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# Intimate Partner Violence

## Predictors of Recidivism in a Sample of Arrestees

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This article seeks to identify the variables that predict intimate partner violence recidivism. Neither prosecutorial actions, such as case rejection or subsequent dismissal, nor judicial imposition of jail terms or placement in a batterer treatment program demonstrated predictive value. Extralegal variables associated with the defendant, such as cohabitation status, marital status, gender, employment, and substance use at the time of the incident, all failed to attain significance. Victim support for arrest and prosecution were also unrelated to revictimization. Conversely, use of a weapon, the offender's prior arrest for any offense, and the presence of a protective order at the time of the precipitating incident all predicted rearrest for intimate violence within an 18-month follow-up period. Policy implications are discussed.

**Keywords:** *intimate partner violence; recidivism; revictimization*

Driven in large measure by concerns for victim safety, both social policy and the criminological research agenda have undergone dramatic transformation in the area of intimate partner violence (IPV) during the past 25 years. As a result, institutional changes have occurred in all three segments of the criminal justice system (law enforcement, courts, and corrections) aimed at the reduction of batterer recidivism (Binder & Meeker, 1992; Buzawa & Buzawa, 1996; Mills, 1998; Schmidt & Sherman, 1996; Zorza, 1992). These innovations, for the most part, have been predicated on the assumption of deterrence theory that increases in the severity of punishment will be associated with reductions in rates of reoffending.

In the domain of law enforcement, this perspective is reflected in the adoption in most jurisdictions of mandatory or presumptive arrest laws. Initially, research findings such as those from the Minneapolis Domestic Violence Experiment seemed to provide support for a specific deterrent effect of arrest on recidivism (Sherman & Berk, 1984). The results of the Minneapolis experiment, however, have not been consistently supported by subsequent replications sponsored by the National Institute of

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Justice, and the deterrent effect of arrest thus remains an open question.<sup>1</sup> Following the apparent overall failure of the National Institute of Justice-sponsored replications to support the Minneapolis findings, Sherman, Smith, Schmidt, and Rogan's (1992) reanalysis of the Milwaukee experimental data suggests an interaction effect between arrest and the perpetrator's "stake in conformity" in the prediction of repeat battering. More precisely, those who are employed and married have more to lose and thus are more susceptible to the deterrent effect of arrest than are those who are unmarried and unemployed, for whom an arrest may elevate levels of repeat battering.

A number of authorities have suggested that the deterrent effect of arrest cannot be assessed without consideration of criminal justice system responses subsequent to law enforcement action (Davis, Smith, & Nickles, 1998; Thistlethwaite, Woolredge, & Gibbs, 1998; Tolman & Weisz, 1995; Woolredge & Thistlethwaite, 2005). If, as Feeley (1979) suggests, the "process is the punishment," then deterrence theory would predict that the further a case is continued in the criminal justice system, the greater the negative impact on rates of reoffending. In particular, prosecutorial decision making, such as the decision to file (or not file) charges and the decision to pursue full prosecution (vs. dismissal for insufficient evidence) may well suppress batterer recidivism.

However, research into the effects of prosecutorial decision making on IPV reoffending has produced inconsistent results. Tolman and Weisz (1995) found no deterrent effect of "successful prosecution." Similarly, Davis et al. (1998), utilizing a large sample ( $N = 1,133$ ), concluded that "the likelihood of recidivism was indistinguishable for cases resulting in nolle, dismissals, probation with batterer treatment programs and jail sentences" (p. 441). This conclusion was supported by Thistlethwaite et al. (1998), who also failed to find a statistically significant deterrent effect associated with prosecution and conviction. Conversely, Ford and Regoli (1992) concluded that merely bringing a defendant to court, even if the case is subsequently dismissed, reduced the risk of subsequent violence—a finding supported by Woolredge and Thistlethwaite (2002), who found higher rearrest rates associated with rejection at intake.

Extending the analysis of case processing effects, some researchers have addressed the deterrent impact of "no drop" policies. Paralleling mandatory arrest laws, prosecutors nationwide have committed their offices to these policies, which are explicit in their denial of victim control of the charging process (Cahn, 1992; Corsilles, 1994; Hanna, 1996; Smith, Davis, Nickles, & Davies, 2001). By this means, prosecutors underline their authority to act in the name of society, even if this is contrary to the expressed wishes of the victim.

Some researchers have questioned these policies, suggesting that they may elevate rates of reoffending. Ford and Regoli (1992), for example, utilizing an experimental design, concluded that the lowest rates of recidivism occurred when victims were empowered with control of the charging process and elected to go forward with full prosecution. In their interpretation, victims empowered in this way were able to

negotiate their security with their abusers. In other words, the deterrent effects of prosecutorial decision making may depend, in part, on the support of the victim for prosecution (Ford, 1991, 2003).

