Evaluating the Effectiveness of a Court Sponsored Abuser Treatment Program

Huey-tsyh Chen,[1] **Carl Bersani,**[1] **Steven C. Myers,**[1] **and Robert Denton**[1]

The literature on abuser treatment programs reveals the following problems: (1) the dominance of shelter-related abuser programs, (2) little understanding of the effectiveness of such programs, and (3) low recruitment and high attrition rates in shelter-related programs. This study attempts to deal with some of these difficulties by an evaluation of a court mandated abuser treatment program. The sample used in this study includes a treatment group of 120 court-referred abusers and a control group of 101 nonreferred abusers. The research was designed to deal with problems such as differential participation in the treatment sessions and self-selection bias. Results indicated that the relationship between treatment attendance and recidivism was not linear. Only those defendants who attended 75% of the treatment sessions or more have decreased recidivism; others showed no impact. Some policy implications are also discussed.

KEY WORDS: abuser; treatment; court; evaluation.

INTRODUCTION

Since the 1970s, programs and shelters have proliferated to help the victims of spouse battering as we have become more aware of the magnitude of the problem of family violence. The extent of spousal violence has been documented in the 1975 and 1985 national surveys (Straus and Gelles, 1986; Straus *et al.*, 1980). Early research examined abused victims in safe houses and shelters. Access to these settings for research was easily obtained. Such studies broke new ground and primarily provided qualitative data rich in content. The vast majority of our knowledge about spouse battering, however,

[1]The University of Akron, Akron, Ohio 44325.

has been obtained from very small samples contained in such settings (Gelles, 1985; Gelles and Cornell, 1985). This knowledge is not necessarily generalizable to the larger group of battered women who become visible because the seriousness of their victimization necessitates a legal solution. And most importantly, what information we now possess for understanding the needs of victims does not deal with one central question: how to deal with batterers. Domestic violence must be eradicated at its source (Mott-McDonald Associates, Inc., 1981). In short, we must directly target batterers to better address this widespread violence.

It is only in the last several years that treatment programs have directed their attention to abusers. A review of the literature dealing with abuser treatment programs reveals the following conditions.

The Dominance of Shelter-Related Programs

Only a few studies have surveyed treatment programs dealing with abusers. The survey by Feazell *et al.* (1984) reports on 50 treatment programs for batterers. The average length of these programs was less than two years. Only six reported services over 3 years in duration.

One important finding of shelter-related research is the means by which batterers are referred to treatment: Abusers are influenced by their spouses and shelter workers to undergo treatment (Feazell *et al.,* 1984; Pirog-Good and Stets-Kealy, 1985a). Feazell *et al.* (1984) found that the most common source of referral was the batterer himself; the second most common referral source was the batterer's spouse. A survey by Pirog-Good and Stets-Kealy (1985a) reveals that spouses and shelter personnel are the major sources of referrals. Another important finding reported by Feazell *et al.* (1984) is, with few exceptions, the close working relationship between agencies offering treatment to batterers and women shelters. Thus, our primary source of data concerning client-batterers is from shelter-connected batterers.

Little Understanding of the Effectiveness of Programs

Efforts by researchers to evaluate a few treatment programs for batterers have not enhanced our understanding of program efficacy (Gondolf, 1987). Assessment studies of abuser programs have been found to suffer conventional methodological shortcomings (Edelson and Grusznski, 1986; Gondolf, 1984; Stacy and Shupe, 1984). Findings are especially problematic and elusive on the question of recidivism among batterers. This is because estimates have been based on judgments of program staff and self-reports. Further complications arise from low response rates and the absence of control groups.

Low Recruitment and High Attrition of the Abuser Treatment Program

Groups of batterers evaluated in previous investigations are the final product of low recruitment and high attrition (Pirog-Good and Stets-Kealy, 1985a; Roberts, 1982, 1984). In their survey of treatment programs for batterers, Feazell *et al.* (1984) reported that one third to one half of the batterers drop out after the first treatment session. Gondolf (1984), reporting on the Second Step program in Pittsburgh, indicated that over five times as many men inquire about the program than actually enroll in it.

