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Final Report
Police Intervention and the Repeat of Domestic Assault
Award #2002-WG-BX-0002

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Abstract

Experimental studies of the effects of arrest on domestic violence may have missed the incidents that have the highest risk of being repeated - incidents that are not reported to the police. Our research expands upon that literature by broadening the universe of interest to include all incidents of intimate partner violence, whether the police were involved or not. Our analyses are based on data from the National Crime Victimization Survey (1992-2002). We analyze 2564 assaults committed by spouses, ex-spouses, partners and ex-partners, and determine whether they were repeated during the remaining time the victim remained in the survey. We include both misdemeanors and felonies and assaults committed by both men and women. These data are used to test the deterrent or inflammatory effects of whether: (a) the incident was reported to the police, (b) the victim or third parties called the police, (c) the police made an arrest, and (d) the victim signed a complaint. Our equations include controls for the seriousness of the offense, prior violence of the offender, and socioeconomic variables.

Our longitudinal analyses suggest that police involvement has a strong deterrent effect while the effect of arrest is small and statistically insignificant. In addition, we find no support for the hypothesis that offenders retaliate when victims (rather than third parties) call the police or when victims sign a complaint. Nor do we find evidence that the effects of reporting or arrest depend on the seriousness of the offense, a history of violence by the offender, or social-demographic characteristics. These results suggest that the best policies for reduction of repeated intimate violence will be those that encourage victims and third parties to report domestic violence to the police.

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Introduction

Twenty years ago, Lawrence Sherman and Richard Berk published experimental evidence from Minneapolis showing that arrest is an effective deterrent to domestic violence (Sherman and Berk 1984). Their research received considerable public attention and led to the institution of mandatory arrest laws in many states across the nation (Bachman and Coker 1995; Maxwell, Garner, and Fagan 2001). These laws addressed a concern that the police were showing special leniency toward men who assaulted their female partners, and that the failure to respond more aggressively to domestic assault was leading to its perpetuation (Buzawa, Austin, and Buzawa 1995; Dobash and Dobash 1979; Martin 1976; Roy 1977). A common approach of the police up to that time was to listen to the disputants' concerns, calm them down, separate them, and provide the victim with information about social services (Sherman 1992).

Some scholars have suggested that the Minneapolis study is among the most influential ever generated by social science (Fagan and Browne 1994). Its impact is striking, particularly in light of the fact that most social science research has little or no influence on criminal justice practices and public policy. However, the arrest policies that were affected by this research were relatively easy to change and the politics were apparently favorable. Mandatory arrest presumably appealed both to conservatives who wanted to get tough on violent criminals, and to advocates for women, who wanted to get tough on violent husbands. Later attempts to replicate the study yielded results that were, at best, mixed (discussed below). The mandatory arrest laws, however, remain in place,

in spite of the uncertainty in the scholarly community about whether arrest is really a deterrent.

A substantial limitation of the domestic violence experiments is that they ignored a large number of domestic violence incidents that are of significant theoretical and policy interest - those incidents that are unreported. Expanding the universe of interest to include unreported incidents allows for the study of the effects of police involvement as well as the study of arrest. Since many assaults involving intimate partners go unreported, the inclusion of these incidents has implications for a much larger number of victims who are at risk of repeated violence.

Theoretically, there are a number of reasons why reporting an incident might deter future assaults even if the police do not arrest the offender. First, the presence of police officers may redefine a private incident of family conflict as a criminal act involving outsiders. In other words, offenders may interpret their behavior as a more serious violation if the incident results in police attention. Second, some offenders may view police intervention as stigmatizing and therefore a form of punishment itself. Social disclosure alone may deter offenders who are embarrassed in front of friends and neighbors (Boker 1984; Dutton, Hart, Kennedy, and Williams 1991). Finally, offenders may be deterred if the police threaten future arrest when they intervene or if such a threat is implied.

Our research is based on a longitudinal design applied to data on intimate partners from the National Crime Victimization Survey (NCVS). These data include both misdemeanors and felonies, male and female offenders, and couples in different types of relationships. Our main interest is in the effects of reporting and arrest on the repeat of

assaults against intimate partners. We also examined whether the offender retaliates if the victim reports the incident or signs a complaint against the offender. Finally, we examine whether the effects of arrest and reporting depend upon the situational characteristics of incidents and the social-demographic characteristics of offenders.

Studies of the Deterrent Effect of Arrest

Prior research on the effects of arrest on re-offending is based largely on data from the well-known Minneapolis Domestic Violence Experiment and its replications (Berk, Campbell, Klap, and Western 1992; Dunford, Huizinga, and Elliott 1990; Hirschel, Hutchinson, Dean, Kelley, and Pesackis 1990; Pate and Hamilton 1992; Sherman 1992; Sherman and Berk 1984). These studies focused on men who committed misdemeanor assaults against their female partners. The evidence from these studies is mixed. Some researchers reported that arrest deters future assault (e.g., Sherman and Berk 1984) while other research reported an increase in the likelihood of repeat offenses after arrest under certain conditions (e.g., Sherman, Schmidt, Rogan, and Smith 1992). Maxwell, Garner, & Fagan (2001; 2002) reanalyzed the combined data from these experiments and found that whether a deterrent effect of arrest was observed depended upon the measurement of the outcome. Specifically, official records failed to reveal a deterrent effect, while a modest deterrent effect was observed when analyzing a subsample of victims for whom self-reported measures of repeat victimization were available.

