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**A Test of the Efficacy of Court-Mandated Counseling for  
Domestic Violence Offenders:  
The Broward Experiment**

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**Executive Summary**

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**FINAL REPORT**

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## Introduction

Domestic violence is defined as assaultive behavior involving adults who are married, cohabitating, or who have an ongoing or prior intimate relationship (Goolkasian, 1986). As the literature all too amply indicates, violence against women has a long tradition in western civilization (Cromwell and Burgess, 1996; Davidson 1977). Unfortunately, this practice continues today.

One of the earliest societal responses to domestic violence was the development of shelters for women who were battered (Johnson and Kanzler, 1993). Ironically, the idea of counseling men developed directly out of the women's shelter movement. The greatest growth in spouse abuse abatement programs (SAAPs) was brought about by the rise in pro-arrest laws in the late 1980s (Hotaling and Sugarman, 1986; Gondolf, 1997; Johnson and Kanzler, 1993). As police increased their rates of arrest for domestic violence offenses (Dutton and McGregor, 1991; Feder, 1997), pressure was placed on courts to deal with these offenders (Chen, Bersani, Myers and Denton, 1989; Ford and Regoli, 1993; Pence, 1983). Given this client population's high rates of attrition, court-mandated counseling was therefore viewed as one method to ensure greater compliance with treatment programs while simultaneously serving as an alternative to over-crowded jails (Dutton, 1984; Hamberger and Hastings, 1988; Gondolf, 1991).

Since the earliest programs dealing with batterers grew directly out of the women's shelter movement, it is not surprising that they borrowed heavily from this feminist orientation. Typically, the various programs encouraged men to confront their sexist beliefs and accept responsibility for their past abuse, while teaching them alternative behaviors and reactions (e.g., anger management, assertiveness, relaxation techniques and communication skills) (Davis and Taylor, 1999; Jennings, 1987). The most popular of these feminist cognitive psycho-educational approaches is the Domestic Abuse Intervention Project (DAIP) out of Duluth, Minnesota (Davis and Taylor, 1999). Referred to simply as the Duluth Model, it developed around an understanding of how battering is part of a range of male behaviors used to control women.

Soon after spouse abuse abatement programs began appearing, studies evaluating their efficacy began surfacing. In this first wave of evaluation research, the results indicated suspiciously high rates of success in reducing the frequency and/or severity of subsequent violence amongst this offender population (Deschner and McNeil, 1986; Neidig, Friedman and Collins, 1985; Rosenfeld, 1992). A number of researchers have since noted that these findings more reflected the methodological shortcomings inherent in these studies rather than the programs' actual efficacy in reducing violence (Chen et al., 1989; Ford and Regoli, 1993; Gondolf, 1987). These deficiencies included small sample sizes (and therefore lower power), failure to study the total population to be evaluated (as opposed to only those who complete the program), failure to use random assignment to treatment, lack of appropriate comparison groups, inadequate or variable specification of the primary outcome measures, use of unreliable measures or questionable sources of data to measure treatment outcome, use of inadequate follow-up intervals, and failure to control for the batterer's accessibility to the victim when computing the "success" of the intervention (Hamberger and Hastings, 1993; Harrell, 1991; Tolman and Bennett, 1990).

As more communities are called upon to develop coordinated responses to the problem of domestic violence we will most likely see a continued increase in the number of court-mandated treatment programs (Chen et al., 1989). Evaluation of these programs, therefore, becomes increasingly important. The possibility that these interventions may not only be ineffective in reducing violence but may provide a disservice to victims must be considered. To continue mandating counseling for convicted abusers necessarily means that limited resources will be diverted away from alternative programs for battered women and their children (Gondolf, 1987; Tolman and Bennett, 1990). And, even more problematic, is the possibility that ineffective treatment may be more dangerous for the victim than no treatment at all. Specifically, research indicates that the most influential predictor of an abused spouse's return to her husband is his participation in counseling (Feazell, Mayers, and Deschner, 1984; Gondolf, 1987). However, if treatment is essentially ineffective in decreasing recidivism then we may inadvertently be providing these victims with a false sense of security which, in the end, may lead to a higher likelihood of future injury (Harrell, 1991; Hamberger and Hastings, 1993).

There is no doubt that there is a, "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 above has indicated, doing something may not help and may even harm. Therefore, we need to be guided by rigorous research in helping us set our course. As Saunders (1988) has so elegantly 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, counter-productive. Movements for social justice, then, need to use the scientific search for truth as a guide" (Saunders, 1988: 92).

