

**Testing a Court-Mandated Treatment Program for  
Domestic Violence Offenders:  
The Broward Experiment**

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2004  
NCJ 199729

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This project was supported under award 96–WT–NX–0008 from the National Institute of Justice, Office of Justice Programs, U.S. Department of Justice. Findings and conclusions of the research reported here are those of the authors and do not reflect the official position or policies of the U.S. Department of Justice. For a more complete discussion of the study and its findings, see the final report submitted to NIJ (<http://www.ncjrs.org/pdffiles1/nij/grants/184752.pdf>).

## **Acknowledgments**

It has been a pleasure working with two fine judges, Geoffrey Cohen and Alfred Horowitz, who supported this project because of their commitment to victims of domestic violence. The study was fortunate in being able to enlist the aid of some wonderful research assistants. Thanks to Dulce Guitierrez, Cassandra Harmon, Chelsea Marder, Deborah Rothmond, Linda Ryan, and Laurie Shultz. Appreciation is also due to Dr. Jerry Kolo and Daphne Isaacs who supported this experiment throughout its implementation. Finally, this study would not have been possible without the help of the many victims who agreed to speak with us both candidly and at length. We hope this study repays their efforts, if only in a small way.

Over the past 20 years there has been an explosive growth in policies, procedures, and programs aimed at reducing or curtailing domestic violence in the United States. With the rapid increase in proarrest policies, pressure has been placed on the courts to deal with domestic violence offenders (Feder, 1997). The result has been a rise in the use of court-mandated counseling for batterers. These programs, known as spouse abuse abatement programs (SAAPs) or batterer intervention programs (BIPs), now exist in every State (Harrell, 1991).

Soon after SAAPs first appeared, studies evaluating their effectiveness began to be conducted. The first wave of evaluation research on SAAPs indicated high rates of success in reducing the frequency and/or severity of subsequent violence (Deschner and McNeil, 1986; Neidig, Friedman, and Collins, 1985; Rosenfeld, 1992). Several researchers were quick to note that these findings may have more closely reflected the methodological shortcomings inherent in the evaluations rather than the programs' actual effectiveness in reducing violence (Chen et al., 1989; Ford and Regoli, 1993).

As more communities are called on to develop effective responses to domestic violence, jurisdictions will likely see a continued increase in the number of court-mandated treatment programs. Evaluation of these programs therefore becomes increasingly important. In addition, researchers have become increasingly aware that even the best intended programs can have unintended harmful effects (McCord, 2003; Petrosino, Turpin-Petrosino, and Finckenauer, 2000). For this reason, we must be open to the possibility that these interventions may not only be ineffective in reducing violence but may also provide a disservice to victims. To continue to mandate counseling for convicted abusers necessarily means that limited resources will be diverted from programs for battered women and their children (Tolman and Bennett, 1990). Even more problematic is the possibility that ineffective batterer treatment may be more dangerous for the victim than no treatment at all. Research indicates that the most influential predictor of an abused spouse's return to her husband is his participation in counseling (Gondolf, 1987). Yet, if treatment is essentially ineffective in decreasing recidivism, these victims may feel a false sense of security that, in the end, may lead to a higher likelihood of future injury (Harrell, 1991; Hamberger and Hastings, 1993).

## **The Intervention**

The study took place in Broward County, an area encompassing Fort Lauderdale, Florida, in the two courts exclusively charged with handling domestic violence cases. Judges in both courts, on convicting a man of misdemeanor domestic violence, placed him on probation for 1 year and assigned him to one of five local SAAPs. All programs used the Duluth Model, perhaps the most widely used SAAP in the country<sup>1</sup> (Davis and Taylor, 1999). Each of the five SAAPs provided 26 weeks of group sessions and all were county certified prior to the judges' assignment. The county's probation office was charged with monitoring an individual's progress in complying with conditions of the judge's sentence, including attendance at the SAAPs.

## **Research Design**

The study used a classical experimental design to test whether courts can effect change in men convicted of misdemeanor domestic violence by mandating them to participate in an SAAP. All

men ( $n = 447$ ) convicted of misdemeanor domestic violence in Broward County during a 5-month period in 1997 were randomly assigned to either the experimental or control group. The only exceptions were for those couples in which either defendant or victim did not speak English or Spanish; either defendant or victim was under 18 years of age; the defendant was severely mentally ill; or the judge, at the time of sentencing, allowed the defendant to move to another jurisdiction and serve his probation through mail contact. All other defendants ( $n = 404$ ) were included in the study and randomly assigned to one of the two groups. Men in the control group were sentenced to 1 year's probation. Men in the experimental group were sentenced to 1 year's probation and mandated into one of the five local SAAPs.

