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RESEARCH

Testing the Effectiveness of Batterer Programs and Judicial Monitoring

Results from a Randomized Trial at the Bronx
Misdemeanor Domestic Violence Court

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Executive Summary

Over the past two decades, the criminal justice system has reshaped its response to the crime of domestic violence in an effort to protect victims and hold offenders more accountable. In particular, a growing number of courts have come to rely on batterer programs as their mandate of choice, especially when the legal issues in a case preclude the imposition of jail. To date, only four previous randomized trials have evaluated the effects of batterer programs (Dunford 2000; Feder and Dugan 2002; Palmer, Brown, and Barerra 1992; and Davis, Taylor, and Maxwell 2000). Even fewer studies have considered the effects of judicial monitoring, although many courts are now devoting substantial resources to monitoring, both pre- and post-conviction. Some believe that monitoring can deter recidivism by sending a message that the court is closely watching over the offender and will discover and sanction any noncompliance; but this belief has not been rigorously examined.

Accordingly, the Center for Court Innovation received funding from the National Institute of Justice to provide a more definitive test of the effectiveness of batterer programs and varying intensities of judicial monitoring in a single randomized trial. The research was carefully designed to ensure that the effects of batterer programs and judicial monitoring could be distinguished, so that the impact of one practice would not be mistakenly attributed to the other.

Research Design

The study was implemented at the Bronx Misdemeanor Domestic Violence Court, with enrollment running from July 23, 2002 through February 27, 2004. All eligible offenders were arraigned on a domestic violence misdemeanor, convicted of a violation, and sentenced to a conditional discharge with a one-year protection order in favor of the victim. This was the most common legal status when completion of a batterer program was also a condition of the sentence. The study proceeded in two parts, the primary randomized trial and a quasi-experimental design.

1. Design for the Randomized Trial

The trial involved a two by two factorial design (see next page), in which offenders were randomly assigned to one of four conditions: (1) batterer program plus monthly judicial monitoring; (2) batterer program plus “graduated” monitoring (less frequent court appearances in response to compliance and more frequent appearances in response to noncompliance), (3) monthly monitoring only; and (4) graduated monitoring only. Since all four conditions involved monitoring, the design isolated whether a batterer program requirement (for groups one and two) produces any additional protective value in the form of lower recidivism rates. Further, the trial examined whether the specific approach to monitoring makes a difference: is it more effective to require domestic violence offenders to return to court as a matter of routine each and every month, or to offer incentives by adjusting the required frequency of court appearances as a direct response to past compliance? By using random assignment methods, considered the “gold standard” in social science research, findings can be deemed valid with a high level of certainty. A total of 420 offenders were randomly assigned in this fashion.

	Batterer Program	No Batterer Program
Monthly Monitoring	N = 102	N = 109
Graduated Monitoring	N = 100	N = 109

Other key components pertaining to the implementation of the trial were as follows:

- *Sampling frame:* To be eligible for random assignment, both the prosecution and defense had to be prepared to accept a conditional discharge sentence with a batterer program. The sentencing judges could exclude eligible offenders at their discretion, but used this option in only 14% of all eligible cases; and subsequent analysis detected few significant differences (in only two out of 67 baseline comparisons) between the characteristics of offenders in the randomized trial and those excluded by the judges.
- *Batterer program curricula:* The offenders assigned to a batterer program attended one of two 26-week programs, with classes meeting weekly for 75 minutes. Both are typical of batterer programs run for groups of men nationwide. They seek to educate participants about the roots of battering in societal norms that support male abuse of women and, through a combination of instruction and discussion, encourage participants to take responsibility for their anger, actions, and reactions. One of the programs also includes a substantial cognitive-behavioral module that focuses on identifying early warning signs of abusive behavior and developing skills for managing responses in appropriate ways.
- *Duration and content of judicial monitoring:* For those assigned to a batterer program, the monitoring period runs through program completion, which averages seven months (allowing for three unexcused absences). For those not assigned to a program, monitoring was similarly set at seven months. The offenders appear in a specialized compliance part before a judicial hearing officer (a retired judge), who possesses information about batterer program attendance (if applicable), re-arrests citywide, violations of any orders or protection, and any victim reports of re-abuse conveyed to a victim advocate.
- *Implementation of graduated monitoring:* Offenders on the graduated schedule were required to report to court less frequently when in compliance (e.g., 4, 10, 18, and 26 weeks after sentencing) and more frequently when out of compliance (2 and 4 weeks later); but we found that only the former of these rules was consistently implemented.
- *Research interviews with victims:* One year after sentencing, attempts were made to contact the victim in the case, and 106 interviews were completed. Although the contact rate was only 25%, there were no significant differences in the characteristics of cases for which the victims were and were not interviewed.

2. Design for the Quasi-Experimental Comparison

Since all four experimental conditions included judicial monitoring, the randomized trial could not test whether judicial monitoring produces any additional protective value in comparison with the complete absence of monitoring. However, in the second part of the study, offenders in the randomized trial were compared with another pool of offenders convicted of the same offenses during the same period of time but who, as a result of normal sentencing deliberations, were sentenced to a conditional discharge involving neither a batterer program nor monitoring. This served to add a true “nothing” condition to the study, albeit without the methodological benefits of random assignment.

To minimize the risk of bias, we implemented a propensity score matching technique designed to ensure the comparability of the two samples. This technique served to limit the final “no-monitoring” sample only to those offenders whose background characteristics (e.g., demographics, criminal history, current charges, and relationship to victim) closely matched those of the offenders in the randomized trial. After completing the matching process, 387 offenders from the initial trial were matched to 219 offenders in the no-monitoring group. (Thirty-three of the initial 420 offenders in the randomized trial had to be removed due to missing data on key characteristics required for matching; also, the 219 offenders in the final control sample represented the best matches from a much larger initial pool of 599.) When comparing the two final samples, there was not a single significant difference in their baseline characteristics, suggesting a high degree of internal study validity.

Outcome Results

The offenders were tracked for one year after sentencing. Most were also tracked over a longer 18-month period; a shorter “in program” period (when the offenders were actively monitored); and a one-year “post-program” period (after monitoring had ended). However, we did not detect any notable differences based on the timeframes examined, and for that reason, the results reported below are all for the one year post-sentence period.

1. Official Re-Arrest Records

- *Impact of batterer programs on re-arrest:* Batterer programs did not produce a reduction in re-arrests. There were no significant differences between those assigned and not assigned to a batterer program in the probability of re-arrest for either any offense (29% and 26%) or for domestic violence (16% and 12%).
- *Impact of judicial monitoring schedule on re-arrest:* Neither form of monitoring proved more effective than the other. There were no significant differences between those assigned to monthly and graduated monitoring in the probability of re-arrest for any offense (28% and 27%) or for domestic violence (13% and 14%).
- *Impact of judicial monitoring on re-arrest (compared with the complete absence of monitoring, based on the quasi-experimental design):* Judicial monitoring did not produce a reduction in re-arrests. There were no significant differences between those

in the randomized trial, all of whom were monitored, and those in the no-monitoring sample in the probability of re-arrest for any offense (27% and 24%) or for domestic violence (13% and 14%). Judicial monitoring appeared to produce a modest reduction in the *total number* of domestic violence re-arrests ($p < .10$), but this was the only outcome measure whose impact began to approach statistical significance.

- *Survival analysis:* None of the interventions under examination outperformed any other in delaying the onset of recidivism; there were no significant differences in the average number of crime-free days prior to first re-arrest. For all samples, of those re-arrested over the one year tracking period, approximately two-thirds were re-arrested within the first six months after initial sentencing.
- *Predictors of recidivism:* The strongest predictors of future recidivism were prior criminal history, younger age, lack of a “stake-in-conformity” (e.g., stemming from employment or living with the intimate partner), and more serious current arrest charges. However, taken together, these measures more strongly predicted re-arrest for any offense than for domestic violence in particular; hence domestic violence offending appears to involve dynamics that are less easily captured by standard social background variables.

2. *Victim Reports*

- *Prevalence of victim reports of re-abuse:* Forty-six percent of the women interviewed reported experiencing at least one incident of re-abuse in the year after sentencing: 15% reported physical abuse, 18% reported threats, and 44% reported other forms of abuse (harassing phone calls, confrontation, denying access to money, isolation from friends, stalking, threats of suicide, or threats of harm to children or other people they know).
- *Impact of random assignment conditions on victim reports of re-abuse:* Whether the cases were assigned to a batterer program or not, or to monthly or graduated monitoring, had no significant effects on victim reports of re-abuse (either in general or concerning each specific form of re-abuse: physical, threats, or other).
- *Impact of batterer programs on victim satisfaction with the sentence in the case:* Victims whose cases were assigned to a batterer program were more satisfied with the sentence in the case (77%) than victims whose cases were not assigned to a program (52%). The type of judicial monitoring schedule had no impact on victim satisfaction.

Conclusion

The preponderance of evidence now accumulated in the field calls into question the efficacy of batterer programs based on the most prevalent national models. Indeed, the main findings from our randomized trial are consistent with other recent trials, of which none found that mandating offenders to a batterer program for groups for men produced lower rates of re-abuse. We did find a beneficial impact of a batterer program mandate on victim satisfaction with the

sentence in the case; however, in the absence of a reduction in re-abuse, this finding is difficult to interpret. It may only indicate that victims whose partners are mandated to a program are imbued with optimism that is in the end unjustified; alternatively, it is equally plausible that the victims recognize that the batterer program does not make them safer but want the offenders held more accountable by having to attend it as an added sentencing requirement. Indeed, nearly half (49%) of the victims who were *dissatisfied* with the sentence expressed that they held this view because the sentence was not severe enough.

Contrary to our expectations, we did not find that judicial monitoring leads to lower re-arrest rates; nor did we find that the specific monitoring schedule (monthly or graduated) affects the recidivism outcomes. Our findings are qualified, however, by the nature of judicial monitoring at our site in the Bronx. The feedback conveyed by the judicial hearing officer during monitoring appearances was generally brief, matter-of-fact, and often couched in legal terminology that some offenders may not have understood; and while there was some use of positive incentives with offenders on the graduated schedule, noncompliance did not consistently and immediately trigger sanctions, jail or other, designed to enforce the court's conditions and deter future noncompliance. This study's findings therefore call into question the efficacy of the simple surveillance of domestic violence offenders through ongoing court check-ins; but we did not test the effects of a truly robust form of judicial supervision. Also, whereas our batterer program results affirm those obtained by other experiments, the previous literature is more limited with respect to monitoring. Hence we believe there is a need for replication of our monitoring results, preferably involving a test of a more rigorous judicial supervision regimen in which information is more clearly conveyed to the offenders about their responsibilities, and compliance is enforced through the consistent implementation of sanctions and incentives.

In light of our findings, courts may now wish to consider new experimentation with judicial monitoring (e.g., involving more rigorous applications of positive and negative incentives to foster compliance); changes in program mandates (e.g., involving mandates other than standard batterer programs with an educational or cognitive-behavioral emphasis); a greater emphasis on accountability than rehabilitation when batterer programs are used (e.g., consistently imposing consequences in response to noncompliance); and a refocus on victim services (e.g., involving efforts to develop new resources and methods to assist victims and spread community awareness about the harms of domestic violence). Regrettably, our study suggests that some of the most prevalent court responses to domestic violence crime may be ineffective; but perhaps these findings can liberate the justice system to innovate in as-yet unexplored ways and promote a new period of reflection and experimentation.

Chapter One

Introduction

Over the past two decades, the criminal justice system has reshaped its response to the crime of domestic violence in an effort to protect victims and hold offenders more accountable. Mandatory arrest policies have been promoted by advocates and widely adopted by police departments across the country (Buzawa and Buzawa 1996). A growing number of prosecutors have removed discretion from the victims of domestic violence and pursued cases regardless of the victim's desires or willingness to cooperate (Rebovich 1996; Hanna 1996). Prosecutors have also begun to establish specialized domestic violence bureaus, while courts have responded in kind by creating specialized domestic violence courts. These dedicated units are designed to ensure that staff receive training and develop substantive experience in the handling of domestic violence matters. Also, many domestic violence courts seek to link victims with services inside or outside the courthouse, and include additional case management staff to help monitor offender compliance with program mandates or other court orders. The passage of the Violence Against Women Act in 1994 accelerated these changes by promoting mandatory arrest policies and providing new funding for specialized police and prosecution units.

Accompanying these changes, courts have increasingly come to rely on batterer programs as their sanction of choice, especially when the legal issues in a case preclude the imposition of jail. By one estimate, nearly 80% of batterer program participants are mandated from courts (Healey, Smith and O'Sullivan 1998). Some support these programs in the hopes that they will rehabilitate offenders and prevent re-offending. Treatment for drug offenders through specialized drug courts has proven to be effective at reducing drug abuse and re-offending (e.g., see literature reviews in Cissner and Rempel, 2005; Government Accountability Office, 2005; Roman and DeStefano 2004; and Wilson, Mitchell, and MacKenzie 2002). Thus it would seem reasonable to assume that treatment programs for batterers might achieve similar positive results. However, recent rigorous evaluations of batterer programs have failed to find support for this position (Dunford 2000; Feder and Dugan 2002).

Others, skeptical of these programs' therapeutic value, embrace batterer programs in the belief that they can serve as a meaningful, relevant sanction in cases deemed not to warrant incarceration on the legal merits. Those adopting this second view typically support the use of batterer programs in tandem with intensive judicial monitoring, in which the offenders must return to court for ongoing compliance updates. At these updates courts can confirm whether or not offenders are complying with their batterer program mandate, and impose jail or other sanctions for noncompliance. This position received support from a recent evaluation conducted in Brooklyn by one of the current principals, which found evidence suggesting that assigning batterers to programs reduced recidivism – not due to the batterer classes themselves, but to the continued court control (Davis et al. 2000). However, while the results suggested that court control may play a significant role in suppressing recidivism, the study did not test that proposition directly.

The findings of the Brooklyn research directly motivated the present study, which was specifically designed to test whether a batterer program produced any additional protective effect beyond that which could be achieved simply by the judicial monitoring of batterers. If batterers who were assigned to attend batterer classes in conjunction with judicial monitoring exhibited

lower recidivism rates than batterers who received only monitoring, that would argue that programs are able to bring about behavioral change. If, on the other hand, batterers who attended the program in addition to judicial monitoring did not re-offend at lower rates than batterers who were only mandated to be monitored, that would suggest that batterer programs do not provide additional protective value beyond simply letting the offender know his behavior is being scrutinized.

If the comparison between court-monitored offenders who did and did not attend batterer programs revealed no differences the next question would be whether judicial monitoring itself reduces re-offending. Fortunately, we were able to identify a group of offenders who received neither a batterer program nor judicial monitoring that could be compared to offenders assigned to monitoring. The two sets of comparisons provided a good test of the protective effects of both batterer programs and judicial monitoring.

Study Goals and Objectives

This study seeks to provide a definitive test of whether batterer programs and varying intensities of judicial monitoring reduce re-offending among domestic violence offenders. The study design was carefully developed to ensure that the effects of batterer programs and judicial monitoring could be distinguished, so that the impact of one practice could not be mistakenly attributed to the other.

An understanding of the efficacy of batterer programs and judicial monitoring would be of tremendous value to courts nationwide. For some courts, the need to resolve whether, how, and for whom these practices are effective is exacerbated by practical concerns related to case volume. Especially in large urban jurisdictions, many court administrators would like to identify ways to promote offender accountability and enhance victim safety without over-taxing the system. For instance, the process of ensuring timely and accurate reporting about compliance with batterer programs may be too labor-intensive to justify if such programs do not reduce recidivism. Court administrators also need better information on how best to monitor domestic violence offenders: should they be required to report back to court regularly (e.g., monthly); or might graduated responses produce better outcomes (e.g., reduced reporting in response to compliance and increased reporting in response to noncompliance)? The idea that graduated responses may work better stems from the use of incentives; if compliance produces a benefit in the form of reduced court appearances, and if noncompliance triggers the negative outcome of additional appearances, perhaps some offenders will become more likely to remain compliant, at least during the immediate monitoring period.

Overview of the Design

The study involves randomly assigning convicted domestic violence offenders at the Bronx Misdemeanor Domestic Violence Court to one of four conditions: (1) batterer program plus monthly judicial monitoring; (2) batterer program plus graduated monitoring, (3) monthly monitoring only; and (4) graduated monitoring only. Since all four conditions involve monitoring, the design isolates whether a batterer program requirement (for groups 1 and 2 only) produces any *added* value in the form of lower recidivism rates. Further, the study examines whether the specific nature of the monitoring schedule makes a difference: is it more effective to require domestic violence offenders to return to court as a matter of routine each and every month, or to offer an incentive by promising reduced appearances in response to compliance and

increased appearances in response to noncompliance? By using random assignment methods, considered the “gold standard” in social science research, findings can be deemed valid with a high level of certainty.

However, what the four random assignment conditions do *not* allow is a test of whether judicial monitoring in itself produces any added protective value in comparison with the complete absence of monitoring. This is because the four experimental groups do not include a “nothing” condition (no program *and* no monitoring of any kind). To address this, the study also involves a quasi-experimental comparison between the randomly assigned offenders and a second pool convicted of the same offenses during the same period of time but who, as a result of normal sentencing deliberations, were sentenced to a conditional discharge involving neither a batterer program nor monitoring. In short, we *were* able to include a “nothing” condition in the study, but did so without the methodological benefits of random assignment. (We did, however, use propensity score matching techniques described later in an effort to minimize the risk of bias.)

All offenders were tracked for one year after sentencing to determine whether they fulfilled the conditions of their sentence and whether they committed new domestic violence offenses. Both official criminal records and victim interviews were used to estimate offender recidivism. Also, using official records only, most of the sampled offenders were tracked over a longer 18-month period after sentencing as well as over a one-year “post-program” period that began when the period of judicial monitoring ended. Analyses within these various timeframes enabled us to determine whether the interventions under investigation suppress recidivism during the intervention period only (e.g., when the offender is actively attending the batterer program or returning to court for monitoring) as well as whether any effects persist once the intervention period has ended.

Specific Research Questions. The study design addresses three primary questions:

1. *Do batterer programs work?* Increasingly, state laws are mandating longer and longer periods of batterer program attendance. Yet several recent studies raise serious questions as to whether the most common batterer program models are effective (e.g., Dunford 2000; Feder and Dugan 2002; Davis et al. 2000), while the latest literature reviews on the subject have failed to reach a consensus (e.g., see Babcock, Green, and Robie 2004; Bennett and Williams 2004; Feder and Wilson 2005).

2. *Is judicial monitoring more effective if implemented in a more flexible or “graduated” manner?* To date, domestic violence courts characteristically employ post-disposition compliance monitoring at regular intervals, most often on a monthly basis. Could it be, alternatively, that *graduated* monitoring is as or more effective in suppressing recidivism? Graduated monitoring sends a clearer message to offenders that their behavior will be met with appropriate consequences, since the approach rewards those who are meeting their obligations while imposing more stringent requirements on those who are noncompliant. The concept is similar to that used throughout specialized drug or mental health courts, which tend to reduce the frequency of required court appearances as a reward for reaching various milestones, while increasing the frequency of appearances as a sanction in response to noncompliance. Alternatively, it is also plausible that domestic violence offenders respond better not to a graduated approach but to the certainty of knowing that the court will be checking up on them at

regular, frequent intervals. This study also enables us to assess whether a graduated monitoring approach reduces the overall cost to the court system by reducing the total number of court appearances.

3. *Is judicial monitoring more effective than the complete absence thereof?* This is the only research question that cannot be answered using our random assignment design. Instead, it requires comparing the randomly assigned offenders, all of whom received some form of monitoring, to a group of non-randomly assigned offenders who were convicted on equivalent charges but not sentenced to receive *any* monitoring. Nonetheless, the practical significance of this objective is paramount: Since judicial monitoring absorbs significant court resources in the form of collecting and organizing compliance reports from programs and holding extra court appearances, courts would naturally like to know whether their effort affects offender compliance with all sentencing conditions and affects recidivist behavior.

Organization of Report

The first three chapters provide an overview of the study and its purpose, as well as detailing all aspects of the research design and methodology. Chapter Two is a review of the relevant previous literature. Chapter Three describes the research design in detail, including the study setting, design for the primary randomized trial, and design for the quasi-experimental comparison of the randomly assigned offenders to similar offenders not sentenced to *any* form of judicial monitoring.

The next three chapters present the study findings. For offenders involved in the randomized trial, Chapter Four presents the findings based on official records, while Chapter Five presents the findings based on victim interviews. Chapter Six includes the quasi-experimental comparison of outcomes between offenders in the randomized trial and those who received neither a batterer program nor any form of monitoring. In all three of the analysis chapters, results are also reported concerning what additional background characteristics besides study group assignment (e.g., criminal history, demographics, relationship to victim) predict recidivism outcomes. In addition, each of these chapters begins by presenting descriptive data on all study offenders in that chapter's analysis (e.g., criminal history, employment status, living situation, relationship of the parties, and victim injuries). This serves to describe the offender population and to address study validity issues by testing whether the various study groups are in fact comparable, and whether the research design was implemented as planned.

Finally, Chapter Seven reviews the major findings and discusses possible policy implications, drawing on the authors' own perspectives as well as on those of New York City practitioners who attended a discussion held in June of 2005. The conclusion also considers possible next steps for court administrators, researchers, and other stakeholders interested in improving the court response to domestic violence.

Chapter Two

Previous Research on Batterer Programs and Judicial Monitoring

As the criminal justice system has focused greater energy on the development of effective responses to domestic violence, batterer programs have simultaneously come into greater use and come under greater scrutiny. Batterer programs have long been a part of the justice system's response to domestic violence, but only a handful of rigorous evaluations have examined their effects. Even fewer studies have considered the effects of ongoing judicial monitoring, with none utilizing random assignment methods, despite the fact that growing numbers of courts are devoting significant resources to monitoring, both pre- and post-disposition. This chapter reviews what we know to date, and identifies gaps in our understanding.

Batterer Programs

For many years, the research literature on batterer programs has produced inconsistent findings, although as the available studies have grown in sophistication, the possibility that batterer programs might change offender behavior has become increasingly dubious (see Feder and Wilson 2005). Unfortunately, only four randomized trials have previously been conducted. Such trials comprise the most rigorous research design available, since the offenders are assigned strictly at random to receive the intervention or not; hence there is little chance that those receiving the intervention differ in important ways (e.g., that might make them inherently more or less likely to re-offend) from those not receiving it. Of the four completed trials, one found that batterer programs led to lower rates of re-offending, two found that the programs have no effect, and the fourth reported ambiguous findings. This fourth study, co-authored by one of us, found a significant reduction in violence among offenders assigned to a batterer program but no evidence that the program content itself was responsible for the effect, given that the program did not produce any cognitive changes. Research staff speculated that violent behavior was suppressed during this period due not to any impact of the program but to the fact that offenders were under court control – illustrating the importance of carefully distinguishing batterer program effects and judicial monitoring effects in future research.

Despite the inconclusive findings regarding program impacts, there are compelling reasons for courts' use of batterer programs. Victims are often interested in sanctions that provide safety from violence and that, while possibly punitive, do not jeopardize the perpetrator's ability to earn a living. Further, in less serious misdemeanor cases, batterer program mandates provide judges with additional sentencing options; in their absence, offenders often face either very short jail terms or no sanction at all. Hence, there is a clear need to develop effective sanctions, and of those available, batterer programs appear on their face as most relevant to the offense and, therefore, to hold the greatest potential.

At the same time, in explaining the meaning of program "effectiveness," it is not universally defined as producing reductions in recidivism. Many advocates firmly resist the notion that batterer programs can ever be expected to "cure violent men." They argue that rather than rehabilitation, their proper function is accountability: providing a viable punitive option for courts, probation, or other mandating agencies to hold domestic violence offenders accountable

for their violent behavior. In this view, effectiveness would be measured by the degree to which courts enforce batterer program mandates through the imposition of clear and consistent sanctions in response to noncompliance with program rules and procedures. Therefore, while this study investigates whether batterer programs produce reductions in re-abuse, we acknowledge that this is not the only research question one could explore; and not the only one that all practitioners raise about batterer programs.

The Rise of Batterer Programs

The first batterer groups were developed in the late 1970s. Feminists, victim advocates and others realized that providing services to victims of abuse, many of whom remained in the same relationship, did little to end domestic violence (Healey et al. 1998). Group treatment was believed to be more appropriate than individual or couples counseling because it provides peer support for becoming non-abusive (Crowell and Burgess 1996) and, unlike couples counseling, avoids the risk of blaming the victim (e.g., Healey et al. 1998). Groups were also less expensive than one-on-one counseling sessions. The earliest batterer groups were educational groups which promoted an anti-sexist message (Gondolf 1995). Over time, many (but not all programs) incorporated cognitive-behavioral therapeutic techniques and skills-building exercises, including anger management techniques and learning alternative behaviors for addressing conflict (Healey et al. 1998).

As states introduced pro-arrest statutes during the 1980s, the number of batterers arrested and convicted increased, and group programs became the sanction of choice for the courts. Court mandates to batterer programs significantly increased and diversified the number of batterer programs nationally (Feazell, Mayers, and Deschner 1984). One study estimates that approximately 80% of all batterers attending programs were mandated by a court to do so (Healey et al. 1998). Batterer programs may be required by criminal courts as part of a pre-trial diversion, included as a condition of bail, ordered by judges as part of a sentence, or imposed by probation (e.g., Hamberger and Hastings 1993). In at least one major urban jurisdiction, the district attorney sometimes agrees not to file charges if a brief program is completed (Davis and Smith 1997); and in another jurisdiction, the court will reduce a misdemeanor conviction to a violation if the offender completes a batterer program (Gavin and Puffett 2005). (A violation is not defined as a crime in that state's penal law and, as such, does not create a public conviction record for the offender.) In yet other jurisdictions, civil as well as criminal courts may mandate a batterer to a program, for instance, as a condition of custody or visitation (Ganley 1987). Probation departments run many batterer programs; mental health clinics, family service organizations, and victim service organizations run others. In total, a recent review estimates that there are approximately 2,500 batterer programs operating throughout the country (Labriola 2005).

Although some states are developing guidelines regarding program content and length (Gondolf 1995), with most promoting psycho-educational approaches that originated with the Duluth model, others hold that a "one size fits all" approach may fail to recognize the diversity of batterers (Healey et al. 1998). Thus there is a simultaneous trend in some circles to tailor programs to different batterer types defined by personality, violence history, or substance abuse. Some programs have been specially designed to accommodate socio-cultural differences among batterers, such as poverty, race, ethnicity and sexual orientation. However, a recent study randomly assigning African-American men to (1) conventional group sessions for a racially-mixed group, (2) conventional group sessions for African-American men only, and (3)

culturally-focused sessions for African-American men only found no positive effects on either program completion or re-offending rates of tailoring the program to an African-American clientele (Gondolf 2005).

Early Research Testing the Efficacy of Batterer Programs

Over the last two decades, many empirical studies on batterer programs have been conducted. In the 1980s and 1990s, at least seven reviews of over 35 single-site evaluations were published (Davis and Taylor 1999; Eisikovits and Edleson 1989; Gondolf 1991, 1995; Rosenfield 1992; Saunders 1996; Tolman and Bennett 1990), as well as eight book chapters reviewing this same research (e.g., Hamberger and Hastings 1993; Crowell and Burgess 1996; Dobash, Dobash, Cavanagh and Lewis 1995; Dutton 1988, 1995; Rosenbaum and O'Leary 1986; Saunders and Azar 1989; Tolman and Edleson 1995). Since the available reviews offer differing conclusions, ranging from positive to slightly positive to inconclusive, they have not necessarily brought the field to a consensus (see the most recent review, Feder and Wilson 2005).

The individual studies that are the subject of these reviews represent three generations of research, marked by a movement toward more rigorous science. The older first generation studies utilized research designs that lacked appropriate comparison groups. They encompassed: (a) studies which assessed abusive behavior among mandated offenders only after completing the batterer program (e.g., Feazel, Mayers, and Deschner 1984); (b) studies which measured violence among the same set of offenders before and after program participation (Dutton 1986, study 1); and (c) studies which compared the violence of offenders who completed the programs with offenders who failed to attend or failed to complete the programs (e.g., Hamberger and Hastings 1989).

None of these older research designs can make definitive claims regarding differences between offenders receiving and not receiving the intervention, since none involve a comparison of outcomes between complete samples of offenders mandated to a batterer program (whether or not they completed) and similar offenders not mandated to a program. In this regard, while it may be useful to know that program completers have better outcomes than non-completers or than a comparison group, since courts are initially targeting *all* of the offenders they mandate for an intervention, the more relevant policy question is whether or not mandating all of those offenders in the first place worked on average. Furthermore, program completers may be inherently more prone to compliance and hence less likely to re-offend than non-completers with or without having been ordered to a batterer program, so the isolation of results for completers is simply not a credible test of an intervention. Concerning the research design that tracks the same pool of offenders before and after batterer program participation, the mere fact of a reduction in offenders' violence after batterer program participation is also not proof that the program was responsible for the change; this is particularly true since criminal justice research generally finds that criminality peaks in late adolescence and then gradually recedes over the adult life course, as offenders grow older (e.g., Farrington 1986; Hirschi and Gottfredson 1983).

The second generation of studies used quasi-experimental designs in which offenders assigned to batterer programs were compared with offenders whose sentences did not include programs. In the most rigorous of these studies, Harrell (1991) capitalized on natural variation among judges in the propensity to sentence men to a batterer program. She observed that some judges frequently mandated convicted offenders to programs, while others never did so. She argued that, since assignment to a program was dependent upon which judge did the sentencing (essentially a random event), it was unlikely that there would be systematic pre-treatment

differences between the men mandated to a program and those who received other sentences. Such quasi-experimental designs overcome some of the objections to the earlier generations of studies, but still are subject to the criticism that treatment effects may be confounded with pre-existing differences between treatment groups. (In fact, Harrell found that the men mandated to a batterer program were more likely to be married and employed than those who received other sentences.)

Randomized Trials Testing the Efficacy of Batterer Programs

Although in a review of the batterer program literature from just six years ago (Davis and Taylor 1999), one of us reported that studies to date suggested batterer programs were effective, as results subsequently came in from the randomized trials, this conclusion appeared to be premature. Randomized trials constitute the “gold standard” in social science research, because the background characteristics of the group that receives an intervention and the control group are held constant; all study participants, regardless of their demographics, psychosocial background, criminal history, or other attributes are equally likely to be randomly assigned to each study group. Hence if the group receiving the intervention has better outcomes than the control group, it is safe to conclude that the intervention alone must have caused the difference.

Despite the obvious difficulties in obtaining the agreement of courts and other stakeholders to randomly assign offenders to receive and not to receive an intervention thought to be effective, four such trials have previously been conducted. The first, in Hamilton, Ontario, assigned 59 men convicted of domestic violence either to probation and a 10-week batterer program or to probation with no program (Palmer et al. 1992). Re-arrest records showed that 3 of the 30 men (10%) assigned to the program re-offended over a one-year period, compared with 8 of the 26 men (31%) receiving probation only. Although these findings appear to provide strong support for the effectiveness of batterer programs, the study methodology has been subject to substantial criticism. First, the batterer programs involved only 15 hours of classes, a small number compared with general nationwide practice. Second, although the results were statistically significant, the small number of study participants raises serious unanswered questions concerning the random assignment process: Were large numbers of offenders systematically excluded from the study due either to the subjective preferences of court stakeholders (judge, prosecutor, defense attorney, or others) or to the offender’s ability to choose whether or not to participate? If so, the study’s positive findings may in fact be applicable to only a narrow offender sub-population (see critique in Feder and Dugan 2002).

