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Non-Fatal Strangulation Laws and Intimate Partner Homicides*

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Abstract

Do non-fatal strangulation laws save lives? 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 IPH 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 IPH rates. We estimate that these laws reduce female-victim IPH by 14% and male-victim IPH by 36%, among victims aged 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 disrupt the escalation of IPV and reduce lethal outcomes.

Keywords: Intimate Partner Violence; Gender; Difference-in-Differences; Criminal Justice Policy.

JEL codes: C21; I18; J12; J16; J78; K14; K42; N92.

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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 (Parekh et al., 2024). The lifetime prevalence of women who have experienced choking or being suffocated by an intimate partner based on the National Intimate Partner and Sexual Violence Survey is estimated at 16.2%, representing more than 20 million victims (Leemis et al., 2022). NFS is an important risk factor for *subsequent* intimate partner homicide (IPH) (Glass et al., 2008) and signals an escalation of violence and control within the relationship (Patch et al., 2018).

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 IPH rates of men and women.

Before these statutes, NFS was frequently misclassified as simple assault or went unrecorded by law enforcement, owing to the absence of visible injuries and limited awareness (Stellpflug et al., 2022). 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 [...] when in fact these were the most [...] violent cases in the system."

Our empirical strategy exploits the staggered adoption of NFS laws across states and relies on 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 (e.g., Han, 2023; Smart et al., 2024).

We find that the introduction of NFS laws led to substantial reductions in 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 men), female-victim IPH rates in the same age group fell by 14% (from 1.205 to 1.032 per 100,000 women), and female-victim IPH rates for ages 35-49 decreased by 13% (from 1.241 to 1.076 per 100,000 women). 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 close to zero 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. In addition, a falsification test reassuringly shows no evidence that NFS laws affected homicides committed by strangers.

We also explore whether the effects of NFS laws vary with baseline state measures of gender inequality and economic resources (both measured in 1990), and policing resources (measured in 2000, the earliest year available), but find no evidence of heterogeneous impacts along these dimensions.

While we observe only reduced-form impacts on IPH, our findings are consistent with two channels: increased incapacitation of abusers, and reduced need for preemptive violence by victims. By enlisting NFS as a serious offense, these laws may increase the likelihood that abusive partners are incapacitated by law enforcement (after victims report NFS), thus preventing escalation to lethal violence. NFS laws may also reduce the need for victims to resort to lethal self-defense to protect themselves (Aizer and Dal Bo, 2009; Miller and Segal, 2018), especially in situations of repeated violent abuse. We provide a conceptual framework consistent with these mechanisms.

Taken together, our findings indicate that NFS laws are an effective policy tool for reducing IPH. More broadly, recognizing NFS as a serious stand-alone 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 (Aizer, 2010; Bhalotra et al., 2025).

Globally, many jurisdictions still lack NFS-specific statutes. The Council of Europe's Istanbul Convention on preventing and combating violence against women (Council of Europe, 2011), signed in 2011 and ratified in 2014, does not mention NFS, suffocation, or choking. England and Wales introduced an NFS-specific law only in 2022 (Ministry of Justice and The Rt Hon Victoria Atkins MP, 2022); Victoria, Australia, followed in 2024 (Judicial College of Victoria, 2024); and in Scotland, legislation is under debate as of 2025 (Scottish Parliament, Criminal Justice Committee, 2025). Many other countries, including France, Italy, and Spain, have no specific provisions.

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 extend previous research by focusing on NFS, a previously overlooked IPV phenomenon. Aizer and Dal Bo (2009) and Miller and Segal (2018) show that criminal justice interventions can reduce IPH. 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.

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 fill 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 results. Section 7 offers a conceptual framework to help understand the mechanisms underlying the estimated effects. 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

NFS is a 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). Even when pressure is 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 (Stellpflug et al., 2022).

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 subsequent escalating IPV, and victims who experience NFS are much more likely to later be killed by their intimate partner, usually by gun-shot or stabbing (Glass et al., 2008).[2]

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

[2] The rate of homicide by asphyxiation in the U.S. is 0.2 per 100,000 women (Sorenson et al., 2014).

2.2 NFS Statutory Classification in the United States

Strangulation statutes represent a relatively 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. Explicit recognition in statute ensures accountability for this form of abuse and provides law enforcement with a clear tool to investigate and prosecute cases that previously went unnoticed or uncharged. Even severe NFS incidents were often classified merely as simple assaults or not recorded at all, sometimes with deadly consequences later on. For instance, Monica Weber-Jeter died from stab wounds inflicted by her husband only a few months after he had non-fatally strangled her in their family home in Ohio in 2014 (Jeltsen, 2015). Although she had filed a police report for domestic violence and NFS against him, Ohio did not have an NFS law at that time.

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 [...] 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, the incapacitation of abusers, and prosecution efforts, thereby disrupting the pathway from NFS to IPH. Ultimately, our goal is to investigate whether criminalizing NFS 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 U.S. state through 2025, we identify the relevant bill introducing NFS as a standalone offense, verify its legislative passage history, and record both the date the law was signed by the Governor and the date it became effective. Second, we validate these data with Legislative State Librarians at the Legislative Library or State Law Library of each U.S. state.[3] Table 1 displays, for each state, the year the law was passed, the year it became effective, and the bill number.[4]

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.[5] South Carolina remains the only state without an NFS law as of 2025, and it is not included in our sample.

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

[4]Prior to these laws, a few states referenced NFS only within statutes concerning child abuse and/or abuse of elderly or vulnerable adults; these cases are not included in this table or in our analysis.

[5]The imputation approach we use requires untreated/not-yet-treated 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.

Table 1: NFS Laws: Timing and Bill Numbers by State

State	Year Effective	Year Passed	Bill Number
Alabama	2011	2011	HB512
Alaska	2005	2005	HB219
Arizona	2010	2010	SB1266
Arkansas	2009	2009	HB1040
California	2012	2011	SB430
Colorado	2016	2016	HB1080
Connecticut	2007	2007	SHB7313
Delaware	2010	2010	SB197
Florida	2007	2007	SB184
Georgia	2014	2014	HB911
Hawaii	2006	2006	HB3256
Idaho	2005	2005	SB1062
Illinois	2009	2009	HB0594
Indiana	2006	2006	HB1281
Iowa	2012	2012	SF93
Kansas	2017	2017	SB112
Kentucky	2019	2019	SB70
Louisiana	2007	2007	HB519
Maine	2012	2012	HP1381
Maryland	2020	2020	SB212
Massachusetts	2014	2014	SB2334
Michigan	2013	2012	SB848
Minnesota	2005	2005	HF1
Mississippi	2010	2010	SB2923
Missouri	2000	2000	HB1677
Montana	2017	2017	SB153
Nebraska	2004	2004	LB943
Nevada	2009	2009	AB164
New Hampshire	2011	2010	HB1634
New Jersey	2017	2017	A2061
New Mexico	2018	2018	SB0061
New York	2010	2010	S6987
North Carolina	2004	2004	H1354
North Dakota	2007	2007	SB2185
Ohio	2023	2023	SB288
Oklahoma	2005	2004	HB2380
Oregon	2004	2003	HB2770
Pennsylvania	2016	2016	HB1581
Rhode Island	2012	2012	HB7242
South Carolina	NA	NA	NA
South Dakota	2012	2012	SB156
Tennessee	2011	2011	SB476
Texas	2009	2009	HB2066
Utah	2017	2017	HB0017
Vermont	2006	2006	H856
Virginia	2012	2012	HB752
Washington	2007	2007	SB5953
West Virginia	2016	2016	HB4362
Wisconsin	2008	2008	SB260
Wyoming	2011	2011	SF0132
District of Columbia	2023	2023	B25-0395