The above-mentioned pattern of results cannot be generalized to the larger number of criminally convicted batterers. Therefore, conclusions drawn from voluntary treatment-seeking batterers may not apply to the larger group of criminally convicted batterers. It is not particularly difficult to understand why a larger portion of batterers in a community are not involved in a treatment program: there may not be compelling reasons to change their behavior.

In a study of predictors of treatment acceptance, Hamberger and Hastings (1986) reported results markedly similar to other investigations. They found that few batterers chose treatment even when it was free. Further, of those who initially volunteered, about one-third failed to follow through on the initial commitment to treatment. Among their strategies for motivating batterers to accept treatment, Hamberger and Hastings identified court orders (i.e., part of sentencing) as a powerful external motivator.

In the final analysis, the legal ability to punish lies with the courts. With this type of leverage, it is possible to influence motivation and force batterers to recognize the criminal nature of their violent acts. To the extent a court institutionalizes treatment as part of the sentence, a court sponsored treatment agency may routinely have access to convicted batterers as clients.

An increasing number of communities are moving toward a police-prosecutor pro-arrest policy (Goolkasian, 1986a,b). Further, more judges are giving special attention to domestic violence cases (Goolkasian, 1986b) resulting in the rise of court sponsored-required treatment programs for convicted batterers. These are, however, a very recent development. And due to their embryonic nature, no formal evaluation of the effectiveness of these programs exists (just as no empirical evaluation of the effectiveness of voluntary programs is available) (Gondolf, 1987; Goolkasian, 1986a). To address this deficiency, access is needed to a court sponsored treatment program that is exclusively for convicted male batterers.

The present study attempts to formally evaluate such a court sponsored treatment program in a way that avoids some of the difficulties encountered in previous studies. In this effort, the local municipal court provided an opportunity to examine the court files of men convicted of domestic violence. All convicted offenders received sentences. For some, however, a portion of the actual sentence was set aside until the offender completed the court-

sponsored treatment program called *Time Out*. The primary goal of this study was to examine the effects of treatment on spouse batterers receiving court convictions.

TREATMENT

Time Out consisted of two phases. *Phase one* was essentially informational in character and included four, 2-hr sessions. These identified issues at the heart of family violence problems and exposed participants to key decision makers in the criminal justice system: a judge, the city prosecutor, a defense attorney, a police officer, and a social worker who deals with abused victims. The goal of this phase was to set the agenda, scope, and responsibilities of the program, and to establish group cohesiveness while identifying basic themes common to violent partners. Mechanisms of change were initially explored. The sessions revolved around three videotapes of men in violent settings and their assumptions and responses to the problem of stress. The videotapes provided a format for clients to identify basic conflicts germane to violence in family settings. Special focus was directed to three issues for work in the next phase: control, denial, and isolation.

Phase two initiated the more affective component of the program where more personal issues were explored and techniques for avoiding violent interactions implemented. This phase was structured as an interactive group and consisted of four, two-hour sessions. Building on the content in phase one, a less directive climate was established and the group was given relatively greater control. These subsequent sessions explored self-esteem and male roles/expectations, as well as the operationalization of stress reduction and avoidance techniques as a means to exit from escalating interactions. Expression of feelings was encouraged in a context where denial of violent behavior was minimized. The meaning of "in control/out control" was defined in relationship to male stereotypes and pressures. Group participation was aimed at breaking down the social isolation that clients often experienced. At the end of each series, a report was made to the referring judge or prosecutor providing a summary of the client's attendance and participation.

METHOD

The evaluation model in this study attempted to deal with several weaknesses in current evaluation studies of abuser treatment programs noted by Gondolf (1987) and Leibrich (1986): the lack of a control group, arbitrary

judgment of program effectiveness by the staff, and the failure to deal with selection bias.