A second trial involved personnel at the Navy base in San Diego (Dunford 2000). In this study, 861 men who were convicted of assaulting their partners were randomly assigned to four conditions: (a) six months of weekly cognitive-behavioral treatment, followed by 6 months of monthly group sessions; (b) six months of weekly group sessions for couples, followed by 6 months of monthly group sessions; (c) a rigorous monitoring and case management regimen similar to probation; or (d) safety planning for victims, similar to that done by victim advocates. The study found no significant differences in recidivism outcomes across the four groups. The main drawback to this study is the characteristics of the men, who consisted exclusively of Navy personnel, and excluded those with substance abuse problems, mental health disorders, and prior criminal records, as well as unmarried and unemployed men. In short, the study focused on a relatively “low risk” offender population, although batterer programs may well have more positive effects with “high risk” offenders; by definition, high-risk offenders start out with a

greater propensity to commit future criminal acts and thus offer a greater opportunity for an intervention to make a real difference in curbing future crime.

A third trial involved 404 offenders convicted in a misdemeanor domestic violence court in Broward County, Florida (Feder and Dugan 2002). These offenders were randomly assigned either to probation and a 26-week Duluth-model batterer program or to probation only. At one-year follow-up, there were no differences between batterer program participants and the control group on measures of attitudes toward women, beliefs about wife-beating, attitudes toward treating domestic violence as a crime, and victim or official reports of recidivism. Instead, for both groups, the primary predictors of lower recidivism were “stake-in-conformity” variables, including length of employment, residential stability, older age, and marital status. The theory behind the importance of these variables is that those with more to lose from arrest or incarceration are more likely to remain in compliance with court orders (see Sherman and Smith 1992). This study arguably had the strongest research design of the first three, since all convicted misdemeanor offenders in Broward County entered the random assignment process with extremely limited exclusions. (Offenders were excluded from the study only if they or the victim were under the age of the 18; they or the victim spoke neither English nor Spanish; they had a severe mental illness; or the judge allowed them to move to another jurisdiction to serve their probation sentence.) Also, the intensity of probation monitoring was identical between the batterer program and the probation-only groups.

Contrasting with these last two negative results for the impact of batterer programs, a fourth randomized trial conducted in Brooklyn, New York appeared to detect a large and positive impact (Davis et al. 2000). This study randomly assigned 376 misdemeanor domestic violence offenders to either a batterer program or 40 hours of community service. However, despite the appearance of significantly lower re-offending rates among those assigned to the batterer program both at six months and one year after sentencing, closer inspection of the data revealed three complicating factors. First, while there was a strong effect of *assignment* to a batterer program, there was no effect of actually attending it. Among assigned men, those who completed their batterer program were no less violent than those who attended only some group sessions or who never attended a single session. Second, due to fortuitous circumstances, the treatment sample was split into two sub-samples distinguished by density of group sessions. All offenders randomly assigned to programs were mandated to attend 39 hours of a psycho-educational group intervention. However, some received the 39 hours in 26 weekly sessions while others received it in longer biweekly sessions over 8 weeks. The former treatment model maximized the time that batterers remained in the program while the latter reduced the chances that offenders’ initial motivation to attend would flag over time. The results showed that more men successfully completed the 8-week group than the 26-week group. Since they received a higher dosage of the treatment, it was therefore expected that men assigned to the 8-week group would have a lower recidivism rate. Instead, only those in the 26-week group had a lower recidivism rate than the control group; the 8-week group did not differ from the control group. Third, the Brooklyn experiment found no evidence of cognitive change as a result of the batterer program. Although there are a number of different batterer program models, all assume that they produce cognitive or attitudinal changes that precede a reduction in the frequency of violent acts (e.g., see Healey et al. 1998). If cognitive change did *not* occur among batterers assigned to programs, then what accounts for their lower rate of violence?

Batterer programs can be looked upon in one of two ways. They may stimulate a learning process in which attitudes and behaviors are modified in a relatively permanent way. Or they

may suppress violent behavior for the duration of the intervention, without producing permanent changes. The results of the Brooklyn study do not support the model of the intervention as a change process. If the intervention were truly effecting change, then we would expect (a) more sessions attended would lead to less future violence, (b) no difference in violence between 8-week and 26-week treatment programs, and (c) some evidence of cognitive change. By default, the results, therefore, support the suppression model of batterer programs. That is, men assigned to the intervention were less violent while under court control. Also supporting this model is the fact that those mandated to the intervention after taking a plea were less violent than those mandated under a pre-plea diversion agreement. Those mandated under a diversion agreement were under weaker court control, since they would still have to be adjudicated as guilty before the court could impose a different sentence for a failure to complete the program. Since the study did not include a rigorous, experimental design for testing the learning and suppression models, the data on this point is suggestive but not conclusive.

The current study sought to provide evidence to determine whether cognitive-behavioral change or suppression of abusive behavior through monitoring is key to lowering recidivism. We planned a randomized trial to test whether batterer programs used in conjunction with judicial monitoring added any protective benefit beyond that provided by judicial monitoring alone. A second quasi-experiment tested whether offenders in the trial, all of whom were required to report to a judicial hearing officer for some form of monitoring, re-offended at a lower rate than a second pool not subject to any monitoring.

Judicial Monitoring

Although the Brooklyn study just cited suggests that judicial monitoring may suppress violent behavior among domestic violence offenders, the literature on supervision of offenders on probation and parole generally finds that efforts to surveil or control offenders are ineffective (see Sherman, Gottfredson, Mackenzie, Eck, Reuter, and Bushway 1997; Petersilia 1999; Mackenzie 2000). However, results on the effectiveness of supervision are far from conclusive; a recently published study evaluating the effectiveness of a specialized domestic violence probation supervision unit in Rhode Island found that it produced significantly lower rates of re-offending and arrest-free periods compared with probationers receiving traditional supervision (Klein, Wilson, Crowe, and DeMichele 2005). Taxman (2002) proposes a new model of supervision and suggests that, in order to work consistently, efforts to surveil and control must be accompanied by evidence-based therapeutic practices that engage the offender in a process of change; whereas approaches based upon surveillance alone, though more prevalent in practice, have been found largely ineffective.

There is little research specifically examining the efficacy of *judicial* monitoring practices with domestic violence offenders (as opposed to probation-based or other forms of monitoring); and there have not been any such studies utilizing a rigorous experimental design. Perhaps the most suggestive study on the subject was one focusing on four domestic violence courts in the San Diego Superior Court system (San Diego Superior Court 2000). The study reported two central findings when comparing the periods before and after implementation of the domestic violence courts. First, after implementation, there was increased attendance at required counseling sessions and an increased ability to detect and respond to violations of court orders; and second, the re-arrest rate within one year of the initial arrest dropped from 21% to 14%. The authors attribute these positive findings to the domestic violence court practice of requiring

offenders to attend post-dispositional court hearings for compliance monitoring. Hence the theory is that the institution of a judicial monitoring regimen led to positive compliance and recidivism outcomes, although this causal relationship was not rigorously tested. One other study also points to the role of mandatory compliance hearings in producing increased batterer program completion rates (Gondolf 1998). In this study, conducted at the Pittsburgh, Pennsylvania Domestic Violence Court, batterer program completion rates were assessed before and after the court introduced a mandatory court appearance 30 days following the imposition of a batterer program mandate. With the compliance hearing, the rate of program completion rose from just under half to 65%.

In addition, an Urban Institute study of the Judicial Oversight Demonstration Project, which is now nearing completion, includes an impact evaluation of three specialized courts that use both batterer programs and intensive judicial monitoring. Through data analyses and follow-up interviews with victims and defendants involved and not involved with these three courts, the study proposes to examine the impact of these courts – and their various practices – on offender accountability, victim safety and recidivism. When this study is completed, it may be able to test the impact of various monitoring practices instituted within specialized domestic violence courts (e.g., routine compliance monitoring, and imposition of swift and certain sanctions in response to noncompliance). Our own study complements the multi-site Judicial Oversight Demonstration Project by exploring the effects of different schedules of intensive judicial monitoring, and by distinguishing those monitoring effects from others related to attendance at batterer programs.

At present, lacking a compelling prior research literature with domestic violence offenders, much of what we already know concerning the impact of judicial monitoring stems instead from research on drug treatment courts, designed to serve nonviolent substance-abusing offenders. Some of this research seems to confirm that monitoring is effective. A random assignment study of a Washington, D.C. program found that defendants subject to drug testing coupled with ongoing judicial monitoring and graduated sanctions imposed in response to noncompliance produced reductions in drug abuse and recidivism when compared with defendants assigned to drug testing only, without monitoring or sanctions (Harrell, Cavanagh, and Roman 1998). More recently, a series of randomized trials with drug court participants at multiple northeastern sites found that biweekly judicial status hearings produce better treatment outcomes than “as needed” hearings with “high-risk” participants (defined by the presence of anti-social personality disorder and/or previous failure in drug treatment); but biweekly hearings do not produce significantly different outcomes with “low-risk” participants (e.g., Festinger, Marlowe, Lee, Bovasso, and McLellan 2002; and Marlowe, Festinger, Lee, Schepise, Hazzard, Merrill, Mulvaney, and McLellan 2003). While this second set of results offers a less universally positive conclusion on the efficacy of judicial monitoring, it too finds that monitoring works, at least with a significant sub-category of substance-abusing offenders.

Clearly, domestic violence offenders differ from nonviolent drug offenders, leaving unclear whether the preceding findings are applicable. Nonetheless, the drug court studies are suggestive enough to encourage investigation of the effects of judicial monitoring on different populations.

Summary

In spite of the substantial resources invested in assessing whether batterer programs achieve the goal of rehabilitation, prior results seem to have left us no closer to the answer than we were ten years ago. By one measure, while two randomized trials found that batterer programs work

(the Hamilton and Brooklyn studies), two found that they did not (the San Diego Navy Base and Broward studies), leading two of the most recent literature reviews to conclude that overall, batterer programs have small but positive effects (Babcock, Green, and Robie 2004; and Bennett and Williams 2004). But by another measure, the Brooklyn results, interpreted as positive in these reviews, were in fact ambiguous at best once the findings were carefully interpreted; and the only remaining positive results were based on a sample of merely 59 men in Hamilton, Ontario. These and other considerations led a third in the most recent series of literature reviews to offer a more skeptical assessment (Feder and Wilson 2005). This latest review also points out that while the average effect size across all of the completed randomized trials is slightly positive when relying on official re-arrest reports, the average effect size when relying on victim reports of future abuse is zero.

As for the impact of judicial monitoring, the question has yet to be rigorously examined with a domestic violence offender population. As a starting point, the San Diego and Pittsburgh domestic violence court studies, as well as the growing literature focused on substance-abusing offenders, suggest that it is reasonable to hypothesize, first, that monitoring inhibits re-offending and, second, that monitoring may be particularly effective with certain categories of offenders, since monitoring appeared to work better with “high risk” offenders in drug courts. The present study arises out of this literature in an effort to test and carefully distinguish the effects of both batterer programs and judicial monitoring in a single study with a domestic violence offender population.

Chapter Three

Research Design and Methodology

This study was designed to distinguish the effects of batterer programs and judicial monitoring on the future violent behavior of domestic violence offenders. Offenders convicted of intimate partner violence were randomly assigned to a batterer program or not; and to either monthly or graduated monitoring, with the latter involving reduced court appearances in response to compliance and increased appearances in response to noncompliance. Since all offenders received some form of monitoring, this experiment was designed to determine conclusively whether mandatory attendance at batterer programs produces any added protective value besides that which monitoring already produces. However, precisely because monitoring was universal, the experiment could not determine whether its presence produces better outcomes than its complete absence. Accordingly, a secondary, quasi-experimental study was also implemented, in which recidivism outcomes were compared between the randomly assigned offenders and a control group consisting of offenders convicted of identical offenses, but who, as a result of the normal sentencing process, received neither a batterer program nor any form of monitoring.

This chapter describes the research design and methods for all study components and describes the specific protocols for implementing the randomized trial. The three analysis chapters that follow all begin by testing the performance of the research design – in other words, by assessing the internal and external validity that can be ascribed to the results.

Setting: The Bronx Misdemeanor Domestic Violence Court

The Bronx Misdemeanor Domestic Violence Court (BxMDVC) was established in 1998 by the New York State Unified Court System and the Center for Court Innovation to create an effective, coordinated response to misdemeanor domestic violence crime in the Bronx.¹ The Bronx is a large urban jurisdiction with a population of 1.3 million residents. It has the highest poverty rate of the five boroughs of New York City (31%), the lowest per capita income (\$13,959), and the lowest percentage of residents with a high school diploma (62%). The population is racially and ethnically diverse, as the racial distribution is 38% Black, 32% Caucasian, and 32% other (mostly Hispanic); and 51% expressly identify as Hispanic/Latino regardless of their race (U.S. Census 2000).

The BxMDVC handles all misdemeanor domestic violence cases beginning with their first court appearance after central arraignment. Most cases involve intimate partners, although approximately one-fifth involve friends, siblings, parents, or other domestic relationships. The

¹ The Center for Court Innovation helped to create the Bronx Misdemeanor Domestic Violence Court in 1998 but played no programmatic role during the period when this study was conducted. On a separate matter, it is notable that in November 2004, *after* the period of study enrollment, the Bronx experienced a major court restructuring process merging the Criminal (misdemeanor) and Supreme (felony) Courts into a single countywide Criminal Division. As part of this merger, a single Domestic Violence Court was created to handle both misdemeanor and felony cases. Otherwise, the structure and staffing of the reconstituted domestic violence court remained essentially unchanged.

court's caseload is extremely high, as 5,491 domestic violence cases were arraigned in 2002 and 5,714 in 2003. At any time, the BxMDVC has approximately 2,500 open (pre-disposition) cases and monitors approximately 500 offenders sentenced to attend batterer or other programs.

To handle its caseload, the BxMDVC is staffed by dedicated judges, prosecutors, court staff, and case management staff, all of whom receive expert training in issues pertaining to domestic violence. A team of victim advocates is also available to link victims to services and to help them navigate the court process. The BxMDVC has established close partnerships with community-based batterer programs and other service providers.

The court itself consists of three parts: one for pre-trial appearances (AP10), one for trials (TAP2), and one devoted exclusively to post-sentence compliance monitoring (DVC). In AP10, the part which handles the majority of the pending caseload, a resource coordinator works closely with the judge, attorneys, and victim advocates to gather and disseminate information. If a sentence involving a program mandate is under consideration, the resource coordinator can inform the judge of an appropriate program (e.g., based on the offender's work hours, income, or language needs). The court is also staffed by two defendant monitors, who place offenders in specific programs and monitor their compliance.

Of 6,467 domestic violence cases disposed in 2002, 49% were convicted, 45% were dismissed or acquitted, 3% were adjourned in contemplation of dismissal (meaning the case will be dismissed if the defendant does not commit any further crimes during a certain period, not exceeding one year), and 3% received various other dispositions. Of those convicted, 39% were sentenced to jail, 56% were sentenced to a conditional discharge, and 6% received other sentences including probation, community service, or a fine (see Gavin and Puffett 2005). In addition, all sentenced offenders receive a final order of protection, usually a one-year "full" stay-away order precluding any contact with the victim; in some cases, it is stipulated that attending a program will lead the order to be changed to "limited," which does allow nonviolent contact with the victim.

Of those sentenced to a conditional discharge and a batterer program during the first six months of 2002, just prior to study implementation, 18% were convicted at the misdemeanor level, and 82% pled down to a violation, usually harassment, which is not a crime under New York State penal law and therefore will not appear on the offender's public criminal record. Overall, approximately half of the offenders sentenced to a conditional discharge are ordered to programs. Of those, in the first six months of 2002, 85% were ordered to a batterer program (a percent that is higher than in the years that preceded); the remaining 15% were mostly ordered to alcohol or substance abuse treatment, while some were ordered to other types of programs, such as mental health treatment, parenting classes, or an teen accountability program for domestic violence offenders designed for offenders ages 16-19 (Domestic Violence Court Application 2002).

Immediately after sentencing, offenders ordered to a program are sent to the defendant monitors' office, located on the same floor as the BxMDVC court parts. One of two defendant monitors conducts a brief screening and then schedules the offender for intake at an appropriate program. This appointment always precedes the offender's first one-month appearance in the compliance part. The large majority of offenders ordered to a batterer program are assigned to one of two 26-week educational programs. (See below for discussion of the defendant monitors' criteria for assigning offenders to specific programs and each program's curriculum.)

All programs provide the defendant monitors with monthly reports detailing attendance history, explaining any "excused" absences (e.g., due to illness or family emergency), and

reporting any behavioral problems. The defendant monitor provides all relevant information to the Judicial Hearing Officer (JHO), usually a retired judge, presiding in the compliance part. At monthly court appearances, the JHO can take appropriate action in response to the compliance reports. This can include referring the case back to the sentencing judge in either AP10 or TAP2 for review and possible sanctions. For offenders seriously out of compliance, the sentencing judge may sentence the offender to a jail alternative based on a failure to fulfill the conditional discharge. The maximum jail alternatives are one year for those convicted of an “A” misdemeanor, 90 days for those convicted of a “B” (less serious) misdemeanor, and 15 days for those convicted of a violation.

Research Design and Implementation of the Randomized Trial

Overview of the Design

The two by two factorial design called for random assignment of convicted male domestic violence offenders into four experimental conditions: (1) batterer program plus monthly judicial monitoring, (2) batterer program plus graduated monitoring, (3) monthly monitoring only, and (4) graduated monitoring only. Study enrollment took place over nineteen months from July 23, 2002 through February 27, 2004. Each cell was originally intended to have 200 men assigned, although this number was subsequently reduced (see below). We tracked the men for at least twelve months after sentencing, and for up to eighteen months for most of the men, to determine whether they fulfilled the conditions of their sentence, were re-arrested for domestic violence, or were reported by the victim to have engaged in new incidents of abuse. The final study implementation plan is in Appendix A.

	Batterer Program	No Batterer Program
Monthly Monitoring	N = 102	N = 109
Graduated Monitoring	N = 100	N = 109

The four conditions were defined as follows:

1. *Batterer program/monthly monitoring*: Offenders in this group were assigned to attend a batterer program and to make *monthly* appearances before the Judicial Hearing Officer (JHO) in court part DVC. The JHO monitored the defendant’s progress in the program, compliance with orders of protection, avoidance of re-arrests, and compliance with other conditions imposed by the sentencing judge.

2. *Batterer program/graduated monitoring*: Offenders in this group were assigned to attend a batterer program as above. They were also required to appear before the JHO, but on a graduated schedule. Those *in compliance* throughout the monitoring period were required to report to DVC at four weeks post-sentence for their initial appearance, and then at 10 weeks, 18 weeks, and 26

weeks. Those *out of compliance* at any time were required to reappear in DVC two weeks and then four weeks after the determination of noncompliance; they were then returned to the graduated schedule.

3. *Monthly monitoring only*: Offenders in this group were required to appear monthly before the JHO, as in the first cell, but were not required to attend a batterer program. The JHO monitored attendance at scheduled court appearances and compliance with all existing orders of protection, and would respond to any serious infraction or re-arrest as with the first group.

4. *Graduated monitoring only*: Offenders in this group were required to appear for graduated monitoring before the JHO (as with the second group), but were not required to attend a batterer program. Also, as with the third group, compliance monitoring focused on attendance at scheduled court appearances, compliance with orders of protection, and avoidance of re-arrests.

Sampling Frame

Eligible offenders were convicted of a violation and sentenced to a conditional discharge in court parts AP10 or TAP2 of the Bronx Misdemeanor Domestic Violence Court, with acceptance of a batterer program condition by both prosecution and defense prior to randomization.² The sentencing judge retained the discretion to exclude particular offenders from randomization.

The Random Assignment and Intake Process

The process of random assignment began once the prosecutor and defense attorney in court parts AP10 or TAP2 indicated that they were prepared to accept a plea to a violation and a sentence of a conditional discharge, order of protection, and a batterer program with monthly monitoring (the preexisting status quo). Provided that the judge did not choose to exclude the case from the study, the judge then reads from a standard study allocation script that the judges in the domestic violence court parts themselves developed (see Appendix B). The script described in detail each of the four groups to which the defendant could be sentenced. While the original script was a two-page, five-minute long explanation, it was abbreviated over time, with each judge making personal revisions.

If the defendant agreed to accept the offer at this point, understanding that there were four possible sentencing outcomes, a bench conference was held. During this conference, either the resource coordinator or the project senior research associate performed the random assignment. This was done by shaking and then removing from a cup one of four pieces of folded paper on which were written numbers 1 through 4, representing the four study groups. Following the bench conference, the judge proceeded on record to sentence the defendant.

Before the defense attorney and defendant left the courtroom, the resource coordinator or research associate gave each a sheet, available in English and Spanish, summarizing the offender's responsibilities (see Appendix C). After receiving other paperwork from the court, offenders assigned to the two groups required to attend a batterer program proceeded to the defendant monitors' office. One of the defendant monitors conducted a brief intake interview and assigned the offender to one of the two primary batterer programs used by the court. The research associate also created a research case file including the date of the first scheduled

² A small proportion of misdemeanor domestic violence cases in the Bronx are handled in a separate "Integrated Domestic Violence Court." This court is designed for when parties have simultaneously occurring criminal as well as family or matrimonial cases pending; cases processed in the IDV court were not included in the study.

appearance in DVC, case and offender identifiers, charge information, disposition, sentencing date, experimental assignment, victim name, and victim contact information.

Batterer Program Curricula

Offenders assigned to attend a batterer program (groups 1 and 2) were mandated to either the Domestic Violence Accountability Program (DVAP) run by Safe Horizon or the Men's Choices Program run by the Fordham Tremont Community Mental Health Center. DVAP only offers night classes and does not accept insurance; although DVAP has a sliding scale for payment, offenders that do not have regular income or who work at night are assigned to Fordham Tremont. In addition, DVAP offers classes in Spanish and English while Fordham Tremont only has English classes available.

Both DVAP and Fordham Tremont are 26 weeks in length, with classes meeting weekly for 75 minutes. Both accept new participants on a rotating basis so that, in any given session, some men are beginning the program and some are completing. In addition, both programs allow for three unexcused absences in total; failure occurs after the third unexcused absence or after two consecutive unexcused absences.

Concerning program philosophy and curricula, both programs have similar foundational philosophies as the Duluth model of batterer programs for small groups of men. The Duluth model was designed in 1981 as a coordinated community response of law enforcement, criminal and civil courts, human service providers and all other systems of our society to work together to make communities safer for victims and to hold offenders accountable for their behavior. This model assumes that battering is rooted in societal norms that support male abuse of women. It also assumes that abuse is an instrumental behavior used by males to gain or retain power over women. Changing participant attitudes toward women and sex roles is seen as key to reducing abusive behavior. The batterer program curriculum is educational in nature and includes: defining and identifying domestic violence, understanding the historical and cultural aspects of domestic abuse, stress reduction, anger management, facilitating communication, and ultimately creating healthier relationships with partners and families. Through a combination of instruction and discussion, participants are encouraged to take responsibility for their anger, actions, and reactions.

While there are many similarities between the DVAP and Fordham Tremont programs, there are some differences as well. At DVAP, the focus of all 26 weeks is on education of the participant. The program consists of six modules: defining and understanding domestic violence, history of violence against women, oppression and the abuse of privilege, male socialization, effects of domestic violence on children, and making accountable choices. Important to note, DVAP does not take responsibility for changing individual behavior. The program's philosophy is that only the individual participant can be responsible for his personal growth, development and change, by holding himself accountable.

Fordham Tremont also attempts to re-educate participants, but adds a substantial module aimed at behavior modification. This component includes work on recognizing and controlling anger and ways to improve intimate partner communication. Fordham Tremont's is a psycho-educational program that focuses on issues of power and control as well as on identifying individual abusive behaviors and providing suggestions for alternative action. Multiple sessions focus on recognizing emotions, identifying early warning signs of abusive behavior, and managing appropriate responses.

Judicial Monitoring Practices

At each appearance in the compliance part, the JHO reviewed the offender's responsibilities orally, summarizing the terms of the conditional discharge and noting the results of the criminal history check performed just prior to the appearance. The JHO reminded offenders that they were subject to an order of protection and that they had to stay away from the victim, or "behave themselves" if the order permitted limited contact. If the offenders had child visitation rights, the JHO reminded them that they must return to Family Court if they wish to increase visitation privileges. The JHO also noted whether court surcharges had been paid and admonished offenders who had not yet paid their fees.

For those assigned to a program, the JHO noted the number of sessions attended or missed out of the total number required. For example, "You have attended seven out of 26 sessions, with no absences," or "You have four absences." If the offender missed one or more sessions, there was often a discussion about the number of sessions missed or whether the absences were excused. The JHO reminded offenders that he was the only one who could excuse them from attending a session. For this discourse, the JHO relied on information provided by the programs, which appeared to be accurate and current to within a few days of the appearance. The JHO sometimes warned offenders of the possible consequences of too many absences, including jail time.

For those on a monthly monitoring schedule, the JHO also noted that the offender must report back to court monthly. For those on a graduated schedule, the JHO typically stated, "You are in the graduated monitoring program," and stated that the time between court appearances would vary depending on compliance, with language such as: "If you're in compliance, I'll give you a long date or if you're not in compliance, I'll give you a short date, possibly every other week."

All of the above was restated at each monitoring appearance, though after the first, slightly less explanation was offered, and the JHO sometimes neglected to mention certain points (e.g., often not giving as clear an explanation of the monitoring responsibilities). The JHO concluded each appearance by informing offenders of the next DVC appearance date, which was based on a schedule provided by the senior research associate. For offenders who had successfully discharged their obligations to the court, the JHO acknowledged them for satisfying the monitoring and/or program attendance requirements. For returning offenders, the JHO concluded the appearance by asking if they had any questions. (Questions might include how many program sessions remained, or if the offender's next appearance could be scheduled at a more convenient time. The JHO would accommodate some such requests, for example for an afternoon as opposed to a morning appearance time.)

Certain offender behaviors, including three absences from a batterer program or disruptive behavior at the program, triggered a warning from the JHO. In cases of more serious misconduct, the JHO sent the offender back to the sentencing judge for an appearance later that same day. This occurred most often in response to a termination from the mandated program; failure to appear in DVC as scheduled; re-arrest; violation of an order of protection; or allegations by the victim in a conversation with Safe Horizon staff. Sentencing judges then had the options of resentencing the offender or continuing the current sentence. The JHO did not discuss any allegations during the monitoring session, but simply informed the offender that a question arose about his compliance and he should sit down for now (until a court officer brought the offender to court parts AP10 or TAP2).

The DVC courtroom was relatively small, making most judicial interactions audible to other offenders waiting for their appearance. Some offenders present were not part of the experiment, because they were sentenced before or after the study enrollment period, pled to a misdemeanor, or were mandated to a different type of program, such as substance abuse treatment. Monitoring practices for these offenders were largely identical to the above, except that none of them were on a graduated monitoring schedule, and the research associate did not assist the JHO in determining the next court date.

Sample Size

As noted above, the target sample size was 800 cases. However, intake projections proved to be inflated and sample intake proceeded far more slowly than expected. The inflation was due primarily to incorrect information, which had indicated that nearly all offenders sentenced to a conditional discharge were also required to attend a program (as we later determined, a program mandate was only added to approximately half of all conditional discharges). Intake was further hindered by an unforeseen reduction in domestic violence arraignments during the study period. Thus, shortly after implementation, it became clear that the goal of 800 cases was not attainable. Power calculations suggested that a sample size of 400 would suffice to examine the main effects of program mandate (batterer program or no program) and judicial monitoring schedule (monthly or graduated). The only planned statistical test for which power would be unacceptably low was the interaction between program mandate and monitoring schedule; i.e., whether assignment to a batterer program exerted a relatively greater effect upon recidivism under either a monthly or graduated schedule. Since we had no reason to expect that such interaction effects would occur, a sample size of 400-500 was deemed acceptable.

During the sampling period, 489 defendants were identified as eligible for the study. Judges used their discretion to exclude 69 or 14% of this eligible pool, yielding a final N of 420 offenders. The final breakdown was as follows:

Group 1: Batterer program and monthly monitoring (N=102);

Group 2: Batter program and graduated monitoring (N=100);

Group 3: Monthly monitoring only (N=109); and

Group 4: Graduated monitoring only (N=109).

Other Implementation Issues and Changes

Approval for the study had been obtained from the Chief Administrative Judge of the New York City Criminal Court prior to submission of the application to NIJ. Upon receiving funding, we still needed to obtain the support of the Bronx judges who would be directly involved with the study; the Bronx District Attorney; and the Legal Aid Society and Bronx Defenders, who represent the vast majority of Bronx defendants. This was done through individual meetings with each of the organizations and individuals involved, followed by a series of group meetings bringing all of the interested parties to the table. (See description of stakeholders in Appendix D.) At these meetings, we discussed the details of the random assignment process and other difficult procedural questions. Several notable issues and changes were as follows.

1. Charge severity: In the original proposal, any defendant prepared to accept a plea with a conditional discharge and a batterer program mandate was eligible for the study. After meeting with the District Attorney's Office, however, eligibility was restricted only to those pleading guilty to a violation. This modification was believed to ensure victim safety by requiring that

more violent defendants, those misdemeanants who were unable to plead to a reduced charge, to receive what the D.A.'s office originally deemed appropriate. Based on data from the first half of 2002, only 18% of the originally proposed sample was lost due to this restriction.

2. *Random assignment protocol:* Several protocols were considered, each with advantages and disadvantages. If the random assignment was performed at the point of sentencing, the judge, prosecutor, and defense attorney would all have to agree to it and take the time during the court session to implement it. If the assignment was performed prior to sentencing, there would still be time for the prosecutor or defense attorney to back out (e.g., if the luck of the draw led to an unfavorable random assignment). If the assignment was performed after sentencing, issues of due process and judicial accountability would arise, since the sentence imposed by the judge in court would not have clearly outlined the exact sentencing conditions; instead, those would have to be assigned subsequently by non-judicial staff, off the record. While it was originally proposed that the random assignment would occur after sentencing, for due process reasons, the judges felt uncomfortable with this plan and indicated a willingness to take the time to perform the random assignment in court. Thus the protocol described above reflects the judges' preferences and input.

