

DOMESTIC VIOLENCE LEGISLATION: EXPLORING ITS IMPACT ON THE LIKELIHOOD OF DOMESTIC VIOLENCE, POLICE INVOLVEMENT, AND ARREST*

LAURA DUGAN

University of Maryland

National Consortium on Violence Research

Research Summary: *This research uses data from the National Crime Victimization Survey to test whether legislation affects domestic violence, police involvement, and arrest. Findings suggest that most laws do reduce the chances of family or intimate violence. Fewer appear to influence police involvement, and none relate to more arrest. This suggests that laws may deter would-be offenders from harming family and partners.*

Policy Implications: *The findings suggest that states should continue to aggressively pursue domestic violence offenders. However, local officials should recognize that mandatory arrest laws could reduce the number of cases that enter the system. Further, more protection should go toward victims when the state awards them custody of the children.*

KEYWORDS: Domestic Violence, Legislation, Policing, Arrest

State legislatures have increasingly passed statutes authorizing criminal justice officials to pursue domestic violence offenders more aggressively (Dugan et al., 2003, Harvard Law Review, 1993). Domestic violence laws are designed to either reduce subsequent violence after an incident (e.g., statutes authorizing the courts to issue protection orders) or to prevent latent violence from surfacing (Harvard Law Review, 1993). For instance, by changing domestic violence offenses from misdemeanors to felonies, temptations to attack an intimate could be tempered by the risk of lengthy incarceration. Pursuant to these goals, one would also expect domestic violence law to impact victims' reporting behavior and police officers' arresting decisions. Strong laws can assure victims that they will be safer once the police intervene. Further, if officers are more confident that the

* Modified versions of this paper were presented at the Homicide Research Working Group in St. Louis, June 2002, and the National Consortium on Violence Research Summer Workshop, also in June 2002. This project was supported under the National Institute of Justice Grant 97WTVX0004 and the National Consortium on Violence Research. I would like to thank Daniel Nagin, Jacqueline Cohen, David Merrill, and Renee Fields for their support during this project.

system will successfully detain violent offenders, they may be more compelled to arrest.

Yet, as proactive domestic violence legislation receives widespread political support, only a small body of research assesses its efficacy. Limitations in crime-related data sources preclude analysts from accurately measuring behavioral responses to legislative changes (Crowell and Burgess, 1996). For example, data from the Uniform Crime Reports (UCR) only include offenses that are known to the police. Because laws may encourage or discourage victims from seeking police support, the true policy effect on violence as determined by UCR data is likely obscured by reporting bias.¹ Further, UCR data combine domestic violence with other forms of domestic disturbances. Thus, "domestics" also include drunken brawls between friends during a football game.

A second crime data source is the National Crime Victimization Survey (NCVS), which is administered to a sample of U.S. residents living in homes selected randomly using a stratified cluster sampling strategy. Respondents are questioned about their experiences as crime victims regardless of whether the police were contacted. Additional detail distinguishes incidents by the relationship of the offender and victim and reports the consequences of each crime. Yet, NCVS data also have shortcomings. Confidentiality restrictions suppress geographic identifiers, making it impossible to link policy information to specific respondents or incidents. To remedy this, the government recently changed procedure to allow "sworn-in" researchers access to the geocoded NCVS data under strict protocols that preserve confidentiality. The current research uses the geocoded NCVS data.

Knowing the legislative profile of the states where family or intimate partner crimes were committed is crucial if we are to more fully understand how policy relates to violent outcomes. By linking specific statutes to NCVS data, this research is the first to estimate how legislation impacts the probability that a household suffers from domestic violence using a nationally representative sample. Further, because effective policy depends upon its implementation, this research also explores how legislation influences police involvement and arrest.

LEGISLATIVE IMPACT ON DOMESTIC VIOLENCE

The primary objective of this research is to estimate the influence of domestic violence statutes on the likelihood that a household suffers from family and intimate partner violence. To date, two research strategies dominate the literature examining policy effects on domestic violence.

1. One exception is the UCR's Supplementary Homicide Reports, because homicides are almost always reported to the police. (See Dugan et al., 1999, 2000).

The first relies on experimental or observational data and includes only individuals known to be victimized by a partner or family member. Tolman and Weisz (1995) use official police and court records in a jurisdiction with strict policy to examine the relationship between arrest and successful prosecution on the likelihood that the offender recidivates (see, also, Berk et al., 1986; Sullivan and Bybee, 1999). They found that arrested persons without a prior record of abuse are most likely to refrain from further abuse.

A series of arrest experiments sponsored by the National Institute of Justice (NIJ) produced mixed results, leaving policy makers unsure of the efficacy of mandatory arrest laws and pro-arrest policies (Garner et al., 1995). The much-publicized evaluations conducted by Sherman and Berk (1984) and five other research teams on the impact of arrest on subsequent violence also relied on a sample that only included already violent homes in specific jurisdictions who entered the criminal justice system.² The original Sherman and Berk finding led to a widespread adoption of pro- and mandatory-arrest policies. However, replication studies found that by arresting offenders, police may not be protecting the victim. In fact, one study found that arrest could even increase offenders' proclivity toward future violence (Hirshel et al., 1990). Sherman concludes that the efficacy of arrest depends heavily on the perpetrators' perceived cost of being detained (Sherman, 1992).

The above observational and experimental studies are limited in three important ways that restrict the authors' abilities to generalize the findings. First, because they rely on official records to identify the sample and to record offending, information is missing on households that fail to enter the criminal justice or social service systems. The "selected" sample could systematically differ in their reactions to policy from offenders who have successfully avoided the system. Second, by only examining offenders, the findings fail to inform readers whether the policy prevents potential perpetrators without prior offenses from violently offending. Finally, because each experiment was limited to one jurisdiction, generalization to other regions must be interpreted cautiously (Sherman, 1992). The current research addresses all three limitations by using data from a nationally representative survey of victims and non-victims who may or may not have had previous criminal justice contact.