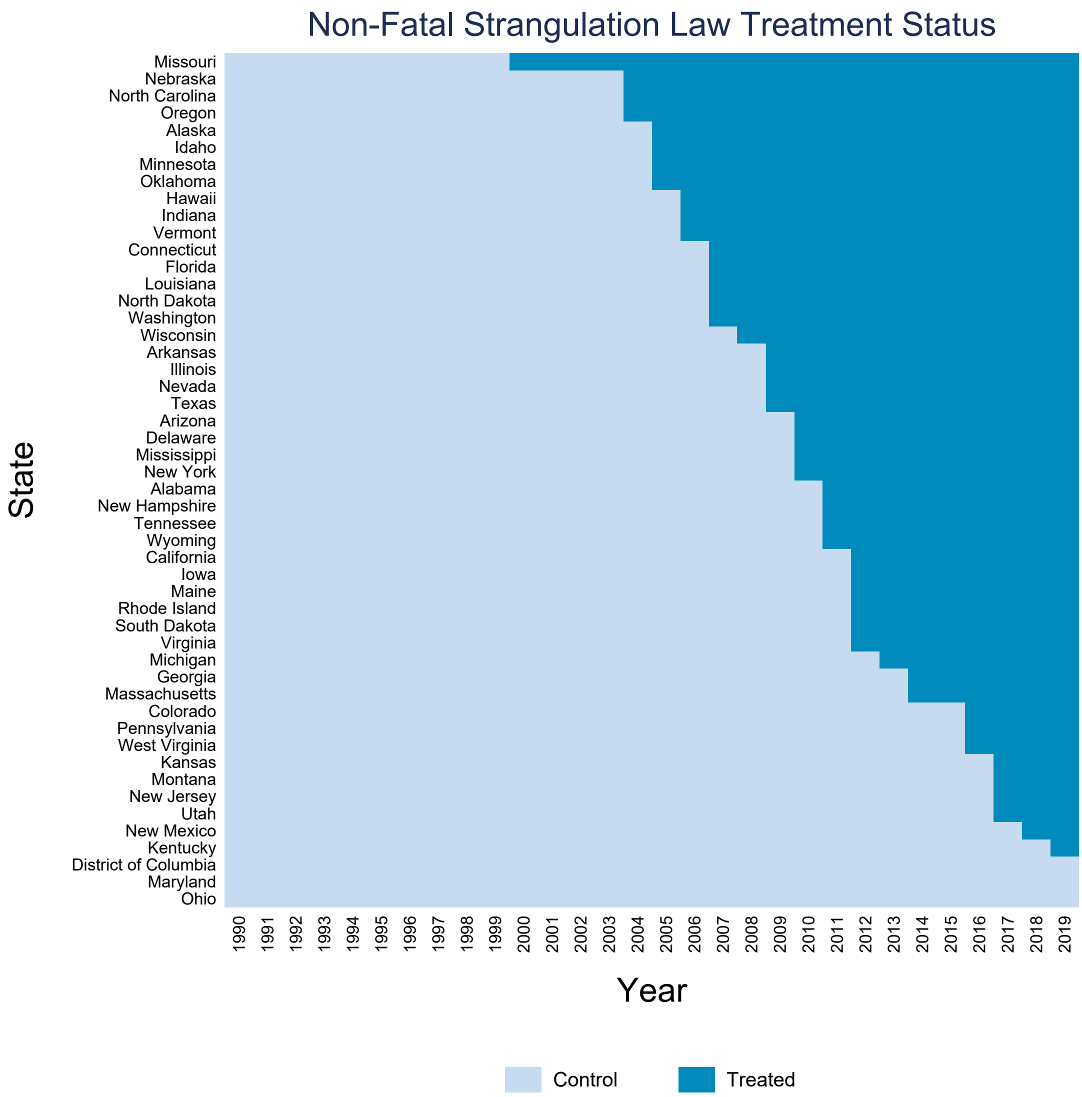


Figure 1: Staggered implementation of NFS Laws

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.

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

Treatment Cohort	**States**	**Frequency (absolute)**	**Frequency (relative)**	**Population (relative)**
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 (relative) 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. Online 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 by state and year. 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).[6] 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 their age, sex, and race. The unit of reporting in the SHR is the homicide incident. We focus on incidents involving a single victim and exclude those with multiple offenders—retaining the vast majority of homicides—to ensure accurate coding of victim-offender relationships. Our analysis focuses on the victim file.

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 male (female) 100,000 population) in each age group. We also present a complementary analysis using homicide counts.

Our dataset covers 50 states (including D.C. and excluding South Carolina) over 30 years (1990–2019), yielding 1,500 potential state-year observations. Following Chin and Cunningham (2019), we do not use the imputed values in this dataset. Homicide

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

reporting is missing for 21 state-year cases, resulting in a final sample of 1,479.[7]

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 enforcement agencies were reporting to NIBRS, whereas the SHR receives reports from nearly all agencies nationwide. The SHR provides a consistent time series of over 30 years; although reporting is voluntary, submission rates by local law enforcement agencies 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 homicides committed by strangers as a falsification test because they are among the most prevalent homicide types and NFS laws specifically target escalating violence within intimate relationships. We disaggregate strangers homicides by sex and age group of the victim, measured at the state-year level and expressed per 100,000 male (female) population in the same age ranges used for IPH. This falsification test helps validate our identification strategy, which we discuss in the next section.

[7] See Table A2.

4 Empirical Strategy

This section presents our identification strategy and regression specifications. All regressions are weighted by state population.

4.1 Identification of Overall ATT estimates

TWFE via OLS estimation. We begin with a two-way fixed effects (TWFE) regression model:

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

where Y_{st} denotes the number of male-victim (or female-victim) intimate partner homicides per 100,000 men (or women) in state s and year t. The variable D_{st} is a binary indicator equal to one in the year the NFS law becomes effective in state s and in all subsequent years. State fixed effects α_s absorb time-invariant characteristics of states, while 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, applying OLS to equation (4.1) yields a consistent estimate for β under the parallel trends and no anticipation assumptions (Roth et al., 2023).[8] However, as shown by de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021) and others, OLS estimation is problematic if treatment effects vary across states and over time. Indeed, as explained by Gardner et al. (2025), the TWFE regression model can be rewritten as:

$$Y_{st} = \beta D_{st} + \alpha_s + \gamma_t + u_{st}, \tag{4.2}$$

where $u_{st} = (\beta_{st} - \beta)\, D_{st} + \varepsilon_{st}$. In this case, applying OLS to equation (4.2) yields inconsistent estimates of β, unless we are in the two-state, two-year case, or when $\beta_{st} = \beta$ for all s and t, in which case the regression is correctly specified.

[8] An implicit assumption is SUTVA.

TWFE via Two-Stage (2SDID) Estimation. To address the limitation the OLS estimator, 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/not-yet-treated observations ($D_{st} = 0$) in a first stage. In the second stage, the outcomes are residualized using these estimates, and the overall ATT (average treatment effect on the treated) 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/not-yet-treated 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., Han, 2023; Smart et al., 2024).[9]

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

4.2 Identification of Dynamic ATT estimates

We also estimate treatment effects relative to the year of treatment adoption. 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, after estimating the state and year fixed effects among untreated/not-yet-treated observations ($D_{st} = 0$). Thus, we follow Gardner et al. (2025) and use the 2SDID approach to estimate

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

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 $k \geq 0$.

This approach yields unbiased estimates of the dynamic ATT profile under the same assumptions required for the static 2SDID estimator.

Weighting and interpretation of estimated ATT. All regressions are weighted by state population, using population counts from the 2000 Census. Hence, we estimate average causal effects of NFS laws intimate partner homicide rates among men and women in the relevant age group, in states that passed such laws. We apply weights that reflect the corresponding population in each group.[10]

[10] Technically, 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 A3.

5 Descriptive Statistics

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 (the year before Missouri passed the first NFS law) on the year of adoption for all states, including Maryland (2020), DC (2023) and Ohio (2023).

Table 3 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 3: Regressions of Change in IPH from 1990 to 1999 on Year of Adoption

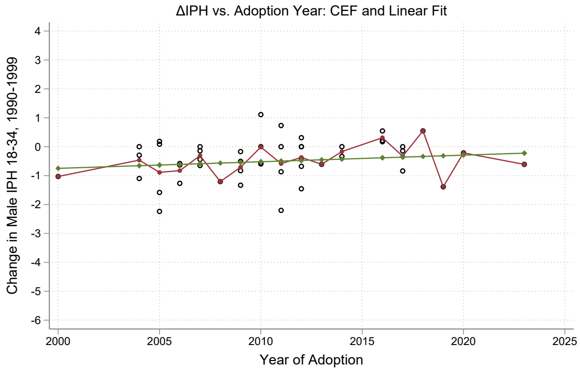
Dependent variable	Coefficient	R-squared
Δ male-victim IPH 18–34	0.023	0.043
	(0.019)	
Δ female-victim IPH 18–34	-0.001	0.000
	(0.025)	
Δ male-victim IPH 35–49	-0.004	0.001
	(0.034)	
Δ female-victim IPH 35–49	0.018	0.019
	(0.017)	
Δ male-victim IPH 50–70	0.012	0.011
	(0.016)	
Δ female-victim IPH 50–70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

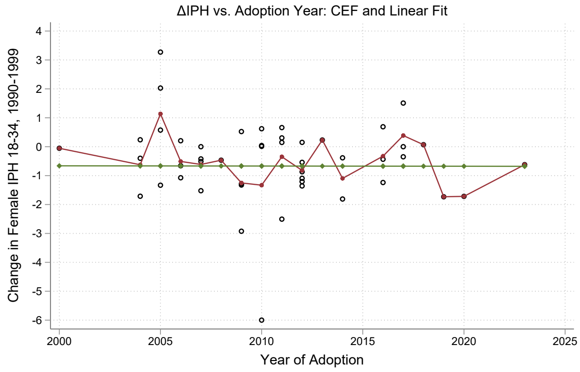
Regressing the change in covariates from 1990 to 1999 on the year of adoption reveals no significant relationship either, as shown in Table A5 in the Online Appendix. Moreover, the covariates in 1990 do not appear to be correlated with year of adoption (Table A6).