The work of Sherman et al. (1992), establishing the importance of the conditioning effects of a "stake in conformity" on the deterrent power of arrest, suggests the possibility of a similar relationship between this concept and prosecutorial decision making. Thistlethwaite et al. (1998), using an ordinal measure of sentence severity, found some support for the proposition that those with a stake in conformity were more likely (than those without) to be deterred by more punitive dispositions. I know of no other studies that address this relationship, and the influence of a stake in conformity on the deterrent effect of prosecutor decision making thus remains an open question.

In addition to innovations in the domains of law enforcement and prosecution, changes in judicial sentencing practices have also been driven by a concern with IPV recidivism. One important development has been the widespread imposition of participation in a batterer treatment program (BTP) as a condition of probation.

Several recent experimental studies call into question the cautious optimism of Davis and Taylor (1999) in their literature review suggesting that the null hypothesis of treatment efficacy cannot be rejected. Dunford (2000), Taylor, Davis, and Maxwell (2001), and Feder and Dugan (2002) all employed random allocation designs and found scant support for the effectiveness of treatment in reducing rates of reoffending. A meta-analysis by Babcock, Green, and Robie (2004) concluded that even when positive effects of treatment are found, they are small, and that the more rigorous the design, the smaller the effect. Taken together, these recent studies suggest a more skeptical conclusion than heretofore on the effects of treatment on batterer recidivism.

Though less frequently researched in their consequences, jail terms are also widely relied on when sentencing IPV offenders. Thistlethwaite et al. (1998) found that qualitative increases in sentence severity (i.e., moving progressively from those sentenced only to pay a fine to a jail and probation term) was associated with reduced levels of reoffending. Within these sentences, however, increases in length of jail or probation terms had no such effect. Among the few available studies, most have found no main effect of jail terms on subsequent recidivism (Davis et al., 1998; Woolredge & Thistlethwaite, 2005). On the other hand, Woolredge and Thistlethwaite (2005) did find support for an interaction effect, suggesting the odds of rearrest can be reduced by more severe dispositions (i.e., probation plus jail) for offenders with prior histories of violence.

In sum, the number of studies is too few, and the evidence too conflicting, to permit confident conclusions regarding the impact of a number of important variables (and the relationships among them) on intimate partner revictimization. The deterrent effect of a stake in conformity, victim preference for prosecution, prosecutorial filing of charges, dismissal versus full prosecution, BTPs,<sup>2</sup> and jail terms remains unresolved. This article addresses this important issue.

## Method

### Sample Characteristics

In 1997, the Sacramento County Court System established a specialized Domestic Violence Court responsible for processing all nontrial domestic violence cases (i.e., 98.6% of all such cases in this county). Concomitantly, the district attorney's office enlarged its Domestic Violence Unit to expand its processing capacity beyond felony cases to include all misdemeanors. All domestic violence prosecutions (trial and non-trial) are processed through that unit. Between January 1, 2000, and April 30, 2000, the district attorney's office closed 1,182 IPV arrests.

To promote homogeneity of the sample, I followed the common practice of confining the analysis to heterosexual couples ( $n = 1,157$ ), though unlike most recidivism studies, I retained cases involving female offenders and male victims. In addition, to facilitate comparison with other studies, I eliminated cases either filed as felonies ( $n = 218$ ) or filed as misdemeanors and resolved as felonies ( $n = 4$ ). Only cases filed either as misdemeanors or violations of probation were included in the final selection. After removal of 63 cases with missing values on critical variables (e.g., case identification numbers),<sup>3</sup> a final sample of 872 cases remained for analysis. This data set, therefore, represents the total population of IPV arrests in Sacramento County closed by the district attorney's office during this period, after excluding the groups identified above.<sup>4</sup> All coding was conducted either by the prosecutor handling the case or by legal interns working in the district attorney's office. Sources of information employed by coders include Law Enforcement Arrest and Crime Reports, California Department of Justice Arrest Histories, and, of course, prosecutor files, including Victim Services Reports. Variables used in the analysis and their values are contained in the Appendix.

### Methodological Issues

Recidivism in this article is defined as rearrest for an IPV offense within an 18-month follow-up period.<sup>5</sup> All authorities agree that reliance on official sources significantly undercounts recidivism, and I, therefore, make no claim of completeness of data. Victim interviews were beyond the resources available for this study, and the findings must therefore be qualified by this limitation. On the other hand, as several authorities have noted (e.g., Woolredge & Thistlethwaite, 2005), though the results may differ between victim self-report and rearrest, the empirical relationships in the data frequently remain the same. Moreover, official data have the advantage over self-report of being available for all cases in the sample and not just those victims contacted and willing to respond (Davis et al., 1998).

