

The Influence of Legal Reform on the Probability of Arrest in Domestic Violence Cases

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Using domestic violence incidence and arrest data from Maryland (1991-1997), this research examines whether the proportion of incidents that result in arrest increased due to a legislative initiative implemented in 1994 and, if so, whether this change is uniform across different types of offenders (race and gender) and offense characteristics. Using interrupted time-series analysis (ARIMA), we observe an increase in both the number of incidents reported to police and the percent of reported cases resulting in arrest. The legislative intervention has a significant positive impact on arrest likelihood above and beyond the increase over time for the state as a whole. While arrest probabilities increased across the board for males and females, African American and Whites, the ARIMA models do not suggest that the legislation differentially impacted arrest probabilities for these groups.

Keywords domestic violence; legal reform; probability of arrest

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Introduction

Since the inception of mandatory and/or preferred arrest policies for domestic violence, scholars and activists alike have been interested in several research questions. Perhaps the most investigated research question stemming from these policies is whether arrest actually deters batterers (Klinger, 1995). Less attention has focused on whether these policies and subsequent changes to these policies influence the behavior of criminal justice officials. This study addresses whether expansion of a pro-arrest policy changes how police respond to domestic violence incidents by considering two questions. First, has the proportion of cases resulting in arrest increased as a consequence of legislative changes in policy? Second, have the laws affected the arrests of some types of suspects more than others?

Police and the Arrest Decision

By virtue of their power to arrest, police have been described as the most powerful of all criminal justice actors. Cole and Gertz (1998, p. 81), suggest that "the patrol officer, the most numerous and lowest-ranking of officers, has the greatest amount of discretion. He or she deals with clients alone and is almost solely in charge of enforcing the most ambiguous laws." Officers have many potential responses available when they confront a disturbance. Arrest is one of the most consequential for citizens in that it triggers further criminal justice attention. Factors believed to affect police arrest decisions include both legal (e.g., crime seriousness) and extra-legal characteristics (e.g., race of the suspect). However, the relative importance and relevance of different factors are thought to vary over time and by crime types. Legally relevant criteria, such as offense seriousness and prior arrest record, appear to have a more consistent and powerful affect on arrest decisions than extra-legal factors (Klinger, 1995; Mastrofski, Worden, & Snipes, 1995). Some studies indicate that arrest in domestic violence situations may be related to such extra-legal factors as the relationship between offenders and victims and cohabitation status (see Bachman & Coker, 1995; Buzawa & Austin, 1993; Buzawa, Austin, & Buzawa, 1995; Eigenberg, Scarborough, & Kappeler, 1996; Fyfe, Klinger, & Flavin, 1997).

Historically, scholars argued that police were less apt to arrest batterers in domestic violence situations compared to other violent offenders (Dobash & Dobash, 1979; Fyfe et al., 1997; Martin, 1976; see also review by Eigenberg et al., 1996). Indeed, prior to the 1980s, officers were advised to mediate and separate in domestic violence cases—not because it produced a safer environment for the victim but because the strategy was believed to be safer for the responding officers (Garner, 1997). Some recent studies, however, challenge this portrait. Feder (1998), for example, found no evidence of disparity regarding police handling of domestic violence cases when she compared domestic violence cases with other assault cases in one jurisdiction. Research by Klinger (1995) also found no evidence in support of the leniency thesis. Instead, he found

that arrest is an unlikely outcome for either spousal assault or other violent disputes. Such disparate findings highlight the need for careful consideration of comparison groups, differences between jurisdictions, and change over time.

In this area of research, one important source of change in police practice can be traced to the first of the famous domestic violence experiments in which arrest (relative to mediation or separation) was reported to deter batterers (Sherman & Berk, 1984). In light of these findings and with the support of many victim advocates, most jurisdictions across the country enacted preferred or mandatory arrest policies that either *allowed* officers to make an arrest without a warrant in misdemeanor domestic assault cases or *required* them to do so (Polsby, 1992, emphasis in original).

Impact of Administrative Policy on Police Behavior

Much debate surrounding mandatory or preferred arrest policies focuses on whether or not arrest has a deterrent effect on future violent incidents (see Buzawa & Buzawa, 1993; Dunford, Huizinga, & Elliott, 1990; Garner, Fagan, & Maxwell, 1995; Sherman & Berk, 1984; Sherman et al., 1992; Sherman & Smith, 1992). Research addressing this question has produced mixed results (see Garner et al., 1995). One area that has received less attention is whether or not the policies enacted on paper are translated into actual changes in police action. Studies exploring the impact of policy on the behavior of criminal justice agents have examined various policies, including those specifically related to domestic violence (Lawrenz, Lembo, & Schade, 1988).

Policies have been used to guide discretion in a variety of different areas of the criminal justice system, including pretrial release, sentencing, high-speed pursuits, use of force, and response to domestic violence (White, 2001). In an examination of the effect of administrative policy on police use of deadly force, White (2000, 2001) found that the removal of a policy guiding police use of force resulted in an increase in police shootings. Furthermore, the implementation of a guiding policy reduced shootings. Thus, the creation and elimination of policy had a significant impact on actual police behavior.

