



Revisiting the effect of warrantless domestic violence arrest laws on intimate partner homicides☆

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ABSTRACT

Warrantless domestic violence arrest laws allow officers to make arrests of alleged offenders of domestic violence without warrants given probable cause. Existing literature classifies these laws into three groups based on the degree of arrest authority given to officers: *discretionary*, *preferred*, and *mandatory*. Using our updating of each type of warrantless domestic violence arrest law, we examine the causal effect of these laws on intimate partner homicides using differences-in-differences. In contrast to [Iyengar \(2009\)](#), we find no evidence that *mandatory* arrest laws, which remove officer discretion by making arrest a required action, increased intimate partner homicides. Instead, we find some evidence that *discretionary* arrest statutes, which allowed officer discretion to make arrests, decreased current and former spousal homicides.

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1. Introduction

More than in 5 women and almost 1 in 7 men in the United States have been a victim of severe physical violence perpetrated by intimate partners at some point in their life ([Black et al., 2011](#)). Despite its prevalence and importance, intimate partner violence had long been considered as a personal matter rather than a criminal offense. A change in the criminal justice system's stance emerged following the battered women's movement in the 1970s that brought violence within the home to the public arena. Further, a series of events in the 1980s, including a randomized study on the effectiveness of arrest on domestic violence, as well as a liability lawsuit against police inaction towards an intimate partner violence case, led to a drastic change in the justice system's response to domestic violence — from inaction and passivity to proactive intervention.

We consider three types of similar laws governing police enforcement of domestic violence statutes which we refer to as discretionary, preferred and mandatory arrest laws. These three laws are similar in

that they allow warrantless arrests of the alleged offenders of domestic violence conditional on probable cause of domestic violence. The laws differ in that some make arrest up to the officer's discretion (discretionary) while others “nudge” police officers towards making arrests (preferred and mandatory). Preferred arrest laws are merely suggestive, whereas mandatory arrest laws require a police officer to arrest a violent perpetrator even over the victim's protests so long as there is probable cause to believe that violence has occurred. For convenience, we group all of these laws together as “warrantless domestic violence arrest laws”. The unifying theme across all three is that each law changes the incentives an officer faces towards arrest but via separate mechanisms.

While the laws symbolically demonstrate the justice system's intention to devote resources to reducing domestic violence, their effectiveness at accomplishing this goal remains controversial. Opponents of mandatory arrest laws point to the possibility of unintended consequences of the laws, such as backlash against victims, an increase in dual arrests regardless of the main offender, and a decrease in reporting by victims for fear of arrest of their significant others ([Zelcer, 2014](#)).

Nearly all prior empirical work regarding these types of statutes has focused on the mandatory arrest laws, some of which have been based on randomized studies. The effect of mandatory arrest on domestic violence is inconclusive. Early studies found a deterrence effect of arrest ([Sherman and Berk, 1984](#)), while others found a backfire effect among certain sub-groups of population ([Berk et al., 1992](#); [Pate and Hamilton, 1992](#); [Sherman et al., 1992a](#)). A critical shortcoming of the

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earlier randomized studies is that arrest was assigned as a *probabilistic* event, whereas arrest under contemporary mandatory arrest laws is a *certain* event (Iyengar, 2009). Iyengar (2009) improved over earlier research designs by exploiting the geographic staggered roll out of actual mandatory arrest laws. Using variations in the timing of law passage across 50 states and the District of Columbia, she found evidence that mandatory arrest laws *increased* intimate partner homicides, suggesting a startling and unintended consequence in the real world of these policy environments.

There is, however, no consensus on the way these laws are classified (Zeoli et al., 2011a). While most researchers base their classification on the same rule, which is the degree of officer discretion in arrests, there are major discrepancies in law classifications among researchers. To take just one example, Iyengar (2009), classifies 14 states and D.C. as mandatory arrest law states, whereas Hirschel (2008), a legal scholar, finds 22 states and D.C. as mandatory arrest law states. Further, there are also significant differences in the years of law passages in the previous literature (Zeoli et al., 2011b).

The importance of careful legal coding as well as correct years of passage in policy analyses has been highlighted in other studies. Myers (2017), for example, points out significant discrepancies in the legal coding of contraceptive pill policy in the existing literature, and shows that the estimated policy effects are sensitive to differential legal coding. Zeoli et al. (2011b), like us, note the heterogeneity in legal classifications and in the years of law passages of warrantless arrest laws, and find that variations in legal classifications and law timing have resulted in differing effects of the laws.¹ In this paper, we revisit the effect of warrantless domestic violence arrest laws on intimate partner homicides. Given the importance of mandatory arrest as the justice system's primary response to domestic violence in many states and the controversy over its effectiveness, careful robustness checks of Iyengar's original study are warranted. This is made even more pressing given subsequent disagreement among researchers in classifications as well as the timing of the laws of domestic violence arrest laws.

We updated classification and effective dates of adoption using the Hirschel's (2008) taxonomy of warrantless arrest laws (preferred, discretionary and mandatory). Based on our thorough archival research, we find a full list of years of law passages covering each coded intimate partner relationship in warrantless domestic violence statutes. Our approach prevents potential misleading inferences of the law effects due to the changes in the definition of intimate partners over time.

We find that there is no evidence that mandatory arrest laws increase intimate partner homicides. But we do find new evidence that *discretionary* arrest laws reduce intimate partner homicides among current and former spouses. These effects represent large reductions relative to the prevalence of these homicides in our sample. Using our preferred specification, discretionary arrest laws reduce current spouse homicides by 0.1553 per 100,000 and former spouse homicides by 0.0321 per 100,000 and are significant at the 10 and 5% levels respectively. A 0.1553 decrease current spouse homicides is equivalent to a 43% decrease with an upper bound of 94% decrease and a lower bound of 7.4% increase. A 0.0321 decrease in former spouse homicides is equivalent to a 107% decrease with an upper bound of 12% decrease and a lower bound of 201% decrease. The results are robust to the inclusion of law-specific pre-treatment leads and group-specific trends, but become very small in magnitude and statistically insignificant once when we include state-specific trends. We investigate the issue of whether trends appear related to treatment adoption using event study analysis, and find that the negative effect is not preceded by differential trends. By all appearances, the adoption of the law is followed

by level shift downward in current spousal homicide rates which persists until the end of the panel. Like mandatory arrest, we find no evidence that preferred arrest caused intimate partner homicides to change though. We conclude that study of these laws should be revisited to better understand whether they can disrupt domestic violence before it reaches the level of homicide.

Given a noticeable difference in the average homicide rates between Iyengar's study and ours, we suspected that this null result of mandatory arrest laws is not merely driven by different law classifications. For a closer examination, we attempted to replicate her initial results but were unsuccessful. Radha Iyengar generously shared her data sets and coding, despite the many years and many computers since the article's original publication. With the sharing of her data and code, we were able to discover that the original counter-intuitive results were most likely the byproduct of a syntax error introduced during the data merging process. This seemingly benign error had inadvertently restructured the population variable used to construct the homicide rate in such a way that was correlated with the passage of mandatory arrest laws, purely by coincidence. We believe that this error was solely responsible for the positive correlation between mandatory arrest and increased intimate partner homicides. Once the syntax error is addressed, and the data is correctly constructed, we do not find evidence that mandatory arrest laws affected intimate partner homicides. The effect is sometimes positive, sometimes negative, but always insignificant and small in relative magnitude.