### Research Methodology

The study used a classical experimental design. Men convicted of misdemeanor domestic violence were randomly assigned into experimental or control conditions. The study took place in Broward County (an area encompassing Fort Lauderdale, Florida) in the two courts exclusively charged with handling domestic violence cases in that jurisdiction. During a five-month period in 1997, all men convicted of misdemeanor domestic violence in this jurisdiction were placed in the study.<sup>1</sup> The only exceptions were for those couples where either defendant or victim did not speak English or Spanish,

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<sup>1</sup> Note that the terms "convicted" or "adjudicated" have legal significance. The population we used included men who had either (1) pled guilty or no contest to domestic violence battery charges or who were found guilty after trial and were placed on probation, or (2) persons placed on probation, whether adjudicated guilty or not, for the offense of domestic violence battery, or (3) persons found guilty of or placed on probation for crimes of domestic violence. In fact, the vast majority of defendants (96%) pled no contest to the charges. For ease of use, throughout this report we refer to this entire group of men as those adjudicated or convicted of a misdemeanor domestic violence charge.

either defendant or victim was not 18 years of age or older, the defendant was severely mentally ill or, at the time of sentencing, the judge 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 into one of two groups.

Random assignment was based on the court docket number assigned to the case - a number randomly generated by the court computer. Based on this number, the judge announced the assignment at the time that the defendant was adjudicated. The defendant was placed in the experimental condition if his court docket number ended in an even number; control condition if the last digit was odd. This method allowed the judges to quickly implement the random assignment process. Just as critical, this method also allowed the researchers to know when departures from the random assignment process occurred in the course of the experiment.

Men placed in the control group were sentenced to one year probation. Men placed in the experimental group were sentenced to one-year probation and twenty-six weeks of group sessions from a local batterers' intervention program. At the time of sentencing, the judge would refer the defendant into one of five county certified batterers' treatment programs. Each program used the Duluth Model. As with all conditions of probation, the county's probation office was charged with monitoring the individuals' progress in meeting the judges' sentence.

In an effort to capture the true amount of change in individuals undergoing court-mandated counseling, the researcher included various measures from several different sources. The batterer was interviewed at time of adjudication and again six months after adjudication. The victim was interviewed at adjudication and six and twelve months post-adjudication. Standardized measures were used whenever possible since they have a known reliability. Probation records and computer checks with the local police for all new arrests were used to track the defendants for one-year post-adjudication.

While the ultimate purpose of the study was to test whether court-mandated counseling reduced the likelihood of future violence for men convicted of misdemeanor domestic violence, we were also looking to test the underlying theory arising from the meta-analyses of the Minneapolis Experiment and Spouse Assault Replication Programs (SARPs). Specifically, that stake in conformity variables could explain when an intervention (whether an arrest or court-mandated treatment) was effective in reducing the likelihood of subsequent violence. Therefore, we begin with two hypotheses. First, that the men who are mandated into counseling will demonstrate a lower likelihood of repeat violence in comparison to men assigned into the control (no treatment) group. Second, that men with high stake in conformity will have a lower likelihood of recidivating than those with low stake in conformity.

### **Experimental Integrity**

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. As Robert Boruch has noted, "No randomized field test is perfectly run, just as services are never perfectly provided. Mistakes ought to be expected and, when understood, exploited: They are a vehicle for building new knowledge" (Boruch, 1997: 234).

### **Outcome of Random Assignment**

The assignment obtained through our process of randomization did not differ from chance ( $t=1.42$ ,  $p>.05$ ). However, in 14 cases (3.5%) the judges overrode the random assignment. In each of these cases, the judge placed a man assigned into the control (no treatment) group into the treatment group. A total of 188 men were placed in the control group (43%) while 216 men were placed in the experimental condition (57%). The likelihood of a split of this magnitude is very low ( $t=2.81$ ,  $p<.01$ ). However, further analysis indicated that there were no significant differences between the misassigned cases and the control men.

