Officer Discretion and Domestic Violence

The Impact of Mandatory Arrest Laws

on the Incidence of Intimate Partner Homicide

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Abstract

Since the 1980s, many states have passed different arrest laws in an attempt to combat domestic violence. A mandatory arrest law requires that a person be arrested at the scene of a domestic disturbance, while discretionary arrest laws allow for, but do not mandate, a warrantless arrest. Preferred arrest laws promote arrest as a response, but also do not mandate its use. In spite of multiple papers on the subject, the impact these laws have on domestic violence remains an open question. After replicating previous work, I create my own set of classifications for state laws and redo previous analysis. The basic qualitative results are not sensitive to this revised classification. Then, to look more deeply into the impact a law has on a given state, I estimate bounds on the treatment effect of mandatory and discretionary arrest laws using bounded-variation assumptions. Results from Maryland, Pennsylvania, Michigan, and Nevada provide a mixed answer, with estimated bounds on the treatment effect ranging from [-0.0465, 0.1464] for a discretionary arrest law in Maryland to [-0.8614, -0.7014] for a mandatory arrest law in Nevada. The different bounds suggest that arrest laws have a variable impact across states and that these laws warrant future studies that focus on a more local level than has previously been attempted.

I Introduction

Intimate partner violence remains one of the major sources of violence across the world, with 1 in 3 women experiencing some version of it in their lifetime. Domestic violence is correlated with bad economic outcomes, poor mental health, and deleterious physical health (Trabold et al. 2018). In the United States, law enforcement's response to domestic violence over time has been underwhelming and ineffective. Until the early 20th century, it was widely regarded as a private matter- something to be dealt with between a husband and a wife. Whenever a domestic disturbance or similar issue was reported, police departments settled upon a response- deescalation. So, when officers arrived at the scene, the goal was to prevent more violence from occurring. With that in mind, prior to the 1960s and the 1970s, the major police response was to remove the aggressor from the situation and drop them off in a different location (Darst 2019). Women's groups and other advocates led a push through the 1960s to provide better protections and interventions for domestic violence victims. Alongside the creation of specialized domestic violence shelters, hot lines, and improved victims' resources, all levels of government passed laws to punish abusers and to protect those in an abusive household (Goldfarb 2005). Due to a documented reluctance to arrest and prosecute abusers, many of these laws were mandatory measures- mandatory reporting for doctors, mandatory prosecution with or without the victim's consent, and mandatory arrest for suspected abusers in domestic disturbance calls (Sherman and Berk 1984).

Although mandatory measures were widely accepted and widely passed, the impact of such policies remains the subject of much study and much debate. This paper thus seeks to measure the impact of mandatory and discretionary arrest laws on the incidence of domestic violence, specifically the incidence of intimate partner homicides. Previous work uses a linear difference-in-difference model, using the year a state switched laws as the instrument. However, the analysis relies on classifying laws into distinct categories while the way some laws are written falls somewhere in-between a mandatory law and a discretionary law. An incorrect classification of one such law could impact the analysis. Moreover, this might suggest that the impact of the different laws varies across states.

This paper replicates previous results while re-doing the analysis with two additional different sets of legal classifications. Then, the final analysis will be a partial-identification analysis using bounded-variation assumptions to identify a plausible range on the impact of these laws in a particular state, as opposed to attempting to point-identify the effect it has on intimate partner homicides across all states.

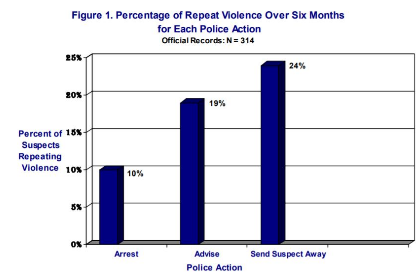
Section II provides the context for the discussion of these arrest laws, with subsection II.1 on the history of such laws, subsection II.2 on the work done by other researchers, and subsection II.3 on the classification of individual state laws. Section III covers the replication and re-analysis of previous work. Subsection III.1 discusses the data, with subsection III.2 focusing on the results of the analysis. Section IV examines the partial identification portion of the analysis, with subsection IV.1 discussing the motivation and literature for partial identification and subsection IV.2 for a primer on the subject. Subsection IV.3 contains the results of the partial identification analysis. Finally, section V reviews limitations and potential future work, while section VI concludes.

II History, Classifications, and Previous Work

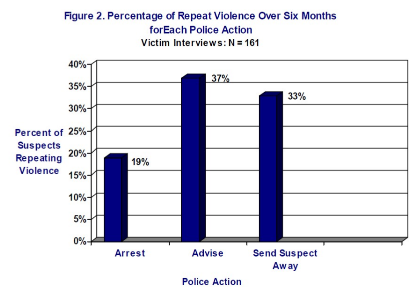
II.1 Warrantless Domestic Violence Arrests

In 1981, Lawrence W. Sherman and Richard A. Berk, with the help of the Minneapolis Police Department and the Police Foundation, ran a randomized controlled study to empirically identify the best response to domestic violence calls. When a Minneapolis police officer arrived on the scene of a misdemeanor domestic violence call, they would have a treatment randomly assigned to the incident. The three treatments were arresting the aggressor, removing the aggressor from the premises, or advising the couple in question through whatever methods the officer deemed appropriate. Then, the experimenters attempted to follow up with the households for six months afterwards to ascertain the aggressor's response to police action. After 18 months and 314 recorded calls, the experiment ended with clear results (Figure 1). Those individuals that were advised by the officer or sent away for a few hours were far more likely to have another domestic violence incident than those that were arrested.

Sherman and Berk published their results in 1984 with the cautionary note that more experiments were needed. Six additional experiments were planned, although only five were carried out. State governments, in response to a lawsuit alleging police negligence towards domestic violence victims (see Appendix A), began to pass mandatory arrest laws before the experiment could be replicated (*Thurman v. City of Torrington* 2019).



(a) Rate of Re-offense based on official record



(b) Rate of Re-offense based on Victim Interviews

Figure 1: Results from Sherman and Berk 1984

II.2 Previous Work

After the work of Sherman and Berk in Minneapolis, the U.S. Attorney General's Task Force on Family Violence planned the Spouse Assault Replication Program, in which six additional cities to attempt to replicate the results. The cities in question were Miami-Dade, Omaha, Charlotte, Milwaukee, Colorado Springs, and Atlanta, although the Atlanta experiment was never actually completed (Maxwell, Garner, and Fagan 2002). The results across the different studies were conflicting- no city perfectly replicated the results of another. The contrast in results pushed researchers to return to the Minneapolis Domestic Violence Experiment to reanalyze the results. For example, Tauchen and Witte in their 1995 paper began to analyze the impact of arrest on domestic violence over time. Using the Minneapolis data, they found that, although arrest did have a larger impact than the other treatments, the impact disappeared after a short period of time (Tauchen and Witte 1995). Other problems with the Minneapolis experiment came to light, such as the impact of high-rate officers, the difference between assigned and delivered treatment for aggressive offenders, and the unique arrest policies within Minneapolis (Gartin 1995, Tauchen and Witte 1995).

To better identify the impact of mandatory arrest laws on domestic violence, research turned away from the experimental evidence towards the observational evidence from states that passed these laws. Firstly, in Mills's 1998 paper on the impact of mandatory arrest laws, she also found that mandatory arrest laws disproportionately affect certain communities, like the African-American community and those without a high school degree (Mills 1998). Then, later work established that the passage of a mandatory arrest law or a preferred arrest law leads to an overall increase in arrests for domestic violence, with a disproportionate impact on women (D. Hirschel et al. 2007). Women make up an estimated 13% of aggressors in domestic disturbances, but make up 30% of arrests (Hafemeister 2010). Mandatory arrest laws also lead to an increase in the number of dual arrests, in which both individuals involved are taken into custody. Preferred arrest laws do not show a similar rise in dual arrests (D. Hirschel et al. 2007).

Iyengar (2009) analyzed the impact of mandatory versus discretionary arrest laws on intimate partner homicides across the nation. She used intimate partner homicides as her indicator on domestic violence due to reporting issues with domestic violence. Her results showed that the passage of a mandatory arrest law was associated with a 60% increase in the rate of intimate partner homicides and that a discretionary arrest law did not have a statistically significant effect. She attributed the rise in homicides to a decrease in reporting from victims who were not ready to leave their partner as well as retribution from an abusive partner towards a victim who reported an incident (Iyengar 2009). Then in 2019, Dr. Chin and Dr. Cunningham found a syntax error in the way population data was merged in Iyengar (2009), and, when the error was fixed, the significance in her model disappeared (Chin and

Cunningham 2019). Iyengar acknowledged the problem in a 2019 corrigendum to her paper, but asserted that her overall theoretical model was sound (Iyengar 2019).

