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Décrio de Assis, Arpita Ghosh, Sonia Oreffice, and Climent Quintana-Domeque

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

Dérico de Assis Arpita Ghosh Sonia Oreffice Climent Quintana-Domeque

December 3, 2025

Abstract

Non-fatal strangulation (NFS) is a dangerous form of intimate partner violence (IPV) and a strong predictor of homicide. We compile data on state NFS statutes in the United States and link them to FBI Supplementary Homicide Reports from 1990 to 2019 to estimate their causal effects on intimate partner homicide (IPH) rates. Using a two-stage difference-in-differences estimator that accommodates staggered adoption and heterogeneous treatment effects, we find that, NFS laws reduce female-victim IPH rates by 14% and male-victim IPH rates by 27%, among those aged 18–49, with no detectable effects for victims aged 50–70 or for homicides committed by strangers. Event-study estimates show flat pre-trends and sustained declines following enactment of NFS laws. Using National Incident-Based Reporting System (NIBRS) data and the same identification strategy, we show that NFS laws increase the share of reported IPV incidents that are classified as aggravated assault among female victims aged 18–49—the most exposed to NFS—and the share of aggravated assaults that result in an arrest for both male and female victims aged 18–49. Our estimates are consistent with NFS laws strengthening the criminal justice response to life-threatening IPV and helping to prevent homicides.

Keywords: Abuse; Assaults; Arrests; Criminal Justice Policy; Gender; Intimate Partner Violence.

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

*Dérico de Assis, University of Nottingham; Arpita Ghosh, University of Exeter; Sonia Oreffice, University of Exeter, HCEO and IZA (s.oreffice@exeter.ac.uk); Climent Quintana-Domeque, University of Exeter, HCEO and IZA (c.quintana-domeque@exeter.ac.uk). Any errors in this paper are our own.

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 many lives.¹ 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 states had enacted NFS statutes,² and these laws explicitly defined and criminalized the act. By recognizing NFS as a distinct and serious offense, these reforms are expected to expand 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 (with near-national coverage of homicide incidents) to estimate the effects of these laws on all intimate partner homicides, disaggregated by the sex and age of the victim. Using NIBRS (which covers incidents reported by law-enforcement agencies with limited and changing geographical coverage), we then examine the share of reported IPV incidents that are recorded as aggravated assaults and arrest rates among aggravated IPV incidents.

¹For example, see the intimate partner homicides of Diana Gonzalez in California in 2010 and Monica Weber-Jeter in Ohio in 2014, detailed in Section 2.

²By 2025, all but one (South Carolina) had done so.

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.³ We find that NFS laws led to substantial reductions in IPH rates among adults aged 18–49. In states that enacted NFS laws, the male-victim IPH rate declined by 27% (from 0.337 to 0.247 per 100,000 men aged 18–49), and the female-victim IPH rate declined by 14% (from 1.221 to 1.052 per 100,000 women aged 18–49). These effects are robust to the inclusion of baseline state covariates interacted with linear time trends.

Estimated effects for older adults (aged 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 year), 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 aged 18–49.

Event-study results based on 2SDID estimates support the parallel trends assumption and show sustained declines following the adoption of NFS laws. 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 violence 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 might NFS laws reduce intimate partner homicides? Using NIBRS data and the same identification strategy, we show that, among agencies reporting to NIBRS, NFS laws increase the share of reported IPV incidents that are classified as aggravated assaults for female victims aged 18–49—the group most exposed to non-fatal strangulation, which is overwhelmingly perpetrated by men (e.g., Sorenson et al.,

³This imputation-based approach has been effectively applied to other staggered policy reforms (e.g., Han, 2023; Smart et al., 2024).

2014; Parekh et al., 2024). We also find that the NFS laws increase the share of aggravated IPV incidents that result in an arrest, for both female and male victims.⁴ While these NIBRS-based estimates reflect a combination of compositional changes (including changes in agency coverage over time and space, crime reporting, and deterrence) and changes in classification and law-enforcement behavior, they are consistent with NFS laws strengthening the criminal justice response to high-risk IPV and helping to prevent homicides.

Our paper makes four contributions. First, we assemble a new state-by-year dataset on non-fatal strangulation (NFS) statutes in the United States. Second, using this dataset linked to the FBI Supplementary Homicide Reports, we provide the first causal evidence on the impact of NFS laws on intimate partner homicide (IPH), estimating heterogeneous effects by victim sex and age.⁵ Third, we study enforcement mechanisms by combining the same identification strategy with NIBRS data, documenting how NFS laws change recorded classification and arrest outcomes in NIBRS-reporting jurisdictions. Fourth, we contribute to broader debates on intimate partner violence (IPV) and gender inequality by showing that a targeted legal reform addressing a predominantly female form of victimization can substantially reduce IPH for both women and men.

Our evidence suggests that NFS laws are 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 offense only in 2022 (Bows and Herring, 2024); Northern Ireland and Ireland in 2023; while in Scotland legislation remains under debate as of

⁴The NIBRS does not allow NFS incidents to be identified separately.

⁵Previous descriptive research by Harruff et al. (2024) analyzes female strangulation homicides in King County, Washington, over four decades, coinciding with the introduction of a felony strangulation statute.

2025 (see Members' Business Motion S6M-19504 (Scottish Parliament, 2025)). Many other countries, including France, Italy, and Spain, still have no standalone offense addressing NFS.

Our findings complement and extend research on legal and institutional changes that shape abusive relationship dynamics by focusing on NFS, an overlooked yet highly predictive form of IPV. Previous work has shown that compulsory schooling reforms in Turkey affect IPV (Erten and Keskin, 2022), that stricter arrest policies in the United States reduce IPV (Chin and Cunningham, 2019), that abortion restrictions raise IPV reports to law enforcement (Dave et al., 2025), that easing access to divorce reduces domestic violence (Brassiolo, 2016), that domestic violence arrests generate incapacitation and deterrence effects (Amaral et al., 2023), and that pressing charges reduces recidivism (Black et al., 2023). They are also consistent with a combination of effects on earlier intervention against abusive partners and reduced reliance on lethal self-defense (Aizer and Dal Bo, 2009; Miller and Segal, 2018).

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 by providing evidence on the enforcement channels through which these effects may 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 aggravated-assault classification and arrests in NIBRS-reporting agencies. 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.⁶ 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).⁷ 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).⁸ Fatal strangulation (including asphyxiation) by an intimate partner accounts for 4.1% of female IPH victims aged 18–49 and 0.2% of male IPH victims aged 18–49, based on our calculations using SHR data. 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 were missed or misattributed, and up to 50% of cases show no vis-

⁶There 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).

⁷“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.

⁸Patch 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.

ible injuries (California District Attorneys Association, 2020, and references therein).

Historically, NFS was often treated as a simple assault, if it was recorded at all. Inadequate statutory tools for classifying NFS as a serious violent offense impeded arrest and prosecution. Quoting Strack and Gwinn (2020, p. 194),

“The lack of physical evidence was causing the criminal justice system to treat many “choking” cases as minor incidents, when, in fact, such cases were the most lethal and violent cases in the system.”

Gwinn and Strack (California District Attorneys Association, 2020, p. 16) further note that such assaults were frequently treated

“much like a slap to the face [...].”

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, but no charges were filed. A few weeks later, he fatally stabbed her on the campus of San Diego City College.⁹ In March 2014 in Ohio, Monica Weber-Jeter was non-fatally strangled by her husband. Despite her police report for non-fatal strangulation, he pled no contest to domestic violence. A few months later, he stabbed her 28 times, and she died from her injuries about a month afterward.¹⁰ 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.¹¹

⁹See San Diego Union-Tribune (2010) and California District Attorneys Association (2020, p. 20).

¹⁰See Jeltseen (2016).

¹¹See Shaffer (2019).

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 states had done so. By 2025, all but one state (South Carolina) had enacted NFS laws.¹² Missouri and North Carolina (H1354) recognized strangulation as a serious 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.¹³

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)

6 Section 1. Title 18 of the Pennsylvania Consolidated

7 Statutes is amended by adding a section to read:

8 § 2718. Strangulation.

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

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

(b) Pennsylvania HB1581 (2016)

¹²See Table A1.

¹³The underlined text in the Figure is added to the statutes by the bills.

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

Testimony in state legislative hearings underscored the legal gaps NFS statutes were intended to fill. For instance, in North Dakota's 2007 hearings on SB2185, a retired police chief urged legislators to "specifically add Strangulation" to strengthen protections for victims.¹⁴ 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.¹⁵ In Montana's 2017 hearings on SB153, advocates similarly emphasized that recognizing strangulation "will help to save lives."¹⁶

¹⁴Dan Draovitch, retired police chief:

"Please, on behalf of our Law Enforcement folks (...) please modify this law to specifically add Strangulation, and strengthen our laws and give us a well defined law to better protect the victims (...)." (North Dakota Legislative Assembly, 2007)

¹⁵"I can also tell you how hard it is to explain to a victim of strangulation that the person who about ended their life could only be charged with a "simple assault" because the victim only had a red mark on their neck and no other visible injury. Imagine having to explain to this person that the maximum penalty for this offense is only 30 days in jail." (North Dakota Legislative Assembly, 2007)

¹⁶"Quite simply, SB 153 will help to save lives." (Montana Coalition Against Domestic and Sexual Violence, 2017)

3 Main Data Sources and Variables

3.1 NFS Laws Taxonomy: Treatment Variable

Despite the widespread adoption of NFS statutes, we are not aware of any systematic dataset documenting their passage and implementation across U.S. states. Prior work has identified 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.¹⁷ Table A1 reports, for each state, the year the law was passed, the year it became effective, and the bill number.

Our main treatment variable is a binary indicator that equals one from the year in which an NFS law became effective in a state onward.¹⁸ 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 (in 2018) and Kentucky (in 2019). Three jurisdictions—Maryland, Ohio, and 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. South Carolina remains the only state without an NFS law as of 2025 and is not included in our main sample.¹⁹

¹⁷We are grateful to Legislative State Librarians across the United States for their assistance in validating the statutory histories.

¹⁸There are only four states where the passage date and effective date differ. In additional robustness checks, we use the passage date to define treatment.

¹⁹The imputation approach we use requires untreated or not-yet-treated observations to identify both state and year fixed effects. Because three jurisdictions never adopted NFS laws by 2019, we have at least three untreated units 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. In additional robustness checks, we include South Carolina in an augmented sample.

Figure 2: Staggered implementation of NFS Laws

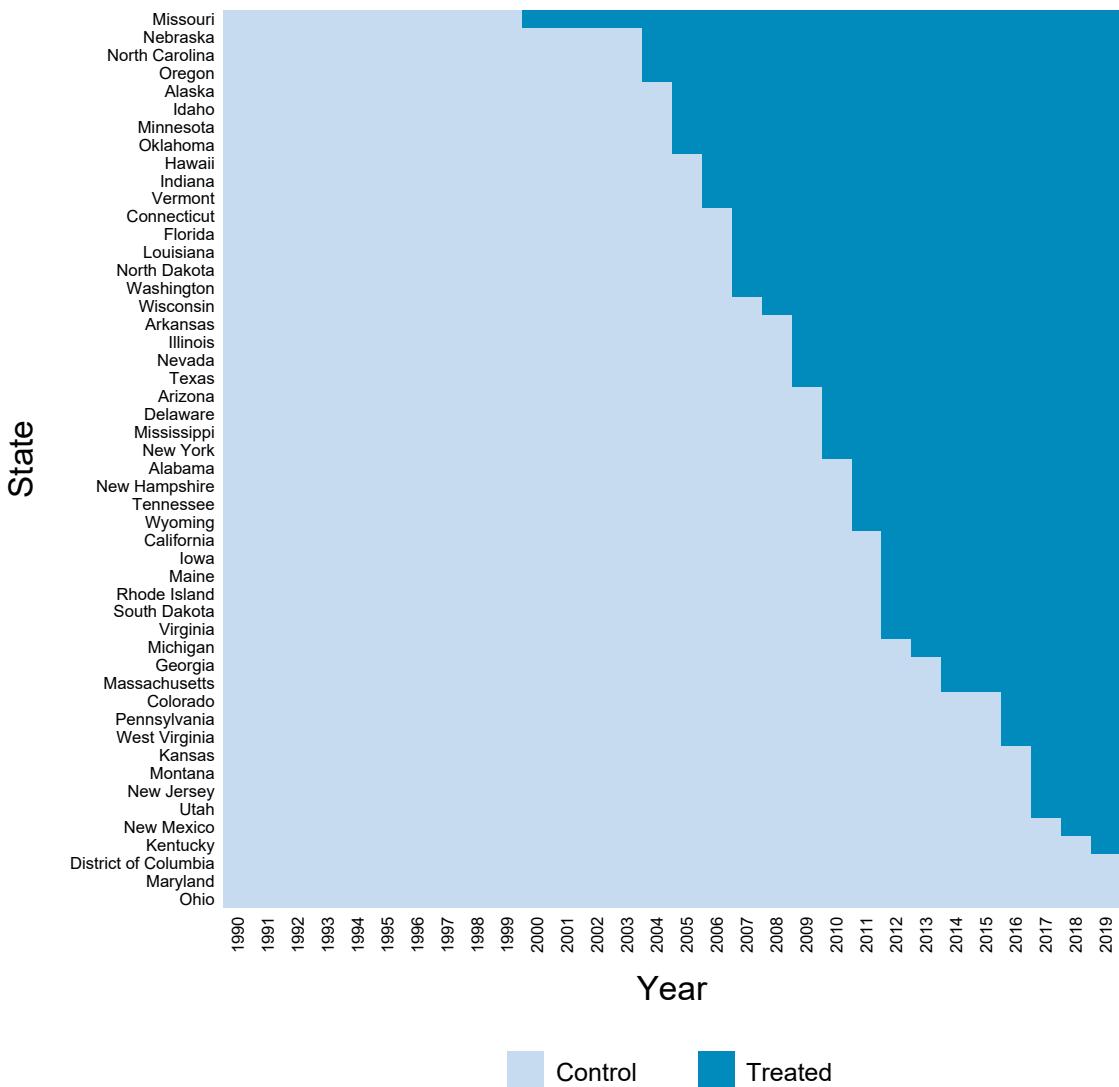


Table 1 reports the distribution of treatment cohorts by year of implementation, showing both the percentage of treated states and the share of the U.S. adult population (ages 18–70) covered by each cohort. As the table shows, cohort size varies substantially across years.

