THE INDIANAPOLIS DOMESTIC VIOLENCE PROSECUTION EXPERIMENT

Final Report
for research conducted under
NIJ grant number 86-IJ-CX-0012

Submitted to
The National Institute of Justice

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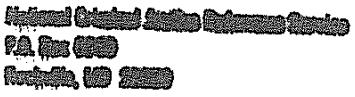
October, 1993

The Indianapolis Prosecution Experiment was made possible with additional funds from Ruth Rodefeld, from University Research Associates, from Indiana University, and from the Family Research Laboratory, University of New Hampshire, under a National Institute of Mental Health Post-Doctoral Fellowship, grant #MH15161-13. The views expressed are those of the researchers and not necessarily those of the funding agencies or of cooperating Marion County criminal justice agencies.



Acknowledgements

The Indianapolis Domestic violence Prosecution Experiment was conducted with the extraordinary cooperation of several criminal justice agencies, including especially personnel of the Marion County Prosecutor's Office, the Marion County Municipal Courts and the Probation Department, the Indianapolis Police Department, and the Marion County Sheriff's Department. The respective administrators, former Prosecutor Stephen Goldsmith, former Presiding Judge Harold Kohlmeyer (now deceased), former Chief Paul Annee, former Chief and current Sheriff Joseph McAtee, and former Sheriff James Wells (now deceased) not only authorized the research, but also lent full support of their offices and staff.

The success of the experiment depended on day-to-day commitment to its implementation. Such was insured by former Deputy Prosecutor Ruth Reichard and by Judge Evan Goodman. The research simply would not have happened without their dedication.

Literally scores of employees from the various agencies assisted the research (and tolerated its intrusiveness) in the course of their normal job responsibilities. We are grateful to all. Several were exceptional in making certain that their activities recognized and facilitated this research. At risk of omitting some names, we wish to acknowledge the following individuals from participating agencies: Faye Bailey, Jon Bailey, Pete Bolles, Ruth Brown, Jim Campbell, Mike Cook, Gary Crane, Linda Crocheran, Billy Crowe, Bertha English, Theresa Farrington, Glen Folco, Joy Haggarty, Pam Kramer-Gaunt, Keith Matthews, Ellen O'Conner, Mike Pfau, Jean Ritsema, Melanie Schwartz, Mark Stoner.

NIJ Project Managers Joel Garner and Winifred Reed offered much-appreciated support, tolerance, and encouragement throughout the project.

Others provided helpful consultation at various points during our work: Frank Dunford, Wendy Ford, Terrance Lankford, Joan McCord, H. Laurence Ross, Karl Schuessler, Murray Straus, Neil Weiner.

Finally, we want to thank members of the research team whose talents and special efforts brought it all together: Donna Augenbergs, Deborah Long, Sam Wheeler, Marshall Walz, Nancy Jewell, Tonya Conour, Denise Pate, Ruth Rodefeld, Beckie Hostetter, Patricia Liell, Leigh Turner, Erin McDonald, Linda Bellner, Stephanie Miller, Lucinda Fuller-Bassili, Barbara Bogue.

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ABSTRACT

This Final Report describes the design, implementation, and outcomes of the Indianapolis Domestic Violence Prosecution Experiment (IDVPE) -- a randomized field experiment on the specific preventive effects of alternative criminal justice policies for processing misdemeanor wife battery cases. The study evaluated policies meant to protect battered women from suspects brought to the prosecution process following either a police officer's warrantless, on-scene arrest or a victim's complaint directly to the prosecutor. Suspects included all men formally charged with a misdemeanor assault against a female conjugal partner in Indianapolis between June, 1986, and August, 1987, who met certain eligibility requirements (e.g., no previous conviction for felony violence).

The IDVPE is actually two simultaneous experiments distinguished by the manner in which defendants entered the system. The On-Scene Warrantless Arrest (OSA) Experiment involved 198 suspects arrested at the scene of a violent domestic disturbance. The Victim-Initiated Complaint (VC) Experiment involved 480 suspects identified by victims' affidavits filed at the Prosecutor's Office. Based on randomized policy recommendations, prosecutors tracked cases toward one of three outcomes -- pretrial diversion to rehabilitative counseling; adjudicated guilt with counseling as a condition of probation; or other sentencing such as fines, probation, and jail time. Victims were not permitted to "drop charges" under any of these policy tracks. Each VC defendant was also processed according to a randomized "entry" condition (summons or warrant) and a fourth randomized prosecution policy allowing the victim to drop charges.

Following settlement in court, each case was monitored to determine whether any of the alternative criminal justice policies had, after six months, reduced any of several measures of repeat violence against the same victim. Data were gathered through interviews with victims, through official records, and through interviews with the accused offenders. Outcome measures include indicators of the prevalence, severity, and frequency of violence in each relationship and the length of time until a new episode of violence following court settlement.

Analysis of OSA cases shows that neither of the policy alternatives to traditional sentencing is any more effective in protecting victims six months following case settlement. Similarly, among VC cases, neither of the "no drop" alternatives to traditional sentencing is significantly more effective. However, when VC defendants are arrested under a warrant and their victims are permitted to drop charges, the women are significantly more likely to be safe from continuing violence. This finding is interpreted in terms of criminal justice serving to empower victims to make informal arrangements for their safety in alliance with agents of criminal justice.

SUMMARY OF RESEARCH FINDINGS

Characteristics of Cases in the Experiments:

- The majority (71%) of misdemeanor wife battery cases filed with the Marion County Prosecutor in 1986-87 were initiated by victim complaints (VC cases) rather than by warrantless on-scene police arrests (OSA cases).

- The average age of men in each sample was just under 31 years; their victims were about 2 years younger. Sixty percent of OSA defendants and 54% of VC defendants were white; 40% of defendants and 38% of victims did not graduate from high school; 31% of OSA defendants and 24% of VC defendants were unemployed.

- Suspects who lived with their victims were more likely to be arrested on-scene than accused in a victim's complaint to the Prosecutor. Among OSA couples, 41% were married and cohabiting, and 12% were separated or divorced; 27% were unmarried but cohabiting, and 14% previously cohabited. Among VC couples, 31% were married and cohabiting, and 17% were separated or divorced; 20% were unmarried but cohabiting, and 25% previously cohabited. Sixty-one percent of the couples in both samples had children together.

- Seventy-four percent of all defendants had been arrested before, 21% for crimes of violence. OSA and VC defendants had comparable prior criminal histories.

Findings from the On-Scene Arrest and Prosecution Experiment:

- By victim accounts, 75% of OSA defendants had battered them at least once before in the six months prior to the study incident; 20% battered the same victims again before their cases were settled in court.

- Thirty-eight percent of OSA defendants battered the same women at least once in the six months following case settlement. There is no evidence from victim reports in followup interviews that any one prosecution policy has a unique preventive impact on the prevalence of six-month followup violence.

- There is no evidence from OSA victim followup interviews that any one prosecution policy has a unique preventive impact on the severity of six-month followup violence; 26% of OSA victims reported incidents of severe followup violence.

- The mean frequency of battering by OSA defendants during the followup period was 4.6 incidents. There is no evidence from victim interviews that alternative prosecution

policies had any differential impact on the frequency of new violence. Nor did they differ on the timing of new violence in the six months following court settlement.

- Sixty-four percent of OSA victim interviewees said they felt more secure and 75% reported feeling in greater control six months after their cases were settled than they did before. There were no significant differences across prosecution policies.

- Although prosecution policy tracks ended in desired court outcomes in just 58% of the cases, there were no significant differences in the prevalence of followup violence across court outcomes.

Findings from the Victim-Complaint and Prosecution Experiment:

- By victim accounts, 72% of VC defendants had battered them at least once before in the six months prior to the study incident; 27% of VC defendants summoned to court and 19% of those arrested on a warrant had battered their victims again before their cases were settled.

- Twenty-nine percent of all VC defendants battered the same women at least once in the six months following case settlement.

- In contrast to the traditional policy of summoning a suspect to court and tracking him toward a conviction with "other" sentencing (Summons/Other), only the policy of arresting on a warrant and permitting the victim to drop charges (Warrant/Drop-Permitted) resulted in a significantly lower percentage of defendants who battered the same victim in a six-month followup period (44% versus 13%).

- Nineteen percent of all VC victims reported severe six-month followup violence. The traditional Summons/Other policy had the highest prevalence of severe violence (36%).

- The mean frequency of violent assaults by VC defendants in the six-month followup period was 2.8 assaults. The rate for Warrant/Drop-Permitted (.45) was significantly lower than for Summons/Other (6.85).

- Twelve percent of VC defendants battered their victims anew within 30 days of case settlement. The Warrant/Drop-Permitted policy resulted in significantly lower rates of violence throughout the six-month followup period in comparison to Summons/Other.

- Sixty-nine percent of VC victim-interviewees said they felt more secure and 77% reported feeling in greater control six months after their cases were settled than they did before. There were no significant differences across prosecution policies.

- Prosecution policy in VC cases resulted in desired court outcomes in 67% of the cases, and there were no significant differences in the prevalence of followup violence across court outcomes. However, whether a case entered by summons or by warrant, victims who were permitted to drop but elected to pursue charges were significantly less likely to be battered again during the six-month followup period.

I. INTRODUCTION

The Indianapolis Domestic Violence Prosecution Experiment (IDVPE) was initiated in March, 1986, to look beyond police intervention in cases of domestic violence for other criminal justice experiences with specific preventive impacts on potential repeat offenders. Following the lead of the Minneapolis Experiment on police practices (Sherman and Berk, 1984a,b), the Indianapolis Experiment examined the effectiveness of alternative prosecutorial policies in reducing the chance of renewed violence by an accused wife-batterer against his partner within six months of case settlement.[1]

The IDVPE is actually two randomized experiments implemented simultaneously and distinguished by the manner in which defendants entered the prosecution system. The first, the On-Scene Arrest and Prosecution (OSA) experiment, involved suspects arrested by police officers responding to the scene of a violent domestic disturbance. Each OSA defendant was randomly assigned to one of three prosecutorial tracks under a "no drop" policy for subsequent processing toward adjudication. The second experiment, the Victim-Complaint and Prosecution (VC) experiment, involved suspects identified by victims' sworn affidavits filed in person with the Prosecutor's Office. Each VC defendant was processed following random assignment to both an "entry" condition and one of four prosecutorial tracks. Thus, with two modes of entry and four tracks, a VC case was processed under one of eight equally likely treatments.

This report presents findings from both experiments on the relative effectiveness of alternative prosecution policies in preventing suspected wife batterers from continuing to abuse their partners.

The research design called for information on the violent behavior of men in conjugal relationships who were brought to the attention of the Prosecutor's Office by on-scene arrest or by victim complaint over the course of a year. Each victim was interviewed shortly after

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[1] The term "case settlement" is used herein to indicate that a case was processed to a point of dismissal or admitted guilt, but not necessarily full adjudication. For example, we describe a court-approved diversion agreement as a "settlement," even though it required a final court appearance some months later.

the criminal incident. Each defendant was interviewed later, after his case was dismissed, diverted, or adjudicated. These interviews gathered background information on the couple's relationship, their descriptions of violent episodes, and their perceptions of various aspects of the criminal justice process.

Interest in the role and effects of criminal justice intervention in controlling domestic violence has increased in the time since the IDVPE was first proposed. Several writers have pointed to the need for rigorous research to address the issue (e.g., Dutton, 1986; Goolkasian, 1986b; Lempert, 1987; Elliott, 1989). Findings from NIJ-funded police arrest experiments point to a need for studying broader criminal justice policies.[2] The IDVPE was timely in capitalizing on an extraordinary opportunity for comprehensive research on prosecuting wife batterers. Its successful completion provides findings informed by theory with immediate significance to practitioners seeking effective policies for controlling wife batterers. Perhaps most important, this experiment brings balance to discussions of criminal justice impacts heretofore dominated by concern over police actions without regard for prosecutorial and judicial processing.

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[2] See, for example, articles and reports on Omaha (Dunford, 1992; Dunford, Huizinga, and Elliott, 1989, 1990), Milwaukee (Sherman, et al., 1991, 1992) Charlotte (Hirschel and Hutchison, 1992), Colorado Springs (Berk, Campbell, Klap, and Western, 1992), Dade County (Pate and Hamilton, 1992).

II. WIFE BATTERY AND CRIMINAL JUSTICE

A. Wife Battery as a Crime

Violence against wives has long been recognized as a social problem in America. Society's historical interest in controlling wife beating demonstrates the presumed impact of both formal and informal sanctions. As early as 1655, a criminal statute prohibited wife beating in the Massachusetts Bay Colony: "no man shall strike his wife nor any woman her husband on penalty of such fine not exceeding ten pounds for one offense, or such corporal punishment as the County shall determine" (Sprague, 1884, cited by Pleck, 1979:61). By the end of the nineteenth century, many states had explicit anti-wife-beating laws, while others punished batterers under general assault and battery statutes.

Although violence against wives has apparently stood throughout American history as a criminal matter, the burden of responsibility for effective control of violence has not always rested with crimial justice agencies. The history of wife beating in America is marked more by informal negative sanctioning through church authority and vigilantism than by law enforcement (Pleck, 1979, 1987). Although secular courts punished offenders, they showed greater concern for maintaining the family unit than for protecting victims. During colonial times, the principal controls on wife beaters were those brought by informal sanctions -- community surveillance, with the threat of ultimate disgrace by trial before a church court. By Pleck's interpretation, informal sanctions, however harsh, were effective deterrents:

> The very qualities that appear so distasteful in the system of informal regulation -- its intense scrutiny of behavior, its almost Biblical method for punishing transgressors -- represented its great strengths as well as its weaknesses. Informal regulation did not have to depend on the complaint of the victim; third parties were watching a husband's behavior and reporting his misdeeds to a policing group. The sanctions relied on informal regulation -- swift physical punishment, public shaming, extreme community disapproval -- probably were far more powerful deterrents to continued wrongdoing than a suspended sentence from a judge or a stern lecture from a policeman. (1979: p. 71)

Today criminal laws regulating wife battery exist in every state. Some have separate laws pertaining specifically to wife battery (Lerman and Livingston, 1983). Many have laws enabling police to make warrantless arrests without witnessing violence, laws enacted in response to concerns over domestic violence in particular. They specify court-imposed sanctions as the principal, manifest punishments of criminal justice. But even as the sanctioning of wife batterers has become institutionalized in criminal justice, informal punishments incidental to formal processing persist. In Feeley's terms, "the process is the punishment" (1979). He observes that "it is the cost of being caught up in the criminal justice system itself that is often most bothersome to defendants accused of petty offenses, and it is this cost which shapes their subsequent course of action once they are entrapped by the system" (p. 30).

Arrest and prosecution, for example, promise many of the informal punishments, albeit within the formal legal structure. To be arrested and jailed, however briefly, is as punishing an encounter with criminal justice as any citizen could expect to experience. Most of the costs are obvious -- inconvenience, loss of freedom, bail, lost wages, embarrassment, stigmatization, and even loss of employment. When arrest follows a conjugal battery, there may be additional, perhaps more subtle, punishments unique to the intimate relationship -- threatened role relationships, imbalances of power, loss of respect.

B. Wife Battery and Criminal Justice in Indianapolis

We refer to this experiment as a study of criminal justice in Indianapolis, the capitol and largest city in Indiana. More precisely, it is a study of prosecution in Marion County, which under "unigov" encompasses Indianapolis and three smaller towns. In 1986, the county's population was about 770,000. According to the 1980 census, 20% of the population was black; other minorities constituted 1% of the total. Sixty-eight percent of adults in Marion County had graduated from high school. About 7% of the work force was unemployed, and the median family income was $20,819 (U.S. Census Bureau, 1983). By comparison, this study showed that those prosecuted for wife battering tend to be disproportionately non-white with less education and higher unemployment rates.

The IDVPE considered misdemeanor offenses only. Misdemeanor battery charges are far more numerous than felony charges. Moreover, misdemeanor batteries have special theoretical interest because they allow for greater victim control over the prosecution process and because alternatives to formal punishment, including dismissal, are common.

A man who batters his conjugal partner in Indianapolis may be prosecuted following either a police or citizen complaint. Figure II.1 summarizes the processes for handling cases of wife battery brought to the Prosecutor's Office in Marion County.

Following a violent attack on a woman, someone may or may not call the police to the scene. If the police are at the scene, they are expected to investigate for evidence to support probable cause for a warrantless arrest. If they find probable cause, they may arrest at their discretion.[1] Upon making such an on-scene arrest, officers fill out a probable cause affidavit and slate the suspect into court for an initial hearing. In such cases, the victim stands as a witness with no option to drop charges other than by non-cooperation (for which she risks being charged with contempt of court).