A second strategy in domestic violence policy research is to aggregate information across all persons living in specific localities to examine policy impacts on rates of intimate partner homicide (Browne and Williams, 1989; Dugan et al., 1999, 2003). By examining domestic violence policy

2. Sherman et al., 1992; Dunford et al., 1990; Hirshel et al., 1990; Berk et al., 1992; Pate and Hamilton, 1992.

effects with aggregated data, conclusions are drawn based on the experiences of both victims and non-victims. Aggregated intimate partner homicide research relies on evidence that a large number of these killings resulted from homes where partner abuse is prevalent (Browne et al. 1998; Campbell, 1992; Goetting, 1995; Smith and Stanko, 1998). In their study of state-level domestic violence services, Browne and Williams (1989) found that greater service availability is significantly associated with lower rates of married women killing their husbands, suggesting that these services may reduce incidence of violence. This finding was replicated in a longitudinal analysis of domestic violence services in 29 large U.S. cities. Dugan et al. (1999) found that increases in legal advocacy services are associated with reduced victimization for married men, but not for women. A second study by Dugan et al. (2003) expanded this sample to 48 U.S. cities and examined the association of several domestic violence laws, policies, and services on homicide victimization by gender, marital status, and race over a 20-year period. It found that domestic violence resources can positively or negatively relate to homicide depending on the type of policy and type of victim.

The above aggregate studies inform policy-makers of overall patterns of association, but are limited in ways that prohibit the authors from drawing strong conclusions. First, by only studying homicide, outcomes are restricted to only a small subset of domestic violence cases—those ending in death. Results can only suggest policy impact on homicide—not lower levels of intimate violence. Nonlethal violent offenders could be more responsive to policy than those with tendencies toward homicide. The second limitation is inherent to aggregated research. Household or individual characteristics cannot be directly linked to domestic violence participants. For example, we cannot conclude that educated persons are less likely to kill their intimate partners because cities with a high percent of well-educated residents have lower homicide rates. The third limitation is that by aggregating cases to geographic units, information on the process relating individual behavior to policy is missing. Coefficient estimates fail to tell us if a policy effect is due to changes in the likelihood that an individual will access the system, to the accuracy in which a policy is implemented, or both. This research improves on the above aggregate analyses by directly testing how policy relates to the chances of nonlethal domestic violence victimization for a large number of U.S. households while controlling for important household characteristics.

I further explore how legislation influences police involvement and arrest. Through increased public awareness and an enhanced perception of protection, the adoption of domestic violence legislation can lead more cases into the criminal justice system. Also, as domestic violence is legally expressed to be a more serious offense, officers may be more inclined to

arrest. If, in fact, criminal justice involvement and arrest do reduce recidivism, decreases in violence due to legislation could be partially attributed to effective implementation of the criminal justice system.

DOMESTIC VIOLENCE STATE LAWS

The domestic violence statutes selected for this research relate to civil protection orders, which are legally binding court orders that prohibit assailants from further abusing victims. Civil protection orders provide a remedy for noncriminalized abuse and misdemeanor criminal offenses that lack sufficient evidence to charge or convict (Finn, 1989). Such orders also provide an alternative to the victim who wants the abuse to stop but does not necessarily want to jail the perpetrator. Although appealing in theory, protection orders have been criticized for failing to prevent further abuse, especially if the offender is strong-willed and aggressive. Widespread lack of enforcement further weakens victims' safety while promising protection.

To strengthen the effectiveness of civil protection orders, states adopt statutes that delineate the eligibility of victims, the types of relief authorized, and provisions for enforcement (Finn, 1989). For instance, by 1997, 48 of the 50 states have statutory provisions allowing orders to direct the assailant to refrain from having any contact with the victim (Dugan et al., 2003). The purpose of "no-contact" protection orders is not to punish the offender for past conduct, but to prevent future assaults (Harvard Law Review, 1993). Other statutory provisions are designed to allow judges to customize protection orders to better suit the needs of the victim in each case. For example, if the parties are parents, the order may require that immediate custody of the children be granted to victims. A third type of statute reinforces the order by strengthening sanctions toward violators. Potential offenders may more likely refrain from further contact if violating the order would incarcerate them on felony charges or cause them to relinquish their firearms.

Several key provisions are examined here. The first expands the eligibility of protection orders to cover victims who do not live with the abuser, beyond cohabitation. This provision concerns eligibility for receiving a protection order. Two important advances have occurred in the statutory definition of "eligible petitioner." The first is to make orders available to persons who are not currently or formerly married to the abuser. The second involves expanding eligibility to include persons who do not live with the abuser. The importance of eligibility criteria is illustrated by the substantial increase in filings of protection from abuse orders following

Pennsylvania's excision of the cohabitation requirement.³

Custody is a second provision that could encourage more victims to petition for protection orders. It authorizes judges to award temporary custody of children to the victim. Batterers sometimes warn their partners that they will not be allowed to leave with the children, and threaten to kidnap, hurt, or even kill the children. Women are less likely to leave abusive relationships if they think it will endanger their children. Therefore, a battered woman may be more likely to file for a protection order if she knows that she is likely to obtain temporary custody.

Three legal provisions relate to the consequence of violating an order. Violation of a protection order can be classified as a misdemeanor, contempt (either civil or criminal), or a felony depending on, among other things, the provision that was violated.⁴ Arrest and confinement are more likely to occur if the violation is classified as criminal contempt or felony. In general, police officers cannot make an arrest without a warrant for a misdemeanor based on probable cause alone, thereby hampering enforcement in instances where violation of protection order is classified as a misdemeanor offense (Finn, 1991). As statutes allow judges discretion when classifying offenses, they are free to base sanctioning decisions on the specifics of each case.

The firearm confiscation provision is a controversial state law that requires offenders to relinquish all weapons once convicted for a misdemeanor crime of domestic violence. Another version of this statute limits possession and purchase of firearms to offenders who are served protection orders. In 1996, this statute became federal law. The last statute mandates police officers to arrest offenders who violate orders. Mandatory arrest provisions, in principle, eliminate the police officer's discretion in making an arrest once probable cause is established.

