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Mandatory, Preferred, or Discretionary: How the Classification of Domestic Violence Warrantless Arrest Laws Impacts Their Estimated Effects on Intimate Partner Homicide

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Abstract
Warrantless arrest laws for domestic violence (DV) are generally classified as discretionary, preferred, or mandatory, based on the level of power accorded to police in deciding whether to arrest. However, there is a lack of consensus in the literature regarding how each state's law should

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be categorized. Using three classification schemes, this study examined whether variations among these schemes impact research outcomes by analyzing the effects of discretionary, preferred, and mandatory warrantless arrest laws on intimate partner homicide (IPH). Variations in classification schemes and in the dates of law passage presented in the literature resulted in differing estimated effects of the laws.

Introduction

Since the late 1970s, state statutes allowing police officers to make warrantless arrests of alleged perpetrators of domestic violence (DV) given probable cause have proliferated across the United States. These laws have been controversial since their inception, and there are multiple conflicting hypotheses regarding their effects on DV victims, perpetrators, and outcomes. A review of the literature reveals that researchers are not in agreement on how to define and classify these laws. Due to a lack of consensus regarding how DV warrantless arrest laws are classified, as well as data limitations and modeling difficulties, the existing research on these laws is insufficient to reach firm conclusions about their effects.

Arguably, the most salient aspect of DV warrantless arrest laws is the amount of discretion a police officer has in making the decision to arrest, which varies considerably from state to state. In the generally accepted typology of DV warrantless arrest laws, there are three tiers of decision-making power: full discretion (heretofore referred to as discretionary arrest laws); discretion with the state indicating a preference for arrest (preferred arrest laws); and little to no discretion (mandatory arrest laws). However, there is a lack of consensus in the literature as to how each state's law should be categorized under this typology, leading to discrepancies between law classification schemes used in the research. Little is known about the effects of DV warrantless arrest laws on outcomes such as DV perpetration, recidivism or injury, and the use of differing classification schemes in research complicates the interpretation and integration of the few existing research findings in this area. This article will attempt to isolate the effects of using differing law typologies on research outcomes. Using three classification schemes found in the literature, this study seeks to discern whether variations in such classification schemes impact research outcomes by

analyzing the effects of discretionary, preferred, and mandatory warrantless arrest laws on intimate partner homicide (IPH).

The effects of DV warrantless arrest laws, particularly those termed mandatory arrest laws, are important to discern because there exists a controversy over the usefulness and appropriateness of these laws. Whether mandatory arrest laws will ultimately reduce the incidence of DV, have no effect on DV, or have unintended negative consequences, such as an increase in dual arrests or even IPH, is yet unknown. The answers to this question have clear policy implications. The debate over mandatory arrest laws is summarized below.

The DV warrantless arrest laws examined here are triggered when someone, usually the victim or a third party, calls the police to report a DV event. They allow police officers to arrest a suspected perpetrator of DV without a warrant even if they did not witness the event; laws exist allowing police officers to make warrantless arrests for restraining order violations as well, however, this research focuses specifically on warrantless arrests for the crime of DV. In states with laws that include a mandatory arrest component, a police officer may believe that arrest is necessary upon a finding of probable cause, and therefore arrest. This may occur regardless of the unique circumstances of the case, including whether the victim requests that an arrest not be made. If the state law indicates a preference for arrest, a police officer may arrest more often than not but take the circumstances of the cases into consideration to some degree. If the state has what is viewed as a discretionary arrest law, a police officer may take any of a number of factors into consideration in deciding whether arrest is or is not an appropriate action.

Presumably then, mandatory and preferred arrest laws will result in increased arrest rates in comparison with discretionary arrest laws, a premise generally supported by research (Hirschel et al. 2007; Simpson et al. 2006). Using a cross-sectional design and data from the National Incident Based Reporting System for 19 states in the year 2000, Hirschel et al. (2007) found that the presence of mandatory or preferred arrest laws increased arrest rates for DV incidents. While the research controlled for many factors, including offender demographics and incident seriousness, they were unable to control for other factors that may increase the propensity of police officers to arrest for DV, such as specialized training. Furthermore, the cross-sectional design of the research does not allow investigation into the temporal association between the laws and arrest rates; it is possible that states that are less tolerant of DV are both more likely to have high arrest rates for DV and to enact stricter arrest laws.

It is argued that mandatory arrest laws result in a more consistent and punitive response to DV by police officers, who, upon a finding of probably cause, lack the power to make the decision not to arrest; this sends a message that DV is a crime that will not be tolerated (Stark 1993). This message could help to transform society's attitudes toward and decrease tolerance for DV, thereby leading to an eventual reduction in DV. Furthermore, it is hypothesized that simply increasing the risk and severity of criminal sanctions, such as arrest, will protect victims from harm by deterring perpetrators from future acts of violence (see Williams 2005 for a discussion on deterrence theory, arrest for DV, and the state of the research).

However, there is concern that mandatory or preferred arrest laws have lead to an increase in arrests of DV victims, either singly or as part of dual arrests (circumstances in which both parties are arrested). Indeed, in their research Hirschel et al. (2007) found that the increased arrest rate in states with mandatory arrest laws was attributable, at least in part, to an increase in the arrest of females, who comprise the majority of DV victims, both as part of dual arrests and single arrests. These arrests may have the effects of punishing victims; legitimizing perpetrators; and discouraging victims from further contacting the police, which could encourage violence.