This study employed the non-equivalent control group design (Campbell and Stanley, 1963). This quasi-experimental design combines treatment and control groups with pre- and post-measures of the outcome variables. The structure of this design is similar to that of a randomized experiment except that the participants are not randomly assigned to either treatment or control groups. This design minimizes many threats to internal validity (e.g., history, maturation, testing, instrumentation). Its major weakness is its inability to deal with selection bias (Campbell and Stanley, 1963). That is, it is possible that judges only refer the more manageable cases to treatment in the first place. Therefore, they are likely to show inprovement even without intervention. Selection bias modeling (see Maddala, 1983) was utilized to deal with this problem. More specifically, the evaluation model consisted of two equations: an assignment equation and an outcome evaluation equation. The assignment equation attempted to model how judges determine who should/should not be sent to the treatment program. This assignment equation creates a membership variable which indicates the likelihood that abusers are assigned to the treatment group. This new membership variable then replaces the original dichotomous treatment-control variable in the outcome equation to obtain an unbiased estimate of the treatment effect.

Subjects

The treatment group consisted of 120 male convicted batterers. They were referred to the *Time Out* program between October 1983 and June of 1985 by six different municipal court judges in a city with a population of over 200,000. The control group was created via a systematic sampling from the Crime Index (a yearly court record from the municipal court). The control group included 101 convicted male batterers and was proportionately matched to that of the convicted batterers in the treatment group for each year studied.

Characteristics of the Sample

Social and demographic characteristics, prior criminal charges, and subsequent charges for treatment and control groups were gathered from files in the offices of the municipal clerk of courts (i.e., Criminal Index, Criminal Docket, and Criminal Files). Attendance performance was obtained from the records of the *Time Out* treatment program.

Assessment

Treatment Variable

The treatment variable in program evaluation has traditionally been operationalized as a dichotomous variable, with the treatment group coded as "1" and the control group coded as "0." However, as mentioned earlier, in order to deal with selection bias, the treatment variable was replaced by an instrumental variable which indicated the probability of being assigned to the treatment group. Unfortunately, this modeling of the selection process deals only with the problem of self-selection. It does not address the problem of the treatment implementation process: differential attendance.

The participant's assignment to the treatment group does not guarantee that all the participants will attend all the sessions of the treatment (Chen and Rossi, 1983). Some participants may attend a few sections, some participants attend every section, and some are in between. To classify all participants in the treatment program uniformly as members of the treatment group would thus ignore the variation among members with regard to level of exposure to treatment. Consequently, not only is the power of the test in estimating the treatment effect reduced, but the estimation of treatment effect may also be biased.

This differential exposure to the treatment process is especially problematic in abuser treatment programs because, as previously mentioned, the literature has reported a very high attrition rate of batterers from treatment programs (e.g., Gondolf, 1987). For example, the survey by Feazell *et al.* (1984) indicated that one half of the batterers dropped out after the first session. Roberts (1984) reported that "75 percent dropped out after one or two sessions."

In the *Time Out* treatment program, attendance was mandated by the court, and was higher than staff reports of other abuser programs. Approximately 63% of the participants attended 75% of the sessions or more (Bersani *et al.,* 1988). Still, a substantial number of abusers failed to attend a major portion of sessions. Since about 37% of participants failed to attend 75% or more of the sessions, it would be misleading to ignore the variation in attendance among the abusers in the treatment group. Actually, one-third of the 37% did not attend any sessions.

To deal with the problem of differential exposure to the treatment processes, the actual number of treatment sessions attended was also used as another treatment variable in the evaluation equation.

Outcome Variable: Recidivism

It is obvious that the use of recidivism as an outcome variable in evaluating a treatment abuser program is much more appropriate than program

staffs' subjective reporting of program effectiveness (Gondolf, 1987). How to conceptualize and measure recidivism in an abuser program is less apparent.

