US state.[2] In some cases, these bills were short and focused solely on NFS; in others, NFS provisions were embedded within broader legislation. Table 1 displays the year of effectiveness of NFS Laws and bill numbers by state.[3]

Our treatment variable is a binary indicator equal to one from the year the NFS law became effective in a state, and in all subsequent years. Figure 1 shows the staggered implementation of these statutes across states. Missouri was the first state to implement NFS legislation, followed by Nebraska, North Carolina, and Oregon in 2004. The most recent adopters by 2019 were New Mexico (2018) and Kentucky (2019). Three states—Maryland, Ohio, and Washington D.C.—had not adopted such statutes by 2019 and serve as "never-treated" states in our sample, which focuses on pre-pandemic years (1990–2019) to avoid COVID-related disruptions.[4] South Carolina remains the only state without an NFS law as of 2025, and it is not included in our sample.

Table 2 reports the distribution of treatment cohorts by year of implementation, showing both the percentage of treated states and the percentage of U.S. adult population (aged 18–70) covered by each cohort. As the table illustrates, the size of treatment cohorts varies.

[2] We are grateful to the Legislative State Librarians across the United States for their time and assistance in helping us verifying the statutory histories.

[3] A few states reference NFS only within statutes concerning child abuse and/or abuse of elderly or vulnerable adults; these cases are not included in this table or our analysis.

[4] The imputation approach we use requires untreated observations to identify both the state and year fixed effects. Since three states never adopted NFS laws by 2019, we have at least three states contributing to the identification of the year fixed effects. As it happens, this is the minimum number required in Smart et al. (2024), where the authors truncate the sample "to reduce the potential leverage of a single state driving estimates of the counterfactual" [p. 9].

Table 1: Year of Effectiveness of NFS Laws and Bill Numbers by State

State	Year Effective	Bill Number
Alabama	2011	HB512
Alaska	2005	HB219
Arizona	2010	SB1266
Arkansas	2009	HB1040
California	2012	SB430
Colorado	2016	HB1080
Connecticut	2007	SHB7313
Delaware	2010	SB197
Florida	2007	SB184
Georgia	2014	HB911
Hawaii	2006	HB3256
Idaho	2005	SB1062
Illinois	2009	HB0594
Indiana	2006	HB1281
Iowa	2012	SF93
Kansas	2017	SB112
Kentucky	2019	SB70
Louisiana	2007	HB519
Maine	2012	HP1381
Maryland	2020	SB212
Massachusetts	2014	SB2334
Michigan	2013	SB848
Minnesota	2005	HF1
Mississippi	2010	SB2923
Missouri	2000	HB1677
Montana	2017	SB153
Nebraska	2004	LB943
Nevada	2009	AB164
New Hampshire	2011	HB1634
New Jersey	2017	A2061
New Mexico	2018	SB0061
New York	2010	S06987
North Carolina	2004	H1354
North Dakota	2007	SB2185
Ohio	2023	SB288
Oklahoma	2004	HB2380
Oregon	2004	HB2770
Pennsylvania	2016	HB1581
Rhode Island	2012	HB7242
South Carolina	NA	NA
South Dakota	2012	SB156
Tennessee	2011	SB476
Texas	2009	HB2066
Utah	2017	HB0017
Vermont	2006	H856
Virginia	2012	HB752
Washington	2007	SB5953
West Virginia	2016	HB4362
Wisconsin	2008	SB260
Wyoming	2011	SF0132
District of Columbia	2023	B25-0395

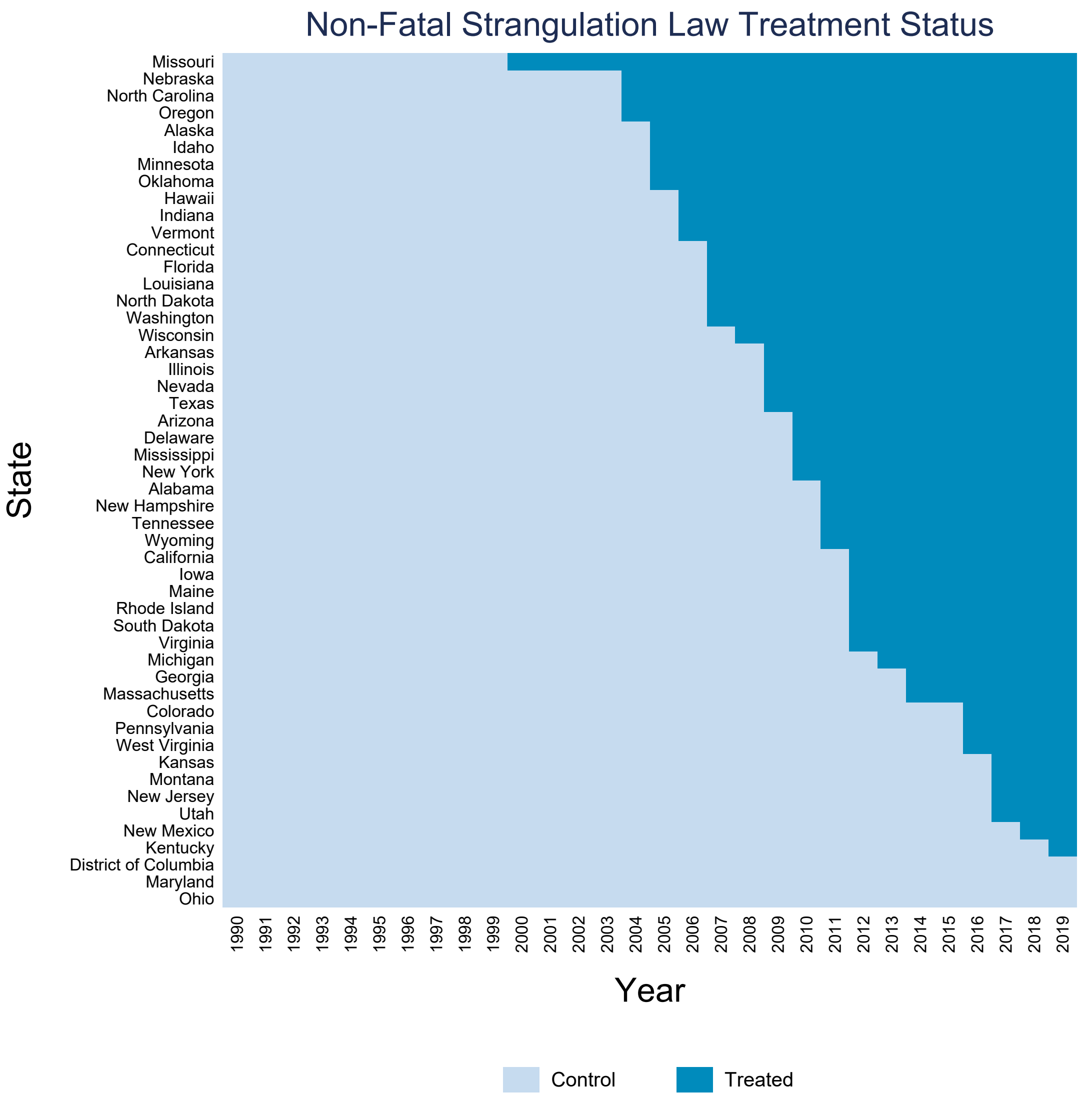


Figure 1: Staggered implementation of Non-Fatal Strangulation Laws

Table 2: Cohorts of treated and never treated states: 2000–2019

Treatment Cohort	States	States (number)	States (%)	Population (%)
2000 cohort	MO	1	2%	1.99%
2004 cohort	OR, NC, NE	3	6%	4.83%
2005 cohort	AK, ID, MN, OK	4	8%	3.67%
2006 cohort	HI, IN, VT	3	6%	2.85%
2007 cohort	CT, FL, LA, ND, WA	5	10%	10.89%
2008 cohort	WI	1	2%	1.92%
2009 cohort	AR, IL, NV, TX	4	8%	13.61%
2010 cohort	DE, MS, NY	3	6%	10.01%
2011 cohort	AL, AZ, NH, TN, WY	5	10%	4.33%
2012 cohort	CA, IA, ME, RI, SD, VA	6	12%	16.93%
2013 cohort	MI	1	2%	3.56%
2014 cohort	GA, MA	2	4%	5.34%
2016 cohort	CO, PA, WV	3	6%	6.65%
2017 cohort	KS, MT, NJ, UT	4	8%	5.07%
2018 cohort	NM	1	2%	0.64%
2019 cohort	KY	1	2%	1.48%
Never treated	DC, MD, OH	3	6%	6.22%
Total		**50**	**100%**	**100%**

Notes: Population (%) reports each cohort's share of the population aged 18–70 in 2000, across those 50 states.

3.2 Homicides Data: Outcome and Placebo variables

Our analysis combines newly collected legislation data on NFS statutes with homicide data from the Federal Bureau of Investigation. This subsection describes the homicide data used in our study and how it compares to other data sources. Appendix A1 provides further details on data construction and control variables.

Supplementary Homicide Reports of the Federal Bureau of Investigation. Our main outcomes of interest are female-victim and male-victim homicides committed by intimate partners. We obtain these data from the FBI's Supplementary Homicide Reports (SHR), part of the Uniform Crime Reporting (UCR) system, as explained by Fox and Swatt (2009).[5] The SHR is among the most comprehensive sources on homicides in the United States. It provides detailed information on the relationship between victim and offender, as well as the age, sex, and race of both parties, the weapon used, and the location of the incident. The unit of reporting in the SHR is the homicide incident. In our analysis, we focus on incidents involving a single victim and a single offender (the vast majority of homicides), to ensure accurate coding of victim-offender relationships, and focus on the victim file and the raw data. Following Chin and Cunningham (2019), we do not use the imputed values in this dataset.

