ABSTRACT

TICHAVSKY, LISA P. The Adoption of Mandatory Arrest Legislation for Domestic Assaults in the U.S., 1980 - 2000: A Test of Three Explanations. (Under the direction of Margaret A. Zahn.)

During the two decades associated with America’s surge of policies that got “tough on crime”, the twenty years between 1977 and 1997, American also witnessed twenty-seven states and the District of Columbia pass legislation that required or strongly encouraged police officers to make an arrest when responding to a domestic assault. Unlike explanations for the adoption of other laws that imposed harsher treatment of non-domestic violence related lawbreakers, empirical tests of explanations for the adoption of laws imposing arrest in domestic violence cases were lacking.

Through a series of discrete time event history analyses for the years 1980 - 2000, this study conducts an exploratory analysis of factors associated with state adoption of mandatory and preferred arrest policies within three competing theoretical explanations for policy change. The first theoretical explanation tested is a conservative “policy regime” (Beckett, 1997), which takes a conflict theory approach in viewing increases in punitive legislation as a result of conservative “tough on crime” political platforms that serve an ulterior purpose of controlling marginalized populations. The second theoretical explanation is the diffusion of law, which takes an interactionist approach in proposing knowledge of legislation adopted in one state will leads to the adoption of similar legislation by neighboring states due to interaction among state policymakers. The final theoretical explanation examined in this study is political opportunity structures, which also takes an interactionist approach in
suggesting activists or social movements can effect change only in the presence of a supportive political structure (opportunity).

Support is found for political opportunity structure explanations in that mandatory or preferred arrest laws for domestic assaults were more likely to be adopted at times when the state’s governmental structure was more liberal than conservative, as measured on a political ideology scale (Berry et al., 1998), and when women held at least 15 percent of the state legislative seats. Implications of these findings for feminist scholarship and for advocates for victims of domestic violence are discussed. However, no support is found for policy regime/conflict explanations and diffusion of law explanations. The failure to find support for these two explanations are discussed in terms issue creation of domestic violence at the state level and different processes for laws involving the public and private spheres.
The Adoption of Mandatory Arrest Legislation for Domestic Assaults in the U.S., 1980 - 2000: A Test of Three Explanations

by

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DEDICATION

This dissertation is dedicated first and foremost to my grandfather, Paul Playton, who from the moment I could talk encouraged me to ask questions. His amazing life exemplifies his core values of faith, courage, integrity, and service to others and in his light I strive to be better. Also, to my daughter Jennifer Lynn Tichavsky, as she is a constant reminder to never assume what can or cannot be accomplished. And to my grandmother, Lillian Playton, whose encouragement and love for the written word inspires me. Finally, I dedicate this to my husband Pat, who has been patiently hoping that someday he will have more of my attention…well, be careful what you wish for. Thanks for hanging in there honey.
BIOGRAPHY

Lisa Tichavsky obtained a Bachelor’s of Science in Psychology and a Bachelors of Art in Sociology from Arizona State University in 2003, graduating Summa Cum Laude. She received a Master of Science degree in Sociology from North Carolina State University in 2008. Her primary research interests include both interpersonal and collective violence. She has conducted or collaborated on research on homicide recidivism, terrorist target selection and particularly, intimate partner and family violence and the response of the legislative and criminal justice systems particularly when issues of child custody are involved. In addition, her research includes studies on juvenile delinquency with a special focus on gender-specific prevention and intervention programming and program evaluation. She enjoys scuba, white water rafting, aviation, music, reading, hiking, and traveling. But her favorite ways to spend her free time is either “hanging on hook” in Red Bluff Bay, Alaska, fishing with her parents or sitting in her grandparents’ living room spending the day talking about everything or about nothing at all.
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CHAPTER ONE: INTRODUCTION AND STUDY OVERVIEW

1.1 Introduction

In the twenty years between 1977 and 1997, twenty-seven states and the District of Columbia passed legislation that required or strongly encouraged police officers to make an arrest when responding to an assault incident involving domestic violence. Three explanations for why states adopted these “mandatory” or “preferred” arrest policies dominate the domestic violence literature and imbue criminological texts on the topic, namely 1) the influence of social science research, 2) fear of legal liability, and 3) pressure from women’s advocacy groups. While each of these explanations has inherent merit, empirical evidence for their use as explanatory frameworks is weak or non-existent and thus mostly hypothetical. Consequently, our understanding of the factors associated with the adoption of these laws has been limited, which is particularly concerning considering the potentially negative consequences of these laws. Empirical tests of explanations situated within a theoretical framework would significantly improve our understanding of why some states pass these laws and other states do not.

At the time of this writing, this will be the first study to examine and empirically test the adoption of mandatory and preferred arrest laws for domestic assaults utilizing sociological theory. Through a series of discrete time event history analyses for the years 1980 - 2000, this study conducts an exploratory analysis of factors associated with state
adoption of mandatory/preferred arrest policies within three competing theoretical explanations for policy change.

The first theoretical explanation that will be tested is a conservative “policy regime” (Beckett, 1997), which takes a conflict theory approach in viewing increases in punitive legislation as a result of conservative “tough on crime” political platforms that serve an ulterior purpose of controlling marginalized populations. The second theoretical explanation is the diffusion of law, which takes an interactionist approach in proposing knowledge of legislation adopted in one state will lead to the adoption of similar legislation by neighboring states due to interaction among state legislators. The final theoretical explanation examined in this study is political opportunity structures, which also takes an interactionist approach in suggesting activists or social movements can effect change only in the presence of a supportive political structure (opportunity). These theoretical explanations are discussed in more detail in Chapter 2.

This study attempts to locate legislation related to mandatory/preferred arrest legislation within the broader criminological discussion on America’s increased punitiveness during the “tough on crime” era of the 1980s-1990s. Since there is no previous research connecting to the theories that have explained other legislative activity unique to this time frame, an exploratory study testing multiple explanations to provide some direction for future research will considerably advance our knowledge in this field.

The start of the legislative activity, peak years, and cessation of mandatory/preferred arrest legislation share much in common with other laws in this period. In addition, the laws
passed during America’s tough on crime era came under heavy criticism in the sociological literature as disproportionately affecting minorities and lower class populations. Mandatory/preferred arrest legislation has also been criticized for disproportionately affecting lower class and minority populations, as well as having unintended negative consequences for women and children. With these similarities one can’t help but question why mandatory/preferred arrest legislation has been absent from this body of criminological literature?

One reason for the absence of mandatory/preferred arrest legislation from criminological literature regarding America’s “tough on crime” years may be the lack of a theoretical paradigm congruent with the criminological or sociological literature associated with this legislation. This study attempts to redress that. Another reason might be that domestic violence often occurs within the home, or the “private sphere” (Habermas, 1989) and the processes at work may be different when the laws are related to the more private, interpersonal family life vs. the public domain. This study has the opportunity to examine that possibility and advance our knowledge on whether or not the processes are similar for laws related to the private sphere.

However, more important than the reason for its absence is the need for bringing it into the discussion. This study is the first step in doing so by locating mandatory/preferred arrest legislation within criminological literature on America’s tough on crime era and the theoretical perspectives that attempted to explain the policies adopted during that time. First,
an overview of the type of arrest law in each state and a brief discussion of the criticisms of these laws are provided for context.

1.2 Disparity in Domestic Violence Arrest Laws

A considerable degree of disparity exists across state laws regarding police response to domestic violence. As of 2012, twenty-two states and the District of Columbia have passed some version of a mandatory arrest law while six states have passed a preferred arrest law. Twenty-two states still leave the arrest decision to the discretion of the officer. Figure 1.1 visually presents the relationship between state and type of arrest law. ¹

![Figure 1.1 Domestic Violence Arrest Policies by State](image)

¹ A hierarchal rule was applied in the event that a state had a preferred law but mandated an arrest under certain conditions (e.g. Ohio), the more restrictive law was. Coding of the type of arrest law is discussed in more detail in Chapter 3.
When responding to domestic disturbance calls, states with a mandatory arrest law require the responding officer(s) to make an arrest if there is any indication of a crime having occurred, while states with a preferred arrest law specify that making an arrest is the preferred course of action, but not mandatory. States with a discretionary arrest law generally allow for warrantless arrest, but leave the arrest decision to the responding officer(s).

Substantial variation exists between the states in the type of laws states adopt and how, and to whom, the laws should be applied. For the purposes of this study, when considering whether a state has a mandatory, preferred, or discretionary arrest law, only the response to the assault incident is considered. For example, Minnesota law states that an officer may make an arrest in cases of domestic assault, but must arrest a person whom the officer has reason to believe has violated a restraining order. In this case, the mandate to arrest for the violation of a restraining order, even though it occurred concomitantly with the assault is not of interest. For the sake of consistency, what is of interest is a stated requirement or preference for arrest in cases of domestic assault.

Thus, what is consistently clear is that 56% of the states and the District of Columbia have intensified their efforts in controlling domestic violence at the level of police response, while 44% of the states have not. Several explanations for the increase in the adoption of

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2 Since the purpose of this document is to propose a study examining the influential factors for states’ adoption of a particular type of arrest laws, the degree of variation that exists within each type is not discussed here. For a more detailed review of the variations within each type of law, see (Miller, 2006; Zeoli, Norris, & Brenner, 2011).

3 Minn. Stat. §§ 629.341; 518B.01, subd. 14; 629.75, subd. 3.
mandatory arrest laws in the mid 1980’s to 1990’s have been offered, but the explanations are unidirectional. This unidirectional approach fails to address why some states take a moderate stance by creating a preferred arrest law or why some states do not create a more punitive law at all. Any viable explanation should be able to explain the behavior in question as well as counterfactual behavior. In other words, a good explanation should be able to account for the factors that influence one state to adopt a mandatory or preferred arrest law while also explaining why another state might not adopt such a law.

Given the lack of evidence that these laws are effective in reducing domestic violence (Christopher Maxwell, Joel H. Garner, & Jeffrey A. Fagan, 2001) and the array of evidence that the laws may be causing women and girls, especially economically disadvantaged minority women, more harm than help (Miller, 2005; Miller & Meloy, 2006; Sherman & Harris, 2013; Stark, 1993, 2004) gaining insight to the factors that are associated with the adoption of these policies could help redirect legislative efforts to improve or repeal these laws to eliminate the more harmful effects. For example, some of the criticisms could be addressed by amending the statutes to include a requirement to determine the primary aggressor, as some states already do, or to set more specific conditions on arrest determinations to prevent the net-widening affect that brings children in conflict with their parents into the juvenile justice system for minor fighting (Strom, Warner, Tichavsky, Zahn, 2014). Amending the statutes to require serious injury or threat and setting minimum age requirements before arrest becomes mandatory would be a good start in eliminating some of the criticisms of these policies.
1.2 Consequences of Mandatory Arrest Laws

The adoption of mandatory\(^4\) arrest laws for domestic assault cases shares an additional parallel to other legislative activity that occurred within roughly the same time period in which America witnessed a trend across the states to legislate more punitive laws in the name of “getting tough” on crime (Beckett, 1997; Western & Pettit, 2005). The more punitive legislation associated with get tough policies were controversial in that they generally lacked evidence of any solid evidence for their effectiveness and consequences of these policies tended to disproportionately affect lower income minority populations (Tonry, 2013) and therefore these laws were not without criticism in the social science research (Buzawa & Buzawa, 1996; Stark, 1993, 2004).

Domestic violence arrest laws, intended for the protection of adult women from abusive partners (Buzawa & Buzawa, 2003), have been criticized for increasing women’s victimization rather than ensuring protection from violence. These laws have been associated with increases in women’s arrests for assault in numerous studies (DeLeon-Granados, Wells, & Binsbacher, 2006; Finn & Bettis, 2006; Hirschel & Buzawa, 2002; Miller, 2005; Miller & Meloy, 2006; Mills, 1998) as well as juvenile arrests for assaults against family members (Bureau of Criminal & Analysis, 1999; Buzawa & Hotaling, 2006; Hirschel, Buzawa, Pattavina, & Faggiani, 2007; Strom, Warner, Tichavsky, & Zahn, 2014).

\(^4\) For the purpose of this study, the state is considered to have taken a more punitive stance towards batterers once the policy is no longer solely discretionary. Therefore, for the sake of brevity, from this point forward in the text the term “mandatory arrest” is understood to include preferred arrest as well as mandatory.
Further, several states enacted laws that required both parties to be arrested (dual-arrest) if there is evidence that the parties are mutually combative (Finn, Blackwell, Stalans, Studdard, & Dugan, 2004; Martin, 1997; Mills, 1998). In these states, women who “fight back” or inflict any sign of injury upon their attacker in self-defense may also be at risk of being arrested (Mills, 1998). To address this concern, other states have enacted a primary aggressor law, which instructs the officer to make a determination on the degree of aggression for either party in the event there is evidence of injury to both parties (Miller, 2006). Further, researchers have reported evidence of retaliatory arrests, in which the abuser falsely claims to have been abused so that their spouse will be arrested as a retaliation for her prior accusations that resulted in his arrest (Frye, Haviland, & Rajah, 2007).

Finally, just as “tough on crime” laws have been reported to disproportionately affect members of minority and lower-class populations (Beckett, 1997; Jacobs & Helms, 1996; Stucky, Heimer, & Lang, 2005; Western & Pettit, 2005; Yates & Fording, 2005), mandatory arrest policies have been found to disproportionately affect these same populations (Mills, 1998). For example, there is evidence that mandatory arrest policies, particularly in areas with large concentrations of Blacks, may be particularly dangerous for Black women as it tends to increase the likelihood of recidivism rather than reduce it (Mills, 1998; Ruttenberg, 1994; Sherman & Harris, 2013).

Other researchers have called attention to the fact that some women may be more reluctant to involve the police when they know an arrest is mandatory because the abuser may be the main source of income for the family, or the victim may fear the offender will
react with vengeance against whoever called the police (Mills, 1998). Given the debates embedded within the discourse, and the long-standing interest in the social processes associated with the creation of law (Gurvitch, 2001) and any increase in the state’s role in formal social control (Beckett & Western, 2001; Garland, 2001; Michalowski & Carlson, 1999; Schanzenbach, 2005), it is surprising that little attention has been directed to the factors associated with the legislation of domestic violence arrest laws.

Why do some states adopt these laws while others either fail to legislate domestic violence assaults or enact weaker/harsher laws much later than their predecessors? Any attempt to redress latent harmful consequences of domestic violence laws should start with an empirical analysis of the factors associated with the enactment of the law so that the original intent can be identified, and provide opportunities for advocates to influence lawmakers towards making any needed corrections to the laws through avenues similar to those which helped to create them.

1.3 How Can the Increased Adoption of Mandatory/Preferred Arrest Laws Be Explained?

With a little more than half of the states passing legislation requiring, or strongly encouraging an officer to make an arrest when responding to a domestic disturbance call, and the latent consequences of these laws, it is surprising that we have not more rigorously examined explanations for their adoption. Some scholars have speculated that social science research, fear of litigation, or the women’s movement and the Violence Against Women Act are responsible for the increases in state adoption of mandatory/preferred arrest laws in
domestic cases. However, with the exception of social science research, these explanations for state adoption of mandatory or preferred arrest laws have not been empirically examined.

Likewise, explanations within the context of the timing of states’ adoption of mandatory/preferred arrest laws are nonexistent. Since influential factors are unlikely to be fixed across years, it would stand to reason that the timing of the legislation should take a prominent role in any explanation of legislative change. Further, the failure of any of these assumed explanations to explain the negative case, in other words the inability to also explain why a state does not adopt a mandatory/preferred arrest law, is troublesome.

Thus, compared to other types of laws, we know very little about the sociocultural and structural factors associated with state adoption of domestic violence arrest laws. Therefore, potential explanations that have added to our understanding of the processes in state or Federal adoption of more punitive legislation in other areas that have not been explored in relation to state adoption of arrest laws in response to domestic violence cases will be utilized here. Specifically, the interactionist perspectives of political opportunities (Eisinger, 1973; McAdam, McCarthy, & Mayer, 1996), and the diffusion of law theory (Walker, 1969), and the conflict theory approach through policy regimes (Beckett, 1997) have not been examined in regards to this legislation. This study will address that gap by empirically testing these potential useful explanations.

Additionally, much of the research on increased punitiveness centers on laws against crimes committed outside of the “private sphere” (Habermas, 1989) of the home and family. For example, drug crimes, rape, and hate crimes are all more likely to involve acquaintances
or strangers\(^5\) and be treated as public issues, or within the “public sphere” (Habermas, 1989). Prior research in this area offers moderate to strong evidence supporting the spread of legislation through the process of diffusion; yet few similar studies for laws based in the private sphere of the family have been found (see McMahon-Howard et al., 2009 for an exception). Mandatory or preferred arrest laws that encourage or require an arrest of one family member who has attacked or threatened another family member are certainly examples of laws that impinge upon the private sphere. Whether our current explanations for increased legislative action against crimes associated with the public sphere can also account for increased legislative action for crime committed in the \textit{private} sphere is unknown.

For decades women’s advocate groups have worked to make domestic violence a public issue so that law enforcement agents and private citizens will cease to view domestic violence as something that goes on “behind closed doors”, or a matter between a husband and wife therefore something with which not to get involved (Bush, 1992; Miccio, 2005). While there have been significant advances towards changing this mindset, the belief that couple violence is “only the couple’s business” still persists as evidenced even in youth attitudes toward dating violence (Weisz & Black, 2008, p. 187). The results of these analyses will provide the additional benefit of helping to determine whether existing explanations for

\(^5\) Rape does occur in families, but the crime is categorized as either child molestation (minors) or spousal rape (adults) and are often treated differently in the justice system than those crimes that occur outside of the family (\textit{see for example} McMahon-Howard, Clay-Warner, and Renzulli (2009)).
policy change operate similarly for laws impacting the “private sphere” as they do for laws centered in the “public sphere” (Habermas, 1989).

