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

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Abstract

Non-fatal strangulation (NFS) is a form of intimate partner violence (IPV) and a strong predictor of intimate partner homicide (IPH). We compile NFS statute data, link them to homicide reports, 1990–2019, and estimate effects on IPH using a two-stage difference-in-differences estimator for staggered adoption and heterogeneous effects. NFS laws reduce IPH for ages 18–49, by 0.169 female-victim homicides per 100,000 women and 0.090 male-victim homicides per 100,000 men. We find no effects on IPH for ages 50–70 or on stranger-perpetrated homicides. Evidence from reported IPV incidents is suggestive of changes in reported incident classification and arrest patterns.

Keywords: Criminal Justice; Domestic Abuse; Gender; Intimate Partner Violence.

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

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1 Introduction

Intimate partner violence (IPV) is a pervasive and devastating social, economic, and public health problem (Adams 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). Within IPV, non-fatal strangulation (NFS) is a critical warning sign: it often marks escalation in abusive relationships (Thomas et al., 2014; 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—or not documented at all—prior to the adoption of NFS statutes. The absence of a specific serious-offense classification may have limited early criminal-justice intervention in abusive relationships, with potentially lethal consequences.[1] This legal gap began to close in 2000, when Missouri became the first state to recognize NFS as a serious criminal offense (HB1677). Nebraska, North Carolina, and Oregon followed in 2004. By 2019, 47 states had enacted NFS statutes,[2] creating a specific legal basis for treating NFS as serious violence.

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 intimate partner homicides. 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.

Because NFS is a highly gendered form of IPV that disproportionately affects women, our primary empirical focus is female-victim intimate partner homicide. Conceptually,

[1] For 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.

[2] As of 2025, all but one state, South Carolina, had done so.

NFS statutes may affect lethal IPV through several channels. By defining strangulation as a distinct serious offense, these laws may increase the expected legal cost of severe IPV, affect reporting of these incidents, change police and prosecutorial classification of reported incidents, increase arrest or incapacitation of high-risk offenders, and reduce repeated severe abuse.

We also estimate effects on male-victim IPH because effective criminal-justice interventions in abusive relationships can reduce lethal violence against male partners, by disrupting severe abuse culminating in self-defensive killing (Saunders, 2002; Aizer and Dal Bo, 2009; Miller and Segal, 2019). If earlier intervention prevents further escalation, the laws may also reduce lethal self-defense or desperation killings by female victims of abuse. The actual channels through which NFS laws decrease IPH cannot be separately identified in either the SHR or the NIBRS data.

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 reduce IPH rates among adults aged 18–49. In states that enacted NFS laws, the female-victim IPH rate declined from 1.221 to 1.052 per 100,000 women aged 18–49, and the male-victim IPH rate declined from 0.337 to 0.247 per 100,000 men aged 18–49. A back-of-the-envelope calculation based on these estimates implies approximately 1,029 fewer female and 547 fewer male intimate partner homicides. These effects are robust to several checks, including the addition of baseline state covariates interacted with linear time trends.

We find no comparable effects for older adults (aged 50–70) or heterogeneous effects by baseline state characteristics such as gender inequality, economic resources, and policing resources. In addition, because NFS signals violence escalation and coercive control within a relationship (Thomas et al., 2014; Patch et al., 2018), homicides committed by strangers—where such dynamics are absent—provide a natural placebo falsification test (Chin and Cunningham, 2019). Reassuringly, we find no effect of NFS

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

laws on homicide rates perpetrated by strangers.

A key assumption of our identification strategy is that of parallel trends. Event-study estimates show no evidence of systematic pre-treatment divergence and indicate sustained declines in IPH following the adoption of NFS laws. Additionally, a sensitivity analysis of potential violations of parallel trends (Rambachan and Roth, 2023) suggests that the estimates are robust to modest deviations from parallel trends.

We then turn to NIBRS data, finding evidence consistent with NFS laws changing reported IPV case processing. Specifically, NFS laws are associated with an increase in the share of reported IPV incidents classified as aggravated assaults for female victims aged 18–49—the group most exposed to IPV, which is overwhelmingly perpetrated by men (e.g., Sorenson et al., 2014; Parekh et al., 2024). We also find an increase in the share of aggravated IPV incidents that result in an arrest, for both female and male victims. Because NIBRS covers reported IPV incidents in participating police agencies and does not identify NFS incidents directly, these estimates should be interpreted as suggestive evidence on reported case processing rather than as a decomposition of mechanisms.

Our paper makes three contributions. First, we assemble a new state-by-year dataset on NFS statutes in the United States, documenting the timing of a major but previously understudied wave of IPV-related criminal-law reforms. Second, using this dataset linked to the FBI Supplementary Homicide Reports, we provide the first national causal evidence on the effect of NFS laws on IPH.[4] Third, we provide suggestive evidence on reported IPV incident classification and arrest patterns using NIBRS data.

Our findings complement and expand research on legal and institutional changes that shape abusive relationship dynamics, including divorce laws, arrest policies, prosecution rules, police composition, and criminal charging decisions (Aizer and Dal Bo, 2009; Aizer, 2010; Brassiolo, 2016; Chin and Cunningham, 2019; Miller and Segal, 2019; Amaral et al., 2023; Black et al., 2023). NFS statutes are distinct because they legally

[4]Previous 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.

recognize a specific high-risk escalation marker that is difficult to observe, often leaves limited visible injury, and is strongly associated with lethal violence. Our empirical strategy identifies reduced-form effects of statutory adoption on population-level homicide outcomes.

More broadly, related work examines economic and institutional determinants of IPV, spanning factors from the gender wage gap (Aizer, 2010) and compulsory schooling reforms (Erten and Keskin, 2022) 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 et al., 2024). We help fill this gap by estimating the effects of NFS laws on lethal IPV.

Our evidence suggests that NFS laws are a legal tool for addressing a common and highly predictive form of abuse. 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). In the UK, there are material differences across UK jurisdictions in the legal treatment of NFS-type conduct (Cherkassky et al., 2026). England and Wales introduced a specific offense only in 2022 (Bows and Herring, 2024); Northern Ireland in 2023; while in Scotland legislation remains under debate as of 2026 (see Members' Business Motion S6M-19504 (Scottish Parliament, 2025)). Many other countries, including France, Italy, and Spain, still have no standalone offense addressing NFS.

The paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 examines adoption timing and offers a preliminary analysis of pre-treatment trends. Section 6 reports the effects of NFS laws on intimate partner homicides. Section 7 examines reported IPV incidents, 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— is a highly gendered form of violence which can be fatal or non-fatal.[5] 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).[6] Among IPV victims, estimates of lifetime strangulation by a partner range from 27 to 80% among women, depending on the data source, including domestic violence hotlines, shelter intake samples, and clinical settings (McQuown et al., 2016; Stellpflug et al., 2022).[7] 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

[5] The act of strangulation is complete once pressure to the neck obstructs blood flow and/or airflow (California District Attorneys Association, 2020).

[6] "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.

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

many of these signs were missed or misattributed, and up to 50% of cases show no visible injuries (California District Attorneys Association, 2020, and references therein).

NFS was often treated as simple assault, if it was recorded at all. Quoting Strack and Gwinn (2020),

> "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 [...]."

Several widely documented cases illustrate how the absence of an NFS-specific offense could limit intervention after severe non-fatal violence and before later homicide. 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.[8] 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.[9] Male-victim IPH may also occur in the context of prior abuse by the male partner. 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.[10]

2.2 NFS Statutory Classification in the United States

The strangulation statutes adopted across states recognized non-fatal strangulation as a serious criminal offense. They did not simply relabel choking or strangulation

[8] See San Diego Union-Tribune (2010) and California District Attorneys Association (2020, p. 20).
[9] See Jeltsen (2016).
[10] See Shaffer (2019).

incidents in police data, rather they created a legal basis for treating strangulation as a serious criminal offense.

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, law-enforcement witnesses urged legislators to add strangulation explicitly to the statute.[11] 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.[12] In Montana's 2017 hearings on SB153, advocates similarly emphasized that recognizing strangulation "will help to save lives."[13]

The NFS statutes varied in how explicitly they defined strangulation. While the first two adopters, Missouri and North Carolina, did not formally define the act of strangulation, Nebraska, Oregon, and later adopting jurisdictions added statutory definitions. In particular, later statutes often defined strangulation by its effects, such as impeding breathing or blood circulation; by its means, such as applying pressure to the throat or neck; or by 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.[14]

[11] Dan Draovitch, retired police chief:

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

[12] "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)

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

[14] The underlined text in the Figure is added to the statutes by the bills.

Figure 1: Excerpts from NFS bills

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

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

(a) Nebraska LB943 (2004)

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

§ 2718. Strangulation.

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

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

(b) Pennsylvania HB1581 (2016)

Note: The figure shows examples of statutory definitions of non-fatal strangulation.

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 criminal 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.[15] Appendix Table A1 reports, for each state, the year the law was passed, the year it became effective, and the bill number.

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

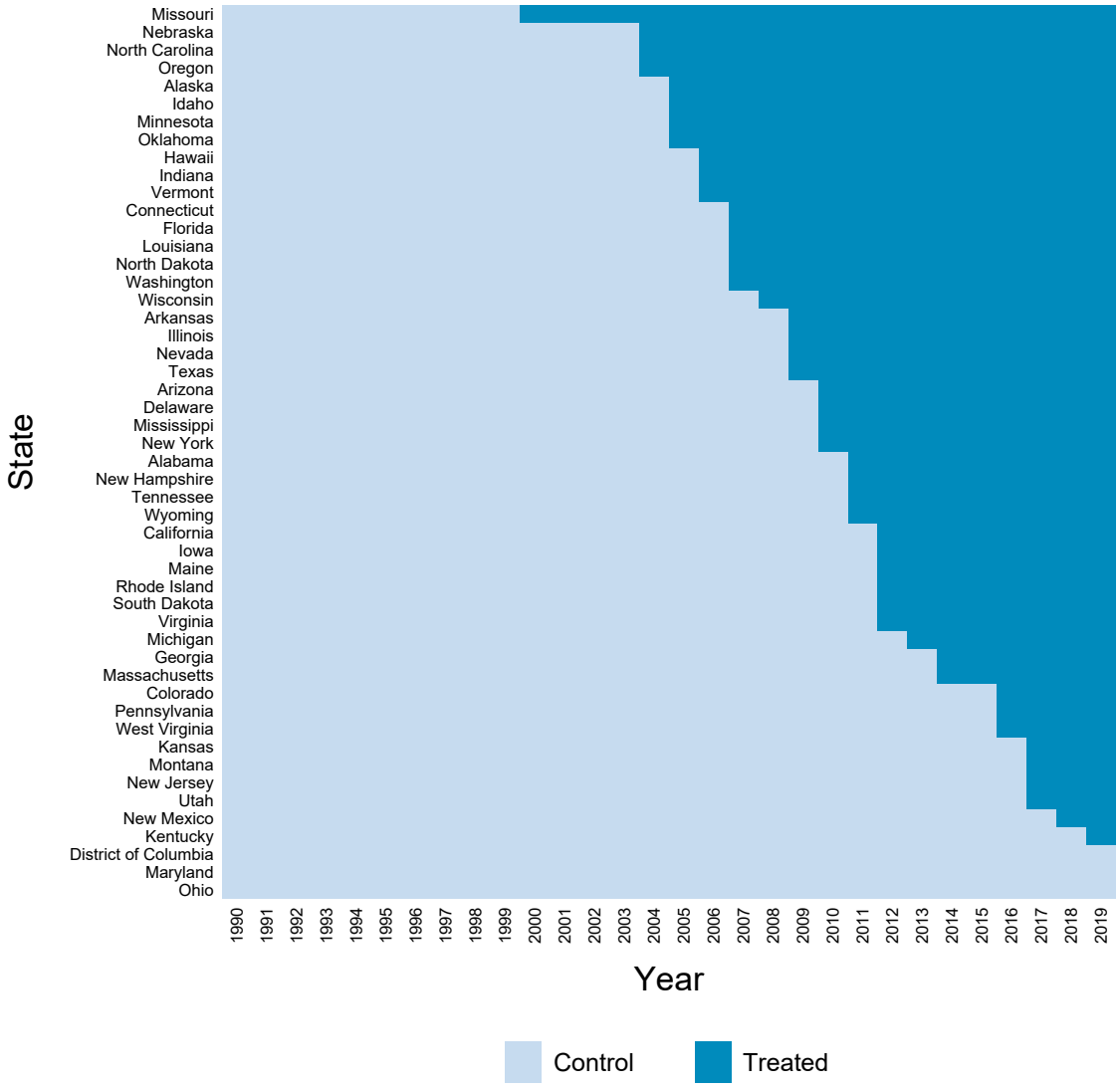
Our main treatment variable is a binary indicator that equals one from the year in which an NFS law became effective in a state onward.[16] This binary coding captures the introduction of an NFS-specific statutory criminal offense but abstracts from variation in statutory design and implementation across states, such as penalty severity and evidentiary standards. The estimates should therefore be interpreted as average effects of NFS law adoption across states, rather than as the effect of any single statutory formulation. However, our heterogeneity analysis provides no evidence that this binary classification masks differential effects across states with different baseline characteristics, such as proxies for gender inequality, economic resources, or policing resources.

Figure 2 shows the staggered rollout of the NFS 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 laws 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.[17]

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

[17]The 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



Note: The treatment year is defined as the year in which the law became effective.

Appendix Table A2 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.

3.2 Homicide Data: IPH and Homicides perpetrated by Strangers

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

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

where Intimate Partner Homicides$_{d,s,t}$ denotes the number of victims in demographic group d (defined by age group and sex) killed by an intimate partner in state s and year t, and Population$_{d,s,t}$ is the corresponding population of that demographic group in state s and year t.[18]

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).[19] 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 intimate partner homicides are excluded because they account for an extremely small number of IPH incidents in the SHR during our sample period. 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

[18] Appendix Table A3 provides additional details on data sources for population and other control variables.

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

imputed values from the SHR, following Chin and Cunningham (2019). Homicide reporting is missing for 21 state-year cells, resulting in a final sample of 1,479 observations.[20]

As a falsification outcome, we construct analogous homicide rates for victims killed by strangers (Chin and Cunningham, 2019). Because these homicides occur outside intimate partner relationships, they should not respond directly to NFS statutes. We disaggregate stranger homicides by victim 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 $\text{Stranger Homicides}_{d,s,t}$ 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 $\text{Population}_{d,s,t}$ is the population in demographic group d, state s and year t. The absence of effects on this placebo outcome provides a validity check for our identification strategy, as 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 police agencies, 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 state coverage is limited and changes over time. Before 1996, fewer than ten states had at least one reporting agency; coverage expanded substantially thereafter, reaching the mid-40s by 2018–2019, although several large states never report to the NIBRS (AK, CA, FL, NJ, NY; see Appendix Figure A1). In contrast, the SHR receives reports from nearly all agencies nationwide

[20] See Appendix Table A4. Appendix Table A5 shows that NFS rollout does not predict missing IPH.

across all states and D.C. (Fox and Swatt, 2009). For these reasons, and consistent with previous research (Pampel and Williams, 2000; Aizer and Dal Bo, 2009; Cunningham et al., 2024; Chin and Cunningham, 2019; Miller and Segal, 2019), we use the SHR as our source for homicide outcomes and NIBRS for complementary evidence on reported IPV incidents, their classification and arrest patterns.