Because mandatory arrest statutes promote a consistent police response to DV, they may be seen as rigid laws that do not permit police officers to decide on the appropriate course of action given the unique circumstances of each situation and instead prescribe a "cookie-cutter" approach to all DV cases. This may put the laws at odds with police officers' preferences. A survey of police officers revealed that they desire more discretion in DV cases, believing that they are capable of appropriately determining when an arrest is necessary (Toon et al. 2005). Not everyone agrees that mandatory arrest laws disallow police discretion, however. Hoctor (1997) points out that arrest still requires a police officer's determination of probable cause, thereby granting a level of discretion to the police officer even under the strictest mandatory arrest law. Furthermore, many state statutes that appear to mandate arrest have a level of discretion built into them beyond the determination of probable cause; for example, the Nevada statute states that arrest is mandatory "unless mitigating circumstances exist" (Nev. Rev. Stat. Ann. §171.137). Presumably, a police officer must decide both what constitutes a "mitigating circumstance" and whether that circumstance exists to determine whether an arrest must be made under the law.

If it is well-advertised to the public that the law requires a police officer to arrest for DV, providing a level of discretion in what is viewed as a mandatory arrest law may have unintended negative consequences. If the victim

calls the police expecting them to arrest the alleged perpetrator but they do not, the victim may believe the police do not view her safety as a priority or that they do not believe her claims of victimization. Much like arresting the victim, failure to arrest the perpetrator when the victim expects arrest may legitimize the perpetrator and discourage the victim from subsequently contacting the police.

Police officers are not the only group to whom mandatory arrest laws limit choice. Mandatory arrest policies remove choice from victims by directing officers not to take the victims' arrest preferences into account. This can be argued as a positive or negative feature of these laws. It can be viewed as negative because victims may not want their perpetrators arrested; some may simply want to stop the violent events at hand. For example, in a study of DV victims in one Alabama county (a state that researchers agree has a discretionary arrest law; Hirschel et al. 2007; Iyengar 2009; Miller 2004) who called the police, some indicated that they called with the goal of ending the abuse in mind but did not want the perpetrators of the abuse arrested (Johnson 2007). Furthermore, a victim may not want her perpetrator arrested because she fears retaliation from her perpetrator for the arrest. If an arrest occurs against the victim's expressed wishes, she may believe she is not being listened to and her concerns, particularly those of retaliatory violence, are not being taken seriously (Rajah, Frye, and Haviland 2006). As the dynamic of DV is one in which the victim is disempowered by her perpetrator, further removing the victim's agency by substituting the state's judgment for her own may compound the problem (Mills 1999).

In fact, researchers have argued that victim preference should be considered in the decision to arrest. In a study conducted in the discretionary arrest state (Hirschel et al. 2007; Iyengar 2009; Miller 2004) of North Carolina, Hirschel and Hutchison (2003) found that victim preference for offender arrest was associated with a greater likelihood of revictimization. Victims who were at a high risk of future victimization were more likely to want their perpetrators arrested and, conversely, those who did not desire arrest were less likely to be revictimized. Because a victim's preference for arrest predicts future violence, and because her preference is predicated upon many elements that the police consider relevant in decisions to arrest, Hirschel and Hutchison (2003) concluded that the police should take victim arrest preferences into account to determine whether arrest is the appropriate option.

Yet, removing victim choice is also seen as a positive feature of mandatory arrest laws. There is a belief that forcing victims to request or advocate for

arrest, as they often must under discretionary laws, may increase the risk of retaliatory violence by the perpetrator (Rajah, Frye, and Haviland 2006). Mandatory arrest laws remove the responsibility for the perpetrator's arrest from the victim, who can truthfully say that she had no influence on the decision to arrest. Removing this responsibility can hypothetically reduce the risk of retaliation against the victim for a perpetrator's arrest.

In sharp contrast to the hypothesis that mandatory arrest laws may reduce violence, one of the few research studies on violent outcomes associated with warrantless arrest laws for DV found evidence to suggest that, at the state level, mandatory arrest laws for DV increased the risk of IPH (Iyengar 2009). Iyengar hypothesized that mandatory arrest laws may increase IPH because they may lead victims who do not want their partners arrested to be less willing to call the police, and thereby not receive the protective effects of police involvement. Unlike much social science research, Iyengar's research findings were presented in the popular press and were noted by several advocacy groups that have called for the repeal of mandatory arrest laws (see, for example, Alliance for Non-Custodial Parents Rights 2007; Harris 2007; Iyengar 2007; PR Newswire 2010; Rosenthal 2007). Due to the similarity of Iyengar's methodology and our own, a critique of this methodology is found in the Discussion section of this article.

