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STUDIES

FEMALE SPOUSE ABUSE AND THE POLICE RESPONSE: THE CHARLOTTE, NORTH CAROLINA EXPERIMENT†

J. DAVID HIRSCHEL* AND IRA W. HUTCHISON, III**

I. INTRODUCTION

Efforts to stem the tide of spouse abuse have recently focused on the role of law enforcement. Pivotal hopes have been placed on the possibility that the arrest of abusers might constitute a more effective deterrent than traditional police responses. Awakened to the scope of this problem in the mid-1970s, both the public and social scientists began foraging for ways to combat a problem of epidemic proportions. Fueled by both research evidence and first hand accounts of abuse, policy makers began addressing the issues in-

† The authors would like to acknowledge Joel Garner, Craig Uchida, Kinley Larntz, Robert Boruch, Albert Reiss Jr., Allen Andrews, and Lucy Friedman for their advice and assistance in the design and evaluation of the Charlotte experiment. We would also like to thank Chief Killman, Major Kelley and the members of the Charlotte Police Department without whose assistance the project would never have been implemented.

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volved. By the 1980s pressure had increased on police departments to "do something" about the problem. What was once a private family matter became a "criminal" issue, bringing the police to the forefront of the problem.

This article reports the results of one empirical study, an experiment conducted in Charlotte, North Carolina, to assess the relative efficacy of three police responses to female spouse abuse. The responses included: (a) advising and sometimes separating the couple; (b) issuing a citation for the offender to appear in court; and (c) arresting the offender at the scene of the incident. A brief historical overview illustrates the traditional lack of intervention in domestic disputes. This is followed by an examination of public utilization of police services in such incidents, and a review of changes in the role of law enforcement. We provide baseline data on the universe of calls for police service; however, the primary focus is on the methodology and results of the Charlotte experiment. Specifically, we delineate the relative failure rates of each of the three police responses in deterring subsequent abuse and compare recidivism as measured by rearrest of the offender with victim reports of recidivism. Finally, we discuss possible reasons why arrest did not more effectively deter subsequent spouse abuse.

A. ISSUES OF DEFINITION

Although commonly understood as involving marriage, the concept of spouse utilized in the empirical research includes spouse-like relationships. The more common definition of spouse in research literature consists of conjugal-like relationships including married, separated, and cohabiting couples.¹ These are important inclusions to note since a large proportion of abusive incidents reported to police involve cohabiting couples, an issue discussed in more detail later.

A second definitional issue is that of abuse as measured by aggressive actions versus the *outcome* of such acts, i.e., injuries.² Ac-

¹ See, e.g. Sarah F. Berk & Donileen R. Loseke, "Handling" Family Violence: Situational Determinants of Police Arrest in Domestic Disturbances, 15 LAW & SOC'Y REV. 317 (1981); EVA S. BUZAWA & CARL G. BUZAWA, DOMESTIC VIOLENCE: THE CRIMINAL JUSTICE RESPONSE (1990); David A. Ford & Mary J. Regoli, *The Preventive Impacts of Policies for Prosecuting Wife Batterers* in EVA S. BUZAWA & CARL G. BUZAWA, ESSAYS ON THE CRIMINAL JUSTICE RESPONSE TO DOMESTIC VIOLENCE; GAIL A. GOOLKASIAN, U.S. DEPT' OF JUSTICE, CONFRONTING DOMESTIC VIOLENCE: THE ROLE OF CRIMINAL COURT JUDGES 1-2 (1986); Kirk R. Williams & Richard Hawkins, *The Meaning Of Arrest for Wife Assault*, 27 CRIMINOLOGY 163, 163 n.1 (1989).

² Murray A. Straus, *Conceptualization and Measurement of Physical Abuse of Spouses: Impli-*

ording to Berk et al.,³ there is little consensus on what the term "abuse" means. Feld and Straus, for example, define violence as "an act carried out with the intention or perceived intention of causing physical pain or injury to another person."⁴ Straus,⁵ Schulman,⁶ and Straus et al.⁷ also used acts as a definition of abuse. On the other hand, Berk et al. stressed the severity of injuries as the focus of their study.⁸

For purposes of this article, spouse abuse includes married, separated, divorced, cohabiting and ex-cohabiting couples and we will focus primarily on female victims of aggressive actions or outcomes.

B. HISTORICAL BACKGROUND

Roman law, which has served as a basis for many legal systems in the western world, originally gave a husband sovereign authority over his wife. At marriage, she acquired the legal status of daughter. This authority, known as *patria potestas*, included the power of life and death and unrestrained physical chastisement of the wife and other family members.⁹

A modified form of *patria potestas* was incorporated into English common law¹⁰ under the guise of family protection.¹¹ While the male's authority did not include the power of life and death, physi-

cations for Public Policy in REDEFINING CRIME: RESPONSES TO SPOUSE ABUSE (Michael Steinman ed. 1990).

³ Richard A. Berk et al., *Mutual Combat and Other Family Violence Myths in THE DARK SIDE OF FAMILIES: CURRENT FAMILY VIOLENCE RESEARCH 197, 198* (David Finkelhor et al., eds. 1983).