One issue of importance discussed in the research literature is identification of the appropriate length of the follow-up period. The classic studies, such as the Minneapolis

Domestic Violence Experiment and its replications, tended to rely on a 6-month time frame. Dunford (1992), however, argues (with strong supporting data) that this is too short and that a substantial number of instances of recidivism will be lost, which, if included, could reveal important relationships not otherwise apparent. For this reason, all suspects in the present sample were tracked for 18 months following the precipitating incident.<sup>6</sup>

A second major issue concerns the starting point for the follow-up period. To a degree, the answer to this question depends on the purpose in view. If the research issue is identification of the relationship among offender, victim, and incident characteristics and recidivism, this suggests the follow-up period should commence with the initial incident. On the other hand, if the research purpose is to assess the deterrent impact of a particular intervention, then the follow-up period should begin with the commencement or termination of that intervention (e.g., the prosecutor's decision to file charges, the jail term). This, however, raises a number of problems.

First, as Ford and Regoli (1992) demonstrate, a number of cases recidivate prior to the case closing date (e.g., prosecutor's decision to dismiss, judgment, and sentence). If the follow-up analysis begins with the closing date, then these cases will be lost. This is surely misleading because these rearrests represent a failure of the criminal justice system to deter during the case processing period. In the present study, for example, 19.5% of recidivists committed their repeat offense prior to the case closing date. Following Ford and Regoli (1992), therefore, I analyze presettlement and postsettlement recidivism separately (and jointly) to not lose those cases that repeat their abuse prior to the case closing date. (Recidivists during the earlier period are not removed from the sample and remain at risk during the later period.)

A second difficulty is the variability in the length of the prefilling and preclosing periods. It is surely reasonable to anticipate that cases requiring months before intake screening or final case disposition will differ in their recidivism rates (both before and after disposition) from cases requiring only a few days. Therefore, when assessing the impact of the prosecutor's decision at intake to file charges, I include a variable measuring length of time (days) from the precipitating incident to arraignment or rejection for insufficient evidence. When analyzing the impact of dismissal versus full prosecution,<sup>7</sup> I, like Ford and Regoli (1992), include a variable measuring length of time from incident to case closing date.

Related to the concerns above is the need to identify the appropriate commencement date of the follow-up period when assessing the impact of system responses, such as jail terms and BTPs. In the case of those receiving a jail term, the date of sentence completion seems most appropriate because such offenders are not at risk for recidivism while in jail. Unfortunately, I do not have information indicating the date of release from confinement for those offenders sentenced to a jail term and, therefore, must rely on the case closing date (i.e., judgment and sentence). I think it is unlikely that this has an effect on the analysis for two reasons. First, the average sentence to jail for those in the sample who were convicted is 28.5 days—a period

unlikely to have a significant effect on outcomes during an 18-month follow-up, particularly considering the availability of early release time for good behavior. Second, I recalculated the 18-month follow-up period on the assumption that everyone sentenced to jail served their full term to observe the effect on the analysis. This did not require removal of any recidivists from the sample, and there was, therefore, no effect on the conclusions.

In the case of those sentenced to a 1-year BTP, the sentence completion date is problematic because this would exclude those incidents of recidivism occurring during treatment and also those offenders who failed to complete the treatment program. This would introduce self-selection factors favoring treatment success. The analysis, therefore, includes all offenders placed in a BTP (i.e., the “intent-to-treat” group), regardless of completion. As Gondolf (2002) observes, all the recent fully experimental evaluations have used the intent-to-treat group as the basis for comparisons. This is unsurprising because, as Sherman (2003) notes, experimentalists prefer the intent-to-treat group to the “treatment received” group as the basis for evaluating program effectiveness.

## Findings

Table 1 reports the characteristics of the sample and the number of cases for which information is available on each variable. The table indicates that 77.8% of cases were continued for further action at intake ( $n = 679$ ), either with criminal filings or as violations of probation. Of these cases, 23.4% were subsequently rejected for insufficient evidence, leaving 520 cases from the original sample (59.6%) for which prosecutors sought a conviction.

Table 1 also indicates considerable prior criminal activity on the part of perpetrators, with 66.8% recorded as having a prior arrest record (any offense). Most suspects for whom convictions were sought were processed with only one charge against them. A protective order was in place at the time of the arrest in 12.9% of cases.