The experimental nature of these studies inspires confidence in causal inferences about the effects of arrest. The studies did, however, have some important limitations. Sherman (1992) and Sherman and Berk (1984), for example, noted evidence that the

random assignment to the arrest condition in the original experiment was not strictly followed by the police officers. This problem was addressed, however, in the subsequent replication studies. Another limitation was that, for practical reasons, all of the studies limited the analysis of arrest effects to cases where the offender was present when the police arrived. Thus, the external validity of the experimental studies is problematic because only some incidents were eligible for inclusion in the study, and because the police used some discretion in applying the rules of inclusion.

Finally, because of ethical issues, the studies analyzed only misdemeanor assaults. From a policy standpoint, it may be even more important to understand whether punishment deters more serious criminal offenders because they are the most dangerous (Piliavin, Gartner, Thornton, and Matsueda 1986). Formal deterrence policies may be less effective against the most violent offenders, if these offenders are less likely to engage in long-range risk assessment (Jacobs and Wright 1999; Shover and Honaker 1992). In other words, offenders who commit the most serious types of violence may be the most difficult to deter.

Reporting Effects

We are aware of only one study that examined whether reporting a domestic assault to the police reduces the probability of a repeat offense. In that study, Langan and Innes (1986) used the National Crime Survey to study a sample of married women who had been assaulted by their husbands. They examined whether reporting the incident to the police affected the risk of subsequent assaults within an average six-month period. Their evidence suggested that reporting had a deterrent effect: only 15 percent of women who called the police were re-assaulted while 41 percent of women who did not call the

police were re-assaulted. That study has a number of limitations, however. First, the study was based on bivariate analyses, and the relationship is open to alternative interpretations (see Elliott 1989). As Langan and Innes acknowledged, there is the possibility of a systematic selection bias if women who are at a lower risk for a repeat assault are more likely to call the police. In addition, the effect of reporting could be due to the effects of arrest rather than the effects of reporting because some of the reported incidents produced an arrest. Second, many researchers believe that a substantial number of cases of domestic violence were overlooked by the early National Crime Survey due to limited follow-up questions and other inadequacies in the survey instrument (Bachman and Saltzman 1995; Bachman and Taylor 1994; Kinderman, Lynch, and Cantor 1997; Perkins, Klaus, Bastian, and Cohen 1996). The revised version of this survey, the National Crime Victimization Survey (NCVS), was designed in part to address this problem by including special probes to elicit more instances of domestic violence. Finally, the data used in the Langan and Innes study was collected from 1978 to 1982. Much has changed in the last 25 years in the public's response to domestic violence.

The Retaliation Hypothesis

Some scholars have suggested that offenders may retaliate if they believe that the victim played some role in causing them to be arrested (e.g., Sherman 1992). Offenders may be angry with victims for reporting them to the police and subsequent retaliation for the victim's actions may offset any deterrent effect of reporting and/or arrest, resulting in no overall effect. One method of disentangling these processes is to examine whether recidivism is affected by who reported the incident. The retaliation hypothesis suggests

that third party reporting discourages repeat offenses while victim reporting encourages it (or has no effect).

The retaliation hypothesis also suggests that offenders may be angry with victims for signing a complaint against them, particularly if the complaint resulted in an arrest. The relationship between victim complaint and arrest is probably reciprocal. Victims who sign a complaint may influence the police to make an arrest and police who are intent on making an arrest may urge the victim to sign a complaint. Prior research has consistently shown that the victim's preference for police action is a predictor of police arrest practices (Bell 1984; Berk and Loseke 1981; Black 1976; Buzawa, Austin, and Buzawa 1995; Felson and Ackerman 2001; Smith and Klein 1984; Worden and Pollitz 1984). In some jurisdictions, a signed complaint may be required for the police to make an arrest or for the courts to prosecute. Whatever the policy, the victim's decision to sign a complaint may lead to retaliation if offenders believe the victim is cooperating with the police. Conversely, offenders may believe that a victim who refuses to sign a complaint is acting on their behalf and may appreciate it. Thus, we hypothesize that offenses are more likely to be repeated if the victim signed a complaint against the offender.

Social-demographic Factors

Some researchers have suggested that the effects of arrest depend on socioeconomic factors that indicate an offender's "stake in conformity" (Toby 1957). According to this perspective, offenders with strong bonds to the community are likely to perceive arrest (and perhaps police intervention alone) as more costly than are offenders with weak bonds. Several of the experimental studies suggested that the effect of arrest depends on the marital and employment status of the offender (Berk, Campbell, Klap, and

Western 1992; Pate and Hamilton 1992; Sherman 1992; Sherman and Smith 1992). In those studies, arrest deterred married and employed offenders from re-offending, but increased the likelihood of re-offending for unmarried and unemployed offenders. Berk (1992, p.706) pointed out, however, that evidence supporting this interaction is not conclusive.

