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Disrupting Violence, Protecting Lives: Strangulation Laws and Intimate Partner Homicides*

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Abstract

Non-fatal strangulation (NFS) is a dangerous form of intimate partner violence (IPV) and a strong predictor of homicide. We collect information on state NFS statutes and link them to FBI Supplementary Homicide Reports, 1990–2019, to estimate their causal effects on intimate partner homicide (IPH) rates. Using the two-stage difference-in-differences estimator (Gardner et al., 2025), which accommodates staggered adoption and heterogeneity, we find that NFS laws reduce female-victim IPH by 14% and male-victim IPH by 27% among those aged 18–49, with no detectable effects for those aged 50–70 or for homicides committed by strangers. Event-study profiles show flat pre-trends and sustained declines following enactment of NFS laws. Using the National Incident-Based Reporting System data, we estimate that NFS laws increase the share of IPV incidents classified as aggravated assaults—especially when the victim is a woman—and increase arrests conditional on IPV aggravated assaults, providing a two-step mechanism by which NFS laws disrupt the escalation of violence and protect lives.

Keywords: Intimate Partner Violence; Abuse; Gender; Two-Stage Difference-in-Differences; Criminal Justice Policy; IPV Aggravated Assaults; Arrests.

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

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1 Introduction

Intimate partner violence (IPV) is a pervasive and devastating social, economic, and public health problem (Adams-Prassl et al., 2023; Adams et al., 2024). In the United States, approximately one in three women who are murdered are killed by an intimate partner (Smith, 2022; Black et al., 2023). Non-fatal strangulation (NFS), in particular, is a critical warning sign: it represents an escalation in violence (Thomas et al., 2013; Patch et al., 2018) and is among the strongest predictors of intimate partner femicide (Glass et al., 2008).

Despite its severity, NFS often leaves no visible injury and was historically treated as a simple assault—if documented at all—prior to the adoption of NFS statutes. This legal vacuum, in which the act was not formally acknowledged or defined as a serious crime, likely came at the cost of lives; for example, see the intimate partner homicides of Diana Gonzalez in California in 2010 and Monica Weber-Jeter in Ohio in 2014 (Section 2). Missouri became the first state to recognize "choking or strangulation" as a serious offense in 2000 (HB1677). North Carolina, Nebraska, and Oregon followed in 2004. By 2019, 47 jurisdictions (46 states + D.C.) had enacted NFS statutes,[1] and these laws explicitly defined and criminalized the act. By recognizing NFS as a distinct and serious offense, these reforms are expected to expand arrest and prosecution options available to law enforcement (California District Attorneys Association, 2020).

We compile a new dataset on the timing of NFS statute adoption across U.S. jurisdictions and link it to the FBI Supplementary Homicide Reports (SHR) (1990–2019) and the National Incident-Based Reporting System (NIBRS) (1991–2019). Leveraging variation in when states criminalized NFS, we use the SHR to estimate the effects of these laws on intimate partner homicides, disaggregated by the sex and age of the victim. Using NIBRS, we then examine potential mechanisms through two intermediate outcomes: classification of IPV incidents as aggravated assaults, and arrests for IPV aggravated assaults.

[1] By 2025, all but one (South Carolina) had done so.

We evaluate whether the adoption of NFS statutes reduces intimate partner homicides and whether, consistent with an enforcement channel, these laws increase the share of IPV incidents classified as aggravated assaults, and the likelihood of arrest conditional on aggravated IPV assault. Examining these intermediate outcomes allows us to trace a pathway from legal recognition to heightened enforcement and, ultimately, lower intimate partner homicide (IPH) rates.

Our empirical strategy exploits the staggered adoption of NFS laws across states and relies on the two-stage difference-in-differences estimator (2SDID) of Gardner et al. (2025), which addresses bias from heterogeneous treatment effects under staggered timing.[2] We find that NFS laws led to substantial reductions in IPH rates among adults aged 18–49. In states that enacted NFS laws, male-victim IPH declined by 27% (from 0.337 to 0.247 per 100,000 men), and female-victim IPH declined by 14% (from 1.221 to 1.052 per 100,000 women). These effects are robust to the inclusion of baseline state covariates interacted with linear time trends.

Estimated effects for older adults (ages 50–70) are smaller in magnitude and statistically insignificant. We also examine heterogeneity by baseline gender inequality and economic resources (both measured in 1990), as well as policing resources (measured in 2000, the earliest available), and find no systematic differences across these dimensions. A back-of-the-envelope calculation suggests that, through 2019, these laws prevented approximately 1,029 female and 547 male IPHs among adults ages 18–49.

Event-study results based on 2SDID estimates support the parallel trends assumption. A sensitivity analysis of potential violations of parallel trends (Rambachan and Roth, 2023) further reinforces the interpretation of our estimates as average treatment effects on the treated. In addition, because NFS signals escalation and coercive control within a relationship (Thomas et al., 2013; Patch et al., 2018), homicides committed by strangers—where such dynamics are absent—provide a natural placebo test. Reassuringly, we find no effect of NFS laws on homicide rates involving strangers.

How do NFS laws reduce intimate partner homicides? We present evidence con-

[2]This imputation-based approach has been effectively applied to other staggered policy reforms (e.g., Dan Han, 2023; Smart et al., 2024).

sistent with a two-step mechanism. First, legal salience increases: the share of IPV incidents classified as aggravated assault rises by 5.5 percentage points among female victims ages 18–49 (from 7.8% to 13.3%), the group most exposed to NFS, which is overwhelmingly perpetrated by men (e.g., Sorenson et al., 2014; Parekh et al., 2024). Second, conditional on aggravated classification, enforcement strengthens: arrests increase by 12 percentage points for female victims and by 15 percentage points for male victims (from 48% to 60% and from 47% to 62%, respectively).

Our identification strategy yields causal estimates for IPH and enforcement outcomes, and the combined patterns point to both the incapacitation of abusive partners and reduced reliance on lethal self-defense (Aizer and Dal Bo, 2009; Miller and Segal, 2018). This early-enforcement mechanism aligns with Miller and Segal (2018), who show that higher shares of female police officers increase victim reporting and reduce IPH. In our context, however, statutory clarity itself enables the classification of IPV incidents as aggravated assaults and thus serves as the key trigger for heightened enforcement—whether reporting originates from victims or from third parties. These channels do not require that the prevented intimate partner homicides would themselves involve strangulation; rather, NFS laws target a high risk abuse thereby reducing escalation to lethal outcomes more broadly.

NFS laws thus emerge as an effective, scalable policy lever that targets a common and highly predictive form of abuse. Our findings offer actionable guidance for policymakers seeking to reduce gender-based violence and its deadliest consequences. Yet globally, many jurisdictions still lack NFS-specific statutes. For example, the Council of Europe's Istanbul Convention on preventing and combating violence against women—signed in 2011 and ratified in 2014—does not explicitly reference strangulation, suffocation, or choking (Council of Europe, 2011). England and Wales introduced a specific offence only in 2022 (Ministry of Justice and The Rt Hon Victoria Atkins MP, 2022); Northern Ireland and Ireland in 2023; Victoria (Australia) in 2024; and in Scotland, legislation remains under debate as of 2025. Many other countries, including France, Italy, and Spain, still have no standalone offence addressing NFS.

Our analysis contributes to three strands of research. First, it advances the growing evidence on the effects of criminal-justice interventions on IPV. Second, it sheds new light on gendered patterns in violent crime and homicide. Third, it informs broader debates on gender inequality and relationship dynamics by showing that legislation targeting a gendered form of IPV—namely NFS—can reduce IPH rates of both women and men, and clarifies the channels through which these reductions occur.

Aizer and Dal Bo (2009) find that no-drop prosecution policies significantly reduce male-victim IPH, and Miller and Segal (2018) show that increasing the share of female police officers reduces both male- and female-victim IPH. We extend this line of work by focusing on NFS—an overlooked yet highly predictive form of IPV.

Our findings complement and extend research on legal and institutional changes that shape abusive relationship dynamics: compulsory schooling reforms in Turkey affect IPV (Erten and Keskin, 2022); evidence on stricter arrest policies in the United States (Chin and Cunningham, 2019); abortion restrictions raise IPV reports to law enforcement (Dave et al., 2025); easing access to divorce reduces domestic violence (Brassiolo, 2016); domestic violence arrests generate incapacitation and deterrence effects (Amaral et al., 2023); and pressing charges reduces recidivism (Black et al., 2023).

More broadly, related work examines economic and institutional determinants of IPV, spanning factors from the gender wage gap (Aizer, 2010) to labor-market shocks and unemployment benefits (Bhalotra et al., 2025). Despite this progress, credible evidence on which laws and policies effectively reduce IPV remains limited (Adams-Prassl et al., 2023; Adams et al., 2024). We help fill this gap by identifying the effects of NFS statutes on IPH and the enforcement channel through which these effects operate.

The paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 outlines the empirical strategy, and Section 5 provides descriptive statistics. Section 6 reports the effects of NFS laws on intimate partner homicides. Section 7 examines the enforcement channel by analyzing impacts on aggravated-assault classification and arrests. Section 8 concludes.

2 Institutional Background

2.1 Non-Fatal Strangulation

Strangulation—the application of external pressure to the neck, by any means, that impedes airflow, blood flow, or both—can be fatal or non-fatal.[3] In the US, data on non-fatal strangulation (NFS) are not collected in nationally representative surveys. However, the 2016/17 National Intimate Partner and Sexual Violence Survey reports that 16.2% of women and 4.1% of men have been "choked or suffocated" by an intimate partner during their lifetime (Leemis et al., 2022).[4] Among IPV victims, 27–80% of women report having been strangled by a partner during their lifetime; the wide range reflects heterogeneous data sources, including domestic violence hotlines, shelter intake samples, and clinical settings (McQuown et al., 2016; Stellpflug et al., 2022).[5] Fatal strangulation (including asphyxiation) by an intimate partner is estimated at 4.1% for women ages 18–49 and 0.2% for men ages 18–49. These patterns align with findings from emergency medicine and forensic science: strangulation is a distinct form of violence that disproportionately affects women (Sorenson et al., 2014; Parekh et al., 2024).

While strangulation can cause death within 1–5 minutes, NFS has severe and lasting health consequences. Loss of consciousness can occur within 5–10 seconds, and survivors face risks of hypoxic brain injury as well as neck, laryngeal, and vascular trauma, with associated neurological and physiological sequelae (Stellpflug et al., 2022). Commonly documented symptoms include voice changes (reported in 50% of cases), memory loss, bowel or bladder incontinence when accompanied by loss of consciousness, and agitation or the appearance of intoxication due to cerebral hypoxia. Yet many of these signs are missed or misattributed, and up to 50% of cases show no

[3] There is no such thing as "attempted strangulation": the act is complete once pressure to the neck obstructs blood flow and/or airflow (California District Attorneys Association, 2020).

[4] "Choking" and "suffocation" differ from strangulation: choking typically involves an internal airway blockage (often food), and suffocation is the external obstruction of airflow to and from the lungs.

[5] Patch et al. (2021) notes that data from health-care settings may be subject to selection biases—for example, overestimation if those with more severe injuries are more likely to seek medical care, or underestimation if fear of retaliation discourages victims from seeking assistance.

visible injuries (California District Attorneys Association, 2020).

Historically, NFS was treated as a simple assault, if recorded at all. Inadequate statutory tools for classifying NFS as a serious violent offense impeded arrest and prosecution. As Gael Strack, former prosecutor, co-founder of Alliance for HOPE International, and a leading U.S. expert on NFS, explains:

> "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."

Because NFS signals escalating violence and coercive control (Thomas et al., 2013; Patch et al., 2018), this legal vacuum likely carried real costs in lives lost—among victims and, ultimately, among offenders as well. Several widely documented cases illustrate the limitations of the law prior to the adoption of NFS statutes.

In 2010 in California, Diana Gonzalez was strangled unconscious by her common-law husband. Although he was arrested, no charges were filed. After his release, he fatally stabbed her on the campus of San Diego City College. In March 2014 in Ohio, Monica Weber-Jeter was nonfatally strangled by her husband. Despite her police report for non-fatal strangulation, he pled no contest to domestic violence and served only 11 days in jail. A few months later, he stabbed her 28 times, and she died from her injuries about a month afterward. Men murdered by intimate partners often have histories of abusing their partners. In 2004, Thomia Hunter stabbed her partner in the leg, severing his femoral artery, while he was choking, beating, and attacking her with a knife in their apartment.

2.2 NFS Statutory Classification in the United States

Strangulation statutes are a relatively recent development in criminal justice. The first major legal shift occurred in 2000, when Missouri (HB1677) recognized "choking or strangulation" as a serious criminal offense. In 2004, North Carolina, Nebraska, and Oregon followed. Over the next two decades, nearly all states enacted NFS laws: by 2019, 47 U.S. jurisdictions (46 states plus D.C.) had done so. Missouri and North Carolina (H1354) recognized strangulation as a criminal offense but did not formally define the act. Nebraska, Oregon, and the remaining jurisdictions added statutory definitions based on the effects (impeding breathing or blood circulation), the means (applying pressure to the throat or neck), or both.

Figure 1a reproduces an excerpt from Nebraska's LB943 (2004), where we highlight the statutory language defining the act of strangulation by its effects. Figure 1b shows an excerpt from Pennsylvania's HB1581 (2016), where we highlight the language defining the act by its means.[6]

Figure 1: Excerpts from NFS bills

28-101. Sections 28-101 to 28-1350 and sections 2 and 3 of this act shall be known and may be cited as the Nebraska Criminal Code.

Sec. 2. (1) A person commits the offense of strangulation if the person knowingly or intentionally impedes the normal breathing or circulation of the blood of another person by applying pressure on the throat or neck of the other person.

(a) Nebraska LB943 (2004)

Section 1. Title 18 of the Pennsylvania Consolidated Statutes is amended by adding a section to read:

§ 2718. Strangulation.

(a) Offense defined.--A person commits the offense of strangulation if the person knowingly or intentionally impedes the breathing or circulation of the blood of another person by:

(1) applying pressure to the throat or neck; or

(b) Pennsylvania HB1581 (2016)

By 2025, all but one state (South Carolina) had enacted NFS laws.[7]

Recognizing and defining strangulation as a criminal offense is expected to expand arrest and prosecution options (California District Attorneys Association, 2020), disrupting the pathway from NFS to IPH. These statutes address the pre-reform pattern in which NFS was either charged as simple assault or not recorded at all, with potentially deadly consequences.

[6]The underline text in the Figure is added to the statutes by the bills.

[7]See Table A1.

Testimony in state legislative hearings underscored the legal gaps NFS statutes were intended to fill. In North Dakota's 2007 hearings on SB2185, a retired police chief urged legislators to "specifically add strangulation" to strengthen protections for victims.[8] A state's attorney described cases in which victims were nearly killed by NFS but offenders could be charged only with simple assault because the victim had only a red mark—or no visible injury—with a maximum penalty of 30 days in jail.[9] In Montana's 2017 hearings on SB153, advocates similarly emphasized that recognizing strangulation "will help to save lives."[10]

[8] Dan Draovitch, retired police chief:

> "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."