In an effort to determine the true amount of change in individuals undergoing court-mandated counseling, the researchers included various measures from several sources. Each batterer was interviewed at time of adjudication and again 6 months after adjudication. The victim was also interviewed at adjudication and 6 and 12 months after adjudication. Standardized measures with known reliability were used when possible. Scales included an abbreviated version of the Inventory of Beliefs About Wife Beating and Attitude Towards Women. Additionally, researchers asked whether the batterer believed that the offense should be considered criminal, whether he thought he was responsible for the offense, and how likely he was to engage in physical abuse again. The revised Conflict Tactics Scale (CTS2) (Straus et al., 1996) was also used in the defendant and victim surveys to assess the use of verbal, physical, or sexual abuse in the previous 6 months. Finally, probation records and computer checks with the local police for all new arrests were used to track the defendants for 1 year after adjudication.

The ultimate purpose of the study was to test whether court-mandated counseling reduced the likelihood of repeat violence by men convicted of misdemeanor domestic violence. However, researchers also tested the underlying theory arising from the reanalyses of the Minneapolis experiment and Spouse Assault Replication Programs (SARPs). This theory proposes that having a stake in conformity predicts when an intervention (whether an arrest or court-mandated treatment) will be effective in reducing the likelihood of subsequent violence (Berk et al., 1992; Sherman, 1992). The researchers therefore began with two hypotheses. First, men who are mandated into counseling will demonstrate a lower likelihood of repeat violence compared with men assigned to the control (no treatment) group. Second, men who have a high stake in conformity will have a lower likelihood of recidivism than those with a low stake in conformity.

## Results

### Experimental Integrity

**Random assignment.** Given the many problems inherent in running an experiment, it becomes imperative to separately address the question of the integrity of the experiment as implemented. The misassignment rate, or rate of error when an individual was placed in a group that he was not randomly assigned to, was quite low (4 percent). Additionally, a comparison of the men in the control and experimental groups on all variables that probation and the courts had access to at the time of adjudication indicates that the groups were comparable prior to the intervention. There were no significant differences between the two groups in offender demographics, stake in conformity, criminal record, and instant incident, with one exception. The average age of the control group was 2 years younger than that of the experimental group. Because research

consistently shows that younger men are more likely to abuse their partners and recidivate, the difference between these two groups would lead to a positive bias in favor of finding treatment effects. That is, it might lead to a finding of differences between the groups even if the individual intervention had no actual effect on recidivism.

**Integrity of experimental and control conditions.** The judges had the opportunity to order additional non-SAAP programs that would increase monitoring and/or supervision (and in that way compensate for what those in the control group did not receive). The researchers compared judicial orders for men in the experimental and control groups. They found no differences between groups; that is, the judges assigned evaluations, supervision, and non-SAAP programs equally to men in both groups. Similarly, probation could have increased the monitoring and supervision of the men in the control group in an effort to compensate for the fact that they were not participating in the batterers' treatment programs. Results again suggest that there were no differences in probation monitoring. Therefore, there is no reason to conclude that probation officers treated the two groups differently. An alternative possibility is that probation may not have sufficiently monitored and sanctioned failure to attend the SAAP, thereby nullifying this as a true test of the effectiveness of court-mandated counseling. However, examination of the data indicates that probation adequately monitored and sanctioned men when they failed to comply with the SAAP.

**Survey response rates.** Although a large percentage of victim nonresponse was due to problems in tracking the victims, a high percentage of defendant nonresponse was due instead to their refusal to be interviewed. The study's low response rate to a large extent reflects the charged environment in which researchers conducted the experiment. Response rates for defendants were 80 percent ( $n = 321$ ) for first surveys and 50 percent ( $n = 203$ ) for interviews 6 months after adjudication. Survey completion rates for victims were even lower, 49 percent ( $n = 199$ ) for first, 30 percent ( $n = 122$ ) for second, and 22 percent ( $n = 87$ ) for third interviews. Sample attrition analyses of defendant and victim surveys indicated equivalent response rates for individuals in the experimental and control conditions. Although such low response rates are common when working with victims of domestic violence (Hirschel and Hutchinson, 1992; Palmer, Brown, and Berrera, 1992; Steinman, 1991; Tolman and Weisz, 1995), the authors believe that the low victim response rates limited the study. To counter this limitation, the study collected information on outcomes from other sources. Specifically, official reports of all arrests during the 1-year postadjudication followup period were collected for all men in both groups.