1.4 Bridging Existing and New Explanations Under a New Unifying Framework

Importantly, missing from the discussion is a unifying theoretical framework. Ideally, a theoretical frame would be able to incorporate the explanations that have been proposed in the domestic violence literature for state adoption of mandatory arrest laws as well as the explanations based on legislative processes such as political opportunity and diffusion of law. As Jenness and Grattet (2001) suggests, one way to accomplish this is to frame mandatory arrest legislation as a policy domain situated within a social constructionist perspective. A policy domain is an area around which a political system is organized that gives meaning to a common problem (Burstein, 1991). Scholars suggest that policy domains are comprised of four overlapping phases; 1) issue creation, 2) adoption, 3) rule-making, and 4) application (Burstein, 1991; Jenness & Grattet, 2001).

An example of these phases as Jenness & Grattet (2001) observed them for the policy domain of hate crime legislation is helpful in order to better understand the appropriateness of viewing mandatory arrest legislation as a policy domain. In the first phase, the issue was create as activist groups documented and published data on violence motivated by hate, thus raising the problem to an “epidemic” in the U.S. The documentation of data and statistics gave the acts life and gave the issue a larger focus while at the same time offering empirical credibility to violence motivated by bias or hate. This phase has clearly been in progress for domestic violence. Viewing mandatory arrest legislation as a policy domain allows us to
acknowledge the activism of women’s organizations in the creation of domestic violence as a social issue. It also accommodates fear of litigation and social science research in response to the media coverage of the attention given to the issue as it gained momentum.

In the adoption phase of a policy domain, the issue is reframed in a statutory light and a solution to the issue is adopted into actual legislation via legislatures and policymakers. Here, we can draw upon theoretical perspectives of policy change to identify how this actually occurs. For hate crime legislation, Jenness & Grattet observed the process of diffusion of the law, with adoption of statutes slow at first and then gaining momentum as more and more states adopted the legislation. Differences and commonalities soon weakened or blended leading to consensus (homogenization) and expansion, as later states adopted more stringent policies than earlier states. For mandatory arrest legislation as a policy domain, this phase is the key focus of this study. This phase has apparently stalled as there is no consensus and there has been no further expansion since 1996.

In the “rule making” phase (courts and legal action) of hate crime legislation there were challenges to the legislation, mainly that the statutes were too vague, or that they were favoring minorities to the neglect of whites, too inclusive, or that they violated free speech. Jenness & Grattet outline legal challenges, court decisions, legislative revisions and amendments with the passage of new legislation and during the final phase, the application phase (e.g. policing and prosecuting).

This study is concerned with the adoption phase of the policy domain of domestic violence and therefore is focused on the factors associated with the timing of state adoption.
of a mandatory or preferred arrest law in cases of domestic assault. Viewing domestic violence legislation as a policy domain identifies the role that social movements played in creating the issue of domestic violence deserving of the attention of the State. It also acknowledges the roles of social science as a structural contributor to the creation of that issue, and the media as a sociocultural contributor in the issue creation. Therefore, domestic violence as a policy domain allows us to embrace existing explanations and expands on them as well.

1.5 Structure of Dissertation

This research examines potential explanations for the spread of the adoption of laws requiring or encouraging arrest in response to domestic assaults. In this first chapter I introduced and outlined the mandatory and preferred arrest laws and the variation in state legislation of mandatory or preferred arrest in the United States as of 2012. I briefly described the three prominent explanations for the adoption of mandatory/preferred arrest laws, fear of social science research, fear of litigation, and women’s activism, and problematized the lack of a theoretical framework and empirical evidence for these explanations. I also discuss three additional theoretical explanations that might also be useful for explaining why some states adopted mandatory or preferred arrest legislation for domestic assaults that should be explored.

In Chapter 2, I set the context of America’s “tough on crime” period and highlight some of the parallels of the mandatory/preferred arrest legislation to other arrest legislation of this period. I then review the several existing explanations grounded in sociological theory.
that have empirical support for influencing legislative processes in studies examining laws not related to domestic violence. Following a brief recap of the disparity across states in laws regarding arrest policies in response to domestic assaults, a more detailed review of the literature regarding some of the reasons that have been offered for why states have adopted mandatory or preferred arrest laws is presented. At the end of the chapter, I present the hypotheses that will be tested in the study.

All data sources for all variables and years are outlined in detail in Chapter 3, where I also describe the methodological approaches employed in the study. There I will substantiate the choice of discrete-time event history analyses to test the effects of covariates of the diffusion of law, availability of elite allies, and policy regime gendered opportunity structure on the timing of the adoption of a mandatory/preferred arrest law. I will also discuss the various options for modeling time dependence and the analyses leading to the decision to use a truncated power function of a natural cubic spline to model time. I describe each model that will be tested, and in each model the event of interest with the covariates and identify how they were measured and the data collected. I then outline each statistical model that will test the hypotheses and detail the model notations.

In Chapter 4, I present descriptive results as well as individual survival plots for select covariates of interest. I also present the results of the event history analyses for each model individually. Subsequently, in Chapter 5, I summarize and discuss these findings and identify how these results inform sociological theory on policy change. I show how this work has implications for researchers in the areas of domestic violence, public policy, women’s
advocacy, social movement, and political activism. I also discuss the limitations of the research and suggest next steps for future research in this area. I conclude with a summary of the main points of the work and the contribution that the research has made to the field.
CHAPTER TWO: A REVIEW OF THE LITERATURE

2.1 Introduction

Laws are not created in a vacuum, nor are they solely a response to a realized need for social control. In fact, the processes involved in the legislation of new laws are often complex and understanding these complexities has been an important focus in sociological scholarship (Black, 1976, 1998; Durkheim, 1933; Garland, 2001; Gurvitch, 2001). This is particularly true for sociologists in the area of criminology. This chapter begins with a discussion of America’s trend towards increased punitiveness between 1980 and 1995 in order to provide context for the period that most domestic violence mandatory arrest legislation, as well as other more punitive legislation was passed.

With this context in mind, various theories regarding the catalyst or the driving force behind America’s tough on crime period are discussed. This is followed with a discussion of the three explanations that have dominated the literature as to why states adopted mandatory arrest policies in domestic assault cases. Next, mandatory arrest legislation is incorporated into the broader sociological and criminological literatures through the suggestion that we consider explanations that have been helpful in explaining other legislative changes. Framed within broad theoretical frameworks, these early tests will help us develop more robust sociological theory of the process of policy change, both in the private and public spheres. The chapter ends with a section that states the research questions in the study along with hypothesis that are to be tested.
2.2 America’s Increasing Punitiveness

During the past two decades, sociological research has examined the trend of American states to legislate more punitive laws in the name of “getting tough” on crime. Criminologists have questioned the social and structural processes related to the enactment of legislation that enhanced penalties for drug crimes during the “war on drugs” (Beckett, 1997; Peterson, 1984), and “three-strikes” legislation (Beckett, 1997), increased sentences for repeat offenders (Kovandzic, 2001), exacted stiffer penalties for crimes based on bias (Black, 1989; Grattet & Jenness, 2008; Jenness & Grattet, 2001; Soule & Earl, 2001), and passed more inclusive rape laws (McMahon-Howard, 2011; McMahon-Howard et al., 2009).

Though more generally cited as occurring in the 1980s and 1990s, America’s “tough on crime” period was most heavily concentrated between the years 1984 through 1996 (Tonry, 2013). In fact, Tonry (2013) points out that it was in these years that mandatory minimum sentencing proliferated, life-without-the-possibility-of-parole (LWOP) policies flourished, more states began allowing juveniles to be transferred to criminal (adult) courts, and the era that gave birth to numerous sexual predator laws.

It was also during this period that the majority of the mandatory/preferred arrest legislation was passed. As shown in Figure 2.1, all but five of the 27 mandatory/preferred arrest laws were passed during this period (81%). The last 2 states to adopt this legislation did so just at the end of this time frame. While some states made adjustments to their policies after this time frame, for example adding primary aggressor statutes, no state adopted a new mandatory/preferred policy after 1996.
Figure 2.1 Number of Mandatory/Preferred Arrest Laws Adopted By Year

Clearly, adoption of mandatory arrest legislation occurred concomitantly with American legislatures’ trend towards increased punitiveness in the decades in which politicians proudly touted platforms that took harsh stands towards crime.

The sociopolitical culture at the time may have caused legislatures to become more open to harsher measures proposed by women’s advocates and thus become elite allies for social movement groups. Alternatively, the politically charged atmosphere promoting a tough stand on any form of crime may have given conservative politicians an opportunity to include mandatory arrest of batterers as part of a war on crime policy regime. Examining the way these processes operated for legislative reform for other forms of crime during this period will provide a theoretical framework from which to perform preliminary tests of these ideas.
2.3 Conflict Theory: Republican Policy Regimes

Perhaps the most prolific explanations encountered in the literature on America’s increased punitiveness are ones based on conflict theory, particularly racial and economic threat models. One that has been popularized in criminological literature is both a conflict and constructionist frame is a “policy regime” (Beckett, 1997) in which partisan politics are responsible for passing legislation for the purpose of serving a (generally) conservative agenda. For example, Beckett’s (1997) found political motives and rhetoric, not the actual prevalence of crime, as the driving force behind America’s fear of crime and subsequent control policies and categorizes the Republican war on crime/drug platform as a “policy regime” serving to control marginalized populations. Scholars that view the war on crime as a policy regime acknowledge that Democratic presidential administrations can also take a “tough on crime” stance (e.g. the Johnson and Clinton administrations). However, Democrats holding a similar platform are viewed to have acquiesced out of political necessity - no party candidate could secure an election with a weak stance on crime control (Beckett, 1997; Tonry, 2013). Yet Beckett argues that the motives behind conservative punitive policies served more than to win elections. She argued that, feeling the political pressure to expand the welfare state, conservatives bifurcated citizens into “deserving” or “undeserving” by creating the perception of a black underclass (e.g. culture of poverty) that leached off its benefactor (the public dole) and engaging in criminal activity. Increasingly, this policy-regime has used the war on crime/drugs stance to serve the purpose of controlling the marginalized (undesirable or costly) populations.
The conflict theory as expressed through conservative policy regimes has received empirical support. For example, numerous studies have found a positive relationship marginalized populations, usually measured as percent of the population that is Black (Beckett & Western, 2001) and Republican congressional power, to increased incarceration rates, which they propose to be indicative of a shift towards punitive control of marginal populations (Carlson & Michalowski, 1997; Carmichael, 2005; Heimer, Stucky, & Lang, 2005; Jacobs & Carmichael, 2001; Jacobs & Helms, 1996; Keen & Jacobs, 2009; Michalowski & Carlson, 1999; Stucky et al., 2005; Yates & Fording, 2005). Studies examining the policy regimes with the conflict approach generally find a positive relationship between Republican congressional power and more punitive legislation or social control, despite the various measures employed to measure Republican congressional power or “Republican dominance” (Stucky et al., 2005).

When measuring conservative policy regimes, most studies include a measure of the political party of the governor of the state, the political party that controls the state house and senate, and sometimes the political party of the state’s U.S. Congressional representatives and senators. Sometimes a mathematical computation is employed, for example providing a positive score for a Republican governor and multiplying the score by the percent of Republican legislators (Jacobs & Carmichael, 2001), or a weighted scale of ideological scores recorded given to congressional delegations by political watchdog groups (Berry, Ringquist, Fording, & Hanson, 1998).

Since there is a strong consensus in the domestic violence literature that domestic violence is more often experienced in the minority and lower income classes, (Bent-Goodley,
2005; Brush, 2004; Caetano, Vaeth, & Ramisetty-Mikler, 2008; Cunradi, Caetano, & Schafer, 2002; Frias & Angel, 2005; Hampton, Oliver, & Magarian, 2003; Huisman, Martinez, & Wilson, 2005; Plass, 1993; Vazquez, Stohr, Skow, & Purkiss, 2005; Vazquez, Stohr, & Purkiss, 2005; Weisz, 2005; West, 2004), the possibility of domestic violence laws resulting from class conflict is a real possibility. This study will test the association between Republican dominance in the legislature and the adoption of mandatory/arrest legislation for domestic assaults.

2.4 The Interactionist Perspective: Policy Diffusion and Political Opportunity Structures

Two other explanations that will also be examined fall under the interactionist theoretical approach to policy change; diffusion of law and political opportunity structures. These alternative explanations have been shown to be useful in explaining other legislative action such as state lottery adoption (Berry & Berry, 1990), same-sex marriage bans, state-level reforms in eligibility to welfare programs (Soule & Zylan, 1997), the adoption of hate crime legislation (Jenness & Grattet, 2001), and state ratification of the Equal Rights Amendment (Soule & King, 2006).

Political Opportunity Structures Approach

The political processes, or “political opportunity” approach (Eisinger, 1973; McAdam, 1996) suggests social movements have agency in the creation of law if the political structure is supportive (Meyer, 2004). Protests or activism without the assistance of
political agents in positions of power to advance the cause do not result in policy change and this is an important aspect that is missing from existing explanations that attribute the adoption of mandatory arrest legislation to the women’s movement.

Instead, Eisinger (1973) suggested that social movement organization (SMOs) were able to bring about legislative change only when the government was open to the issue for which they were advocating. Under this theory, no amount of protest, lobbying, or even riots (Eisinger, 1973) could advance an agenda if the requests fell on unsympathetic or politically closed ears. Of course the government is not a single cell entity and is in fact comprised of multiple layers and multiple actors and therefore it is possible for portions of the government to be receptive to the goals of SMOs while other factions remain closed to policy change or the agenda of the SMO. One specific aspect of McAdam’s (1996:25) conception of political opportunities theory addresses this possibility through the concept of the availability of elite allies. Elite allies are persons who hold the power to enact (or contribute to the passage of) legislation favorable to the social movement’s agenda. Contrary to the conflict perspective which focuses on Republican congressional power, most researchers taking a political opportunity structures theory approach to policy change focus on Democratic congressional power since historically, liberal ideology has tended to be more open to causes championed by social movement organizations (Meyer, 2004).

Female Legislators as Elite Allies (Gendered Opportunity Structures)

Especially in relation to issues of particular concern to women, several researchers have identified the relationship between the percentage of female legislators and policy
implementation or change that is more favorable for women. Some include the percent female legislators as a variable within a general political opportunity structures model. For example, in a relevant study in the political science literature, Murphy (1997) found women’s participation in the state legislature and gubernatorial to be the strongest predictor for state legislation affecting women. Keiser (1997) also finds that women's representation in government positions may be important for creating pressure on the system to respond to women's needs and increase the legislative attention paid to domestic violence issues.

McCammon, Campbell, Granberg, and Mowery (2001) suggests that the effect of female legislators on the passage of legislation favorable towards women is sufficient enough to warrant the distinction of a “gendered opportunity structure” rather than subsumed under the general category of elite allies. But for the purposes of this study, female legislators will be treated as elite allies within the general political opportunity structures model due to the limited information available on female legislators. For the years included in this study, the US Census Statistical Abstracts consistently reports the number of state congressional seats and (inconsistently) some offices held by women in each state but not political party, length of term, voting record, and other more detailed information that would be necessary to adequately distinguish gendered opportunity structures as a separate influence.

**Diffusion of Law**

The literature on domestic violence arrest laws often suggests that legislators are generally unaware of the domestic violence-related laws in other states (Miller & Banaszak-Holl, 2005; Schneider, 2000a). This contradicts other research that has found support for the
diffusion of legislation across neighboring states, clearly indicating awareness of the policies of neighboring states (Grattet, Jenness, & Curry, 1998; McMahon-Howard, 2011; Soule & Earl, 2001).

Diffusion of law may occur through a simple contagion effect where the more common a law becomes within an area in which lawmakers interact with other lawmakers, the more likely information regarding the policy will spread among policymakers (Karch, 2007; Walker, 1969). Contagion is generally measured with a variable indicating the percent of the states within the region that have the law to be diffused (Bergin, 2011). While contagion gets at the risk of being exposed to a law due to saturation within an area, a similar concept within diffusion of law theory views risk of exposure to the law through interaction based on proximity, indicating the number (or percentage) of neighboring states, defined as states that share a border, that have the law to be diffused.

However, studies have also found that legislation adopted in neighboring states may negatively influence a state’s adoption of similar legislations as well, especially if the law is controversial (McMahon-Howard, 2011; Soule & Earl, 2001). For example, Soule and Earl (2001) found that a state was less likely to adopt a hate crime law when one or more neighboring states had adopted a similar law that drew controversial media attention. When the political party of the state’s governor and the party controlling congress are split, in other words, of the opposite political party, the issue may be ripe with controversy and thus more likely to be covered in the media. Soule and Earl theorized that media reports of the controversy surrounding a neighboring state’s hate crime law might have dissuaded another
state from adopting such a law. However, when neighboring states are not politically split, the policies of neighboring states may influence legislators to adopt similar laws (Grattet et al., 1998; McMahon-Howard et al., 2009). Therefore, any political split in neighboring states should be tested, as well as the possibility of legislative diffusion from one state to a neighboring state.