[9] "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 had only a red mark on their neck and no other visible injury? ... The maximum penalty for this offense is only 30 days in jail."

[10] "Quite simply, SB 153 will help to save lives."

3 Main Data Sources and Variables

3.1 NFS Laws Taxonomy: Treatment Variable

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

We construct a new dataset through a two-step process. First, we manually review state legislative archives and proceedings. For each U.S. state through 2025, we identify the bill introducing an NFS offense, verify its legislative history, and record both the date it was signed by the governor and the date it became effective. Second, we validate these data with Legislative State Librarians at each state's Legislative Library or State Law Library.[11] Table A1 reports, for each state, the year the law was passed, the year it became effective, and the bill number.

Our treatment variable is a binary indicator equal to one from the year an NFS law became effective in a state onward. Figure 2 shows the staggered rollout of these statutes. Missouri was the first adopter in 2000, followed by Nebraska, North Carolina, and Oregon in 2004. The most recent adopters by 2019 were New Mexico (2018) and Kentucky (2019). Three jurisdictions—Maryland, Ohio, and Washington, D.C.—had not adopted NFS statutes by 2019 and serve as "never-treated" units in our main sample, which covers 1990–2019 to avoid COVID-related disruptions.[12] South Carolina remains the only state without an NFS law as of 2025 and is not included in our main sample.

[11] We are grateful to Legislative State Librarians across the United States for their assistance in validating the statutory histories.

[12] The imputation approach we use requires untreated or not-yet-treated observations to identify both state and year fixed effects. Because three states never adopted NFS laws by 2019, we have at least three untreated states contributing to identification of the year fixed effects. This matches the minimum in Smart et al. (2024), who truncate samples to avoid a single untreated unit driving counterfactual estimation.

Figure 2: Staggered implementation of NFS Laws

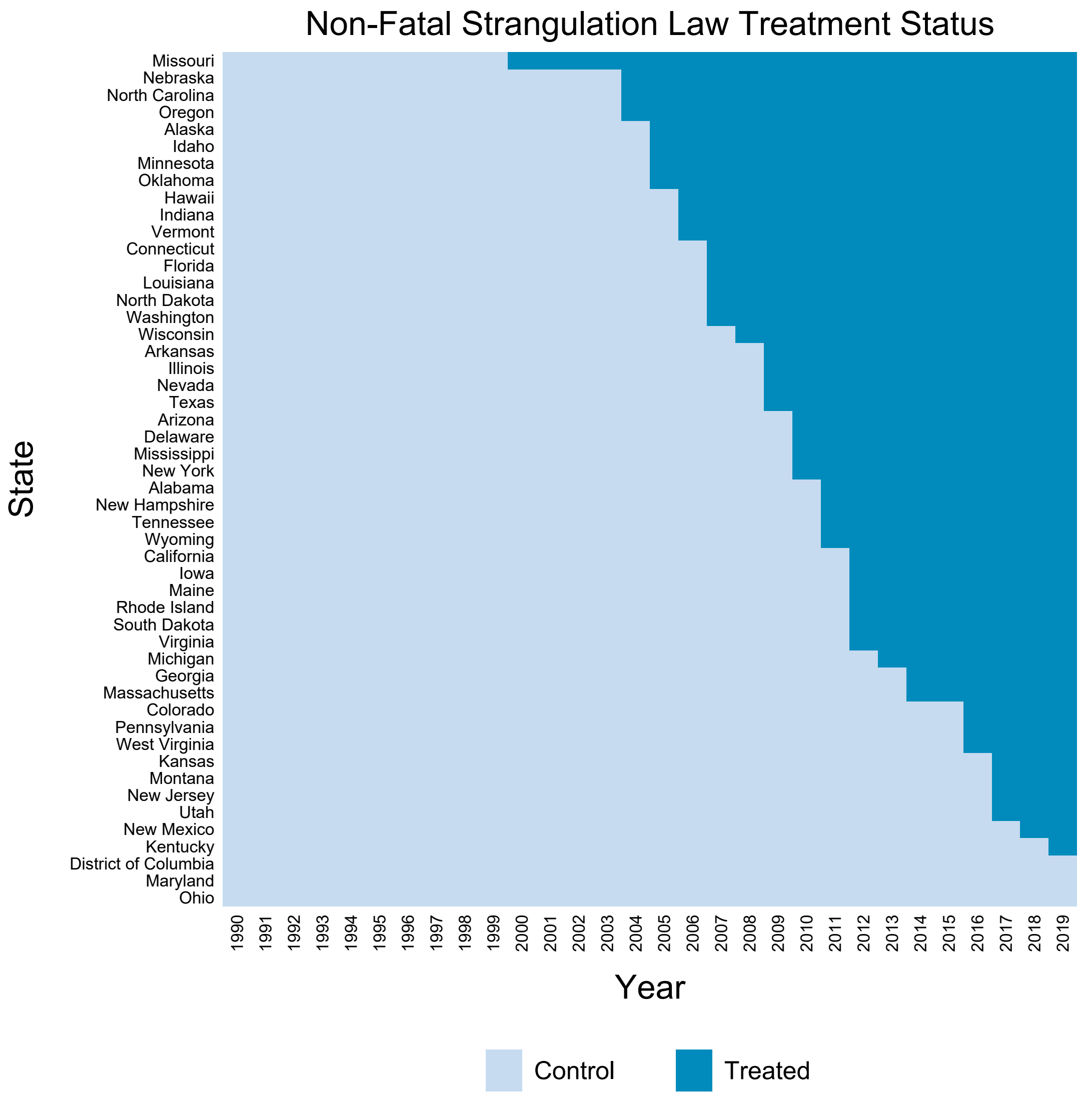


Table 1 reports the distribution of treatment cohorts by year of implementation, showing both the percentage of treated states and the share of the U.S. adult population (ages 18–70) covered by each cohort. As the table shows, cohort size varies substantially across years.

Table 1: 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: IPH and Placebo variables

Our main outcome variables are intimate partner homicide (IPH) rates, disaggregated by the victim's age group and sex for each state and year:

$$IPH_{d,s,t} = \frac{\text{Intimate Partner Homicides}_{d,s,t}}{\text{Population}_{d,s,t}} \times 100{,}000,$$

where Intimate Partner Homicides$_{d,s,t}$ denotes the number of victims in demographic group d (defined by age group and sex) killed by an intimate partner in state s and year t, and Population$_{d,s,t}$ is the corresponding population of that demographic group in state s and year t.[13]

Homicide data come from the FBI's Supplementary Homicide Reports (SHR), part of the Uniform Crime Reporting (UCR) system, as described in Fox and Swatt (2009).[14] The SHR is one of the most comprehensive sources of homicide data in the United States, providing detailed information on victim–offender relationships and on the age and sex of victims and offenders. The unit of reporting in the SHR is the homicide incident. To ensure accurate coding of victim–offender relationships, we focus on single-victim incidents and exclude cases with multiple offenders—retaining the vast majority of homicide incidents. Our analysis uses the victim-level file.

We define intimate partner (IP) relationships as current spouse, ex-spouse, boyfriend or girlfriend, and common-law spouse, following standard practice in the literature. Same-sex relationships are excluded due to their extremely small number among IP homicides. We stratify our analysis by two victim age groups: 18–49 and 50–70. While our primary outcome is the IPH rate, we also report complementary results using homicide counts.

Our dataset covers 50 jurisdictions (49 states and the District of Columbia) over 30 years (1990–2019), yielding 1,500 potential state-year observations. We exclude

[13] Online Appendix A2 provides additional details on data sources for population and other control variables.

[14] We obtained the dataset directly from James Alan Fox, who generously provided the 1976–2020 version in 2023.

imputed values from the SHR, following Chin and Cunningham (2019). Homicide reporting is missing for 21 state-year cells, resulting in a final sample of 1,479 observations.[15]

Furthermore, we use homicides committed by strangers as a falsification (placebo) test (Chin and Cunningham, 2019). Because NFS signals an escalation of violence and coercive control within IP relationships, stranger homicides should not be affected by NFS laws. We disaggregate stranger homicides by the victim's sex and age group, measure them at the state–year level, and express them per 100,000 male or female population in the same age ranges used for IPH. Our placebo variables are stranger homicide (SH) rates:

$$SH_{d,s,t} = \frac{\text{Stranger Homicides}_{d,s,t}}{\text{Population}_{d,s,t}} \times 100{,}000,$$

where Stranger Homicides$_{d,s,t}$ denotes the number of victims in demographic group d (defined by age group and sex) killed by strangers in state s and year t, and Population$_{d,s,t}$ is the corresponding demographic population. This placebo test provides an additional validity check on our identification strategy, discussed in Section 4.

3.3 IPV Incidents Data: Aggravated Assaults and Arrests

To analyze intermediate outcomes, we use the National Incident-Based Reporting System (NIBRS) from 1991 to 2019. NIBRS records incident-level data on crimes reported to police, including offense type (e.g., aggravated assault, simple assault, intimidation), victim and offender characteristics, their relationship, and arrest information.

To maintain consistency with our SHR homicide analysis, we focus on single-victim incidents with no multiple offenders, where the victim is an individual. We restrict attention to IPV incidents defined as aggravated assaults, intimidation, or sim-

[15] See Table A3. To assess whether missingness is related to NFS adoption, we regress an indicator for missing homicide reporting on treatment timing, controlling for state and year fixed effects, using both OLS and two-stage difference-in-differences (2SDID) estimators (Gardner et al., 2025), as discussed in Section 4. In both cases, the estimated effects are small and statistically indistinguishable from zero—TWFE: 0.0037 (SE = 0.0078); 2SDID: 0.0061 (SE = 0.0096)—suggesting that NFS rollout does not predict missingness and that missingness is plausibly unrelated to treatment.

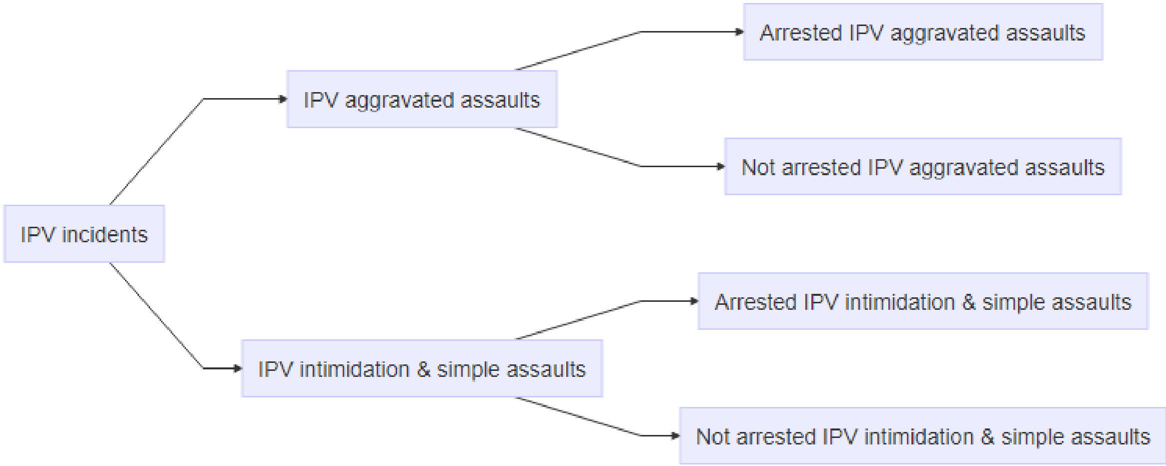
ple assaults committed by a spouse, ex-spouse, boyfriend or girlfriend, or common-law spouse, following the literature (e.g., Card and Dahl, 2011; Lin and Pursiainen, 2023) and the same IP definition used in our SHR analysis. As before, we analyze two victim age groups: 18–49 and 50–70.

For each state–year, we count these IPV incidents and construct two intermediate-outcome measures: (i) the fraction of IPV incidents classified as aggravated assaults (classification outcome), and (ii) the fraction of IPV aggravated-assault incidents associated with an arrest (enforcement outcome), both disaggregated by age group and victim sex.[16] These measures capture channels through which NFS laws may increase legal salience and enforcement, thereby disrupting the escalation of violent abuse. Because NIBRS does not contain a specific code for NFS, it is not possible to identify NFS incidents directly.

While NIBRS provides rich incident-level detail, coverage was limited in the early years and expanded gradually. Figure A1 shows that before 1996, fewer than ten states had at least one reporting agency. This number doubled by 2001, tripled by 2005, and reached the mid-40s by 2018–2019. Between 1991 and 2005—the period of fastest expansion—the mean and median number of reporting agencies fluctuated, but once coverage exceeded roughly 30 states (after 2005), both increased steadily. In contrast, the SHR receives reports from nearly all agencies nationwide across all states and D.C. (Fox and Swatt, 2009). For these reasons, and consistent with previous research (Pampel, Fred C and Williams, Kirk R, 2000; Jennings and Piquero, 2008; Aizer and Dal Bo, 2009; Cunningham et al., 2023; Garrett et al., 2017; Chin and Cunningham, 2019; Miller and Segal, 2018), we use the SHR as our source for homicide outcomes.

[16] In the vast majority of IPV incidents that result in arrest, only one individual—the offender—is arrested.

Figure 3: NIBRS data: From IPV incidents to Arrests



Consistent with the NIBRS coverage dynamics described above, missingness in these variables declines substantially over time—from roughly 34–37 states in 1999 to about 15–19 in 2009–2010, and to approximately 5–8 in 2019 (varying by measure and sex–age group). Several large states (AK, CA, FL, NJ, NY) still do not report in 2019, and DC, PA, and WY are missing in some sex–age groups. As a result, regression samples for these IPV intermediate outcomes range from 635 to 732 state–year observations.[17]

[17] See Table A5. To assess whether missingness is related to NFS adoption, we regress a missingness indicator for the classification and enforcement ratios on treatment timing, controlling for state and year fixed effects (TWFE), using both OLS and the two-stage difference-in-differences estimator (Gardner et al., 2025), the estimators used in this paper (see Section 4). In all cases, the estimated effects are small and statistically indistinguishable from zero, indicating that NFS rollout does not predict missingness of the intermediate outcomes; see Table A6.

4 Empirical Strategy

4.1 Identification of Overall ATT estimates

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

$$Y_{d,s,t} = \beta_d D_{s,t} + \alpha_{d,s} + \gamma_{d,t} + \varepsilon_{d,s,t}, \tag{4.1}$$

where $Y_{d,s,t}$ denotes the final outcome, placebo, or intermediate outcome of interest for demographic group d (defined by the victim's age group and sex) in state s and year t. The variable $D_{s,t}$ 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 $\alpha_{d,s}$ absorb time-invariant characteristics of states that may differentially affect each demographic group, while year fixed effects $\gamma_{d,t}$ capture time-varying nationwide shocks that may differentially affect each demographic group. Standard errors are clustered at the state level.