When the police are not called, or if they are called but do not arrest, a victim may initiate charges on her own by going to the Prosecutor's Office and swearing out a probable cause affidavit with her allegation against the man. Following a judge's approval, the alleged batterer may either be summoned to court or be arrested on a warrant and taken to court for his initial hearing.[2] A victim-initiated case will be treated in the same manner as a

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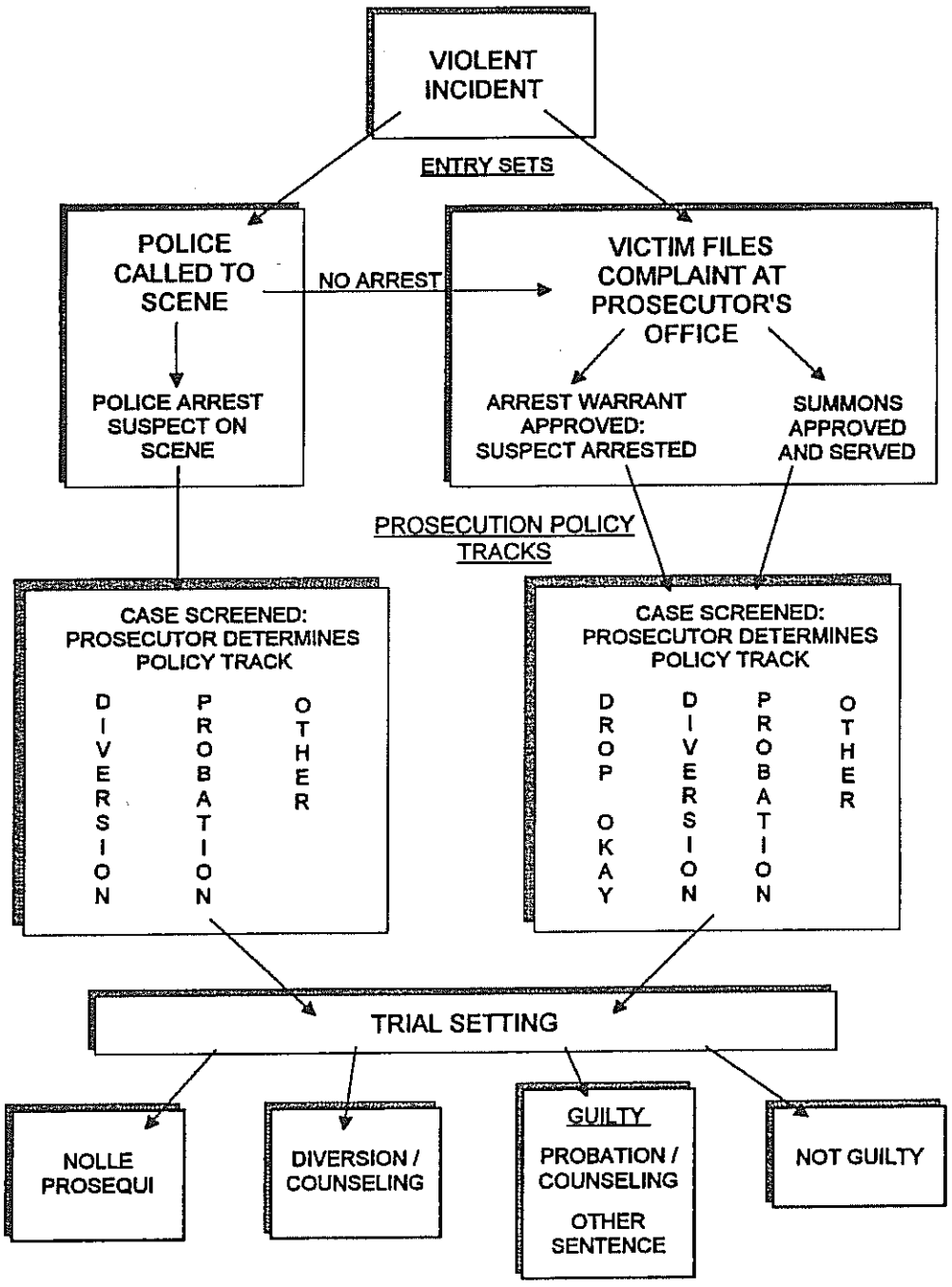
[1] At the time of the experiment, police officers in Indiana were authorized to make a warrantless arrest on the scene of a domestic disturbance when they believed a battery had been committed resulting in bodily injury to the victim and when they had reason to believe that violence would likely reoccur when the police left. The law has since been amended so that it is no longer necessary for an officer to believe that violence may reoccur as a condition for arrest.

[2] A summons is an official notification to a suspect that he is scheduled for a court appearance on a particular date, usually about a month from the time it is issued. A summons is normally hand-delivered to the suspect by a civil sheriff, although it may be mailed. In contrast, a warrant is served in person, and the man is immediately arrested. However, if a suspect knows that there is an outstanding warrant for his arrest, he may avoid arrest by turning himself in at the Prosecutor's Office or at court.

Figure II.1: The Prosecution Process for Misdemeanor Battery



police case except that the victim may exercise some direct influence over whether or not charges are dismissed prior to adjudication.

At the initial hearing, defendants are advised of charges against them, read their rights, and told that they might qualify for a public defender if they cannot afford a private attorney. They may also be informed of possible plea agreements or, if eligible, terms of diversion. If they express interest in diversion, their next court appearance will be a pretrial conference. Otherwise, they are scheduled for trial.

A broad range of statutory options and creative discretionary alternatives is available to criminal justice officials in determining how a case of conjugal violence should be processed and ultimately settled. Nevertheless, under Marion County prosecution policy, we can categorize outcomes under four sets: no prosecution (as may occur when victims are permitted to drop charges); pretrial diversion of defendants to treatment under batterer rehabilitation programs; prosecution to conviction with sentencing to rehabilitative treatment as a condition of probation; and conviction with other conditions, including the possibility of jail. At the time of initial screening, the prosecutor determines a track for processing a case according to what outcome should be pursued. This early judgment guides future discretionary actions as a case moves to court.

A prosecutor's decisions throughout the process rely not only on legal considerations (including convictability and procedural convenience), but also on guesses about which track might be in the victim's best interests. Those decisions have traditionally reflected both personal biases and prosecutorial lore (e.g., Ford, 1983; Rauma, 1984). Given the absence of research findings pointing toward an effective track, prosecutors' decisions today are likely to be informed by guides to action such as offered by Lerman (1981), the Attorney General's Task Force Report (1984), or Goolkasian (1986a). Each calls for greater consideration of victim security by advocating policies presumed to "work" in the same sense that police actions "work" -- batterers processed under the guidelines should be less likely to repeat their violence than they would otherwise.

Figure II.1 shows the difference in policy tracks associated with OSA and VC entry sets. The "Drop-Permitted" (Drop Okay) track is available for VC cases only. It anticipates victims dropping charges, contrary to the more widely advocated "No-Drop" policy. Responding to victim advocates who called for denying victims the opportunity to drop charges (e.g., Lerman, 1981), the Marion County Prosecutor's Office had previously implemented a No-Drop policy under the assumption that it would provide victims greater protection from continuing abuse. Though eagerly accepted as an appropriate policy, it has never been shown to have a preventive impact, and some have argued that it may function to disempower victims, thereby placing them at greater risk (Elliott, Giddings, and Jacobson, 1985; Ford, 1991b; Fields, 1978).

For both entry sets, if a woman is denied the opportunity to drop charges (or if she has permission but elects to proceed), any of three other broad prosecution policies may be activated. The first option, a "no prosecution" alternative, offers the defendant "Pretrial Diversion" to a counseling program. If he is willing to admit his guilt and to participate in an anti-violence program for batterers, his trial date is continued to allow time for him to complete the program. Successful completion of the program results in the dismissal of charges. Committing new violence or failing to abide by the terms of the agreement results in the case going to trial. In the event a defendant is prosecuted (regardless of whether or not he had been diverted) and found guilty, he faces sentencing alternatives ranging in punitive severity from having to enter a counseling program as a condition of probation through serving time in jail.

A second policy option calls for prosecuting to conviction with a request for sentencing to anti-violence counseling as a condition of probation (the same batterer treatment program offered under Diversion). We call this "Probation with Counseling."

The third and final prosecution policy option is to seek a conviction with presumptive sentencing to fines, probation, and jail, our "Other" category. This option corresponds to

the Prosecutor's preferred policy at the time the experiment: rigorous prosecution to seek a conviction with harsh presumptive sentencing.[3]

C. The Research Problem

Theories of crime control view criminal justice as functioning to maintain social order principally through punishment or the threat of punishment. Criminal justice systems identify criminals, arrest and prosecute them, and ultimately punish them under a court's formally imposed sanctions. As a deterrent to crime, formal punishment enjoys support from both popular intuition and criminological doctrine. Criminal behavior is thus presumed to be controlled once a person is convicted and punished. Yet traditionally few detected criminals are convicted and subjected to presumptive sentences. Instead, the punishments most commonly experienced are those borne with arrest and prosecution, sanctions incidental to sentencing and corrections.

Sherman and Berk's (1984a,b) Minneapolis police experiment was the first significant contribution to theory and policy on the preventive consequences of punishment for domestic violence. Their findings argue persuasively, if tentatively, that on-scene arrest, per se, reduces the chance of these batterers assaulting their wives again. The study stands as an important addition to the body of knowledge on arrest as a specific deterrent for crime control, and it has influenced policy for combatting wife battery. Above all, it demonstrates that criminal justice intervention can make a difference in shaping acceptable behavior, notwithstanding contradictory evidence elsewhere.[4] Still it is but a first step toward what could be an integrated strategy for intervention by the criminal justice system as a whole.

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[3] In agreeing to sponsor the experiment, the Marion County Prosecutor specified that no request for harsher sanctioning than the norm -- prosecution to conviction with recommendation for maximum presumptive sentencing -- would be honored.

[4] The Omaha Police Experiment (Dunford, Huizinga, and Elliott, 1989) replicated the Minneapolis Experiment and failed to detect a deterrent impact for warrantless arrest. Similarly, experimental findings from police studies in Charlotte, North Carolina (Hirschel, Hutchison, and Dean, 1992), Milwaukee (Sherman, et al., 1992), Colorado Springs (Berk, Campbell, Klap, and Western, 1992), and Dade County (Pate and Hamilton, 1992) do not show any one police action to be significantly more effective in preventing continuing violence than another.

Criminal justice practices other than warrantless on-scene arrest may likewise have punishing, presumably preventive, effects on wife batterers. In the Omaha Police Experiment, for example, Dunford, Huizinga, and Elliott (1989) found that when a suspect fled the scene before the police arrived but was arrested later under a police-initiated warrant, he was significantly less likely to batter the same victim again within a one-year follow-up period.

Criminal justice action may also be initiated without police intervention, as we saw above when victims file charges with the prosecutor. Indeed, in 1986, the majority of cases prosecuted were initiated by victims filing charges at the Prosecutor's Office. If a man is to be prosecuted following a victim complaint, he may be summoned to court or arrested on a warrant. He may then be forced to prepare for trial unless the woman chooses to drop charges or he is diverted to counseling. If the case is adjudicated, he may be found guilty and punished under legal sanctions. While the process is inherently punishing, a given policy may enhance or mitigate the overall impact of the experience in the interest of protecting the victim from further assaults.

The Indianapolis Prosecution Experiment was designed first to assess whether an alternative prosecution policy (Diversion or Probation with counseling) following an on-scene police arrest is more effective than prosecution to conviction with presumptive sentencing calling for fines, probation, and/or jail. For cases initiated by victim complaints, the experiment sought to discover which if any combination of entry (Summons or Warrant) and prosecution tracks (Diversion, Probation with counseling, or Permitting victims to "Drop Charges") is more effective than summoning a suspect to court and then prosecuting to conviction with presumptive sentencing.[5]

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[5] Prior to 1986, the prosecution process for misdemeanor battery typically began with a victim complaint to the prosecutor's office. If accepted, the defendant would be summoned to court and prosecuted under a No-Drop policy with the deputy prosecutor seeking presumptive (Other) sentencing.

1. Theory and Practice

Protecting battered women entails preventing their partners from assaulting them. For a victim who has already suffered a violent assault at the hands of her conjugal partner, prosecution may prevent continuing violence in one of three ways -- by punishing him so that he will desist in order to avoid future punishments; by using the power of the court to force him into rehabilitative treatment; by empowering the woman to take whatever steps she deems appropriate, beyond court sanctions, to arrange for her own security.

Deterrence theory specifies conditions under which punishment of criminal behavior brings about compliance with criminal laws. The basic behavioral principle underlying deterrence is that the punishment of criminal acts will prevent potential wrongdoing by making the perceived negative consequences or costs of crime greater than any expected benefits or rewards. The specific deterrent impact of punishment may vary from one crime to another according to the degree of rational choice a potential offender has in determining a course of action. As Lempert (1981-82) notes in discussing the prospects for a general theory of deterrence, each specific offense needs attention. Crimes that require some degree of planning, such as burglary or robbery, may very well be inhibited by anticipated punishment, provided that those who might commit the acts cannot shape their actions to avoid detection and punishment. More impulsive violent acts are more difficult to explain under a rational choice model but are nevertheless susceptible to control through fear of punishment as a psychological response.

Whether calculated or impulsive, wife battery is typically a response to some unsettling or stressful condition, including, for example, attacks on self-esteem, frustration, ineffective communication, powerlessness, or some unhappy condition a man perceives as caused by his conjugal partner. It differs from other crimes in that violence, for many men, is truly a deep-seated, role-oriented behavior that is often repeated. Those men learn to use violence as a means of responding to and controlling intimates. Many have low inhibitions against using violence in angered states. Nevertheless, specific deterrence predicts that once a batterer is punished, the resulting threat of future punishment will be more credible, he will

have a greater fear of punishment, and therefore, he will be more inhibited against resorting to violence. To the extent that wife battery is both calculated and repetitive, it may be more susceptible to specific deterrence than other types of crime.

Rehabilitation theories assume that abusers can be made not to batter by changing their attitudes, perceptions, and interpersonal skills. The several extant rehabilitative strategies rest on alternative theories of behavioral change (see review by Saunders and Azar, 1989; also, Hamberger and Hastings, 1993). From a criminal justice perspective, rehabilitation is effected indirectly through the court's power to coerce batterers' participation in programs meant to reduce the chance of continuing violence. Successful rehabilitation ultimately depends on the effectiveness of treatment under coercion.

A third theoretical perspective on controlling wife battering through criminal justice describes prosecution as a power resource available to a victim in arranging her personal circumstances *vis a vis* her abuser. She may use the threat of criminal justice sanctions to bargain for what she perceives as key to her future security, such as separation, promises of reform, agreements for counseling, etc. (Ford, 1991b). Alternatively, she may be empowered to act to protect herself through alliance with agents of criminal justice (Ford and Regoli, 1993).

2. Policy Relevance

As a practical matter, concern over whether an effect of the prosecution process is a result of punishment and deterrence or some other mechanism is unlikely to excite criminal justice policy makers. The significant applied issue is whether or not a prosecution policy has some preventive impact on criminal behavior.

Family violence researchers are especially sensitive to the impact of criminal justice, in part because of its inept handling of domestic violence. For example, Straus (1980) notes that officials' denigration of legal interventions contributes to the popular perception that violence is permissible among intimates. When police fail to respond to domestic disturbances, when prosecutors suggest that wives get a divorce before filing criminal charges, when judges refuse to approve warrants for domestic violence or show leniency for

wife batterers, they announce to the community that husband-on-wife violence is less important than similar acts among non-intimates (Straus, 1980).

Gelles (1983) formalizes similar observations as causes of family violence in what he calls "an exchange/social control theory." Its "central proposition" is that "people hit and abuse other family members because they can" (p. 157). The Gelles formulation highlights the failure of inhibitors as a principal cause of family violence. The immediate prospect for a solution is not through a "service oriented" approach (Tierney, 1982) but through control of violent individuals via criminal justice (Gelles, 1983).

But even as policy-oriented deterrence research has proceeded with impetus from the Sherman-Berk study, there is growing interest in rehabilitative strategies for controlling violent behavior, using the threat of punitive criminal justice sanctions. Goolkasian, for example, in Confronting Domestic Violence: A Guide for Criminal Justice Agencies (1986a), outlines normative expectations for criminal justice actions to control batterers. Among these is the use of court-mandated counseling for their violence. In a statement reminiscent of pre-Minneapolis demands for arrest, Goolkasian states, "there is compelling evidence that court-ordered counseling is appropriate and, in many cases, effective in ending violent behavior" (1986b:6). She goes on to acknowledge that there have been no formal evaluations of the effectiveness of such programs.

Dutton's (1987) research on two non-randomized samples of 50 convicted wife batterers -- one group treated for violence, the other untreated -- shows a long-term reduction in rates and severity of violence for treated batterers. His findings support calls for court-mandated counseling, but significant methodological shortcomings limit their justifying major policy changes. Dutton argues for the likely effectiveness of an arrest-treatment combination to reduce wife assaults. Yet, he notes that "a serious need exists for a design through which men convicted of wife assault are assigned at random to treatment or non-treatment conditions" (1986:173). The Indianapolis Domestic Violence Prosecution Experiment fills that need.

D. Criminal Justice Policy and Hypothesized Outcomes

On-scene arrest satisfies classical premises for deterrence. It is swift, and it is likely to be perceived as reasonably severe. It is also likely to contribute to the perception that further punishment is certain, in two senses. First, if arrest is viewed as an initial step in criminal proceedings (rather than an end in itself), it signifies the certainty of those proceedings. Second, it should enhance the expectation of arrest for future battering. The findings of the Minneapolis experiment lend credibility to a deterrence hypothesis. But for the majority of cases in Indianapolis -- those initiated by victim complaints to the Prosecutor's Office -- the impact of on-scene (warrantless) arrest is moot. The concern, instead, is whether or not the manner in which a judge brings the defendant to court will affect subsequent violent behavior.

A warrant arrest differs from an on-scene arrest in being less swift and, arguably, less certain. It also differs in that the alternative to an on-scene arrest may be no legal punishment, whereas the alternative to a warrant is a summons, a less punishing treatment but one which promises further criminal processing. While arrest brings immediate inconvenience and discomfort, a summons to appear in court allows a defendant to plan for his initial encounter with the judge, however inconvenient that may otherwise be. To be sure, a summons constitutes a significant threat of punishment. But because here both a warrant and a summons signify the same subsequent criminal processing, the difference in actual punishment should vary with the combined effects of entry and prosecution treatments.

If one thinks of the overall prosecution process as a punishing experience, men who are prosecuted should exhibit different subsequent behavior than those who are not prosecuted. Whether or not a case is prosecuted may be manipulated in ways that do not actually deny a victim the opportunity to press charges. There are two options for a no-prosecution treatment under the Prosecutor's policies. One is to allow victims to drop

charges, knowing that upwards of 80% may do so.[6] The other is to divert cases from prosecution by offering defendants an opportunity to participate in a counseling program.