## Outcomes

**Offender attitudes, beliefs, and self-reported behaviors.** Surveys of offenders were used to compare men in the experimental and control groups. Differences between the groups at time of adjudication (Time 1), at least 6-months postadjudication (Time 2), and changes between Time 1 and Time 2 were examined. At the time of their second interview, the experimental sample had completed an average of 22 of the 26 mandated counseling sessions, or approximately 85 percent of the intended "dosage" of batterers' intervention.

The results from the analyses indicate that men's beliefs about the legitimacy of wife beating, their sense of responsibility for these incidents, and their attitudes regarding the proper roles for women had not changed significantly for those court mandated into the BIPs compared with the no-treatment control group. Furthermore, using the revised Conflict Tactics Scale (CTS2), 30 percent of the men self-reported what Straus and colleagues (1996) would consider a minor abusive action against their partner (e.g., grabbing or slapping one's partner), and 8 percent of the men self-reported severe physical abuse (e.g., choking, beating up, or using a knife or gun on one's partner) within 6 months after adjudication. Again, the researchers found no differences between groups or within groups over time in men's self-reported likelihood to engage in any of the five subscales listed in the CTS2 (negotiation, psychological coercion, physical abuse, sexual coercion, and injury). The researchers used regression analysis to determine the effects of treatment assignment, treatment received (number of domestic violence classes attended), and stake-in-conformity variables (marital status, residential stability, employment, and age) on men's self-reported use of severe physical violence. Consistent with the results from the study's analysis of attitudes and beliefs, these results indicated that neither assignment to an SAAP nor attending the classes significantly explained any differences in individual men's likelihood to self-report engaging in further severe physical violence. Instead, stake in conformity was important in accounting for this variation. Specifically, younger men with no stable residence were significantly more likely to self-report acts of severe physical violence against their partners.

**Victim reports on partner violence.** The study found no difference between groups or within groups over time in women's reports of their partners' likelihood to engage in any of the five subscales listed in the CTS2. Fourteen percent of the women reported an act of severe physical violence occurring during the followup period. Using regression analysis to determine the effects of treatment group assigned, treatment received, and stake-in-conformity variables on the dependent variable, the researchers once again saw the primacy of stake-in-conformity variables in predicting recidivism among batterers. Specifically, the offender's age and marital status achieved statistical significance, while his employment status, although not statistically significant, demonstrated a strong relationship to the victim's reports of his use of severe physical violence. That is, women involved with younger, unemployed men who were not married to them were more likely to report one or more incidents of severe physical violence.

**Official measures—rearrests.** Twenty-four percent of men in both the experimental and control groups were rearrested on one or more occasions during their 1 year's probation. Five men from the control group who voluntarily chose to attend one or more counseling sessions were eliminated to clearly distinguish the control from the experimental group. Because a man could be mandated to attend counseling but not attend some or all of his sessions, researchers examined two measures related to the treatment intervention. The first measured assignment to the experimental group without accounting for the number of court-mandated SAAP sessions attended. The second is a more dynamic measure that accounted for the number of classes attended. Exhibit 1, model 1 shows no significant difference in rearrest between the experimental and control groups. However, if members of the experimental group are allowed to vary by the number of sessions attended, there is a significant association (model 2).<sup>2</sup> This would seem to suggest that each additional SAAP session attended reduced the likelihood that the offender would be rearrested.

Continuing from the findings of the reanalyses from the Minneapolis experiment and the Spouse Assault Replication Programs, researchers next investigated the impact of the experimental intervention while controlling for the batterers' stake in conformity (employment status, residential stability, marital status, and age). Because prior criminality is also a predictor of future arrest (Farrington, 1991), the number of jail terms the batterer served prior to his involvement with this study was added as a control variable.

Therefore, models 3 and 4 in exhibit 1 include control variables that measure stake in conformity and past criminality. Results indicate that stake in conformity, as measured by age and employment, are significantly related to rearrest, while marital status and residential stability are not. In addition, the proportion of months employed was significantly and negatively related to the likelihood of a rearrest. Importantly, the nullification of SAAP sessions attended suggests that stake in conformity and/or prior criminality may explain why some men attended more classes than others.

To explore this issue further, the study divided the experimental group into two categories: those who attended all court-mandated SAAP sessions (compliers) and those who failed to attend all their assigned sessions (noncompliers). Given the sanctions that applied to noncompliers, their failure to be deterred from violating their conditions of probation (attending the court-mandated SAAP) may also predict their failure to be deterred from reoffending. Exhibit 2 reports the coefficient estimates comparing the effect of compliers and noncompliers with that for the control group. When control variables were omitted, the men in the experimental group who attended all classes were significantly less likely to be rearrested. By taking the exponent of this estimate (0.503), the odds that compliers would be rearrested are about half that of the control group. In contrast, the odds of rearrest for men who attended fewer sessions than assigned were 2.53 times higher than the control group (exponent (0.930)).