After their attempts at replicating Iyengar's work, Chin and Cunningham also reported on their own analysis of the impact of mandatory arrest laws. They made several changes to the previous work to better identify where these laws were helping and where they were causing harm. First, they categorized all states as being a mandatory arrest law state, a preferred arrest law state, or a discretionary arrest law state. This contrasts with Iyengar who only categorized some states and only categorized those states as mandatory or discretionary arrest law states. Chin and Cunningham's analysis also separated the impact by type of relationship; the relationships that were included were spouses, ex-spouses, common-law spouses, and those in dating relationships. They categorized each state in each year between 1977 and 2014 based on what type of law applied to what type of relationship. For example, in 1998, Connecticut had a mandatory arrest law for spouses, former spouses, and common-law spouses, but not for those in dating relationships. They then ran regressions analyzing the impact of one type of law on the homicide rate for that particular relationship. None of the types of laws included in their work decreased the rate of homicides in dating relationships in a statistically significant way. In contrast to previous research, their results found that mandatory arrest laws and preferred arrest laws did not impact the homicide rate for any type of relationship, but that discretionary arrest laws decreased homicides rates for current spouses by 0.125, former spouses by 0.017, and common-law spouses by 0.002.

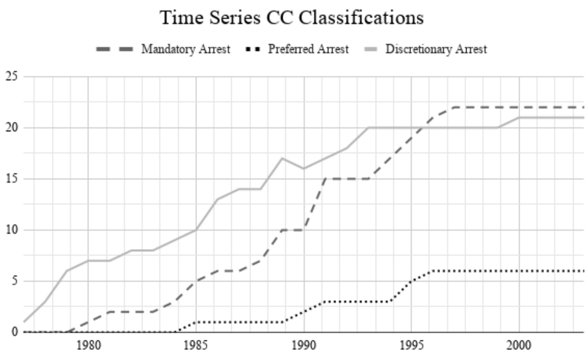
II.3 Legal Classification

As states began to pass their own mandatory arrest laws, three main types of laws appeared. The first type of law came directly from the Minneapolis Domestic Violence Experiment- the mandatory arrest law. Typically, mandatory arrest laws state that an officer must arrest at least one person at the scene of a misdemeanor domestic violence call. The intention is for the officer to arrest the main aggressor in the situation, but more than one person could be arrested. The second type of law is a discretionary arrest law. Instead of mandating arrest, a discretionary arrest law allows officers to arrest a person at the scene of a domestic disturbance without a warrant if they suspected there was domestic violence occurring in that home. If they did not believe there would be a repeat offense or if they believed there was not domestic violence, the officer could choose to do nothing. The third type, a preferred arrest law, lies somewhere in between the other two. This law allows for a warrantless arrest, and in fact, labels arrest as the preferred response to a domestic disturbance. However, if the officer strongly believed that there was no reason to arrest someone, they could choose to do nothing. These laws were passed between 1977 and 2002 to cover those in intimate partnerships.[1]

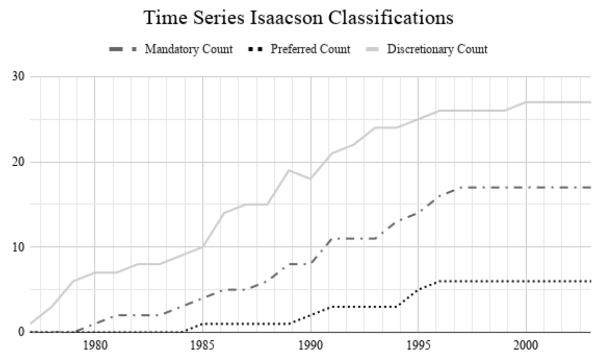
Figure 2 shows the number of states with each type of law over time- one of the graphs is based on the Chin and Cunningham classifications and the other is based on this paper's classifications, discussed later. Both graphs show a steady rise in the number of states in each category, with a few years showing a dip as states switched categories. The overall numbers across the classifications schemes differ, although the trends are identical. For example, in 2001, my classifications show 17 mandatory arrest law states, 6 preferred arrest law states, and 27 discretionary arrest law states. In the same year, Chin and Cunningham classify 22 mandatory arrest states, 6 preferred arrest states, and 21 discretionary arrest states.

The classification of state laws are one of the points of uncertainty in analyzing the impact of warrantless arrest for domestic violence. There is no set structure for a mandatory or preferred arrest law, and, as such, different states have varying degrees of officer discretion inside of their statutes. One of the most difficult portions of examining these laws is determining exactly which laws qualify as a mandatory arrest law. Which domestic relationships must be covered? Does a suspicion of the violation of an order of protection cause the same impact as one that requires merely the suspicion of a crime at all? Different researchers use a

[1] Typically, the legal definition of an intimate partner covers those who are married, were previously married, have a child together, or are common-law married. Often, a separate law would be passed to cover those in dating relationships.



(a) Chin and Cunningham Classification



(b) Isaacson Classification

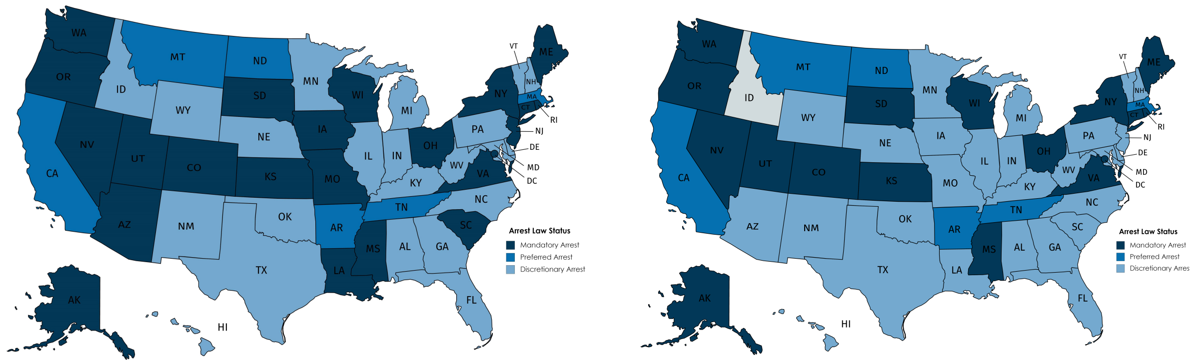
Figure 2: Time Series of the Number of States with a particular arrest law

different classification system. Iyengar identifies approximately 13 states and the District of Columbia as having mandatory arrest laws while Hirschel et al. find 23 states with mandatory arrest laws in 2007 (Iyengar 2009,D. Hirschel et al. 2007). As seen in figure 3, Chin and Cunningham find 22 mandatory states in 2014. This ambiguity can cause variation between studies.[2]

For this paper, I use information from J. D. Hirschel 2008 as well as conversations with legal experts to classify the state laws as of 2019.[3] Figure 3 summarizes that state classification used in Chin and Cunningham and those used in this paper. There are seven states that differ between their classifications for this paper and the work by Chin and Cunningham. They are Arizona, Idaho, Iowa, Louisiana, Missouri, New Jersey, and South Carolina. The primary difference stems from how to characterize arrest laws in states where the actions that mandate arrest are also crimes that would lead to arrest, even without suspicious of domestic violence. For instance, violating an order of protection should warrant arrest, regardless of the belief about the presence of domestic violence within the household. Chin and Cunningham tend to classify these types of laws as mandatory, whereas I classify them as discretionary. For example, Arizona, classified as a mandatory arrest law state in Chin

[2]Hirschel also identifies some police departments that establish their own departmental policy regarding domestic disturbances that strengthen state laws within a given jurisdiction.

[3]The classifications were informed by discussions with Prof. Rachel Harmon, a legal professor at the University of Virginia, and Jane Darst, a veteran prosecutor from Missouri.