Given the controversy surrounding mandatory arrest laws for DV, and their possible negative consequences, it is vitally important that we examine the existing literature in terms of its limitations and ability to make causal statements. Many limitations, including limitations of the available data, omitted variable bias, and modeling procedures, particularly data-driven model selection (Leeb and Potscher 2005), plague research of this nature; these limitations have been acknowledged in regards to the death penalty literature (Donohue and Wolfers 2009). Unfortunately, by varying model specification and selection choices, the production of differing estimates of policy effects is facilitated (Donohue and Wolfers 2009; Leeb and Potscher 2005), leading some to the conclusion that we are far from being able to arrive at a valid answer to the question of whether a certain policy reduces homicide. However, as these modeling difficulties have been discussed at length, our focus here is on the contribution of varying law classification schemes to the problem of determining whether discretionary, preferred, or mandatory DV warrantless arrest laws affect IPH levels, and not on model specification problems per se.

The current state of the literature is one in which multiple classification schemes exist for the arrest law typology based on police power to arrest, further complicating the integration of research results, and making it

difficult to draw conclusions. A body of research that is inconsistent in which laws are considered "mandatory" impedes our understanding of the effects of these laws. It is possible that, for any given research question and study on the effects of these laws, differing classification schemes result in differing outcomes. To use information from the research in advocacy or policymaking, one must be clear about what precisely is meant by the construct "mandatory arrest law."

To address the research question of whether research results depend upon the arrest law classification scheme used, the present research tests three classification schemes based on the amount of discretion police officers have to arrest. To explore this larger question, we conducted research designed to examine a variant of the question posed by Iyengar (2009): do discretionary, preferred, and mandatory warrantless arrest laws for DV impact IPH risk?

Method

To estimate the association between discretionary, preferred, and mandatory arrest laws and IPH risk, we used a panel data design comprised of 46 of the largest cities in the United States. from 1979 to 2006. While this limited our sample of states to 25, we retained a relatively representative mix of states with discretionary, preferred, and mandatory arrest laws under each of the three tested law classification schemes. We employed the same basic statistical approach as Iyengar (2009) in order to facilitate comparisons of model results.

Dependent Variables

Our main dependent variables, counts of total, female, and male IPH victims aged 15 years and above, were constructed using data from the Supplementary Homicide Reports (SHR; Fox and Swatt 2009). Replicating Iyengar's (2009) approach, we created two sets of IPH victim count variables. The first set was comprised of counts of victims whose relationships with perpetrators were that of current or former spouse or common-law spouse. These dependent variables are referred to as "covered" IPH because at a minimum, all of the state arrest laws under study covered marital relationships, though what constitutes common-law marriage and whether this is covered by state law is not entirely clear (Iyengar 2009).

Some states included or expanded the coverage of their DV laws to include intimate relationships such as dating or same-gender relationships.

We therefore constructed a second set of dependent variables of total, female, and male IPH victimization counts that included all possible intimate relationships identified by the SHR (hereafter referred to as ''all'' IPH). Even for state laws that do not cover dating or same-gender relationships, it is possible that there may be a spillover effect of mandatory or preferred arrest laws into these relationships: if police have the power to make warrantless arrests for misdemeanors they did not witness for intimate relationships not covered by DV laws, then the presence of laws favoring arrest for DV may lead them to favor arrest for DV cases without qualifying intimate relationships as well. Such a spillover effect was seen in research by Hirschel et al. (2007) which found that arrests increased for nonintimate relationships in states that had mandatory arrest laws for DV. Therefore, DV arrest laws may affect dating and same-gender relationships even if they do not cover them.

While research suggests an increase in arrests for nonintimate relationships in response to DV mandatory arrest laws, it is not thought that these laws will have a significant impact on nonintimate homicide. Therefore, we used counts of all non-DV homicides as a nonequivalent dependent variable for a robustness test to determine whether omitted factors confound our results.

Given the limitations of the SHR regarding underreporting, we adjusted each of the dependent variables using the procedure described in Dugan, Nagin, and Rosenfeld (2001), which adjusts the SHR counts upwards by the inverse of the proportion of total SHR homicide victim counts to Uniform Crime Reports (UCR) homicide counts when that proportion is less than 1.

Independent Variables

Research publications laying out classification schemes for a DV warrantless arrest law typology based on discretion to arrest are rare. We chose three schemes from the literature for which we found adequate information to reconstruct the law categorization. These three law classification schemes were defined in the literature by Iyengar (2009), Hirschel et al. (2007), and Miller (2004). For ease, we call these classifications A, B, and C, respectively. While each of these classification schemes were used to produce separate dichotomous variables indicating the presence of a mandatory, preferred, or discretionary arrest law in a city-year, with the reference group being those city-years where no law was in place, the decision regarding how to model the timing of the law was complicated by the information included, or not, in the literature.

Classification A is based on the work of Iyengar (2009), which listed the year of law passage and categorization for states deemed as having either mandatory or preferred arrest laws. For the present research, the state laws not listed as mandatory or preferred were considered discretionary. In the publications from which we created classifications B (Hirschel et al. 2007) and C (Miller 2004), all state laws were labeled as either discretionary, preferred, or mandatory; however, neither publication contained information on the year of law passage or implementation.

