DOMESTIC PROTECTION ORDERS AND THE PREDICTION OF SUBSEQUENT CRIMINALITY AND VIOLENCE TOWARD PROTECTEES

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A random, comparative, archival study of select variables across a six-year period of time—three years prior to, and three years after, the issuance of a protective (restraining) order against 200 persons—was conducted. Logistic regression analyses were used to test hypotheses that certain variables would predict arrest for criminal and violently criminal acts toward the protectees. The most important finding was that most subjects were not arrested for a subsequent criminal or violent act toward protectees. Nonmutual service of a protection order, however, increased the risk of victim-related criminal arrests over no service and mutual service across all three racial groups, but was statistically significant only for Hispanics (p = .01). Nonmutual service also increased the risk of a victim-related violent criminal arrest over no service or mutual service, but race was not a significant predictor. We were able to correctly classify arrest or nonarrest with an 83.5% overall accuracy rate. Protection orders appear to be related to, and may deter criminal and violently criminal offenses toward protectees.

The fundamental purpose of a civil protection (restraining) order is not to punish past conduct, but to prevent future harm. Although legal opinion concerning protection orders is readily available (Schollenberg & Gibbons, 1992), there is less empirical evidence concerning their effectiveness in reducing violence since their proliferation twenty years ago as a response to domestic abuse (Klein, 1995).

In a thorough review of the research, the authors found 11 studies published in the past 15 years that attempted to measure the effectiveness of protection orders, usually in domestic-violence cases. These studies were usually nonrandom samples of convenience in which victims of abuse were either interviewed or surveyed, and asked a series of questions concerning their perceptions of the protection order and its usefulness in preventing future harm through restraint of the defendant. In six studies (Chaudhuri & Daly, 1990; Committee on Criminal Courts, 1993; Finn & Colson, 1990; Grau, Fagan, & Wexler, 1984; Horton, Simonidis, & Simonidis, 1987; Kaci, 1994) the protection orders were judged to be effective; in one study (Berk, Berk, Loseke, & Rauma, 1983) the protection orders were judged to have no deterrent effect, and in four studies Fiedler, Briar, & Pierce, 1984; Harrell, Smith, & Newmark, 1993; Kaci, 1992; Sherman & Berk, 1984) the protection orders were perceived to have mixed results. Most researchers agreed that

the severity of violence of the defendant and the laxity of enforcement of the protection order were likely to reduce its effectiveness as a deterrent.

This study moved beyond the extant research in two ways. First, it included a large, random sample of individuals against whom protection orders had been issued and/or served over a lengthy period of time (three years), and was not limited to battered women. Second, more sophisticated research methodology and statistics were utilized, namely logistic regression analyses, to see which variables, if any, predict the violation of a protection order. Safety of the protectee is most jeopardized when the defendant engages in criminal or violently criminal behavior; therefore, we focused on subsequent victim-related arrests as our measure of protection-order violations.

Legal Understanding

Protection or restraining orders authorize courts to enjoin, or prohibit, parties from contacting, molesting, attacking, striking, threatening, sexually assaulting, battering, telephoning, harassing, or disturbing the peace of another party or parties, their family, and/or household members. Courts can enjoin knowing and willful conduct that seriously alarms, annoys, or harasses without legitimate purpose when it causes substantial emotional distress. Courts use protection or restraining orders to direct parties to do such things as stay 100 yards away from the protected person's place of residence or work or place of worship, or to not contact in person or by telephone the protected person except to arrange visitation with minor children or for the return of personal property.

Such orders are issued when, to the satisfaction of the court, based on evidence contained in declarations or affidavits, there is reasonable proof of the fact of the occurrence of an offending incident or a course of conduct, and that harm would result if the same or similar conduct is repeated or allowed to continue. One of the intended results of such orders, at a minimum, is the protection of one party from the violent conduct of the opposing party (Chaudhuri & Daly, 1990; Finn & Colson, 1990).

Methods

Research Design

The research design was a random, comparative, archival study of select variables across a

six-year period of time—three years prior to, and three years after, the issuance of a protection order against 200 persons (defendants). The hypotheses of the study were (1) Certain variables will predict violation of a protection order, and (2) Certain variables will predict a violent violation of a protection order.

Subject Selection

The San Diego County Marshal's office generated a list of 503 names of individuals who were defendants in domestic or civil protection-order cases from June 18, 1990 through August 20, 1990. Eight cases from the pool of 503 were eliminated in which the protection order lasted less than three years, leaving a subject pool of 495 cases.