(a) Chin and Cunningham (b) Isaacson

Figure 3: U.S. Maps Comparing Classifications

and Cunningham 2019, has a law that requires arrest after a physical injury or the use of a weapon in an argument or assault. If there is no evidence of physical injury or a weapon, then the arrest is up to the discretion of the officer in question. The classifications that differ between Chin and Cunningham and this paper are structured much like Arizona's law- arrest is discretionary, unless there are additional circumstances. [4]

What these laws highlight is the continuous nature of these classifications. Although many laws fall distinctly into one category or another, many fall somewhere in between the two. The law in Connecticut, which clearly belongs in the category of a mandatory arrest law, states that "Whenever a peace officer determines upon speedy information that a family violence crime has been committed within such officer's jurisdiction, such [an] officer shall arrest the person or persons suspected of its commission and charge such person or persons with the appropriate crime." (*2012 Connecticut General Statutes* 2020) The state of Missouri allows for discretionary arrest- however, after a decision to not arrest any of the involved parties, the officer must file paperwork stating that there was an incident and they decided not to arrest anyone. If there is another call to that address within 12 hours and the second officer believes that there has been another offense, then the law mandates arrest. There are clearly discretionary and mandatory portions of the law, and the state cannot be

[4] Idaho was categorized correctly as a discretionary arrest state between 1978 and 2017, the years contained in the data. However, the Idaho Supreme Court made a ruling that warrantless arrests of all types violate the state's constitution. Thus, the state's arrest law was vacated, and Idaho is unclassified as of 2018.

directly classified either way (*2015 Missouri Revised Statutes* 2020). The map of arrest law classifications in 2018 can be found in figure 3, while appendix B contains a table of the same information.

III Difference-in-Difference Analysis

III.1 Data

The data in this paper comes from Chin and Cunningham's 2019 paper on the same topic. State population data and characteristics come from the Current Population Survey, which includes data on race, socioeconomic status, and age groups. Multiple covariates are in the form of a rate of some demographic in the state population, but there were problems with the rate calculations. Appendix C details the problems and an attempt at a solution. Another covariate of interest is the status of universal divorce laws in each state. There is evidence that the presence of unilateral divorce laws in a state significantly reduces the incidence of domestic violence (Stevenson and Wolfers 2000). The researchers use two different papers to determine when a unilateral divorce law was passed- Friedberg 1998 and Gruber 2004. To quantify the impact of other types of violent crime on the incidence of intimate partner homicides, the paper uses the total number of violent crimes committed in a state in a given year from the FBI's Uniform Crime Report. They also use the presence of legal capital punishment as a potential explanation for changes in the number of homicides. The capital punishment status of a state in a given year comes from procon.org and their history of capital punishment status. Finally, the actual homicide data is drawn from the FBI's Supplemental Homicide Report, which gives data on the crime, the perpetrator, and the relationship between the victim and their killer.

The major variable of interest in this paper is the annual state level number of intimate partner homicides from 1977 to 2014, with information on the relationship between the victim and the offender. The average rate of intimate partner homicides per 100,000 people

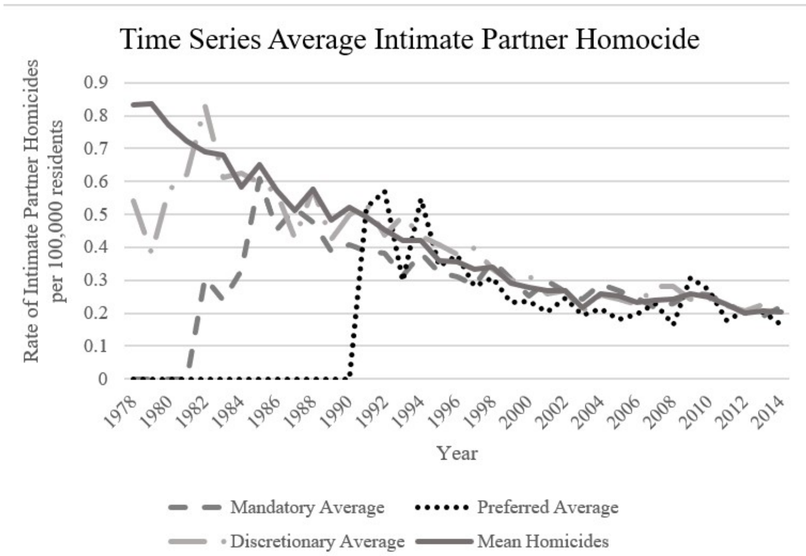


Figure 4: Intimate Partner Homicides per 100,000 People over Time and across Treatments

Covariate	Model 1	Model 2	Model 3	Model 4	Model 5
Unemployment Controls	No	Yes	Yes	Yes	Yes
Violent Crime Controls	No	Yes	Yes	Yes	Yes
Law-specific pre-trend	No	No	Yes	Yes	No
Group-specific trend	No	No	No	Yes	No
State-specific trend controls	No	No	No	No	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes

Table 1: Covariates by Regression

across states, categorized by the type of law that was passed in those states, can be seen in figure 4. Each dotted line represents that average rate for a different state category[5] while the solid line shows the mean across all states.

III.2 Results I

Replicating the regressions from Chin and Cunningham (2019) involves five different linear panel data regressions with state and year fixed effects. Each model includes a different

[5]I take the average rate of intimate partner homicides per 100,000 people across all states with the same categorization, given that more than one state has passed such a law.

C & C Classifications	Impact on Total Intimate Partner Homicides				
Variables	Model 1	Model 2	Model 3	Model 4	Model 5
Discretionary Arrest	-0.1249+	-0.1239+	-0.1777*	-0.1853+	-0.0102
Mandatory Arrest	-0.0436	-0.0452	-0.0766	-0.0605	0.0072
Preferred Arrest	0.0038	0.0059	-0.0966	-0.0651	0.0123
Observations	1,889	1,889	1,889	1,889	1,889
R-Squared	0.6666	0.6669	0.6722	0.6951	0.7995

** $p<0.01$, * $p<0.05$, + $p<0.1$

Table 2: Intimate Partner Chin and Cunningham Regression Results

set of covariates, which is described in table 1 with the response variable being the rate of a specific type of homicide per 100,000 residents. Their legal classifications can be seen in figure 3a. Using their classifications, data, and code, I was able to successfully replicate their results for the impact of intimate partner homicides on each relationship type (see appendix D for the full regression output). Chin and Cunningham focused on more granular results, divided by type of relationship, while Iyengar (2009) examined the impact of a law on the overall level of intimate partner homicides. Thus, following Iyengar, I created a new regression using intimate partner homicides per 100,000 residents as the response variable, with an intimate partner being defined as an ex-spouse, spouse, or common-law spouse. This new regression is repeated for Chin and Cunngingham's classification. These regression results are reported in table 2.

The next step was to rerun the same regression models using my classifications, which can be found in appendix B and figure 3b. The final set of regressions dropped all of the states with a questionable status, and reran the different regression models using only the easily classified states that were the same between Chin and Cunningham and my classifications. The three are referred to going forward as the Chin and Cunningham or CC regression, the Isaacson regression, and the dropped regression. The results from all models on the rate of homicides for an individual relationship type can be found in appendix D. The results of the intimate partner regression for Isaacson and Dropped regressions can be found in tables 3 and 4.

Isaacson Classifications	Impact on Total Intimate Partner Homicides				
Variables	Model 1	Model 2	Model 3	Model 4	Model 5
Discretionary Arrest	-0.1240*	-0.1233*	-0.1639*	-0.1489+	-0.0145
Mandatory arrest	-0.0187	-0.0208	-0.0677	-0.0608	0.0197
Preferred arrest	0.0066	0.0082	-0.0948	-0.0801	0.0139
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.6680	0.6683	0.6737	0.6961	0.7996

** $p < 0.01$, * $p < 0.05$, + $p < 0.1$

Table 3: Intimate Partner Isaacson Regression Results

Dropped Classifications	Impact on Total Intimate Partner Homicides				
Variables	Model 1	Model 2	Model 3	Model 4	Model 5
Discretionary arrest	-0.1163+	-0.1156+	-0.1760*	-0.1824	-0.0040
Mandatory arrest	0.0045	0.0030	0.0591	0.0751	0.0318
Preferred arrest	-0.0295	-0.0280	-0.1202	-0.0834	0.0001
Observations	1,624	1,624	1,624	1,624	1,624
R-squared	0.6435	0.6436	0.6504	0.6782	0.7705

** $p<0.01$, * $p<0.05$, + $p<0.1$

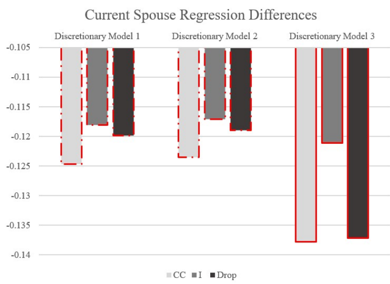
Table 4: Intimate Partner Dropped Regression Results

With varying levels of significance, all three sets of regressions showed a statistically significant decrease in the level of intimate partner homicide in states that passed a discretionary arrest law. For example, all three specifications find a coefficient on the discretionary law that is statistically significant and negative in model 1. The value from Chin and Cunningham is about -0.125, while the Isaacson coefficient is -0.124 and Dropped is -0.116. This means that, for all sets of specifications, the passage of a discretionary arrest law in a state leads to a decrease in the rate of intimate partner homicides per 100,000 people of about 0.12. While small in magnitude, the average level of intimate partner homicides per capita over this period is 0.419. Thus, a decrease of 0.12 represents a 28.6% decrease on average. A fall in the rate of intimate partner homicides from a discretionary law concurs with the overall findings from Chin and Cunningham. Neither the mandatory nor the preferred arrest laws have a statistically significant impact on homicide rates.