2.5 Existing Explanations for Adoption of Domestic Arrest Laws

Researchers have offered several explanations for the increase in states’ adoption of mandatory arrest laws, and these explanations can be categorized as 1) fear of legal liabilities, 2) the influence of social science research, or 3) pressure from activist groups (Buzawa & Buzawa, 2003). Yet, while it is highly likely that the suggested explanations played some role in the adoption of a stronger arrest law for some states, and it is possible that several factors worked in tandem to exert influence, they are insufficient for several reasons. Discussions of these three explanations are ripe in the literature, but missing from the discourse is a discussion of the potential problems with each of these explanations, based on the timeline in which these laws were adopted and the lack of evidence to support some of the assumptions behind the explanation. Below, I offer a critical analysis of each explanation and later, suggest additional explanations based on past criminological research on increased punitiveness and the creation of law.6

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6 Frequently, researchers mention more than one of these reasons. Citations reflect this multiplicity.
Evidence of Deterrence from Social Science Research

One of the three dominant explanations for the increased adoption of mandatory arrest laws suggests that the increase in states’ adoption of mandatory arrest laws is direct result of the Sherman and Berk (1984) study that reported arresting the offender led to a reduction in recidivism (Sherman & Berk, 1984). Their study, the Minneapolis Domestic Violence Experiment (MDVE), employed an experimental design to assess the effect of arrest on future domestic violence offending. Over a 6-month period, Minneapolis police officers responding to a domestic violence call selected a card from a randomized stack of cards containing one of three methods of handling the call. Depending on the card selected, the officer was to either 1) arrest the offenders, 2) separate the batter and victim for 8 hours, or 3) offer advice to both parties (at the officer’s discretion). Follow-up interviews showed offenders in the “arrest” condition had lower recidivism rates (19%) than those who were separated (33%) or counseled (37%).

However, later studies that attempted to replicate those findings showed mixed support for mandatory arrest in reducing recidivism. In fact, one study found that arrest increased the likelihood of reoffending for those with the least stakes in conformity, such as the unemployed or unmarried (Sherman, Smith, Schmidt, & Rogan, 1992). Further, Maxwell and his colleagues (2001) performed a thorough exegesis of all the replication studies and concluded that the effect of arrest as a deterrent to future abuse against the same victim was modest, especially compared to other factors such as the offender’s age and prior criminal history. They also noted that the majority of domestic violence offenders did not recidivate
regardless of whether an arrest was made or not. Establishing the specific deterrence effect of arrest in the prevention of future crime is certainly difficult, and it is suggested that studies that have examined the impact of arrest on recidivism have not generally found support for a deterrence effect of arrest (Manning, 1996).

Still, the Minnesota Domestic Violence Experiment conducted by Sherman and Berk (1984) is perhaps the most frequently cited reason for the adoption of mandatory and preferred arrest laws (Buzawa & Buzawa, 1996; Buzawa & Buzawa, 2003; Finn & Bettis, 2006; Mills, 1998; Sherman & Cohn, 1989). This is also the only explanation in the sociological literature for which any form of empirical evidence has been offered, however slight. Acknowledging that their study was performed with the express purpose of informing public policy, the researchers strategically publicized the results of their study and then conducted a survey of 117 police departments immediately following the first round of publicity (1984) and then again, 1 year (1985) and two years (1986) after the results had been publicized (Sherman & Cohn, 1989). Results indicated that departments’ awareness of the experiment increased over the three-year period, as did the use of mandatory arrest (Table 2.1 next page).
Sherman and Cohn attempted to control for external factors by specifically asking a department spokesperson if the Sherman and Berk (1984) findings had influenced their arrest policies. At the end of the interviews, 51% of the departments claimed the study had no influence (Sherman and Cohn 1989:124). In a bivariate analysis of the departments’ knowledge of the findings of the experiment and a policy change (Table 2.2 *next page*), the majority of police departments that had heard of the experiment did not experience a change in their arrest policy (74%) during the first two years. Although this percentage was reduced to 67% in the follow-up period, it also tells us that only a third of the police agencies surveyed (27 agencies out of 112) were influenced by the findings of the study.
Sherman and Cohn’s results suggest that social science research may have some informative influence on the policy decisions of some police departments, but was not the definitive reason states adopted mandatory arrest laws, especially since the study didn’t address policy making at the state level. In addition, the International Association of Chiefs of Police (2012) released findings from a focus group involving law enforcement leaders in which the majority of the focus group members reported that social science research does not play much of a role in policy decisions.

Still, if we assume that the legislation is a response to research on the effectiveness of mandatory arrest practices, the abject results of the replications studies might have a negating effect and thus change the influence of social science research to one that discourages arrest. Why did several departments report that they were aware of the findings but did not adopt mandatory arrest policies, while other agencies that were unaware of the findings adopted mandatory arrest law? Consequently, the scant evidence we do have for social science research as an explanation for adopting a mandatory arrest law offers little reason to expect a main effect of research findings on the adoption of a mandatory or preferred arrest law.


**Fear of Legal Liability or Negative Publicity**

Dobash and Dobash (1992) review two landmark cases that they believe to have led to an increase in police agency’s fear of litigation and subsequently, the adoption of domestic violence mandatory arrest laws. They cite the 1976 case of Scott v. Hart, brought against the Oakland, California police for failing to protect women victims of domestic violence, as the first class action lawsuit on a domestic violence issue. In the same year, a class action suit (Bruno v. Codd) was filed against the New York City Police on behalf of twelve women who were not given assistance after they were attacked by their spouse (Bush, 1992). One claim in this case was that even though a man attempted to strangle his intimate partner in front of a NYPD officer, the man was not arrested (Dobash & Dobash, 1992).

Another highly publicized case in 1984 resulted in a jury awarding $2.3 million dollars to Tracey Thurman who sued the City of Torrington after police failed to protect her from her estranged husband (*Tracey Thurman et al. v. City of Torrington, Connecticut*). The Thurman case is often cited as influencing states to become more punitive towards domestic violence offenders (Buzawa & Buzawa, 1996; Buzawa & Buzawa, 2003; Finn & Bettis, 2006; Frye et al., 2007; Gelles, 1996; David Hirschel et al., 2007; Miccio, 2005). For example, Buzawa et al. (1996:102) credit the Thurman case to be the “seminal case forcing police change”. The case was sensationalized in the press after Thurman’s estranged

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7 The case also generated the made-for-television movie “A Cry for Help: The Tracey Thurman Story” which aired on NBC in 1989 (Directed by Robert Markowitz). Available online at http://www.watch-
husband beat her to the ground, stabbed her multiple times, slit her throat, and stomped on her head, all while a City of Torrington police officer stood and watched nearby. Thurman and her relatives had made several requests to the Torrington police for protection from her soon to be ex-husband, but had received virtually no help from the police who behaved as if they believed that whatever occurs between married persons was a private matter.

Some researchers suggest that highly publicized criminal cases involving domestic violence served to increase the saliency of the deadly nature of domestic violence in the minds of the public and the minds of police agencies (Buzawa & Buzawa, 2003; Miccio, 2005; Schneider, 2000a). For example, Miccio (2005:1) notes that immediately following the media frenzy of the 1994 O.J. Simpson murder charge, New York “joined a majority of states in passing mandatory arrest laws in cases involving domestic violence”. However, while it is certainly true that New York, Ohio, and Colorado passed a mandatory arrest law in 1994, there is no indication whether these states already had domestic violence legislation pending prior to the attention given the O.J. Simpson case. Further, the publicity of the Simpson trial and news of other celebrities involved in domestic violence during that same year (i.e. comedienne Roseanne Arnold and rock singer Axl Rose) that led to dramatic “increases in calls for service to domestic violence hotlines, shelters, the police, and the courts”, may have created the demand for tougher laws (Buzawa et al. 1996:101). However, it is not legislatively feasible that the states that adopted a mandatory arrest law in 1994 could

movies.net/movies/a_cry_for_help_the_tracey_thurman_story/. Marketers of the film claim that Connecticut (Thurman’s Law) and “several states” adopted a mandatory arrest law following the trial.
have attended to the reports in the national press and enacted the law in the same year. There is generally a lag between the call for action and the effective date of the law.

Finally, if we assume that fear of litigation is an incentive in adopting a mandatory arrest law, wouldn’t all states share this concern as these cases are usually picked up in the National news? Furthermore, high profile cases involving civil suits against police departments for the failure to protect domestic violence victims are rare, so it seems unlikely that fear of litigation would play much of a role in the state adoption of a mandatory arrest law unless the individual state experienced litigation and sanctions. The sensationalizing of a particular case in one state might raise concerns in another, but if we are to promote this concern as a call for action, then we need to ask why some states were motivated by the national story while others were not.

**Influence of Women’s Groups and the Violence Against Women Act**

A third explanation for the increase in state adoption of mandatory arrest laws is the influence of the battered women’s movement as well as other women-orientated activism (Frye et al., 2007; Miccio, 2005) and pressure from activist groups such the National Coalition Against Domestic Violence (Buzawa & Buzawa, 2003; Charles, 2004; Miccio, 2005; Mills, 1998). For example, women’s activist groups are credited with helping to pass the Violence against Women Act (VAWA) in 1994. VAWA was believed to have a strong effect on states’ punitiveness towards domestic violence offenders due to the language of VAWA, which “reflects a strong pro-criminalization position” and funding provided for the implementation of mandatory or preferred arrest laws (Schneider, 2000a, p. 184). Further, as
Beckett (1997) notes, federal funding, when made available to states for the purpose of increasing formal social control, is likely to play a role in states’ increased punitiveness towards crime. VAWA became a source for funding that encouraged increases in formal social control.

Through the US Department of Justice, the Office on Violence Against Women uses money provided by VAWA for Grants to Encourage Arrest Policies and Enforcement of Protection Orders. The Act directs “…funds be used to implement mandatory arrest or preferred arrest programs and policies in police departments, including mandatory arrest programs or preferred arrest programs and policies for protection order violations.” (VAWA Title 28, Subpart D, §90.62 (a) (1)). With such clear intent, it is no surprise that VAWA funds have been cited as an explanation for the increased number of states adopting tougher arrest policies.

However, as an explanation for states’ increased adoption of mandatory arrest laws, there are two major points that need to be addressed. First, how can account for the passage of mandatory arrest laws prior to VAWA funds becoming available? VAWA was passed in 1994, the Office on Violence against Women (OVW) and the grant funding office and staff was set-up in 1995. Therefore, even though the Act passed in 1994, actual funding didn’t commence until late 1995-early 1996, several years after some states had adopted mandatory arrest polices. Second, these funds are often awarded at the agency level. This means that the funds are used to change the specific police agency’s policies, or enable the agency to fund special domestic violence task force but VAWA does not fund “the state”. Adoption of these
arrest laws are certainly at the state level, not the agency level, although there may be differential enforcement at the agency level (Buzawa & Hotaling, 2006). Unless these points are resolved, this explanation remains weak. In light of the fact that no states adopted a mandatory or preferred arrest law after VAWA begun funding precludes it from being a predictor in models examining the influence of the adoptions of mandatory or preferred arrest laws.

Still, women’s groups and advocates are often credited with making violence against women a social issue, and amid the background of the “epidemic” of violence against women, the Violence Against Women Act was formed (Schneider, 2000a). Therefore, another aspect of the women’s movement as an explanation for the increased adoption of mandatory arrest laws is based on women’s organizations and lobbyist groups’ creation of domestic violence as a social issue. It has been suggested that in turn, the view of domestic violence as a social issue created momentum for legislative change, much in the same way Jenness described for hate crime legislation.

Since activists for ending domestic violence often equate violence against woman as a product of patriarchy and oppression based on sex, it is not surprising that domestic violence legislation has been compared to hate-crime legislation (Jenness & Grattet, 2001; Schneider, 2000a). In fact, Jenness and Grattet (2001) called attention to several attempts that were made to include women as a protected category during the drafting of hate crime legislation in some states. They point out that “hate crime is nothing new” - every country from any point in early history to the present day has a long history of acts of violence (a tool of
oppression) against members of marginalized groups by members of the dominant group” and the same can be said of domestic violence. They also note that the late 60’s saw the growth of activist groups representing various religions, ethnicities, women, nationalities, sexual orientations, and any group that felt marginalized, and these organizations, through their documentation and publication of the data, raised the problem to an “epidemic” in the U.S. Thus, the similarities to the processes that have been proposed to be involved in the adoption of mandatory arrest laws to the processes of hate-crime legislation are striking.

There is a strong consensus that women’s organizations and activist groups are linked to the creation of domestic violence as a social issue (Bevacqua & Baker, 2004; Dunn, 2004; McLean, 2002; Miccio, 2005; Schneider, 2000a). Therefore it is no surprise that Jenness and Grattet’s (2001) constructionist and interactionist-based “docuhistory” of the formation and organization of one policy domain (hate-crimes) suggests that other legislative rallies around a cause would have similar structure, organization and processes. Indeed, the authors recognize “similar processes operative in other policy domains, one of which is domestic violence” (p. 167).

However, whether these groups had a direct effect on individual state adoption of mandatory or preferred arrest laws is unclear. An assessment of the influence of women’s groups at the state level would require individual case studies with detailed content analyses of all state legislative hearings, subcommittee, and special committee meeting minutes. Securing such information is beyond the scope of this study. Further, considering domestic
violence has been identified as an issue, we can acknowledge this first step in the policy process and move on to other influential factors.

2.6 Research Question and Hypotheses

The primary research question is what factors influence the adoption of mandatory arrest legislation? In the attempt to answer this question, several existing explanations that have been helpful in explaining the process of other legislation are examined. In particular, the following hypotheses will be tested.

**Hypotheses related to the Diffusion Model**

The diffusion of law approach suggests that laws diffuse in the process of interaction between lawmakers. Lawmakers learn about new legislation adopted within their region or in a neighboring state and then adopt the legislation in their own states. Therefore, if states adopted a mandatory or preferred arrest law due to the influence of the legislation of neighboring states, then:

**Hypothesis One (H1):** Larger numbers of neighboring states with a mandatory or preferred arrest law will increase the risk of adopting a mandatory arrest law in domestic violence cases.

However, political debate over the legislation is believed to occur if the state congress and the governor are of different political parties, or “politically split”. Presumably, if the congress passes the legislation and the governor threatens to veto the bill, this will be reported in the press. The politically split component argues that neighboring states hear of
the debate and hold off on adopting the legislation. If more than one neighboring state is politically split, the effect may work in the opposite direction, and therefore:

**Hypothesis Two (H2):** Politically split neighbors will mediate the diffusion effect in that as the percentage of politically split neighboring states increase, the effect of diffusion will decrease.

**Hypotheses related to the Policy Regime/Conflict Model**

Prior research supports the explanation of a conservative policy regime based on conflict theory as influential in the passage of punitive legislation. As discussed above, this explanation suggests that conservatives run on a “tough on crime” political platform that is favorable to their conservative constituents and therefore enact policies that are more punitive than their liberal counterparts. However, this policy regime also serves the purpose of controlling low-income, minority populations. Therefore, if mandatory/preferred arrest legislation occurred as part of the Republican “tough on crime” policy regime then the following hypotheses should be true:

**Hypothesis Three (H3):** States with higher proportions of Republican legislators, Republican Governorship, and a more conservative citizen constituency base (as measured by presidential voting trends) will be more likely to adopt a mandatory arrest law compared to more liberal states.
**Hypothesis Four (H4):** States with higher proportions of Black populations will increase the likelihood of adopting a mandatory arrest law than states where Blacks make up a lower percentage of the populations.

**Hypotheses Related to the Political Opportunity Structures Models**

Alternatively, political opportunity structures theory suggests that social movements will be successful only in the presence of a supportive political structure. A political structure is supportive when a movement has political elites, or “friendly agents” in the legislature that are sympathetic to their cause and willing to help in the efforts of drafting and defending a bill on the issue. Liberal legislators have been known to serve as friendly agents for social movements, especially those concerning the protection of women. Also, as previously noted, the literature shows issues relating to the needs of the minority populations tend to gain more support from Democratic legislators than Republican, and domestic violence disproportionately affects minority populations. Thus lower scores on the Conservative ideology scales would indicate a more liberal ideology which should be more receptive to social movements and women activist groups with the agenda of passing domestic violence legislation. Therefore,

**Hypothesis Five (H5):** Lower scores on the state conservative ideology scale (meaning Republican *minority* in the state legislature, Democratic governor, and majority of voters voted Democrat in the last Presidential election), will decrease the risk of adopting a mandatory or preferred arrest law for domestic violence cases.
Another dimension of the political opportunities approach is gendered opportunity structures as discussed above, prior research reports female legislators to be more supportive of legislative change in general, and particularly on issues proposed by women’s organizations. As evidenced by early work for the formation of the Violence against Women Act, and subsequent funds to encourage mandatory arrest legislation by VAWA once it was established, there is little reason to believe that mandatory arrest legislation for domestic violence assaults would not be considered a social movement championed by women’s organizations. If the female legislators act as elite allies for women’s movements and, as the domestic violence literature suggests, the women’s movement encouraged the passage of this legislation, then:

**Hypothesis Six (H6):** Having a legislature in which at least 15% of legislative members are women\(^8\) will increase the risk of adopting a mandatory arrest law.

Detailed descriptions of the variables for each model as well as the analytical methods employed will be discussed in the following chapter.

Testing these theoretical perspectives should provide a strong foundation for research on legislation on domestic violence and other forms of legislation in which the justice system

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\(^8\) Several researchers have found support for a threshold effect for female congressional participation. Female legislators behave much like their male counterparts when few in number, but diverge when they reach critical mass, which is somewhere around 15%. Some suggest that lower participation may cause women to feel like tokens and thus are more hesitant to stray too far from the status quo. When critical mass is reached, women may feel like part of an organized gendered force and are more confident in differing from their male colleagues. See Keiser (1997) for a more thorough discussion of this research.
intersects with the presumably private sphere of the family. Information on whether legislative processes are influenced differently for domestic violence related legislation compared to other legislation will provide insight on the degree to which domestic violence is no longer seen as a private issue between partners. One goal of the women’s movement was to make the personal political (Eisenstein, 1984), and to pull domestic violence out from behind closed doors and into public discourse (Ferraro, 1989, 1996; Miller, 2005). Through the examination of potential explanation for the adoption of mandatory arrest laws in the following chapters, the results will also shed light on rigidity of the separation of the private and public spheres. Towards those ends, the following chapter describes the methods and analyses employed in the examination of the potential explanations.
3.1 Introduction

Recall that the purpose of this study is to test three theoretical frameworks, 1) policy regimes/conflict theory, 2) political opportunity structures, and 3) diffusion of law as potential explanations for the adoption of mandatory arrest legislation for cases of domestic assault. The previous chapter called attention to existing research that has found support for these theoretical frames as explanations for the adoption of other forms of punitive legislation that were passed around the same time period as mandatory arrest legislation. Due to the harmful effects these laws are reported to have for women and children, particularly in low-income families, the need for an examination of the factors that influence state legislation of mandatory arrest laws is clear. This chapter describes the methods used in that examination.