The paper proceeds as follows. Section 2 discusses background of the legal interventions in intimate partner violence and related literature. Section 3 discusses legal coding used in this study. Section 4 discusses our data and identification strategy. Section 5 presents our main results including robustness checks. Section 6 concludes.

2. Background

Existing economic studies on intimate partner violence are mostly based on bargaining theories. Bargaining theories predict a violence-curbing effect of favorable economic conditions for women through the enhanced bargaining power within the home (Aizer, 2011; Farmer and Tiefenthaler, 1997; Tauchen et al., 1991). An alternative explanation is based on extraction theories. These theories predict a higher rate of violence against women who possess more economic resources if men use violence as an instrument of extraction (Bloch and Rao, 2002; Bobonis et al., 2013). Most of the existing economic studies on intimate partner violence primarily focus on how women's economic status determines their violence experience.

There are only a handful of economic studies that investigate the effects of direct legal interventions in intimate partner violence. Aizer and Dal Bo (2009), for example, find no-drop policies² in domestic violence prosecution lead to a decrease in male homicides by female intimate partners, suggesting that women who have inconsistent preferences on violent relationships take advantage of no-drop policies as a less extreme commitment device than a murder.³ Miller and Segal (2019) study the impact that increased hiring of female officers had on violence against women and find that hiring additional female officers increased reporting of violence against women and decreased intimate partner violence.

Iyengar (2009) finds evidence that mandatory arrest laws increased intimate partner homicides. She interpreted her results as evidence that in domestic violence cases, mandatory arrest discouraged the reporting

¹ Unlike Iyengar (2009), they find either negative or no effects of mandatory arrest laws on intimate partner homicides depending on which classification schemes or years of law passages they use. However, given that they used a panel of 46 cities from 25 states only, it is unclear whether the discrepancies are caused by sample selection or different classifications.

² Once an alleged abuser is charged with domestic violence, a prosecutor is not allowed to drop the charge even when the victim wishes to drop it.

³ Women have a tendency to greatly devalue the relationships with abusive partners shortly after the incidence of violence and want to leave the relationships. After a while, however, they often change their minds and decide to stay with their partners due to the emotional attachment to them. For a woman with such inconsistent preferences, a murder of an abusive partner can serve as a commitment device of her initial determination.

of violence and/or increase retaliation by arrested abusers, leading to an increase in the number of intimate partner homicides. Given strong and often-irreversible impacts of direct legal interventions in intimate partner violence, a careful investigation of the effectiveness of such policies is warranted. In this paper, we revisit mandatory arrest laws, one of the most controversial policies in domestic violence interventions.⁴

Domestic violence arrest laws shape the incentives of officers by encouraging them to arrest violent perpetrators of domestic violence, but accomplish this through subtly different mechanisms. In the extreme case of mandatory arrest itself, the police officer is obligated to make the arrest regardless of the victim's preference so long as there is probable cause to believe that violence has occurred. Not even protests by the victim can stop an arrest in a state with a mandatory arrest statute because the statute removes officer discretion entirely – he must arrest the violent offender if there is probable cause. Many states have adopted mandatory arrest laws as a primary law enforcement response to domestic violence.

The drastic change in the police stance towards domestic violence cases from minimal intervention to proactive deterrence are majorly driven by 1) a law suit in Connecticut in 1984, *Thurman v. City of Torrington*, in which police officers and the city of Harrington were held liable for their inaction in a domestic violence case that eventually resulted in permanent paralysis of the victim (Lyengar, 2009); 2) the publication of the Minneapolis Domestic Violence Experiment in 1984, a randomized experiment in criminology, which finds arrest the most effective way of deterring recidivism of battery (Sherman and Berk, 1984).

An array of replication studies followed the Minneapolis experiment to verify a deterrence effect of mandatory arrest laws, but the results were mixed. Some replication studies find evidence that arrest decreases future violence among the employed but increases subsequent violence among the unemployed, suggesting that an arrest may not always deter recidivism and can even have a harmful effect for certain groups (Berk et al., 1992; Pate and Hamilton, 1992; Sherman et al., 1992a). Sherman et al. (1992b) find a deterrence effect in the short run but a backfire effect in the long run in certain measures of outcome in some replication studies, while other studies report no deterrence effects. In some other replications that report a deterrence effect, he questions the credibility of the results due to the low response rate of the victim follow-up interviews.⁵ In a follow-up study of the Milwaukee domestic violence experiment that interviewed the same sample after 23 years, Sherman and Harris (2015) find that victims whose partners were arrested are more likely to have premature deaths than those victims whose partners were allowed to stay, suggesting unintended long-term consequences of mandatory arrest laws.

Although randomized experiments are generally a powerful tool for identification of a policy effect, they have serious limitations in evaluating the effectiveness of domestic violence arrests. The experiment captures how the abusers and victims behave when the arrest is probabilistic. But they might act differently if an arrest surely ensues from a report of violence (Lyengar, 2009). In order to identify the effect of an arrest with certainty, Lyengar (2009) evaluates the effects of actual passage of mandatory arrest laws on intimate partner homicides by exploiting variations in the timing of law passages across 50 states and the District of Columbia. She classifies arrest laws as mandatory or

recommended, depending on whether a law directs that a police officer “shall” or “may” arrest the perpetrator.

There is, however, no consensus on the way laws are classified among researchers. We also find heterogeneity in the years of law passages in the previous literature. Therefore, we depend primarily on our own legal archival research for effective dates of adoption by warrantless law type and the statute's reference to the victim's relationship to the offender.

3. Statutory classification and effective dates

3.1. Statutory classification

All 50 states and the District of Columbia have arrest laws around the incidence of domestic violence (Zeoli et al., 2011a).⁶ Hirschel (2008) decomposes warrantless into three possible types which differ depending on the degree to which an officer has discretion in making the arrest. The full taxonomy of these laws are mandatory (must arrest), discretionary (may arrest), or preferred (arrest as a preferred action). While contemporary legal scholars generally agree on the taxonomy itself, there exists disagreement among researchers in the classification of the individual laws (Zeoli et al., 2011a).⁷ After a careful review of these statutes, we established a comprehensive and most up-to-date classification of these laws of our own (Table 1). Our classification largely corresponds with that of Hirschel's except for 3 states that switched classifications.⁸ We document these states in Table 1 in italics. Overall, we define 24 states as discretionary, 23 states as mandatory and 4 states as preferred, with 3 of them switching between classifications.

3.2. Effective dates

Given significant differences in the years of law passages in the previous literature (Zeoli et al., 2011b), we found the effective dates for all 50 states and D.C. through our own archival research on each state's session law.⁹ We compare our findings with the effective dates reported in the existing literature and where we disagree, we provide our reasoning. Details are in Appendix I.3.

Coded relationships in domestic violence arrest laws have expanded over time. In many states, for example, dating relationship was added as an intimate partner relationship in later years. But the timing of these amendments has not been documented in the existing literature. We augment this shortcoming by reporting the years when each intimate partner relationship (current spouse, former spouse, common-law spouse, dating partner) began to be covered by the arrest laws along with the relevant statutes in Table 2. We use these years as effective dates of the arrest laws for each relationship.