Only 27.8% of offenders were known to be employed, although this figure rises to 59.0% after eliminating the large proportion of cases (52.9%) for whom no information was provided in the available sources. In one third of the incidents, the couple was married. Although 84.3% of victims wanted an arrest, this figure falls dramatically to less than half who are willing to support prosecution.

With respect to race/ethnicity, there are important differences to be noted between Table 1 and the larger demographic profile of Sacramento County. Suspects of Asian or Pacific Islander descent are underrepresented, constituting only 4.5% of the data set, in contrast to 11.6% of the county population (U.S. Department of Commerce, Bureau of the Census, 2000). Conversely, African Americans make up 10.0% of the county population and 35% of suspects in this sample. Hispanic suspects are present in approximate proportion to their representation in the county (19.7% and 16.0%, respectively). Whites are underrepresented, accounting for 39.3% of suspects in the

**Table 1**  
**Descriptive Statistics**

Variable	%	<i>n</i> <sup>a</sup>
Case processing outcomes		
Intake		872
Misdemeanor filing	70.5	
Violation of probation filing	7.3	
Rejected insufficient evidence	22.2	
Postintake		679
Fully prosecuted	76.6	
Dismissed insufficient evidence	23.4	
Independents (categorical)		
Any prior arrest	66.8	808
Cohabitation	64.9	844
<i>n</i> charges filed		627
1	87.2	
2+	12.8	
Protective order	12.9	837
Suspect race/ethnicity		871
Asian	4.5	
Black	35.0	
Hispanic	19.7	
Other	2.7	
White	39.3	
Suspect substance use	25.7	872
Suspect employed	27.8	410
Suspect gender (male)	85.0	872
Spouse	33.3	872
Treatment program	47.6	501
Victim wants arrest	84.3	744
Victim wants prosecution	46.4	375
Weapon used	16.3	851
Independents (interval)		
Incident to intake (days)	43.9	872
Incident to closing (days)	77.1	872
Suspect age ( <i>M</i> )	33.5	863
Jail days ( <i>M</i> )	27.8	513
Rearrest for intimate partner violence within 18 months of precipitating incident	15.3	

Note: *N* = 872.

a. Number of cases for which data are available. In a number of instances, this will be substantially less than the full sample, primarily because of case attrition as a result of prosecutorial actions (i.e., rejection and dismissal).

sample and 64.0% of the county population.<sup>8</sup> Males constitute 85% of the sample and females 15%. These demographic characteristics of IPV offenders are very similar to those found in other studies (e.g., Woolredge & Thistlethwaite, 2005).

On average, convicted offenders received a jail term of 28.5 days, and almost half were placed in a treatment program.<sup>9</sup> Finally, of the 872 arrestees, 15.3% were rearrested for a new IPV offense within the 18-month follow-up period.

## Multivariate Analysis

Table 2 presents three models of rearrest for an IPV offense within 18 months of: the precipitating incident (Model 1), the prosecutor's filing decision (Model 2), and the decision to fully prosecute (Model 3).<sup>10</sup> Some of the variables to be included in the table have a sizeable number of missing values (see Table 1). Following Sherman et al. (1992), dummy variables were created for the missing values on these variables (i.e., suspect employment, victim wants arrest, and victim supports prosecution). This maintains a feasible sample size without making unwarranted assumptions about the meaning of these missing values.

In Model 1, few variables attain statistical significance. The two measures of prior offending (any prior arrest and presence of a protective order) are both highly influential as indicated by the odds ratios, along with use of a weapon (a measure of offense severity). None of the extralegal variables, including stake in conformity, attains significance.

Model 2 redefines the follow-up period as beginning with the intake decision to file charges or reject for insufficient evidence. This permits expansion of the analysis to include variables that measure prosecutorial decision making. Specifically, I now include measures indicating whether or not charges were filed, along with the number of days elapsing between the incident and the filing decision. In addition, a new interaction term that is the product of a stake in conformity and the filing decision is included. In this way, I hope to measure whether the deterrent impact of filing charges varies by stake in conformity.

As in Model 1, the presence of any prior arrest record is highly significant and increases the odds of recidivism more than fourfold. Similarly, the odds of rearrest are almost twice as great in the presence of a protective order, and the use of a weapon in the precipitating incident again predicts recidivism. Among the extralegal variables, African Americans are more likely than Whites to reoffend during this follow-up period. Neither the fact of filing charges nor possession of a stake in conformity has an impact on recidivism. The interaction term comprising these two variables also has no effect.

Model 3 pursues the analysis further into the case processing period by considering the impact on recidivism of full prosecution versus case dismissal. At this stage of processing, I have also substituted the more appropriate variable of victim support for prosecution for victim desire for the arrest.