Two hundred subjects were then randomly selected from the pool of 495 defendants using a random number table. One hundred and one names were selected from the subjects where protection orders were issued and served on the same date ($N = 274$), and 99 names were selected from the subjects where protection orders were issued but served on a later date, or never served ($N = 221$). These 200 subjects comprised the study sample and consisted of 144 males (72%) and 56 females (28%). One hundred fourteen were Caucasian (57%), 45 were black (22.5%), 35 were Hispanic (17.5%), and 6 were other/unknown (3%). Average age of the subjects was 38 ($SD = 9.8$), with an age range of 15–70 years. Mental health contact was also determined for each subject. This was done by searching the San Diego County Mental Health Services records to determine if a subject had ever had at least one contact with a public mental health provider. Twenty-two subjects (11%) had at least one contact, 178 (89%) did not. Socioeconomic status (SES) was not an available demographic variable due to the unreliable or absent nature of the data.

Independent Variables

The independent variables consisted of the demographic information noted above and the following variables: (1) whether the protection order was mutual (issued at the same time to both parties); (2) the order type (domestic or civil; the former requires that the parties lived as cohabitants for a period of time prior to the issuance of the order); (3) the criminal arrest records of the defendant three years prior to issuance of the order. Criminal record was determined by searching

the San Diego County database for criminal arrests, the California Information Index (CII) records, the California Law Enforcement Telecommunications System (CLETS) records, and the National Crime Information Center (NCIC) records; (4) Violence or nonviolence of the prior criminal record. This latter variable was determined by listing all the crimes for all the subjects and submitting them to an expert panel of three judges and two forensic psychologists to determine whether they were violent or nonviolent. A criminal arrest was determined to be violent or nonviolent by a majority decision of the panel, rendered independently by each expert. Examples of violent crime included kidnapping, carrying a loaded firearm in public, and battery against a police officer. Examples of nonviolent crime included forgery, petty theft, and possession of a controlled substance; and (5) Whether the criminal arrest involved drugs/alcohol. This was determined by the nature of the offense charged.

Dependent Variables

Dependent variables consisted of: (1) All criminal arrests of the subjects during the three years following the issuance of the protection order. Determination of criminal arrests followed the same procedure noted above; (2) Whether or not the crime was related to the protection order and the victim was the protection order plaintiff, or petitioner; (3) Whether or not the criminal arrest was violent, determined by the same procedure noted above; and (4) Whether or not the criminal arrest involved drugs/alcohol, determined by the nature of the offense charged. (If there was more than one criminal arrest per subject, the arrests were collapsed to create a dichotomous variable, arrest or no arrest.)

Statistical Measures Utilized

In addition to the descriptive results of the study, we used logistic regression analyses to determine the best model for predicting violent or nonviolent, victim-related arrest following issuance of a protection order. We entered the independent variables noted above in a forward, stepwise procedure, and looked for both main effects and the interaction effects of protection order issuance with other independent variables. Goodness of fit was assessed by likelihood ratio chi-square (χ^2) values. Significance for variable entry was set at $p = .05$. A logistic regression is a statistical procedure that allows us to assess the individual

effects, if any, of the independent variables on the probability that a person will be subsequently arrested for violent or nonviolent crimes, and/or will violate the restraining order issued against him or her.

Results

Subject Demographics

To determine the representativeness of the study sample, general demographic data were collected on the population of individuals in San Diego County, and compared to the subject pool demographics. The source of the San Diego County data was the U.S. Census, and represented the population as of April 1, 1990. The sample is clearly overrepresented by men (72%).

The sample is also overrepresented with respect to blacks, and underrepresented with respect to Caucasian and those classified as "other." San Diego County census data indicated that as of April 1, 1990, the median age of individuals in the county of San Diego was 30.9 years. The median age within the sample was 36, indicating that the sample was older than the population at large.

Protection Order (PO) Data

Two types of POs were issued: mutual and nonmutual. Mutual POs ($N = 71$, 36%) were issued to both parties, while nonmutual POs ($N = 129$, 64%) were issued only to the offending party. Not all POs issued were then served. Some POs were immediately served ($N = 101$, 51%), some were served at a later date ($N = 59$, 29%), and some were never served at all ($N = 40$, 20%). POs that are issued mutually are more likely to be served immediately than are POs that are issued nonmutually ($\chi^2 = 14.2$, $p = .0002$).

Seventy-eight percent ($N = 156$) of the protection orders were domestic and 22% were civil ($N = 44$). A domestic protection order requires that the parties were cohabitants for an unspecified length of time at some time prior to the issuance of the order. Cohabitation does not imply an intimate relationship, and could include a family member or nonrelative. Domestic protection orders are usually issued by California courts when there is evidence of prior physical abuse of the protectee.