To maintain consistency with our SHR homicide analysis, we focus on single-victim incidents with no multiple offenders and an individual victim. 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) 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 aggravated IPV incidents that result in 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, recorded by, and submitted by law-enforcement agencies participating in NIBRS in state s and year t. As a result, the regression samples for these IPV ratios cover only a subset of states and years, with sample sizes ranging from 635 to 732 state–year observations depending on the measure and sex–age group (see Appendix Table A6).[21]

We discuss the interpretation of these ratios in Section 7. Because NIBRS coverage varies across states and over time and does not separately identify NFS incidents, these outcomes should be interpreted as measures of reported case processing in participating agencies, while the SHR homicide analysis remains our primary focus.

[21] Appendix Table A7 shows that NFS rollout does not predict missingness in the classification or enforcement ratios.

4 Empirical Strategy

This section describes our empirical strategy. We compare the conventional two-way fixed-effects OLS estimator (TWFE–OLS) with the two-stage difference-in-differences estimator (2SDID) by Gardner et al. (2025). Both methods rely on the parallel trends assumption, no anticipation assumption, and Stable Unit Treatment Value assumption (SUTVA). The key difference between the two approaches is how the untreated counterfactual is constructed.

TWFE–OLS estimates state and year fixed effects on the full regression sample, jointly with the treatment indicator. The implied untreated counterfactual for treated state–year observations is obtained by taking the fitted value from this full-sample regression and setting treatment status to zero. In contrast, 2SDID estimates the state and year fixed effects only from untreated and not-yet-treated observations, and then uses these estimates to predict the untreated potential outcome for treated observations. When treatment effects vary across states or over time and treatment adoption is staggered, the TWFE–OLS coefficient need not recover the average treatment effect on treated state–year observations. However, the 2SDID estimator is designed to recover this parameter.

We use the same logic to estimate dynamic effects. After estimating state and year fixed effects using untreated and not-yet-treated observations, we subtract the resulting predicted untreated outcome from the observed outcome and regress this residualized outcome on event-time indicators. Pre-adoption coefficients therefore provide a diagnostic for whether treated observations deviate from their model-implied untreated counterfactual path before adoption. Adoption-year and post-adoption coefficients trace the dynamic treatment-effect profile as the difference between observed outcomes and predicted untreated outcomes at each event time. Because the comparison is directly to the model-implied untreated counterfactual, the event-study coefficients are not normalized relative to an omitted pre-treatment year.

4.1 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.[22] 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,

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

where $\alpha_{d,s}$ and $\gamma_{d,t}$ denote state and year fixed effects, respectively, for demographic group d. Hence, the observed outcome for demographic group d in state s and year t is

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

[22] An implicit assumption is the Stable Unit Treatment Value Assumption (SUTVA). Cross-state spillovers through advocacy, training, or diffusion of best practices are possible, particularly because statutory adoption may be accompanied by changes in implementation, training, and enforcement. However, major changes in protocols and enforcement are likely to 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. We therefore assume that such spillovers are limited. To the extent that control states adopt informal NFS-style practices before statutory reform, the estimates may be attenuated.

4.2 Two-way fixed effects (TWFE)–OLS estimator

A common empirical approach is to estimate the regression

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

by OLS with standard errors clustered at the state level.[23] The TWFE–OLS fitted value for a treated state–year observation includes the estimated treatment component. The implied untreated counterfactual is therefore constructed by subtracting this treatment component from the fitted value:

$$\widehat{Y}_{d,s,t}(0)^{TWFE} = \widehat{Y}_{d,s,t}^{TWFE} - \widehat{\beta}_d^{\text{TWFE}} D_{s,t}.$$

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). In other words,

$$\mathbb{E}[\widehat{Y}_{d,s,t}(0)^{TWFE} \mid D_{s,t} = 1] = \mathbb{E}[\widehat{Y}_{d,s,t}^{TWFE} - \widehat{\beta}_d^{\text{TWFE}} \mid D_{s,t} = 1] = \mathbb{E}[Y_{d,s,t}(0) \mid D_{s,t} = 1].$$

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

[23] In 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.

estimates the linear-projection coefficient β_d^{TWFE}, β_d^{TWFE} will generally differ from the overall ATT, β_d,[24] because

$$\mathbb{E}[\widehat{Y}_{d,s,t}(0)^{TWFE} \mid D_{s,t} = 1] \neq \mathbb{E}[Y_{d,s,t}(0) \mid D_{s,t} = 1].$$

4.3 Two-stage difference-in-differences (2SDID) estimator

We address the limitations of the TWFE–OLS estimator using 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 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).

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, regardless of the estimator being used. However, the counterfactual 2SDID mean outcome for treated observations, $\mathbb{E}[Y_{d,s,t}(0) \mid D_{s,t} = 1]$, is computed as the average of the predicted outcome $\widehat{Y}_{d,s,t}$—based on state and year fixed effects estimated from untreated/not-yet-treated observations ($D_{s,t} = 0$)—evaluated for treated observations ($D_{s,t} = 1$). The overall ATT for demographic group d, β_d, is therefore estimated as the sample counterpart of

$$\mathbb{E}[Y_{d,s,t}(1) \mid D_{s,t} = 1] - \mathbb{E}[\widehat{Y}_{d,s,t}(0)^{2SDID} \mid D_{s,t} = 1].$$

[24] 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.

4.4 Comparison with other imputation estimators

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.

The 2SDID estimator delivers point estimates numerically equivalent to the imputation estimator of Borusyak et al. (2024) for the corresponding specification, but differs in its approach to inference. Whereas Borusyak et al. (2024) develop an imputation-based variance estimator under a different asymptotic framework, Gardner et al. (2025) formulate the two-stage estimator as a GMM problem and derive corresponding standard errors. Simulation evidence in Gardner et al. (2025) suggests that this GMM-based approach performs well in finite samples, particularly when treatment cohorts are small or numerous.[25]

4.5 Identification of Dynamic ATT Estimates

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 regression:

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

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

[25] We 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 $D_{s,t}$.

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 the adoption year or 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.

Unlike standard TWFE–OLS event-study specifications, this 2SDID event-study specification does not require omitting a reference event-time period. Because the state and year fixed effects are estimated using untreated/not-yet-treated observations and then subtracted from the outcome, each second-stage coefficient is interpreted relative to the model-implied untreated counterfactual rather than relative to an omitted pre-treatment period.

This procedure yields estimates of the dynamic ATT profile. Adoption-year and post-adoption coefficients trace duration-specific average treatment effects among treated observations observed at each relative time. Lead coefficients are used as a pre-treatment diagnostic: if treated observations deviate systematically from their model-implied untreated counterfactual path before adoption, this would cast doubt on the credibility of the identifying assumptions. However, lead coefficients equal to zero should not be interpreted as directly validating these assumptions. Rather, as highlighted by Ghanem et al. (2026), they provide, at most, indirect evidence on the plausibility of the parallel counterfactual trends required to identify post-adoption effects. Under parallel trends (in untreated potential outcomes after adoption), no anticipation, and correct specification of the untreated potential outcome model, the post-adoption coefficients consistently estimate the dynamic treatment-effect profile for demographic group d.

4.6 Weighting and Interpretation of ATT Estimates

For notational simplicity, the estimands above are written using unweighted expectations. In the empirical implementation, all regressions are weighted by state population in the relevant age group, using population counts from the 2000 Census. Thus, under the identifying assumptions above, our homicide estimates can be interpreted

as population-weighted average treatment effects of NFS laws for men and women in states that passed such laws.[26] For NIBRS outcomes, the estimated effects are subject to the reporting, coverage, and compositional limitations discussed below.

The identifying assumption is that, absent NFS adoption, treated states would have followed the same model-implied untreated potential outcome path as untreated and not-yet-treated states within each sex–age group. This assumption would be violated if NFS adoption systematically coincided with unobserved changes that differentially affected intimate partner homicide, such as local IPV enforcement initiatives, police protocols, victim-service capacity, or public awareness campaigns. The event studies, placebo stranger-homicide outcomes, covariate-trend specifications, leave-one-state-out robustness checks, and exclusions of states with domestic-violence policing changes help assess and mitigate identification concerns, but they cannot eliminate them.

5 Timing of NFS Laws and pre-treatment trends in IPH

5.1 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 jurisdictions, assigning eventual adoption years to Maryland (2020) and D.C. and Ohio (2023).

Appendix Figure A2 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[27]) and the

[26] Percentages of the population by cohort and age group in 2000 are shown in Appendix Table A8.

[27] Given the small sample size and potential heteroskedasticity, we report heteroskedasticity-consistent HC3 standard errors.

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 (Appendix Table A9). In addition, the same variables measured in 1990 are not systematically correlated with adoption timing (Appendix Table A10).

5.2 Pre-treatment trends in IPH: a preliminary assessment

A key identifying assumption of a difference-in-differences estimator is that, absent treatment, outcomes in treated states would have followed the same model-implied trends as untreated and not-yet-treated states. Although this assumption is fundamentally untestable, we provide descriptive evidence on its plausibility by examining pre-treatment IPH trends.

Appendix Table A11 compares changes in male- and female-victim IPH rates across age groups between 1990 and 1999 for eventually treated and never-treated jurisdictions. Because only three jurisdictions remain untreated by 2019, this comparison is necessarily limited and should be interpreted as descriptive rather than as a formal diagnostic check of the identifying assumptions. The differences vary in sign and magnitude; only one is statistically significant at conventional levels, for male victims aged 18–49.[28]

Our primary assessment of pre-treatment dynamics is provided by the 2SDID event-study estimates in the next section, which use the same untreated/not-yet-treated

[28] Appendix Table A12, 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 jurisdictions. This comparison is subject to the same limitation. On average, the two groups are broadly similar. Only one statistically significant difference at conventional levels ($p < 0.05$) emerges, for poverty rates, while differences in the remaining variables are small and statistically insignificant. No mean differences are found in 1999 (Appendix Table A12, Panel B), the year before the enactment of the first NFS law. In terms of changes over 1990–1999 (Table A12, Panel C), we detect one marginally significant difference in the trend of the male-to-female unemployment ratio ($p < 0.10$).

comparison structure as the main estimator. We will also evaluate the robustness of our findings to violations of parallel trends of varying magnitudes (Rambachan and Roth, 2023) and to the inclusion of baseline covariates interacted with linear time trends.

6 Estimated Effects of NFS Laws on Homicide Rates

6.1 Overall ATT Estimates on IPH Rates

Main estimates. Table 1 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 TWFE–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 2SDID counterfactual mean IPH, 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.

Panel A shows that NFS laws are estimated to reduce IPH rates, among younger adults. For individuals aged 18–49, the 2SDID estimate implies a decline in the female-victim IPH rate of 0.17 per 100,000 women—a 14% reduction relative to the counterfactual mean (from 1.22 to 1.05). This is our primary outcome of interest because NFS is a highly gendered form of IPV, disproportionately experienced by women and overwhelmingly perpetrated by men, and because prior NFS is a strong marker of lethal escalation. The estimate should be interpreted as evidence that legal criminalization of a high-risk escalation marker reduces population-level female-victim IPH, not necessarily that the homicides prevented would have been committed by strangulation.[29]

For male victims aged 18–49, the estimated reduction is 0.09 per 100,000 men, corresponding to a 27% decrease relative to the counterfactual mean, from 0.337 to 0.247.

[29] In the SHR data, fatal strangulation, including asphyxiation, accounts for 4.1% of female IPH victims aged 18–49 and 0.2% of male IPH victims aged 18–49 in our sample. The estimated effect should therefore be interpreted as operating through the criminalization of non-fatal strangulation as a high-risk escalation marker, rather than through reductions in strangulation homicides alone.

Because our analysis excludes same-sex intimate-partner homicides, male-victim IPH corresponds to killings by female intimate partners. This larger percentage reduction should not be interpreted as evidence that men are more directly exposed to NFS. Rather, the percentage decline is larger for male victims because the baseline rate of male-victim IPH is much lower than the corresponding rate for female victims. In addition, the reduction may partly reflect fewer lethal self-defense or desperation killings by female victims of abuse, as suggested by prior work on IPH (Saunders, 2002; Aizer and Dal Bo, 2009; Miller and Segal, 2019). However, the SHR data do not identify whether a homicide occurred in self-defense or in response to prior abuse.

Table 1: Effects of NFS Laws on Intimate Partner Homicides per 100,000 (IPH rate) by Victim Sex

	TWFE–OLS	2SDID	IPH rate Mean 1999	IPH rate Mean Counterfactual 2SDID
Panel A. Without Controls				
Female victim	-0.102*	-0.169**	1.189	1.221
	(0.055)	(0.079)		
Male victim	-0.076**	-0.090**	0.354	0.337
	(0.035)	(0.040)		
Panel B. With Controls				
Female victim	-0.107*	-0.199***	1.189	1.252
	(0.054)	(0.069)		
Male victim	-0.064**	-0.096**	0.354	0.343
	(0.031)	(0.044)		

Notes: Victims aged 18–49. The first two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on IPH rates per 100,000. TWFE–OLS models include state and year fixed effects. 2SDID estimates are obtained from regressions of residualized IPH rates on the NFS adoption indicator, where residuals are constructed after estimating state and year fixed effects using untreated and not-yet-treated observations. Panel B adds baseline 1990 demographic and socioeconomic controls interacted with linear time trends. The final two columns report the mean IPH rate in 1999 (the year before any state enacted an NFS law) and the 2SDID counterfactual mean, estimated from the untreated potential outcome model among treated observations. Regressions are weighted by the 2000 state population aged 18–49. Standard errors clustered by jurisdiction (50 = 49 states + D.C.) are reported in parentheses. $N = 1{,}479$. *p<0.10, **p<0.05, ***p<0.01.

Panel B in Table 1 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).[30] The resulting estimates are very similar to those in Panel A. Appendix Figure A3 summarizes estimates with no controls, all controls, and with one control at a time.

For the 50–70 age group, Appendix Figure A3 and Appendix Table A13 show that the 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 larger effects among individuals more exposed to IPV risk (Aizer and Dal Bo, 2009).

Our estimates align with prior evidence on different criminal-justice interventions (Aizer and Dal Bo, 2009; Chin and Cunningham, 2019; Miller and Segal, 2019). 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 (2019) 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.

To aid interpretation, we translate the estimated overall ATT effects on IPH rates per 100,000 into the implied number of IP homicides averted for each demographic group d from enactment through 2019. These reductions are computed using the Panel A estimates in Table 1 and the corresponding state-year sex-age populations for treated observations. For group d, the implied reduction is calculated as

$$\widehat{\beta}_d \times \frac{\sum_{(s,t):\, D_{s,t}=1, t<2020} \text{Population}_{d,s,t}}{100{,}000},$$

[30] Covariates include measures of state-level socioeconomic resources (log income per capita, unemployment rate, poverty rate) and gender inequality (male-to-female unemployment ratio), following Aizer (2010). See Appendix B for details.

where $\widehat{\beta}_d$ is the estimated overall ATT on the IPH rate from Panel A of Table 1. This back-of-the-envelope calculation implies approximately 1,029 fewer female and 547 fewer male intimate partner homicides among adults aged 18–49 from enactment through 2019.

Additional estimates. We next assess whether the main estimates are sensitive to alternative outcome definitions, statutory classifications, influential states, contemporaneous DV-policy changes, and observable state heterogeneity.

Appendix Table A14 reports Poisson estimates using homicide counts rather than rates; the results are qualitatively consistent with our baseline specifications. Moreover, 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 Appendix Table A15, the results are virtually unchanged. We then demonstrate that our 2SDID estimates are not driven by any specific state: when we sequentially drop one state at a time (see Appendix Figure A4), the resulting coefficients closely match those in Table 1, alleviating concerns that our findings are sensitive to the composition of the control pool.