⁴ Scott L. Feld & Murray A. Straus, *Escalation and Desistance of Wife Assault in Marriage*, 27 *CRIMINOLOGY* 141, 143 (1989).

⁵ Murray A. Straus, *Measuring Intrafamily Conflict and Violence: The Conflict Tactics (CT) Scales*, J. MARRIAGE FAM. 75 (Feb. 1979).

⁶ MARK A. SCHULMAN, U.S. DEP'T OF JUSTICE, *A SURVEY OF SPOUSAL VIOLENCE AGAINST WOMEN IN KENTUCKY* 6 (1979).

⁷ MURRAY STRAUS ET AL., *BEHIND CLOSED DOORS: VIOLENCE IN THE AMERICAN FAMILY* 20 (1980).

⁸ Berk et al., *supra* note 3, at 199.

⁹ EUGENE A. HECKER, *A SHORT HISTORY OF WOMEN'S RIGHTS* 13 (1914); Nan Oppenlander, *The Evolution of Law and Wife Abuse*, 3 *LAW & POLICE Q.* 382, 386 (1981); ELIZABETH PLECK, *DOMESTIC TYRANNY: THE MAKING OF SOCIAL POLICY AGAINST FAMILY VIOLENCE FROM COLONIAL TIMES TO THE PRESENT* 9 (1987).

¹⁰ Oppenlander, *supra* note 9, at 386.

¹¹ Denise J. Gamache et al., *Coordinated Police, Judicial, and Social Service Response to Woman Battering: A Multiple Baseline Evaluation Across Three Communities in COPING WITH FAMILY VIOLENCE* 193, 194 (Gerald T. Hotaling et al., eds. 1988); ROBERT T. SIGLER, *DOMESTIC VIOLENCE IN CONTEXT: AN ASSESSMENT OF COMMUNITY ATTITUDES* 2 (1989); GILLIAN A. WALKER, *FAMILY VIOLENCE AND THE WOMEN'S MOVEMENT: THE CONCEPTUAL POLITICS OF STRUGGLE* 48 (1990).

cal chastisement was both accepted and expected.¹² Blackstone, in 1768, described the husband's right to chastise his wife moderately in order to enforce obedience.¹³ The criterion for "moderate" was the "rule of thumb," which allowed a husband to use any reasonable instrument including a rod no thicker than his thumb to correct his "wayward" wife.¹⁴

The English heritage of the Puritans guaranteed similar attitudes in the United States. Indifference to or outright approval of the right to chastise was maintained until the late 1830s.¹⁵ However, the womens' rights movement gained momentum during the 19th century. A focus on law and order and a humanitarian concern for the victims of spouse abuse led to attempts to enact legislation that dealt with the problem of abuse.¹⁶ Between 1876 and 1906, bills were introduced in twelve states, albeit passed in only three, providing for the punishment of wife beaters with a whipping.¹⁷ Passing a statute and enforcing it are not, however, the same. After Maryland enacted a law in 1882 to punish wife beaters with either a whipping or a year in jail, no one was prosecuted under the statute for a year. In three years there were only two convictions. Although unused, the provision remained on the books until 1948.¹⁸ In the 1874 North Carolina case of *State v. Oliver*,¹⁹ a lower court convicted a husband who had whipped his wife with two thin switches leaving bruises. The North Carolina Supreme Court affirmed this judgment stating that the old "rule of thumb" was no longer the law in North Carolina. Despite the rejection of the right to chastise in the legal culture, the belief that spouse abuse was to be kept within the privacy of the home continued to be very much a part of broader American culture.

C. CURRENT ATTITUDES

An ambivalence toward spouse abuse still persists today. A number of reasons are advanced for this: privacy of the home, the

¹² R. Emerson Dobash & Russell P. Dobash, *Wives: The 'Appropriate' Victims of Marital Violence*, 3-4 VICTIMOLOGY 426, 429-30 (1977-78); LORNA J. F. SMITH, HOME OFFICE RESEARCH STUDY NO. 107: HER MAJESTY'S STATIONERY OFFICE, DOMESTIC VIOLENCE, 3 (1989).

¹³ WILLIAM BLACKSTONE, COMMENTARIES 432 (1765).

¹⁴ Oppenlander, *supra* note 9.

¹⁵ Elizabeth Pleck, Criminal Approaches to Family Violence 1640-1980 in CRIME AND JUSTICE: A REVIEW OF RESEARCH, 11, 19 (Lloyd E. Ohlin & Michael Tonry eds. 1989).

¹⁶ *Id.* at 35.

¹⁷ *Id.* at 40.

¹⁸ *Id.* at 41.