Little is known about the efficacy of specific changes in state statutes on protection orders. Most research and evaluation of legal reforms designed to reduce domestic violence focus on operational goals instead of their effect on subsequent violence (Fagan, 1995). Research by Finn and Colson (1998) concludes that the utility of protection orders depends on their specificity, consistency of enforcement, and the ease in which they are obtained. Efforts are needed within the criminal justice system and in domestic violence agencies to provide information and support to help domestic violence victims to petition for appropriate orders that will most protect them from further abuse.

3. This material is drawn from personal communication with Dawn Henry and Barbara Hart of the Pennsylvania Coalition Against Domestic Violence.

4. For the purposes of this study, we examine the type of violation that corresponds with the no-contact provision.

In summary, because the goal of domestic violence legislation is to curb violence, I expect that domestic violence policy will be associated with a decreased probability that a household member will violently victimize a family member or intimate partner. Further, I hypothesize that more cases are likely to enter the system in response to enhancing a state's legislative profile, and that those cases will more likely lead to arrest.

DATA AND METHODOLOGY

Three levels of analyses are conducted. The primary investigation uses all interviewed households in the NCVS, including both victims and non-victims. By structuring the sample this way, I can estimate legislative effects on the likelihood of victimization while controlling for individual and family characteristics. Two secondary analyses explore how legislation affects important components of policy implementation. Both use all violent NCVS incidents where the offender was a family member or intimate partner (current or ex). The first analysis estimates the policy effects on the probability that the police are informed of the crime. The second also uses NCVS violent incidents, but only includes the subset of cases in which the police were informed. Results are generated to estimate the legislative impact on the chances that an arrest is made.

DATA

NATIONAL CRIME VICTIMIZATION SURVEY DATA

The NCVS, sponsored by the Bureau of Justice Statistics (BJS), is the second largest ongoing government-run U.S. survey (Bachman, 2000). Since 1972, the NCVS has collected a rich assortment of information from residents 12 years and older living in randomly selected housing units.⁵ Most importantly for this research, respondents of the NCVS report the experiences and consequences of criminal victimization during the six months preceding the interview.⁶ All dependent variables and many of the independent variables are created from the responses to the redesigned NCVS from January 1992 to June 1998. Because the accuracy of all survey data relies on how well the survey is designed, how willing the

5. NCVS sampling design is a stratified cluster sample.

6. The NCVS is an ongoing survey that uses a rotating panel designed to interview residents in select housing units seven times within a three-year period. Every six months a subgroup of housing units is interviewed for the first time and another subgroup is interviewed for the last time. Data collected from the first set of interviews are used only for bounding purposes, not for analysis or estimation. Bounded interviews provide a reference point to the respondents in the following interview to reduce the chances that they will report about crimes committed prior to the six-month window (ICPSR, 1997).

respondents are to disclose personal information, as well as how dependably each resident participates in the survey, survey data are inherently fallible. For this reason, several issues related to the survey design are modeled to reduce biases due to inconsistencies in respondent disclosure.

Probability of Household Violence The primary dependent variable was constructed from all NCVS households to indicate whether any resident was recently and violently victimized by a family member or intimate partner. Although efforts have been made in the most recent survey design to compel victims to disclose family and intimate victimizations, NCVS estimates incidence of domestic violence at lower rates compared to other sources (Crowell and Burgess, 1996; Tjaden and Theonnes, 2000). The survey was designed as a general crime survey and cannot afford the same level of care to prompt respondents to disclose all types of sensitive information. Because nondisclosure is possible, the dependent variable is more accurately generated from the joint distribution combining the probabilities that a household member was victimized and that he or she disclosed the incident to the interviewer. Independent variables are selected to account for survey characteristics that could affect a respondent's candidness.

Violent victimizations are defined as completed and attempted incidents of rape, robbery, and aggravated assault, simple assault, sexual attack with serious assault or minor assault, threatened assault with weapon, sexual assault without injury, unwanted sexual contact without force, assault without weapon or injury, verbal threats of rape, sexual assault, or assault, and completed burglary with unlawful entry with or without force. Three groupings of domestic violence are constructed according to the victim's relationship to the offender. The first includes all cases of non-intimate family violence in which there was only one offender and he or she was a parent, step parent, child, step child, sibling, or other relative.⁷ The two remaining groups examine intimate partner domestic violence by the victim's marital relationship to the offender. Spousal violence includes spouses and ex-spouses, and boy/girlfriend violence includes cases where

7. Because children under the age of 12 are omitted from the sample, some cases of child abuse are not measured in this study. Additionally, if an adult household member objects to a 12- or 13-year-old member being interviewed, then that or another member will serve as a proxy and respond to the questions for the child. If the proxy interviewee is unaware of the child's victimization or prefers not to disclose a crime, it is unlikely that those incidents will be reported to the interviewer. Finally, if a particular household member is physically or mentally unable to answer the questions, or is temporarily absent and not expected to return before the closeout date, the interviewer will accept information from another knowledgeable household member. All proxy interviews can reduce the chances that an actual incident is recorded in the NCVS.

the offender was a current or ex- boyfriend or girlfriend. A summary of all dependent variables is found in Table 1.

TABLE 1. SUMMARY OF DEPENDENT VARIABLES

Variable	Measure
<u>Deterring Violence</u>	
Family Violence	An indicator variable identifying households where at least one respondent was violently victimized by a non-intimate family member
Spousal Violence	An indicator variable identifying households where at least one respondent was violently victimized by a spouse or ex-spouse
Boy/girlfriend Violence	An indicator variable identifying households where at least one respondent was violently victimized by a boy/girlfriend or ex-boy/girlfriend
<u>Informing Police and Arrest</u>	
Police Informed	An indicator variable identifying domestic violence incidents of which police became informed
Arrest	An indicator variable identifying reported domestic violence incidents in which the police made an arrest

Informing Police and Arrest Two secondary dependent variables were constructed with incident-specific data to show criminal justice involvement. The first indicates whether the police were informed of the violence based on the dichotomous response to the survey question, "Were the police informed or did they find out about this incident in another way (ICPSR, 1997: 251-252)?" A general measure of police involvement is used because policy implementation is likely to rely more heavily on whether the police are involved than on what led to their involvement.