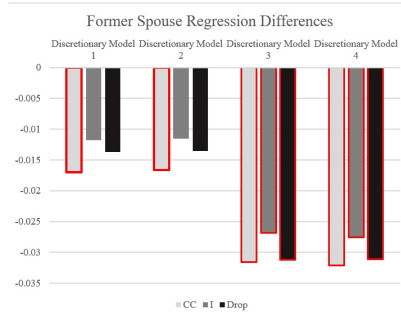
The complete replication of Chin and Cunningham's work as well as the full regression output of the Isaacson and Dropped regressions can be found in appendix D. This includes the values of every coefficient on the mandatory, discretionary, and preferred arrest laws for all models and all classifications. Figure 5 seeks to summarize the important differences between the regressions. Each of the bar graphs represents a different set of variables, either current spouse, former spouse, common-law spouse, or the overall intimate partner results. There is no bar graph for the changes in the dating relationship coefficients, as none of the coefficients was significant in any of the three sets of states. The bar graphs only include the coefficients that were statistically significant in at least one of the three regressions. The outline of the bar demonstrates the level of significance- a solid outline is significant at the 10% level, a dot-dash outline is significant at the 5% level, and a dotted outline is significant at the 1% level.

The current spouse results show that the discretionary arrest law has a negative impact on the rate of intimate partner homicide. Each of the different classifications has a slightly different value of the coefficient, but overall, the results are similar. They are similarly significant with the same conclusion across all sets of results. The former spouse results are less strong than the current spouse results. The third and fourth model, which take fixed effects, violent crime controls, unemployment controls, and law-specific pre-trends into account in their results, have significant results across all three classifications. Discretionary laws once again have a negative impact on intimate partner homicides. However, these results are only significant at the 10% level. The first two models have significant results in the Chin and Cunningham classifications, but not for the classifications developed in this paper.

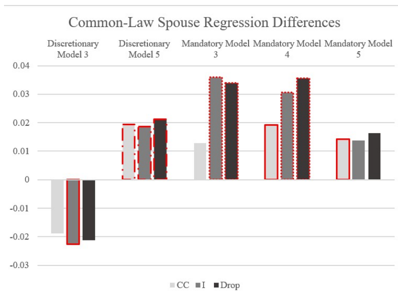
The common-law spouse regressions produce a very odd series of results, contrasting with the overall trends from other categories. For a discretionary law in model five and for a mandatory law in models three, four, and five, the impact of that law on common-law spouse homicides was positive and significant. The other categories had some positive coefficients, but none of them were statistically significant. The discretionary law causes a roughly 0.02 increase in the level of intimate partner homicides per capita, at the 5% level. The mandatory



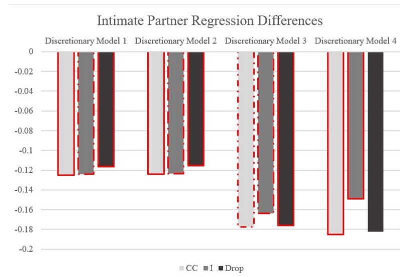
(a) Current Spouse



(b) Former Spouse



(c) Common-Law Spouse



(d) Intimate Partner

Figure 5: Spousal Regression Differences

laws also record statistically significant increases across the last three models. The intimate partner results are fairly consistent, but the impact is larger than the individual categories. The intimate partner regressions record a relatively large decrease in the rate of homicides per capita under a discretionary arrest law.

Overall, the regressions above suggest very similar conclusions about the impact of arrest laws in domestic violence situations. The classifications across states may vary, but, in general, discretionary arrest laws decrease the rate of intimate partner homicides in a way that is both statistically significant and substantial in impact. However, the Minneapolis Domestic Violence Experiment also found a significant impact of arrest on the rate of domestic violence. Follow-up experiments in other cities could not replicate the same result. Thus, there is a case to be made that geographic differences in terms of demographics, details of the legal statute, and in terms of police department policies could impact the way a city or a state responds to the introduction of an arrest law. In the next section, I turn my attention to a partial identification procedure aimed at teasing out the differential impact of arrest laws across different states and different years.

IV Partial Identification

IV.1 Motivation

As subsection III.2 makes clear, measuring the impact of a mandatory or a discretionary arrest law is difficult. The impact of a law is sensitive to state classifications and possibly time trends, which points towards the use of a partial identification strategies. To do this, I have used a modeling approach similar to Manski and Pepper in their 2018 paper about Right-To-Carry Laws as well as Miller and Pepper in their forthcoming 2020 paper (Manski and Pepper 2018, M. Miller and Pepper 2020). The partial identification approach is arguably appropriate for questions that can be impacted by a large number of factors that are impossible to hold constant. Clearly, domestic violence and intimate partner homicides fall into this category- domestic violence levels are impacted by everything from male employment to the number of female police officers in a given police department (Witte and Tauchen 1993, A. R. Miller and Segal 2018). There is also evidence from the Minneapolis Domestic Violence Experiment and the Spouse Assault Replication Program that the impact of a similar policy in a different state and a different time can produce a very different impact on domestic violence. The bounded-variation approach in this paper bounds the treatment effect of a law for a given state in a given year, allowing the treatment effect to vary over time and by state. For example, Pennsylvania passed their mandatory arrest law in 1986, and I estimate a bound on the impact of the law in 1987, the year after it was passed.

IV.2 Partial Identification

When a state passes a mandatory arrest law, the rate of intimate partner homicides changes, but by how much? The treatment effect comes from the solution to a simple equation, in which the rate of intimate partner homicide is Y and the treatment X equals

Year	Pennsylvania	New York
1985	0.3313	0.3710
1987	0.2794	0.3190

Table 5: Murder Rate in Pennsylvania and New York Before and After the law change

1 with the law and 0 without it. Consider Pennsylvania in 1987. The treatment effect of the mandatory law is the rate of intimate partner homicides in Pennsylvania in 1987, given that the law was passed, minus the rate of intimate partner homicides in Pennsylvania in the same year without the discretionary arrest law.

$$TE = [Y_{Pennsylvania,1987} \mid X = 1] - [Y_{Pennsylvania,1987} \mid X = 0]$$

As seen in table 5, $[Y_{PE,1987} \mid X = 1]$ equals 0.2794, but the counterfactual where the law has not been passed in Pennsylvania ($[Y_{Pennsylvania,1987} \mid X = 0]$) cannot be known. To estimate the counterfactual, researchers use different invariance assumptions. The first is an inter-state invariance, in which Pennsylvania would have had an identical murder rate to New York in 1987, were it not for the mandatory arrest law. The second assumption is inter-temporal invariance, in which Pennsylvania would have the same murder rate in 1987 as in 1985 without the law. Finally, the difference-in-differences invariance assumption states that the difference between Pennsylvania in 1985 and 1987 would be the same as the difference between New York in 1985 and 1987 without the law change. Thus, the treatment effect for each invariance assumption would be

$$TE_{inter-state} = [Y_{PE,87} \mid X = 1] - [Y_{NY,87} \mid X = 0] = -0.0396 \tag{1}$$

$$TE_{inter-temporal} = [Y_{PE,87} \mid X = 1] - [Y_{PE,85} \mid X = 0] = -0.0519 \tag{2}$$

$$TE_{DID} = ([Y_{PE,87} \mid X = 1] - [Y_{PE,85} \mid X = 0]) - ([Y_{NY,87} \mid X = 0] - [Y_{NY,85} \mid X = 0]) = 0 \quad (3)$$

As seen in equations 1, 2, and 3, different assumptions lead to different magnitudes, with at least one being approximately zero.