Using information from session laws, we determined the implementation date of each law. For classifications B and C, we coded the dichotomous law variables as 1 for the first city-year for which the law was implemented for more than 6 months and all city-years thereafter, and as 0 otherwise. When we compared the years of law passage collected from our legal research with those published in Iyengar (2009), we noted some discrepancies. We could not simply base our modeling of classification A's law variables on the collected implementation dates because some of those implementation dates occurred before the year of law passage published in Iyengar (2009). For this reason, we modeled the law variables for classification A in two ways. First, we coded the dichotomous law variables as 1 for the first city-year after the law was passed and all city-years thereafter, and as 0 otherwise. The year of law passage was supplied by Iyengar (2009) for mandatory and preferred arrest laws, and by our legal research for the discretionary laws (for which Iyengar 2009 published no information). This manner of modeling the timing of classification A laws is referred to as A1. Second, we coded the dichotomous law variables as 1 for the first city-year for which the law was implemented for more than 6 months and all city-years thereafter, and as 0 otherwise, based on implementation dates collected by our legal research. This is referred to as A2.

We chose to model classification A as we did because it is possible that Iyengar's interpretation of the law rests on a different law section, with a different implementation date, than does our interpretation. The first coding scheme, which rests on Iyengar's years of passage, reflects this possibility and is also the manner in which Iyengar represented her law variables (2009). The second coding scheme more precisely models whether the law was present in a given city-year, and allows us to directly compare outcomes given differing law classification schemes by removing variability in law timing between schemes.

As the three coding schemes were developed by the original researchers based on the laws as they read in the year 2000 or later, and the most recent state law change relevant to this research occurred in the year 2000, we were confident that we had up-to-date law classifications.

We controlled for numerous city-year and state-year factors that are potentially associated with the incidence of DV and DV responses and may in turn affect the IPH rate. We controlled for city crime levels using city-level rates for violent and nonviolent crimes as calculated from the FBI's UCR (U.S. Dept. of Justice 2007), as well as the city-level adjusted adult, nonintimate homicide rate as calculated with data from the SHR (Fox and Swatt 2009).[1] We also controlled for the ratio of prisoners under state and federal jurisdiction per population in a state (Bureau of Justice Statistics 2005) and legality of the death penalty in a state (Death Penalty Information Center 2009). Economic conditions such as the city-level log of personal income per capita and the ratio of females to males aged 16 and over who were employed, and state-level unemployment rates, as taken from the decennial U.S. Census (U.S. Census Bureau 1973, 1981, 1991, 2001), were also controlled for. In addition, we included the social policy controls of the average Aid to Families with Dependent Children/Temporary Assistance for Needy Families (AFDC/TANF) payment for a family of four in a state (Committee on Ways and Means 1996, 1998, 2000, 2004) and presence of unilateral divorce laws (Gruber 2004).

We reduced omitted variable bias by controlling for differences between cities that may affect the IPH rate with city fixed effects, and differences from year to year that may affect the IPH rate on a national level with year fixed effects. We also tested a linear time trend to control for factors that caused the IPH rate to change nationally over time, and we allowed city trends to vary over time by interacting the city fixed effects with the linear trend term.

Statistical Method

Krippendorff's α scores were calculated for each two-way comparison of law classification schemes both for the sample of states under study and for all states for the year 2000 alone. We also inspected the text of the publications and the laws to determine where discrepancies in construct explication or law assessment lay.

To estimate the association between arrest laws and IPH counts, we considered both Poisson and negative binomial regression models. The Poisson models ultimately proved a better fit. Our unit of analysis was the city-year and we used the natural logarithm of the population at risk as the offset for the models: the offset for the models of total IPH was therefore the natural logarithm of the total population aged 15 years and above for each city-year; the offsets for the models of male IPH and female IPH were

the natural logarithms of the totals of the male and female populations aged 15 years and above, respectively. Because we took repeated measures of each city across time, autocorrelation was a threat to statistical validity. To correct for the effect of autocorrelation of the IPH counts on estimated standard errors, we used generalized estimating equations (GEE), clustering on cities. We used the standard criteria of *p* value <.05 for a two-tailed test to denote significance.

Preliminary analyses were run to separately test the effect of each control variable on the two dependent variables of total covered IPH counts and all IPH counts. The models also included city fixed effects, a linear time trend, and city-specific linear trend variables. Those control variables with a *p* value >.10 for both models were dropped from the analysis, resulting in the removal of AFDC/TANF benefits, unilateral divorce law, and death penalty policy variables. It should be noted that the inclusion of these variables in the models did not produce substantially different estimates. Preliminary analyses were also run to test the effects of the arrest law dummy variables on total covered IPH counts, again controlling only for city fixed effects, a linear time trend, and city-specific linear trend variables.

Endogeneity through feedback (in this case, the concept that the passages of the arrest policies depend in part on IPH levels) is not thought to bias the research estimates as DV warrantless arrest laws are not believed to have been passed in response to changes in IPH levels. In fact, the history of how warrantless arrest laws for DV perpetration proliferated across the United States has been well-documented (see, for example, Buzawa and Buzawa 1993) as depending upon the growing women's rights movement that agitated for DV to be viewed as a crime; a political climate in which the populace and lawmakers favored more punitive measures for offenders; high-profile court cases against jurisdictions, such as Thurman v. the City of Torrington (1984) in which a victim of DV successfully sued the city for not giving her equal protection under the law; and an influential research experiment that suggested that perpetrators of DV were less likely to recidivate when they were randomly assigned to the arrest versus standard response groups (Sherman and Berk 1984). Therefore, no additional measures were employed to control for endogeneity.