A remaining concern is that the estimated effects could reflect contemporaneous changes in other domestic-violence (DV) policies rather than NFS laws themselves. 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., 2011a,b).[31] Consequently, potentially con-

[31]Chin 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).

founding statewide policies such as mandatory or pro-arrest laws, protection-order provisions, unilateral divorce statutes, firearm regulations for domestic violence convictions, and victims' rights protections were largely implemented well before the first adoption of NFS laws in 2000. As shown in Appendix Table A16, the estimated effect of NFS laws on intimate partner homicide is very similar when excluding states that experienced post-1999 changes in domestic-violence policing regimes.

Finally, because statutory implementation may interact with state capacity or baseline social conditions, we also examine whether effects vary by baseline economic conditions, gender inequality, and policing resources (see Appendix G, Figures A5–A8). While there is apparent heterogeneity in unadjusted splits, these patterns are not robust to group-specific trends or multiple-testing adjustment.[32]

The checks conducted in this section reduce the concern that the estimates are driven by major contemporaneous statewide DV policy changes. However, our estimates should be interpreted as the effect of adopting NFS laws in the institutional environments in which they were implemented, rather than as the effect of statutory language isolated from all possible implementation changes.

6.2 Event-Study Estimates on IPH Rates

Figure 3 presents event-study estimates of dynamic effects using the two-stage difference-in-differences (2SDID) approach, separately by victim sex among those aged 18–49. The pre-treatment coefficients (shown as red squares) are generally small and do not indicate systematic pre-treatment divergence, although some event-time estimates are imprecise. This pattern provides some support for the plausibility of the parallel counterfactual trends required for identification, but should not be interpreted as direct

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.

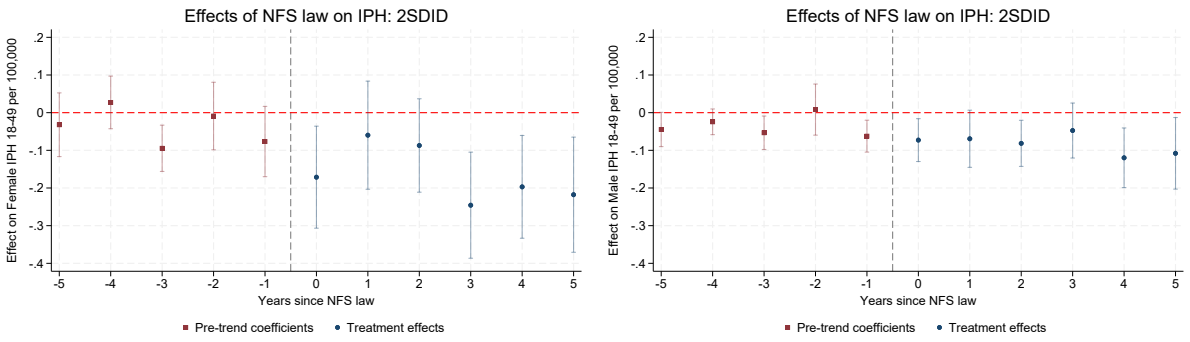
[32]The characteristics examined include proxies for economic resources (income per capita, poverty rate, unemployment rate), gender inequality (male-to-female unemployment ratio), and police resources (sworn personnel per 100,000 and uniformed officers responding to calls per 100,000), using data from the 2000 Census of State and Local Law Enforcement Agencies (Reaves and Hickman, 2002). The first measure reflects overall law-enforcement capacity—the size and potential reach of police agencies—while the second captures staffing dedicated specifically to frontline response, indicating how well-resourced agencies are for incidents requiring immediate intervention.

validation of the identifying assumption (Ghanem et al., 2026). It also aligns 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 1. In contrast, Appendix Figure A9 shows that 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.

Figure 3: 2SDID Dynamic Effects of NFS Laws on Intimate Partner Homicides per 100,000

(a) IPH rate, Female-victim 18–49 **(b)** IPH rate, Male-victim 18–49



Notes: Estimates are from a 2SDID event-study specification implemented using the `did2s` Stata package (Butts, 2021). The first stage estimates state and year fixed effects using untreated and not-yet-treated observations ($D_{s,t} = 0$); the second stage regresses residualized outcomes on event-time indicators. Unlike standard TWFE–OLS event studies, no reference event-time period is omitted. See Section 4.5 for interpretation. Vertical bars denote 95% confidence intervals based on jurisdiction-clustered standard errors.

Appendix Figure A10 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 A11 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 modest deviations of parallel trends, as shown in Appendix Figure A12.

6.3 Falsification test: Homicide Rates by Strangers

We conduct a falsification test by examining whether NFS laws affected homicides committed by strangers in the spirit of Chin and Cunningham (2019). Because NFS indicates violence escalation and coercive control within IP relationships (Thomas et al., 2014; Patch et al., 2018), stranger-perpetrated homicides—where these dynamics are absent—offer a natural placebo test.

Table 2 reports the main placebo estimates for female- and male-victims aged 18–49, using TWFE–OLS and 2SDID estimators and including baseline covariates (measured in 1990) interacted with linear time trends. The estimated effects are small and statistically insignificant. Similar null effects are found for female- and male-victims aged 50–70 (see Appendix Table A17).[33]

Table 2: Effects of NFS Laws on Stranger-Perpetrated Homicides per 100,000 (SH rate) by Victim Sex

	TWFE–OLS	2SDID	SH rate Mean 1999	SH rate Mean Counterfactual 2SDID
Female victim	0.021	0.011	0.127	0.092
	(0.018)	(0.032)		
Male victim	-0.025	-0.007	1.116	0.899
	(0.139)	(0.213)		

Notes: Victims aged 18–49. The first two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on SH rates per 100,000. TWFE–OLS models include jurisdiction and year fixed effects. 2SDID estimates are obtained from regressions of residualized SH rates on the NFS adoption indicator, where residuals are constructed using untreated and not-yet-treated observations. All specifications include baseline 1990 socioeconomic controls interacted with linear time trends. The final two columns report the mean SH rate in 1999, before any jurisdiction enacted an NFS law, and the 2SDID counterfactual mean estimated from the untreated potential outcome model among treated observations. Regressions are weighted by the 2000 state population aged 18–49. Standard errors clustered by jurisdiction are reported in parentheses. There are 50 clusters, corresponding to 49 states and D.C. $N = 1{,}479$. *p<0.10, **p<0.05, ***p<0.01.

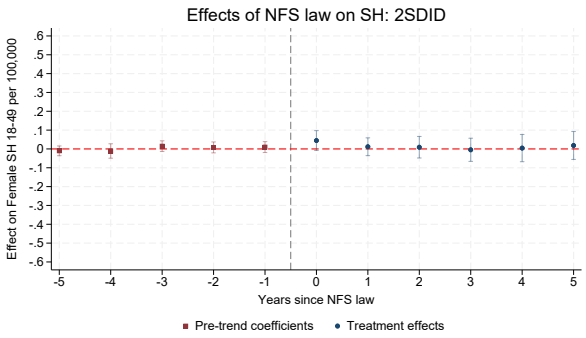
We also present event-study estimates for the placebo outcome. As shown in Figure 4, the event-study coefficients provide no evidence of systematic post-treatment ef-

[33]In the Appendix, we also report analogous placebo estimates without controls (Appendix Table A18), and show that the placebo 2SDID estimates are not driven by any single state by re-estimating the regressions while dropping one state at a time (Appendix Figure A13).

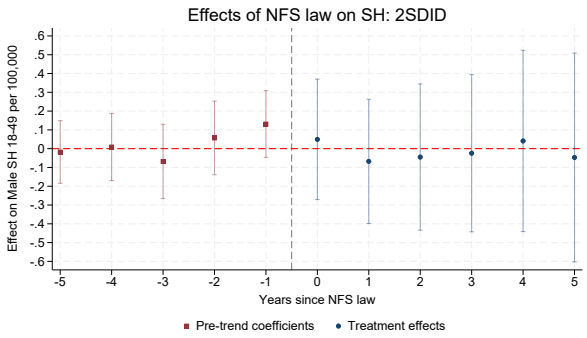
fects for victims aged 18–49, offering additional support for our identification strategy. Appendix Figure A14 shows similarly null dynamic effects for victims aged 50–70. This falsification exercise is consistent with NFS laws not affecting broader homicide trends unrelated to intimate partner violence.

Figure 4: 2SDID Dynamic Effects of NFS Laws on Stranger-Perpetrated Homicides per 100,000

(a) SH rate, Female-victim 18–49



(b) SH rate, Male-victim 18–49



Notes: Estimates are from a 2SDID event-study specification implemented using the `did2s` Stata package (Butts, 2021). The first stage estimates state and year fixed effects, and the coefficients on covariates for the baseline controls interacted with a time trend, using untreated and not-yet-treated observations ($D_{s,t} = 0$); the second stage regresses residualized outcomes on event-time indicators. Unlike standard TWFE–OLS event studies, no reference event-time period is omitted. See Section 4.5 for interpretation. Vertical bars denote 95% confidence intervals based on jurisdiction-clustered standard errors.

6.4 Additional Robustness Checks

We report additional robustness exercises in Appendix Table A19. These include: (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 treatment based on the NFS law's passage year rather than its effective year. Results are generally similar across weighting schemes and timing definitions. In the unweighted specifications, point estimates for male-victim IPH are somewhat larger in magnitude, but the overall qualitative conclusions are unchanged. These checks provide additional support for the robustness of the main results.

7 Reported IPV Incidents, Assaults and Arrests

The SHR results above indicate that NFS laws 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), aggregated to state-year ratios, to examine whether incident classification and arrest patterns of reported IPV incidents change after NFS laws are adopted. Specifically, we study whether NFS law adoption is associated with changes in the share of reported IPV incidents classified as aggravated assaults, and in the share of aggravated IPV incidents that result in an arrest.

This is not a mediation analysis. Because NIBRS records only incidents reported to the police, that are formally recorded and submitted by participating police agencies, changes in our classification and arrest ratios may reflect changes in reporting, recording, agency participation, or incident composition, as well as changes in police classification or arrest behavior. We therefore use NIBRS data on reported IPV incidents to examine whether observed incident classification and arrest patterns move in directions consistent with the homicide results, not to identify a unique mechanism.

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 police 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 ($C = 1$); 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 the sample of reported incidents covered by NIBRS ($I = 1, C = 1$), we focus on two conditional probabilities. First, we study the conditional probability that the

IPV incident is classified as an aggravated assault ($A = 1$),

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

Second, within the sample of reported incidents covered by NIBRS that are recorded as aggravated assaults ($I = 1, C = 1, A = 1$), we study the probability that the incident is associated with an arrest ($R = 1$),

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

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.[34]

Figure 5 summarizes the selection problem that arises when using reported NIBRS incidents to study classification and arrest outcomes. 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). Reporting I affects classification A, and classification A in turn affects arrest R.

[34]The 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, $\text{Classification}_{d,s,t}$ approximates $P(A = 1 \mid I = 1,\ C = 1)$, and $\text{Enforcement}_{d,s,t}$ approximates $P(R = 1 \mid A = 1, I = 1,\ C = 1)$.

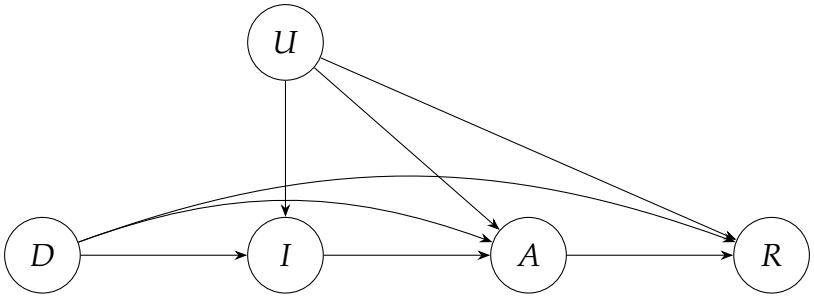


Figure 5: Selection and Case Processing of Reported IPV Incidents, Conditional on NIBRS-Reporting Agencies

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. Consequently, changes in the conditional probabilities $P(A = 1 \mid I = 1, C = 1)$ and $P(R = 1 \mid A = 1, I = 1, C = 1)$—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 reduced-form changes in these conditional probabilities among NIBRS-reporting agencies, reflecting both changes in recorded case processing and changes in the composition of incidents that reach the reporting and recording stage.

NFS laws may affect the frequency and composition of IPV incidents, reporting to police, formal recording, incident coding, and agency participation in NIBRS. Indeed, Appendix Table A20 shows that reported IPV incidents per 100,000 decline after NFS adoption. Hence, the ratio estimates reported below should be interpreted as changes within a changing set of reported and recorded incidents.

7.2 NFS Laws, Classification and Enforcement Ratios

Table 3 shows that NFS laws are associated with an increase in the share of reported IPV incidents classified as aggravated assaults for female victims aged 18–49. Appendix Table A21 shows that the estimate for women aged 50–70 is also positive and marginally significant, though this group is not the primary exposure group for NFS.

Table 3: NFS Laws and Reported IPV Aggravated-Assault Classification by Victim Sex

	$\text{Classification} = \frac{\text{Reported IPV Aggravated Assaults}}{\text{Reported IPV Incidents}}$				
	TWFE–OLS	2SDID	Classification Mean 1999	Classification Mean Counterfactual 2SDID	N [clusters]
Female victim	0.027**	0.055***	0.104	0.078	732
	(0.012)	(0.021)			[45]
Male victim	0.007	0.029	0.177	0.121	730
	(0.013)	(0.022)			[45]

Notes: Victims aged 18–49. The first two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on the classification ratio. TWFE–OLS models include state and year fixed effects. 2SDID estimates are obtained from regressions of residualized classification ratios on the NFS adoption indicator, where residuals are constructed using untreated and not-yet-treated observations. Reported IPV incidents include aggravated assault, simple assault, and intimidation incidents involving spouse, ex-spouse, boyfriend/girlfriend, or common-law spouse relationships in NIBRS-reporting agencies. The final three columns report the 1999 mean, the 2SDID counterfactual mean, and the sample size and number of clusters. Regressions are weighted by the 2000 state population aged 18–49. Standard errors clustered by jurisdiction are reported in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Table 4 shows increases in arrest rates conditional on aggravated IPV classification for female and male victims aged 18–49. Appendix Table A22 reports the corresponding estimates for victims aged 50–70.

Table 4: NFS Laws and Arrests among Reported IPV Aggravated Assaults by Victim Sex

	$\text{Enforcement} = \frac{\text{Arrests for Reported IPV Aggravated Assaults}}{\text{Reported IPV Aggravated Assaults}}$				
	TWFE–OLS	2SDID	Enforcement Mean 1999	Enforcement Mean Counterfactual 2SDID	N [clusters]
Female victim	0.058**	0.122***	0.615	0.483	720
	(0.027)	(0.032)			[45]
Male victim	0.062**	0.146***	0.588	0.471	710
	(0.026)	(0.043)			[44]

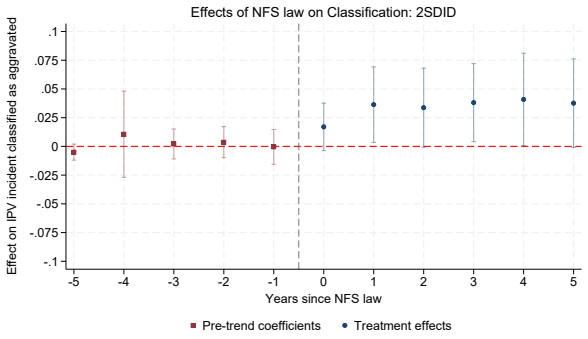
Notes: See footer of Table 3.