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

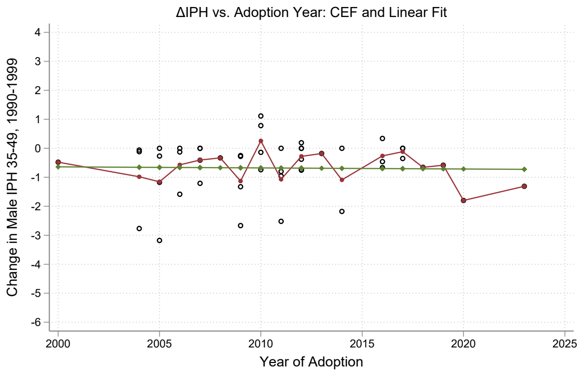
(a) Δ Male-victim IPH 18-34 & Year of Adoption



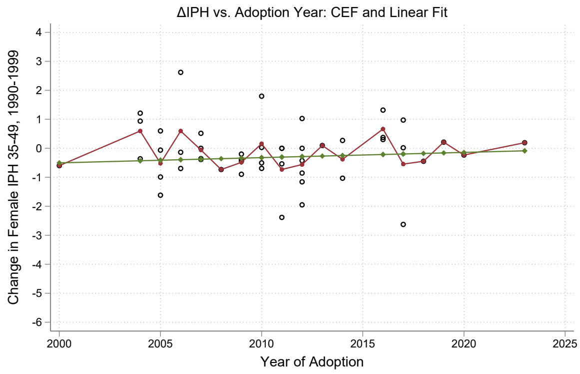
(b) Δ Female-victim 18-34 & Year of Adoption



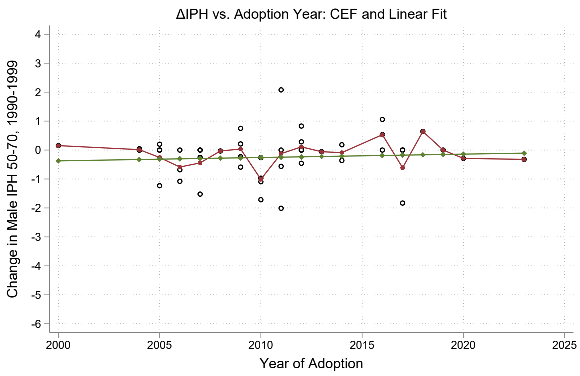
(c) Δ Male-victim 35-49 & Year of Adoption



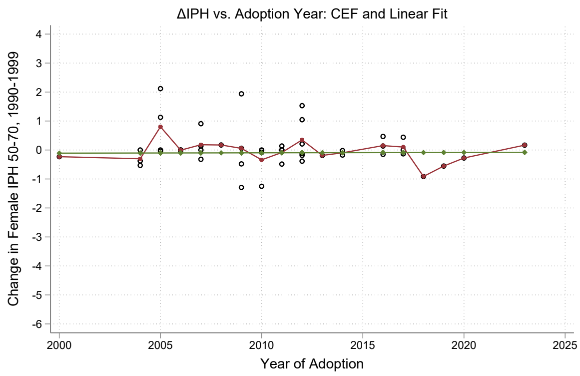
(d) Δ Female-victim 35-49 & Year of Adoption



(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.

Pre-Treatment Outcome Trends: Eventually Treated vs. Never-Treated States. A key identifying assumption in a 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 4 reports changes in male- and female-victim IPH across age groups between 1990 and 1999. 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 4: 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.[11]

[11] 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/not-yet-treated 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}.

Table A4 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.

6 Results

6.1 Overall ATT Estimates

Main specifications. Table 5 reports the estimated effects of NFS laws on male and female IPH, expressed per 100,000, by victim's age group. The first two columns show results from OLS and two-stage difference-in-differences (2SDID) estimators; the latter is our preferred approach. The last two columns 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.[12]

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.

[12] 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/not-yet-treated observations ($D_{st} = 0$).

The results in Table 5 show that NFS laws are associated with sizable reductions in IPH rates, particularly among younger adults. Among individuals aged 18–34, male-victim IPH declines by 0.120 per 100,000 men under the 2SDID specification, a 36% reduction, from a counterfactual mean of 0.332 to 0.212. For female victims in the same age group, the 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. Our estimates align 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. 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. The existing literature explains the sizable impact on male-victim IPH with a decrease in the number of women killing their abusers.

Robustness checks. Table A7 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 (e.g., Bailey and Goodman-Bacon, 2015; Conti and Ginja, 2020; Mora-García et al., 2024), as shown in Table A8.[13] 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. [14]

Summary of Overall ATT estimates. These substantial drops in IPH highlight the effectiveness of explicitly adding NFS to statutes and defining it as a stand-alone serious offense. In the absence of a specific NFS law, law enforcement often lacked the tools and awareness needed to systematically break the cycle of violent abuse that can escalate from NFS to IPH (Strack and Gwinn, 2011). NFS laws represent an actionable and scalable policy that our estimates show to be effective in saving lives and that can be readily implemented by states and countries around the world.

[13]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 Online Appendix A1 for further details.

[14]Potentially confounding policies such as IPV mandated arrests laws, protection order laws, and nearly all unilateral divorce laws, were adopted by US states by 1990, which is our baseline year and well before our first treated state in the year 2000.

6.2 Dynamic ATT Estimates

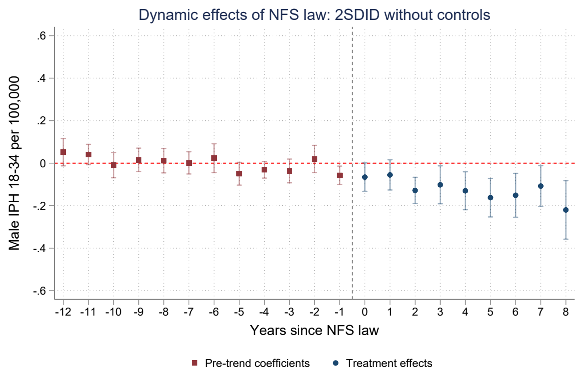
Figure 3 presents dynamic treatment effects estimated using the two-stage difference-in-differences (2SDID) approach, by victim gender and age group. The pre-treatment coefficients (shown in red squares) are close to zero for most of the pre-treatment years across all panels, providing additional support—consistent 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 close to zero for both male and female victims aged 50–70. As with the overall ATT estimates (Table 5, Table A8), when controlling for baseline covariates (measured in 1990) interacted with linear time trends (Figure A2), similar patterns of dynamic effects are found.

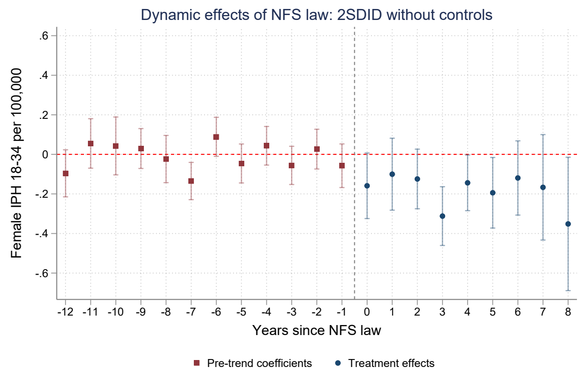
Our dynamic ATT estimates show that NFS laws contribute to sizable and sustained reductions in IPH among younger victims: we show that specifically adding NFS to the statutes as a serious offense represents a successful policy to tackle escalating violent abuse and to save lives.