3. *Victim contacts:* Both the District Attorney's Office and Safe Horizon, the victim advocacy organization serving victims in the Bronx, were concerned that since half of the offenders in the study would not be assigned to a batterer program, additional steps should be taken to monitor victim safety. Consequently, Safe Horizon victim advocacy staff agreed to call victims at two points – within the first month after sentencing, and then again between the third and fourth months post-sentence – in order to determine whether the victim had suffered any new abuse or threats of abuse from the offender. If there was any reason to suspect that the victim was in danger, Safe Horizon staff would notify the D.A.'s office with the victim's consent. Additionally, at the second contact, Safe Horizon staff asked if the victim was willing to participate in a research interview at one year post-sentence. If the answer was yes, Safe Horizon staff verified and updated the victim's contact information. (Safe Horizon's research department was subsequently responsible for administering the interviews.) Safe Horizon's advocacy staff successfully contacted 24% of victims within one month post-sentence and 13% between three and four months post-sentence. These low contact rates were due largely to a lack of contact information for many victims, and to limited staff resources in Safe Horizon's Bronx office, which restricted the agency's ability to make repeated follow-up calls after the first two.³ In two cases, Safe Horizon staff members did inform the D.A.'s office with the victim's consent of reports of new abuse and subsequent action was taken.

4. *Compliance checks:* Several stakeholders were concerned that, for offenders assigned to monitoring without a batterer program, there would be limitations on what monitoring could involve; for those assigned to programs, monitoring consists primarily of checking on program attendance and compliance. Therefore, the day prior to each court appearance, the senior research associate checked the New York State criminal arrest database, the Criminal Records and Information Management System (CRIMS), for any new arrests in the five boroughs of New

³ Very low contact and retention rates for both perpetrators and victims of domestic violence are extremely common in the literature; see discussion in Dutton, Holtzworth-Munroe, Jouriles, McDonald, Krishnan, McFarlane and Sullivan 2003; or Gondolf and Deemer 2004.

York City; and the New York State Domestic Violence Registry for any new orders of protection issued by other judges in the city. These checks added to the compliance information available to the JHO, who might not otherwise have been informed of new criminal acts that were not routed to the Bronx Misdemeanor Domestic Violence Court.

5. *Duration of monitoring:* The batterer programs are 26 weeks long, and each offender is allowed three unexcused absences. This means that, on average, it takes offenders approximately seven months to complete the batterer program, during which time offenders must continue to report to court. The research team wanted to ensure that those offenders assigned to the monitoring-only conditions were under court supervision for a comparable period of time. Therefore, those offenders were required to return to court during the seven-month post-sentence period, with the caveat that, as with the offenders assigned to a batterer program, the judges and JHO could always extend the monitoring period in response to compliance problems.

6. *Judge exclusions:* During the planning process, judges asked for the ability to exclude an offender from the study if they deemed it necessary. Possible reasons could include a particularly violent criminal history, a history of domestic violence with the same victim, or an explicit request by the victim that the offender receive a program. In practice, the exact reasons for exclusion were often not apparent nor discussed with the senior research associate. Judge exclusions were rare in the first six months of the study (only 9% of eligible defendants were excluded). However, as time passed and new Assistant District Attorneys rotated into the court that were not familiar with the study, it became apparent that not only were the judges occasionally excluding offenders, but the ADAs were often strongly recommending exclusion as well. The judges were technically the only ones with the express power to exclude; yet, if an ADA strongly advocated exclusion, the judges often went along. To address this problem of rising ADA-driven judge exclusions in early 2003, the research team met with the District Attorney's Domestic Violence Bureau Chiefs about the issue. The result was a decline in exclusions for the remainder of the study intake period.

7. *Age of eligible offenders:* This issue was raised slightly over three months after study implementation began, and was easily solved with the help of the court staff. The question was whether offenders under the age of 18 would be allowed into the study (in New York State, offenders 16 years or older are prosecuted as adults). However, when mandated to a program, these offenders are not sent to DVAP or Fordham Tremont but to a different program designed specifically for young men ages 14 through 19 who have exhibited abusive behavior to girlfriends or mothers. It is both an anger management and batterer program. It was decided that offenders who would normally be sent to this program would *not* be eligible for the study.

8. *Inclusion of a pre-study group in the research:* While most study protocols pertained only to the implementation of the experiment itself, a few involved slight adjustments to court practice (e.g., prosecutor and defense attorney awareness of the study when negotiating pleas, the allocution script read to all study defendants, attempts by Safe Horizon advocacy staff to contact victims at two points after sentencing, and re-arrest and domestic violence registry checks prior to each DVC appearance). Given that randomized trials are sometimes criticized for changing the nature of an intervention in order to accommodate study requirements, we wanted to investigate whether these or other changes might have caused a "study effect." We therefore compared the

baseline characteristics and recidivism outcomes of offenders assigned to group one (batterer program plus monthly monitoring) with a “pre-study” group consisting of offenders assigned to the same conditions in the almost seven-month period preceding study implementation (January 1 - July 22, 2002). We detected few significant differences in baseline characteristics and none in recidivism outcomes, supporting the integrity of the experiment and the comparability of the offenders in our study to those who were previously mandated to batterer programs in the Bronx. (Comparisons are presented in Appendix E.)

Research Design of the Quasi-Experimental Study

While not part of our original proposal, in lieu of the nearly 400 cases lost from the experimental design, we decided in consultation with NIJ staff to draw a separate sample of “conditional discharge-only” cases. These were cases in which the offender pled guilty to a violation and was sentenced to a conditional discharge (CD). However, a batterer program was not initially considered for the sentence and the cases were, therefore, ineligible for the randomized trial. Since the offenders received neither a batterer program nor any judicial monitoring, they represented a true “nothing” condition – and could be productively compared with the offenders in the randomized trial, all of whom received some form of monitoring.

Accordingly, we identified 712 CD-only cases sentenced in the court parts AP10 or TAP2 during a contemporaneous period with the experiment (July 23, 2002 through February 27, 2004). Of these 712 cases, 113 were immediately discarded because they did not involve an intimate partner relationship, as did those in our primary experiment, but instead involved other forms of domestic abuse.

We then had to address the possibility that the remaining 599 offenders differed in other ways from those in the randomized trial (e.g., in criminal history, charges, or demographics). They obviously differed in at least one respect; namely, the prosecutor, defense attorney, and judge did not agree to a sentence that involves monitoring and possibly a batterer program. But other underlying differences might also exist; and these differences might predict an inherently greater or lesser probability of recidivism. Without controlling for all relevant differences, we might therefore obtain spurious results when attempting to determine solely whether the presence or absence of judicial monitoring predicts recidivism. In short, additional steps were needed to refine the CD-only sample so that it would be truly comparable to the offenders in the initial randomized trial. Propensity score matching techniques are designed to do this (see Rubin 1973).

Propensity score matching essentially replaces an approach of “exact matching on covariates,” whereby each “experimental” case is matched to a “control” case that is entirely identical on all observable background characteristics (e.g., identical age, race, criminal history, and charges) known to affect both the probability of participation in the experimental group and the outcomes of interest (e.g., recidivism). Exact matching becomes problematic when, as in this study, many background characteristics are involved. Propensity score matching solves this problem by matching, not on each and every individual characteristic, but on the overall effect of all baseline characteristics in generating a predicted probability of participation in the experimental sample. After the matching process ends, experimental and control cases may still diverge on certain individual characteristics, but they will be “balanced” on all relevant characteristics taken in their totality. This balancing outcome is the critical quality that enables the net effect of the technique to be an artificial re-creation, or at least approximation, of the situation present in a random assignment study (e.g., Bryson, Dorsett, and Purdon 2002).

For the present research design, the main steps were as follows (see also Chapter Six, which shows the background characteristics of the offenders both before and after the implementation of propensity score matching). First, we performed t tests comparing the experimental and CD-only samples on all known background characteristics to determine what, if any, significant differences existed. As the results in Chapter Six will show, many such differences existed at this stage. Second, we entered all characteristics on which differences existed at the modest .10 level or better into a logistic regression, for which the dependent variable was sample membership (0 = CD-only group; 1 = experimental randomized trial group). This regression yielded a “propensity score” for each offender: a predicted probability of inclusion in the randomized trial (monitoring) sample, based not on *actual* membership in that sample but on the *statistical probability* of membership in it, as computed from the observed set of background characteristics (see Rosenbaum and Rubin 1983).

We then matched each offender in the randomized trial to that offender in the CD-only group with the nearest propensity score, sometimes matching multiple offenders from the initial trial to the same CD-only offender. This “nearest-neighbor” method tends to generate higher quality matches than the alternative “one-to-one” method, which would require that each experimental case be matched to a different control case, even if this means increasing the average distance in propensity scores between the matches. Our method ultimately led 387 offenders from the randomized trial to be matched with 219 CD-only offenders. (The final experimental sample declined from 420 to 387 due to missing data for 33 offenders on key background characteristics required for the logistic regression model predicting sample membership.)

Analysis Plan

Dependent Variables

We tracked several outcomes of interest. The most important was new domestic violence crimes committed within one year after sentencing. For offenders in the randomized trial, a series of earlier, intermediate outcomes were also tracked concerning formal compliance with court-imposed sentencing conditions. These measures included number of warrants issued due to failure to appear for required monitoring court dates; number of program terminations (for those assigned to a batterer program); re-arrests during the monitoring period (which averaged approximately seven months); and eventual fulfillment of all conditional discharge requirements.

1. Official records outcomes: Using each offender’s New York State criminal identification number, we obtained complete criminal record files, including prior criminal history and recidivism, from the New York State Division of Criminal Justice Services (DCJS). The one key variable missing from the DCJS files, whether arrests in the criminal record involved domestic violence, was obtained separately from the court system, with the assistance of staff from the Office of the Chief Administrative Judge of New York City. This data enabled us to construct recidivism measures for all study offenders at one-year post-sentence. In addition, as shown in Table 3.1, outcomes for most offenders could be analyzed over eighteen months post-sentence and one year post-program (defined as one year after the last court date on the instant case).

The data enabled us to isolate several different types of re-offending, of which the most important were the following three: (1) re-arrest for any crime, (2) re-arrest for domestic violence, and (3) re-arrest for criminal contempt. The latter criminal contempt category (which may accompany other more or less serious charges applied to the same case) is notable, because

it usually signifies a domestic violence case involving the same victim. Since most victims receive a one-year order of protection, criminal contempt charges are only relevant over the immediate one-year post-sentence period, not over the subsequent eighteen-month or post-program periods. Besides performing comparisons on the probability of at least one re-arrest or new incident on the above measures, we also performed comparisons on the frequency or total number of incidents (0, 1, 2, 3, etc.). Also, equivalent analyses were conducted replacing re-arrest with reconviction measures, but since the same patterns emerged in the conviction data, those results are typically not shown.

2. *Victim report*: Recognizing that many domestic violence incidents go unreported, we also interviewed victims about new domestic incidents committed within one year of sentencing. Research staff at Safe Horizon, the local victim services organization, was trained to conduct such interviews by Safe Horizon's clinical staff, in a daylong orientation session concerning how to interact with victims and protect victim safety. Interviewers were instructed not to leave messages on answering machines and to ensure that victims were in a secure environment before asking them to complete the brief (10 minute) interview. When the one-year milestone arrived, interviewers attempted to establish phone contact with all victims except those who had earlier indicated to Safe Horizon that they did not wish to be contacted. Each eligible victim was tried at least five times or more, with at least three attempts coming during evening or weekend hours. Victims who completed telephone interviews were given \$25 stipends. In cases where victims could not be reached by phone, Safe Horizon staff attempted home visits to encourage victims to call research staff for an interview. When that procedure yielded little in the way of additional interviews, it was discarded in favor of sending letters to unreachable victims, offering a \$50 incentive for victims to participate in interviews.

Victim interviews included a variant of Straus' (1979) Conflict Tactics Scale. The scale assessed the frequency of 16 forms of physical abuse ranging from grabbing to choking to attempted murder. We created two other scales measuring five forms of threats and 11 miscellaneous abusive behaviors ranging from harassing phone calls to attempts to obtain personal information to financial abuse. The interviews also included questions about whether the victim wanted the offender to be arrested and prosecuted (and, if not, whether she expressed that to criminal justice officials); whether the victim was satisfied with the sentence and whether it made her feel safer; and whether the victim would report another incident of domestic violence to authorities. The interview instrument is contained in Appendix F.

Independent Variables and Relevant Hypotheses

For the randomized trial, the primary independent variables were whether the offender was assigned to a batterer program or not; and whether the offender was assigned to monthly or graduated monitoring. For the quasi-experimental study, the primary independent variable was whether the offender was in the initial trial and was therefore sentenced to monitoring; or whether the offender was sentenced to a "CD-only" (conditional discharge with neither a monitoring nor program requirement). The working hypotheses were as follows:

Hypothesis 1: Batterer program assignment predicts a lower probability of recidivism.

Hypothesis 2: Graduated monitoring (due to the behavioral incentives for compliance) predicts a lower probability of recidivism than monthly monitoring.

Hypothesis 3: Judicial monitoring predicts a lower probability of recidivism than its complete absence.

Table 3.1. Sample Size for Outcome Analyses

	Random Assignment					CD-Only
	Cell 1 Program & Monthly	Cell 2 Program & Graduated	Cell 3 Monthly Only	Cell 4 Graduated Only	Total	
Official Records						
Intermediate compliance outcomes	102	100	109	109	420	
Re-arrests 12 months post-sentence	102	100	109	109	420	219
Re-arrests 18 months post-sentence	94	79	94	93	360	184
Re-arrests 12 months post-program	79	78	83	88	328	
Victim Report						
New abuse 12 months post-sentence	22	25	32	27	106	
Percent of total sample (contact rate)	22%	25%	29%	25%	25%	

In addition, data was collected on a large number of offender background and case characteristics that could be included as predictors of outcomes in multivariate analyses and be used in the propensity score matching process that led to the refinement of the CD-only sample.

1. *Basic demographics*: race (coded as black, Hispanic, or white/other), age, and educational attainment (coded both as years of schooling completed and as whether or not the offender completed high school or obtained a G.E.D.).

Hypothesis 4: Older age predicts a lower probability of recidivism.

2. *Stake in conformity* (to test whether those with “more to lose” tend to be more compliant and less likely to re-offend): employment status, years at current job (if employed); marital status, living situation, and years at current address (to test residential stability).

Hypothesis 5: Higher stake in conformity (employed, married, greater residential stability) predicts a lower probability of recidivism.

3. *Criminal history*: data on multiple categories of prior offending, including both arrests and convictions (any priors, felony, violent felony, misdemeanor, criminal contempt and drug-related for the entire history, and domestic violence for the prior three years).

Hypothesis 6: Prior criminal activity predicts future recidivism.

4. *Current offense*: arrest charge type (coded into assault, aggravated harassment, and other), whether the arrest was at the felony or misdemeanor levels, and, for cases in the randomized trial, whether the victim reported physical injuries and/or required medical attention.

Hypothesis 7: More serious (felony as opposed to misdemeanor) instant case arrest charges predict future recidivism.

5. *Case processing*: type of defense firm representing the offender, name of the sentencing judge, and time elapsed from arrest to sentencing.

Hypothesis 8: More time from arrest to disposition (i.e., a less immediate court response to the criminal behavior) predicts future recidivism.

6. *Characteristics of the victim:* for cases in the randomized trial, whether the victim cooperated with the prosecution and whether she had visible injuries (coded as no visible injuries, visible injuries but did not require medical attention, and required medical attention); and for the subset reached for interviews, the victim's age, race, educational attainment, source of income, and relationship status. No particular hypothesis is advanced in regard to these characteristics.
7. *In-program measures:* for certain recidivism analyses, interim compliance measures (warrants, in-program arrests, and conditional discharge completion status) could be treated as independent variables to determine whether compliance during the monitoring period in turn predicts future compliance in the form of less recidivism.
Hypothesis 9: Greater in-program compliance predicts a lower probability of post-program recidivism.

Methods

We performed bivariate comparisons to determine whether there were statistically significant differences in the recidivism and other outcomes of our key subgroups (i.e., batterer program versus no program; monthly versus graduated monitoring; and monitoring versus CD-only). For these comparisons, we relied on the tau-b measure of association. Tau-b is a standard non-parametric measure, meaning the underlying math assumes that the two variables in the analysis have neither a normal distribution nor a linear relationship with each other. Based on test analyses, had we used the more common Pearson's R correlation or simple T tests, it would not have meaningfully affected the results.

We also performed multivariate regression analyses that controlled for the impact of our other independent variables (demographics, stake in conformity indicators, criminal history, etc.). To avoid entering an excessive number of variables into our final models, we began by performing simple correlations of all background characteristics with our primary outcomes of interest. For example, in the analyses based on official re-arrest reports, our primary outcomes were re-arrest for any crime and re-arrest for domestic violence. We then limited our independent variables to those demonstrating statistically significant effects in these simple correlations.

Concerning specific procedures, logistic regressions were used with dichotomous outcomes (e.g., any re-arrest) and Poisson or Negative Binomial regressions were used with continuous outcomes (e.g., number of re-arrests). The Poisson and Negative Binomial specifications are both tailored to outcome measures that are heavily right-skewed (e.g., with many zeros and decreasing numbers of higher values). As a general rule, one shifts from the Poisson to the Negative Binomial whenever the variance is greater than the mean; but because we opted to censure the right tail of our continuous measures, for instance recoding the extremely small numbers of offenders with five or six re-arrests into four, the Poisson was generally acceptable.

Finally, survival analyses were conducted to determine whether offenders in each category (e.g., batterer program versus no program) were crime-free for significantly different amounts of time before their first recidivism incident.

Chapter Four

Results from the Randomized Trial: Official Records

This chapter presents the findings from the primary random assignment experiment. The chapter begins with a discussion of study integrity – was the randomized trial implemented as planned? After providing a background profile for all study offenders, it then goes on to compare in-program compliance and official recidivism outcomes between those that were randomly assigned to a batterer program and those that were not; and between those that were assigned to monthly as opposed to graduated monitoring.

Performance of the Design

All study offenders were sentenced and randomly assigned between July 27, 2002 and February 27, 2004 (19 months). This section investigates whether the randomization process went as planned, and whether the implementation of monthly and graduated monitoring followed the intended protocols. This assessment bears on questions of both internal validity (are the findings valid in themselves) and external validity (to what extent can the findings be generalized to a range of sites and offender populations other than those specifically studied).

Outcome of the Random Assignment Process

Table 4.1 presents and compares the baseline characteristics of offenders that were and were not assigned to a batterer program; and that were assigned to monthly and graduated monitoring. The results indicate that the random assignment process was successful; there were no baseline differences between the groups. Of the 67 comparisons (t-tests) run to determine differences between the characteristics of offenders that were and were not assigned to a batterer program, only one was significantly different at the .05 level; and of 67 comparisons between offenders assigned to monthly and graduated monitoring, only two were significantly different at the .05 level. (Chance probability would lead one to expect a significant difference in one out of every 20 comparisons, so these small numbers of differences are the statistical equivalent of none.) The few differences were:

- Offenders assigned to a batterer program were more likely than those not so assigned to have caused visible injuries; and
- Offenders assigned to graduated monitoring averaged more prior felony arrests and more prior felony convictions than those assigned to monthly monitoring (but there were no significant differences across 28 other prior criminal history measures examined).⁴

Use of Judge Exclusions

Few offenders from the formally eligible pool were removed from the randomized trial. As discussed previously, the sentencing judges had the option to prevent the random assignment whenever they believed a batterer program was necessary. Over the period of study

⁴ Comparisons were also run to determine differences between the characteristics of offenders that were assigned to each of the four groups and only two were significantly different at the .05 level; visible injuries and charge severity.

Table 4.1. Baseline Characteristics of Study Participants

	Batterer Program (n=202)	No Batterer Program (n=218)	Monthly Monitoring (n=211)	Graduated Monitoring (n=209)	Total (n=420)
Basic Demographics					
Race					
Black	41%	38%	36%	43%	40%
Hispanic	40%	44%	45%	39%	42%
White/other ¹	19%	18%	19%	18%	18%
Age (mean)	31.2	30.5	30.2	31.5	30.8
Years of education (mean)	11.0	10.6	10.8	10.7	10.8
Completed high school or G.E.D.	53%	48%	50%	50%	50%
Stake in Conformity					
Employed	59%	58%	58%	59%	59%
Years Employed (mean, of those employed) ²	4.3	3.3	3.7	3.9	3.8
Married	30%	23%	27%	25%	26%
Lives with intimate partner	47%	49%	47%	49%	48%
Years living at current address	6.2	6.6	6.0	6.8	6.4
Victim Characteristics					
Injury to Victim					
No visible injury	22%	31% *	22%	31% +	27%
Injury but no medical attention received	48%	45%	47%	45%	46%
Received medical attention	30%	25%	30%	24%	27%
Victim Cooperation					
Cooperation (of non-missing cases) ³	64%	58%	63%	58%	61%
Non-cooperation (of non-missing cases) ⁴	36%	42%	37%	42%	39%
Prior Criminal History					
Prior arrests					
Yes	54%	54%	54%	54%	54%
Mean	2.8	2.6	2.3	3.1	2.7
Prior domestic violence arrests (past 3 yrs.)					
Yes	10%	11%	9%	12%	10%
Mean	0.1	0.1	0.1	0.1	0.1
Prior felony arrests					
Yes	37%	39%	37%	38%	38%
Mean	1.2	1.1	0.9	1.4 *	1.1
Prior violent felony arrests					
Yes	22%	23%	22%	23%	22%
Mean	0.4	0.4	0.3	0.53 +	0.4
Prior misdemeanor arrests					
Yes	47%	45%	46%	46%	46%
Mean	1.6	1.5	1.4	1.8	1.6
Prior drug arrests					
Yes	33%	34%	33%	34%	33%
Mean	1.0	1.1	1.0	1.1	1.0
Prior convictions					
Yes	39%	39%	39%	39%	39%
Mean	1.5	1.3	1.2	1.6	1.4
Prior domestic violence convictions (past 3 yrs.)					
Yes	3%	5%	5%	3%	4%
Mean	0.03	0.05	0.05	0.03	0.04
Prior felony convictions					
Yes	14%	13%	10%	17% +	14%
Mean	0.2	0.2	0.2	0.28 *	0.2
Prior violent felony convictions					
Yes	4%	5%	3%	6%	5%
Mean	0.04	0.06	0.04	0.07	0.05

Table 4.1. (Continued)

	Batterer Program (n=202)	No Batterer Program (n=218)	Monthly Monitoring (n=211)	Graduated Monitoring (n=209)	Total (n=420)
Prior misdemeanor convictions					
Yes	22%	17%	18%	20%	19%
Mean	0.6	0.6	0.4	0.78 +	0.6
Prior violation convictions					
Yes	33%	31%	33%	31%	32%
Mean	0.7	0.6	0.7	0.6	0.7
Prior drug convictions					
Yes	14%	15%	13%	16%	15%
Mean	0.3	0.4	0.3	0.4	0.4
Prior cases with at least one bench warrant					
Yes	29%	30%	30%	29%	30%
Mean	0.7	0.6	0.6	0.7	0.6
Prior months in jail or prison (mean)	3.3	3.0	2.2	4.1	3.2
Number of prior prison sentences					
0	95%	95%	96%	94%	95%
1 or more	5%	5%	4%	6%	5%
Current Arrest Charges					
Charge type					
Assault	78%	80%	78%	80%	79%
Aggravated harassment	9%	6%	10%	5% +	7%
Other charges	13%	14%	12%	14%	13%
Charge severity: felony (not misdemeanor)	14%	13%	13%	14%	14%
Case Processing					
Defense Firm					
Legal Aid	56%	58%	59%	56%	57%
Bronx Defenders	23%	25%	23%	25%	24%
18B Panel (appointed counsel)	13%	13%	13%	13%	13%
Private attorney/Law student ⁵	7%	4%	5%	6%	6%
Sentencing judge					
Judge #1	44%	45%	45%	44%	45%
Judge #2	22%	23%	24%	21%	23%
Judge #3	12%	8%	10%	11%	10%
Judge #4	16%	16%	16%	15%	16%
Judge #5	6%	6%	5%	8%	6%
Other judge	1%	1%	1%	1%	1%
Processing time: months, arrest to sentencing	2.3	2.4	2.2	2.5	2.3
Batterer Program					
DVAP	63%	0%	32%	29%	31%
Fordham Tremont	29%	0%	13%	15%	14%
Did not wait for referral	6%	0%	2%	3%	3%
Other program	2%	0%	1%	1%	1%
No program mandated	0%	100%	52%	52%	52%

***p<.001 **p<.01 *p<.05 +p<.10

Note: Unless otherwise noted, missing values for all variables range from 0% to 10%.

¹ "Other" represents only 3% of those in the white/other category.

² This indicates years employed at current job.

³ This variable contains 41% missing data.

⁴ This includes cases with mixed indications of whether there was victim cooperation.

⁵ This includes one case (.2%) where a law student represented defendant.

implementation, however, the judges used this option in only 14% of all possible cases (69 times). Also, comparisons between the baseline characteristics of those in the random assignment group and the judge exclusion group indicate significant differences on only two characteristics (out of 67):

- One specific sentencing judge was more likely than the others to exclude offenders from the random assignment ($p < .05$); and
- The judges were more likely to exclude offenders if they were initially arrested on felony as opposed to misdemeanor charges ($p < .01$). These “higher risk” offenders (14% of the random assignment and 28% of the judge exclusion group) were sometimes deemed inappropriate for random assignment by both the judge and prosecution. However, the judges were *not* more likely to exclude offenders based upon any other risk factors, prior criminal history in particular.

Integrity of the Experimental Conditions

Next we analyzed how well the intervention conditions were implemented and maintained throughout the study period.

1. Integrity of batterer program assignment: Of the 420 offenders, 48% were assigned to a batterer program. Of those 202 offenders, 63% were assigned to DVAP, 29% were assigned to Fordham Tremont, and 2% (3 offenders) were assigned to another program – one needed a program that offered a class in Korean and two needed a program that was closer to their home. The remaining 6% (12 offenders) failed to walk down the hall to the defendant monitors’ office to be assigned to a program following their court appearance and random assignment. All twelve of these offenders later returned to court on a new arrest or probation violation, or were returned by the warrant squad, and were resentenced to jail. None of them ever went to the batterer program. In effect, in these twelve cases, the random assignment was implemented exactly as designed, leading to an “intent to treat” on the part of the court; but because offender noncompliance was immediate and final upon leaving the courtroom, these twelve offenders never enrolled in a program.

We also investigated whether the group of offenders who were *not* assigned to a batterer program received something additional to make up for their not receiving the experimental intervention. This did not happen; the 218 offenders that were not randomly assigned to a batterer program did not receive any other program or service mandate either.

As discussed in Chapter Three, there were two programs to which offenders were assigned, DVAP and Fordham Tremont. Due to the differences in eligibility requirements, the two programs were assigned participants that significantly differed on a number of background characteristics. Offenders that were assigned to DVAP were more likely to be Hispanic, while those assigned to Fordham Tremont were more likely to be black, which can be explained by the fact that Fordham Tremont does not offer classes in Spanish. Also, offenders assigned to DVAP were more likely to be employed (and were probably more advantaged on other unobserved socioeconomic measures as well), since DVAP does not accept Medicaid. In addition, offenders assigned to Fordham Tremont had a much more serious criminal history. Prior criminal history is not a criterion for program selection, and the defendant monitors were unable to explain that finding (although it may serve as a proxy for unobserved socioeconomic measures if those with a prior criminal history are also lower in average socioeconomic status).

Table 4.2. Logistic Regression Predicting Re-Arrest: Does Program Site Matter (DVAP or Fordham Tremont)?

	Model 1	Model 2
Type of Analysis	Logistic Regression	
Dependent Variable	Any Re-Arrest	
	Odds Ratios	
Assigned to DVAP	.533 *	0.732
Race		
Hispanic		.325**
Age		0.963 †
Employed		.555
Living with Intimate Partner		1.043
Number of prior convictions		1.087
Arrest charge severity (felony)		1.682

***p<.001 **p<.01 *p<.05 †p<.10

Note: This regression was computed only for offenders randomly assigned to a batterer program and subsequently placed in DVAP or Fordham Tremont (187 of 202 offenders assigned to a program). The N for the regression models was 175 (12 cases had missing data for one of the predictor variables).

We next analyzed whether the choice of program predicted recidivism. Before controlling for other characteristics, we found that offenders assigned to DVAP were less likely to re-offend than those assigned to Fordham Tremont at the modest .10 level; but after controlling for background characteristics we found that program site no longer predicted recidivism. In other words, DVAP serves a generally less recidivist population, and it is this fact, not participation in the DVAP program as opposed to Fordham Tremont per se, that leads those assigned to DVAP to re-offend at a lower rate (see Table 4.2).

2. *Integrity of monitoring:* As Table 4.3 indicates, the *duration* of monitoring was quite consistent for all four study groups. With regard to the *frequency* of monitoring, groups 1 and 3, both assigned to monthly monitoring, averaged one more court appearance than did groups 2 and 4. The results for those assigned to monthly monitoring were expected; they are under court supervision for an average of seven months and should therefore come to court approximately seven times – which they did. The offenders assigned to the graduated monitoring schedule would come less often if they were in compliance (and appeared to average between five and six appearances).

To further investigate whether the frequency of court appearances was appropriate, we examined the adjourn dates given to offenders at each appearance. We found that offenders on the monthly monitoring schedule, whether they were in compliance or not, were correctly adjourned for one month. We also found that offenders on the graduated monitoring schedule, when in compliance, were correctly adjourned for increasingly longer periods; on average, they returned to court after 38.5 days, and the median was 51.9 days (see Table 4.4).

Unfortunately, the graduated monitoring schedule was not implemented as planned for those out of compliance. They returned to court an average of 24.5 days later, whereas if the schedule was implemented correctly, they would have returned to court an average of 14 days later (two-week adjournment). A possible explanation for this is that when an offender was out of compliance (new arrest, warrant, program termination), the judicial hearing officer sent the case

Table 4.3. Duration and Frequency of Monitoring Appearances

	Duration of Monitoring (months)	Frequency of Court Appearances
Group 1	7.59	6.65
Group 2	7.16	5.43
Group 3	7.01	6.52
Group 4	7.06	5.27

Table 4.4. Length of Time between Court Appearances

		Mean (days)	Median (days)
Monthly Monitoring	Compliant	29.1	28.0
	Non-Compliant	24.7	26.5
Graduated Monitoring	Compliant	38.5	51.9
	Non-Compliant	24.5	23.4

to the sentencing judge for punishment. When the sentencing judges did not re-sentence the offender but instead returned the case to the compliance calendar, they were unwilling to be responsible for adjourning the case to the two-week schedule and instead the judicial hearing officer was supposed to enforce the two-week appearance after the next court date. It looks as if by the time the case was returned to the compliance judge, which was often a month later, the two-week schedule was still not implemented most of the time. In sum, compliant offenders received the reward of less frequent appearances, but noncompliant offenders did not consistently receive the sanction of more frequent appearances.

Offender Profile

In addition to comparing the four study groups, Table 4.1 (above) also includes a “total” column presenting the summary baseline characteristics of all study offenders combined.