4. Econometric model and data

Our main empirical analysis uses a regression difference-in-difference model following Lyengar (2009) with an additional arrest law defined by:

$$y_{st} = \gamma_d D_{st} + \gamma_p P_{st} + \gamma_m M_{st} + \delta X_{st} + \sigma_s + \tau_t + \beta^g t + \epsilon_{st} \quad (1)$$

where y_{st} is the number of intimate partner homicides per 100,000 in state s in year t , D_{st} is a dummy variable indicating whether state s had a discretionary arrest law in year t , P_{st} is a dummy variable indicating whether state s had a preferred arrest law in year t , M_{st} is a dummy

⁴ Claims that mandatory arrest laws need to be repealed are based on the following reasons: 1) mandatory arrest laws do not work consistently and depend on the background of the people involved; 2) they deprive women of autonomy in decision-making and disregard their preferences; 3) they increase the number of dual arrests regardless of the main offender; 4) they are particularly harmful for women with children, who tend to underreport in fear of losing custody; 5) they increase the cost of the criminal justice system (Zelcer, 2014).

⁵ Confronted with the mixed results of these replication studies, Sherman et al. (1992), who first designed and undertook the Minneapolis experiment, dramatically changed their stance from pro-mandatory arrest to anti-mandatory arrest.

⁶ Delaware is different in that Del. Code Ann. Tit.11 §1904(a)(4) allows a warrantless arrest for misdemeanors at officer's discretion (may arrest), but the statute is not specifically for domestic violence cases.

⁷ A comparison of the existing classifications can be found in Appendix I.1.

⁸ Further details of our classification can be found in Appendix I.2.

⁹ We found the session laws in U.S. State Session Laws Library in HeinOnline. Also, we received excellent legal assistance from Chris Goleczka.

Table 1
Arrest laws by states.

States	Statutes	Classification
Alabama	Ala. Code § 15-10-3 (a)(8)	Discretionary
Alaska	Alaska Stat. §18.65.530(a)	Mandatory
Arizona	Ariz. Rev. Stat. Ann. §13-3601(B)	Mandatory
Arkansas	Ark.Code Ann.§ 16-81-113	Preferred
California	Cal. Penal Code § 13701(b)	Preferred
Colorado	Colo. Rev. Stat. §18-6-803.6(1)	Mandatory
Connecticut	Conn. Gen. Stat. §46b- 38b(a)	Mandatory
DC	D.C. Code Ann. §16-1031	Mandatory
Delaware	Del. Code Ann. tit.11 §1904(a)(4)	Discretionary
Florida	Fla. Stat. ch. 741.29(3)	Discretionary
Georgia	Ga.Code Ann. § 17-4-20(a)	Discretionary
Hawaii	Haw. Rev. Stat. §709-906(2)	Discretionary
Idaho	Idaho Code § 19-603(6)	Discretionary
Illinois	725 Ill. Comp. Stat. 5/112A-30	Discretionary
Indiana	Ind. Code Ann. §35-33-1-1(a)(5)(B)	Discretionary
Iowa	Iowa Code §236.12(2)	Mandatory
Kansas	Kan.Stat.Ann. § 22-2307(b)(1)	Mandatory
Kentucky	Ky. Rev. Stat. Ann. §431.005(2)(a)	Discretionary
Louisiana	La. Rev.Stat.Ann § 46:2140;Ch.C.Art.1573(1)	Mandatory
Maine	Me. Rev. Stat. Ann. tit.19-A, §4012(5)	Mandatory
Maryland	Md.Code Ann. §2-204	Discretionary
Massachusetts	Mass.Gen.Laws Ann. ch.209A § 6(7)	Mandatory;
Michigan	Mich.Comp.Laws §764.15a	preferred
Minnesota	Minn. Stat. § 629.341(1)	Discretionary
Mississippi	Miss. Code Ann. §99-3-7(3)	Discretionary
Missouri	Mo. Rev. Stat. §455.085	Mandatory
Montana	Mont. Code Ann. § 46-6-311(2)(a)	Mandatory
Nebraska	Neb. Rev. Stat. § 29-404.02(3)	Preferred
Nevada	Nev. Rev. Stat. Ann. §171.137(1)	Discretionary
New Hampshire	N.H. Rev. Stat. Ann. §594:10(1)(b)	Mandatory
New Jersey	N.J. Stat. Ann. §2C:25-21(a)	Discretionary
New Mexico	N.M. Stat. Ann §40-13-7(B)(5)	Discretionary
New York	N.Y. Crim. Proc. Law §140.10(4)(a)	Mandatory
North Carolina	NC Gen. Stat. §15A-401(b)(2)	Discretionary
North Dakota ^a	N.D. Cent. Code § 14-07.1-11; N.D. Cent. Code § 14-07.1-10(1)	Discretionary;
Ohio	Ohio Rev. Code Ann. §2935.032(A)(1)(a)(i)	preferred
Oklahoma ^b	Okla. Stat. tit. 22, § 40.3 (B); 22 Okl.st. § 196 (6)	Mandatory
Oregon	Or. Rev. Stat. §133.055(2)(a)	Discretionary;
Pennsylvania	18 Pa. Cons. Stat. § 2711(A)	mandatory
Rhode Island	R.I. Gen. Laws §12-29-3(c)(1)	Discretionary
South Carolina	S.C. Code Ann. §16-25-70(B)	Mandatory
South Dakota	S.D. Codified Laws §23A-3-2.1	Mandatory
Tennessee	Tenn.Code Ann.§ 36-3-619	Preferred
Texas	Tex. Code Crim. P. Ann. Art. 14.03(a)(4)	Discretionary
Utah	Utah Code Ann. §77-36-2.2(2)(a)	Mandatory
Vermont	Vt. R. Cr. P. 3(C)	Discretionary
Virginia	Va. Code Ann. §19.2-81.3(B)	Mandatory
Washington	Wash. Rev. Code Ann. §10.31.100(2)(c)	Mandatory
West Virginia	W.Va.Code § 48-27-1002(a)	Discretionary
Wisconsin	Wis. Stat. § 968.075(2)(a)	Mandatory
Wyoming	Wyo. Stat. Ann. § 7-20-102(a); §35-21-107	Discretionary

^a N.D. Cent. Code § 14-07.1-11 allowed discretionary arrest (may arrest) in case of probable cause for assault of family members within four hours of the ascertainment of the probable cause. Later, N.D. Cent. Code § 14-07.1-10(1) prescribed arrest as a preferred response. We classify North Dakota first as a discretionary arrest law state based on N.D. Cent. Code § 14-07.1-11, and later as a preferred arrest state once N.D. Cent. Code § 14-07.1-10(1) took effect.
^b 22 Okl.st. § 40.3 is not relevant to domestic violence any more. As of 2017, domestic violence arrest law is found in 22 Okl.st. § 196 (6).

variable indicating whether state *s* had a mandatory arrest law in year *t*.¹⁰ Our trend variable *t* in some specifications includes group-specific linear trends (Goodman-Bacon, 2018) and in other specifications state-specific trends which we indicate with the *g* superscript. We

¹⁰ We add a separate indicator for Delaware because it is the only state that does not have a warrantless arrest law specifically for domestic violence. As explained earlier, its warrantless arrest statute is for misdemeanor crimes involving physical injury, which may or may not be the result of domestic violence.

define a “group” as any set of states which received that particular law in the same year. This is important to explore as when there are multiple units that receive the treatment at the same time, then it is variation at the group level that technically is used for identification (Goodman-Bacon, 2018). The matrix X_{st} are state-year covariates covering other violent crime rates and unemployment rates, σ_s is a vector of state fixed effects, τ_t is a vector of year fixed effects, and ε_{st} is a disturbance term. The outcome variable y_{st} is disaggregated into four categories: current spouse homicides, former spouse homicides, common-law spouse homicides, and dating partner homicides. The definition of intimate partners has changed over time gradually covering a broader relationship. We use disaggregated outcome variables to eliminate the possibility that the intimate partner homicide rates change simply because the definition has changed. We cluster our standard errors by state (Bertrand et al., 2004).