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 $\text{Intimate Partner Homicides}_{d,s,t}$ 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 $\text{Population}_{d,s,t}$ is the corresponding population of that demographic group in state s and year t .²⁰

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).²¹ 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 imputed values from the SHR, following Chin and Cunningham (2019). Homicide

²⁰ Appendix A2 provides additional details on data sources for population and other control variables.

²¹ We obtained the dataset directly from James Alan Fox, who generously provided the 1976–2020 version in 2023.

reporting is missing for 21 state-year cells, resulting in a final sample of 1,479 observations.²²

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 population in demographic group d , state s and year t . This placebo test provides an additional validity check on our identification strategy, discussed in Section 4.

3.3 Reported IPV Incidents Data: Aggravated Assaults and Arrests

We use the National Incident-Based Reporting System (NIBRS) from 1991 to 2019, which records incident-level information submitted by reporting agencies on crimes reported to the police, including offense type (e.g., aggravated assault, simple assault, intimidation), victim and offender characteristics, their relationship, and whether an arrest occurred.

NIBRS does not separately identify NFS, and its coverage is limited. 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 (see Figure A1 in the

²²See Table A3. To assess whether missingness is related to NFS adoption, we regress an indicator for missing homicide reporting on our main treatment variable, 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.

Appendix), but several large states never report (AK, CA, FL, NJ, NY). 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 and Williams, 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.

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 (aggravated assaults, simple assaults, or intimidation) 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 in the homicide data, we analyze two victim age groups: 18–49 and 50–70.

For each demographic group d , state s , and year t , we count these IPV incidents and construct two measures: (i) the fraction of reported IPV incidents classified as aggravated assaults, and (ii) the fraction of reported IPV aggravated-assault incidents associated with an arrest:

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

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

Crucially, both measures are defined only for incidents reported to, and recorded by, law-enforcement agencies that participate in NIBRS in state s and year t . As a result, our regression samples for these IPV ratios cover only a subset of states and years, and the outcomes are constructed from incidents in NIBRS-reporting agencies (see Table A5). 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). D.C., PA, and WY are missing in some sex–age groups. Thus, regression samples for these IPV variables range from 635 to 732 state–year observations.²³ We discuss the interpretation of these ratios, and the selection and compositional issues inherent in NIBRS-based analyses, in Section 7.

²³To 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). 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 in Appendix A3 for details.

4 Empirical Strategy

4.1 Identification of Overall ATT Estimates

Causal parameter and potential outcomes. Let $Y_{d,s,t}(0)$ and $Y_{d,s,t}(1)$ denote, respectively, the potential outcomes for demographic group d (defined by victim's age and sex) in state s and year t in the absence and presence of an NFS law. We define the (possibly heterogeneous) treatment effect as

$$\beta_{d,s,t} \equiv Y_{d,s,t}(1) - Y_{d,s,t}(0),$$

and the observed outcome as

$$Y_{d,s,t} = Y_{d,s,t}(0) + \beta_{d,s,t} D_{s,t},$$

where $D_{s,t}$ is a binary indicator equal to one in the year the NFS law becomes effective in state s and in all subsequent years.²⁴ Our main target parameter is the *overall average treatment effect on the treated* (overall ATT) for group d ,

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

We assume a two-way fixed effects structure for untreated potential outcomes,

$$Y_{d,s,t}(0) = \alpha_{d,s} + \gamma_{d,t} + \varepsilon_{d,s,t}.$$

Hence, the observed outcome for demographic group d in state s and year t is

$$Y_{d,s,t} = \beta_{d,s,t} D_{s,t} + \alpha_{d,s} + \gamma_{d,t} + \varepsilon_{d,s,t}.$$

²⁴An implicit assumption is SUTVA. Cross-state spillovers through advocacy, training, or diffusion of best practices are possible, but major changes in protocols and enforcement typically follow explicit statutory reforms defining non-fatal strangulation as a serious offense. Charging and enforcement practices in homicide and IPV cases are primarily determined by each state's own criminal statutes and procedures. Any adoption of NFS-style practices in control states is therefore likely to be limited.

Two-way fixed effects (TWFE)–OLS estimator. A common empirical approach is to estimate the regression

$$Y_{d,s,t} = \beta_d^{\text{TWFE}} D_{s,t} + \alpha_{d,s} + \gamma_{d,t} + u_{d,s,t}, \quad (4.1)$$

by OLS with standard errors clustered at the state level.²⁵ Following Gardner et al. (2025), we note that

$$u_{d,s,t} = (\beta_{d,s,t} - \beta_d^{\text{TWFE}}) D_{s,t} + \varepsilon_{d,s,t}.$$

If treatment effects for demographic group d are constant across states and over time ($\beta_{d,s,t} = \beta_d$ for all s, t), then equation (4.1) is correctly specified and the TWFE-OLS estimator yields a consistent estimate of β_d , under no anticipation and parallel trends (Roth et al., 2023).

However, as shown by de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), Borusyak et al. (2024), Gardner et al. (2025), and others, when treatment effects vary across states and over time and treatment adoption is staggered, equation (4.1) is misspecified. In that case, while the TWFE-OLS estimator consistently estimates the linear-projection coefficient β_d^{TWFE} , β_d^{TWFE} will generally differ from the overall ATT, β_d . Only in the two-state, two-year case, or when $\beta_{d,s,t} = \beta_d$ for all s, t , does $\beta_d^{\text{TWFE}} = \beta_d$ hold.

Two-stage difference-in-differences (2SDID) estimator. To address the limitations of the TWFE–OLS estimator, we employ the two-stage difference-in-differences (2SDID) estimator proposed by Gardner et al. (2025). The 2SDID procedure estimates state and year fixed effects 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, β_d , is obtained by regressing the residualized outcomes on the

²⁵In standard difference-in-differences settings, state fixed effects $\alpha_{d,s}$ are used to absorb time-invariant state characteristics that may differentially affect each demographic group d , and year fixed effects $\gamma_{d,t}$ are used to capture time-varying nationwide shocks that may differentially affect each demographic group d .

treatment indicator $D_{s,t}$. This procedure yields a consistent estimate of

$$\mathbb{E}[\beta_{d,s,t} \mid D_{s,t} = 1] = \beta_d,$$

provided that parallel trends hold, treatment is not anticipated, and the untreated potential outcome is correctly specified (Gardner et al., 2025).

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. The counterfactual mean, $\mathbb{E}[Y_{d,s,t}(0) \mid D_{s,t} = 1]$, is computed as the average of the predicted outcome $\hat{Y}_{d,s,t}$ —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

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

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 GMM-based procedure (Gardner et al., 2025).²⁶

While the standard TWFE–OLS estimator is based on a specification that treats the conditional mean of both treated and untreated outcomes as linear in state and year fixed effects with a single coefficient on treatment, the 2SDID estimator instead assumes a TWFE structure only for untreated potential outcomes and allows treatment effects to vary flexibly across states and years.

²⁶We implement the 2SDID estimator using the `did2s` Stata package developed by Butts (2021), which has been used in previous research (e.g., 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 as treatment variables in the second stage, after estimating state and year fixed effects among untreated/not-yet-treated observations. Following Gardner et al. (2025), in the second stage we regress

$$Y_{d,s,t} - \hat{\alpha}_{d,s} - \hat{\gamma}_{d,t} = \sum_{k=-K}^K \beta_{d,k} D_{k,s,t} + \eta_{d,s,t}, \quad (4.2)$$

where $\hat{\alpha}_{d,s}$ and $\hat{\gamma}_{d,t}$ are obtained from the first-stage regression of $Y_{d,s,t}$ on state and year fixed effects using only untreated/not-yet-treated observations, and $D_{k,s,t}$ is a binary indicator equal to one if state s is exactly k years relative to adoption in year t , and zero otherwise. When $k < 0$, $D_{k,s,t}$ denotes a *lead* of adoption (an indicator for k years before the adoption of the NFS law). For example, $D_{-1,s,t} = 1$ in the year immediately before adoption. When $k \geq 0$, $D_{k,s,t}$ denotes a *lag* of adoption (an indicator for k years after the adoption of the NFS law); for example, $D_{0,s,t} = 1$ in the year the NFS law becomes effective.

This procedure yields consistent estimates of the dynamic ATT profile $\{\beta_{d,k}\}_{k=-K}^K$ 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 in the relevant age group, using population counts from the 2000 Census. Thus, our estimates can be interpreted as average causal effects of NFS laws on intimate partner homicide rates and on IPV classification and enforcement ratios for men and women in states that passed such laws.²⁷

²⁷Percentages of the population by cohort and age group in 2000 are shown in Table A4.

5 Descriptive Statistics

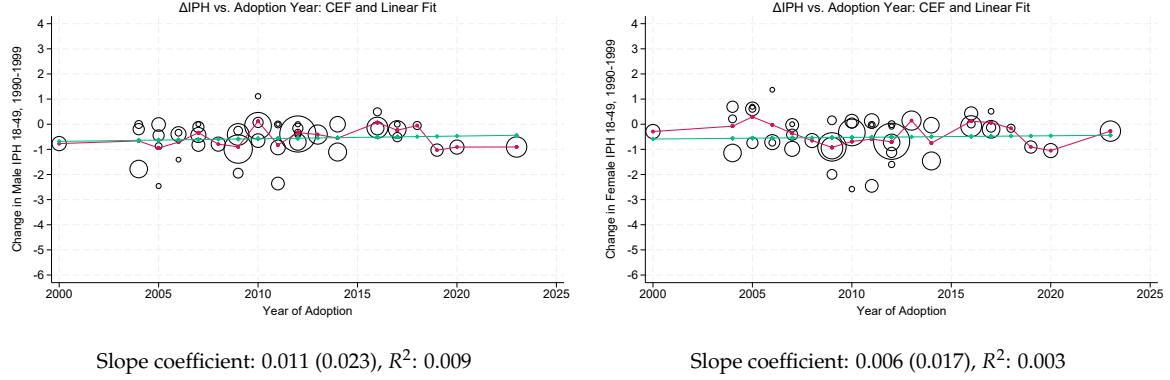
Timing of NFS Law Adoption. We begin by examining whether the timing of NFS law adoption is correlated with pre-treatment trends in IPH. To do so, we regress the change in IPH between 1990 and 1999—the year before Missouri enacted the first NFS statute—on the year in which each state adopted an NFS law. While our main analysis focuses on the period 1990–2019, so that Maryland, D.C., and Ohio are treated as “never-treated” units, in this exercise we include all states, since all states in our main sample eventually adopted NFS laws, including Maryland in 2020 and D.C. and Ohio in 2023.

Figure 3 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.