¹⁹ *State v. Oliver*, 70 N.C. 44 (1874).

social approval of violence, the inequality of women in society, and the lack of intervention by criminal justice officials.²⁰

Spouse abuse has been viewed differently from "regular" assault between strangers. It has been regarded as a less serious issue on the grounds that the wife belongs to the husband, that the home is his castle, and that what happens in the castle is not a concern for neighbors or the criminal justice system.²¹ Although the state can punish violators, it continues to give family members special immunity to protect family life and the marriage.²² Recently, Senator Jesse Helms argued against federal funding of domestic violence shelters because they constituted "social engineering," removing the husband as the "head of the family."²³ Moreover, cultural beliefs in the sanctity of family privacy have prevented societal, legal or personal intervention.²⁴

According to some, men are given implicit social permission to beat their wives when nothing is done to stop abuse.²⁵ Difficulties abound in developing a comprehensive response to spouse abuse because problems exist at all stages of the criminal justice system. Any effective response requires convincing the police to arrest, the prosecutors to prosecute vigorously, and the court system to sanction.²⁶ This reluctance to punish batterers²⁷ gives the impression that there is only a small threat of sanctions for the abuser.²⁸ It is

²⁰ RICHARD J. GELLES & MURRAY A. STRAUS, *INTIMATE VIOLENCE* 25 (1988); Oppenlander, *supra* note 9, at 385; Pleck, *supra* note 15, at 20-21.

²¹ Joanne Belknap, *Police Training in Domestic Violence: Perceptions of Training and Knowledge of the Law*, XIV (2) AM. CRIM. JUST. SOC'Y 248, 248 (1990); Paper by Russel P. Dobash & R. Emerson Dobash, *Gender, Methodology, and Methods in Criminological Research: The Case of Spousal Violence*, BRITISH CRIMINOLOGY CONFERENCE (July, 1991); Paper by James J. Fyfe & Jeanne Flavin, *Differential Police Processing of Domestic Assault Complaints*, ACADEMY OF CRIMINAL JUSTICE SCIENCES (1991); Pleck, *supra* note 15, at 21.

²² Oppenlander, *supra* note 9, at 385; Pleck, *supra* note 15, at 20.

²³ 126 CONG. REC. 24, 12058 (1980).

²⁴ Beverly Balos & Katie Trotzky, *Enforcement of the Domestic Abuse Act in Minnesota: A Preliminary Study*, 6 LAW & INEQUALITY 38, 83 (1988); Berk & Loseke, *supra* note 1, at 319; Michael A. Buda & Teresa L. Butler, *The Battered Wife Syndrome: A Backdoor Assault on Domestic Violence*, 23 J. FAM. L. 359, 366 (1984-85); GELLES & STRAUS, *supra* note 20, at 27; Maria Roy, *Some Thoughts Regarding the Criminal Justice System and Wife Beating in BATTERED WOMEN: A PSYCHOSOCIOLOGICAL STUDY OF DOMESTIC VIOLENCE* 138 (Matia Roy ed. 1977); SIGLER, *supra* note 11, at 2; Kathleen Waits, *The Criminal Justice System's Response to Battering: Understanding The Problem, Forging the Solutions*, 60 WASH. L. REV. 267, 299 (1985).

²⁵ Eva S. Buzawa & Carl G. Buzawa, *Legislative Trends in the Criminal Justice Response to Domestic Violence in CRIME AND THE FAMILY* 143 (Alan J. Lincoln & Murray Straus eds. 1985); Roy, *supra* note 24, at 138; WALKER, *supra* note 11, at 48.

²⁶ Balos & Trotzky, *supra* note 24, at 106; GOOLKASIAN, *supra* note 1, at 3.

²⁷ Waits, *supra* note 24, at 271.

²⁸ GELLES & STRAUS, *supra* note 20, at 24.

little surprise that many incidents are never reported to the police.²⁹

D. ABUSIVE INCIDENTS REPORTED TO THE POLICE

Estimates of the proportion of spouse assaults actually reported to the police range from one-tenth to two-thirds of all abused women. This divergence is due, in part, to varying methods of measurement.³⁰ In a survey of 1,793 Kentucky women, Schulman found that women called police in only nine percent of the incidents.³¹ Compared to data from other studies, this represents an extremely high proportion of unreported incidents. The generally accepted estimate (based in particular on national samples) is that approximately one-half of all incidents are reported to the police. Analysis of 1973-1976 National Crime Panel Survey data revealed that approximately fifty-five of every 100 incidents of intimate violence went unreported to law enforcement.³² Analysis of 1978 to 1982 data showed that forty-eight percent of the incidents were not reported to the police.³³ In a study of 420 women who sought treatment in a domestic abuse program in Washington state, Kuhl discovered that sixty-six percent of the women had not filed a report.³⁴

Citizens who call the police to report a criminal action are not a representative sample of all those who have been victimized by that crime. There is an inherent self-selection process in police services utilization. No one concludes that abused women who call the police are an accurate demographic representation of all those who experience abuse. Although domestic disturbances cut across all demographic boundaries, police are involved most often in domestic disturbances among the poor and uneducated.³⁵ Bowker reports that the police are more likely to come into contact with couples of relatively low socioeconomic status who experience low-quality mar-

²⁹ U.S. DEP'T OF JUSTICE, *A STUDY OF VIOLENCE AMONG FRIENDS AND RELATIVES* (1980).

³⁰ Lee H. Bowker, *Battered Wives and The Police: A National Study of Usage and Effectiveness*, 7 *POLICE STUDIES* 84 (1984).

³¹ SCHULMAN, *supra* note 6.