Results

For both the sample of study states and all states for the year 2000, interrater agreement as assessed by Krippendorff's α was fair between classifications A and B and classifications A and C, and excellent between classifications

Table 1. Interrater Agreement Between Law Coding Schemes for the Study States and for all States for the Year 2000

	Krippendorff's α for Study States	Krippendorff's α for all States
A and B	.537	.581
A and C	.475	.525
B and C	.871	.904

B and C (see Table 1), although agreement was somewhat lower for the sample of study states. An analysis of the text of the publications showed that the three classification schemes used in this research seem to have similar classification criteria, but, for several state laws, differing assessments of whether those criteria apply. The small amount of text devoted to describing the law classification criteria in the publications makes it difficult to discern the reason for differences in the construct explications. What is apparent is that classification A diverged from classifications B and C in arguably substantial and potentially important ways.

Table 2 shows precisely how the law classification schemes categorize each state's law, and therefore how the classifications differ. For classification A, the year of passage included in Iyengar (2009) is also listed. A discrepancy between classification A's year of passage and ours is indicated by presenting our year of passage in parentheses and italics next to the classification A year of passage. Study states are indicated with an asterisk. At least some of the discrepancy between the years of passage provided by classification A and those provided by our legal research may be due to classification A's examination of a passage of the law other than that describing police arrest powers for DV cases. For example, the passage of the California law cited (Cal. Penal Code §836(c)(1)) describes police powers to make a warrantless arrest for restraining order violation, and not for the more general crime of DV.

The results of the main models are presented in Tables 3 and 4. The estimates produced using classification C are not presented because they were nearly identical to those produced using classification B. Table 3 lists the incidence rate ratios (IRRs) and 95% confidence intervals (CIs) for the preliminary models, and the covered total IPH, covered female IPH, and covered male IPH models (Models 1 through 4, respectively). The results of the preliminary analysis of arrest law effects on covered total IPH indicate that while the differences in the law classification schemes included in the research influence the estimated effects of the laws, the more substantial differences in estimated effects were based on how the timing of the laws

Table 2. Law Categorization by State and Classification Scheme for all States

State	Classification A	Classification B	Classification C
AL	Discretionary	Discretionary	Discretionary
AK	Mandatory 1996	Mandatory	Mandatory
AZ*	Recommended 1991	Mandatory	Mandatory
AR	Discretionary	Preferred	Preferred
CA*	Recommended 1993 *(1996)*	Preferred	Preferred
CO*	Mandatory 1994	Mandatory	Mandatory
CT	Mandatory 1987 *(1986)*	Mandatory	Mandatory
DC	Mandatory 1991	Mandatory	Mandatory
DE	Discretionary	Discretionary	Discretionary
FL	Discretionary	Discretionary	Preferred
GA*	Discretionary	Discretionary	Discretionary
HI*	Discretionary	Discretionary	Discretionary
ID	Discretionary	Discretionary	Discretionary
IL*	Discretionary	Discretionary	Discretionary
IN*	Discretionary	Discretionary	Discretionary
IA	Mandatory 1990 *(1986)*	Mandatory	Mandatory
KS	Recommended 2000 *(1984)*	Mandatory	Mandatory
KY	Discretionary	Discretionary	Discretionary
LA*	Discretionary	Mandatory	Mandatory
ME	Mandatory 1995 *(1979)*	Mandatory	Mandatory
MD*	Discretionary	Discretionary	Discretionary
MA*	Discretionary	Preferred	Preferred
MI*	Discretionary	Discretionary	Preferred
MN*	Discretionary	Discretionary	Discretionary
MS	Recommended 1995	Mandatory	Mandatory
MO*	Recommended 1989	Mandatory	Discretionary for first inci dent; mandatory for sec ond incident within 12 hr
MT	Discretionary	Preferred	Preferred
NE	Discretionary	Discretionary	Discretionary
NV	Mandatory 1989 *(1985)*	Mandatory	Mandatory
NH	Discretionary	Discretionary	Discretionary and preferred
NJ	Mandatory 1991	Mandatory	Mandatory
NM*	Discretionary	Discretionary	Discretionary
NY*	Recommended 1994	Mandatory	Mandatory
NC*	Discretionary	Discretionary	Discretionary
ND	Discretionary	Preferred	Preferred
OH*	Recommended 1994	Mandatory	Mandatory
OK*	Discretionary	Discretionary	Discretionary

Table 2 (continued)

State	Classification A	Classification B	Classification C
OR*	Mandatory 2001 *(1977)*	Mandatory	Mandatory
PA*	Discretionary	Discretionary	Discretionary
RI	Mandatory 2000 *(1988)*	Mandatory	Mandatory
SC	Recommended 2002 *(1995)*	Mandatory	Mandatory
SD	Mandatory 1998 *(1989)*	Mandatory	Mandatory
TN*	Discretionary	Preferred	Preferred
TX*	Discretionary	Discretionary	Discretionary
UT	Mandatory 2000 *(1995)*	Mandatory	Mandatory
VT	Discretionary	Discretionary	Discretionary
VA*	Mandatory 2002 *(1996)*	Mandatory	Mandatory
WA*	Mandatory 1999 *(1984)*	Mandatory	Mandatory
WV	Discretionary	Discretionary	Discretionary
WI*	Mandatory 1996 *(1987)*	Mandatory	Mandatory
WY	Discretionary	Discretionary	Discretionary

Note: A discrepancy between Classification A's year of passage and ours is indicated by presenting our year of passage in parentheses and italics next to the Classification A year of passage.
*State included in the study.

were modeled. For classification A1, both the mandatory and preferred warrantless arrest laws were associated with a decrease in covered total IPH risk (IRR: 0.60, 95% CI: 0.51, 0.71; IRR: 0.82, 95% CI: 0.68, 0.99, respectively). Using classifications A2, B, or C, no significant law effects were found.