Whether or not a victim is permitted to drop charges prior to court obviously diverges from policies typically considered as punishments. However, a victim's decision is probably critical to the effectiveness of criminal sanctions in controlling violent behavior. Though not direct punishments, alternative policies toward victims dropping charges may have important mediating influences on the effectiveness of explicitly punishing policies in the criminal justice process. Lempert (1981-82) has demonstrated the efficacy of "targeted threats" under conditions of perceived certainty of punishment for non-compliance. At issue in the IDVPE is the significance of enacting a relatively non-punishing policy to bring about a targeted threat for deterring wife battery.

Lerman (1981) advocates a policy prohibiting battered women from dropping charges prior to trial. Her reasoning represents a significant school of thought embraced by many prosecutors (we consider alternative arguments below). Lerman hypothesizes that a no-drop policy will result in a lower chance of repeated violence prior to settlement than if charges can be dropped. Her argument rests on three premises. The first is essentially an assumption about the inclination of batterers: if threatened with criminal sanctions, a wife batterer will retaliate with violence (Field and Field, 1973; Lerman, 1981; Maidment, 1978). If policy allows for dropping charges, the batterer may use violence as a lever for intimidation to force the victim to drop charges. She is likely to experience greater harassment and possibly violence if she persists in prosecuting. If she is denied the choice of dropping, it will do no good for the batterer to try to coerce her. A second premise is that a battered woman can use the prosecution process as a targeted threat which she can invoke to deter the man. If she were allowed to drop, the threat would not provide necessary leverage (cf. Field and Field, 1973; Hall, 1975) since the man could force her to withdraw her

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[6] Reports from jurisdictions around the United States indicate that between 50 and 80 percent of battered women will drop charges either by requesting dismissal or by failing to appear in court as a witness (Field and Field, 1973; Ford, 1983; Parnas, 1970; Bannon, 1975).

complaint (Lerman, 1981). A third premise is a general deterrence proposition: if battered women are not allowed to drop charges, batterers in general will come to understand that when women file charges against them, they will be brought to trial (cf. Fromson, 1977; Walker, 1979).

Whatever leverage may accrue through a no-drop policy is effective either before the prosecution process is mobilized or near its termination. When the case comes to trial, the court can use its threatened sanctions as leverage to force a batterer into compliance with a victim's interests (Lerman, 1981). On the other hand, the victim can use the threat of prosecution and its sanctions as leverage for arranging her own security. The impact of her threat is likely to be weak unless she can convince him it is genuine, as by invoking the prosecution process.

Ford (1984, 1991b) has presented tentative evidence that a battered woman's ability to file and then drop charges at her discretion gives her power to manipulate circumstances outside the legal process in such a way as to provide for her own security. If he is correct, a no-drop policy works against a victim's interests. He describes prosecution as a victim power resource, one which can be manipulated through her ability to drop charges (cf. DuBow and Becker, 1976:149). Synthesizing theories of exchange and power, especially those relevant to conjugal relationships, Ford argues that victims can be empowered by controlling prosecution as a resource for managing conjugal violence. As a power resource, the threat of prosecution, like the threat of violence, is more significant than actual prosecution in the strategy of managing conflict. Theoretically, a victim should be able to use the threat of prosecution to bargain for arrangements satisfactory to her wishes (see Schelling, 1960).

Ultimately, however, she must be able to withdraw the threat if she secures a favorable settlement. In short, the effective use of prosecution as a power resource is

premised on the victim's ability both to demonstrate a significant threat and to control activities relevant to the threat, including its withdrawal (Ford, 1991b).[7]

Any criminal justice policy portends punishment if pursued to court. Some policies, however, may be relatively less punishing and may even bring benefits to an offender. For example, victims commonly state that they want their abusers to "get help." Defendants who expect mandated counseling may begin to reform prior to sentencing in hope of "winning back" a partner.

The Diversion policy combines rehabilitation with deterrence in attempting to force reform, but does so under a definite threat of swift and certain punishment. Short of having a case dismissed or being judged not guilty, counseling under a diversion agreement is the least punishing court outcome but also one of the most threatening. As such, it poses ambiguous predictions for deterrence. Diversion to counseling carries a threat of future punishment, but punishment for failing to cooperate is not certain, it merely brings the defendant back to the situation he was in before agreeing to diversion, i.e., that he is liable to be prosecuted.

Probation, in contrast, carries more certain sanctions when violated. Prosecution to conviction in cases of domestic violence may be effective only as a means of coercing participation in rehabilitative counseling programs as a condition of probation. When such counseling is mandated, a convicted offender has reason to fear additional punishment for violations. This provides rehabilitative treatment more of an opportunity to work (Dutton, 1987).

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[7] Power resource theory predicts the control of a batterer's violence through victim manipulation of the criminal justice system. At base, it incorporates the same deterrence ideas as does the argument for a No-Drop policy -- a credible threat of punishment will prevent a batterer from resorting to further violence. Indeed, were one to apply power resource theory to the initial threat to prosecute, one could argue in support of a no-drop policy. If a victim threatens to file charges in order to punish her abuser, the threatened punishment is made credible if there is no room for dissuasion should it be initiated. But if the threat to file under a No-Drop policy fails to deter (as must have happened for a case to be filed), there is no reason to believe the No-Drop policy will prevent further battering.

There are two sets of other possible adjudicated outcomes: not guilty, and guilty with some other form of probation, a fine, or a jail sentence. The latter -- our "Other" category -- varies in degree of perceived punishment relevant to preventive effects. Whereas mandated counseling represents a relatively compassionate response to crime, acknowledging a defendant's capacity to change his behavior without experiencing intentional punishment, a control perspective calls for purposely punishing an offender in order to deter future crime. The Other prosecution track signifies that the state wants to punish a defendant and that he can anticipate as much. The deterrence model predicts that men tracked toward Other sentencing will be less likely to batter again.

What matters ultimately is how policy impacts on victim security. Although policy impacts are mediated by actual court outcomes, the analyses presented later in this report take policy as something resulting in behavioral consequences either in anticipation of or by experience with actual court outcomes consistent with prosecution tracking.

III. THE INDIANAPOLIS PROSECUTION EXPERIMENTS: DESIGN AND METHODOLOGY

A. Overview

The Indianapolis Domestic Violence Prosecution Experiments (IDVPE) were designed as randomized field experiments to discover which policies "work" as cases move beyond police action into the realm of prosecutorial and judicial action. They examined the alternatives for processing cases of wife battery discussed above -- Diversion to counseling, Probation with counseling, Other sentencing, and (for VC cases) Drop-Permitted,[1] as shown earlier in Figure II.1.

The experiment coincided with the prosecutor's adoption of a special domestic violence prosecution unit funded by the Bureau of Justice Assistance (86-SD-CX-008) in June, 1986, as a demonstration project -- "The Domestic-Family Violence Intervention Program." Except for the Drop-Permitted track, alternative IDVPE policies for counseling represented innovations proposed under the Intervention Program.

Policies calling for rehabilitative counseling as a condition of either pretrial diversion or probation were accommodated by the availability of counseling services through any of three local agencies. Each used a form of anger-control instruction in the context of group counseling to address broader issues of violence by men against women. In assigning a case to one of the counseling tracks, the prosecutor anticipated the defendant entering such a program.

The Other Sentencing track called for the prosecutor to seek jail time for a convicted offender, consistent with the county prosecutor's preferred policy at the time the experiment was initiated. In practice, Other is a residual category within which a variety of traditional

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[1] The Drop-Permitted policy was available only to cases initiated by victim complaint to the prosecutor. When the IDVPE began, the law enabling warrantless, probable cause arrests for misdemeanor battery had just been implemented. All agencies participating in the experiment agreed that to allow victims of on-scene arrest cases to drop charges brought by a police officer would hurt their efforts to encourage arrest.

sanctions might be exercised.[2] The analyses presented below use Other as an experimental control or base category against which the remaining policies are compared.

In the Victim-Complaint experiment, the Drop-Permitted track was expected to be equivalent to not prosecuting, as a high proportion of victims drop charges when permitted -- a *nolle prosequi* treatment. Randomly selected victim complainants were informed that they were permitted to drop charges if they so desired. They were not encouraged to drop, nor were they explicitly discouraged.[3] The only constraint imposed was that a woman could not drop prior to the man's initial court hearing. Obviously, we could not force a victim to drop charges in order to satisfy a "Nolle" condition. However, dropping was facilitated by explaining how to do it and whom to contact. Given historically high rates of dropped charges prior to the experiment, even when official policy claimed to prohibit dismissals, we expected to fill a Nolle condition with no less than 70% of the cases permitted to drop (Ford, 1983).[4] As it happened, victims apparently found support for prosecuting in the Prosecutor's Domestic Violence Program, and many fewer actually dropped charges.

B. Population and Sample

Cases for the IDVPE study were selected from all cases of misdemeanor battery or criminal recklessness brought to the attention of the Marion County (Indiana) Prosecutor's Office between June 30, 1986, and August 10, 1987. To qualify for the study, a defendant had to be an adult male (aged 18 years or older) alleged to have physically victimized a female conjugal partner. Also, at the time of the study incident, the couple had to fit one of

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[2] "Other" represents "business as usual" by pre-experimental practice. We have no way of knowing what prosecutors recommended as typical sentences prior to the experiment, except that counseling was not an option generally available to defendants. Thus, traditionally, batterers' counseling options could not be requested.

[3] We suspect, however, that zealous victim advocates may have made it clear that permitting victims to drop charges ran counter to their beliefs in what was best for the women. We know that the advocates expressed concern over the Drop-Permitted policy in evaluating the prosecutor's demonstration program (Reichard, 1988).

[4] For comparison, this figure is similar to the maximum variation from assigned treatments reported in the Minneapolis experiment (Sherman and Berk, 1984).

the following relationships: married, previously married, cohabiting, previously cohabited, or had a child together.

The only cases rejected from the study were those in which the suspect fell into one or more of the following categories: he had previously been convicted of felonious violence (e.g., criminal homicide, robbery, burglary with injury, rape, arson with injury, aggravated assault); he had previously been convicted or had a warning letter sent or had a pending case for an act of violence against the same victim as in the new case; he was known to be on probation, was subject to violation for the new offense, and the prosecutor wanted him to be violated. Cases were also rejected if, prior to randomization and in the judgment of the prosecutor, the defendant posed such danger to the victim that he should be arrested immediately and given no chance of less than rigorous prosecution with harsh punishment.

The success of the Indianapolis Prosecution Experiment rested with the willingness of criminal justice officials to exercise their discretion with consideration for experimental treatments in the absence of compelling reasons to act otherwise. The Prosecutor and Presiding Judge supported randomization with the understanding that they reserved the right to override an experimental assignment and would do so when an offender was considered too dangerous for non-punitive treatment. To avoid high misassignment and attrition rates, the IDVPE used the Prosecutor's willingness to drop or divert a case as a final criterion for acceptance into the study.

A total of 678 cases were identified for study. Of those, 480 cases (71%) were brought by a victim's direct in-person complaint to the Prosecutor; 198 cases (29%) entered the prosecution process following warrantless, on-scene police arrests of suspected batterers.[5] Within each of these "Intake Sets" (i.e., entry by Victim Complaint [VC] or by On-Scene Police Arrest [OSA]), cases were randomly assigned recommendations for prosecutorial

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[5] The percentage distribution of alternative intake sets is an artifact of the experimental design which called for a fixed distribution of cases to treatment cells. The study continued until all cells were filled under both entry sets. The total count of on-scene arrest cases is no doubt somewhat higher than obtained because further investigation of potential cases was terminated when all cells were filled. We estimate that about 15 cases were "lost" because they could not be identified as eligible prior to satisfying the designed count.

treatment (as described below). In addition, the Victim Complaint cases were randomly assigned to either a warrant or a summons condition as the means of bringing a defendant to court.

There is little difference between those men arrested for battery on the scene of a violent incident and those against whom victims filed complaints. Table III.1 summarizes background characteristics of victims and defendants under each intake set, based on data from the IDVPE tracking files and initial victim interviews.

A majority of IDVPE defendants were white, though blacks were over-represented (44%) relative to their distribution in the population of Marion County (about 22%). Whites were somewhat more likely to be arrested by police on the scene of a violent domestic. A typical defendant was 30 years old -- two years older than his victim. A majority of the couples had children and were living together. Forty percent of the defendants did not finish high school and about one out of four was unemployed at the time of the incident resulting in the charges for IDVPE study.

The data on conjugal status point to a bias in the chance of arrest. Suspects who cohabited with their victims were more likely to be arrested on-scene by the police. No doubt they would be less inclined than non-cohabitants to leave or to have left when the police were called. They were more difficult to force away from their homes, and perhaps they were more adamant over having their own way while in their own homes (a situational factor triggering arrest). Whatever the reason, the bias is an important consideration when comparing differences in treatment outcomes by intake sets. Couples who cohabit have more opportunities for conflict and violence, thus the chance for recidivism may be higher. Also, the defendants in cohabiting relationships are more likely to have official records of recidivism, when it occurs, because the police appear to be more likely to act against them.

By design, at intake no IDVPE defendant had been convicted either of a violent felony against any victim or of a violent misdemeanor against the victim filing charges in the study. Thus, the men viewed as most dangerous to study victims were excluded. Still,

Table III.1: Characteristics of Victims and Defendants by Intake Set

	Intake Set			
	Victim Complaint		On-Scene Arrest	
	Interview Sample	Total Sample	Interview Sample	Total Sample
Defendant Race				
White	54%	54%	58%	60%
Non-white	46	46	42	40
	(324)	(480)	(106)	(198)
Victim Race				
White	55%	55%	59%	61%
Non-white	45	45	41	39
	(324)	(480)	(106)	(197)
Defendant Mean Age	30.7	30.8	30.6	30.9
	(324)	(480)	(106)	(198)
Victim Mean Age	28.7	28.7	28.0	28.2
	(324)	(479)	(104)	(194)
Conjugal Status				
Married, Cohabiting	31%		41%	
Married, Separated	8		7	
Divorced	9		5	
Unmarried, Cohabiting	20		27	
Previously Cohabited	25		14	
Children Only	6		7	
	(324)		(106)	
Couple has One Child or More	61%		61%	
	(324)		(106)	
Defendant Non-HS Graduate	41%		40%	
	(306)		(101)	
Victim Non-HS Graduate	36%		42%	
	(324)		(106)	
Defendant Unemployed	24%		31%	
	(324)		(106)	
Victim Unemployed	41%		48%	
	(324)		(106)	
Defendant Criminal History				
No Criminal History	24%	26%	23%	26%
Violent Crimes	20	20	19	23
Other Crimes Only	56	54	58	51
	(324)	(480)	(106)	(198)

many defendants had previously committed acts of misdemeanor violence against others, and many had records of non-violent criminality.

Seventy-four percent the accused batterers had been arrested before, frequently for crimes of violence. Those in the On-Scene Police Arrest intake set were somewhat more likely to have been arrested in the past, especially for violent offenses. This record of prior police contact may have increased their susceptibility to arrest and thus influenced their place in the experiment.[6]

C. Randomization

The experiment used a "discretion-dependent" design whereby each case entering the study was randomly assigned to a treatment condition that was recommended to a deputy prosecutor for consideration as a prosecution goal. The experiment did not apply any direct treatment; its influence was mediated by prosecutorial and judicial discretion. Prior to the experiment, the normal policy was to prosecute every case to conviction with the goal of attaining presumptive sentencing (the experiment's Other policy), i.e., fines, probation, and executed jail time. Pretrial activities set each case on a course toward that end, what we have been calling a prosecution policy "track." With the implementation of the IDVPE, other less punishing treatments were designated as possible outcomes. Cases in the IDVPE had a greater chance of receiving a less punishing outcome, as each case now had an equal opportunity of being tracked toward one of two or three alternate outcomes, as we saw in Figure II.1.

In effect, the prosecutor was asked to let chance govern her discretion so that each defendant was equally likely to receive any one of the treatments, including less punitive alternatives.[7] For example, under the IDVPE, a case might be randomly assigned to a

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[6] Sherman and Berk (1984) reported that about 60% of the men in their study had ever been arrested by the police, with just over 30% having a record of crimes against persons. While IDVPE defendants are more likely to have been arrested, they appear to be less violent, as expected given the chance of exclusion for some prior violence under our screening criteria.

[7] The term "prosecutor" is used throughout to describe any one of many deputy prosecuting attorneys who might have some involvement with a study case. Virtually all of the initial screening of cases was done by a female prosecutor, hence the pronoun "her" is used in this section.

pretrial diversion treatment. Diversion would be recommended to the prosecutor in hope that she would offer the defendant an opportunity to participate in a batterers' anger control program instead of prosecution. If she had no reason to argue against the IDVPE recommendation, the prosecutor would endorse it by entering it as her recommendation on a standardized form in the case file. The success of the experiment rested with the prosecutor's willingness to set aside common, biasing factors (including personal values and prosecutorial lore) which might otherwise guide her discretion.

Randomization occurred at the point when a case was determined to be eligible for the experiment and before any formal prosecutorial processing began. In both OSA and VC experiments, randomized treatments were assigned by attaching an unmarked, sealed packet of paperwork, including tracking recommendations, to each new case file. After reviewing a case to insure its eligibility, the screening prosecutor opened the packet, noted the experimental recommendation and then entered her recommendation for case processing on an official cover sheet for the file. From that point on, anyone looking at the file would see how the prosecutor was handling the case. OSA packets simply presented the prosecutor with one of the three equally likely tracks: Diversion, Probation or Other sentencing. VC packets gave two recommendations, one for means of entry (by summons or by warrant) and one for prosecutorial tracking (Drop-Permitted, Diversion, Probation, or Other), as determined by random selection of one of their eight equally likely combinations.