The chapter begins with the descriptions of the datasets used and the dependent variable, which is common to all models. Next, the covariates are detailed in separate sections according to the model in which they will be applied. The final section of this chapter includes a discussion of the analytic plan, including the statistical technique and software employed, the models in the study, and a discussion of the statistical approaches that will be shared across all models to be tested. This includes a discussion of how the control for the effect of time was created and modeled as well as the methods utilized to
evaluate the fit of the analytic models. The next section begins with an overview of the data used in this study.

3.2 Data

This study includes data from 49 states for the years 1980-2000. With the exception of Oregon’s adoption of a mandatory arrest law in 1977, all mandatory arrest legislation occurred during these years. Several states have made small revisions to their arrest law since 1997, for example adding a requirement that the responding officer determine and arrest only the primary aggressor. However, no state made a transition from a discretionary arrest law to a mandatory or preferred arrest law after 1997 and it is the transition to a more punitive stance via a mandatory or preferred law that is of interest in this study.

Because the exclusion of Oregon increases the chance of sample selection bias as Oregon becomes a left truncated case, careful consideration was given to extending the data collection period to 1977 in order to include Oregon in the study. However, there were inconsistencies in the availability and reporting of the data prior to 1980 that would have threatened measurement reliability by requiring the use of alternative measures for the same variable (Schneider, 2000b). Therefore, Oregon was dropped from the analytic dataset.

Dataset Creation

Data on arrest laws for all 49 states in the study were collected in two file formats. The first file was constructed as a case-level file in which each state contributed a single line of information in the dataset. This file was used to assess baseline survival and hazard
functions (Singer & Willett, 2003). A second dataset was constructed as an event-level (e.g. person-period) file in which each state contributed one record for each variable every year in which they were at risk of adopting a law during the study period. For a state that adopted a law in 1988, a “0” was recorded for the variable “mandatory arrest law” every year up until 1988, the year in which the state adopted the mandatory (or preferred) arrest law and then a “one” in 1988 when the law is adopted. This dataset was used for all discrete-time logit event history analyses (Allison, 1984, 1995; Singer & Willett, 2003).

**Dependent Variable (Event of Interest)**

The dependent variable in the analyses is the probability\(^9\) of adopting a mandatory or preferred arrest law for domestic assaults. Since all states have some form of warrantless arrest\(^10\), and all states were discretionary prior to adopting tougher arrest policies, the event of interest will be the transition from a discretionary to a mandatory or preferred arrest law. The event is considered to have occurred whether the adopted law is mandatory arrest or preferred arrest since the change to a more punitive law is the event of interest, not the gradient change in law.

Data on whether a state has a mandatory or preferred arrest law were collected from multiple sources including the American Bar Association’s Commission on Domestic

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\(^9\) Unlike Cox proportional hazard models, the hazard modeled in discrete-time logit models are conditional probabilities.

\(^10\) States with discretionary arrest laws still allow for an arrest without a warrant, but the law does not encourage an arrest or mandate one.
Violence (ABA, 2007), the Institute for Law and Justice’s 2004 report on state legislation involving the duties and powers of police in domestic violence cases (Ruttenberg, 1994), and from social science research on domestic violence and mandatory arrest legislation (David Hirschel et al., 2007; Ruttenberg, 1994; Zeoli et al., 2011). These sources provided a reference to the relevant state statutes which were then accessed through the Westlaw database (Thomson-Reuters, 2011) and the wording of the statute and the year it became effective was recorded. Based on the wording of the law, the law was classified as either 1) *mandatory*, indicating that an officer was required to make an arrest if he or she believed an assault had occurred or if there was a potential risk of continued harm or threat to the victim\(^\text{11}\) or 2) *preferred*, when the law specified arrest was the preferred action or stated the officer “should” make an arrest, or 3) *discretionary*, indicating the state did not have a law mandating or stating a preference for an arrest in domestic assault cases. As the interpretation of the wording of laws can be subjective, the codes were compared with those reported in three other published works (Zeoli et al. 2011; Hirschel et al. 2007; Miller 2005) to ensure intersubjective agreement in the translation of the law. Consistent with regional measurement in other diffusion research (Bergin, 2011; Grattet et al., 1998), states were divided according to the nine regions defined by the U.S. Census Bureau (Figure 3.1).

\[^{11}\] Some states set additional limits on the mandatory arrest conditions such as requiring the assault to be a felony assault or if a misdemeanor, there is impending danger to victim. See for example Louisiana (La. Rev. Stat. Ann. § 46:2140; Ch. C. Art. 1573[1]). However, for the purposes of this study any additional stipulations do not affect the transition from discretionary to a more punitive form of the law and therefore are not of concern here.
The event of interest is represented by the variable, *Mandatory Arrest Law (MLAW)* coded 0, 1) indicating whether a mandatory or preferred arrest law was adopted in a given year. Each year that the state law remains discretionary or censored this variable is coded as “0”. When the state introduces a mandatory or preferred arrest law, this variable is coded as “1”. Those states that do not adopt a mandatory or preferred arrest law are right censored (n=22) and those states that adopted a mandatory or preferred arrest law during the period of study are considered to have experienced the event (n=27). Oregon adopted the law in 1977 and for reasons previously mentioned, was excluded from the study so no cases were left censored.

**Covariates**

Covariates are presented in the following sections according to the analytical model in which they will appear. The U.S. Census Statistical Abstract of the United States’ table numbers and pages references where the data were found changed from year to year. However, *Appendix A* lists the full record for each variable including table titles, table numbers, and page numbers. The legislative process involves the introduction of legislation followed by discussion, debate, and often there are revisions before the final vote. Once the legislation passes the effective date is rarely immediate, so several more months may pass before the legislation becomes effective. Since the passage of a mandatory arrest law is measured by the date the law became effective, any influence on the passage of the law
would have occurred in the year prior to the effective date. Therefore, all of the covariates are lagged by one year.

**Diffusion model**

As previously discussed, the diffusion of law approach proposes that the adoption of legislation spreads from state to state via a contagion effect or geographic proximity.\(^{12}\) This is believed to be the result of lawmakers’ interaction with other lawmakers within a general area of exposure, for example lawmakers that might attend the same regional meetings or have access to local media sources of the neighboring states near a shared border. In essence, the concept of geographic proximity proposes that the spread of legislative ideas and information is simply an effect of one state being in close geographic proximity to a state that has already adopted the new law.

Geographic proximity has been measured in prior research by 1) the number of states in the region (Zorn, 2000) that have already adopted the law 2) the percent of states in the region that have already adopted the law (Grattet et al., 1998) or, 3) the number of neighboring states that have already adopted the law (Berry & Berry, 1990; McMahon-Howard, 2011) or, 4) the percent of neighboring states that have already adopted the law (Kane, 2007). There is no evidence for considering any one of these measures to be superior.

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\(^{12}\) Contagion and geographic proximity is often used interchangeably though some researchers make a compelling argument that they are conceptually different (Greve, Strang, & Tuma, 1995). However, the measures defined here have been termed measures of contagion and/or geographic proximity depending on the study. For simplicity, the term geographic proximity is used from this point forward.
to another as there is mixed support for each of these measures (Bergin, 2011). This could be one reason that it is not uncommon for researchers to test (Chamberlain & Haider-Markel, 2005) or footnote that they tested one or more of these measures in addition to the measure that is shown in the final model (Grattet et al., 1998; Soule & Earl, 2001). In view of this, data were collected for all four measures and all four measures will be tested in separate models.

In order to test the hypothesis that proximity to other states with a mandatory or preferred arrest law will positively influence a state to adopt a more punitive law, (H₁) two variables representing geographic proximity with the region was constructed, **Percentage of States in the Region with a M/P Law (%)** and **Number of States in the Region with a Mandatory Law**. Consistent with regional measurement in other diffusion research (Bergin, 2011; Grattet et al., 1998), states were divided according to the nine regions defined by the U.S. Census Bureau (Figure 3.1 next page).

The next two measures testing diffusion through geographic proximity are the **Percentage of Neighboring States with the Law** and the **Number of Neighboring States with the Law**. Consistent with the way neighboring state has been defined in multiple diffusion studies (see Bergin, 2011 for a review), neighboring states are defined as states that share a common border. For a list the neighboring states, see Appendix (B).

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13 Grattet et al. (1998, p. 297) also test a “systemwide contagion effect” measured by the total number of states with a hate crime law but they did not find it useful and this way of method of measuring the contagion effect is rare. Therefore it will not be included in the contagion measures to be tested here.
Political Split. Hansen (1983) proposed that laws are more likely to become adopted when states have a unified government in which both chambers of congress and the governor are of the same political party. Conversely, when a state has a “split-party” government in which the governor and the two chambers of congress are of the opposite political party, (see McMahon-Howard, 2011, p. 6), it is more difficult to get laws passed. Additionally, states with split-party governments may experience greater ideological debates in the press over respective positions on novel or controversial legislation. In fact, prior research on the diffusion of law has found neighboring states that have a split-party government have a negative effect on whether a law gets adopted by a neighbor state (Soule & Earl, 2001). Thus, the second hypothesis (H2) within this theoretical frame proposes that neighboring states with a “split-party” government may have a negative effect on a state adopting a
similar law. A dichotomous variable *Politically Split Government* (1 = Yes, 0 = No) was created indicating whether the governor and legislative majority of the neighboring state were of different political parties. This measure captures political difference at the interstate level, not at the intrastate level, and is expected to modify interstate diffusion. Information regarding the political party of the governor of the state for each year of the study was collected from the U.S. Census, Statistical Abstracts of the United States, *Votes Cast For and Governor Elected, by States* (see Appendix B).

In order to determine legislative majority, information on the political parties of the state’s House and Senate members was collected from the U.S. Census Statistical Abstract of the United States table *Composition of State Legislators* (see Appendix B). The total number of Republican and Democrat senators and representatives were totaled by party and each party total was divided by the total of all state Senate and House members combined. A party was considered to have a legislative majority in the state if the party’s percentage of combined House and Senate seats totaled 51% or greater. The state’s legislative majority was recorded for each year of the study and the majority party was compared to the political party of the governor. If the two parties were different, the state was coded with a “1” indicating a politically split government for that year. If the party of the governor and the legislative

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14 This measure differs from the variables in the political model because it is not measuring within state characteristics that define the state’s political ideology. It measures a characteristic of a neighboring state that might have a modifying effect on the diffusion of law from state to state.
majority were the same, the state was coded with a “0” indicating the state was not politically split that year.

In order to ensure the accuracy of the data collected, two separate datasets were constructed and the data were collected on two separate occasions. Collecting the data on two separate occasions was a quality control measure in that an accidental misread of a number from a census document or a data entry error was unlikely to happen for the same entry on both occasions. A comparison of discrepancies will then catch errors that would have otherwise gone unnoticed. The two data sets were reconciled for consistency and any discrepancies were researched back to the original Statistical Abstracts table to verify the correct entry. The only issue with missing data involved Nebraska since Nebraska has a unicameral congress. Therefore data could not be collected for party affiliation for Nebraska’s representatives or senators so Nebraska could not be included in models using the Political Split variable due to missing data on the political party of its legislature. However, Nebraska was included in all analyses involving State Conservative Ideology since, as discussed in the following section, that measure had imputed values for Nebraska’s missing data.

*Conflict Theory-Conservative Policy Regime*

Hypotheses three and four are based on the theoretical framework of conflict based on a conservative policy regime. This framework proposes that more conservative states will be more likely than less conservative states to adopt mandatory arrest laws as part of a
conservative policy ($H_3$) and that this conservative policy is an attempt to control the lower classes ($H_4$).

**State Conservative Ideology.** The conservative ideology of the state has been measured in two ways in the literature and both measures were collected and tested in this study. The first measure is consistent with the measure of state conservative ideology used in criminological research on more punitive legislation or social control due to conservative policy agendas and or racial threat. Since Hypothesis three and four are testing this same theoretical frame it is useful to use the same measure which has been employed with some success in this literature (Beckett, 1997; Carmichael, 2005; Jacobs & Carmichael, 2001; Keen & Jacobs, 2009; Welch & Payne, 2010).

Based on Beckett’s (1997) finding of the positive relationship between Republican party ideology and more punitive positions on crime control, and consistent with prior literature (Carmichael, 2005; Jacobs & Carmichael, 2001; Jacobs & Helms, 1996; Keen & Jacobs, 2009; Stucky et al., 2005), “Republican” is used as the trait value to be included as a positive in an additive index indicating a Republican party ideology. Using the U.S. Census Statistical Abstracts of the United States (see Appendix A), one point was given for each indicator present in the state (1=Yes, 0=No) including; Republican governor, Republican majority in the State Senate, Republican majority in the State House of Representatives, both U.S. Senators Republican, Republican majority in US Representatives, and representing citizen ideology, whether the majority of voters voted Republican in the most recent presidential election (Iyengar, 2009). If any one indicator did not reflect a majority, in other
words the parties were evenly split between Republicans and Democrats, no point was given. Additionally, in rare cases in which an Independent Governor held office, or a majority was not reached by either major party due to the presence of Independent party members, no points were given. Finally, the state of Nebraska House of Representatives and State Senate is unicameral and therefore could not be coded along party lines. Thus, Nebraska is eliminated from the sample in any analyses involving this variable, which is noted in the analysis. The result is an additive index of the state’s **Political Ideology Index** with a range of 0 if the state has no Republican majority or Governor in that particular year to a 6 if they had a Republican majority in every category for that particular year.

The second measure of state conservative ideology is one that is often used by political scientists and, at times, in Sociological research on diffusion. This second measure, **State Conservatism Scale**, uses the revised 1960-2006 government ideology measure originally created by Berry et al. (1998) and updated to include more years in 2010 (Berry, Fording, Ringquist, Hanson, & Klarner, 2010 though see Berry et al. 1998 for a complete description of the formulas and calculations used in the creation of the measure). While Berry et al. (1998) also rely on the composition of state elected officials, their measure differs in that it 1) excludes US senators and representatives, 2) is weighted according to a formula of relative strength in the government, for example separate weights for each chamber and the governor, 3) requires a 60% majority rather than a 51% majority, 4) includes ideological scores reported by the political special interest groups (e.g. Americans
for Democratic Action and Americans for Constitutional Action), and 5) imputes substituted values for Nebraska’s unicameral congress.

There are three benefits to using this measure. The first benefit is that Berry and his colleagues have already compensated for Nebraska’s missing data by substituting a weighted score obtained from the average governor’s score and interest group ratings for the given year thus eliminating the need for any additional missing data imputations. Second, the Berry et. al. (1998) measure has been rigorously evaluated and the reliability and validity of the measure are well established (Berry & Berry, 1991; Berry et al., 2010; Berry et al., 1998; Berry, Ringquist, Fording, & Hanson, 2006, 2007a, 2007b; Brace, Arceneaux, & Johnson, 2006). Finally, the measure is substantively a stronger measure for ideology as it accounts for ideological scores rather than just the composition of the congress.

Data containing the revised 1960-2006 government ideology measure (ide06) was obtained via download from Richard Fording’s State Ideology Data website (Fording, 2014), and the data were clean with no missing or corrupted information. Only data for each state for the years 1980 through 2000 were saved and imported in the SAS dataset and scaled so that higher scores indicated a conservative government ideology and lower scores indicated a liberal ideology. This measure was also used as a comparison in cross-validation analyses that were performed to test the face validity of the first measure. If both measures are measuring the same construct, effects should be somewhat similar when one is substituted for the other in a model. Cross-validation analyses found this to be true. Test models were
performed substituting one variable for another and while the size of the effect varied slightly, the direction and overall significance of the effect were similar using either variable.

The conflict component which was hypothesized within this theoretical frame (H4) suggested that states with higher percentages of black populations would be at greater risk for adopting a mandatory or preferred arrest law. The Percent Black in the state was obtained through the U.S. Decennial Census for the years 1980, 1990, and 2000 (U.S. Census, 1980, 1990, 2000) and the U.S. Census’ Intercensal Population Estimates for the years 1981 through 1989 (U.S. Census, 1999), and the Center for Disease Control Bridged-Race Population Estimates for the years 1991 through 1999 (CDC-Wonder, 2014).

**Political Opportunity Structures**

The Political Opportunity Structures Model has two dimensions, 1) the presence of political allies via the presence of a more liberal state government ideology and, 2) the political influence of women in state legislature. Political ideology of the state is measured with the same Berry et al.’s revised 1960-2006 government ideology variable (1998) described in the previous section.

The second aspect of political influence is women’s participation in state legislature. Since prior research has found that the percentage of women in the legislature does not affect policy until they reach “critical mass” of 15% (Kaiser 1997), women’s political influence is measured with a dichotomous variable representing whether women equaled 15% or more of the state legislature. The total number of women in the legislature was collected from the
U.S. Census, Statistical Abstracts of the United States, *Women Holding State and Local Public Offices, by Office, and by States* for all years except for 1983, 1991, 1993, and 1994. These data were consistently available for state house and senate seats only. It would have been preferable to collect data on females holding other forms of government office but the data were unavailable for a good portion of the 1980s and when data were found for the 1990s, it was incomplete, and in some cases found to be inaccurate. Therefore only female Senators and Representatives for the state legislature are used.