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

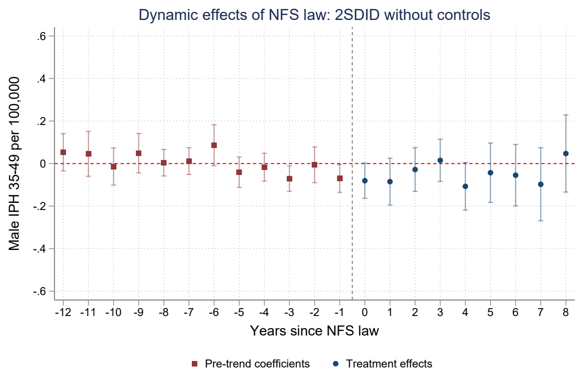
(a) Male-victim IPH 18-34



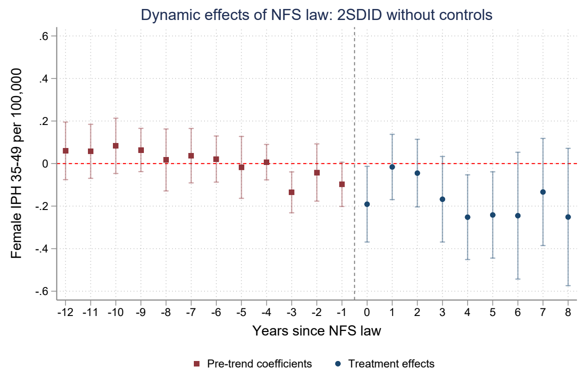
(b) Female-victim IPH 18-34



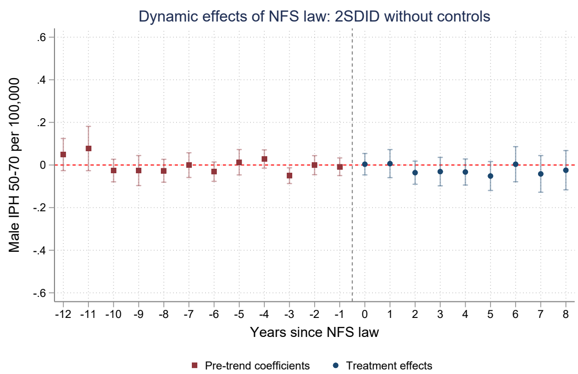
(c) Male-victim IPH 35-49



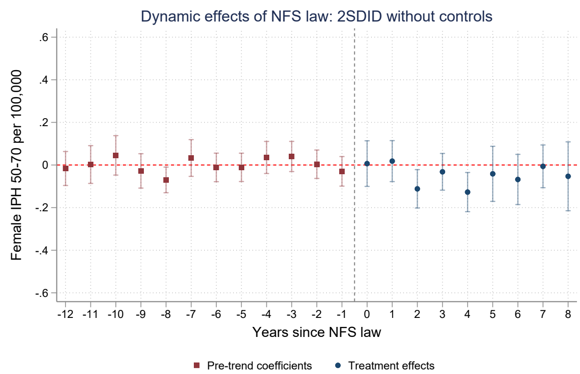
(d) Female-victim IPH 35-49



(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/not-yet-treated 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. Online Appendix Table A9 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.

The result from this falsification exercise is consistent with NFS laws not affecting 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. $N = 1479$. Standard errors clustered at the state level (50 clusters), shown in parentheses.
*p<0.1, **p<0.05, ***p<0.01.

6.4 Heterogeneous effects by baseline characteristics

In this subsection, we investigate whether the effects of NFS laws vary across states with different socioeconomic conditions, gender inequality, and local police resources. We conduct this analysis by interacting the treatment variable with baseline state characteristics measured in 1990 (or in 2000 where earlier data are unavailable). These characteristics include proxies for economic resources (income per capita, poverty rate, and unemployment rate), gender inequality (measured as the male-to-female unemployment ratio), and local police resources (the number of sworn personnel per 100,000 population and the number of uniformed officers whose regular duties include responding to calls for service per 100,000 population), using data from the 2000 Census of State and Local Law Enforcement Agencies (Reaves and Hickman, 2002).[15]

For each characteristic, we define a binary indicator equal to one if the value is above the median and zero otherwise. Figure A5 shows substantial apparent heterogeneity, especially with respect to the male-to-female unemployment ratio. While our analysis in previous sections suggests that parallel trends are plausible when comparing treated states to never-treated or not-yet-treated states, caution is warranted when exploring heterogeneous effects by baseline characteristics. Splitting the sample raises concerns that states above and below the median of each characteristic may have followed different underlying trends in IPH. Indeed, once we control for group-specific linear trends—by allowing states above and below the median of each characteristic to follow their own linear time trends—in Figure A6, the apparent heterogeneity documented in Figure A5 disappears. We therefore find no support for heterogeneous impacts of NFS laws across states based on the proxies of gender inequality and economic resources in 1990.[16]

[15]The first measure proxies overall law enforcement capacity, reflecting the size and potential reach of police agencies, which may influence the capacity to enforce new laws. The second measure captures staffing specifically dedicated to frontline response activities, indicating how well-resourced agencies are to handle incidents requiring immediate intervention.

[16]We conduct a total of 24 heterogeneity tests (six outcomes × four baseline characteristics). Applying a Bonferroni correction at the 5% significance level requires p-values $\leq \frac{0.05}{24} = 0.0021$ to reject the null of no heterogeneity. Under this criterion, not even the difference for the impact on male-victim IPH among 35–49-year-olds is statistically significant (p-value = 0.007).

We also explore potential heterogeneity in the effects of NFS laws by local police resources in the year 2000 (data not available in 1990). Without controlling for group-specific trends, Figure A7 indicates modest differences, if any, in the estimated effects of NFS laws across states above and below the median for these policing measures, although none are statistically significant.[17] Once group-specific linear trends are included in Figure A8, these differences further attenuate, and confidence intervals widen substantially. Thus, we do not find evidence of heterogeneity in the impacts of NFS laws based on measured policing resources in the year 2000.

[17]We conduct a total of 12 heterogeneity tests (six outcomes × two baseline characteristics). Applying a Bonferroni correction at the 5% significance level requires p-values $\leq \frac{0.05}{12} = 0.0042$ to reject the null of no heterogeneity. Under this criterion, not even the differences for the impact on female-victim IPH among 50–70-year-olds are statistically significant (p-values = 0.029, 0.022).

7 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, which is formalized in the Online Appendix A3, an exogenous fraction of men—*stranglers*—engage in NFS, 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 our conceptual framework.

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 NFS 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 strangulation episodes, broader changes in reporting, behavioral adaptation by offenders, and other factors. We interpret our findings in light of both the model's predictions and additional potential channels not captured by the model.

8 Conclusion

Strangulation statutes are a relatively recent development in criminal justice, introduced to tackle NFS: a common and gendered form of intimate partner abuse, often occurring at the most dangerous stage of violence escalation, just prior to 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. These reduced-form estimates are consistent with mechanisms such as increased incapacitation of abusers and reductions in preemptive violence by victims of repeated abuse. Our analysis investigates the impact of NFS laws by codifying them as binary variables. Future research could expand this analysis by examining effects based on statutory severity—for example, differences in minimum or maximum prison sentences—which could provide additional policy-relevant insights.

Our study contributes to ongoing policy debates around NFS criminalization, how to prevent IPV, and IPH: it shows that adding NFS to the statutes as a stand-alone offense is a successful policy tool that reduces IPH substantially. Policymakers and practitioners can directly use these insights to design and scale up interventions that protect IPV victims at critical points in the escalation of abuse and potentially prevent lethal outcomes. This research also speaks to broader literatures on gender-based violence, deterrence, and legal protection mechanisms.

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Online 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)[18], 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
Sworn personnel per 100,000	BJS
Responding to calls per 100,000	BJS

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

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).

A2 Additional Descriptive Statistics and Robustness Checks

Table A2: Missing Data on Homicides

State	Year
District of Columbia	1996
District of Columbia	1998
District of Columbia	1999
District of Columbia	2000
District of Columbia	2008
District of Columbia	2012
Florida	1990
Iowa	1991
Kansas	1994
Kansas	1995
Kansas	1996
Kansas	1997
Kansas	1998
Kansas	1999
Maine	1991
Maine	1992
Montana	1993
Montana	1994
Montana	1996
New Hampshire	1997
Wisconsin	1998

Table A3: 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 A4: Mean Covariates in 1990 and 1999, and Mean Change

Variable	Eventually Treated	Never-Treated	Difference (SE)
Panel A: 1990 (Baseline)			
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 A5: 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 A6: 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 A7: 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.

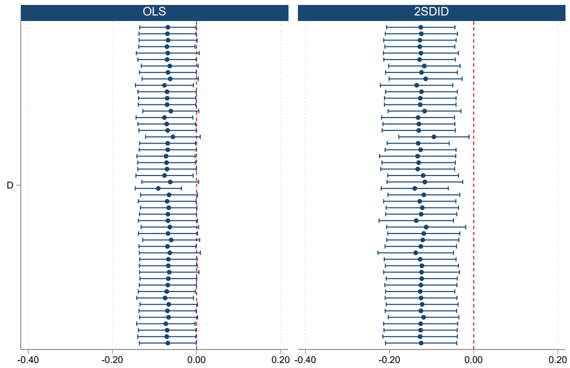
Table A8: Effects of NFS Law on Male-victim and Female-victim IPH (per 100,000) with baseline covariates interacted with linear time trends

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)		

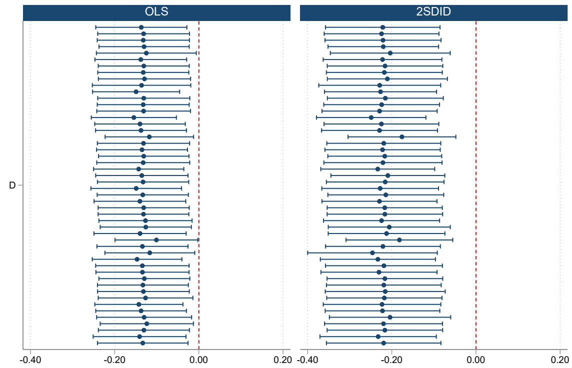
Notes: All regressions include state and year fixed effects, and baseline (1990) covariates (log income per capita, unemployment rate, poverty rate, and male-to-female unemployment ratio) interacted with linear time trends. $N = 1479$. Clustered standard errors (50 clusters). *p<0.1, **p<0.05, ***p<0.01.