Thanks to cooperating deputy prosecutors, the experiment attained near perfect agreement between the randomized treatment recommendation and the prosecutor's decision, upon screening a case, to track it toward the designed outcome. In only one case did the prosecutor choose to override a randomized prosecution recommendation.[8] That nearly 100% of the cases were tracked as recommended demonstrates the prosecutor's commitment to learning what prosecution policy "makes a difference."

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[8] That case called for a summons and tracking toward "probation with counseling" but was tracked for supposedly harsher treatment under "other" sentencing. In one other VC case, the prosecutor's screening decision was not recorded.

Since the initial tracking decisions provided the principal basis for evaluating policy, successful randomization assured the most rigorous possible evaluation of policy. At the outset, however, there was no guarantee that the stated policy would be implemented as intended. By the time a case came to court some months later, a prosecutor might have had reason to ignore the initial tracking recommendation. Perhaps the defendant had harassed or committed new violence against the victim and thereby motivated the prosecutor to seek a harsher punishment than recommended. Or perhaps a plea bargain was struck which might ensure a conviction with punishment other than that recommended, in the interest of guaranteeing at least the conviction. Or a victim, a defendant, or his attorney might argue successfully against the prosecutor's recommendation. Or perhaps the trial prosecutor simply did not like the screening prosecutor's recommendation and chose to ignore it. And beyond prosecution policy, a judge might choose not to hear or to ignore a prosecutor's recommendations or request for disposing of a case. All such "failures" of policy implementation meant failures in the realization of experimental tracks in court outcomes.

Ultimately, successful delivery of experimental tracks as court outcomes depended not only on the discretion of actors in the criminal justice process, but also on the wishes of victims and defendants. In particular, the Drop-Permitted and Diversion conditions called for the prosecutor to offer those opportunities to a victim and/or defendant. If the opportunity was rejected, it represented another failure in implementation of policy.[9]

D. Data Sources and Outcome Measures

The IDVPE analyzed four indicators of recidivism -- whether or not violence recurred within six months of settlement, its severity, the frequency of violence during that period, and how much time passed between court settlement and a new violent incident. All of this information was available from victim interviews. Additional evidence of any of these

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[9] Cases where VC victims elected to proceed despite the opportunity to drop charges were randomly assigned to one of the remaining tracks -- Diversion, Probation, or Other. The victim was not aware of what alternative option would follow her decision to proceed. If Diversion was offered and rejected under either experiment, the case proceeded to trial with a randomized recommendation for either Probation with counseling or Other.

indicators was found in official records -- calls from victims to the Prosecutor's Office, new victim complaints filed, police arrests for battery or recklessness, police reports of other involvement with the victims and defendants, complaints to the court (as when "no contact" orders were breached), and complaints to other agencies placed in the court's files on broken diversion agreements or probation violations.

Each victim was contacted to be interviewed after charges were filed in order to learn the nature of her personal situation and her relationship to the accused batterer; to record details of the history of violence in their relationship, including the incident for which charges were filed; to get her expectations of what would or should happen through criminal justice intervention; and to learn how she expected the man to react to the charges. Different initial interviews were used depending on whether the respondent was a victim-complainant (i.e., she filed charges at the Prosecutor's Office) or strictly a victim-witness (i.e., the victim in a case where police made an on-scene warrantless battery arrest). The Victim-Complainant Interview (Attachment A) differed from the Victim-Witness Interview (Attachment B) in that it asked questions concerning her reasons for filing charges. At the conclusion of the initial interview, each victim was asked for permission to contact her later, after the case was settled. Contact information was then requested.

Defendants were interviewed after their cases had been settled (i.e., dismissed, approved for diversion, or adjudicated and sentenced). The Defendant Interview (Attachment C) sought from each man his side of the story, his description of the violence in the relationship with the victim, and, especially, his perception of punishing aspects of his criminal justice experience.

Follow-up interviews with victims were conducted one month and six months after case settlement. The one-month Victim Follow-up Interview (Attachment D) was designed to be conducted by telephone. It requested information on new violence or other problems and on changes in the victim-defendant relationship which may have occurred between the time charges were filed and the settlement date. It also asked for the victim's feelings toward the criminal justice system as she experienced it -- whether she was satisfied with the outcome,

whether she felt it would be effective, and whether she would use criminal justice processes to deal with future violence. The six-month Victim Follow-up Interview asked many of the same questions, but sought details on any new incidents of violence in the six months following case settlement.

Initial interviews were successfully completed with the majority of both victims and defendants. Among victims, 89% of the victim-complainants (in VC cases) and 74% of the victim-witnesses (in OSA cases) were interviewed. Among defendants, 81% of those in VC cases were interviewed; 10% refused. Of those arrested on-scene, 76% were interviewed and 17% refused.

Efforts to obtain post-disposition victim interviews were less fruitful, due in part to understaffing of interviewers and to the "disappearance" of some victims who had previously agreed to follow-up interviews. Ultimately, for cases settled and available for six month follow-up, 72% of 452 VC victims and 56% of 190 OSA victims were interviewed. These

Table III.2: Victim Interview Summary

	Intake Set			
	Victim-Complaint		On-Scene Arrest	
Initial Interviews Completed	427	89%	146	74%
Initial Interviews Refused	29	6	23	12
Initial Interviews Not Obtained	24	5	29	15
(Total Settled Cases)		(480)		(198)
1-Mo TX Interviews Completed	345	76%	116	61%
1-Mo TX Interviews Refused	40	9	28	15
1-Mo TX Interviews Not Obtained	67	15	46	24
(Total Settled Cases)		(452)		(190)
6-Mo Interviews Completed	324	72%	106	56%
6-Mo Interviews Refused	55	12	30	16
6-Mo Interviews Not Obtained	73	16	54	28
(Total Settled Cases)		(452)		(190)

figures represent completed interview rates of 80% and 74% of the respective VC and OSA cases previously interviewed. The full summary of interviews is presented in Table III.2.[10]

Table III.3 gives the completion rates for defendant interviews under each intake set. Defendants granted interviews in 81% of the VC cases and 76% of the OSA cases. We found defendants to be no harder to locate than victims, but they were more elusive. For example, in one case a defendant scheduled, then broke appointments on four occasions with three different interviewers, yet called back to say he still wanted to be interviewed. The low rate of refusal is probably illusory in that we do not count a refusal until the man tells us outright that he will not do the interview. Thus even in cases where a man seems to be

Table III.3: Defendant Interview Summary

	VC Cases		OSA Cases	
Defendant Interviews Completed	368	81%	144	76%
Defendant Interviews Refused	46	10	32	17
Defendant Interviews Not Obtained	38	8	14	7
		(452)		(190)

avoiding an interviewer, if he previously agreed to talk and did not tell us otherwise, he remained on our list to be interviewed at the end of the project.

Subjects' unwillingness to provide information through interviews, questionnaires, etc., generally results in analysis subsamples reflecting whatever bias accompanies the decisions of those who do grant interviews. Criminologists therefore appreciate the availability of official records on all subjects in order to avoid the selection bias of missing interviews. We use official records here. However, official records are likely to provide far less reliable data than interview reports of interpersonal violence. Almost all official

[10] Part of the difficulty in obtaining follow-up interviews was in locating the victims six months after case disposition (which could be as much as a year or more after the initial interview). A second cause of the shortfall was the scarcity of skilled interviewers willing to do the extensive legwork required to find respondents.

records of violence are generated by victim reports to criminal justice agencies. If a report is archived, it is at best a third party's interpretation of events relayed by the victim. An arrest record, for example, typically follows a sequence of "noisy" events -- a victim's decision to call the police; a dispatcher's decision to send officers; officers' willingness to investigate and arrest; victim and suspect veracity and persuasiveness for or against arrest; officers' finding probable cause to arrest and doing so. Other types of official records, such as simple calls to the police or other assistance agencies, have few, if any, validity checks. Victims are more likely to exaggerate or even lie to get favorable consideration in reporting abuse to official agencies than in giving a research interview. In short, what is "officially" recorded is probably less reliable than interview data, being subject to selectivity bias in reporting comparable to the bias of missing interviews. We report analyses of data from both sources later in this report.

E. Implementation Summary

The experiment succeeded in supplanting prosecutorial and judicial discretion with randomized tracks. All On-Scene Arrest cases had prosecution tracks assigned in accordance

Table III.4: Frequency of Court Settlements by Randomized Prosecution Track for OSA Cases

Court Settlement	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
Dismissed	8	17	13	38
Diversion	42	1	1	44
Probation	4	31	4	39
Other	3	9	37	49
Not Guilty	6	6	8	20
Total	63	64	63	190

with the experiment's recommendations. At the time a case was settled in court (about 6 months after initial tracking), the judge either diverted or sentenced in accordance with the original prosecution track in 83% of the OSA cases. Overall, that is, including OSA cases that were dismissed or had defendants found not guilty, settlements were consistent with the original tracks in 58% of the cases. Table III.4 presents the frequencies of court outcomes by experimental recommendations for tracking.

For Victim-Complaint cases, the prosecutor tracked all but 1 case toward a settlement recommended by randomization. Entry tracks were delivered as designed for 96% of the cases, and prosecution tracks ended with intended settlements (excluding dismissals and not guilty findings) in 78% of the cases. When one counts all settled VC cases, 67% followed randomized tracks to the expected settlement, as computed from Table III.5.

Moreover, the number of cases obtained for experimental treatment over the intake period closely matched the experiment's estimates. After one year, the number of victim complaints meeting study criteria was just 12 cases below expectations, while the number of eligible police arrests exceeded expectations by 6 cases. An additional month of case intake

Table III.5: Frequency of Court Settlements by Randomized Prosecution Track for VC Cases

Court Settlement	Prosecution Track				Total
	Drop Permitted	Diversion to Counseling	Probation w/ Counseling	Other Sentence	
Dismissed	60	5	18	13	96
Diversion	16	84	3	2	105
Probation	13	9	74	9	105
Other	17	9	8	83	117
Not Guilty	6	5	13	5	29
Total	112	112	116	112	452

raised the total number in each intake set sufficiently to cover a higher-than-anticipated rate of attrition.

Unfortunately, the rate at which cases were expected to be adjudicated and available for follow-up analysis was underestimated. Notwithstanding promises of improved case handling under the Prosecutor's Domestic Violence Program, the length of time from intake to disposition exceeded that of cases in the "Dark Ages" of ten years ago (Ford, 1983). The problem was rooted in slow judicial processing and long court continuances. The consequence for the IDVPE was a delay in prosecution, with some cases still awaiting court settlement at the end of the fieldwork.

1. Procedure

The design was implemented under the close supervision and participation of research staff. Recommended IDVPE treatments were reviewed by a single designated prosecutor and transferred to a recommendation form in the case file for action in court. Actually, all cases handled by the Prosecutor's Domestic Violence Program had such a recommendation form affixed to their files (see Section C above). Each IDVPE file was discreetly marked to flag the case as one deserving a special effort to follow the recommended action.

In Victim-Complaint cases, when a woman sought to file charges against a man, she was immediately asked about her relationship to the man and the nature of her complaint. If her case suited the experiment, she was directed to a research assistant for additional screening and guidance on how to fill out a probable cause affidavit. The assistant reviewed the affidavit to confirm the case's suitability for the study. If acceptable, the assistant pulled the top packet from a randomized stack and informed the victim per instructions. At that point the victim was told whether or not she would be permitted to drop charges, she was instructed on how she could contact the Prosecutor's Office again, and she was given the assistant's name and office number as a contact person (especially if she was permitted to drop charges). When the assistant completed all of the Prosecutor's business with the victim, she informed the victim of her dual role as research assistant and asked for the victim's consent to be interviewed.

An attempt was made to interview all victim-complainants when they filed charges. Due to the length of the Prosecutor's intake (at least an hour), it was often necessary to arrange for interviews at a later date. In On-Scene Arrest cases, victims were identified through the Prosecutor's records and police reports. These victims were then contacted to arrange an initial interview at the Prosecutor's Office or some other "safe place."

The initial victim interview was a principal source of baseline information on all couples. Every victim granting an initial interview was paid $10 and up to $3 for travel expenses. Following the initial interview, each woman was asked for her permission to be contacted later for follow-up interviews.

An effort was made to contact each victim by telephone one month after the court date on which her batterer's case was nolled, diverted, or adjudicated. This brief interview served primarily to update contact information (addresses and telephone numbers) for later interviews. At this time, however, she was asked to discuss any violent episodes experienced since filing charges. She was also questioned about her attitudes toward the prosecution process as she had experienced it. This interview proved difficult to obtain as planned. Fewer than half the victims had telephones in service. We did not have funds for doing these interviews face-to-face, so they were often conducted after six months with the regular in-person follow-up Interview.

The main Victim Follow-up Interview was conducted face-to-face at least six months after the last court date. When possible, victims were first contacted by telephone to arrange a meeting at a safe place. Frequently, however, we sent interviewers to last known home addresses. The six-month follow-up interview provided the essential victim-reported data on repeat violence. Women agreeing to this interview were paid another $10 for their time.

Interviews with defendants were arranged immediately following the court appearance when their case was nolled, diverted, or adjudicated. We had hoped to interview them before they left court. But with long court sessions, the presence of friends and attorneys, or commitments to the victim, few defendants were willing to spend additional time on an interview. Most of these interviews were scheduled later, with the consequence that many

defendants could not be located, failed to keep appointments, or refused outright. Men who granted interviews were also paid $10 for their time.

2. Managing Threats to Implementation

American jurisprudence places a high premium on discretion Thereby providing institutional legitimacy for officials to apply personal criteria in deciding the course of events for a criminal case. Evaluation of criminal justice policies challenges discretion to the extent that it requires officials to acknowledge that they do not have all the answers. Program evaluation is possible only when officials concede that their judgments might be better informed by research. Yet, the mere suggestion that discretion may be ill-advised or faulty threatens those officials who view discretion as guaranteeing not only fair play but also self-worth.

Randomized field experiments, in particular, require close cooperation from those who may find research burdensome if not threatening. Such experimental designs encounter implementation problems when decisions normally governed by discretion are to be supplanted by randomization, even when, as in this research, it is checked by the provision for officials to exercise discretionary overrides of randomized treatments.

Although we realized at the outset that we would need to monitor the randomization process closely, only in its implementation did we realize how time-consuming that would be. Daily attention was needed to keep the vagaries of criminal justice in practice from undermining the randomized design. Some examples illustrate the types of problems we confronted:

- Upon intake of an eligible victim-complaint case, some members of the Prosecutor's staff would take it upon themselves to "walk through" a warrant (contrary to the designed Summons entry track), intentionally by-passing the screening prosecutor, to find a prosecutor who would approve the warrant and get a judge to sign it.

- Officers designated to arrest a suspect on a summons would invite the suspect to turn himself in at his convenience.

- In court, advocates might counsel a victim permitted to drop charges under the IDVPE treatment, not to drop.

- At trial, substitute prosecutors frequently ignored the screening prosecutor's recommendations for treatment.

- Men sometimes showed up in court to turn themselves in and plead guilty -- the prosecutor would not have a file available and would not know whether the case was part of the IDVPE.

Only through the presence of IDVPE staff in court were most of these threats to randomization immediately detected and averted. The most serious threat to the experiment occurred when one of the regular deputy prosecutors (who professed high commitment to the research) started overriding the screening prosecutor's recommendations, without explanation, and thereby overrode IDVPE recommended treatments in close to 3 out of 4 cases. Following discussions between the Principal Investigator and the Prosecutor, the deputy prosecutor in question was assigned elsewhere. Although we had only sought guidance on how to handle the situation, the outcome was immediately satisfactory. On the other hand, it created some hostility among factions of the Prosecutor's staff, which made it difficult to find cooperative substitutes later.

F. Court Outcomes

Comparisons of effects across experimental tracks are meaningful only when their court outcomes are sufficiently distinct as to account for differences in effects. Any ambiguities in the nature of the court outcomes will yield either no difference in effects or uninterpretable differences. In the IDVPE, no treatment was without some ambiguity. Experimental findings should be interpreted in light of the following discussion of alternative court outcomes and how they might diverge from recommended policy tracks.

The VC entry tracks (Warrant-Arrest versus Summons) are seemingly distinct. A summons is simply an invitation to court on a specified date. It is usually hand-delivered, but sometimes mailed, to a defendant. A warrant, on the other hand, requires a law enforcement officer to arrest a defendant. Sometimes men have no idea they are "wanted." Other times they know, but are not prepared to be arrested at their place of employment or in the middle of the night. When served with a warrant, a man can expect to be transported to the county lock-up and held for at least an hour or two.

In practice, warrant service may not be all that different from a summons, at least not from a defendant's perspective. First, if a man is aware of the warrant, he can always turn himself in at police headquarters, at the Prosecutor's Office, or at court. If he chooses to go to court, he will have a brief hearing and will almost always be released. The bailiff will then recall the warrant. In such a case, the warrant may be more convenient and less punishing than a summons. The defendant, in effect, sets his own initial hearing. If he turns himself in at the Prosecutor's Office, he may be handcuffed and walked across the building to the lock-up. Or he may simply be escorted without any appearance of arrest. The only control the IDVPE had over what happened to men scheduled for warrant arrest was by agreement with the prosecutor's domestic violence investigator who served most of the IDVPE warrants. He tried to serve the warrant without notice to the suspect. He also handcuffed any man who turned himself in, before taking him to the lock-up for police processing. We do not have an accurate count on numbers arrested, or on who turned themselves in.