Arrest Data

Arrest records prior to issuance of the POs and postissuance of the POs were inspected. In addi-

tion, arrests that were considered violent were also inspected pre- and postissuance of the POs. The frequency of individuals arrested pre- and postissuance (N = 86 vs. 84) was virtually the same. The differences in proportions of nonviolent and violent arrests from pre- to post–PO issuance approached significance (χ^2 = 3.80, p = .07).

Of the 84 postissuance-arrested subjects, 36 committed victim-related crimes. The timing of postissuance arrests was of interest to the researchers. In other words, how soon after the issuance of the PO did the arrest occur? Figure 1 shows the time intervals in which the victim-related arrests occurred.

As can be seen in Figure 1, 12 (33%) of the 36 subjects were arrested in the first 60 days. Twenty-one (58%) were arrested in the first six months.

Hypothesis One

The first research hypothesis looked for variables that would predict the occurrence of a victim-related arrest. That is, could we predict if an individual would be arrested for a victim-related offense after the issuance of a PO?

In order to address this research question, a logistic regression approach was used. The dependent variable was presence or absence of a victim-related arrest after issuance of the PO. Independent variables included race, gender, prior history of drug or alcohol arrests, prior history of any arrests, and prior mental health history. A PO variable was constructed such that there were three levels. Level 1 indicated that a PO was issued but never served. Level 2 indicated that a

nonmutual PO was served to the defendant only. Level 3 indicated that a mutual PO was served to both the defendant and the victim. Specific hypotheses included that service of the PO would alter the probability of postissue, victim-related arrests. Furthermore, it was hypothesized that demographic variables, including mental health contact, would moderate this effect.

The goodness of fit for the best fit model was χ^2 (191, N = 200) = 188.84, p = .53. The best fit model included the interaction effect of race and PO service, and the interaction effect of mental health history and PO service. Table 1 presents the significant parameters from the analysis.

The first column in Table 1 lists the predictor interaction. The second column is the logistic regression coefficient for that predictor interaction. The third and fourth columns show the standard error and significance of the coefficient, respectively. The column labeled Exp(B) gives the factor by which the odds change for the occurrence of a victim-related arrest, given the presence of the interaction. For example, when a nonmutual PO is served to a Hispanic restrainee, the odds of a victim-related arrest increase by a factor of 8.37 (over the odds of a victim-related arrest occurring with no service of a PO with the average probability of arrest across all demographic variables).

Classification Results Using the Logistic Regression Equation

To further assess the goodness of fit for the logistic regression equation, a classification analysis was carried out. There was an observed 18% base rate of postissuance, victim-related arrests.
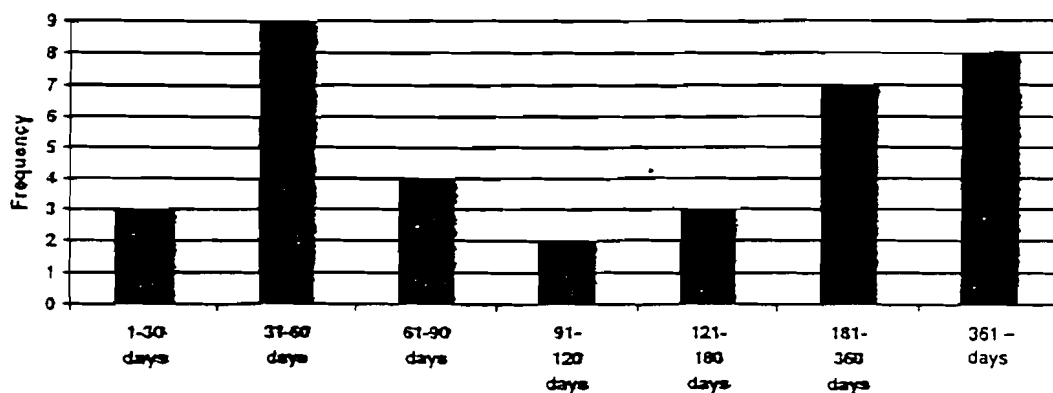

Figure 1. Number of Days Following Issuance of PO That Victim-Related Arrest Occurs (n = 36)

TABLE 1. Logistic Regression Model for the Likelihood of Being Arrested for Victim-Related
Offenses Following Issuance of a Protection Order