With respect to domestic violence policy, some proponents of mandatory or pro-arrest policies argue that these policies remove discretion, mandate arrest of the offender, and send a message that domestic violence will not be tolerated (Stark, 1993). Many advocates also believe that these policies will encourage victims to take action through the criminal justice system or shelters (Mignon & Holmes, 1995). While some suggest that police have a strong incentive to implement these policies in order to appear proactive, Mignon and Holmes (1995) argue that police may not necessarily want these policies and may be reluctant to change their behavior. Research with respect to community-oriented policing suggests that police (and other bureaucratic) organizations are notoriously resistant to change (see Greene & Decker, 1989). Similarly, the implementation or expansion of mandatory arrest policies for domestic violence may not produce immediate changes in police behavior.

Empirical evidence regarding police practices in light of new domestic violence initiatives is equivocal. Some researchers have examined the impact of new administrative policies on police response to domestic assaults. Policy changes have taken a variety of forms, including directives issued by police departments, presumptive arrest policies, or mandatory arrest policies. In interviews with police officers, Mignon and Holmes (1995) found that officers generally approved of these policies, suggesting that they clarify police duties and responsibilities in domestic assault incidents. In their study of the impact of a mandatory arrest statute in Massachusetts, Mignon and Holmes (1995) note that the use of arrest increased more than five times its previous level after the new law. The law emphasized mandatory arrest for violation of a restraining order, changes in probable cause requirements, and the avoidance of dual arrest. Additionally, arrest occurred in nearly half of cases in which there was an active restraining order (Mignon & Holmes, 1995). On the other hand, Dugan (2003) found that intimate violence incidents were significantly less likely to be reported to police in states with mandatory arrest laws. The presence of mandatory arrest laws also had no effect on arrest rates across states.

Similarly, Lawrenz and colleagues (1988) examined the impact of an operational directive requiring officers to write a report for every case, to make an arrest with probable cause whether or not the victim wished to prosecute, and to provide written justification for why an arrest was not made if probable cause existed. In a period of time including 6 months before and 9 months after the directive was issued, the authors found no evidence that the directive increased the use of arrest in domestic violence cases (Lawrenz et al., 1988).

Finally, Ferraro (1989) studied the impact of a presumptive arrest policy in Arizona. This policy not only specified arrest as the preferred response, even despite victim's preference against arrest, but was also followed by officer training from both police administrators and members of the domestic violence coalition. While the rate of arrest appeared to increase shortly after the policy went into effect, further clarification of the policy (i.e., specifying that probable cause was still required for arrest) confused the issue, and arrest rates dropped again (Ferraro, 1989). In her study conducted soon after the policy was implemented, Ferraro (1989) notes that police only made an arrest in 18 percent of battering incidents to which they responded, despite the policy. In about half of the incidents, police chose to use a more conciliatory response, attempting to mediate the dispute.

Studies of the impact of mandatory arrest policies on police behavior are limited, and results clearly are equivocal. Some results indicate that these policies have little or no effect on the use of arrest (Dugan, 2003; Ferraro, 1989; Lawrenz et al., 1988) while others suggest a large impact (Mignon & Holmes, 1995). Importantly, although these studies have addressed the impact of policy on police behavior more generally, they have not considered whether the policy may have a differential impact depending on offender or offense characteristics. In this study, we build on this literature by looking at the expansion of a preferred arrest policy in the state of Maryland. Specifically, we examine police

use of arrest statewide before and after legislative initiatives went into effect. We also examine pre/post effects broken down by offense seriousness and the race and gender of the offender.

Domestic Violence Legislation in Maryland

Since the late 1970s, the State of Maryland has passed a number of legislative initiatives dealing with domestic violence. In 1980, legislation established state funding of battered spouse shelters and provided the first Civil Order of Protection for victims. In 1986, a policy was put into effect wherein a police officer, who had probable cause to believe that a spouse had been battered and that the suspect was likely to flee or cause more injury, could arrest without a warrant. Family law was amended in 1987 to redefine domestic violence household members to include "unmarried persons living together and having at least one minor child in common who is residing with the parents" (www.mnadv.org/dv_laws.htm). In 1990, the warrantless arrest policy was expanded to protect individuals living together in a domestic (not necessarily marital) relationship and again, in 1994, to situations where officers had probable cause to believe a violation of an Order had occurred. The same legislation also broadened the definition of domestic violence victim to include unmarried parties who were current *or former* cohabitants (Maryland Network Against Domestic Violence, 1995). This 1994 legislation is important because it gave police authority to consider unmarried former partners who were no longer living together as *domestics*. Also, police were given the authority to arrest persons if they had *probable cause* to believe a violation of a civil protection order had occurred. Prior to this act, police could only arrest when they *observed* a violation of an Ex-Parte or Order of Protection (Maryland Network Against Domestic Violence, 1995).