³² U.S. DEP'T OF JUSTICE, *supra* note 29.

³³ PATRICK A. LANGAN & CHRISTOPHER A. INNES, U.S. DEP'T OF JUSTICE, *PREVENTING DOMESTIC VIOLENCE AGAINST WOMEN* 3 (1986).

³⁴ Anna F. Kuhl, *Community Responses to Battered Women*, 7 *VICTIMOLOGY* 49, 51 (1982).

³⁵ L. Kevin Hamberger & James Hastings, *Characteristics of Male Spouse Abusers Consistent with Personality Disorders*, 39 *HOSPITAL COMMUNITY PSYCHIATRY* 763 (1988); ANNE S. HELTON, *PROTOCOL OF CARE FOR THE BATTERED WOMEN* (1986); DONNA M. MOORE, *BATTERED WOMEN* (1979); Raymond I. Parnas, *The Police Response to the Domestic Disturbance*, 1967 *WIS. L. REV.* 914, 915 (1967).

ital relationships and severe violence.³⁶ Nonwhite and lower income women (under \$7,500) are more than twice as likely to report an incident to the police than are white, higher income (over \$15,000) females.³⁷ Under-reporting occurs at all socioeconomic status levels, but it is particularly likely at middle and higher income levels.

There is no single reason which adequately explains massive under-reporting. Perhaps because spouse abuse is so common in society, an unknown number of victims do not consider their assaults to be crimes. If they do, they do not report the incidents or disclose to any official how their injuries were sustained.³⁸ As Langley and Levy note, these are the missing persons of official statistics.³⁹ Some women will not perceive a slap in the face as abuse, so there is no report; others will perceive such an action as abusive, but only a fraction will report it. The more severe the behavior the more likely it is to be both perceived as abusive and reported. Even very severe abuse, however, is often never reported. Langan and Innes found that the primary reason women offered for not reporting an abusive incident to the police was that it was considered a private or personal matter (forty-nine percent).⁴⁰ A further twelve percent of the victims did not report because they feared reprisal. Similar proportions of victims failed to report because they thought the crime was not important enough, or because they believed the police could not, or would not, do anything.⁴¹

E. COHABITANT ABUSE

Any discussion of spouse abuse must also include some attention to cohabitants. The number of cohabitants has increased almost fourfold in the past twenty years, and those cohabiting are primarily young; two-thirds of males and three-fourths of females are less than thirty-five years old.⁴² There is increasing evidence that cohabitants are particularly prone to abusive relationships. In their comparison of married, dating and cohabitating couples, Stets

³⁶ Lee H. Bowker, *Police Services to Battered Women: Bad or Not so Bad*, 9 CRIM. JUST. BEHAVIOR 476, 482 (1982).

³⁷ SCHULMAN, *supra* note 6, at 2-3.

³⁸ Warren J. Breslin, *Police Intervention in Domestic Confrontations*, 6 J. POLICE SCI. & ADMIN. 293, 293 (1978).

³⁹ Roger Langley & Richard C. Levy, *Wife Abuse and The Police Response*, 47 FBI LAW ENFORCEMENT BULL., May 1978, at 4, 5 (1978).

⁴⁰ LANGAN & INNES, *supra* note 33.

⁴¹ *Id.*

⁴² Graham B. Spanier, *Married and Unmarried Cohabitation in the United States*, 45 J. MARRIAGE FAM. 277 (1983).

and Straus report that the highest rates of assault and the most severe assaults are found among cohabiting couples.⁴³ Hutchison et al. found that the number of calls by cohabitants to police for abusive incidents equalled the number of similar calls by married couples despite the far greater size of the latter group within the population.⁴⁴ Stets concluded that cohabitants are more prone toward aggression than married couples due to a combination of factors: youth, minority status, problems including depression and alcohol usage, and the lack of "social control associated with participation in organizations" ⁴⁵

II. THE LAW ENFORCEMENT ROLE

A. TRADITIONAL POLICE RESPONSES

In the past, domestic violence calls were often assigned low priority.⁴⁶ Police reluctantly responded to abuse calls, attempted to restore peace and order between the disputants, and typically left without taking more formal action.⁴⁷

Many explanations exist for the long-term avoidance of formal action. First, violence within the family had been considered to be essentially a private matter. This allowed adults to use force to solve personal disputes.⁴⁸ Second, female victims had been perceived as uncooperative, thus making the arrest and prosecution of abusers a waste of time.⁴⁹ Third, taking action against abusers was considered potentially injurious to their families, especially to those members financially dependent on the offenders.⁵⁰ Fourth, intervening in family disputes was not regarded as "real police work."⁵¹ Finally, Martin and others argued that responding officers, who

⁴³ Jan E. Stets & Murray A. Straus, *The Marriage License as a Hitting License: Comparison of Assaults in Dating, Cohabiting, and Married Couples*, 4 J. FAM. VIOLENCE 161-176 (1989).

⁴⁴ Paper by Ira Hutchison III et al., *Domestic Variations in Domestic Violence Calls to Police*, SOUTHERN SOCIOLOGICAL ASSOCIATION, Nashville Tennessee (1988).