While selection and composition concerns remain (see Appendix Table A20), the patterns in Tables 3 and 4 are suggestive of changes in incident classification for aggravated assault and in arrest behavior among these cases.

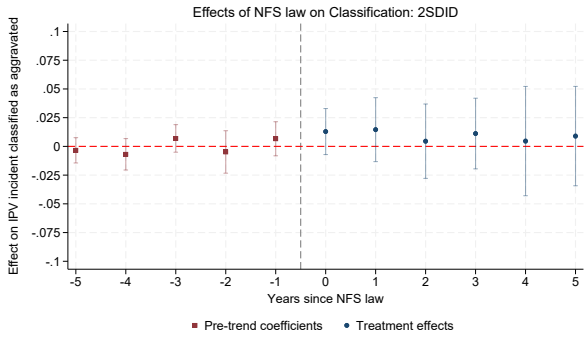
2SDID event-study estimates of dynamic effects echo these findings. The coefficients show no systematic pre-trends and, within one year of adoption, indicate increases in both the share of reported IPV incidents recorded in NIBRS that are classified as aggravated assaults for female victims aged 18–49 (Figure 6, panel (a)) and the arrest rate among IPV aggravated assaults recorded in NIBRS for both female and male victims aged 18–49 (Figure 7, panels (a) and (b)). These changes precede or coincide with the timing of the reductions in intimate partner homicides observed in the SHR data, which emerge with a one- to two-year lag.[35]

Figure 6: 2SDID Dynamic Effects of NFS Laws on $\frac{\text{Reported IPV Aggravated Assaults}}{\text{Reported IPV Incidents}}$

(a) Classification ratio, Female victim



(b) Classification ratio, Male victim

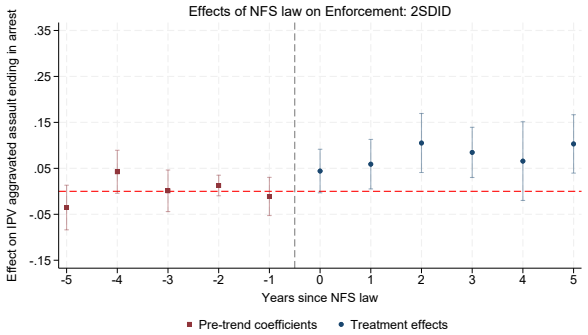


Notes: Victims aged 18–49. Estimates are from a 2SDID event-study specification implemented using the `did2s` Stata package (Butts, 2021). The first stage estimates state and year fixed effects using untreated and not-yet-treated observations ($D_{s,t} = 0$); the second stage regresses residualized outcomes on event-time indicators. Unlike standard TWFE–OLS event studies, no reference event-time period is omitted. See Section 4.5 for interpretation. Vertical bars denote 95% confidence intervals based on jurisdiction-clustered standard errors.

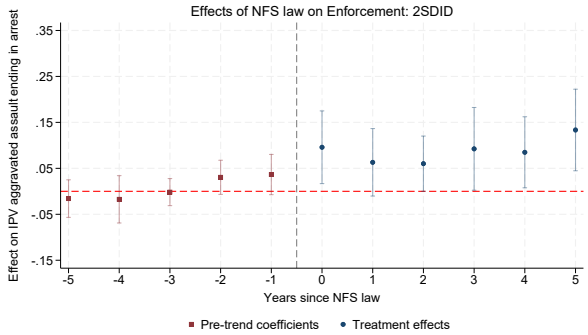
[35] Appendix Figure A15 and Appendix Figure A16 display the corresponding event-study estimates for female and male victims aged 50–70.

Figure 7: 2SDID Dynamic Effects of NFS Laws on $\frac{\text{Arrests for Reported IPV Aggravated Assaults}}{\text{Reported IPV Aggravated Assaults}}$

(a) Enforcement ratio, Female victim



(b) Enforcement ratio, Male victim



Notes: Victims aged 18–49. Estimates are from a 2SDID event-study specification implemented using the `did2s` Stata package (Butts, 2021). The first stage estimates state and year fixed effects using untreated and not-yet-treated observations ($D_{s,t} = 0$); the second stage regresses residualized outcomes on event-time indicators. Unlike standard TWFE–OLS event studies, no reference event-time period is omitted. See Section 4.5 for interpretation. Vertical bars denote 95% confidence intervals based on jurisdiction-clustered standard errors.

Taken together, the NIBRS patterns are suggestive of changes in reported case processing and enforcement after NFS adoption. They cannot distinguish whether the homicide reductions operate through deterrence, reporting changes, reclassification, arrest, prosecution, incapacitation, or reduced lethal self-defense. These patterns are therefore best read as suggestive evidence of a stronger criminal-justice response after NFS adoption, rather than as conclusive evidence of a specific channel.

Finally, our measure of arrests per reported aggravated IPV assault does not capture downstream prosecutorial or sentencing outcomes. However, evidence from a specific jurisdiction case study (Maricopa County in Arizona) with detailed strangulation-specific investigative protocols, suggests that improved recognition and documentation of strangulation may also shape later prosecutorial decisions (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 part-

ner violence (IPV) that often occurs at a dangerous stage of escalation and is strongly associated with later homicide.

This paper contributes to the literature in three ways. First, we assemble a new state-by-year dataset on NFS statutes, documenting the timing of a major but previously understudied wave of IPV-related criminal-law reforms. Second, we provide the first national causal evidence on the effect of these statutes on intimate partner homicide with data from the FBI Supplementary Homicide Reports, using a two-stage difference-in-differences estimator for staggered adoption and heterogeneous effects. Third, we connect the homicide effects to suggestive evidence on reported IPV incident classification and arrest patterns using NIBRS data.

We find evidence that NFS laws substantially reduce intimate partner homicide (IPH) among adults aged 18–49: the preferred estimates imply reductions of 0.169 female victims per 100,000 women and 0.090 male victims per 100,000 men, relative to counterfactual rates of 1.221 and 0.337, respectively. We find no comparable effects on IPH among victims aged 50–70 or on homicides committed by strangers. Event-study and sensitivity analyses do not indicate systematic pre-treatment divergence.

The NIBRS evidence is suggestive of changes in reported IPV case processing after NFS law adoption. NFS laws are associated with increases in the share of reported IPV incidents classified as aggravated assaults for female victims aged 18–49 and in the share of arrests among reported aggravated IPV assaults for both female and male victims aged 18–49.

Overall, the results indicate that statutory recognition of NFS as a serious criminal offense can reduce intimate partner homicide in the institutional environments in which these laws were adopted. The patterns for reported IPV aggravated assaults and arrests are compatible with earlier law-enforcement intervention disrupting high-risk abuse, but they also point to an important limitation: the lack of specific data on NFS incidents, charging decisions, and prosecutions.

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Appendix (not for publication)

A 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	No NFS statute by 2025		
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

Table A2: 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: Never treated means untreated by 2019. Population (relative) reports each cohort's share (%) of the population aged 18–70 in 2000, across those 50 jurisdictions (49 states + D.C.).

B 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 A3: 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., 2022, 2024), and state poverty rates (United States Census, 2023a). Sworn personnel and responding to calls data come from the BJS (Bureau of Justice Statistics); Reaves and Hickman (2002).

C Sample Coverage and Missingness

Table A4: Missing Data on Intimate Partner 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

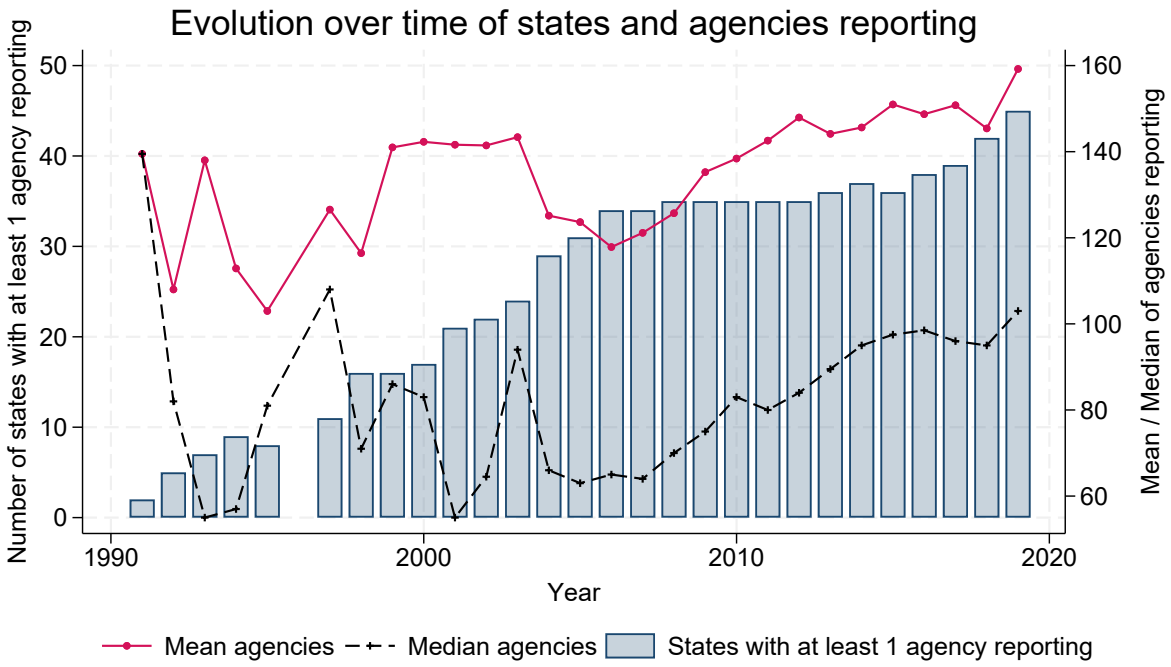
Notes: Whenever IPH is missing, it is missing for both female and male victims.

Table A5: Effect of NFS Laws on Missing IPH (1 if missing, 0 else)

	TWFE–OLS	2SDID
Missing IPH	0.004	0.006
	(0.008)	(0.010)

Notes: The first two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on an indicator for missing IPH in a state-year. When data are missing, they are missing for both female and male victims. TWFE–OLS models include state and year fixed effects. 2SDID estimates are obtained from regressions of residualized IPH rates on the NFS adoption indicator, where residuals are constructed using untreated and not-yet- treated observations. Regressions are weighted by the relevant 2000 state population. Standard errors clustered by jurisdiction (50 clusters = 49 states + D.C.) are reported in parentheses. $N = 1{,}500$. *p<0.10, **p<0.05, ***p<0.01.

Figure A1: NIBRS reporting over time



Notes: Whenever IPH is missing, it is missing for both female and male victims.

Table A6: NIBRS Missingness and Regression Sample

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

Notes: Missing (%, n) reports the percent and count of missing observations for **Classification**=$\frac{\text{Reported IPV Aggravated Assaults}}{\text{Reported IPV Incidents}}$ and **Enforcement**=$\frac{\text{Arrests for Reported IPV Aggravated Assaults}}{\text{Reported IPV Aggravated Assaults}}$.

Table A7: Effects of NFS Laws on missing Classification and Enforcement ratios

	TWFE–OLS	2SDID
Panel A. Classification missing (1 if missing, 0 else)		
Female-victim 18–49	−0.016 (0.060)	0.002 (0.063)
Male-victim 18–49	−0.015 (0.060)	0.003 (0.071)
Female-victim 50–70	−0.003 (0.063)	0.004 (0.071)
Male-victim 50–70	−0.004 (0.065)	0.007 (0.071)
Panel B. Enforcement missing (1 if missing, 0 else)		
Female-victim 18–49	−0.020 (0.061)	−0.002 (0.072)
Male-victim 18–49	−0.021 (0.062)	−0.013 (0.069)
Female-victim 50–70	−0.003 (0.063)	0.001 (0.071)
Male-victim 50–70	−0.013 (0.065)	−0.033 (0.065)

Notes: Each panel reports TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on the missing classification ratio (Panel A) and the missing enforcement ratio (Panel B). TWFE–OLS models include state and year fixed effects. 2SDID estimates are obtained from regressions of residualized outcomes on the NFS adoption indicator, where residuals are constructed using untreated and not-yet-treated observations. Regressions are weighted by the 2000 state population in the relevant age group. Standard errors clustered by jurisdiction are reported in parentheses. There are 50 clusters, corresponding to 49 states and D.C. $N = 1{,}500$. *p<0.10, **p<0.05, ***p<0.01.

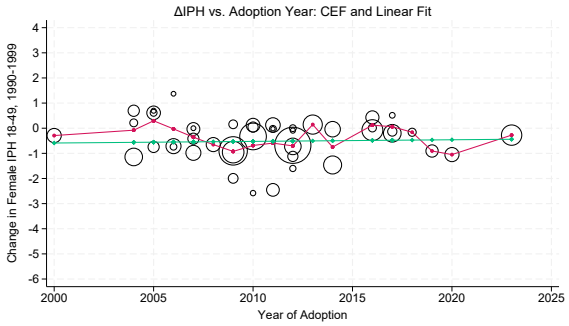
Table A8: Percentage of Population by Cohort and Age Group

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

Notes: Never treated refers to jurisdictions untreated by 2019. Population (relative) reports each cohort's share (%) of the relevant age-group population in 2000, across the 50 jurisdictions: 49 states and D.C.

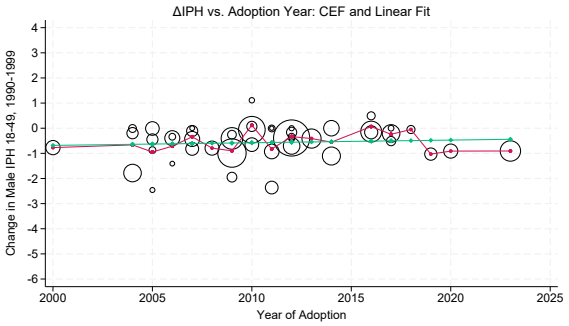
Figure A2: Change in Intimate Partner Homicides per 100,000 (IPH rate) from 1990 to 1999 and Year of NFS Law Adoption, by Victim Sex and Age Group

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



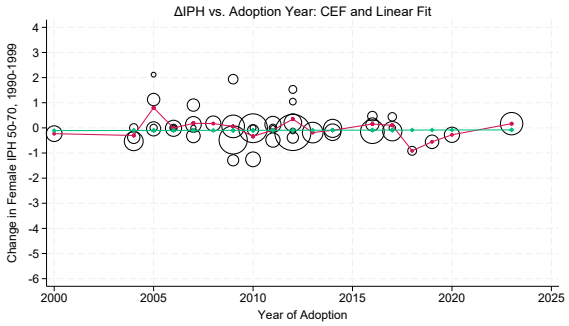
Slope coefficient: 0.006 (0.017), R^2: 0.003

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



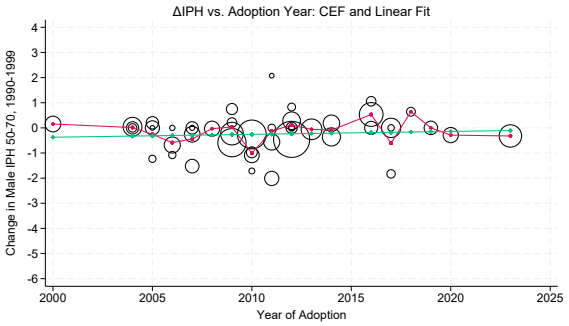
Slope coefficient: 0.011 (0.023), R^2: 0.009

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



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

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



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

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 between 1990 and 1999).