The second incident-level dependent variable is an indicator of whether an arrest was made. The survey question asks the respondent, "As far as you know, was anyone arrested or were charges brought against anyone in connection with this incident (ICPSR 1997: 279)?" Unfortunately, the question fails to specify who was arrested, and a "yes" response may refer to someone other than the offender. Keeping this caveat in mind, the

analysis assumes that the party was the offender.⁸

State Statutes The Pennsylvania Coalition Against Domestic Violence and Julie Kunce Field compiled longitudinal data from all 50 states documenting changes in state statutes related to protection orders. The statutory provisions used in the current research include beyond cohabitation, custody, felony, contempt, misdemeanor, firearm confiscation, and mandatory arrest during the years 1991 to 1997. Contempt and misdemeanor are combined to index the discretion of the judge to sentencing outcomes. See Table 2.

TABLE 2. SUMMARY OF VARIABLES DESCRIBING STATE STATUTES

Variable	Measure
Beyond Cohabitation	An indicator variable identifying states that allow victims who do not live with the offender to petition for a protection order
Custody	An indicator variable identifying states that authorize the court to award temporary custody of children to the victim once a protection order is issued
Discretion Index	An index describing the type of sanction available to the judge in cases of protection order violation, 1 = misdemeanor, 2 = civil or criminal contempt, 3 = both misdemeanor and contempt
Felony	An indicator identifying states that classify protection order violation as a felony
Mandatory Arrest	An indicator identifying states with mandatory arrest laws for protection order violations
Firearm Confiscation	An indicator identify states that confiscate the offenders' firearms once a protection order is served

Control Variables Because factors other than law influence violent behavior, reporting, and arrest, several control variables were constructed from NCVS data. Table 3 lists household-specific attributes used in the

8. One pattern that seems to have followed the adoption of mandatory arrest laws is that the victim is arrested along with the offender (Jones and Belknap, 1999).

primary analysis estimating policy effects on household violence. Those characterizing individuals, such as separated/divorced, describe traits of the reference person, the person who is identified as owning or renting the living quarters. Household characteristics were selected because they either relate to the household's stability, to the likelihood of exposure to violence, or because they describe the demographics already found to be related to violence. Further, several variables were created to control for survey characteristics that could affect the likelihood that the victim discloses the crime to the interviewer. Finally, incident-specific attributes are displayed in Table 4. These variables describe the victim, offender, incident, or location.

METHODOLOGY

The primary goal of this research is to determine if any of the selected domestic violence state statutes relate to a shift in the probability that any household member is violently victimized by a family member or intimate partner. Logistic regressions are used to estimate policy, control, survey design, and time coefficients associated with the three types of domestic violence. As described in Table 1, the dependent variables are dichotomous outcomes identifying households with any nonintimate family, spousal, or boy/girlfriend violence. Figure 1 shows a diagram of the components of the logistic model used to estimate the probability that a household suffers from one of these forms of domestic violence. As discussed above, because of inherent fallibility associated with survey data, the dependent variables are generated from the joint probability that the incident occurred and the victim disclosed it to the interviewer.⁹ Tables 1 and 2 describe the dependent variables and state statutes used in the model. All legislation is measured during the calendar year falling six months prior to the interview date to assure that the law was in place at the time of the incident.

The control variables relate to household stability, possible exposure to violence, demographics, or survey issues as described in Table 3. Indicator variables are also included for all but one year. The stability variables that are expected to be related to less violence are Months, Own Home, and College (Rennison and Welchans, 2000).¹⁰ The remaining stability variables are expected to be related to more violence since they describe less

9. The model is weighted with the normalized household weight, which was created by dividing the household weight by the average weight of all households.

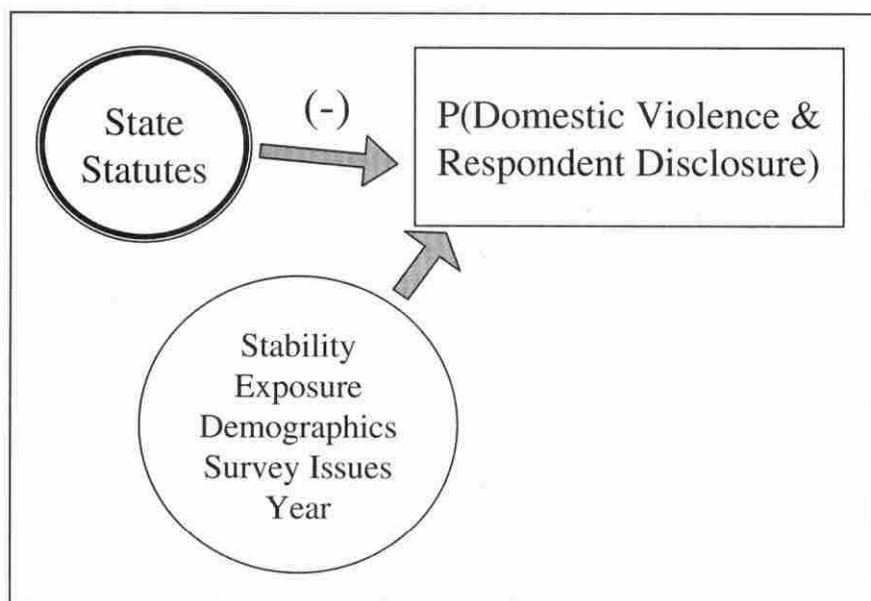
10. Some respondents failed to report the number of months at the current address. To avoid missing data, those values were set at zero and an indicator was created to control for victimization differences between those and other households. Zero was chosen because in some cases, the number of months is missing if the household lived in the housing unit for less than one month.