Once again, the existence of any prior arrest record has a statistically significant and strong association with recidivism in this period, increasing more than fourfold

**Table 2**  
**Logistic Regression Analysis of Rearrest 18 Months After Initial Incident (Model 1), Prosecutorial Filing Decision (Model 2), and Full Prosecution (Model 3)**

Variables	Model 1		Model 2		Model 3	
	<i>B</i>	Exp( <i>b</i> )	<i>B</i>	Exp( <i>b</i> )	<i>B</i>	Exp( <i>b</i> )
Any prior arrest	1.544*** (0.401)	4.683	1.534*** (0.428)	4.635	1.545** (0.508)	4.688
Cohabitant	0.287 (0.287)		0.111 (0.290)		0.269 (0.338)	
Protective order	1.248*** (0.343)	3.484	0.692* (0.375)	1.998	0.410 (0.434)	
Suspect race/ethnicity <sup>a</sup>						
Asian	0.596 (0.653)		0.686 (0.664)		0.868 (0.696)	
Black	0.520 (0.286)		0.696* (0.296)	2.005	0.354 (0.329)	
Hispanic	-0.261 (0.376)		-0.329 (0.412)		-0.912 (0.511)	
Other	0.058 (0.896)		0.266 (0.895)		-0.001 (0.989)	
Suspect employment						
Yes <sup>b</sup>	-0.145 (0.400)		-0.383 (0.411)		-0.509 (0.483)	
Unknown <sup>b</sup>	0.031 (0.330)		-0.065 (0.337)		0.114 (0.389)	
Suspect gender (male)	0.726 (0.494)		0.700 (0.500)		0.183 (0.590)	
Suspect age	-0.017 (0.013)		-0.013 (0.014)		-0.009 (0.017)	
Suspect substance use	-0.162 (0.300)		-0.211 (0.309)		-0.241 (0.354)	
Spouse (yes)	-0.393 (0.631)		-0.451 (0.627)		-0.856 (0.868)	
Victim wants arrest						
Yes <sup>c</sup>	0.045 (0.415)		0.103 (0.437)		— —	
Unknown <sup>c</sup>	-0.688 (0.613)		-0.784 (0.636)		— —	
Victim wants prosecution						
Yes	—		—		1.527 (0.801)	
Unknown	—		—		0.444 (0.398)	
Weapon used	0.760* (0.329)	2.138	0.661* (0.332)	1.936	0.301 (0.396)	

(continued)

**Table 2 (continued)**

Variables	Model 1		Model 2		Model 3	
	<i>B</i>	Exp( <i>b</i> )	<i>B</i>	Exp( <i>b</i> )	<i>B</i>	Exp( <i>b</i> )
Stake in conformity	-0.125 (0.815)		-0.273 (1.285)		0.600 (1.071)	
Incident to intake (days)	—		-0.004 (0.002)		—	
Incident to closing (days)	—		—		-0.005* (0.002)	0.995
Charges filed	—		-0.282 (0.415)		—	
Charges Filed × Stake	—		0.441 (1.211)		—	
<i>n</i> original charges	—		—		-1.185*** (0.368)	3.272
Full prosecution	—		—		0.442 (0.537)	
Prosecution × Support	—		—		-0.817 (0.848)	
Constant	-3.611*** (0.833)	0.027	-3.250 (0.881)		-4.972*** (1.218)	0.007

  

	Model 1	Model 2	Model 3
<i>n</i>	621	620	471
$\chi^2$	71.020	63.284	59.339
-2 log likelihood	449.978	424.879	322.355
<i>p</i>	< .000	< .000	< .000

Note: Full prosecution refers to the prosecutor’s decision to seek a conviction rather than dismiss for insufficient evidence. In only 4 of 517 cases fully prosecuted did this decision not result in a conviction. These cases were acquitted at trial and have been excluded from the analysis. This exclusion has not affected the significance levels of any variables in the table.

- a. Reference category = White.
  - b. Reference category = suspect unemployed.
  - c. Reference category = victim does not want arrest.
- \**p* < .05. \*\**p* < .01. \*\*\**p* < .001.

the odds of reoffending. One new measure of offense severity—the number of charges in the information—also attains high significance. The odds of recidivism for offenders with two or more charges are 3 times greater than those for offenders with only one charge. An increase in the number of days elapsing between the incident and the case closing date is associated with a reduced likelihood of recidivism after the closing date.

A number of variables of theoretical interest do not attain significance in Model 3. Neither the prosecutor’s decision to pursue full prosecution (vs. dismissal) nor the

interaction term combining this variable with victim support for prosecution is influential. The perpetrator's stake in conformity has no impact. (Unlike the decision to file charges, I was unable to test the interaction between a stake in conformity and full prosecution owing to insufficient cell size.)