D Pre-Adoption Balance

Table A9: Regression of Change in Covariates from 1990 to 1999 on Year of Adoption

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

Notes: Each cell 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 (49 states + D.C.). Robust HC3 standard errors in parentheses. *p-value<0.1, **p-value<0.05, ***p-value<0.01.

Table A10: Regression of Dependent Variable in 1990 on Year of Adoption

Dependent variable	Coefficient	R-squared
income per capita	78.98	0.020
	(90.72)	
log(income per capita)	0.0038	0.018
	(0.0044)	
unemployment rate	-0.0126	0.007
	(0.0246)	
poverty rate	-0.081	0.014
	(0.0861)	
male-to-female unemployment	-0.0075	0.010
	(0.0157)	
IPH rate, female-victim 18–49	-0.0141	0.007
	(0.0263)	
IPH rate, male-victim 18–49	-0.0255*	0.030
	(0.0134)	
IPH rate, female-victim 50–70	-0.0020	0.001
	(0.0143)	
IPH rate, male-victim 50–70	-0.0202	0.046
	(0.0121)	

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 (49 states + D.C.) 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 A11: 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, Female-victim 18–49	-0.52	-0.53	-0.01 (0.59)
Δ IPH rate, Male-victim 18–49	-0.54	-0.91	-0.37 (0.09)***
Δ IPH rate, Female-victim 50–70	-0.10	0.03	0.13 (0.34)
Δ IPH rate, Male-victim 50–70	-0.24	-0.31	-0.07 (0.09)

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.

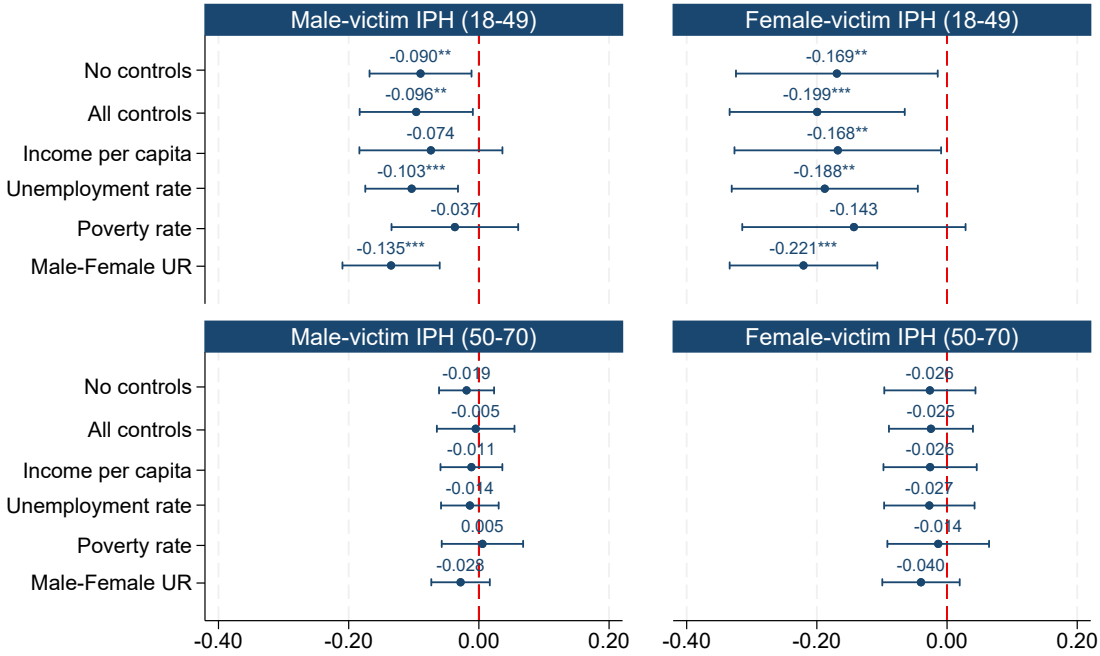
Table A12: Mean Covariates in 1990, 1999, and 1990–1999 Change

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 (49 states + D.C.). Robust HC3 standard errors in parentheses. *p-value<0.1, **p-value<0.05, ***p-value<0.01.

E IPH effects on individuals 50–70, and comparison of specifications with and without controls

Figure A3: 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. Male-Female UR = $\frac{\text{male unemployment rate}}{\text{female unemployment rate}}$.

Table A13: Effect of NFS Laws on Intimate Partner Homicides per 100,000 (IPH rate), by Victim Sex among individuals aged 50–70

	TWFE–OLS	2SDID	IPH rate Mean 1999	IPH rate Mean Counterfactual 2SDID
Panel A. Without Controls				
Female-victim 50–70	-0.029	-0.026	0.480	0.511
	(0.028)	(0.036)		
Male-victim 50–70	-0.014	-0.019	0.266	0.224
	(0.019)	(0.022)		
Panel B. With Controls				
Female-victim 50–70	-0.028	-0.025	0.480	0.509
	(0.027)	(0.033)		
Male-victim 50–70	-0.004	-0.005	0.266	0.210
	(0.021)	(0.030)		

Notes: Victims aged 50–70. The first two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on IPH rates per 100,000. TWFE–OLS models include state and year fixed effects. 2SDID estimates are obtained from regressions of residualized IPH rates on the NFS adoption indicator, where residuals are constructed after estimating state and year fixed effects using untreated and not-yet-treated observations. Panel B adds baseline 1990 demographic and socioeconomic controls interacted with linear time trends. The final two columns report the mean IPH rate in 1999 (the year before any state enacted an NFS law) and the 2SDID counterfactual mean, estimated from the untreated potential outcome model among treated observations. Regressions are weighted by the 2000 state population aged 50–70. Standard errors clustered by jurisdiction (50 clusters = 49 states + D.C.) are reported in parentheses. $N = 1{,}479$. *p<0.10, **p<0.05, ***p<0.01.

F Poisson estimates, exclusion of early adopters, exclusion of one state at a time, and exclusion of states with post-1999 DV-policing changes

Table A14: Effects of NFS Laws on Intimate Partner Homicides (IPH counts) by Victim Sex and Age Group: Poisson Model

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

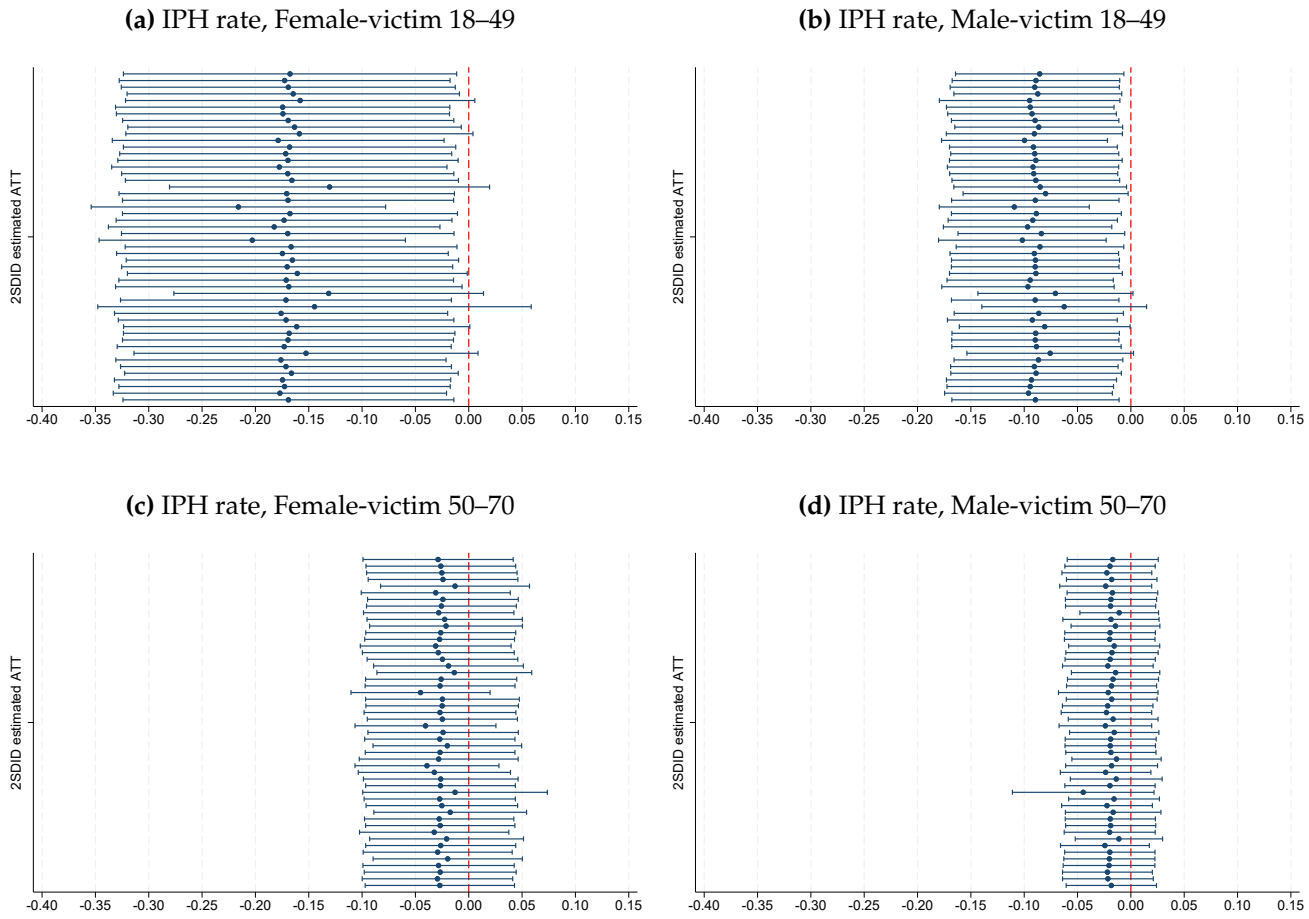
Notes: All regressions are Poisson count models with state and year fixed effects. The dependent variable is the count of intimate partner homicides, and the relevant sex-age population is included as an exposure. Reported coefficients are log incidence-rate ratios. Standard errors are clustered at the jurisdiction level, with 50 clusters, and are shown in parentheses. $N = 1{,}479$. *p<0.10, **p<0.05, ***p<0.01.

Table A15: 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)

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

Notes: Missouri and North Carolina are excluded because their statutes did not define strangulation. The first two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on IPH rates per 100,000. TWFE–OLS models include state and year fixed effects. 2SDID estimates are obtained from regressions of residualized IPH rates on the NFS adoption indicator, where residuals are constructed after estimating state and year fixed effects using untreated and not-yet-treated observations. Regressions are weighted by the relevant cohort-age population in 2000. Standard errors clustered by jurisdiction (48 clusters) are reported in parentheses. $N = 1,419$. *p<0.1, **p<0.05, ***p<0.01.

Figure A4: 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 1, panel B, while omitting one state at a time.

Table A16: 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	
	TWFE–OLS	2SDID	TWFE–OLS	2SDID
Panel A. Excluding 7 states (Clarke et al., 2024)				
Female-victim 18–49	-0.125**	-0.229***	0.020	0.015
	(0.056)	(0.066)	(0.019)	(0.034)
Male-victim 18–49	-0.059*	-0.111***	-0.032	-0.004
	(0.031)	(0.042)	(0.148)	(0.231)
Female-victim 50–70	-0.017	-0.028	0.006	0.012
	(0.027)	(0.033)	(0.013)	(0.018)
Male-victim 50–70	0.004	-0.005	-0.003	-0.029
	(0.022)	(0.033)	(0.035)	(0.059)
Panel B. Excluding 5 states (Dasgupta and Pacheco, 2018)				
Female-victim 18–49	-0.105*	-0.188**	0.027	0.021
	(0.057)	(0.079)	(0.018)	(0.034)
Male-victim 18–49	-0.059*	-0.095**	-0.025	0.028
	(0.034)	(0.048)	(0.149)	(0.248)
Female-victim 50–70	-0.017	-0.010	-0.002	0.007
	(0.028)	(0.033)	(0.013)	(0.018)
Male-victim 50–70	-0.011	-0.012	0.003	-0.019
	(0.023)	(0.034)	(0.036)	(0.063)
Panel C. Excluding 12 states in panels A and B				
Female-victim 18–49	-0.125**	-0.217***	0.027	0.029
	(0.060)	(0.079)	(0.020)	(0.037)
Male-victim 18–49	-0.054	-0.112**	-0.030	0.048
	(0.034)	(0.046)	(0.160)	(0.275)
Female-victim 50–70	-0.005	-0.012	0.003	0.012
	(0.027)	(0.033)	(0.014)	(0.018)
Male-victim 50–70	-0.003	-0.011	0.000	-0.018
	(0.023)	(0.037)	(0.037)	(0.065)

Notes: The first (last) two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on IPH (SH) rates per 100,000. TWFE–OLS models include state and year fixed effects. 2SDID estimates are obtained from regressions of residualized IPH (SH) rates on the NFS adoption indicator, where residuals are constructed after estimating state and year fixed effects using untreated and not-yet-treated observations. 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 (SC was already excluded) with $N = 1{,}335$. Panel C excludes both groups; the sample contains 38 clusters (SC was already excluded) with $N = 1{,}125$. Clustered standard errors in parentheses. *p<0.10, **p<0.05, ***p<0.01.