Our homicide data come from the Uniform Crime Reports Supplementary Homicide Reports with Multiple Cumulative Files 1976–2014 (Fox and Swatt, 2014).¹¹ Population data come from Survey of Epidemiology and End Results (SEER) U.S. State Population Data from 1969 to 2015. Other violent crime data come from the Uniform Crime Reporting (UCR) Program using UCR data tool. Unemployment data come from March CPS in IPUMS CPS. Due to missing states in the 1976 CPS, we restrict some of our analysis to the 1977–2014 panel period.

5. Results

5.1. Main results

Table 3 reports the estimation results of Eq. (1) for current (Panel A) and former (Panel B) spouse homicide rates. Column 1 controls state and year fixed effects only, column 2 controls additional covariates (other violent crime rates and unemployment rates), column 3 controls for pre-treatment lead dummies, column 4 controls for group-specific linear trends, and column 5 controls state-specific linear trends. Mandatory arrest laws have negative but insignificant effects on current/former spouse homicide rates in all the specifications. Across all the specifications in Panel A of Table 3, the upper bound of the 95% confidence intervals on mandatory arrest is approximately 0.04, which is about an 11% (=0.04/0.36) increase in homicide rates, while the lower bound ranges from 49% to 17% decrease. The result, therefore, rules out that mandatory arrest laws increased the spousal homicide rates by >11%. This stands in stark contrast with the magnitude of the effect in Table 3 of Iyengar's, which reported a positive effect of mandatory arrest laws on intimate partner homicides that ranges from a low of 51% (column 3) to a high of 77% (column 4 and 5). Preferred arrest laws negative and insignificant effects in all specifications. As a percentage, though, our estimate in column 4 represents a 27% decline with the 95% confidence interval of +26% to −79%.

Discretionary arrest laws, on the other hand, reveal a different pattern. In our initial specifications, the passage of discretionary arrest laws is associated with a 0.125 reduction, or a 35% decrease, in the intimate partner homicide rate involving current spouses. Controlling for unemployment rates and violent crime levels does not affect the result (column 2). It is possible, though, that the adoption of laws themselves could be tied to changing homicide rates, so we investigate this possibility by including a vector of pre-treatment leads. This reduces the precision of our estimates, but the results remain fairly similar. Next, due to the fact that groups of states adopt the law at the same time, we control for linear group trends following suggestions by Goodman-Bacon (2018). The standard errors increase but the effect remains significant at the 10% level. Last, we check whether the result is robust to the inclusion of state-specific linear trends, and it is not. Controlling for state-

¹¹ See <http://www.icpsr.umich.edu/icpsrweb/NAJD/studies/24801> for the 2007 version. Fox generously sent us directly the 2014 version. While the data report multiple imputed data, we use only the raw homicide data in the file.

Table 2

Effective years for coded relationships.

States	Statute	Current spouse	Former spouse	Common-law spouse	Dating partners
Alabama	Ala. Code § 15-10-3 (a)(8); Code of Ala. § 13A-6-139.1	1989	1989	1989	2000
Alaska	Alaska Stat. § 18.66.990(5)	1996	1996	1996	1996
Arizona	Ariz. Rev. Stat. Ann. §13-3601(A)	1991	1991	1991	2009
Arkansas	Ark.Code Ann.§ 16-81-113	1991	1991	1991	2005
California	Cal Fam Code § 6211	1996	1996	1996	1996
Colorado	Colo. Rev. Stat. §18-6-800.3	1994	1994	1994	NA
Connecticut	Conn. Gen. Stat. §46b- 38a	1986	1986	1986	1999
DC	D.C. Code Ann. §16-1001	1991	1991	1991	1991
Delaware ^a	NA	NA	NA	NA	NA
Florida	Fla. Stat. § 943.171(2)(b); Fla. Stat. § 741.28(3)	1992	1992	1992	NA
Georgia	O.C.G.A. § 19-13-1	1981	1988	1988	NA
Hawaii	HRS § 709-906(1)	1973	1985	1985	2013
Idaho	Idaho Code § 39-5202; Idaho Code § 18-918(1)(a)	1979	1982	1982	NA
Illinois	725 Ill. Comp. Stat. 5/112A-3(3)	1993	1993	1993	1993
Indiana	Ind. Code Ann. § 34-6-2-44.8	2000	2002	2002	2002
Iowa	Iowa Code § 236.2	1986	1986	1986	2002
Kansas	K.S.A. 60-3102; K.S.A. 21-3110 (7) (A) & (B); K.S.A. § 21-5111	1991	1991	1991	2010
Kentucky	Ky. Rev. Stat. Ann. §431.005	1980	1988	1988	2017
Louisiana	La. R.S. § 46:2132	1985	1985	1986	2015
Maine	19-M.R.S.A. § 762	1980	1980	1980	2007
Maryland	An. Code 1957, art. 27, § 594B(d); Md.Code Ann. §2-204	1986	NA	2001	NA
Massachusetts	ALM GL ch. 209A, § 1	1978; 1991	1978; 1991	1991	1991
Michigan	Mich.Comp.Laws §764.15a	1978	1978	1978	2002
Minnesota	Minn. Stat. § 629.341; Minn. Stat. § 518B.01	1978	1983	1978	2001
Mississippi	Miss. Code Ann. § 93-21-3; Miss. Code Ann.§ 99-47-1(1)(c)	1995	1995	1995	2001
Missouri	Mo. Rev. Stat. §455.010	1989	1989	1989	2000
Montana	Mont. Code Ann. § 45-5-206(2)	1985	1985	1985	1993
Nebraska	Neb. Rev. Stat. § 29-404.02(3)	1989	1989	1989	2004
Nevada	Nev. Rev. Stat. Ann. §171.137(1)	1985	1985	1985	1999
New Hampshire	RSA 173-B:1	1979	1979	1979	NA
New Jersey	N.J. Stat. § 2C:25-19	1991	1991	1991	1994
New Mexico	N.M. Stat. Ann. § 40-13-2	1987	1987	1987	1993
New York	NY CLS CPL § 530.11(1)	1996	1996	1996	2008
North Carolina	NC Gen.Stat. § 15A-401(b)(2); N.C. Gen. Stat. § 50B-1	1991	1991	1991	1997
North Dakota	N.D. Cent. Code, § 14-07.1-01(4)	1989; 1995	1989; 1995	1989; 1995	1989; 1995
Ohio	ORC Ann. 2919.25	1994	1994	1994	NA
Oklahoma	22 Okl. St. § 60.1	1986	1986	1986	1995
Oregon	ORS § 107.705 (4)	1977; 1981	1977; 1981	1977; 1981	1993
Pennsylvania	18 Pa. Cons. Stat. § 2711(A); 23 Pa.C.S. § 6102	1986	1986	1986	2001
Rhode Island	R.I. Gen. Laws § 12-29-2	1988	1988	1988	1994
South Carolina	S.C. Code Ann. § 16-25-10	1995	1995	1995	NA
South Dakota	S.D. Codified Laws §23A-3-2.1; S.D. Codified Laws § 25-10-3.1	1989	1989	1989	2015
Tennessee	Tenn. Code Ann. § 36-3-601	1995	1997	1997	1997
Texas	Tex. Fam. Code § 71.01; Tex. Fam. Code § 71.003	1989	1989	1989	2001
Utah	Utah Code Ann. §30-6-1(2); Utah Code Ann. § 78B-7-102	1991	1991	1991	NA
Vermont	15 V.S.A. § 1101(2)	1985	1985	1985	2000
Virginia	Va. Code Ann. § 18.2-57.2; Va. Code Ann. §16.1-228	1997	1997	1997	NA
Washington	Wash. Rev. Code Ann. §10.31.100(2)(b); Rev. Code Wash.§ 10.99.020(3)	1984	1984	1984	1996
West Virginia	W.Va.Code § 48-2A-2; W. Va. Code § 48-27-204	1993	1993	1993	1998
Wisconsin	Wis. Stat. §968.075(1)(a)	1989	1989	1989	NA
Wyoming	Wyo.Stat.Ann. § 35-21-102	1987; 1982	1987;1982	1987;1982	2000