Total female legislators by state was collected for 1983 from Council of Women in Politics (Council, 1983) and the remaining years were obtained via the Almanac of the 50 States (Information Publication, 1990). For each year, the total of women holding a State House or Senate seat was divided by the total (male and female) state House and Senate seats to get the percentage of women in the state’s legislature for that year. If the percentage of women in the state’s legislature was 15% or greater, then the dichotomous variable *Female Legislators >15%* was coded as “1”, otherwise it was coded as “0”.

**Missing Data**

Other than what has been mentioned in the description of the variables, missing data was not an issue and no other statistical computations or accommodations were required. This was partly due to the use of Berry et al.’s (1998) government ideology variable had already made imputations for any missing data, and the resources available for state information through the U.S. Census Bureau Statistical Abstracts of the United States.
3.3 Analytic Approach

*Discrete-time Hazard Models*

Careful consideration was given to the analytic technique that would be employed to test the hypotheses in these theoretical frameworks. Modern statistical software packages provide researchers the tools to engage with advanced longitudinal analyses thus providing several options for this type of study. However, discrete-time event history analyses is an appropriate choice for several reasons, the three most important being that 1) the laws are passed on an annual basis, 2) all of the time-varying covariates to be included in the models are collected every one to four years, and 3) there are a number of states that adopted the law in the same year (“ties”).

These data are discrete in nature because the passage of a law occurs at a single point in time during the year. While legislatures meet and debate legislation throughout the legislative session, the law becomes official only after it has passed both chambers and becomes enforceable on the effective date, typically January 1st of the following year for non-emergency legislation. For researchers who study the passage of legislation, the date of the vote is of little concern as the primary interest is in the timing of the adoption of the law relative to the adoption of the law by other states (Box-Steffensmeier & Jones, 1997). Therefore the effective date is sufficient for the purposes in this study and the legislation becomes effective just once per year, which is to say at discrete points in time.
Legislation becomes effective once per year thus increasing the chance that two or more states will have adopted the law in the same year. In the terminology of event history analysis, states that adopt the law in the same year are considered to have “tied” in that they experience the event in the same year (Singer & Willett, 2003; Yamaguchi, 1991). As shown in Figure 2.1 in the previous chapter, this will be particularly salient to these data as there are several years where more than one state adopted law. Discrete-time event history analysis is better equipped to handle ties (Allison, 1984, 1995; Singer & Willett, 2003; Yamaguchi, 1991).

Event history analysis has the ability to account for cases that are right censored, or have yet to experience the event of interest when calculating the hazard (Yamaguchi, 1991). States that have not adopted a law by the end of the study period are considered to be “right-censored” in that there is still a chance they could adopt a law after the observation period has ended.

**Modeling time duration**

Without accounting for duration, a discrete-time logit model assumes a constant hazard rate. This assumption becomes more problematic with discrete-time data because of the multiple records provided by each case. Recall that the data set was transformed from a case-level data set (each case contributes one line of data) to an event-level dataset (each state contributes multiple lines of data until they have adopted the law or the study period ends). This can introduce unmeasured bias in the form of temporal dependence, or “duration
dependency” (Box-Steffensmeier & Jones, 2004:75) in discrete-time data and increase the potential for unobserved heterogeneity (Box-Steffensmeier, Reiter, & Zorn, 2003; Zorn, 2000).

The reason for this is because as time passes and more states drop out of the analysis due to having adopted the law, the remaining cases become populated with states more and more resistant to the law and the average hazard cannot account for this and appears to continue to decline over time (Zorn, 2000). In other words, the early adopters may be states that had some unobserved or unmeasurable predisposition to adopt, or the non-adopters may have some unmeasurable resistance to the law. Without being able to adjust for these unobserved differences, the effect of heterogeneity will “always” be to cause the conditional hazard to increase more (or decrease less) over time (Zorn, 2000, p. 369).

While unobserved heterogeneity is something a research needs to be wary of, it does not preclude the use of discrete-time methods. In Allison (1995) notes that unobserved heterogeneity tends to attenuate the estimated coefficients towards zero, however standard errors and test statistics are not biased so tests of the null hypothesis maintain validity despite the presence of unobserved heterogeneity. Allison points out that attenuation of coefficients is not a problem unique to hazard models. Still, several ways have been suggested to reduce the possibility of unwanted bias due to unobserved heterogeneity.

Some researchers have suggested that the addition of an error term to the model might control for unobserved heterogeneity (Allison, 1995; Box-Steffensmeier & Jones, 2004).
However, Blossfeld (2002) demonstrates mathematically how the addition of an error term to the hazard rate model is problematic because, as one makes assumptions about the distribution of the hazard rate in the model, one would also need to make assumptions about the distribution of the error term. Allison (1995) agrees that the addition of a random disturbance term into the model is “highly sensitive” to the researcher’s choice of hazard model (e.g. Weibull), noting the subjectivity in the imposition of the functional form of the model. Another way to control for unobserved heterogeneity is to have well specified models (Box-Steffensmeier et al., 2003; Zorn, 2000), which is the ideal of every researcher but in practice is not always possible. A more practical method for controlling for duration unobserved heterogeneity is to control for duration dependence (Box-Steffensmeier & Jones, 1997; Box-Steffensmeier et al., 2003; Zorn, 2000).

As Box-Steffensmeier and Jones (1997) point out, some researchers tend to treat time as if it were merely noise or a nuisance and may give little thought to duration dependence or leave time out of the model altogether, a practice they caution against. This point is particularly applicable to these data as evident by the time frame in which the laws were adopted (see Figure 2.1 in the previous chapter). Further, since the adoption of the laws occurred within the same time frame as other more punitive legislation, there is a clear substantive reason to consider the influence of time. Finally, there were several years that no state adopted a law while in other years multiple adoptions occurred creating peaks and valleys in adoption patterns that could not be easily explained. The factors that are believed to influence this adoption (e.g. political parties do not change quickly from year to year) do
not occur with this same pattern suggesting a control for duration dependence is warranted. Although there are multiple ways to control for time within a model (see Singer & Willet, 2003 for a review, also see Buckley & Westerland, 2004 for a comparative analysis), preliminary analyses indicated the best fit for these data is a natural cubic spline. Because of the erratic shape of the baseline hazard, it was determined that manual knot placement would provide a better fit to the data than allowing the software to assign knots at percentiles.

**Cubic spline and knot placement**

A natural cubic spline is a less common but equally viable alternative for modeling duration (Beck & Jackman, 1998; Beck, Katz, & Tucker, 1998; Box-Steffensmeier & Jones, 1997, 2004; Buckley & Westerland, 2004). A cubic spline fits cubic polynomials together at segment points or “knots” that can be predetermined by the analyst (Beck et al., 1998; Hastie et al., 2009). A natural form of the cubic spline was chosen as the natural cubic spline forces a linear function beyond the boundary knots thus freeing several degrees of freedom (Hastie, 2009) and better fitting the data which take on a linear form after 1997.

Because of the irregular shape of the hazard, manually fixed knots or “regression splines’ (Hastie et al., 2009, p. 144) would ensure a better fit to the data. Using the SPLINE EFFECT statement in SAS, a natural cubic spline with a truncated power basis was created with knots placed at times that appeared to hold the greatest transition; time 0 (1980), 5 (1985), 9 (1989) and 14 (1994). SAS generated boundary knots with the end result of the effect being three variables related to time in the models - a time intercept (time 0) and two
SAS generated boundaries to the form the natural cubic spline. A graph of the smoothed baseline hazard function compared to a graph of the smoothed hazard function transformed with a natural cubic spline with knots placed at these points shows that this is a fair approximation of the shape of the hazard with the lowest cost in degrees of freedom (See Chapter 4, pg. Figure 4).

**Analytic Models**

Here, the analytic models are briefly summarized beginning with the model for the baseline survival function (Equation 3.1) where survival (S) at any given time or interval (t) is equal to the probability (Pr) that the event (T) occurring beyond that time or interval. In terms of these data, the likelihood of a state not adopting a mandatory or preferred arrest law in a given year is equal to the probability of adopting the law in subsequent years.

**Equation 3.1**  \[ S(t) = Pr(T>t) \]

The baseline hazard model is presented in Equation 3.2 where the “h(t)” represents the hazard at a given time or interval, “Pr” is the probability, “T” is the event of interest, and the pipe-bar “|” indicates the following term is conditional. Therefore the model suggests that the hazard during a given interval is equal to the probability of the event occurring in that interval, given that the event has not occurred in any previous interval. In terms of these data, the hazard of a state adopting a law in any given year is equal to the probability of it adopting the law that particular year, given that it has not already adopted the law and been dropped from the analysis.
Equation 3.2 \( h(t) = \Pr(T=t \mid T \geq t) \)

The model for all discrete-time logit event history analyses is represented in Equation 3.3 where the log odds (logit) of the hazard \( (h) \) at a given time or interval \( (t) \) is equal to logit of the baseline hazard which is some function \( (a) \) of time \( (t) \) plus the effects \( (\beta) \) of the covariates \( (x_j) \) of the model. These covariates can be time varying or time invariant (Steele, 2005). With the inclusion of the spline effect, the model no longer assumes that the hazard is constant across years. Instead, the model is a piecewise constant discrete-time model in that it assumes the hazard is constant within each year but allows it to vary across years (Singer & Willet, 2003; Allison, 1982).

Equation 3.3 \( \logit[h(t)] = a(t) + \beta x_j \)

Though the general rule has been 10 events per variable (EPV), Vittinghoff and McCulloch (2007) have demonstrated that this number might be too conservative. Their tests of 5-9 events per variable showed comparable effects on several performance measures in event history analyses. Still, given that there are 27 events (changes in law), the models will need to be limited in the number of predictor variables that can be included.

Evaluation Criteria

Statistical significance of the overall model are determined through the Chi-square statistic of the model -2 log likelihood (-2LL), and the statistical significance of the estimates are determined by the Chi-square value of the Wald Statistic for the coefficient. In either case, if the p. value is < .05, it will be considered statistically significant. When comparing
nested models, the difference in -2 LL scores is used. When comparing non-nested models, the difference in Akaike Information Criterion (AIC) statistics is used.

While binary logistic regression does not have an adequate equivalent to the OLS $R^2$ statistic, there are several options available to assess the goodness of fit of the models one of which is a receiver operating characteristic (ROC) curve analysis (Sarkar & Midi, 2010). Therefore, goodness of fit is assessed via ROC curve analyses using the value of the concordance index, (aka the c-statistic or area under the curve [AUC]). The concordance index is a measure of the model’s ability to correctly discriminate between those who do and do not adopt the law, and can range from a minimum of .50 (which is no better than occurring by chance), to a maximum of 1.0 (which is perfect prediction).

In the next chapter, summary statistics, are presented followed by graphs of the baseline survival, survival by region, and baseline hazard. In separate sections, results testing the hypotheses under the diffusion of law, conflict-policy regime, and opportunity structures theoretical frameworks are presented. When the results include statistically significant covariates other than the control for duration dependence, a discussion of the model fit statistics follow the model.
CHAPTER FOUR: RESULTS

4.1 Introduction

In this chapter, the results of all analyses including descriptive, diagnostic, and analytic modeling are presented. The first section in this chapter reports the descriptive statistics for the dependent variable and the covariates used in the analyses. This is followed by images of the baseline survival and hazard functions, including a side by side comparison of the baseline hazard with and without the cubic spline interpolation of the function. Next, information on the collinearity and influence diagnostic tests of the covariates are reported. The remaining sections report the results of discrete-time event history analyses organized by theoretical model.

4.2 Descriptive Information and Statistics

The summary statistics for the dependent variable and the covariates for the discrete-time event history analyses Table 4.1. As previously mentioned, there were no issues with missing data. Because each state contributes information for every year up through and including the year in which they adopt a mandatory arrest law, the frequency shown for the categorical variables reflect contributions from each state for all years in which they contributed information. Therefore the frequency of the variable distribution is not very informative. However, the interpretation of the percentage of the distribution (% column) is straightforward.
Table 4.1 Summary Statistics of Variables in Models

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Min</th>
<th>Max</th>
<th>Median</th>
<th>Std Dev</th>
</tr>
</thead>
<tbody>
<tr>
<td>Period</td>
<td>8</td>
<td>0</td>
<td>20</td>
<td>0</td>
<td>5.73</td>
</tr>
<tr>
<td>Year</td>
<td>1988</td>
<td>1980</td>
<td>2000</td>
<td>1980</td>
<td>5.73</td>
</tr>
<tr>
<td>Law Type (1=Discretionary, 2=Preferred, 3=Mandatory)</td>
<td>1.06</td>
<td>1</td>
<td>3</td>
<td>1</td>
<td>0.34</td>
</tr>
<tr>
<td>Adopted law (Event)</td>
<td>0.03</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>0.18</td>
</tr>
<tr>
<td>Percent Black (1-yr lag)</td>
<td>10.1%</td>
<td>0.2%</td>
<td>36.3%</td>
<td>0.3%</td>
<td>0.09</td>
</tr>
<tr>
<td>State Conservative ideology (1-yr lag)</td>
<td>50.79</td>
<td>1.25</td>
<td>97.92</td>
<td>90.63</td>
<td>22.52</td>
</tr>
<tr>
<td>Female legislators GTE 15% (1-yr lag)</td>
<td>0.45</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>0.50</td>
</tr>
<tr>
<td>Political Index (1-yr lag)</td>
<td>2.38</td>
<td>0</td>
<td>6</td>
<td>2</td>
<td>0.84</td>
</tr>
<tr>
<td>Number in Region with Law (1-yr lag)</td>
<td>1.04</td>
<td>0</td>
<td>5</td>
<td>0</td>
<td>1.34</td>
</tr>
<tr>
<td>Percent in Region with Law (1-yr lag)</td>
<td>17.4%</td>
<td>0</td>
<td>80.0%</td>
<td>1</td>
<td>21.6%</td>
</tr>
<tr>
<td>Number of Neighboring States with Law (1-yr lag)</td>
<td>0.80</td>
<td>0</td>
<td>5</td>
<td>0</td>
<td>1.20</td>
</tr>
<tr>
<td>Percent of Neighbors with law (1-yr lag)</td>
<td>19.3%</td>
<td>0</td>
<td>100.0%</td>
<td>0</td>
<td>27.7%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Categorical Variables</th>
<th>Frequency</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mandatory Arrest law</td>
<td>21</td>
<td>2.72</td>
</tr>
<tr>
<td>Preferred Arrest law</td>
<td>6</td>
<td>0.78</td>
</tr>
<tr>
<td>Discretionary Arrest law</td>
<td>745</td>
<td>96.5</td>
</tr>
<tr>
<td>Region - East North Central</td>
<td>88</td>
<td>11.4</td>
</tr>
<tr>
<td>Region - East South Central</td>
<td>74</td>
<td>9.59</td>
</tr>
<tr>
<td>Region - MidAtlantic</td>
<td>48</td>
<td>6.22</td>
</tr>
<tr>
<td>Region - Mountain</td>
<td>124</td>
<td>16.06</td>
</tr>
<tr>
<td>Region - Northeast</td>
<td>71</td>
<td>9.20</td>
</tr>
<tr>
<td>Region - Pacific</td>
<td>60</td>
<td>7.77</td>
</tr>
<tr>
<td>Region - South</td>
<td>153</td>
<td>19.82</td>
</tr>
<tr>
<td>Region - West North Central</td>
<td>94</td>
<td>12.18</td>
</tr>
<tr>
<td>Region - West North Central</td>
<td>60</td>
<td>7.77</td>
</tr>
</tbody>
</table>

(N=772 state-years)

Dependent Variable: Life Table and Baseline Survival & Hazard Functions

Figure 4.1 models the probability of “surviving”, in this case not adopting a mandatory or preferred arrest law, for all 49 states for the years 1980-2000. As reflected in Figure 4.1 (next page), the survival probability is relatively high and stable the first several years with few states adopting the law between 1980 and 1984. There is a lower probability of survival beginning is 1985, with the decrease falling sharply in 1991 and again from 1994.
The survival probability continues to decline until it hits a plateau in 1997 after which the survival probability is monotonic at .45 (45%).

Figure 4.1 Baseline Survival Probability: Modeling Probability of a State Not Adopting a Mandatory or Preferred Arrest Law by the Year 2000

However, when the baseline survival function was stratified by region of the country, an interesting pattern emerged (Figure 4.2 next page). States in the Northeast and Midwest regions had lower probabilities of survival at the beginning of the study period. In

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15 Figure 4.2 shows the four main Census regions of the United States rather than the 9 extended regions for the sake of clarity. The lines of the extended region survival function were so close together that they became unintelligible.
terms of this study “survival” is retaining a discretionary law and “failure” would be adopting a mandatory arrest law. Among all the states, those in the Northeast and Midwest had the lowest probability of survival (least chance of keeping arrest at the officer’s discretion) through 1996. States in the West had high probabilities of survival well into 1991 and 1993, but then their chances of survival drop sharply in 1996 and then remains the lowest of all the regions for the duration.

Figure 4.2 Baseline Survival Function Stratified by Region: Modeling Probability of a State Not Adopting a Mandatory or Preferred Arrest Law by the Year 2000.

The South however, has the highest probability of survival. This is interesting to note because the Southern states tend to have greater Black populations. This would stand in contradiction to the conflict component of a conservative policy regime attempting to control
a minority underclass (H₄) which will be evaluated via the discrete-time logit regression analysis.

**Baseline Hazard Function and Spline Approximation**

Figure 4.3 shows the baseline hazard probability of a state adopting a law independent of any predictors in the model. There are two features worth special mention - the first is the number of intervals in which no state adopted a law (hazard = 0) and the second is the number of intervals in which several states adopted a law in the same year (indicated by sharp peaks in the hazard).