Two measures of recidivism were used in the analysis. The first was a binary measure of whether or not the participants have been charged with subsequent domestic violence. The binary measure may be too narrow in terms of conceptualizing recidivism and the focus of the treatment. Thus, the second measure was a general indice of recidivism which takes into consideration subsequent charges. Glaser (1973) strongly criticizes this binary measure of recidivism:

> Success is too often measured as though it were an all-or-nothing matter. . . . Recidivism, for example, is measured in terms of one rearrest, reconviction, or imprisonment. . . . Any measure. . . . [which classifies] all research subjects as either success or failure is thereby limited in its sensitivity as an index of variations in the effectiveness of alternative programs and policies. (pp. 22-23)

As discussed in an earlier section, the *Time Out* program involved intensive exposure to the operation of the criminal justice system, illustrating factors related to spouse abuse, as well as intensive counseling on stress reduction and avoidance techniques. While the focus of treatment was explicitly oriented toward family violence, the investigators questioned whether it might also impact on other criminal offenses, such as other violence, property, drugs, alcohol offenses, etc.

Following the suggestion of Glaser (1964), in addition to the binary measure, this study also constructed an offense scale as a general measure of recidivism. In this general measure, recidivism was measured by a weighted scale. Specifically, both subsequent domestic violence and any kind of other subsequent violence charges were coded "2," subsequent nonviolent charges of any kind were coded "1," and no subsequent charges were coded "0." In this scale, subsequent violence charges were weighted heavier than subsequent nonviolent charges of any kind.

Other Variables

There were six judges in the municipal court who decided which abusers should go to the treatment program. Since judges may have disparate ideas or beliefs about sending abusers to *Time Out*, a judge variable was constructed (see Heiss, 1972) to capture the differential judges' preferences.

Functional Form

The relationship between attendance at treatment sessions and recidivism was initially analyzed using a linear form. However, it was suspected that the relationship between these two variables is not linear. Recent evaluation efforts reveal that the strength of treatment is crucial in assessing the impact of treatment (see Chen and Rossi, 1983; Scott and Sechrest, 1989).

The exposure to treatment has to be sufficient in order to have an impact. Attending none, one, or a few sessions rather than a substantial portion of the sessions may not be strong enough to produce positive results. Accordingly, the nonlinear relationship between sessions attended and recidivism was also assessed.

Since there is no extant theory which suggests a cutting point for the threadhold of the nonlinear model, empirical judgment was necessary in determining when the impact of treatment was sufficient to have an effect. A decision was made to use the 75% or more sessions attended as the cutting point to assure that participants experienced both treatment phases. For analysis, this cutting point ensured enough variation between either side of the treatment threadhold.

Abuser Characteristics for Treatment and Control Groups

Characteristics of the treatment and the control groups were very similar. In general, 43% were unemployed, 39% were non-white; 71% of these batterers abused a wife, and 29% abused a girlfriend. The average age was 33 years.

Our sample characteristics regarding employment status and race differed from the findings of Pirog-Good and Stets-Kealy (1985a) based on their national survey of batterers in treatment programs. They reported that 24% of program participants were unemployed and 75% or more were white. Concerning employment status and race, our proportions were consistent with the national survey by Straus *et al.* (1980), and other findings (Prescott and Letko, 1977; Steinmetz, 1978).

These discrepancies were not unexpected. Nationally, treatment programs for batterers are predominantly shelter-connected (Feagan *et al.,* 1983; Pirog-Good and Stets-Kealy, 1985a) and do not tap the full range of batterers common to the criminal justice system. Essentially, survey findings (Feazell *et al.,* 1984; Pirog-Good and Stets-Kealy, 1985b) indicate that most of the batterers were encouraged to volunteer for treatment and only a small portion were court-mandated to treatment.

Prior domestic violence charges for the treatment group and control group were similar. Approximately 10.8% in the treatment group had been charged with prior domestic violence versus 9.9% in the control group. This indicated that for the primary study characteristic, domestic violence, the comparison group and treatment group are virtually identical.