The results of Models 2 and 3 are similar. Under classification A1, the mandatory arrest law is estimated to reduce covered total IPH (IRR: 0.70, 95% CI: 0.57, 0.86) and covered female IPH (IRR: 0.79, 95% CI: 0.68, 0.93); however, neither preferred nor discretionary arrest laws were estimated to impact covered total or covered female IPH. The direction of the point estimate for the effect of mandatory arrest laws differs between classification A1, in which the IRR suggests a negative relationship, and classifications A2, B, and C, in which the IRRs suggest a positive relationship. Under classifications A2, B, and C, none of the DV warrantless arrest laws had a significantly different effect from no DV warrantless arrest law. However, the direction of the point estimate for the effect of preferred arrest laws differs between classification A2 and those of classifications B and C.

Under classification A1 in Model 4, mandatory arrest laws are associated with a 68% decrease in covered male IPH (IRR: 0.32, 95% CI: 0.14, 0.76),

Table 3. Poisson Regression on Estimates of the Effects of Three Classifications on Schemes of DV Warrantless Arrest Laws on Total Covered IPH, Female IPH, and Male IPH

	Preliminary Model 1 Covered IPH	Model 2 Covered IPH	Model 3 Covered Female IPH	Model 4 Covered Male IPH
	IRR (95% CI)	IRR (95% CI)	IRR (95% CI)	IRR (95% CI)
Law classification on A1				
Mandatory	0.60*** (0.51, 0.71)	0.70** (0.57, 0.86)	0.79** (0.68, 0.93)	0.32* (0.14, 0.76)
Preferred	0.82* (0.68, 0.99)	0.95 (0.73, 1.24)	0.92 (0.66, 1.28)	0.90 (0.63, 1.30)
Discretionary	1.08 (0.89, 1.31)	1.10 (0.96, 1.26)	1.07 (0.87, 1.33)	1.10 (0.92, 1.30)
Law classification on A2				
Mandatory	1.03 (0.44, 2.41)	0.92 (0.49, 1.73)	1.09 (0.58, 2.05)	0.56 (0.24, 1.29)
Preferred	0.89 (0.78, 1.03)	0.96 (0.76, 1.21)	0.87 (0.67, 1.13)	1.11 (0.73, 1.69)
Discretionary	1.12 (0.93, 1.34)	1.11 (0.99, 1.25)	1.12 (0.95, 1.32)	1.06 (0.89, 1.26)
Law classification on B				
Mandatory	1.14 (0.77, 1.70)	1.04 (0.81, 1.33)	1.11 (0.85, 1.46)	0.82 (0.59, 1.13)
Preferred	0.92 (0.81, 1.03)	1.12 (0.92, 1.36)	1.01 (0.75, 1.37)	1.42 (0.94, 2.13)
Discretionary	1.06 (0.89, 1.26)	1.07 (0.92, 1.23)	1.05 (0.88, 1.24)	1.06 (0.86, 1.30)

Note: Preliminary model is a Poisson regression of covered IPH on city fixed effects, linear trend, city-specific linear trends and law variables. Models 2 through 4 include city fixed effects, linear trend, city-specific linear trend, year fixed effects, and control variables. DV = domestic violence; IPH = intimate partner homicide; IRR = incidence rate ratio; CI = confidence interval.
*p < .05. **p < .01. ***p < .001.

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Table 4. Poisson Regression on Estimates of the Effects of Three Classification on Schemes of DV Warrantless Arrest Laws on all IPH, all Female IPH, all Male IPH, and all non-IPH Homicides

	Model 5 All IPH IRR (95% CI)	Model 6 All Female IPH IRR(95% CI)	Model 7 All Male IPH IRR (95% CI)	Model 8 All Non-DV Homicides IRR (95% CI)
Law classification on A1				
Mandatory	0.69*** (0.57, 0.83)	0.87 (0.76, 1.00)	0.38*** (0.24, 0.62)	0.85 (0.72, 1.01)
Preferred	1.15 (0.98, 1.36)	1.06 (0.86, 1.31)	1.26 (0.99, 1.62)	1.20 (0.98, 1.45)
Discretionary	1.09 (0.93, 1.27)	1.08 (0.92, 1.26)	1.11 (0.93, 1.31)	0.94 (0.82, 1.07)
Law classification on A2				
Mandatory	1.16 (0.67, 2.03)	1.28 (0.85, 1.93)	0.94 (0.35, 2.55)	1.27 (0.92, 1.74)
Preferred	1.15 (0.96, 1.38)	1.03 (0.84, 1.27)	1.30 (0.98, 1.72)	1.00 (0.83, 1.20)
Discretionary	1.05 (0.91, 1.21)	1.07 (0.92, 1.24)	1.03 (0.89, 1.19)	0.91 (0.81, 1.03)
Law classification on B				
Mandatory	1.24 (0.99, 1.55)	1.23 (0.96, 1.58)	1.19 (0.86, 1.65)	1.20 (0.94 1.53)
Preferred	1.16 (0.90, 1.50)	1.01 (0.75, 1.35)	1.45** (1.10, 1.93)	0.78* (0.64, 0.96)
Discretionary	1.00 (0.87, 1.17)	1.04 (0.90, 1.19)	0.96 (0.81, 1.14)	0.95 (0.85, 1.07)