The experimental studies also examined main effects of the social-demographic characteristics of the offender. For example, Maxwell and colleagues(2001) found that re-offending was associated with the offender's age, race, employment status, and use of intoxicants. Their evidence suggested that offenders who were young, unemployed, and who were intoxicated during the incident, had higher prevalence or frequency rates, or both. Blacks had higher rates of recidivism than did whites, according to official reports, but lower rates according to interview data. Finally, Sherman and Smith (1992) found that race, but not offender's education, was related to repeat violence.

Prior research has focused almost exclusively on men's violence against their female partners. Given gender differences in violent crime and risk-taking, however, one might predict that reporting and arrest are more likely to deter women than men (Keane, Gillis, and Hagan 1989). On the other hand, perhaps gender has only main effects on the repeat of the offense. Research based on the Conflict Tactics Scale is unclear about whether husbands or wives are more likely to repeat their violent behaviors. Archer's (2000) meta-analysis found that wives were more likely than were husbands to engage in repeated violence. Tjaden and Thoennes (2000), on the other hand, found no gender difference in the prevalence of repeated violence, although they found that husbands had a higher incidence of violence than wives did.

The Current Study

In the current study, we use longitudinal data from the NCVS to examine whether reporting and arrest affect whether intimate partner assault is repeated. Unlike the domestic violence experiments, we include both misdemeanors and felonies so we can determine whether deterrence depends on the seriousness of the offense. We also examine whether deterrence depends on whether the offender had a prior history of violence against the victim or not, and whether the offense was a sexual assault (versus a physical assault).

Controlling for offense severity allows us to address the possibility that the most serious offenders are more likely to be reported and arrested and also more likely to re-offend. Such a pattern would produce a spurious, positive relationship between police intervention and the repeat of the offense. In addition, we control for violence by the offender against the victim *before* the incident where we measure police intervention. This measure serves essentially as a lagged variable in our longitudinal analyses, and increases confidence in causal inferences.

We test the retaliation hypotheses by determining whether recidivism is more likely if the victim, rather than a third party, reported the incident and if the victim signed a complaint. In addition, we examine whether the victim's actions are particularly likely to result in recidivism if they led to an arrest, in other words, whether there is an interaction between arrest and victim reporting or complaint.

Following the stake in conformity hypothesis, we examine statistical interactions between reporting and arrest, on the one hand, and marital status, race, and socioeconomic status, on the other. Reporting and arrest may have stronger effects on

offenders who are married, white, and of higher socioeconomic status if they have stronger bonds to their communities. Finally, we examine the main effects of the offender's social-demographic characteristics and use of alcohol or drugs. It is possible that offenders who commit violence while under the influence of alcohol or drugs are more likely to repeat their offense.

Methods

We based our analyses on NCVS data for the years 1992-2002. Because respondents are interviewed six times over the course of a three-year interview cycle, these data permit an examination of multiple assaults involving the same offender and victim through the construction of a longitudinal file. We examined characteristics of assaults that occur early in the interview cycle (the “intervention incident”) and determined whether the victim was assaulted later in the cycle by the same offender (the “repeat incident”). The sample included 2564 respondents who were victimized by their spouses, ex-spouses, or other intimate partners.

Constructing a Longitudinal File

Constructing a longitudinal person-level file of victims of intimate partner violence from NCVS data is a labor intensive and complicated task for two primary reasons. First, the public-use data sets lack a single variable that uniquely identifies respondents across survey waves. Instead, we must rely on several household- and individual-level measures that must be combined to create a unique respondent-level identifying variable. Second, we were unable to use the “concatenated” NCVS file, which is the NCVS dataset most commonly used in victimization research. Although the

concatenated file would normally facilitate most analyses by properly aggregating each of the separate yearly survey files into a single data set, this file only contains information from respondents who report victimizations. Without information from respondents who do not report victimizations, respondents who experience a single victimization cannot be distinguished from those who are no longer participating in the survey (e.g., those who dropped out, finished their scheduled interviews, or moved). It is, therefore, necessary to aggregate the full files from each year in order to obtain person-level information when no victimizations are reported.

A complication with using the full files is that the public use files, while they are stored and labeled by calendar year, actually contain data for 18-month periods. This is because (a) the NCVS uses a rotating interview design to maximize efficiency (i.e., so that interviewers do not have to conduct tens of thousands of interviews in a one week period) and (b) the interview covers events for a six month period that will extend into the prior year. The net result is that when constructing a longitudinal file by aggregating the full files, one must correct for the redundancy by deleting cases with duplicate information.

Another complication occurs for the interviews conducted during the period between 1995 and 1996, during which time the U.S. Bureau of the Census updated their sampling frame to incorporate 1990 decennial census information. This resulted in a change in the household identifying variable. The result is that one cannot rely upon the household identifier to match respondents across these years. However, other ID variables permit us to match these individuals. Still, despite our best efforts, we cannot be entirely confident that we successfully matched all cases. To rule out the possibility that

this affected our analysis, we removed all cases exposed to this problem and re-estimated our models. The results for this reduced sample were equivalent to those we present. We therefore rely upon all years of data in the analyses to follow.