TABLE 3. SUMMARY OF HOUSEHOLD-LEVEL CONTROL VARIABLES

Variable	Measure
<u>Stability</u>	
Months ^a	The number of months the reference person has lived at the current residence
Own Home	At least one resident owns the home
College	The reference person has completed at least 4 years of college education
Low Income	Household income is less than \$15,000/year
Public Housing	The household lives in public housing
Other Units	Multiple unit living quarters
Separated/Divorced	The reference person is separated or divorced
Lone With Child	The reference person is an adult living alone with one or more children
<u>Exposure</u>	
Alone	Single person household
Male Out	Male reference person or the husband of the reference person reports spending almost every evening away from home for work, school, or entertainment
Female Out	Female reference person or the wife of the reference person reports spending almost every evening away from home for work, school, or entertainment
Female Not Shopping	Female reference person or the wife of the reference person reports that she never goes shopping
<u>Demographics</u>	
White	The reference person is white
Hispanic	The reference person is of hispanic origin
Over 60	The reference person is over 60 years old
Urban	The household lives in an urban setting
South	The household lives in a southern state
<u>Survey Issues</u>	
Missing Month	An indicator variable identifying households in which the reference person failed to report the number of months at the current residence
Proxy	At least one interview was a proxy
Unbounded	The household was not interviewed in the previous six month period
Interview Period	The number of times that housing unit was scheduled to be interviewed
Year	Year of Interview

^aMissing months were set to zero.

FIGURE 1. MODEL TO ESTIMATE LEGISLATIVE IMPACT ON THE PROBABILITY THAT ANY HOUSEHOLD MEMBER IS VIOLENTLY VICTIMIZED BY A FAMILY MEMBER OR INTIMATE PARTNER



stable households—Low Income, Public Housing, Other Units, Separated/Divorced, and Lone with Child (Allard et al., 1997; Rennison and Welchans, 2000). The exposure variables describe either a decreased or increased likelihood of exposure to violence—Alone, Male Out, Female Out, and Female Not Shopping. Because the data do not reveal whether the female is with her partner as she spends time away from home, this measure could indicate high exposure for some and low exposure for others. Demographics include White, Hispanic, Over 60, Urban, and South (Rennison and Welchans, 2000; Tjaden and Theonnes, 2000).

Survey lists the interview characteristics that could relate to whether or not a victim discloses the incident to the interviewer. Because NCVS accuracy relies on disclosure, three potential sources of bias are controlled for (1) attrition, (2) respondent fatigue, and (3) proxy ignorance or reluctance. Survey variables include Proxy, Unbounded, and Interview Period for reasons described below.

As discussed in footnote 7, when other persons answer the survey questions for absent or unable respondents, important information, including

relevant violent incidents, could be omitted. For this reason, the indicator Proxy is placed in the model to control for households with at least one proxy interview. It is expected that households with at least one proxy interview will have, on average, fewer incidents, and will, therefore, be negatively related to the probability of violence.

Because households residing in sampled housing units are interviewed over a three-year period, mobility will alter the composition of respondents. Using National Crime Survey (NCS) data from the middle 1980s to 1990, Dugan (1999) showed that about 23% of the households interviewed in period one (not including the bounded interview) moved before their final interview.¹¹ Although those households are usually replaced, data from the replacement households are "unbounded." Bounding interviews provide a reference point to respondents, reducing the chances that they report crimes that were committed before the six-month reference period. Therefore, unbounded interviews are likely to contain more incidents than those that actually happened during the previous six months. Biderman and Cantor (1984) used early NCS data and estimated that unbounded households inflate victimization rates by more than 10% (117.5 to 131.89). To control for this type of bias, Unbounded is added to the model.

The final source of bias also relates to the repeated interview design. Because all housing units have seven interview periods, the longer each unit is in the sample, the more likely its occupants have been interviewed multiple times. One consequence is that victimization rates may appear to decrease the longer the household is in the sample because "older" households may be more reluctant to disclose incidents to the survey interviewer (Biderman and Cantor, 1984). "Respondent fatigue" can result from a loss of interest, an accumulation of burden, or other conditions that make later interviews less novel. For instance, the respondent now knows that if he or she mentions an incident during the screening questions, another batch of lengthy questions will be asked, substantially extending the length of the interview. To control for this type of bias, Interview Period is included in the model.

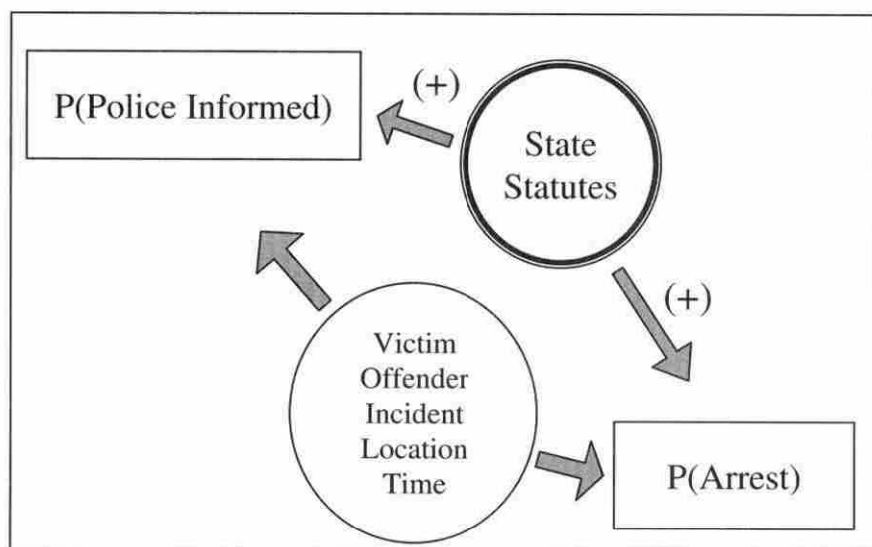
INFORMING POLICE AND ARREST

Logistic models are also used to estimate legislative coefficients associated with outcomes measuring whether police are informed of the incident and if they make an arrest. Figure 2 shows diagrams of the estimated models.¹²

11. National Crime Survey is the name of the National Crime Victimization Survey prior to the 1992 redesign.

12. Both models are weighted with the normalized incident weight, which was created by dividing the incident weight by the average weight of all incidents.