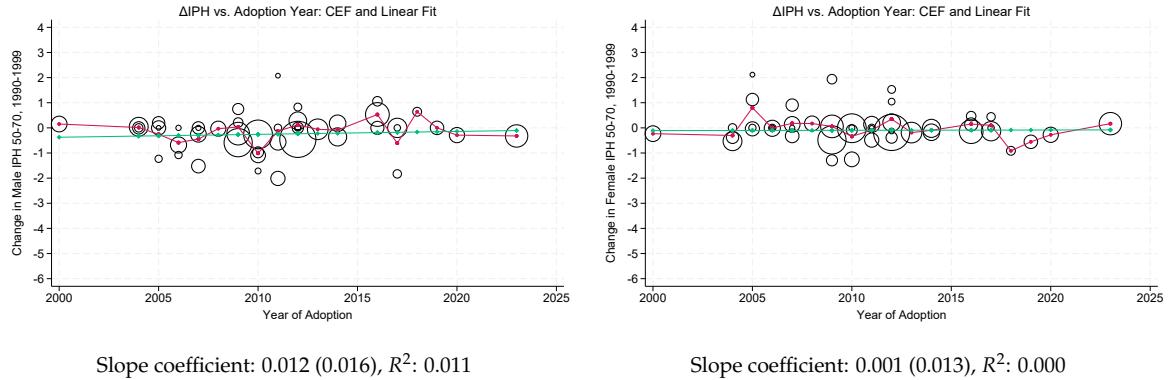
Similarly, regressions of changes in poverty rates, income per capita, unemployment rates, and the male-to-female unemployment ratio from 1990 to 1999 on the year of adoption show no statistically significant relationships (Table A7). In addition, the same variables measured in 1990 are not systematically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

Figure 3: 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 (b) Δ IPH Female-victim 18–49 & Year of Adoption



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



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 descriptive evidence on its plausibility 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 at conventional levels. For male victims aged 18–49, the 1990–1999 change in the IPH rate is significantly more negative in never-treated states. For the other three demographic groups, changes in IPH rates are similar in treated and never-treated states over this period. While this descriptive analysis of pre-trends is informative, a fuller assessment is provided by the event-study estimates in subsection 6.2.²⁸

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, Panel A, compares baseline (1990) 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 at conventional levels ($p < 0.05$) emerges (poverty rates), while differences in the remaining variables are small and statistically insignificant. No mean differences are found in 1999 (Table A9, Panel B), the year before the enactment of the first NFS law. In terms of changes over 1990–1999 (Table A9, Panel C), we detect one marginally significant difference in the trend of the male-to-female unemployment ratio ($p < 0.10$).

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.

²⁸Note that the event-study estimates need not align mechanically with this descriptive evidence.

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 in the absence of NFS laws, computed as described in Section 4 and detailed in the footnote to Table 3.

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 the male-victim IPH rate 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.²⁹

²⁹Covariates 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 Appendix A2 for further details.

Table 3: Effect of NFS Laws on Intimate Partner Homicides per 100,000 (IPH rate), by Victim Sex and Age Group

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

Notes: The first two columns report OLS and 2SDID estimates from regressions of the corresponding 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 last two columns show the 1999 mean and the counterfactual mean. The counterfactual mean, $\mathbb{E}[Y_{d,s,t}(0) | D_{s,t} = 1]$, 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 different 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. As in Aizer and Dal Bo (2009) and Miller and Segal (2018), we also attribute the sizable declines in the male-victim IPH rate 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:

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

Using the estimated $\mathbb{E}[\beta_{d,s,t} \mid D_{s,t} = 1]$, $\hat{\beta}_d$, from panel A in Table 3, this back-of-the-envelope calculation implies approximately 1,029 fewer female and 547 fewer male IP homicides among adults aged 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 regressions 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 D.C. had enacted statutes providing civil remedies for battered women through protection orders (Hart, 1991; Benitez et al., 2010). By the early 1990s, 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; subsequent reforms in the 1990s mainly expanded the scope of these policies and increased federal support. For instance, legal

chronologies indicate that most warrantless domestic-violence arrest statutes were enacted in the 1980s or early 1990s, with only a small number of adoptions or recodifications around 2000 (e.g. Dasgupta and Pacheco, 2018; Zeoli et al., 2011b,a).³⁰ Consequently, potentially confounding statewide policies such as mandatory or pro-arrest laws, protection-order provisions, unilateral divorce statutes, and victims' rights protections were largely implemented well before the first adoption of NFS laws in 2000, with only modest further evolution thereafter. As shown in Appendix Table A12, the estimated effect of non-fatal strangulation laws on intimate partner homicide is very similar when excluding states that experienced post-1999 changes in domestic-violence policing regimes.

We also test for heterogeneous effects of NFS laws (see Appendix A4, Figures A4–A7) and find no evidence that the effects of the NFS laws vary by baseline socioeconomic conditions or gender inequality in 1990, or by local police resources in 2000 (earliest available year). 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).³¹

³⁰Chin and Cunningham (2019) show that coverage of current, former, and common-law spouses under these statutes was already in place in virtually all states by the late 1980s, with most post-2000 changes reflecting expansions to dating partners rather than new arrest powers *per se* (their Table 2). Clarke et al. (2024) likewise classify most states in their 36-state NIBRS sample as having preferred or mandatory warrantless arrest provisions already in place by 2000 and treat these regimes as stable over 2000–2009.

³¹The 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 4 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 as 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.

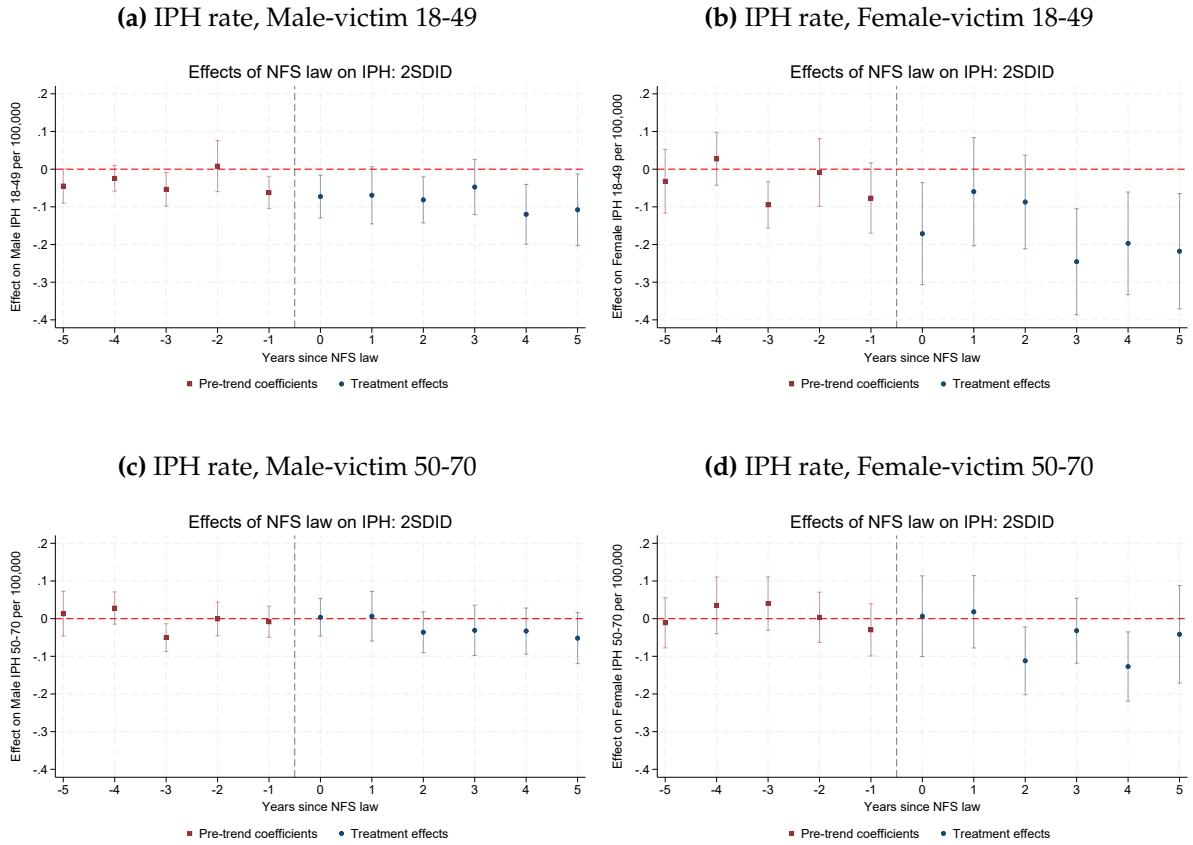
The post-treatment coefficients (shown as blue dots) indicate substantial and persistent declines in IPH for both male and female victims aged 18–49, consistent with the overall ATT estimates in Table 3. In contrast, dynamic effects are close to zero for male victims aged 50–70 and smaller in magnitude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

Despite the single significant difference in the 1990–1999 changes in IPH between eventually-treated and never-treated states for male victims aged 18–49 (Table 2), the event-study estimates in Figure 4 (panel (a)) show pre-treatment coefficients that are close to zero. This is not inconsistent with the descriptive evidence in Table 2, which compares average changes in IPH between 1990 and 1999 for never-treated and eventually-treated states, whereas the event study reports average treatment effects by event time, that is, relative to the year of adoption.

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, Appendix Figure A9 shows that the dynamic patterns remain similar when adding baseline covariates (measured in 1990) 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 4: 2SDID Event Studies of NFS Laws on IPH rates



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 using the *did2s* Stata package developed by Butts (2021).

6.3 Falsification test: Homicides Rates by Strangers

We conduct a falsification test by examining whether NFS laws affected homicides committed by strangers, disaggregated by victim sex and age group, in the spirit of Chin and Cunningham (2019). Because NFS indicates violence 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.³² In the Appendix, Table A13 reports analogous placebo estimates without covariate trends, and Figure A11 shows that the placebo 2SDID estimates are not driven by any single state by re-estimating the regressions while dropping one state at a time.

³²Appendix Table A12 shows that the estimated effect of non-fatal strangulation laws on stranger-homicide rates remain small and statistically indistinguishable from zero across all specifications when excluding states that experienced post-1999 changes in domestic-violence policing regimes.

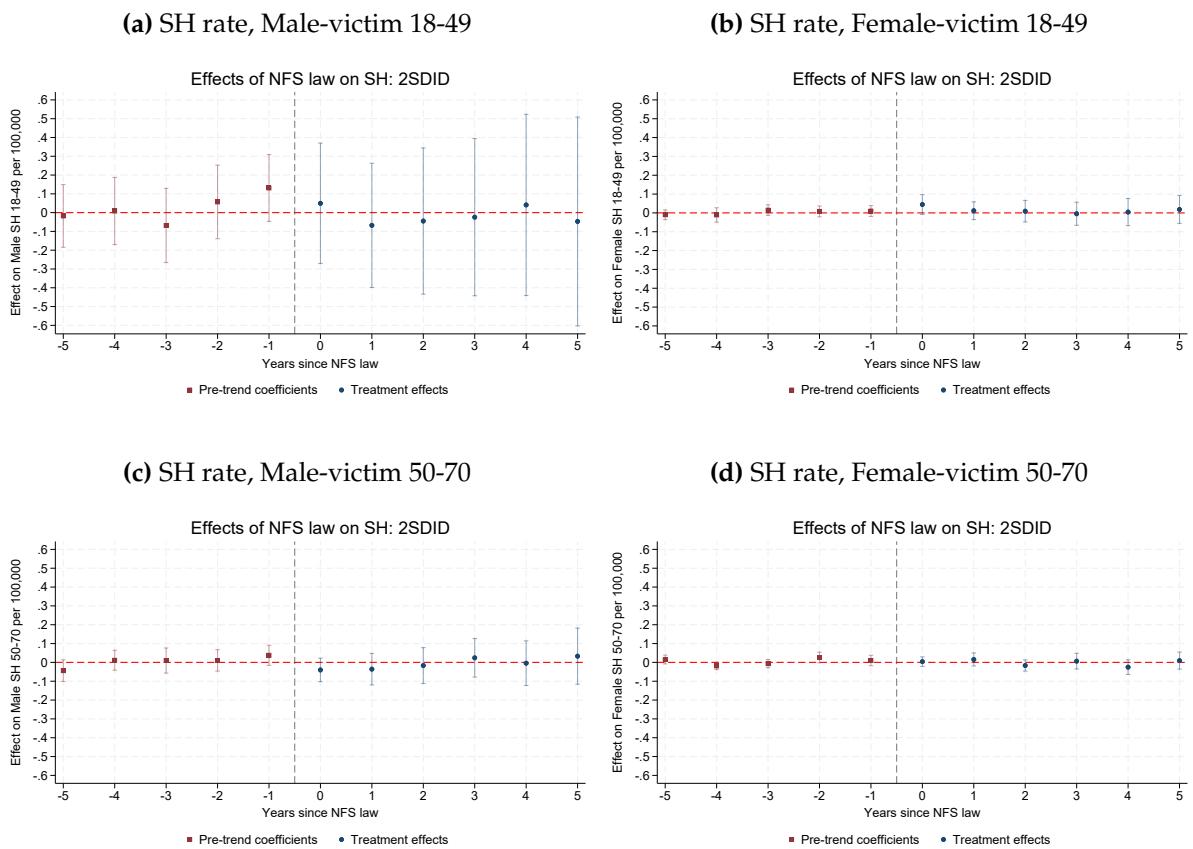
Table 4: Effects of NFS Laws on Stranger-Perpetrated Homicides per 100,000 (SH rate) by Victim Sex and Age Group, with Baseline Covariates Interacted with Linear Time Trends

	OLS	2SDID	Mean 1999	Counterfactual Mean
Male-victim 18-49	-0.025 (0.139)	-0.007 (0.213)	1.116	0.899
Female-victim 18-49	0.021 (0.018)	0.011 (0.032)	0.127	0.092
Male-victim 50-70	-0.000 (0.034)	-0.029 (0.057)	0.273	0.333
Female-victim 50-70	0.001 (0.013)	0.008 (0.018)	0.073	0.044

Notes: The first two columns report OLS and 2SDID estimates from regressions of the corresponding SH rate on an indicator for NFS law adoption, including 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. The last two columns show the 1999 mean and the counterfactual mean. The counterfactual mean, $\mathbb{E}[Y_{d,s,t}(0) | D_{s,t} = 1]$, 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 at the state level (50 clusters) are reported 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 5, the event-study coefficients display no evidence of systematic post-treatment effects, providing additional support for our identification strategy. This falsification exercise is consistent with NFS laws not influencing broader homicide trends unrelated to intimate partner violence.