Our conversations with police, court administrative officers, and victim advocates in the state led us to believe that the 1994 legislation, because of its breadth and expansion of warrantless arrest conditions, would have the greatest effect on the proportion of arrests by police in domestic violence situations. However, advocates and police officers in specialized domestic violence units were also concerned that this legislation, along with others that broadened police arrest powers, had unintentionally increased the number of women arrested as suspects in domestic violence cases (see also Henning & Feder, 2004; Martin, 1997; Miller, 2001). For instance, one battered woman's shelter that provided counseling services to batterers reported a substantial rise in the number of women who were court referred for batterer counseling, and in 1995, the Maryland legislature took up the issue with new legislation that actively discouraged the dual arrest of perpetrator and victim.

Current Study

Our study attempts to answer two questions derived from previous research, namely (1) what has been the impact of a significant legislative reform on police

practice in domestic violence cases, and (2) has the effect of the reform been uniform (across offender and offense characteristics)? To examine these questions, we use data collected from local police departments, sheriff's offices, and state police barracks throughout the state of Maryland. These data include all domestic violence incidents in the state that were reported to state police by law enforcement agencies between 1991 and 1997. We include only official police reports of cases that are defined by responding officers as domestic (e.g., spouse, former spouse, boyfriend or girlfriend, cohabitants, etc.).

Hypotheses

We have developed a number of hypotheses about the suggested effect of this legislative change on the use of arrest. Our first hypothesis suggests that, after arrest powers were expanded through the legislative mandate, the proportion of domestic violence cases resulting in arrest will increase.

- H1: The legislative change will increase the use of arrest in domestic violence cases throughout the state

As noted, police and activists in the state raised concerns that shifts in legislative mandates led to more women being arrested in domestic violence cases, and in 1995, legislative provisions were passed to discourage dual arrests. The current literature on dual arrests also suggests that women are at increased risk of arrest after pro or mandatory arrest policies are put into effect (see Martin, 1997). We therefore expect that female arrest probabilities will increase after 1994. However, this risk may be affected by the race of the suspect. Although the dual arrest literature seems to reveal a greater risk for White females (Martin, 1997), other research on police behavior suggests that this change will primarily affect African American instead of White females (Visher, 1983). Our second hypothesis therefore expects that the legislative change in 1994 should affect the arrest probabilities of females more than males and that the arrest probabilities of females will be conditioned by race.

- H2a: The legislative change specifically will increase the use of arrest in domestic violence cases with a female offender
 H2b: Female arrest probabilities after the legislative change will be conditioned by race

Due to the nature of the legislative change, we expect that its impact may be limited to specific situations. In particular, the 1994 expansion of preferred arrest specifically expanded police arrest powers to those cases involving former cohabitants. Therefore, we expect that this change will have a greater effect in those cases involving victims and offenders who are no longer living together.

- H3: The legislative change will increase the use of arrest more in cases involving victims and offenders who are estranged than in cases involving cohabitants

Finally, we expect that the main impact of the legislative change may be seen in less serious cases. Harris (1993, p. 172) notes that "the more serious the crime, the less the elasticity in the social system's arrest ... response." For instance, in most cases of homicide, police have little choice but to arrest suspects. Similarly, we would argue that domestic violence cases involving weapons are viewed as being more serious by police and result in arrest more often. Because arrest is already likely in these cases, the impact of the legislative change may be limited to those cases that are less serious (i.e., no weapon involved).

- H4: The legislative change will increase the use of arrest more in cases that do not involve weapons

Data Source

Beginning in the late 1970s, the Maryland House of Delegates mandated the State to collect information about the extent and nature of spousal assault in Maryland. This resolution led to the development of the *Battered Spouse Report*, a form, similar to the Uniform Crime Report forms, in which information about spousal assault incidents is recorded by police departments and reported monthly to the Maryland State Police. Beginning in November of 1977, these data (in hard copy) have been compiled by the Maryland State Police. In 1991, the data were automated, and, with one exception, it is the automated data (1991-1997) that are used in our study.

The *Battered Spouse Report* (BSR) contains fairly comprehensive information about the domestic violence incident. The Report contains details about: (1) the reporting agency; (2) characteristics of offenders and victims (including living arrangements); (3) the date and time of the incident; (4) the degree of injury; and (5) the circumstances surrounding the event. Finally, and of primary concern for our purposes, each case reported in the BSR indicates whether an arrest was made.

Our data include all cases reported by police agencies as domestic violence incidents between 1991 and 1997 ($n = 147,712$). Of these, 83 percent were classified as a simple assault. The majority of cases involved no reported injury to the victim. About 42 percent of cases involved slight injury, and nearly 7 percent involved serious injury. The vast majority of offenders were men, and the vast majority of victims were women. More than 80 percent of incidents involved a male offender and a female victim. About 19 percent involved a female offender and male victim. About half of both offenders and victims were White. Somewhat less than half were African American. Fewer than 1 percent of offenders and victims were categorized in other racial groups (Asian, Native American, or Other). Approximately 94 percent of incidents were intra-racial.