FIGURE 2. MODELS TO ESTIMATING LEGISLATIVE IMPACT ON THE PROBABILITY THAT THE POLICE ARE INFORMED OF AN INCIDENT, AND THAT THEY MAKE AN ARREST



Although each controls for characteristics of the victim, offender, incident, location, and time, the composition of each model varies slightly according to the outcome. Victim characteristics include all variables listed under victim in Table 4—Age, Female, Separated, Young Children, White, Hispanic, College, and Job (see Bachman, 1998; Johnson, 1990; Bachman and Coker, 1995; Berk et al., 1984; Conaway and Lohr, 1994; Felson et al., 1999; Rennison and Welchans, 2000).¹³ Offender characteristics include Spouse, Parent, Child, Sibling, Other Family, Gang, Previous Offense, Similar Gender, and Similar Race (see Bachman, 1998; Berk et al., 1984; Conaway and Lohr, 1994; Felson et al., 1999).¹⁴ The incident characteristics include Drugs or Alcohol, Weapon, Injury, Others Present, Robbery, and Unlawful Entry (see Bachman, 1998; Johnson, 1990; Bachman and Coker, 1995; Berk et al., 1984; Conaway and Lohr, 1994; Felson et al., 1999; Skogan, 1984).¹⁵ Location Characteristics include Urban,

13. The arrest model omits *College*.

14. The reporting model omits *Similar Gender*.

15. The arrest model omits *Others Present*, and additionally includes *Victim Reported* and *Within Hour*.

TABLE 4. SUMMARY OF INCIDENT-SPECIFIC CONTROL VARIABLES

Variable	Measure
Victim	
Age	The age of the victim
Female	The victim is female
Separated	The victim is separated or divorced
Young Children	Number of victim's children under the age of 12
White	The victim is white
Hispanic	The victim is of hispanic origin
College	The victim has completed at least 4 years of college
Job	The victim has a job
Offender	
Spouse	The offender was the victim's spouse or ex-spouse
Parent	The offender was the victim's parent or step parent
Child	The offender was the victim's child or step child
Sibling	The offender was the victim's sibling
Other Family	The offender was the victim's extended family member
Immediate Family	The offender was the victim's immediate family member
Gang	The offender was known to be a member of a street gang
Drugs or Alcohol	The offender was on drugs or alcohol
Previous Offense	The incident is a series event or the victim reported that the offender has done this before
Similar Race ^b	The victim and offender are of a similar race
Same Gender	The victim and offender are the same gender
Incident	
Weapon	The offender had a weapon
Injury	The victim was injured
Others present	Others were present during the incident
Robbery	The offender robbed or attempted to rob the victim
Unlawfully Entry	The offender unlawfully entered the victim's residence
Assault	The offender assaulted (aggravated or simple) the victim with injury, attempted aggravated assault with a weapon, or threatened an assault with a weapon
Victim Reported	The victim reported the crime to the police
Within Hour	The police arrived at the scene within an hour of being informed of the incident
Location	
Public	The incident occurred in a public setting

^aMissing months were set to zero.

^bThis variable only considers the groupings of white and non-white. Therefore, a victim's and offender's race could be considered similar if one is Native American and the other is African American.

South, and Public (see Bachman, 1998).¹⁶ Finally, the Time vector includes year dummies and the Interview Period.

RESULTS

In total, 529,829 households were interviewed using the revised NCVS between January 1992 and June 1998.¹⁷ From those, 2,873, or 0.5%, reported at least one incident of domestic violence (unweighted). Between January 1992 and December 1997, 3,508 domestic violence incidents were recorded in the NCVS. Police were informed of less than half of them, (1,730 unweighted), suggesting that the "dark figure" in domestic violence (the difference between what happens and what is reported) is about the same as the number of incidents known to the police. A little more than one-third of the violent domestic situations known to the police resulted in arrest (594 unweighted). Specifically, one out of every two incidents gets reported to the police, and one out of every six ends in arrest.

HOUSEHOLD VIOLENCE

Of the 529,829 households sampled, only one-half of 1% informed the NCVS interviewer of at least one incident of domestic violence, 0.16% disclosed at least one incident of family violence, 0.18% disclosed at least one incident of spousal violence, and a little more than 0.2% disclosed at least one incident of non-marital intimate violence.

Table 5 presents the results for logistic models for all three types of domestic violence.¹⁸ The first column lists the hypothesized associations of each variable with any domestic violence. The body of the table displays the odds ratios for each variable on each outcome. All significant odds ratios below one are negatively associated with violence, and those

16. *Public* is omitted in the arrest model.

17. Because this research only uses data from the revised NCVS, fewer households from 1992 and 1993 are included in the data, the years that only a portion of the sample got the revised survey instrument.

18. Because housing units are selected using a stratified, multistage cluster design, analysis that assumes a simple random sample could bias standard errors downward. The concern is that by clustering a sample, it will be more homogeneous than the population it represents. However, NCVS data collection is based on a large number of clusters consisting of about four housing units, making it likely that the heterogeneity of the population is captured (ICPSR, 1997). This concept is more obvious when we consider that a simple random sample is a cluster sample with n clusters consisting of one unit each. Two robustness tests were conducted to assure the robustness of the above findings. First, an earlier version of the current analysis corrected for the cluster sampling design, producing similar results. Second, using the publicly available redesigned NCVS data, the average estimated design effect is 1.04. After adjusting the standard errors by this figure, the significance of results is the same.

TABLE 5. ODDS RATIOS FROM LOGISTIC REGRESSIONS PREDICTING VIOLENCE ($n = 529,829$)