We define intimate partner (IP) relationships as current spouse, ex-spouse, boyfriend or girlfriend, and common-law spouse. All other relationship categories are classified as non-IP. Same-sex relationships are omitted due to extremely small numbers in IP homicides. Our analysis is stratified by three victim age groups (18-34, 35-49, and 50-70). Our outcome variables are male-victim and female-victim IP homicide (IPH) rates (per 100,000 population) in each age group. We also present a complementary analysis using homicide counts.

The dataset includes 50 states (including the District of Columbia) over 30 years (1990–2019), yielding 1,500 potential state-year observations. Twenty-one state-years lack complete information on IP homicides, resulting in a final sample of 1,479 observations.

Comparison with other homicide data sources. Two main sources provide homicide data in the United States: the SHR and the National Incident-Based Reporting System (NIBRS). While NIBRS includes more detailed data on crime contexts, its coverage over the period of analysis is limited. In the early 1990s, only nine states reported under NIBRS developmental standards (Chilton and Jarvis, 1999). By 2013, 33 states were certified to report NIBRS statistics (Federal Bureau of Investigation, 2013), but even in 2018, NIBRS covered only 30% of the U.S. population and captured 28% of UCR-reported crimes (Fegadel and Heide, 2018). As of 2020, only about 49% of U.S. law

[5] We requested the data from Fox, who generously sent us directly the 1976-2020 version in 2023.

enforcement agencies were reporting to NIBRS, whereas the SHR receives reports from nearly all agencies nationwide. The SHR compiles monthly submissions from local law enforcement agencies and provides a consistent time series of over 30 years. Although reporting is voluntary, submission rates are high (Fox and Swatt, 2009).

For these reasons, and consistent with previous research on homicide and IPV in economics and other fields (Pampel and Williams, 2000; Jennings and Piquero, 2008; Aizer and Dal Bo, 2009; Cunningham et al., 2017; Garrett et al., 2017; Chin and Cunningham, 2019; Miller and Segal, 2018), we use the SHR as our data source.

Placebo. Following Chin and Cunningham (2019), we use stranger homicides as a falsification test because they are among the most prevalent homicide types and there is no plausible mechanism linking them to the policy, as NFS laws specifically target escalating violence within intimate relationships. We use homicides committed by strangers, disaggregated by sex and age group of the victim, measured at the state-year level and expressed per 100,000 population in the same age ranges used for IPH. This falsification test helps validate our identification strategy, which we discuss in the next section.

4 Empirical Strategy

This section presents our identification strategy and regression specifications. All regressions are weighted by state population and use standard errors clustered at the state level.

4.1 Identification of Overall ATT estimates

TWFE via OLS estimation. We begin with a two-way fixed effects (TWFE) regression model estimated by ordinary least squares (OLS), as a natural starting point:

$$Y_{st} = \beta D_{st} + \alpha_s + \gamma_t + \varepsilon_{st} \tag{4.1}$$

where Y_{st} is the homicide rate (per 100,000 population) committed by intimate partners in state s and year t, computed separately for male-victim and female-victim homicides. D_{st} is a binary indicator that equals one in the year the NFS law becomes effective in state s and in all years thereafter. State fixed effects α_s absorb time-invariant characteristics of states; year fixed effects γ_t capture common time-varying national shocks. Standard errors are clustered at the state level.

If treatment effects are constant across states and over time, this OLS estimator will be consistent for β under the parallel trends assumption (Butts and Gardner, 2022; Roth et al., 2023). However, as shown by Goodman-Bacon (2021) and emphasized by Gardner et al. (2025), when treatment effects vary across groups and over time, the TWFE regression model becomes:

$$Y_{st} = \beta_{st} D_{st} + \theta_s + \tau_t + \varepsilon_{st}, \tag{4.2}$$

and a TWFE regression model estimated by OLS may yield inconsistent estimates of the (overall) average treatment effect on the treated (ATT), because the conditional expectation of outcomes is no longer linear in state, year, and treatment status.

TWFE via Two-Stage Estimation (2SDID). To address this concern, we employ the two-stage difference-in-differences (2SDID) estimator proposed by Gardner et al. (2025). The 2SDID procedure estimates state and year fixed effects from untreated observations ($D_{st} = 0$) in a first stage. In the second stage, the outcomes are residualized using these estimates, and the overall ATT is obtained by regressing the residualized outcomes on the treatment indicator D_{st}. This procedure yields a consistent estimate of $\mathbb{E}[\beta_{st} \mid D_{st} = 1]$, provided that the parallel trends assumption holds, treatment is not anticipated, and the model correctly specifies untreated potential outcomes.

Under this procedure, the observed mean outcome for treated observations, $\mathbb{E}[Y_{st}(1) \mid D_{st} = 1]$, is given by the average actual outcomes Y_{st} among treated observations ($D_{st} = 1$). The counterfactual mean, $\mathbb{E}[Y_{st}(0) \mid D_{st} = 1]$, is computed as the average of predicted outcomes $\widehat{Y}_{st}$ based on state and year fixed effects estimated from untreated observations ($D_{st} = 0$) among treated observations ($D_{st} = 1$). The overall ATT is therefore estimated as the sample counterpart of:

$$\mathbb{E}[\beta_{st} \mid D_{st} = 1] = \mathbb{E}[Y_{st}(1) \mid D_{st} = 1] - \mathbb{E}[Y_{st}(0) \mid D_{st} = 1].$$

The 2SDID approach is robust to small-sample concerns (especially with few observations per cohort), and delivers point estimates numerically equivalent to those of Borusyak et al. (2024), but with a GMM-based inference procedure that provides better finite-sample properties (Gardner et al., 2025). In practice, we implement the 2SDID estimator using the `did2s` Stata package developed by Butts (2021), which has also been used in previous research (e.g., Smart et al., 2024).[6]

4.2 Dynamic ATT estimates

We also estimate treatment effects relative to the year of treatment adoption. The dynamic TWFE regression takes the form:

$$Y_{st} = \sum_{k=L}^{-1} \beta_k D_{st}^k + \sum_{k=0}^{M} \beta_k D_{st}^k + \theta_s + \tau_t + \eta_{st}, \tag{4.3}$$

where D_{st}^k is an indicator equal to one if the observation corresponds to event time k, i.e., k years relative to the first year in which the NFS law became effective in state s, and zero otherwise. Pre-treatment periods (leads) are indexed by $k < 0$. Post-treatment periods (lags) are indexed by $k \geq 0$.

As shown by Gardner et al. (2025), the 2SDID method can be extended to estimate dynamic effects by including the event-time indicators D_{st}^k as treatment variables in the second stage. This approach yields unbiased estimates of the dynamic ATT profile under the same assumptions required for the static 2SDID estimator: parallel trends, limited anticipation, and correct specification of untreated potential outcomes.

Weighting and interpretation of estimated ATT. All regressions are weighted by state population, using population counts from the 2000 Census. Under the identifying assumptions for the ATT, our estimated ATT can therefore be interpreted as the average causal effect of NFS laws on intimate partner homicide rates among female or male in the relevant age group, in states that passed such laws. Since we estimate effects separately by age group, we apply weights that reflect the corresponding population in each group.[7]

[6] Butts and Gardner (2022) have also developed an R package.

[7] Specifically, the weight for each state-year observation equals the proportion of that state's population (in the given age group) relative to the total U.S. population for that group. The population weights of each treatment cohort by age groups are displayed in Table A2.

5 Descriptive Statistics

Pre-Treatment Outcome Trends: Eventually Treated vs. Never-Treated States. A key identifying assumption in our difference-in-differences design is that, absent treatment, outcomes in treated and control states would have followed parallel trends. While this assumption is ultimately untestable, we provide supporting evidence by examining pre-treatment trends in intimate partner homicides (IPH).

Table 3 reports changes in male- and female-victim IPH across age groups between 1990 and 1999 (the year before Missouri passed the first NFS law). The pre-treatment differences between never-treated and eventually treated states vary in sign and magnitude, with only one statistically significant difference. This pattern provides suggestive evidence of broadly similar pre-treatment trends across groups.

Table 3: Changes in IPH from 1990 to 1999: Eventually Treated vs Never-Treated

Variable	Eventually Treated	Never-Treated	Difference (SE)
Δ male-victim IPH 18–34	-0.49	-0.48	0.01 (0.31)
Δ female-victim IPH 18–34	-0.65	-0.97	-0.32 (0.84)
Δ male-victim IPH 35–49	-0.63	-1.47	-0.85 (0.39)**
Δ female-victim IPH 35–49	-0.32	0.05	0.37 (0.33)
Δ male-victim IPH 50–70	-0.24	-0.31	-0.07 (0.09)
Δ female-victim IPH 50–70	-0.10	0.03	0.13 (0.34)

Note: The difference is the estimated coefficient on a never-treated indicator from a regression of the change in IPH from 1990 to 1999. There are 47 observations (three states have missing information to compute the change), and regressions are weighted by the relevant cohort-age population share in 2000. Robust (HC3) standard errors in parentheses.
*p-value<0.1, **p-value<0.05, ***p-value<0.01.