Additionally, a comparison of the men in the control and experimental groups on all variables that Probation and the Courts had at the time of adjudication, indicated that there were no significant differences on variables associated with offender demographics, stake in conformity, criminal record and instant incident with one exception. The only variable that significantly distinguished the experimental from the control group was the offender's age. Specifically, the control group was two years younger than the experimental group. Age is a factor that has consistently been found to negatively relate to spouse abuse and recidivism in the domestic violence research literature (Edleson et al., 1985; Hamberger and Hastings, 1990; Hotaling and Sugarman, 1986). Therefore, our finding that the control group was significantly younger than those in the experimental group would make it easier to find treatment effectiveness.

### **Survey Response Rates**

Individuals did not have to volunteer to be part of the experiment. However, they had to consent before we could interview them. Though all defendants meeting sample criteria were included in the sample, not all defendants and their victims consented to be interviewed. While a large percentage of our victim non-response was due to problems in tracking the victims, a high percentage of defendant non-response was due instead to their refusal to be interviewed.

Our low response rate, to a large extent, reflects the charged environment within which we conducted our experiment. Our response rates for defendants' was 80% ( $n=321$ ) for their first surveys, and 50% ( $n=203$ ) for their second interview six months post-adjudication. Sample attrition analysis was conducted and it indicated equivalent response rates for men in the experimental and control conditions. Our survey completion rates for victims were even lower. Forty-nine percent ( $n=199$ ) for the first interview, 30% ( $n=122$ ) for the second and 22% ( $n=87$ ) for the third and final interview. Sample attrition analysis of victim surveys also showed no significant difference in response rates between victims whose partners were assigned into the experimental or control groups. While such low response rates are typical when working with victims of domestic violence (Hirschel and Hutchinson, 1992; Palmer et al., 1992; Steinman, 1991; Tolman and Weisz, 1996), there is no doubt that this presents a limitation to our study.

As one would expect, it was much easier to track defendants' progress through the use of official measures. We were able to code all the probation folders at the time that defendants were sentenced and all but one at twelve months post-adjudication. As a further check, we also ran each defendant's name against the computerized files from the county's sheriff's office providing all arrests for defendants in their county.

### **Integrity of Experimental and Control Conditions**

The literature provides examples where knowledge of the experiment changed the control condition (Petersilia, 1989). Babbie (1998) speaks specifically about the problem of compensation. That is, where those involved in the program provide the control group with something additional to make up for the lack of their receiving the intervention. Obviously, this serves as a threat to internal validity - the control group is no longer a genuine control group.

Since the judges had the opportunity to order additional non-SAAP programs that would increase monitoring or supervision, we compared judicial orders for men in the experimental and control groups to see if there were differences. In all, our results indicated that judges equivalently assigned evaluations, supervision and treatment programs to the men in the experimental and control groups. Similarly, Probation may have increased their monitoring and supervision of the men in the control group given that they were not receiving the batterers' treatment program. We compared the two groups on the number of months they were not reporting to Probation and were not violated, the number of probation meetings scheduled, missed and rescheduled, the number of months there were Written Monthly Reports for each probationer, whether they had alcohol or drug testing done and the number of times they were alcohol or drug tested. None of these comparisons were significant or showed a tendency towards significance. Therefore, there is no reason to conclude that probation officers treated the two groups differently.

Potentially, there is an alternative possibility, that Probation may not have sufficiently monitored the attendance of men placed into the experimental condition (court-mandated spouse abuse abatement program). If men were not sufficiently sanctioned for failing to comply with the court-ordered treatment, then this experiment would not offer a true test of the efficacy of court-mandated counseling. We tested for this possibility looking at treatment attendance history. Where men failed to attend any batterers' intervention sessions, 89% (n=70) were, in fact, violated on one or more occasions. Further, of nine individuals (11%) who did not attend all twenty-six batterers' intervention sessions and were not violated, four missed only one session and one missed only two sessions. These results indicate that Probation adequately monitored and sanctioned these mens' compliance with the court-mandated SAAP.

In summation, the results of our analyses indicate that random assignment was successful in ensuring that the experimental and control groups were comparable prior to the implementation of the treatment. Furthermore, there is no reason to believe that the two groups did not continue to receive the same amounts and kinds of monitoring, supervision and treatment throughout the test period with one exception. Specifically, the experimental group was mandated to receive the batterers' treatment program while the control group was not.

## **Results**

### **Batterer Profile**

The typical offender was 35 years old (SD=10) ranging from 19 to 71 years. In terms of a racial or ethnic breakdown, 57% were White, 36% Black and 6% Hispanic.



Almost equal amounts said they were married (45%) or single (43%) with another 13% reporting being separated or divorced.