Variables and Analytic Strategy

The dependent variable of interest in our study is the likelihood that police will arrest the suspect in domestic violence incidents. For each case, the arrest variable is binary coded (0 = no arrest was made, 1 = an arrest was made at the scene). Overall, between 1991 and 1997, about one-third of domestic violence incidents resulted in an arrest. Our independent variable for this study is the legislative change. To investigate whether the change in policy impacted police behavior and, if so, whether the arrest probabilities of some groups were affected more than others, we first need to specify when the intervention of interest went into effect. The Domestic Violence Act of 1994 (House Bill 630) was passed by the Maryland Legislature on May 26, 1994 and took effect in October of that year. Because the automated data are collected monthly, we are able to categorize cases as occurring prior to the intervention (before November 1, 1994) or after the intervention (from November 1994 until the end of the data collection period).

We will use ARIMA techniques to assess the impact of the legislative change on the use of arrest (McCleary & Hay, 1980; McDowall, McCleary, Meidinger, & Hay, 1980). As a tool for impact assessment, this strategy has two components. First, the underlying social process (in our case, the likelihood of arrest) is modeled as a time series. Second, a discrete intervention (in our case, legislative change) divides the series into those observations occurring before and after the intervention. In this way, the statistical model provides a comparison of the pre- and post-intervention periods while accounting for the dependence of observations (McCleary & Hay, 1980; McDowall et al., 1980). With monthly data compiled over 7 years, we have 84 data points in our analyses. Forty-six of those data points occur before the intervention (specified as November 1994), and 38 occur after the intervention. For each month, we calculated the proportion of cases that resulted in an arrest, and these are the data modeled in the time-series analyses.

Results

Since data on the number of domestic violence incidents and the number of arrests in the State of Maryland are available in non-electronic form starting in 1978, we are able to provide the reader with a long-term view of changes in the use of arrest over time. As is apparent in Figure 1, both the number of domestic violence incidents reported to police and the percent of those incidents resulting in arrest have risen fairly steadily over time. In 1978, about 16 percent of police contacts resulted in arrest. By 1997, that number had increased to nearly 41 percent of incidents. This is an impressive increase in the use of arrest over time (approximately 250 percent), especially when the number of incidents reported has also substantially increased, from 1,622 in 1978 to 10,517 in 1997 (Garner, Hickman, Simpson, Allen, & Woods, 1999). Since our main goal is to

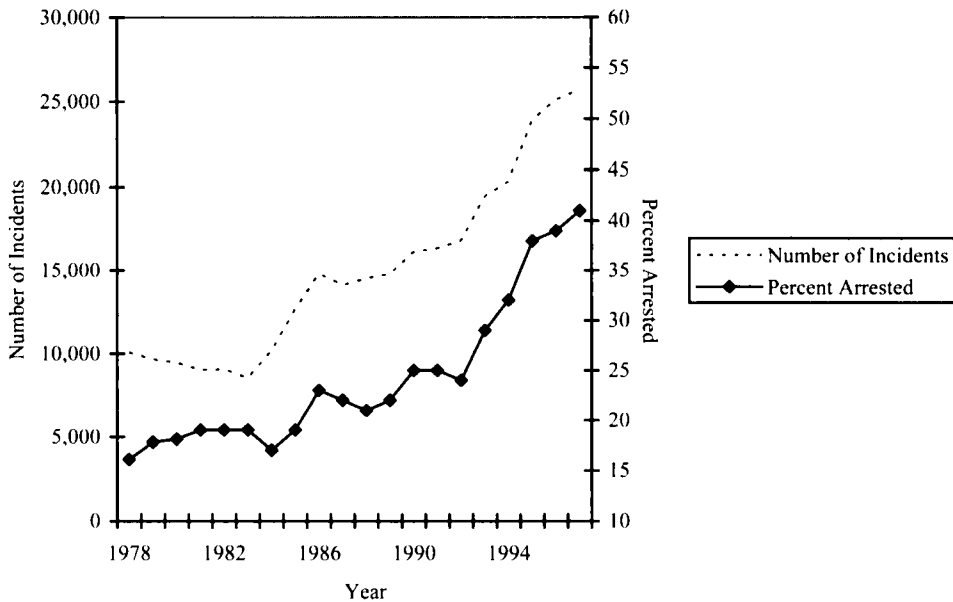


Figure 1 Maryland battered spouse/domestic violence reports, 1978-1997.

assess whether a specific policy change influenced arrest practices overall and for specific groups of people or incidents, we now turn to the more detailed electronic data (1991-1997) for subsequent analyses. The electronic data will allow us to examine monthly fluctuations in reported incidents and arrests—something that we cannot do with the non-electronic data.