G Assessing heterogeneity by baseline characteristics

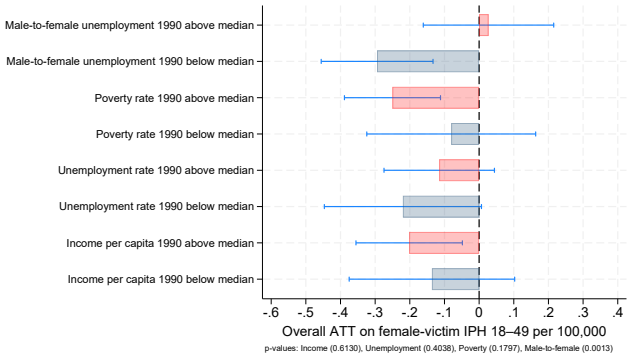
For each characteristic, we define a binary indicator equal to one if the value of the characteristic is above the median and zero otherwise. Figure A5 shows substantial apparent heterogeneity, especially with respect to the male-to-female unemployment ratio. Given that states above and below the median 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 A6. The apparent heterogeneity documented in Figure A5 disappears. We therefore find no support for heterogeneous impacts of NFS laws across states based on the proxies of gender inequality and economic resources in 1990.[36]

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

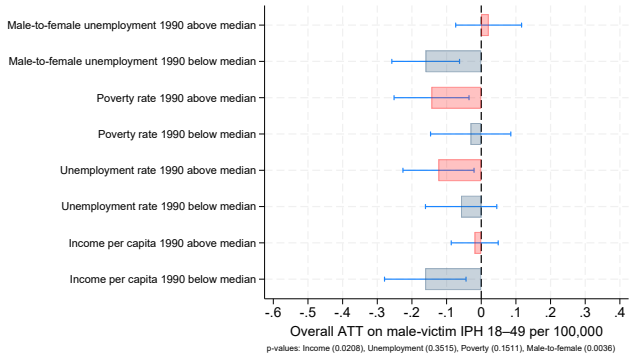
[36]Indeed, once we control for group-specific linear trends, and accounting for the fact that we conduct a total of 16 heterogeneity tests (four outcomes × four baseline characteristics). Applying a Bonferroni correction at the 5% significance level requires p-values $\leq \frac{0.05}{16} = 0.003125$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

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

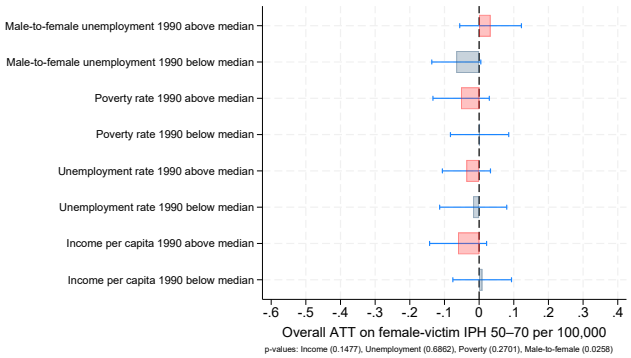
(a) IPH rate, Female-victim 18–49



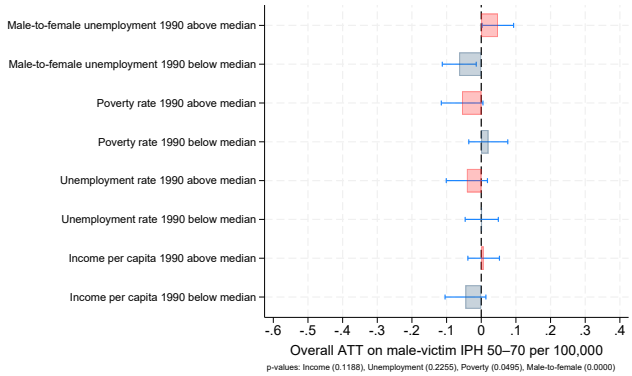
(b) IPH rate, Male-victim 18–49



(c) IPH rate, Female-victim 50–70



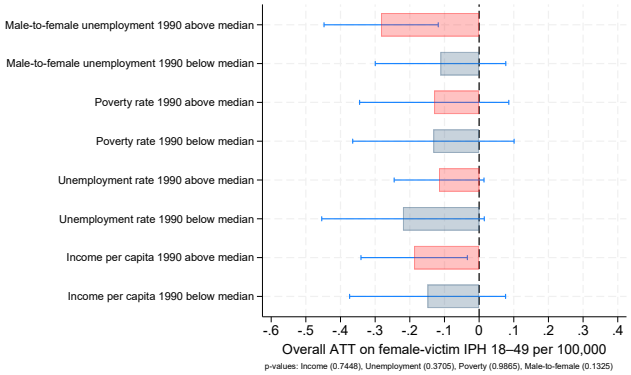
(d) IPH rate, Male-victim 50–70



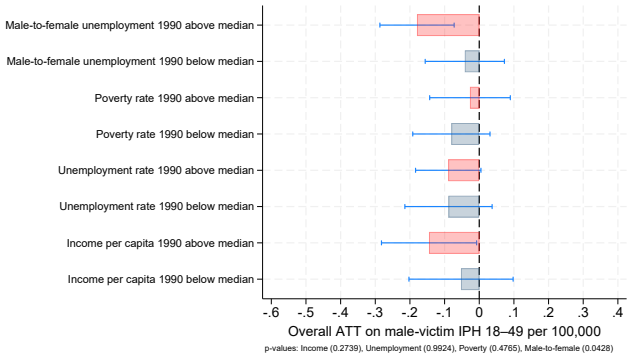
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. State and year fixed effects are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). Estimation is conducted using the `did2s` Stata package developed by Butts (2021). Horizontal bars denote 95% confidence intervals based on jurisdiction-clustered standard errors.

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

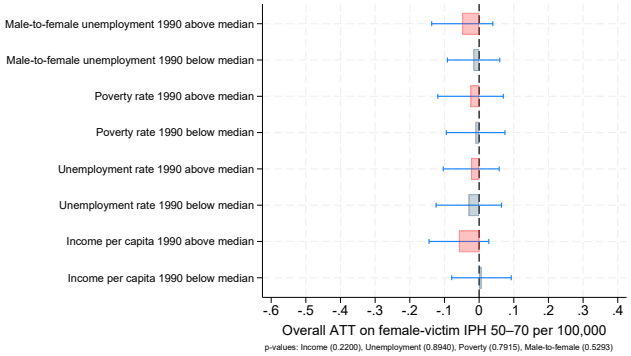
(a) IPH rate, Female-victim 18–49



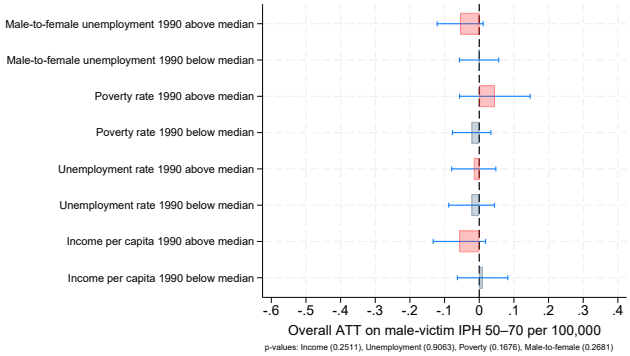
(b) IPH rate, Male-victim 18–49



(c) IPH rate, Female-victim 50–70



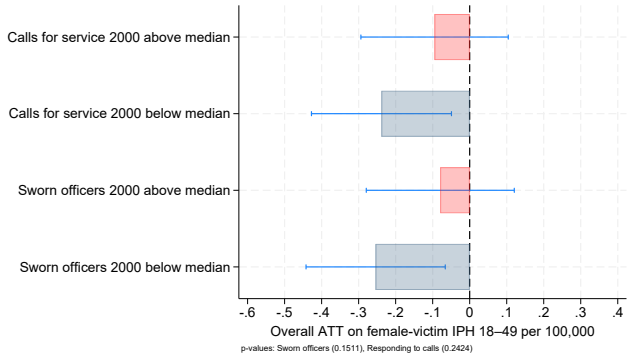
(d) IPH rate, Male-victim 50–70



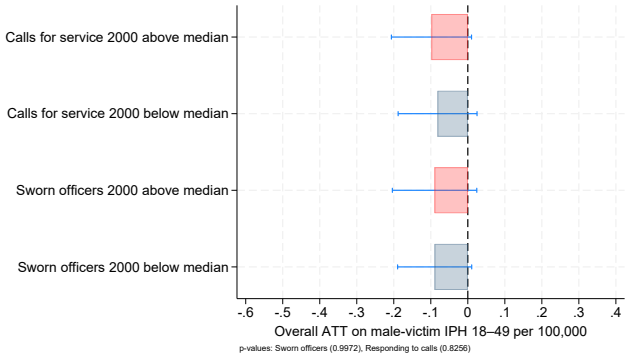
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. The coefficients on the control variable $X_s \times t$, as well as state and year fixed effects, are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). Estimation is conducted using the `did2s` Stata package developed by Butts (2021). Horizontal bars denote 95% confidence intervals based on jurisdiction-clustered standard errors.

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

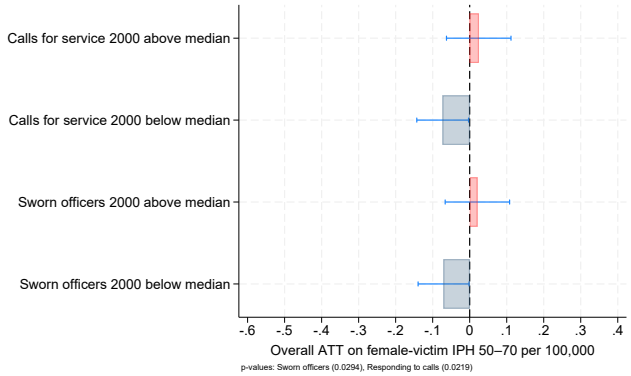
(a) IPH rate, Female-victim 18–49



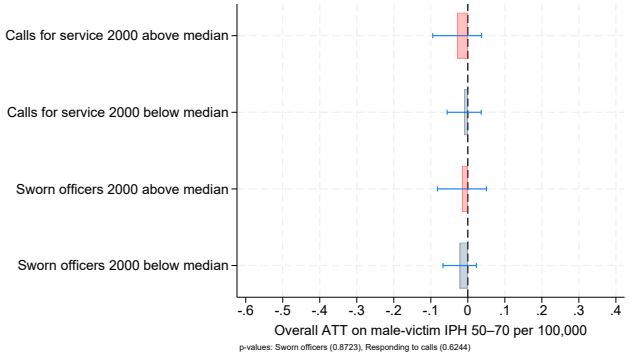
(b) IPH rate, Male-victim 18–49



(c) IPH rate, Female-victim 50–70



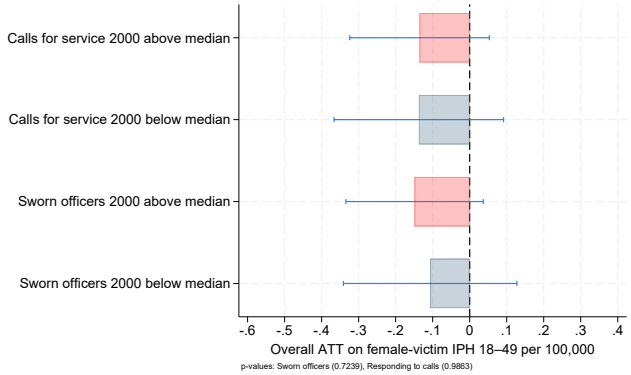
(d) IPH rate, Male-victim 50–70



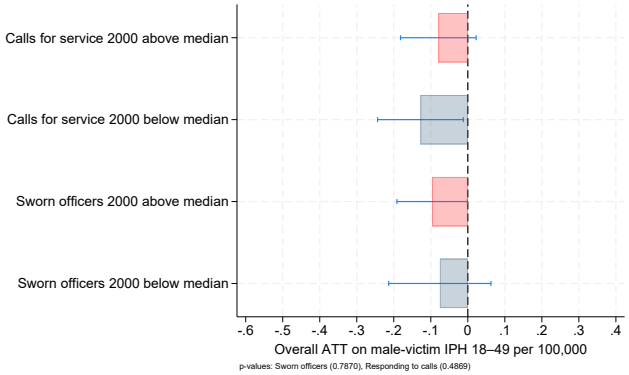
Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. State and year fixed effects are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). Estimation is conducted using the `did2s` Stata package developed by Butts (2021). Horizontal bars denote 95% confidence intervals based on jurisdiction-clustered standard errors.

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

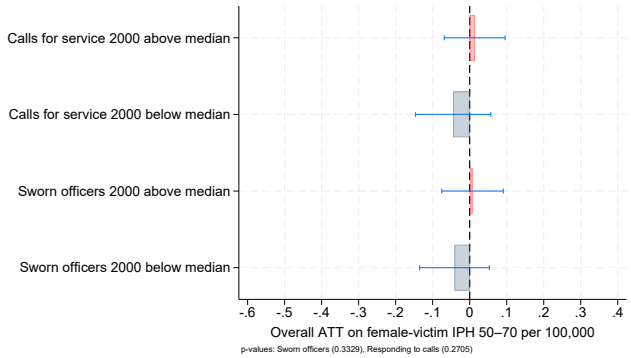
(a) IPH rate, Female-victim 18–49



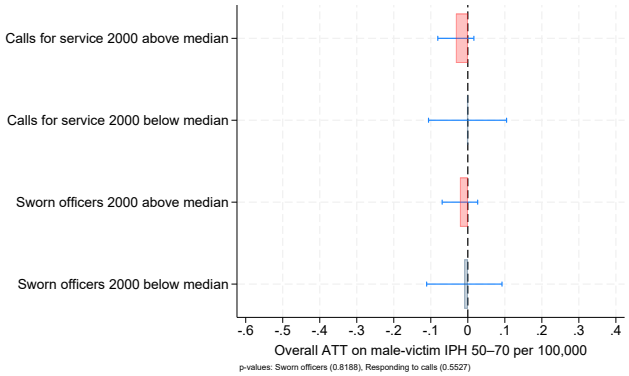
(b) IPH rate, Male-victim 18–49



(c) IPH rate, Female-victim 50–70



(d) IPH rate, Male-victim 50–70

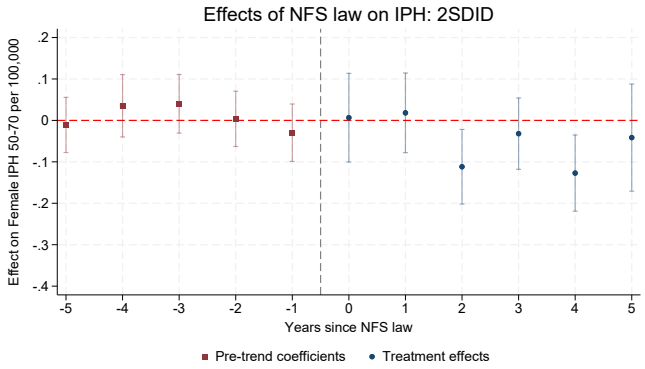


Notes: The heterogeneous estimates are based on 2SDID estimation, including the treatment variable $D_{s,t}$ and its interaction with the baseline characteristic X_s (i.e., $D_{s,t} \times X_s$) as regressors in the second stage. The coefficients on the control variable $X_s \times t$, as well as state and year fixed effects, are estimated in the first stage using the sample of untreated/not-yet-treated observations ($D_{s,t} = 0$). Estimation is conducted using the `did2s` Stata package developed by Butts (2021). Horizontal bars denote 95% confidence intervals based on jurisdiction-clustered standard errors.

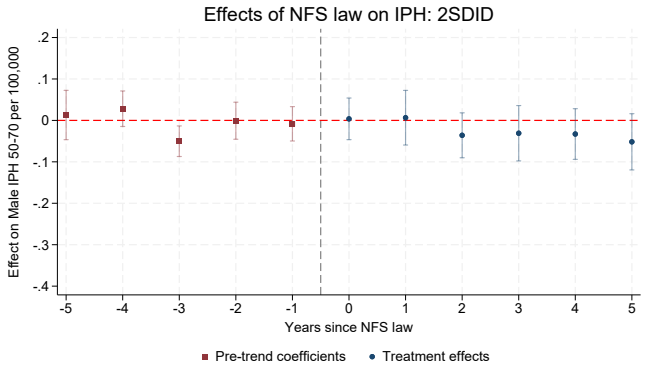
H Event-study figures for IPH victims aged 50–70 and Honest DiD

Figure A9: 2SDID Dynamic Effects of NFS Laws on IPH rates among 50–70

(a) IPH rate, Female-victim 50–70



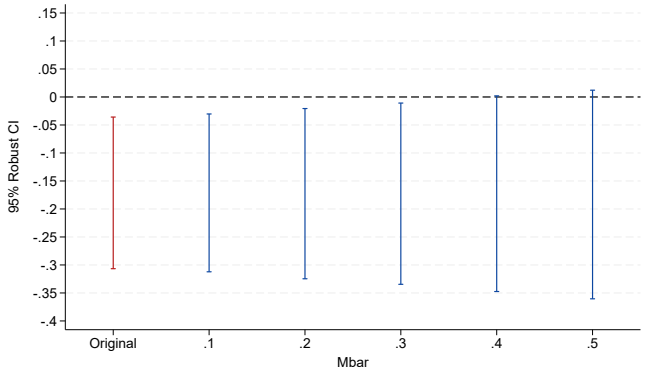
(b) IPH rate, Male-victim 50–70



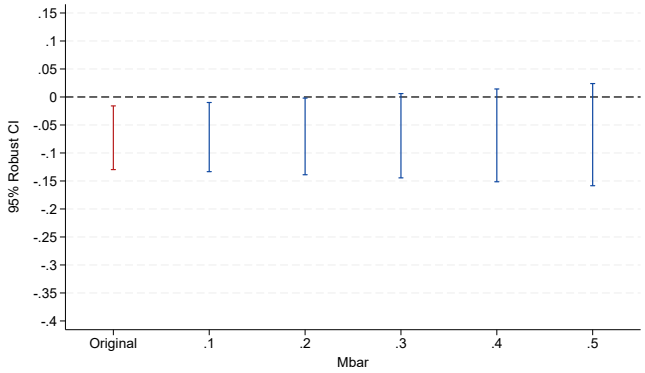
Notes: Estimates are from a 2SDID event-study specification implemented using the `did2s` Stata package (Butts, 2021). The first stage estimates state and year fixed effects using untreated and not-yet-treated observations ($D_{s,t} = 0$); the second stage regresses residualized outcomes on event-time indicators. Unlike standard TWFE–OLS event studies, no reference event-time period is omitted. See Section 4.5 for interpretation. Vertical bars denote 95% confidence intervals based on state-clustered standard errors.