While these assumptions are commonly applied, there may be reasons to question their validity. In particular, the three assumptions do not hold up within the years before Pennsylvania adopted a mandatory arrest law in 1987- at no point between 1977 and 1986 does one of the strict invariance assumptions hold. For example, in 1978, the difference between the year 1978 and 1977 is 0.060, meaning the time invariance assumption does not hold. Then, the difference between Pennsylvania and New York is 0.189 intimate partner homicides per capita, while the DID value is about 0.197. Thus, every version of invariance is not applicable in 1978. So, instead of choosing one strict assumption, I apply the bounded variation assumption in Manski Pepper (2018). The idea is to allow the counterfactual moment to vary within a a given δ. The equations for the treatment effects for each assumption are then

$$\mid [Y_{PE,87} \mid X = 0] - [Y_{NY,87} \mid X = 0] \mid \leq \delta_{(PE-NY),87} \quad (4)$$

$$\mid [Y_{PE,87} \mid X = 0] - [Y_{PE,85} \mid X = 0] \mid \leq \delta_{PE,(85,87)} \quad (5)$$

$$\mid ([Y_{PE,87} \mid X = 1] - [Y_{PE,85} \mid X = 0]) - ([Y_{NY,87} \mid X = 0] - [Y_{NY,85} \mid X = 0]) \mid \leq \delta_{(PE,NY),(85,87)} \quad (6)$$

Then, using bounded-variation assumptions, we can trace out the potential bounds on the treatment effect for different values of delta to determine which ones yield bounds that

Table 6: Sensitivity Bounds for Pennsylvania and New York

Time Variance —

δ	LB	UB
0	-0.052	-0.052
0.01	-0.062	-0.042
0.02	-0.072	-0.032
0.03	-0.082	-0.022
0.04	-0.092	-0.012
0.05	-0.102	-0.002
0.06	-0.112	0.008
0.07	-0.122	0.018

State Variance

δ	LB	UB
0.00	-0.040	-0.040
0.01	-0.050	-0.030
0.02	-0.060	-0.020
0.03	-0.070	-0.010
0.04	-0.080	0.000
0.05	-0.090	0.010
0.06	-0.100	0.020
0.07	-0.110	0.030

DID Variance

δ	LB	UB
0	0.000	0.000
0.01	-0.010	0.010
0.02	-0.020	0.020
0.03	-0.030	0.030
0.04	-0.040	0.040
0.05	-0.050	0.050
0.06	-0.060	0.060
0.07	-0.070	0.070

do not contain zero. This analysis does not identify a delta that is accurate- rather, the sensitivity analysis provides an idea on the values of delta that would identify the sign of the effect. For Pennsylvania, the potential bounds on the treatment effect in 1987 for each assumption are found in table 6.

A visual representation of the bounds over time can be found in figure 6. The dotted line shows the time bounds, the dashed line shows the state bounds, and the solid line shows the DID bounds. At a delta of 0.01, DID bounds include 0, while state bounds include 0 at 0.04. Finally, at a delta of 0.05, the time bounds include 0. Then, the different assumptions can be combined to create narrow bounds. This involves taking the maximum of the lower bounds and the minimum of the upper bounds, which creates the tightest possible range on the treatment effect. For Pennsylvania, the upper bound would be the time-invariance bound and the lower bound would be the DID-invariance. This leads to the following bound of [-7.0E-02, 0.01808] for a $\delta = 0.07$, which still includes 0. This sensitivity analysis can be

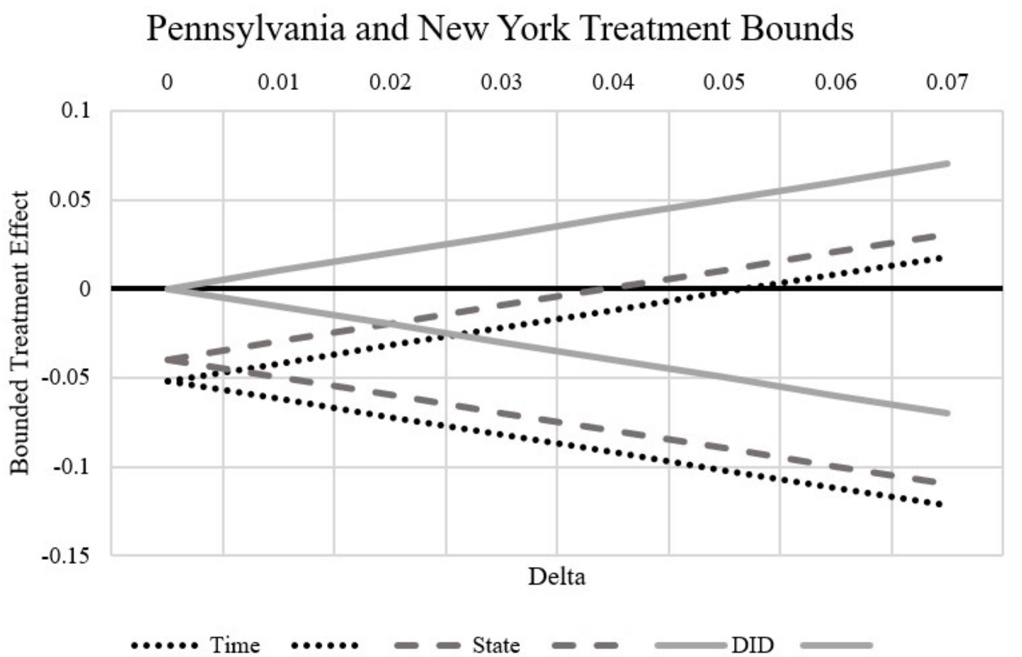


Figure 6: Pennsylvania Upper and Lower Bounds

repeated for different states, all of which yield a different set of bounds.

IV.3 Results II

The states being analyzed here include Pennsylvania, Michigan, Maryland, and Nevada. As part of the inter-state and DID invariance assumptions, I chose four other states for comparison- New York, Indiana, Virginia, and California. Table 7 summarizes the states and the types of laws they passed. The states were chosen for the years they passed laws, the type of law they passed, and the clarity of their laws. These eight states were classified identically across the Chin and Cunningham and Isaacson classifications. Michigan and Maryland each passed discretionary laws, in 1986 and 1978 respectively. The bounded-variation assumptions create a bound for the treatment effect of a discretionary law compared to no law at all in Michigan and in Maryland. Thus, the pairs of Pennsylvania and New York, and California and Nevada can be used to identify the treatment effect of a mandatory arrest law versus nothing. Table 8 features the before-and-after tables for each pair of states, including the

State	Type of Law Passed	Year Passed
Maryland	Discretionary	1986
Michigan	Discretionary	1978
Nevada	Mandatory	1985
Pennsylvania	Mandatory	1986

Table 7: Legal Details for Partial ID States

Discretionary Arrest

Year	Michigan	Indiana
1977	0.9501	0.6845
1979	0.7136	0.8950

Mandatory Arrest

Year	Pennsylvania	New York
1985	0.3313	0.3709
1987	0.2794	0.3190

Discretionary Arrest

Year	Maryland	Virginia
1985	0.3172	0.9448
1987	0.3285	0.7080

Mandatory Arrest

Year	Nevada	California
1984	1.1893	0.6965
1986	0.4079	0.6162

Table 8: State by State Intimate Partner Homicide Data

Pennsylvania-New York data from table 5 above.