Given the staggered timing of policy adoption, we will further investigate the plausibility of the parallel trends assumption using event-study estimates of dynamic treatment effects. We also evaluate the robustness of our estimates to the inclusion of baseline covariates (measured in 1990) interacted with linear time trends.[8]

Table A3 compares baseline characteristics between eventually treated and never-treated states. On average, the groups are broadly similar. Only one statistically significant difference emerges (poverty rate), while differences in income per capita, unemployment, and gender inequality (male-to-female unemployment ratio) are small and statistically insignificant.

[8] As shown by Gardner et al. (2025), the 2SDID procedure can easily accommodate the inclusion of control variables. In this case, the first stage involves estimating state fixed effects, year fixed effects, and the coefficients on control variables using only untreated observations $D_{st} = 0$. In the second stage, the outcomes are residualized using these estimates, and the overall ATT is obtained by regressing the residualized outcomes on the treatment indicator D_{st}.

Timing of NFS Law Adoption. We assess whether the timing of NFS law adoption can be considered as-good-as-random with respect to pre-treatment IPH trends. Specifically, we regress the change in IPH from 1990 to 1999 on the year of adoption for all states, including Maryland (2020), DC (2023) and Ohio (2023).

Table 4 contains the estimates from six regressions, showing that the year of adoption is not significantly correlated with changes in IPH between 1990 and 1999. In addition, Figure 2 plots changes in IPH against year of adoption for each gender and age group, using saturated year-of-adoption dummies to estimate conditional expectation functions. Across all subgroups, we observe no systematic relationship between pre-treatment IPH changes and the timing of NFS law adoption, providing further support for the assumption that adoption timing is as-good-as-random.

Table 4: Regressions of Change in IPH from 1990 to 1999 on Year of Adoption

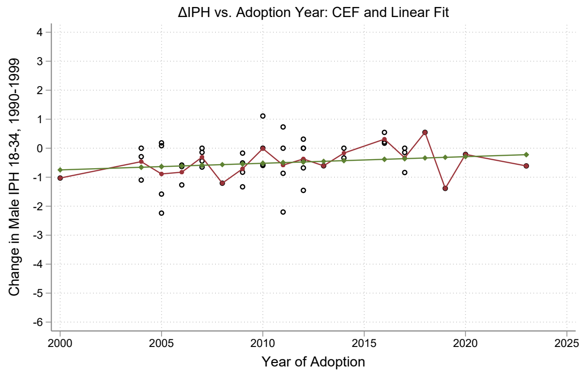
Dependent variable	Coefficient	R-squared
Δ male-victim IPH 18–34	0.023	0.043
	(0.019)	
Δ female-victim IPH 18–34	-0.001	0.000
	(0.025)	
Δ male-victim IPH 35–49	-0.004	0.001
	(0.034)	
Δ female-victim IPH 35–49	0.018	0.019
	(0.017)	
Δ male-victim IPH 50–70	0.012	0.011
	(0.016)	
Δ female-victim IPH 50–70	0.001	0.000
	(0.013)	

Note: Each cell reports the coefficient from a separate regression of the change in IPH from 1990 to 1999 on year of adoption, weighted by the relevant cohort-age population share in 2000. There are 47 observations. Robust HC3 standard errors in parentheses.
*p-value<0.1, **p-value<0.05, ***p-value<0.01.

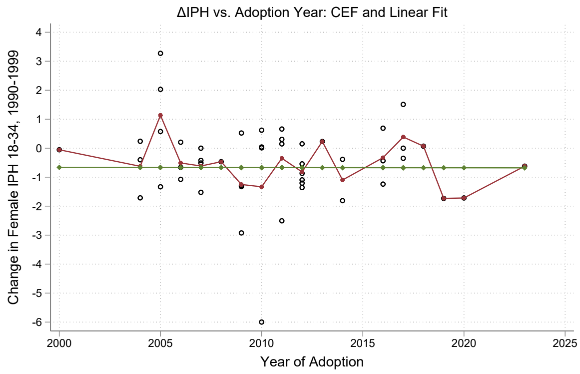
Regressing the change in covariates from 1990 to 1999 on the year of adoption reveals no significant relationship either as shown in Table A4 in the appendix. Moreover, the covariates in 1990 do not appear to be correlated with year of adoption (Table A5).

Figure 2: Change in IPH from 1990 to 1999 and Year of Adoption

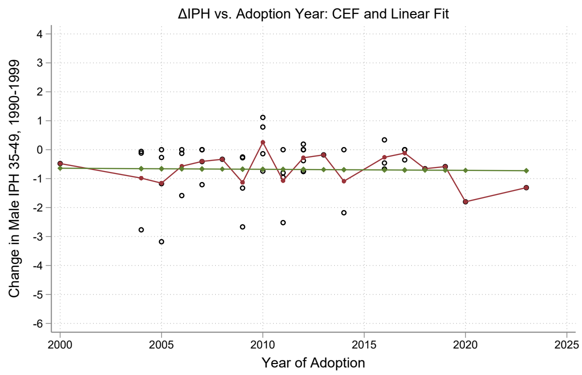
(a) Δ Male-victim IPH 18-34 & Year of Adoption



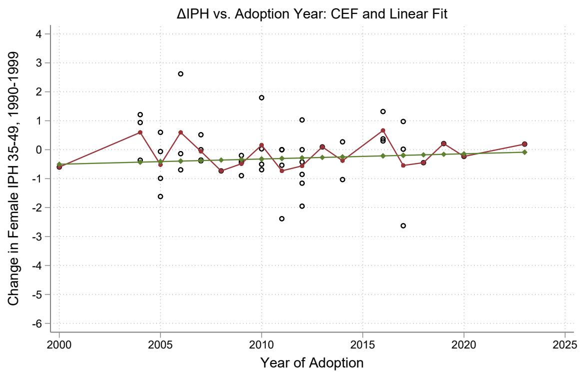
(b) Δ Female-victim 18-34 & Year of Adoption



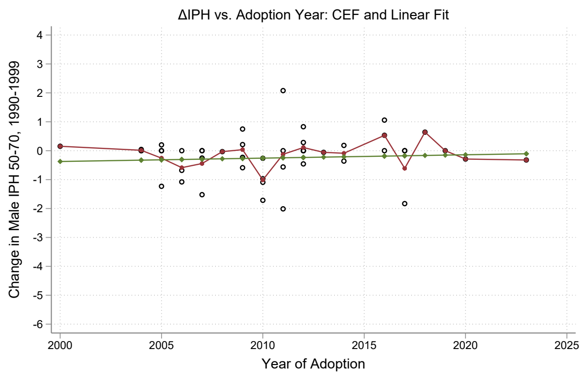
(c) Δ Male-victim 35-49 & Year of Adoption



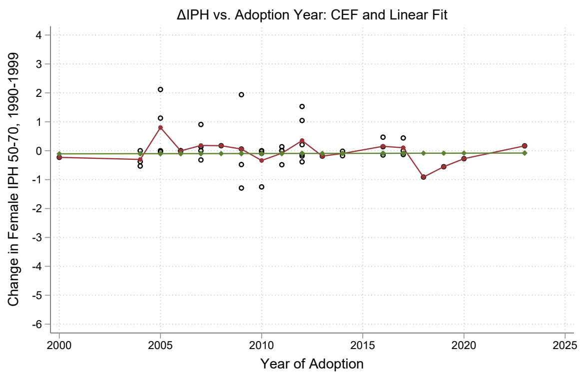
(d) Δ Female-victim 35-49 & Year of Adoption



(e) Δ Male-victim 50-70 & Year of Adoption



(f) Δ Female-victim 50-70 & Year of Adoption



Note: Green line = fitted regression line; Red line = estimated conditional expectation function.

6 Results

6.1 Overall ATT Estimates

Main specifications. Table 5 reports the estimated effects of non-fatal strangulation (NFS) laws on male and female intimate partner homicides (IPH), expressed per 100,000 population, by victim's age group. Columns 1 and 2 show results from OLS and two-stage difference-in-differences (2SDID) estimators; the latter is our preferred approach. Columns 3 and 4 present the observed mean IPH in 1999—the year before states began passing NFS laws—and the estimated counterfactual mean, i.e., the mean IPH that would have been observed in treated states had the laws not been enacted.[9]

Table 5: Effects of NFS Law on Male-victim and Female-victim IPH (per 100,000)

Dependent variable	OLS	2SDID	Mean in 1999	Counterfactual Mean
Male-victim homicides 18–34	-0.079**	-0.120***	0.307	0.332
	(0.035)	(0.035)		
Female-victim homicides 18–34	-0.132**	-0.173**	1.233	1.205
	(0.060)	(0.085)		
Male-victim homicides 35–49	-0.072	-0.054	0.402	0.344
	(0.044)	(0.056)		
Female-victim homicides 35–49	-0.072	-0.165**	1.145	1.241
	(0.066)	(0.077)		
Male-victim homicides 50–70	-0.014	-0.019	0.266	0.224
	(0.019)	(0.022)		
Female-victim homicides 50–70	-0.029	-0.026	0.480	0.511
	(0.028)	(0.036)		

Notes: All regressions include state and year fixed effects. $N = 1479$. Standard errors clustered at the state level (50 clusters), shown in parentheses.
*p<0.1, **p<0.05, ***p<0.01.

The results in Table 5 show that non-fatal strangulation (NFS) laws are associated with sizable reductions in intimate partner homicides (IPH), particularly among younger adults. Among individuals aged 18–34, male-victim IPH declines by 0.120 per 100,000 under the 2SDID specification (Column 2), a 36% reduction, from a counterfactual mean of 0.332 to 0.212 per 100,000 population. For female victims in the same age group, the

[9] As previously discussed, the counterfactual mean, $\mathbb{E}[Y_{st}(0) \mid D_{st} = 1]$, is estimated as the average of predicted IPH based on state and year fixed effects estimated from untreated observations ($D_{st} = 0$).

estimated reduction is 0.173, or 14% relative to the counterfactual mean (from 1.205 to 1.032).

