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Domestic Violence Legislation: Exploring Its Impact on Domestic Violence and the

Likelihood that Police are Informed and Arrest

A Final Report to the National Institute of Justice for Grant # 97WTVX0004

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Abstract

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 lead 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 limit the number of cases that enter the system. Further, more protection should go towards victims when the state them awards custody of the children.

Narrative

State legislatures have increasingly passed statutes authorizing criminal justice officials to pursue domestic violence offenders more aggressively (Harvard Law Review, 1993; Dugan, Nagin, Rosenfeld, 2000). Domestic violence laws are designed to either reduce subsequent violence after an incident (e.g., statues authorizing the courts to issue protection orders) or to prevent latent violence from surfacing (Harvard Law Review, 1993). For example, 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, officers may

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become more confident that the system will successfully detain violent offenders, thus be 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. Since laws can encourage or deter victims from seeking police support, the true policy effect on violence as determined by USR data will likely be obscured by bias.[1] Further, UCR data combine domestic violence with other forms of domestic disturbances. Thus, a "domestic" could also be a drunken brawl between friends during a football game.

A second crime data source is the National Crime Victimization Survey (NCVS), which is administered randomly to a sample of US residents. Respondents are questioned about their experiences as crime victims regardless of whether the police were contacted. Details are provided about each incident making it easy to distinguish incidents by the relationship of the offender and victim. 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.

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[1] One exception is the UCR's Supplementary Homicide Reports, since homicides are almost always reported to the police. (See Dugan, Nagin, and Rosenfeld, 1999, 2000).

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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. The first relies upon experimental or observational data and includes only individuals known to be victimized by a partner or family member. Tolman and Weiss (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, Newton, and Berk, 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, Fagan, and Maxwell, 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 upon a sample that only included

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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 towards future violence (Hirshel, Hutchinson, Dean, Kelley, and Pesackis, 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 if the policy keeps potential perpetrators without prior offenses from offending. Third, 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 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

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[2] Sherman, Schmidt, Rogan, Smith, Gartin, Collins, and Bacich, 1992; Dunford, Huizinga, and Elliott, 1990; Hirshel, Hutchinson, Dean, Kelley, and Pesackis, 1990; Berk, Campbell, Klap, and Western, 1992; Pate and Hamilton, 1992.

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rates of intimate partner homicide (Browne and Williams, 1989; Dugan, Nagin, and Rosenfeld, 1999, 2000). By examining domestic violence policy 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 (Smith and Stanko, 1998; Browne, Williams, and Dutton, 1998; Campbell, 1992; Geotting, 1995). In their study of how 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 US 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., (2000) expanded this sample to 48 US cities and examined the association of several domestic violence laws, policies, and services on the 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. Non-lethal violent offenders could be more responsive to policy than those with tendencies towards

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homicidal proclivity. 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 upon the above aggregate analyses by directly testing how policy relates to the chances of non-lethal domestic violence victimization for a large number of US households while controlling for important household characteristics.

Further, I 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 to non-criminalized abuse and

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for 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. While appealing in theory, protection orders have been criticized for failing to prevent further abuse, especially if the offender is strong-willed and aggressive. Wide-spread lack of enforcement further weakens victims' safety while promising protection.

To strengthen the effectiveness of civil protection orders states adopt statues to delineate the eligibility of victims, the types of relief authorized, and provisions for enforcement (Finn, 1989). For instance, by 1997 forty-eight of the fifty states have statutory provisions allowing orders to direct the assailant to refrain from having *any* contact with the victim (Dugan, et. al., 2000). 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 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 towards violators. Potential offenders may refrain from further contact, if violating the order would incarcerate them on felony charges or cause them to give up 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.

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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 orders. It authorizes judges to award temporary custody of children to the victim. Batterers sometimes warn their partner that she 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 an abusive relationship 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 can not 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.

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

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

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.

In summary, since 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. However, since previous evidence suggests that some policy may lead to increased violence, all tests will be two-tailed (Dugan, et. al., 2000; Hirshel, et. al., 1990; Sherman, 1992). 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 where 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. Most importantly for this research, respondents of the NCVS report the experiences and consequences of criminal victimization during the six months preceding the interview.[5] All dependent variables and many of the independent

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[5] The NCVS is ongoing going 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

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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 clearly and truthfully humans communicate, as well as how dependably each resident participates in the survey, survey data is inherently fallible. For this reason several issues related to the survey design will be modeled to reduce biases due to inconsistencies in respondent disclosure.