Another concern is the issue of sample attrition. Ybarra and Lohr [, 2002 #4481], found that victims of domestic violence in the NCVS are more likely to drop out of the sample than are other respondents. Therefore, our estimates could be biased if attrition is associated with repeat offending *and* whether incidents are reported or offenders arrested. For several reasons this problem is minimized for the present analyses. First, victims had the opportunity to report more than a single offense even if they had no subsequent interview, because the second offense could have occurred during the same interview wave. Second, analyses not presented show that neither arrest nor reporting are associated with time of survey exposure. The coefficients are close to zero and not statistically significant. Third, analyses not presented show that neither arrest nor reporting were associated with the number of interviews conducted with the respondent. Finally, unlike other analyses involving respondent attrition problems.

Identifying Repeat Assaults

While there are no codes in the NCVS data to positively link offenders across incidents, there is information that allows us to determine whether an assault is repeated by the same offender with reasonably high accuracy. For each assault, respondents are asked whether that offender assaulted them previously. If a repeat incident is reported, and if the victim of that incident indicated that the offender assaulted them before, we coded the case as a repeat incident. For example, if victims of the repeat incident indicated that their spouse assaulted them before and the intervention incident is a

spousal assault, then the incident was repeated. We coded an assault as “not repeated” if the respondent was included among non-victims in all survey waves subsequent to the intervention incident or if the respondent indicated that the offender had not attacked them before when describing a later victimization.

Unfortunately, there is a remote possibility of misidentifying a repeat offender among the unmarried couples. Suppose that during the three-year survey period a woman was assaulted by her partner, discontinued the relationship, started a new relationship, and was then assaulted by her new partner twice. (If she were assaulted only once during this period by the new partner, then she would not indicate on the second incident that this partner had attacked her before). *If* such a pattern occurred, we would be examining the effect of arrest of the old partner on whether the new partner repeated the incident. This mismatch would introduce a minor amount of measurement error.

Also note that we are unable to determine if assaults are repeated against victims who moved out of their residence after the intervention incident.¹ Our results are therefore only generalizable to incidents involving victims who remain in their residence. However, the sample does include cases in which the violent partner left the residence and this pattern is much more common.

Some offenses involve what are called “series victimizations,” i.e., instances where multiple victimizations are coded as a single record because respondents could not distinguish among the incidents due to their similarities. When the NCVS interviewers note a series incident, the interviewers only ask respondents for information about the last incident in the series. These offenses (N = 328) were omitted from analyses, because we could not determine which incidents were reported and which were not. However, in

analyses not presented, we included the series offenses and treated each of them as a single repeated offense. Our main results were similar to those we present.

Analytical technique

Recall that for each victim of intimate partner violence, the file created for this study contains: (a) person and household information collected every six months, and (b) data on every incident of intimate domestic violence reported by each victim. This structure seems to lend itself to a multi-level analysis. A multi-level approach would view incidents as nested within each individual victim, with the number of incidents varying depending on how many incidents of intimate violence the victim reported. A multi-level approach is attractive at first glance because it takes advantage of all incident information for each victim and allows one to separate individual victim characteristics from the incident and police intervention. This approach, however, is not feasible for two reasons. First, the nature of the three-year limit on the survey necessarily means that the dependent variable for the last reported incident for all victims in the sample will be coded as a non-repeat. This would artificially inflate the number of false negatives and mask true differences in effect sizes. Second, using multiple incidents per victim involves selecting on the dependent variable, i.e., selecting those who have been victimized again (and for whom intervention has failed). Incidents involving repeat victimization would thus be oversampled.

Our approach therefore is to restrict the analysis to a single intervention incident per victim. Selecting only the first incident reported (the intervention incident) allows the greatest amount of natural variation in the dependent variable within the constraints of the three-year survey period, diminishing the risk of false negatives and removing the

problem of cases entering the analysis in a manner related to the outcome of the intervention incident. Thus, the current study utilizes only the first incident reported for all victims of intimate partner violence as the intervention incident.

Independent Variables

We measured police intervention by two dichotomous variables representing three possible outcomes: (1) the incident was not reported to the police; (2) the incident was reported and the police made an arrest; (3) the incident was reported to the police but the police did *not* make an arrest (the reference category). This allowed us to determine the effects of reporting (versus not reporting) and arrest versus no arrest (for those incidents reported). In addition, in alternative analyses, we distinguished between whether it was the victim or some third party who reported the incident.

To determine whether an incident was reported to the police, victims were asked: “Were the police informed or did they find out about this incident in any way?” To determine whether the offender was arrested, victims are asked, “as far as you know, was anyone arrested or where charges brought against anyone in connection with this incident?” If respondents answered that they did not know on these measures, we excluded the incident from our analyses.

Our measures of characteristics of the intervention incident include weapon use and injury, whether the incident was a sexual assault, and whether the offender was drinking or using drugs. Weapon use was coded as “firearm,” “other-weapon,” and “no-weapon” (the reference category). We coded four categories of injury with “no injury” as the reference category. We considered an injury “minor” if the victim did not go to an emergency room or hospital; “moderate” if the victim went to an emergency room or

hospital but did not stay overnight; and "severe" if the victim stayed overnight in the hospital. If the incident involved rape, attempted rape, or sexual assault, we coded the sex-offense variable as 1. If it involved a physical assault or a threat of physical assault, we coded this variable as 0. The offender's drinking and drug use, as perceived by the victim, was coded as three categories: offender was drinking or using drugs; do not know if offender was drinking or using drugs; offender was not drinking or using drugs (the reference category).