For individuals aged 35-49, we find a statistically significant reduction in female-victim IPH (-0.165, from 1.241 to 1.076), amounting to a 13% decrease relative to the counterfactual mean. The estimated decline in male-victim IPH for this group is smaller (-0.054) and not statistically significant. For the 50-70 age group, estimated effects are close to zero for both genders and not statistically significant. Individuals under age 50 are generally more likely to experience IPV (Aizer and Dal Bo, 2009), with the highest levels of violence concentrated among younger adults.

Overall, the results suggest that NFS laws reduce IPH most strongly among younger populations, consistent with these laws disrupting the escalation of violence during the more active phases of abusive relationships. This pattern aligns with prior evidence on criminal justice interventions. Aizer and Dal Bo (2009) estimate a 15-22% decline in male-victim IPH among individuals aged 20-55 across 49 U.S. cities in the 1990s following the implementation of no-drop prosecution policies. Similarly, Miller and Segal (2018) find that a 6 percentage point increase in the share of female police officers leads to a 14% decline in female-victim IPH and a 22% decline in male-victim IPH among adults. Chin and Cunningham (2019) estimate a 43% reduction in spousal homicides associated with discretionary arrest laws enacted between the 1970s and 1990s.

Robustness checks. Table A6 reports estimates from Poisson and negative binomial models using homicide counts rather than rates. The results are qualitatively consistent with our OLS estimates. We further assess the robustness of our findings to differential state trends by including baseline covariates (measured in 1990) interacted with linear time trends (Bailey and Goodman-Bacon, 2015; Conti and Ginja, 2020; Mora-García et al., 2024), as shown in Table A7.[10] In Figure A1, we also show that the estimates are not driven by any particular state by re-estimating the models after dropping one state at a time. These additional estimates closely match those reported in Table 5, reinforcing our main finding: NFS laws lead to significant reductions in IPH among young adults.

[10]The covariates include measures of state-level resources (log of income per capita, unemployment rate, poverty rate) and gender inequality (male-to-female unemployment rate), in the spirit of Aizer (2010), constructed from the Current Population Survey (Flood et al., 2022), Census Bureau data on poverty (United States Census, 2023a), and St. Louis Fed data on income per capita (U.S. Bureau of Economic Analysis and Federal Reserve Bank of St. Louis, 2023). See the Appendix for further details.

6.2 Dynamic ATT Estimates

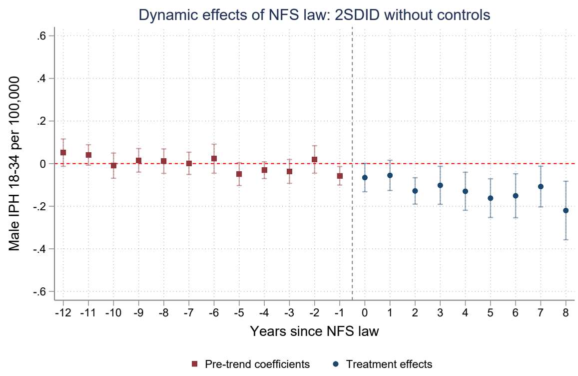
Figure A2 presents dynamic treatment effects estimated using the two-stage difference-in-differences (2SDID) approach, by victim gender and age group. The pre-treatment coefficients (shown in red squares) are close to zero for most of the pre-treatment years across all panels, providing additional support—consistent with the evidence reported earlier in Section 5—for the parallel trends assumption.

The post-treatment estimates (shown in blue dots) reveal substantial and sustained declines in IPH for both male and female victims aged 18-34, and female victims aged 35-49, in line with the main effects reported in Table 5. In contrast, treatment effects are smaller and statistically insignificant for male victims aged 35-49 and for both male and female victims aged 50–70. As with the overall ATT estimates (Table 5, Table A7), when controlling for baseline covariates (measured in 1990) interacted with linear time trends (Figure **??**), similar patterns of dynamic effects are found.

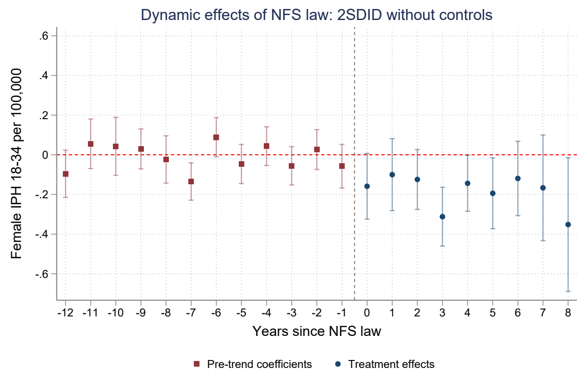
Overall, these results suggest that NFS laws contribute to sizable and sustained reductions in IPH among younger victims.

Figure 3: 2SDID Dynamic Effects of NFS Laws on Male-victim and Female-victim IPH (per 100,000)

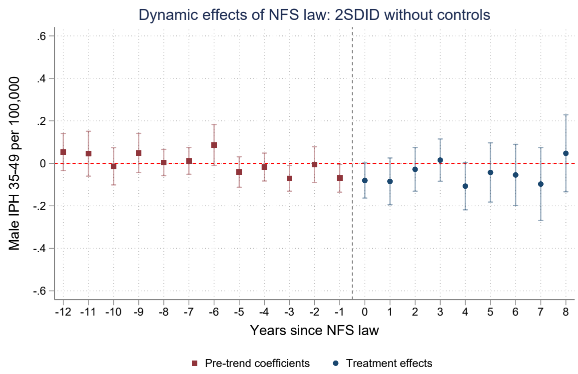
(a) Male-victim IPH 18-34



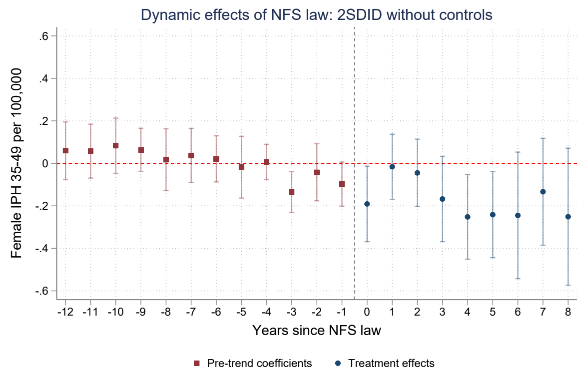
(b) Female-victim IPH 18-34



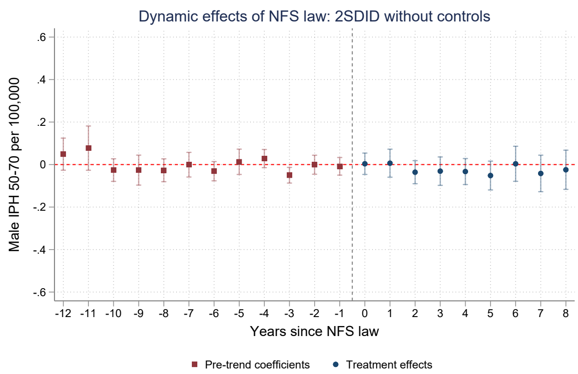
(c) Male-victim IPH 35-49



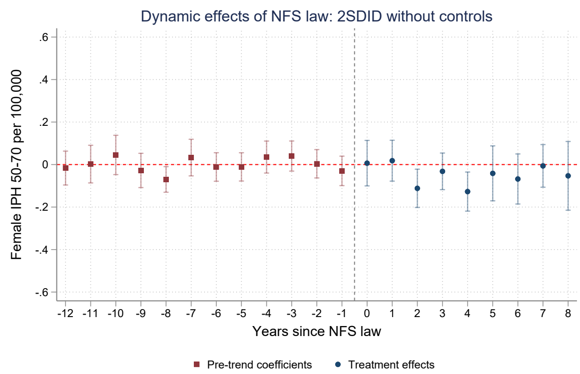
(d) Female-victim IPH 35-49



(e) Male-victim IPH 50-70



(f) Female-victim IPH 50-70



Note: The event study estimates are based on 2SDID estimates by including the event-time indicators D_{st}^{k} as treatment variables in the second stage. State and year fixed effects are estimated in the first stage for the sample of untreated observations ($D_{st} = 0$). Estimation is conducted simultaneously using the (GMM) framework in Gardner et al. (2025) and using the `did2s` *Stata package developed by Butts (2021).*

6.3 Falsification test: Homicides by Strangers

In the spirit of Chin and Cunningham (2019), we conduct a falsification test where we examine whether NFS laws have any impact on homicides committed by strangers, by victim gender and age group. Since NFS laws should specifically disrupt the lethal escalation of IPV, no effect should be observed on this placebo variable.

Table 6 reports the main placebo estimates, using OLS and 2SDID models and including baseline covariates (measured in 1990) interacted with linear time trends. Across all panels, estimated effects are small, statistically insignificant, and show no consistent pattern across gender or age group. Appendix Table A8 presents placebo estimates without covariate trends, and Figure A3 shows that the placebo estimates are not driven by any particular state by re-estimating the models after dropping one state at a time.

We further present event-study estimates for the placebo outcome. The event studies in Figure A4 show no evidence of systematic post-treatment effects, providing additional support for our identification strategy.

Taken together, these placebo results confirm that NFS laws do not affect broader homicide trends unrelated to intimate partner violence.