Probability of Household Violence

The primary dependent variable was constructed from all NSVS households to indicate whether any resident was recently and violently victimized by a family member or intimate partner. While efforts have been made in the most recent survey design to compel victims to disclose family and intimate violence victimization, NCVS estimates incidence of domestic violence at lower rates when compared to other sources (Crowell and Burgess, 1996; Tjaden and Theonnes, 2000). The survey was designed as a general crime survey and cannot afford to use the same level of care to encourage 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

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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).

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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.[6] 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 the

offender was a current or ex- boyfriend or girlfriend. A summary of all dependent

variables is found in Table 1.

--Insert Table 1 About Here--

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

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[6] 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 interviewer is unaware of the child's victimization or is invested in keeping the crime a secret, 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.

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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 as to 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.

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.

--Insert Table 2 About Here--

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 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.

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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. Also, 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, the offender, the incident, or the location.

--Insert Table 3 & Table 4 About Here--

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 models 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 non-intimate 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.[7] Tables 1 and 2 describe the dependent variables and state statutes used in the model. All legislation is measured

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[7] The model is weighted with the normalized household weight which was created by dividing the household weight by the average weight of all households.

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during the year six months prior to the interview date to assure that the law was in place at the time of the incident.

--Insert Figure 1 About Here--

The control variables relate to household stability, risk for 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).[8] The remaining stability variables are expected to be related to *more* violence since they describe less stable households—*Low Income, Public Housing, Other Units, Separated/Divorced,* and *Lone with Child* (Rennison and Welchans, 2000; Allard, Albelda, Colten, and Cosenza, 1997). The risk variables describe either low or high risk for violence—*Alone, Male Out, Female Out*, and *Female Not Shopping*. Because it is not clear in the data whether the female is spending her time away from home with her partner, this measure could indicate high risk for some and low risk for others. Demographics include *White, Hispanic, Over 60, Urban,* and *South* (Tjaden and Theonnes, 2000; Rennison and Welchans, 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.

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[8] 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.

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As discussed in footnote six, 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.

Since 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 percent of the households interviewed in period one (not including the bounded interview) moved before their final interview.[9] While 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. Since 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

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[9] National Crime Survey is the name of the National Crime Victimization Survey prior to the 1992 redesign.

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 (Bidderman 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 if police are informed of the incident and if they make an arrest. Figure 2 shows diagrams of the logistic models used to estimate these models.[10]

--Insert Figure 2 About Here--

While 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 3—*Age, Female, Separated, Young Children, White, Hispanic, College,* and *Job* (see Felson, Messner, and Hoskin, 1999; Bachman, 1998; Johnson, 1990; Bachman and Coker, 1995; Conaway and Lohr, 1994; Berk, Berk, Newton, and Loseke, 1984; Rennison and Welchans, 2000).[11] Offender characteristics include *Spouse, Parent, Child, Sibling, Other Family, Gang,*

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[10] Both models are weighted with the normalized incident weight which was created by dividing the incident weight by the average weight of all incidents.

[11] The arrest model omits *College.*

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Previous Offense, Similar Gender and *Similar Race* (see Felson, et. al., 1999; Bachman, 1998; Conaway and Lohr, 1994; Berk, et. al., 1984).[12] The incident characteristics include *Drugs or Alcohol, Weapon, Injury, Others Present, Robbery,* and *Unlawful Entry* (see Skogan, 1984; Felson, et. al., 1999; Bachman, 1998; Johnson, 1990; Bachman and Coker, 1995; Conaway and Lohr, 1994; Berk, et. al., 1984).[13] Location Characteristics include the *Urban, South,* and *Public* (see Bachman, 1998).[14] 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.[15] From those, 2,873, or 0.5 percent, reported at least one incident of domestic violence (unweighted). Between January 1992 and December 1997, 3,508 domestic violence incidents were recorded using the revised 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 at least twice that of what is known to the police. A little more than a third of the violent domestic situations where police were involved 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.

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[12] The reporting model omits *Similar Gender.*

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

[14] *Public* is omitted in the arrest model.

[15] 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 intrument.