Variables related to their stability indicated that most of the men were long-term county residents averaging 160 months ( $SD=142$ ). Only 25% reported that they failed to complete high school and 9% said that they had graduated college. Most of the men rented (67%) as compared to owning (33%) their own homes. Seventy-two percent reported being employed at the time of adjudication with most of these saying that they were at this place of employment for two years or less. Forty-seven percent of the men reported working in an unskilled or semi-skilled position. However, 8% of the men reported working as officials and managers. The wide range in these batterers' monthly income is consistent with this finding. For those working, their salary ranged from a low of \$250 to a high of \$10,000 per month with an average income of \$1777 per month.

In terms of their criminal histories, a large number of the men had a criminal record prior to this instant offense. Forty percent of the sample had one or more misdemeanor arrests (averaging about 0.9 misdemeanor offenses per individual) and 20% had one or more felony arrest (averaging 0.3 prior felony arrests per offender). Additionally, though fewer in number, there were still substantial numbers of convictions and incarcerations in jail (44% of the men had one or more jail stays) and prison (7% of the men had one or more imprisonment). Finally, for 85% of the men in the sample, this was their first domestic violence arrest.

Turning our attention to the instant offense, police reports noted approximately 28% of the instant incidents involved alcohol and another 3% involved drugs. Police reports also noted victim injuries in 74% of the cases. Of incidents with injuries, these incidents consisted most often of black and blue marks (58%) though 8% were severe enough to require hospitalization of the victim. Men were taken into custody 99% of the time.

### **Victim Profile**

A profile of the women involved in this study is drawn from women's' responses to the victim survey at time of adjudication. The typical victim was 34 years old ( $SD=9$ ) ranging from 18 to 63 years of age. Comparing his age to her age, women averaged two years younger than men ( $SD=6$ ) with these differences ranging from her being 23 years younger to 14 years older. About 53% of women reported that the defendant was their husband and 37% said he was their live-in boyfriend. Victims reported the average length of their relationship with the defendant as 7 years ( $SD=7$ ).

About 23% of women reported that they had less than a grade 12 education, and about 10% had graduated from college. Forty-seven percent said they were employed full-time, 19% reported part-time employment, 11% said they were a homemaker and approximately 3% said they were unemployed and looking for a job. Of those who were working, 63% reported they were in unskilled or semi-skilled positions. Yet there were almost 20% who reported they were in professional or managerial positions. This finding suggests that the victim sample included the range of women in the sample. However, we suspect that higher occupational status women were over-represented in our victim sample. We say this because 90% of the time these women reported that their husband or boyfriend was working. This is substantially higher than the 72% of men who reported they were working at the time of adjudication.

### **Treatment Delivery Measures**

The men were usually assigned to attend 26 group sessions occurring over 26 weeks from the batterers' intervention programs. If a man missed a session, he was required to make up that class. Almost 29% made all their sessions without missing any of them and approximately 95% missed five or fewer sessions. Eventually, approximately two-thirds attended all of the sessions and about 13% attended no classes. Of the control group, 97% attended no classes while 3% (N=5) did. The qualitative assessment of the progress by batterers assigned to these programs indicated that they described the men as having made satisfactory or rapid progress in their group counseling sessions (though the reliability of this estimate is questionable given that a low number of reports available in the folders).

### **Experimental and Control Groups' Standing on Outcome Measures**

A number of standardized scales were used in the offender and victim interviews to assess the outcomes of the experimental intervention. These included an abbreviated version of the Inventory of Beliefs about Wife Beating and Attitudes Towards Women. Additionally, we asked whether the batterer believed that the offense should be considered criminal, whether he thought he was responsible for the instant offense and how likely he was to engage in physical abuse again. We also used the revised Conflict Tactics Scale (CTS2) to assess his self-reported use of verbal, physical or sexual abuse within the previous six months. Since men may have been assigned into a SAAP and may not attend any or all of it or, alternately, not be assigned and may attend on their own, we analyzed our data in terms of treatment assigned as well as treatment received.

A parallel interview was developed for the victims asking them about the batterer's behavior and their beliefs about who was responsible and whether they thought another physical incident was likely. The study design asked offenders questions about self-reported partner abuse at time of adjudication and six months post-adjudication. Victim surveys were to be administered at time of adjudication, six months, and one-year post-adjudication. The information of survey data presented analyzed differences between the experimental group and control group at each point of time, and then looks to see whether there were changes over time.