Figure A10: Robustness of 2SDID estimates to violation of parallel trends

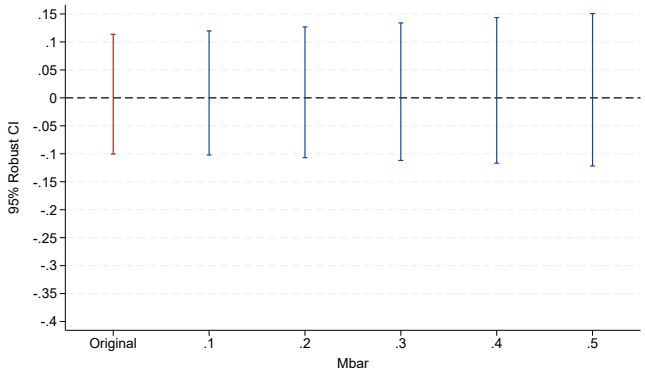
(a) IPH rate, Female-victim 18–49



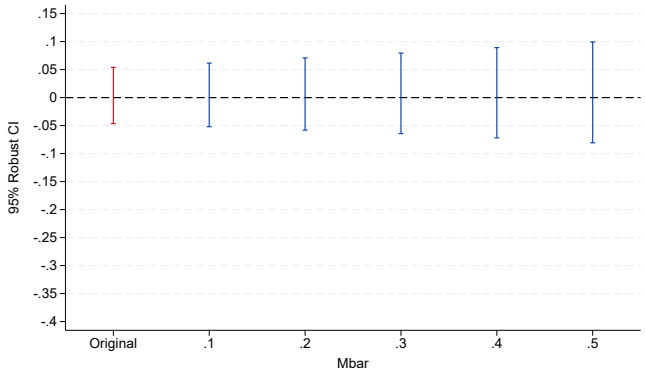
(b) IPH rate, Male-victim 18–49



(c) IPH rate, Female-victim 50–70



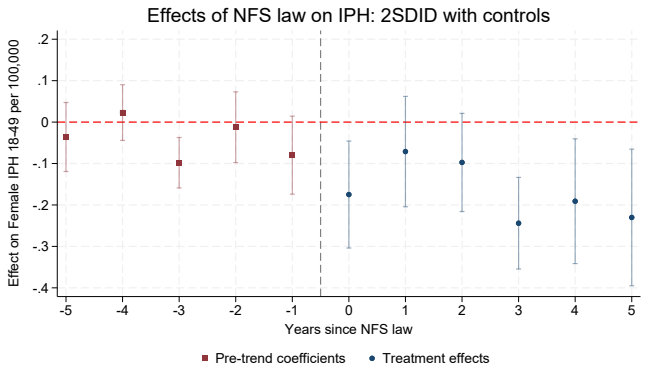
(d) IPH rate, Male-victim 50–70



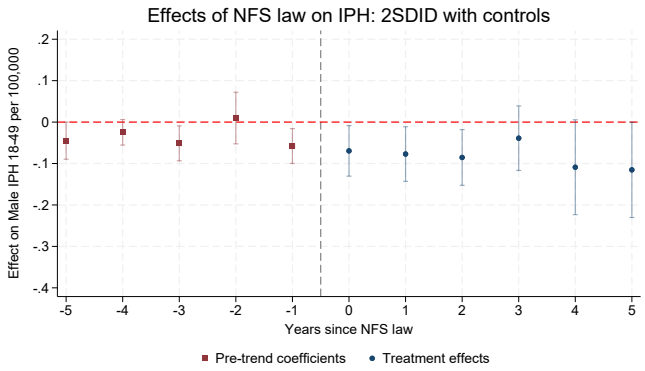
Notes: The figure reports robust confidence sets for the treatment effect in year zero (the year the NFS became effective) under different magnitudes of parallel trends violations (Mbar). The Honest DiD framework was developed by Rambachan and Roth (2023), and the calculations were performed using the `honestdid` Stata package.

Figure A11: 2SDID Dynamic Effects of NFS Laws on IPH rates (per 100,000) with Baseline Covariates Interacted with Linear Time Trends

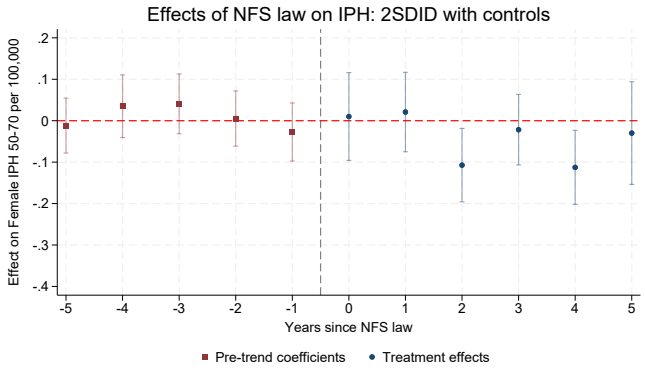
(a) IPH rate, Female-victim 18–49



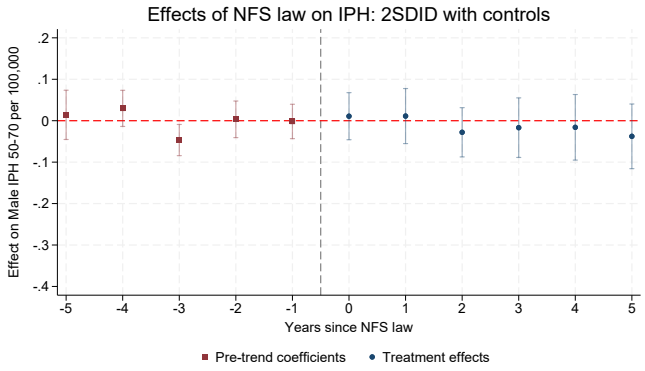
(b) IPH rate, Male-victim 18–49



(c) IPH rate, Female-victim 50–70

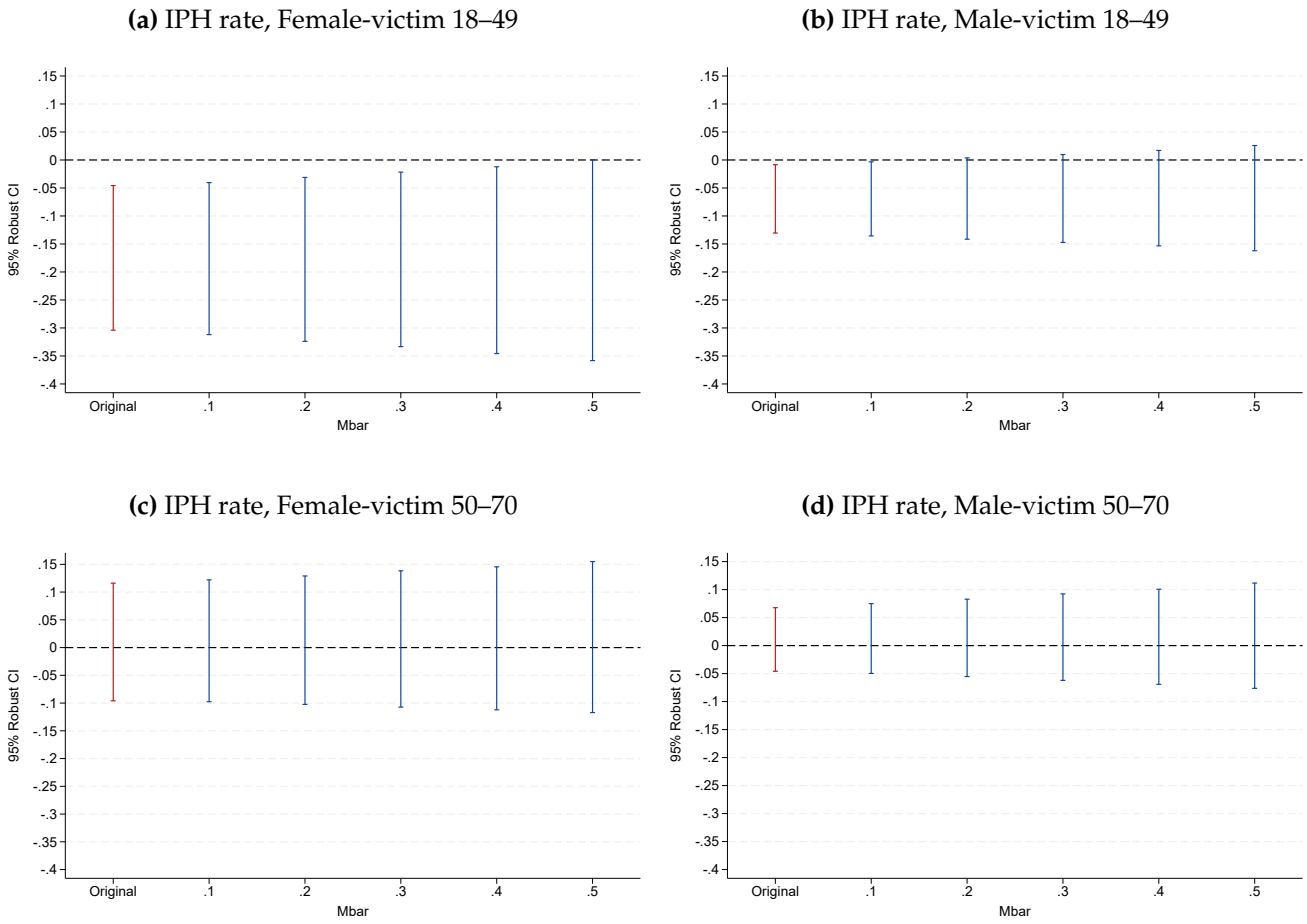


(d) IPH rate, Male-victim 50–70



Notes: Estimates are from a 2SDID event-study specification implemented using the `did2s` Stata package (Butts, 2021). The first stage estimates state and year fixed effects, and the coefficients on covariates for the baseline controls interacted with a time trend, using untreated and not-yet-treated observations ($D_{s,t} = 0$); the second stage regresses residualized outcomes on event-time indicators. Unlike standard TWFE–OLS event studies, no reference event-time period is omitted. See Section 4.5 for interpretation. Vertical bars denote 95% confidence intervals based on state-clustered standard errors.

Figure A12: Robustness of 2SDID estimates (with controls) to violation of parallel trends



Notes: The figure 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.

I Stranger-homicide rates for victims aged 50–70, without controls, and dropping one state at a time

Table A17: Effects of NFS Laws on Stranger-Perpetrated Homicides per 100,000 (SH rate) by Victim Sex among individuals aged 50–70, with Baseline Covariates Interacted with Linear Time Trends

	TWFE–OLS	2SDID	SH rate Mean 1999	SH rate Mean Counterfactual 2SDID
Female-victim 50–70	0.001	0.008	0.073	0.044
	(0.013)	(0.018)		
Male-victim 50–70	-0.000	-0.029	0.273	0.333
	(0.034)	(0.057)		

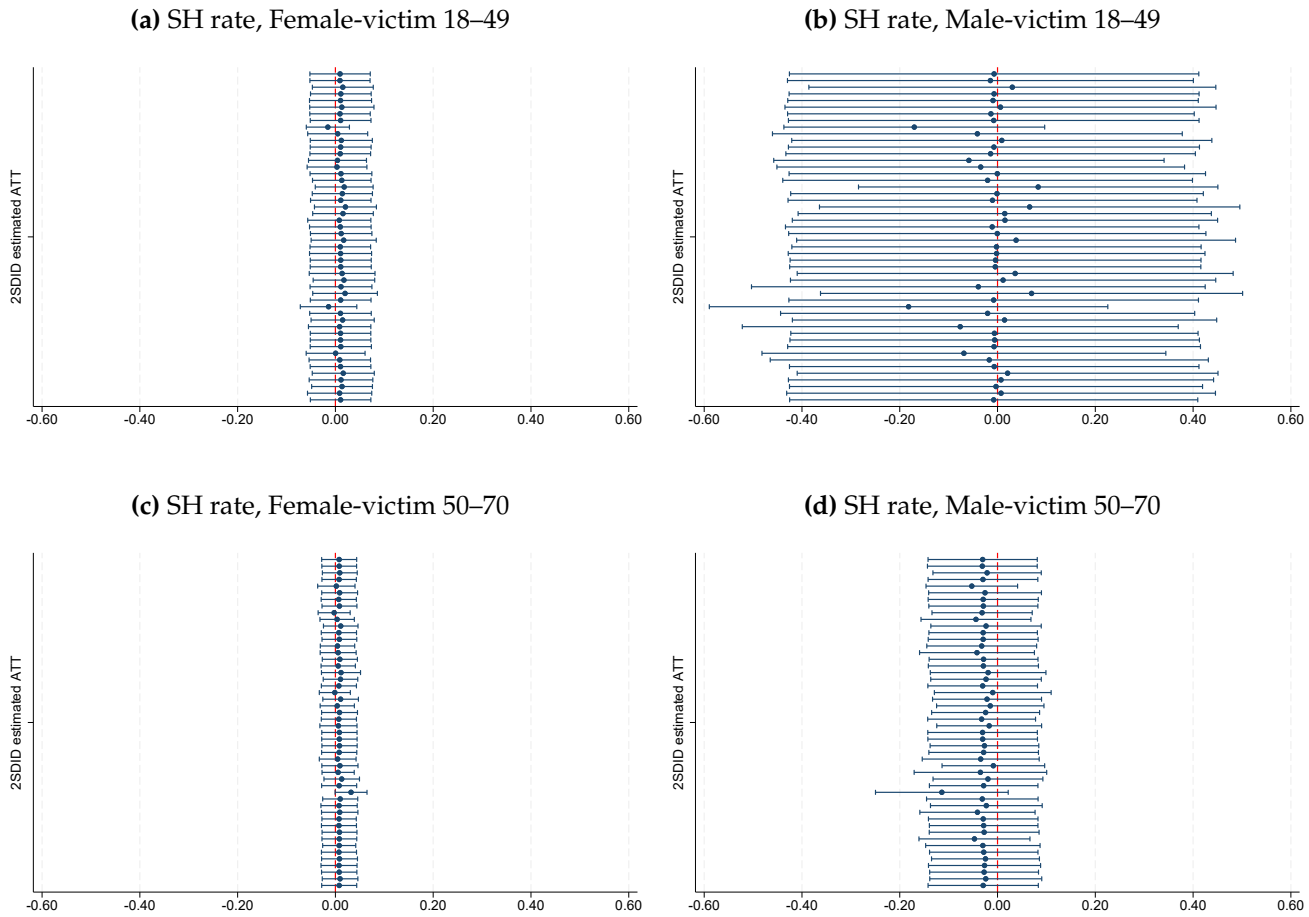
Notes: Victims aged 50–70. The first two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on SH rates per 100,000. TWFE–OLS models include jurisdiction and year fixed effects. 2SDID estimates are obtained from regressions of residualized SH rates on the NFS adoption indicator, where residuals are constructed using untreated and not-yet-treated observations. All specifications include baseline 1990 socioeconomic controls interacted with linear time trends. The final two columns report the mean SH rate in 1999, before any jurisdiction enacted an NFS law, and the 2SDID counterfactual mean estimated from the untreated potential outcome model among treated observations. Regressions are weighted by the 2000 state population aged 50–70. Standard errors clustered by jurisdiction are reported in parentheses. There are 50 clusters, corresponding to 49 states and D.C. $N = 1{,}479$. *p<0.10, **p<0.05, ***p<0.01.