![Baseline Hazard Probability: 1980-2000. (© Indicates knot location for natural cubic spline at time).](image)

The shape of the hazard function does not indicate a need for concern regarding unobserved heterogeneity as the hazard probability does not reflect a continuing decrease or
increase over time (Zorn, 2000). However, a control for duration dependence is still warranted since, as previously discussed, the years included in this study were a period in which America saw many forms of increased punitiveness, increasing the chance that there may be some underlying factor(s) unique to that time period that are unaccounted for in this study. The patterns observed and discussed in the survival and hazard functions also suggest that there may be some effect of time given the similar trend lines for regions in the early years, periods of non-activity vs. heightened activity, and cessation of adoption in 1996.

Preliminary trials comparing the goodness of fit for models using more conventional methods of controlling for time indicated the shape of the hazard is clearly non-linear, and the flattened U or inverted U shaped curve of a quadratic polynomial, or the long smoothed, s-shaped curve of a cubic polynomial also performed poorly. A smoothed, natural cubic spline performed better, and while a higher degree (e.g. 4th order) spline would improve the fit, it was not enough of an improvement to warrant the sacrifice in degrees of freedom. Therefore a variant of a truncated power function spline basis for a natural cubic spline proved to be the best fitting option.\(^\text{17}\)

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\(^{16}\) These preliminary trials compare deviance statistics between baseline discrete-time hazard models with only the main effect of time modeled (e.g. polynomials, dummy variables) are not shown here but are available upon request. See Singer & Willet, 2003 for a discussion on comparing models with time as a main effect in baseline discrete-hazard models.

\(^{17}\) A truncated power function basis is a piecewise polynomial power function with a value of zero to the left of the knot. Since the natural cubic spline basis forces a linear function to the right of the outermost knot, this combination could account for the sharp peaks and valleys of the hazard function as well as the monotonic flat function after adoptions cease. For further reading on natural cubic splines and truncated power function basis
In order to estimate the fit of the knot placement, the same baseline hazard analysis was performed and the hazard function smoothed for comparison (Figure 4.4, Panel A). Figure 4.4 Panel B shows the hazard with estimated knot placement at periods 0, 7, 12, 15, which is the closest approximation while still keeping the number of time variables that would be required to be included in the models at a minimum.

![Hazard Function Estimate at Midpoint (A) and Spline Effect on Hazard (B)](image)

*Figure 4.4 Smoothed Hazard Function at Midpoint (A) and Spline Effect on Hazard (B)*

With each additional knot the fit of the hazard would be improved. However, every additional knot would result in a loss of one degree of freedom as well. This fitting was determined to be the most parsimonious.

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on how these effects are constructed in SAS software, see SAS/STAT(R) 9.3 User's Guide, SAS Institute, Cary, NC (http://support.sas.com/documentation/cdl/en/statug/63962/HTML/default/viewer.htm#statug_introcom_a0000003344.htm)
4.3 Collinearity and Influence Diagnostics

**Collinearity**

Covariance and collinearity matrices were included in the analysis of each model. Prior to constructing the event history analysis models, collinearity and influence diagnostics were conducted to assess potential bias to the estimates due to issues with multicollinearity and outliers. Since logistic regression programs do not have a method for including tolerance and variance inflation factors in model diagnostic statistics, each model was regressed as a linear regression model and the variance inflation factors and tolerance statistics for the covariates in each model were analyzed. Regressing the model as a linear ordinary least squares regression model has no effect on the collinearity of the covariates and the information for the VIF and tolerance is valid despite having a dichotomous dependent variable (Liu, Kuang, Gong, & Hou, 2003).

The variance inflation factor (VIF) calculates the extent that the estimated effect for one variable is inflated by multicollinearity with other variables in the model. A VIF of less than two is generally an acceptable level (e.g. low collinearity) and values greater than four warrants further investigation or correction (Chen, Yang, Chen, & Chen, 2008; Liu et al., 2003). The tolerance statistic is a measure of the percent of the variance in the estimate that is unique to that variable (e.g. cannot be attributed to other variables in the model). The statistic can range between 0% and 100%, with higher values more desirable.
Values of less than 10% warrant further investigation or correction (Chen et al., 2008; Liu et al., 2003). In no model was the tolerance less than 90% or the VIF greater than 1.5 indicating no problem with multicollinearity with the covariates and the dependent variable within any of the models. Table 4.2 (next page) shows the VIF and tolerances for all covariates except the time spline.

Table 4.2 VIF and Tolerance for Covariates-All Models

<table>
<thead>
<tr>
<th>Model</th>
<th>Tolerance</th>
<th>VIF</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Political Opportunity Models</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>State Ideology (1-yr lag)</td>
<td>.991</td>
<td>1.009</td>
</tr>
<tr>
<td>Female legislators GTE 15% (1-yr lag)</td>
<td>.991</td>
<td>1.009</td>
</tr>
<tr>
<td><strong>Conflict-Policy Regime Models</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>State Ideology (1-yr lag)</td>
<td>.998</td>
<td>1.002</td>
</tr>
<tr>
<td>Percent Black (1-yr lagged)</td>
<td>.998</td>
<td>1.002</td>
</tr>
<tr>
<td><strong>Diffusion of Law Models</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number in Region with Law (1-yr lag)</td>
<td>1.000</td>
<td>1.000</td>
</tr>
<tr>
<td>Percent in Region with Law (1-yr lag)</td>
<td>1.000</td>
<td>1.000</td>
</tr>
<tr>
<td>Number of Neighboring States with Law (1-yr lag)</td>
<td>1.000</td>
<td>1.000</td>
</tr>
<tr>
<td>Percent of Neighboring States with Law (1-yr lag)</td>
<td>1.000</td>
<td>1.000</td>
</tr>
</tbody>
</table>

To assess collinearity with time, correlation and covariate matrices were analyzed with the spline effect.¹⁸ As expected, and similar to correlations when polynomials are included in the same models, the three variables for the cubic spline effect are correlated with

¹⁸ SAS PROC LOGISTIC was used to analyze the models and the correlation and covariances were requested and analyzed with each model (COV & COVB).
each other. However, this is not necessarily an issue as there are no concerning correlation between time and other variables in any model. In correlation analyses, units of measure are standardized to take on a value between -1 and 1 so that variables with dissimilar units of measure can be compared. However, the three time variables created by the spline share the same unit of measurement (years) so there is no need to standardize the measurement to take on a value between -1 and 1. A covariance analysis, where time is not forced to take on a restricted value, shows little covariance between the time variables.

In all models, there were no correlations of concern between covariates and the time variables associated with the cubic spline (Table 4.3). Further, the time variables, while correlated when forced to take a standardized metric, do not reflect any collinearity issues in covariance analyses (Table 4.4). Therefore, collinearity was not a concern in any of the models presented here.

Table 4.3 Correlations of Covariates and Time

<table>
<thead>
<tr>
<th></th>
<th>Estimated Correlation Matrix</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Time 1 (Intercept)</td>
</tr>
<tr>
<td>T1 (Intercept)</td>
<td>1.00</td>
</tr>
<tr>
<td>Time 2</td>
<td>-0.97</td>
</tr>
<tr>
<td>Time 3</td>
<td>0.94</td>
</tr>
<tr>
<td>State Ideol.</td>
<td>-0.34</td>
</tr>
<tr>
<td>Female 15%</td>
<td>-0.05</td>
</tr>
<tr>
<td>% Black</td>
<td>-0.12</td>
</tr>
</tbody>
</table>
Table 4.4 Covariance of Covariates and Time

<table>
<thead>
<tr>
<th></th>
<th>Time 1 (Intercept)</th>
<th>Time 2</th>
<th>Time 3</th>
<th>Conserv. Ideology</th>
<th>Female 15%</th>
<th>% Black</th>
</tr>
</thead>
<tbody>
<tr>
<td>T1 (Intercept)</td>
<td>0.04</td>
<td>-0.01</td>
<td>0.02</td>
<td>0.00</td>
<td>0.00</td>
<td>-0.06</td>
</tr>
<tr>
<td>Time 2</td>
<td>-0.01</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.01</td>
</tr>
<tr>
<td>Time 3</td>
<td>0.02</td>
<td>0.00</td>
<td>0.01</td>
<td>0.00</td>
<td>0.00</td>
<td>-0.01</td>
</tr>
<tr>
<td>State Ideol.</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>-0.01</td>
</tr>
<tr>
<td>Female 15%</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.05</td>
<td>0.22</td>
</tr>
<tr>
<td>% Black</td>
<td>-0.06</td>
<td>0.01</td>
<td>-0.01</td>
<td>-0.01</td>
<td>0.22</td>
<td>5.60</td>
</tr>
</tbody>
</table>

**Influence Statistics**

In order to determine if any specific cases were introducing potential bias to the estimates, influence statistics were requested through SAS PROC LOGISTIC - specifically, Cook’s D statistic, plots of the deviance residuals, and leverage statistics. Given there are only 49 states and any unusual patterns would have been observed and corrected at data entry (there were none other than Nebraska’s unicameral congress), it is not surprising that there were no outliers of any concern in the study.

**4.4 Results of Discrete-time Logit Event History Analyses**

**Diffusion of Law Models**

The results for the discrete-time logit event history analysis which tested the possibility the diffusion of law theoretical framework is presented in Table 4.5. Model 1 contains only the effect of the natural cubic spline. The first variable in the cubic spline (Time 1) serves as both the intercept and the first parameter estimate for spline effect. The
other two variables represent the remaining two segments, or intervals, between the knots of the cubic spline. This is similar to having cubic polynomial in the model with one major difference – the estimate for the first time variable is the estimate at time interval zero which is the same as the intercept so there is no need to include an additional intercept in the model. This saves one degree of freedom and reduces the number of covariates when considering the maximum number of covariates per event. Overall the model is statistically significant (-2LL = 316.26, \(X^2 = \)753.96, p. <.0001), as are all of the coefficients for the cubic spline effect (p. <.0001 for all).

**Table 4.4 Diffusion of Law Models: Discrete-Time Logit Regression, Hazard Probability of Adopting a Law Based on Geographic Proximity/Contagion, 1980-2000**

<table>
<thead>
<tr>
<th>Diffusion Measure</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
<th>Model 5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Time 1 (Intercept)</td>
<td>-1.28**</td>
<td>-1.27**</td>
<td>-1.27**</td>
<td>-1.28**</td>
<td>-1.28**</td>
</tr>
<tr>
<td></td>
<td>(.13)</td>
<td>(.13)</td>
<td>(.13)</td>
<td>(.13)</td>
<td>(.13)</td>
</tr>
<tr>
<td>Time 2</td>
<td>.16**</td>
<td>0.17**</td>
<td>.16**</td>
<td>.16**</td>
<td>.16**</td>
</tr>
<tr>
<td></td>
<td>(.02)</td>
<td>(.02)</td>
<td>(.02)</td>
<td>(.02)</td>
<td>(.02)</td>
</tr>
<tr>
<td>Time 3</td>
<td>-.34**</td>
<td>-.34**</td>
<td>-.34**</td>
<td>-.34**</td>
<td>-.34**</td>
</tr>
<tr>
<td></td>
<td>(.05)</td>
<td>(.05)</td>
<td>(.05)</td>
<td>(.05)</td>
<td>(.05)</td>
</tr>
<tr>
<td>Percentage of States in Region w/ Law</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-.02</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.01)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of States in Region w/ Law</td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-.19</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.19)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Percentage of Neighboring States w/ Law</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-.01</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.19)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Neighboring States w/ Law</td>
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<td></td>
<td></td>
<td></td>
<td>.002</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(.01)</td>
</tr>
<tr>
<td>(c)</td>
<td>0.567</td>
<td>0.573</td>
<td>0.570</td>
<td>0.562</td>
<td>0.595</td>
</tr>
<tr>
<td>AIC</td>
<td>322.26</td>
<td>322.47</td>
<td>323.14</td>
<td>324.26</td>
<td>324.20</td>
</tr>
<tr>
<td>SC(BIC)</td>
<td>336.21</td>
<td>341.07</td>
<td>341.74</td>
<td>342.85</td>
<td>342.80</td>
</tr>
<tr>
<td>-2LL</td>
<td>316.26</td>
<td>314.47</td>
<td>315.14</td>
<td>316.26</td>
<td>316.20</td>
</tr>
<tr>
<td>Deviance (Likelihood-ratio (X^2))</td>
<td>753.96**</td>
<td>755.75**</td>
<td>755.08**</td>
<td>753.96**</td>
<td>754.02**</td>
</tr>
<tr>
<td>Degrees of freedom</td>
<td>3</td>
<td>4</td>
<td>4</td>
<td>4</td>
<td>4</td>
</tr>
</tbody>
</table>

**Notes:** Numbers in parentheses are standard errors. Number of observations in state-years = 772.

**p. < .0001**
Models 2 through 5 test the first hypothesis predicting that state adoption of mandatory arrest laws is based on diffusion of law process in which a state is influenced to adopt a law based on the geographic proximity to states that have already adopted the law. Since the concept of geographic proximity has been measured four different ways in the literature, and each measurement has had some success as well failure as a predictor of diffusion, all four measures are tested in separate models in order to evaluate which geographic measure, if any, affects the hazard probability of adopting the law. All four of the measures tested in Models 2 through 5 were lagged by one year as this approximates the period in which the legislation was up for the vote.\(^{19}\) While all models are statically significant overall, (all have p. values of <.0001), this is solely due to the inclusion of the control for time.\(^{20}\) Geographic proximity was not a predictor of state adoption of mandatory arrest laws. The number of states in the region, or the percentage of states in the region that had already adopted the law (Models 2 & 3) failed to reach statistical significance, Similarly,

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\(^{19}\) Consideration was given some that legislation may take years of planning and debate before making it to the floor for a vote. This might be particularly true for novel or controversial legislation as mandatory arrest legislation might have been in some states. However it is the representatives in place during the vote that ultimately advances or kills a bill, so a one year lag is sufficient as most legislation becomes effective the first of the following year. Still, additional analyses were conducted with a 2-year lag for comparison and the statistical significance of the predictor variables did not change.

\(^{20}\) Separate sensitivity analyses were performed to test the effect of changes in the knot placement in the time spline. Changes to the placement of the knots in the spline did not change the direction of the effect or the overall significance of any of the coefficients in the models, nor did it change the overall significance of any model.
neither the number or percentage of neighboring states that already adopted the law (Models 4 & 5) were statistically significant.

The second hypothesis (H²) predicted that the effects of diffusion would be modified by politically split states. Since the first hypothesis was disconfirmed and there are no effects to be modified, the variable for politically split states was not entered into any model and this hypothesis is not tested.

**Policy regime-Conflict model**

Table 4.5 (next page) presents the results of the discrete-time event history analysis testing hypotheses three and four. Model 1 contains only the cubic spline which is simultaneously both the intercept and a control for time. As found in the diffusion models, Model 1 containing only the effect of the cubic spline (e.g. the intercept and two more time segments of the cubic spline), is statistically significant (-2LL = 316.262, X² = 753.96, p. <.0001).

Model 2 adds the first measure of conservative ideology that is frequently employed in research on conservative policies and conflict in the criminological literature in which conservatism is measured by Republican majority in state and Federal congressional representation, Republican governorship, and citizen votes for a Republican presidential candidate. Model 2b adds the conflict component measured with the variable Percent Black in the population to the model as predicted by the fourth hypothesis (H₄). Model 3 tests the second measure of government conservative ideology obtained from Berry et al. (1998), here
abbreviated as “State Ideology”. Model 3b adds the conflict component to the Model 3, also measured with the percent Black in the population. All variables are lagged by one year except for the cubic spline effect for time.

Table 4.5 Policy Regime Models: Discrete-time Logit Event History Predicting Probability of Adopting Mandatory/Preferred Arrest Legislation, 1980-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 1 Time</th>
<th>Model 2 Political Index Measure</th>
<th>Model 3 PI w/ Threat</th>
<th>Model 4 Berry et al. Measure</th>
<th>Model 4 Threat Model</th>
</tr>
</thead>
<tbody>
<tr>
<td>Time 1 (Intercept)</td>
<td>-1.282</td>
<td>-0.809</td>
<td>-0.638</td>
<td>-0.684</td>
<td>-0.655</td>
</tr>
<tr>
<td></td>
<td>(0.13)</td>
<td>(0.14)</td>
<td>(0.14)</td>
<td>(0.14)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Time 2</td>
<td>0.163</td>
<td>0.107</td>
<td>0.092</td>
<td>0.102</td>
<td>0.099</td>
</tr>
<tr>
<td></td>
<td>(0.02)</td>
<td>(0.02)</td>
<td>(0.02)</td>
<td>(0.02)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Time 3</td>
<td>-0.343</td>
<td>-0.233</td>
<td>-0.207</td>
<td>-0.232</td>
<td>-0.227</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Political Index</td>
<td>-0.46**</td>
<td>0.631</td>
<td>-0.40**</td>
<td>0.672</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.09)</td>
<td>[0.521/.748]</td>
<td>(0.09)</td>
<td>[0.560/.790]</td>
<td></td>
</tr>
<tr>
<td>Percent Black</td>
<td>-8.38</td>
<td>0.00</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.48)</td>
<td>[13.17/4.344]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>State Conservative Ideology</td>
<td>-0.043**</td>
<td>0.958</td>
<td>-0.038**</td>
<td>0.96</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>[0.945/.970]</td>
<td>(0.007)</td>
<td>[0.950/.977]</td>
<td></td>
</tr>
<tr>
<td>Percent Black</td>
<td>-3.67</td>
<td>n.s.</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.15)</td>
<td>n.s.</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>c-</td>
<td>0.567</td>
<td>0.558</td>
<td>0.596</td>
<td>0.622</td>
<td>0.621</td>
</tr>
<tr>
<td>AC</td>
<td>322.26</td>
<td>286.49</td>
<td>288.84</td>
<td>260.620</td>
<td>259.29</td>
</tr>
<tr>
<td>SC(BIC)</td>
<td>336.21</td>
<td>304.96*</td>
<td>291.94</td>
<td>279.21*</td>
<td>282.54</td>
</tr>
<tr>
<td>-2LL</td>
<td>316.26</td>
<td>278.49*</td>
<td>258.84*</td>
<td>252.62*</td>
<td>249.29</td>
</tr>
<tr>
<td>Deviance (Likelihood-ratio $X^2$)</td>
<td>753.96**</td>
<td>762.62**</td>
<td>782.27**</td>
<td>817.60**</td>
<td>820.93**</td>
</tr>
<tr>
<td>Degrees of freedom</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>4</td>
<td>5</td>
</tr>
</tbody>
</table>

Note: Numbers in parentheses are standard errors. Numbers in brackets are 95% confidence intervals. Number of observations in state-years = 772.