^a Since Del. Code Ann. Tit.11 §1904(a)(4) is not specifically about domestic violence, relationships covered by the statute were not found. In the main analysis, we controlled Delaware indicator using 1984 as the effective date for all four relationships.

specific trends eliminates the correlation we were finding suggesting one of two possible explanations. First, insofar as we believe that the state-specific trend is adequately capturing a confounder, then we are unable to reject the null that there's no effect of discretionary laws on current spousal homicides. Or second, it may be that the inclusion of state-specific trends has "over controls" and remove the needed variation in the data for identification in the first place. Later we discuss whether there is evidence for differential trends by treatment status; we cannot reject the null of differential trends. Thus, we suspect that the inclusion of unit-specific trends is too stringent a specification.

The identifying assumption in any differences-in-differences design is the parallel trends assumption. Put simply, absent the treatment, the evolution of the outcome would've followed the same course as variance weighted control units (Goodman-Bacon, 2018), as it is the changes in controls that are being explicitly used for mapping out the counterfactual comparisons. This is necessarily an untestable assumption as there does not ever exist data on counterfactual trends.

But, while we cannot directly test this assumption, we can investigate the earlier periods with an event study to see whether prior to treatment there is evidence for declining intimate partner homicide rates. This, we argue, can at least give us some sense as to whether legislation was correlated with changing homicide rates in the treatment states relative to control states. Fig. 1 produces coefficients on discretionary arrest leads and lags in a regression that additionally controlled for state and year fixed effects, state unemployment and violent crime index. Standard errors were clustered at the state level. The points above a given year are the point estimate of that particular lead or lag, and the dashed lines are the upper and lower bound magnitudes from a 95% confidence interval. For leads 1 through 5+, the relationship between discretionary laws and current spousal homicide is not statistically significant and confidence intervals encompass positive and negative effects. But, one year after the passage of the law, there is a level shift in current spouse homicide rates which are marginally significant. An F test on the joint significance that these coefficients sum to

Table 3
Effects on current/former spouse homicides with new classification.

	(1)	(2)	(3)	(4)	(5)
Panel A: effects on current spouse homicides					
Dependent variable	Current spouse homicides per 100,000 inhabitants (mean: 0.36)				
Discretionary arrest	−0.1247 [*] (0.0578)	−0.1235 [*] (0.0572)	−0.1378 ⁺ (0.0778)	−0.1553 ⁺ (0.0928)	−0.0407 (0.0496)
Mandatory arrest	−0.0455 (0.0417)	−0.0483 (0.0416)	−0.0664 (0.0559)	−0.0649 (0.0525)	−0.0118 (0.0263)
Preferred arrest	−0.0139 (0.0601)	−0.0105 (0.0620)	−0.1015 (0.0904)	−0.0959 (0.0973)	−0.0097 (0.0448)
Observations	1889	1889	1889	1889	1889
R-squared	0.6328	0.6342	0.6385	0.6594	0.7487
Panel B: effects on former spouse homicides					
Dependent variable	Former spouse homicides per 100,000 inhabitants (mean: 0.03)				
Discretionary arrest	−0.0170 [*] (0.0083)	−0.0166 [*] (0.0083)	−0.0316 [*] (0.0122)	−0.0321 [*] (0.0145)	−0.0063 (0.0074)
Mandatory arrest	0.0030 (0.0052)	0.0025 (0.0050)	−0.0085 (0.0104)	−0.0063 (0.0106)	0.0046 (0.0060)
Preferred arrest	−0.0046 (0.0113)	−0.0039 (0.0111)	−0.0270 (0.0291)	−0.0231 (0.0299)	−0.0021 (0.0118)
Observations	1889	1889	1889	1889	1889
R-squared	0.2864	0.2885	0.2985	0.3097	0.3692
Estimation method	OLS	OLS	OLS	OLS	OLS
Controls for other violent crime rates	N	Y	Y	Y	Y
Controls for unemployment rate	N	Y	Y	Y	Y
Law-specific pre-trend	N	N	Y	Y	N
Group-specific trend	N	N	N	Y	N
State-specific trend controls	N	N	N	N	Y
State fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y

A law-specific pre-trend is a dummy for each law-specific pre-treatment lead. A group specific trend is a linear trend interacted with a group dummy variable, where a group indicates a set of states that received the treatment in the same year. We use data spanning 1977 to 2014. Robust standard errors, clustered by states, are in parenthesis.

^{*} $p < 0.05$.

⁺ $p < 0.1$.

zero was performed and the F test was 2.96 with a p -value of 0.09. All other leads and lags on mandatory arrest (Fig. 2) and preferential arrest (Fig. 3) showed no clear evidence of changing current spouse homicide rates.

We also performed this analysis for former spouse homicide. The estimated lag coefficients for discretionary arrest have a different pattern for former spousal homicide (Fig. 4) than we found with current spouse homicide. But, several coefficients are negative and significant, particularly in the years immediately after the law's passage. And an F test was

done testing that the sum of the lags equal zero, and we are able to reject this null at a p -value of 0.08 ($F = 3.1$). Similar to our non-findings on current spousal homicides, though, we find no evidence from our event study analysis that mandatory arrest laws (Fig. 5) or preferred arrest laws (Fig. 6) have any relationship with former spouse homicide rates.