Household Violence

Of the 529,829 households sampled, only 0.5 percent reported to the NCVS interviewer at least one incident of domestic violence, 0.16 percent reported at least one incident of family violence, 0.18 percent reported at least one incident of spousal violence, and a little more than 0.2 percent reported at least one incident of non-marital intimate violence.

Table 5 presents the results for logistic models for all three types of domestic violence listed. The first column lists the hypothesized associations of each variable with 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 greater than one are positively associated.

--Insert Table 5 About Here--

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 with at least once. Surprisingly, households living in multiple-family units are *less* not more likely to suffer from family or spousal violence. Since 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 can possibly call the police. Another unexpected finding is that households where the primary female goes out every night show a higher likelihood of suffering from intimate partner violence. This suggests that increased social activity is a risk factor for

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women. However, the "opportunity hypothesis" is supported for other forms of family violence. Those households with highly social women are less at risk for non-intimate family violence. Finally, two demographic results contradict the known violent patterns of their related groups. After controlling for other things, white headed-household are more likely to experience violence, and southern households are less likely.

Policy Effects on Violence

All six of the legislative variables are significantly related to at least one form of domestic violence. However, one 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 tend less 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 non-marital 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 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

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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 non-spousal 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. Two policies are significantly associated with police discovering the domestic violence incident: felony and mandatory arrest. The odds that officers in states with this type of felony statute discover and incident are 1.59 higher than officers in states without the statute. This suggests that if the courts treat violations more seriously, more cases will enter the system. However, mandating arrest appears to reduce the chances that police discover an incident. The odds that police in mandatory arrest states will discover an event are only .875 to police in other states.

--Insert Table 6 About Here--

While mandating arrest seems to have a gag effect on some who would otherwise report domestic violence to the police, there is no strong evidence that the police will arrest more even if mandated. Further, none of the other statutes have a significant association with officers' arresting decisions.

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Summary

The goal of this research was to better understand the influence of policy on violent behavior between family members and intimate partners. Because we would like to believe that aggressive domestic violence legislation will stop violence, I hypothesized that those households residing in states with aggressive legislation have a lower probability of family and intimate violence. Laws can affect violent behavior in at least two ways. First, would-be offenders could be deterred by threats of sanctions. In most states, abusive boyfriends can now be served court orders prohibiting any contact with their girlfriends. The federal government now bans those convicted of domestic violence from owning a firearm. Some states require police to arrest once probable cause of a misdemeanor is established. Second, laws can decrease domestic violence if by aggressively adhering to the law and pursuing domestic violence offenders, recidivism drops. This, however, assumes that aggressive laws are aggressively implemented. To explore this, I also tested whether select statutes increase the likelihood that the police discover an incident, and if any influence officers' decisions to arrest.

Figure 2 summarizes the findings by illustrating how each statute influences 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 figure shows that while relatively few policies have an impact on reporting and arrest, most seem to reduce the overall probability of domestic violence.

--Insert Figure 2 About Here--

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One of the more interesting of these are the results for *Mandatory Arrest*, which are significant in two of the three components of the process. The findings suggest households in states that mandate arrest are less likely to suffer from spousal violence. Interestingly, police in these same states are less likely to discover an event suggesting that the mandatory arrest law not only deters violence, but it keeps people from calling the police. This is not entirely surprising since many women are not interested in punishing their abusers. They simply want the violence to stop (Harvard Law Review, 1993). Notably absent from Figure 2 are any assurances that statutory mandates to arrest will lead to an increased propensity to arrest.

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. First, 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 speculation as to the process responsible for difference in the average behavior of victims and offender depending on the legislative profile.

The findings in Figure 2 strongly suggest that a household's proclivity towards family and intimate violence is sensitive to the statutory profile of its state. Would-be offenders who perceive a high cost to violence appear to refrain from acting out their aggression. This is most evident by 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

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victims and offenders who more naturally cohabitate, such as family members or spouses, are unaffected when eligibility is expanded. Further, three statutes strongly imply that the state will assuredly sanction those who violate a protection order. Some states mandate arrest, some confiscate firearms, and some prosecute the offender as a felon. All three responses appear to deter one or more forms of domestic violence.

Findings further suggest that when the state expresses that it will prosecute violators at 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. However, the results only weakly suggest that the police are more likely to arrest a suspected felon (see Table 6).