Table 3 identifies those variables predicting recidivism for the population of suspects who were convicted and adds two additional variables—length of jail term and assignment to a treatment program—to measure the possible impact of these institutional responses. An interaction term is also included to measure whether the deterrent effect of the length of a jail term is conditional on the presence of a protective order.

Consistent with previous findings, the existence of a prior arrest record, the number of charges in the filing document, and the number of days between the incident and the closing date all attain significance. Hispanic offenders are less likely than Whites to reoffend during this postconviction period. Neither the length of a jail term nor placement on a treatment program has an observable effect, and longer jail terms do not interact with a protection order to produce lower rates of recidivism.

To explore more fully the possible policy implications of these data, I conclude with a closer analysis of those 133 offenders who recidivated within 18 months of the precipitating incident. Within this group, 26 offenders (19.5%) recidivated prior to the closing date, and Table 4 indicates the variables associated with this outcome.

The logistic regression analysis indicates that the odds of recidivism before final case disposition are dramatically greater among those recidivists who had a protective order against them at the time of the arrest compared with those who did not. In fact, within this group of repeat offenders, 45.5% will reoffend prior to the case closing date. African Americans who recidivate are less likely than Whites to do so prior to the closing date, and, finally, the number of days before closing is associated, in a positive direction, with preclosing recidivism.

## Discussion

The data presented here suggest a number of conclusions. Overall, most of the extralegal variables—cohabitation, suspect substance use, employment status, gender, marital relationship, and suspect age—appear unrelated to recidivism when controlling for legal variables. Nor do the data lend support to the finding by Hirschel and Hutchison (2003) that victim support for arrest predicts revictimization. And I cannot confirm Ford and Regoli's (1992) finding that victim support for prosecution, when accompanied by prosecutorial pursuit of conviction, is associated with a reduced rate of recidivism.

The exception to this pattern among the extralegal variables is the offender's race/ethnicity. Although this variable is unrelated to the overall likelihood of reoffending, African Americans are more likely than Whites to recidivate within 18 months of the prosecutor's filing decision, and Hispanic offenders are less likely to reoffend in

**Table 3**  
**Logistic Regression Analysis of Rearrest 18 Months After Conviction**

Variables	<i>B</i>	Exp ( <i>b</i> )
Any prior arrest	1.742** (0.579)	5.710
Cohabitation	0.366 (0.388)	
<i>n</i> charges filed	1.050** (0.402)	2.858
Protective order	-0.066 (0.621)	
Suspect race/ethnicity <sup>a</sup>		
Asian	0.868 (0.735)	
Black	0.145 (0.355)	
Hispanic	-1.431* (0.609)	0.239
Other	-0.166 (1.071)	
Suspect employment		
Yes <sup>b</sup>	-0.469 (0.547)	
Unknown <sup>b</sup>	0.367 (0.439)	
Suspect gender (male)	-0.002 (0.702)	
Suspect age	-0.021 (0.019)	
Suspect substance use	-0.346 (0.385)	
Spouse (yes-no)	-0.824 (0.912)	
Victim wants prosecution		
Yes	0.951 (0.490)	
Unknown	0.588 (0.450)	
Weapon used	0.346 (0.460)	
Stake in conformity <sup>c</sup>	0.841 (1.133)	
Days (incident to closing)	-0.005* (0.002)	0.995
Jail days	-0.009 (0.006)	
Treatment program	0.338 (0.339)	

(continued)

**Table 3 (continued)**

Variables	<i>B</i>	Exp ( <i>b</i> )
Jail × Protective Order	0.024 (0.020)	
Constant	-4.169** (1.357)	0.015
<i>n</i>	364	
$\chi^2$	52.845	
-2 log likelihood	263.090	
<i>p</i>	< .001	

Note: Values in parentheses are standard errors.

a. Reference category = White.

b. Reference category = suspect unemployed.

c. Suspect Employed × Spouse.

\**p* < .05. \*\**p* < .01. \*\*\**p* < .001.