### **Offender Attitudes**

We used offender surveys to compare men in the experimental and control group. Our comparisons looked at differences between groups at time of adjudication (Time 1), at least six-months post-adjudication (Time 2) and changes between Time 1 and Time 2. Note that at the time of their second interview, 30% (n=35) of these batterers had concluded their entire counseling program and, more importantly, our sample had completed an average of 22 of the 26 mandated counseling sessions. This means that these men had already received approximately 85% of the intended "dosage" of counseling.

Approximately one-half of the men viewed wife-beating as acceptable behavior in various situations. No differences maintained between the experimental and control groups at Time 1 or Time 2 or over time. Similarly, the men's perceptions about the proper roles of women did not show differences between groups or between groups over time. No significant differences maintained between groups over time in their perceptions of whether wife beating should be considered criminal and whether the state had a right to

intervene. Additionally, no significant differences emerged in men's self-reported likelihood of hitting their partner between groups.

The only change noted in all of our comparisons was a small but significant change in the men's view of how responsible their partners were for the instant offense that led them to court. Specifically, over time, those in the control group viewed their partners as increasingly responsible. In comparison, in the six-months following adjudication, those in the experimental group saw the woman as a bit less responsible. However, it needs to be noted that, even with this change, the men in the experimental group still viewed their partners as "somewhat" to "equally" responsible for the incident.

There are a number of studies indicating that men who batter hold more traditional views about woman and their proper roles. The cognitive behavioral approach is based on the assumption that educating men about the incorrectness of the use of verbal, physical or sexual control over their partners will lead to changes in their beliefs ultimately leading to changes in their behaviors. The results from these analyses seem to indicate that men's beliefs about the legitimacy of wife beating, their responsibility for these incidents and their attitudes regarding the proper roles for women to play has not significantly changed for those court-mandated into the intervention programs in comparison to the no-treatment control group.

### **Victim Attitudes**

The victim interviews clearly indicated that the vast majority of women viewed wife beating in most all contexts as inappropriate behavior. Not surprisingly, this runs counter to what most of the men reported. There were no differences between victims whose partners were in the experimental or control groups. And there were no differences between groups of victims over time. Victims also reported a more liberal view of women's roles than had their partners. Again, there were no differences in women's attitudes about the appropriate role for women between the control and experimental groups nor did these views significantly change over time.

The survey indicated no significant differences in victims' perceptions of whether the instant offense that brought them to court should be viewed as criminal. About 57% of women felt the offense should be viewed as a crime compared to 26% of the men. There were no differences between groups and between groups over time.

Victims also rated their level of responsibility for the instant offense as falling between not at all responsible (1) and somewhat responsible (2) whereas men rated the women as almost equally responsible. Again, there were no significant differences in women's perceptions of responsibility between the experimental and control groups.

Finally, no significant differences accrued at Time 1, Time 2, or in the difference over time, between the two groups in the victims' perceptions of the likelihood that their partner would hit them again. Their ratings of the likelihood of this event were higher than men's ratings of this event (20% versus 5%, respectively). Overall, though, victims seemed to view the likelihood of this happening again as only a remote possibility.

### **Offender Self-Reported Likelihood to Engage in Minor or Severe Abuse**

Based on the revised Conflict Tactics Scale, 30% of the men self-reported what Straus would consider a minor abusive action taken against their partner (this includes grapping and slapping) within six-months after adjudication. In contrast, 32% of the

women reported such an incident within the same time frame. In terms of more severe physical abuse (for instance, using a knife or gun, choking or beating up their partner), 8% of the men self-reported taking such actions compared to 14% of the women.

As Table One indicates, we find no differences between groups and over time in men's self-reported likelihood to engage in any of the five subscales listed in the revised Conflict Tactics Scale. We regressed treatment assignment, treatment received (number of domestic violence classes attended) and stake in conformity variables (marital status, residential stability and employment) on men's self-reported use of severe physical violence. Consistent with the results from our analysis of attitudes and beliefs, the results indicated that neither assignment to a batterers' program nor attending the classes were significant in explaining severe physical violence. Instead, stake in conformity variables were important in accounting for this variation. Specifically, younger men lacking residential stability were significantly more likely to self-report acts of severe physical violence against their partners than their older more residentially stable counterparts.