Before assessing the impact of the legislative change, the dependent series (i.e., arrest over time) must be reduced to a white-noise process. In this procedure, referred to as "prewhitening," variation in the dependent series attributable to prior observations in that series is removed (see Cochran, Chamlin, & Seth, 1994 for a more detailed discussion of this procedure). Figure 2 provides a plot of the 1991-1997 raw time series. An examination of the autocorrelation function (ACF) and partial autocorrelation function (PACF) for the raw series and the augmented Dickey-Fuller test indicates that the series is nonstationary in its level (i.e., the level of the series varies over time). The series therefore requires differencing in which each observation is subtracted from the subsequent observation. Additionally, the series appears to be nonstationary in its variance (i.e., the variance is not constant throughout the length of the series). To account for this, we use a natural log transformation of the series. Finally, examination of the ACF and PACF for the differenced series indicates the presence of a first-order moving average process in which each current observation is influenced by the previous observation (McDowall et al., 1980). Thus, the final univariate model for this time series takes the form of an ARIMA (0, 1, 1) model with a log transformation. Examination of the

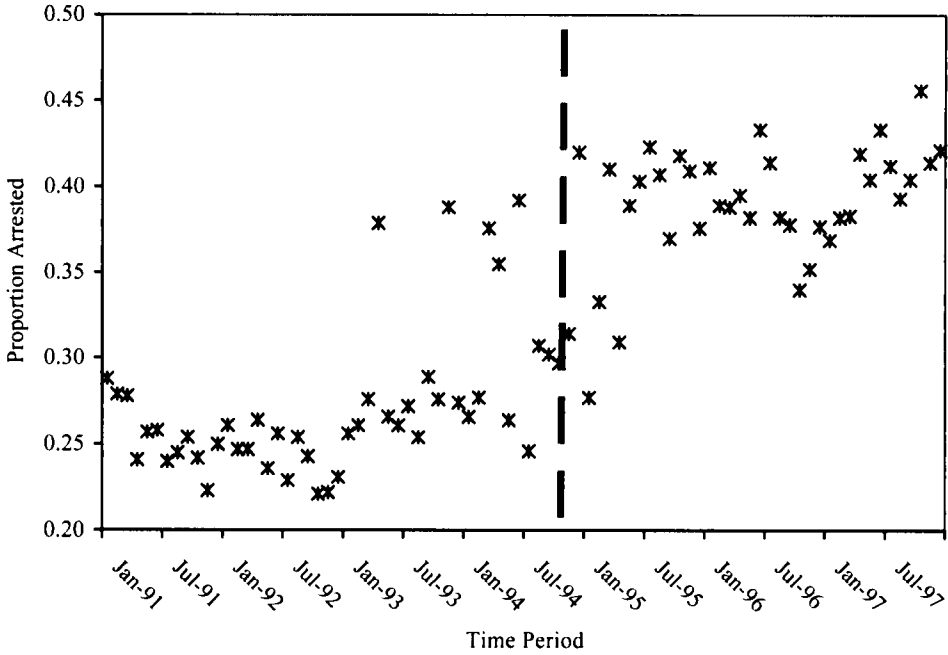


Figure 2 Proportion of domestic violence cases resulting in arrest statewide, January 1991- December 1997. The vertical line represents the legislative change effective November 1994.

model residuals indicates that all sources of systematic correlation among the variables were removed (i.e., the model residuals are white noise, as indicated by the Ljung-Box chi-square). Using this model, additional analyses examine the specific impact of the legislative change on arrest probabilities overall and for specific subgroups.

Effect of the Legislative Change

One way to examine whether arrest probabilities change post-intervention is to compare the mean level of pre-intervention arrests with those post-intervention. Figure 2 illustrates monthly data on the proportion of cases resulting in arrest between 1991 and 1997. In addition to an apparent increase in the likelihood of arrest over time, there also appears to be a jump in arrest around the time of the policy change. Pre- and post-intervention data presented in Table 1 show differences in the mean level of arrest between the pre- and post-intervention periods. Prior to the legislative change in November 1994, police made an arrest in about 27 percent of cases statewide. After the legislation went into effect, about 39 percent of domestic violence cases resulted in an arrest. Unfortunately, this analytic technique, while visually compelling, cannot statistically

Table 1 Percent of offenders arrested in Maryland before and after the legislative change took effect (November 1994)

	Percent arrested	
	Before legislation	After legislation
<i>Overall</i>	27.3	39.1
<i>Offender sex</i>		
Female	21.1	31.4
Male	28.6	41.1
<i>Offender race/sex Categories</i>		
White females	17.9	25.5
Black females	25.3	38.1
White males	26.8	36.1
Black males	30.7	45.8
<i>Cohabitation status</i>		
Living together	27.9	37.8
Estranged	24.2	41.9
<i>Weapon</i>		
Weapon used	35.7	44.6
No weapon	25.6	38.8

distinguish between an effect of the legislation and an ongoing process over the time period in question (i.e., it is possible that there were general increases in the likelihood of arrest throughout this time period that were unrelated to the legislative change). Therefore, a time-series model is needed to provide an adequate statistical test of the hypothesis.