- *Basic demographics:* The majority of offenders were either Hispanic (42%) or black (40%); and they averaged 31 years of age at the time of arrest. Fifty percent had received their high school diploma.
- *Stake in conformity:* Fifty-nine percent of the offenders were employed full-time, and had been at their current job an average of 3.8 years. Also, 26% were married and 48% lived with their partner. On average, they had been living at their current address for 6.4 years.
- *Victim information:*
 - Injury to Victim – There was no visible injury in 27% of the cases, while 46% of the victims were injured but did not require medical attention, and 27% did require medical attention.
 - Victim Cooperation – The District Attorney’s Office reported some level of victim cooperation in 61% of the cases.
- *Prior criminal history:* Fifty-four percent of the offenders had a prior arrest, 39% had a prior conviction, and 30% had a prior case with at least one bench warrant issued

(signifying a prior history of noncompliance with court orders). (Results for other measures, including breakdowns for specific types of charges, are in Table 4.1.)

- *Current arrest charges:* The majority of the offenders were arrested on an assault charge (80%); also, 14% faced felony charges at the time of arrest (although these were downgraded to the misdemeanor level by the arraignment stage).
- *Case processing:*
 - Defense Firm – Fifty-seven percent of the offenders were represented by the Legal Aid Society, 24% by the Bronx Defenders, 13% by the 18B Counsel (comprised of other attorneys serving indigent defendants), and the remainder by private counsel.
 - Sentencing Judge – Forty-five percent of the random assignment pleas were accepted by one judge that usually presided in the all-purpose sentencing court part (AP10). There were five other judges that alternately sat in that part or in the misdemeanor domestic violence trial part (TAP2) and accepted random assignment pleas.
 - Processing Time – On average, it took 2.3 months to dispose of the case, from arrest to sentence.
 - Batterer Program – Forty-eight percent of the offenders were assigned to a batterer program. Of those, 63% were assigned to DVAP, 29% to Fordham Tremont, and the remaining 8% were either assigned to another program or were immediately noncompliant after sentencing and never reached the defendant monitors' office for a program assignment.

Outcome Results

The critical questions motivating this analysis are whether batterer programs or a particular monitoring schedule can keep offenders from returning to the criminal justice system and from re-abusing their partners. If there are significant effects, it is also important to understand their durability: only during the period of active judicial supervision (an average of seven months in this study) or over a longer-term period? Accordingly, recidivism analyses were conducted for four distinct time periods: (1) in-program (spanning seven months on average), (2) one year post-sentence, (3) eighteen months post-sentence, and (4) one year post-program (following the last court date on the instant case). Results for the last two of these four periods could be analyzed for most, but not all, study offenders. In many cases, the substance of the results was identical across all four periods, in which case results for some are either not shown or displayed separately in Appendix G.

In-Program Compliance

We set the stage by investigating measures of interim compliance during the in-program period: warrants, program terminations (for those assigned to a batterer program), and formal fulfillment of the terms of the conditional discharge (see Table 4.5). In general, those assigned to a batterer program exhibited greater noncompliance than those not so assigned; this is conceivably explained by the more onerous obligations, and hence greater opportunities to be noncompliant, of those assigned to a program. On the other hand, there were not any significant differences between those assigned to monthly versus graduated monitoring, and the raw percentages were nearly identical on most measures; for example, 27% of those assigned to both monitoring schedules failed to complete the mandate of the conditional discharge.

Table 4.5. In-Program Compliance (N=420)

In-program Compliance Measure	BP (n=202)	No BP (n=218)	Monthly (n=211)	Graduated (n=209)
Warrant Issued (no-show for court or program appearance)	47%	31% ***	41%	36%
Batterer Program Termination (of those assigned to a program)	44%		43%	45%
Re-Arrest while In-Program				
Any re-arrest	25%	23%	21%	27%
Any domestic violence re-arrest	15%	10%	10%	14%
One or More Non-compliant Events	62%	39% ***	51%	49%
Did Not Complete Court Mandate	39%	16% ***	27%	27%

* p < .10 * p < .05 ** p < .01 *** p < .001 (2-tailed t-test)

1. *Warrants:* A significantly higher percentage of offenders assigned than not assigned to a program warranted at least once during the monitoring period (47% versus 31%, $p < .001$). A bench warrant is almost always issued when an offender does not show up for a required court appearance, but can also be issued when an offender is reported absent from an assigned program. For this reason, there are more ways that an offender assigned to a program can incur a warrant.

2. *Program terminations:* Of the 202 offenders that were mandated to a batterer program, 44% were terminated at some time. In most such cases, the court re-referred the offenders to the program, sometimes to restart from the beginning and sometimes to continue from where they left off. These decisions were based on the discretion of the sentencing judge and were decided on a case-by-case basis.

3. *Conditional discharge completion:* A significantly higher percentage of the offenders assigned than not assigned to a batterer program failed to complete the mandate of their conditional discharge (29% versus 16%, $p < .001$). This is not surprising, since those assigned to a program had greater responsibilities (i.e., completing the 26-week program).

Official Reports of Recidivism: Bivariate Results

Across all four periods examined, there were not any significant differences in re-offending between those who were and were not assigned to a batterer program; and between those who were assigned to monthly and graduated monitoring.

1. *The nature of offender recidivism:* Before considering differences across the intervention conditions, Table 4.6 indicates the general nature of the recidivist behavior for the entire sample. For those re-arrested during the one year post-sentence period, the table shows the prevalence of various top charges. Interestingly, the results indicate that re-arrests were more likely to be for non-domestic violence charges (59%), such as drug or property crimes, than for domestic violence charges (41%). The offenders appeared to be engaged in a great deal of drug offending in particular, as 24% of all first re-arrests were for illegal drug sales or possession.

**Table 4.6. Types of Charges in Recidivism Cases:
Top Arrest Charge for the First New Arrest within One Year Post-Sentence**

Recidivism Measure	
Number of Defendants with New Arrest	116 (28%)
Domestic Violence	40%
Criminal Contempt Charge	23%
Assault Charge	10%
Aggravated Harassment Charge	3%
Other Domestic Violence Charge ¹	4%
Non-Domestic Violence	59%
Drug Charges	24%
Property Charges ²	17%
Violent Charges ³	6%
Other ⁴	12%

¹This includes menacing or reckless endangerment, controlled substances-related, burglary and other.

²This includes criminal trespass, burglary, criminal mischief, petit or grand larceny, robbery and theft.

³This includes assault, menacing or reckless endangerment, sexual abuse, and aggravated harassment.

⁴This includes criminal possession of a weapon, forgery, related to illegal recording and other.

2. *Impact of batterer programs:* As shown in Figure 4.1, batterer program mandates did not lead to reduced recidivism over any time period examined. For example, over the one year post-sentence period, 29% of those assigned to a program and 26% of those not so assigned were re-arrested; and 16% and 12%, respectively, were re-arrested for domestic violence. None of the differences reported in Figure 4.1 were statistically significant.⁵

A further finding is that during the one-year *post-program* period, 16% of those offenders who completed the program compared with a slightly higher 23% of those who did not complete were re-arrested; and 6% and 13% of completers and non-completers were respectively re-arrested for domestic violence. While the raw percentages suggest that completion may signal at least a slightly lower probability of re-arrest, none of these differences was statistically significant. And of course while interesting, a comparison of completers and non-completers cannot inform an analysis of batterer program efficacy in any case, since completers of any intervention often have a predisposition to be more compliant than non-completers, with or without the intervention *per se*. For this reason, the most relevant comparison to a test of efficacy is the one reported in Figure 4.1 between the average performance of *all* offenders randomly assigned to the batterer program and those randomly assigned *not* to receive the program.

3. *Impact of judicial monitoring schedule:* As shown in Figure 4.2, the type of judicial monitoring did not generally make a difference in outcomes. During the in-program period, 21% of those assigned to monthly monitoring and 27% of those assigned to graduated monitoring were re-arrested. While these raw percentages suggest slightly better results for those assigned to

⁵ Based on ANOVA tests comparing outcomes across all four study conditions, there were also not any significant differences in re-offending during the one year post-sentence period between those assigned to group 1 (31%), group 2 (27%), group 3 (25%), and group 4 (28%). Similarly, there were not significant differences when isolating domestic violence recidivism during the same time period between those assigned to group 1 (16%), group 2 (17%), group 3 (11%), and group 4 (12%).

Figure 4.1. The Impact of Batterer Program Assignment on The Probability of Re-Arrest: Results from Four Measurement Periods

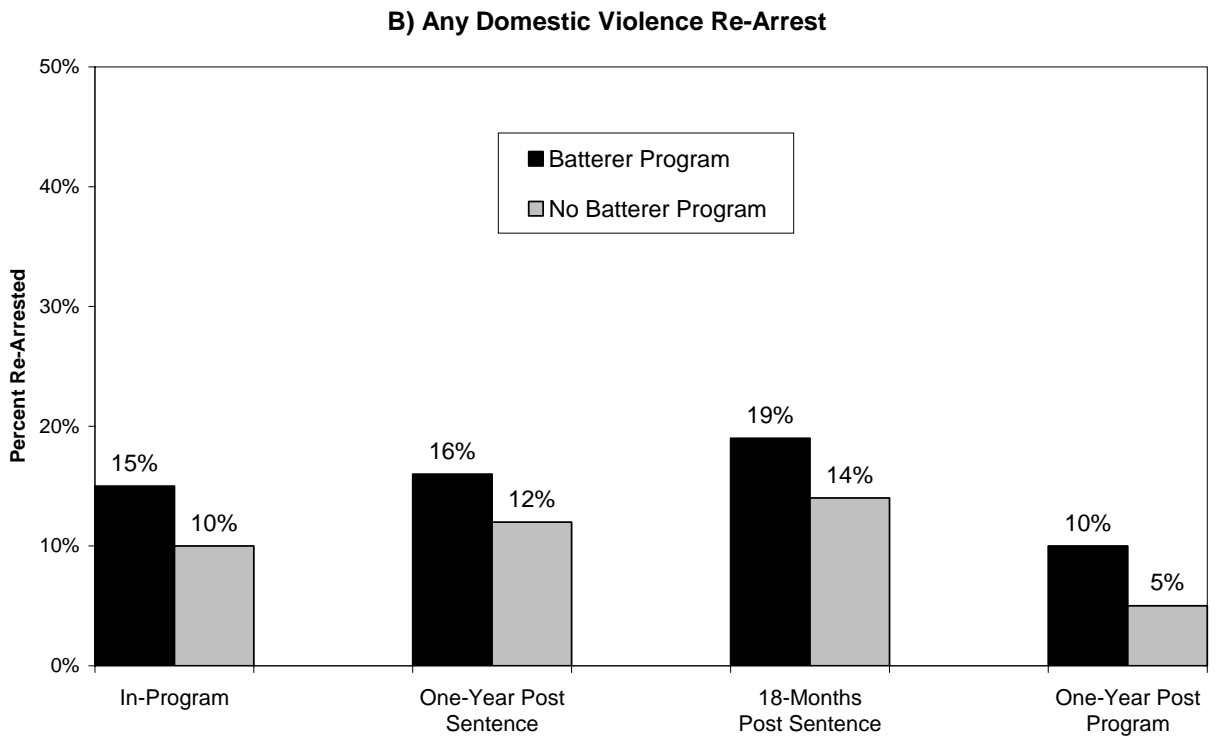
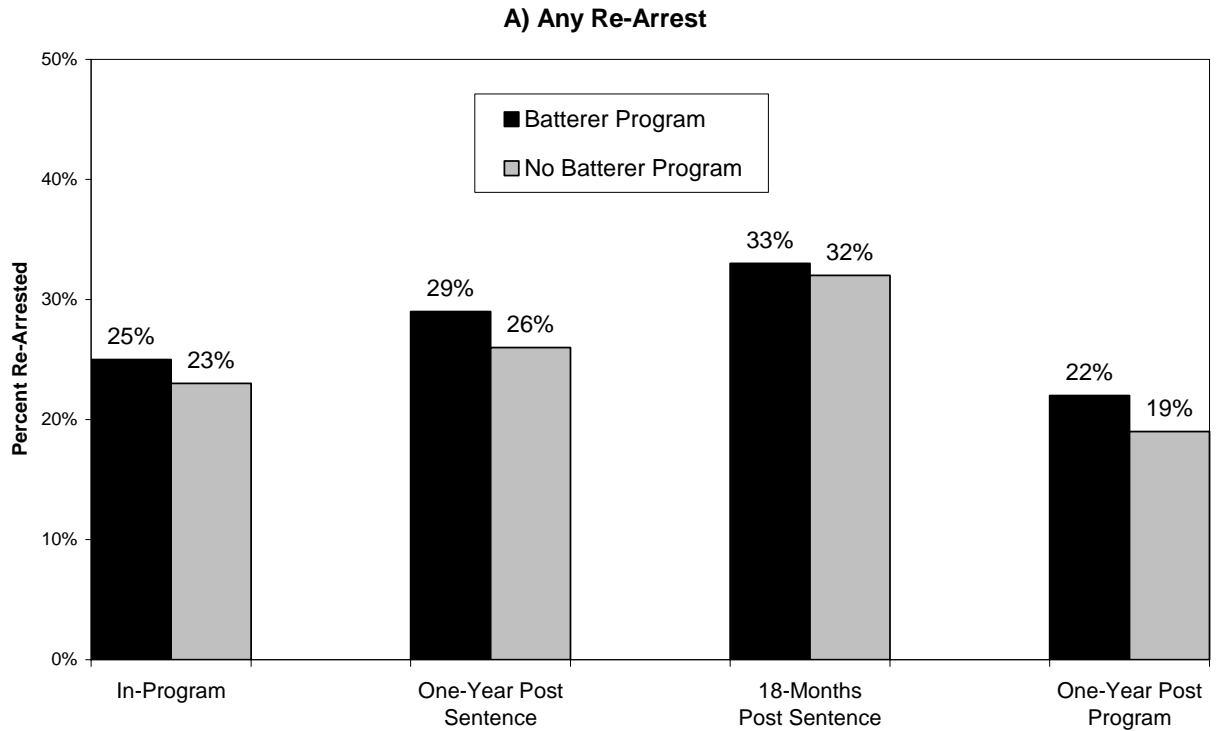
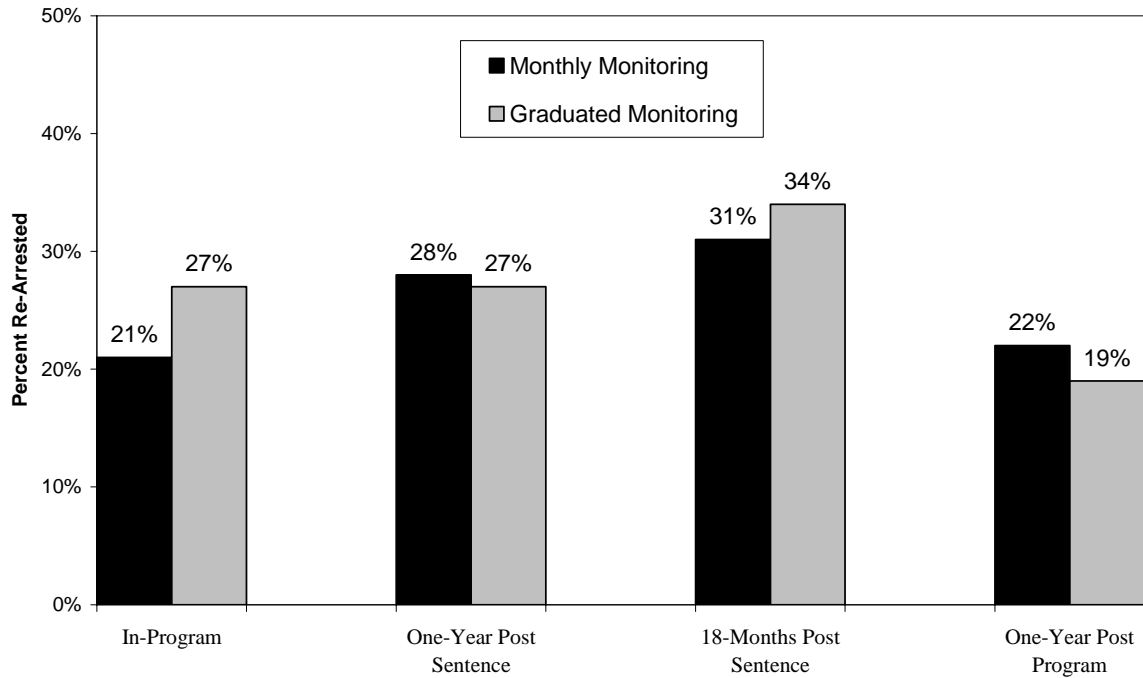


Figure 4.2. The Impact of Judicial Monitoring Schedule on the Probability of Re-Arrest: Results from Four Measurement Periods

A) Any Re-Arrest



B) Any Domestic Violence Re-Arrest

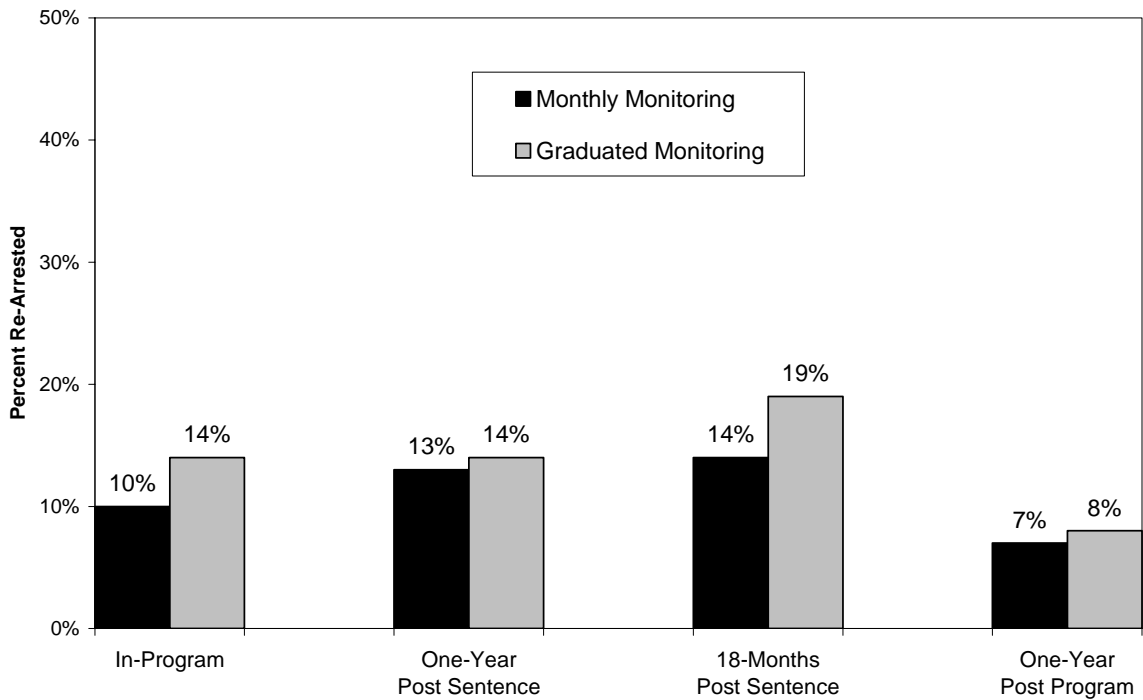
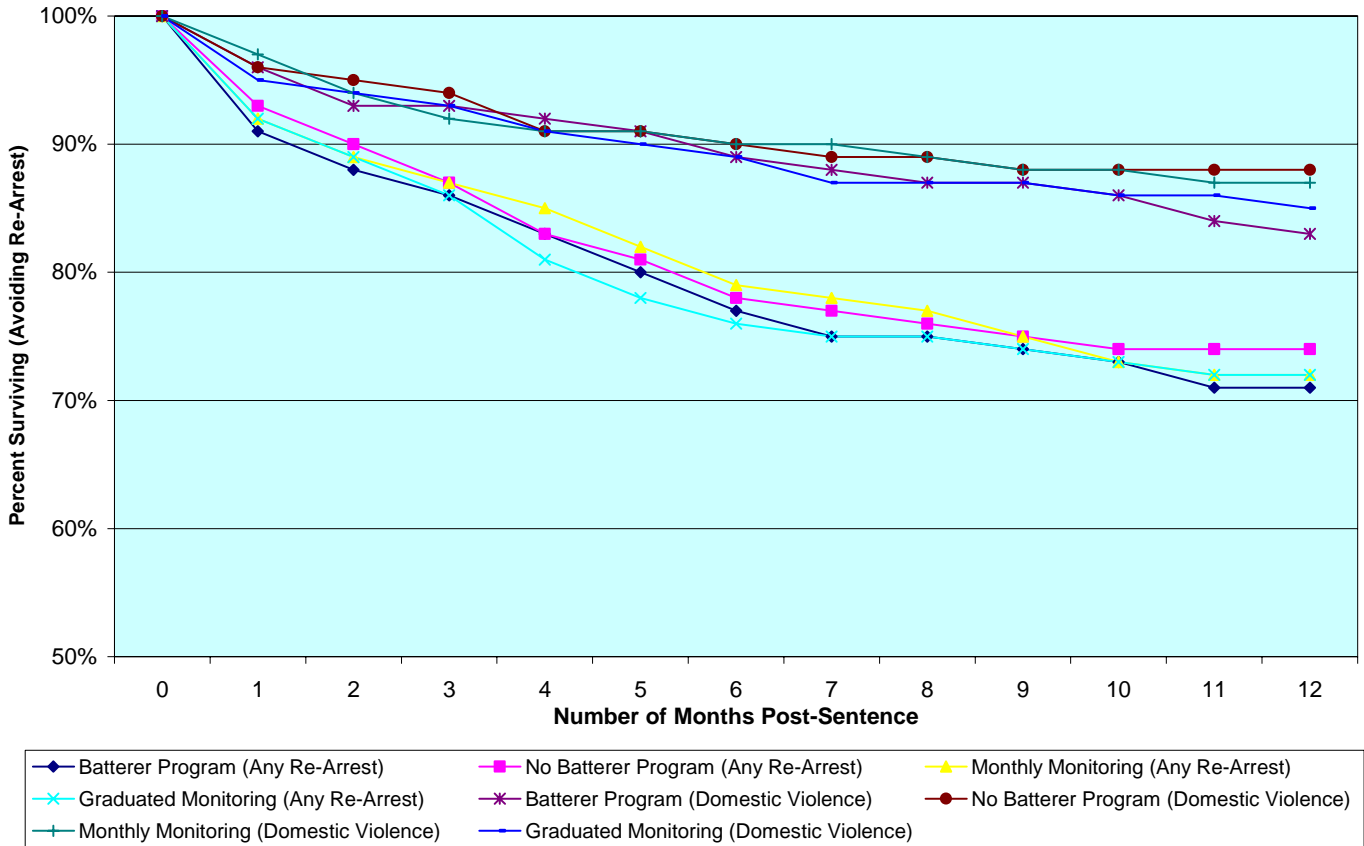


Figure 4.3. Survival Curves over One-Year Post-Sentence for Randomly Assigned Offenders: Any Re-Arrest and Any Domestic Violence Re-Arrest



monthly monitoring, the appearance of a difference (which is non-significant) disappears when examining the results for the other periods. For instance, the re-arrest rate over the one year post-sentence period was 28% for those assigned to monthly monitoring and 27% for those assigned to graduated monitoring. Further, when isolating domestic violence recidivism, the results are similar again; for example, 13% and 14% of those respectively assigned to monthly and graduated monitoring were re-arrested for domestic violence over the one year post-sentence period.

4. *Survival analysis:* Figure 4.3 presents the full one-year survival curves for the offenders assigned to each intervention condition. We did not find any differences across conditions in “survival time” (days to first re-arrest); as the figure graphically demonstrates, the curves for all groups are almost identical.

Predictors of Recidivism: Multivariate Results

In the previous chapter, we hypothesized that offenders who were younger, had less “stake in conformity” (e.g., due to investments resulting from a positive marital or employment status), had a prior criminal record, and had more serious instant case arrest charges would be more

Table 4.7. Regression Models Predicting One Year Post-Sentence Recidivism

	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Type of Analysis	Logistic Regression				Poisson Regression			
Dependent Variable	Any Re-Arrest	Any Re-Arrest	Any DV Re-Arrest	Any DV Re-Arrest	Total Re-Arrests	Total Re-Arrests	Total DV Re-Arrest	Total DV Re-Arrests
	Odds Ratios				Regression Coefficients			
Assigned to Batterer Program	1.080	1.079	1.155	1.136	-.790	-.706	.299	
Assigned to Monthly Monitoring	1.103	1.049	1.010	1.016	-.120	-.103	-.122	
Race								
Hispanic		.471***		.461**				
Black		1.522*		1.403		.553***		.309
Age		.960*		.989		-.427***		.659
Employed		.708		1.384		.785		
Living with Intimate Partner		.491*		.652		-.129*		-.713*
Married		1.157		1.532		-.334		
Number of prior convictions		1.164**		1.149**		.672***		.537*
Arrest charge severity (felony)		1.683		1.704		.564*		.643*
Months, arrest to sentencing		.962		.870		-.700*		-.115+
Nagelkerke R ²	0.001	0.205	0.001	0.131				

***p<.001 **p<.01 *p<.05 +p<.10

Note: The N for the multivariate regression analyses was 350 due to removal of cases with missing data. Also, fewer independent variables were included in the Poisson regression predicting total domestic violence re-arrests; the nature of the distribution (less than 15% of cases with at least one re-arrest and inter-correlations among many of the predictor variables) necessitated limiting the model.

¹ A deviation coding scheme was used for race in the logistic regression models, meaning that coefficients are in relation to the average.

likely to re-offend. And with respect to case processing, we hypothesized that a more immediate court response (less time from arrest to sentencing in the initial case) would deter recidivism.

To test these hypotheses, we first performed simple correlations of all baseline characteristics with re-arrest for any offense and for domestic violence over the one year post-sentence period (see Appendix G). We then retained all characteristics with significant bivariate correlations (except for retaining only one of the many criminal history measures). Regression analyses then measured effects on both the probability of re-arrests (yes/no) using logistic regression and on the total number of re-arrests using Poisson regression; both are reported in Table 4.8 for the one-year post-sentence period (other time periods are in Appendix G).

Several predictors exerted consistent effects: having more prior convictions, non-Hispanic race, younger age, and not living with intimate partner. In general, the regression models had less explanatory power and fewer predictors were significant when the outcome measures concerned re-arrest for domestic violence (as opposed to any offense).

The results again indicate that neither assignment to a batterer program nor type of judicial monitoring affect the probability of re-arrest. While the Poisson results indicate that monthly monitoring (as opposed to graduated) appeared to have a modest effect leading to fewer total re-arrests, this was only detected at the .10 level and only in the in-program time period and 18-months post-sentence time period (see Appendix G).

Finally, when we attempted to model “post-program” recidivism, we added in-program compliance as a possible predictor (defined as the total number of noncompliant events during the monitoring period); but we found that it did not predict re-arrest, either for any offense or for domestic violence in particular (see Appendix G). In addition, due to a previous finding that found completion of a batterer program signaled a slightly lower probability of re-arrest and the previous literature, which has often found that completing a batterer program is correlated with lower recidivism (E.g., see Gondolf 1998; Puffett and Gavin 2004; but for a study not finding

this relationship see Davis et al. 2000), among those offenders assigned to a program, we tested whether completing it was associated with lower post-program recidivism; but we found no such effect (results not shown).

Of particular note, when the outcome measures concern domestic violence in particular, the regression models have less explanatory power and many of the individual coefficients suggest weaker relationships (see Table 4.8). For example, age loses its significance entirely; although non-Hispanic, living with an intimate partner, prior criminal history, felony arrest charge severity, and longer case processing remain significant. The results again indicate that neither assignment to a batterer program nor type of judicial monitoring affect the probability of re-arrest for domestic violence. Finally, the number of non-compliant events while in-program again did not predict post-program domestic violence re-arrest. However, offenders who were re-arrested during the in-program period (for any crime and domestic violence in particular) were significantly more likely to recidivate during the post-program time period as well (results not shown).

Qualitative Perspective

As an added component to the study, we conducted interviews with a small number of study offenders (n=7). The goals of the interviews were to obtain a better understanding of offenders' recent experiences within the criminal justice system, to explore their views about the batterer program if they were assigned to one, and to explore their perceptions of monitoring, whether monthly or graduated. We decided to conduct this qualitative component after study implementation was underway, primarily because we were concerned about whether the offenders assigned to graduated monitoring truly understood the incentives built into their schedule. While recognizing the limitations of a sample of only seven offenders, we intended this component to be strictly exploratory. Our sample size and composition were constrained by several factors: First, the IRB required that we conduct the interviews immediately following an offender's last court appearance, when offenders would no longer be under court mandate and thus might be less likely to feel coerced to participate; however, they may also have been less likely to agree to remain for an interview. In addition, we only interviewed offenders who successfully completed their mandate; but their perspective may differ from offenders who were resentenced. Finally, interviews were only held in English due to the language limitations of the interviewer (see Appendix H for the interview instrument).

Treatment during Court Process

All offenders felt that they were treated fairly prior to pleading guilty, and that both their lawyer and the judge listened to what they had to say. One offender indicated that his lawyer understood that he wanted a sentence that satisfied him, and his lawyer was patient and worked hard. When asked how they felt during the period after they pled guilty, two offenders indicated that they were unhappy about pleading guilty because they did not commit the crime. But they all felt that they were treated fairly during their compliance monitoring appearances.

Consequences of Noncompliance

We asked the offenders if they understood their obligations to the court during the monitoring period, and the penalties that would arise if they did not meet these obligations; we

also asked whether they thought the obligations were reasonable. They all clearly understood their obligations (complete program, obey the order of protection, and come to court when scheduled) and thought they were reasonable. They were very aware of the 15-day jail alternative if they were noncompliant. One offender said “they seemed very serious about that option, and I wasn’t going to let that happen!”

Judicial Monitoring Schedule

We wanted to gain a sense of whether the offenders understood their monitoring schedule and how often they were supposed to appear in court. Four offenders were on a monthly schedule and understood that they were to report to court once a month. Three were on a graduated schedule, and all of these offenders incorrectly believed that they were also supposed to report to court once a month. When asked if they were told of their graduated monitoring schedule, all said that they were not and had never been told anything about it. One of the researchers was present at every compliance appearance and knows this not to be true (see Chapter Three, section on judicial monitoring practices); but it is important to learn that the offenders apparently did not hear or did not understand the judicial hearing officer’s explanation.

Batterer Program

All of the men had just completed the batterer program (none were in the no-program condition), so we wanted to gain a sense of how they felt about the classes. Surprisingly, all felt positive overall about the experience. They thought their instructors were very effective in teaching the class and really listened to them. One indicated that the class “taught him something about himself and that he wanted to become a different person.”

They all had formed some sort of relationship with others in their class; one indicated that they were his friends in class, but he would never socialize with them outside of class; others said that they did socialize outside of class; one even indicated that he and another classmate were going into business together.

When asked what they liked about the class, one said he liked learning “the ways you control a person without knowing it.” Another said he liked learning about “different options about how to deal with anger and how to communicate better.” Often we heard that it was the instructors offenders liked best, that they were patient and listened. When asked what they disliked, all mentioned the fee and the inconvenience of going every week. One indicated that class discussion often strayed from the issue of domestic violence.