Predictor	Coefficient	SE	Significance	Exp(B)
Mental Health Contact X Service			.03	
Mental Health Contact X Mutual Issuance/Service	3.82	1.80	.03	45.86
Mental Health Contact X Nonmutual Issuance/Service	7.03	2.73	.01	1126.39
Race X Service			.002	
Caucasian X Nonmutual Issuance/Service	.54	.76	.48	1.71
Caucasian X Mutual Issuance/Service	−.56	.89	.53	.57
Hispanic X Nonmutual Issuance/Service	2.12	.99	.03	8.37
Hispanic X Mutual Issuance/Service	−2.50	1.92	.19	.08
Black X Nonmutual Issuance/Service	.92	1.15	.42	2.52
Black X Mutual Issuance/Service	−.82	1.55	.60	.44
Constant	−2.05	.28	.0001	

By just predicting that no postissuance arrests would occur, one would be accurate 82% of the time. By using the logistic regression equation derived above for predictive purposes, however, overall predictive accuracy increased only 1.5% (from 82% to 83.5%). Accuracy in predicting subsequent arrests, however, increased from 0% (using the base rate of 82% no arrests; by choosing no arrests, predicted arrests are 0% of the time) to 36.1%. At the expense of making Type I errors (predicting an arrest that does not occur), Type II errors have been decreased (predicting that no arrest will occur when it does in fact occur). As the authors believe that Type II errors (false negatives) are more serious in this situation, the improvement is substantial.

Univariate Inspection of Interaction Effects

To further illustrate the effects of the interactions of mental health contact, type of PO service, and race, on subsequent victim-related arrests, univariate chi-square analyses were carried out for each mental health contact status, race and PO type. Tables 2 through 6 present the results of these analyses.

Hypothesis Two

The second research hypothesis looked for variables that would predict a *violent*, victim-related arrest following issuance of a protection order. We addressed this research question by using another logistic regression. The dependent variable was presence or absence of a violent, victim-related arrest after issuance of the PO. As in hypothesis one, independent variables included race, gender, prior history of drug or alcohol arrests, prior history of any arrests, and prior mental health contact. The same three-level PO variable was used that reflected both service and mutuality of the PO. Specific hypotheses predicted that service of the PO would alter the probability of post-issue, violent, victim-related arrests. Furthermore, it was hypothesized that demographic variables, including mental health contact, would moderate this effect.

The goodness of fit for the best fit model was χ^2 (194, $N = 200$) = 223.5, $p = .07$; the best fit model included the interaction effect of mental health contact and PO service, the main effect of prior history of drug or alcohol arrests, and the

TABLE 2. Frequency of Arrests by PO Service for Those with No Mental Health Contact ($N = 178$)

PO Type	No Postissue Arrest	Postissue Arrest
Nonmutual Issue/Service N(%)	56 (73)	21 (27)
Mutual Issue/Service N(%)	58 (94)	4 (6)
No Service N(%)	35 (90)	4 (10)

χ^2 (2, $N = 178$) = 12.25, $p = .002$.

TABLE 3. Frequency of Arrests by PO Service for Those with Mental Health Contact ($N = 22$)

PO Type	No Postissue Arrest	Postissue Arrest
Nonmutual Issue/Service N(%)	13 (72)	5 (28)
Mutual Issue/Service N(%)	1 (33)	2 (67)
No Service N(%)	1 (100)	0 (0)

χ^2 (2, $N = 22$) = 2.28, $p = $ n.s.

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TABLE 4. Frequency of Arrests by PO Service for Blacks
(N = 45)

PO Type	No Postissue Arrest	Postissue Arrest
Nonmutual Issue/Service N(%)	17 (77)	5 (23)
Mutual Issue/Service N(%)	16 (94)	1 (6)
No Service N(%)	6 (100)	0 (0)

χ^2 (2, N = 45) = 3.42, p = n.s.

main effect of PO service. Table 7 presents the significant parameters from the analysis. As can be seen in Table 7, a prior history of drug or alcohol offenses increased the odds of a postissuance, violent, victim-related offense by a factor of 2.96. Furthermore, a nonmutual PO service increases these odds by a factor of 2.19, while service of a mutual PO decreases the odds by 80%. Unlike the model from hypothesis one, race was not a significant predictor of violent, victim-related arrests, either as a main effect or in interaction with other variables.

Classification Results Using the Logistic Regression Equation

To further assess the goodness of fit for the logistic regression equation, a classification analysis was carried out. There was an observed 14% base rate of postissuance, violent, victim-related arrests. By just predicting that no postissuance, violent arrests would occur, one would be accurate 86% of the time. By using the logistic regression equation derived above for predictive purposes, however, overall predictive accuracy increased only 0.5% (from 86% to 86.5%). Accuracy in predicting subsequent violent arrests increased from 0% (we are using the base rate of 86% no arrests; by choosing no arrests, we predict arrests 0% of the time) to 7.1%. We do not get the same level of predictive accuracy as was found in

the derivation of the logistic regression model in hypothesis one. These classification results, as well as the goodness of fit χ^2 indicate that this logistic regression model did not fit the data as well as the previous model. This is most likely due to the relatively fewer subjects in each examined cell.