While we theoretically expect that the implementation of this legislation should have an abrupt and permanent impact on the likelihood of arrest, the appropriate functional form of the intervention should be explored empirically (see Cochran et al., 1994). Therefore, Table 2 presents results for various functional forms of the November intervention. These results immediately call into question a view of the intervention as a pulse function (i.e., an abrupt, temporary intervention). The intervention parameter (ω) is not significant, and the rate-change parameter (δ) is a negative value which lies outside the bounds of system stability (i.e., δ must be between zero and one). Therefore, we can conclude that this intervention is not characterized by a pulse function. While both the zero-order and first-order transfer functions indicate a significant intervention parameter, AIC values suggest that the November intervention appears to be best characterized by a zero-order transfer function (i.e., an immediate, permanent impact of the policy change). Additionally, a 95 percent confidence interval around the rate change parameter (δ) for the first-order transfer function lies outside the bounds of system stability. Therefore, a gradual impact of the intervention is not supported, and we use the zero-order transfer function (i.e., an abrupt, permanent change as expected) for this and subsequent analyses. As

Table 2 Statewide impact assessment of legislative change effective November 1994

Variables	Estimate (SE)	t-ratio	AIC
<i>Pulse function</i>			
Moving average (θ)	.72 (.08)	8.89***	-534.75
Intervention (ω)	-.14 (.10)	-1.45	
δ	-.59 (.38)	-1.56	
<i>Zero-order transfer function</i>			
Moving average (θ)	.77 (.07)	10.48***	-544.02
Intervention (ω)	.15 (.07)	2.08*	
<i>First-order transfer function</i>			
Moving average (θ)	.80 (.07)	11.51***	-537.61
Intervention (ω)	.06 (.03)	1.66*	
δ	.80 (.14)	5.90***	

Note. Model identification (0, 1, 1) with log transformation. Ljung-Box chi-square not significant at $p = .05$ and 25 lags for all models.

* $p < .05$; ** $p < .01$; *** $p < .001$.

predicted in our first hypothesis, results from the zero-order transfer function model suggest that the legislative change significantly increased the likelihood of arrest statewide ($\omega = 0.15$, $t = 2.08$).

Differential Effects by Gender and Race

An important question, addressed in the next set of hypotheses, is whether the overall likelihood of arrest is consistent for different groups of people. In particular, research has raised a concern that mandatory or preferred arrest policies may inadvertently increase the arrest of women in domestic assault cases (Henning & Feder, 2004). Both before and after the intervention, the likelihood of arrest was higher for male offenders (see Table 1). Additionally, the average probability of arrest was higher in the post-intervention time period (compared to pre-intervention) for cases involving both male and female offenders. In cases with male suspects, the proportion resulting in arrest increased from 29 percent before the policy change to 41 percent after the policy. With female suspects, the proportion of cases resulting in arrest increased from 21 percent to 31 percent. It appears that the difference between pre- and post-intervention levels is similar for both male and female suspects (increasing by a little more than 10 percentage points).

Table 3 presents the impact assessment of the legislative change modeled separately by gender of the offender. Modeled as a zero-order transfer function, the legislative change significantly increased the likelihood of arrest for both female ($\omega_0 = 0.23$, $t = 2.16$) and male offenders ($\omega_0 = 0.15$, $t = 2.02$). While the magnitude of the intervention coefficient appears to be greater for female offenders, a coefficient comparison test finds no significant difference between

Table 3 Impact assessment of proportion of cases resulting in arrest by gender (November 1994 as abrupt, permanent intervention)

Variables	Male offender		Female offender	
	Estimate (SE)	t-ratio	Estimate (SE)	t-ratio
Moving average (θ)	.78 (.07)	10.79***	.78 (.08)	10.30***
Intervention (ω)	.15 (.07)	2.02*	.23 (.10)	2.16*
AIC	-534.90		-535.46	

Note. Model identification (0, 1, 1) with log transformation. Ljung-Box chi-square not significant at $p = .05$ and 25 lags for all models.
 * $p < .05$; ** $p < .01$; *** $p < .001$.

the models.¹ Thus, there appears to be an increase in the use of arrest as a result of the legislative change but little difference in that change by gender.

We also specifically suggest that the legislative change in 1994 would have a greater impact on the arrest probabilities of African American females compared to White females. Our simple bivariate analyses comparing the average probability of arrest before and after the intervention (see Table 1) show that the probability of arrest for Black females increased from about 25 percent before the legislation to about 38 percent after the legislation, while cases involving White females increased from about 18 percent before the legislation to about 26 percent after the legislation. Thus, on average, African American female offenders are arrested in domestic violence cases more often than White females both before and after the legislative change. As noted, however, bivariate analyses cannot untangle long-term causal processes that could be producing changes in arrest independent of the intervention.

Table 4 shows the time-series results of the impact assessment of the legislative change modeled separately for the four gender/race offender categories. The effect of the legislative change appears to differ across the groups. The November intervention did not significantly impact the likelihood of arrest for White female offenders ($\omega_0 = 0.18$, $t = 1.51$). However, the intervention significantly increased the likelihood of arrest for White male ($\omega_0 = 0.11$, $t = 1.69$), Black male ($\omega_0 = 0.19$, $t = 2.00$), and Black female offenders ($\omega_0 = 0.28$, $t = 2.68$). Yet, despite the differences in significance, coefficient comparison tests show no significant difference in the impact of the intervention across the four groups.²

1. The formula for the coefficient difference z-test is: $z = \frac{b_1 - b_2}{\sqrt{SEb_1^2 + SEb_2^2}}$. See Paternoster, Brame, Mazerolle, and Piquero (1998), for a discussion of coefficient difference tests.