When asked if the program had made them less likely to use physical violence when conflicts arise, all except one of the offenders indicated that it would. One said, “I don’t want to come back here, I may be controlling but I can see that now and want to change.” Another said, “I won’t use my hands anymore, nothing condones violent behavior.”

Summary

With this part of the study, we tested the hypotheses that batterer programs or different monitoring schedules lead to reduced re-offending among domestic violence offenders. It seems clear that this study provided a valid test; the random assignment process was successful, the judge exclusion option was used infrequently, and those offenders that the judge did exclude did not differ significantly from the others on nearly all observed baseline characteristics. While the batterer program conditions and duration and frequency of judicial monitoring were generally

implemented as planned, one important qualification is that the graduated monitoring schedule was not consistently implemented with offenders who were out of compliance.

We found that neither batterer programs nor either type of judicial monitoring schedule led to a reduction in recidivism. This was based upon an analysis spanning both the period of active judicial monitoring and a substantial follow-up period. The results failed to show significant differences in the probability of re-arrest, either for any offense or for domestic violence, between those assigned and not assigned to a batterer program; and between those assigned to monthly and graduated monitoring. Instead, the results point primarily to prior criminal history, age, some (but not most) indicators of having a “stake-in-conformity,” and race as the key predictors of re-arrest.

Chapter Five

Results from the Randomized Trial: Victim Interviews

This chapter presents the findings from the interviews with victims whose cases were part of the randomized trial. The chapter begins with a discussion of study validity issues and briefly describes the background profile of those victims who were interviewed. We then report the major findings related to new incidents of abuse and several other outcomes, including victim satisfaction with the court outcome; perceptions of safety; and likelihood of calling the police in the event of any future incident.

Performance of the Research Design

We provided research staff at Safe Horizon, the local victim advocacy agency, with the names and contact information for all victims whose cases were part of the randomized trial (n = 420) and for 69 additional cases that were formally eligible for the trial but were excluded by the sentencing judge. Contact information was obtained from the District Attorney's office and from previous contacts made by Safe Horizon's victim advocacy staff. This section looks at the interview success rate and the possibility of attrition and selection bias.

Interview Success Rate

We obtained interviews with 106 victims in the 420-case sample, a success rate of 25%. The main reasons that interviews were not conducted were that we never were able to obtain contact information from the prosecutor or Safe Horizon files (45% of the unsuccessful cases) or the phone number that we had obtained was not in service or a wrong number (39%). In just 11% of the cases where no interview was obtained did we fail to reach the victims after the requisite five attempts, and in just 5% because the victim refused to be interviewed. While low, the 25% success rate in obtaining interviews is comparable to results obtained from other studies using court-based populations and information gleaned from police and prosecutor files. Unfortunately, the possibility of contacting victims in this type of study is severely limited by lack of contact information in prosecutor files and by the fact that many women move and/or unlist their phone numbers immediately following police involvement in a domestic incident. Some studies (e.g., McFarlane, Malecha, Gist, Watson, Batten, Hall, and Smith 2004) have claimed far higher success rates, but they used a methodology in which face-to-face contact was made and address and phone information collected from the victim at the time of sample recruitment.

The interviews were scheduled to be one year, or 365 days, after sentencing: In fact, the median time to interview was 410 days (or 13.5 months).⁶

Internal Validity

We compared the case characteristics of victims who were interviewed with those who were not. As Table 5.1 shows, we found no differences that approached significance (even at the modest .10 level) in terms of arrest charge severity (felony versus misdemeanor); type of arrest

⁶ The median is a better indicator than the mean of the standard time to complete interviews, because interviews resulting from incentive letters (mailed after phone attempts were exhausted) strongly skew the mean.

Table 5.1. Comparison of Victims Interviewed and Not Interviewed

	Proportion of victims interviewed
Arrest charge class	
Felony (n=57)	25%
Misdemeanor/violation (n=363)	25%
Arrest charge type	
Assault (n=333)	24%
Other (n=87)	31%
Nature of relationship	
Currently married (n=89)	23%
Other (n=290)	29%
Victim injury	
No visible injuries (n=100)	32%
Injury, no treatment (n=175)	27%
Medical treatment (n=103)	23%
Victim noted uncooperative in DA file?	
Yes (n=88)	22%
No (n=291)	30%
Offender age	
30 years or younger	25%
31 years or older	25%
Offender employed	
Employed full-time (n=229)	23%
Other (n=162)	25%
Offender graduated high school	
Yes (n=196)	26%
No (n=193)	23%
Offender had prior arrest(s)	
Yes (n=226)	28%
No (n=194)	21%
Offender had prior conviction(s)	
Yes (n=226)	27%
No (n=194)	24%
Batterer program assignment	
Batterer program (n=202)	23%
No batterer program (n=218)	27%
Monitoring assignment	
Monthly (n=280)	20%
Graduated (n=209)	25%

No differences approached statistical significance ($p > .10$)

charge (assault vs. other); victim/offender relationship (married vs. other); victim injury; or prosecutor notes on victim cooperativeness. Similarly, there were no differences on offender characteristics, including SES indicators (employment and educational level) and prior arrests and convictions.

Most importantly, we found no differences between those interviewed and not interviewed based on the intervention condition (e.g., batterer program or not; and monthly versus graduated

Table 5.2. Initial Differences between Treatment Groups

	Batterer Program	No Batterer Program	Monthly	Graduated
Victim characteristics				
Hispanic	49%	71% +	65%	57%
Born in U.S.	66%	69%	70%	65%
Age (mean)	34.0	30.2 *	33.1	30.6
Years of education (mean)	11.9	11.7	12.0	11.6
Married	38%	43% *	32%	51% *
Lived with offender prior to arrest	38%	63% *	48%	57%
Wanted perpetrator arrested	20%	30%+	67%	84%+
Offender characteristics				
Age (mean)	33.1	28.5**	30.7	30.4
Employed	64%	49%	53%	59%
Hispanic	38%	38%	45%	31%
Felony charge	10%	18%	17%	11%
Prior conviction	48%	37%	38%	46%

***p<.001 **p<.01 *p<.05 +p<.10

monitoring). Thus, although the low interview success rate raises concerns about generalizing results from the survey to the whole sample, we have no indication on the observed variables that the interview sample differs substantially from the portion of the sample not interviewed.

We next focused just on those who were interviewed (n = 106) to determine whether, within the interview sample, there were differences between the cases that were and were not assigned to a batterer program; and between the cases that were assigned to monthly and graduated monitoring. We tested for baseline differences in victim age, education, race, country of origin, relationship to perpetrator, frequency of contact between victim and perpetrator prior to arrest, and victim wish to have the perpetrator arrested; and we also tested for standard offender background characteristics.

The results are displayed in Table 5.2. Since the sample size for the set of cases interviewed is small (N=106), we set p=.10 as the significance threshold in conducting these tests. The table shows that there were several differences between the characteristics of cases assigned to the different experimental conditions. This may seem somewhat surprising since randomization should result in a relatively even distribution of the various values of variables across cases; but the quarter of the cases in which interviews were successful are not, of course, a random subsample of the entire sample of cases.

Victims whose perpetrator was mandated to a batterer program were more likely than those whose perpetrator was not mandated to a program to be black (and less likely to be Latino); more likely to be older; and less likely to have had frequent contact with the perpetrator prior to the arrest. In addition, relative to victims whose perpetrator was sentenced to monthly judicial monitoring, those whose perpetrator was sentenced to graduated monitoring were more likely to be married and more likely to have wanted the perpetrator arrested. Also, concerning offender characteristics, those assigned to a batterer program were significantly older than those not assigned to a program. There were raw percentage differences in other offender characteristics as well but, with the small sample size, these differences did not approach statistical significance.

In sum, while the characteristics of victims interviewed and not interviewed did not differ across the entire sample, among those actually interviewed, several subgroup differences did arise between cases assigned to the different intervention conditions. For this reason, in testing for treatment effects, we control for these initial differences in any analysis where we detected a significant treatment effect based on a bivariate comparison.

Victim Profile

The median age of victims interviewed was 29 years. Eight in ten were involved in a current relationship with the perpetrator, 47% as a girlfriend and 33% as a spouse, while 12% were former girlfriends and 8% former spouses of the perpetrator. Fifty-nine percent finished high school, and 35% had some college experience. More than half of the women (57%) reported salary from employment and 24% reported public assistance as their main source of income. Twenty-nine percent reported having been born outside the U.S., and nearly all of these were from Latin America. Nearly two-thirds (62%) of the women interviewed reported being Hispanic, while most of the remainder (31%) said they were black. Just 5% reported being Caucasian and 3% Asian.

Three-quarters of victims said they wanted the perpetrator arrested, and 62% wanted him prosecuted. Those who wanted the perpetrator prosecuted were evenly divided between wanting jail and wanting a treatment program. All of the victims reported that they were issued an order of protection: All but seven victims said that was what they wanted. Thirteen percent later tried to get a judge to drop the order; and in five cases the judge agreed to do so.

Outcome Results for Entire Sample

Victim Reports of New Abuse

Almost half of the victims (46%) reported experiencing some form of abuse since the perpetrator was sentenced (see Table 5.3). Only a small proportion reported any new physical abuse (15%), and similarly, only a small proportion (18%) had been threatened (with physical attack, with knife or gun, with forced sex, with homicide, or with other harm). Most of the abuse was in the areas of non-physical harm (including harassing phone calls; isolation from friends or activities; stalking; preventing access to money; threats or attacks against children or acquaintances; threats of suicide; and violating private information), with more than four in 10 victims experiencing one of these forms of abuse.

Victim Satisfaction with the Court

Victims had positive feelings about the effect of actions taken by the court. Two in three victims (64%) were satisfied with the sentence in their case; 26% were dissatisfied, and 10% had mixed feelings. In response to an open-ended question, of those who were dissatisfied, 49% expressed that the sentence was not severe enough, 30% expressed that the defendant needed a treatment program, 9% expressed that the sentence was too severe, 9% expressed that the court failed to follow-up with them, and 3% expressed that the sentence failed to help due to the continuation of abuse.

In addition, more than three in four victims (77%) felt that the sentence had increased their safety, while just 9% felt that they were less safe as a result of the court action. Two in three

Table 5.3. Frequency of Post-Sentence Abuse (N=106)

Any form of abuse	46%
Threats ¹	18%
Threat to hit	16%
Threat to attack with weapon	1%
Threat to force sex	3%
Threat to kill	10%
Other threat of bodily harm	2%
Physical abuse ²	15%
Other abuse ³	44%

¹ This includes threats to hit (16%), threats to attack with weapon (1%), threats to force sex (3%), threats to kill (10%), and other threats of bodily harm (2%).

² This includes pushing or shoving (14%); forcibly restraining (7%); pulling hair (6%); slamming against wall (8%); slapping (6%); kicking (2%); punching (5%); throwing something (7%); attacking with weapon (1%); choking (5%); burning (1%); sexual assault (3%); threats to coerce sex (4%); attempts to kill (3%); and other physical attacks (2%).

³ This includes harassing phone calls (26%); socially isolating victim (11%); preventing victim from going out (11%); following victim (9%); preventing access to money (2%); stealing money (8%); confronting victim (19%); harming or threatening children (3%); harming victim's acquaintances (10%); threatening to take his own life (16%); and trying to obtain personal information (20%).

Table 5.4. Changes in Contact with Offenders, Pre-Arrest to One Year Post-Sentencing (N=106)

	Pre-arrest	Post-sentence
Live together	53%	5%
Contact weekly or more	34%	16%
Contact several times per month	5%	16%
Contact less than monthly	1%	15%
No contact for past two months	8%	48%

victims (66%) felt that it was unlikely that the perpetrator would bother them again. Fully 95% of victims said that they would call the police again if a similar event happened in the future.

Victim contact with the perpetrator declined markedly since the sentence was handed down. Although three in four victims (77%) had some form of contact with the perpetrator post-sentencing, the frequency of that contact was far lower than it had been prior to the arrest (see Table 5.4). Only one in three victims reported seeing the perpetrator several times a month or more, while nearly half (48%) said that they had no contact for at least the past two months.

Impact of Batterer Programs and Type of Monitoring

Bivariate Results

Table 5.5 displays the bivariate effects of intervention condition upon dichotomous measures of new abuse. Whether or not victims' cases were assigned to a batterer program had no effect upon new incidents of physical abuse, threats, or other forms of abuse. Similarly, new incidents reported by victims were unaffected by whether the offenders were assigned to monthly or graduated monitoring.⁷

⁷ We ran a negative binomial regression on group differences in the frequency of new abusive incidents, controlling for frequency of contact with the abuser, victim age, and race. The treatment effect did not approach significance.

Table 5.5. Treatment Effects upon Measures of New Abuse

	BP	No BP	Monthly	Graduated
Any New Abuse	45%	48%	43%	50%
Threats	17%	19%	19%	17%
Physical abuse	13%	17%	11%	19%
Other abuse	45%	44%	43%	46%

Table 5.6. Treatment Effects on Victim/Perpetrator Relationships and Court Actions

	BP	No BP	Monthly	Graduated
Satisfied with the sentence	77%	52% **	62%	65%
Offender likely to bother again	27%	38%	39%	27%
Any contact past two months	47%	56%	46%	58%

Table 5.6 displays bivariate effects of treatment condition on victims' relationships and beliefs about the court actions. The table reveals that offender assignment to a batterer program had a significant positive effect on victim satisfaction with the sentence in the case. While victims whose cases were assigned to a batterer program were less likely to report contact with the offender in the past two months and less likely to believe that the offenders would bother them again, these latter effects failed to reach statistical significance. (Also, since victims assigned to a program were less likely to be living with the offender at baseline, the finding relating to post-sentence contact with the offender may have been found to be spurious even if it had reached significance.)

Finally, the results in Table 5.6 reveal no substantial effects of the type of monitoring on victims' relationships or on their beliefs about the court actions. Victims whose perpetrator was assigned to graduated monitoring were somewhat more likely to have had recent contact with the perpetrator and somewhat less likely to believe that the perpetrator would bother them again, but these effects did not approach significance.

Multivariate Results

In light of the small effective sample size for outcomes based on victim interviews, we pursued a limited strategy in conducting multivariate analyses. We decided to control only for those victim characteristics that differentiated the victims linked to different intervention conditions at baseline (see significant differences in reported in Table 5.2 above). Using this strategy, Table 5.7 presents the results of logistic regressions predicting any new abuse, new physical abuse, new threats, and other forms of new abuse. The table confirms the bivariate results indicating no effects of either the batterer program or type of monitoring. With just one exception (frequency of contact for the threats measure), none of the covariates bore a significant relation to abuse in the multivariate models either.

For the victim perception measures, there was one effect that was found to be statistically significant in bivariate analysis: whether victims were satisfied with the sentence in the case. Table 5.8 presents the results of the multivariate analysis of satisfaction with the sentence. In addition to the variables included in the other models, we also examined the impact of whether the offender had prior convictions for domestic violence, arrest charge type (assault or other),

Table 5.7. Regression Models Predicting New Incidents of Abuse

Type of Analysis	Model 1	Model 2	Model 3	Model 4
Dependent Variable	Any New Abuse	Threats	Any Physical Abuse	Other Forms of Abuse
	Odds Ratios			
Assigned to Batterer Program	.798	.862	.621	0.934
Assigned to Monthly Monitoring	.683	1.210	.535	0.813
Race				
Hispanic	0.892	1.346	1.351	0.933
Offender Age	1.013	1.028	1.033	1.013
Frequency of Contact	1.033	.712+	.745	1.065
Married (according to victim reports)	0.734	1.34	1.272	0.845
Nagelkerke R ²	0.019	0.075	0.089	0.009

***p<.001 **p<.01 *p<.05 +p<.10

Note: The N for logistic regression analyses was 106.

Table 5.8. Regression Models Predicting Victim Satisfaction

Type of Analysis	Logistic Regression
Dependent Variable	Victim Satisfaction
	Odds Ratios
Assigned to Batterer Program	3.439*
Assigned to Monthly Monitoring	.863
Race	
Hispanic	0.701
Offender Age	1.033
Frequency of Contact	0.794
Married (according to victim reports)	1.286
Any New Abuse	0.188**
Arrest Charge Severity (felony)	0.275
Severity of Injury to Victim	0.37*
Number of Prior DV Convictions	0.215
Nagelkerke R ²	0.347

***p<.001 **p<.01 *p<.05 +p<.10

Note: The N for logistic regression analyses was 103.

presence of injuries to the victim on the instant case, and whether the victim reported re-abuse. (We hypothesized that reports of re-abuse might be an important control variable, since victims experiencing re-abuse might, in retrospect, express less satisfaction with how the sentence in the instant case was handled.)

The analysis confirmed a significant effect of assignment to a batterer program on victim satisfaction with the sentence. In addition, victim reports of re-abuse and severity of injury had a significantly negative effect on satisfaction with the sentence issued by the court.

Summary

The survey data confirmed the findings based on official criminal justice data: Neither assignment to a batterer program nor type of judicial monitoring affected the likelihood of new abusive incidents in the year after sentencing. However, we did find that assignment to a batterer program gave victims more confidence in the court process, as expressed in greater satisfaction with the sentence in the case. While it is possible that greater victim satisfaction when a batterer program was assigned stems from a false sense of security that the program makes them safer, when in fact it does not, this is not the only interpretation available. In this regard, it is revealing that almost half (49%) of the victims who were *dissatisfied* expressed that they had wanted a more severe sentence, suggesting that some of the victims may have preferred a batterer program for punitive reasons, because it required the men to do more as a consequence of their violence.

Several other findings do not address the effects of batterer programs or type of monitoring per se but are important to observe concerning the positive feedback conveyed by victims about the actions of the court and their own future plans. In particular, more than three in four of the victims interviewed (77%) felt that the sentence in the case had increased their safety, 95% expressed that they would call the police again if a similar event happened, and only one in three victims reported seeing the perpetrator several times a month or more since the sentence was imposed, which represents far less contact than prior to the arrest.

Chapter Six

Results for the Impact of Judicial Monitoring

This chapter compares recidivism outcomes for the randomly assigned offenders, all of whom received some form of judicial monitoring, to offenders sentenced during a contemporaneous period of time neither to monitoring nor a batterer program. The sample not receiving any monitoring was available as a result of standard sentencing practices in the Bronx. Often, the prosecution and defense will agree to a guilty plea on a violation charge with a conditional discharge (CD) sentence involving a one-year order of protection in favor of the victim but without a monitoring or program requirement. This generally occurs when a case is deemed too weak on the legal merits to elicit a more demanding set of conditions, or sometimes for other reasons discussed below. The principal benefits are providing victims with an order of protection and enabling the court to re-sentence the offender to a more serious sanction in response to any violation or re-arrest during the one-year conditional discharge period. Importantly, offenders sentenced in this fashion were automatically ineligible for random assignment, which required that prosecution and defense both agree to monitoring, with the possibility of a batterer program; however, these offenders could be included in a “quasi-experimental” study of the impact of judicial monitoring as compared with its complete absence.

The basic design is depicted below. As discussed in Chapter Three, we implemented a propensity score matching technique designed to limit the final no-monitoring, or “CD-only,” sample to only those offenders whose background characteristics (e.g., criminal history, arrest charges, socio-demographic characteristics, and relationship with the victim) closely match those of the “monitoring” sample drawn from the randomized trial. This technique produced final samples that were comparable on all relevant offender characteristics, suggesting a high degree of internal study validity. After first considering the validity issues, this chapter reports all results testing the operative hypothesis that judicial monitoring deters re-offending.

Sample #1: Monitoring	Sample #2: CD-Only / No Monitoring
<p>Offenders in the Random Assignment: All Sentenced to Monitoring and Half to a Batterer Program</p> <ul style="list-style-type: none"> ■ Initial N = 420 ■ Final N = 387 (after removal of 33 offenders with missing data on key characteristics required for matching with sample #2). 	<p>Offenders Convicted Contemporaneously on Identical Charges and Sentenced to a CD with Neither a Monitoring nor Program Condition</p> <ul style="list-style-type: none"> ■ Initial N = 599 ■ Final N = 219 (after removal of 380 offenders whose background characteristics did not match those of sample #1)

Comparison of Sample Characteristics

Table 6.1 compares the monitoring and CD-only samples on all available background characteristics both before and after the implementation of propensity score matching. Considering first the *initial* samples, as they were prior to matching (Table 6.1, leftmost columns):

- *Socio-demographics*: Offenders sentenced to monitoring were older ($p < .10$), averaged fewer years of schooling ($p < .05$), and were more likely to live with the victim ($p < .05$);
- *Prior criminal behavior*: Offenders sentenced to monitoring were less likely to have prior arrests and convictions ($p < .001$ or $p < .01$ on virtually all criminal history measures);
- *Current charges*: Offenders sentenced to monitoring were more likely to have been charged with assault ($p < .001$) and to face misdemeanor as opposed to felony charges at arrest ($p < .05$); and
- *Case processing*: Offenders sentenced to monitoring averaged significantly *less* processing time from arrest to sentencing ($p < .001$); and had two differences in the identity of the judge who imposed their sentence ($p < .01$ and $p < .10$ respectively).

All of these characteristics with significant differences at the modest .10 level or better were included as independent variables in a logistic regression model designed to predict sample membership (0 = CD-only; 1 = monitoring). An exception was that since the many criminal history measures were all highly inter-correlated, we included only two such measures, the total number of prior convictions and total number of prior domestic violence convictions; including these two measures produced the highest pseudo- R^2 statistic of various combinations of criminal history measures that were included in test models. At this point, 33 monitoring and 72 CD-only offenders were excluded due to missing data on the years of schooling measure, the living situation measure, or both. Table 6.2 gives regression coefficients and significance levels for the resulting model.

The propensity model significantly predicted sample membership ($p < .001$, Nagelkerke pseudo $R^2 = .164$). In other words, the initial CD-only sample did *not* comprise a good match to the initial monitoring sample; rather, many significant differences prevailed. Hence propensity score matching techniques were indeed necessary. Propensity scores were obtained from the regression model and, as discussed in Chapter Three, each offender sentenced to monitoring was matched with the offender sentenced to a CD-only who had the nearest propensity score. After completing the matching process, the 387 monitoring offenders for whom a propensity could be computed were matched to 219 CD-only offenders. As shown in the rightmost columns of Table 7.1, after matching, there was not a single significant difference between the samples at the .05 level.

The Possibility of Unobserved Sample Differences

Having achieved comparable samples on all *observed* characteristics – i.e., on all of the specific characteristics represented in Table 6.1 – it remains possible that the offenders sentenced to monitoring and to a CD-only differed in important *unobserved* ways, not discernible from the available data. For example, one possible reason for a CD-only sentence is a lack of victim cooperation with the prosecution, which would weaken the case's legal merits. Yet, measures of victim cooperation were unavailable for the CD-only offenders, meaning that there was no way

Table 6.1. Baseline Characteristics of Monitoring and CD-Only Samples Before and After Propensity Score Matching

	Pre-Matching		Final Comparisons		Change in Magnitude of Sample Differences
	Monitoring Sample	CD-Only Sample	Monitoring Sample	CD-Only Sample	
Sample Size	(N = 420)	(N = 599)	(N = 387)	(N = 219)	
Basic Demographics					
Race					
Black	40%	42%	40%	42%	-1%
Hispanic	42%	42%	42%	42%	0%
White or other	18%	16%	18%	16%	0%
Age (mean)	30.8 ⁺	31.9	30.7	31.4	-0.4
Years of education (mean)	10.8*	11.2	10.8	11.0	-0.2
Completed high school or G.E.D.	50% ⁺	56%	50%	53%	-2%
Stake in Conformity					
Employed	59%	55%	59%	52%	3%
Years employed (mean, of those employed)	3.8	3.5	3.7	3.2	0.1
Married	26%	23%	26%	24%	-1%
Lives with intimate partner	48%*	41%	48%	47%	-6%
Years living at current address	6.4	6.8	6.5	6.9	0.0
Prior Criminal History					
Prior arrests					
Yes	54%**	63%	54% ⁺	47%	-2%
Mean	2.7***	4.4	2.6	3.0	-1.5
Prior domestic violence arrests (past 3 yrs.)					
Yes	10%**	18%	11%	9%	-6%
Mean	0.1***	0.3	0.1	0.1	-0.2
Prior felony arrests					
Yes	38%***	50%	38%	37%	-11%
Mean	1.1***	2.1	1.1	1.5	-0.6
Prior violent felony arrests					
Yes	22%***	32%	22%	23%	-9%
Mean	0.4**	0.8	0.4	0.5	-0.3
Prior misdemeanor arrests					
Yes	46%**	55%	46%	42%	-5%
Mean	1.6**	2.3	1.5	1.5	-0.8
Prior drug arrests					
Yes	33%**	42%	33%	32%	-8%
Mean	1.0**	1.5	1.0	1.1	-0.4
Prior convictions					
Yes	39%***	53%	39%	39%	-14%
Mean	1.4***	2.4	1.4	1.5	-0.9
Prior domestic violence convictions (past 3 yrs.)					
Yes	4%***	11%	4%	3%	-6%
Mean	0.04***	0.15	0.05	0.04	-0.1
Prior felony convictions					
Yes	14%***	24%	13%	17%	-6%
Mean	0.2***	0.4	0.2	0.3	-0.1
Prior misdemeanor convictions					
Yes	19%***	34%	19%	23%	-11%
Mean	0.6**	1.2	0.5	0.7	-0.5
Prior violation convictions					
Yes	32%**	42%	32%	30%	-8%
Mean	0.7**	0.9	0.6	0.6	-0.3
Prior drug convictions					
Yes	15%**	24%	14%	16%	-7%
Mean	0.4*	0.6	0.3	0.4	-0.1

Table 6.1. (Continued)

	Pre-Matching		Final Comparisons		Change in Magnitude of Sample Differences
	Monitoring Sample	CD-Only Sample	Monitoring Sample	CD-Only Sample	
Sample Size	(N = 420)	(N = 599)	(N = 387)	(N = 219)	
Prior cases with at least one bench warrant					
Yes	30%***	39%	30%	27%	-7%
Mean	0.6***	1.2	0.6	0.8	-0.4
Prior months in jail or prison (mean)	3.4**	6.6	3.1	5.1	-1.2
Current Arrest Charges					
Charge type					
Assault	79%***	52%	79%	74%	-22%
Aggravated harassment	7%***	25%	7%	9%	-16%
Other charges	14%***	23%	13%	17%	-5%
Charge severity: felony (not misdemeanor)	14%*	19%	14%	18%	-1%
Case Processing					
Defense Firm					
Legal Aid	57%	54%	56%	53%	0%
Bronx Defenders	24%	26%	24%	27%	1%
18B Panel (appointed counsel)	13%	14%	14%	15%	0%
Private attorney	5%	6%	6%	5%	0%
Law student	0%	0%	0%	0%	0%
Sentencing judge					
Judge #1	45% ⁺	51%	45%	48%	-3%
Judge #2	23%**	16%	22%	18%	-3%
Judge #3	10%	12%	10%	11%	-1%
Judge #4	16%	16%	15%	16%	1%
Judge #5	6%	6%	7%	7%	0%
Other judge	1%	0%	1%	0%	0%
Processing time: months, arrest to sentencing	2.3***	3.5	2.3	2.5	-1.0

* p < .10 * p < .05 ** p < .01 *** p < .001 (2-tailed t-test)

Note 1: The final CD-only sample is substantially smaller than the pre-matching sample due to the effect of the matching process in removing poor matches. Also, the final monitoring sample loses 33 cases due to missing data on one or more variables, of years of education and lives with intimate partner, that needed to be included in the logistic regression equation predicting sample membership.

Note 2: In the final samples, the only variable with a significant number of missing cases is marital status (39 offenders in monitoring and 38 CD-only offenders). Otherwise, race and employment status are each missing for 1 offender in monitoring; years employed is missing for 2 offenders in monitoring; and defense agency is missing for 1 CD-only offender.

to control for this potential difference. If victim cooperation predicts recidivism – that is, if offenders with cooperating victims are significantly more or less likely to re-offend – then our inability to control for the extent of victim cooperation may subsequently lead to biased outcome results.

To evaluate the possibility that unobserved sample differences created a real bias, we discussed with the supervising District Attorneys and the Project Director of the Bronx Misdemeanor Domestic Violence Court why a case might end in a CD-only sentence. These discussions led to several possibilities listed below:

1. *Lack of victim cooperation:* As already discussed, if the victim does not cooperate with the prosecution, the criminal case is weakened, possibly leading to a CD-only sentence as opposed to one with additional conditions.

Table 6.2. Logistic Regression Model Predicting Sample Membership

Variable	Coefficient
Summary Statistics	
Total sample included in the analysis	914
Offenders sentenced to monitoring	387
Offenders sentenced to CD-only	527
Chi-square for model	118.918
Nagelkerke pseudo R ²	0.164
Logistic Regression Coefficients	
Age	-.101
Years of education	-.050 ⁺
Living with intimate partner	.076
Number of prior convictions ¹	-.063 [*]
Number of prior domestic violence convictions	-.658 [*]
Top arrest charge type ²	
Assault	.808 ^{***}
Aggravated harassment	-.703 [*]
Top arrest charge severity (felony, not misdemeanor)	-.250
Sentencing judge ³	
Judge #1	-.127
Judge #2	.148
Processing time: months, arrest to sentencing ⁴	-.004 ^{***}
Constant	.629

+ p<.10 *p<.05 **p<.01 ***p<.001

Note: The dependent variable is whether the offender was sentenced to a conditional discharge with a monitoring requirement or without such a requirement (0 = CD-Only, 1 = Monitoring). Variables included in the model were significant at the .10 level or better in separate bivariate comparisons (see Table 6.1).

¹ Number of prior convictions was censored at 20 (8 cases originally had more than 20 priors).

² The third unlisted category for top arrest charge included a small number of "other" charges (14% of the monitoring and 23% of the CD-only samples), mostly menacing, criminal contempt, and property-related charges with an underlying domestic violence incident.

³ The third unlisted category included all other sentencing judges (32% of the monitoring and 33% of the CD-only samples).

⁴ Processing time was censored at 872 days/28.6 months (3 cases originally took more time).

2. *Lack of corroborating evidence:* If medical records of injury, a "911" tape, or corroborating affidavits from witnesses do not exist or are unavailable, the criminal case is weakened, also possibly leading to a less severe CD-only sentence.
3. *Serious charges and prior criminal history coupled with a weak case:* If it is believed that an offender poses a risk to the victim (e.g., due to a lengthy criminal history), it is possible that the prosecution will attempt to hold out for a violation conviction with an order of protection in favor of the victim, even if the current case is too weak on the legal merits to obtain a more severe sentence outcome (this may partly explain the observed finding that the initial CD-only sample averaged a more serious prior criminal record).
4. *Program issues:* If the offender's employment precludes attending any program sessions, or if the offender is already enrolled in a program due to a conviction on a previous case, then the parties may agree to a CD-only sentence (this too may partly explain the

observed finding that the initial CD-only sample averaged a more serious prior criminal record).

5. *Interest in disposing the case:* If a case has been pending for a significant period of time (for whatever reason), the parties may seek to reach a disposition by agreeing to a violation conviction with a CD and order of protection only (this may partly explain the observed finding that the initial CD-only sample averaged more time from arrest to sentencing).