Univariate Inspection of Effects

To further illustrate the significant effects of predictors on subsequent violent, victim-related arrests, univariate chi-square analyses were carried out for mental health contact status, drug and alcohol arrest history and PO service. Tables 8 through 11 present the results of these analyses.

Table 12 presents factual summaries for six different arrests for victim-related offenses where sufficient data existed to tell what happened.

Discussion

The overrepresentation of males in this study when compared to the population demographics from which our random sample was drawn is not surprising. Males are more aggressive than females, and have higher base rates for both criminality and violence (Wilson & Hermstein, 1985). In this study men were more likely to violate a restraining order than women (p = .04), and the likelihood increased with the number of prior arrests (p = .004). The overrepresentation of blacks in our study is also predicted given their significantly higher criminal and violent criminal arrest patterns (Federal Bureau of Investigation, 1995). We will not speculate on the reasons for this stable phenomenon in the United States, but do note that we did not control for socioeconomic status, which in recent studies has accounted for any racial differences between groups when violence is studied (Klassen & O'Connor, 1994).

An intriguing corollary with the older mean age of our sample (38 years) is the finding that

TABLE 5. Frequency of Arrests by PO Service for Caucasians (N = 114)

PO Type	No Postissue Arrest	Postissue Arrest
Nonmutual Issue/Service N(%)	39 (80)	10 (20)
Mutual Issue/Service N(%)	38 (88)	5 (12)
No Service N(%)	19 (86)	3 (14)

χ^2 (2, N = 114) = 1.42, p = n.s.

TABLE 6. Frequency of Arrests by PO Service for Hispanics (N = 35)

PO Type	No Postissue Arrest	Postissue Arrest
Nonmutual Issue/Service N(%)	9 (45)	11 (55)
Mutual Issue/Service N(%)	5 (100)	0 (0)
No Service N(%)	9 (90)	1 (10)

χ^2 (2, N = 35) = 9.04, p = .01.

Predictor	Coefficient	SE	Significance	Exp(B)
Mental Health Contact X Service			.01	
Mental Health Contact X Mutual Issuance/Service	4.88	2.06	.01	132.3
Mental Health Contact X Nonmutual Issuance/Service	9.65	3.28	.003	15549
Prior History of Drug or Alcohol Arrests	1.08	.48	.02	2.96
RO Service			.04	
Nonmutual Issuance/Service	.78	.61	.20	2.19
Mutual Issuance/Service	−1.67	1.14	.15	.19
Constant	−3.05	.48	.0001	

the mean age for obsessional followers (those who "stalk") in a recent review of the research was 35–40 years (Meloy, 1996), significantly older than a random sample of offenders with mental disorders (Meloy & Gothard, 1995), and most offenders in general (Federal Bureau of Investigation, 1995). Although our study did not focus on stalking per se, the latter criminal behavior is a pattern of unwanted pursuit that often necessitates a protection order. It is likely that an unknown proportion of our sample did "stalk" their victims (one subject was twice arrested for the crime of stalking), and we note this age convergence between independent studies focusing on different aspects of adult relationship problems that may come to the attention of the civil or criminal court system. Any "stalking" behavior within our study, moreover, would have been unlikely to be criminally charged, since the crime of stalking in California had only become law six months *after* our sample was selected (January 1, 1991). Subjects in our sample who "stalked" would also likely represent only the proportion of obsessional followers who were prior sexual intimates of the victims (Meloy, 1996).

The mutual vs. nonmutual service indicates that judges decide in each case whether one or

both parties will be restrained in their behavior. Mutual issuance is usually determined by negotiation of both parties or counterclaimed by the defendant and then ordered by the court. In the latter situation, there must be reasonable proof of the occurrence of an offending incident or a course of conduct by each party, and that harm would result if each party repeats or is allowed to continue the conduct (Topliffe, 1992). In this study, 36% of the subjects were issued mutual restraining orders (N = 71). We also note that issuance did not always lead to service, even over the course of three years. Twenty percent of the subjects were never served, and immediate service was less likely (p = .0002) if the order was nonmutual.