Figure 5: 2SDID Dynamic 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



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 using the `did2s` Stata package developed by Butts (2021).

6.4 Additional Robustness Checks

We report additional robustness exercises in Appendix Table A14, including: (a) unweighted regressions; (b) regressions using time-varying state population weights; (c) inclusion of South Carolina, the only state without an NFS statute by 2025; and (d) coding the treatment indicator based on the law's passage year rather than its effective year. Results are generally similar across weighting schemes and timing definitions. In unweighted specifications, point estimates for male IPH are somewhat larger in magnitude, but the overall qualitative conclusions and significant patterns remain unchanged, for both the main estimates in Table 3 and the placebo estimates in Table 4, reinforcing the reliability of our main findings.

7 How NFS laws may reduce intimate partner homicides: evidence from reported IPV incidents

The SHR results above show that NFS laws substantially reduce intimate partner homicides among adults aged 18–49. In this section, we use IPV incident-level data from the National Incident-Based Reporting System (NIBRS) to shed light on how these laws may operate. We examine whether reported IPV incidents are more likely to be classified as aggravated assaults, and whether incidents classified as aggravated assaults are more likely to result in an arrest.

7.1 NIBRS incident-level framework and interpretation

NIBRS records only incidents that are reported to the police and formally recorded and submitted by participating agencies (see subsection 3.3 for details). In our framework, this means that we observe classification and arrest outcomes only for reported incidents ($I = 1$) in NIBRS-reporting agencies ($N = 1$); by definition, an unreported incident ($I = 0$) cannot be classified or result in an arrest. Therefore, any NIBRS analysis is necessarily based on a selected sample of incidents that reach this reported-and-recorded stage.

Within this reported ($I = 1$) and covered by NIBRS ($N = 1$) sample, we focus on two conditional probabilities. First, we study the conditional probability that the IPV incident is classified as an aggravated assault ($A = 1$),

$$P(A = 1 \mid I = 1, N = 1).$$

Second, among reported IPV incidents ($I = 1$) that are recorded as aggravated assaults ($A = 1$), we study the probability that the incident is associated with an arrest ($R = 1$),

$$P(R = 1 \mid A = 1, I = 1, N = 1).$$

These conditional probabilities characterize the fraction of IPV incidents classified as aggravated-assaults, and the fraction of aggravated-assaults that result in an arrest, among NIBRS-reporting agencies. Empirically, we estimate how these probabilities change using the classification and enforcement ratios defined in subsection 3.3.³³

Figure 6 summarizes how NFS laws may initiate a causal chain within the law enforcement system’s response to intimate partner violence (IPV) incidents. Let D denote whether an NFS law is in place and U an unobserved measure of underlying severity or risk (for example, how dangerous the relationship, offender, or incident is). Let I indicate that an IPV incident is reported to the police and formally recorded, A that a recorded IPV incident is classified as an aggravated assault, and R that the incident results in an arrest. The graph allows both D and U to influence whether an IPV situation becomes a reported incident (I), how reported incidents are classified (A), and whether they lead to an arrest (R). Moreover, the severity or risk of the incident (U) may affect whether an incident is reported, how it is classified, and whether it results in an arrest. Reporting I affects classification A , and classification A in turn affects arrest R .

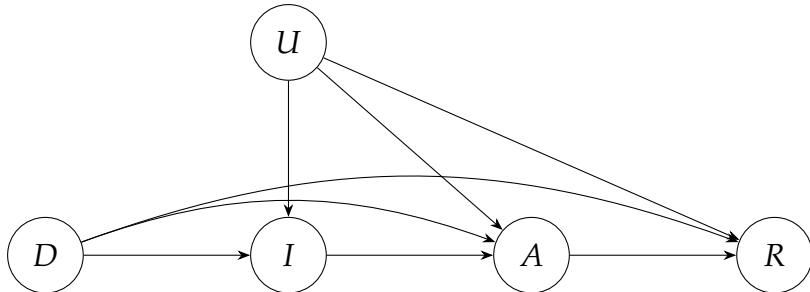


Figure 6: Incident-level causal diagram for reporting, classification, and arrest

Because our NIBRS outcomes are defined only for incidents with $I = 1$, we are effectively conditioning on I , which is a collider between D and the unobserved severity U on paths such as $D \rightarrow I \leftarrow U \rightarrow A$ and $D \rightarrow I \leftarrow U \rightarrow R$. Conditioning on $I = 1$ thus opens backdoor paths from D to A and R through U . Conse-

³³The classification and enforcement ratios in subsection 3.3 can be interpreted as aggregate counterparts to the conditional probabilities in an incident-level framework. In particular, Classification _{d,s,t} approximates $P(A = 1 | I = 1, N = 1)$, and Enforcement _{d,s,t} approximates $P(R = 1 | A = 1, I = 1, N = 1)$.

quently, changes in the conditional probabilities $P(A = 1 | I = 1, N = 1)$ and $P(R = 1 | A = 1, I = 1, N = 1)$ —and in their empirical counterparts—cannot be interpreted as pure stage-specific causal effects of NFS laws on classification or arrest holding the distribution of incidents fixed. Instead, our estimates describe the “total” effects of NFS laws on these conditional probabilities in NIBRS-reporting agencies, which reflect a combination of *changes in recorded case processing* (how reported IPV incidents are classified and whether aggravated assaults result in arrest) and *changes in the composition of incidents* that reach the reporting stage, driven by both reporting behavior and law-induced changes in the underlying distribution of IPV (for example, via deterrence).³⁴

7.2 Estimated Effects on Classification and Enforcement Ratios

Table 5 shows that NFS laws increase the share of reported IPV incidents recorded in NIBRS that are classified as aggravated assaults—and only for female victims, who are far more likely to experience NFS. Among women aged 18–49, this fraction rises by approximately 5.5 percentage points, from 7.8% to 13.3%. While this pattern is consistent with an environment in which, under NFS laws, a larger share of reported IPV incidents involving women are recorded as serious violent offenses, it may also reflect compositional changes driven by both reporting behavior and deterrence-induced shifts in the underlying incidence and severity of IPV.

Conditional on aggravated classification, we also observe higher arrest rates. Table 6 shows that NFS laws increase the arrest rate among IPV aggravated assaults recorded in NIBRS. For female victims, the likelihood that an aggravated IPV assault results in an arrest rises by about 12 percentage points (from 48% to 60%); for male victims, the increase is roughly 15 percentage points (from 47% to 62%). These shifts in conditional arrest rates are consistent with an enforcement mechanism in which aggravated IPV incidents are more likely to trigger decisive police action when NFS

³⁴NFS laws may change the frequency and type of IPV incidents, which incidents are reported to the police or formally recorded, how recorded incidents are coded, and which agencies participate in NIBRS.

laws are in place, although, as with the classification results, they may also reflect compositional changes in which aggravated IPV incidents enter NIBRS and reach the recorded-aggravated stage.

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

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

Notes: The first two columns report OLS and 2SDID estimates from regressions of the classification ratio on an indicator for NFS law adoption, and state and year fixed effects. The last three columns show the 1999 mean, the counterfactual mean, and the sample size and number of clusters. 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 Enforcement by Victim Sex and Age Group

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

Notes: The first two columns report OLS and 2SDID estimates from regressions of the enforcement ratio on an indicator for NFS law adoption, and state and year fixed effects. The last three columns show the 1999 mean, the counterfactual mean, and the sample size and number of clusters. 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.

The patterns for male victims help further interpret our enforcement estimates: we do not detect a statistically significant change in the share of IPV incidents classified as aggravated assaults (the classification ratio is flat), but we do find an increase in the arrest rate among aggravated IPV incidents (the enforcement ratio). At the state–year level, this suggests that, in NIBRS-reporting agencies, NFS adoption is associated with a higher probability that a male-victim aggravated IPV incident recorded in NIBRS results in an arrest, even though the overall share of male IPV incidents coded as aggravated does not change. While selection and composition concerns remain, the absence of a detectable association between NFS laws and the classification rate for men makes it less compelling to attribute the increased arrest rate among male-victim aggravated assaults solely to large shifts in which male IPV incidents are classified as aggravated. Instead, the pattern is consistent with changes in arrest behavior for aggravated cases that would likely have been recorded as such even in the absence of the law. These interpretive remarks should be viewed as suggestive rather than as separately identified causal channels.

Dynamic 2SDID event-study estimates echo these findings. The coefficients show

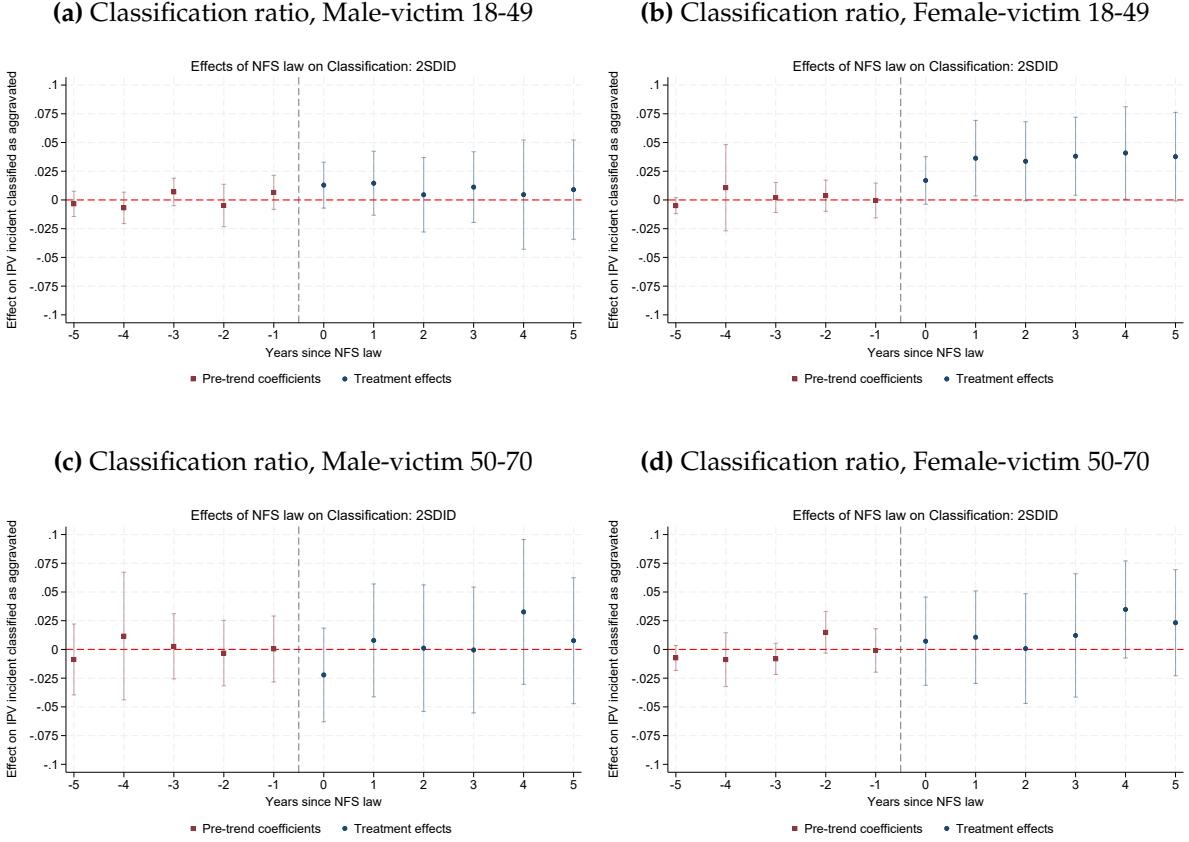
no systematic pre-trends and, within one year of adoption, we see increases in both the share of reported IPV incidents recorded in NIBRS that are classified as aggravated assaults (for female victims aged 18–49; see Figure 7, panel (b)) and the arrest rate among IPV aggravated assaults recorded in NIBRS (for both female and male victims aged 18–49; see Figure 8, panels (a) and (b)), while reductions in intimate partner homicides in the SHR data emerge with a one- to two-year lag.