**Table 4**  
**Logistic Regression Analysis of Recidivism Prior to Case Closing Date Among Recidivists**

Variables	<i>B</i>	<i>SE</i>	Exp ( <i>b</i> )
Any prior arrest	-2.189	1.414	
Cohabitation	1.710	1.016	
Incident to closing (days)	0.020***	0.006	1.020
Protective order	2.866**	0.994	17.566
Spouse	0.655	0.958	
Suspect employment			
Yes	0.195	1.229	
Unknown	-0.304	1.107	
Suspect age	0.021	0.049	
Suspect gender (male)	-1.383	1.339	
Suspect race/ethnicity <sup>a</sup>			
Black	-1.715*	0.852	0.180
Hispanic	0.171	1.207	
Suspect substance use	-0.398	1.112	
Victim wants arrest	1.191	1.238	
Weapon used	-1.716	1.196	
Constant	-2.061	2.691	
<i>N</i>	103		
$\chi^2$	48.462		
-2 log likelihood	55.720		
<i>p</i>	.000		

a. Reference category = White. Owing to the small number of cases, "suspect Asian" (*n* = 2), "suspect other" (*n* = 4), and "victim wants arrest-unknown" (*n* = 7) were omitted from the analysis.

\**p* < .05. \*\**p* < .01. \*\*\**p* < .001.

the 18 months following conviction. Moreover, among recidivists, African Americans are less likely to be rearrested prior to case closing. It is beyond the scope of this article to address these differences, but I note that they could reflect a variety of factors, including variations in victim reporting behavior and/or differential law enforcement response. It is also possible that the various racial/ethnic groups experience differential rates of pretrial release, which could also affect the timing of recidivism. Further investigation of these relationships is an important area of inquiry for future research.

In this jurisdiction, the efforts of the criminal court system seem to have little effect on the problem of IPV recidivism. I was unable to find support for the finding of Ford and Regoli (1992) and Woolredge and Thistlethwaite (2002) that prosecutorial filing of charges (regardless of subsequent actions) has a deterrent effect. Similarly, the subsequent decision to dismiss for insufficient evidence or pursue full prosecution also seems unrelated to recidivism. Moreover, jail terms and treatment program participation do not seem to produce the intended results. Nor am I able to identify an interaction effect between jail terms and prior violence (as measured by the presence of a protective order) in the suppression of recidivism.

The only system response variable associated with recidivism is the number of days to case closing. The longer this period, the lower the rate of reoffending after the closing date. This may simply reflect the decline of recidivism rates with time. In this study, for example, rates of reoffending fell from 8.4% to 4.6% to 2.3% in each of the three 6-month periods composing the 18-month follow-up.

Contrary to most of the system response variables, the variables that are consistently influential are those associated with the perpetrator, namely offense severity and prior record. The use of a weapon and the number of charges in the filing document, and the presence of an arrest record (IPV and non-IPV) and a protective order in place at the time of the incident, are consistently—and strongly—related to the likelihood of recidivism.

Offenders against whom a protective order is in place at the time of the precipitating incident are a particularly important group meriting further study. As noted, this group has a higher rate of recidivism than other offenders and will recidivate in a shorter period. In fact, recidivists with this characteristic are twice as likely as those without to reoffend (30.6% vs. 13.3%) and will do so in a shorter period (112 days vs. 221 days on average). This accounts, in part, for the observation that 45.5% of recidivists against whom a protective order had been issued will reoffend prior to the case closing date. However, they also experience a substantially longer period before case closing (134.0 vs. 69.2 days,  $p < .001$ ). This may reflect the fact that offenders with a protection order against them are less likely to be living with their victims and thus are less likely to be at the scene when law enforcement officers arrive. Consequently, these offenders are more likely to be arrested on a warrant (30.6% vs. 19.1%,  $p < .001$ ). Lapses of time associated with acquiring the warrant, locating the offender, and serving the warrant may well explain the longer period to case closing for this group.

These findings bear on an important policy issue in the criminal justice system. Schmidt and Sherman (1996) have argued that the criminal justice system response to IPV suffers from the “one shoe fits all” problem.<sup>11</sup> As an alternative strategy, Schmidt and Sherman propose that the more appropriate response is to focus on the “chronic offender.”

The data presented here suggest that not only prior arrest record but also the presence of a protective order, along with the use of a weapon and the number of filing charges, would be important components of identifying such offenders. Although punitive interventions prior to conviction would clearly pose constitutional difficulties, there seems to be no reason why criminal justice system officials could not develop strategies compatible with constitutional protections. Law enforcement could give high priority to such cases when responding to calls for assistance and arresting on warrants. Prosecutors could fast-track such cases for resolution. Victim service providers could concentrate resources on victim needs and support systems. Because these victims have sought official support in the past (as indicated by the presence of the protective order), it seems likely that prosecutors and others would not experience the frequently reported frustration of victim nonsupport for official action (Rebovich, 1996).<sup>12</sup>

## Appendix

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### Variables in the Analysis and Coding Values

#### Case processing outcomes

##### Intake

Misdemeanor filing	yes = 1, no = 0
Violation of probation filing	yes = 1, no = 0
Rejected insufficient evidence	yes = 1, no = 0