The relatively stable general arrest patterns of the subjects are well known to criminologists. Absent a major change in the individual or his or her environment, criminality and violent criminality are usually stable, multidetermined behaviors that are unlikely to be affected by a relatively benign, one-event factor such as the issuance and service of a restraining order.

Forty-three percent of all postissuance arrests were victim related (N = 36) and the majority (58%) occurred within the first six months that

TABLE 8. Frequency of Violent Arrests by PO Service for Those with No Mental Health Contact (N = 178)

PO Type	No Postissue Arrest	Postissue Arrest
Nonmutual Issue/Service N(%)	60 (78)	17 (22)
Mutual Issue/Service N(%)	61 (98)	1 (1.6)
No Service N(%)	35 (90)	4 (10)

χ^2 (2, N = 178) = 13.48, p = .001.

TABLE 9. Frequency of Violent Arrests by PO Service for Those with Mental Health Contact (N = 22)

PO Type	No Postissue Arrest	Postissue Arrest
Nonmutual Issue/Service N(%)	13 (72)	5 (28)
Mutual Issue/Service N(%)	1 (33)	2 (67)
No Service N(%)	1 (100)	0 (0)

χ^2 (2, N = 22) = 2.28, p = n.s.

the restraining order was in effect (see Figure 1). These findings underscore the continued target selection of the protection order victim almost half the time for those who will reoffend, and also the decreasing risk over time of a victim-related reoffense (violent or nonviolent), particularly after the first 60 days. These findings are convergent with both attachment theory, assuming that the subject was in a prior relationship with the victim, and also the decreased risk of violence over time, particularly homicide, following separation from a spouse, once the initial high-risk period immediately following estrangement has been traversed (Wilson & Daly, 1993). We point out that the risk of a victim-related arrest was three times higher in the second month following the restraining order issuance than the first month, a curious finding that needs replication since our actual numbers of arrest are small.

Our first research question was to determine what variables that we studied, if any, could predict a victim-related arrest following issuance and service of a protection order. We found that the most significant predictor was an interaction effect between race and type of service ($p = .002$). Mental health contact and type of service also significantly predicted a victim-related arrest, but not as strongly ($p = .03$). Since only 11% ($N = 22$) of our sample had a public mental health contact, further research needs to be done before drawing any useful conclusions from this latter interaction effect.

Our predictive equation for criminality toward protectees only increased overall predictive accuracy by 1.5% over the base rate for no arrests (82%). This was disappointing, but not if the Type II error rate is scrutinized. Here we were able to reduce our false negatives to 6.1%; in other words, our predictive equation allowed us to cut the risk of predicting that someone would not commit a victim-related crime, when in fact they would. Our false positive rate, however, was 63.9%.

The most surprising findings of this study are elaborated on in Tables 4–6. For every race (Black, Caucasian, and Hispanic), issuance and service of a *nonmutual* protection order increased the probability of a victim-related arrest when compared to mutual service or no service at all. This finding was particularly apparent among Hispanics where nonmutual service led to a 55% victim-related arrest rate over three years, and mutual service completely eliminated a subsequent victim-related arrest. This finding was significant ($p = .01$) for Hispanics, but it should be noted that trends were in the same direction for Caucasians and Blacks. The victim-related rearrest rate for Hispanics when there was no service was 10%.

When the interaction effect of no mental health contact and type of service is compared (Table 2), the same result is apparent. Mutual service reduces the frequency of arrests to 6%, and nonmutual service increases the frequency of arrests to 27%, a significant finding ($p = .002$). For those with a mental health contact, this pattern does not apply (Table 3); but we advise against interpretation of this finding due to our small mental health sample ($N = 22$) and no significance.

Our second research hypothesis narrowed the focus to variables that would predict a *violent*, victim-related offense subsequent to the issuance of a protection order. Although the goodness of fit for this logistic regression model was very low ($p = .07$), the strongest predictor of violent, victim-related arrest was the interaction of public mental health contact and, once again, type of service. In fact, six of the 22 subjects with a mental health contact had a violent, victim-related arrest after issuance; there were only seven total arrested subjects in this group. From another perspective, six out of the 28 violent, victim-related, arrested subjects in the entire sample were persons

TABLE 10. Frequency of Violent Arrests by PO ($N = 200$)

PO Type	No Postissue Arrest	Postissue Arrest
Nonmutual Issue/Service N(%)	74 (78)	21 (22)
Mutual Issue/Service N(%)	62 (95)	3 (5)
No Service N(%)	36 (90)	4 (10)

χ^2 (2, $N = 200$) = 10.45, $p = .005$.