Following Manski and Pepper (2018), I select a plausible delta for each pair. The first step is to find the deltas for each year where the states had the same classifications. For example, with Michigan and Indiana, it is necessary to calculate the difference between the states, the difference between Michigan in one year and Michigan in another, and the difference-in-differences for each year that both had no law and both had a discretionary law. Then, we can find both the average, the median, and the maximum delta for each variation assumptions over the appropriate period. Using the median deltas displayed in table 9, tables 10 and 11 show the bounds on the treatment effect for different states under

Median Delta Values

State Pair	Time	State	DID
Michigan	0.05013	0.03641	0.07504
Maryland	0.10778	0.30676	0.27657
Pennsylvania	0.05602	0.07380	0.03910
Nevada	0.08002	0.70688	0.16224

Table 9: Median Delta Values used in Analysis

Michigan in 1978

Category	Lower Bound	Upper Bound
Time	-0.2845	-0.1884
State	-0.2491	-0.1137
DID	-0.5287	-0.3651
Overall	-0.2491	-0.3651

Maryland in 1986

Category	Lower Bound	Upper Bound
Time	-0.1238	0.1464
State	-0.6885	-0.0704
DID	-0.0465	0.5428
Overall	-0.0465	-0.0704

Table 10: Impact of Discretionary Arrest Laws

different assumptions.[6]

Table 10 shows bounds on the treatment effect of a discretionary arrest law in Maryland and Michigan.[7] To focus on Michigan, each bounded variation assumption yields a bound that does not include zero, meaning the joint assumptions are not necessary to identify the sign of the effect. The time-variation assumption bounds the treatment effect between [-0.2845, -0.1884], while the state bounds are [-0.2491,-0.1137] and the DID bounds are [-0.5287, -0.3651]. Overlaying bounds can more narrowly identify the impact, but all three bounds are not required. Maryland, in contrast, has some negative bounds and some positive bounds, meaning that a joint model is the only way to potentially identify the sign of the effect. However, the overlapping bounds created by all three assumptions lead to infeasible bounds, where the upper bound is below the lower bound, for both states. If all three bounds cannot be combined to create a real-valued set of results, then at least one of the bounded variation assumptions cannot be valid. Then, the next step is to select which of the bounds, given the particular delta values for that state, is more likely. The process of selecting reasonable assumptions is done in creating the final treatment effect bounds in table 12.

Table 11 shows the partially identified effect of a mandatory arrest law in the different pairs of states. The DID model for Pennsylvania and New York estimates a bound on the impact of the mandatory arrest law, given a difference-in-difference value for 1985 and 1987 of [-0.039, 0.039]. The bounds still include zero. However, the impact is certainly not large

[6]The median was used to resist the effect of a single outlying year on the overall value of the delta. In general, the medians and the means were close with means being slightly above the median.

[7]The overall bounds were chosen by taken the minimum of all of the upper bounds and the maximum of all of the lower bounds.

Pennsylvania in 1986

Category	Lower Bound	Upper Bound
Time	-0.1079	0.0041
State	-0.1134	0.0342
DID	-0.0391	0.0391
Overall	-0.0391	0.0041

Nevada in 1985

Category	Lower Bound	Upper Bound
Time	-0.8614	-0.7014
State	-0.9152	0.4986
DID	-0.8633	-0.5389
Overall	-0.8614	-0.7014

Table 11: Impact of Mandatory Arrest Laws 1 year Post-Law Change

and positive as suggested by Iyengar. In contrast, the results from Nevada and California suggest a large negative impact from a mandatory arrest law in 1986.

State	Lower Bound	Upper Bound
Michigan	-0.2491	-0.1884
Maryland	-0.0465	0.1464
Pennsylvania	-0.0391	0.0041
Nevada	-0.8614	-0.7014

Table 12: Final Results for Partial Identification

The partially identified results in tables 10 and 11 from mandatory and discretionary arrest laws show that the same law can have a different impact in one state than in another, and that different assumptions can yield different bounds. Table 12 demonstrates the final results of overlaying the disparate bounds from previous tables. For Maryland and Michigan, creating a set of feasible bounds requires picking which of the different assumptions is more appropriate in the given year. Based on the maximum and standard deviation of the deltas from the sensitivity analysis, I chose to eliminate the inter-state bounded variation assumption for Maryland and the DID bounded variation assumption for Michigan. Each of those delta values over time were more varied and larger than their counterparts, which made them less likely to hold. After overlaying the DID bounds and the intertemporal invariance assumption for Maryland, the results showed a bound on the treatment effect of [-0.0465, 0.1464]. These results provide an inconclusive impact on the rate of intimate partner homicides in 1986. In contrast, the bounds of the same process for Michigan show that the treatment effect ranges between [-0.2491, -0.1884]. Thus, the discretionary arrest law passed in Michigan caused a decrease in the rate of intimate partner homicides, and the decrease ranges between 26% and 19.8% compared to the same value in 1985.

The mandatory arrest law results for Pennsylvania and Nevada show similarly mixed messages. The overlap of the bounds on the treatment effect from all three assumptions created feasible bounds, which are found in table 12. The results from Pennsylvania bound the treatment effect on their mandatory arrest law between [-0.0391, 0.0041]. These bounds indicate a small impact of the law on the homicide rate, which aligns with some previous results. On the other hand, Nevada has a bound of [-0.8614, -0.7014], which is large, negative, and does not include zero. Compared to the year before the law was passed, this bound represents a decrease in the intimate partner homicide rate between 59% and 72%. In contrast, previous research found that mandatory arrests laws had little impact or caused an increase in the homicide rate, both of which do not hold in Nevada in 1986.

When all states with a given law are aggregated, the different state-by-state and year-to-year impacts can be disguised. Nevada benefited from the mandatory arrest law it passed in 1986. In contrast, the impact of the same law in Pennsylvania in 1986 cannot be exactly determined, but the impact would be small no matter what. The discretionary arrest laws have the same varied impact- such a law in Maryland may have done absolutely nothing. Michigan saw an impressive decrease in their rate of intimate partner homicides per 100,000 people. The magnitude of this coefficient is larger than the average impact estimated in the linear regression models above. The sensitivity analysis as well as empirical evidence from experiments in different cities show that inter-state assumptions are often overly strong and can identify an impact that may not exist. Based on assumed similarity across states before a law is passed, Chin and Cunningham found that, generally, discretionary arrest laws caused a small decrease in the level of intimate partner homicides, with a decrease in overall intimate partner homicides of about 0.125. This paper's first analysis found the same. However, the partial identification analysis suggests a more variable impact across states, with both mandatory and discretionary arrest laws having an impact in some states.

V Limitations and Future Work

Although the results above imply either no impact or a negative impact from these laws on intimate partner homicides, there is still more to be analyzed and learned about the overall impact of these laws. Firstly, the given bounds only apply for the given years and the given laws. They do not estimate the overall treatment effect of the law.The same law can have a different impact depending on how individuals and police departments react and analyzing the impact on a state-by-state basis could offer better insight. Analyzing the results in a particular state could also circumvent the issues with discrete classifications. Instead of grouping together laws that may be written differently, one could analyze the impact of that particular law in that particular state. In the future, further granularity towards the legal classifications and towards the geographic boundaries could offer more insight.

It is also important to recognize the limitations of this analysis. The Violence Against Women Act of 1994 offered funding to police departments to combat domestic violence. However, funding would only be given to those police departments with a mandatory arrest policy towards domestic abusers. This led to several states and several local municipalities passing their own arrest laws. A police department could not lower the severity of their state's law, but they could introduce a tighter policy. Given this, many discretionary arrest states could have large municipalities with a different policy than the rest of the state. In the future, it would be important to analyze the impact a law has on a more local level or at least include the percentage of the population living in a police department with a different policy than the overall state as a potential covariate within the regression.

VI Conclusion

Domestic violence remains a large and lethal problem across the United States, but the different types of arrest laws represented one of the first steps towards taking it seriously. Mandatory, discretionary, and preferred arrest laws were followed by different mandatory measures and an expansion of the definition of an intimate partner. The results in this paper suggest that, in some states, these laws do have the desired impact. They have brought down the rate of intimate partner homicides, which is a benefit to all. For other states, there may be no impact at all- they may need to look towards different measures in order to bring down their own rates. This represents a step forward in the analysis of the impact, but there needs to be more granular research than what is currently available. Providing policymakers with more nuanced views of the impact a law can have can lead to better policies, better policing, and better protection for victims of domestic violence.

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A Thurman v. City of Torrington

In Torrington, Connecticut, Tracey Thurman was being harassed and stalked by her estranged husband, Charles, and continually asked the local police department for help. Each time she called to have her husband arrested or removed, Thurman was told to call back or simply refused. However, in 1983, Charles brutally attacked Thurman and their son, Charles Jr., in full view of the Torrington police. He was arrested after stabbing, kicking, and threatening his wife. Thurman, after this incident, sued the Torrington police department and the city of Torrington for failing to afford her equal protection as a victim of domestic violence. Her claim was that a victim of similar abuse under a different circumstance would not have been turned away so frequently without protection or some offer of help. The court agreed with Ms. Thurman and awarded her 2.3 million dollars for her treatment and for the failure to protect her under equal protection laws.