### **Victim Self-Reported Likelihood to Engage in Minor or Severe Abuse**

As Table One indicates, we find no difference between groups and over time in women's reports of their partners likelihood to engage in any of the five subscales listed in the revised Conflict Tactics Scale (CTS2: Negotiation, Psychological Coercion, Physical Abuse, Sexual Coercion and Injury). Fourteen percent of the women reported an act of severe physical violence occurring during the follow-up period. Regressing treatment group assigned, treatment received and stake in conformity variables on the dependent variable we once again see the primacy of stake in conformity variables in predicting recidivism amongst batterers. Specifically, offender's age and marital status achieved significance while his employment, though not significant, demonstrated a strong tendency to relate to her 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 – Violations of Probation**

Comparisons between men assigned into the experimental and control groups would be unfair where one group could be violated for reasons that did not apply to the other group. This would seem to be the case to the extent that men in the experimental group could be violated for failing to attend the batterers' program (a condition of probation that did not apply to those in the control group). However, analysis indicated that, while probationers may have been revoked for failing to attend treatment, in all cases save one, this was one of several reasons listed in their revocation. In other words, it does not seem that, realistically, men were violated on probation for exclusively failing to attend the domestic violence classes.

**Table One**  
**Revised Conflict Tactics Scales**  
**Average Score on Scale by Survey**

Mean (N)	Defendant Time 1		Defendant Time 2		Victim Time 1		Victim Time 2		Victim Time 3	
	Contro l	Exp	Control	Exp	Control	Exp	Control	Exp	Control	Exp
Negotiation	2.11 (117)	2.08 (153)	1.58 (87)	1.68 (90)	1.77 (81)	1.75 (98)	1.99 (45)	1.86 (54)	1.82 (34)	1.86 (36)
Psychological	.88 (117)	.83 (155)	.41 (82)	.30 (93)	1.23 (79)	1.43 (102)	.84 (45)	1.00 (56)	0.95 (35)	0.97 (34)
Physical	.27 (115)	.25 (143)	.06 (82)	.03 (90)	.62 (80)	.65 (98)	.13 (42)	.11 (55)	.14 (33)	.15 (35)
Sexual coercion	.11 (113)	.10 (154)	.07 (85)	.04 (88)	.21 (78)	.17 (97)	.06 (44)	.05 (57)	.03 (35)	.13 (36)
Injury	.16 (123)	.16 (159)	.06 (86)	.02 (94)	.37 (84)	.43 (103)	.02 (45)	.06 (58)	.09 (35)	.08 (36)

Note: Response categories were 0=Never, 1=1, 2=2-5, 3=6+

Our data indicated that 48% of the experimental group and 45% of the men in the control group were violated one or more occasions during their year on probation. This difference between groups was not significant. Once again, we regressed treatment assigned, treatment received and stake in conformity variables on violations. For the first time in our analyses, results indicated that assignment to treatment, number of classes attended and the interaction term were significant in explaining variation in VOPs. Additionally, two stake in conformity variables - marital status and residential stability - were also predictive of a violation. Specifically, those who were not married and who had less residential stability (more moves during their year on probation) were more likely to be violated.

What is surprising is the direction of the relationship as it pertains to the treatment variables. Assignment to the experimental group made it significantly *more* likely, all things being equal, that the man would be violated on probation. But this increase in likelihood of violation does not seem to be due to increased monitoring since we earlier found no significant differences in Probation's monitoring between groups. Countering this increased likelihood of violation for those assigned into the experimental group, the regression equation indicated that attending the domestic violence classes reduced the likelihood of violation. However, the reduced effect of attending classes has to be understood as a discounted effect because domestic violence classes were mandatory. We estimated the interaction effect of experimental group by attending classes and clearly found an increased likelihood of violation on probation when classes were mandatory.

Logistic regression allows us to measure the effect of a *single* factor (e.g., group assignment) on an outcome (violation on probation) while controlling for all other variables. However, the men in our sample came as complete packages. As such, each man represented many different variables. Therefore, to make these numbers more meaningful, we provide a table with the estimated probabilities of being violated for men representing different combinations of variables.