The summons treatment has less room for alternative actions, yet the way it is served may also be ambiguous. If a man is served with a summons but does not receive it in time for court, he will fail to appear for his initial hearing. An FTA (failure to appear) warrant will be issued, and he may end up being brought to court under arrest. He is treated no differently than had a warrant been approved as his initial entry track. We had even less control over this. Part of the problem was that sometimes summonses were issued but not served in time for the court date. We alerted the judge to this early in the study, and he helped to reduce the chance of such occurrences by speeding up the approval process. The only other way we could control the effect of unwanted arrest was to have the investigator invite the FTA-Summons group to turn themselves in and not arrest them. The investigator tried to do this with IDVPE cases.

The integrity of prosecution tracking is more troublesome than entry tracking, as opportunities for variation in treatment delivery abound. Drop-Permitted is the least ambiguous policy. But in practice it is not always easy to distinguish attrition due to victims

who desire to drop charges from attrition due to victims simply not showing, perhaps against their wills. We had copies of signed statements from victims who dropped charges under the experimental treatment. Otherwise we had to rely on interview data to find out why a victim might have failed to appear for trial. One unresolved catch is that the victims who missed court were also likely to miss interview appointments (if, indeed, we could find them to set up an appointment).

Diversion to counseling is a distinct outcome, but if a man failed to participate, he would be prosecuted and sentenced. Thus, the integrity of the Diversion track depended on the man following through with the agreement. If he did not, we recorded him as diverted, but we looked for his case again when it was brought to trial. Fortunately, the Prosecutor's Domestic Violence Program provided for close supervision over diverted cases. Each participant was literally graded for his performance, attitude, etc., on a monthly progress report filed with a monitor.

The two tracks leading to a conviction are most likely to create interpretation problems. Counseling for violence/anger-control as a condition of probation was offered under the same agencies as the diversion program. Men on probation and men diverted took classes and counseling together. Probation was the only difference. In practice, however, the counseling treatment was often contaminated by other sentencing conditions, such as community service hours, jail time, or additional counseling for alcohol abuse.

Finally, the Other track was intended to be the most harsh of possible treatments. Under the Prosecutor's Domestic Violence Program, men were to be prosecuted with requests for jail time whenever possible. This did not occur. In fact, Other turned into a residual category where a range of sentences (excluding violence counseling) were given. As always, sentencing to executed jail time is unusual. For the IDVPE, the Other track serves to identify a prosecuted control group representing the judge's traditional response to the Marion County Prosecutor's expressed sentencing preference.

Despite ambiguities in the way IDVPE cases might be settled, the prosecution policies were clear. When a victim or defendant had contact with the prosecutor she or he was told

what to expect as the case moved forward. Both victim and defendant could alter their behaviors in anticipation of outcomes consistent with policy. The analyses presented below concentrate on differences by policy, true to the experimental design. Recognizing that policy does not perfectly predict the nature of court settlements, analyses are also presented to account for the policy impacts in interaction with actual outcomes.

G. Baseline Violence and Recidivism

By victim accounts, 78% of all IDVPE defendants, at the time their cases entered this study, were being accused of a second violent episode in twelve months. Sixty percent of defendants had assaulted the same victim three or more times in the preceding year. In just six months prior to entering the study, 74% of the IDVPE men reportedly had battered their partners, with 45% having repeated their violence three or more times in that period. We found no significant differences among prosecution tracks for either the OSA or VC cases in rates of prior violence, as expected with randomization.

The IDVPE six-month baseline rate is only slightly below that reported for Sherman and Berk's (1984) suspects. In Minneapolis, 80% of the men had assaulted their victims in the six months prior to their arrests. Such high rates of baseline violence stand in marked contrast to rates as low as 12% reported in studies of batterers who were not identified through contact with police or other criminal justice agencies (Straus, Gelles, and Steinmetz, 1980; Straus and Gelles, 1990). However, if one looks only at cases from national surveys where the man had battered two or more times in a year (a rate comparable to IDVPE cases with at least one other violent incident in a year before the study incident), we find a rate between 60% and 80% (Straus and Gelles, 1990). The comparable baseline rates in criminal justice studies indicate that batterers arrested or brought to court are more likely to be the habitual offenders from the general population. This observation is reinforced for IDVPE defendants by the finding of only a small difference between violence in the year prior to prosecution (78%) and violence in the previous six months (74%). The Indianapolis defendants had already demonstrated a propensity for chronic battering.

IV. THE INDIANAPOLIS ON-SCENE ARREST AND PROSECUTION EXPERIMENT: ANALYSIS AND FINDINGS

A. Policy Impacts

This chapter presents basic experimental findings from an analysis of the randomized prosecution tracks for OSA cases. We analyze the effectiveness of alternative policies relative to the standard policy of prosecuting to adjudication with a recommendation for presumptive sentencing -- the control treatment ("Other"). Our analysis begins with outcomes based on official records. Next we consider experimental findings based on available followup victim interviews. We focus on reports of violence from victim interview data to analyze the prevalence, severity, frequency, and timing of new violence within the six months following case settlement. We also present findings on the prevalence of new violence within 6 months of arrest for comparison to the NIJ-funded police experiments. We conclude the discussion of policy impacts with a quasi-experimental analysis of the chance of violence during and after prosecution, given expectations from pre-arrest rates. In the final section, we examine the impacts of actual court outcomes on the chance of followup violence.

1. Official Records

Do official records of violence against victims during a six month followup period show either the Diversion or the Probation with Counseling track to be any more effective in reducing the chance of new violence than tracking toward Other sentence? There were surprisingly few reports to officials on continuing abuse -- too few to discern any significant differences across prosecution policy tracks. As shown in Table IV.1, only 9 cases have any kind of violence reported against the original victims. If we broaden the meaning of "recidivism" to include other kinds of harassment along with violence, we find a slight

Table IV.1: Prevalence of Officially Recorded Violence, Harassment, and Crime against Victims in the Six Months Following Court Settlement, by Prosecution Track for OSA Cases

Type of Reported Violence	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
Violence	1	4	4	9
Harassment	4	7	4	15
Criminal Charge	1	4	2	7
Total Cases:	63	64	63	190

increase in the level of reports, but any apparent effect is lost for the 15 cases of harassment officially noted for the followup period.[1]

Official records are likely to reflect differential victim access to agencies under different treatments. For example, in these cases, men who are in counseling may have acts of violence hidden from official records because victims will report such acts to the counseling agency (rather than criminal justice officials) and the agency may choose not to notify criminal justice officials. The third row of Table IV.1 presents findings for a new "criminal charge" of violence against the original victim -- the official indicator that is least likely to be affected by treatments. It shows no more than four men with new charges under any of the treatments. In summary, we find no evidence from official records that alternative prosecutorial tracks have any effect on the prevalence of new violence within the six months following case settlement.

[1] Our choice of the term "harassment" is meant to include non-violent acts or disturbances between the victim and defendant along with physical violence directed against the victim, i.e., actual recidivism.

2. Victim Interview Reports

Claims of new violence made by victims in response to interview questions far outnumber those in official records. We take the victim interview data as not only more comprehensive, but also more reliable than official reports (for reasons discussed earlier). In this section, we present conditional findings based on interview responses.

a. Violence Within Six Months of Court Settlement

Table IV.2 gives findings for all cases, as randomized, including those without interviews (labelled "missing data"). The analysis of outcome measures is confounded by missing case information. Whether or not we obtained an OSA followup interview varied by prosecutorial track. What variation we observe in reported violence reflects our interview success in addition to any policy impacts. We draw no conclusion from this table apart from noting that the findings could be very different depending on how the violence of missing cases might have been distributed.

The remainder of this section focuses exclusively on reports from victim followup interviews. These are conditional findings, i.e., indicators of victim abuse, *given successful completion of followup interviews*. Missing cases are ignored. We saw earlier (Table III.1) that the subset of OSA cases with completed victim followup interviews was generally

Table IV.2: Prevalence of Violence Within Six Months of Court Settlement as Reported in Victim Followup Interviews (Includes Missing Data), by Prosecution Track for OSA Cases

Type of Report	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
Violence	23.8%	23.4%	15.9%	21.1%
No Violence	46.0	28.1	30.2	34.7
Missing Data	30.2	48.4	54.0	44.2
N	63	64	63	190

representative of all OSA cases. Comparing available indicators for the total sample with those of the interview subset within tracks similarly shows no significant differences. Thus, in proceeding, we take findings from analyses of the interview subsample as representative of the total.

Table IV.3 presents the conditional effects recomputed from Table IV.2. There is little difference in the prevalence of followup violence for either counseling track in comparison to Other Sentence, as confirmed by the non-significant coefficients of the corresponding logit analysis.[2] Similar findings obtain for followup violence measured by the Conflict Tactics Scale (CTS) (Straus, 1990). Tables IV.4 and IV.5 show findings that closely match those for general reports of violence given in the previous two tables. The

Table IV.3: Prevalence of Violence Within Six Months of Court Settlement as Reported in Completed Victim Followup Interviews, by Prosecution Track for OSA Cases

Type of Report	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
Violence	34.1%	45.4%	34.5%	37.7%
No Violence	65.9	54.6	65.5	62.3
N	44	33	29	106
Logit Estimates:			(reference)	
coef.	-.02	.46	-.64	
t	-0.04	0.88	-1.64	
p(t)	.97	.38	.10	

[2] The analyses presented here compare a control or reference policy (Other) with each of two experimental policies (Diversion and Probation with counseling). A logit regression with the experimental policies represented as dummy regressors estimates coefficients used to test their effects in comparison to the reference policy. The antilog of a dummy coefficient gives the odds of experiencing violence under the corresponding policy versus the reference policy. The t-test indicates the statistical significance of b in contrast to the reference, i.e., the significance of the difference in their effects on the odds of new violence.

Table IV.4: Prevalence of CTS Violence Within Six Months of Court Settlement as Reported in Victim Followup Interviews (Includes Missing Data), by Prosecution Track for OSA Cases

Type of Report	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
CTS Violence	23.8%	26.6%	14.3%	21.6%
No CTS Violence	46.0	25.0	31.7	34.2
Missing Data	30.2	48.4	54.0	44.2
N	63	64	63	190

Table IV.5: Prevalence of CTS Violence Within Six Months of Court Settlement as Reported in Completed Victim Followup Interviews, by Prosecution Track for OSA Cases

Type of Report	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
CTS Violence	34.1%	51.5%	31.0%	38.7%
No CTS Violence	65.9	48.5	69.0	61.3
N	44	33	29	106
Logit Estimates:			(reference)	
coef.	.14	.86	-.80	
t	0.27	1.62	-1.99	
p(t)	.79	.11	.05	

high rates of missing data in Table IV.4 again confound the interpretation of differences in proportions of reported violence. Findings for CTS followup violence against victims with completed interviews suggest that defendants tracked toward counseling as a condition of

probation are somewhat more likely to batter their victims, but its difference from Other Sentencing is not statistically significant.

b. Severity of Violence in the Six Months Following Court Settlement

The lack of significant differences in the chance of violence across tracks does not preclude the possibility that whatever new violence occurs is more or less severe depending on how the case was prosecuted. Victim interviews provide two different measures of severe violence -- a subscale of the CTS and a single-item indicator of the severity of the first incident of followup violence.

The severe CTS Violence subscale measures whether or not the victim experienced any one of several more dangerous acts -- kicked, bit, punched, hit with an object, beat up, threatened with or injured by a knife or gun (see Straus, Gelles, and Steinmetz, 1980). Table IV.6 shows fewer reports but an otherwise similar pattern of violence by tracks as seen for general CTS violence. A greater percentage (36%) of women with cases tracked toward counseling as a condition of probation rather than Diversion or Other suffered severe

Table IV.6: Prevalence of Severe CTS Violence Within Six Months of Court Settlement as Reported in Completed Victim Followup Interviews, by Prosecution Track for OSA Cases

Type of Report	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
Severe CTS Violence	20.4%	36.4%	20.7%	25.5%
Not Severe or None	79.6	63.6	79.3	74.5
N	44	33	29	106
Logit Estimates:			(reference)	
coef.	-.01	.78	-1.34	
t	-0.02	1.34	-2.93	
p(t)	.98	.18	.004	

followup violence. As before, however, the rate is not significantly greater than that experienced under the Other track.

Women who said they had been battered during the 6 months following court settlement were asked whether or not the first new incident of violence was more severe, the same, or less severe than the violence that had resulted in arrest and initiation of the prosecution process. Only two said the new violence was more severe; five others said it was "about the same." There were no significant differences in reports of severity by prosecution track. Thus, by either measure of severity, prosecution policy calling for counseling is no different than tracking toward Other sentencing in affecting the severity of new violence.

c. Frequency of Violence in the Six Months Following Court Settlement

There is some concern among researchers and practitioners today that the frequency

Table IV.7: Frequency of Violence Within Six Months of Court Settlement as Reported in Victim Followup Interviews, by Prosecution Track for OSA Cases

Number of Violent Incidents	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
0	65.9%	54.6%	65.5%	62.3%
1	6.8	15.2	10.3	10.4
2	2.3	6.1	6.9	4.7
3	4.6	6.1	6.9	5.7
4	6.8	0.0	3.4	3.8
5-10	2.3	9.1	3.4	4.7
11-49	2.3	9.1	3.4	4.7
50-100	9.1	0.0	0.0	3.8
N	44	33	29	106
Mean	7.05	4.09	1.48	4.60
Std. Dev.	20.07	9.34	3.86	14.17
Analysis of Log(X+1):			(reference)	
Mean	.731	.774	.471	
Std. Dev.	1.30	1.12	.78	
Dunnett's t	.98	1.14	--	
p(t)	> .05	> .05		
			$MS_{error} = 1.2548$	

of battering may be as important as severity in predicting future lethal violence (Straus, personal communication). Is one prosecution policy better than another in reducing (or increasing) the frequency of followup violence? Apparently not, as reported in Table IV.7. Victims' recollections of the numbers of violent incidents against them in the six months following court settlement are unrelated to assigned prosecution policies. Overall, OSA victims report an average of 4.6 violent episodes over six months. The distribution of incidents displayed suggests that perhaps those defendants offered diversion agreements have the highest frequencies of battering after their cases are settled. Indeed, among defendants who batter again, those processed on the Diversion track have the highest mean frequencies, although the relatively small numbers of such cases exaggerate the observed differences.

The bottom of Table IV.7 shows a comparison of the means of log frequencies for Diversion and Probation against the control policy.[3] Once again we find no significant

Table IV.8: Victim Feelings that Men are Less Violent Six Months Following Court Settlement as Reported in Completed Followup Interviews, by Prosecution Track for OSA Cases

Type of Report	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
Less Violent	70.4%	60.6%	48.3%	61.3%
More or Same	29.6	39.4	51.7	38.7
N	44	33	29	106
Logit Estimates:			(reference)	
coef.	.94	.50	-.07	
t	1.89	0.97	-0.19	
p(t)	.06	.33	.85	

[3] A logarithmic transformation of reported frequencies was done to reduce the magnitude of variances inflated by a few extremely high counts of violent incidents and thus to enable statistical comparisons of means across tracks.

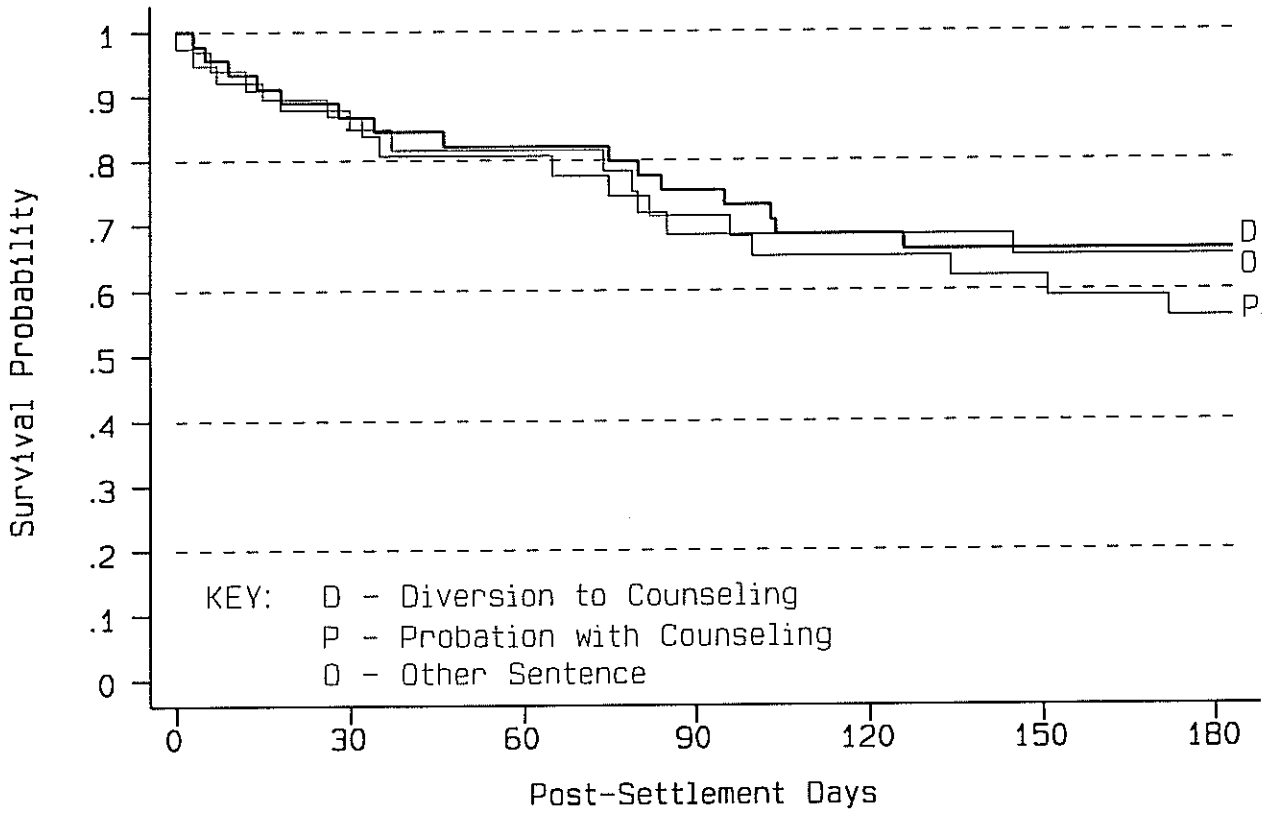
differences. Whichever counseling policy is pursued following an on-scene warrantless arrest, it is no better or worse than Other sentencing in protecting victims from frequent, repetitive violence.