As complementary evidence, Appendix Table A15 reports estimated effects of NFS laws on reported IPV incidents and arrests per 100,000 population (in a given demographic group, state and year). We estimate sizable reductions in reported IPV incidents per 100,000 for both men and women and across age groups.³⁵ Among female victims aged 18–49, the 2SDID estimate implies a decline of roughly one quarter, from a counterfactual mean of 550.07 to 417.76 reported IPV incidents per 100,000. Among male victims aged 18–49, the 2SDID estimate implies a reduction of roughly one fifth, from a counterfactual mean of 115.11 to 93.08.³⁶

³⁵These rates are per 100,000 of the state population, not the NIBRS-covered population.

³⁶Estimated effects on aggravated IPV per 100,000 and on arrests per 100,000 are smaller in magnitude and imprecisely estimated.

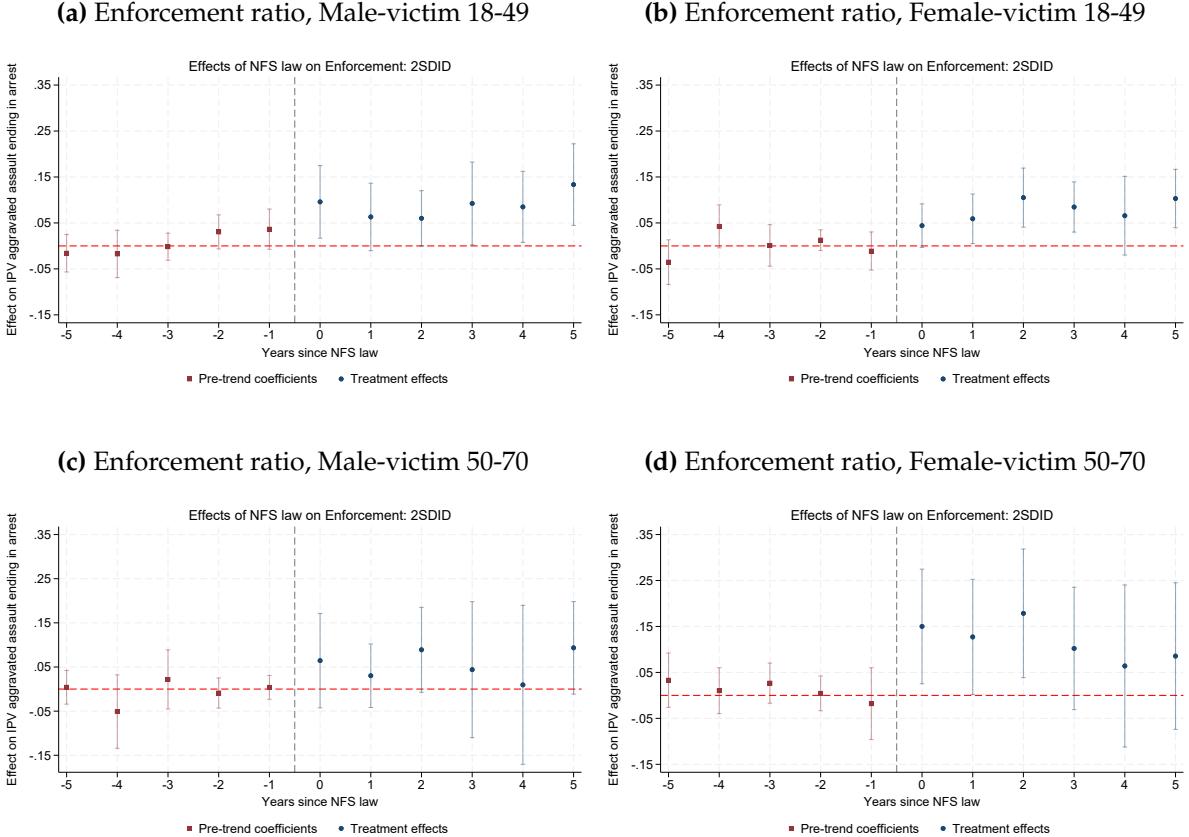
Figure 7: 2SDID Event Studies of NFS Laws on Classification



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 using the did2s Stata package developed by Butts (2021).

Even though our analysis with NIBRS has several limitations, taken together with the SHR results on intimate partner homicide, it is consistent with law-enforcement mechanisms—through deterrence, earlier intervention, or both—in which NFS laws reduce the incidence of high-risk abuse and/or increase incapacitation of abusive partners, thereby lowering lethal outcomes. We cannot, however, separately identify these mechanisms.

Figure 8: 2SDID Event Studies of NFS Laws on Enforcement



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 using the did2s Stata package developed by Butts (2021).

Finally, we note that our enforcement proxy (arrests per reported aggravated IPV assault) captures, at best, policing responses rather than downstream prosecutorial or sentencing outcomes. To illustrate how improved recognition of strangulation can affect downstream judicial and forensic responses, we observe that jurisdictions that have adopted detailed strangulation-specific investigative protocols, such as Maricopa County, Arizona, have reported increases in prosecution rates (Maricopa County Attorney's Office, 2013).

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 and is associated with later homicide.

This paper makes four contributions. First, we make available a new state-by-year dataset on NFS statutes, which can support future work on legal responses to high-risk abuse. Second, merging these data with the FBI Supplementary Homicide Reports (1990–2019), we estimate the causal effects of NFS laws on intimate partner homicides, documenting sizable reductions for both women and men aged 18–49. Third, by combining the same identification strategy with National Incident-Based Reporting System data (1991–2019), we document changes in NIBRS-reporting jurisdictions—toward more frequent aggravated-assault in IPV classification and higher arrest rates in aggravated IPV cases—that are consistent with earlier and stronger criminal-justice intervention. Fourth, our results inform broader debates on IPV and gender inequality by illustrating how a targeted legal reform addressing a predominantly female form of victimization can also improve safety for men.

Taken together, the evidence indicates that NFS laws can substantially reduce intimate partner homicides, plausibly by enabling earlier intervention against high-risk abuse, although we cannot separately identify the underlying channels. 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 meaningfully enhance victim safety.

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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

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. 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 unemployment 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

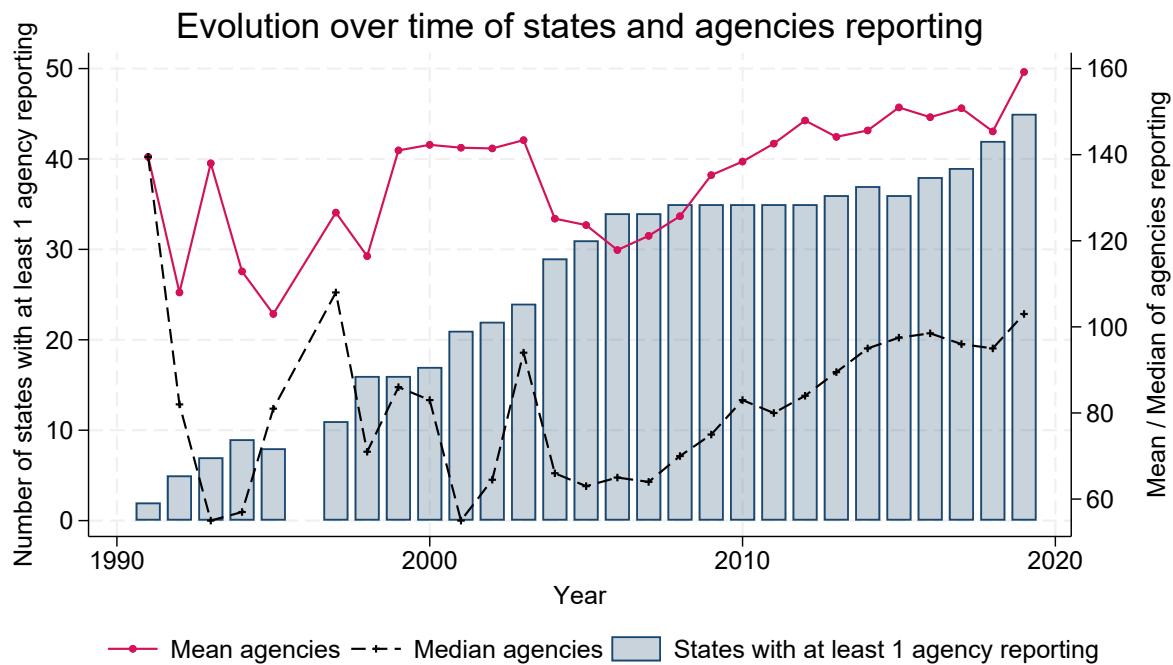


Table A4: Percentage of Population by Cohort and Age Group in 2000

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)

Group	Classification		Enforcement	
	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 (1 if missing, 0 else)		
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 (1 if missing, 0 else)		
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 OLS and 2SDID estimates 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. 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 (44.93)	0.016
Δ log(income per capita)	-0.0002 (0.0009)	0.001
Δ unemployment rate	0.044 (0.041)	0.037
Δ poverty rate	0.002 (0.086)	0.000
Δ male-to-female unemployment	0.028 (0.022)	0.064

Notes: Each cell in the first column reports the coefficient from a separate bivariate 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 (90.72)	0.020
log(income per capita)	0.0038 (0.0044)	0.018
unemployment rate	-0.0126 (0.0246)	0.007
poverty rate	-0.081 (0.0861)	0.014
male-to-female unemployment	-0.0075 (0.0157)	0.010
IPH rate, male-victim 18-49	-0.0255* (0.0134)	0.030
IPH rate, female-victim 18-49	-0.0141 (0.0263)	0.007
IPH rate, male-victim 50-70	-0.0202 (0.0121)	0.046
IPH rate, female-victim 50-70	-0.0020 (0.0143)	0.001

Notes: Each cell in the first column reports the coefficient from a separate bivariate 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 Laws on Intimate Partner Homicides (IPH counts) by Victim Sex and Age Group: Poisson Model

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

Notes: All regressions include state and year fixed effects. 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 Laws on Intimate Partner Homicides per 100,000 (IPH rate) by Victim Sex and Age Group, excluding Missouri (2000) and North Carolina (2004)

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

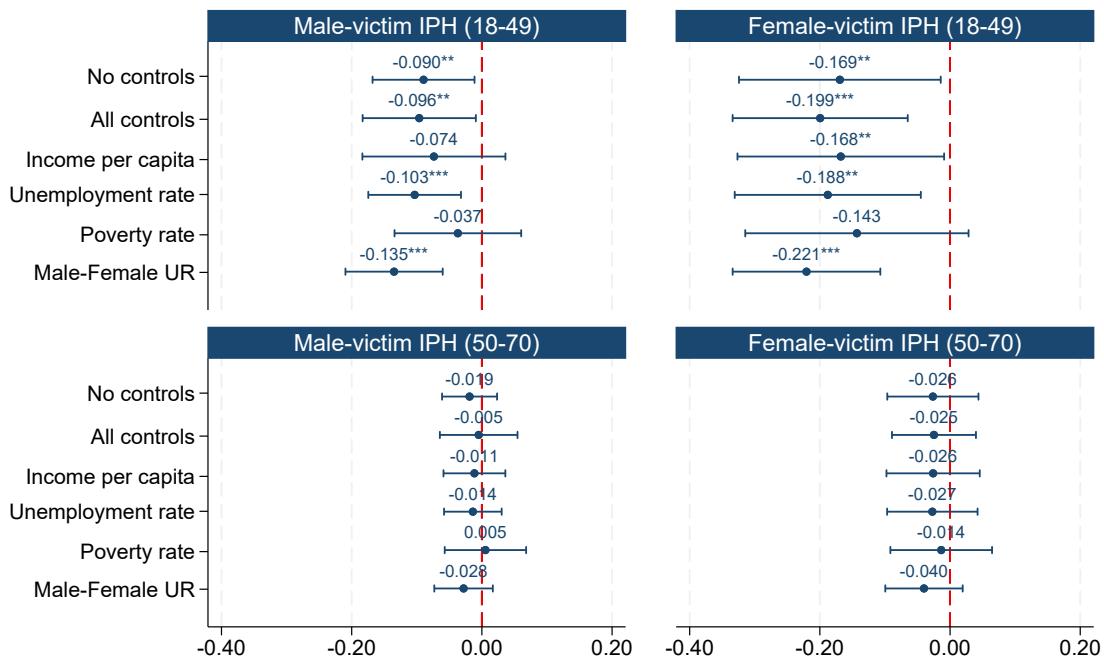
Notes: The first two columns report OLS and 2SDID estimates from regressions of the corresponding IPH rate on an indicator for NFS law adoption, including state and year fixed effects. The last two columns show the 1999 mean and the counterfactual mean. The counterfactual mean, $\mathbb{E}[Y_{d,s,t}(0) | D_{s,t} = 1]$, is estimated as the average of predicted IPH based on state and year fixed effects 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 (48 clusters) are reported in parentheses. $N = 1,419$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Effect of NFS Laws on Intimate Partner Homicide (IPH) and Stranger Homicide (SH) Rates (per 100,000): Excluding States with DV Policing Changes