Comparing the effect on former and current spousal homicide reveals large differences in percentage terms due to differences in base rates. If we look at only the coefficient size, then the effect of

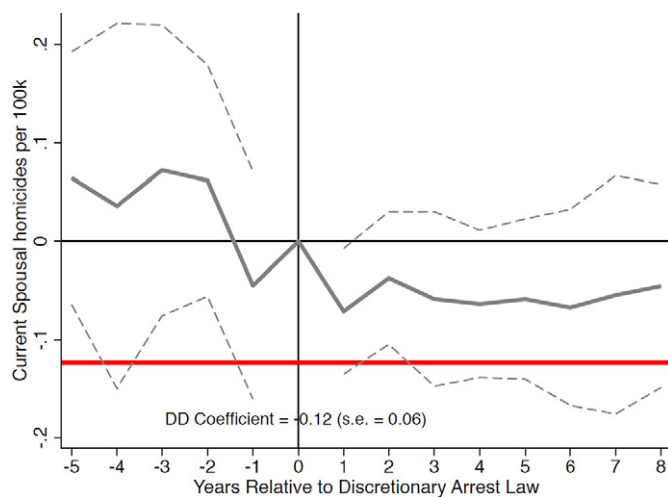


Fig. 1. Event study estimate of the effect of discretionary arrest on current spousal homicide rates. These coefficient estimates and their corresponding 95% confidence interval are from a single regression. DD coefficient for constant post treatment effects is in red line.

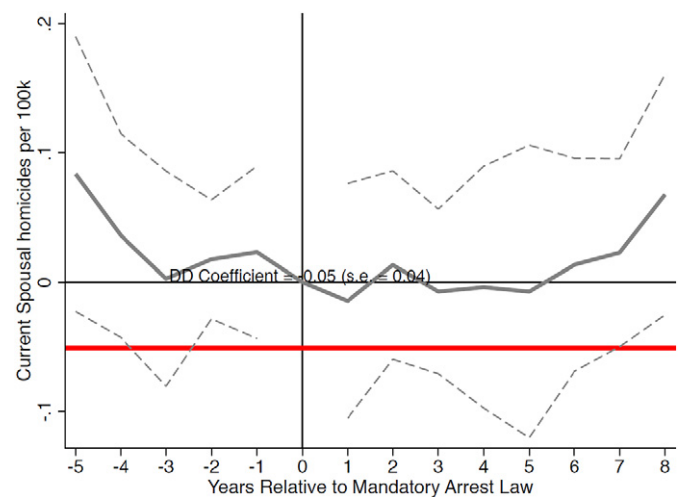


Fig. 2. Event study estimate of the effect of mandatory arrest on current spousal homicide rates. These coefficient estimates and their corresponding 95% confidence interval are from a single regression. DD coefficient for constant post treatment effects is in red line.

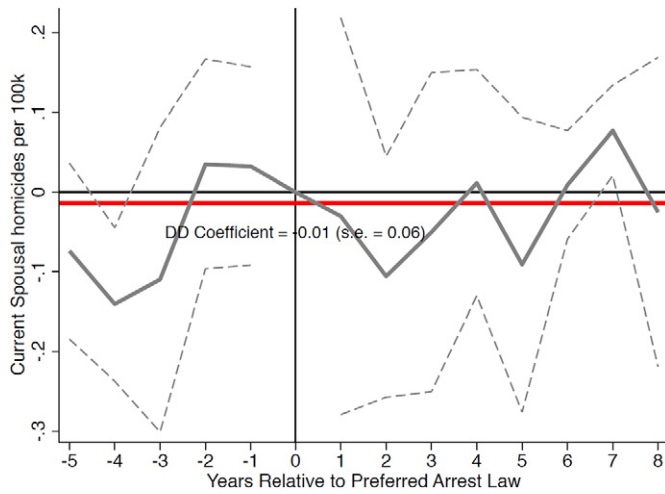


Fig. 3. Event study estimate of the effect of preferred arrest on current spousal homicide rates. These coefficient estimates and their corresponding 95% confidence interval are from a single regression. DD coefficient for constant post treatment effects is in red line.

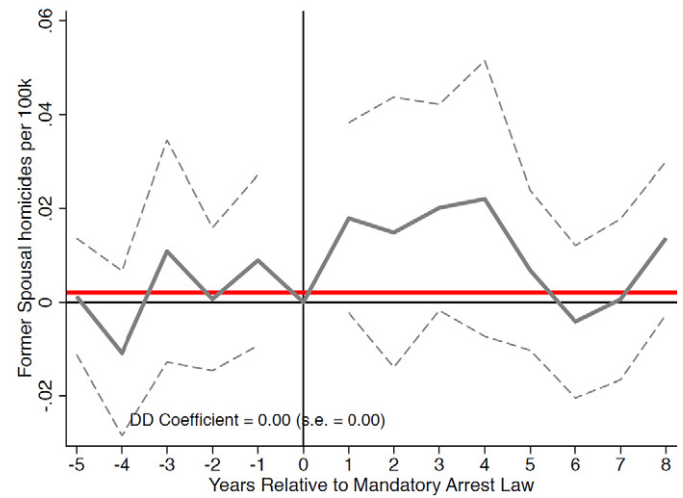


Fig. 5. Event study estimate of the effect of discretionary arrest on former spousal homicide rates. These coefficient estimates and their corresponding 95% confidence interval are from a single regression. DD coefficient for constant post treatment effects is in red line.

discretionary laws is larger for current spouses (-0.1553) than former spouses (-0.0321). But in percentage terms, the effect is much larger for former spouses (107% decrease) than current spouses (43%). These differences must be taken, though, with some caution due to the 95% confidence intervals being large.

Table 4 reports the estimation results of Eq. (1) for common-law spouse (Panel A) and dating partner (Panel B) homicide rates. Overall, we can find no evidence that mandatory arrest and preferred arrest laws affected either of the two homicide rates in any specifications. Discretionary arrest laws, on the other hand, have a significant positive effect (at the 10% level) on the common-law spouse homicide rates when state-specific time trends are controlled (column 5 Panel A). It is important to note, though, that this is the only specification where we found such a positive effect, and therefore we are skeptical that it should be considered a robust result. We also estimated the effect on dating partners, but found no evidence for any of the laws' role in changing dating partner homicides (Panel B).

5.2. Robustness

Our results are for obvious reasons not robust to any group-level change that occurred simultaneously with the passage of discretionary arrest laws. This type of omitted variable would need to be unpredicted by pre-treatment leads and group-level linear trends, as we control for both. Here we consider a variety of robustness checks to rule out some competing hypotheses. In Table 5 and Appendix III, we produce these falsification exercises.