2. There is no global test for differences between more than two coefficients (Paternoster, 2005, personal communication). In this analysis, we use the standard coefficient comparison z-test (Paternoster et al., 1998). With multiple comparisons, this does raise a concern about alpha inflation. To account for this possibility, we have used a higher level of significance (.01).

Table 4 Impact assessment of proportion of cases resulting in arrest by gender and race (November 1994 as abrupt, permanent intervention)

Variables	White male offender		White female offender	
	Estimate (SE)	t-ratio	Estimate (SE)	t-ratio
Moving average (θ)	.67 (.08)	8.01***	.81 (.07)	11.40***
Intervention (ω)	.11 (.06)	1.69*	.18 (.12)	1.51
AIC	-599.17		-538.89	

Variables	Black male offender		Black female offender	
	Estimate (SE)	t-ratio	Estimate (SE)	t-ratio
Moving average (θ)	.81 (.07)	11.99***	.86 (.06)	14.36***
Intervention (ω)	.19 (.10)	2.00*	.28 (.10)	2.68**
AIC	-467.26		-463.32	

Note: Model identification (0, 1, 1) with log transformation. Ljung-Box chi-square not significant at $p = .05$ and 25 lags for all models.

* $p < .05$; ** $p < .01$; *** $p < .001$.

Cohabitation Status

Our third hypothesis suggests that because of the specifics of this legislation, the intervention will have a greater impact on the use of arrest in cases involving individuals who are no longer living together compared to cases with cohabitating parties. Table 1 demonstrates that in cases involving a cohabitating victim and offender, police made an arrest about 28 percent of the time prior to the legislative change and in about 38 percent of cases after the legislation took effect. The change was even greater in cases involving a victim and offender who were estranged. Prior to the legislative change, police arrested in about 24 percent of cases, and after the change, police arrested in nearly 42 percent of cases.

Table 5 presents the results of the impact assessment modeled separately for cases involving cohabitating victims and offenders and cases involving estranged victims and offenders. Both models indicate a significant increase in the likelihood of arrest when the legislation took effect ($\omega_0 = 0.12$, $t = 1.90$ for cohabitating; $\omega_0 = 0.30$, $t = 1.85$ for estranged). While there do appear to be differences in the magnitude of the intervention coefficients, coefficient comparison tests indicate that the differences are not statistically significant. Contrary to our hypothesis, the legislative change does not seem to have differentially impacted cases in which the victim and offender were no longer living together despite the specific language in the legislation.

Table 5 Impact assessment of proportion of cases resulting in arrest by relationship (November 1994 as abrupt, permanent intervention)

Variables	Living together		Estranged	
	Estimate (SE)	t-ratio	Estimate (SE)	t-ratio
Moving average (θ_1)	.74 (.08)	9.54***	.80 (.07)	11.84***
Intervention (ω_0)	.12 (.06)	1.90*	.30 (.16)	1.85*
AIC	-580.27		-408.34	

Note: Model identification (0, 1, 1) with log transformation. Ljung-Box chi-square not significant at $p = .05$ and 25 lags for all models.

* $p < .05$; ** $p < .01$; *** $p < .001$.

Use of a Weapon

Our final hypothesis suggests that the intervention will have a greater impact on the use of arrest in cases that do not involve a weapon compared to those cases involving the use of a weapon, which may be viewed as more serious. In cases involving a weapon, police made an arrest in about 36 percent of cases prior to the legislative change and in about 45 percent of cases after the legislation took effect (see Table 1). The change was even greater in cases not involving a weapon. Prior to the legislative change, police arrested in about 26 percent of cases, and after the change, police arrested in nearly 39 percent of cases.

Table 6 presents the results of the impact assessment modeled separately for serious cases involving weapons and those not involving the use of a weapon. The impact of the legislative change is only significant for cases not involving a weapon ($\omega_0 = 0.17$, $t = 1.96$). For the presumably more serious cases that involve weapons, there was no significant impact of the legislation ($\omega_0 = 0.08$, $t = 1.40$). These results support our hypothesis that the nature of the legislative change would have a greater effect in those cases that may be viewed as less serious by police because they do not involve a weapon. However, coefficient comparison tests again indicate no difference between the groups.

Summary and Conclusions

A great deal of research has examined the deterrent effects of police decisions to arrest in domestic violence cases. However, few studies have addressed the impact of pro-arrest policies on police use of arrest. Using official data drawn from the Battered Spouse Reports (1991-1997) in the state of Maryland, we explored whether a legislative initiative to expand police arrest powers positively affected the percent of reported cases that resulted in arrest. We also examined whether the effect of the policy was uniform. Generally, our findings demonstrate that there was both an increasing likelihood of arrest during the

Table 6 Impact assessment of proportion of cases resulting in arrest by weapon use (November 1994 as abrupt, permanent intervention)

Variables	Weapon		No weapon	
	Estimate (SE)	t-ratio	Estimate (SE)	t-ratio
Moving average (θ_1)	.77 (.07)	10.48***	.78 (.07)	10.70***
Intervention (ω_0)	.08 (.06)	1.40	.17 (.09)	1.96*
AIC		-546.12		-515.92

Note: Model identification (0, 1, 1) with log transformation. Ljung-Box chi-square not significant at $p = .05$ and 25 lags for all models.