	IPH rate		SH rate	
	OLS	2SDID	OLS	2SDID
Panel A. Excluding 7 states (Clarke et al., 2024)				
Male-victim 18–49	-0.059*	-0.111***	-0.032	-0.004
	(0.031)	(0.042)	(0.148)	(0.231)
Female-victim 18–49	-0.125**	-0.229***	0.020	0.015
	(0.056)	(0.066)	(0.019)	(0.034)
Male-victim 50–70	0.004	-0.005	-0.003	-0.029
	(0.022)	(0.033)	(0.035)	(0.059)
Female-victim 50–70	-0.017	-0.028	0.006	0.012
	(0.027)	(0.033)	(0.013)	(0.018)
Panel B. Excluding 6 states (Dasgupta and Pacheco, 2018)				
Male-victim 18–49	-0.059*	-0.095**	-0.025	0.028
	(0.034)	(0.048)	(0.149)	(0.248)
Female-victim 18–49	-0.105*	-0.188**	0.027	0.021
	(0.057)	(0.079)	(0.018)	(0.034)
Male-victim 50–70	-0.011	-0.012	0.003	-0.019
	(0.023)	(0.034)	(0.036)	(0.063)
Female-victim 50–70	-0.017	-0.010	-0.002	0.007
	(0.028)	(0.033)	(0.013)	(0.018)
Panel C. Excluding 13 states in panels A and B				
Male-victim 18–49	-0.054	-0.112**	-0.030	0.048
	(0.034)	(0.046)	(0.160)	(0.275)
Female-victim 18–49	-0.125**	-0.217***	0.027	0.029
	(0.060)	(0.079)	(0.020)	(0.037)
Male-victim 50–70	-0.003	-0.011	0.000	-0.018
	(0.023)	(0.037)	(0.037)	(0.065)
Female-victim 50–70	-0.005	-0.012	0.003	0.012
	(0.027)	(0.033)	(0.014)	(0.018)

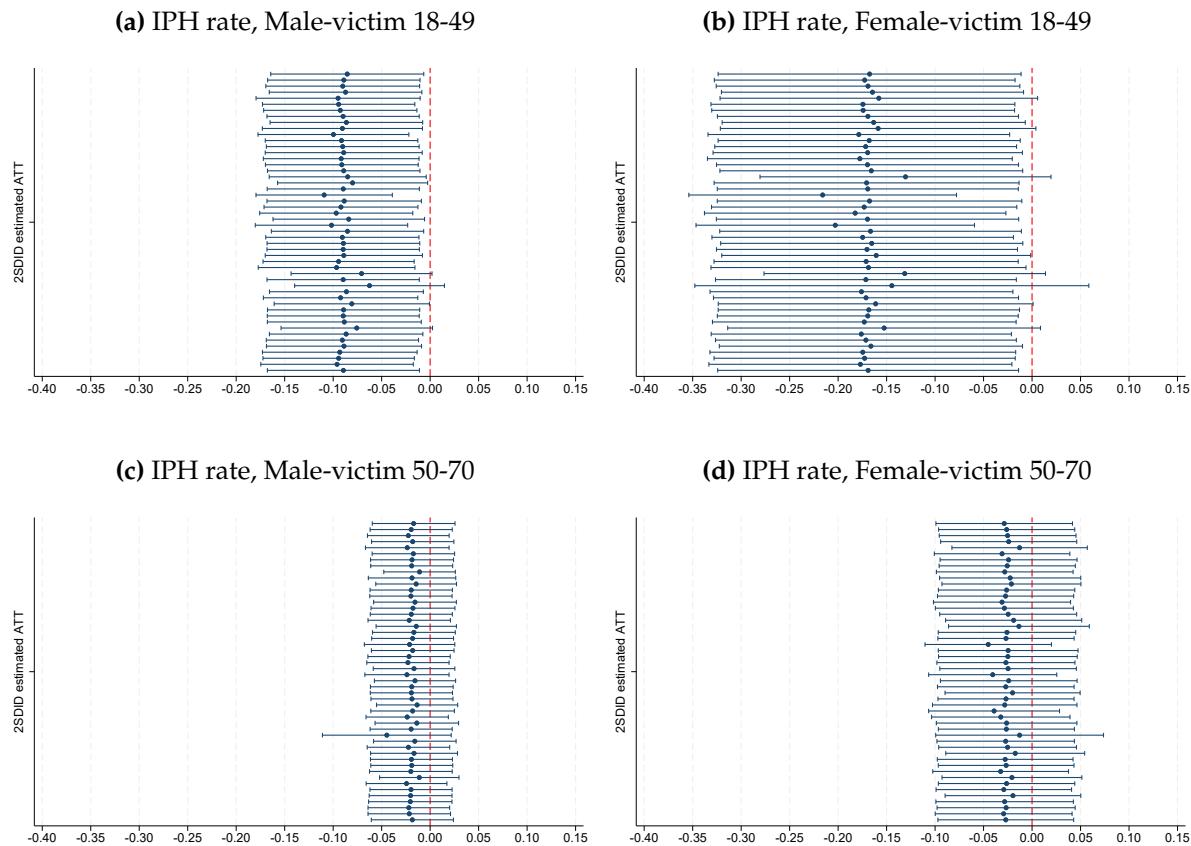
Notes: Each row displays the coefficients from regressions of the IPH rate or SH rate on an indicator for NFS law adoption, including state and year fixed effects and baseline (1990) covariates interacted with linear time trends. Regressions are weighted by the relevant cohort-age population in 2000. Panel A excludes the seven post-1999 primary-aggressor adopters identified by Clarke et al. (2024) (AR, LA, MS, NE, ND, OK, SD); the sample contains 43 clusters with $N = 1,269$. Panel B excludes the six states whose warrantless arrest statutes are dated 2000–2002 in Dasgupta and Pacheco (2018) (KS, OR, RI, SC, UT, VA); the sample contains 45 clusters with $N = 1,335$. Panel C excludes both groups; the sample contains 38 clusters with $N = 1,125$. Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 for different specifications.

Figure A3: 2SDID ATT estimates of NFS Law on IPH 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 regressions in Table 3, panel B, while omitting one state at a time.

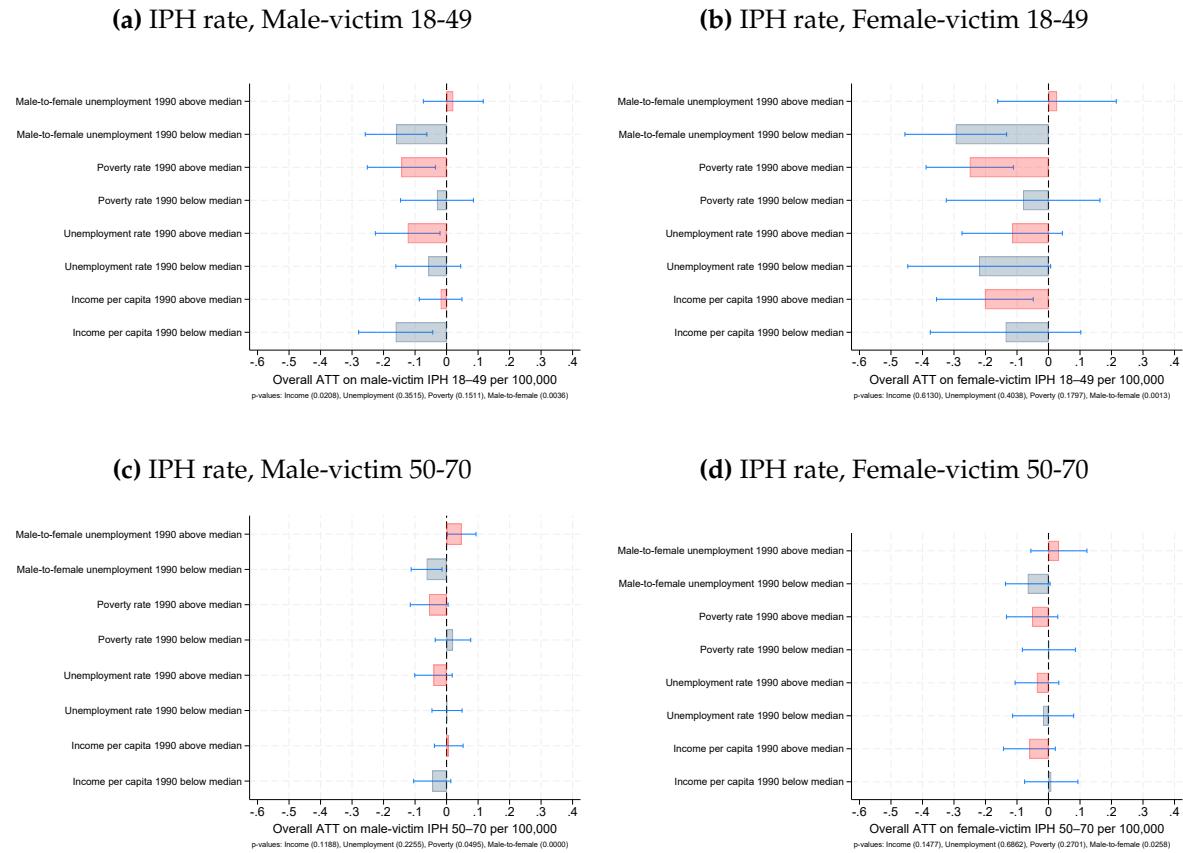
A4 Assessing heterogeneity by baseline characteristics

For each characteristic, we define a binary indicator equal to one if the value of the characteristic 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. Given that states above and below the median of each characteristic may have followed different underlying trends in IPH, 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 1990.³⁷

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.

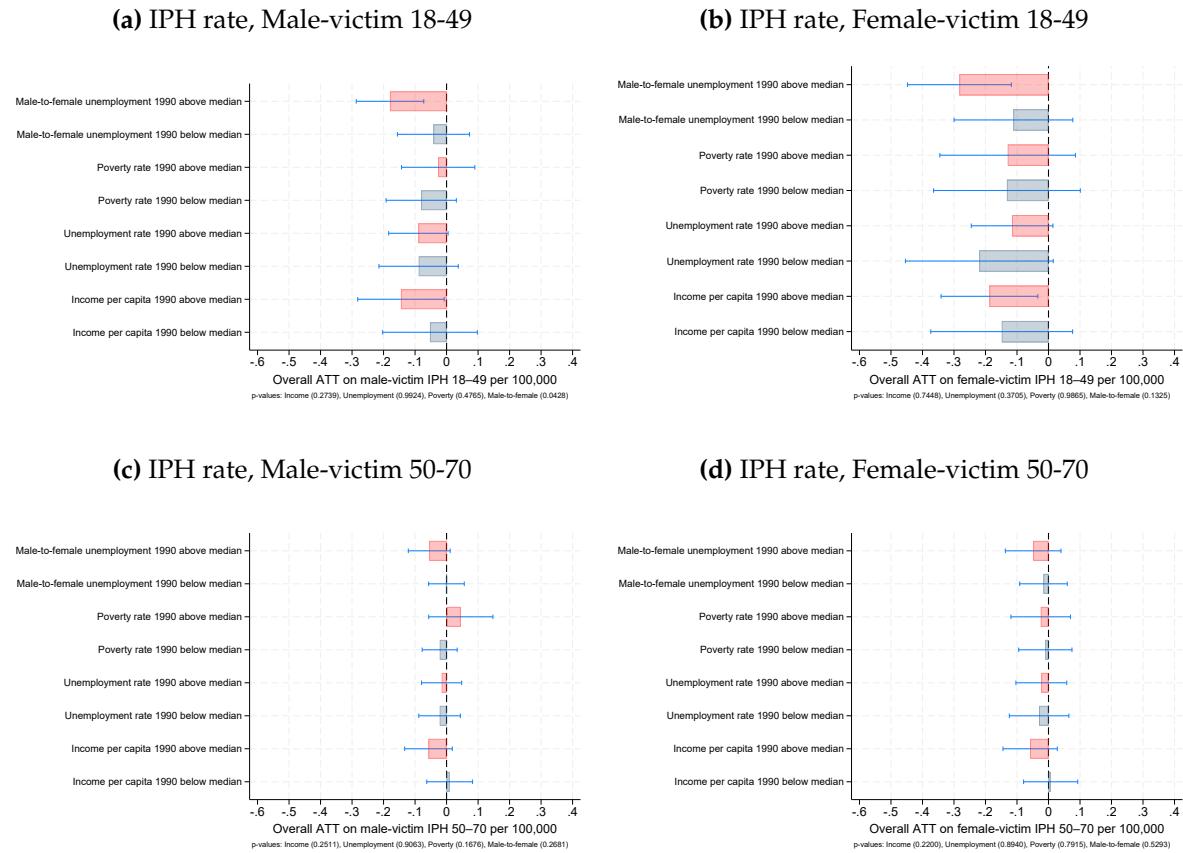
³⁷Indeed, once we control for group-specific linear trends, and accounting for the fact that we conduct a total of 16 heterogeneity tests (four outcomes \times four baseline characteristics). Applying a Bonferroni correction at the 5% significance level requires p -values $\leq \frac{0.05}{16} = 0.003125$ 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)