If treatment effects for demographic group d are constant across states and over time, then OLS estimation of equation (4.1) yields a consistent estimate for β_d under correct specification, parallel trends and no anticipation (Jonathan Roth and Pedro H.C. Sant'Anna and Alyssa Bilinski and John Poe, 2023).[18] However, as shown by de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021) and others, OLS estimation is problematic when treatment effects vary across states and over time. As explained in Gardner et al. (2025), the TWFE regression model can be rewritten as:

$$Y_{d,s,t} = \beta_d D_{s,t} + \alpha_{d,s} + \gamma_{d,t} + u_{d,s,t}, \tag{4.2}$$

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

[18] An implicit assumption is SUTVA.

TWFE via Two-Stage (2SDID) Estimation. To address the limitations of the OLS estimator under treatment-effect heterogeneity, 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 using only untreated or not-yet-treated observations ($D_{s,t} = 0$) in the 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 $D_{s,t}$. This procedure yields a consistent estimate of $\mathbb{E}[\beta_{d,s,t} \mid D_{s,t} = 1]$, provided that the parallel trends assumption holds, treatment is not anticipated, and the untreated potential outcome is correctly specified.

Under this procedure, the observed mean outcome for treated observations, $\mathbb{E}[Y_{d,s,t}(1) \mid D_{s,t} = 1]$, is simply the average of the actual outcome $Y_{d,s,t}$ among treated observations ($D_{s,t} = 1$). The counterfactual mean, $\mathbb{E}[Y_{d,s,t}(0) \mid D_{s,t} = 1]$, is computed as the average of the predicted outcome $\widehat{Y}_{d,s,t}$—based on state and year fixed effects estimated from untreated/not-yet-treated observations ($D_{s,t} = 0$)—evaluated for treated observations ($D_{s,t} = 1$). The overall ATT for demographic group d, β_d, is therefore estimated as the sample counterpart of:

$$\mathbb{E}[\beta_{d,s,t} \mid D_{s,t} = 1] = \mathbb{E}[Y_{d,s,t}(1) \mid D_{s,t} = 1] - \mathbb{E}[Y_{d,s,t}(0) \mid D_{s,t} = 1].$$

The 2SDID estimator is robust in small samples (particularly when some cohorts have few observations), and delivers point estimates numerically equivalent to those of Borusyak et al. (2024), while providing improved finite-sample inference through a a GMM-based procedure (Gardner et al., 2025).[19]

[19]We implement the 2SDID estimator using the `did2s` Stata package developed by Butts (2021), which has been used in previous research (e.g., Dan Han, 2023; Smart et al., 2024). An R package is also available (Butts and Gardner, 2022). As shown by Gardner et al. (2025), the 2SDID approach easily accommodates the inclusion of control variables: the first stage estimates state fixed effects, year fixed effects, and the coefficients on control variables using only untreated/not-yet-treated observations ($D_{s,t} = 0$), and the second stage obtains the overall ATT by regressing the residualized outcomes on the treatment indicator $D_{s,t}$.

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 estimator can be extended to estimate dynamic effects by including event-time indicators $D^k_{s,t}$ as treatment variables in the second stage, after estimating the state and year fixed effects among untreated/not-yet-treated observations ($D_{s,t} = 0$). Following Gardner et al. (2025), we estimate:

$$Y_{d,s,t} = \sum_{k=-K}^{K} \beta_{d,k} D_{k,s,t} + \theta_{d,s} + \tau_{d,t} + \eta_{d,s,t}, \tag{4.3}$$

where we define $D_{k,s,t}$ to index time relative to treatment adoption. When $k < 0$, the variables $D_{k,s,t}$ correspond to *leads* of adoption (years before adoption of NFS law). When $k \geq 0$, the variables $D_{k,s,t}$ correspond to *lags* of adoption (years after adoption of NFS law, with k indicating how many years have elapsed). Each $D_{k,s,t}$ is a binary indicator equal to 1 if state s is exactly k years relative to adoption in year t, and 0 otherwise (e.g., $D_{-1,s,t} = 1$ one year before adoption; $D_{1,s,t} = 1$ one year after adoption).

This procedure yields unbiased estimates of the dynamic ATT profile for demographic group d under the same assumptions required for the static 2SDID estimator—parallel trends, no anticipation, and correct specification of untreated potential outcomes.

4.3 Weighting and Interpretation of ATT Estimates

All regressions are weighted by state population, using population counts from the 2000 Census. Thus, we estimate average causal effects of NFS laws on intimate partner homicide rates among men and women in the relevant age group, in states that passed such laws.[20]

[20] Percentages of population by cohort and age group in 2000 are shown in Table A4.

5 Descriptive Statistics

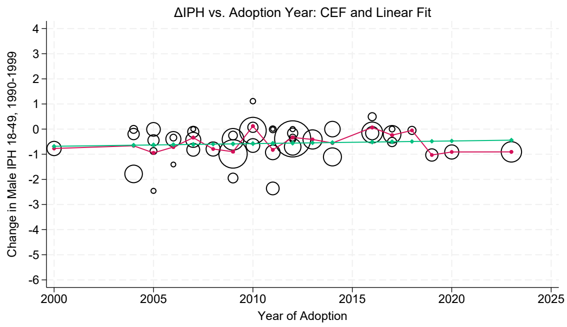
Timing of NFS Law Adoption. We begin by examining whether the timing of NFS law adoption is correlated with pre-treatment trends in IPH. To do so, we regress the change in IPH between 1990 and 1999—the year before Missouri enacted the first NFS statute—on the year in which each state adopted an NFS law. This exercise includes all adopting states, including late adopters such as Maryland (2020), Washington, DC (2023), and Ohio (2023).

Figure 4 plots the relationship between the year of NFS law adoption and the change in IPH from 1990 to 1999 for each victim sex–age group. Each panel reports the estimated slope coefficient (with robust HC3 standard errors) and the associated R^2 from the bivariate regression, along with both the fitted linear regression line and a nonparametric conditional expectation function. Across all demographic groups, there is no systematic association between pre-treatment IPH changes and the timing of NFS law adoption.

Similarly, regressions of changes in poverty rates, income per capita, unemployment rates, and the male-to-female unemployment ratio from 1990 to 1999 on the year of adoption show no statistically significant relationships (Table A7, Online Appendix). In addition, the same variables measured in 1990 are not systematically correlated with adoption timing (Table A8).

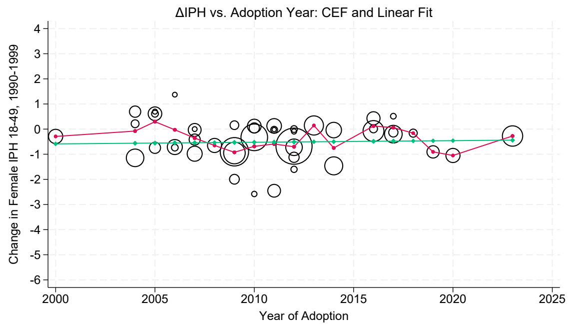
Figure 4: Change in Intimate Partner Homicides per 100,000 (IPH rate) from 1990 to 1999 and Year of NFS Law Adoption, by Victim Sex and Age Group

(a) Δ IPH Male-victim 18–49 & Year of Adoption



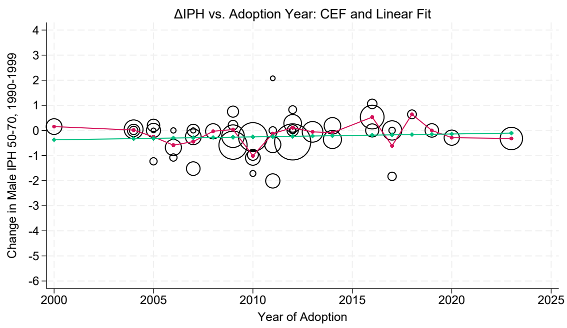
Slope coefficient: 0.011 (0.023), R^2: 0.009

(b) Δ IPH Female-victim 18–49 & Year of Adoption



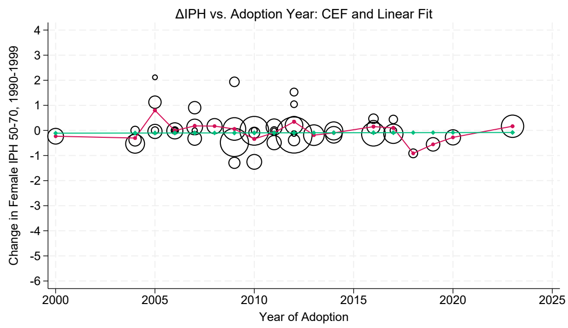
Slope coefficient: 0.006 (0.017), R^2: 0.003

(c) Δ IPH Male-victim 50–70 & Year of Adoption



Slope coefficient: 0.012 (0.016), R^2: 0.011

(d) Δ IPH Female-victim 50–70 & Year of Adoption



Slope coefficient: 0.001 (0.013), R^2: 0.000

Notes: The green line shows the fitted regression line, and the red line shows the estimated conditional expectation function. The size of each dot is proportional to the state (jurisdiction) population in the year 2000. Each panel reports the slope coefficient (with its robust HC3 standard error in parentheses) and the R^2 from the corresponding bivariate regression. Regressions are weighted by the corresponding cohort-age population in 2000. There are 47 observations (three states have missing information to compute the change).

Pre-Treatment IPH 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. Although this assumption is fundamentally untestable, we provide preliminary descriptive evidence consistent with it by examining pre-treatment trends in IPH rates.

Table 2 reports changes in male- and female-victim IPH rates across age groups between 1990 and 1999. Pre-treatment differences between never-treated and eventually treated states vary in sign and magnitude, and only one difference is statistically significant. Overall, this pattern suggests broadly similar pre-treatment trends across groups.

Table 2: Changes in Intimate Partner Homicides per 100,000 (IPH rate) from 1990 to 1999, by Victim Sex and Age Group: Eventually Treated vs Never-Treated

Variable	Eventually Treated	Never-Treated	Difference (SE)
Δ IPH rate, Male-victim 18-49	-0.54	-0.91	-0.37 (0.09)***
Δ IPH rate, Female-victim 18-49	-0.52	-0.53	-0.01 (0.59)
Δ IPH rate, Male-victim 50-70	-0.24	-0.31	-0.07 (0.09)
Δ IPH rate, Female-victim 50-70	-0.10	0.03	0.13 (0.34)

Notes: The difference is the estimated coefficient on a never-treated indicator from a regression of the change in the IPH rate from 1990 to 1999, by victim sex and age group. There are 47 observations, and regressions are weighted by the relevant cohort-age population in 2000. Robust (HC3) standard errors in parentheses. *p-value<0.1, **p-value<0.05, ***p-value<0.01.

Table A9 compares baseline characteristics—poverty rates, income per capita, unemployment rates, and the male-to-female unemployment ratio—between eventually treated and never-treated states. On average, the two groups are broadly similar. Only one statistically significant difference emerges (poverty rate), while differences in the remaining variables are small and statistically insignificant.

We further assess the plausibility of the parallel trends assumption using event-study estimates of dynamic treatment effects, and evaluate the robustness of our findings to (i) violations of parallel trends of varying magnitudes (Rambachan and Roth, 2023) and (ii) the inclusion of baseline covariates interacted with linear time trends.

6 Estimated Effects of NFS Laws on Homicides Rates

6.1 Overall ATT Estimates on IPH Rates

Main specifications. Table 3 reports the estimated effects of NFS laws on IPH rates (per 100,000) for each sex–age victim group. The first two columns present estimates from the OLS and two-stage difference-in-differences (2SDID) estimators, with 2SDID being our preferred approach. The final two columns report the observed mean IPH rate in 1999—the year before any state enacted an NFS law—and the corresponding counterfactual mean, i.e., the predicted mean IPH that would have been observed in treated states absent the laws.

Panel A shows that NFS laws are associated with sizable reductions in IPH rates, particularly among younger adults. For individuals aged 18–49, the 2SDID estimate implies a decline in male-victim IPH of 0.09 per 100,000 men—a 27% reduction relative to the counterfactual mean (from 0.337 to 0.247). For female victims in the same age group, the estimated reduction is 0.17 per 100,000 women, corresponding to a 14% decrease relative to the counterfactual mean (from 1.22 to 1.05).

For the 50–70 age group, estimated effects are smaller—much closer to zero than the corresponding effects for ages 18–49—and statistically insignificant. These age gradients are consistent with NFS laws having a larger impact among individuals who are more likely to experience IPV (Aizer and Dal Bo, 2009).

Panel B investigates the robustness of our findings to differential state trends by interacting baseline covariates (measured in 1990) with linear time trends (e.g., Bailey and Goodman-Bacon, 2015; Conti and Ginja, 2023; Mora-García et al., 2024). The resulting estimates are very similar to those in Panel A. Figure A2 summarizes estimates with no controls, all controls, and with one control at a time.[21]

[21] Covariates include measures of state-level socioeconomic resources (log income per capita, unemployment rate, poverty rate) and gender inequality (male-to-female unemployment ratio), following Aizer (2010). These variables are constructed from the Current Population Survey (Flood et al., 2022), Census Bureau poverty data (United States Census, 2023a), and St. Louis Fed income data (U.S. Bureau of Economic Analysis and Federal Reserve Bank of St. Louis, 2023). See Online Appendix A2 for further details.

Table 3: Effect of NFS Law on Intimate Partner Homicide Rates (per 100,000), by Victim Sex and Age Group

	(1) OLS	(2) 2SDID	(3) Mean 1999	(4) Counterfactual Mean
Panel A. Without Controls				
Male-victim 18–49	-0.076**	-0.090**	0.354	0.337
	(0.035)	(0.040)		
Female-victim 18–49	-0.102*	-0.169**	1.189	1.221
	(0.055)	(0.079)		
Male-victim 50–70	-0.014	-0.019	0.266	0.224
	(0.019)	(0.022)		
Female-victim 50–70	-0.029	-0.026	0.480	0.511
	(0.028)	(0.036)		
Panel B. With Controls				
Male-victim 18–49	-0.064**	-0.096**	0.354	0.343
	(0.031)	(0.044)		
Female-victim 18–49	-0.107*	-0.199***	1.189	1.252
	(0.054)	(0.069)		
Male-victim 50–70	-0.004	-0.005	0.266	0.210
	(0.021)	(0.030)		
Female-victim 50–70	-0.028	-0.025	0.480	0.509
	(0.027)	(0.033)		

Notes: Each panel reports coefficients from regressions of the IPH rate on an indicator for NFS law adoption, including state and year fixed effects. Panel B adds baseline (1990) controls for demographic and socioeconomic covariates (log income per capita, unemployment rate, poverty rate, and male-to-female unemployment ratio) interacted with linear time trends. The counterfactual mean, $\mathbb{E}[Y_{d,s,t}(0) \mid D_{s,t} = 1]$, is estimated as the average of predicted IPH based on state and year fixed effects (and coefficients on controls) estimated from untreated/not-yet-treated observations ($D_{s,t} = 0$). Regressions are weighted by the relevant cohort-age population in 2000. Standard errors clustered by state (50 clusters) are reported in parentheses. $N = 1{,}479$. *p<0.10, **p<0.05, ***p<0.01.