In Table 5, we analyzed disaggregated types of homicides that in principle shouldn't be determined by the arrest laws under consideration. Those outcomes were total murders of acquaintances, other known offender, friends, neighbors, employers, employees, strangers and murders where the relationship status was missing. We use our group specific trend specification (column 4 of Table 3) because we are concerned that the inclusion of state-specific trends is too stringent

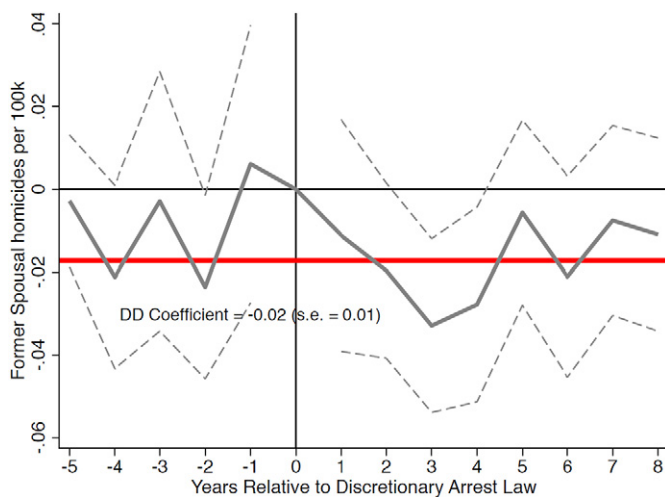


Fig. 4. Event study estimate of the effect of discretionary arrest on former spousal homicide rates. These coefficient estimates and their corresponding 95% confidence interval are from a single regression. DD coefficient for constant post treatment effects is in red line.

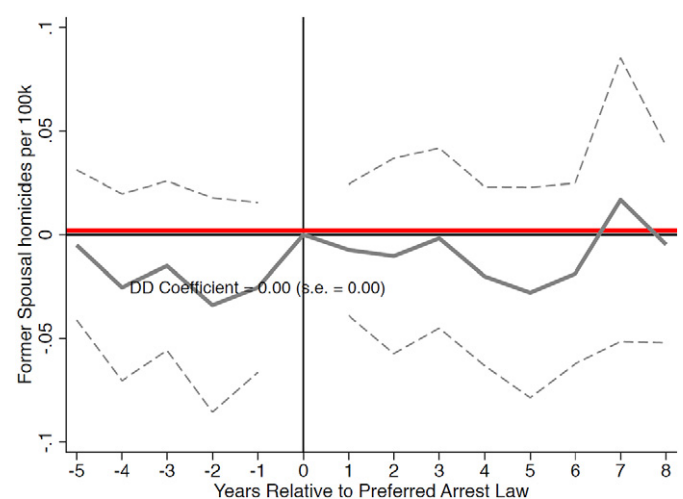


Fig. 6. Event study estimate of the effect of discretionary arrest on former spousal homicide rates. These coefficient estimates and their corresponding 95% confidence interval are from a single regression. DD coefficient for constant post treatment effects is in red line.

Table 4
Effects on common-law spouse/dating partner homicides with new classification.

	(1)	(2)	(3)	(4)	(5)
Panel A: effects on common-law spouse homicides					
Dependent variable	Common-law spouse homicides per 100,000 inhabitants (mean: 0.04)				
Discretionary arrest	−0.0023 (0.0160)	−0.0018 (0.0155)	−0.0189 (0.0133)	−0.0112 (0.0188)	0.0194* (0.0096)
Mandatory arrest	−0.0021 (0.0117)	0.0003 (0.0117)	0.0129 (0.0152)	0.0192+ (0.0109)	0.0142+ (0.0084)
Preferred arrest	0.0177 (0.0168)	0.0162 (0.0156)	0.0149 (0.0179)	0.0271 (0.0197)	0.0123 (0.0080)
Observations	1889	1889	1889	1889	1889
R-squared	0.4762	0.4827	0.4876	0.5290	0.6517
Panel B: effects on dating partner homicides					
Dependent variable	Dating partner homicides per 100,000 inhabitants (mean: 0.25)				
Discretionary arrest	−0.0173 (0.0300)	−0.0188 (0.0299)	−0.0283 (0.0403)	−0.0240 (0.0458)	0.0052 (0.0249)
Mandatory arrest	−0.0402 (0.0562)	−0.0384 (0.0600)	−0.0521 (0.0672)	−0.0548 (0.0712)	−0.0513 (0.0367)
Preferred arrest	0.0293 (0.0425)	0.0266 (0.0407)	0.0158 (0.0507)	0.0187 (0.0502)	0.0194 (0.0369)
Observations	1889	1889	1889	1889	1889
R-squared	0.4028	0.4036	0.4052	0.4331	0.5198
Estimation method	OLS	OLS	OLS	OLS	OLS
Controls for other violent crime rates	N	Y	Y	Y	Y
Controls for unemployment rate	N	Y	Y	Y	Y
Law-specific pre-trend	N	N	Y	Y	N
Group-specific trend	N	N	N	Y	N
State-specific trend controls	N	N	N	N	Y
State fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y

A law-specific pre-trend is a dummy for each law-specific pre-treatment lead. A group specific trend is a linear trend interacted with a group dummy variable, where a group indicates a set of states that received the treatment in the same year. We use data spanning 1977 to 2014. Robust standard errors, clustered by states, are in parenthesis.

* $p < 0.05$.

+ $p < 0.1$.

Table 5
Falsification tests using other homicides.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variables:	Acquaintance (mean: 1.62)	Other known (mean: 0.22)	Friend (mean: 0.19)	Neighbor (mean: 0.07)	Employer (mean: 0.005)	Employee (mean: 0.003)	Stranger (mean: 0.97)	Missing (mean: 2.21)
Discretionary arrest	−0.7527* (0.3361)	−0.2291* (0.1141)	−0.1512 (0.0922)	−0.0295 (0.0242)	−0.0018 (0.0027)	−0.0015 (0.0040)	−0.0562 (0.2025)	0.6175 (0.5512)
Mandatory arrest	−0.0915 (0.1841)	−0.0236 (0.0893)	−0.0508 (0.0867)	0.0068 (0.0153)	0.0006 (0.0031)	0.0033+ (0.0019)	0.1701 (0.1946)	0.3383 (0.7187)
Preferred arrest	0.0839 (0.3648)	−0.1661 (0.1716)	−0.1607 (0.2036)	−0.0074 (0.0290)	−0.0075 (0.0079)	0.0009 (0.0033)	0.0916 (0.2075)	0.8205 (1.0197)
Observations	1889	1889	1889	1889	1889	1889	1889	1889
R-squared	0.7258	0.5369	0.3005	0.3996	0.0883	0.0895	0.5823	0.7138

All regressions control for state and year fixed effects, violent crime rate, unemployment rate, group specific linear trends, and pre-treatment law-specific leads. We use data spanning 1977 to 2014. Robust standard errors clustered at the state level are in parentheses.

* $p < 0.05$.

+ $p < 0.1$.

a specification. Thus all of Table 5 uses the same specification which appeared in column 4 of Table 3.

As can be seen, of the eight falsifications under consideration, six are not significant at conventional levels. The two that are significant – murders by acquaintances and murders of “other known” – may suffer from misclassification and/or ascertainment bias due to the way in which murders are cataloged in the SHR.¹² Brewer et al. (2006) note that due to idiosyncratic features to the way in which police record a murdered

victim's relationship to an offender, some homicides will be misclassified. This is because the monthly reporting schedule for agencies voluntarily participating in the SHR require that agencies report homicides in the month that they are discovered even if the social context of the homicides has not been determined in that month. It is therefore plausible that some intimate partner homicides were misclassified as an acquaintance homicide for no other reason than that the intimate partner relationship was initially thought to be an acquaintance, and by the time it was corrected, the SHR time requirements had passed.