Notwithstanding these "objective" victim reports indicating no differences in new violence by tracks, women whose batterers were tracked toward pretrial diversion were much more likely to report "feeling" that their abusers were less violent at the end of the six month followup period, as shown in Table IV.8.

d. Time to Failure Within Six Months of Court Settlement

Analysis of violent episodes within a fixed, six-month followup period gives us both a basis for comparing the prevalence of violence before and after prosecution and a perspective on the overall chance of new violence during that timeframe. However, a closer examination

Figure IV.1: Kaplan-Meier Estimates for Time-to-Failure



of when the new violence occurs gives additional insight into a woman's risk of victimization throughout the period. Moreover, as a technical matter, methods for analyzing the time leading up to new incidents avoid problems associated with fixed-time analyses while accounting for the possibility of new violence beyond the fixed timeframe (see, for example, Allison, 1984, Cox and Oakes, 1984, or Yamaguchi, 1991). With these considerations, here we report results of an analysis of the time from settlement to the first episode of new violence.

Figure IV.1 is the Kaplan-Meier plot for each experimental policy track. Clearly, there is little difference between tracks in survival probabilities through the six-month followup period. Thirteen percent of defendants battered their victims anew within the first 30 days post-settlement. Survival probabilities gradually fall another 20% over the next 5 months. Table IV.9 gives detail on the distribution of cases with followup violence by

Table IV.9: Time to New Violence up to Six Months After Court Settlement as Reported in Victim Followup Interviews, by Prosecution Track for OSA Cases

Months before First New Violent Incident	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
0	6	5	4	15
1	2	2	2	6
2	3	3	4	10
3	3	2	0	5
4	1	1	1	3
5	0	2	0	2
Censored at 1 mo.	1	5	2	8
Censored at 6 mos.	29	18	20	67
N	45	38	33	116
Cox Regression:			(reference)	
coef.	-.05	.24	---	
t	-.13	.61	---	
p(t)	.90	.55	---	

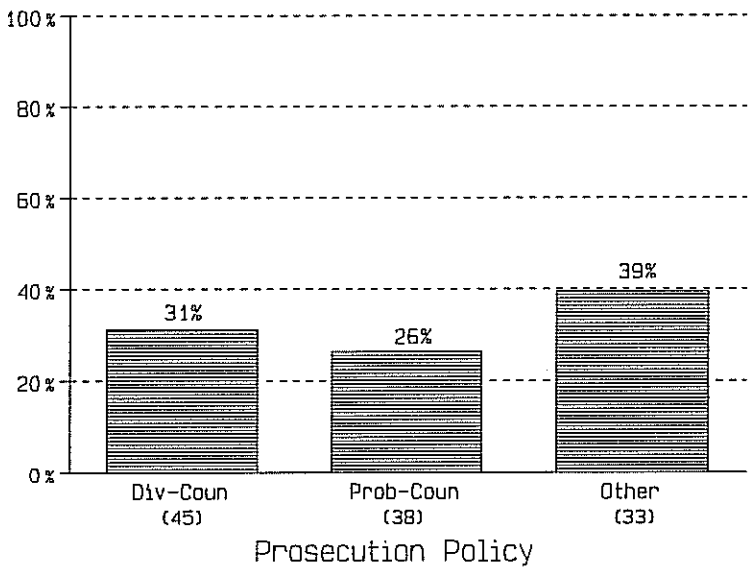
month. The Cox proportional hazards analysis confirms the lack of significant differences in survival rates among tracks.

e. Violence Within Six Months of Arrest

Our principal concern is to analyze measures of new violence following the settlement of cases in court. However, it is of interest to examine the chance of violence within six months of an on-scene arrest for comparison to police experiments conducted in Minneapolis and elsewhere. What protection can victims expect in the six months after an abuser's arrest if followed by prosecution?

Figure IV.2 charts the prevalence of followup violence for each OSA prosecution track (cf. Sherman and Berk, 1984a).

Figure IV.2: Prevalence of Violence Within Six Months of an On-Scene Warrantless Arrest by Prosecution Track



No one prosecution track is more effective than another in preventing continuing violence. Still, an on-scene arrest followed by any sort of prosecutorial tracking seems to result in less violence than expected based on victim interview reports of incidents in the six months prior to arrest. But is prosecutorial tracking responsible for the reduced rate? We cannot say based on IDVPE data, although a comparison with findings from police arrest experiments suggests that arrest alone may be more effective in protecting victims than is arrest and prosecution. Sherman and Berk (1984a) report a 19% rate of six-month followup violence following arrest without prosecution. The IDVPE rate of 37% appears remarkably high by

comparison.[4] Other experiments on arrest found prevalence rates at least as high as in Indianapolis: Omaha, 38%; Charlotte, 59%; Milwaukee 24% (of "hotline" calls).

We do not have data to consider the question of whether prosecution versus no prosecution following arrest truly makes a difference on the chance of new violence. However, this is not a question of practical interest for Marion County officials, given their commitment to prosecuting following arrest. But given arrest, subsequent victim security is unaffected by how the case is tracked for prosecution.

A final concern, given any violence following arrest, is whether arrest provokes new violence. Followup interviews with OSA victims showed that few (3) attributed new violence to retaliation for arrest. What violence occurred represented a continuation of an established pattern rather than a criminogenic effect (see Ford, 1991a).

f. Quasi-Experimental Before-After Comparison for Prevalence of Violence

Apart from differences across policies, does on-scene arrest followed by prosecution reduce the chance of new violence? Apparently it does. Each defendant in this study is presumed to have committed an act of violence just prior to his case being taken up by the Prosecutor's Office, i.e., the sample has a 100% rate of violence in the six months preceding contact with criminal justice agents. Thus, any prevalence rate below 100% for followup violence, such as already shown, demonstrates that the criminal justice process, through court settlement, has a desirable preventive impact on batterers. However, allowing for the possibility that the presenting violent incident is an anomaly and should not be counted in the baseline expectation for violence in a six-month timeframe, we take victim interview reports of violence in the six months prior to the presenting incident as baseline data for violence before prosecution.

OSA victims report that 75% of their abusers had battered them in the six months prior to arrest on the study incident. A remarkable 20% of all OSA defendants battered again before their cases were settled, and, as we saw earlier, 38% batter during the six-

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[4] The rates of repeat violence based on official records show an opposite influence -- 10% in Minneapolis versus 5% in Indianapolis.

month followup period. If prosecution prevents continuing violence by most defendants, some clearly are unfazed by the experience. Differences across tracks are non-significant.[5]

Figure IV.3: Victim Reports of Violence by OSA Defendants Estimated for 6 Months Before, During, and After Prosecution

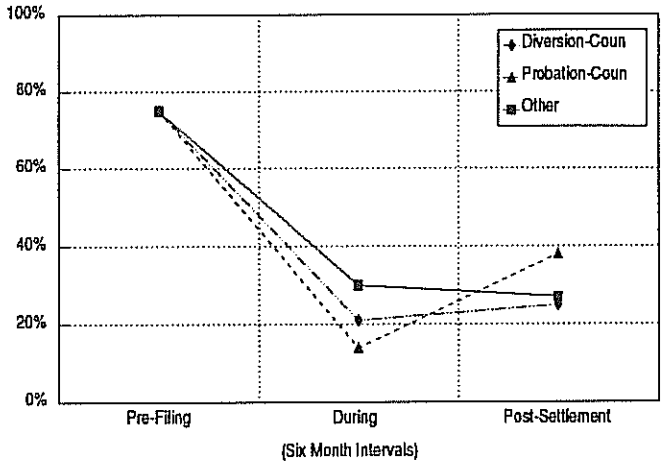

Figure IV.3 summarizes the change in a victim's expected chance of being battered by the same man before, during, and after prosecution, correcting for differences in levels of prior violence and using estimated rates for a standard 6-month processing ("during") time.[6]

g. Effects on Other Outcome Measures

Apart from indicators of new violence within a six-month followup period, victims were asked about other feelings of danger or fear as well as impressions of the effectiveness of the criminal justice intervention in changing their batterers' behavior. We first asked about threatened violence. Forty-two percent of the OSA victim interviewees said they were threatened with violence at some point in the six-month followup period; 28% said their partners threatened to kill them; 49% felt they were in danger of being battered at least once

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[5] Different tracks result in different average numbers of days between filing and settlement (Diversion, 99; Probation, 138; Other, 137) and thus more or less opportunity for pre-settlement violence. Nevertheless, differences in pre-settlement violence across tracks remain non-significant after controlling for days in the system.

[6] Percentages for "Pre-Filing" are set to the overall OSA percentage of 6 month pre-filing violence. "During" percentages are estimated for a standardized 6-month process using a logit regression of violence during the process on prosecution tracks, reports of violence in the previous 6 months, and length of time in the process. "Post-Settlement" percentages are estimated from a logit regression of 6-month followup violence on prosecution tracks and the predicted "during" percentages.

during the followup period. Thirty-eight percent of the victims estimated they had at least a 50-50 chance of being victimized again six months after case settlement.

Fear and violence after court did not necessarily leave victims feeling worse off than before. Sixty-four percent said they felt more secure and 75% reported feeling in greater control of their situations as a result of having charges filed against their abusers than before. Sixty-nine percent expressed satisfaction with the criminal justice system. None of these alternative outcome measures varies significantly with prosecution policy. In light of Figure IV.3, what seems to matter more than how a defendant was processed is the fact that he was processed at all.

3. Summary of On-Scene Arrest Policy Impacts

Men arrested on-scene for battery and processed under alternative prosecution policies show no difference in the prevalence, severity, mean frequency, or timing of new violence in a six-month followup period. Nor do their victims report differences in their feelings of fear, danger, security, or control according to how their partners were prosecuted. The fact that defendants are arrested and prosecuted apparently reduces the chance of new violence during and six months after prosecution, regardless of prosecution track.

B. Impacts of Court Outcomes

One normally expects criminal justice policy to describe real, predictable criminal justice activities. We saw earlier, however, that criminal justice practice often diverges from policy guidelines. Innovative policies, in particular, risk failing to be implemented as designed because they lack a history of structural support and compliance. The IDVPE policy findings are mediated by successful implementation of practices consistent with policy, and as discussed earlier, prosecution policy invites many opportunities for untracking by the time a case comes to court for final settlement. In this section we examine how effectively policy determines court outcomes -- "delivered treatments." First, we look at the impact of court outcomes on prevalence rates for followup violence. Next, we discuss the relationship between policy tracks and court outcomes. Finally, we analyze the impacts of alternative court outcomes *within policy tracks*.

Table IV.10: Prevalence of Violence Within Six Months of Court Settlement as Reported in Completed Victim Followup Interviews, by Court Outcome for OSA Cases

Type of Report	Court Outcome					Total
	Case Dismissed	Diversion to Couns.	Probation w/Couns.	Other Sentence	Not Guilty	
Violence	50.0%	37.5%	45.8%	34.6%	0.0%	37.7%
No Violence	50.0	62.5	54.2	65.4	100.0	62.3
N	16	32	24	26	8	106
Logit Estimates:				(reference)	[dropped]	
coef.	1.02	.51	.85	-1.02		
t	1.61	.96	1.51	-2.63		
p(t)	.11	.34	.13	.01		

Could our finding of "no difference" among policies in the prevalence of followup violence somehow mask significant differences arising from actual court outcomes? Apparently not, according to the figures in Table IV.10. Apart from those found not guilty, OSA defendants are no more likely to continue battering under one court outcome than another. Specifically, neither of the counseling outcomes is more effective in reducing violence than Other Sentencing. And based on the small sample of cases that were dismissed, even dropping charges does not pose a significantly greater risk of followup violence.

Court outcomes are the culmination of decisions and actions by all players in the prosecution process. Table IV.11 shows large discrepancies between policy tracks and actual court outcomes for all OSA cases, including those settled with a finding of not guilty and those dismissed contrary to the No-Drop policy.[7] Obviously, the No-Drop policy is not entirely effective in moving cases to expected settlements. One out of 5 OSA cases was

[7] See Table III.4 for complete frequencies.

Table IV.11: Distribution of Court Settlements by Prosecution Track for OSA Cases (Excludes Open Cases and Findings of Not Guilty)

Court Settlement	Prosecution Track			Total
	Diversion to Counseling	Probation with Counseling	Other Sentence	
Case Dismissed	14.0%	29.3%	23.6%	22.4%
Diversion-Counsel.	73.7	1.7	1.8	25.9
Probation-Counsel.	7.0	53.5	7.3	22.9
Other Sentence	5.3	15.5	67.3	28.8
N	57	58	55	170

dismissed, most often because the victim failed to appear for more than one scheduled court date or because the arresting police officer failed to appear and the victim would not corroborate the officer's probable cause affidavit justifying arrest.[8]

Given court outcomes contrary to policy, it remains to be seen if a unique combination of policy and practice might matter in preventing new violence. Though initiated under written policy and processed according to prosecution tracks anticipating specific outcomes, how a case is ultimately settled and whether or not it results in continuing violence may be affected by a number of uncontrolled factors associated with unique randomized tracks. In a final set of OSA analyses, we examined the effect of court settlements on six-month followup violence within each prosecution track, with controls for the following:

Defendant's age, race, education, and employment status
Defendant's relation to the victim and whether or not they cohabited during the process
Number of children victim and defendant had together

[8] If one ignores the nolled and not guilty cases, there is an 83% consistency rate between prosecution policy and court outcomes.

Defendant's criminal history, including a record of prior criminal violence
Victim claims of prior violent victimization by the defendant
Victim claims of battery against her by the defendant during the process
Defendant's alcohol consumption as described by the victim
Number of days from filing to settlement of the case and the number of continuances during that time
Days spent in jail prior to court settlement
Whether or not defendant had an attorney

Under each prosecution track, any control variable found to be either a consequence of prosecution tracking (e.g., pre-settlement violence) or a possible determinant of how a case is settled was used in further analyses. Most such indicators in the IDVPE are available only from completed interviews, thus reducing the size of the analysis subsamples and restricting possible statistical controls. For each prosecution track, we ran a logit regression of followup violence on two variables -- a dummy variable for one court settlement (Diversion, Probation, or Other) and a measure from among the candidate controls.

The chance of six-month followup violence is unaffected by how a case is ultimately settled in court pursuant to tracking under a particular policy. Logit regressions for violence on court settlement, within prosecution tracks, replicate the overall finding of no difference in the impacts of alternative outcomes. Moreover, we found that no variable other than prosecution policy accounts for how a case is settled in court. And none of the measured variables alters the general conclusion that followup violence is unaffected by actual court outcomes. In short, we find no evidence that the conclusion of no difference in the prevalence of violence is due to the suppressing effect of any of our control variables.

V. THE INDIANAPOLIS VICTIM COMPLAINT AND PROSECUTION EXPERIMENT: ANALYSIS AND FINDINGS

A. Policy Impacts

Prosecutorial tracking after a victim complaint to the Prosecutor's Office differs from the process for on-scene arrests in two respects. First, it entails an initial decision to issue either a summons or an arrest warrant to bring a suspect into the criminal process. Second, it incorporates an additional policy -- Drop-Permitted -- under which a victim is allowed to have charges dismissed as an exception to the usual No-Drop policy in effect for other treatments. Analysis of the impacts of policies for victim-complaint cases evaluates the effect of each pair of entry and prosecution tracks against the traditional (control) track of Summons/Other. As before, we present findings on policy impacts first for official records and then for reports in victim followup interviews.

1. Official Records

Officially recorded complaints of violence, harassment, or crime allegedly committed by VC defendants have the same shortcomings discussed for OSA cases -- relatively few reports and treatment-related reporting. Nonetheless, Table V.1 suggests that men processed under the Warrant/Drop-Permitted track are least likely to reoffend with violence, harassment, or a new criminal charge[1] during the six month followup period than under other policies. Warrant/Diversion also has very few reports. However, Summons/Diversion has the highest number of new reports, indicating that the combined entry/prosecution track experience affects the prevalence of new offenses independent of treatment-related reporting.

2. Victim Interview Reports

Victim interview reports are both more numerous and more demonstrative of policy impacts on VC cases. This section presents analyses of alternative measures for effects associated with tracking under the several VC policy options.

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[1] Refer to Section IV.A.1 for a discussion of these measures.

Table V.1: Officially Recorded Violence, Harassment, and Crime against Victims in the Six Months Following Court Settlement, by Prosecution Track for VC Cases

Type of Report	Prosecution Track: Warrant – Drop Permitted	Warrant – Diversion to Couns.	Warrant – Probation w/ Couns.	Warrant – Other Sentence	Summons – Drop Permitted	Summons – Diversion to Couns.	Summons – Probation w/ Couns.	Summons – Other Sentence	Total
Violence	0	0	2	1	3	6	2	3	17
Harassment	1	3	5	5	4	10	4	9	41
Criminal Charge	0	0	2	1	2	4	2	3	12
Total Cases:	56	57	58	56	56	55	58	56	452

a. Violence Within Six Months of Court Settlement

Victims who initiate charges against their abusers with a complaint to the prosecutor are more likely to grant followup interviews and less likely to report new violence than OSA victims. But at least 20% of all VC victims experience new violence within six months of settlement. As Table V.2 shows, this figure varies across tracks for the entire sample of settled cases with responses to the single-item measure of violence.