Table A18: Effects of NFS Laws on Stranger-Perpetrated Homicides per 100,000 (SH rate) by Victim Sex and Age Group

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

Notes: The first two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on SH rates per 100,000. TWFE–OLS models include jurisdiction and year fixed effects. 2SDID estimates are obtained from regressions of residualized SH rates on the NFS adoption indicator, where residuals are constructed using untreated and not-yet-treated observations. The final two columns report the mean SH rate in 1999, before any jurisdiction enacted an NFS law, and the 2SDID counterfactual mean estimated from the untreated potential outcome model among treated observations. Regressions are weighted by the relevant cohort-age population in 2000. Standard errors clustered by jurisdiction are reported in parentheses. There are 50 clusters, corresponding to 49 states and D.C. $N = 1{,}479$. *p<0.10, **p<0.05, ***p<0.01.

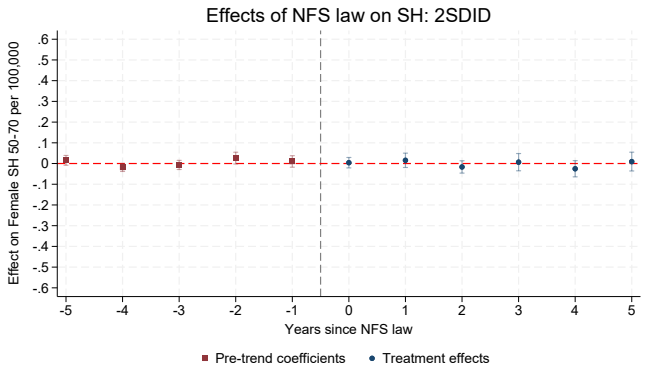
Figure A13: Overall ATT estimates on SH rates: Dropping one state at a time



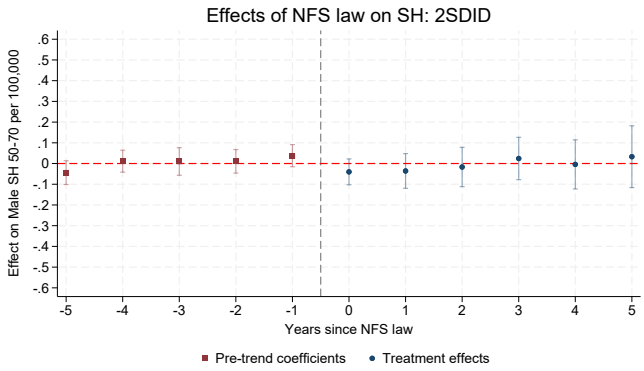
Notes: Each panel reports the 2SDID point estimates together with their 95% confidence intervals, obtained by re-estimating the regressions in Table 2, while omitting one state at a time.

Figure A14: 2SDID Dynamic Effects of NFS Laws on Stranger-Perpetrated Homicides per 100,000 (SH rate) among Victims Aged 50–70, by Victim Sex, with control variables

(a) SH rate, Female-victim 50–70



(b) SH rate, Male-victim 50–70



Notes: Estimates are from a 2SDID event-study specification implemented using the `did2s` Stata package (Butts, 2021). The first stage estimates state and year fixed effects, and the coefficients on covariates for the baseline controls interacted with a time trend, using untreated and not-yet-treated observations ($D_{s,t} = 0$); the second stage regresses residualized outcomes on event-time indicators. Unlike standard TWFE–OLS event studies, no reference event-time period is omitted. See Section 4.5 for interpretation. Vertical bars denote 95% confidence intervals based on state-clustered standard errors.

J Additional robustness checks: without weights, with time-varying weights, including South Carolina and using passage instead of effective date

Table A19: Effect of NFS Laws on Intimate Partner Homicide (IPH) and Stranger Homicide (SH) Rates (per 100,000)

	IPH rate		SH rate	
	TWFE–OLS	2SDID	TWFE–OLS	2SDID
Panel A. Unweighted				
Female-victim 18–49	-0.105*	-0.180**	0.032	-0.033
	(0.058)	(0.089)	(0.029)	(0.094)
Male-victim 18–49	-0.131***	-0.167***	0.080	-0.610
	(0.045)	(0.048)	(0.222)	(1.021)
Female-victim 50–70	-0.023	-0.028	0.021	0.024
	(0.046)	(0.062)	(0.022)	(0.035)
Male-victim 50–70	-0.035	-0.058	-0.043	-0.251
	(0.034)	(0.054)	(0.043)	(0.233)
Panel B. Time-varying weights				
Female-victim 18–49	-0.103*	-0.205***	0.023	0.013
	(0.052)	(0.069)	(0.019)	(0.033)
Male-victim 18–49	-0.061**	-0.092**	-0.021	-0.008
	(0.030)	(0.043)	(0.139)	(0.212)
Female-victim 50–70	-0.035	-0.034	-0.001	0.007
	(0.028)	(0.032)	(0.012)	(0.018)
Male-victim 50–70	-0.001	-0.000	-0.002	-0.030
	(0.020)	(0.028)	(0.032)	(0.055)
Panel C. South Carolina included				
Female-victim 18–49	-0.078	-0.145*	0.023	0.022
	(0.062)	(0.075)	(0.018)	(0.032)
Male-victim 18–49	-0.059*	-0.096**	-0.008	0.064
	(0.031)	(0.043)	(0.140)	(0.221)
Female-victim 50–70	-0.031	-0.019	0.003	0.013
	(0.026)	(0.030)	(0.013)	(0.019)
Male-victim 50–70	0.001	-0.002	0.018	0.013
	(0.021)	(0.027)	(0.037)	(0.054)
Panel D. Passage-year treatment				
Female-victim 18–49	-0.110**	-0.204***	0.014	0.006
	(0.053)	(0.067)	(0.018)	(0.033)
Male-victim 18–49	-0.059*	-0.094**	-0.045	-0.021
	(0.032)	(0.046)	(0.139)	(0.220)
Female-victim 50–70	-0.019	-0.020	-0.004	0.002
	(0.026)	(0.033)	(0.012)	(0.018)
Male-victim 50–70	-0.009	-0.011	-0.015	-0.039
	(0.021)	(0.030)	(0.028)	(0.054)

Notes: All regressions include baseline (1990) covariates interacted with linear time trends. Standard errors clustered at the jurisdiction level. Panels A,B and D: $N = 1,479$. (clusters = 50); Panel C: $N = 1,509$. (clusters = 51). *p<0.10, **p<0.05, ***p<0.01.

K Additional NIBRS estimates

Table A20: NFS Laws and Reported IPV incidents per 100,000 (Reported IPV rate)

	TWFE–OLS	2SDID	Reported IPV rate Mean 1999	Reported IPV rate Mean Counterfactual 2SDID
Female-victim 18–49	-156.301***	-132.311**	411.47	550.07
	(38.962)	(61.267)		
Male-victim 18–49	-28.891***	-22.026*	81.23	115.11
	(6.518)	(12.403)		
Female-victim 50–70	-16.798***	-17.031**	35.87	78.37
	(4.734)	(7.071)		
Male-victim 50–70	-6.729**	-7.939**	13.62	38.65
	(2.773)	(3.610)		

Notes: The first two columns report TWFE–OLS and 2SDID estimates of the effect of NFS law adoption on the reported IPV rate. TWFE–OLS models include state and year fixed effects. 2SDID estimates are obtained from regressions of residualized reported IPV rate on the NFS adoption indicator, where residuals are constructed using untreated and not-yet-treated observations. Reported IPV incidents include aggravated assault, simple assault, and intimidation incidents involving spouse, ex-spouse, boyfriend/girlfriend, or common-law spouse relationships in NIBRS-reporting agencies. The last two columns show the 1999 mean and the counterfactual mean. $N = 734$. Regressions are weighted by the relevant cohort-age population in 2000. Standard errors are clustered at the jurisdiction level (45 clusters). *p<0.10, **p<0.05, ***p<0.01.

Table A21: NFS Laws and Reported IPV Aggravated-Assault Classification, by Victim Sex among individuals aged 50–70

	$\text{Classification} = \frac{\text{Reported IPV Aggravated Assaults}}{\text{Reported IPV Incidents}}$				
	TWFE–OLS	2SDID	Classification Mean 1999	Classification Mean Counterfactual 2SDID	N [clusters]
Female-victim	0.013	0.043*	0.148	0.084	713
	(0.018)	(0.025)			[45]
Male-victim	0.003	0.033	0.214	0.150	701
	(0.022)	(0.034)			[45]

Notes: Regressions are weighted by the relevant cohort-age population in 2000. Standard errors clustered at the jurisdiction level are shown in parentheses. *p<0.1, **p<0.05, ***p<0.01.

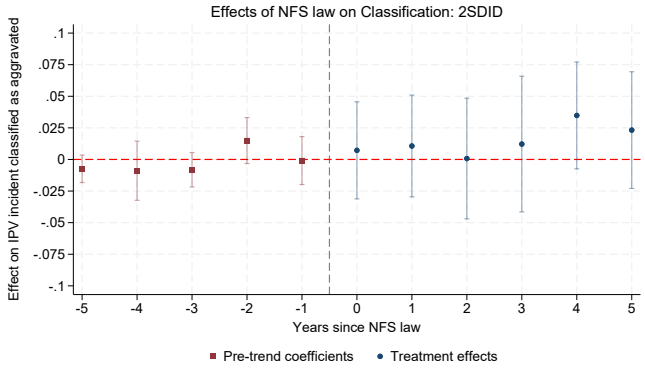
Table A22: NFS Laws and Arrests among Reported IPV Aggravated Assaults, by Victim Sex among individuals aged 50–70

	Enforcement $= \frac{\text{Arrests for Reported IPV Aggravated Assaults}}{\text{Reported IPV Aggravated Assaults}}$				
	TWFE–OLS	2SDID	Enforcement Mean 1999	Enforcement Mean Counterfactual 2SDID	N [clusters]
Female-victim	0.051	0.227***	0.793	0.411	656
	(0.036)	(0.079)			[44]
Male-victim	0.029	0.027	0.680	0.609	635
	(0.034)	(0.064)			[45]

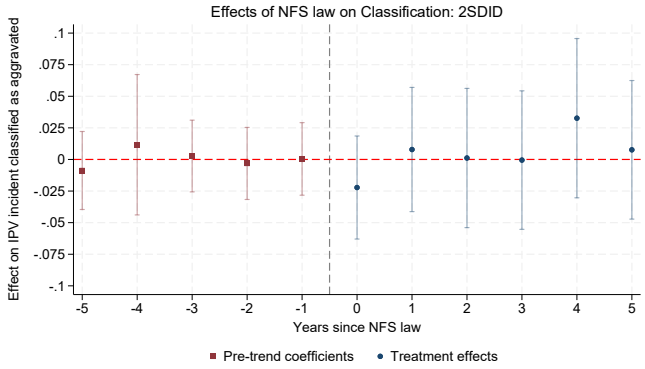
Notes: Regressions are weighted by the relevant cohort-age population in 2000. Standard errors clustered at the state level are shown in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Figure A15: 2SDID Event-study for Reported IPV Aggravated-Assault Classification among 50–70

(a) Classification ratio, Female-victim 50–70



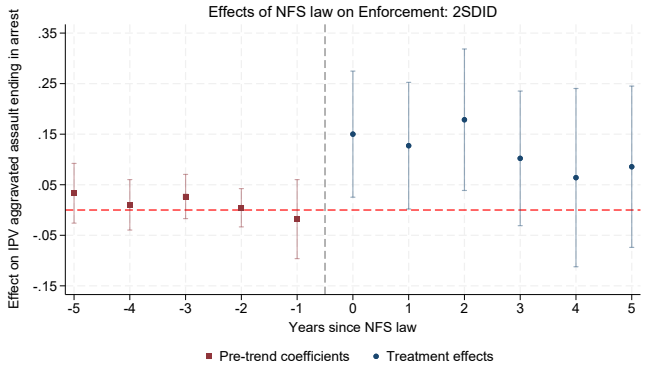
(b) Classification ratio, Male-victim 50–70



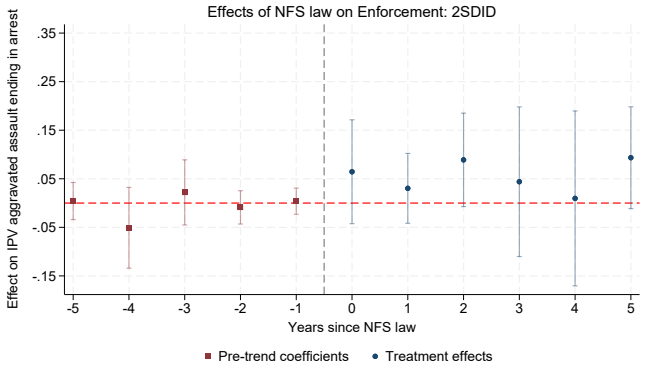
Notes: Estimates are from a 2SDID event-study specification implemented using the `did2s` Stata package (Butts, 2021). The first stage estimates state and year fixed effects using untreated and not-yet-treated observations ($D_{s,t} = 0$); the second stage regresses residualized outcomes on event-time indicators. Unlike standard TWFE–OLS event studies, no reference event-time period is omitted. See Section 4.5 for interpretation. Vertical bars denote 95% confidence intervals based on state-clustered standard errors.

Figure A16: 2SDID Event-study for Arrests among Reported IPV Aggravated Assaults among 50–70

(a) Enforcement ratio, Female-victim 50–70



(b) Enforcement ratio, Male-victim 50–70



Notes: Estimates are from a 2SDID event-study specification implemented using the `did2s` Stata package (Butts, 2021). The first stage estimates state and year fixed effects using untreated and not-yet-treated observations ($D_{s,t} = 0$); the second stage regresses residualized outcomes on event-time indicators. Unlike standard TWFE–OLS event studies, no reference event-time period is omitted. See Section 4.5 for interpretation. Vertical bars denote 95% confidence intervals based on state-clustered standard errors.