While the findings here are encouraging, they do raise concerns for local officials. While mandatory arrest laws seem to protect wives from potentially abusive husbands, they show no significant influence on officers' arresting behavior. Laws will only continue to effectively deter 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 called without getting relief. In their study of state laws influence on intimate partner homicide, Dugan, et. al. (2000) found that cities in states with mandatory arrest laws had higher rates of wife homicide. A second concern is that mandatory arrest laws, on average, *keep* victims from seeking police protection. Victims who are in dire need of protection may avoid police contact to avoid arrest.

A final concern is that husbands appear to become more antagonistic when the state awards temporary custody of the children to the victim. Officers and policy makers should consider additional measures to protect victims when children are involved.

Further, sharing children with the perpetrator increases a mothers potential for to be revictimized during visitation. How then can laws aggressively pursue domestic violence offenders without compromising victims' safety?

In sum, aggressive policy does appear to reduce domestic violence suggesting that we need to continue adopting statutes that protect victims and sanction offenders. However, we also need to assure that local officials are properly 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 arrest relates to future violence in different policy environments. Important next steps would be to examine the interactive relationships between legislation and an incidents' context. Does the "retaliatory" effect of a custody statue only impact homes with children? Does the "gag" effect of mandatory arrest laws weaken as the violence becomes more severe?

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Appendix

Table A-1: Odds Ratios Predicting Reporting

Variable	Police Informed	Arrest
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
Victim		
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
Offender		
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
Incident		
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
Location		
Urban	1.090	1.068
South	1.190**	0.843
Public	0.920	--
Time		
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***

$* = p < 0.10, ** = p < 0.05, *** = p < 0.01$

Tables and Figures

Table 1: Summary of Dependent Variables

Variable	Measure
Deterring Violence	
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
Informing Police and Arrest	
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

Table 2: Summary of Variables Describing State Statutes

Variable	Measure
Beyond Cohabitaton	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

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Table 3: Summary of Household-Level Control Variables

Variable	Measure
Stability	
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
Risk	
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
Demographics	
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
Survey Issues	
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

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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

[a]Missing months were set at zero.

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, attemped aggravated assault with a weapon, or threatened an assault with a weapon

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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

[a]Missing months were set at zero.