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

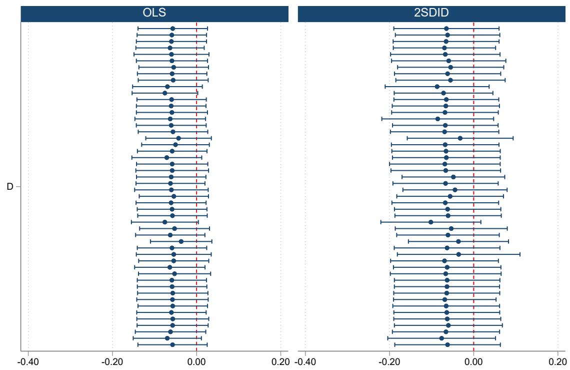
(a) Male-victim IPH 18-34



(b) Female-victim IPH 18-34

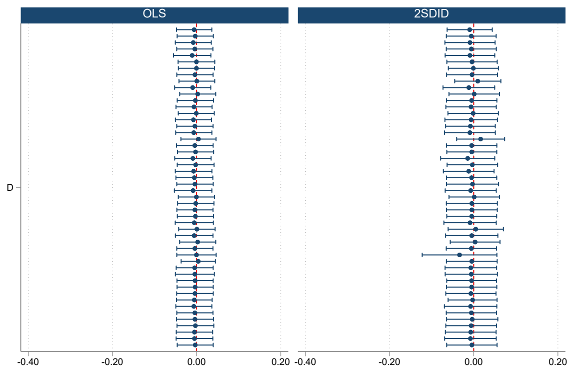


(c) Male-victim IPH 35-49

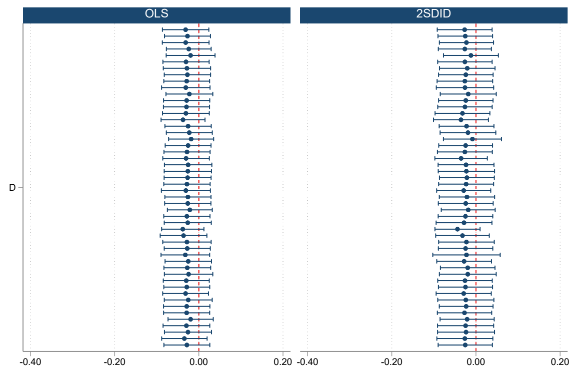


(d) Female-victim IPH 35-49

(e) Male-victim IPH 50-70



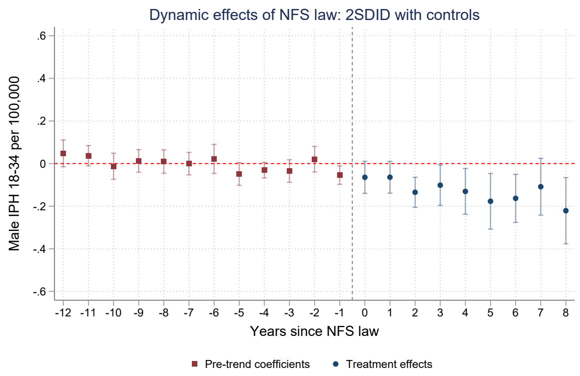
(f) Female-victim IPH 50-70



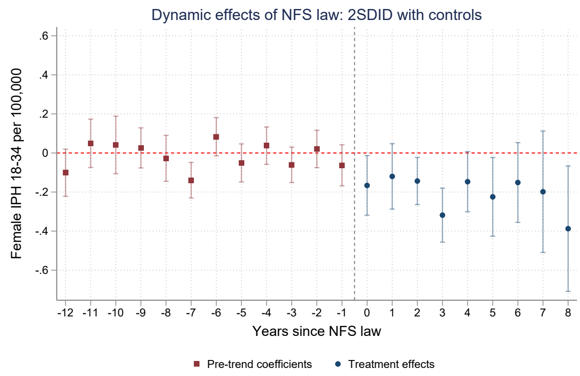
Note: Each panel replicates the analysis in Table A8 after dropping one state at a time.

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

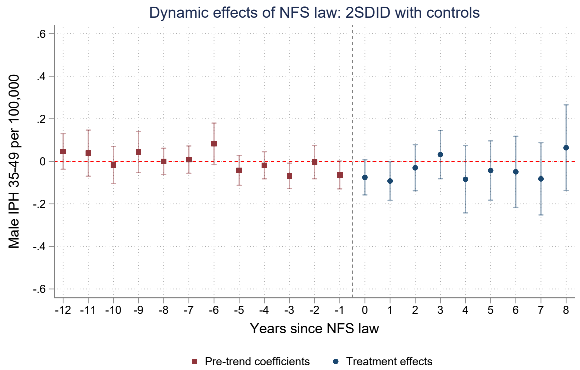
(a) Male-victim IPH 18-34



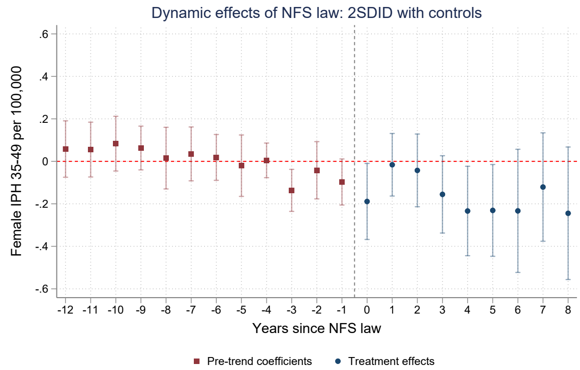
(b) Female-victim IPH 18-34



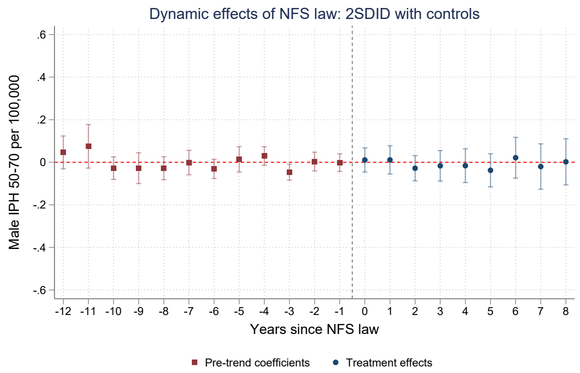
(c) Male-victim IPH 35-49



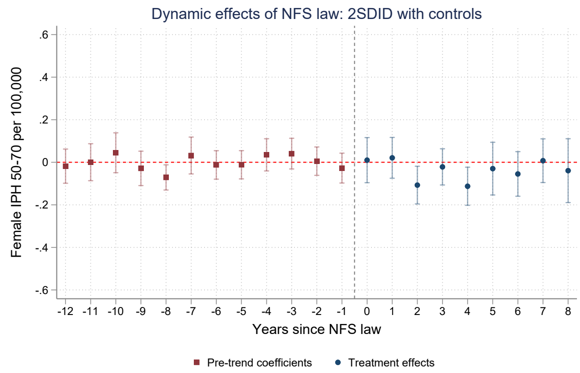
(d) Female-victim IPH 35-49



(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^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/not-yet-treated observations ($D_{st} = 0$). The event study estimates are based on 2SDID

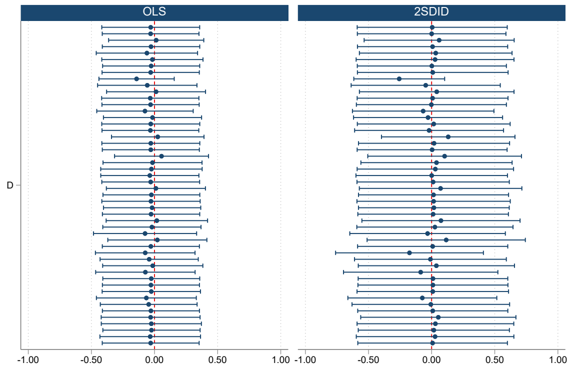
Table A9: Effects of NFS Law on Male-victim and Female-victim Homicides by Strangers (per 100,000)

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

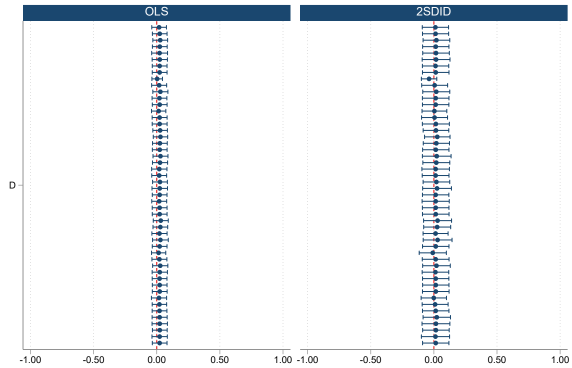
Notes: All regressions include state and year fixed effects. $N = 1479$. Clustered standard errors (50 clusters). *p<0.1, **p<0.05, ***p<0.01.