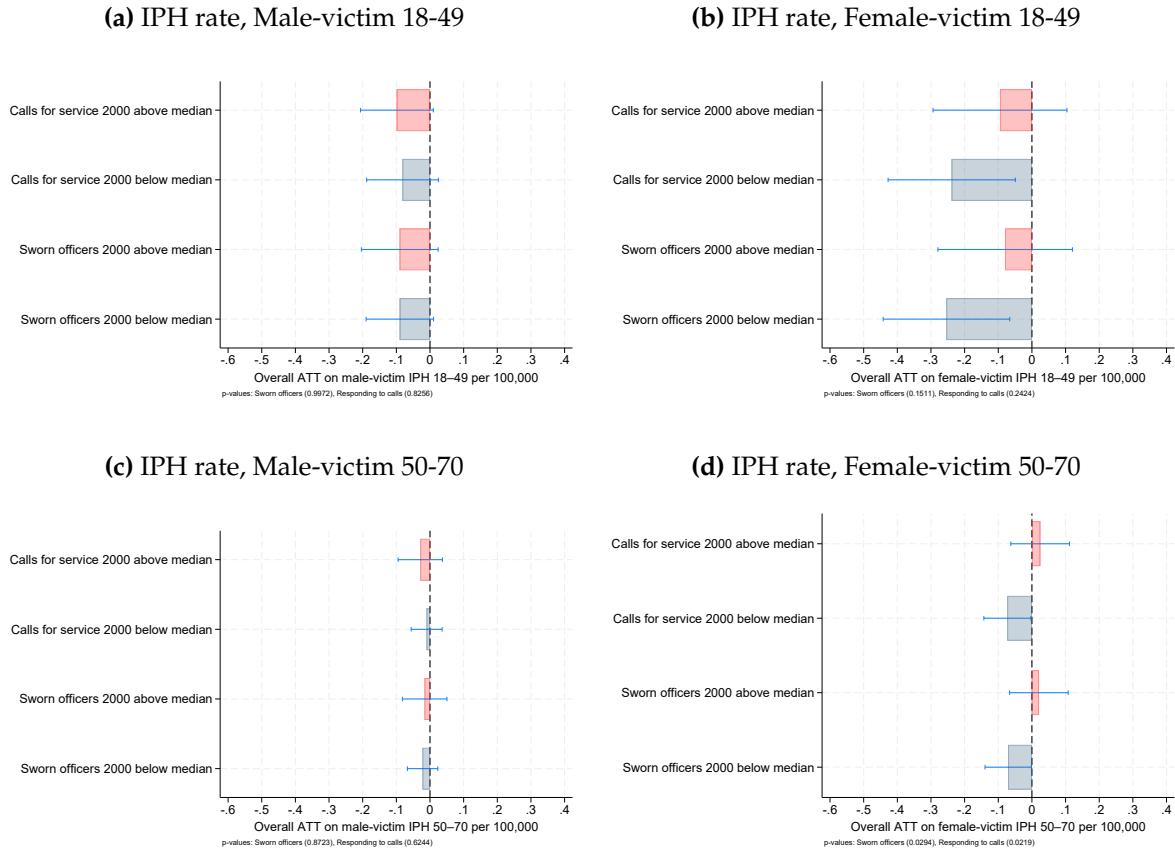
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. State and year fixed effects are estimated in the first stage using the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



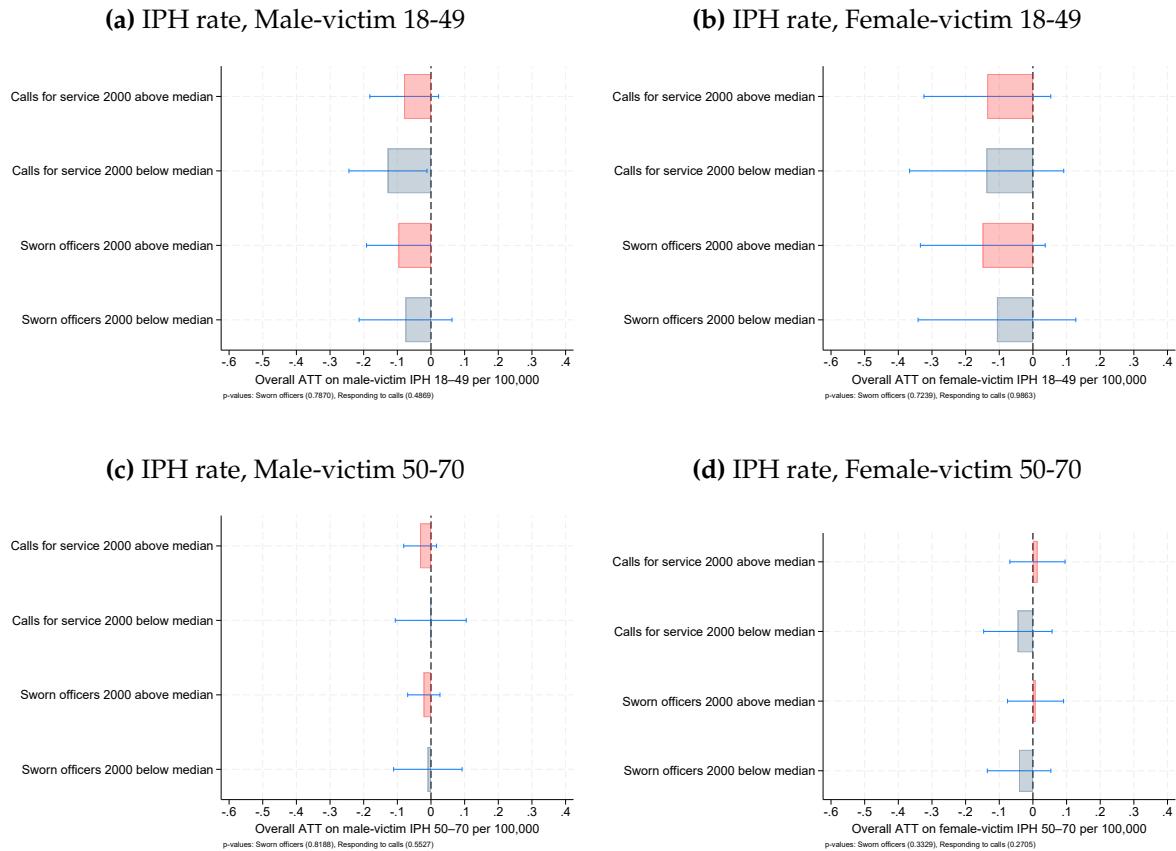
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. The coefficients on the control variable $X_s \times t$, as well as state 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 using the did2s Stata package developed by Butts (2021).

Figure A6: Heterogeneity Analysis by Local Police Resources at baseline (2000)



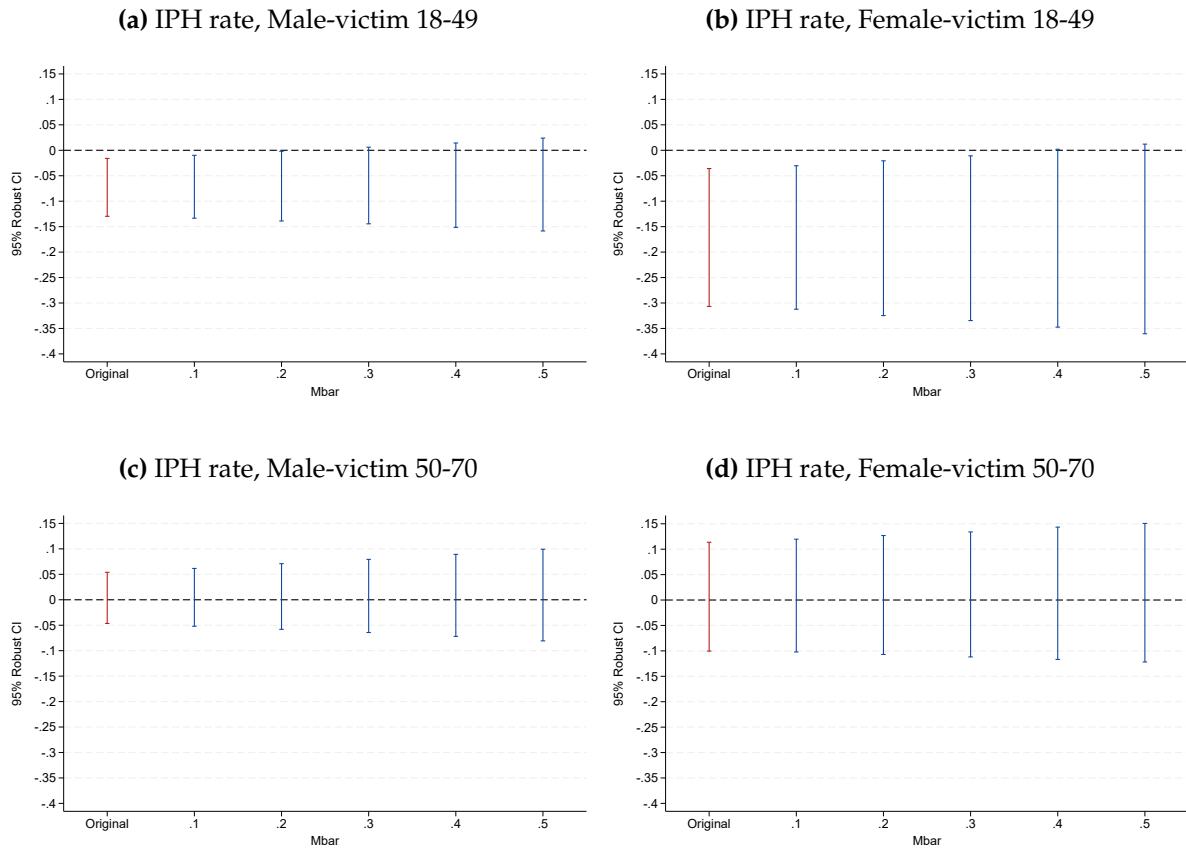
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. State and year fixed effects are estimated in the first stage using the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



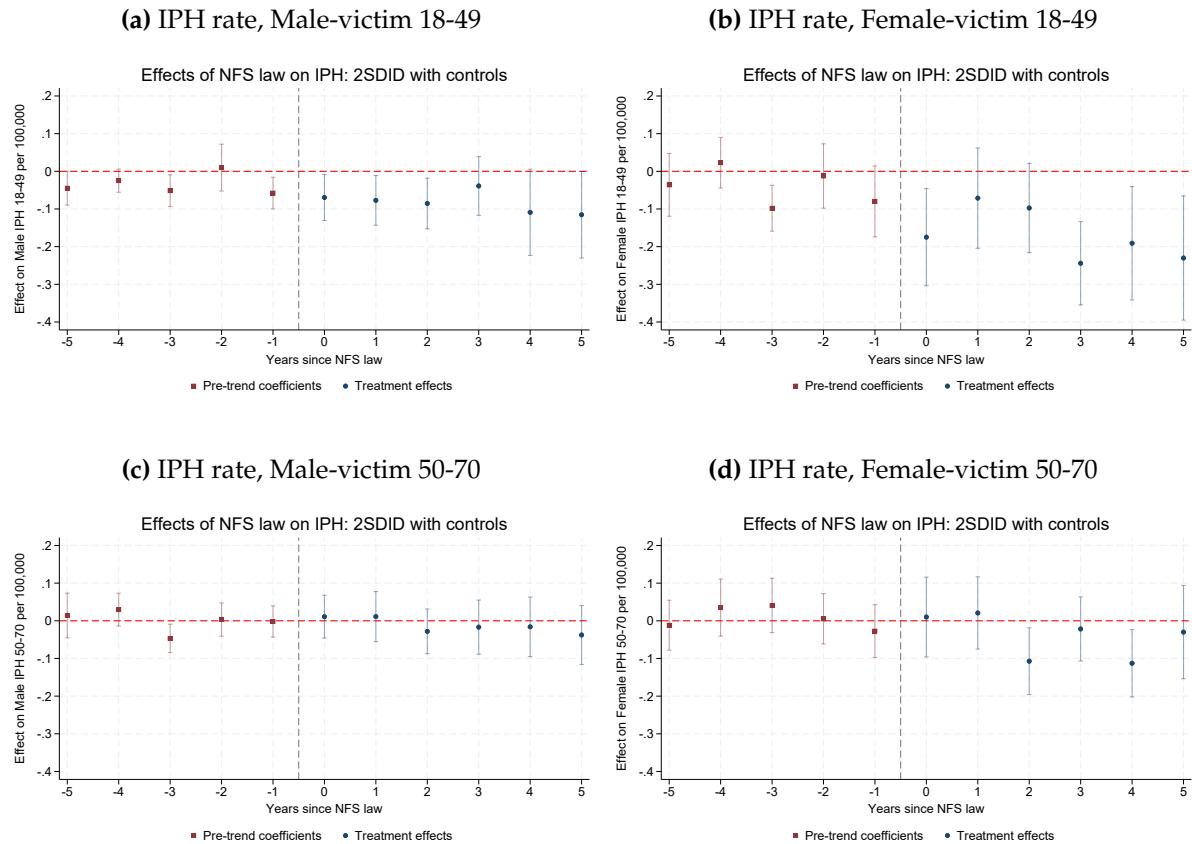
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. The coefficients on the control variable $X_s \times t$, as well as state 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 using the did2s Stata package developed by Butts (2021).