In terms of prior charges for other offenses, the treatment group was more crime-prone than the comparison group. Twenty-five percent of the treatment group had prior charges for violence of any kind (including domestic violence) compared to 21.8% of the comparison group. In terms of charges

for non-violence prior offenses, about 35% of the treatment group had such charges versus only 29% of the comparison group.

Focusing on charges subsequent to the current conviction for domestic violence indicated that the treatment group had a lower percentage of subsequent charges for domestic violence than the comparison group. About 5% of the treatment group were charged with subsequent domestic violence, while the control group had about 10% so charged. The treatment group also had a lesser percentage of subsequent charges for other crimes. Subsequent charges for any kind of violence totaled 8.3% in the treatment group; for the control group, this value was 16.8%.

The treatment group also had a slightly lower percent of subsequent charges for any nonviolence offense compared to the control group. Of the treatment group members, 18.3% had subsequent nonviolence charges, whereas 19.8% of the members of the comparison group were so charged. In general, a larger portion of members in the treatment group had a history of prior charges compared to the comparison group but a lower portion had subsequent charges.

RESULTS

As discussed previously, the evaluation model consisted of two equations: assignment and outcome. The assignment equation was analyzed by the use of a logistic model. The dependent variable in the assignment equation was a binary measure of actual group membership (treatment versus control). The independent variables in the equation were as follows:

Victim variable: A dichotomous variable which indicated whether the victim was a spouse (coded as 1) or girlfriend (coded as 0).

Prior record: A dichotomous variable indicating whether the abuser had any prior record (coded 1 if yes; 0 otherwise).

Judge variable: A continuous variable, as discussed in the last section.

Age: The actual age of the abusers.

The selection of these assignment variables was based on both an interview with a judge on his decision-making process and a statistical analysis of the data. The result of this analysis is shown in Table I.

Table I indicates that all four independent variables have statistical significance at least at the .05 level. The purpose of this equation is mainly to create a membership variable. This instrumental variable is created from the coefficients of the independent variables found in Table I and indicate the probability of being in the treatment group. [See Achen (1986) and Maddala (1983). The membership variable is the predicted probability that the binary variable, y, equals one. That is, $\Pr(y_i = 1|x) = (1 + \exp(-Z_i))^{-1}$ which

Table I. Logistic Analysis for Assignment Process

Independent variables	Mean for treatment	Mean for control[a]	Coefficients	Chi-square	p-value
Intercept	1.00	1.00	−0.44	−0.39	0.531
Victim					
(1 = spouse)	0.80	0.63	1.02[b]	8.12	0.004
(0 = girlfriend)		(2.61)[b]			
Judge variable	0.16	0.13	4.65[c]	4.42	0.035
		(2.09)[c]			
Record					
(1 = prior record)	0.57	0.39	0.76[c]	5.76	0.016
(0 = no prior rec.)		(2.41)[b]			
Age of abuser	31.9	35.0	−0.04[c]	4.89	0.027
Model Chi-square		(2.11)		21.51	0.0003

[a] t test for mean difference between treatment and control in parentheses.
[b] Significant at 0.01 level.
[c] Significant at 0.05 level.

represents the logistic cumulative distribution function with $z_i = \sum_{j=1}^{k} x_{ij} B_j$ where the B_j are the k logit coefficients on the set of independent variables, x. This prediction of membership is inserted in the outcome equation to control for the nonrandom treatment-control group selection.]

Both binary and weighted scale measures of recidivism were included in analyzing the outcome equation. The treatment had no impact on the binary measure of recidivism both in the linear and nonlinear models. This may have been due to the small number of cases charged with subsequent domestic violence. However, in the case of the recidivism scale, the impact of treatment was more informative. Subsequent discussions will concentrate on the findings using the scale variable. The assessment of the effects of the treatment in the linear model is shown on the left hand side of Table II.