Our estimates align with prior evidence on criminal-justice interventions (Aizer and Dal Bo, 2009; Chin and Cunningham, 2019; Miller and Segal, 2018). 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. Chin and Cunningham (2019) estimate a 43% reduction in spousal homicides associated with discretionary arrest laws enacted between the 1970s and 1990s. Miller and Segal (2018) find that a 6 percentage-point increase in the share of female police officers leads to a 14% reduction in female-victim IPH and a 22% reduction in male-victim IPH among adults. Aizer and Dal Bo (2009) and Miller and Segal (2018) attribute the sizable declines in male-victim IPH to reductions in lethal self-defense by female victims.

To aid interpretation, we translate the estimated overall ATT effects on IPH rates (per 100,000) into the implied reduction in IP homicides for each demographic group d between enactment and 2019. The reduction for group d is computed as:

$$-\mathbb{E}[\beta_{d,s,t} \mid D_{s,t} = 1] \times \frac{\sum\limits_{(s,t):\, D_{s,t}=1, t<2020} \text{Population}_{d,s,t}}{100{,}000}.$$

This back-of-the-envelope calculation implies approximately 1,029 fewer female and 547 fewer male IP homicides among adults ages 18–49.

Robustness checks. Table A10 reports Poisson estimates using homicide counts rather than rates; the results are qualitatively consistent with our baseline specifications. Because neither Missouri (2000) nor North Carolina (2004) defined the act of strangulation in their statutes, we re-estimate the models excluding these states. As shown in Table A11, the results are virtually unchanged. In addition, Figure A3 demonstrates that our 2SDID estimates are not driven by any single state: when we sequentially drop one state at a time, the resulting coefficients closely match those in Table 3, alleviating concerns that our findings are sensitive to the composition of the control pool.

What about the potential influence of other contemporaneous domestic violence (DV) policies? By 1989, all 50 states and DC had enacted statutes providing civil remedies for battered women through protection orders (Hart, 1991; Benitez et al., 2010). By 1990, the major DV policy interventions—protection orders, mandatory or pro-arrest statutes, custody reforms, and victims' rights protections—were already widely in place across U.S. states. Reforms during the 1990s primarily expanded the scope of these policies and increased federal support. Consequently, potentially confounding policies such as mandatory arrest laws, protection-order provisions, and unilateral divorce statutes were largely implemented before our baseline year (1990) and well before the first adoption of NFS laws in 2000.

We also test for heterogeneous effects of NFS laws (see Appendix A4, Figures A4–A7) and find no evidence that they vary by baseline socioeconomic conditions or gender inequality in 1990, or by local police resources in 2000 (earlier data are unavailable). The characteristics examined include proxies for economic resources (income per capita, poverty rate, unemployment rate), gender inequality (male-to-female unemployment ratio), and police resources (sworn personnel per 100,000 and uniformed officers responding to calls per 100,000), using data from the 2000 Census of State and Local Law Enforcement Agencies (Reaves and Hickman, 2002).[22]

[22]The first measure reflects overall law-enforcement capacity—the size and potential reach of police agencies—while the second captures staffing dedicated specifically to frontline response, indicating how well-resourced agencies are for incidents requiring immediate intervention.

6.2 Event-Study Estimates on IPH Rates

Figure 5 presents event–study (dynamic) estimates using the two-stage difference-in-differences (2SDID) approach, separately by victim sex and age group. The pre-treatment coefficients (shown in red squares) are close to zero in nearly all pre-treatment periods across panels, providing evidence consistent with the parallel trends assumption and with the descriptive patterns reported in Section 5.

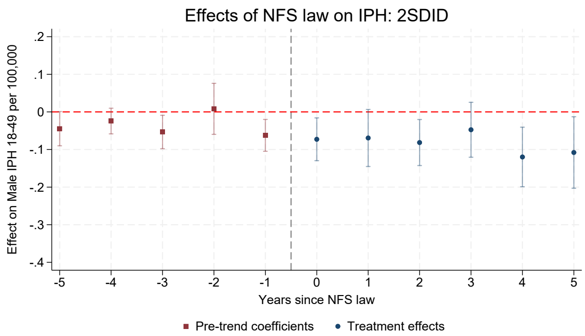
The post-treatment coefficients (shown in blue dots) show substantial and persistent declines in IPH for both male and female victims aged 18–49, consistent with the overall ATT estimates in Table 3. In contrast, dynamic effects are close to zero for male victims aged 50–70 and smaller in magnitude for female victims aged 50–70 relative to their 18–49 counterparts.

Appendix Figure A8 reports a sensitivity analysis following Rambachan and Roth (2023) to assess the robustness of these estimates to possible violations of parallel trends. For male victims aged 18–49, the estimated effect in the first treatment year is negative and remains statistically significant under modest deviations from parallel trends. For female victims in the same age group, the first-year effect is likewise negative and remains significant under somewhat larger deviations.

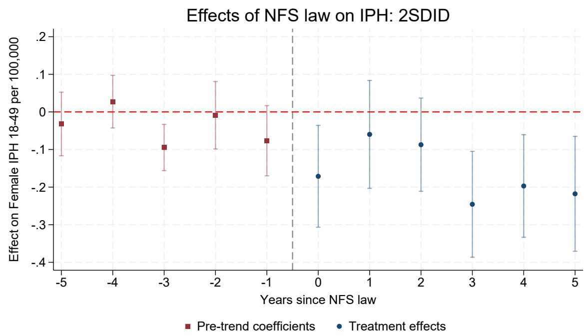
Finally, Figure A9 shows that the dynamic patterns remain similar when baseline covariates (measured in 1990) are interacted with linear time trends. These control-adjusted event studies are also robust to potential violations of parallel trends, as shown in Appendix Figure A10.

Figure 5: 2SDID Event Studies of NFS Laws on IPH rates (per 100,000)

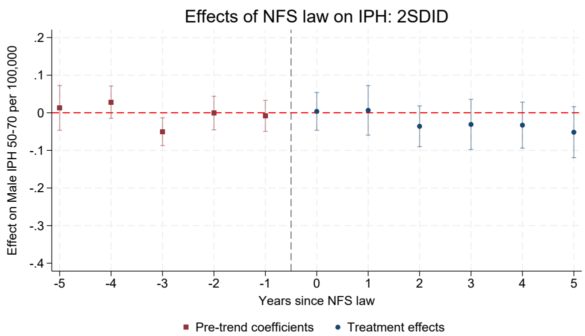
(a) IPH rate, Male-victim 18-49



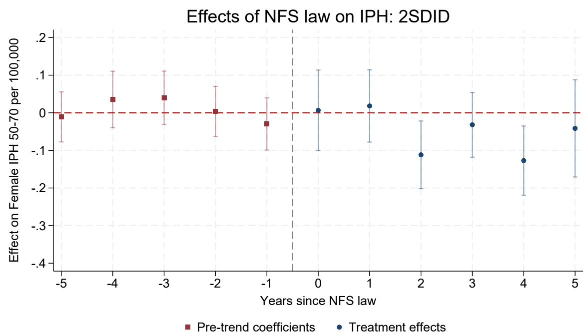
(b) IPH rate, Female-victim 18-49



(c) IPH rate, Male-victim 50-70



(d) IPH rate, Female-victim 50-70



Notes: 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: Effects on Homicides Rates by Strangers

In the spirit of Chin and Cunningham (2019), we conduct a falsification test by examining whether NFS laws affected homicides committed by strangers, disaggregated by victim sex and age group. Because NFS indicates escalation and coercive control within IP relationships (Thomas et al., 2013; Patch et al., 2018), stranger-perpetrated homicides—where these dynamics are absent—offer a natural placebo test.

Table 4 reports the main placebo estimates, using OLS and 2SDID estimators and including baseline covariates (measured in 1990) interacted with linear time trends. Across all panels, estimated effects are small, statistically insignificant, and display no consistent pattern across sex or age groups. Table A12 reports analogous placebo estimates without covariate trends. In the Online Appendix, Figure A11 shows that the placebo 2SDID estimates are not driven by any single state by re-estimating the models while dropping one state at a time.

Table 4: Effects of NFS Laws on Stranger-Perpetrated Homicides per 100,000 (SH rate) by Victim Sex and Age Group, with Baseline Covariates Interacted with Linear Time Trends

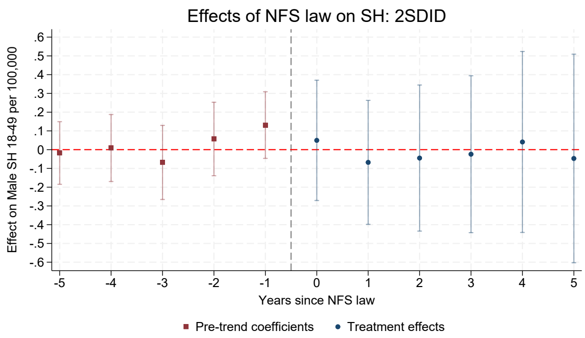
Dependent variable	OLS	2SDID	Mean in 1999	Counterfactual Mean
Male-victim 18-49	-0.025	-0.007	1.116	0.899
	(0.139)	(0.213)		
Female-victim 18-49	0.021	0.011	0.127	0.092
	(0.018)	(0.032)		
Male-victim 50-70	-0.000	-0.029	0.273	0.333
	(0.034)	(0.057)		
Female-victim 50-70	0.001	0.008	0.073	0.044
	(0.013)	(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. Regressions are weighted by the relevant cohort-age population in 2000. Standard errors clustered at the state level (50 clusters), shown in parentheses. $N = 1,479$. *p<0.1, **p<0.05, ***p<0.01.

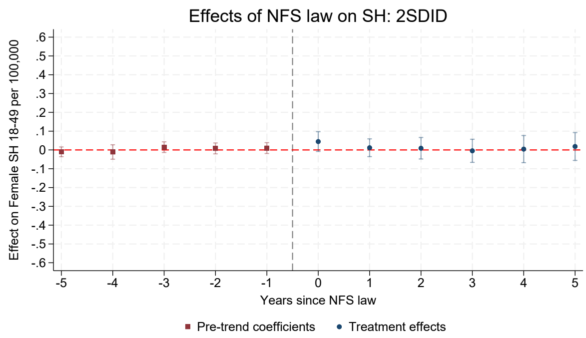
We also present event-study estimates for the placebo outcome. As shown in Figure 6, the event-study coefficients display no evidence of systematic post-treatment effects, providing additional support for our identification strategy.

Figure 6: 2SDID Dynamic Effects of NFS Law on Stranger-Perpetrated Homicides per 100,000 (SH rate) by Victim Sex and Age Group, with Baseline Covariates Interacted with Linear Time Trends

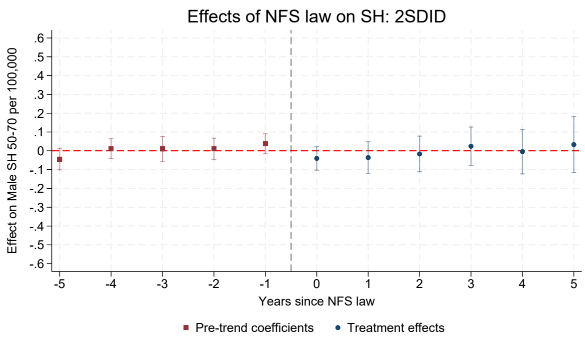
(a) SH rate, Male-victim 18-49



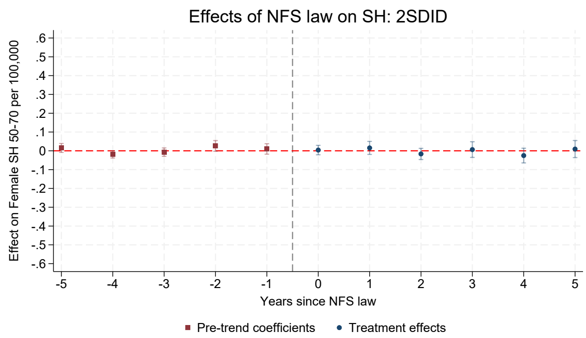
(b) SH rate, Female-victim 18-49



(c) SH rate, Male-victim 50-70



(d) SH rate, Female-victim 50-70



Notes: The event study estimates are based on 2SDID estimates by including the event-time indicators $D_{k,s,t}$ 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 ($D_{s,t} = 0$). Estimation is conducted simultaneously using the (GMM) framework in Gardner et al. (2025) and using the `did2s` Stata package developed by Butts (2021).

This falsification exercise is consistent with NFS laws not influencing broader homicide trends unrelated to intimate partner violence.

6.4 Additional Robustness Checks

We report a set of additional robustness exercises in Appendix Table A13. (a) unweighted regressions; (b) regressions using time-varying state population weights; (c) inclusion of South Carolina, the only state without an NFS statute by 2019; and (d) coding the treatment indicator based on the law's passage year rather than its effective year. Across all specifications, the estimated effects remain very similar in magnitude and significance to those reported in Tables 3 and 4, reinforcing the reliability of our main findings.

7 How NFS Laws Reduce Intimate Partner Homicides: Evidence on Mechanisms

In this section, we provide evidence that NFS laws operate through a two-step mechanism: increasing the legal salience of NFS—by clearly defining and elevating the seriousness of the act—and strengthening enforcement conditional on this new salience. By enabling earlier and more effective intervention, NFS laws disrupt the escalation of violence that can culminate in intimate partner homicide, both by reducing abusers' opportunities to kill and by lowering victims' reliance on lethal self-defense. We study this disruptive mechanism by estimating the effects of NFS laws on two intermediate outcomes derived from the National Incident-Based Reporting System (NIBRS).

7.1 Measuring legal recognition and conditional enforcement

The first step of the mechanism is classification as aggravated assault due to legal recognition. By defining NFS as a serious criminal offense and introducing strangulation-specific statutory language, NFS laws make a historically minimized form of violence legally salient. This increased salience leads the legal system to treat incidents involving NFS with greater severity—specifically, to classify them as aggravated assaults rather than simple assaults or unrecorded altogether. We measure this legal recognition effect using the share of intimate partner violence (IPV) incidents that are officially classified as IPV aggravated assaults:

$$\text{Classification}_{d,s,t} = \frac{\text{IPV Aggravated Assaults}_{d,s,t}}{\text{IPV Incidents}_{d,s,t}}.$$

for victims of demographic group d, in state s and year t. Because men overwhelmingly perpetrate strangulation against female partners (e.g., Sorenson et al., 2014; Parekh et al., 2024), we expect the passage of NFS laws to increase this ratio primarily for IPV incidents with female victims.