Of the above list, we possessed data relevant to reasons #3, #4 and #5 (i.e., we had data on the current charges, prior criminal history, offender employment status, and case processing time prior to sentencing); but we lacked data relevant to reasons #1 and #2 (victim cooperation and corroborating evidence). Fortunately, we were able to obtain measures of victim cooperation and victim injuries as part of data collection for the primary randomized trial. This information, while unavailable for the CD-only sample, did enable us to test whether, at least for those receiving monitoring and thus participating in the initial trial, victim cooperation or injuries predict recidivism. We found that they do not (see results in Chapter 4). Given that those characteristics do not predict our key outcomes of interest, it may be inferred that controlling for them is unnecessary and hence the lack of data for them in the CD-only sample should not create a meaningful bias. It remains possible that the few remaining unobserved factors may be a source of bias (e.g., lack of “911” tapes, lack of corroborating witness affidavits, or other factors we did not identify). Yet, having addressed the major possibilities, it seems reasonable to assume with a high degree of confidence that the quasi-experimental design created comparable samples on the measures that matter most.

Outcome Results

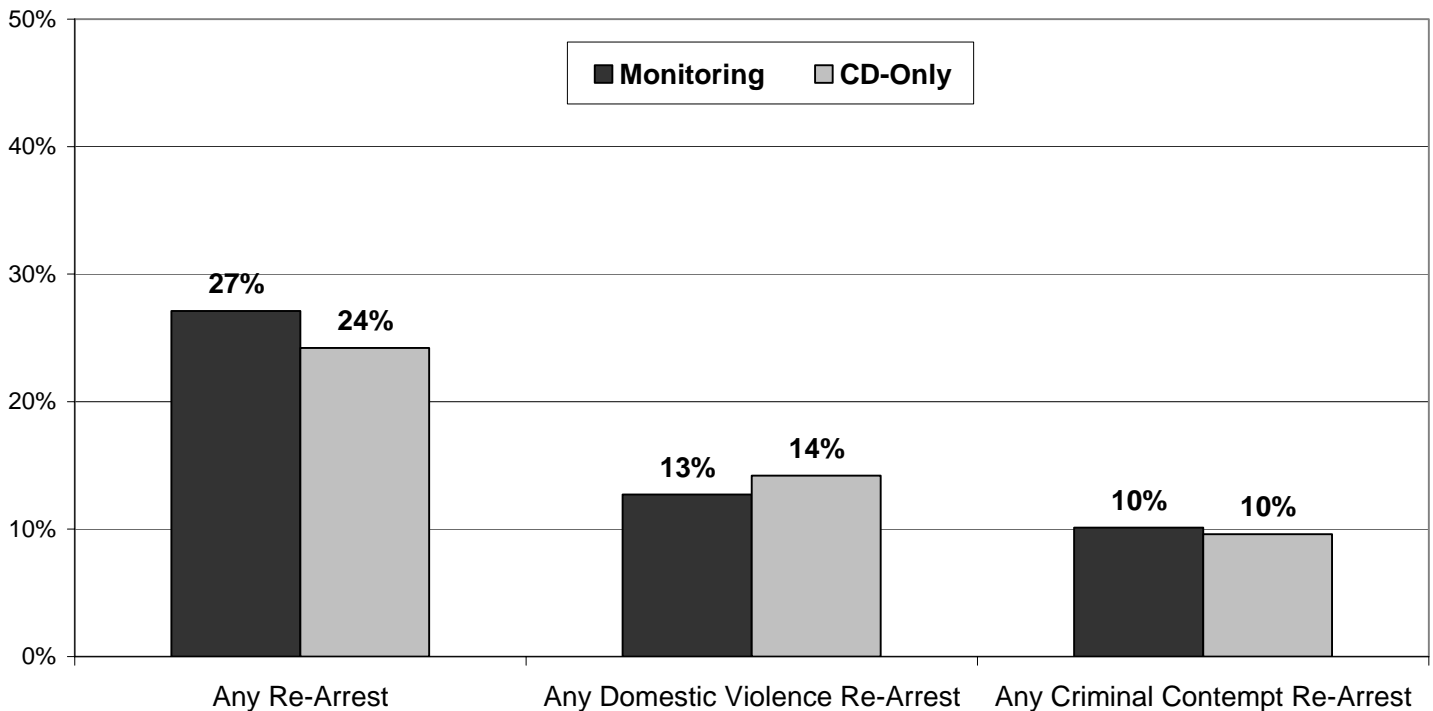
There were no significant differences between the two samples in the probability of re-arrest (see Figure 6.1). Within one year post-sentence, 27% of those sentenced to monitoring and 24% of those sentenced to a CD-only were re-arrested for any crime; 13% and 14%, respectively, were re-arrested for domestic violence; and 10% of both samples were re-arrested on charges including criminal contempt, signaling domestic violence with the same victim.

At the same time, as detailed in Table 6.3, of those re-arrested at least once, offenders sentenced to monitoring averaged fewer *total* re-arrests for any crime ($p < .10$) and for domestic violence in particular ($p < .05$) than those sentenced to a CD-only. In other words, while failing to reduce the probability of re-offending when defined strictly as a yes/no outcome, of those who did re-offend, monitoring appeared to produce a slight reduction in the net quantity of re-offending. However, when extending the tracking period to eighteen months post-sentence, there were not any statistically significant differences on any measure considered.

Predictors of Recidivism: Multivariate Results

The results were further examined using multivariate methods that controlled for other significant predictors of re-offending (e.g., offender race, age, employment status, living situation, prior criminal history, current charges, and case processing time from arrest to sentencing). As in the preceding chapters, these analyses measure effects on both the probability

**Figure 6.1. Impact of Judicial Monitoring on Recidivism:
Percentage of Offenders with Any Re-Arrest and Any Domestic Violence
Re-Arrest within One Year Post-Sentence (N = 606)**



Note: All differences non-significant. A criminal contempt re-arrest involves domestic violence with the same victim as the initial case.

of re-arrest (yes/no) using logistic regression and on the total number of re-arrests using Poisson regression. The models reported in Table 6.4 omit all possible predictor variables that did not show significant effects in simple bivariate associations; and also omit marital status, because it was highly inter-correlated with living situation, and the latter proved to be a stronger predictor of re-arrest outcomes in test models (see simple correlations in Appendix I).

The results again indicate that monitoring did not have a significant impact on the probability of re-arrest, either for any offense or for domestic violence in particular. Monitoring also did not have a significant effect on the total *number* of re-arrests for any offense; however, monitoring appeared to have a modest effect leading to fewer total domestic violence re-arrests ($p < .10$).

Concerning other predictors, similar to the previous analyses conducted in Chapter Four, two general patterns were in evidence. First, when the outcome measures concerned re-arrests for any offense, several predictors were significant, spanning basic demographics (race and age), social characteristics (employment status and living situation), prior criminal history, current charges, and case processing speed. Of these, the strongest and most consistent predictors of recidivism were having more prior criminal convictions and a younger age ($p < .001$ for both). Second, when the outcome measures concerned domestic violence re-arrests in particular, the regression models had less explanatory power overall, fewer individual predictors had significant effects, and age and “stake in conformity” indicators lost their relevance entirely. It would seem that domestic violence offending involves dynamics that are less likely to be captured by standard social background variables.

Table 6.3. Impact of Judicial Monitoring on Recidivism

Recidivism Measure	Monitoring	CD-Only
1. Recidivism within One Year Post-Sentence	(N = 387)	(N = 219)
Probability of Re-Arrest		
Any re-arrest	27%	24%
Any domestic violence re-arrest	13%	14%
Any criminal contempt re-arrest	10%	10%
Any felony re-arrest	11%	12%
Any misdemeanor re-arrest	21%	19%
Frequency of Re-Arrest		
Mean re-arrests		
All offenders	0.41	0.44
Of those with at least one (1) re-arrest	1.5 ⁺	1.83
Mean domestic violence re-arrests		
All offenders	0.15	0.21
Of those with at least one (1) D.V. re-arrest	1.18 [*]	1.52
Mean criminal contempt re-arrests		
All offenders	0.12	0.12
Of those with at least one (1) C.C. re-arrest	1.21	1.24
Mean felony re-arrests		
All offenders	0.14	0.20
Of those with at least one (1) fel. re-arrest	1.23 [*]	1.65
Mean misdemeanor re-arrests		
All offenders	0.27	0.24
Of those with at least one (1) misd. re-arrest	1.30	1.27
2. Recidivism within Eighteen Months Post-Sentence		
Probability of Re-Arrest		
Any re-arrest	32%	33%
Any domestic violence re-arrest	15%	17%
Any felony re-arrest	15%	17%
Any misdemeanor re-arrest	24%	27%
Frequency of Re-Arrest¹		
Mean re-arrests		
All offenders	0.58	0.66
Of those with at least one (1) re-arrest	1.76	2.00
Mean domestic violence re-arrests		
All offenders	0.20	0.27
Of those with at least one (1) D.V. re-arrest	1.26	1.54
Mean felony re-arrests		
All offenders	0.19	0.26
Of those with at least one (1) fel. re-arrest	1.26	1.51
Mean misdemeanor re-arrests		
All offenders	0.39	0.40
Of those with at least one (1) misd. re-arrest	1.58	1.45

⁺ p < .10 ^{*} p < .05 ^{**} p < .01 ^{***} p < .001 (based on the tau-b statistic)

Note on criminal contempt re-arrests: Criminal contempt was only analyzed within one year post-sentence, since a one-year order of protection was standard practice; therefore, such charges would, by definition, be less likely to arise between the one-year and eighteen-month marks.

Table 6.4. Regression Models Predicting Re-Arrest Outcomes within One Year Post-Sentence

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Type of Analysis	Logistic Regression				Poisson Regression			
Dependent Variable	Any Re-Arrest	Any Re-Arrest	Any D.V. Re-Arrest	Any D.V. Re-Arrest	Total Re-Arrests	Total Re-Arrests	Total D.V. Re-Arrests	Total D.V. Re-Arrests
	Odds Ratios				Regression Coefficients			
Sentenced to Monitoring	1.152	1.233	.880	.866	-.878	-.761	-.359 ⁺	-.347 ⁺
Race ²								
Black		1.033		.949		.273***		.156
Hispanic		.584***		.604**		-.269***		
Age		.960***		.980		-.485***		-.135
Employed		.591**		1.107		-.972		
Living with intimate partner		.698 ⁺		1.029		-.125		
Represented by private attorney		.472		1.039		-.591		
Number of prior convictions ³		1.148***		1.138***		.901***		.859***
Arrest charge severity (felony)		1.761*		1.620		.377*		.339
Months, arrest to sentencing ⁴		.998 ⁺		.995*		-.281**		-.448*
Constant	.279***	1.279	.188***	.377	-.727***	.664*	-1.180***	-.800

+ p<.10 *p<.05 **p<.01 ***p<.001

Note: The N for the regression analysis was 603, since data was missing for 3 cases (2 monitoring and 1 CD-only) from the total sample of 606. Also, fewer independent variables were included in the Poisson regression predicting total domestic violence re-arrests; the nature of the distribution (less than 15% of cases with at least one re-arrest and inter-correlations among many of the predictor variables) necessitated limiting the model.

¹ Both dependent variables in the Poisson regressions were censored (total re-arrests at four and total domestic violence re-arrests at three) to avoid the possibility that just a few outliers from one of the independent variable categories might inordinately affect the results.

² A deviation coding scheme was used for race in the logistic regression models, meaning that coefficients are in relation to the average. The third unlisted category combines white (15% of the total sample) and other (2% of the total sample).

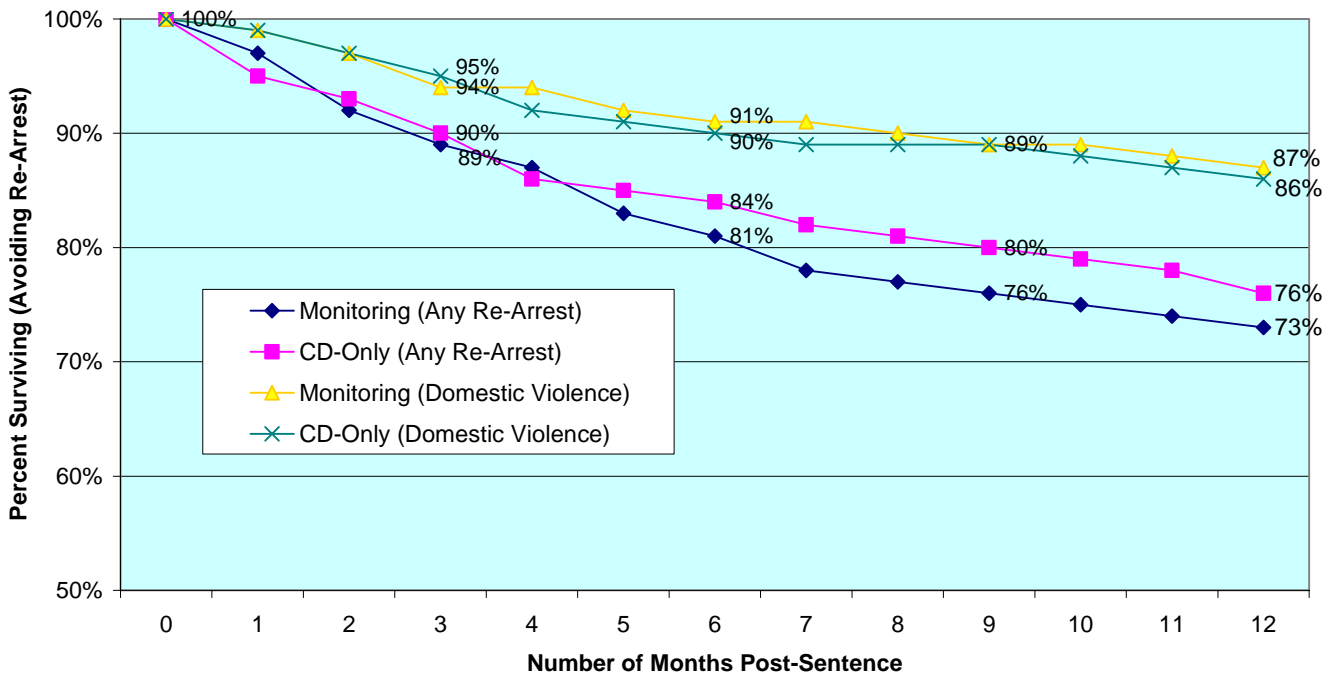
³ Number of prior convictions was censored at 20 (8 cases originally had more than 20 priors).

⁴ Processing time was censored at 872 days/28.6 months (3 cases originally took more time).

Survival Analysis

Although judicial monitoring did not produce a lower probability of recidivism over our entire measurement periods, we considered the possibility that monitoring delayed the onset of recidivism. This possibility was important to explore, since the monitoring period averaged only about seven months, meaning that the court did not directly supervise offenders in the latter part of the one-year tracking period. We found, however, that there were no differences between the groups in “survival time.” Among recidivists, offenders sentenced both to monitoring and to a CD-only averaged exactly 138 days to first re-arrest; and averaged a statistically identical 147 and 145 days to first domestic violence re-arrest. As the survival curves in Figure 6.2 demonstrate, re-offending tends to happen early; of those failing to survive for the full year, approximately two-thirds re-offended within the first six months post-sentence. This can be seen visually in the much steeper downward slope of the curves over the initial six to seven months.

**Figure 6.2. Survival Curves:
Survival Over One Year Post-Sentence for Offenders Sentenced to
Monitoring and to a CD-Only (Any Re-Arrest and Domestic Violence Re-Arrest)**



Note: The survival experience of offenders sentenced to monitoring and to a CD-only does not significantly differ either for any re-arrest or for domestic violence in particular.

Summary

This component of the study tested the hypothesis that judicial monitoring leads to reduced re-offending among domestic violence offenders. We found this not to be the case for nearly all outcomes and measurement periods examined. Offenders who were and were not sentenced to monitoring demonstrated no significant differences in probability of re-arrest for any crime, for domestic violence, or for criminal contempt, which signals domestic violence with the same victim. Nor were there significant differences between the two groups in survival time (the number of crime-free days prior to first re-arrest). One finding did suggest a possible positive impact of judicial monitoring in reducing the total number of domestic violence re-arrests; however, considered in light of a series of other outcome measures for which monitoring did not make a difference, the general finding was that judicial monitoring failed to produce the hypothesized results.

Chapter Seven

Conclusion and Policy Implications

This chapter reviews the major study findings and assesses them in light of the previous literature. We also identify important study limitations. In concluding, we suggest possible new directions for policy and research, drawing on feedback received from practitioners just prior to finalizing this report.

Discussion of Major Findings

The Impact of Batterer Programs

This study's randomized trial failed to demonstrate any impact of attendance at a batterer program on recidivism; both official re-arrest records and victim reports showed nearly identical rates of re-abuse between offenders assigned to a batterer program with judicial monitoring and those assigned to judicial monitoring only.

We did find a beneficial impact of a batterer program sentence on victim perceptions of the court process: Victims whose partners were assigned to a batterer program were more likely to report satisfaction with the sentence in the case. This finding is interesting, as improving victim satisfaction with criminal justice processing is generally no small achievement. Yet, the finding is difficult to interpret in the absence of a reduction in re-abuse or additional information regarding the reasoning of victims. It may only indicate that victims whose partners are mandated to a batterer program are imbued with optimism that in the end is unjustified. It would be troubling if the court's use of batterer programs imparts to victims a false sense that rehabilitation is taking place, when in fact it is not. Another, equally plausible interpretation is that many of the victims surveyed recognized that the batterer program does not make them any safer but wanted the offenders held more accountable by having to attend it as an added punitive requirement. Indeed, of those dissatisfied with the sentence, almost half (49%) expressed that they had wanted the sentence to be more severe than it was. Absent additional information, it is difficult to determine which of these interpretations is accurate (or whether each applies to different subgroups of the victims surveyed).

The preponderance of evidence now accumulated in the field calls into serious question the efficacy of batterer programs based on the most prevalent national models. The main findings from our randomized trial are consistent with those of other recent trials, including three other random assignment studies, none of which found that mandating offenders to a batterer program produces lower rates of re-abuse.⁸ This trial offers particularly strong evidence, since it included two different batterer programs, both based on an educational model, but with substantial differences in curricula (one program included a significant cognitive-behavioral module, and one did not). Also, this study controlled for the potentially confounding effects of judicial monitoring by having all of the offenders in the trial subject to equivalent levels of monitoring.

⁸ The older Canadian study (Palmer et al. 1992) did find a reduction in re-arrests among men sentenced to a batterer program. However, this study is suspect because of a very small N (59 cases) and an extremely large effect size.

The Impact of Judicial Monitoring

We anticipated that monitoring would suppress recidivism, at least during the monitoring period itself. But judicial monitoring had no more impact on re-offending than did batterer programs: When we compared the offenders in our randomized trial – all of whom were monitored – to a matched group of offenders who pled to violations and were sentenced to a conditional discharge without monitoring, we found no differences in nearly all re-arrest measures.⁹ While the design used to assess the effects of monitoring was not randomized, we did employ a control group matched on critical dimensions; thus the design constituted a strong quasi-experiment, and we are reasonably confident of the validity of the findings.

In addition to this comparison of monitoring and no monitoring, the randomized trial allowed for a comparison between two different types of monitoring: monthly and graduated. The latter involved a positive incentive for compliance by rewarding offenders with fewer trips to court and a disincentive for noncompliance by sanctioning offenders with additional trips to court. Yet here, too, neither official re-arrest rates nor victim reports of re-abuse indicated a difference in outcomes.

We were, frankly, surprised to find no effect of judicial monitoring, since there were strong indications from the Brooklyn batterer field test that court control was critical to reducing recidivism (Davis et al. 2000). That study found that offenders assigned to attend a 26-week program had significantly lower re-arrest rates than those assigned to attend an 8-week program. This was true even though both programs involved the same number of class hours and even though the successful completion rate among the 8-week participants was twice as high as among the 26-week participants. Further, offenders who were assigned to a batterer program under a conditional discharge (following a guilty plea) had significantly lower re-arrest rates compared with offenders assigned under an adjournment in contemplation of dismissal (a disposition in which no guilty plea is entered). These findings are consistent with an explanation that greater court control suppresses recidivism; participants in the 26-week group were under court scrutiny for longer than those in the 8-week group, and the participants assigned to a batterer program under a conditional discharge were under threat of resentencing if they violated the terms of the discharge.¹⁰

Yet, the Brooklyn study was not originally designed to test the impact of court control; also, control as conceptualized in that study differed from judicial monitoring as conceptualized in this one. The concept of court control embodies the threat of sanction for failure to comply with a court mandate. That was ostensibly true for judicial monitoring in the present study as well. However, the way that monitoring hearings were conducted in our Bronx site may not have engendered in offenders a real sense that they were under careful scrutiny or that they would face significant consequences for a failure to comply with court requirements. On the other hand, the Brooklyn study did not test any form of judicial monitoring per se (the offenders did not regularly report back to court), but simply involved the overarching threat of sanction posed by the continuance of a program mandate for either 8 or 26 weeks.

⁹ No significant differences were observed when examining the proportion re-arrested, frequency of re-arrest, and the time to first re-arrest over both one year and eighteen month post-sentence periods. The sole exception was that among offenders with at least one re-arrest for domestic violence, we did find that those monitored had somewhat fewer total re-arrests than those not monitored.

¹⁰ Other research has also suggested that monitoring may be effective with domestic violence offenders, though these studies did not involve a rigorous test of its effects on recidivism (e.g., see Gondolf 1998; San Diego Superior Court 2000).

Essentially, the present study isolated the impact of simple surveillance: requiring offenders to report back to a judicial hearing officer multiple times over an approximately seven-month period for a brief compliance check (e.g., of program and court attendance, possible re-arrests, and possible violations of the order of protection). The judicial hearing officer's feedback was brief, matter-of-fact, and often couched in legal terminology that the offenders did not appear to grasp. Indeed, many offenders assigned to graduated monitoring seemed to be under the false impression that they were required to report to court monthly. This would nullify any incentive effects of the graduated monitoring, since offenders cannot respond to an incentive they do not perceive. Moreover, in separate research with drug offenders, a critical factor predicting compliance was the level of information offenders received explaining their responsibilities and the consequences of noncompliance; and the number of times offenders themselves had to echo those responsibilities through their own promises to comply (Young and Belenko 2002).

Further, in the Bronx, the judicial hearing officer lacked the authority to impose sanctions on noncompliant offenders; therefore, without explanation or verbal admonishment, the hearing officer would generally respond simply by telling noncompliant offenders to sit back down until they could be escorted to a different courtroom; in that courtroom, out of sight of the other offenders reporting for monitoring that day, the sentencing judge could re-sentence the offenders to jail, impose other sanctions, or order them to continue monitoring as before. There was not a formal schedule of sanctions that could be shared with the offenders in advance to inform them of what type of court response would be likely to follow various noncompliant behaviors.¹¹

In short, the Bronx court engaged in an extremely common form of judicial monitoring consisting of regular check-ins; but offered limited information about what behavior was expected from offenders and did not implement a rigorous system of incentives designed to induce better compliance. This leads us to conclude that while this study calls into question the effectiveness of simple surveillance, it does not test the effects of a more robust form of judicial supervision. It thus remains to be fully explored whether, for whom, and with what kind of approach to incentives, sanctions, and judicial interaction in the courtroom, monitoring might work with domestic violence offenders.

Study Limitations

Our study population was distinctive in a number of ways. First, the Bronx is a large urban jurisdiction; and most of its domestic violence defendants are nonwhite, unmarried, and low in socioeconomic status. Second, and perhaps more importantly, our sample consists only of offenders allowed to take pleas to violations (though this led to the removal of only about 20% of those who would have otherwise entered the study). Perhaps in part related to their less serious charges, our sample's base re-arrest rate was already very low; just 12% of the offenders not assigned to a batterer program were re-arrested for domestic violence within the primary one year follow-up period. It could be argued that batterer programs or judicial monitoring only have a visible impact on offenders who inflict more serious harm or who have lengthier prior histories of abuse than the offenders we studied. However, with respect to batterer programs, the other recent randomized trials included more dangerous offenders based on the current charges, yet came to similar conclusions. Hence these limitations seem more notable with respect to the

¹¹ When the Bronx Misdemeanor Domestic Violence Court opened in 1998, a formal sanctions schedule existed, but the sentencing judges never followed it, preferring to maintain discretion to consider each case on its merits.

judicial monitoring results, particularly in light of previous research with drug offenders, which found that monitoring is more effective with “high risk” offenders (e.g., Marlowe et al. 2003).

Our sample size was not large enough to determine whether there were any program effects within specific categories of offenders (e.g., those with a more or less extensive prior criminal history; higher or lower socioeconomic profile; or a psychological profile that varies on certain dimensions). It is conceivable that for certain categories of offenders, batterer programs do have a positive impact. (It is equally plausible that for certain types of offenders, batterer programs are deleterious by bringing together violent men for group discussions.) Again, given the convergence between our results and those of other batterer program trials using different populations, we would caution against pinning great hopes on this area. On the other hand, we do believe that this limitation provides good reason to continue experimentation with different approaches to judicial monitoring for different offender populations.

Another limitation with respect to monitoring was described in the preceding section: While we tested the efficacy of simple surveillance, we did not test a robust monitoring regimen incorporating clear, detailed, and repeated instructions to offenders about their responsibilities; nor did we test a rigorous implementation of positive and negative incentives. In addition, the graduated monitoring schedule in the randomized trial was not implemented quite as planned; although offenders were supposed to receive less frequent court appearances in response to compliance and more frequent appearances in response to noncompliance, only the former practice was consistently implemented.

Finally, the victim interviews had several limitations. First, we only obtained a 25% response rate; fortunately, we did not detect any significant differences in the baseline characteristics of cases where we did and did not reach the victim, but it remains possible that certain unobserved differences existed nonetheless. Even so, since the victim interview results mirrored those using official records, we are inclined to attribute a high degree of confidence to both. Second, had we known in advance that victims were going to express a more favorable perception of the sentence when a batterer program was assigned, we would have added interview questions to clarify victims’ reasoning in this regard; for example, it would have been useful to know whether an expectation of rehabilitation contributed to victim satisfaction with a batterer program, whether victims instead preferred the batterer program as a way of holding the men accountable for their violent behavior; or whether other reasoning explained this particular finding. Finally, because the quasi-experimental comparison of monitoring and no-monitoring conditions was added midway through implementation, we did not interview any victims linked to offenders in the no-monitoring sample.

Policy Implications

Our study was a collaborative effort that involved an unusual partnership of stakeholders: judiciary, prosecution, defense, victim services, and batterer programs. In June of 2005, we convened a meeting of the involved parties in the Bronx to discuss the findings of the research and explore future directions. Many were concerned by the study findings and believed that further meetings needed to be held and programmatic changes instituted. (Both the Bronx Criminal Division and the larger New York State court system have since proceeded in this direction.) A number of those present saw the findings as a justification for refocusing resources on victim services. Advocates argued that victims need to be told that, even if their partner is

mandated to a batterer program, they are no safer than if he was not.¹² They also suggested that, since batterer programs do not seem to reduce re-offending, additional resources should be devoted to expanded services that might assist victims, such as shelters or housing relocation.

The Bronx working group discussed the need for continued programmatic experimentation and research to develop ways of dealing with the offenders that will truly make victims safer.

Thoughts on Future Experimentation with Judicial Monitoring

In discussing the failure of our work to show a protective effect of judicial monitoring, several stakeholders opined that, in many or even most courts, monitoring often fails to hold offenders accountable, because noncompliance is not consistently met with sanctions. In other words, the stakeholders believed that judicial monitoring as implemented in the Bronx typified the more general state of the practice, but lamented this reality. Some wondered if monitoring would be more effective if based on a better application of “behavior modification” principles (e.g., involving consistent and certain responses to any infraction). Some raised the particular disadvantage in the Bronx of having monitoring conducted not by the sentencing judge but by a judicial hearing officer, who lacks the authority to respond with immediate sanctions. A memorandum subsequently disseminated by the Deputy Chief Administrative Judge for Operations and Planning in New York State embraces the greater use of rewards and sanctions in response to good or bad behavior by domestic violence offenders under court supervision; and recommends developing compliance scripts designed to explain to offenders in straightforward language how the court will respond to noncompliance (see Appendix J).

In light of previous suggestive research on judicial monitoring and our own finding that those monitored had fewer total domestic violence re-arrests than those not monitored, the efficacy of more rigorous approaches to monitoring remains worth exploring. Such approaches would include significant and sure consequences for noncompliance, public sanctioning of noncompliant offenders, and continued experimentation with the use of positive incentives.

These suggestions regarding experimentation with judicial monitoring practices also have implications for research. Future studies of judicial monitoring would ideally select sites engaged in the kinds of rigorous monitoring practices and uses of incentives suggested by the working group, rather than simple “checking-in” or surveillance. It would also be useful for research to determine the impact of more clearly and frequently communicating responsibilities to the offenders, since one concern with the Bronx site was that the offenders may not have clearly understood the court’s policies, due to the use of brief explanations often laden with legal jargon. Through both structured court observation and systematic interviews with the offenders, it would be useful to examine how much explanation is given to them of their obligations across different court sites, whether communicating more information indeed yields a greater and more accurate understanding among the offenders, and, most importantly, whether noncompliance can be deterred when offenders have a clearer comprehension of the consequences.

Thoughts on Future Experimentation with Batterer Programs

The working group of Bronx stakeholders recognized that many courts are unlikely to suddenly abandon the use of group batterer programs for misdemeanor domestic violence offenders. Neither short-term jail sentences nor fines nor community service are sentencing options that are likely to change the propensity of the offenders to abuse. Since the existing array

¹² Safe Horizon advocates who work with many of the victims in the Bronx indicated that they already discuss the importance of not relying on a batterer program for safety.

of criminal justice sanctions do not appear to be achieving the goal of rehabilitation, some stakeholders expressed that it is especially important to promote the alternative goal of accountability by carefully tracking program attendance and consistently sanctioning those offenders who are noncompliant. Indeed, the DVAP program, one of the two involved in the present study, stresses accountability rather than behavioral change as its principal *raison d'être*.

The group also felt that it is time to explore new kinds of programs, possibly with different conceptual bases and curricula. For instance, some stakeholders believed that batterer programs should move away from a focus on education. One former batterer program director observed that the predominant models assume that education will produce cognitive change, and that will in turn produce behavioral change; but these linkages are tenuous at best, and a more direct behavior modification emphasis might yield better results.

In fact, the research literature has yielded little support for the rehabilitative efficacy of *any* type of batterer program model, including anger management, marital therapy, or cognitive restructuring (e.g., see the review in Bennett and Williams 2004). Yet, the bulk of rigorous batterer program studies have been conducted on programs based on a group educational or cognitive-behavioral model. Other approaches that include substance abuse treatment or employment counseling, for example, have not received much attention from researchers (for an exception, see the evaluation of a Florida program that combined a batterer program with substance abuse counseling by Goldkamp, Weiland, Collins, and White 1996). Since a third of the offenders in our study had a prior criminal history of drug offending, this suggests that experimentation with combined batterer/substance abuse treatment programs may make sense for certain categories of domestic violence offenders. While exploration of alternative models would appear to be in order, improvements in the use of programs as accountability and monitoring tools remain a wise initial step for courts to take, given the present state of the literature on rehabilitation.