⁴⁵ Jan E. Stets, *Cohabiting and Marital Aggression: The Role of Social Isolation*, 53 J. MARRIAGE & FAMILY 669, 678 (1991).

⁴⁶ JENNIFER B. FLEMING, STOPPING WIFE ABUSE: A GUIDE TO THE PSYCHOLOGICAL AND LEGAL IMPLICATIONS FOR THE ABUSED WOMAN AND THOSE HELPING HER (1979); Raymond Parnas, *Police Discretion and Diversion of Incidents of Intra-Family Violence*, 36 LAW & CONTEMP. PROBS. 539 (1971); U.S. COMMISSION ON CIVIL RIGHTS, UNDER THE RULE OF THUMB: BATTERED WOMEN AND THE ADMINISTRATION OF JUSTICE 12 (1982).

⁴⁷ U.S. COMMISSION ON CIVIL RIGHTS, *supra* note 46, at 12.

⁴⁸ Breslin, *supra* note 38; DEL MARTIN, BATTERED WIVES (1976).

⁴⁹ Parnas, *supra* note 35, at 931; U.S. COMMISSION ON CIVIL RIGHTS, *supra* note 46.

⁵⁰ Parnas, *supra* note 35, at 931; U.S. DEPARTMENT OF JUSTICE, ATTORNEY GENERAL'S TASK FORCE ON FAMILY VIOLENCE: FINAL REPORT (1984).

⁵¹ BUZAWA & BUZAWA, *supra* note 1, at 29; Fyfe & Flavin, *supra* note 21, at 4; Parnas, *supra* note 35, at 542.

were usually male, typically sided with offenders.⁵² This taking of sides reinforced a cultural norm which stressed male superiority.

There was little change in the police response to spouse abuse until the 1960s. Under the influence of social scientists, psychologists, and a developing women's movement, the order maintenance approach underwent changes. Mediation and crisis intervention were promoted as the appropriate tools for dealing with family violence. This led to police training in crisis intervention techniques,⁵³ the establishment of police family crisis intervention units,⁵⁴ and the formation of crisis teams composed of police officers and social workers.⁵⁵

Despite the added training and the use of specialized units, there is little evidence that crisis intervention and mediation considerably reduced abuse. Oppenlander, for one, reported that police tended to make more arrests in abusive situations than in other cases, even though crisis intervention approaches often took precedence over arrest.⁵⁶ Relatively few evaluations⁵⁷ however, assessed the impacts of these changes, most of which occurred before controlled experimental research was conducted.

Concerns about "crisis intervention and mediation," coupled with arguments that the rights of female victims were violated by the failure of police enforcement, produced demands from women's groups for the arrest of abusers.⁵⁸ In some jurisdictions, women's groups filed suits to effect this change in policy.⁵⁹ The rationale for advocating arrest was clear. As the Attorney General's Task Force

⁵² See MARTIN, *supra* note 48.

⁵³ MORTON BARD, TRAINING POLICE AS SPECIALISTS IN FAMILY CRISIS INTERVENTION (1970) [hereinafter BARD, TRAINING POLICE AS SPECIALISTS]; Morton Bard, *Family Crisis Intervention: From Concept to Implementation* (1973); Morton Bard, *Role of Law Enforcement in the Helping System in* ALAN R. COFFEY & VERNON E. RENNER, *CRIMINAL JUSTICE AS A SYSTEM: READINGS* (1975) [hereinafter Bard, *Role of Law Enforcement*]; Joseph H. Spitzner & Donald H. McGee, *Family Crisis-Intervention Training, Diversion, and the Prevention of Crimes of Violence*, POLICE CHIEF, Oct. 1975, at 252.

⁵⁴ BARD, TRAINING POLICE AS SPECIALISTS, *supra* note 53; Bard, *Role of Law Enforcement*, *supra* note 53.

⁵⁵ Bruce B. Burnett et al., *Police and Social Workers in a Community Outreach Program*, SOC. CASEWORK 41, 41 (1976).

⁵⁶ Nan Oppenlander, *Coping or Copping Out: Police Service Delivery in Domestic Disputes*, 20 CRIMINOLOGY 449, 462 (1982).

⁵⁷ See, e.g., Jack B. Pearce & John R. Snortum, *Police Effectiveness in Handling Disturbance Calls: An Evaluation of Crisis Intervention Training*, 10 CRIM. JUST. & BEHAV. 71 (1983); P. B. Wylie et al., *An Approach to Evaluating a Police Program of Family Crisis Intervention in Six Demonstration Cities: Final Report* (1976).

⁵⁸ Langley & Levy, *supra* note 39; U.S. COMMISSION ON CIVIL RIGHTS, *supra* note 46, at 12.