The second step of the mechanism is enforcement (arrests) conditional on aggravated classification. For the law to have practical force, the aggravated classification must trigger a decisive law-enforcement response—specifically, the incapacitation of offenders at an earlier stage of the violence cycle.[23] We measure the power of the law by using the arrest rate conditional on aggravated classification:

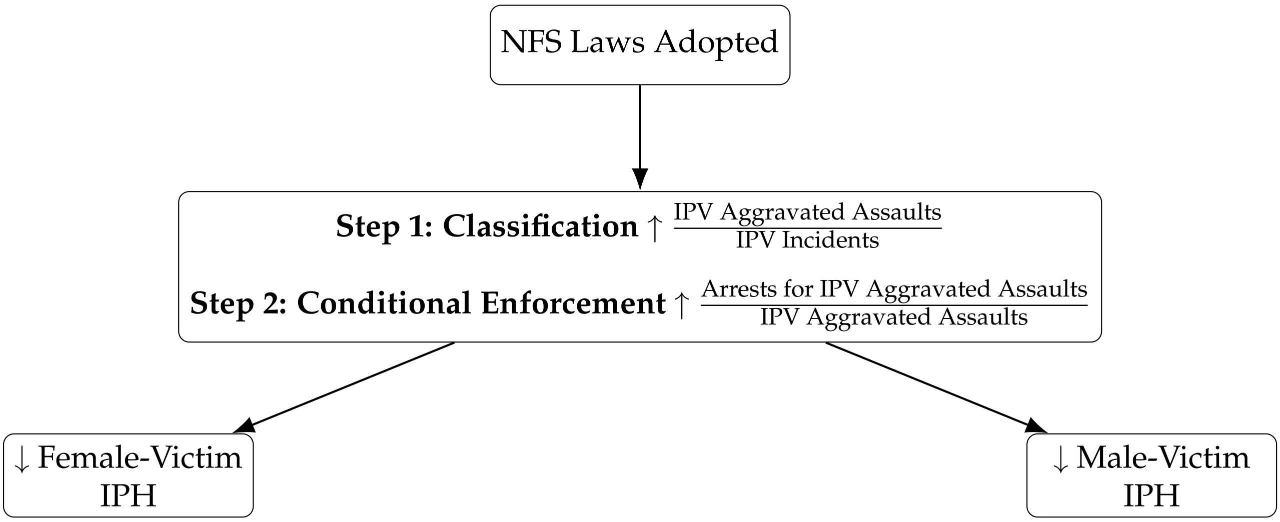
$$\text{Conditional Enforcement}_{d,s,t} = \frac{\text{Arrests for IPV Aggravated Assaults}_{d,s,t}}{\text{IPV Aggravated Assaults}_{d,s,t}}.$$

We expect this conditional arrest rate to rise following NFS law adoption, indicating that newly recognized and properly classified incidents elicit a stronger police response.

We refer to the combined pathway as an early–law-enforcement mechanism, comprising two linked steps. The first step, classification, captures the shift in how IPV incidents involving NFS are legally recognized and recorded as aggravated assaults once NFS statutes explicitly define and acknowledge this crime. The second step, conditional enforcement, reflects the strengthened police response conditional on aggravated classification—specifically, the increased likelihood that an IPV aggravated assault results in an arrest. Together, these stages constitute an early-intervention enforcement chain that disrupts the escalation of violence before it becomes lethal. Figure 7 summarizes this causal pathway from statutory adoption to reductions in intimate partner homicides.

[23] This may also operate through deterrence.

Figure 7: The Two-Step Mechanism Linking NFS Laws to Reduced Homicides



7.2 Effects of NFS laws on Classification and Enforcement

Our analysis of NIBRS data provides direct support for the early–law-enforcement mechanism. Table 5 shows that NFS laws significantly increase the probability that an IPV incident is classified as an aggravated assault—and only for female victims, who are far more likely to experience NFS. Among women aged 18–49, the share of IPV incidents recorded as aggravated assaults increases by 5.5 percentage points, from 7.8% to 13.3%. This pattern is consistent with the classification stage, in which NFS laws elevate the legal salience of NFS and prompt police to record IPV incidents involving strangulation as serious violent offenses.

Crucially, this enhanced classification is met with a stronger enforcement response. Table 6 shows that NFS laws increase the arrest rate conditional on an aggravated IPV assault. For female victims, the likelihood that an aggravated IPV assault results in an arrest rises by 12 percentage points (from 48% to 60%); for male victims, the increase is 15 percentage points (from 47% to 62%). This aligns with the conditional enforcement stage of the mechanism, where aggravated classification triggers more decisive police action.

Together, these responses help explain the reductions in intimate partner homicides. Earlier incapacitation of stranglers plausibly reduces the risk of lethal violence against women, while improved intervention lowers the likelihood that women resort to lethal self-defense, contributing to declines in male-victim IPH.

Table 5: Effects of NFS Laws on Classification by Victim Sex and Age Group

	$\frac{\text{IPV Aggravated Assaults}}{\text{IPV Incidents}}$				
Victim	OLS	2SDID	Mean in 1999	Counterfactual Mean	N [clusters]
Male 18-49	0.007	0.029	0.177	0.121	730
	(0.013)	(0.022)			[45]
Female 18-49	0.027**	0.055***	0.104	0.078	732
	(0.012)	(0.021)			[45]
Male 50-70	0.003	0.033	0.214	0.150	701
	(0.022)	(0.034)			[45]
Female 50-70	0.013	0.043*	0.148	0.084	713
	(0.018)	(0.025)			[45]

Notes: All regressions include state and year fixed effects. Regressions are weighted by the relevant cohort-age population in 2000. Standard errors clustered at the state level, shown in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table 6: Effects of NFS Laws on Conditional Enforcement by Victim Sex and Age Group

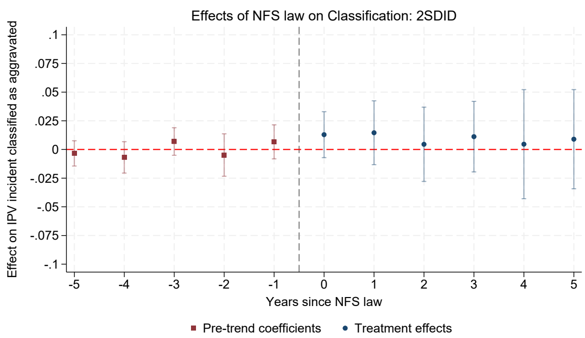
	$\frac{\text{Arrests for IPV Aggravated Assaults}}{\text{IPV Aggravated Assaults}}$				
Victim	OLS	2SDID	Mean in 1999	Counterfactual Mean	N [clusters]
Male 18-49	0.062**	0.146***	0.588	0.471	710
	(0.026)	(0.043)			[44]
Female 18-49	0.058**	0.122***	0.615	0.483	720
	(0.027)	(0.032)			[45]
Male 50-70	0.029	0.027	0.680	0.609	635
	(0.034)	(0.064)			[45]
Female 50-70	0.051	0.227***	0.793	0.411	656
	(0.036)	(0.079)			[44]

Notes: All regressions include state and year fixed effects. Regressions are weighted by the relevant cohort-age population in 2000. Standard errors clustered at the state level, shown in parentheses. *p<0.1, **p<0.05, ***p<0.01.

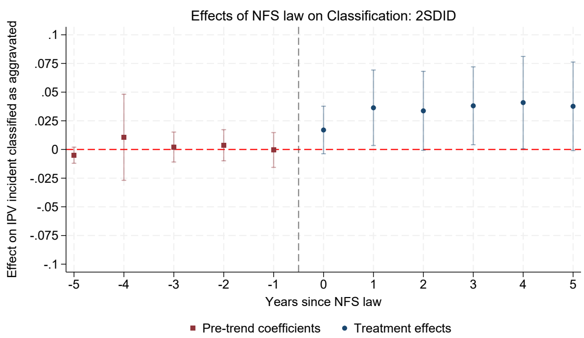
Figures 8 and 9 report dynamic ATT estimates based on the 2SDID approach, separately by victim sex and age group. Consistent with parallel trends, we observe no systematic effects in the pre-treatment period. Following adoption, however, both aggravated assault classification and conditional arrest rates increase, mirroring the patterns in the overall ATT estimates.

Figure 8: 2SDID Event Studies of NFS Laws on Classification: $\frac{\text{IPV Aggravated Assaults}}{\text{IPV Incidents}}$

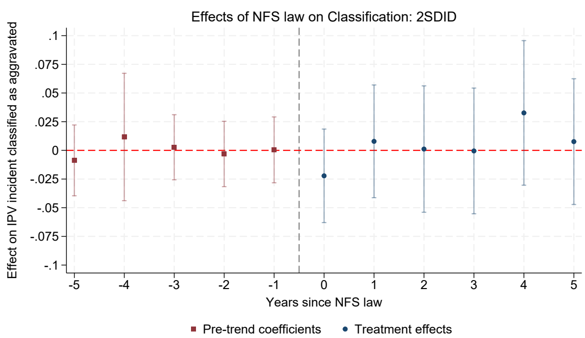
(a) Classification rate, Male-victim 18-49



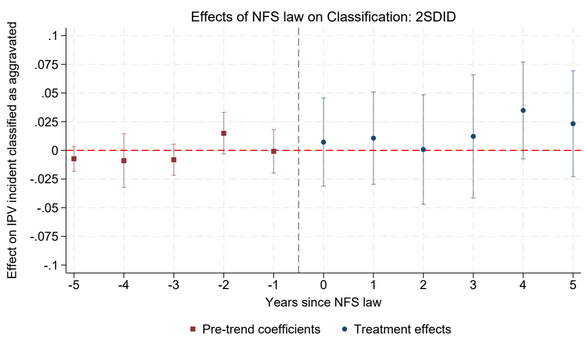
(b) Classification rate, Female-victim 18-49



(c) Classification rate, Male-victim 50-70



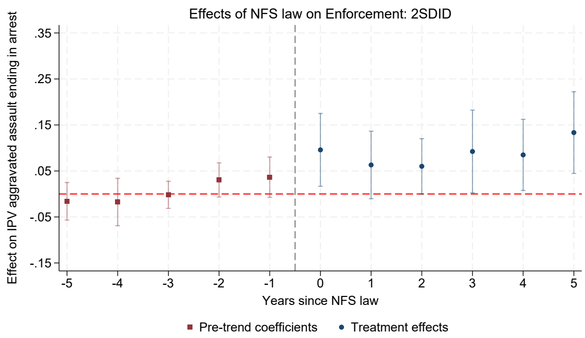
(d) Classification rate, Female-victim 50-70



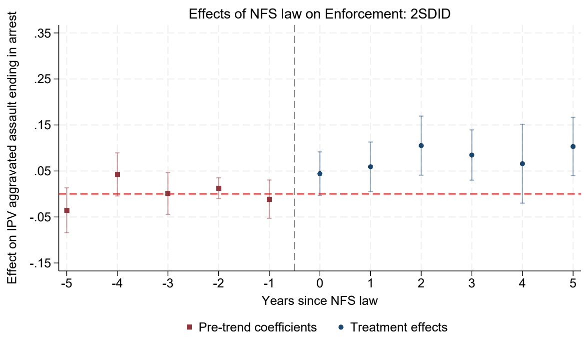
Notes: The event study estimates are based on 2SDID estimates by including the event-time indicators $D_{k,s,t}$ as treatment variables in the second stage. State and year fixed effects are estimated in the first stage for the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). Estimation is conducted simultaneously using the (GMM) framework in Gardner et al. (2025) and using the `did2s` Stata package developed by Butts (2021).

Figure 9: 2SDID Event Studies of NFS Laws on Enforcement: $\frac{\text{Arrests for IPV Aggravated Assaults}}{\text{IPV Aggravated Assaults}}$

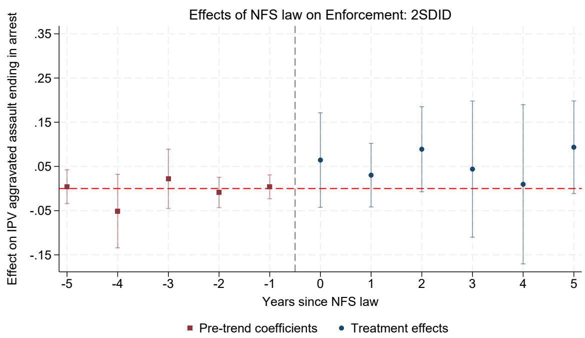
(a) Enforcement rate, Male-victim 18-49



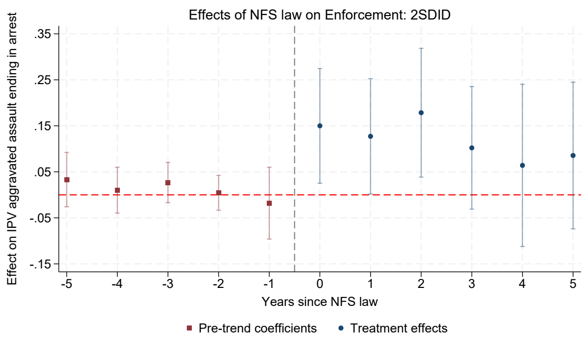
(b) Enforcement rate, Female-victim 18-49



(c) Enforcement rate, Male-victim 50-70



(d) Enforcement rate, Female-victim 50-70



Notes: The event study estimates are based on 2SDID estimates by including the event-time indicators $D_{k,s,t}$ 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 ($D_{s,t} = 0$). Estimation is conducted simultaneously using the (GMM) framework in Gardner et al. (2025) and using the `did2s` Stata package developed by Butts (2021).

The dynamic profiles of classification (Figure 8) and enforcement (Figure 9) show responses within one year of adoption, whereas reductions in IPH emerge with a one- to two-year lag (Figure 5). This temporal sequencing is consistent with an early-intervention mechanism, rather than a contemporaneous recoding of homicides.

7.3 Data limitations and ancillary evidence

As discussed earlier, the NIBRS provides incident-level detail useful for constructing intermediate outcomes, but it has well-known limitations. Coverage is uneven across states and over time, and participation is incomplete and non-representative.[24] Moreover, while NFS is expected to be recorded under aggravated assault (California District Attorneys Association, 2020), NIBRS does not separately identify NFS, and our enforcement proxy (arrests per aggravated IPV assault) captures policing responses rather than downstream prosecutorial or sentencing outcomes.

Given these data limitations, our analysis focuses solely on the effects of statutory adoption of NFS laws. We do not observe the implementation of local forensic or investigative protocols, and our identification strategy does not rely on them. Instead, we interpret the observed increases in aggravated-classification and conditional enforcement as consistent with NFS statutes making the conduct legally salient and enabling earlier incapacitation of abusers, thereby reducing both opportunities for lethal violence and victims' reliance on self-defense.

As ancillary descriptive evidence—intended solely to illustrate the types of downstream judicial responses that can accompany improved recognition of strangulation—some jurisdictions that adopted detailed strangulation-specific investigative protocols, such as Maricopa County, Arizona, have reported increases in prosecution rates (Maricopa County Attorney's Office, 2013). While these descriptive patterns are not part of our empirical identification, they are consistent with the broader interpretation that recognizing strangulation as a serious offense can help disrupt violent escalation.

[24] In 2012, 32 states were certified to report via NIBRS, but only 15 submitted complete data; coverage was roughly 30% of the U.S. population and 28% of reported crime. Estimates therefore pertain to reporting agencies.

7.4 Early Enforcement in Light of Prior Research

Our evidence points to an early-intervention channel triggered by statutory clarity. By explicitly defining and criminalizing NFS, the laws increase the legal salience of severe IPV. Conditional on aggravated classification, enforcement intensifies. These shifts align with the substantial and sustained declines in IPH that we document for adults ages 18–49.