##### Postintake

Fully prosecuted	yes = 1, no = 0
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##### Independents (categorical)

Any prior arrest	yes = 1, no = 0
Cohabitation	yes = 1, no = 0
<i>n</i> of original charges	2+ = 1, 1 = 0
Protective order	yes = 1, no = 0
Stake in conformity	yes = 1, no = 0
Suspect Race/ethnicity	
Asian	yes = 1, no = 0
Black	yes = 1, no = 0
Hispanic	yes = 1, no = 0
Other	yes = 1, no = 0
White	yes = 1, no = 0
Suspect employed	yes = 1, no = 0
Suspect gender (male)	yes = 1, no = 0

Suspect substance use	yes = 1, no = 0
Spouse	yes = 1, no = 0
Treatment program	yes = 1, no = 0
Victim wants arrest	yes = 1, no = 0
Victim supports prosecution	yes = 1, no = 0
Weapon used	yes = 1, no = 0
Independents (interval)	
Incident to filing (days)	0-1,385
Incident to closing (days)	0-1,724
Jail days	0-365
Suspect age	18-82

### Notes

1. A recent meta-analysis of the National Institute of Justice projects suggests that abandonment of the earlier conclusion from the Minneapolis experiment may be premature (Maxwell, Garner, & Fagan, 2001).

2. It could be argued that the concept of deterrence is not appropriate when assessing the impact of treatment programs whose goal is not punishment but rehabilitation. This, however, assumes the perspective of society rather than that of the offender, who may well perceive such programs as an additional punishment. That 70% of those placed in such programs (in this jurisdiction) fail to complete strongly suggests this is the case.

3. The Sacramento Police and Sacramento Sheriff's departments track individual cases by an *x* reference number that is the personal identifying number associated with each arrestee and follows that individual through a criminal career. Without this number, it is impossible (utilizing county data) to track cases for purposes of identifying recidivism. I have no reason to believe that cases lacking this identifying datum are anything other than random coder error.

4. The Sacramento Police Department (SPD) and Sacramento County Sheriff's Department (SSD) both have policies requiring that all domestic violence arrests be forwarded to the district attorney's office for further processing. Prosecutors believe that all arrests are forwarded to them by law enforcement, and this is confirmed by the Supervisors of the domestic violence units in SSD and SPD (personal communication).

5. Rearrest for an intimate partner violence (IPV) offense refers to one or more of the following violations of the California Penal Code and where the victim was in an intimate relationship with the perpetrator: 242 (battery), 243 (noncohabiting battery), 245 (assault with deadly weapon), 273.5a (willful infliction of injury on cohabitant), 273.6 (violation of protective order), 415 (fighting, noise, offensive words), 422 (criminal threats), 459 (burglary), 591 (telegraph, telephone, cable television or electric lines), 594 (vandalism), 646.9 (stalking).

6. As previously noted, all arrests in Sacramento County are forwarded to the district attorney's office for further processing, and this data source is therefore a complete record of arrests. Coding of all IPV arrests by the district attorney's staff terminated in December 2001, thus limiting the follow-up period to 18 months for this sample selected in the first 4 months of 2000.

7. The term *full prosecution* refers to all cases that are not terminated during the postintake period. I do not analyze conviction rates as a separate variable because, with the exception of four cases that went to trial and were acquitted, all fully prosecuted cases resulted in conviction.

8. It is beyond the scope of this article to address these disproportionalities, but I note that they could reflect a variety of factors including differences in rates of offending, variations in victim reporting behavior, or differential law enforcement response.

9. California law requires that all offenders convicted of a domestic violence incident be placed in a 52-week batterer treatment program. This form of mandatory sentencing when not accompanied by provision for prosecutorial discretion is resisted by prosecutors who see their room for maneuver in negotiation as constrained. For this reason, prosecutors in Sacramento County sought to interpret the law in a manner that enabled them to recapture discretion by exploiting perceived ambiguities in its language. This resulted in substantial numbers of offenders not being required to complete a treatment program.

10. Full prosecution is contrasted with dismissal for insufficient evidence. The date of this latter decision is the closing date of all such dismissed cases. There is no comparable single decision point for seeking full prosecution, and that date is therefore indicated in this study by the closing date of those cases convicted or acquitted.

11. California law requiring all IPV offenders regardless of prior record and offense severity to complete a 52-week batterer treatment program could be regarded as an excellent example of this problem.

12. Surveys of victims who have sought protective orders report high proportions of women with favorable views of the process and who found the orders helpful (Chaudhuri & Daly, 1992; Fleury, 2002; Harrell & Smith, 1996).

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