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

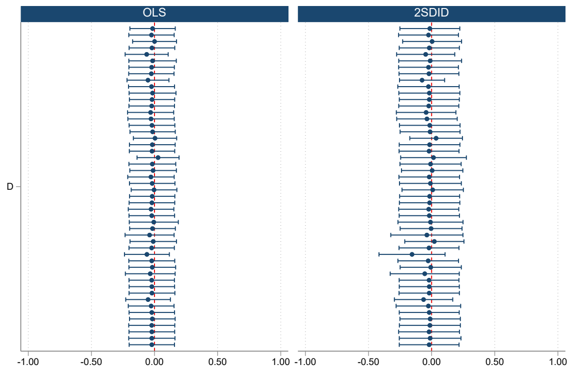
(a) Male-victim Homicide by strangers 18-34



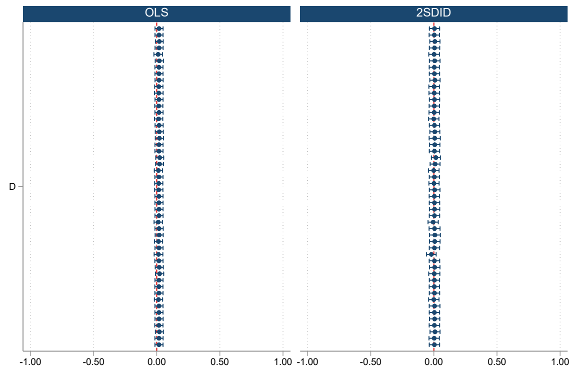
(b) Female-victim Homicide by strangers 18-34



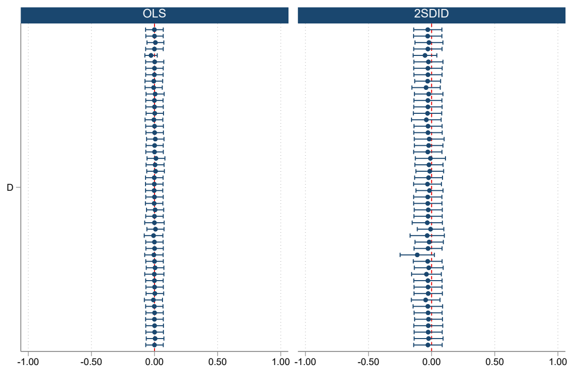
(c) Male-victim Homicide by strangers 35-49



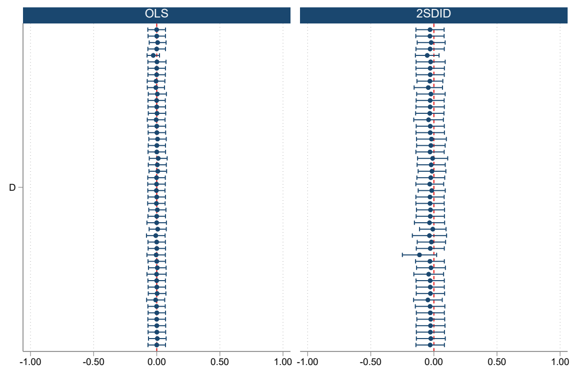
(d) Female-victim Homicide by strangers 35-49



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



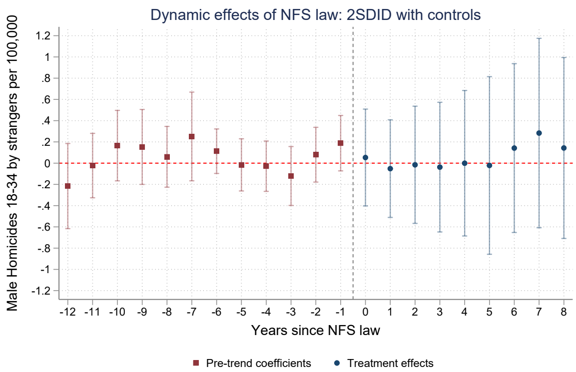
(f) Female-victim Homicide by strangers 50-70



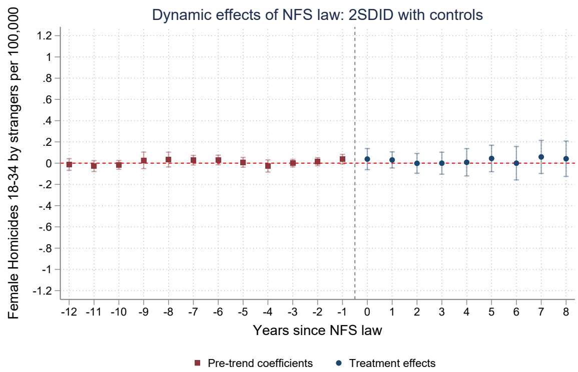
Note: Each panel replicates the analysis in Table 6 after dropping one state at a time.

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

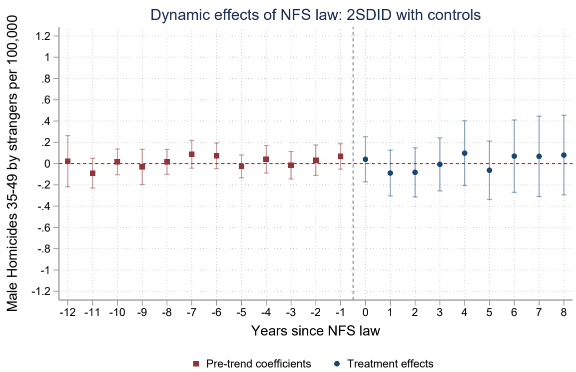
(a) Male-victim IPH 18-34



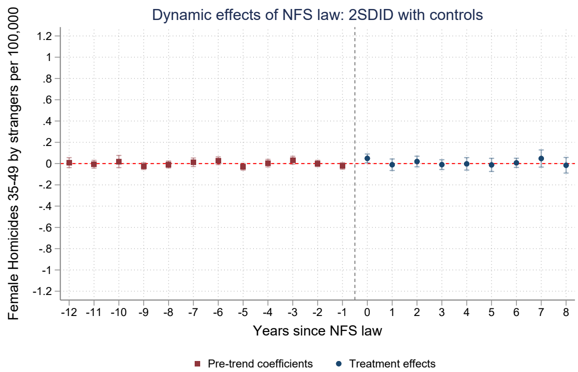
(b) Female-victim IPH 18-34



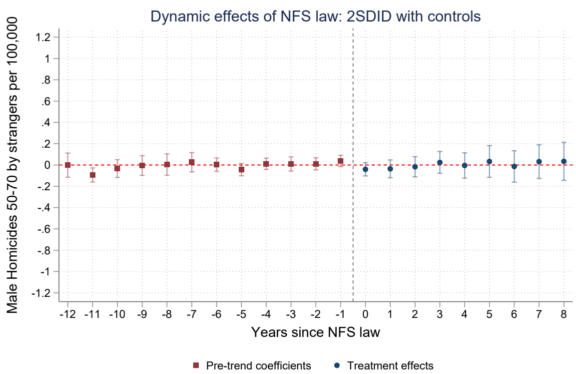
(c) Male-victim IPH 35-49



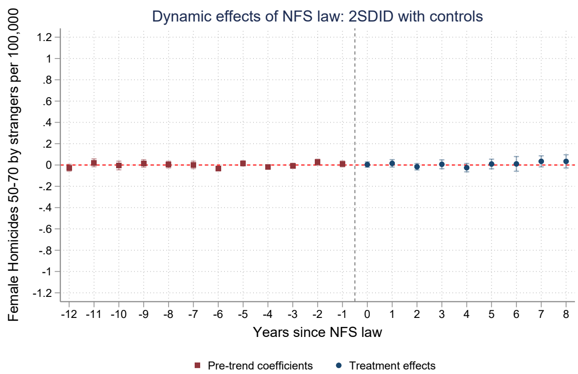
(d) Female-victim IPH 35-49



(e) Male-victim IPH 50-70



(f) Female-victim IPH 50-70

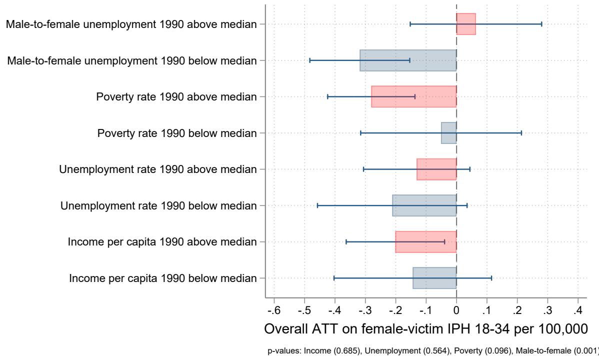


Note: The event study estimates are based on 2SDID estimates by including the event-time indicators D_{st}^{k} as treatment variables in the second stage. State fixed effects, year fixed effects and the coefficients on covariates for the baseline controls interacted with a time trend are estimated in the first stage for the sample of untreated/not-yet-treated 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).*

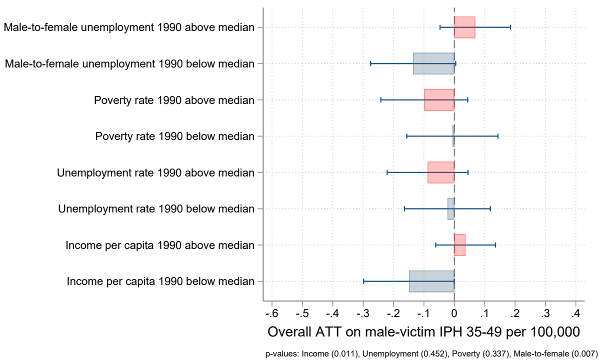
Figure A5: Heterogeneity Analysis by Gender Inequality and State Resources at baseline (1990)