Table Two varied group assignment (experimental versus control), domestic violence classes attended (0, 20 and 26) and employment (employed versus unemployed for the 12 months on probation). The table clearly indicates that employed probationers are much less likely to be violated than unemployed probationers. Additionally, attending more domestic violence classes – whether one is in the experimental group or the control group – is associated with a lowered likelihood of being violated. Interestingly, the men assigned to the batterers' treatment program have a *higher* likelihood of being violated in comparison to the control (no treatment) group in every comparison except where they have attended all the treatment sessions. And even in this scenario, the difference is not substantial. (Compare the men represented by Cells A to Cells I. There is a 5% versus 8% likelihood of being violated.) More realistically, compare the man represented by Cells B (a fully employed married man assigned to the experimental group who attended 20 sessions) to the man in Cells I (a fully employed married man assigned to the control group who attended no domestic violence classes). The man mandated into the batterers' treatment program has a 21% likelihood of being violated in comparison to the man assigned no treatment who has a 8% likelihood. Clearly, assignment to the batterers' intervention program is associated with more violations unless it is offset by attendance at all of the sessions.

**Table Two**  
**Estimated Probabilities of Violation of Probation According to**  
**Treatment Assignment and Batterer Characteristics**

Variables											
Cell	Group	DV Classes	Group by DV classes	Married	Div/Sep	Age	Number of moves	Months worked	Constant	E	Estimated probability VOP
A	Exp	26	26	Yes	No	35	Zero	12	2.98	.0505	.048
B	Exp	20	20	Yes	No	35	Zero	12	2.98	.261	.207
C	Exp	0	0	Yes	No	35	Zero	12	2.98	1.445	.591
D	Exp	26	26	Yes	No	35	Zero	0	2.98	.7513	.4290
E	Exp	20	20	Yes	No	35	Zero	0	2.98	1.6291	.6196
F	Exp	0	0	Yes	No	35	Zero	0	2.98	21.499	.9556
G	Control	26	0	Yes	No	35	Zero	12	2.98	.0001	.0001
H	Control	20	0	Yes	No	35	Zero	12	2.98	.0002	.0002
I	Control	0	0	Yes	No	35	Zero	12	2.98	.0843	.0778
J	Control	26	0	Yes	No	35	Zero	0	2.98	.0005	.0005
K	Control	20	0	Yes	No	35	Zero	0	2.98	.0029	.0029
L	Control	0	0	Yes	No	35	Zero	0	2.98	1.2548	.5565

### Official Measures – Rearrests

Twenty-four percent of men in both the experimental and control groups were rearrested on one or more occasions during their one year on probation. A logistic regression equation was developed using treatment assigned, treatment received and stake in conformity variables to predict rearrest. The results indicated that assignment to the experimental group was not significantly related to likelihood of being rearrested. However, attending domestic violence classes (treatment received) and the interaction between group assignment and treatment received were significant. Additionally two stake in conformity variables, employment and age, were also significant in predicting rearrests. In fact, the logistic regression indicated that employment was the most important factor accounting for variation in rearrest in our sample of batterers.

Table Three provides examples of different types of men in the sample. Again, we begin by taking the best-case scenario. We compare a man represented by Cells A (a married fully employed man assigned into the treatment group who attended all the sessions) with one represented by Cells I (a married fully employed man assigned into the control group who attended none of the sessions). The man in the experimental group, under the best situation, has a 7% likelihood of being arrested while on probation. Alternately, the man assigned into the control group has a 9% likelihood of rearrest. Again, this difference is not substantial. More importantly, in most other scenarios we see how assignment into mandatory counseling leads to a *higher* likelihood of rearrest. Compare the man represented in Cells B (a married fully employed man assigned into the treatment group who attended 20 of the counseling sessions) with Cells I. Notice that the man in this instance who was assigned to counseling is expected to have a higher likelihood of rearrest than a comparable man who was not assigned into this treatment program (16 versus 9%). Next, comparing this high stake in conformity man (full employment) assigned to the experimental group who completes some but not all of his counseling (cell B) with a low stake in conformity man (no employment) assigned to the experimental condition who completes a similar amount of sessions (cell E) we see a difference in likelihood of rearrest of 16% versus 57% respectively.

These comparisons indicate two primary findings from our study. First, where the court assigns counseling and the individual fails to attend most or all of the sessions, there is an increased likelihood of rearrest when compared to a similarly situated man (on stake in conformity variables) who was never assigned counseling. Second, the above comparisons indicate the primacy of employment over attendance in predicting rearrest amongst men convicted of misdemeanor domestic violence.