This mechanism parallels the victim-engagement channel documented in Miller and Segal (2018), where greater female officer representation increases reporting, reduces domestic violence, and lowers IPH. In both settings, earlier detection and stronger enforcement incapacitate abusers and reduce reliance on lethal self-defense—though achieved here through statutory clarity and salience, and in Miller and Segal (2018) through personnel composition and enhanced trust.

8 Conclusion

Strangulation statutes are a relatively recent development in criminal justice, designed to address non-fatal strangulation (NFS)—a common, gendered form of intimate partner abuse that often occurs at the most dangerous stage of escalation, associated with later homicide.

This paper makes three contributions. First, we assemble a new state-by-year dataset documenting the timing of NFS statutes across the United States. Second, merging these data with the FBI Supplementary Homicide Reports (1990–2019), we estimate the causal effects of NFS laws on intimate partner homicides of women and men. Third, we provide evidence on the mechanisms through which these laws operate, using incident-level information from the National Incident-Based Reporting System (1991–2019) to examine changes in aggravated-assault classification and conditional enforcement.

Our results show that NFS statutes led to substantial reductions in IPH, concentrated among adults ages 18–49: male-victim IPH declines by 27% (from 0.337 to 0.247 per 100,000 men) and female-victim IPH by 14% (from 1.221 to 1.052 per 100,000 women). A back-of-the-envelope calculation suggests that, between enactment and 2019, these laws prevented roughly 1,029 female and 547 male intimate-partner homicides in this age group. We also find that NFS laws increase legal salience—raising the likelihood that IPV incidents are classified as aggravated assaults—and, conditional on that classification, strengthen enforcement through higher arrest rates.

Taken together, the evidence indicates that NFS laws reduce intimate partner homicides by enabling earlier intervention. Explicitly defining and criminalizing NFS appears to be a scalable and actionable policy tool for preventing lethal IPV. More broadly, the findings contribute to research on gender-based violence and legal protections by showing how precise statutory design can shift enforcement earlier in the violence cycle and meaningfully enhance victim safety.

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Online Appendix

A1 Timing and Bill Numbers by State

Table A1: 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

A2 Data Construction and Sources

We requested the data from Fox, who generously sent us directly the 1976-2020 version in 2023. Here, we provide additional details on the construction of our main sample. We start from the raw SHR data by Fox and Swatt (2009), 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 check these counts to align with the total number of homicides reported in each state-year, recoding when needed. 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.[25] We followed this systematic approach throughout the sample to ensure accurate counts and correct handling of true zeros versus missing values.

Table A2: 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

We then merged population data (United States Census, 2023b) by sex 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 un-

[25]For instance, 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.

employment rate and female and male unemployment ratio from CPS (Flood et al., 2024), and state poverty rates (United States Census, 2023a).

A3 Additional Descriptive Statistics and Robustness Checks

Table A3: 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

Figure A1: NIBRS reporting over time

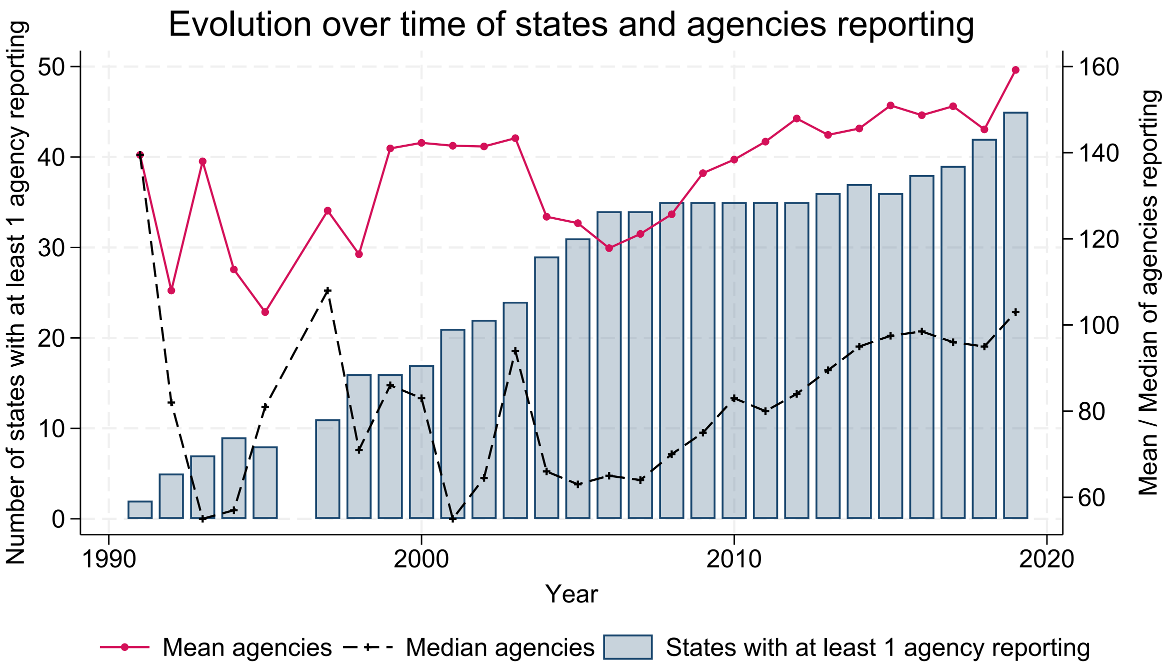


Table A4: Percentage of Population by Cohort and Age Group in 2000

Treatment Cohort	% Pop 18–70	% Pop 18–49	% Pop 50–70
2000 cohort	1.99	1.96	2.09
2004 cohort	4.83	4.80	4.89
2005 cohort	3.67	3.69	3.63
2006 cohort	2.85	2.83	2.89
2007 cohort	10.89	10.55	11.71
2008 cohort	1.92	1.92	1.93
2009 cohort	13.61	13.91	12.87
2010 cohort	10.01	9.94	10.18
2011 cohort	4.33	4.24	4.56
2012 cohort	16.93	17.33	15.92
2013 cohort	3.56	3.54	3.60
2014 cohort	5.34	5.45	5.09
2016 cohort	6.65	6.50	7.00
2017 cohort	5.07	5.08	5.03
2018 cohort	0.64	0.63	0.66
2019 cohort	1.48	1.46	1.54
Never treated	6.22	6.15	6.40

Table A5: Missingness and regression sample in the NIBRS by victim sex and age group (state-year units)

	Classification		Enforcement	
Group	**Missing (%, n)**	**Sample**	**Missing (%, n)**	**Sample**
Male-victim 18–49	51.3%, 770	730	52.7%, 790	710
Male-victim 50–70	53.3%, 799	701	57.7%, 865	635
Female-victim 18–49	51.2%, 768	732	52.0%, 780	720
Female-victim 50–70	52.5%, 787	713	56.3%, 844	656

Notes: Missing (%, n) reports the percent and count of missing observations.

Table A6: Effects of NFS laws on missing Classification and Enforcement ratios

	OLS		2SDID	
Panel A: Classification (missing)				
Male-victim 18–49	−0.015	(0.060)	0.003	(0.071)
Male-victim 50–70	−0.004	(0.065)	0.007	(0.071)
Female-victim 18–49	−0.016	(0.060)	0.002	(0.063)
Female-victim 50–70	−0.003	(0.063)	0.004	(0.071)
Panel B: Enforcement (missing)				
Male-victim 18–49	−0.021	(0.062)	−0.013	(0.069)
Male-victim 50–70	−0.013	(0.065)	−0.033	(0.065)
Female-victim 18–49	−0.020	(0.061)	−0.002	(0.072)
Female-victim 50–70	−0.003	(0.063)	0.001	(0.071)

Notes: Each panel reports coefficients from regressions of the missing classification ratio (panel A) and the missing enforcement ratio (panel B) on an indicator for NFS law adoption, including state and year fixed effects. The TWFE models are estimated using OLS and 2SDID. All regressions are weighted by the relevant cohort–age population in 2000. Standard errors, clustered at the state level (50 clusters), are reported in parentheses. $N = 1,500$. *p-value<0.1, **p-value<0.05, ***p-value<0.01.

Table A7: 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)	

Notes: 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 population (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 A8: 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)	
IPH rate, male-victim 18-49	-0.0255*	0.030
	(0.0134)	
IPH rate, female-victim 18-49	-0.0141	0.007
	(0.0263)	
IPH rate, male-victim 50-70	-0.0202	0.046
	(0.0121)	
IPH rate, female-victim 50-70	-0.0020	0.001
	(0.0143)	

Notes: 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) in 2000 for regressions of covariates, and cohort-age 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 A9: 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)*

Notes: 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 (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 A10: Effects of NFS Law on Intimate Partner Homicides (IPH counts) by Victim Sex and Age Group: Poisson Model

Dependent variable	Poisson	Mean in 1999
Male-victim 18-49	-0.169*	8.886
	(0.093)	
Female-victim 18-49	-0.071*	34.022
	(0.041)	
Male-victim 50-70	0.014	2.640
	(0.088)	
Female-victim 50-70	-0.077	5.843
	(0.059)	

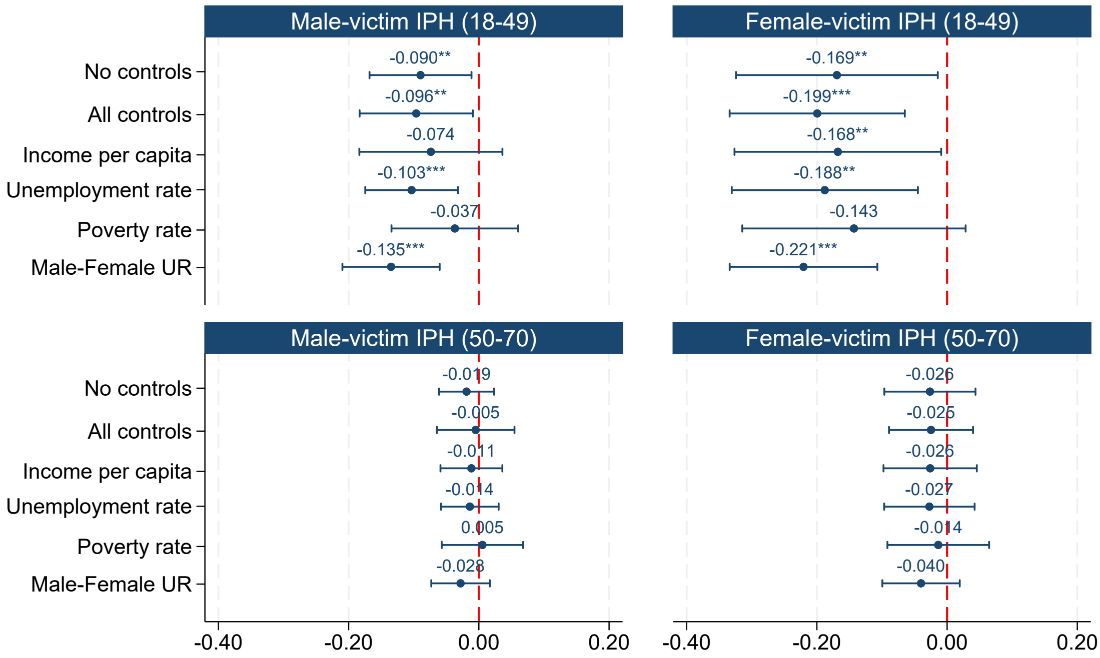
Notes: All regressions include state and year fixed effects. Exposure is proportional to the relevant population by cohort-age in 2000. Standard errors clustered at the state level (50 clusters), shown in parentheses. $N = 1,479$. *p<0.1, **p<0.05, ***p<0.01.

Table A11: Effects of NFS Law on Intimate Partner Homicides per 100,000 (IPH rate) by Victim Sex and Age Group, excluding Missouri (2000) and North Carolina (2004)

Dependent variable	OLS	2SDID	Mean in 1999	Counterfactual Mean
Male-victim 18-49	-0.078**	-0.082**	0.355	0.310
	(0.033)	(0.037)		
Female-victim 18-49	-0.106**	-0.165**	1.188	1.188
	(0.050)	(0.067)		
Male-victim 50-70	-0.017	-0.019	0.249	0.210
	(0.021)	(0.023)		
Female-victim 50-70	-0.035	-0.041	0.486	0.519
	(0.032)	(0.035)		

Notes: Each row displays the coefficients from regressions of the IPH rate on an indicator for NFS law adoption, including state and year fixed effects. Regressions are weighted by the relevant cohort-age population in 2000. Clustered standard errors (48 clusters). $N = 1,419$. *p<0.1, **p<0.05, ***p<0.01.

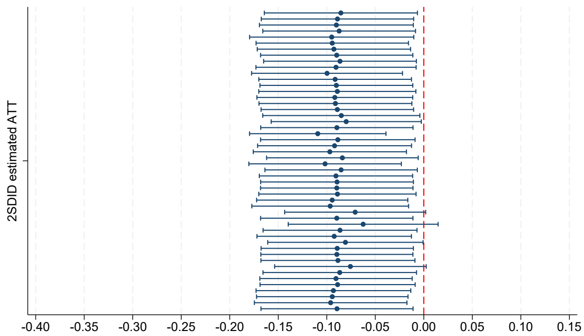
Figure A2: 2SDID ATT estimates of NFS Law on IPH rates: no controls vs. controls



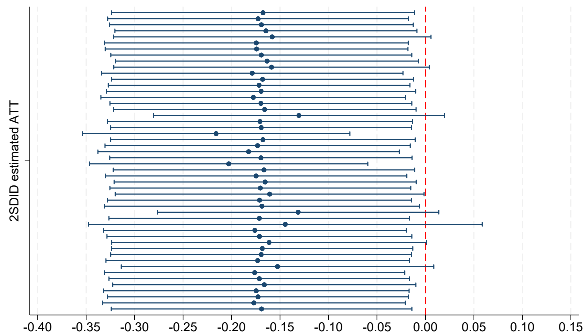
Notes: Each graph reports the point estimate of the overall ATT together with its associated 95% confidence interval.

Figure A3: 2SDID ATT estimates of NFS Law on IPH rates: Dropping one state at a time

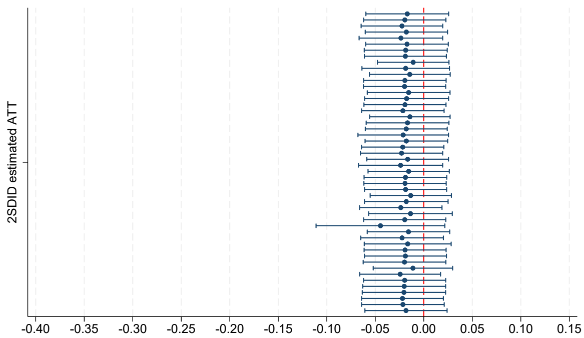
(a) IPH rate, Male-victim 18-49



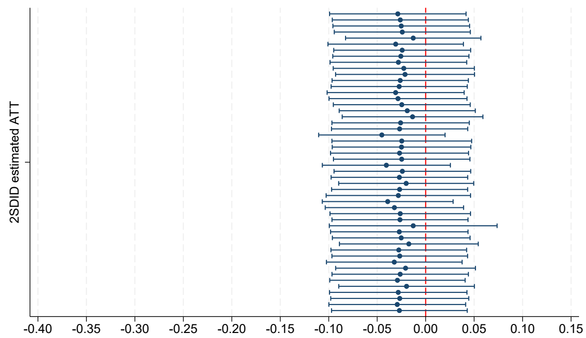
(b) IPH rate, Female-victim 18-49



(c) IPH rate, Male-victim 50-70



(d) IPH rate, Female-victim 50-70



Notes: Each panel reports the 2SDID point estimates together with their 95% confidence intervals, obtained by re-estimating the model in Table 3, panel B, while omitting one state at a time.