Table 6: Effects of NFS Law on Male-victim and Female-victim Homicides by Strangers (per 100,000) with baseline covariates interacted with linear time trends

Dependent variable	OLS	2SDID	Mean in 1999	Counterfactual Mean
Male-victim homicides 18–34	-0.028	0.009	1.600	1.120
	(0.191)	(0.302)		
Female-victim homicides 18–34	0.023	0.015	0.141	0.104
	(0.029)	(0.053)		
Male-victim homicides 35–49	-0.021	-0.020	0.630	0.631
	(0.089)	(0.121)		
Female-victim homicides 35–49	0.018	0.004	0.114	0.080
	(0.016)	(0.021)		
Male-victim homicides 50–70	-0.000	-0.029	0.273	0.333
	(0.034)	(0.057)		
Female-victim homicides 50–70	0.001	0.008	0.073	0.044
	(0.013)	(0.018)		

Notes: All regressions include state and year fixed effects, and baseline (1990) covariates (log income per capita, unemployment rate, poverty rate, and male-to-female unemployment ratio) interacted with linear time trends. $N = 1479$. Clustered standard errors (50 clusters). *p<0.1, **p<0.05, ***p<0.01.

7 How many lives do NFS laws save and how?

In this section, we first estimate the number of lives saved using our overall ATT estimates. We then present a simple conceptual framework to illustrate two potential mechanisms through which NFS laws may reduce IPH.

7.1 Translating ATT Estimates into Lives Saved

To assess the practical significance of our estimated overall ATTs, we transform these estimates into approximate numbers of lives saved. Our overall ATT estimates are expressed in terms of IPH rates per 100,000 population. To translate these effects into number of individuals, we multiply each estimated overall ATT by the estimated population size in the relevant age group, using data from the U.S. Census Bureau.[11]

For male victims aged 18-34, we estimate an overall ATT of −0.120 IPH per 100,000. Applying this rate to the corresponding U.S. population (75,787,105) yields an estimated reduction of approximately 91 homicides:

$$-0.120 \times \frac{75,787,105}{100,000} \approx -91.$$

For female victims, we estimate an overall ATT of −0.173 for the 18-34 age group and −0.165 for the 35-49 age group. Applying these estimates to their respective populations (also from Census data: 75,787,105 for ages 18-34; 64,635,814 for ages 35-49), we estimate reductions of approximately 131 and 107 homicides, respectively:

$$-0.173 \times \frac{75,787,105}{100,000} \approx -131, \qquad -0.165 \times \frac{64,635,814}{100,000} \approx -107.$$

Together, these estimates imply that NFS laws may save roughly 238 women and 91 men per year in the U.S.

7.2 How do NFS laws save lives?

We develop a simple conceptual framework to highlight the key mechanisms through which NFS laws may affect IPH. In the model, an exogenous fraction of men—*stranglers*—engage in nonfatal strangulation, which as previously discussed is a particularly severe form of domestic violence that sharply elevates the risk of subsequent homicide. Women partnered with stranglers face a life-threatening choice: they may respond to strangulation by reporting their partners to the authorities, by doing nothing, or by preemptively killing their abuser. Crucially, only women paired with stranglers face these choices, as only stranglers pose a direct fatal threat in this setting.

[11] Source: `https://data.census.gov/table?q=Population+by+age`. Last accessed: 26 June 2025.

Absent the law, reporting is ineffective: it does not lead to the incapacitation of the abuser, and women who report remain exposed to eventual lethal violence. As a result, reporting and doing nothing yield equivalent outcomes. Killing the partner entails substantial emotional and economic costs, but may be rationally chosen by women whose value of the relationship is sufficiently low relative to the combined risk of escalating abuse, being murdered and the cost of murder prosecution—that is, when the disutility of remaining exposed to lethal violence, net of legal costs, exceeds the value placed on the relationship.

The introduction of an NFS law fundamentally alters this calculation: reporting now becomes an effective strategy, leading to the partner's incapacitation through arrest, and is preferred by women whose disutility of continued exposure to violence exceeds the value of the relationship. The law thus reduces female-perpetrated IPH through substitution (as fewer women resort to preemptive lethal violence), and reduces male-perpetrated IPH through incapacitation (as violent men are removed from the victim's home and can no longer kill their partners).

Our conceptual framework focuses on IPH that occurs following nonfatal strangulation attempts—cases where the introduction of an NFS law most directly alters victims' decisions and perpetrators' incapacitation risk. It clarifies two key mechanisms: substitution and incapacitation.

While the model sharpens intuition for these core effects, our empirical analysis estimates the law's impact on overall IPH, which may also reflect spillover effects on relationships without documented strangulation, broader changes in reporting, and behavioral adaptation by offenders. We interpret our findings in light of both the model's predictions and additional potential channels not captured by the model, which is formalised in the Appendix A3.

8 Conclusion

Strangulation statutes are a relatively recent development in criminal justice, introduced in response to evidence that NFS is a common and gendered form of intimate partner abuse, often occurring at the most dangerous stage of escalation, before homicide.

In this paper, we make two main contributions. First, we compile a novel dataset on the timing of state-level NFS laws across the United States. Second, we combine this new dataset with detailed data from the FBI Supplementary Homicide Reports from 1990 to 2019 to estimate the causal effect of NFS laws on IPH of men and women in the United States.

Our findings indicate that NFS laws led to substantial reductions in IPH, particularly among younger adults. We estimate that these laws save approximately 238 women's lives and 91 men's lives each year. These reduced-form estimates are consistent with mechanisms such as increased incapacitation of abusers and reductions in preemptive violence by victims of repeated abuse.

This study contributes to ongoing policy debates around NFS criminalization and speak to broader literatures on gender-based violence, deterrence, and legal protection mechanisms. Policymakers and practitioners can use these insights to design interventions that protect IPV victims at critical points in the escalation of abuse and potentially prevent lethal outcomes.

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Appendix

A1 Data Construction and Sources

Here, we provide additional details on the construction of our main sample. We start from the raw SHR data by Fox and Swatt (2009)[12], and collapse the number of homicides per state-year for intimate partner (IP) and non-IP cases (non-IP includes other family members, friends, acquaintances, strangers, unknown, etc.). We then recode these counts to align with the total number of homicides reported in each state-year. For example, in cases where all homicides in a state-year are classified as non-IP by relationship, we code IP homicides as zero for that state-year. Similarly, where the only listed victims (excluding those with missing or undisclosed sex) are male, and the total homicide count matches male victims only, we code the corresponding female homicide count as zero for that state-year.

For example, in Georgia in 2013, the total number of non-IP homicides for females aged 18–49 was 34. The disaggregated victim-offender relationships indicated that out of these 34 cases, 10 were by other known offenders, 1 by a friend, 4 by strangers, and 19 by unknown offenders. This implies that homicides by other family members for this group were zero in that year. We followed this systematic approach throughout the sample to ensure accurate counts and correct handling of true zeros versus missing values.

Table A1: Key variables and sources

Variable Name	**Source**
Homicides	FBI-SHR
Population	Census
Personal income per capita	St Louis Federal Reserve
Poverty rate	Census
Female/male unemployment rate	CPS
Total unemployment rate	CPS

We then merged population data (United States Census, 2023b) by gender and age group for each state-year to construct outcome variables (homicides) as rates per 100,000. In addition, we merged state-year control variables: personal income per capita (U.S. Bureau of Economic Analysis and Federal Reserve Bank of St. Louis, 2023), total unemployment rate and female and male unemployment ratio from CPS (Flood et al., 2024), and state poverty rates (United States Census, 2023a).

[12]We requested the data from Fox, who generously sent us directly the 1976-2020 version in 2023.

A2 Additional Descriptive Statistics and Robustness Checks

Table A2: Percentage of Population by Cohort and Age Group in 2000

Treatment Cohort	% Pop 18–70	% Pop 18–34	% Pop 35–49	% Pop 50–70
2000 cohort	1.99	1.93	1.99	2.09
2004 cohort	4.83	4.87	4.73	4.89
2005 cohort	3.67	3.64	3.74	3.63
2006 cohort	2.85	2.82	2.84	2.89
2007 cohort	10.89	10.24	10.87	11.71
2008 cohort	1.92	1.85	1.98	1.93
2009 cohort	13.61	14.37	13.44	12.87
2010 cohort	10.01	10.07	9.82	10.18
2011 cohort	4.33	4.20	4.28	4.56
2012 cohort	16.93	17.66	17.00	15.92
2013 cohort	3.56	3.47	3.62	3.60
2014 cohort	5.34	5.52	5.37	5.09
2016 cohort	6.65	6.27	6.74	7.00
2017 cohort	5.07	5.03	5.14	5.03
2018 cohort	0.64	0.62	0.64	0.66
2019 cohort	1.48	1.47	1.45	1.54
Never treated	6.22	5.97	6.34	6.40

Table A3: Mean Covariates in 1990 and 1999, and Mean Change

Panel A: 1990 (Baseline)			
Variable	Eventually Treated	Never-Treated	Difference (SE)
income per capita	19574.52	20325.81	751.29 (3392.44)
log(income per capita)	9.87	9.91	0.04 (0.16)
unemployment rate	3.91	3.47	-0.45 (1.00)
poverty rate	13.64	11.35	-2.29 (0.93)**
male-to-female unemployment	1.53	1.24	-0.29 (0.35)
Panel B: 1999			
Variable	Eventually Treated	Never-Treated	Difference (SE)
income per capita	28633.60	29384.94	751.34 (4622.50)
log(income per capita)	10.25	10.28	0.03 (0.15)
unemployment rate	3.26	2.82	-0.43 (0.28)
poverty rate	11.95	10.63	-1.31 (3.04)
male-to-female unemployment	1.32	1.61	0.29 (0.65)
Panel C: Change from 1990 to 1999			
Variable	Eventually Treated	Never-Treated	Difference (SE)
Δ income per capita	9059.08	9059.13	0.05 (1247.00)
Δ log(income per capita)	0.38	0.37	-0.01 (0.01)
Δ unemployment rate	-0.65	-0.64	0.01 (0.97)
Δ poverty rate	-1.69	-0.71	0.98 (2.48)
Δ male-to-female unemployment	-0.20	0.37	0.58 (0.31)*

Note: The table reports means of key covariates in 1990 and 1999 and changes over the decade. Differences are estimated as coefficients on the never-treated indicator from separate regressions, weighted by population share (18-70) in 2000. There are 50 observations (one per state). Robust HC3 standard errors in parentheses.
*p-value<0.1, **p-value<0.05, ***p-value<0.01.