This finding strongly implies that men who are unlikely to be deterred by the consequences of missing their court-mandated SAAP sessions are also less likely to be deterred by the consequences of reoffending. But what is it that distinguishes these men? After controlling for stake in conformity and prior criminality, the differences between the compliers, noncompliers, and control group disappear (see exhibit 2, column 2). This powerfully suggests that those men who attended all of their SAAP sessions would have successfully avoided rearrest even had they not been mandated into the batterer treatment program. Results from a third logistic regression (exhibit 2, column 3), using only men from the experimental group ( $n = 229$ ) to estimate the effects of stake in conformity and prior criminality on noncompliance show that the same characteristics that predict rearrest also predicted missing at least one court-mandated SAAP session.

**Exhibit 1. Logistic Regression Models Predicting Rearrest**

Variable	Coefficient Estimate (Standard Error)			
	Model 1	Model 2	Model 3	Model 4
<i>Batterers' counseling</i>				
Group assigned	0.056		0.051	
	0.240		0.272	
Sessions attended		-0.033**		-0.007
		0.010		0.012
<i>Stake in conformity</i>				
Age			-0.038*	-0.037*
			0.015	0.016
Married			0.094	0.130
			0.312	0.316
Divorced or separated			0.188	0.182
			0.439	0.440
Number of moves			0.148	0.149
			0.110	0.111
% Months employed			-2.230**	-2.181**
			0.423	0.434
<i>Prior criminality</i>				
Past jail terms			0.220**	0.237**
			0.071	0.073
<i>Controls for missing data<sup>a</sup></i>				
Marital status			0.850	0.149
			0.756	0.111
Probation folder			0.142	0.086
			0.479	0.508
Past jail terms			0.492	0.434
			0.494	0.514
Pseudo R <sup>2</sup>	0.0001	0.026	0.162	0.168

\*  $p \leq .05$ , \*\*  $p \leq .01$ ; all tests are two tailed.

<sup>a</sup> Missing values were set at zero and the control variables in this group are dummy variables for the missing values.

**Exhibit 2. Logistic Models Predicting Rearrest and Compliance**

Variable	Coefficient Estimate (Standard Error)		
	Rearrest (n = 395)	Rearrest (n = 393)	Noncompliance in experimental group (n = 229)
<i>Batterers' counseling</i>			
Compliers	-0.688*	-0.217	
	0.307	0.338	
Noncompliers	0.930**	0.318	
	0.288	0.331	
<i>Stake in conformity</i>			
Age		-0.035*	-0.052**
		0.016	0.019
Married		0.106	-0.149
		0.313	0.392
Divorced or separated		0.215	-0.390
		0.441	0.607
Number of moves		0.139	0.164
		0.111	0.148
% Months employed		-2.030**	-3.238**
		0.446	0.549
<i>Past criminality</i>			
Prior jail terms		0.212**	0.194
		0.071	0.107
<i>Controls for missing data<sup>a</sup></i>			
Marital status		0.805	1.264
		0.757	1.201
Probation folder		0.092	0.044
		0.480	0.635
Prior jail terms		0.460	0.688
		0.495	0.794
Pseudo R <sup>2</sup>	0.059	0.167	0.2774

\*  $p \leq .05$ , \*\*  $p \leq .01$ ; all tests are two tailed.

<sup>a</sup> Missing values were set at zero and the control variables in this group are dummy variables for the missing values.

These comparisons indicate two primary findings. First, where courts mandate attendance in an SAAP, men who do not comply (i.e., do not attend all their sessions) are the same men who are likely to be rearrested on a new offense; 30 percent of the noncompliers were rearrested compared with 13 percent of the compliers. However, the findings show that failure to attend all sessions of the SAAP does not have a harmful effect in and of itself. Rather, it seems to be a signal identifying the men who are more inclined to reoffend. The second finding indicates the primacy of employment and youth (both viewed as stake-in-conformity variables), not SAAP attendance, in predicting rearrest among the batterers in this study.