A4 Heterogeneous effects by baseline characteristics

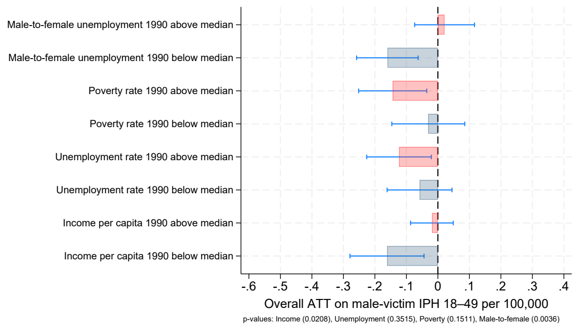
For each characteristic, we define a binary indicator equal to one if the value is above the median and zero otherwise. Figure A4 shows substantial apparent heterogeneity, especially with respect to the male-to-female unemployment ratio. While our analysis in previous sections suggests that parallel 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 A5, the apparent heterogeneity documented in Figure A4 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.[26]

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 A6 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. Once group-specific linear trends are included in Figure A7, 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.

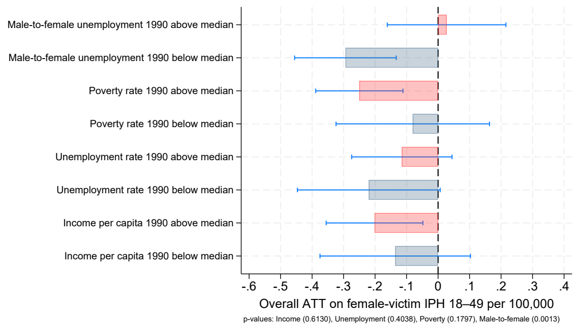
[26] Indeed, once we control for group-specific linear trends, and accounting for the fact that we conduct a total of 16 heterogeneity tests (four outcomes × four baseline characteristics). Applying a Bonferroni correction at the 5% significance level requires p-values $\leq \frac{0.05}{16} = 0.003125$ to reject the null of no heterogeneity. Under this criterion, not even the difference for the impact on male-victim IPH among 18–49-year-olds is statistically significant (p-value = 0.0428).

Figure A4: Heterogeneity Analysis by Gender Inequality and Economic Resources at baseline (1990)

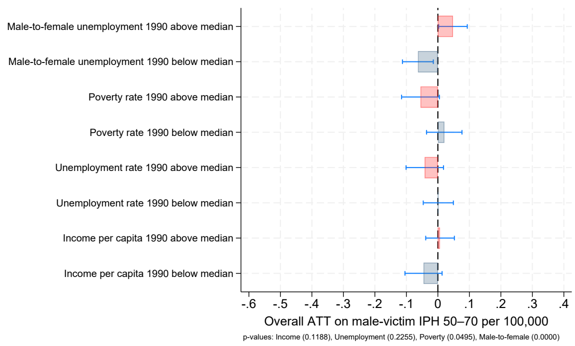
(a) IPH rate, Male-victim 18-49



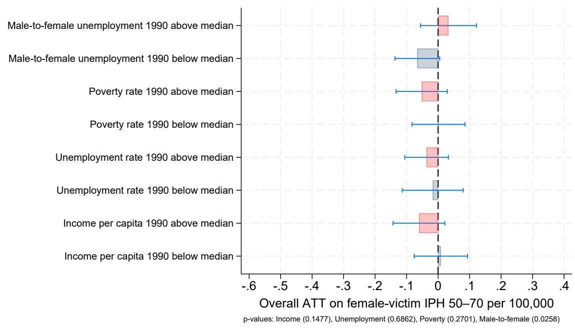
(b) IPH rate, Female-victim 18-49



(c) IPH rate, Male-victim 50-70



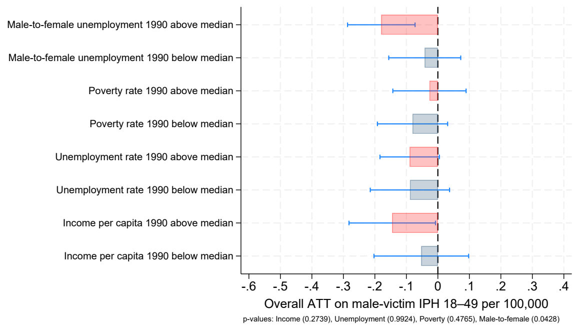
(d) IPH rate, Female-victim 50-70



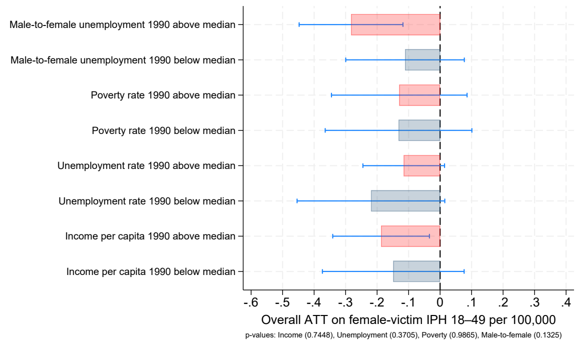
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable D_{st} and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. State and year fixed effects are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). 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 A5: Heterogeneity Analysis by Gender Inequality and Economic Resources at baseline (1990) controlling for group-specific linear trends

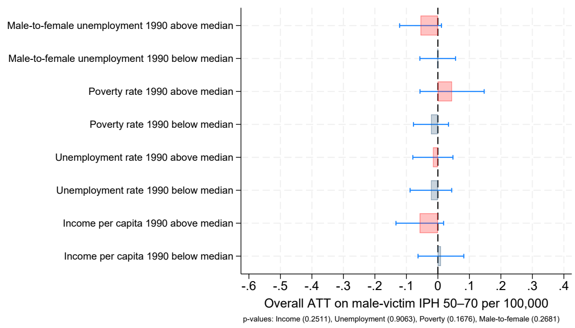
(a) IPH rate, Male-victim 18-49



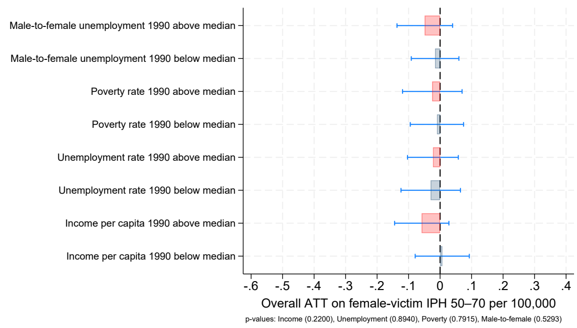
(b) IPH rate, Female-victim 18-49



(c) IPH rate, Male-victim 50-70



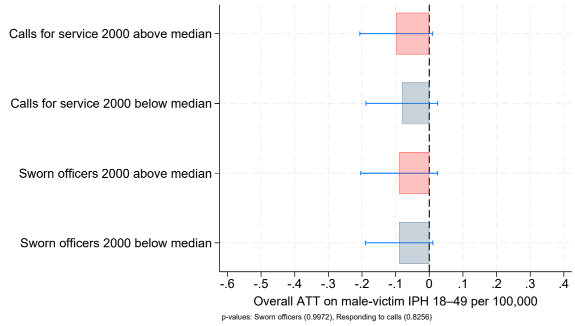
(d) IPH rate, Female-victim 50-70



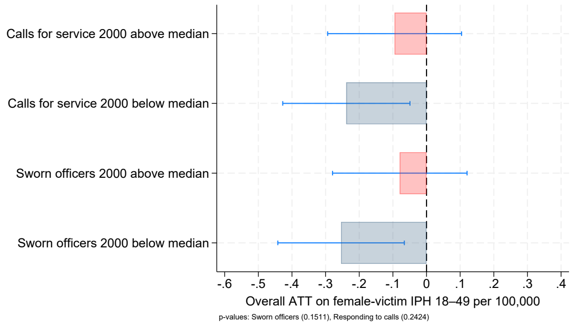
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. The coefficients on the control variable $X_s \times t$, as well as state and year fixed effects, are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). 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 Local Police Resources at baseline (2000)

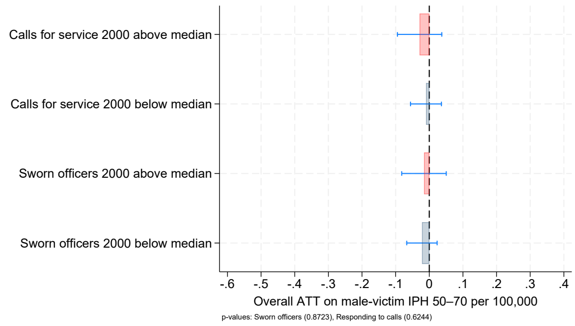
(a) IPH rate, Male-victim 18-49



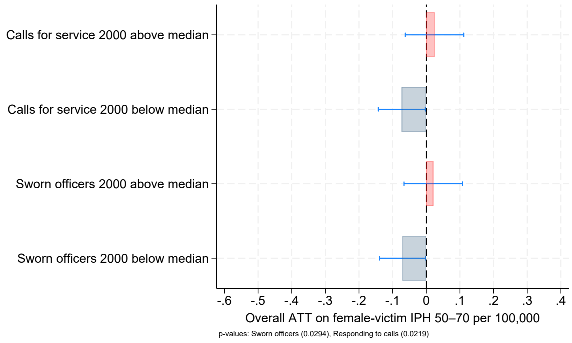
(b) IPH rate, Female-victim 18-49



(c) IPH rate, Male-victim 50-70



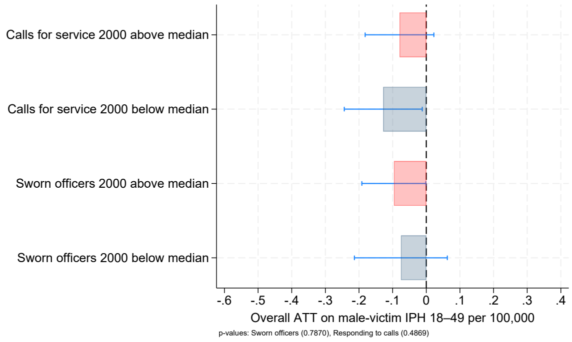
(d) IPH rate, Female-victim 50-70



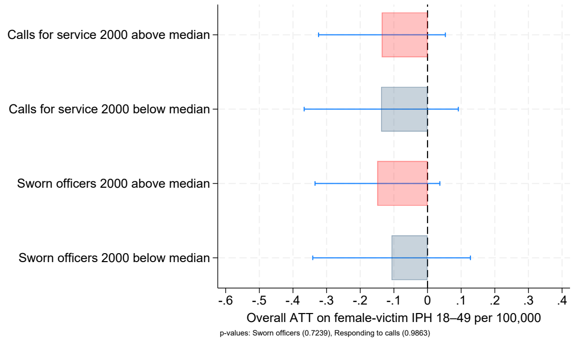
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. State and year fixed effects are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). 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) controlling for group-specific linear trends

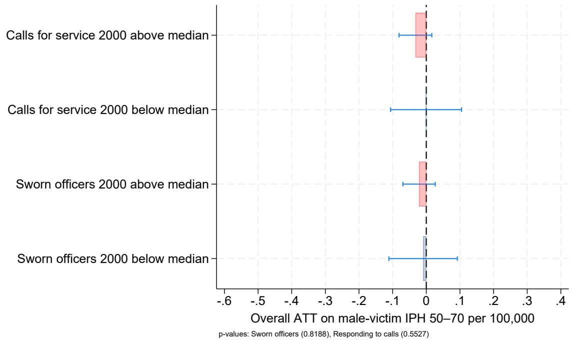
(a) IPH rate, Male-victim 18-49



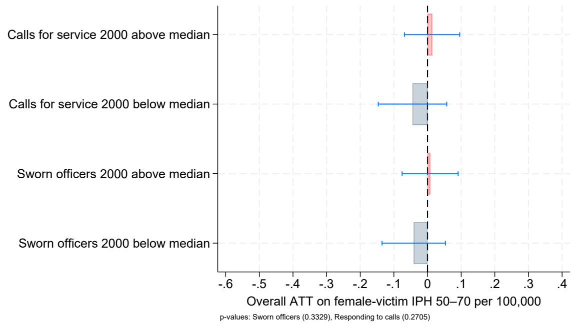
(b) IPH rate, Female-victim 18-49



(c) IPH rate, Male-victim 50-70



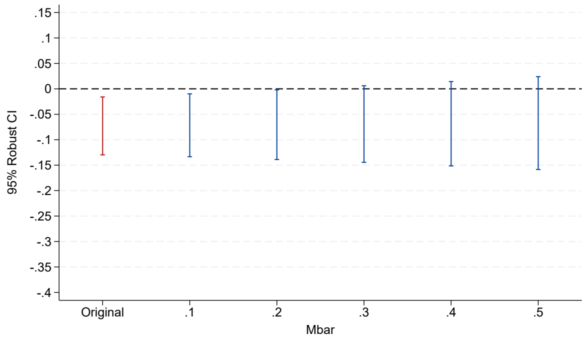
(d) IPH rate, Female-victim 50-70



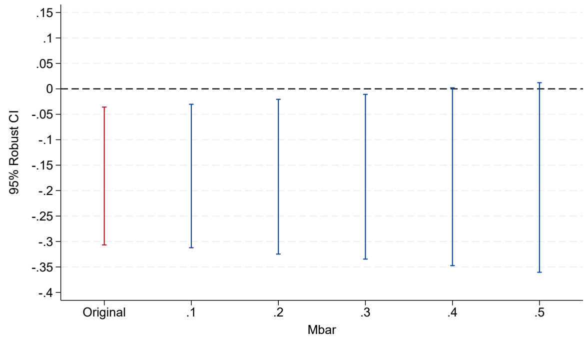
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. The coefficients on the control variable $X_s \times t$, as well as state and year fixed effects, are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). 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: Robustness of 2SDID estimates to violation of parallel trends: ATT for the first year of treatment