The negative effect on “other known” may suffer from a similar problem if officers at the time of the body's discovery simply did not possess enough information to classify the victim's relationship to the offender, even though they suspected the offender was known by the victim. We included these two additional falsifications for the sake of transparency, but as there is some ambiguity as to whether they are

¹² It is perhaps important to note that the two falsifications which fail are the two outcomes that could either be misclassified at the time the body was found (e.g., acquaintance) or an ambiguous outcome which must encompass any of the other outcomes, including intimate partner homicide, since all other outcomes are mutually exclusive and exhaustive. If police officers, at precisely the time the body was discovered, know that the offender was “known”, but are unsure of the precise identity, then they will classify as “other known” even though in time they may learn he or she was an intimate partner.

true falsifications, we tend to think the other six are clearer falsifications as these represent murders about which the officer possessed sufficient information to make a judgment about the social context of the murder.

Of the six, we consider “stranger” murders to be the most compelling falsification for a few reasons. First, stranger murders are one of the most common types of murders (mean is 0.97) whereas several of the other categories are quite rare. Second, stranger murders clearly should not be affected by the discretionary policy whereas all other offender–victim relationships operate through victim knowledge of the offender. And while the law is only about intimate partners and family members, it’s conceivable that a law that expanded officer discretion in those situations could have spillover effects to friends, neighbors, employers, and employees since each have personal relationships with the victim. In conclusion, while we cannot rule out large negative effects due to the relative size of the standard error, the effect on stranger murders is not statistically different from zero.

In addition to falsifications based on the victim’s relationship to the offender, we also re-analyzed our main results by including our own compilation of Iyengar’s suggested state-level covariates from a variety of sources (non-violent crime rates covering burglary, larceny, and mother vehicle theft, female-to-male employment ratio, income per capita, unilateral divorce laws indicators, indicators for whether the state has the death penalty, and AFDC/TANF maximum benefit for a family of 3), Poisson fixed effects, and a “balance” test on state-level covariates. We report our analysis on state-level covariates in Appendix Table III.1. Including these covariates does not qualitatively change our results, though it does usually reduce coefficient magnitudes. Also, the precision slightly weakens for Panel A, column 2 which has a *p*-value of 0.065 once state-level covariates are included.

We report our Poisson results in Appendix Table III.2 to 5. For our Poisson models, we modeled the dependent variable two ways – as counts with population included as a control variable and rates.¹³ Our results mostly hold up for current spousal homicides when homicides are modeled as counts, though they weaken somewhat when modeled as a rate. Our analysis of former spouse homicides becomes less precise when modeled as Poisson fixed effects.¹⁴

Finally, we examined the exchangeability of states with discretionary laws on observable state-level covariates. In Panel A, we use the same specification as in Table 3 which included controls for violent crime and unemployment but omitted the new state-level controls that Iyengar suggested. In Panel B, we examined each covariate’s relationship to warrantless arrest laws controlling for the Iyengar’s state-level covariates. The only statistically significant effects are on unemployment rate and income per capita. States that adopted discretionary laws were slightly different along these two dimensions and had a 1 percentage point higher level of unemployment (mean is 6.6%) and \$6464 lower income per person (mean is \$28,793). But this latter difference flips sign and becomes small when including state-level controls.¹⁵ All other state-level covariates are statistically indistinguishable from zero.

5.3. Reconciling our results with Iyengar (2009)

One of the issues we discovered early on when trying to replicate Iyengar (2009) was the considerable differences in the mean number of intimate partner homicides between our analysis and her published analysis. Motivated by an interest in reconciling our results with hers, we requested her data and do files, to which she graciously complied.¹⁶ And in more closely evaluating her files, we discovered what we believe was the source of the discrepancy, which was a merge syntax error.¹⁷ Once the error is corrected, Iyengar’s results are no longer statistically significant. The reason our results differ is not due to differences in our classification or the extending of the panel, but rather because we believe that there was an error when compiling the dataset. We discuss this in detail in Appendix II.

6. Conclusion

Can domestic violence be deterred through commitment and credible threats of arrest? The evidence remains mixed. Iyengar (2009) raised a warning flag by providing alarming evidence that mandatory arrest laws had the unintended consequence of increasing intimate partner homicide. Economists and others speculated that the mechanism for this perverse effect was through the increased cost of reporting a domestic violence offense to the victim which may have caused victims to reduce reporting, and consequently incur more serious harm from offenders over time. If true, policymakers were making things worse by focusing on these types of mechanisms.

While this economic mechanism paints a credible and intriguing possibility, careful robustness checks of the Iyengar’s original study are necessary, given disagreement among researchers in classifications of domestic violence arrest laws. We revisited this question with an updated legal appendix using original effective dates of arrest laws collected through thorough archival research. Our new regression results using the corrected legal coding and effective dates indicate that there is no evidence that mandatory arrest or preferred arrest laws have significant impacts on any type of intimate partner homicides.

On the other hand, we find evidence that discretionary arrest laws had economically meaningful and statistically significant reductions in spousal homicides, both current and former. Our results are robust up to group-specific trends, but fall apart once we control for state-specific trends. We believe this may be because state-specific time trends are too demanding a specification and evaporate much needed variation in the outcomes for identification. We investigate the issue more carefully with event study analysis and find no evidence that trends played a role in adoption. The laws are associated with pre-treatment flat differences that are not significant, but upon adoption, almost immediately lead to a level shift reduction in intimate partner homicide rates.

Overall, our results emphasize the importance of replication studies and sensitivity checks by revealing a case where an inadvertent mistake unnoticed led to incorrect conclusions. Unlike Iyengar (2009), we conclude that the evidence that mandatory arrest harms victims is lacking. Ultimately, we find no material impact either way of the mandatory arrest laws on intimate partner homicides, but we find suggestive evidence that simple increases in officer discretion to make arrests did reduce spousal homicide.

¹³ Poisson is preferable to negative binomial when controlling for fixed effects because Poisson does not suffer from the incidental parameters problem. Likewise, the negative binomial model with conditional fixed effects has been shown to not be a true fixed effects model (Allison and Waterman, 2002). One shortcoming of Poisson is it requires the mean and variance to be the same, but we correct for overdispersion by calculating sandwiched standard errors (Cameron and Trivedi, 2005).

¹⁴ It’s possible that the rareness of former spouse homicides (mean equal to 0.03) could be the ultimate cause of this failure to find robust evidence when using Poisson. We therefore interpret our main results as suggestive only.

¹⁵ Intuitively, a rise in unemployment and a decline in income is suggestive of a state level business cycle which is perhaps not captured with our year fixed effects. But we might expect intimate partner violence to rise when incomes fall, not decline as we find. Nevertheless, it is important in light of this finding to control for unemployment at minimum which we do starting with the second column of Tables 3–4.

¹⁶ Iyengar consistently cooperated with all our requests, despite her many computer changes and the eleven years since her publication, showing considerable scientific integrity.

¹⁷ Because of the time that had passed from publication to the present (11 years), Iyengar was unable to find the final do file. But, she did provide an earlier do file from which we were able to replicate most of her results. Upon closer look, we found what we believe was the source of the discrepancy.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.jpubecon.2019.104072>.

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