Note: Models 5 through 8 include city fixed effects, linear trend, city-specific linear trend, year fixed effects, and control variables. DV = domestic violence; PH = intimate partner homicide; RR = incidence rate ratio; C = confidence interval.
*p < .05. **p < .01. ***p < .001.

whereas the estimated relationships of mandatory arrest laws to covered male IPH under classifications A2, B, and C are negative but not significant. Preferred and discretionary arrest laws are not estimated to impact the risk of covered male IPH under any of the law classifications, and the point estimates are positive for all but the relationship between preferred arrest laws under classification A1 and covered male IPH.

Table 4 lists the estimated effects of the laws on all IPH, all female IPH, all male IPH, and all non-DV homicides (Models 5 through 8, respectively). Again under classification A1, the mandatory arrest law is associated with a reduction in IPH risk (IRR: 0.69, 95% CI: 0.57, 0.83); however, preferred and discretionary laws are not associated with any significant impact. Under classifications A2, B, and C, none of the laws are estimated to have an impact on all IPH risk (Model 5). Likewise in Model 6, none of the law classifications produced estimates that suggested the laws significantly reduced all female IPH risk; however, the estimated effect of the mandatory arrest law under classification A1 nears significance (IRR: 0.87, 95% CI: 0.76, 1.00).

In Model 7, under classification A1, mandatory arrest laws were estimated to significantly reduce all male IPH risk (IRR: 0.38, 95% CI: 0.24, 0.62), but the same effect was not seen under classifications A2, B, or C. Under classifications B and C, the preferred arrest law was estimated to increase the risk of all male IPH (IRR: 1.45, 95% CI: 1.10, 1.93). The discretionary arrest law did not have a significant impact on all male IPH under any of the classification schemes, however, the direction of the point estimates differed between classifications A1 and A2 and classifications B and C.

In Model 8, the effect of the laws and law classification schemes on all nonintimate partner homicides, produced some statistically significant correlations. This suggests the relationships between the preferred arrest laws under classifications B and C with IPH and its subcategories are confounded by a factor or factors that are both associated with the passage of these laws and with a decrease in nonintimate partner homicide. Such a factor may also impact IPH, casting doubt on the results of previous models.

Discussion

This research examined whether differences in the classification of laws governing warrantless arrests for DV affect estimates of the laws' effects on IPHs. To do so, we examined whether these laws, as categorized under three classification schemes, impacted IPH risk in 46 large US cities.

In doing this, we discovered differences in the timing of the laws between those years of passage associated with classification A and those we researched ourselves, prompting us to develop a fourth classification scheme—classification A with its published timing of the laws (A1) and classification A with our implementation dates (A2). We found that the magnitudes, directions, and significance levels of the estimated effects of the law constructs on IPH varied between the schemes, with the most notable differences resulting from varying the timing of the laws, leading us to hypothesize that differences in these schemes and law timing may also impact the estimated effects of the laws on other DV outcomes.

This conclusion represents a very real problem for the integration of research findings, hypothesis building, and practice. To illustrate, one might hypothesize that the increase in dual arrests found in states with mandatory arrest laws by Hirschel et al. (2007) and the possible consequences of victim arrest discussed earlier, such as an unwillingness to call the police for future violent events, play a small role in the increase in IPH seen in states with mandatory arrest laws in Iyengar's research (2009). However, as illustrated here, the level of agreement between the classification schemes used by Hirschel et al. (2007) and Iyengar (2009) is troublingly low; the group of states in which Hirschel et al. (2007) found an increase in dual arrests due to mandatory arrest laws is not the same group of states in which Iyengar (2009) found an increase in IPH associated with mandatory arrest laws.

The issues addressed here are construct validity and law timing. The laws under study in this research are complex and multifaceted and the constructs of these laws used in research need to take that complexity into account. While the prototypical feature of these laws is thought to be the level of discretion allowed to police officers in making the decision to arrest, there are other provisions in these laws that may also affect whether an arrest occurs. A reading of the literature uncovered no precisely defined construct of these arrest laws accepted by the field at large, indeed the definitions found tended to be simplistic given the complexity of the laws, leaving the assessment of which laws fit which construct murky for some laws. For example, Louisiana's statute states that officers *shall* arrest if there is probable cause to believe a DV felony or a DV misdemeanor that "endangers the physical safety" of the victim occurred, however, "if there is no cause to believe there is impending danger, arresting the abusive party is at the officer's discretion" (La. R.S. §46:2140). Likely because the law lays out both a discretionary and a mandatory circumstance, it has been assessed to be both a discretionary (Iyengar 2009) and a mandatory arrest law (Hirschel et al. 2007).