Unfortunately, the NCVS does not have direct information on the socioeconomic status of the offender. It does include a measure of total family income, however, which identifies joint income, at least for married couples. We used this information to construct a dichotomous measure of poverty. We preferred this measure to a continuous measure of income given past research suggesting that the relationship between income and violence is not linear (Brownfield 1986). The poverty measure is based on family income and family size following the formula for Poverty Thresholds for 2002 (U.S. Census Bureau 2003). The NCVS income codes are based on 14 categories of income in \$10,000 units; for our calculations of the poverty measure, we used the mid-point of each category. In cases where income information was missing, we imputed income based on education, employment status, job type, marital status, and age.

We also included a measure of the victim's years of education. Victim's education can be interpreted as another measure of the couple's socioeconomic status because couples tend to have similar levels of education. The respondent also identified the race of the offender, which we coded as either African-American or other. The

estimated age of the offender was coded as a series of dichotomous variables: 30 and above; 18 to 29; and under age 18 (the reference category).²

We also attempted to disentangle the effects of marital status from the effects of cohabitation. Some research suggests that married couples have lower rates of violence than do cohabiting couples (Magdol, Moffitt, Caspi, and Silva 1998; Stets and Straus 1990). On the other hand, partners living together are likely to have more opportunity to engage in violence regardless of their marital status. To disentangle these effects, we constructed a series of dichotomous variables: spouses or ex-spouses living apart; other intimate partners living apart; spouses or ex-spouses cohabiting (the reference category); and ambiguous information. The ambiguous category primarily included partners, but also some spouses, whose living arrangements could not be determined with certainty (see below) because the NVCS does not directly ask whether the respondents were living with the offender.

We relied on multiple items to construct these dichotomous variables. First, the respondents indicated their relationship to the offender (spouse, ex-spouse, or current or former boyfriend or girlfriend). Second, the head of household (not necessarily the victim) was asked to indicate who lived in their household. If the victim was the head of household and mentioned that the spouse lived there, and they were assaulted by their spouse, the offender was coded as a cohabiting spouse. If the spouse did not live there, or if the offender was an ex-spouse, the offender was coded as spouse or ex-spouse living apart. Determining whether the respondent was cohabiting with an assaultive non-marital partner is more difficult, because the NCVS does not code non-marital partners in their list of household members, but instead, would classify these persons as non-relatives.

When the respondent replied that a boy- or girl-friend was the offender, we subsequently relied on the respondent's response to another question asking them whether the offender lived at the residence or had a right to be there. If the respondent said no, then we coded the offender as a partner living apart. If the respondent said yes, we coded it as ambiguous, because we cannot determine whether the partner was living there or was just a visitor (and thus had a right to be there). In addition, in some cases, the respondent was not the head of household and was not married to the head of the household, but indicated that their spouse was the offender. We also included these cases in our ambiguous category because we could not determine whether the spouse who committed the assault was living with the victim.

Victims who remained in the survey for longer periods after the intervention incident are more likely to be assaulted during the survey. We controlled for time-in-survey differences by determining the number of months the respondents remained in the survey after the intervention incident. For this measure, we calculated the time between the month and year of the intervention incident and the month and year of the last survey contact. Thus, if the intervention incident occurred during March of 2000, and the last survey contact was in May of 2000, we coded two months of exposure.

Results

In Table 1, we present descriptive statistics. The results show that approximately 17% of incidents are repeated within the period of the survey. More than half of the intervention incidents were reported to the police, primarily by victims. Approximately 40% of the reported incidents resulted in an arrest (22% of all incidents). Victims signed complaints in approximately half of the reported incidents. Almost 60% of the victims

had been assaulted by the partner at some point in the past (prior to the survey).

Relatively few (8.7%) of the couples were cohabiting spouses when the victim reported the intervention incident. Presumably, many separated after the assault.

< Tables 1 and 2 About Here >

In Table 2, we present the main additive model, based on logistic regression. To determine whether the model adequately fit the distributional properties of the data, we performed a Hosmer and Lemeshow test (2000). This test indicated a reasonably good fit (Chi-square = 10.3, df = 8, p = .24). The coefficients in Table 2 reveal strong evidence of a deterrent effect for reporting. (Recall that the sign is reversed because reporting without arrest is the reference category). The results show that offenders are much less likely to repeat their offense when an incident is reported to the police. Not reporting increases the odds of a repeated offense by 89% ($e^{.64} - 1 = .89$). However, our results do not indicate evidence of a deterrent effect for arrest. The coefficient is in the predicted direction, but it is small and not statistically significant. The results suggest that reporting deters offenders from committing another assault whether the police make an arrest or not.

To give a better idea of strength of reporting effects we computed the probability of a repeat offense for incidents that were reported, incidents that resulted in an arrest, and incidents that were unreported (see Table 3). The probabilities are estimated using the modal response on the other categorical variables and means for the continuous variables. We also estimated the probabilities of repeat offenses separately for those who had never been assaulted by the offender before the intervention incident. The table shows, for example, that offenders with prior offenses reoffended in 35% of the unreported incidents but only 22% of incidents that were reported with no arrest.