*p. < .001, p. < .05 (two-tailed test), † indicates a statistically significant X for the change in -2LL

The first measure of conservative ideology as measured by the Political Index in Model 2 is statistically significant ($\beta = -.46$, p. <.0001), but in the opposite direction predicted. High scores on the Political Index, meaning the more conservative the state scores on the index, are associated a reduced risk of adopting the law. Model 2b adds the conflict component to the model as measured by the percent black in the population (1-year lag).
While the coefficient reaches statistical significance, the effect on the log odds is so slight that the point estimate (exponentiated parameter estimate, or Exp[est]) is in effect .000 (Wald 95% confidence interval for the odds ratio is <-.001 through -.013).\textsuperscript{21}

Model 3 tests Berry et al.’s state conservative ideology measure which includes delegates’ ideological scores in addition to weighted representation of power within the state government. The model is statistically significant (-2LL =252.617, p<.001) and a significant improvement (p. <.001) over the model containing only the effect of time (Model 1). The coefficient of -.043 (p. < .000), however the direction of the coefficient is in the opposite direction than predicted. The negative coefficient for Conservative State Ideology indicates that the states that are more conservative are less likely to adopt a mandatory arrest law. So regardless of which measure is used to test the hypothesis of more punitive legislation being a result of a conservative policy agenda, in the case of mandatory arrest legislation the hypothesis is not supported. Model 3b adds the conflict component to the model and the effect does not reach statistical significance.

\textit{Political Opportunity Structures Models}

Table 4.6 presents the results of the discrete-time logit event history models testing hypotheses five and six which predicted that lower scores on the state conservative ideology

\textsuperscript{21} Wald confidence intervals are known to perform poorly with smaller N’s. For this reasons, profile confidence intervals were analyzed and compared to the Wald CI’s but the difference between the two statistics were negligible, (in most cases less than .01 for any interval) and therefore the profile confidence intervals are not reported here but are available upon request.
scale, (indicating the presence of liberal political allies to women’s issues), and greater female presence in the state legislature would increase probability of adopting mandatory arrest laws. Model 1 containing only the effect of the cubic spline (e.g. the intercept and two more time segments of the cubic spline), is statistically significant (-2LL= 316.26, $X^2 = 753.96$, p. <.0001). Model 2 adds the variable State Conservative Ideology. Overall, Model 2 is statistically significant (-2LL 252.617, $X^2$=817.60, p<.001), as is the coefficient for State Conservative Ideology of -.043 (p. < .000). The value of the -2LL of Model 2 containing the State Conservative Ideology variable (252.62) is subtracted from the 316.262, which is the value of the -2LL of the model with time alone. The result of 63.645, with 1 degree of freedom (df=k_{Model2}-k_{Model1}) is statistically significant at p. < .001. This indicates that the addition of the State Conservative Ideology score was an improvement over just controlling for the effects of time alone.

The negative coefficient for Conservative State Ideology indicates that states with lower scores on conservatism, in other words states with more liberal leaning leadership, are more likely to adopt mandatory arrest laws. Conversely, as the score on Conservative State Ideology increases towards more conservative leadership, the risk of adopting a mandatory arrest law decreases by an estimated .043 for every one unit change towards conservatism in the log odds of the ideology score, net of the effects of time.
Table 4.6 Discrete-time Logit Event History Analysis: Political Opportunity Structure Models, 1980-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\beta$</td>
<td>$\beta$</td>
<td>$\beta$</td>
</tr>
<tr>
<td>Time 1 (Intercept)</td>
<td>-1.28**</td>
<td>-1.28**</td>
<td>-1.28**</td>
</tr>
<tr>
<td></td>
<td>(.13)</td>
<td>(.14)</td>
<td>(.14)</td>
</tr>
<tr>
<td>Time 2</td>
<td>.16**</td>
<td>.16**</td>
<td>.16**</td>
</tr>
<tr>
<td></td>
<td>(.02)</td>
<td>(.02)</td>
<td>(.02)</td>
</tr>
<tr>
<td>Time 3</td>
<td>-.34**</td>
<td>-.34**</td>
<td>-.34**</td>
</tr>
<tr>
<td></td>
<td>(.05)</td>
<td>(.05)</td>
<td>(.05)</td>
</tr>
<tr>
<td>State Conservative Ideology</td>
<td>-.04**</td>
<td>.09**</td>
<td>-.04**</td>
</tr>
<tr>
<td></td>
<td>(.01)</td>
<td>[.95 / .97]</td>
<td>(.01)</td>
</tr>
<tr>
<td>Female Legislators &gt;15% (Ref=&quot;No&quot;)</td>
<td>0.51*</td>
<td>1.67</td>
<td>0.51*</td>
</tr>
<tr>
<td></td>
<td>(.20)</td>
<td></td>
<td>(.20)</td>
</tr>
</tbody>
</table>

**Note:** Numbers in parentheses are standard errors. Numbers in brackets are 95% confidence intervals.

AIC: 322.26

SC(BIC): 336.21

Deviance (Likelihood-ratio $X^2$): 753.96**

Degrees of freedom: 3

---

The effect of this variable in this model is identical to its effect in the policy regimes/conflict theoretical model. However, where the policy regime/conflict framework predicted that those states scoring higher on the scale indicating more conservative leadership would be more likely to adopt (a positive relationship), the political opportunity structure framework predicts states with lower scores, or more liberal leadership will be more likely to adopt. Thus, there is support for the political opportunity structure framework, but not the policy regime/conflict framework.
The odds ratio for the effect is expressed by exponential of the estimate [Exp(Est)], which is $e^{-0.043}$, or .96 or about a 4% reduction in the risk of adopting law. The Wald 95% CI for State Conservative Ideology is between .95 and .97 indicating that, net of the effect of time, the model estimates that 95% of the time the odds ratio in the true population would be between those two values. The low range of .02 in the confidence interval lends confidence in the stability of the estimate.

Model 3 adds the variable indicating whether the state had females comprising 15% or greater of its legislature. That model is also statistically significant and is an improvement over the previous model, though slight. The difference in the -2LLs of Model 3 and Model 2 is 7.18, with 1 degree of freedom, and this is statistically significant at p. <.01). The odds ratio for the effect is expressed by exponential of the estimate [Exp(Est)], which is $e^{0.514}$, or 1.67, controlling for the effects of time. As compared to states where fewer than 15% of the states’ legislators are women, states in which females hold 15% of state congressional seats are just over 1.5 times more likely than states where females make up a lower proportion of the legislature to adopt mandatory arrest legislation, controlling for the effect of the State’s Conservative Ideology and time.

In order to determine the relative fit of the model, fit was estimated by the value of the concordance index representing the area under the receiver operator curve (ROC) and a comparison of the reduction in the -2LL value for the models because the models are nested.
The diagonal line is the .50 line representing the midpoint between specificity (true negatives) and sensitivity (true positives). In terms of these data, true positives are states that adopted a mandatory arrest law that were predicted by the model to adopt a law. True negatives would be states that did not adopt a mandatory arrest law that the model predicted would not adopt a law. The horizontal line indicates the value of chance occurrence. In other words, anything on this line has no predictive value. Of interest here is the area above the line which indicates correctly predicted adopters of mandatory arrest laws.

Figure 4.5 displays the ROC curve for Model 3 containing time and the State Conservative Ideology score. The graph in Figure 4.5 shows that the trend is above the 50% mark, so the Model 3 estimating State Conservative Ideology while controlling for the effects of time is better than chance alone. The concordance index (aka c-statistic quantifies the area under the curve and the c-statistic for Model 3 is .635, or 64% which is 14% better than random chance alone, yet only 2% better than Model 2 (c=.62 or 62%) but still 7% better than Model 1 containing only the control for time (c=.567 or 57%). Overall, the support is found for both hypotheses five and six.
The next chapter will recap these results and frame these findings within the context of the theoretical frameworks studied. The implications of this study for domestic violence researchers, women’s advocates, and lawmakers will also be addressed. The chapter also discusses suggested directions for the field of mandatory arrest legislation research to begin a more extensive study based on these preliminary findings.
CHAPTER FIVE: DISCUSSION AND CONCLUSION

5.1 Introduction

The goal of this study was to test three potential explanations for the passage of state legislation of mandatory arrest in the cases of domestic assaults. The theoretical explanations tested, 1) diffusion of law, 2) policy regimes/conflict theory, and 3) political opportunity structures have been shown to be useful in explaining the passage of other forms of punitive legislation. Mandatory and preferred arrest legislation reached its peak activity around the same time period as other forms of more punitive legislation that received a fair amount of research attention in the criminological literature, it was particularly surprising then that more attention had not been given to the application of these theories as potential explanations for domestic violence arrest legislation as well. While criminological literature and family violence text books provide reasonable assumptions about what might have led states to adopt mandatory or preferred arrest laws, this study is the first to empirically evaluate dominant theoretical explanations for the adoption of mandatory arrest legislation.22

In addition to identifying areas lacking empirical research and being able to tease out new questions yet to be addressed, one of the key advantages of exploratory research is identifying the most promising direction for continued research in the topic area (Stebbins, 1977).

22 Sherman and Cohn (1989) surveyed police agencies asking if they adopted a mandatory arrest policy and if they had heard of the Minneapolis Domestic Violence Experiment, but this was not a test of these theoretical frameworks.
While the previous chapter presented the results of the analyses testing each theoretical explanation for the adoption of mandatory arrest legislation, this chapter discusses the implications of those findings. The first section following this introduction discusses the findings in terms of each theoretical framework’s potential as an explanation for the adoption of mandatory arrest legislation. This is followed by a discussion of the implications of these findings for domestic violence researchers and advocates. Next, the limitations of the study are discussed and followed by suggestions for future research.

5.2 Theoretical Implications

Implications for Diffusion of Law Explanations

Although the diffusion of law theoretical framework has been successful in explaining hate crime legislation (Jenness & Grattet, 2001), state lottery adoption (Berry & Berry, 1990), and rape law reform (McMahon-Howard, 2011), it was not successful as an explanation for states adoption of mandatory arrest laws in this preliminary test. Measuring the contagion effect by the number or percentage of states in the region with the law, or the number or percentage of neighboring states with the law did not make a difference as the only affect that was significant in any of the models was the effect of time.

One possible explanation for the lack of findings for diffusion might be in the first phase of the policy domain - the issue creation. Of the four phases of a policy domain, adoption is the second phase yet the first phase of issue creation must be successful before adoption takes place (Burstein, 1991). Clearly, feminist and women’s advocates were
successful in making violence against women an issue, as evidenced by the passage of the Violence against Women Act (VAWA) in 1994 and subsequently the establishment of the Office on Violence Against Women (OVW) in 1995. The OVW credits the passage of VAWA to the “efforts of a broad, grassroots coalition of advocates and survivors who informed the work of Congress” during the two decades preceding the passage of the act (King & Zeng, 2001). So while there was certainly an issue created, one that prompted the passage of legislation at the federal level, the issue creation at the state level may not be as incontrovertible.

Issue creation at the state level would have a crucial role. In order for legislation to diffuse across states the issue needs to generate enough interest to further dissemination. In other words, in order for state legislators to hear about what’s happening in neighboring states and to know about the legislation adopted by their neighbors, it needs to be considered important or newsworthy and one way this happens is through issue creation. So one question might be, “Why was issue creation successful at the federal level yet failed to launch at the state level”? Certainly dedicated women’s advocates were talking to state congressional leaders, opening shelters, and working to advance victim services and educate people in their own state about the cycle of violence and its potential lethality. The chances for failure or success of issue creation at the state level might be related to differences in public and legislator perceptions of domestic violence victims and offenders.

Residents and legislators in one state may have different perceptions of domestic violence victims and offenders than residents in another and these perceptions might be
different than perceptions of victims of hate crimes or victims of rape. In very conservative states, citizens and legislators potentially could have perceived domestic violence to be an issue that should remain in the private sphere, with intervention from police occurring only in the event of the potential for serious injury or death. In these states the legitimacy of sexual symmetry in offending may be more prevalent with common couple violence perceived as the norm, for example in states that elect judges that openly display this sentiment in family court (Epstein, 1999; Harrison, 2008). These perceptions of a woman “giving as good as she gets” and the knowledge that many women stay in or return to abusive relationships make it difficult for those with little knowledge of the complexities surrounding issues of domestic violence to identify these women as victims and in turn, recognize domestic violence as a serious issue (UNICEF, 2000).

Historically, domestic violence has been seen as a private issue or has not been taken seriously by the criminal justice system, and in some states this disregard is clearly more visible than others. In some states, judges presiding in domestic violence cases often visibly display how little regard they hold for the seriousness of the domestic violence as a crime (Florida Supreme Court, 1996; Harrison, 2008; Hemmens, Strom, & Schlegel, 1998). Deborah Epstein (1999) offers examples to illustrate this problem, such as a Maryland judge who, (while handing down an 18 month sentence to a man who had shot his wife in the head for cheating on him), states "The most difficult thing that a judge is called upon to do ... is sentencing non-criminals as criminals."(p. 43). In another example she describes a Florida judge, who after hearing testimony that a man had doused his wife with lighter fluid and set
her on fire, “burst into song in open court, crooning, "You light up my wife," to the tune of "You Light Up My Life." (p. 43). Others report similar evidence of judges’ failure to realize the seriousness of domestic violence charges with opening comments such as “I don’t have time for this” (Florida Supreme Court, 1996; Appendix 4), or "Well, well, well, we had a little domestic squabble, did we? Naughty, naughty. Let's kiss and make up and get out of my court." (Epstein, 1999:43) or telling a women whose husband had raped and choked her in front of her child “Our forefathers used to have sex in front of the kids. There are no effects on a child of 2 years witnessing this” (Florida Supreme Court, 1996; Appendix 4).

This is not to say that all judges hearing domestic violence cases are insensitive to the seriousness of domestic violence, however those that openly display their trivialization of domestic assaults may also be representative of those who are wise enough to avoid displaying their sentiments for the public record. One indication that these sentiments existed even if not vocalized can be found in the lighter sentences imposed on offenders in domestic violence cases compared to similar attacks on strangers (Epstein, 1999).

This same denial of the victim was occurring during the creation of rape as an issue and still occurs to a lesser extent today. While rape myths and blaming a rape victim for being complacent in, or partially responsible for, their own victimization, it is arguably less acceptable to promulgate such views. We can see this in media backlash against judges who perpetuate rape myths by engaging in victim blaming, often resulting in a call for sanctions against the judge. For example, a Dallas judge who sentenced an admitted rapist to probation because the 14 year old victim “wasn’t the victim she claimed to be” essentially meaning she
was not a virgin, had to recuse herself from the case after women’s advocates expressed outrage over the light sentence and her callous remarks (Emily, 2014; Mazza, 2014). In another recent case citizens picketed the courthouse of a judge demanding the recall of a judge who sentenced a 49 year old teacher convicted of three counts of rape of a 14 year student whom he had coerced into having sex with him over a period of years to just 31 days in jail. As part of the rationale for his leniency, the judge said that the girl “seemed older than her chronical age”. However, the judge could only have gotten that impression from videotapes as he had never met the girl; she had committed suicide while waiting for the case to go to trial (Vercammen & Lah, 2013).

Increasingly then, it could be argued that public perceptions of rape victims are changing in the direction of holding women blameless in their victimization. However, the same cannot be said for domestic violence victims. In this way domestic violence differs from victims of rape and hate crimes. A victim of a hate crime is generally not subject to blame for their own victimization. Therefore, it may be more difficult to create an issue of domestic violence at the state level given the variability in perceptions of women as domestic violence “victims”.

Although the analysis testing the diffusion of law explanation was preliminary in nature, the model tested in this study included the theoretically relevant variables. While there may be unidentified factors influencing the diffusion process that are not measured here, for example participation and attendance of legislators in regional conferences, or
membership in professional organizations and other venues in which policy ideas would be shared, diffusion did not play a role in the adoption of mandatory arrest legislation.

**Implications for Policy Regimes/Conflict Explanations**

Conservative policy regimes rooted in conflict theory has evidence of strong support in the criminological literature as explanations for punitive legislation. This is particularly true of the increased punitiveness in the time period of the 1980s and 1990s, which saw the passage of three strikes legislation, state adoption of mandatory sentencing laws, and harsher penalties for drug crimes in America’s “war on drugs” (Tonry, 2013). Although all domestic violence mandatory arrest legislation except for Oregon was also passed in the 1980s and 1990s, no evidence was found for conservative policy regimes as an explanation.