Table A4: Regression of Change in Covariates from 1990 to 1999 on Year of Adoption

Dependent variable	Coefficient	R-squared
Δ income per capita	32.49	0.016
	(44.93)	
Δ log(income per capita)	-0.0002	0.001
	(0.0009)	
Δ unemployment rate	0.044	0.037
	(0.041)	
Δ poverty rate	0.002	0.000
	(0.086)	
Δ male-to-female unemployment	0.028	0.064
	(0.022)	

Note: Each cell reports the coefficient from a separate regression of the change in the covariate from 1990 to 1999 on year of adoption, weighted by the population share (18-70) in 2000. There are 50 observations (states). Robust HC3 standard errors in parentheses.
*p-value<0.1, **p-value<0.05, ***p-value<0.01.

Table A5: Regression of Dependent Variable in 1990 on Year of Adoption

Dependent variable	Coefficient	R-squared
income per capita	78.98	0.020
	(90.72)	
log(income per capita)	0.0038	0.018
	(0.0044)	
unemployment rate	-0.0126	0.007
	(0.0246)	
poverty rate	-0.081	0.014
	(0.0861)	
male-to-female unemployment	-0.0075	0.010
	(0.0157)	
male-victim IPH 18–34	-0.0255*	0.053
	(0.0134)	
female-victim IPH 18–34	-0.0141	0.007
	(0.0263)	
male-victim IPH 35–49	-0.0097	0.003
	(0.0345)	
female-victim IPH 35–49	-0.0345*	0.059
	(0.0202)	
male-victim IPH 50–70	-0.0202	0.046
	(0.0121)	
female-victim IPH 50–70	-0.0020	0.001
	(0.0143)	

Note: Each cell reports the coefficient from a separate regression of the level of the variable in 1990 on year of adoption, weighted by population (18-70) share in 2000 for regressions of covariates, and cohort-age share in 2000 for regressions of IPH measures. There are 50 observations (states) for covariates and 49 observations for IPH measures (one state has missing information for IPH in 1990). Robust HC3 standard errors in parentheses.
*p-value<0.1, **p-value<0.05, ***p-value<0.01.

Table A6: NFS Law and IPH (counts): Poisson and NB (Negative Binomial) Models

Dependent variable	Poisson	NB	Mean in 1999
Male-victim IPH 18–34	-0.180*	-0.180*	3.746
	(0.104)	(0.104)	
Female-victim IPH 18–34	-0.081**	-0.083**	17.392
	(0.038)	(0.040)	
Male-victim IPH 35–49	-0.065	-0.066	5.088
	(0.066)	(0.066)	
Female-victim IPH 35–49	0.045	0.034	16.606
	(0.055)	(0.058)	
Male-victim IPH 50–70	0.129	0.129	2.640
	(0.089)	(0.089)	
Female-victim IPH 50–70	-0.069	-0.069	5.843
	(0.046)	(0.046)	

Note: All regressions include state and year fixed effects. N = 1479. Standard errors clustered at the state level (50 clusters), shown in parentheses.
*p<0.1, **p<0.05, ***p<0.01.

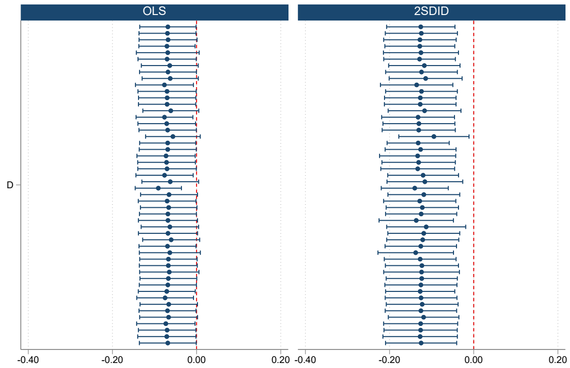
Table A7: Effects of NFS Law on Male-victim and Female-victim IPH (per 100,000) with baseline covariates interacted with linear time trends

Dependent variable	OLS	2SDID	Mean in 1999	Counterfactual Mean
Male-victim homicides 18–34	-0.069**	-0.125***	0.307	0.338
	(0.034)	(0.043)		
Female-victim homicides 18–34	-0.133**	-0.219***	1.233	1.252
	(0.054)	(0.069)		
Male-victim homicides 35–49	-0.059	-0.064	0.402	0.353
	(0.041)	(0.064)		
Female-victim homicides 35–49	-0.079	-0.174**	1.145	1.250
	(0.069)	(0.077)		
Male-victim homicides 50–70	-0.004	-0.005	0.266	0.210
	(0.021)	(0.030)		
Female-victim homicides 50–70	-0.028	-0.025	0.480	0.509
	(0.027)	(0.033)		

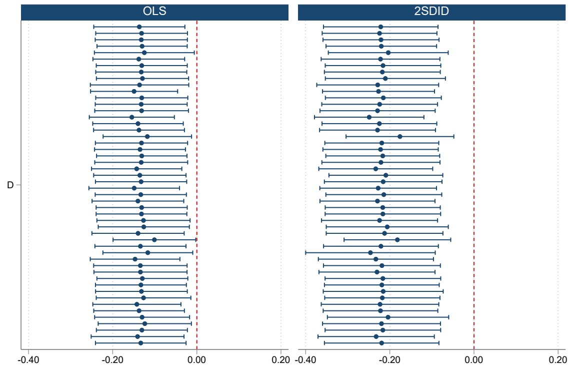
Notes: All regressions include state and year fixed effects, and baseline (1990) covariates (log income per capita, unemployment rate, poverty rate, and male-to-female unemployment ratio) interacted with linear time trends. $N = 1479$. Clustered standard errors (50 clusters). *p<0.1, **p<0.05, ***p<0.01.