⁵⁹ See, e.g., *Bruno v. Codd*, 396 N.Y.S. 2d 974 (1977); *Scott v. Hart*, No. 6-76-2395 (N.D. Cal.: 1976); *Thurman v. City of Torrington*, 595 F.Supp. 1521 (D. Conn. 1984).

on Family Violence unequivocally stated: "*The legal response to family violence must be guided primarily by the nature of the abusive act, not the relationship between the victim and the abuser.*"⁶⁰

The 1980s saw the beginnings of a movement toward arrest of abusers as a more common occurrence. While still in its infancy, this historically unprecedented movement may be faltering.⁶¹

B. THE MOVEMENT TOWARD ARREST POLICIES

There are many issues involving the meaning, implementation, and effects of the preferred arrest movement. In recent years, an increasing number of police departments have established such policies. The extent to which this is attributable to changes in state statutes or through police initiative, however, is uncertain. Lerman et al. observed in 1983 that "twenty-seven of the recent state laws on domestic violence expand(ed) police power to arrest in domestic abuse cases."⁶² Ferraro noted that, as of 1986, six states had passed laws requiring arrest with a positive determination of probable cause and the presence of the offender on the scene.⁶³ By 1988, there were ten states with such laws.⁶⁴

The existence of conditional requirements often limit the potential of such statutory provisions. Some state laws require the existence of a visible injury and/or the lapse of only a short period of time between the commission of the offense and the arrival of the police.⁶⁵ Hirschel and Hutchison reported that, while all of the police departments with preferred arrest policies abided by the policies in situations where there was visible injury or a threat with a deadly weapon, only a minority of the departments abided by the policies in situations that involved only verbal threats or property damage.⁶⁶ Moreover, subject to jurisdictional variation, about half of all offenders leave the scene prior to police arrival and were not arrested unless the victims swore out arrest warrants.⁶⁷ Finally, preferred or mandatory arrest policies do not necessarily embrace all

⁶⁰ U.S. DEP'T OF JUSTICE, *supra* note 50, at 4 (emphasis in original).

⁶¹ See *infra* section III.

⁶² Lisa Lerman et al., *State Legislation on Domestic Violence in ABUSE OF WOMEN: LEGISLATION, REPORTING, AND PREVENTION* 44 (Joseph J. Costa ed. 1983).

⁶³ Kathleen J. Ferraro, *Policing Woman Battering*, 36 SOC. PROBS. 61 (1989).

⁶⁴ VICTIM SERVICES AGENCY, *THE LAW ENFORCEMENT RESPONSE TO FAMILY VIOLENCE: A STATE BY STATE GUIDE TO FAMILY VIOLENCE LEGISLATION* 3 (1988).

⁶⁵ J. David Hirschel & Ira W. Hutchison III, *Police-Preferred Arrest Policies in WIFE BATTERING: POLICE RESPONSES* 49 (Michael Steinman ed., 1991).

⁶⁶ *Id.*

⁶⁷ Paper by J. David Hirschel & Ira W. Hutchison III, *Experimental Research on Police Response to Spouse Assault: The Charlotte Project*, AM. SOC'Y CRIMINOLOGY 11 (Montreal, Canada 1987).

abuse victims. Cohabiting women call police disproportionately more than married women for domestic assault situations,⁶⁸ but some states do not include nonmarried couples in preferred arrest policies.⁶⁹

Preferred arrest (also called proarrest or presumptive) policies are far more common than mandatory policies. A 1986 study by the Crime Control Institute investigated arrest policies and found a fourfold increase over the number of departments with such policies in 1984.⁷⁰ The study did not include police departments in cities with less than 100,000 population, however, so it is not possible to determine the extent to which the larger city trend exists in less populous areas.

Available information suggests that most police departments have not played a leading role in adopting arrest policies, and that many have been reluctant to do so. For example, Arizona's legislature passed a law in 1980 that expanded police arrest powers. Only when faced with the possibility of more legislation *mandating* arrest however, did the Chief of the Phoenix Police Department adopt a presumptive arrest policy.⁷¹ Based on ride-along observational data, Ferraro reports that, despite the presumptive arrest policy, arrests were made in only eighteen percent of the battering incidents to which her research team responded.⁷² This low percentage was due, in part, to the use by police officers, in interpreting the probable cause requirement, of "a level of evidence high enough for felony arrests."⁷³ Miller in Oregon,⁷⁴ Bell in Ohio,⁷⁵ and Buzawa in New Hampshire⁷⁶ have all noted a similar reluctance on the part of police departments to conform their policies to new statutory provisions.

The success of formal policies depends upon the support of both command and line personnel. Buzawa's research in New Hampshire⁷⁷ clearly demonstrates the impact of negative attitudes

⁶⁸ Hutchison et al., *supra* note 44, at 14.

⁶⁹ Ferraro, *supra* note 63, at 61.

⁷⁰ Lawrence W. Sherman & Ellen G. Cohn, *The Impact of Research on Legal Policy: The Minneapolis Domestic Violence Experiment*, 23 LAW & SOC'Y REV. 117, 125 (1989).

⁷¹ Ferraro, *supra* note 63, at 63.

⁷² *Id.*

⁷³ *Id.* at 64.

⁷⁴ MARILYN G. MILLER, DOMESTIC VIOLENCE IN OREGON 16 (1979).

⁷⁵ Daniel J. Bell, *Domestic Violence: Victimization, Police Intervention, and Disposition*, 13 J. CRIM. JUST. 525, 532 (1985).