Finally, another approach might be to combine new batterer program models with alternative sanctions such as community service. Such sanctions at least have the benefit of “paying back” the larger communities harmed by the criminal behavior when jail is not a feasible sentencing option. Some stakeholders have suggested an approach along the lines of replacing a 26-week batterer program mandate with a six-month community service mandate requiring one day of service each month, coupled with rigorous judicial or probation monitoring and enforcement.

Closing Reflection: Considering Court Responses to Other Types of Offenders

While any comparisons should be formulated with extreme caution, perhaps domestic violence initiatives could benefit from reflecting on current approaches with drug offenders. The increasingly popular drug court model involves a combination of judicial supervision and community-based treatment to encourage recovery. Judicial supervision encompasses regular court appearances, direct and often extensive in-court interaction between judge and defendant, and judicial rewards and sanctions. Many drug courts have elaborated formal response schedules that are shared with defendants and spell out the sanctions and rewards that will be imposed in response to various infractions; and behavioral research suggests that the use of such clear schedules is the preferred approach (e.g., Marlowe and Kirby 1999).

The drug court model of course serves a quite different population than domestic violence offenders and may have limited applicability. In particular, a medical model is generally accepted as appropriate to drug offenders (e.g., they have a disease which needs to be “treated”

and from which they need to “recover”) but is not considered appropriate when discussing domestic violence offenders. Also, drug courts have unique monitoring tools at their disposal, drug testing in particular, to elicit compliance. Finally, drug courts often deal with felony offenders, over whom courts may have more leverage to impose added conditions. Nonetheless, it is worth noting that a series of recent literature reviews all conclude that the drug court model is effective in reducing recidivism (Aos, Phipps, Barnoski, and Lieb 2001; Cissner and Rempel 2005; Government Accountability Office 2005; Roman and DeStefano 2004; Wilson, Mitchell, and MacKenzie 2003).

How does this model differ from popular approaches with misdemeanor domestic violence offenders? First, the dosage of the intervention is exponentially greater. Substance abuse treatment commonly involves from a half to full day of classes attended anywhere from three to seven days per week for a total of six to as many as eighteen months; this compares with just one hour of classes per week over a 26-week or at most a 52-week period in most batterer programs. Second, substance abuse treatment tends to be somewhat more focused on the individual: helping participants work through problems that are triggering their destructive behavior, not educating them concerning how to think about broader societal issues. As Taxman (2002) notes, active engagement of offenders in the change process may be a key component of an effective program. Third, either the court or treatment programs often provide additional services, such as directing participants toward obtaining their high school diploma or G.E.D., participating in job training, and gaining employment. Problems in these areas may have initially contributed to or resulted from a defendant’s drug use and addiction; hence addressing these multiple concerns may be critical to the larger recovery process. Fourth and most relevant, the judicial supervision component in drug courts is far more rigorously developed than is, for example, monitoring as applied in the domestic violence court setting examined within the present study.

Again, we do not suggest that drug and domestic violence offenders are the same or that all practices that have succeeded with one group could or should be transferred to the other; many, and possibly even most, should not. We do suggest that future domestic violence court practice and research could productively draw upon such comparisons to raise new questions; find out what, if any, components of other models might be effective in reducing future violence; adapt those components to the domestic violence population; and continue to push the horizons of thought about how courts can respond to the serious problem of domestic violence.

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Appendix A. Study Implementation Plan

IMPLEMENTATION PLAN

“Testing the Impact of Court Monitoring and Batterer Intervention Programs at the Bronx Misdemeanor Domestic Violence Court” (A Study Funded by the National Institute of Justice)

Purposes of the Research

To control defendant behavior and promote victim safety, a growing number of courts require convicted domestic violence offenders to participate in both a batterer intervention program and judicial monitoring. Recent research suggests that judicial monitoring alone may be effective in reducing offender recidivism, and this research also raises serious questions about the effectiveness of batterer intervention programs. The proposed study seeks to provide a more definitive test of what reduces offender recidivism by distinguishing the respective effects of batterer programs as well as two distinct types of monitoring. The first type will be *monthly* – appearing before a Judicial Hearing Officer each month until fulfillment of a conditional discharge. The second type will be *graduated* – appearing less frequently in response to compliant behavior and more frequently in response to problems. The study will randomly assign defendants to one of four conditions: (1) batterer intervention program and monthly monitoring, (2) batterer intervention program and graduated monitoring, (3) monthly monitoring only, and (4) graduated monitoring only. The study will be implemented at the Bronx Misdemeanor Domestic Violence Court.

The following presents the study implementation plan.

I. The Study-Eligible Population

Eligible defendants are those who, under current procedures, would be convicted of a violation and sentenced to a conditional discharge with a batterer intervention program in Court Parts AP10 or TAP2. All eligible defendants will enter the study with two exceptions. The first is that the sentencing judge has the ability to determine that a particular defendant should be excluded. The second is that defendants represented by neither Legal Aid nor The Bronx Defenders will be excluded at the outset of the study, although they may be included at a later time, after other defense attorneys have learned more about the nature of the study.

(In line with this definition, note that defendants are not study-eligible if they would currently be sentenced to a c.d. with substance abuse treatment, mental health treatment or any other type of program besides batterer intervention. Also, defendants are not eligible if they would currently be sentenced to a c.d. without a program. Finally, defendants are not eligible if they would currently be sentenced to jail, jail followed by a c.d., or probation.)

II. Sample Size and Timing of Study

All eligible defendants will be placed in the study until the sample size reaches 800. (This size may be reduced somewhat if it takes significantly longer than 8 months to reach 800.)

III. Random Assignment Protocols:

Step 1: Determination of Eligibility: The random assignment process will begin once the D.A. and defense attorney / defendant in Court Parts AP10 or TAP2 all indicate that they are prepared to accept a plea to a violation and sentence to a conditional discharge, order of protection, and a batterer intervention program.

Step 2: Allocation: Before proceeding to the allocution, the Judge may exercise the study opt-out provision (see I above). Otherwise, the judge will read from a standard study allocation script developed by several of the judges who rotate between AP10 and TAP2. The script will describe in detail all four groups to which the defendant may be sentenced (copy available upon request). The four groups are:

- (1) Batterer Intervention Program and Monthly Monitoring;
- (2) Batterer Intervention Program and Graduated Monitoring;
- (3) Monthly Monitoring Only; and
- (4) Graduated Monitoring Only.

If the defendant wishes to accept the offer, understanding that there are four possible groups that may be assigned, the random assignment process will continue.

Step 3: Bench Conference and Random Assignment: Note that two persons will always be available to perform the random assignment, enabling one always to be available to each of the two key court parts, AP10 and TAP2. The Project Research Associate, Melissa Labriola, will be assigned to TAP2. Thus at all times, she will either be stationed at her desk in Room 3-29 (718-590-5967) or will be seated in the TAP2 courtroom. (At the outset of the study, she will primarily be seated in the courtroom). Also, the Resource Coordinator or another designated court personnel will be assigned to AP10. At this time, the lawyers of record will approach the bench for a short conference, during which the Research Associate, Resource Coordinator, or other designated person will randomly assign the defendant to one of the four groups noted above.

Step 4: Sentence: Following the bench conference and random assignment, the Judge will proceed on record to sentence the defendant.

Step 5: Next Court Appearance: No change from normal procedure: All defendants will be told to appear before the JHO one month after sentencing. Only subsequently, the appearance schedule will differ between the groups assigned to monthly and to graduated monitoring.

Step 6: Notification of Defendant Responsibilities: Before the defense attorney leaves the courtroom, a designated person will hand to both the attorney and the defendant a sheet briefly summarizing, in clear, readable language, the defendant's responsibilities. Different sheets have been developed for each of the four groups; and sheets are available in English or Spanish. Also, the Judicial Hearing Officer (JHO) will review the responsibilities orally at the first compliance part (DVC) appearance.

Step 7: Assessment: After sentence, and after receiving all relevant paperwork from the court, defendants assigned to groups (1) or (2) will proceed to the Defendant Monitor's office. There the Defendant Monitor will assign these defendants to attend a specific batterer intervention program (the Domestic Violence Accountability Program (DVAP) or Fordham Tremont).

Step 8: Record-Keeping: At the end of each day, the Research Associate will open a data record for each new defendant in the study. Each defendant will be assigned an id number (starting at 1 for the first defendant).

IV. Circumstances Triggering a Forthwith of the Case or a Warning by the JHO

Once a defendant enters the study, the circumstances that will lead the JHO either to forthwith the case back to the sentencing judge or to issue a *warning* to the defendant should follow normal procedures – i.e., it is not necessary to make changes on account of the study. However, especially for defendants assigned to graduated monitoring, it may help to review these circumstances to indicate when the defendant's reporting schedule will be upgraded.

Cases That Must Be Forthwithed to the Sentencing Judge:

1. Failure to make a scheduled DVC appearance;
2. Expulsion from program;
3. Credible allegations of a re-arrest or violation of the Order of Protection; and
4. Report by Safe Horizon staff to the D.A. of a possible violation and / or threat to the victim's safety.

Note that to obtain information on re-arrests, the Research Associate will run a NYSID check in CRIMS, and a DV registry check, on all defendants before each appearance before the JHO.

Situations Requiring a Warning by the JHO:

5. Three absences from the assigned batterer intervention program; and
6. Reports of disruptive behavior at the program.

When the JHO instead issues a warning, note that if the defendant is assigned to graduated monitoring, the reporting schedule will be upgraded and the next DVC appearance date will be two weeks later.

V. Procedures for Referral to the Sentencing Judge:

All procedures concerning the referral of defendants to the sentencing judge will remain unchanged. If after considering the facts, the sentencing judge determines that the defendant will return to DVC for monitoring, the judge will schedule the next DVC appearance date one month later for defendants assigned to monthly monitoring and two weeks later for defendants assigned to graduated monitoring. Importantly, where the sentencing judge would impose jail ordinarily, that should still take place.

VI. Graduated Monitoring Protocols:

Scheduling: The Research Associate will track court appearance dates for all defendants in the study. For defendants assigned to graduated monitoring, on the dates of these defendants' appearances in the Domestic Violence Compliance Part (DVC), the Research Associate will provide the JHO with two follow-up dates. The first proposed date will be scheduled for six or eight-weeks later (depending on the length of time the defendant has been in the study) and will be used if the JHO neither forthwiths the case to the sentencing judge nor issues a warning. The second proposed date will be for two weeks later and will be used if the JHO issues a *warning*. (In the event that the JHO forthwiths the case to the sentencing judge, the JHO will not assign any subsequent DVC appearance date.) The JHO will of course adjust these proposed dates to accommodate particular date/time problems of the court or of the defendant.

Outline of the Graduated Monitoring Schedule:

- (1) For Defendants In Compliance: 4 weeks after sentence, 10 weeks after sentence, 18 weeks after sentence, and 26 weeks (6 months) after sentence.
- (2) For Defendants Determined Out of Compliance: 2 weeks after the non-compliant appearance and two weeks after that; then the schedule will return to the one outlined under (1). When a defendant is placed on an upgraded (i.e., more frequent) reporting schedule, the JHO will state to the defendant that he must report back to see him in two weeks as a result of the warning issued by the JHO or the determination made by the sentencing judge; but if he remains in compliance in the future, the frequency of required appearances will be reduced.

Additional Appearances at the End of the Schedule: At the 6-month appearance, if certain conditions have *not* been fulfilled and an additional appearance would have been required under current procedures, that final appearance will be scheduled at the discretion of the JHO. This applies both to those assigned to monthly and graduated monitoring.

VII. Contacts by Safe Horizon Staff

Daily, the Research Associate will supply Safe Horizon with a list of defendants entering the study, the complainant, docket #, arrest #, NYSID, adjourn date and batterer program that they are attending (if assigned to study groups (1) or (2)). Safe Horizon staff will then

attempt to contact all victims linked to defendants in the study within one month after disposition and between the third and fourth months after disposition. Based upon a conversation with the victim and an assessment of the victim's safety, Safe Horizon staff may notify the D.A.'s office of the results of its assessment.

In addition, Safe Horizon will give the list of study defendants back to the Research Associate with an indication of whether or not the victim was reached and, if so, whether the D.A. was contacted. If the D.A. was contacted, the JHO will be notified at the next scheduled court appearance and will forthwith the case to the sentencing judge. In these instances, the JHO will not discuss any allegation within the DVC Court Part but will simply inform the defendant that a question arose as to the defendant's compliance, necessitating the case be forthwithed to the sentencing judge. At the appearance in front of the sentencing judge, a bench conference will occur and the A.D.A will give the status of the investigation and the judge will determine what, if any, sanction or other action is appropriate.

Additionally, during the second contact, Safe Horizon staff will ask if the client would like to participate in a research interview in eight or nine months (i.e., at the one-year post-sentence mark). If yes, Safe Horizon staff will verify and update the contact information. (When the research interview occurs, all information will be strictly confidential and anonymous and intended for research purposes only.)

VIII. Review of Research Results

Every two months, preliminary research results will be obtained and reported to the court to determine whether there are any clear patterns in the data and, based on the results, to revisit procedures outlined in this document.

IX. Availability of Information on the Study

The final version of this document will be public information and will be available in both Court Parts AP10 and TAP2 to attorneys or others who may be interested. For additional information, please contact: Melissa Labriola, Senior Research Associate, Center for Court Innovation, phone (718) 590-5967 or e-mail mlabriol@courts.state.ny.us.

Appendix B. Allocution Script

Mr. _____

The DA's office and the Court are extending a plea offer to you. Here is how the offer works. If you wish, you may plead guilty to Harassment. Harassment is a violation. It is not a crime. That means this plea will not give you a criminal record. Your sentence will be a conditional discharge. A conditional discharge means that you will not have to go to jail, provided that you satisfy certain conditions.

In your case, there are four different possible groups of conditions that may be required. If you agree to this plea, we will select the particular group of conditions for your case in just a moment. If you agree to this plea, you are saying that you are willing to accept any of the four possible groups or sets of conditions.

I will explain each of the four groups of conditions in detail. But first, a quick and easy way to describe and understand the four groups is to say that:

Group One – includes the batterers' program and regular monthly monitoring;

Group Two – includes the batterers' program and graduated monitoring;

Group Three – includes regular monthly monitoring only; and

Group Four – includes graduated monitoring only.

In any of these groups you will of course be required to obey your Order of Protection. Now let me explain the four groups in a little more detail.

Group #1

If you are selected for Group No. 1, the conditions will be as follows. You will be required to attend and complete a batterers' intervention program. This class meets once a week, with a cost each time you go. You must also return to the court once a month for regular monthly monitoring and obey a (full or limited) order of protection. If you fail to comply with any of these conditions you may be sent to jail.

Group #2

If you are selected for Group No. 2, the conditions will be as follows. You will be required to attend and complete the batterers' intervention program. Once again, this class meets once a week, with a cost each time you go, and if you fail to complete the program, you may be sent to jail.

You will also be required to come back to court for what we call "graduated monitoring". Your first appearance will be one month from now. After that, the frequency of your court appearances will depend on your behavior. If you comply with all of the court orders, you will have to return

to court less frequently. On the other hand, if there has been any problem with your compliance with court orders, you will have to return to court more frequently. You must also obey a (full or limited) order of protection. If you fail to comply with any of these conditions you may be sent to jail.

Group #3

If you are selected for Group No. 3, the conditions will be as follows. You will be required to return to the court once a month for regular monitoring. You must also obey a (full or limited) order of protection. If you fail to comply with any of these conditions you may be sent to jail.

Group #4

If you are selected for Group No. 4, the conditions will be as follows. You will be required to come back to court for graduated monitoring. That means you will return to court in one month for monitoring. The frequency of your court appearances after that will depend on your behavior. If you comply with all of the court orders, you will have to return to court less frequently. On the other hand, if there has been any problem with your compliance with court orders, you will have to return to court more frequently. You must also obey a (full or limited) order of protection. If you fail to comply with any of these conditions you may be sent to jail.

So, to repeat the offer, if you wish to plead guilty to the violation of harassment you may do so, and your sentence will be a conditional discharge, with one of the four groups of conditions that I just described.

Do you understand all that?

Do you want to talk to your lawyer about that before you make up your mind?

Counsel, does the defendant wish to accept the offer? Please offer defendant's plea.

Very good, Mr. _____,

Do you wish to plead guilty to Harassment? Has any one forced you to take this plea? Has any one promised you anything to take this plea? Good. Your plea is accepted. [To the attorneys] Please approach and we will select the terms of the conditional discharge.

Appendix C. Offender Responsibilities

[Note: The following eight sheets are the English and Spanish versions of the descriptions of offender responsibilities handed to offenders in the random assignment and their attorneys. Each offender received the one sheet applicable to him in that offender's preferred language. These sheets were originally designed to print horizontally.]

BATTERERS INTERVENTION PROGRAM AND MONTHLY MONITORING

Responsibilities	Explanation
Batterers Intervention Program and Monthly Court Monitoring	You have been court ordered to: <ul style="list-style-type: none">• Pay for and attend a batterers intervention program once a week for 26 weeks.• Comply with the court's Order of Protection.• Avoid re-arrests and other criminal activity.• Report back to the Bronx Domestic Violence Compliance Part (DVC) on the date indicated below.

You must report back to court on _____.

**BATTERERS INTERVENTION PROGRAM AND
GRADUATED COURT MONITORING**

Responsibilities	Explanation
<p align="center">Batterers Intervention Program and Graduated Court Monitoring</p>	<p>You have been court ordered to:</p> <ul style="list-style-type: none"> • Pay for and attend a batterers intervention program once a week for 26 weeks. • Comply with the court’s Order of Protection. • Avoid re-arrests and other criminal activity. • Report back to the Bronx Domestic Violence Compliance Part (DVC) on the date indicated below.

You must report back to court on

_____.

MONTHLY COURT MONITORING

Responsibilities	Explanation
Monthly Court Monitoring	<p>You have been court ordered to:</p> <ul style="list-style-type: none">• Comply with the court's Order of Protection.• Avoid re-arrests and other criminal activity.• Report back to the Bronx Domestic Violence Compliance Part (DVC) on the date indicated below.

You must report back to court on

_____.

GRADUATED COURT MONITORING

Responsibilities	Explanation
Graduated Court Monitoring	<p>You have been court ordered to:</p> <ul style="list-style-type: none">• Comply with the court’s Order of Protection.• Avoid re-arrests and other criminal activity.• Report back to the Bronx Domestic Violence Compliance Part (DVC) on the date indicated below.

You must report back to court on

_____.

LA DESCRIPCION DEL CONDENADO

Responsabilidades	La explicación
<p>El programa de batterer e regresa al tribunal una vez al mes</p>	<p>Usted necesita a:</p> <ul style="list-style-type: none"> • Asiste un programa de batterer por 26 semanas una vez a la semana. • Evite otro arresto y otra actividad criminal. • Se conforma con la orden de tribunales de la protección. • El regresa al juez una vez al mes para revisar a asistencia de programa de batterer y para cerciorarselo sigue todas instrucciones del tribunal.

Usted necesita regresar al tribunal en

_____.

Usted necesita asistir el programa de batterer en

_____.

LA DESCRIPCION DEL CONDENADO

Responsabilidades	La explicación
<p>El programa de batterer e regresa al tribunal en una programa graduada</p>	<p>Usted necesita a:</p> <ul style="list-style-type: none"> • Asiste un programa de batterer por 26 semanas. • Se conforma con la orden de tribunales de la protección. • Evite otro arresto y otra actividad criminal. • Regresa al juez en un mes. Después que eso, sus fechas de la apariencia al tribunal dependerán de su conducta. Usted vendrá al tribunal menos a menudo si usted sigue todo tribunal ordena y evita otro arresto, pero usted vendrán más a menudo si usted no sigue las órdenes.

Usted necesita regresar al tribunal en

_____.

Usted necesita asistir el programa de batterer en

_____.

LA DESCRIPCION DEL CONDENADO

Responsabilidades	La explicación
Regresa al tribunal una vez al mes	Usted necesita a: <ul style="list-style-type: none">• Se conforma con la orden de tribunales de la protección.• Evite otro arresto y otra actividad criminal.• El Regresa al juez una vez al mes para revisar a asistencia de programa de batterer y para cerciorarselo sigue todas instrucciones del tribunal.

Usted necesita regresar al tribunal en

_____.

LA DESCRIPCION DEL CONDENADO

Responsabilidades	La explicación
Regresa al tribunal en una programa graduada	<p>Usted necesita a:</p> <ul style="list-style-type: none">• Se conforma con la orden de tribunales de la protección.• Evite otro arresto y otra actividad criminal.• Regresa al juez en un mes. Después que eso, sus fechas de la apariencia al tribunal dependerán de su conducta. Usted vendrá al tribunal menos a menudo si usted sigue todo tribunal ordena y evita otro arresto, pero usted vendrán más a menudo si usted no sigue las órdenes.

Usted necesita regresar al tribunal en

Appendix D. List of Stakeholders

The study was planned with the cooperation and input of the following stakeholder offices. (Other offices and batterer program stakeholders became involved subsequently during study implementation and as part of the stakeholder discussion session held in June of 2005.)

1. *New York State Unified Court System:*

- a) Office of the Chief Administrative Judge of the New York City Criminal Court: The then-Chief Administrative Judge of the New York City Criminal Court (who has since been promoted to Deputy Chief Administrative Judge of New York State) and her staff helped to formulate the research questions for the original proposal submitted to NIJ. After the study was funded and while the specific implementation plan was under discussion, this office played a critical convening role, organizing and chairing planning meetings with other stakeholders, advocating for the study to proceed, and recommending refinements to specific protocols. Key staff included the Chief Administrative Judge and three additional staff from her office: Executive Assistant, Director of Domestic Violence Court programs, and Legal Counsel.
- b) Bronx Criminal Court: Important planning input was provided in a series of meetings with the Chief Administrative Judge of the Bronx Criminal Court, the three sentencing judges (two regular and one alternate) then assigned to AP10 and TAP2, the judicial hearing officer assigned to DVC, the Project Director of the Bronx Misdemeanor Domestic Violence Court (BxMDVC), and the Chief Clerk of the BxMDVC. This input largely focused in large part on exactly when the random assignment would occur and on developing the allocution script describing the four sentencing possibilities to the offenders (see Appendix B). Other court staff (the resource coordinator, defendant monitors, court clerks, and security officers assigned to the BxMDVC) were not involved in these meetings; but when a near-final implementation plan was drafted, the research team met with these additional court staff to discuss the study and how it might affect the court process. Some of the staff noticed issues that needed to be dealt with, leading to several final changes.

2. *Bronx District Attorney:*

The research team originally met with the Bronx District Attorney, one of his deputies, and the Bureau Chief and Deputy Bureau Chief of the Domestic Violence and Sex Crime Bureau to gain approval for conducting the study. Approval was provided, so long as the sampling frame was limited to offenders pleading guilty to a violation. Later, the research team met again with the Bureau Chief and the Deputy Bureau Chief to obtain their input on specific implementation issues. It was up to those two senior staff members to inform the assistant district attorneys assigned to the BxMDVC of the details of the study. Also, one of the two chiefs was present in AP10 throughout the first week of implementation.

3. *Bronx Defense Bar*

The research team met separately with the directors of Bronx Legal Aid and Bronx Defenders, the two groups that provide free legal representation to defendants in the Bronx and whose clients make up the majority of defendants in the BxMDVC. A follow-up meeting was also held with senior Legal Aid staff to answer questions and concerns. One important suggestion was to develop a readable, plain-language handout for offenders and their attorneys about the responsibilities of offenders assigned to each of the four study groups (see Appendix C).

Within the court, there is also an 18B panel (consisting of other assigned counsel from multiple firms), and a private attorney presence; but there is no distinct structure to their organizations; thus we could not meet with them beforehand. When they were present in the court, the project research associate gave the details of the study and the opportunity to discuss the details with their clients.

4. *Safe Horizon*

The leading citywide victim advocacy agency was also a key partner in the study. Safe Horizon was responsible for making all contacts with victims in the study, both to check on their safety during the first month post-sentence; to check again between months three and four; and to conduct the one-year research interviews. (Bronx-based victim advocates performed the safety checks, while Safe Horizon research staff performed the one-year interviews.) Directors from Safe Horizon's main citywide office requested this level of participation, and other stakeholders were pleased with Safe Horizon's involvement.

All told, the implementation process involved a series of twelve planning meetings held between December 7, 2001 and July 22, 2002, most of which were convened by staff of the Office of the Chief Administrative Judge of New York City, and approximately half included one or more judges from the two sentencing parts of the domestic violence court.

An important lesson learned from the study implementation process is that a random assignment study of this nature depends on the participation, input, and assent of a wide range of stakeholders, but especially those members of the judiciary who are directly involved in the court process and accountable for the sentencing outcomes. As the above should serve to indicate, the planning process for this study occurred over a substantial period of time spanning over half of calendar year 2002; but was ultimately successful due mainly to the involvement of all relevant parties as well as the guiding support provided by the then Chief Administrative Judge of the New York City Criminal Court.

Appendix E. Comparisons with “Pre-Study” Offenders

Comparison of Baseline Characteristics and Recidivism Outcomes Between Offenders Assigned to a Batterer Program and Monthly Monitoring Before and After Implementation of the Randomized Trial

	Study Sample (Group 1)	Pre-Study Sample
Sample Size	(N = 102)	(N = 154)
A) <u>Baseline Characteristics</u>		
<i>Basic Demographics</i>		
Race		
Black	43%	36%
Hispanic	40%*	55%
White or other	18% ⁺	10%
Age (mean)	31.2	33.0
Years of education (mean)	11.1	11.1
Completed high school or G.E.D.	55%	56%
<i>Stake in Conformity</i>		
Employed		
Employed	61%	59%
Years employed (mean, of those employed)	4.1	4.4
Lives with intimate partner	50%	57%
Years living at current address	6.2	6.3
<i>Prior Criminal History</i>		
Prior arrests		
Yes	51%	54%
Mean	2.4	3.2
Prior domestic violence arrests (past 3 yrs.)		
Yes	8%	12%
Mean	0.09	0.15
Prior felony arrests		
Yes	37%	43%
Mean	1.0*	1.5
Prior violent felony arrests		
Yes	22%***	32%
Mean	0.4**	0.8
Prior misdemeanor arrests		
Yes	47%	46%
Mean	1.4	1.7
Prior drug arrests		
Yes	33%	38%
Mean	1.0	1.2
Prior convictions		
Yes	38%	42%
Mean	1.3	1.7

Appendix E. (Continued)

	Study Sample (Group 1)	Pre-Study Sample
Sample Size	(N = 102)	(N = 154)
Prior domestic violence convictions (past 3 yrs.)		
Yes	4%	5%
Mean	0.05	0.05
Prior felony convictions		
Yes	12% ⁺	20%
Mean	0.2 ^{***}	0.4
Prior misdemeanor convictions		
Yes	21%	23%
Mean	0.4	0.7
Prior violation convictions		
Yes	32% ^{**}	42%
Mean	0.7	0.8
Prior drug convictions		
Yes	14% ⁺	22%
Mean	0.3	0.5
Prior cases with at least one bench warrant		
Yes	28%	37%
Mean	0.6	0.8
Prior months in jail or prison (mean)	2.7	3.6
Current Arrest Charges		
Charge type		
Assault	72% [*]	83%
Aggravated harassment	13% [*]	5%
Other charges	16%	12%
Charge severity: felony (not misdemeanor)	13%	20%
Case Processing		
Processing time: months, arrest to sentencing	2.0 ^{**}	1.3
B) <u>Key Recidivism Outcomes</u>		
Any Re-Arrest	31%	25%
Any Domestic Violence Re-Arrest	15%	14%
Any Criminal Contempt Re-Arrest (same victim)	14%	12%

⁺ p < .10 ^{*} p < .05 ^{**} p < .01 ^{***} p < .001 (2-tailed t-test)

Appendix F. Victim Interview Instrument

BRONX BATTERER INTERVENTION FIELD TEST

VICTIM INTERVIEW

CCI CASE # _____ DATE _____ INTERVIEWER _____

DOCKET # _____ LENGTH OF INTERVIEW _____

NAME OF VICTIM _____ PHONE # _____

Hello, I am _____, calling from Safe Horizon. We are doing a study to understand how the behavior of men who abuse their partners is affected by arrest and prosecution. Information you share with me will be used to help other people like yourself. We are calling about the incident that involved (*INSERT DEFENDANT'S NAME*) who was sentenced on (*INSERT DATE*) in Bronx Criminal Court.

We will pay you \$25 for 15 minutes of your time to answer questions about how you feel about the way your case was handled. The interview is confidential and will not be shared with the police or prosecutor. If you are uncomfortable at any time you are free to not answer any, or all, of the questions. Is this a good time to talk? (If not, when can I call you back? _____) If at any time during the interview, you need to hang up the phone for any reason (such as someone walks in who you do not want to speak in front of), please do so.

1. At the time of arrest, how often did you see (*INSERT DEFENDANT'S NAME*)?
 - (1) Lived together
 - (2) Saw each other one or more times per week
 - (3) Saw each other several time per month
 - (4) Saw each other less than once per month
 - (5) Had not seen each other at all for at least two months
 - (9) D/K

2. At the time of the arrest, what was your relationship to (*INSERT DEFENDANT'S NAME*)?
 - (1) Defendant was victim's current spouse or common-law spouse
 - (2) Defendant was victim's current boyfriend
 - (3) Defendant was victim's ex-spouse
 - (4) Defendant was victim's ex-boyfriend
 - (5) Other--Defendant was victim's _____
 - (9) D/K

3. Did you want him/her arrested?

(1) Yes

(2) No⇒ What did you want instead? _____

⇒ Did you tell the officer not to arrest? ____ Yes ____ No

(8) NA

(9) DK

4. Did you want him/her to go to be prosecuted in court?

(1) Yes⇒ What did you want the court to do?

(READ THE FOLLOWING LIST AND CIRCLE ALL THAT APPLY)

(1) Jail ⇒ How long? _____

(2) Put in treatment program ⇒ what types(s)? _____

(3) Order to stay away from you

(4) Let him go

(5) Restitution

(6) Other ⇒ what? _____

(8) NA

(10) No Opinion

(2) No⇒Did you try to stop the case from going forward?

(1) Yes⇒What did you do?

(2) No

(9) DK

5. Did the judge ever issue an order of protection saying that the defendant must not contact you or must limit contact?

(1) Yes⇒Did you want the court to issue that order? ____ Yes ____ No ____ Don't know

(2) No

(9) DK

(IF NO ⇒ SKIP TO Q7)

6. Did you ever ask the judge to drop the order? ____ Yes ____ No ____ DK

Did the judge ever drop the order? ____ Yes ____ No ____ DK

7. Overall, were you satisfied or dissatisfied with the sentence (*INSERT DEFENDANT'S NAME*) received?

- (1) Satisfied
- (2) In-between satisfied and dissatisfied
- (3) Dissatisfied
- (9) D/K

IF DISSATISFIED: What outcome would you have wanted instead?