Figure A1: Overall ATT estimates: Dropping one state at a time

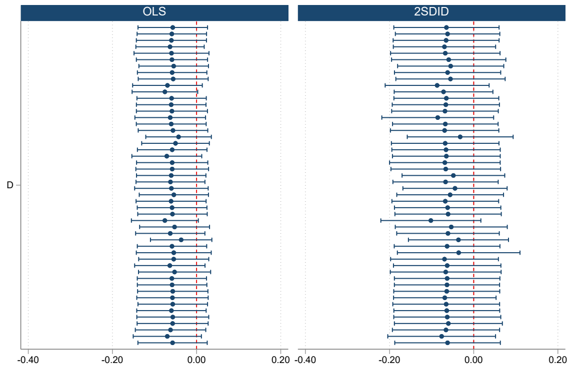
(a) Male-victim IPH 18-34



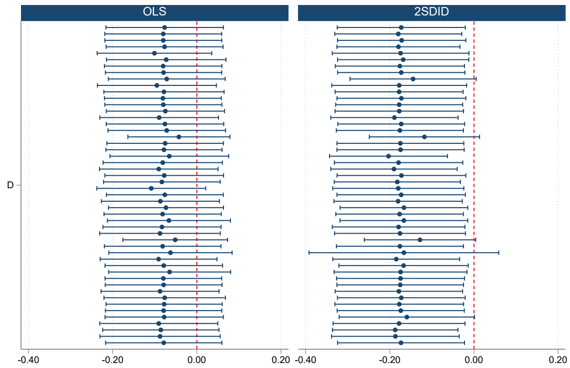
(b) Female-victim IPH 18-34



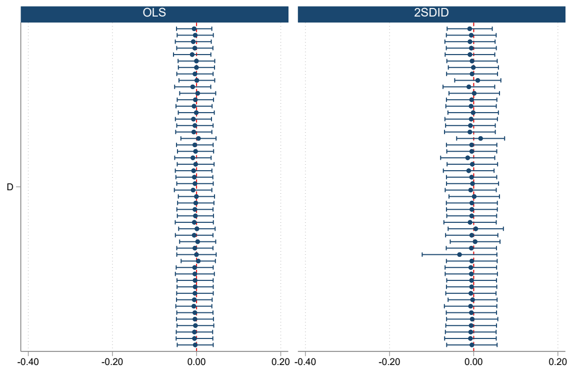
(c) Male-victim IPH 35-49



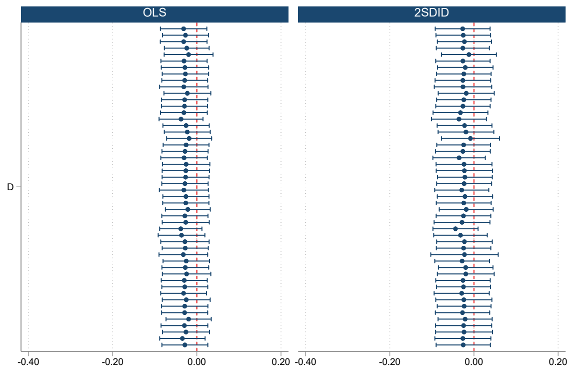
(d) Female-victim IPH 35-49



(e) Male-victim IPH 50-70



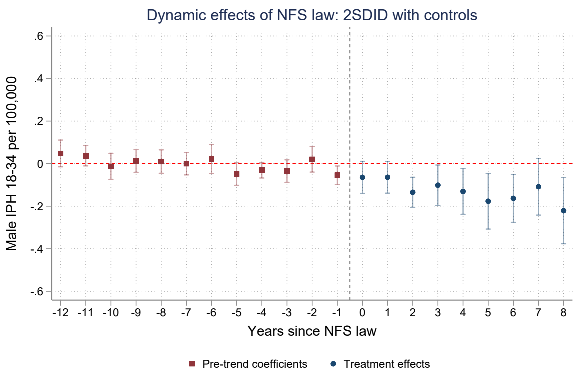
(f) Female-victim IPH 50-70



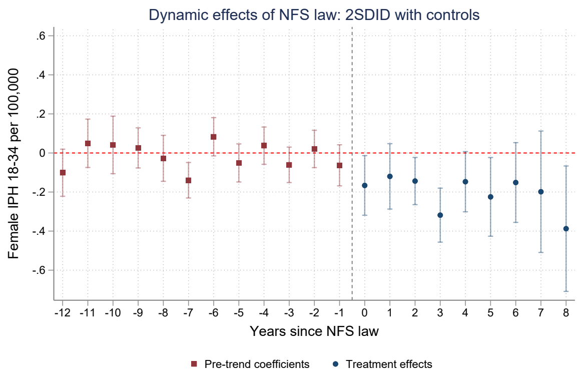
Note: Each panel replicates the analysis in Table A7 after dropping one state at a time.

Figure A2: 2SDID Dynamic Effects of NFS Laws on Male-victim and Female-victim IPH (per 100,000) with baseline controls interacted with a time trend

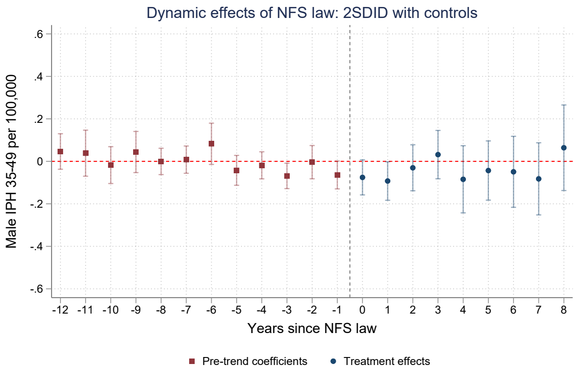
(a) Male-victim IPH 18-34



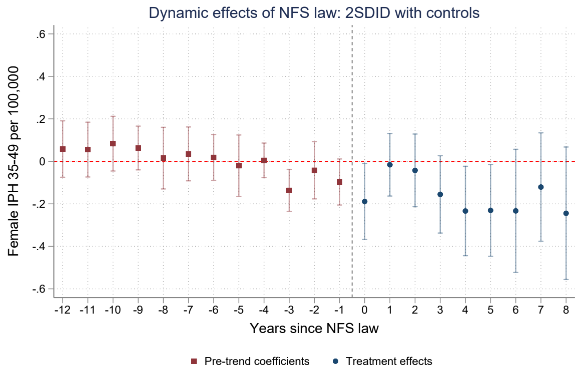
(b) Female-victim IPH 18-34



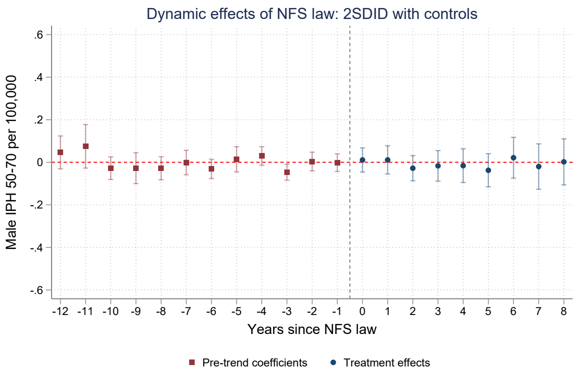
(c) Male-victim IPH 35-49



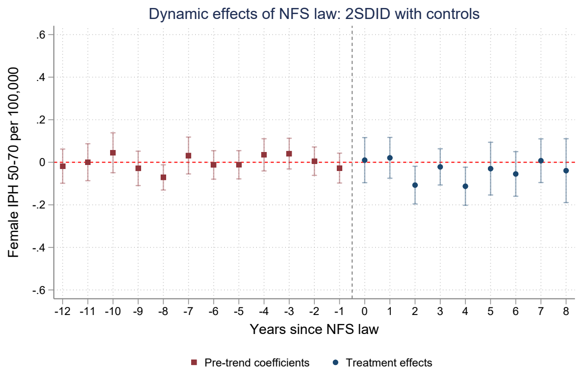
(d) Female-victim IPH 35-49



(e) Male-victim IPH 50-70



(f) Female-victim IPH 50-70



Note: The event study estimates are based on 2SDID estimates by including the event-time indicators D_{st}^k as treatment variables in the second stage. State fixed effects, year fixed effects and the coefficients on covariates for the baseline controls interacted with a time trend are estimated in the first stage for the sample of untreated observations ($D_{st} = 0$). The event study estimates are based on 2SDID

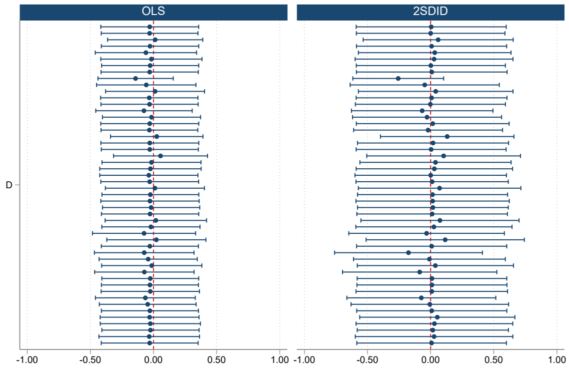
Table A8: Effects of NFS Law on Male-victim and Female-victim Homicides by Strangers (per 100,000)

Dependent variable	OLS	2SDID	Mean in 1999	Counterfactual Mean
Male-victim homicides 18–34	-0.114	-0.242	1.600	1.374
	(0.165)	(0.228)		
Female-victim homicides 18–34	0.013	-0.008	0.141	0.127
	(0.025)	(0.037)		
Male-victim homicides 35–49	-0.054	-0.115	0.630	0.727
	(0.084)	(0.081)		
Female-victim homicides 35–49	0.014	-0.007	0.114	0.092
	(0.016)	(0.020)		
Male-victim homicides 50–70	-0.006	-0.034	0.273	0.338
	(0.032)	(0.033)		
Female-victim homicides 50–70	-0.002	-0.001	0.073	0.053
	(0.013)	(0.019)		

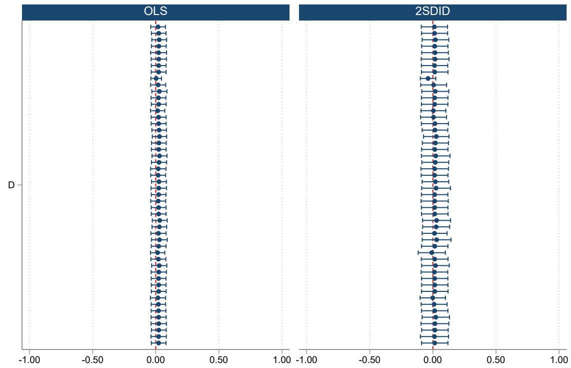
Notes: All regressions include state and year fixed effects. N = 1479. Clustered standard errors (50 clusters). *p<0.1, **p<0.05, ***p<0.01.