TABLE 11. Frequency of Violent Arrests by History of Prior Drug or Alcohol Arrest ($N = 200$)

History Drug/Alcohol	No Postissue Arrest	Postissue Arrest
No Prior History of Drug or Alcohol Arrest N(%)	138 (90)	15 (10)
Prior History of Drug or Alcohol Arrest N(%)	34 (72)	13 (28)

χ^2 (1, $N = 200$) = 8.09, $p = .004$.

with a mental health contact (21%), an overrepresentation of subjects with a mental health contact (11%) in the entire sample.

This finding of a relationship between mental health contact and violent arrest is consistent with a growing body of research that substantiates that a psychiatric diagnosis, particularly an active psychosis, makes a small, but significant contribution to violence-risk prediction (Monahan, 1992). More important, however, were the main effects of prior drug and alcohol arrests and nonmutual service (Tables 10 & 11), which both increased the risks of a violent, victim-related offense by more than a factor of two. In other words, a nonmutual protection order more than doubled the risk of a violent arrest when compared to no service, while mutual service decreased the risk of a violent arrest by half when compared to no service (Table 10; note in the logistic regression analysis, Table 7, there was an 80% reduction when other variables were controlled). A prior history of drug or alcohol arrest increased the risk of a violent, victim-related arrest to 28% (Table 11).

Most notably, race was not a significant predictor variable when we focused on violent, victim-related arrests. This was an expectable finding and consistent with other research that has found that race does not contribute to violence-risk equations when other variables, such as socioeconomic status, are carefully controlled (Monahan & Steadman, 1994), something we could not do. Predictive accuracy of our logistic regression model for violent, victim-related arrests showed virtually no improvement over a simple prediction tied to the three-year base rate: no victim-related, violent arrests would occur 86% of the time subsequent to the issuance of a protection order.

We think we have discovered, rather serendipitously, an important factor that heretofore has remained hidden in the controversy over the effectiveness of protection orders: whether service is mutual or nonmutual. It may be that those professionals in both the mental health and criminal justice systems that have witnessed the failures of protection orders have been staring at the impact of nonmutual service. Those that are satisfied with the usefulness of protection orders, and continue to advocate for them in their respective settings, have unwittingly been privy to the effectiveness of mutual service. Why would this judicial decision have such an impact on empirical outcome, as we have demonstrated? Data are explained with theory, and we offer three different perspectives.

TABLE 12. Protection Order Violations—Factual Summaries of Six Cases

Case 1

On Friday at 0805, male subject came onto male victim's premises and was using a metal club to break personal property on the balcony of the residence, and used a crowbar to cut off electrical connections to the premises. On contact he threatened, "to kill everyone."

Case 2

On Monday at 1600, female victim returns home to find her exhusband has broken into the house. An argument ensues and he throws a plastic bottle at her, striking her head.

Case 3

On Friday at 1915, exhusband goes to the residence of the female victim, engages her when she answers the door, and tries to deliver some papers to her and to visit his children. The victim is visibly shaken by his appearance.

Case 4

On Saturday at 1630, son comes within 100 yards of parents' residence, confronts them, threatens to kill mother and stepfather, and burn down the house. He later starts a grass fire in the backyard. Subject was angry because parents reported his prior vandalism to police, resulting in his incarceration.

Case 5

On Tuesday at 1155, former girlfriend comes to former boyfriend's residence, bangs on his door for two hours, shouting angry obscenities at him.

Case 6

On Monday at 0730, boyfriend blocks victim from exiting her residence by stopping his car in front of her car and pointing a rifle at her in a menacing manner. He bumps into her car with his car, but she is able to drive away.

A Behavioral Perspective

When viewed from the perspective of respondent conditioning, the nonmutual service of a protection order functions as both an aversive stimulus, and a stimulus event that removes a positive stimulus, the victim, from the respondent. Behavioral psychology predicts that the combination of these two stimulus events will increase the aggression of the respondent. In our study, such nonmutual service substantially and significantly increased the risk of both violent and nonviolent, victim-related arrests subsequent to service.

Mutual service, however, is less of an aversive stimulus because it is mediated by cognitions, or thoughts, concerning fairness of treatment, and what we refer to as equity of consequences. The removal of a positive stimulus (the victim), however, still occurs. The combination of these two behavioral stimulus events, however, would predict less aggression toward the victim, which is what we found both in violent and nonviolent criminality (Hutchinson, Pierce, Emley, Proni, & Sauer, 1977). Mutual service appears to suppress reoffense below the base rate for no service.