⁷⁶ Eve S. Buzawa, *Explaining Variations in Police Response to Domestic Violence: A Case Study in Detroit and New England in COPING WITH FAMILY VIOLENCE* 174-5 (Gerald T. Hotaling et al., eds. 1988).

⁷⁷ *Id.*

among chiefs of police regarding the use of arrest in abuse cases. She found that a lack of support by chiefs was associated with low enrollments in the voluntary state-administered training program, the absence of written departmental policies, low or non-existent arrest rates for domestic violence incidents, and a feeling among officers that responding to abuse calls was usually a waste of time.⁷⁸ In one jurisdiction, the chief even went as far as to say that "he could not recall a 'genuine' call for domestic violence in his numerous years as an administrator" and as a consequence "did not highly value the role of police intervention in this area."⁷⁹

In addition, police officers are accustomed to making their own decisions on the street and are traditionally antagonistic to policies that limit their discretion. In his survey of Minneapolis officers conducted after the Sherman and Berk experiment, Steinman found a strong indication of independence. He noted that ninety-nine percent of respondents voiced the belief that they "should make their own decisions about problems that arise on duty;" seventy-seven percent reported that they "usually do what they think necessary even if they expect supervisors to disagree," and forty-three percent declared that "they should use their own standards of police work even when department procedures prohibit them from doing so."⁸⁰

Police reluctance to respond to domestic violence calls also arises from their perceptions of the danger posed by such calls. A common perception has persisted that domestic disturbances are unusually dangerous for police in terms of the frequencies of both assaults and homicides.⁸¹ This perception has been "transmitted largely through police folklore."⁸² The perception has also been supported by the interpretation of FBI "disturbance call" data. These data grouped family quarrels with other types of disturbances, such as bar fights and "man with gun" calls. Hence, they were easily misinterpreted by some who took all of the disturbance calls to constitute domestic disturbance calls.⁸³ Moreover, it has been suggested that this perception of danger was purposefully projected by crisis intervention trainers to obtain the attention of antag-

⁷⁸ *Id.* at 175-78.

⁷⁹ *Id.* at 175.

⁸⁰ Michael Steinman, *Anticipating Rank and File Police Reactions to Arrest Policies Regarding Spouse Abuse*, 4 CRIM. JUST. RES. BULLETIN 1, 2 (1988).

⁸¹ BUZAWA & BUZAWA, *supra* note 1, at 29.

⁸² David N. Konstantin, *Homicides of American Law Enforcement Officers, 1978-1980*, 1 JUST. Q. 29, 32 (1984).

⁸³ See, e.g., Bard, *Role of Law Enforcement*, *supra* note 53, at foreword; Darrel V. Stephens, *Domestic Assault: The Police Response in BATTERED WOMEN: A PSYCHOSOCIOLOGICAL STUDY OF DOMESTIC VIOLENCE* 164 (Maria Roy ed. 1977).

onistic recruits.⁸⁴ Only a small percentage of police officers killed in the line of duty, however, are killed while responding to abuse calls.⁸⁵ An in-depth analysis by Garner and Clemmer concluded that domestic disturbances rarely result in police homicide.⁸⁶ The danger of assault and injury has likewise been exaggerated.⁸⁷ Some recent studies, however, indicate that domestic calls may in some locations constitute the most dangerous category of call the police receive, both in terms of assault⁸⁸ and injury.⁸⁹

Finally, some police officers are reluctant to make arrests in domestic violence cases because of a desire to avoid civil liability for false arrest. The likelihood of recovery in such suits, however, has been greatly exaggerated. Although officers in police departments that adopt preferred arrest policies frequently raise this issue, in reality civil suits for false arrest are infrequent and not often successful.⁹⁰ Indeed, one may argue that officers and police departments, are as likely to be sued successfully for failure to arrest an alleged offender⁹¹ as for wrongful arrest of an alleged offender. Such suits for failure to arrest have been based on allegations of denial of due process or equal protection of the law⁹² or infringement of rights granted victims by state statutes.⁹³ Despite some large awards (e.g. \$2.3 million in the *Thurman*⁹⁴ case), there have not been many successful suits and it has been suggested that recent Supreme Court case law (i.e., *DeShaney v. Winnebago County Department of Social Services*⁹⁵) will make it more difficult for abused women to win civil suits

⁸⁴ Fyfe & Flavin, *supra* note 21, at 8.

⁸⁵ See, e.g., Konstantin, *supra* note 82; Mona Margarita, *Killing the Police: Myths and Motives*, 452 ANNALS AM. ASS'N POL. SOC. SCI. 63 (1980).

⁸⁶ JOEL GARNER & ELIZABETH CLEMMER, *DANGER TO POLICE IN DOMESTIC DISTURBANCES: A NEW LOOK* (1986).

⁸⁷ See, e.g., WILLIAM A. GELLER & KEVIN J. KARALES, *SPLIT-SECOND DECISIONS: SHOOTINGS OF AND BY CHICAGO POLICE* (1981); Dissertation by Mona Margarita, *Criminal Violence Against Police*, University of New York at Albany, University Microfilms, 80-18426 (1980).