Variable	Hypothesized Association	Family	Spousal	Boy/ Girlfriend
<u>Statute Provisions</u>				
Beyond Cohabitation	-	0.864	0.865	0.798**
Custody	-	0.939	1.214*	0.976
Discretion Index	-	0.926*	0.944	0.996
Felony	-	0.620*	1.294	0.653**
Mandatory Arrest	-	0.935	0.885*	0.909
Firearm Confiscation	-	0.866*	0.958	0.861**
<u>Stability</u>				
Months	-	1.000	0.998**	0.999**
Own Home	-	0.802**	1.001	0.825**
College	-	0.508**	0.838**	0.497**
Low Income	+	1.764**	1.051	1.686**
Public Housing	+	1.378	0.628	1.192
Other Units	+	0.739**	0.857*	1.066
Separated/Divorced	+	1.349**	4.295**	1.170**
Lone With Child	+	1.529**	3.170**	3.762**
<u>Exposure</u>				
Alone	-	0.377**	0.480**	1.031
Male Out	-	1.057	0.798*	0.703**
Female Out	-	0.686**	1.410**	1.624**
<u>Demographics</u>				
White	-	1.149	1.908**	1.156*
Hispanic	-	0.686**	0.666**	0.494**
Over 60	-	0.427**	0.347**	0.226**
Urban	+	1.021	0.991	1.389**
South	+	0.827**	1.067	0.692**
<u>Survey Issues</u>				
Missing Month	-	0.883	1.659**	0.772
Proxy	-	0.910	0.716**	0.682**
Unbounded	+	1.490**	1.738**	1.794**
Interview Period	-	0.948**	0.907**	0.947**
Year 96	-	1.116	1.056	1.261**
Year 95	-	0.987	1.124	0.951
Year 94	-	1.026	0.994	1.168
Year 93	-	1.000	1.122	0.956
Year 92	-	0.791*	0.872	0.918

* $p < 0.05$, ** $p < 0.01$, all tests are one-tailed

greater than one are positively associated. The asterisks indicate the level of significance for one-tailed tests.

Nineteen control variables have predicted associations on violence. Of those, 15, or 79%, are significantly related to at least one form of violence in the expected direction. One variable, public housing, has no significant association with any form of violence, and four oppose the hypothesized associations in at least one case. Surprisingly, households living in multiple-family units are less, not more, likely to suffer from family or spousal violence. Because both types of violence are most likely to occur in the home, this finding suggests that other apartments serve as guardians to the victim. Would-be offenders may be reluctant to turn smaller altercations into violent episodes if a neighbor can hear and possibly call the police. One mixed finding is in the results for households where the female reports that she goes out every night. I expected that if a female is out frequently, her risk of family or intimate violence would be reduced due to less exposure to the potential offender. The positive association with intimate violence suggests that when a woman goes out, she is accompanied by a partner, thus, increasing her exposure. This hypothesis is further supported by the negative association with nonintimate family violence, suggesting that when a woman goes out, she spends less time with siblings, parents, and other relatives. Finally, two demographic results contradict the known violent patterns of their related groups. After controlling for other things, white-headed households are more likely to experience violence, and southern households are less likely.

POLICY EFFECTS ON VIOLENCE

All six of the legislative variables at least have marginal significance with one or more forms of domestic violence. One finding, however, opposes expectation. It was hypothesized that the statute awarding immediate custody to the victim after a protection order is issued would create an incentive for a father to keep peace in the household. Instead, households in states with the statute are more likely to suffer from spousal violence than those without it. Namely, the odds that households in those states will be victimized by a spouse or ex-spouse are 1.214 higher than households in other states. This suggests that violent fathers may be prone to retaliate if they lose custody of their children. Not surprisingly, the custody statute is unrelated to all other forms of domestic violence, which are less likely to involve only parents.

Another unsurprising offender-specific result is that households in states that expand eligibility of protection orders to victims living separately from the offender have a lower probability of suffering from nonmarital intimate violence—the group least likely to live together. The statute with the strongest apparent impact on reducing violence makes

protection order violation a felony offense. The odds of victimization are lowest for family violence, followed closely by non-marital intimate violence. Surprisingly, the likelihood of victimization by a spouse is unrelated to the felony statute. This result pattern is similar for the firearm confiscation statute. Households in states with laws directing offenders to surrender their firearms once convicted of a domestic violence charge are less likely to suffer from family or nonspousal intimate violence. Spousal violence is, however, less probable in states with mandatory arrest laws. Finally, family violence is less likely to affect households in states with more sanctioning options available to judges.

POLICY EFFECTS ON INFORMING THE POLICE AND ARREST

Table 6 lists the odds ratios of all six domestic violence statute measures on informing the police and arrest. The full table for each model appears in the Appendix. The only two policies that show significance are associated with the likelihood that police discover the incident: felony and mandatory arrest. The odds that officers in states with felony statutes are involved are 1.59 higher than officers in states without the statute. This suggests that if the courts signal violations as serious, more cases will enter the system. However, mandating arrest appears to reduce the chances that police discover an incident (odds ratio = 0.875), suggesting that by assuring arrest, persons are less inclined to seek police assistance.

TABLE 6. ODDS RATIOS OF POLICY ON
INFORMING POLICE AND ARREST

Variable	Hypothesized Association	Police Informed (<i>n</i> = 3,508)	Arrest (<i>n</i> = 1,730)
Statute Provisions			
Beyond Cohabitation	+	0.882	1.191
Custody	+	0.970	0.788
Discretion Index	+	1.025	1.072
Felony	+	1.585*	1.636
Mandatory Arrest	+	0.875*	1.209
Firearm Confiscation	+	0.971	0.903

* $p < 0.05$, ** $p < 0.01$, all tests are one-tailed.

The null findings, however, for arrest suggest that mandating arrest does not assure that an arrest will occur. Further, none of the other statutes have a significant association with officers' arresting decisions.

SUMMARY

The goal of this research was to better understand the influence of policy on violence inflicted on family members and intimate partners. Because the intention of aggressive domestic violence legislation is to stop violence, I hypothesized that those households residing in states with aggressive legislation have a lower probability of domestic violence. Results suggest support for that proposition. Five of the six statutory powers are associated with a significantly lowered probability of at least one form of domestic violence. I further test whether statutory powers directly relate to police intervention and arrest. Figure 3 summarizes all results by illustrating the direction of association of each statute on the tested outcomes: reporting, arrest, and violence. Flat arrows signal null associations. Upward and downward arrows show significantly positive and negative associations, respectively. Column three displays a tilted arrow if the statute is significantly associated with any of the three violent outcomes. The most notable pattern is that although five of six findings appear to reduce violence, only the felony statute seems to decrease violence and increase the chance that a case becomes known to the criminal justice system. This leaves us uncertain of the direct mechanism that translates the other state statutes into nonviolent behavior.