The independent variables include some typical variables used in criminal justice studies, such as age and race; a variable which indicates social supports (e.g., marital status and employment); and an interacting variable for those employed and married. It is possible that social supports will reduce recidivism. Two variables were included here to measure prior criminality: the total number of prior violence offenses, and the total number of prior nonviolent offenses. It is possible that those with higher prior criminality may be more likely to be recidivists. The period after the initial sentence is received was also included in the analysis since the longer the period after sentence, the longer the chance that abusers may be charged again for domestic violence or other crimes.

As discussed in a previous section, two treatment variables were introduced in the outcome equation. First, the instrumental membership variable is the probability of being in the treatment group. As indicated earlier,

Table II. Regression Analysis for the Effect of Recidivism: Linearity Assumption Between Attendance and Recidivism

Independent variables	Mean for treatment	Mean for control[a] (t values)	Coefficients (t values) linear	Coefficients (t values) nonlinear
Recidivism	0.35	0.53 (1.92)[b]		
Intercept	1.00	1.00	−0.44 (−0.42)	−0.156 (−0.44)
Number of prior violence charges	0.35	0.29 (−0.72)	0.165[b] (2.17)	0.174[b] (2.27)
Number of prior nonviolence charges	0.94	0.62 (−1.84)	−.004 (−0.10)	0.005 (−0.11)
Length of time after sentencing (No. of days)	430	471 (1.43)	.001[c] (4.39)	.001[c] (4.43)
Employed	0.60	0.52 (−1.20)	−0.158 (−0.88)	−0.082 (−0.89)
Race (1 = white) Race (2 = nonwhite)	1.40	1.38 (−0.30)	−0.010 (−0.11)	−0.002 (−0.02)
Percentage of sessions attended	0.69	0 (−19.65)[c]	−0.002 (−1.43)	— —
Session attendance below 75%	0.24	0 (−5.65)[c]	— —	−0.168 (−1.31)
Session attendance 75 to 100%	0.63	0 (−13.15)[c]	— —	−0.223[b] (−2.12)
Marital status (1 = married, 0 = not)	0.78	0.69 (−1.08)	−0.202 (−1.17)	−0.074 (−0.52)
Age of abuser	31	35 (2.42)[c]	0.002 (0.33)	0.002 (0.01)
Instrumental membership selection variable			0.589 (1.46)	0.596 (1.22)
N	101	120	221	221
R²			0.161	0.166
F			4.04[c]	4.19[c]

[a]t test for mean difference between treatment and control in parentheses.
[b]Significant at 0.05 level.
[c]Significant at 0.01 level.

this is used to replace the traditional treatment condition variable for dealing with potential selection bias problem. Second, the treatment attendance variable is the measure of actual treatment sessions attended. The outcome equation is estimated by Ordinary Least Squares. The result of the regression analysis is shown in Table II. Table II indicates that only two variables had a substantive impact on recidivism: the total number of prior violent charges, and the period after sentence. Both were in the expected direction. The larger the number of prior violent charges, the higher the recidivism.

The longer the period after the sentence, the greater the probability of recidivism. However, the membership variable and the attendance variable had no statistically significant impact on recidivism.

The assessment of the effects of treatment in the nonlinear model using the recidivism scale is shown on the right hand side of Table II. The attendance variable is reformulated as two attendance variables: completion and noncompletion. The noncompletion variable is a binary one which compares those who fail to complete 75% of the treatment sessions with the comparison group. The completion variable is a binary variable which compares those who complete at least 75% of the treatment sessions with the comparison group. The results of the analysis, indicated as the nonlinear model as also shown in Table II, reveal that the effect of the completion variable on recidivism was statistically significant (0.05).

Abusers who attended 75% or more of the sessions were less likely to be recidivists. In contrast, abusers attending less than 75% of the sessions did not have a reduction in recidivism relative to the comparison group. The data suggest that attendance in treatment sessions must at least pass some threshold, in this case 75% of the sessions, in order to show a desirable effect.