* $p < .05$; ** $p < .01$; *** $p < .001$.

time period of the study and a significant and positive impact of the policy on the likelihood of arrest. However, we found less support for the idea that the arrest policy affected groups of offense types differently.

Statewide, males in general tended to be arrested more than females in cases of domestic violence both before and after the policy went into effect. Additionally, arrest rates increased similarly and significantly for both male and female offenders as a result of the legislative change. Contrary to our expectation, it does not appear that expanding police powers to arrest necessarily impacts women *more* than men. Nor are African American women at a greater risk for arrest as a consequence of the legislation than are White women. However, the mere fact that female arrest rates have increased as a result of the domestic violence policy suggests that interventions in this area must necessarily focus greater attention around these "new offenders." Who are these women? Are they similar or different from their male counterparts and women who are not arrested in dual arrest situations (Henning & Feder, 2004)? There is some evidence that many of these women are victims who have used violence defensively for protection or that the women offenders are significantly less likely than males to have histories that would put them at risk for future violence (Henning & Feder, 2004). Police are unable to sort out the offender from the victim in some of these cases; hence, both parties are arrested, but as Martin (1997) points out, dual arrest traumatizes victims psychologically, emotionally, and economically. They are discouraged from seeking police help when battering occurs. Feminist scholars have recognized the conundrum faced by battered women of color who mistrust the justice system and are loath to use it, even when they desperately need to do so. Our results suggest that this concern may not be misplaced, and indeed, women themselves may be at increased risk for arrest when the police are called. Clearly, more empirical work is needed to study women who are arrested and mandated for treatment.

One of the primary aims of the legislative mandate was to expand arrest powers beyond partners currently sharing a domicile. Thus, we anticipated that the new policy would have a greater impact on those offenders who were not

living with the victim. Our analyses, however, did not support this hypothesis. Because these data do not allow us to disentangle married from unmarried cohabitating partners, it is unclear whether our null findings are a methodological artifact. The Battered Spouse Reports did not begin collecting information about the marital status of the suspect and victim until 1996. Therefore, we cannot address any differential effects by marital status or the interaction between cohabitation and marital status.

Finally, we expected that cases viewed as less serious by police because the offender did not use a weapon would be more amenable to change as a result of the legislative policy. We anticipated that when weapons were involved, police would have less arrest flexibility (i.e., less elasticity in the social system's response to serious crime; Harris, 1993). Legislative policies thus would have more of an impact on cases that the police define as less severe. While our preliminary analysis showed a significant difference in arrest probabilities post-intervention for cases without a weapon but not for those in which a weapon was used (consistent with our hypothesis), the coefficients for each of these groups did not differ significantly from one another. Thus, overall, it appears that the change in policy had a consistent impact across various offender and offense types.

In closing, it is important to highlight some data problems which limit the kinds of analyses and conclusions that can be drawn from this research. In addition to the already noted lack of information about marital status, violent incidents in these data are not limited to those in which the offender was present at the scene when police arrived. We therefore are unable to determine which offenders were present or what variables predict whether an offender leaves the scene prior to arrest. We do know that the coding of these cases is inconsistent. Interviews with reporting staff within police agencies revealed that, on some occasions, arrests occurring after the incident (not on-scene) were reported on subsequent monthly reports to the Maryland State Police. These are then counted as arrests in our data. In other jurisdictions, cases in which warrants were issued but an arrest was not made at the scene were coded as open (outcome unknown). We also discovered that some cases were coded as exceptionally cleared. These discrepancies obviously affect our outcome variable (arrest versus not), but it is difficult to predict whether arrest probabilities are over- or under-counted as a consequence of these practices. Additionally, because we are looking at changes in the proportion of cases resulting in arrest over time, there is no reason to believe that the number of offenders who left the scene or the reporting practices of police agencies would change in any consistent way. Thus, we should still be able to detect changes in arrest practices due to the legislation.

In sum, it appears that police are generally using arrest more frequently over time and that legislative initiatives (at least in Maryland) have had the positive impact on arrest that advocates had hoped. On a more problematic note, however, more women, especially women of color, are entering into the criminal justice system as domestic violence offenders. While this trend does

not appear to be attributable to the legislative change identified in our study, it is unclear whether these offenders are really victims caught up in a dual arrest policy or batterers in their own right. Future research should explore the reason behind these increases. We also believe it will be useful to consider how police decision heuristics, conceptions of "typical" crimes and criminals, and the flow of cases may be affected by regional population demographics (Emerson, 1983; Klinger, 1997). These factors are likely to affect arrest patterns over time. Finally, although our research allowed us to explore the general impact of the legislative change, it was not possible to measure officers' subjective understanding of cases or to assess whether these perceptions changed as a consequence of legislative mandates or ecological features. Longitudinal studies of police and their decision frames are necessary to study these processes.

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