Reasons for the discrepancies in law timing between Iyengar's (2009) research and our own are largely unknown. As stated previously, some of the inconsistencies may be due to differing views regarding which section of the law referred to the police power to make a warrantless arrest. Other discrepancies are not so easy to explain. For example, our legal research showed that Oregon passed its mandatory arrest law in 1977 and Iyengar's research suggested it was passed in 2001. In fact, there are several states in which our year of law passage differs from Iyengar's by 5 or more years. Due to these large discrepancies, we have fastidiously checked and rechecked our legal research and are confident in our findings. However, the point remains that varying the timing of the arrest laws in the statistical models can impact the estimated effects of these laws on IPH to a large degree.

Research suggests that differing model specifications can also impact model results (Donohue and Wolfers 2009; Leeb and Potscher 2005). Omitted variable bias may play a role in the estimates produced. For example, the models do not include a measure of arrests for DV, which may be an important component of the potentially deterrent effect of arrest. Similar to Donohue and Wolfers' (2009) argument that a multitude of criminal sanctions should be employed as controls in research on the effects of capital punishment, criminal sanctions for DV, such as the proportion of those arrested who are charged, conviction rates, and severity of sentences, should be controlled for in research on the effects of warrantless arrest laws. Without these variables in the model, one cannot disentangle their impact on DV outcomes from those of warrantless arrest laws. Unfortunately, these data are simply not readily available for numerous geographic units (be they cities, counties, or states) over lengthy study periods, greatly hampering the ability of these models to produce unbiased results.

The results of the question of whether mandatory, preferred, or discretionary arrest laws for DV affect IPH in comparison to no DV-specific arrest laws were mixed in our models. Mandatory arrest laws, as defined by classification A and using Iyengar's (2009) law timing, may reduce the risk of IPH in large US cities when compared with a lack of DV-specific warrantless arrest laws. However, these results were not replicated using classification A with our law timing, nor were they replicated with classifications B or C, which differed considerably from classification A. Based on the results of this research, the authors conclude that the inconsistency in construct explication and law assessment and timing alone can alter the findings of research on DV outcomes.

Interestingly, the results produced here using classification A with Iyengar's timing are contrary to the research from which classification A

was taken, the results of which suggested that mandatory arrest laws increased the risk of IPH at the state level (Iyengar 2009). Some of the difference may derive from the varying designs of the two studies. This research used a narrower sample than Iyengar, who included IPH counts in all states. We focused, instead, on large cities in 25 states. Differential implementation of the laws between states represented in this study and those not included could lead to differing results. Iyengar's hypothesized explanation for the findings of the research, that victims are less likely to report DV to the police in states with mandatory arrest laws because they do not want their partners arrested, is based on the degree to which DV victims are aware of mandatory arrest laws. Mandatory arrest laws may have been passed and implemented with a great deal of publicity in the states not included in this research, or they may have been better publicized outside of the urban areas represented in this study. The present research also utilized a different reference group (no DV-specific arrest law) than the research of Iyengar (2009; no and discretionary arrest laws), which could also account for some of the difference in results. For each of the models, Iyengar tested the effects of mandatory and preferred arrest laws in reference to discretionary arrest laws, yet the time span covered by her research includes numerous state-years with no DV warrantless arrest law in effect. Iyengar therefore collapsed the two distinct conditions of having no law and having a discretionary warrantless arrest law into one category. It is possible that these two conditions have differing effects on behaviors and other factors that may mediate IPH, making their use as a combined reference group potentially problematic.

We strongly suggest caution when interpreting the estimated effects found in this research of mandatory, preferred, and discretionary arrest laws for DV on IPH. The classification schemes used here produced varying estimates, bringing to light the importance of consistency of meaning of the constructs and assessment of the laws and their timing far more than shedding light on the impacts of the laws. Furthermore, the models suffered from omitted variable bias, as well as the well-known limitations present in the SHR data used to construct our dependent variables (see, for example, Wadsworth and Roberts 2008). Complicating the matter still is the measurement of the intervention as the presence or absence of a warrantless arrest law, a measurement that does not control for whether local jurisdictions actually implement the law in practice.

This study isolated the impact of varying law classification schemes on policy research outcomes. Unfortunately, it is difficult at best to advance theory and practice when the construct being studied is unclear and the manner in which its timing is modeled is inconsistent or flawed. As

evidenced here, disagreement between the classification schemes and law timing can lead to strikingly different estimated effects of arrest laws on IPH. Researchers, policymakers, and advocates who are unaware of the differences between studies may make generalizations about mandatory arrest laws that cannot actually be supported. Researchers must be more transparent in how they defined the constructs of discretionary, recommended, and mandatory arrest laws, and clearly show how each law was assessed, possibly addressing differences between their assessments and that of others, and provide information on law implementation dates so that consumers are aware of these differences and can interpret the research accordingly. While difficulties in model specifications and available data persist, increasing the difficulty of arriving at conclusions regarding law effects, a step must be made toward a greater understanding of DV warrantless arrest laws. Ultimately, social science researchers must team with legal scholars to clearly define these constructs and conduct the legal research required to determine classifications and implementation dates so that the field may advance and inform the ongoing debate on the appropriateness and effectiveness of these laws.

Note

1. The control variable of the adult, nonintimate homicide rate was not used in the model with the nonequivalent dependent variable of all nonintimate homicides per city year.

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