(a) Male-victim IPH 18-34

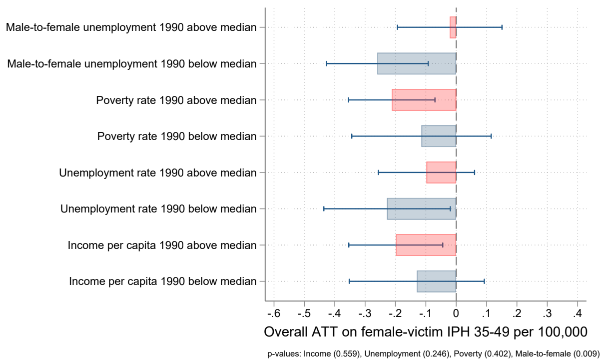
(b) Female-victim IPH 18-34



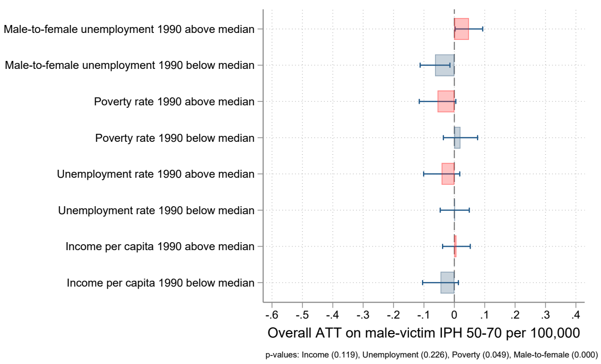
(c) Male-victim IPH 35-49



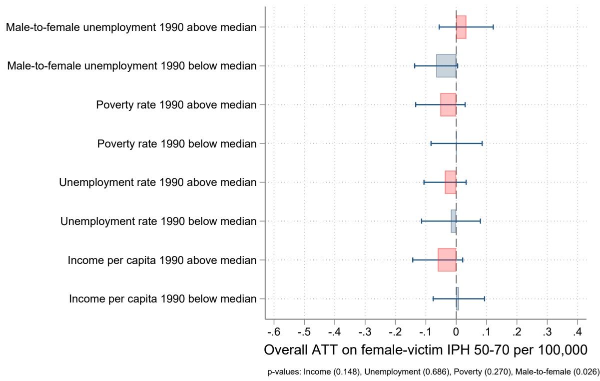
(d) Female-victim IPH 35-49



(e) Male-victim IPH 50-70



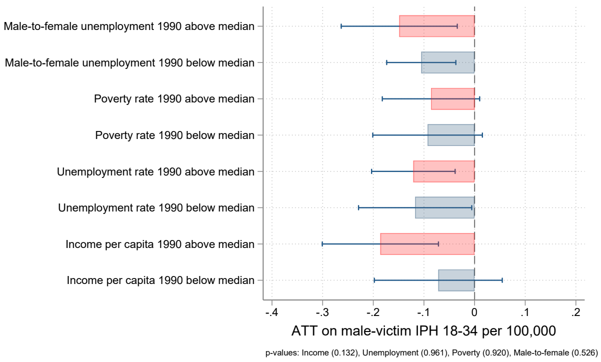
(f) Female-victim IPH 50-70



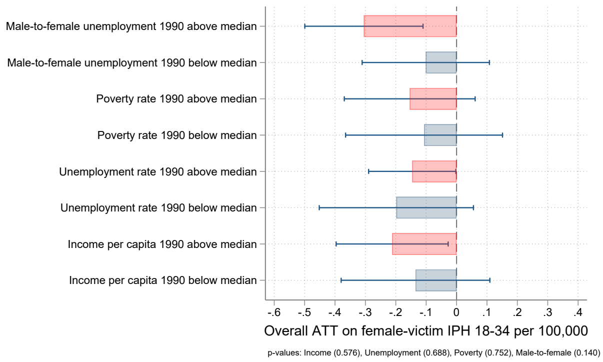
Note: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable D_{st} and its interaction with the baseline characteristic X_s (i.e., $D_{st} \times X_s$) as regressors in the second stage. State and year fixed effects are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{st} = 0$). Estimation is conducted jointly using the GMM framework proposed by Gardner et al. (2025), implemented via the `did2s` *Stata package developed by Butts (2021).*

Figure A6: Heterogeneity Analysis by Gender Inequality and State Resources at baseline (1990) controlling for group-specific linear trends

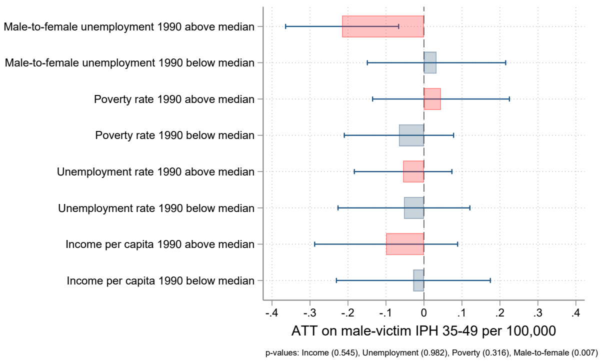
(a) Male-victim IPH 18-34



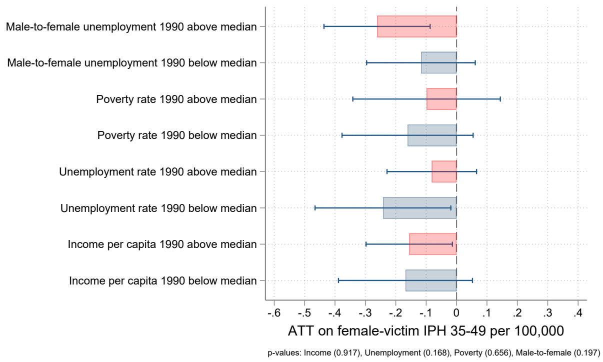
(b) Female-victim IPH 18-34



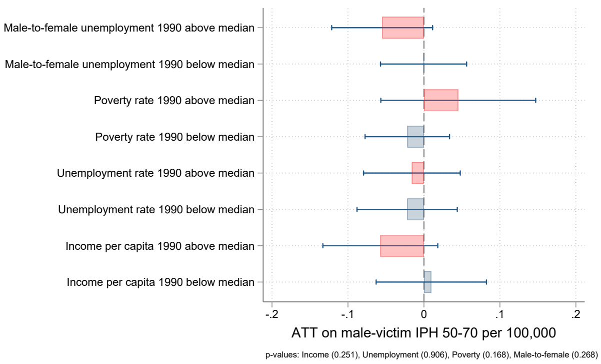
(c) Male-victim IPH 35-49



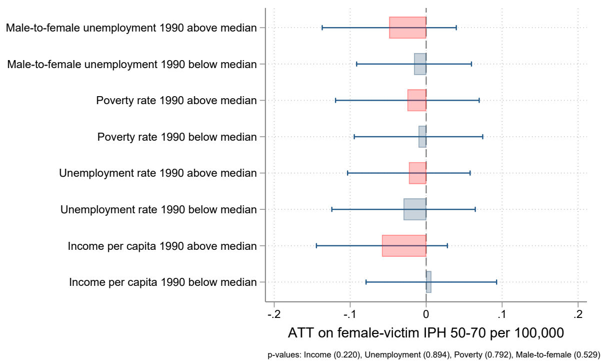
(d) Female-victim IPH 35-49



(e) Male-victim IPH 50-70



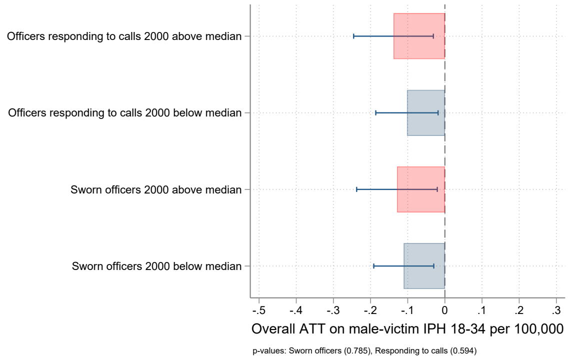
(f) Female-victim IPH 50-70



Note: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable D_{st} and its interaction with the baseline characteristic X_s (i.e., $D_{st} \times X_s$) as regressors in the second stage. The coefficients on the control variable $X_s \times t$, as well as state and year fixed effects, are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{st} = 0$). Estimation is conducted jointly using the GMM framework proposed by Gardner et al. (2025), implemented via the `did2s` Stata package developed by Butts (2021).