8. Do you think the sentence handed down by the court had any effects, good or bad, on your relationship with (*INSERT DEFENDANT'S NAME*)?

- (1) None
- (2) Good effects ⇒ what? _____

- (3) Bad effects ⇒ what? _____

- (9) D/K

9. Did the sentence handed down by the court make you feel more or less safe?

- (1) Very much more safe
- (2) Somewhat more safe
- (3) Neither more safe nor less safe
- (4) Somewhat less safe
- (5) Very much less safe
- (9) D/K

10. How likely do you think it is that (*INSERT DEFENDANT'S NAME*) will bother you again?

- (1) Very likely
- (2) Somewhat likely
- (3) Somewhat unlikely
- (4) Very unlikely
- (9) D/K

11. Have you seen or heard from (*INSERT DEFENDANT'S NAME*) since he was sentenced?

- (1) No
- (2) Yes
- (8) Refused

(IF NO, SKIP TO Q 17)

12. Currently, how often do you see (*INSERT DEFENDANT'S NAME*)?

- (1) Live together
- (2) See each other one or more times per week
- (3) See each other several time per month
- (4) See each other less than once per month
- (5) Have seen or heard from defendant in past two months
- (9) D/K

13. Has (*INSERT DEFENDANT'S NAME*) been physically violent toward you since he was sentenced?

- (1) No
- (2) Yes
- (8) Refused

(IF NO, SKIP TO Q 15)

14. Since he was sentenced, has (INSERT DEFENDANT'S NAME).....			IF YES, ASK: How many separate times has he done this to you since his arrest. WRITE IN NUMBER – IF MORE THAN 20, WRITE '20+'. CIRCLE '888' FOR REFUSED, '999' FOR D/K.		
	NO	YES			
a. Pushed or shoved you in anger?	0	1 →	___ ___	888	999
b. Grabbed, restrained or held on to you against your will?	0	1 →	___ ___	888	999
c. Twisted your arm or pulled your hair?	0	1 →	___ ___	888	999
d. Slammed you against a wall or other hard surface?	0	1 →	___ ___	888	999
E Slapped you?	0	1 →	___ ___	888	999
f. Kicked you?	0	1 →	___ ___	888	999
g. Punched you or hit you with something that could hurt ?	0	1 →	___ ___	888	999
h. Thrown something at you with the intent to hurt you?	0	1 →	___ ___	888	999
i. Stabbed you with a knife or shot you with a gun?	0	1 →	___ ___	888	999
j. Choked or strangled you?	0	1 →	___ ___	888	999
k. Burned or scalded you on purpose?	0	1 →	___ ___	888	999
l. Attempted or committed any unwanted sexual contact such as grabbing or fondling you?	0	1 →	___ ___	888	999
m. Used force or threats of physical force to make you have oral, anal or vaginal sex with him?	0	1 →	___ ___	888	999
n. Attempted to kill you?	0	1 →	___ ___	888	999
o. Physically attacked you in some other way, not already mentioned? (SPECIFY) _____	0	1 →	___ ___	888	999
p. All together, since his arrest, about how many different occurrences have there been in which he physically attacked you in one of the ways we just reviewed?			___ ___	888	999

15. Since he was sentenced, has (<i>INSERT DEFENDANT'S NAME</i>).....			IF YES, ASK: How many separate times has he done this to you since the arrest on __/__/__. WRITE IN NUMBER – IF MORE THAN 20, WRITE '20+'. CIRCLE '888' FOR DON'T KNOW, '999' = REF.		
	NO	YES			
a. <i>Threatened</i> to hit, attack or harm you?	0	1 →	____ ____	888	999
b. <i>Threatened</i> you with a knife or a gun?	0	1 →	____ ____	888	999
c. <i>Threatened</i> to make you have oral, anal or vaginal sex?	0	1 →	____ ____	888	999
d. <i>Threatened</i> to kill you?	0	1 →	____ ____	888	999
e. <i>Threatened</i> you with physical harm in some <u>other</u> way not already mentioned? (SPECIFY) _____	0	1 →	____ ____	888	999

16. Since he was sentenced, has (<i>INSERT DEFENDANT'S NAME</i>).....			IF YES, ASK: How many separate times has he done this to you since the arrest on ___/___/___? WRITE IN NUMBER – IF MORE THAN 20, WRITE '20+'. CIRCLE '888' FOR DON'T KNOW, '999' = REF.		
	NO	YES			
a. Made harassing phone calls?	0	1 →	___ ___	888	999
b. Kept you from spending time or talking with your friends?	0	1 →	___ ___	888	999
c. Stopped you from going some place you wanted to go?	0	1 →	___ ___	888	999
d. Followed you against your will?	0	1 →	___ ___	888	999
e. Kept you from using the family income?	0	1 →	___ ___	888	999
f. Taken or stolen money from you?	0	1 →	___ ___	888	999
g. Confronted you in a public place or your home?	0	1 →	___ ___	888	999
h. Harmed or threatened to harm your children?	0	1 →	___ ___	888	999
i. Seriously harmed or threatened to seriously harm other people you know?	0	1 →	___ ___	888	999
j. Threatened to kill or hurt himself or actually hurt himself?	0	1 →	___ ___	888	999
k. Controlled you in some other way, not already mentioned? (SPECIFY)_____	0	1 →	___ ___	888	999

17. If (*INSERT DEFENDANT'S NAME*) did the same thing to you in the future, would you.....

- (1) Call the police
- (2) Might call the police⇒ Depends on what? _____
- (3) Would not call the police
- (9) D/K

Before we end the interview, I would like to ask you a few questions about your background for statistical purposes.

18. What is your date of birth? _____ (*M/D/Y*)

19. What is the highest grade in school you completed? _____

20. What is your main source of income?

- (1) Own salary
- (2) Support from defendant
- (3) Support from family/friends
- (4) Government assistance
- (5) Unemployment insurance
- (6) Other? _____
- (7) Refused to answer

21. What is your race?

- (1) Caucasian
- (2) African American
- (3) Hispanic
- (4) Asian
- (5) Native American
- (6) Other? _____
- (8) Refused

22. Gender: (1) Male (2) Female

We will send you a check for \$25 for being in our study. It will take about 2 weeks to receive. In order to mail the check, I want to verify the spelling of your name and address.

(FILL OUT PAYMENT FORM)

Thank you very much. Your answers can help criminal justice officials better handle cases like yours and we appreciate your time and patience.

Appendix G. Supplemental Tables for Chapter Four

Appendix G1. Simple Correlations of Case Characteristics with One-Year Post Sentence Re-Arrest Outcomes

Basic Demographics	Any New Arrest	Any New DV Arrest
Basic Demographics		
Race		
black	.188 ***	.101 *
hispanic	-.228 ***	-.190 ***
white/other ¹	.054	.116 *
Age (Total)	-.113 *	.037
Years of education (Total)	.004	.012
Completed high school or G.E.D.	.008	.060
Stake in Conformity		
Employed	-.182 ***	-.053
Years Employed (Total, of those employed) ²	-.056	.021
Married	-.104 *	.008
Lives with intimate partner	-.190 ***	-.076
Years living at current address	-.023	-.028
Victim Characteristics		
Injury to Victim		
No visible injury	.058	.059
Injury but no medical attention received	-.069	-.067
Received medical attention	.020	.016
Victim Cooperation		
Cooperation (of non-missing cases) ³	.062	.020
Non-cooperation (of non-missing cases) ⁴	-.062	-.020
Prior Criminal History		
Prior arrests		
Yes	.198 ***	.052
Total	.203 ***	.165 **
Prior domestic violence arrests (past 3 yrs.)		
Yes	.015	.043
Total	.016	.031
Prior felony arrests		
Yes	.242 ***	.129 **
Total	.129 **	.124 *
Prior violent felony arrests		
Yes	.144 **	.149 **
Total	.077	.110 *
Prior misdemeanor arrests		
Yes	.186 ***	.086 +
Total	.227 ***	.173 ***
Prior drug arrests		
Yes	.256 ***	.114 *
Total	.206 ***	.091 +
Prior convictions		
Yes	.196 ***	.106 *
Total	.207 ***	.179 ***

Appendix G1. (Continued)

	Any New Arrest	Any New DV Arrest
Prior domestic violence convictions (past 3 yrs.)		
Yes	-.019	-.012
Total	-.024	-.016
Prior felony convictions		
Yes	.144 **	.123 *
Total	.106 *	.108 *
Prior violent felony convictions		
Yes	.096 *	.046
Total	.083 +	.026
Prior misdemeanor convictions		
Yes	.161 **	.087 +
Total	.136 **	.146 **
Prior violation convictions		
Yes	.213 ***	.138 **
Total	.236 ***	.170 ***
Prior drug convictions		
Yes	.208 ***	.086 +
Total	.153 **	.080
Prior cases with at least one bench warrant		
Yes	.172 ***	.013
Total	.197 ***	.177 ***
Prior months in jail or prison (Total)	.055	.083 +
Current Arrest Charges		
Charge type		
Assault	.007	.017
Aggravated harassment	-.052	-.060
Other charges	.040	.026
Charge severity: felony (not misdemeanor)	.082 +	.063
Case Processing		
Defense Firm		
Legal Aid	.008	.012
Bronx Defenders	.039	-.015
18B Panel (appointed counsel)	.013	.029
Private attorney/Law student ⁵	-.106 *	-.039
Processing time: months, arrest to sentencing	-.071	-.109 *
In-Program Measures⁶		
At least one warrant	.106 +	.119 *
Terminated from program	.048	.032
Successfully completed CD	-.087	-.117 *
Number of noncompliant events	.239 ***	.138 ***

***p<.001 **p<.01 *p<.05 +p<.10 (based on Tau-B statistic)

¹ "Other" represents only 3% of those in the white/other category.

² This indicates years employed at current job.

³ This variable contains 41% missing data.

⁴ This includes cases with mixed indications of whether there was victim cooperation.

⁵ This includes one case (.2%) where a law student represented defendant.

⁶ These measures were correlated with one-year *post-program* re-arrest measures.

Appendix G2. Regressions Predicting In-Program Recidivism

Type of Analysis	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Dependent Variable	Any Re-Arrest	Any Re-Arrest	Any DV Re-Arrest	Any DV Re-Arrest	Total Re-Arrests	Total Re-Arrests	Total DV Re-Arrests	Total DV Re-Arrests
	Odds Ratios				Regression Coefficients			
Assigned to Batterer Program	1.122	1.154	1.321	1.371	-.274	-.467	.316	
Assigned to Monthly Monitoring	.688	.585	.634	.596	-.359*	-.323+	-.249	-.169
Race								
Hispanic		.459***		.565*				
Black		1.792**		1.288		.862***		.372
Age		.961*		.992		-.398***		.181
Employed		.789		1.252		.686		
Living with Intimate Partner		.418**		.498+		-.114		
Married		.712		1.057		-.137		
Number of prior convictions		1.171**		1.129*		.524*		.595+
Arrest charge severity (felony)		1.706		1.388		.577**		.623*
Months, arrest to sentencing		.942		.903		-.405		
Nagelkerke R ²	.010	.245	.015	.110				

***p<.001 **p<.01 *p<.05 +p<.10

Note: The N for the multivariate regression analyses was 350 due to removal of cases with missing data. Also, fewer independent variables were included in the Poisson regression predicting total domestic violence re-arrests; the nature of the distribution (less than 15% of cases with at least one re-arrest and inter-correlations among many of the predictor variables) necessitated limiting the model.

Appendix G3. Regressions Predicting 18-Month Post-Sentence Recidivism

Type of Analysis	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Dependent Variable	Any Re-Arrest	Any Re-Arrest	Any DV Re-Arrest	Any DV Re-Arrest	Total Re-Arrests	Total Re-Arrests	Total DV Re-Arrests	Total DV Re-Arrests
	Odds Ratios				Regression Coefficients			
Assigned to Batterer Program	1.092	1.028	1.231	1.235	-.930	-.926	.310	
Assigned to Monthly Monitoring	.864	.853	.736	.691	-.245+	-.218+	-.244	-.208
Race								
Hispanic		.456***		.550*				
Black		1.456+		1.331		.486***		
Age		.975		.993		-.509***		-.581
Employed		.670		1.191		.607		
Living with Intimate Partner		.605+		.502+		-.929+		-.512
Married		.977		1.053		-.248		
Number of prior convictions		1.341***		1.14*		.872***		.692**
Arrest charge severity (felony)		1.849+		1.477		.451**		.457
Months, arrest to sentencing		1.011		.889		-.287		
Nagelkerke R ²	.002	.272	.008	.135				

***p<.001 **p<.01 *p<.05 +p<.10

Note: The N for the multivariate regression analyses was 310 due to removal of cases with missing data. Also, fewer independent variables were included in the Poisson regression predicting total domestic violence re-arrests; the nature of the distribution (less than 15% of cases with at least one re-arrest and inter-correlations among many of the predictor variables) necessitated limiting the model.

Appendix G4. Logistic Regression Predicting One-Year Post Program Recidivism

Type of Analysis	Model 1	Model 2	Model 3	Model 4
Dependent Variable	Logistic Regression			
	Any Re-Arrest	Any Re-Arrest	Any DV Re-Arrest	Any DV Re-Arrest
	Odds Ratios			
Assigned to Batterer Program	1.278	1.107	1.600	1.376
Assigned to Monthly Monitoring	1.540	1.514	1.188	1.103
Race				
Hispanic	.796	.834	.602	.622
Black	1.010	.959	1.246	1.142
Age	.966	.969	.989	.992
Employed	.552+	.571+	1.731	1.886
Living with Intimate Partner	.739	.761	.830	.835
Married	1.565	1.607	1.857	1.982
Number of prior convictions	1.233***	1.210***	1.148+	1.128+
Arrest charge severity (felony)	1.019	1.013	1.324	1.264
Months, arrest to sentencing	1.048	1.039	.925	.910
Number of noncompliant in-program events		1.229		1.323
Nagelkerke R ²	.155	.167	.087	.107

***p<.001 **p<.01 *p<.05 +p<.10

Note: The N for the logistic regression analysis was 282 due to removal of cases with missing data..

Appendix H. Offender Interview Instrument

Interview Question Topics and Issues

1. How did you feel about the way you were treated during the process of resolving your criminal court case?
2. How do you feel about the way you were treated during the period after you pled guilty, when you have been reporting back to court?
3. Do you feel like you have a clear idea of what your obligations are to the court? Please explain your responsibilities as best you can.
4. Do you feel like you have a clear idea of the penalties that will arise if you do not meet those obligations?
5. Do you feel the obligations that the court gave to you are reasonable?
6. How often do you report to court? Explain your monitoring schedule.
 - 6a. *If the answer is graduated monitoring schedule:* Is it important to you to make sure to come to court on time and to comply with your obligations, knowing otherwise you would have to report to court more often?
7. *Only for defendants in a batterers program.*
 - 7a. Do you feel that your instructors are effective in teaching the class?
 - 7b. Do you socialize with others in the class?
 - 7c. What do you like most about the program?
 - 7d. What do you like least about the program?
 - 7e. Have you learned anything new?
 - 7f. Do you think the program has made you less likely to use physical force or abusive behavior when conflicts arise? Why or why not?
 - 7g. At the time of sentencing, did you think you could benefit from batterer education? Why or why not? What is your view now?
 - 7h. How could the program be improved?

Appendix I. Supplemental Tables for Chapter Six

Simple Correlations of Case Characteristics with One-Year Post Sentence Re-Arrest Outcomes

	Any New Arrest	Any New DV Arrest
Basic Demographics		
Race		
black	.103 *	.043
hispanic	-.157 ***	-.106 **
white/other ¹	.071 +	.083 *
Age (Total)	-.136 **	-.032
Years of education (Total)	-.028	-.003
Completed high school or G.E.D.	-.036	.001
Stake in Conformity		
Employed	-.161 ***	-.019
Years Employed (Total, of those employed) ²	.005	.052
Married	-.094 *	-.005
Lives with intimate partner	-.121 **	-.010
Years living at current address	.014	-.022
Prior Criminal History		
Prior arrests		
Yes	.208 ***	.066
Total	.214 ***	.114 **
Prior domestic violence arrests (past 3 yrs.)		
Yes	.060	.094 *
Total	.060	.093 *
Prior felony arrests		
Yes	.239 ***	.111 **
Total	.203 ***	.108 **
Prior violent felony arrests		
Yes	.140 ***	.082 *
Total	.128 ***	.082 *
Prior misdemeanor arrests		
Yes	.194 ***	.102 *
Total	.214 ***	.135 ***
Prior drug arrests		
Yes	.286 ***	.156 ***
Total	.273 ***	.159 ***
Prior convictions		
Yes	.210 ***	.137 ***
Total	.216 ***	.152 ***
Prior domestic violence convictions (past 3 yrs.)		
Yes	.014	.021
Total	.014	.020
Prior felony convictions		
Yes	.129 **	.088 *
Total	.125 **	.089 *

Appendix I. (Continued)

	Any New Arrest	Any New DV Arrest
Prior misdemeanor convictions		
Yes	.152 ***	.066
Total	.149 ***	.066 +
Prior violation convictions		
Yes	.209 ***	.179 ***
Total	.220 ***	.191 ***
Prior drug convictions		
Yes	.213 ***	.109 **
Total	.205 ***	.107 **
Prior cases with at least one bench warrant		
Yes	.175 ***	.073 +
Total	.173 ***	.085 *
Prior months in jail or prison (Total)	.135 ***	.083 *
Current Arrest Charges		
Charge type		
Assault	-.014	-.012
Aggravated harassment	-.046	-.040
Other charges	.051	.045
Charge severity: felony (not misdemeanor)	.070 +	.050
Case Processing		
Defense Firm		
Legal Aid	.023	.009
Bronx Defenders	.020	-.001
18B Panel (appointed counsel)	-.005	-.005
Private attorney/Law student ⁵	-.077 +	-.008
Processing time: months, arrest to sentencing	-.093 **	-.092 **

***p<.001 **p<.01 *p<.05 +p<.10 (based on Tau-B statistic)

¹ "Other" represents only 3% of those in the white/other category.

² This indicates years employed at current job.

³ This variable contains 41% missing data.

⁴ This includes cases with mixed indications of whether there was victim cooperation.

⁵ This includes one case (.2%) where a law student represented defendant.

⁶ These measures were correlated with one-year *post-program* re-arrest measures.

**Appendix J. Memorandum to Administrative Judges and Judges of
the Domestic Violence and Integrated Domestic Violence Courts in
New York State**




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JONATHAN LIPPMAN
Chief Administrative Judge

JUDY HARRIS KLUGER
Deputy Chief Administrative Judge
Court Operations & Planning

TO: Administrative Judges and Judges of the Domestic Violence and Integrated Domestic Violence Courts

FROM: Hon. Judy Harris Kluger
Deputy Chief Administrative Judge
Court Operations and Planning 

DATE: August 24, 2005

RE: **Batterers Intervention Program Study**

As some of you may know, the Center for Court Innovation recently completed a study of the effect of batterer intervention programs on recidivism. Attached is a memorandum from the Center for Court Innovation which outlines the goals and preliminary findings of its research and recommended practices with respect to domestic violence cases.

Primarily, the study demonstrates that the use of batterer intervention programs alone does not have an impact on recidivism. Given this finding, it is worth reiterating that batterer intervention programs should not be relied upon to change offenders' behavior. Courts should continue to ensure that enhanced judicial monitoring is in place and, where appropriate, utilize probation and intensified community service as sentence options in domestic violence cases.

I hope that you find the memorandum informative. I will distribute the study when it is released for publication sometime in the fall and inform you of any developments as they occur.

cc Hon. Jonathan Lippman
Hon. Ann T. Pfau
Hon. Joan B. Carey
Hon. Jan H. Plumadore
Hon. Juanita Bing Newton

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Greg Berman, director

Midtown Community Court
Red Hook Community Justice Center
Harlem Community Justice Center
Bronx Community Solutions
Queens Community Clean Up
Brooklyn Treatment Court
Manhattan Family Treatment Court
Brooklyn Domestic Violence Court
Bronx Domestic Violence Courts
Red Hook Youth Court
Harlem Youth Court
Harlem Juvenile Intervention Court
Bronx Juvenile Accountability Court
Brooklyn Mental Health Court
Harlem Reentry Court
Crown Heights Mediation Center

Upstate Office - Syracuse

To: Hon. Judy Harris Kluger, Deputy Chief Administrative Judge for
Court Operations and Planning

Re: Policy Implications of Bronx Recidivism Research

Date: August 22, 2005

Memorandum

Introduction

This memorandum describes a recently completed study by the Center for Court Innovation that examined the impact of batterer intervention programs on defendant recidivism in the Bronx. The study is currently under peer review and will be circulated when it is finalized. In response to the results of this study, we outline possible policy responses and make suggestions for domestic violence and integrated domestic violence court enhancements.

Background

As awareness of domestic violence has increased, so too has the number of domestic violence cases in our courts. Until recently, very little was known about the outcomes of domestic violence cases. Domestic violence and integrated domestic violence courts allow far better tracking of sentences and outcomes, which means that more in-depth study and analysis are possible. For example, we are now able to evaluate batterer intervention programs which have been used by the domestic violence courts and integrated domestic violence courts in New York State.



There has been substantial debate among domestic violence experts about the proper role of batterer intervention programs within the criminal justice system and interest among researchers in their impact.¹ The overall findings of four previous randomized trials conducted in recent years have been mixed, leading many researchers to question whether batterer programs result in any reduction in offending.²

In an effort to address this issue, the Center for Court Innovation received funding from the National Institute of Justice to conduct a study of the impact of batterer programs on recidivism. This study had four goals:

- Test the impact of batterer programs;
- Test the impact of judicial monitoring;
- Test the different impacts of monthly or graduated monitoring; and,
- Stimulate further discussion about domestic violence courts and the handling of domestic violence offenders.

Study intake began in July 2002 at the Bronx Misdemeanor Domestic Violence Court and continued through February 2004. Eligible defendants accepted a plea to a violation and a batterer program and were then randomly assigned into four groups. Group 1 received a sentence of batterer program and monthly monitoring. Group 2 received a sentence of batterer program and graduated monitoring (reduced court appearances in response to compliance and increased appearances in response to noncompliance). Group 3 received monthly monitoring only and Group 4 received graduated monitoring only. Each group had approximately 100 defendants in the sample with a total of 420 defendants involved in the study.

Batterer Program Results: Official Re-Arrest Records

Researchers from our office examined the re-arrest rates for both domestic violence and non-domestic violence offenses one year post-sentence and conclusively found that a batterer program sentence did *not* reduce the chances that defendants would re-offend. There was also no difference in re-arrest rates between those mandated to monthly

¹ See review of the literature in Feder, L. and Wilson, D.B. (2005) "A meta-analytic review of court-mandated batterer intervention programs: Can courts affect abusers' behavior?" *Journal of Experimental Criminology* 1:239. See also, www.opdv.state.ny.us/criminal_justice/corrections/bip/bipchecklist.html.

² Palmer, S., Brown, R., and Barerra, M. (1992) "Group treatment program for abusive husbands," *American Journal of Orthopsychiatry* 62(2):167 (research conducted in Ontario with an extremely small sample size of only 59 men; the study concluded that batterer programs did reduce recidivism); Dunford, F.W. (2000) "The San Diego Navy experiment: An assessment of interventions for men who assault their wives," *Journal of Consulting and Clinical Psychology* 68:468 (research on a San Diego Navy base that found no significant impact); Feder, L. and Dugan, L. (2002) "A test of the efficacy of court-mandated counseling for domestic violence offenders: The Broward County experiment," *Justice Quarterly* 19(2):343 (Florida study of 404 defendants also found no significant difference); Taylor, B.G., Davis, R.C., and Maxwell, C.D. (2001) "Effects of a group batterer treatment program: A randomized experiment," *Justice Quarterly* 18(1):171 (study of 376 misdemeanor domestic offenders in Brooklyn that yielded ambiguous findings and led one of the principal investigators, Rob Davis, to become a partner in the Center for Court Innovation project.)

monitoring and those mandated to graduated monitoring. This held true both for domestic violence re-arrests and non-domestic violence re-arrests.

Other notable findings include:

- Prior criminal convictions significantly predicted re-arrest for any crime and for domestic violence crime in particular;
- Victims were significantly less likely to be living with offenders one year post-sentence (5%) than at the time of arrest (53%);
- Younger age was a strong predictor of re-arrest for any crime but not for domestic violence crime.

Batterer Program Results: Victim Interviews

Because of the concern that domestic violence incidents may not be reported to the police and may not result in arrests, researchers also reached out to victims/complaining witnesses one year post-sentence to check on the actual incidence of re-offending, rather than simply relying on official arrest data. Researchers were able to contact approximately 100 victims. The victims reported more than three times as many domestic violence incidents as were reflected in the arrest data. But this finding did not change the conclusion about batterer programs: there were no significant differences in the percentage of victims reporting new abuses in the batterer and non-batterer program groups.

There were several other interesting findings concerning victim response. First, victims were substantially more satisfied with the case outcome in those cases where the offender received a sentence including a batterer program. Additionally, victims were more likely to believe that the case outcome had a beneficial impact on their relationship when the offender was sentenced to a batterer program than those who were not. The impact of graduated versus monthly monitoring had no impact under any analysis.

Judicial Monitoring

But what about judicial monitoring itself? Did it reduce re-offending? Prior studies with substance-abusing defendants in drug courts suggested that it does. The results of the Bronx study, however, are more ambiguous.

Researchers tested the hypothesis that judicial monitoring leads to reduced re-offending among domestic violence offenders by comparing those defendants who received a conditional discharge with no monitoring to defendants who received some form of monitoring (either monthly court dates or court dates that varied depending on their levels of compliance). In an analysis of one-year post-sentence arrest rates, the study found no significant difference between defendants who were monitored and defendants who were not. Findings *did* suggest a possible positive impact of judicial monitoring on the total number of domestic violence re-arrests. In other words, judicial monitoring did appear to have a modest effect in reducing the total number of domestic violence re-arrests but did not affect the probability that at least one re-arrest took place.

Policy Implications and Suggestions

In many respects, the results of the Bronx study are unsurprising. Indeed, the New York State Unified Court System has consistently urged domestic violence courts to use batterer programs only as part of a system of accountability for defendants, rather than as a “cure” for domestic violence or as a diversion from formal criminal justice case processing.³ Our findings confirm this approach: Batterer programs have not demonstrated success at rehabilitating batterers. Given what we know about batterer programs, it makes sense to think about experimenting with new options, including using probation, community service mandates and more vigorous monitoring protocols.

Using Probation and Community Service Sanctions

The use of probation in domestic violence cases varies widely across New York both because of local practice and local resources. Studies suggest that probation may be effective in reducing domestic violence re-offending.⁴ It is worth, therefore, exploring whether courts can do more in collaboration with their local probation departments to monitor defendants.

Ordering a defendant to participate in community service, usually in conjunction with probation, is an additional option. In thinking about this option for domestic violence offenders, courts should consider the following:

- Is the community service program able to report back to the court on the offender’s compliance?
- Is there a procedure to ensure that referrals are actually followed up on and noncompliance can be addressed through the imposition of further sanctions?
- Can the community service requirement be spread out over time in order to provide regular reports to the court for monitoring?

Community service should be employed as part of a court sentence. One possible model might involve replacing a 26-week batterer program mandate with a 6-month community service mandate requiring one day of community service each month coupled with rigorous judicial supervision and enforcement.

Enhanced Monitoring Protocols

Our study tracked the effects of relatively low-level monitoring. It is worth considering whether increasing the intensity of monitoring might improve its effectiveness. Monitoring can be enhanced in several ways: first, by increasing the frequency; second, by increasing the differential responses to compliance or non-compliance; third, by

³ See Center for Court Innovation Fact Sheet: “Batterers Programs: What You Need to Know”; Report by the Domestic Violence Committee of the Association of the Bar of the City of New York, “Choosing Between Batterer Education Program Models: Recommendations to the New York City Domestic Violence Criminal and Family Court Judges,” distributed statewide by Judge Judy Harris Kluger, Deputy Chief Administrative Judge for Court Operations and Planning, New York State Office of Court Administration.

⁴ Klein, A.R. (2005) “Specialized probation supervision of batterers prevents re-abuse,” *Domestic Violence Report* 11(3):3.

improving the flow of information between the court and victim service and other agencies.

The Bronx study indicates that judicial monitoring may be effective with those defendants who would otherwise commit multiple re-offenses. Based on this finding and other studies on the impact of judicial monitoring,⁵ it is worth exploring whether requiring defendants to reappear on a bi-monthly (twice per month) or even weekly basis, rather than monthly, might lead to increased compliance. (Drug court studies demonstrating the effectiveness of monitoring involved participants who reported to court more frequently than monthly.)

Courts should also consider developing additional responses to compliance and non-compliance that are both clear and consistent. Where appropriate, violations of the court's order can result in the imposition of jail. There are many other sanctions the court can impose short of jail. The research literature indicates that consistency and certainty are critical: that it is important for the same types of actions by defendants to be met with the same responses from the court – and that no infraction occur without some judicial response.⁶ The literature also indicates that defendants must perceive that noncompliance will be met with swift and certain sanctions in order to have any hope of deterrence.⁶ Therefore, it is worth taking the time to explain to defendants, using straightforward language, how the court will respond to compliance and non-compliance.

Additionally, in order to ensure consistent responses, courts should have all available information regarding offender compliance and non-compliance prior to monitoring appearances. Court staff should run registry, NYSID, domestic incident report, and probation checks on each defendant. During the compliance appearance, the defendant should be made aware that the court is consistently checking these sources and will be notified of any violations. Courts should also discuss improving communication among victim service providers, probation, police and other agencies regarding compliance dates. Victim advocates can be encouraged to check in with the complaining witness/victim before each compliance date and, where the victim consents, make a report to the court concerning the defendant's behavior. If the court is aware of non-compliance, the defense counsel must be notified immediately.

⁵Marlowe, D. B., Festinger, D. S., Lee, P. A., Schepise, M. M., Hazzard, J. E. R., Merrill, J. C., Mulvaney, F. D., and McLellan, A. T. (2003) "Are judicial status hearings a key component of drug court? During-treatment data from a randomized trial." *Criminal Justice and Behavior* 30: 141-162.

⁶Marlow, D. and Kirby K. (1999), "Effective use of sanctions in drug courts: Lessons from behavioral research." *National Drug Court Institutional Review* 2(1): 1-31.

⁷Young, D. and Belenko, S. (2002) "Program retention and perceived coercion in three models of mandatory drug treatment." *Journal of Drug Issues* 22 (2): 297-328.

Conclusion

This memorandum is intended to offer a brief overview of the findings of our Bronx study as well as some suggestions for how courts might respond. We are, of course, available and eager to work with you in the days ahead to test these ideas and to generate additional ones as well. The good news is that overall rates of domestic violence are going down both nationally and statewide. Our goal is to keep up the momentum, searching for additional tools to reduce it even further. We look forward to working with you on this effort.