From an operant perspective, mutual service is also likely to reduce intermittent positive reinforcement of the subject by the victim (e.g., her decision to meet with him or have brief contact with him while the protection order is in effect). This may be rationalized in a variety of ways that bespeak the intensity of her attachment to him, perhaps more so because he is physically abusive, what Dutton (1995) labeled "traumatic bonding." One woman said, "I'll just have coffee with him to see how he's doing . . . he's probably so lonely." This behavior by the victim will increase the likelihood of a subsequent protection order violation by the subject.

A Psychoanalytic Perspective

Nonmutual service is likely to be experienced as both shameful and humiliating by the defendant. These emotions, characterized as a public exposure of one's "badness" to others (Wurmser, 1995), are often defended against with rage, an emotion that is much more tolerable, particularly for males. This emotion may then fuel an aggressive pursuit of the victim to devalue her as an object. Such pursuit may have many motives, such as possessiveness, jealousy, or retaliation, or may conceal more subtle feelings, such as envy,

wherein the impulse is to render her worthless so that there is nothing of value to have or possess (Klein, 1975).

Pathologically narcissistic individuals are particularly vulnerable to feelings of shame, and such individuals are also more likely to view others, especially sexual intimates, as objects to be controlled and used, rather than human beings deserving of empathic regard for their own rights and feelings. Narcissistic traits are prevalent in both criminals generally (Meloy, 1988), and obsessional followers (Meloy, 1989, 1992, 1996) and spousal batterers (Dutton, 1995) in particular.

Mutual service may also gratify certain angry or retaliatory impulses, and may serve fantasies of retribution or talionic revenge (an eye for an eye). The primitive wish to hurt in kind those who have hurt oneself may be satisfied through the third-party actions of the court that therefore reduce aggressive impulses toward the victim. The court may also function in a parental transference role, and in a Solomonic fashion be perceived as fair and wise by the subject, who may have had less than adequate parenting, and continues to carry early childhood griefs and angers concerning disappointments with father and mother. Among spousal batterers, mutual service also allows for the continued rationalizing of the violence and blaming of the victim (Topliffe, 1992), both which may contain or reduce aggression.

A Social Perspective

Different racial, ethnic, and cultural groups hold different attitudes toward relationships, what anthropologists call "sexual pair bonding," and the role of the man and woman in such relationships. These attitudes may condone complete domination of the woman and the use of physical force, if necessary, to control her. Such attitudes may lead to the direct behavioral expression of less obvious, but more pathogenic beliefs that conceive of the woman as chattel, and may arise from a dominant patriarchal society (Walker, 1989).

Empirical findings across racial groups may reflect such attitudes. For example, the *machismo* of the Hispanic male may make the issuance and nonmutual service of a protection order intolerable to him, resulting in his predictable violation, particularly if he is young and has a history of prior arrests. Attitudinal differences toward rela-

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tionships, and certain social prohibitions, among Caucasians and blacks may be such that a nonmutual protection order is likely to have a less aggravating effect on the subject, resulting in a lower risk of violation. There also may be socioeconomic differences in our study that would dilute this differential effect across races, but there was insufficient data to test this hypothesis, which will need to await further research.

Limitations of This Study

This study was not an experimental design, and was based on archival data. Therefore, the authors may not be aware of confounding variables that could more parsimoniously explain our findings. For instance, maybe there is a true difference between people who are served mutual protection orders and those who are served nonmutual protection orders that better explains our findings, and the type of service is a corollary, rather than a cause of the restrainee's behavior. Those who are served mutual restraining orders may be, in fact, less prone to violate them, and less likely to transgress in the future. Judges may also recognize important differences among cases, and issue mutual or nonmutual restraining orders based on a private logic unknown to us. We also did not have a control group of subjects where a protection order was warranted but never issued. This would have allowed us to measure whether the court decision to issue a protection order, by itself, could have a suppression effect when compared to no issuance. Also, we did not control for differential enforcement (arrest) of mutual vs. nonmutual ordered defendants.

Our sample size was also relatively small, and a subsequent validation study would benefit by using a larger sample size from multiple geographical areas. This would increase the likelihood of more offenses and more mental health contacts, from which further conclusions could be drawn. We also recognize that arrest records usually underestimate actual criminal behavior. The best data gathering of offensive behaviors in future studies would be a combination of arrest records, self-reports, and collateral reports. The generalizability of our study (external validity) should also be viewed with caution, and our findings should only be applied to other groups whose demographics are similar to the population from which we drew our random sample.

The best single case predictor of one's reaction to a future protection order is likely to be his or

her past reaction to a protection order. In the absence of such history, however, our results should furnish useful information for officers of the court and mental health professionals in recommending and making such important clinical and legal decisions.

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