An alternative interpretation of the findings is that the participants in the group attending 75% or more sessions are those less violent-prone and more motivated to change. Perhaps these personal characteristics rather than the treatment per se, accounted for these findings. Distinguishing between the violent-prone and less violent-prone can be examined by comparing the prior criminal record of abusers attending 75% or more sessions versus those who attended less than 75%.

The data indicate that these two groups were similar in terms of prior violence offenses. In the four years prior to the current offense, about 24% of the less than 75% group had prior charges for violent crime compared to 26% of those who attended the 75% or more sessions.

Data are not available to directly assess the degree of motivation for change held by the participants. However, analysis of available personal characteristics may be helpful. No statistical significance was found between these two groups on such characteristics as marital status, age, race, and prior nonviolence charges. An analysis of employment status did reveal a substantial difference between these two groups. Approximately 22% of those attending less than 75% of the sessions were unemployed, whereas approximately 48% of those attending 75% or more of the sessions were unemployed. One would assume that employed persons would have higher motivation to change, since they have more to lose than those who are not working. However, the proportion of unemployed persons was higher among those attending 75% or more of the sessions than those attending less than 75% of the sessions. Accordingly, being out of work does not seem to be an alternative explanation to account for the treatment results.

DISCUSSION

Despite the current interest in abuser treatment programs, the effectiveness of these programs is not well documented. This study attempted to evaluate the efficacy of a court-sponsored treatment program by avoiding the methodological shortcomings of the few previous efforts in this area. Results indicated that the relation between treatment attendance and recidivism was non-linear. Only those defendants who attended 75% of the treatment, or more, experienced decreased recidivism; others showed no impact.

The findings have important implications for policy, program planning, and subsequent research development. Further, they present specific implications for service delivery, funding, and administration.

The critical advantage in utilizing treatment interventions within the criminal justice system is to eliminate the randomness of participation associated with voluntary programs. This research demonstrates that the criminal justice system is not left to either the mercy of offenders (who may not be strongly self-motivated), or to treatment programs that highly screen at intake and also lose a considerable proportion of their groups to attrition. That is, therapy need not be limited to a highly select population among domestic violence offenders. Courts and prosecutors, thus, possess a realistic tool with which they can address a persistent problem.

These data also have implications for funding decisions. If prevention of subsequent incidents of domestic violence is linked to an attendance variable, then programs which can have an impact on participation may secure the optimal outcome. In a climate of limited resources, funds should be directed to those programs which insure the greatest involvement.

Administratively, the findings suggest the critical need for strong judicial consistency. Commitment, communication, and rapid follow-up (with unambiguous action by prosecutors and judges) for those who refuse to attend, will likely provide motivation for compliance among participants, as well as accountability for behavior, and an increased measure of protection for victims. While flexibility need not be ruled out, it appears that the court can act as a structuring agency for men who have lost or are losing control.

REFERENCES

Achen, C. H. (1986). *The Statistical Analysis of Quasi-experiments,* University of California Press, Berkeley, Calif.

Bersani, C. A., Chen, H. T., and Denton, R. (1988). Spouse Abusers and Court Mandated Treatment. *J. Crime Justice* 11(1): 43-60.

Campbell, D. T., and Stanley, J. C. (1963). *Experimental and Quasi-experimental Designs for Research,* Rand McNally, Chicago.

Chen, H. T., and Rossi, H. T. (1983). Evaluating with sense: The theory-driven approach, *Eval. Rev.* 7(3): 283-302.

Edelson, J., and Grusznski (1986). *Treating Men Who Batter: Four Years of Outcome Data from the Domestic Abuse Project,* Unpublished manuscript from the Domestic Abuse Project, Minneapolis, Minn.

Feagan, J. A., Stewart, D. K., and Hansen, K. V. (1983). Violent men or violent husbands. In Finkelhor, D. (ed.), *The Dark Side of Families: Current Family Violence Research,* Sage, Beverly Hills.