Figure A8: Robustness of 2SDID estimates to violation of parallel trends



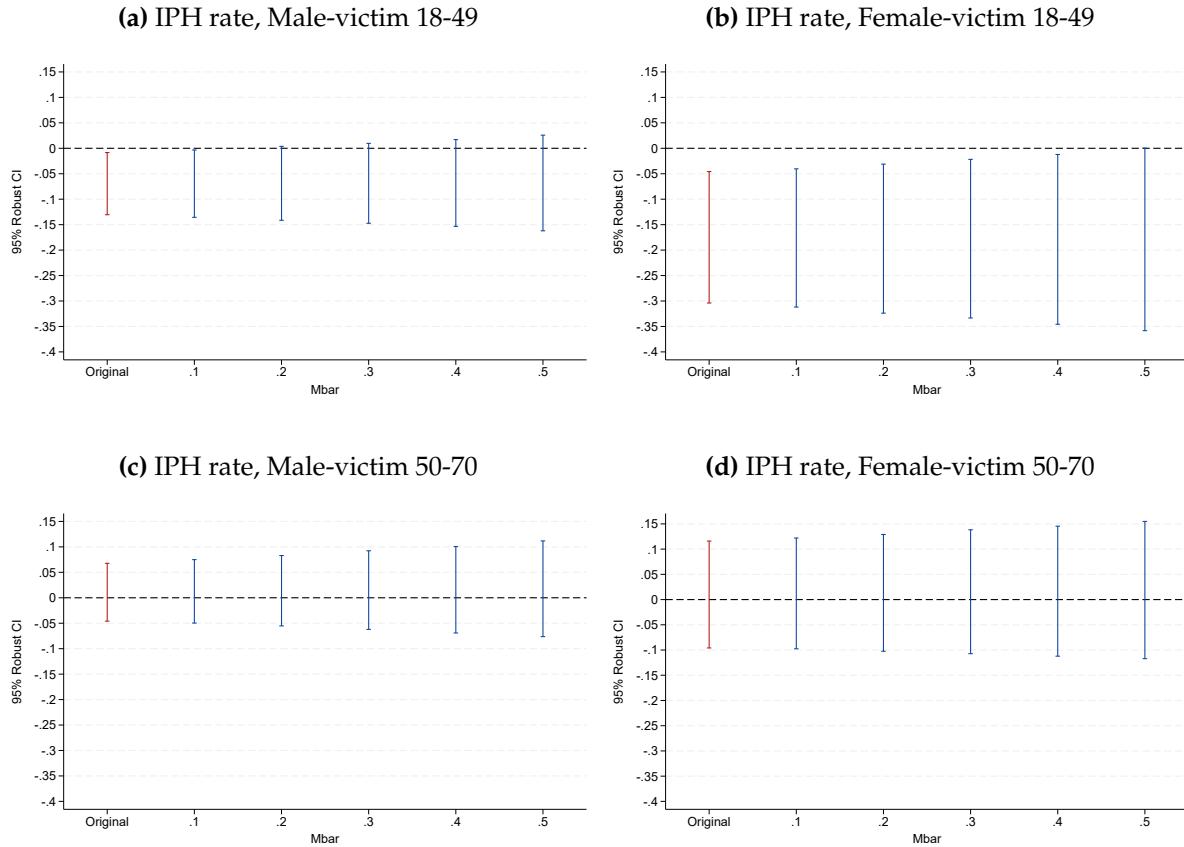
Notes: The figure illustrates the robustness of the 2SDID estimated treatment effect to potential violations of the parallel trends assumption. It reports robust confidence sets for the treatment effect in year zero (the year the NFS became effective) under different magnitudes of parallel trends violations (Mbar). 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



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



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 year the NFS law became effective) under different magnitudes of parallel trends violations (Mbar). The Honest DiD framework was developed by Rambachan and Roth (2023), and the calculations were performed using the `honestdid` Stata package.

Table A13: Effects of NFS Laws on Stranger-Perpetrated Homicides per 100,000 by Victim Sex and Age Group

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

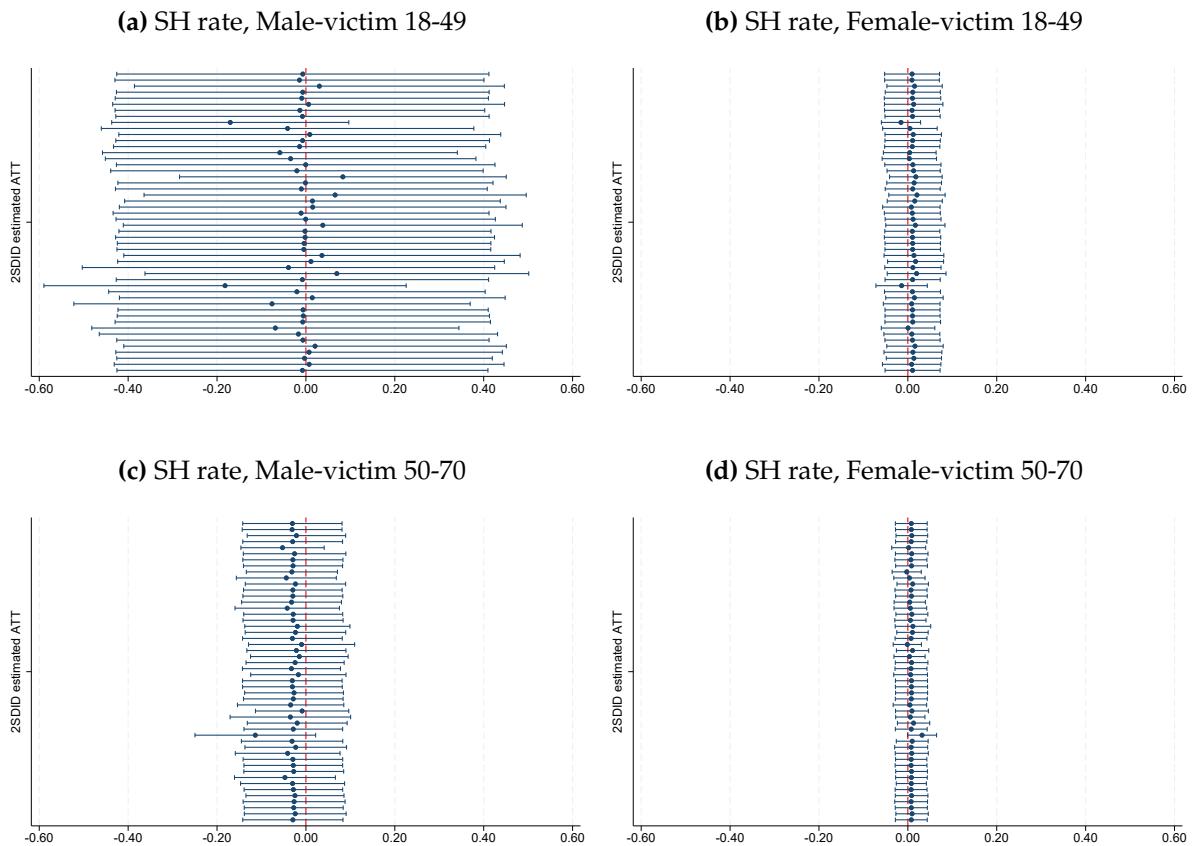
Notes: The first two columns report OLS and 2SDID estimates from regressions of the corresponding SH rate on an indicator for NFS law adoption, including state and year fixed effects. The last two columns show the 1999 mean and the counterfactual mean. The counterfactual mean, $\mathbb{E}[Y_{d,s,t}(0) \mid D_{s,t} = 1]$, is estimated as the average of predicted IPH based on state and year fixed effects 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 at the state level (50 clusters) are reported in parentheses. $N = 1,479$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: 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*** (0.045)	-0.167*** (0.048)	0.080 (0.222)	-0.610 (1.021)
Female-victim 18–49	-0.105* (0.058)	-0.180** (0.089)	0.032 (0.029)	-0.033 (0.094)
Male-victim 50–70	-0.035 (0.034)	-0.058 (0.054)	-0.043 (0.043)	-0.251 (0.233)
Female-victim 50–70	-0.023 (0.046)	-0.028 (0.062)	0.021 (0.022)	0.024 (0.035)
Panel B. With time-varying weights				
Male-victim 18–49	-0.061** (0.030)	-0.092** (0.043)	-0.021 (0.139)	-0.008 (0.212)
Female-victim 18–49	-0.103* (0.052)	-0.205*** (0.069)	0.023 (0.019)	0.013 (0.033)
Male-victim 50–70	-0.001 (0.020)	-0.000 (0.028)	-0.002 (0.032)	-0.030 (0.055)
Female-victim 50–70	-0.035 (0.028)	-0.034 (0.032)	-0.001 (0.012)	0.007 (0.018)
Panel C. Including South Carolina				
Male-victim 18–49	-0.059* (0.031)	-0.096** (0.043)	-0.008 (0.140)	0.064 (0.221)
Female-victim 18–49	-0.078 (0.062)	-0.145* (0.075)	0.023 (0.018)	0.022 (0.032)
Male-victim 50–70	0.001 (0.021)	-0.002 (0.027)	0.018 (0.037)	0.013 (0.054)
Female-victim 50–70	-0.031 (0.026)	-0.019 (0.030)	0.003 (0.013)	0.013 (0.019)
Panel D. Using passage instead of effective date				
Male-victim 18–49	-0.059* (0.032)	-0.094** (0.046)	-0.045 (0.139)	-0.021 (0.220)
Female-victim 18–49	-0.110** (0.053)	-0.204*** (0.067)	0.014 (0.018)	0.006 (0.033)
Male-victim 50–70	-0.009 (0.021)	-0.011 (0.030)	-0.015 (0.028)	-0.039 (0.054)
Female-victim 50–70	-0.019 (0.026)	-0.020 (0.033)	-0.004 (0.012)	0.002 (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 regressions in Table 4, while omitting one state at a time.

Table A15: Effect of NFS Laws on Reported IPV Incidents and Arrests

	OLS	2SDID	Mean 1999	Counterfactual Mean
Panel A. IPV incidents per 100,000				
Male-victim 18–49	-28.891*** (6.518)	-22.026* (12.403)	81.23	115.11
Female-victim 18–49	-156.301*** (38.962)	-132.311** (61.267)	411.47	550.07
Male-victim 50–70	-6.729** (2.773)	-7.939** (3.610)	13.62	38.65
Female-victim 50–70	-16.798*** (4.734)	-17.031** (7.071)	35.87	78.37
Panel B. IPV aggravated assaults per 100,000				
Male-victim 18–49	-3.855*** (0.928)	-3.866*** (1.410)	13.41	17.83
Female-victim 18–49	-2.534 (4.139)	-1.828 (6.701)	43.52	57.46
Male-victim 50–70	-1.190** (0.565)	-1.160* (0.628)	2.83	6.94
Female-victim 50–70	-0.696 (0.823)	-0.238 (0.904)	4.23	8.30
Panel C. Arrests for IPV incidents per 100,000				
Male-victim 18–49	-15.664*** (4.954)	-8.498 (8.830)	43.98	64.58
Female-victim 18–49	-77.799*** (16.765)	-50.444 (34.736)	222.03	274.86
Male-victim 50–70	-3.964* (2.049)	-3.636 (2.804)	8.024	22.52
Female-victim 50–70	-9.744*** (2.897)	-7.875 (4.821)	21.05	43.33
Panel D. Arrests for IPV aggravated assaults per 100,000				
Male-victim 18–49	-1.850*** (0.649)	-0.643 (0.964)	8.43	9.56
Female-victim 18–49	0.467 (3.270)	4.280 (4.671)	28.11	30.62
Male-victim 50–70	-0.745 (0.466)	-0.384 (0.515)	1.97	4.25
Female-victim 50–70	-0.412 (0.683)	0.215 (0.716)	3.04	5.30

Notes: The first two columns report OLS and 2SDID estimates from regressions of the corresponding dependent variable on an indicator for NFS law adoption, including state and year fixed effects. IPV incidents include aggravated assault, simple assault, and intimidation. The last two columns show the 1999 mean and the counterfactual mean. Panels A, B, and D: N = 734; Panel C: N = 733. Regressions are weighted by the relevant cohort-age population in 2000. Standard errors are clustered at the state level (45 clusters). *p<0.10, **p<0.05, ***p<0.01.