In all, while attending classes may reduce the likelihood of rearrest, this reduction will be modest. There were larger differences in rearrest based on stake in conformity variables. While many people may assert that the State cannot control stake in conformity variables (for instance, whether or not a man is employed), the same counter would seem to apply to getting these men to attend the domestic violence sessions. Specifically, the State mandated domestic violence intervention and then monitored and sanctioned when men failed to comply. Despite all of this, Probation was still unable to get all of the men to complete their sessions.



**Table Three**  
**Estimated Probabilities of Rearrests According to**  
**Treatment Assignment and Batterer Characteristics**

Cells	Group	Variables									
		DV Classes	Group by DV classes	Married	Div/Sep	Age	Number of moves	Months worked	Constant	e	Estimated probability Rearrest
A	Exp	26	26	Yes	No	35	Zero	12	.616	.0756	.0703
B	Exp	20	20	Yes	No	35	Zero	12	.616	.1898	.1595
C	Exp	0	0	Yes	No	35	Zero	12	.616	4.0886	.8035
D	Exp	26	26	Yes	No	35	Zero	0	.616	.5196	.3420
E	Exp	20	20	Yes	No	35	Zero	0	.616	1.3055	.5663
F	Exp	0	0	Yes	No	35	Zero	0	.616	28.1233	.9657
G	Control	26	0	Yes	No	35	Zero	12	.616	.0017	.0017
H	Control	20	0	Yes	No	35	Zero	12	.616	.0044	.0044
I	Control	0	0	Yes	No	35	Zero	12	.616	.0941	.0860
J	Control	26	0	Yes	No	35	Zero	0	.616	.0120	.0018
K	Control	20	0	Yes	No	35	Zero	0	.616	.0301	.0292
L	Control	0	0	Yes	No	35	Zero	0	.616	.6474	.3930

## Summary

The primary purpose of this study was to test the efficacy of court-mandated counseling for domestic violence offenders. The results presented show that there were not clear and demonstrable effects of counseling on the offenders' attitudes, beliefs and behaviors. Analysis of self-reported and victim-reported psychological and physical abuse, using the revised Conflict Tactics Scales, suggested that his behavior has not changed over time. Of note, there was still evidence of severe physical abuse at six and twelve months post-sentencing.

Looking at official reports, there was some evidence that men assigned into the counseling programs were more likely to be rearrested unless they attended all of the court-mandated counseling sessions. Some people may say that this just proves that we need to utilize every legal means to get these men to attend the batterers' programs. However, it must be remembered that even if men attended all their sessions they now had only a slightly lowered likelihood of being rearrested when compared to a similarly situated man in the control group who had not been mandated to go to counseling and therefore did not attend any sessions. And, where they did not attend all the sessions, their likelihood of a rearrest, relative to a comparable man in the control condition, increased. In other words, we found an increased likelihood of rearrests where men assigned to counseling failed to attend.

In addition, this charge to throw the full force of the law at the man who does not attend all of his treatment sessions seems to beg the question. In this jurisdiction, unlike those observed by Harrell (1991) and Palmer and her colleagues (1992), men were monitored and sanctioned. While approximately one-third of the men failed to attend the batterers' program, 100% of these men were violated for one or more conditions of probation. (And 71% of the men were specifically violated for failing to attend their counseling.) In all, it seems that Probation monitored and revoked where men did not complete the batterers' program. Still men self-selected into treatment completers and treatment dropouts. Finally, in line with the meta-analyses of Minneapolis and the Spouse Assault Replication Programs (SARPs), our study indicated the importance of stake in conformity variables in predicting arrest amongst men convicted of misdemeanor domestic violence.

There is no doubt that the controversy surrounding the Broward Experiment of random assignment of men into a spouse abuse abatement program was an impediment to the research. It led to low victim response rates, high staff turnover, delays, and other problems. The low victim response rate is especially problematic to the extent that research consistently indicates that victims provide the best information on continuing abuse (Dutton, 1987; Edleson and Bryggers, 1995). To the degree that this study collected information from multiple sources (men's self-report, victims' reports and official measures) and all indicated similar conclusions, we have greater confidence in the results from each separate measure. We believe that this experiment provided a valid and rigorous test of the efficacy of court-mandated counseling as presently conducted in Broward County.

In conclusion, we hope that others decide to replicate this study and put this important policy question to the test in their jurisdictions. We have been candid in our disclosures of the problems (see Feder and Forde, 2000) with the hope that others will learn from our mistakes and build better and stronger experiments.

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