[b]This 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.
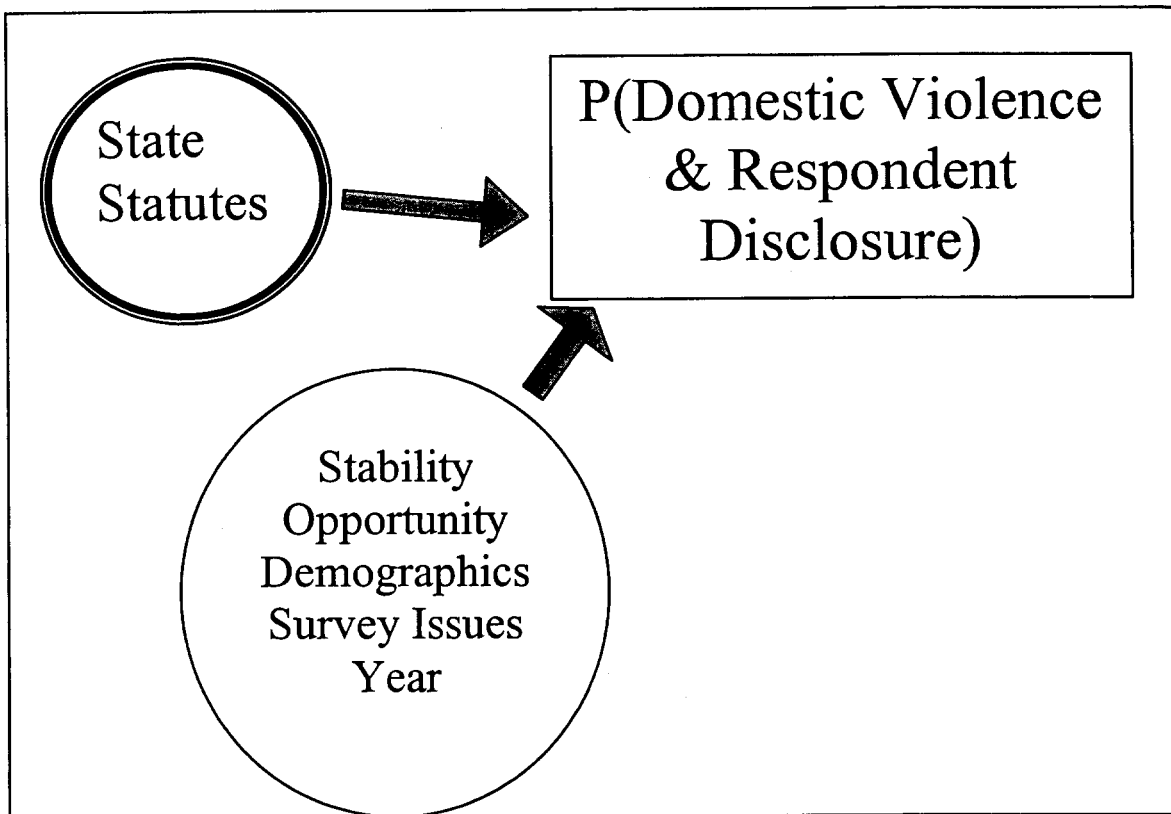
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Figure 1: Model to Estimate Legislative Impact on the Probability that Any Household Member is Violently Victimized by a Family Member or Intimate Partner
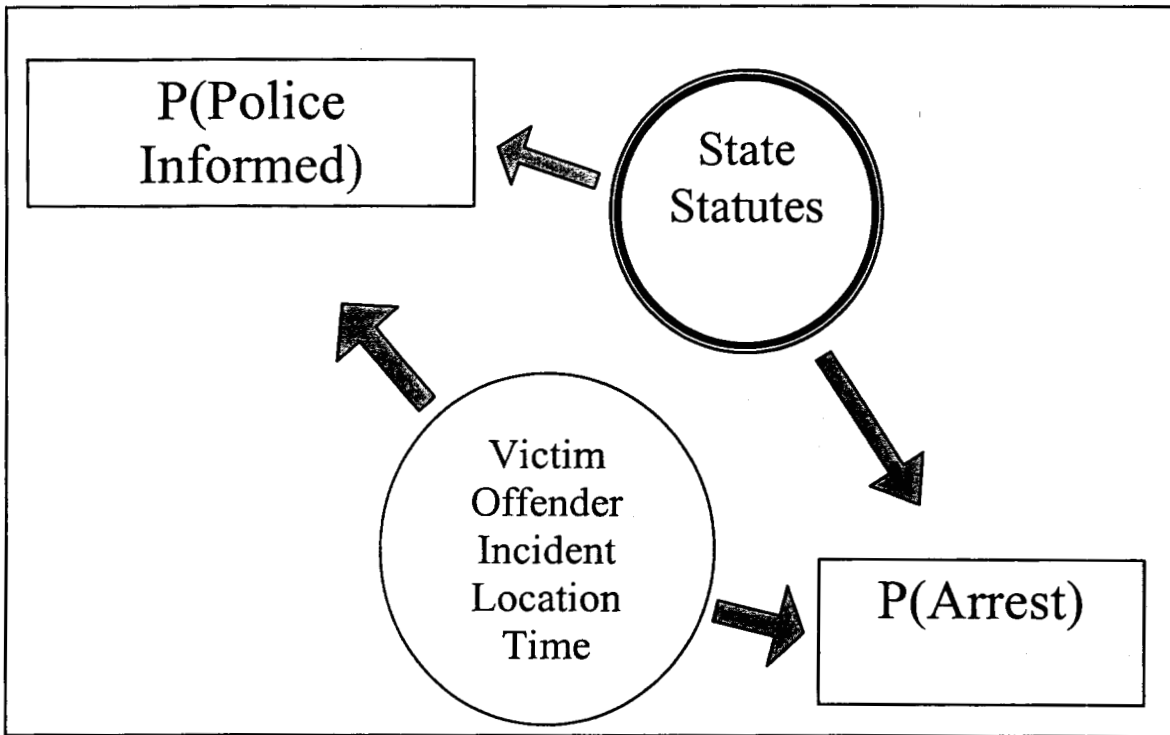
Figure 2: Models to Estimating Legislative Impact on the Probability that the Police are Informed of an Incident, and that they Make an Arrest

Table 5: Odds Ratios from Logistic Regressions Predicting Violence (n = 529,829)

Variable	Hypothesized Association	Family	Spousal	Boy/ Girlfriend
Statute Provisions				
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**
Stability				
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***
Opportunity				
Alone	-	0.377***	0.480***	1.031
Male Out	-	1.057	0.798*	0.703***
Female Out	-	0.686***	1.410***	1.624***
Demographics				
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***
Survey Issues				
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