Conservative policy regimes are based on the idea that the more Republicans holding state Senate and House seats, the more likely more punitive legislation will be passed. This approach is based on the premise that conservative legislators adopt a policy regime or platform that supports higher levels of formal social control (e.g. more police involvement, longer incarceration). This study found the opposite to be true in that mandatory arrest legislation was more likely to be adopted with higher Democratic representation in the House and Senate, not Republican. Therefore, there was no support for conservative policy regimes as an explanation for the adoption of mandatory arrest laws for domestic assaults.

Further, the conflict perspective assumes that legislation is more likely to be passed when minority populations increase thus threatening the position of the dominant class. This
was measured similar to the way it is measured in the research finding it to be a contributing influence to legislation, which is the percent black in the population. However, when the percentage of Black residents in the state’s population was added to the model employing Berry et al.’s (1998) measurement of government ideology the measure was not significant.

However, studies that have found this to be a significant predictor do not measure political ideology with the Berry et al.’s measure. When the state’s political ideology was measured with a political index comparable to those used in studies that have found the percentage of the black population to be a significant predictor of punitive legislation, the coefficient was statistically significant but the size of the effect was close to zero. Thus, of interest here is the lack of support for a conservative policy regime.

Again, finding that a conservative state ideology was not associated with a greater risk of adopting a mandatory arrest law might be explained by the distinction of domestic violence as belonging in the private sphere. Conservatives generally support the traditional, heterosexual, families in which the male is the breadwinner and the female is the homemaker. Conservatives are also known to promote traditional family values that often include a religious element (Catlett & Artis, 2004; Coltrane, 2001). Though not necessarily condoning violence, many faith-based organizations still take the position that domestic violence is a private issue between the husband, wife, and God/Allah (Firth, 1993). Additionally, in the faith-based counseling provided by most religious organizations the focal point is on preserving the marriage at all costs and thus many faith-based counselors are slower to suggest that a battered woman leave the relationship than are secular counselors.
(Firth, 1993). While well-intentioned clergy committed to the goal of helping their flock is the norm, the lack of professional training in domestic violence issues is in part responsible for securing the place of domestic violence in the private sphere and perpetuating optimism that the abuser can change with a little help (Firth, 1993).

For these reasons, domestic violence may be unlike other forms of crime for which conservatives believe warrant state intervention. Much of the punitive legislation passed through conservative policy regimes concerned crimes that affected society as a whole, in that any member of society could either directly or indirectly become a victim. For example, mandatory sentencing and “three-strikes” legislation resulted in removing criminals from society to incarceration, and thereby eliminating risk to every citizen with whom they could potentially come into contact. Increased penalties for drug crimes were intended to deter the manufacturing, sale, and possession of illegal narcotics and thus, in the eyes of most law-abiding citizens, make the streets safer and reduce America’s drug problem. In other words, these are forms of legislation involving crimes that affect the public sphere—a threat to the general public. On the other hand, domestic violence affects an individual and perhaps friends and family in a supportive role in the victim’s life. Otherwise the risk of domestic violence is not salient to the average citizen. Therefore, there may not have been a need for conservatives to run on a platform of getting tough on this form of crime in order to please their constituents.
Implications for Political Opportunity Structure Explanations

Political opportunity structure shows promise as an explanation in the adoption of mandatory arrest laws. It was hypothesized that states demonstrating a more liberal political ideology would be more likely to adopt mandatory arrest laws and there is some support for this in that states with more liberal scores on the Berry et al.’s ideology government ideology scale were more likely to adopt a mandatory arrest laws than those that scored on a conservative end of the scale. This suggests that Democrats may have served as political allies for domestic violence advocates as they attempted to pass mandatory arrest legislation to serve women in their states. Further, when women made up 15% or more of the seats in the state House and Senate, states were 1.6x more likely to pass mandatory arrest laws.

Although not directly tested here, these findings are consistent with scholars who have suggested that the increase in state adoption of mandatory arrest laws was the result of the women’s movement, specifically women-orientated activism and pressure from activist groups. Feminism and the women’s movement has historically been liberal in nature and therefore more closely aligned with Democrats than Republicans (Meyer, 2004).

5.2 Implications for Feminist Scholarship and Advocacy

There are several women’s organizations that are focused on decreasing gender inequality and ending sex discrimination. Many of these organizations work towards influencing the legislature to pass laws that address issues that disadvantage women vis a vis men. The results of this study is useful for these efforts in that they suggest the importance of
having political allies in the state House and Senate, as well as the potential of women legislators as political allies for passing such legislation. States with more liberal ideology in government leadership are more likely to pass a mandatory arrest law. This was true whether it was measured by weighted strength of political party representation and ideological voting scores (e.g. Berry et al. 1998) or by the percentage of seats held by one party in the House and Senate and party of the Governor. Further, states in which females held 15% or more of the congressional seats were 1.6x more likely than states with fewer female congressional representatives to adopt mandatory arrest legislation. In fact, the percentage of female legislators had the largest effect on the probability of adopting a mandatory arrest law than any other predictor tested in any model. However, female representation in state legislatures may not necessarily translate to legislation that is beneficial to women, as the next section illustrates.

**Political Allies or Failed Representation?**

As mentioned previously (Chapter 1), mandatory arrest legislation has received much criticism since its inception with the strongest argument being that the legislation harms the victims it was intended to protect. Since states began enforcement of mandatory and preferred arrest policies, arrests of women increased (Bureau of Criminal & Analysis, 1999) often as a result of men demonstrating defensive wounds from women hitting back or defending themselves from attack (DeLeon-Granados et al., 2006; Feder & Henning, 2005; Finn & Bettis, 2006; Frye et al., 2007; Heany, 2005). Clearly, legislation created to assist
women and get them out of situations that were causing them harm resulted in negative consequences for the very victims they were intended to protect. This is particularly true for women of color and women in lower income groups as mandatory arrest policies often meant the removal an adult income earner from the home. If the male was the sole breadwinner and the arrest resulted in missed time from work, then the arrest had even more far-reaching consequences in that the household no longer had a source of income. In states with mandatory arrest laws, African-American women in low income neighborhoods were more hesitant to call on police for help in times of need often resulting in higher rates of domestic violence-related injury and deaths (Miccio, 2005; Plass, 1993; Stark, 2004).

Certainly women’s advocates who worked with Congress to pass the Violence against Women Act (VAWA) and those who work for states to encourage states to adopt mandatory arrest policies did not intend for dual arrest to occur, nor intentionally planned on the laws making women hesitant to use police for help when needed. Yet this study finds that percentage of female legislators did influence the adoption of mandatory arrest laws, and it’s doubtful that these women legislators had any intention for these negative consequences. What might have gone wrong?

One suggestion is seen in Ange-Marie Hancock’s (2004) study of welfare reform. Hancock identifies the role that female legislators played in welfare reform as having the best of intentions. However, as found with domestic violence legislation, the consequences of the legislation failed to serve the population most at need. In the case of welfare reform, Hancock suggests that one reason the policies championed by female legislators failed these
women is because the legislators did not have the same life experiences as the women whom they were trying to serve. Without getting the input of those for whom the law would be impacting, the legislators had no idea of the complications the legislation would invoke for some of the women. Similarly, champions of mandatory arrest legislation did not share the same lived experiences of the women to whom the law would most likely impact. The (mostly) white, mid-to upper income women may have operated under the belief that by requiring police officers to make an arrest, and remove the assailant from home, they were protecting the woman and at the same time relieving the woman of all responsibility for, (and ergo any anger or retaliation for), the perpetrators’ arrest. This finding points to the need for the involvement of those most impacted by law during the process of enacting legislation intended towards advocacy.

A Strategy for Future Action

The fact that no support was found for diffusion as an explanation for the adoption of mandatory arrest legislation, and the potential reason for this being the failure of issue creation at the state level, suggests a strategy for implementing policy change in the future. In addition to engaging with all stakeholders in advocacy-based legislation, this research also suggests that it will be useful to focus attention on individual state level, as well as the national level when working towards change. This means ensuring that the issue is sufficiently created at the state level. This would involve tailoring national campaigns to reflect the culture and population of the state in which they are trying to pass the legislation.
Extensive research on the attitudes and perceptions of the population and legislators in the state might be helpful in order to prepare plans to combat preconceived stereotypes or prejudices that might prevent the legislation from passing. Such an examination could also be used to identify the key concerns and needs of the voting population in order to find ways to make the legislation more salient and relatable to the voting population.

The sudden cessation of mandatory arrest laws in domestic assault cases also raises some interesting questions. There are two potential explanations for why states might have suddenly stopped adopting these laws. One might be that with the creation of VAWA in 1995, women’s advocates felt that their fight was over and reduced their pressure on states to pass mandatory arrest laws. However, that would be difficult to test outside of qualitative interviews with those that were actually involved in the lobbying effort at the time that the legislation ceased and finding those parties involved might prove to be a daunting task. However, while it would be difficult, it would not be impossible.

The second explanation is that the research that was published post 1990 that voiced concerns related to the laws, such as dual arrest, the denial of women’s advocacy, the reduction in the use of police by low income women, and other such problems, might have become known to advocates and legislators alike. However, testing this as an explanation for the cessation of adoption is as problematic as testing Sherman & Cohn’s (1989) claim that the publication of the Minnesota domestic violence experiment was the impetus to states’ adoption of mandatory arrest laws. State legislators have aides to pull and summarize research for legislative committees before their legislators vote on particular bills for
legislation. It is possible that several states introduced a bill after 1996, but legislators had seen research that criticized these laws, and therefore the bills never made it past a vote. Alternatively, it’s possible that these bills were never introduced after 1996 because of research that pointed out criticisms, and if this is the case this would be very difficult to discern.

However, another question left unanswered is why do these laws persist given there is a fairly large amount of research illustrating negative consequences of these laws, as well as men’s advocacy groups that claim these laws are biased against men? Most recently, Sherman conducted a 25 year follow-up with some of the women in the original study he conducted with Berk in 1984 and found that women from the original study who were in the arrest condition had higher death rates than those in the non-arrest condition (Sherman & Harris, 2013). However, the cause of the death was not from domestic violence. Still, Sherman and Harris suggest that for the low income women who were in the mandatory arrest condition the stress of their partner being arrested resulted in posttraumatic stress disorder (PTSD), which in turn led to health complications and higher mortality. Although Sherman had originally been in in favor of mandatory arrest policies, the replication studies conducted after his and Burke’s original study and his most recent follow-up study has caused him to question whether or not these policies are doing more harm than good.

With this in mind, it is puzzling why adoption stopped but did not reverse. It would not be unheard of for state to reverse a law that has already been passed in the legislation. For example, Nebraska, Tennessee, Idaho, Kentucky, and South Dakota all ratified the Equal
Rights Amendment only to officially rescind it one to six years following its ratification (Epstein, 1999). While the National ERA does not recognize their decision to rescind, the states do not record their state as having ratified the amendment.23 One of the main purposes of doing research on the legislative process is to determine factors that influence legislators to enact one form of legislation over another. Once these factors are identified, these same factors can be employed to enact legislation or reverse legislation depending on the needs of the population. It is with this goal in mind that this study has laid the groundwork for future research to continue to examine these questions in greater detail.

5.3 Limitations and Suggestions for Future Research

As an exploratory study, this study was successful in achieving its goal of identifying promising directions future research might take to expand our understanding of the ways laws aimed at addressing family violence might resemble or differ from laws not involving the private sphere. However, given that only twenty-seven states adopted the law over a twenty-one year period, this study was limited in the number of variables that could be included in each analyses and therefore the models are underspecified. While unobserved heterogeneity did not appear to be a problem, as evidenced by the hazard rate that did not continue to increase or decline with time, this was an indication that the models were not showing evidence of frailty. There are many other factors that could influence a state’s

23 However, the Lt. Governor of Kentucky Thelma Stovall vetoed the rescission bill while acting Governor of the state.
legislative decision making, including activity of lobbyists, pressure from women’s 
advocates, or even a legislator with a personal agenda to see that the legislation is passed. 
While the nature of the study is exploratory and therefore not intended to serve as a 
predictive model, (Stebbins, 2001), it is unclear how these unmeasured influences may have 
affected states’ adoption of these laws. More work is necessary to identify these potential 
influences and control for them in the future.

While this study examines potential influences on state adoption of mandatory arrest 
laws, it can only distinguish between states that have a law based on when it became 
effective and a state that does not have a law. It cannot account for states in which a bill was 
introduced but failed in the legislative vote. This would be important information as the 
factors that would cause such a bill to fail to achieve a passing vote would provide far richer 
information in that it would present both the positive case (adopting the law) and the negative 
case (refusal to adopt the law), and any explanation that could explain both the positive and negative case is a stronger explanation.

Additionally, there are several states that place limitations on when the mandatory 
arrest statute applies, for example it is applicable in cases of a felony assault but not a simple 
assault, and other states with a mandatory arrest law that do not make such a distinction. 
Other small variations to when an arrest is mandated included being called out to the home 
twice within a specific time period, violation of a restraining order, or if the officer feels 
there is still a potential risk of violence should an arrest not be made (see Zeoli et al. for a 
more detailed description of these variations). However, to capture these small specifications
would have meant expanding the number of categories of arrest laws to the point that several categories would have only one state in the category. Such a reduction would be a problem for statistical analysis given the small number of cases that adopted the law during the study. So while the categorizations of arrest laws were consistent with the large body of existing literature discussing these laws, these small differences should be taken under consideration.

One interesting finding is the consistent statistical significance of the control for time duration in every model tested. Although “time” itself cannot be a causal predictor (Allison, 1995), there are clearly some factors present in these particular years that impacted some states more than others. This may be particularly true for states in the Midwest and Northeast as states in these regions were the early adopters as indicated in the survival function by region (see Figure 4.2). Additionally, there are peak years, such as 1991 where the law became effective in seven states, as well as periods of non-activity where the law did not become effective in any state in that year. Identifying the political climate of the states through newspaper articles and archived television news media has the potential to capture additional factors that could affect the legislative process. Additionally, these records might indicate whether or not there was any indication of sensationalism of any domestic violence cases in the media, which might have put pressure on a state to take legislative action. Further, media sources or court records would have an indication of whether or not a police agency was the subject of a lawsuit involving failure to protect in a domestic violence case, akin to the Tracy Thurman case mentioned at the beginning of this study. Any type of civil
litigation against the police department, or the fear of legal liability could be controlled for as an influential factor during those peak years by examining these types of records.

Also noteworthy is the complete lack of adoption of the law after 1996 as previously discussed. Future research might employ a mixed methods design using archival records on the strength of women’s organizations within a state over the years leading up to the adoption of a mandatory arrest law coupled with interviews with women’s advocates within the state who actively worked on campaigns to encourage states to adopt mandatory arrest laws.

Ideally, tracking the activities within a state before the introduction of the bill and then following the bill through committees and then through chambers would be ideal. Granted, this type of study would take a good deal of money and time. However, it would provide the richest source of data and definitive answers to the question of what factors played a key role in the states adoption of mandatory arrest law. Digital technology has provided excellent resources for future research in this area. In 2000, several states began recording or making digital copies of transcripts from state legislative sessions and some subcommittee hearings and making them available electronically. Content analyses of these transcripts would add valuable insight into the reasons these laws were passed in those states.

Finally, given the influence of female legislators, future research should examine the role of female legislators in respect to this legislation in greater detail. Gathering additional information on the political party and the office held, length of term, committees served, committees chaired, and voting record would allow for an examination of gendered
opportunity structures as an explanation in mandatory arrest legislation. More detailed information on female legislators as political allies for domestic violence advocates would be a valuable tool in identifying whether failed representation was indeed a factor in mandatory arrest legislation as it has been suggest to be for welfare reform.

5.5 Conclusion

The primary goal of this study was to test several potential explanations for the adoption of mandatory arrest laws in cases of domestic assaults. In doing so, it became clear that the same explanations that were important in explaining punitive legislation that were not related to domestic violence, (e.g. three strikes legislation, mandatory sentencing) were not as useful in explaining the passage of mandatory arrest legislation. Specifically, the major theoretical perspective that had been most helpful to criminologists in explaining the passage of more punitive legislation during the 1980s and 1990s, policy regime/conflict theory, did not help explain the adoption of mandatory arrest legislation that occurred during the same time period. Likewise, the diffusion of law theoretical framework has been helpful to criminologists in explaining hate crime legislation and the spread and timing of other legislation. However, this too was not very useful for explaining the adoption of mandatory arrest laws.

While the political opportunity structure explanation seems to be a promising direction for researchers in search of answers related to why states adopted a mandatory arrest laws when they did and why other states chose not to adopt such laws, perhaps a more
interesting question is why did all adoption cease in 1996? If these laws were thought to be poor laws and therefore should no longer be adopted, then why have states that already have the laws not repealed them from their statutes? Granted, as discussed earlier, some states with a mandatory arrest laws also passed a primary aggressor laws, presumably in order to address criticisms of unjustifiable dual arrests. States’ efforts at reducing dual arrests via the adoption of primary aggressor legislation proved to be successful as dual arrests in states with primary aggressor laws declined after their adoption (Hirschel, Buzawa, Pattavina, Faggiani, & Reuland, 2007). Yet if adding the requirement that the officer determine and arrest the primary aggressor was the only fix necessary to what was otherwise a functional piece of legislation, then why didn’t the adoption of these laws continue, albeit coupled with primary aggressor laws?

So what are the factors that miraculously ended all adoption of mandatory arrest laws? This might prove to be an important puzzle for domestic violence researchers to answer. If there is merit in the criticisms directed at these laws and these laws are causing more harm than doing good then whatever factors are responsible for causing states to stop adopting the laws may be crucial in encouraging states to repeal the laws, or amend them to make them more beneficial to those they are purported to serve. This research puts us one step closer to identifying how such this process might occur.
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# Appendix A. Sources: Table Titles and Numbers by Years

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* Calculated % by adding upper and lower house for the year as total reps/senators and then calculated % female.
### Appendix B. Neighboring States

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