Offenders with no prior offenses reoffended in 10% of the unreported incidents but only 5% of incidents that were reported with no arrest.

Table 3 about here

Other variables also predict re-offending, according to the results in Table 2. First, as expected, re-offending is associated with the number of months that the victim remained in the survey. Second, those offenders who have committed an assault prior to the intervention incident are much more likely to re-offend. In other words, partners who have committed at least two offenses are more likely than partners who have committed one offense to commit another assault. Third, offenders who were drinking or using drugs at the time of the incident are more likely to re-offend.

The effect of gender (offender male) is positive but nonsignificant in Table 2, suggesting that women are just as likely to re-offend as are men. This result, however, may be affected by the inclusion of prior violence in the equation. Other analyses (not presented in tabular form) show that males are more likely to have engaged in violence before the intervention incident ($b=.68$; $p < .001$). When we re-estimated the equation omitting prior violence, we do observe a gender effect ($b=.48$; $p<.01$), while observing similar (although sometimes slightly larger) effects for the other variables.

Our sample includes some same-sex couples (48 homosexual couples and 97 lesbian couples) and it is possible that they are affecting our results. To explore this possibility, we added first a term for victim gender, and then a multiplicative term for the gender of the offender and victim. These gender effects were small and statistically nonsignificant (analyses not presented), suggesting similar results for heterosexual and homosexual couples.

Retaliation

We hypothesized that offenders might retaliate when victims reported the incident rather than third parties, when victims signed complaints, and when these actions led to arrest. To test these hypothesis, we restricted analyses to those incidents that were reported (see Table 4). The coefficient for victim-reported is not significantly significant ($b=.16, p= .43$). The result suggests that recidivism does not depend on who reports the incident to the police.

To examine whether offenders retaliate when the victim's reporting leads to their arrest, we added an interaction term: victim-reported x offender arrested. The term is not statistically significant (Table 4, upper right quadrant). The evidence suggests that victims who call the police and cause their partner to be arrested do not suffer retaliation.

To examine the hypothesis that offenders retaliate if the victim signs a complaint against them we substituted victim complaint for victim reporting (see Table 4). The results provide no support for this hypothesis. The coefficient is in the opposite direction, but not statistically significance ($b=-.35, p = .07$). To examine whether offenders are more likely to retaliate if the victim's complaint led to an arrest, we added an interaction term, victim complaint x arrest. The term is not statistically significant, suggesting no support for the hypothesis.

< Tables 4 and 5 About Here >

Other Statistical Interactions

To test the hypotheses suggesting that reporting and arrest are less likely to affect more serious offenders requires an examination of statistical interactions. To perform these analyses we added interaction terms individually to the additive equations presented

in Table 2. The equations include multiplicative terms involving either reporting or arrest and the following variables: the offense involved a sexual assault; the offense was a felony (the offender used a weapon or produced a moderate or severe injury); the offender committed an offense before the intervention incident. The coefficients for the interaction terms from these equations are presented in Table 5. None of the terms is statistically significant, suggesting that the effects of reporting and arrest on re-offending are similar for serious and less serious offenders.

We tested for statistical interactions involving socioeconomic factors in the same way. We examined interactions between reporting and arrest and the following variables: victim's education, poverty, marital status, race, and the offender's gender. We used the same methodology as we did in examining interactions involving seriousness. The results (presented in Table 5) show that the effects of reporting and arrest do not depend on the socioeconomic characteristics of the couple.

Discussion

Assaults on intimate partners are often repeated, thus providing an opportunity for intervention (Feld and Straus 1989; Quigley and Leonard 1996; Tjaden and Thoennes 2000). The question is how to intervene. Early research suggested that arrest deters men from re-assaulting their female partners, but the attempts to replicate that research were often not supportive. Our research provides an additional reason to question whether arresting offenders for domestic violence is a deterrent. We find that the coefficient representing the effect of arrest on re-offending is in the predicted direction, but it is small and not statistically significant. Thus, our results, along with at least some of the experimental research, challenge the efficacy of mandatory arrest laws.

Our research addresses a sampling limitation of the experimental research on arrest. Most of that research was based on misdemeanor assaults committed by men against their female partners. Our research suggests that the effects of arrest do not depend on the gender of the offender and victim, nor on the seriousness of the offense. Arrest is not a deterrent whether the offender has committed a misdemeanor or a felony.

We find no support for the hypothesis that arrest is a stronger deterrent for offenders with a greater stake in conformity. The effect of arrest does not depend on the socioeconomic or marital status of offenders. This finding is consistent with some, but not all of the experimental studies. It should be acknowledged, however, that our inability to measure the offender's employment status, did not allow us to rule out the possibility that arrest deters employed offenders but encourages unemployed offenders, as some of the experimental studies have found..

Like the experimentalists, we searched for evidence of an arrest effect in a variety of subsamples. We could find none. Our results and the inconsistent results reported in the experimental literature suggest that the search for identifiable subpopulations that respond differentially to arrest is unlikely to be successful.