Table V.2: The Prevalence of Violence Within Six Months of Court Settlement as Reported in Victim Followup Interviews, by Prosecution Track for VC Cases

Type of Report	Prosecution Track: Warrant – Drop Permitted	Warrant – Diversion to Couns.	Warrant – Probation w/ Couns.	Warrant – Other Sentence	Summons – Drop Permitted	Summons – Diversion to Couns.	Summons – Probation w/ Couns.	Summons – Other Sentence	Total
Violence	8.9%	21.1%	19.0%	19.6%	25.0%	18.2%	22.4%	30.4%	20.6%
No Violence	58.9	47.4	51.7	50.0	53.6	61.8	46.6	39.3	51.1
Missing Data	32.1	31.6	29.3	30.4	21.4	20.0	31.0	30.4	28.3
N	56	57	58	56	56	55	58	56	452

Table V.3: The Prevalence of Violence Within Six Months of Court Settlement as Reported in Victim Followup Interviews, by Prosecution Track for VC Cases

Type of Report	Prosecution Track: Warrant – Drop Permitted	Warrant – Diversion to Couns.	Warrant – Probation w/ Couns.	Warrant – Other Sentence	Summons – Drop Permitted	Summons – Diversion to Couns.	Summons – Probation w/ Couns.	Summons – Other Sentence	Total
Violence	13.2%	30.8%	26.8%	28.2%	31.8%	22.7%	32.5%	43.6%	28.7%
No Violence	86.8	69.2	73.2	71.8	68.2	77.3	67.5	56.4	71.3
N	38	39	41	39	44	44	40	39	324
Logit Estimates:								(reference)	
coef.	-1.63	-.55	-.75	-.68	-.50	-.97	-.47	-.26	
t	-2.82	-1.17	-1.56	-1.41	-1.10	-2.00	-1.01	-0.80	
p(t)	.01	.24	.12	.16	.27	.05	.31	.42	

The 9% recidivism rate for Warrant/Drop-Permitted cases in Table V.2 is less than half the rate for any other policy. When analyzed for cases with completed victim interviews, we find a rate of 13% under the Warrant/Drop-Permitted track and 23% under Summons/Diversion, each significantly lower than Summons/Other, as shown in Table V.3. The odds of new violence under Warrant/Drop-Permitted are 1/5 those of Summons/Other.

Warrant/Drop-Permitted similarly results in the lowest rates of new violence as indicated by the CTS for both the complete sample of settled cases (Table V.4) and the interview sample (Table V.5). Only the Warrant/Probation track also results in a significantly lower chance of CTS violence than under traditional tracking. In short, by either measure of new violence, Warrant/Drop-Permitted has the lowest rate and Summons/Other has the highest rate of followup violence.

b. Severity of Violence in the Six Months Following Court Settlement

The consistency of findings for alternative violence indicators increases our confidence in whatever effects policy has on wife-battering. Of course, it is conceivable that

Table V.4: The Prevalence of CTS Violence Within Six Months of Court Settlement as Reported in Victim Followup Interviews, by Prosecution Track for VC Cases

	Prosecution Track								
	Warrant				Summons				
Type of Report	Drop Permitted	Diversion to Couns.	Probation w/ Couns.	Other Sentence	Drop Permitted	Diversion to Couns.	Probation w/ Couns.	Other Sentence	Total
Violence	12.5%	26.3%	19.0%	19.6%	25.0%	27.3%	24.1%	33.9%	23.5%
No Violence	55.4	42.1	51.7	50.0	53.6	52.7	44.8	35.7	48.2
Missing Data	32.1	31.6	29.3	30.4	21.4	20.0	31.0	30.4	28.3
N	56	57	58	56	56	55	58	60	452

a preventive effect for one indicator may be countered by a negative effect for another. Here we analyze the seriousness of violence to discover whether the Drop-Permitted policy, in particular, reduces severe assaults. We consider both the severe CTS measure and the severity rating of the first incident of followup violence, as done in the OSA analysis.

Table V.5: The Prevalence of CTS Violence Within Six Months of Court Settlement as Reported in Victim Followup Interviews, by Prosecution Track for VC Cases

	Prosecution Track								
	Warrant				Summons				
Type of Report	Drop Permitted	Diversion to Couns.	Probation w/ Couns.	Other Sentence	Drop Permitted	Diversion to Couns.	Probation w/ Couns.	Other Sentence	Total
CTS Violence	18.4%	38.5%	26.8%	28.2%	31.8%	34.1%	35.0%	48.7%	32.7%
No CTS Violence	81.6	61.5	73.2	71.8	68.2	65.9	65.0	51.3	67.3
N	38	39	41	39	44	44	40	39	324
Logit Estimates:								(reference)	
coef.	-1.44	-.42	-.95	-.88	-.71	-.61	-.57	-.05	
t	-2.73	-1.91	-2.00	-1.84	-1.56	-1.35	-1.23	-0.16	
p(t)	.01	.36	.05	.07	.12	.18	.22	.87	

Table V.7: Frequency of Violence Within Six Months of Court Settlement as Reported in Victim Followup Interviews, by Prosecution Track for VC Cases

Number of Violent Incidents	Prosecution Track								Total
	Warrant				Summons				
	Drop Permitted	Diversion to Couns.	Probation w/ Couns.	Other Sentence	Drop Permitted	Diversion to Couns.	Probation w/ Couns.	Other Sentence	
0	86.8%	69.2%	73.2%	71.8%	68.2%	77.3%	67.5%	56.4%	71.3%
1	2.6	10.3	4.9	5.1	13.6	11.4	5.0	10.3	8.0
2	2.6	0.0	7.3	2.6	9.1	4.6	15.0	7.7	6.2
3	0.0	0.0	7.3	2.6	0.0	2.3	0.0	5.1	2.2
4	2.6	2.6	0.0	0.0	2.3	2.3	0.0	2.6	1.5
5-10	5.3	10.3	2.4	12.8	4.5	2.3	5.0	2.6	5.6
11-49	0.0	2.6	4.9	5.1	0.0	0.0	7.5	10.3	3.7
50-100	0.0	5.1	0.0	0.0	2.3	0.0	0.0	5.1	1.5
N	38	39	41	39	44	44	40	39	324
Mean	.45	6.08	1.46	3.46	2.25	.48	1.93	6.85	2.82
Std. Dev.	1.31	20.36	4.35	9.98	10.54	1.11	4.29	17.24	10.96
Analysis of Log(X+1):								(control)	
Mean	.18	.63	.41	.57	.41	.24	.52	.86	
Std. Dev.	0.50	1.21	0.78	1.08	0.81	0.48	0.89	1.30	
Dunnett's t	-3.31	-1.14	-2.22	-1.40	-2.19	-3.04	-1.65	--	
p(t)	$<.05$	n.s.	n.s.	n.s.	n.s.	$<.05$	n.s.		
								MS_{error} = .8440	

significantly lower than the mean number of incidents under the Summons/Other track (6.85) (Table V.7). Only Summons/Diversion has a mean number (.48) that is also significantly lower than the control policy.

Considering the more general subjective feelings of whether a batterer is more or less violent six months after court settlement, Warrant/Drop-Permitted again has the most favorable reports with 78% of the women stating that their partners are "less violent." By these accounts, a majority of defendants are less violent under every policy, but as shown in Table V.8, none was significantly different from Summons/Other.

Table V.8: Victim Feelings that Men are Less Violent Six Months after Court Settlement as Reported in Victim Followup Interviews, by Prosecution Track for VC Cases

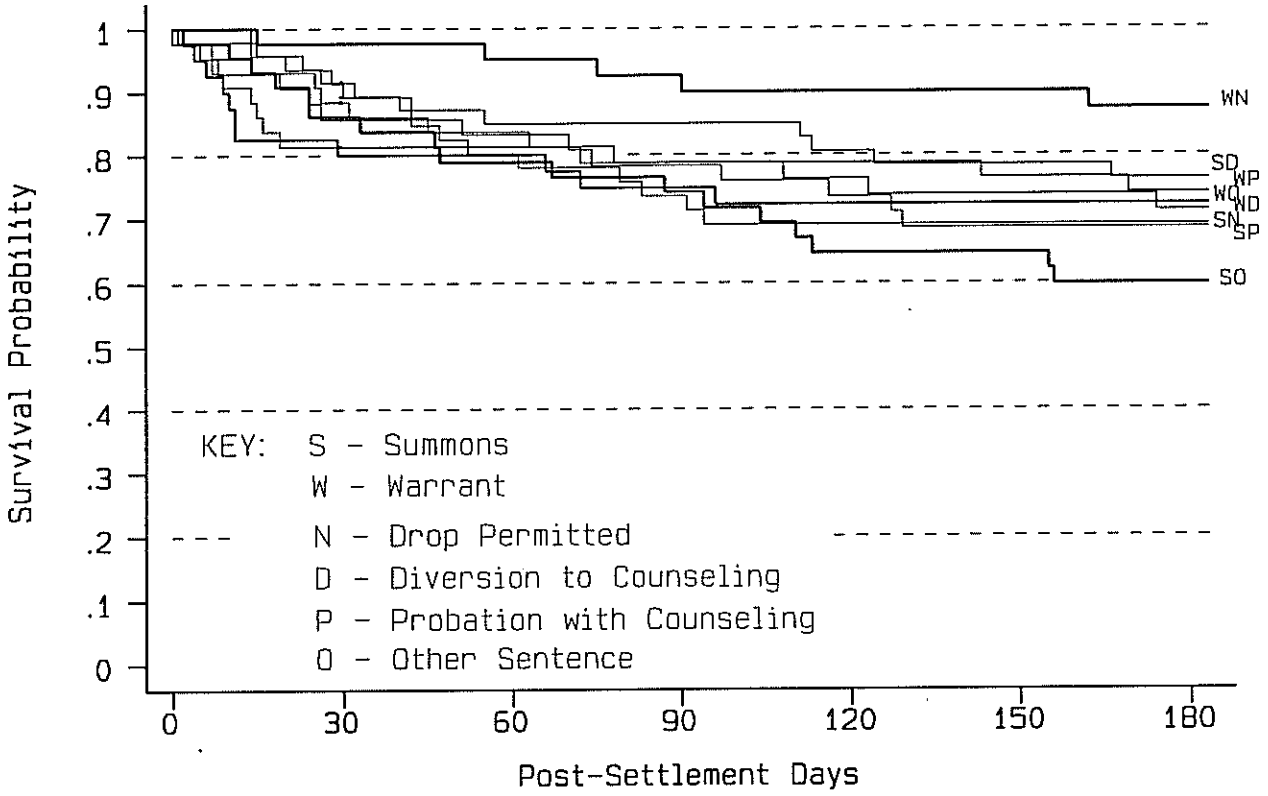
	Prosecution Track								
	Warrant				Summons				
Type of Report	Drop Permitted	Diversion to Couns.	Probation w/ Couns.	Other Sentence	Drop Permitted	Diversion to Couns.	Probation w/ Couns.	Other Sentence	Total
Less Violent	78.4%	76.9%	65.9%	52.6%	68.2%	63.6%	62.5%	69.2%	67.1%
More or Same	21.6	23.1	34.1	47.4	31.8	36.4	37.5	30.8	32.9
N	37	39	41	38	44	44	40	39	322
Logit Estimates:								(intercept)	
coef.	.48	.39	-.15	-.71	-.05	-.25	-.30	.81	
t	0.90	0.76	-0.32	-1.48	-0.10	-0.54	-0.63	2.34	
p(t)	.37	.45	.75	.14	.92	.59	.53	.02	

d. Time to Failure Within the Six Months Following Court Settlement

The consistent finding of greatest victim protection under a policy that allows a victim to drop charges after her abuser's arrest on a warrant may yet belie violence evoked by having had the opportunity to drop. As discussed earlier, some advocates for battered women are concerned that being allowed to drop enables intimidation and coercion that is more likely to result in violence than is being prohibited from dropping. Figure V.1 presents evidence to the contrary. Defendants processed under the Warrant/Drop-Permitted policy are less likely to batter again, in comparison to others, throughout the followup period.

Table V.9 gives detail of the data on the timing of new violence along with a Cox proportional hazard analysis to assess the significance of differences between Other sentence and each other policy. Once again, Warrant/Drop-Permitted is found to differ significantly from Summons/Other, in this instance in delaying new violence throughout the followup period.

Figure V.1: Kaplan-Meier Estimates for Survival Distributions



e. Violence Within Six Months of Filing Charges

To be arrested is a sufficiently punishing event for most people that it might have a deterrent effect within the next six months regardless of whether or not other criminal justice processing follows. This is the reasoning behind research on arrests. On the other hand, to have charges filed against you by a complainant is not such an obvious punishment, and any effects are usually taken as arising from the official outcome of subsequent processing. In fact, 46% of the VC men described having had their victims file charges as "extremely punishing" in contrast to 34% of the OSA men who described being arrested as "extremely punishing." And by comparison to the 37% chance of new violence six months after an on-scene arrest (see section IV.A.2.e), 30% of the VC defendants are alleged to have again battered the same victim. Figure V.2 shows the prevalence of violence within six months of

Table V.9: Time to New Violence up to Six Months After Court Settlement as Reported in Victim Followup Interviews, by Prosecution Track for VC Cases

Months Before First New Violent Incident	Prosecution Track: Warrant – Drop Permitted	Warrant – Diversion to Couns.	Warrant – Probation w/ Couns.	Warrant – Other Sentence	Summons – Drop Permitted	Summons – Diversion to Couns.	Summons – Probation w/ Couns.	Summons – Other Sentence	Total
0	1	8	5	8	4	4	6	6	42
1	1	0	2	0	5	3	1	3	15
2	1	1	2	2	3	0	2	2	13
3	1	1	0	1	2	2	2	4	13
4	0	1	1	0	0	1	2	0	5
5	1	1	1	0	0	1	0	2	6
Censrd at 1 mo.	2	3	1	1	1	1	1	1	11
Censrd at 6 mos.	34	28	31	28	31	35	28	25	240
N	41	43	43	40	46	47	42	43	345
Cox Regression:								(reference)	
coef.	-1.32	-.33	-.49	-.35	-.29	-.62	-.28	---	
t	-2.59	-0.87	-1.26	-0.89	-0.81	-1.60	-0.75	---	
p(t)	.01	.38	.21	.37	.42	.11	.46	---	

victims filing charges by prosecution tracks. Once again, the policy of permitting a victim to drop charges after her batterer has been arrested on a warrant is least likely to result in new violence. Other sentencing is least effective in preventing new violence within six months of filing, regardless of whether a defendant came to court by Summons or by Warrant.

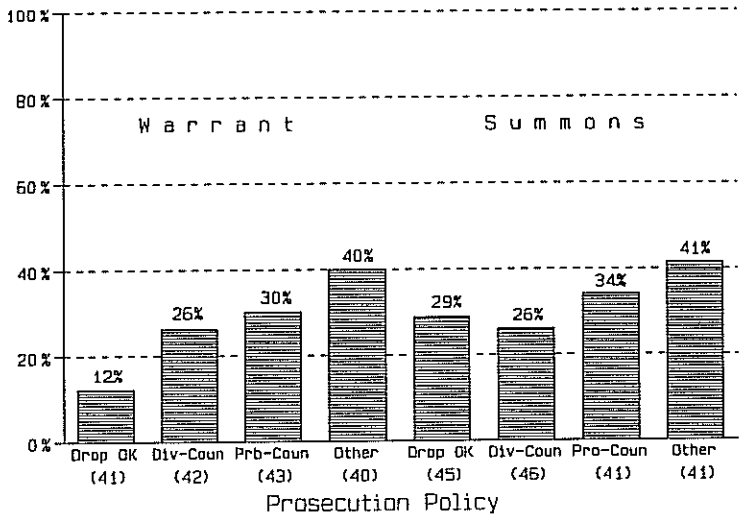
f. Quasi-Experimental Before-After Comparison for Prevalence of Violence

Seventy-two percent of the VC victims interviewed allege to have been battered at least once before in the six months preceding the incident being prosecuted.[2] During the process, 27% of those summoned and 19% of those arrested on a warrant persisted in battering; none of the Warrant/Drop-Permitted victim interviewees reported continuing

[2] In discussing OSA findings (Section IV.A.2.f), we noted that all defendants are presumed to have battered once in the 6 months before the prosecution process and that any followup rate below 100% indicates a desirable reduction in their violence. We elected to count only victim interview reports of violence in the six months prior to the presenting incident as a baseline comparison.

violence. By the end of the prosecution process, defendants under all policies, except those whose victims were allowed to drop charges after a warrant arrest, had again battered the same women.[3] Figure V.3 shows the reduction in the prevalence of battering once a case enters the system under any prosecution track.[4] However, with the exception of Summons/Other, the chance of new violence under the several tracks tends to converge following case settlement. Warrant/Drop-Permitted remains the track with the lowest level of violence.