## Implications for Researchers

There was strong pressure against implementing an experiment to test the efficacy of court-mandated batterer treatment in Broward County (see Feder, Jolin, and Feyerherm, 2000). Many in the community thought the research placed victims at greater danger by not mandating their partners into one of the SAAP programs. Such an assumption, though, was what the study sought to test. The attitude that well-intentioned programs may not help everyone but cannot possibly be detrimental is risky. A number of rigorous studies have recently reported on treatments that have, in fact, caused participants harm (Dishion, McCord, and Poulin, 1999; Oakley, 2000; Petrosino Turpin-Petrosino, and Finckenauer, 2000).

The results presented here show no clear and demonstrable effects of counseling on offenders' attitudes, beliefs, and behaviors. Analysis of self-reported and victim-reported psychological and physical abuse using the revised Conflict Tactics Scales suggests that the behavior of batterers in the treatment programs did not change over time. Of note, evidence of severe physical abuse remained at 6 and 12 months after sentencing. Although bivariate analysis of official reports indicated that the number of SAAP sessions attended decreased the likelihood of future arrest, further analyses suggest that this decrease was driven by variables related to the batterer's stake in conformity. In fact, stake in conformity predicted both an offender's likelihood of complying with the court mandate by attending the SAAP sessions and his ability to avoid reoffending during the followup period.

It needs to be reiterated that the experiment in South Florida was implemented with strong integrity. When a man failed to attend an SAAP, probation officials sought to revoke his probation. In addition, in terms of the study itself, the misassignment rate was low, ensuring that the experimental and control groups were equivalent before treatment (as indicated by baseline comparisons). In addition, evidence suggests that the two groups continued to receive the same amounts and kinds of monitoring, supervision, and treatment (save for the SAAP) throughout the test period. Finally, although the controversy surrounding the Broward experiment led to high attrition in the followup of the victims, it in no way impeded delivery of the treatment program to the convicted batterers. (Those who were opposed to the experiment were arguing for *more* batterers to be mandated into treatment, not fewer!) In all, the authors believe that this experiment provided a valid and rigorous test of the effectiveness of court-mandated counseling, as currently conducted in Broward County, in reducing future reassault among a representative sample of convicted batterers.

Although the study was strong, admittedly it had its limitations. The largest of these was its inability to achieve high victim survey response rates. Victims are widely viewed as the best source of information on the batterers' continued abuse (Feder and Wilson, forthcoming). Thus, retaining them in an experiment testing the effectiveness of any specific batterer intervention program is critical. Additionally, this study provided a test of court-mandated batterer intervention in only one jurisdiction. Although it is thought that this community provided a good and rigorous test of the program as implemented in its jurisdiction, replication in other communities is still needed to put this important issue to the test.

Evidence from rigorous research could provide a strong foundation on which to make beneficial policy decisions (Feder and Boruch, 2000). The argument for evidence-based decisionmaking would seem to be especially compelling during times of limited budgets. However, although batterer intervention programs have been mandated by courts in jurisdictions around the country since the late 1980s (Feder, 1997), researchers still cannot definitively answer whether these programs actually make things better for the victims of domestic violence.

## **Implications for Practitioners**

Results from the Broward experiment clearly show that assuming answers to questions without first exposing them to rigorous research is dangerous. There is no doubt, as one researcher put it, about the “tremendous sense of urgency and alarm in the treatment of domestic violence—and rightly so. After all, protecting the physical and emotional safety of women and their children is the first priority. Consequently, clinicians feel a primary obligation to ‘do something’ immediately and decisively to halt and prevent violence” (Jennings, 1987: 204). But, as the results from this experiment indicate, just “doing something” may not achieve the desired results. Researchers need to be guided by rigorous research. As Saunders (1988) has so eloquently written, “One source of tension seems to arise from the simple fact that social action usually means immediate action, whereas the knowledge gained from science takes a long time to acquire. Yet action that is not well informed can be less than optimal, ineffective, or, worse, counterproductive. Movements for social justice, then, need to use the scientific search for truth as a guide” (Saunders, 1988: 92).

In conclusion, practitioners must continue to try new and innovative methods for reducing domestic violence so as to help its victims. Just as important, though, these interventions need to be rigorously tested for their ability to deliver what is being promised. This is nothing more than making these programs accountable to the taxpayers who are funding them and the victims who are depending on them.

## **Notes**

1. The Duluth Model program uses a feminist, cognitive psychoeducational curriculum provided in a group session. Its intent is to help domestic violence offenders develop an understanding of how battering is part of a range of male behaviors that seek to control women.
2. An additional 0.5 was added to the value of this measure for men in the experimental group to distinguish those who were court mandated to attend sessions but failed to go from those who

were never court mandated to the SAAP sessions. Model 2's finding of treatment efficacy is not driven by the age difference found between the control and experimental groups.

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