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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 < 0.10, ** = p < 0.05, *** = p < 0.01

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Table 6: Odds Ratios of Policy on Informing Police and Arrest

Variable	Police Informed (n = 3,508)	Arrest (n = 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 < 0.10, ** = p < 0.05, *** = p < 0.01

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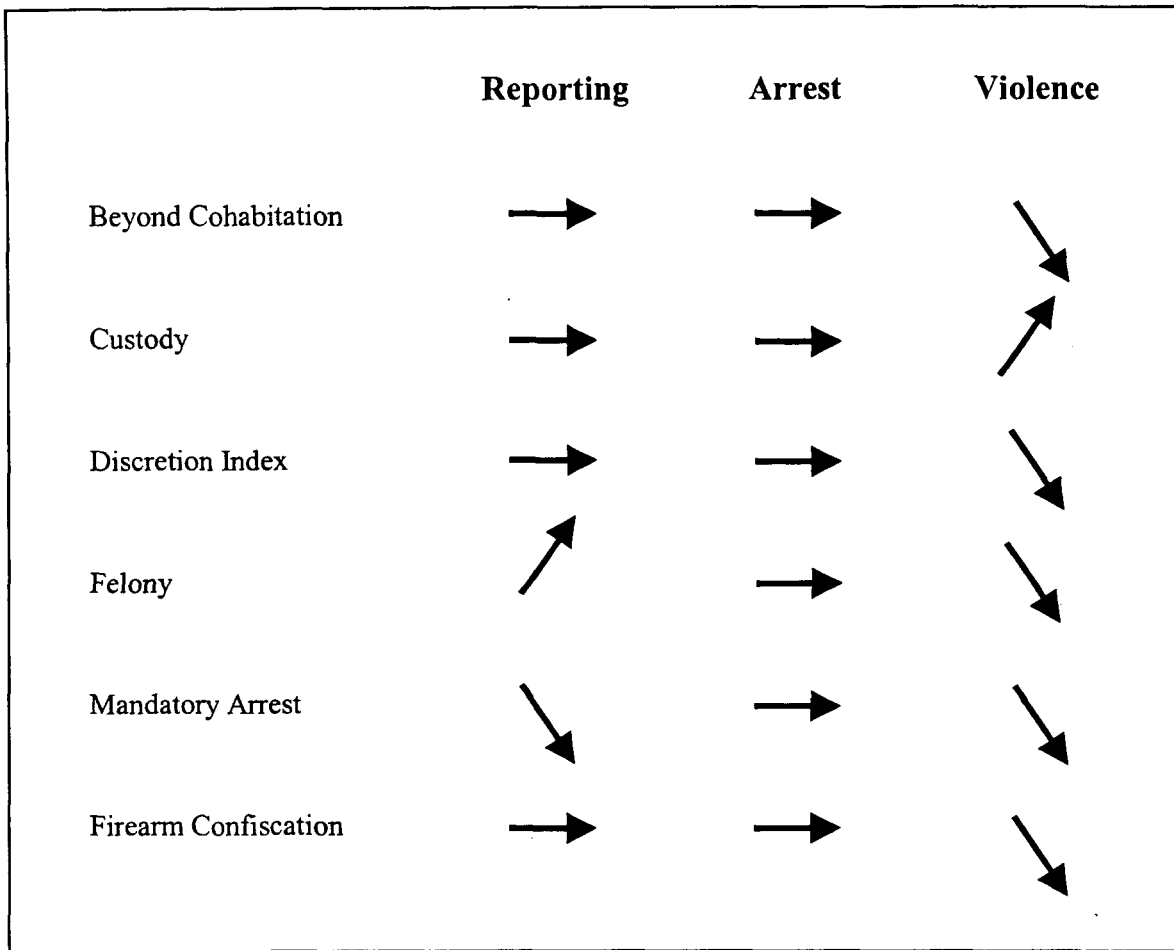|	Reporting	Arrest	Violence
Beyond Cohabitation	→	→	↘
Custody	→	→	↗
Discretion Index	→	→	↘
Felony	↗	→	↘
Mandatory Arrest	↘	→	↘
Firearm Confiscation	→	→	↘

Figure 2: Pattern of Associations Throughout Process