B Table of Classifications

Table 13: Classification of Warrantless Domestic Violence Laws by State

State	Classification	Chin and Cunningham Classification	Year
Alabama	Discretionary	Discretionary	1989
Alaska	Mandatory	Mandatory	1996
Arizona	Mandatory	Discretionary	1991
Arkansas	Preferred	Preferred	1991
California	Preferred	Preferred	1996
Colorado	Mandatory	Mandatory	1994
Connecticut	Mandatory	Mandatory	1986
DC	Mandatory	Mandatory	1991

Continued on next page

Table 13 – *Continued from previous page*

State	Classification	Chin and Cunningham Classification	Year
Delaware	Discretionary	Discretionary	1984
Florida	Discretionary	Discretionary	1992
Georgia	Discretionary	Discretionary	1981
Hawaii	Discretionary	Discretionary	1973
Idaho	Discretionary	None (2018)	1979
Illinois	Discretionary	Discretionary	1993
Indiana	Discretionary	Discretionary	2000
Iowa	Mandatory	Discretionary	1986
Kansas	Mandatory	Mandatory	1991
Kentucky	Discretionary	Discretionary	1980
Louisiana	Mandatory	Discretionary	1985
Maine	Mandatory	Mandatory	1980
Maryland	Discretionary	Discretionary	1986
Massachusetts	Preferred	Preferred	1991
Michigan	Discretionary	Discretionary	1978
Minnesota	Discretionary	Discretionary	1978
Mississippi	Mandatory	Mandatory	1995
Missouri	Mandatory	Discretionary	1989
Montana	Preferred	Preferred	1985
Nebraska	Discretionary	Discretionary	1989
Nevada	Mandatory	Mandatory	1985
New Hampshire	Discretionary	Discretionary	1979
New Jersey	Mandatory	Discretionary	1991

Continued on next page

Table 13 – *Continued from previous page*

State	Classification	Chin and Cunningham Classification	Year
New Mexico	Discretionary	Discretionary	1987
New York	Mandatory	Mandatory	1996
North Carolina	Discretionary	Discretionary	1991
North Dakota	Preferred	Preferred	1995
Ohio	Mandatory	Mandatory	1994
Oklahoma	Discretionary	Discretionary	1986
Oregon	Mandatory	Mandatory	1981
Pennsylvania	Discretionary	Discretionary	1986
Rhode Island	Mandatory	Mandatory	1988
South Carolina	Mandatory	Discretionary	1995
South Dakota	Mandatory	Mandatory	1989
Tennessee	Preferred	Preferred	1995
Texas	Discretionary	Discretionary	1989
Utah	Mandatory	Mandatory	1991
Vermont	Discretionary	Discretionary	1985
Virginia	Mandatory	Mandatory	1997
Washington	Mandatory	Mandatory	1984
West Virginia	Discretionary	Discretionary	1993
Wisconsin	Mandatory	Mandatory	1989
Wyoming	Discretionary	Discretionary	1982

C Data Issues

In the process of replicating the results of the 2019 Chin and Cunningham paper, the mean and standard deviation of the racial rate and age rate covariates were extremely low (less than 0.01). The rate of a given rate in the population was pulled from the Current Population Survey Data. The white_rate variable was created by summing all of the individuals who marked themselves as white on the survey- the black_rate was done similarly. Anyone who marked themselves as mixed-race or a race that was not white or black was grouped into the other category. The same summing procedure was replicated for those between the ages of 0 and 17, 18 and 39, 40 and 59, and over 60. The code for creating the covariate data file showed that this sum was then divided by the overall state population. The sums should have been divided by the number of people participating in the survey, and then potentially adjusted for weighting issues in the CPS. Changing this leads to a different set of means and standard deviations that are much larger than those used in the analysis. I then replicated the analyses and the robustness checks using new rates, the means and standard deviations of which can be found in table 14. The robustness checks remained quite similar, and it did not warrant a complete re-do of the following analysis. However, in the future, further work examining the issue should be completed.

Variable	Original Mean	Original SD	New Mean	New SD
black_rate	0.000107	0.000272	0.1075716	0.1219002
white_rate	0.00102	0.00984	0.8343586	0.1453428
other_rate	9.51e-0.5	0.00249	0.0580698	0.1031625
age017_rate	0.000356	0.00342	0.2877789	0.0293995
age1839_rate	0.000389	0.000368	0.3191567	0.0362442
age4059_rate	0.000294	0.000293	0.2392173	0.0356975
age60plus_rate	-.00181	0.000153	0.153847	0.0276294

Table 14: Mean and Standard Deviation for all impacted rates

D Results I Full Regression Output

Chin and Cunningham Current Spouse Regression Results

VARIABLES	(1) totcshpc	(2) totcshpc	(3) totcshpc	(4) totcshpc	(5) totcshpc
Discretionary arrest	-0.1247*	-0.1235*	-0.1378+	-0.1553	-0.0407
	(0.0578)	(0.0572)	(0.0778)	(0.0928)	(0.0496)
Mandatory arrest	-0.0455	-0.0483	-0.0664	-0.0649	-0.0118
	(0.0417)	(0.0416)	(0.0559)	(0.0525)	(0.0263)
Preferred arrest	-0.0139	-0.0105	-0.1015	-0.0959	-0.0097
	(0.0601)	(0.0620)	(0.0904)	(0.0973)	(0.0448)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.6328	0.6342	0.6385	0.6594	0.7487

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Isaacson Current Spouse Regression Results

VARIABLES	(1) totcshpc	(2) totcshpc	(3) totcshpc	(4) totcshpc	(5) totcshpc
Discretionary arrest	-0.1181*	-0.1171*	-0.1211+	-0.1205	-0.0400
	(0.0493)	(0.0489)	(0.0678)	(0.0753)	(0.0411)
Mandatory arrest	-0.0274	-0.0314	-0.0662	-0.0675	-0.0030
	(0.0452)	(0.0448)	(0.0632)	(0.0608)	(0.0286)
Preferred arrest	-0.0117	-0.0088	-0.0994	-0.1021	-0.0083
	(0.0611)	(0.0628)	(0.0921)	(0.0988)	(0.0449)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.6338	0.6350	0.6397	0.6608	0.7488

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Current Spouse Dropped Regression Results

VARIABLES	(1) totcshpc	(2) totcshpc	(3) totcshpc	(4) totcshpc	(5) totcshpc
Discretionary arrest	-0.1199*	-0.1190*	-0.1372+	-0.1523	-0.0393
	(0.0572)	(0.0568)	(0.0805)	(0.0940)	(0.0501)
Mandatory arrest	-0.0117	-0.0152	0.0207	0.0226	0.0024
	(0.0474)	(0.0478)	(0.0584)	(0.0534)	(0.0303)
Preferred arrest	-0.0382	-0.0352	-0.1201	-0.1063	-0.0205
	(0.0628)	(0.0638)	(0.0957)	(0.1043)	(0.0492)
Observations	1,624	1,624	1,624	1,624	1,624
R-squared	0.6106	0.6115	0.6166	0.6416	0.7195

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Chin and Cunningham Former Spouse Regression Results

VARIABLES	(1) totfshpc	(2) totfshpc	(3) totfshpc	(4) totfshpc	(5) totfshpc
Discretionary arrest	-0.0170*	-0.0166*	-0.0316*	-0.0321*	-0.0063
	(0.0083)	(0.0083)	(0.0122)	(0.0145)	(0.0074)
Mandatory arrest	0.0030	0.0025	-0.0085	-0.0063	0.0046
	(0.0052)	(0.0050)	(0.0104)	(0.0106)	(0.0060)
Preferred arrest	-0.0046	-0.0039	-0.0270	-0.0231	-0.0021
	(0.0113)	(0.0111)	(0.0291)	(0.0299)	(0.0118)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.2864	0.2885	0.2985	0.3097	0.3692

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Isaacson Former Spouse Regression Results

VARIABLES	(1) totfshpc	(2) totfshpc	(3) totfshpc	(4) totfshpc	(5) totfshpc
Discretionary arrest	-0.0118	-0.0115	-0.0268*	-0.0275*	-0.0054
	(0.0070)	(0.0070)	(0.0105)	(0.0121)	(0.0062)
Mandatory arrest	0.0029	0.0021	-0.0091	-0.0057	0.0070
	(0.0058)	(0.0057)	(0.0127)	(0.0131)	(0.0073)
Preferred arrest	-0.0045	-0.0038	-0.0266	-0.0210	-0.0016
	(0.0114)	(0.0112)	(0.0292)	(0.0300)	(0.0119)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.2829	0.2852	0.2953	0.3085	0.3694