Feazell, C. S., Mayers, R. S., and Deschner, J. (1984). Services for men who batter: Implications for programs and policies. *Fam. Relat.* 33: 217-233.

Gelles, R. J., and Cornell, C. P. (1985). *Intimate Violence in Families,* Sage, Beverly Hills.

Gelles, R. J. (1985). Family Violence. In Turner, R. H., and Short, J. F., Jr. (eds.) *Annual Review of Sociology, Vol. 11,* Annual Reviews Inc., Palo Alto, Calif.

Glaser, D. (1973). Routinizing evaluation: Getting feedback on effectiveness on crime and delinquency programs, National Institute of Mental Health, U.S. Department of Health, Education, and Welfare, Rockville, MD.

Glaser, D. (1964). *The Effectiveness of a Prison and Parole System,* The Bobbs-Merrill Company, Inc., New York.

Gondolf, A. (1984). How some men stop battering: An evaluation of a group counseling program. Paper presented at the Second National Conference for Family Violence Researchers, Durham, N.H.

Gondolf, E. W. (1987). Evaluating programs for men who batter: Problems and prospects. *J. Fam. Viol.* 2: 95-108.

Goolkasian, G. A. (1986a). *Confronting Domestic Violence: A Guide for Criminal Justice Agencies,* National Institute of Justice, Washington, D.C.

Goolkasian, G. A. (1986b). *Confronting Domestic Violence: The Role of Criminal Court Judges,* National Institute of Justice, Washington, D.C.

Hamberger, L. K., and Hastings, J. E. (1986). Characteristics of spouse abusers. *J. Interpers. Viol.* 1: 363-373.

Heiss, D. R. (1972). Employing nominal variables, induced variables, and block variables in path analysis. *Sociological Methods Res.* 1: 147-173.

Leibrich, J. (1986). Pitfalls in criminal justice evaluation research: Sampling, measurement, and design problems. *Fed. Prob.,* Sept.: 31-35.

Maddala, G. S. (1983). *The Limited-Dependent and Qualitative Variables in Econometrics,* Cambridge University Press, Cambridge, Mass.

Mott-McDonald Associates (1981). *The Report from the Conference on Intervention Programs for Men Who Batter,* Law Enforcement Assistance Administration, U.S. Government Printing Office, Washington, D.C.

Pirog-Good, M., and Stets-Kealy, J. S. (1985a). Paper presented to the American Sociological Association Meetings, Washington, D.C.

Pirog-Good, M., and Stets-Kealy, J. S. (1985b). Male batterers and battering prevention programs: A national survey. *Response* 8-12.

Prescott, S., and Letko, C. (1977). Battered women: A social psychological perspective. In *Battered Women: A Social Psycho-Sociological Study of Domestic Violence,* Van Nostrand Reinhold, New York.

Roberts, A. (1982). A national survey of services for batterers. In Roy, M. (ed.), *The Abusive Partner,* Van Nostrand Reinhold, New York.

Roberts, A. (1984). Intervention with the abuser partner. In Roberts, A. (ed.), *Battered Women and Their Families,* Springer, New York.

Scott, A. G., and Sechrest, L. (1989). Strength of theory and theory of strength. *Evaluation and Program Planning* (Forthcoming).

Steinmetz, S. K. (1978). Violence between family members. *Marr. Fam. Rev.* 1.

Stacy, W., and Shupe, A. (1984). *An Evaluation of Three Programs for Abuse Men in Texas,* Research Monograph No. 29, Center for Social Research. The University of Texas at Arlington.

Straus, M. A., and Gelles, R. J. (1986). Societal change in family violence from 1975 to 1985 as revealed by two national surveys. *J. Marr. Fam.* 48: 465-479.

Straus, M. A., Gelles, R. J., and Steinmetz, S. K. (1980). *Behind Closed Doors,* Anchor Doubleday, New York.