One of the more interesting patterns is found in the results for Mandatory Arrest, which are significant in two of the three components of the process. Although the findings suggest that households in states that mandate arrest are less likely to suffer from spousal violence, police in these same states are less likely to discover an incident. This suggests that mandatory arrest laws not only reduce the chances of violence, but also keep people from calling the police. Further examination of this result shows that victims of domestic violence are no more likely to report an incident in states with mandatory arrest laws; however, third parties are significantly less likely to report. Perhaps others are less likely to get involved in domestic disputes if an arrest is almost certain.

POLICY IMPLICATIONS

Before discussing relevant policy implications, it is important to highlight the limitations of this research that naturally suggest caution before drawing strong policy conclusions. Because the data are restricted to responses dictated by the National Crime Victimization Survey, we have no information on whether the victims or offenders are aware of current domestic violence legislation. Thus, policy effects found in this analysis can only contribute to the speculation of the process responsible for differences in the average behavior of victims and offenders conditional on the legislative profile of victims' state of residence.

FIGURE 3. PATTERN OF ASSOCIATIONS
THROUGHOUT PROCESS

	Reporting	Arrest	Violence
Beyond Cohabitation	→	→	↘
Custody	→	→	↗
Discretion Index	→	→	↘
Felony	↗	→	↘
Mandatory Arrest	↘	→	↘
Firearm Confiscation	→	→	↘

Having said that, the findings in Figure 3 do suggest that households' proclivities toward family and intimate violence may be sensitive to the statutory profiles of their states, implying that would-be offenders who perceive a high cost to violence may refrain from acting out their aggression. Perhaps the strongest evidence is the finding that shows a lower likelihood of boyfriend or girlfriend violence in states that expand the eligibility of civil protection orders to victims living separately from their abuser. The chances of violence between victims and offenders who more naturally cohabit, such as family members or spouses, are unaffected when eligibility is expanded. Further, the three statutes that impose state sanction on those who violate a protection order—mandatory arrest, felony, and firearm confiscation—are all associated with decreases in one or more forms of violence. Findings further suggest that when the state is willing to prosecute violators as felons, more cases enter the system. This implies that the consequential reduction in violence could directly result from victims and others reporting offenders to the police. Although police

are more likely to get involved, the results suggest that they are no more likely to arrest a suspected felon.

Although the findings are encouraging, they do raise concerns for local officials. Mandatory arrest laws do not necessarily lead to more arrest. Laws will only continue to prevent violence if they are known to be enforced. Further, nominal laws can mislead some victims into seeking police protection, leaving them in greater danger for having sought assistance without getting relief. In fact, Dugan et al. (2003) found that cities in states with mandatory arrest laws had higher rates of wife homicide. A second concern is raised by the findings for the custody statute. Husbands may become more antagonistic when the state awards temporary custody of the children to the victim. Thus, officers and policy makers should consider additional measures to protect victims with children. Sharing children with the perpetrator increases a mother's potential to be revictimized during visitation. Laws must aggressively pursue domestic violence offenders without compromising victims' safety.

In sum, evidence suggests that aggressive policy may reduce domestic violence, implying that victims may benefit if states continue to adopt statutes that protect victims and sanction offenders. However, we also need to assure that local officials are implementing the law while successfully protecting victims. State law can antagonize offenders, and without proper protection, victims can be further harmed. Laws without substance will hardly keep relentlessly violent perpetrators from critically harming their loved ones.

This research goes a long way in informing readers of how law statistically relates to the chances of violence in the home and its possible consequences. However, it fails to determine if policy contributes to the well-being of the victim after he or she seeks help. Arrest is a crude measure of victim support because evidence of its efficacy is mixed. As such, an important next step is to link victims longitudinally and examine how police involvement and arrest relate to future violence in different policy environments. Another important next step is to examine the interactive relationships between legislation and an incident's context. Does the "retaliatory" effect of a custody statute only impact homes with children? Does the "gag" effect of mandatory arrest laws weaken as the violence becomes more severe?

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Laura Dugan is an assistant professor in the Criminology and Criminal Justice Department at the University of Maryland and an active member of the National Consortium on Violence Research. She received her training in Public Policy and Management and Statistics from Carnegie Mellon University. Professor Dugan's research includes examining the consequences of criminal victimization and the efficacy of victimization prevention policy and practice. She also examines the effects of domestic

violence laws, policies, services, and reductions in Aid to Families with Dependent Children benefit levels on the behavior patterns of offenders, victims, and players within the criminal justice system. Further, she investigates ways to improve current methodological procedures to analyze longitudinal data.

APPENDIX-1. ODDS RATIOS PREDICTING REPORTING

Variable	Police Informed	Arrest
<u>Statute Provisions</u>		
Beyond Cohabitation	0.882	1.191
Custody	0.970	0.788
Discretion Index	1.025	1.072
Felony	1.585*	1.636
Mandatory Arrest	0.875*	1.209
Firearm Confiscation	0.971	0.903
<u>Victim</u>		
Age	1.013**	0.994
Female	1.262**	0.844
Separated	1.110	1.018
Young Children	1.136**	0.975
White	0.649**	1.000
Hispanic	1.415**	0.760
College	0.826	—
Job	1.015	0.889
<u>Offender</u>		
Spouse	0.998	1.019
Parent	0.887	0.850
Child	1.105	1.226
Sibling	0.746**	0.832
Other Family	0.847	1.049
Gang	0.643*	1.244
Previous Offense	1.192**	1.019
Similar Race	1.327**	0.960
Same Gender	—	0.794
<u>Incident</u>		
Drugs or Alcohol	1.193**	1.927**
Victim Reported	—	0.784**
Within Hour	—	1.778**
Weapon	1.578**	1.272*
Injury	1.491**	1.809**
Others Present	1.222**	—
Robbery	1.482**	0.837
Unlawful Entry	1.967**	1.540

Variable	Police Informed	Arrest
<u>Location</u>		
Urban	1.090	1.068
South	1.190**	0.843
Public	0.920	—
<u>Time</u>		
Interview Period	1.018	1.027
Year 96	0.883	0.745*
Year 95	0.873	0.741
Year 94	0.781**	0.703*
Year 93	0.693**	0.597**
Year 92	0.963	0.579**

* $p < 0.05$; ** = $p < 0.01$, all tests are one-tailed.