Our study is based on a longitudinal design that includes controls for the seriousness of the offense, drinking and drug use, and prior violence by the offender. We believe that that our methodology is sound and that our controls are reasonably adequate. However, since we do not have experimental data, we must consider the possibility of some sort of a suppressor effect, i.e., that some unmeasured variable is positively related to both arrest and re-offending, and offsets the negative effect of arrest. Perhaps our dichotomous measure of prior offending does not adequately measure the offender's

violent history. Perhaps we have not adequately measured the offender's violent behavior during the event, or afterwards, when the police arrived. For example, it is possible that particularly violent offenders, who are more likely to re-offend, are more likely to display an aggressive demeanor toward the police that leads to an arrest. These effects could suppress any deterrent effects of arrest. Two findings argue against this possibility. First, the coefficient for arrest does not increase when we excluded prior violence from the equation; in fact, it decreased slightly ($b = -.25$ to $-.23$). It therefore seems unlikely that substituting a better measure of violent history would increase the estimated arrest effect. Second, we found no evidence that arrest was a deterrent for misdemeanor offenders who had never assaulted the victim previously. This subsample is less likely to include particularly violent offenders than the total sample.

Another possible confounding factor that could suppress the effects of arrest is the offender's remorse. Perhaps remorseful offenders are better able to avoid arrest and less likely to re-offend. The fact that reporting has a strong deterrent effect argues against this explanation. One would expect that remorseful offenders are better able to convince their partner not to report them than to convince the police not to arrest them. For example, it is easier for a husband to "sweet talk" his wife than it is to persuade the police to "give him another chance." In other words, remorse is more likely to offset the relationship between reporting and reoffending than arrest and reoffending.

More generally, in considering alternative explanations, it is important to point out that we examined the social response of three actors to the offender's violence: the victim, the police, and other third parties. Alternative explanations of our findings must account for why the responses of victims and third parties (i.e., calling the police) have

effects on re-offending, but not the response of police. Specifically, why is arrest not a deterrent? First, it may be that those offenders who would be deterred by an arrest are already deterred by police intervention while those who are not deterred by police intervention are not deterred by arrest. Second, arrest is only a slightly more severe punishment than intervention, because offenders are only incarcerated for a short time. The literature on criminal sanctions suggests that marginal increases in the severity of punishment are not a deterrent (e.g., Blumstein, Cohen, and Nagin 1986).

The effect of reporting on recidivism in this data is fairly strong. The result suggests that the reporting of partner violence to the police is a deterrent, even if the police do not make an arrest. It may be that a visit from the police changes the offenders' attitudes toward their behavior. Offenders may redefine their behavior as a criminal act rather than a private matter. It may also change their perception of the costs of further violence. Offenders may be deterred by the stigma associated with a visit by the police for domestic violence, or they may anticipate future arrest if they re-offend. Future research should examine how offenders view police intervention and why it deters them from re-offending. Finally, it is possible that the couples seek counseling or some other social services as a result of the police visit and that these interventions influence the offender. In other words, these factors may mediate the reporting effect.

It is also possible that the relationship between reporting and re-offending is spurious. Perhaps some of the victims do not call the police because they have committed violence themselves and perhaps couples who engage in mutual violence are more likely to have repeat incidents. Analyses not presented argue against this explanation. We introduced the following dichotomous variables into our equation: "victim was the first to

use or threaten to use physical force;” “the victim used force in resistance;” and “the victim did not use physical force” (the reference category). These variables did not significantly affect the likelihood that the offender re-offended.

We find no evidence that offenders retaliate when victims report the incident or sign a complaint against the offender. Even when these actions lead to arrest, they do not appear to lead to retaliation. Perhaps most offenders do not retaliate when victims seek police protection because they do not believe that they have been wronged. They may not think additional violence will deter their victims from seeking help because the victims have already shown they are willing to call the police or sign complaints. Of course, it is possible that some offenders retaliate when victims seek police protection while most others are deterred.

From a policy standpoint, it is unfortunately much more difficult to change citizen’s reporting behavior than arrest policy. Still, it may be possible to influence reporting decisions. It is important to consider the motivation of victims (and, to a lesser extent, third parties) for reporting or not reporting incidents to the police. Other evidence from the NCVS shows that the most common reason victims give for calling the police is the desire for protection. The most common reason by far that victims of partner violence give for *not* calling the police is that they consider it a “private matter” (Felson, Messner, Hoskin, and Deane 2002). The belief that the incident is trivial is somewhat important while the fear of reprisal and the desire to protect the offender are relatively unimportant.³

One strategy would be to communicate to the public information about the deterrent effect of reporting domestic violence. Even victims who think that the police

will not make an arrest - perhaps because they think the incident is “trivial”- should be encouraged to report. Another strategy is to allow victims to get police attention and still maintain some degree of privacy. For example, perhaps it would be useful if victims had the option of requesting a visit by plain-clothes officers to avoid the stigma associated with a visit by uniformed police officers. Note, however, that it is not clear exactly what victims mean when they mention privacy concerns. It may mean that they would be embarrassed by a visit from the police, but it could also reflect their belief that family conflicts are private matters.

In sum, it is difficult to compete with experimental research using correlational data, even when that data is longitudinal. However, the experimental studies of arrest also have their limitations and they have yielded decidedly mixed results. Our conclusion, based on the current analysis and the prior literature, is that a deterrent effect of arrest has not been demonstrated convincingly. If arrest does have an effect, that effect is likely to be too small to have policy significance. The power to deter domestic violence appears to reside more with victims and third parties who have the opportunity to get the police involved.