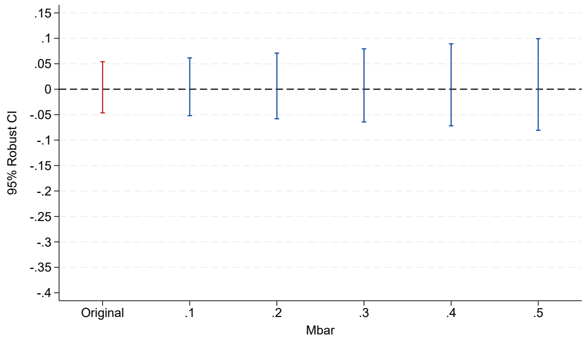
(a) IPH rate, Male-victim 18-49



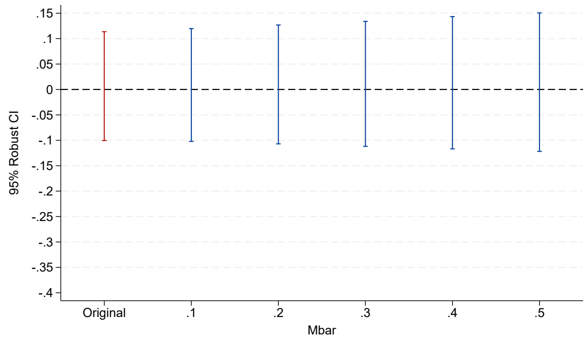
(b) IPH rate, Female-victim 18-49



(c) IPH rate, Male-victim 50-70



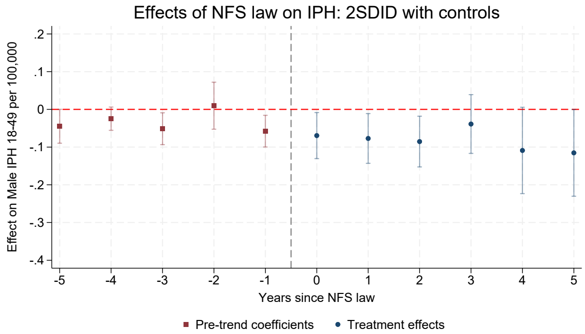
(d) IPH rate, Female-victim 50-70



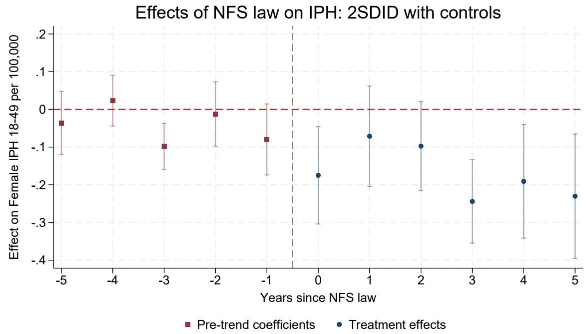
Notes: The figure illustrates the robustness of the 2SDID estimated treatment effect to potential violations of the parallel trends assumption. It reports robust confidence sets for the treatment effect in year zero (the first year after NFS law adoption) under different magnitudes of parallel trends violations (M-bar). The Honest DiD framework was developed by Rambachan and Roth (2023), and the calculations were performed using the `honestdid` Stata package.

Figure A9: 2SDID Event Studies of NFS Laws on IPH rates (per 100,000) with Baseline Covariates Interacted with Linear Time Trends

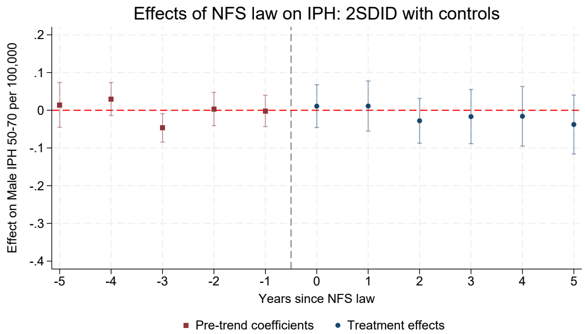
(a) IPH rate, Male-victim 18-49



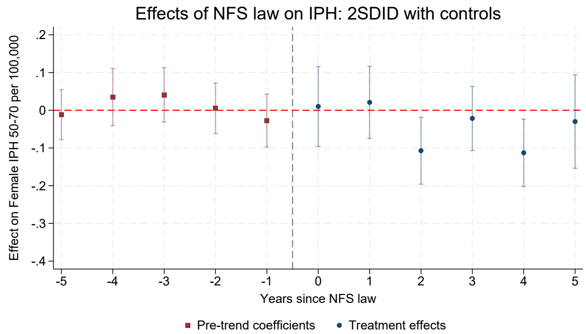
(b) IPH rate, Female-victim 18-49



(c) IPH rate, Male-victim 50-70



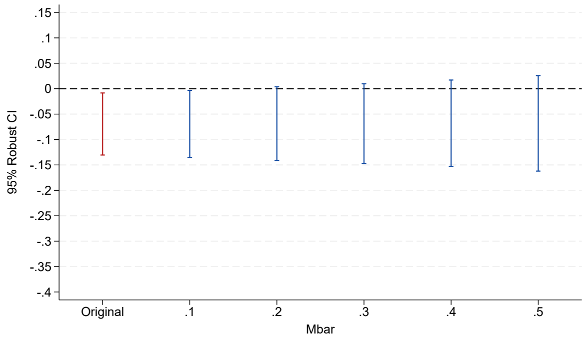
(d) IPH rate, Female-victim 50-70



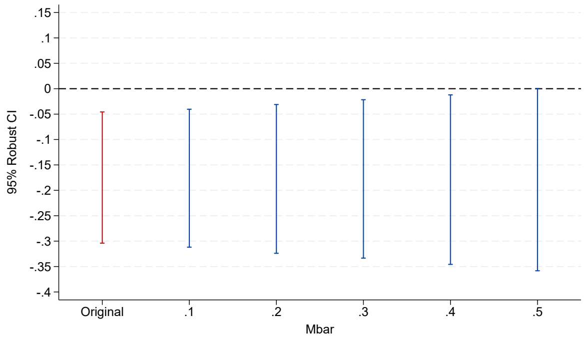
Notes: The event study estimates are based on 2SDID estimates by including the event-time indicators $D_{k,s,t}$ as treatment variables in the second stage. State fixed effects, year fixed effects and the coefficients on covariates for the baseline controls interacted with a time trend are estimated in the first stage for the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). The event study estimates are based on 2SDID.

Figure A10: Robustness of 2SDID estimates (with controls) to violation of parallel trends: ATT for the first year of treatment

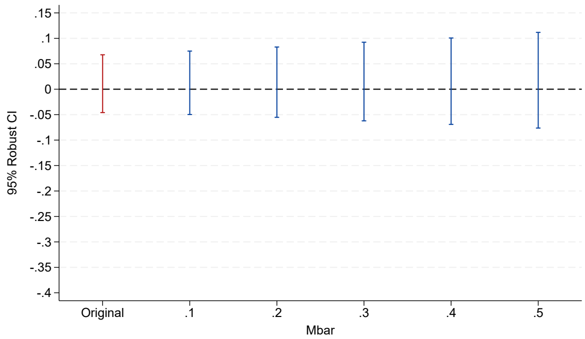
(a) IPH rate, Male-victim 18-49



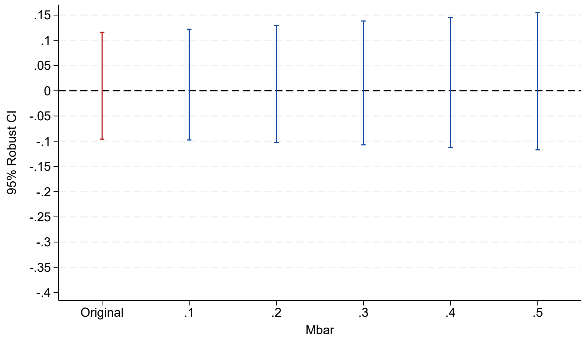
(b) IPH rate, Female-victim 18-49



(c) IPH rate, Male-victim 50-70



(d) IPH rate, Female-victim 50-70



Notes: The figure illustrates the robustness of the 2SDID estimated treatment effect to potential violations of the parallel trends assumption. It reports robust confidence sets for the treatment effect in year zero (the first year after NFS law adoption) under different magnitudes of parallel trends violations (M-bar). The Honest DiD framework was developed by Rambachan and Roth (2023), and the calculations were performed using the `honestdid` Stata package.

Table A12: Effects of NFS Law on Stranger-Perpetrated Homicides per 100,000 by Victim Sex and Age Group

Dependent variable	OLS	2SDID	Mean in 1999	Counterfactual Mean
Male-victim 18-49	-0.086	-0.189	1.116	1.081
	(0.122)	(0.150)		
Female-victim 18-49	0.014	-0.008	0.127	0.111
	(0.017)	(0.022)		
Male-victim 50-70	-0.006	-0.034	0.273	0.338
	(0.032)	(0.033)		
Female-victim 50-70	-0.002	-0.001	0.073	0.053
	(0.013)	(0.019)		

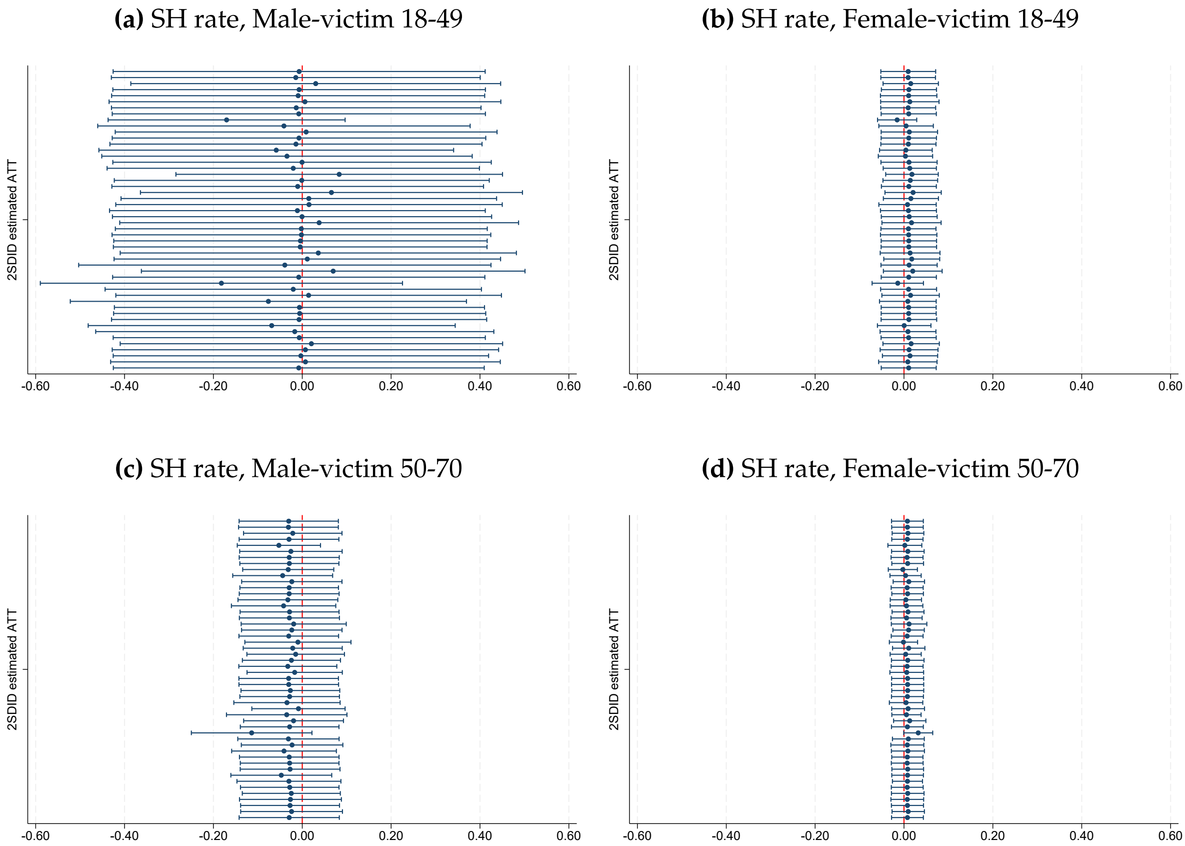
Notes: All regressions include state and year fixed effects. Regressions are weighted by the relevant cohort-age population in 2000. Clustered standard errors (50 clusters). $N = 1,479$. *p<0.1, **p<0.05, ***p<0.01.

Table A13: Effect of NFS Laws on Intimate Partner Homicide (IPH) and Stranger Homicide (SH) Rates (per 100,000)

	IPH rate		SH rate	
	OLS	2SDID	OLS	2SDID
Panel A. Without weights				
Male-victim 18–49	-0.131***	-0.167***	0.080	-0.610
	(0.045)	(0.048)	(0.222)	(1.021)
Female-victim 18–49	-0.105*	-0.180**	0.032	-0.033
	(0.058)	(0.089)	(0.029)	(0.094)
Male-victim 50–70	-0.035	-0.058	-0.043	-0.251
	(0.034)	(0.054)	(0.043)	(0.233)
Female-victim 50–70	-0.023	-0.028	0.021	0.024
	(0.046)	(0.062)	(0.022)	(0.035)
Panel B. With time-varying weights				
Male-victim 18–49	-0.061**	-0.092**	-0.021	-0.008
	(0.030)	(0.043)	(0.139)	(0.212)
Female-victim 18–49	-0.103*	-0.205***	0.023	0.013
	(0.052)	(0.069)	(0.019)	(0.033)
Male-victim 50–70	-0.001	-0.000	-0.002	-0.030
	(0.020)	(0.028)	(0.032)	(0.055)
Female-victim 50–70	-0.035	-0.034	-0.001	0.007
	(0.028)	(0.032)	(0.012)	(0.018)
Panel C. Including South Carolina				
Male-victim 18–49	-0.059*	-0.096**	-0.008	0.064
	(0.031)	(0.043)	(0.140)	(0.221)
Female-victim 18–49	-0.078	-0.145*	0.023	0.022
	(0.062)	(0.075)	(0.018)	(0.032)
Male-victim 50–70	0.001	-0.002	0.018	0.013
	(0.021)	(0.027)	(0.037)	(0.054)
Female-victim 50–70	-0.031	-0.019	0.003	0.013
	(0.026)	(0.030)	(0.013)	(0.019)
Panel D. Using passage instead of effective date				
Male-victim 18–49	-0.059*	-0.094**	-0.045	-0.021
	(0.032)	(0.046)	(0.139)	(0.220)
Female-victim 18–49	-0.110**	-0.204***	0.014	0.006
	(0.053)	(0.067)	(0.018)	(0.033)
Male-victim 50–70	-0.009	-0.011	-0.015	-0.039
	(0.021)	(0.030)	(0.028)	(0.054)
Female-victim 50–70	-0.019	-0.020	-0.004	0.002
	(0.026)	(0.033)	(0.012)	(0.018)

Notes: All regressions include state and year fixed effects and baseline (1990) covariates interacted with linear time trends. Standard errors clustered at the state level (50 clusters). $N = 1,479$.*p<0.10, **p<0.05, ***p<0.01.

Figure A11: Overall ATT estimates on SH rates: Dropping one state at a time



Notes: Each panel reports the 2SDID point estimates together with their 95% confidence intervals, obtained by re-estimating the model in Table 4, while omitting one state at a time.