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Former Spouse Dropped Regression Results

VARIABLES	(1) totfshpc	(2) totfshpc	(3) totfshpc	(4) totfshpc	(5) totfshpc
Discretionary arrest	-0.0137	-0.0135	-0.0312*	-0.0311*	-0.0053
	(0.0084)	(0.0084)	(0.0124)	(0.0145)	(0.0075)
Mandatory arrest	0.0083	0.0072	0.0063	0.0116	0.0093
	(0.0059)	(0.0057)	(0.0142)	(0.0133)	(0.0079)
Preferred arrest	-0.0069	-0.0061	-0.0338	-0.0277	-0.0032
	(0.0121)	(0.0118)	(0.0323)	(0.0329)	(0.0134)
Observations	1,624	1,624	1,624	1,624	1,624
R-squared	0.2896	0.2915	0.3035	0.3164	0.3711

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Chin and Cunningham Common-Law Spouse Regression Results

VARIABLES	(1) totclhpc	(2) totclhpc	(3) totclhpc	(4) totclhpc	(5) totclhpc
Discretionary arrest	-0.0023	-0.0018	-0.0189	-0.0112	0.0194*
	(0.0160)	(0.0155)	(0.0133)	(0.0188)	(0.0096)
Mandatory arrest	-0.0021	0.0003	0.0129	0.0192+	0.0142+
	(0.0117)	(0.0117)	(0.0152)	(0.0109)	(0.0084)
Preferred arrest	0.0177	0.0162	0.0149	0.0271	0.0123
	(0.0168)	(0.0156)	(0.0179)	(0.0197)	(0.0080)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.4762	0.4827	0.4876	0.5290	0.6517

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Isaacson Common-Law Spouse Regression Results

VARIABLES	(1) totclhpc	(2) totclhpc	(3) totclhpc	(4) totclhpc	(5) totclhpc
Discretionary arrest	-0.0074	-0.0070	-0.0226+	-0.0128	0.0186*
	(0.0124)	(0.0121)	(0.0119)	(0.0143)	(0.0078)
Mandatory arrest	0.0060	0.0095	0.0359**	0.0307**	0.0137
	(0.0144)	(0.0143)	(0.0106)	(0.0090)	(0.0108)
Preferred arrest	0.0177	0.0161	0.0144	0.0156	0.0123
	(0.0166)	(0.0154)	(0.0180)	(0.0178)	(0.0080)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.4774	0.4845	0.4896	0.5390	0.6517

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Common-Law Spouse Dropped Regression Results

VARIABLES	(1) totclhpc	(2) totclhpc	(3) totclhpc	(4) totclhpc	(5) totclhpc
Discretionary arrest	-0.0047	-0.0041	-0.0212	-0.0129	0.0212*
	(0.0150)	(0.0147)	(0.0127)	(0.0196)	(0.0095)
Mandatory arrest	0.0031	0.0066	0.0340**	0.0355**	0.0163
	(0.0136)	(0.0137)	(0.0104)	(0.0101)	(0.0105)
Preferred arrest	0.0078	0.0062	0.0101	0.0208	0.0125
	(0.0166)	(0.0150)	(0.0215)	(0.0223)	(0.0095)
Observations	1,624	1,624	1,624	1,624	1,624
R-squared	0.4271	0.4344	0.4425	0.4982	0.5921

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Chin and Cunningham Intimate Partner Relationship Regression Results

VARIABLES	(1) totiphpc	(2) totiphpc	(3) totiphpc	(4) totiphpc	(5) totiphpc
Discretionary arrest	-0.1249+	-0.1239+	-0.1777*	-0.1853+	-0.0102
	(0.0635)	(0.0634)	(0.0812)	(0.1071)	(0.0472)
Mandatory arrest	-0.0436	-0.0452	-0.0766	-0.0605	0.0072
	(0.0488)	(0.0487)	(0.0739)	(0.0734)	(0.0308)
Preferred arrest	0.0038	0.0059	-0.0966	-0.0651	0.0123
	(0.0754)	(0.0768)	(0.1273)	(0.1374)	(0.0624)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.6666	0.6669	0.6722	0.6951	0.7995

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Isaacson Intimate Partner Regression Results

VARIABLES	(1) totiphpc	(2) totiphpc	(3) totiphpc	(4) totiphpc	(5) totiphpc
Discretionary arrest	-0.1240*	-0.1233*	-0.1639*	-0.1489+	-0.0145
	(0.0539)	(0.0538)	(0.0713)	(0.0881)	(0.0407)
Mandatory arrest	-0.0187	-0.0208	-0.0677	-0.0608	0.0197
	(0.0542)	(0.0543)	(0.0877)	(0.0871)	(0.0327)
Preferred arrest	0.0066	0.0082	-0.0948	-0.0801	0.0139
	(0.0767)	(0.0778)	(0.1292)	(0.1382)	(0.0627)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.6680	0.6683	0.6737	0.6961	0.7996

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Intimate Partner Dropped Regression Results

VARIABLES	(1) totiphpc	(2) totiphpc	(3) totiphpc	(4) totiphpc	(5) totiphpc
Discretionary arrest	-0.1163+	-0.1156+	-0.1760*	-0.1824	-0.0040
	(0.0605)	(0.0605)	(0.0849)	(0.1100)	(0.0471)
Mandatory arrest	0.0045	0.0030	0.0591	0.0751	0.0318
	(0.0551)	(0.0560)	(0.0725)	(0.0681)	(0.0348)
Preferred arrest	-0.0295	-0.0280	-0.1202	-0.0834	0.0001
	(0.0785)	(0.0789)	(0.1400)	(0.1497)	(0.0703)
Observations	1,624	1,624	1,624	1,624	1,624
R-squared	0.6435	0.6436	0.6504	0.6782	0.7705

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Chin and Cunningham Dating Relationship Regression Results

VARIABLES	(1) totdhpc	(2) totdhpc	(3) totdhpc	(4) totdhpc	(5) totdhpc
Discretionary arrest	-0.0173	-0.0188	-0.0283	-0.0240	0.0052
	(0.0300)	(0.0299)	(0.0403)	(0.0458)	(0.0249)
Mandatory arrest	-0.0402	-0.0384	-0.0521	-0.0548	-0.0513
	(0.0562)	(0.0600)	(0.0672)	(0.0712)	(0.0367)
Preferred arrest	0.0293	0.0266	0.0158	0.0187	0.0194
	(0.0425)	(0.0407)	(0.0507)	(0.0502)	(0.0369)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.4028	0.4036	0.4052	0.4331	0.5198

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Isaacson Dating Relationship Regression Results

VARIABLES	(1) totdhpc	(2) totdhpc	(3) totdhpc	(4) totdhpc	(5) totdhpc
Discretionary arrest	-0.0070	-0.0078	-0.0201	-0.0096	0.0047
	(0.0262)	(0.0261)	(0.0341)	(0.0370)	(0.0212)
Mandatory arrest	-0.0655	-0.0639	-0.0757	-0.0745	-0.0697
	(0.0715)	(0.0751)	(0.0840)	(0.0906)	(0.0444)
Preferred arrest	0.0303	0.0279	0.0167	0.0230	0.0191
	(0.0427)	(0.0411)	(0.0507)	(0.0514)	(0.0369)
Observations	1,889	1,889	1,889	1,889	1,889
R-squared	0.4049	0.4056	0.4076	0.4307	0.5203

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Dating Relationship Dropped Regression Results

VARIABLES	(1) totdhpc	(2) totdhpc	(3) totdhpc	(4) totdhpc	(5) totdhpc
Discretionary arrest	-0.0129	-0.0136	-0.0257	-0.0220	0.0019
	(0.0308)	(0.0314)	(0.0420)	(0.0462)	(0.0255)
Mandatory arrest	-0.0623	-0.0615	-0.0735	-0.0752	-0.0703
	(0.0721)	(0.0758)	(0.0844)	(0.0915)	(0.0431)
Preferred arrest	0.0346	0.0328	0.0151	0.0212	0.0016
	(0.0518)	(0.0517)	(0.0594)	(0.0591)	(0.0435)
Observations	1,624	1,624	1,624	1,624	1,624
R-squared	0.3775	0.3777	0.3800	0.4107	0.5046

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1