Footnotes

¹ Follow-up victim reports in the domestic violence field experiments had poor response rates, partially due to this problem (Sherman and Berk 1984, p.265).

² For 60 incidents there was more than one offender. In those cases, we coded the age of the oldest offender. If all of the offenders were African-Americans, race was coded as black. Otherwise, offender's race was coded as white.

³ Thus, fear of future violence is a much more important factor encouraging victims to call the police on their partner than fear of reprisal is in inhibiting their calling.

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Table 1. Descriptive Statistics

<u>Variable</u>	<u>Category</u>	<u>Percent (or Mean)</u>
Repeat Victimization	No	82.8
	Yes	17.2
Police Intervention	Unreported	43.5
	Reported, No Arrest	34.3
	Arrest	22.2
Person Reporting	Unreported	43.5
	Third Party	15.9
	Victim	40.6
Complaint Signed	Unreported	43.5
	No	28.9
	Yes	27.6
Survey Exposure	Months Since Incident	Mean = 9.3
Prior Victimization	No	40.7
	Yes	59.3
Type of crime	Sexual Assault	6.1
	Physical Assault	93.9
Weapon	None	83.3
	Firearm	4.8
	Other Weapon	11.9
Injury	None	52.0
	Minor	40.5
	Moderate	7.0
	Severe	.5
Offender Drinking or Drug Use	No	40.0
	Yes	42.1
	Uncertain	17.9
Relationship / Cohabitation	Spouses Cohabiting	8.7
	Spouses Living Apart	35.9
	Partners Living Apart	40.9
	Ambiguous	14.5
Offender Age	Under 18	4.8
	18 to 29	42.7
	Over 30	52.5
Offender Gender	Male	84.2
	Female	15.8
Offender Race	Black	18.4
	Other	81.6
Victim Education	Years	Mean = 12.6
Poverty	Under Threshold	33.0
Number of Cases		2565

Table 2. Determinants of Repeat Victimization - Basic Model ⁺

<u>Variable</u>	<u>b</u>	<u>Exp b</u>
Arrest	-.25 (.17)	0.78
Not Reported	.64 (.13) *	1.89
Survey Exposure	.03 (.01) *	1.03
Prior Victimization	1.62 (.15) *	5.08
Sexual Assault	.17 (.23)	1.18
Firearm	-.16 (.28)	.86
Other Weapon	-.16 (.19)	.85
Minor Injury	.11 (.12)	1.11
Moderate Injury	-.40 (.28)	.67
Severe Injury	.77 (.65)	2.15
Offender Alcohol or Drug Use	.58 (.13) *	1.79
Alcohol or Drug Use Unknown	.20 (.17)	1.22
Spouses Living Apart	-.25 (.20)	.78
Partners Living Apart	-.28 (.21)	.76
Relationship / Cohabitation Ambiguous	-.07 (.24)	.94
Offender Age 18 to 29	-.20 (.32)	.82
Offender Age over 30	-.17 (.32)	.84
Offender Male	.29 (.18)	1.34
Offender Black	-.18 (.16)	.84
Victim Education	.02 (.03)	1.02
Poverty	.23 (.13)	1.26
Constant	-3.93 (.47)	.02

⁺ Logistic regression coefficients with standard errors in parentheses

* p < .05

Table 3. Predicted Probabilities of Re-offending ⁺

	<u>No Prior Offense</u>	<u>Prior Offense</u>
No Report	.10	.35
Reported, No Arrest	.05	.22
Arrest	.04	.18

⁺ With other categorical variables set at their modal values and continuous variables set at their mean

Table 4. Tests of the Retaliation Hypothesis for Reported Incidents ⁺

	<u>Additive Model</u>	<u>Interaction Model</u>
<u>Victim Reporting</u>		
Victim Reported ⁺⁺	.16 (.20)	-.07 (.24)
Arrest	-.15 (.18)	-.57 (.34)
Victim Reported x Offender Arrested		.57 (.39)
<u>Victim Complaint</u>		
Victim Complaint	-.35 (.19)	-.39 (.24)
Arrest	-.01 (.19)	-.07 (.29)
Victim Complaint x Offender Arrested		.11 (.38)

⁺Logistic regression coefficients with standard errors in parentheses. The equations include the same control variables as noted in Table 2. N= 1449

⁺⁺ The reference category is “third-party reporting.”

Table 5. Multiplicative Terms from Interaction Models ⁺

<u>Variable</u>	<u>by Reporting</u> <u>b</u>	<u>by Arrest</u> <u>b</u>
Sexual Assault	-.11 (.57)	-1.13 (1.11)
Felony Offense	-.20 (.29)	.30 (.34)
Prior Offense	-.35 (.29)	.06 (.42)
Victim Education	.04 (.05)	.07 (.07)
Poverty	-.27 (.24)	-.41 (.31)
Victim/Offender Married	.24 (.19)	.23 (.29)
Offender Black	.25 (.32)	-.05 (.39)
Offender Male	-.18 (.35)	.05 (.53)

⁺Logistic regression coefficients with standard errors in parentheses. The equations include the same control variables as noted in table 2.