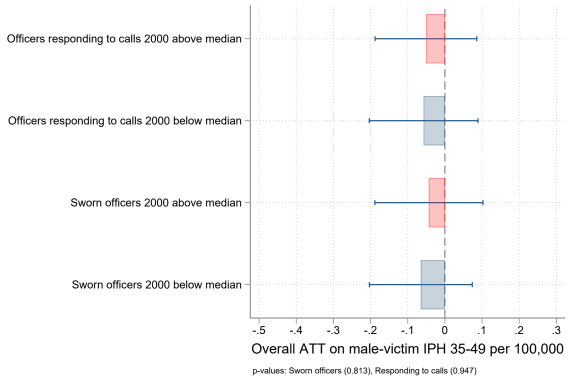
Figure A7: Heterogeneity Analysis by Local Police Resources at baseline (2000)

(a) Male-victim IPH 18-34

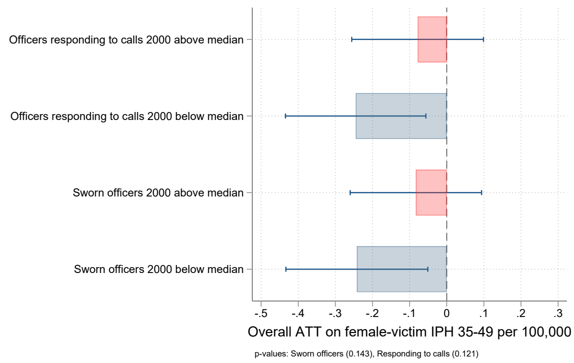


(b) Female-victim IPH 18-34

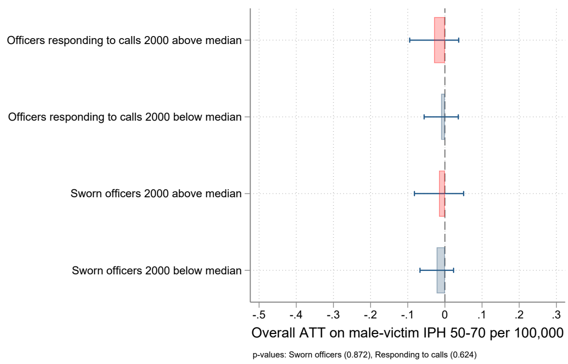
(c) Male-victim IPH 35-49



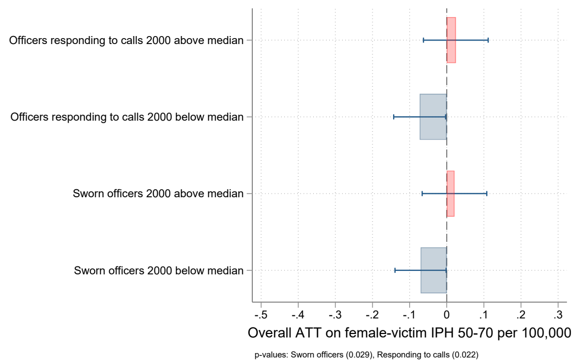
(d) Female-victim IPH 35-49



(e) Male-victim IPH 50-70



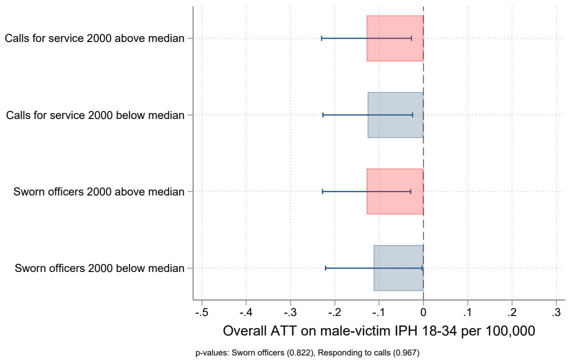
(f) Female-victim IPH 50-70



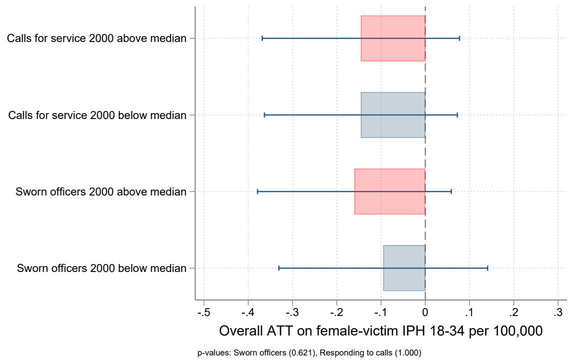
Note: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable D_{st} and its interaction with the baseline characteristic X_s (i.e., $D_{st} \times X_s$) as regressors in the second stage. State and year fixed effects are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{st} = 0$). Estimation is conducted jointly using the GMM framework proposed by Gardner et al. (2025), implemented via the `did2s` Stata package developed by Butts (2021).

Figure A8: Heterogeneity Analysis by Local Police Resources at baseline (2000) controlling for group-specific linear trends

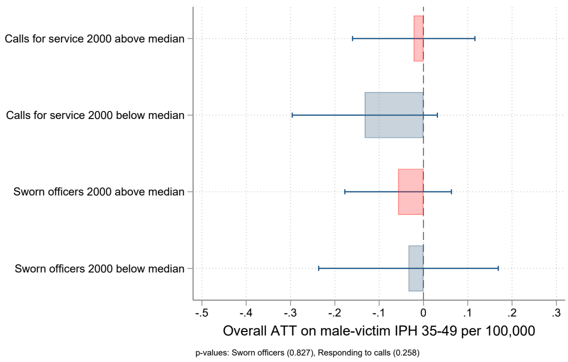
(a) Male-victim IPH 18-34



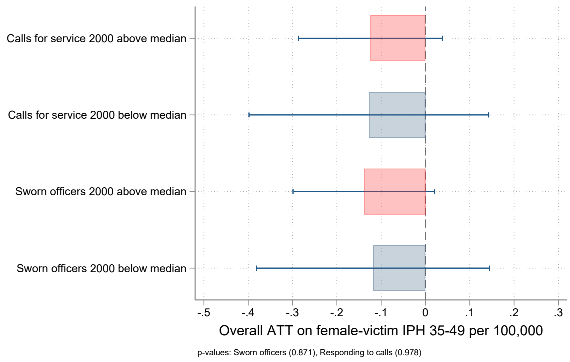
(b) Female-victim IPH 18-34



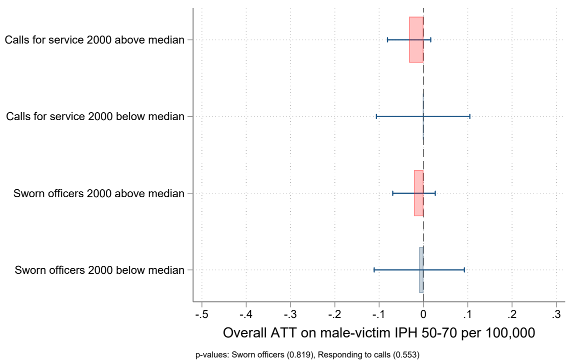
(c) Male-victim IPH 35-49



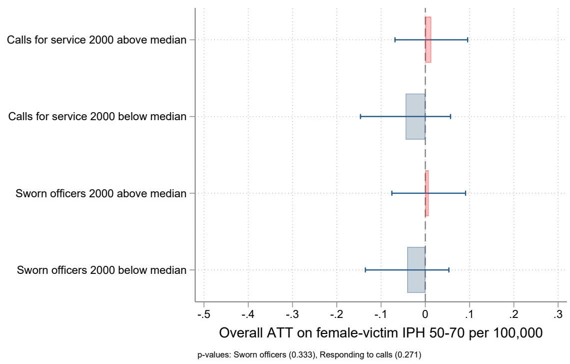
(d) Female-victim IPH 35-49



(e) Male-victim IPH 50-70



(f) Female-victim IPH 50-70



Note: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable D_{st} and its interaction with the baseline characteristic X_s (i.e., $D_{st} \times X_s$) as regressors in the second stage. The coefficients on the control variable $X_s \times t$, as well as state and year fixed effects, are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{st} = 0$). Estimation is conducted jointly using the GMM framework proposed by Gardner et al. (2025), implemented via the `did2s` *Stata package developed by Butts (2021).*

A3 A Stylized Model

This section 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 A10 summarises the expected utility of each option:

Table A10: 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:

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

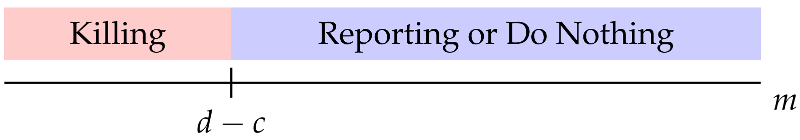
2. **Reduction in female-victim IPH**. Reporting leads to incapacitation of abusive partners, thereby lowering the risk of subsequent fatal violence against women:

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

Thus, NFS laws reduce male-victim IPH through *substitution* (fewer preemptive killings) and female-victim IPH through *incapacitation* (removal of dangerous partners from the victim's home). The magnitudes of these effects depend on the distribution F and on the relative costs d and c. Figure A9 illustrates these mechanisms graphically:

Figure A9: Women's Choices Before and After NFS Law

(a) Without NFS Law



(b) With NFS Law