Figure A3: Placebo estimates: Dropping one state at a time

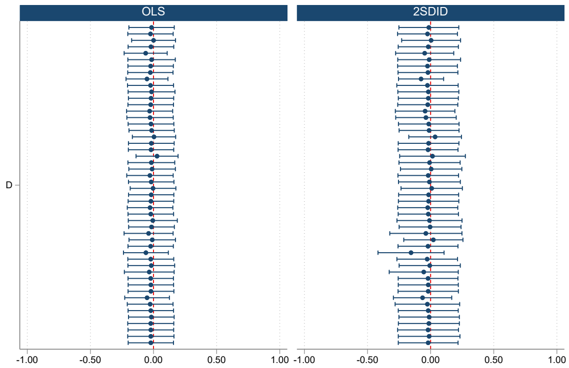
(a) Male-victim Homicide by strangers 18-34



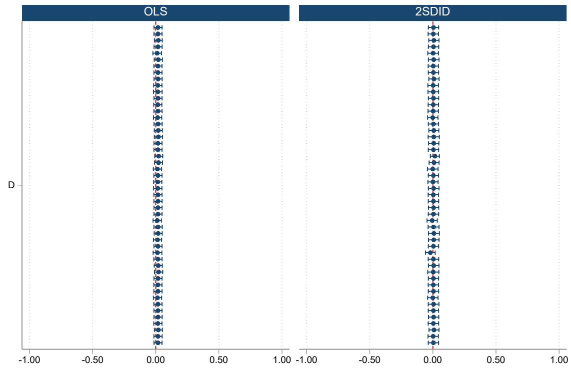
(b) Female-victim Homicide by strangers 18-34



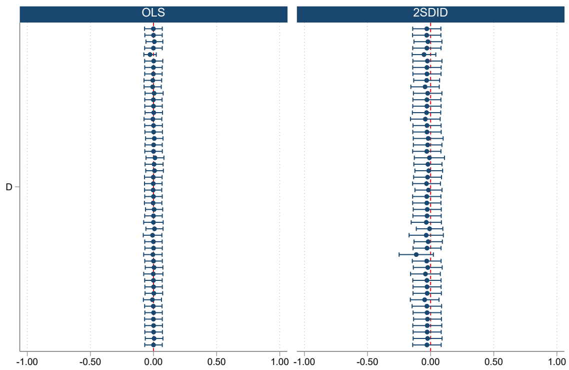
(c) Male-victim Homicide by strangers 35-49



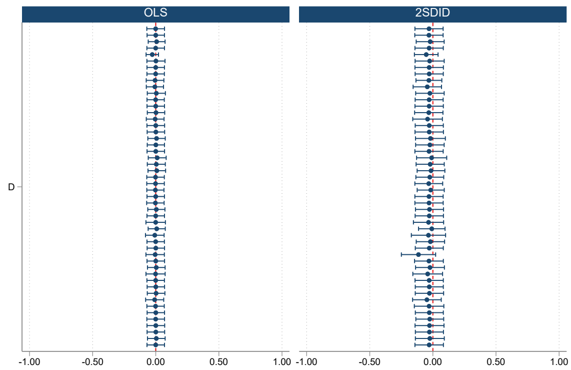
(d) Female-victim Homicide by strangers 35-49



(e) Male-victim Homicide by strangers 50-70



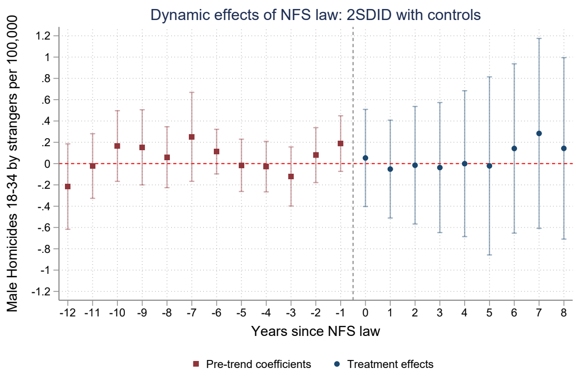
(f) Female-victim Homicide by strangers 50-70



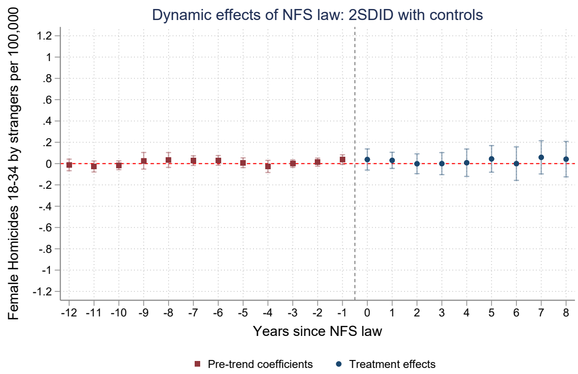
Note: Each panel replicates the analysis in Table 6 after dropping one state at a time.

Figure A4: 2SDID Dynamic Effects of NFS Laws on Male-victim and Female-victim Homicides by strangers (per 100,000) with baseline controls interacted with a time trend

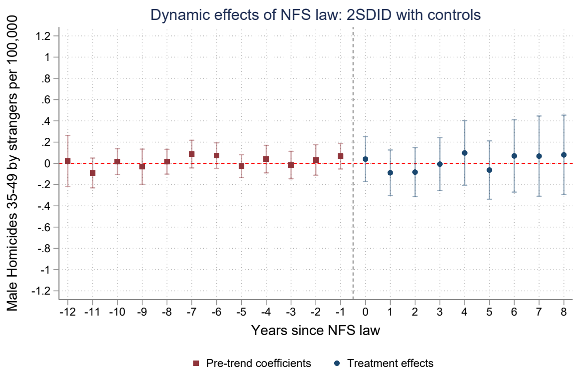
(a) Male-victim IPH 18-34



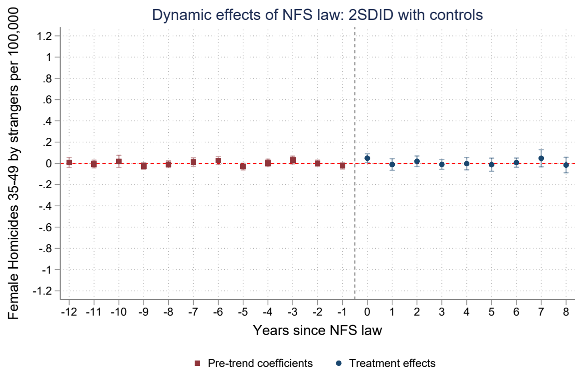
(b) Female-victim IPH 18-34



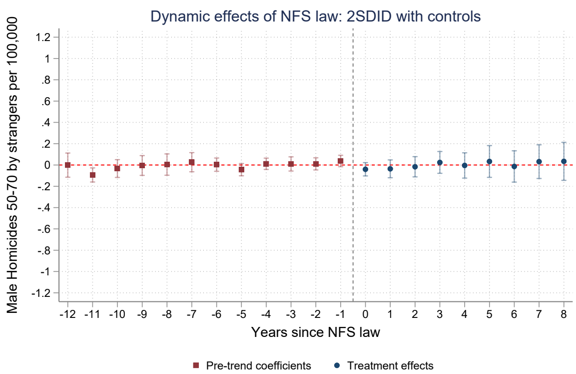
(c) Male-victim IPH 35-49



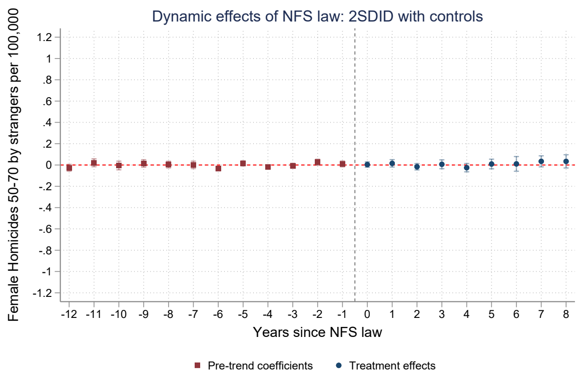
(d) Female-victim IPH 35-49



(e) Male-victim IPH 50-70



(f) Female-victim IPH 50-70



Note: The event study estimates are based on 2SDID estimates by including the event-time indicators D^k_{st} as treatment variables in the second stage. State fixed effects, year fixed effects and the coefficients on covariates for the baseline controls interacted with a time trend are estimated in the first stage for the sample of untreated observations ($D_{st} = 0$). Estimation is conducted simultaneously using the (GMM) framework in Gardner et al. (2025) and using the `did2s` *Stata package developed by Butts (2021).*

A3 A Stylized Model

This appendix presents a simple stylized model that formalizes the key mechanisms discussed in Section 7.

We assume there are two types of male partners: stranglers (S), who engage in nonfatal strangulation, and non-stranglers (NS), who do not. Women paired with NS partners face no fatal threat. Women paired with S partners face three possible choices after being strangled: kill the partner preemptively (K); report to authorities (R); or do nothing (N). A woman's economic and emotional valuation of the relationship is denoted m, drawn from a cumulative distribution function F. Higher values of m reflect more valued relationships with their partner. The woman also faces two types of costs, depending on her chosen action and the legal environment (without the law, with the law): $d > 0$, the disutility of future lethal violence, and $c > 0$, the disutility of facing prosecution for killing her partner. Table A9 summarises the expected utility of each option:

Table A9: Expected Utility by Choice and Legal Environment

Choice	**Without Law**	**With Law**
Kill the partner (K)	$-m - c$	$-m - c$
Report to authorities (R)	$-d$	$-m$
Do nothing (N)	$-d$	$-d$

Without the law, reporting is ineffective: R yields utility $-d$, identical to doing nothing. In this environment, women compare K to N and will choose to kill if $m < d - c$.

With the law in place, reporting becomes effective (i.e., leads to incapacitation of the abusive partner and prevents further fatal violence), yielding utility $-m$. In this case, the woman chooses R if $m < d$, and N otherwise.

Two key predictions arise when moving from an environment without an NFS law to one with an NFS law:

1. **Reduction in male-victim IPH**. The availability of an effective reporting option reduces the incidence of preemptive partner killings. The share of women who choose K falls from $F(d - c)$ to zero:

$$\Delta(\text{Male-victim IPH}) = -F(d - c) < 0.$$

2. **Reduction in female-victim IPH**. Reporting leads to incapacitation of abusive partners, thereby lowering the risk of subsequent fatal violence against women:

$$\Delta(\text{Female-victim IPH}) = F(d) - F(d - c) < 0.$$

Thus, NFS laws reduce male-victim IPH through *substitution* (fewer preemptive killings) and female-victim IPH through *incapacitation* (removal of dangerous partners).

The magnitudes of these effects depend on the distribution F and on the relative costs d and c. Figure A5 illustrates these mechanisms graphically:

Figure A5: Women's Choices Before and After NFS Law