Figure V.2: Prevalence of Violence Within Six Months of Filing Charges by Prosecution Track



g. Effects on Other Outcome Measures

Prosecution gives victims a sense of control over their lives. In response to a question asking VC victims whether they felt more or less control of their situations, 77% reported "more control." In particular, the principal argument in favor of allowing victims to drop charges is that by taking control of the prosecution process they are empowered to minimize the risk of continuing violence. The highest percentage (87%) reporting "more control" is the Warrant/Drop-Permitted group. Though not significantly greater than the Summons/Other policy, the finding of a greater difference for Warrant/Drop-Permitted in comparison to alternative policies underscores its impact on followup violence as reported

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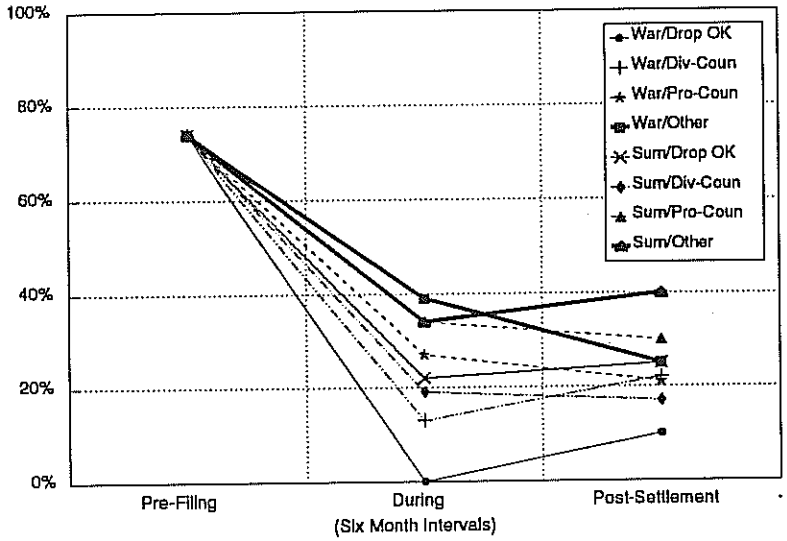
[3] One woman whose case was tracked for Warrant/Drop-Permitted was battered during the process but is not shown here because after granting the 30-day followup interview she was not interviewed again after 6 months.

[4] Figure V.3 estimates violence before, during, and after prosecution given a common starting point (the overall prevalence rate of violence prior to filing) and treating all cases as if settled after 6 months in the prosecution process using the procedures described for Figure IV.3.

above. Similarly, 79% of the Warrant/Drop-Permitted victims report some degree of satisfaction with the way their cases were handled by the criminal justice system in contrast to 65% of the overall VC sample.

Figure V.3: Victim Reports of Violence by VC Defendants Estimated for 6 Months Before, During, and After Prosecution



The effectiveness of Warrant/Drop-Permitted in preventing continuing violence is reflected also in other victim assessments of the prosecution experience. Forty-seven percent of women in the VC experiment report feeling they were in danger of violent victimization at some point within six months of case settlement; Warrant/Drop-Permitted victims have the lowest rate at 34%.[5] Thirty-seven percent of VC victims were threatened with new violence, with rates ranging from 51% for Summons/Other to a low of 24% for Warrant/Drop-Permitted.[6] Twenty-two percent of VC defendants threatened to kill their victims after settlement; Warrant/Drop-Permitted victims reported the lowest rate at 16%. On the other hand, the VC prosecution policies are statistically indistinguishable with respect to feelings of greater security (69%) or that they have a 50-50 chance or greater of suffering new victimization (39%).

The overall VC findings for these outcome measures closely match those reported earlier for the OSA followup interviews. However, in the case of VC victims, how a case is prosecuted makes a difference over and above abusers being charged and processed by the criminal justice system.

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[5] The 34% rate for Warrant/Drop-Permitted victims who felt in danger is significantly lower ($p < .05$) than the rate of 59% for Summons/Other.

[6] The difference is significant at the .05 level.

3. Summary of Victim-Complaint Policy Impacts

Contrary to both traditional criminal justice practice and widely advocated policy, the single policy resulting in consistently superior protection for a battered woman who seeks to prosecute her abuser is to arrest the suspect on a warrant and to tell the victim that she may drop charges if she feels it is in her best interest to do so. Under this policy, victims may expect a lower chance of continuing violence from the time they file through a six-month followup period in comparison to any alternative policy. On average, they will also experience less severe and fewer incidents of violence six months after their cases are settled in court. The protective impact is realized from the time the case enters the prosecution process throughout the followup period. Victim assessments of their circumstances six months after settlement also support Warrant/Drop-Permitted as a preferred policy. Summons/Diversion is the only alternative policy with promise for protecting women from continuing violence. However, its effects under all but one of the various outcome measures analyzed fail to attain statistical significance at the .05 level.

"Business as usual" under the Summons/Other prosecution track does not protect VC victims of domestic violence from further abuse. Nor does tracking toward rehabilitative counseling. It remains to be seen whether or not actual sentencing consistent with the policy tracks delivers the protection expected.

B. Impacts of Court Outcomes

Whatever the impacts of prosecution policy, one would assume that they are due to the effects of court outcomes consistent with policy tracks. But are they? We argued earlier (chapter II, section D) that policy *per se* can result in a reduction of violence. This may be true especially for policy concerning victim wishes to drop charges. On the other hand, it is likely that a prosecution track calling for counseling will only have a preventive effect if the defendant actually receives counseling. This section addresses the issue in analyses of the mediating effect of court settlements on the chance of new violence. As before, we begin with an analysis of the impacts of court settlements on new violence. Then we examine the

Table V.10: Prevalence of Violence Within Six Months of Court Settlement as Reported in Victim Followup Interviews, by Court Outcome for VC Cases

Type of Report	Court Outcomes: Warrant					Summons					Total
	Case Dismissed	Diversion to Couns.	Probation w/ Couns.	Other Sentence	Not Guilty	Case Dismissed	Diversion to Couns.	Probation w/ Couns.	Other Sentence	Not Guilty	
Violence	28.0%	26.8%	22.9%	27.1%	0.0%	48.3%	24.4%	35.0%	31.7%	16.7%	28.7%
No Violence	72.0	73.2	77.1	72.9	100.0	51.3	75.6	65.0	68.3	83.3	71.3
N	25	41	35	48	8	29	45	40	41	12	324
Logit Estimates:					[dropped]				(reference)	[dropped]	
coef.	-.18	-.24	-.45	-.22		.70	-.36	.15	-.76		
t	-0.32	-0.49	-0.86	-0.48		1.39	-0.75	0.31	-2.29		
p(t)	.75	.63	.39	.63		.16	.46	.75	.02		

consistency between policy tracks and court settlements. We conclude with analyses of the prevalence of followup violence as effected by court settlements within policy tracks.

What differences we found among prosecution tracks are not reflected in court outcomes, as can be seen in Table V.10. The lowest rate of followup violence is associated with an adjudicated finding of Not Guilty. The remaining outcomes matching prosecution policy are non-significant in comparison to the Summons/Other (reference) track. The finding of no difference across court settlements is generally consistent with the finding of no difference across prosecution tracks for cases prosecuted under a No-Drop policy. Victims who actually drop charges, however, are no more secure than others, despite the protective consequence of the Warrant/Drop Permitted track. Indeed, the highest percentage of women reporting new violence is for the group whose cases were dismissed after their abusers came to court in response to a summons.

Prosecutorial tracking of VC cases results in expected outcomes in 71% of the cases, even though just 57% of cases where victims were permitted to drop resulted in dismissals.

Table V.11: Distribution of Court Settlements by Prosecution Track for VC Cases (Excludes Open Cases and Findings of Not Guilty)

Court Settlement	Prosecution Track: Warrant – Drop Permitted	Warrant – Diversion to Couns.	Warrant – Probation w/ Couns.	Warrant – Other Sentence	Summons – Drop Permitted	Summons – Diversion to Couns.	Summons – Probation w/ Couns.	Summons – Other Sentence	Total
Case Dismissed	55.6%	7.4%	20.4%	5.8%	57.7%	1.9%	14.3%	18.2%	22.7%
Diversion-Coun.	16.7	74.1	1.9	0.0	13.5	83.0	4.1	3.6	24.8
Probation-Coun.	16.7	7.4	66.7	9.6	7.7	9.4	77.6	7.3	24.8
Other Sentence	11.1	11.1	11.1	84.6	21.2	5.7	4.1	70.9	27.7
N	54	54	54	52	52	53	49	55	423

Table V.11 displays the court outcomes under each prosecution track.[7] The distribution is unremarkable in terms of discrepancies between policy and settlement for No-Drop cases.

Table V.12: The Prevalence of Followup Violence Against Victims Who Were Permitted to Drop Charges, by Entry and Whether or Not Charges Were Dropped

Type of Report	Entry: Warrant – Charges Pursued	Warrant – Charges Dropped	Summons – Charges Pursued	Summons – Charges Dropped	Total
Violence	0.0%	27.8%	13.0%	52.4%	23.2%
No Violence	100.0	72.2	87.0	47.6	76.8
N	20	18	23	21	82

[7] See Table III.5 for complete frequencies.

The largest of those discrepancies is due to cases being dropped contrary to the No-Drop policy (Warrant/Probation and Summons/Other).

Our final set of analyses examines the combined effects of policy and court settlements on followup violence, controlling for possible influences of factors described for the OSA analysis. The only combination of policies and outcomes affecting new violence is permitting victims to drop and whether or not they did so. Specifically, regardless of whether a case enters by Warrant or by Summons, victims who were permitted to drop but did not were significantly less likely to be battered again than those who dropped charges, as shown in Table V.12.[8] The women in greatest jeopardy are those who were permitted to drop and did so after their abusers were summoned to court. These findings are unaffected by any of the candidate control variables.

Finally, when Drop-Permitted victims elected to pursue their charges, they were assigned to one of the three regular prosecution tracks. We find that it does not matter which track was followed. Of 20 Warrant/Drop-Permitted cases where victims followed through, none were battered again, as we saw in Table V.12. Of the 23 Summons/Drop-Permitted cases where victims followed through, 3 were battered again, one under each of the alternative tracks. The small number of cases precludes anything but the most tentative generalization, but it would appear that it does not matter which policy track is pursued after deciding to pursue charges.

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[8] The chi-square value for Table V.12, as constructed, is 17.64, indicating significance of differences at the .001 level. However, cells of the first row with expected values below 5 (4.9, 4.6, and 4.2) indicate that the chi-square distribution is inappropriate for these data. Combining the Entry conditions to create a 2x2 table for violence by whether or not charges are dropped results in a chi-square value of 11.46, also significant at the .001 level.

VI. IMPLICATIONS FOR THEORY AND POLICY

The Indianapolis Domestic Violence Prosecution Experiments offer evidence that prosecuting wife batterers helps to prevent repetitive violence. But none of the common policies meant to either punish or rehabilitate offenders is uniquely effective in stopping abuse. The only policy with preventive impacts significantly more effective than traditional processing is to permit victims to drop charges after their abusers have been arrested on a victim-initiated warrant -- a policy considered anathema to the administrative concern over victims dropping charges in criminal cases.

To the extent that punishment deters crime, one may infer from quasi-experimental findings that experience in an intrinsically punishing prosecution process has a specific deterrent impact. Defendants in Indianapolis are less likely to batter the same victim six months after prosecution than they were six months prior to prosecution. However, the prosecution process begins with either an On-Scene Arrest or a Victim-Complaint to the prosecutor, and we cannot say that the prosecution process has an impact over and above the entry experience.

The IDVPE provides no support for arguments on deterrence versus rehabilitation. The formal punishments ordered upon conviction fail to prevent continuing violence to a degree greater than rehabilitative outcomes. One might argue that sentencing for misdemeanor battery under Indiana law is not sufficiently punishing to expect an effect. Nor is it typically swift or certain. Most offenders are not punished until many months after the incident and then they spend little, if any, time in jail. Of course, other sanctions such as fines, community service, and supervised probation may be perceived by the offender as proportionately severe relative to the offense. But no sanction is more effective than another in deterring the average criminal wife batterer.

Court-mandated rehabilitation for domestic violence attempts to change a man's attitudes and skills in the course of eliminating violence from his repertoire of responses to conflict with his conjugal partner. The IDVPE does not provide for followup assessment at a

time insuring completion of treatment. A six-month followup period only insures that those entering rehabilitation would have at least begun the process. We assume that effective treatment should reduce the chance of violence during treatment if it is to have a preventive impact later. We may be wrong, in which case one cannot say anything with certainty about the impacts of rehabilitative treatment for batterers under court-mandated programs in Indianapolis. We can say, however, that men ordered into treatment for domestic violence are no more likely to stop battering than those sentenced to Other conditions. It makes no difference whether rehabilitation is ordered upon conviction or agreed upon by the defendant as part of a pretrial diversion plan. Still, under any outcome, the chance of renewed violence is less than expected given a history of violence prior to prosecution.

Victim empowerment to prevent battering is an alternative perspective on criminal justice impacts introduced in this experiment as a policy permitting victims to drop charges. Available only in cases where a victim files charges with the prosecutor, the Drop-Permitted policy is significantly more effective following a defendant's arrest on a warrant, than traditional processing. We believe that under a Drop-Permitted policy, women are empowered to take control of events in their abusive relationships. Some are empowered through prosecution such that they can use the possibility of abandoning prosecution as a power resource in bargaining for their security (Ford, 1991b). Others are empowered by the alliance they form with more powerful others, such as police, prosecutors, and judges. As long as the alliance is steadfast, a victim can threaten to invoke her allies' power to deter her abuser. The Warrant arrest signifies their commitment to her grievance.

Victim-complainants who are permitted to drop charges are best off when they continue to pursue the process on one of the regular prosecution tracks (it does not matter which). Bargaining with and then dropping charges appears to be an unwise strategy for securing protection. By following through, a victim can demonstrate her commitment to changing the relationship in alliance with agents of criminal justice. She may then seek to control the course of prosecution and sanctioning.

We have no reservations in advocating a Drop-Permitted policy for Indianapolis, consistent with the IDVPE protocol. We are satisfied that arguments supporting No-Drop policies on the grounds that victims will be subject to more violence are clearly contradicted by IDVPE findings. A battered woman who initiates a complaint with the prosecutor should be told that she may drop charges, after the defendant has been arraigned, if she is certain that doing so is in her best interest. At the same time, she should be strongly encouraged to follow through with the charges to minimize her risk of new victimization. Women who bargain for security with criminal charges and then drop them are more likely to end up battered again than if they proceed to adjudication. It is tempting for some officials to coerce victim cooperation, but the lesson of No-Drop policies suggests that any such disempowering policy could further jeopardize battered women. Instead, prosecutors can offer incentives to keep victims in the system while reinforcing their power. For example, victims can be given the opportunity to determine the prosecution track for court outcomes. Each woman can be shown respect for making choices that only she is competent to make after receiving good information and reasonable options.

We cannot say that a Drop-Permitted policy will be as effective in preventing violence elsewhere as it is in Indianapolis. There are great differences among criminal justice systems in their response to battered women. For one, some jurisdictions do not even accept victim complaints independent of police actions. Others treat citizen-initiated criminal complaints as worthy only if accompanied by a filing fee that is unnecessary for police arrests. Finally, even where citizen complaints are accepted, there is no assurance that the prosecutor's office will follow up with concern and rigorous prosecution. While No-Drop policies have been championed by advocates to protect battered women, they have been embraced by prosecutors to reduce case attrition and associated administrative concerns. Permitting victims to drop charges may not be accompanied by support and encouragement, and it could result in hostility counterproductive victim interests.

If we are correct in our interpretation of the success of a Drop-Permitted policy as prevention through empowerment, there is a general lesson to be heeded in processing cases

of domestic violence: Prosecutorial intervention is an important contributor to the prevention of violence committed by men identified as batterers. But an even greater impact can be realized when victims are empowered to control the course of prosecution, including control over the decision on whether or not to proceed. Victims need the backing of a powerful and steadfast ally to support their decisions. The criminal justice system stands as such an ally when it acts on policies signifying that domestic violence is a serious crime and that victim protection is a serious concern. Prosecutors can help a victim in securing arrangements to minimize the chance of violence by affirming the legitimacy of her criminal complaints and by respecting her decisions on what is best for her unique circumstances, even if contrary to the prosecutor's administrative interests.

Consistent with the Domestic Violence Program in place during the IDVPE, prosecutors can further support and hopefully protect victims by attending to victim interests throughout the prosecution process. They can monitor warrants to see that they are served in a timely manner. They can request orders of protection and see that they are aggressively enforced. They can watch for evidence of obstruction of justice in defense attorneys' contacts with victims (see Ford and Regoli, 1990). And they can make every effort to account for a victim's safety, especially should she fail to appear for scheduled court proceedings. They should not simply let cases slip out of the system without assuring that victims are not in danger. An advantage of permitting victims to drop charges is that they can freely express their concerns without avoiding court. Prosecutors can also facilitate the coordination of services for battered women by providing information on shelters and other agencies offering assistance.

Any policy reform based on the Indianapolis findings should recognize that the Drop-Permitted option was available only to victims who filed charges in person at the prosecutor's office. None of the IDVPE victims called as witnesses following on-scene arrests of their batterers was allowed to drop charges. We have no evidence that some form of Drop-Permitted policy would be effective in cases initiated by on-scene police arrests. Indeed, such a policy may very well be counterproductive in protecting victims as it could

reinforce the prejudices of police officers already predisposed to inaction. The general lesson of victim empowerment might still apply if OSA victims are made to feel secure in alliance with the criminal justice system, and if they are given a voice in determining sanctions for their abusers. Only future research will tell whether or not this strategy offers any greater protection.

The criminal prosecution of wife batterers entails the interaction of police, victims, defendants and their attorneys, prosecutors, judges, and other personnel. Each player in the system carries unique personal motives and institutional perspectives. "Successful" prosecution may be defined differently by each. Policy reforms should acknowledge that institutional definitions of justice may conflict with a particular woman's notions of justice. Policies must be evaluated to insure that prosecutorial action does not place a victim in greater jeopardy than doing nothing at all. And while trying to address the problem of wife battery on a more global scale, the prosecutor's interest in redressing a crime against the state must not displace the goal of stopping violence against individual battered women.

The Indianapolis Domestic Violence Prosecution Experiment provides evidence that prosecution can prevent violence. It also suggests that empowering the victim by allowing her to make choices in the prosecution process (i.e. whether to drop charges) can increase her security. Policies which exclude the victim from the process must be carefully examined to see whose needs are truly being served -- the "system's" or the victim's. The focus should be on providing victims the information and protection needed so they can make truly free choices. Policy should be directed toward encouraging an alliance between police, prosecutors, and victims with a common goal of breaking the cycle of domestic violence.

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