⁸⁸ See, e.g., Craig Uchida et al., *Danger to Police During Domestic Encounters: Assaults on Baltimore County Police, 1984-1986*, 2 CRIM. JUST. POLICE REPORTS 357 (1987).

⁸⁹ See, e.g., Rose M. Stanford & Bonney L. Mowry, *Domestic Disturbance Danger Rate*, 17 J. POLICE SCI. & ADMIN. 244 (1990); Uchida et al., *supra* note 88.

⁹⁰ See, e.g., ROLADO V. DEL CARMEN, *CIVIL LIABILITIES IN AMERICAN POLICING* 2 (1991).

⁹¹ See, e.g., *Nearing v. Weaver*, 670 P.2d 137, 139 (Or. 1983); *Thurman v. City of Torrington*, 595 F. Supp. 1521 (D. Conn. 1984).

⁹² See, e.g., *Thurman v. City of Torrington*, 595 F. Supp. 1521 (D. Conn. 1984); *Dudosh v. City of Allentown*, 665 F. Supp. 381, 387 (E.D. Pa. 1987); *Balistreri v. Pacifica Police Dep't*, 901 F.2d 696, 699 (9th Cir. 1990).

⁹³ See, e.g., *Nearing v. Weaver*, 670 P.2d 137 (Or. 1983); *Turner v. City of Charleston*, 675 F. Supp. 314 (D.S.C. 1987).

⁹⁴ *Thurman v. City of Torrington*, 595 F. Supp. 1521 (D. Conn. 1984).

⁹⁵ *DeShaney v. Winnebago Co. Dep't. of Social Services*, 489 U.S. 189 (1989).

in Federal Court against police departments which have failed to protect them.⁹⁶

C. THE MINNEAPOLIS EXPERIMENT AND ITS PROGENY

The results of the Minneapolis experiment fueled the movement toward arrest as the preferred policy in domestic violence cases. In that study Sherman and Berk⁹⁷ randomly assigned 314 eligible misdemeanor domestic assault cases to one of three treatment responses: advising the couple (including informal mediation in some cases); separating the couple by ordering the offender to leave for eight hours; or arresting the offender, which meant that he stayed overnight in jail. After examining official police records and conducting victim interviews every two weeks for six months, they concluded that arrest was the most effective in deterring subsequent abuse.

The researchers⁹⁸ and others⁹⁹ have pointed out problems with the study. These problems include such issues as inadequate sample size, a disproportionate number of cases submitted by a few officers, inadequate controls over the treatments actually delivered, the possibility of surveillance effects caused by multiple follow-up interviews, and a lack of generalizability of the findings due to attributes of the sample and of the city where the sample was obtained.

Despite these problems, this study contributed to the nationwide movement toward arrest policies.¹⁰⁰ Some, however, contended that police departments proceeded too far on the basis of too little evidence.¹⁰¹ Recognizing the policy implications of the preferred arrest movement, the National Institute of Justice sponsored six additional field experiments in six divergent communities: Omaha, Atlanta, Colorado Springs, Dade County, Milwaukee, and Charlotte.

The Omaha study employed a two part analysis which focused on whether the offender was present when the officers arrived on the scene. If the offender was present, the case was randomly as-

⁹⁶ Marvin Zalman, *The Courts' Response to Police Intervention in Domestic Violence in CRIMINAL JUSTICE RESPONSE TO DOMESTIC VIOLENCE* (Eva Buzawa & Carl Buzawa, eds., 1991-92 forthcoming).

⁹⁷ Lawrence W. Sherman & Richard A. Berk, *The Specific Deterrent Effects of Arrest for Domestic Assault*, 49 AM. SOC. REV. 261 (1984).

⁹⁸ *Id.* at 263-66, 269.

⁹⁹ See, e.g., Arnold Binder & James W. Meeker, *Experiments as Reforms*, 16 J. CRIM. JUST. 347 (1988); Richard Lempert, *Humility is a Virtue: On the Publication of Policy-Relevant Research*, 23 LAW & SOC'Y REV. 145, 152-54 (1989).

¹⁰⁰ Sherman & Cohn, *supra* note 70, at 129.

¹⁰¹ Binder & Meeker, *supra* note 99, at 348.

signed to one of the treatments employed in the Minneapolis study; arrest, separation, or mediation. If the offender was absent when the police arrived, the case was randomly assigned to receive or not to receive a warrant for the offender's arrest. Like the Minneapolis and Milwaukee studies, the Omaha study was not limited to heterosexual couples who were or had been married or cohabiting. It also included same-sex couples as well as victims and offenders in other familial relationships.¹⁰² Analysis of the 247 offender-absent cases indicated that the issuing of a warrant both decreased the likelihood and slowed the onset of further abuse.¹⁰³ Analysis of the 330 eligible cases in which the offender was present, however, revealed no significant differences between the failure rates of the three treatments. Results were the same whether official measures or victim-reported measures of recidivism were employed.¹⁰⁴

The Milwaukee Project employed three treatment responses: (a) full arrest, which was accompanied by a relatively long period of detention in jail (a mean of 11.1 hours); (b) short arrest which resulted in the release of the offender within a couple of hours (a mean of 2.8 hours); and (c) no arrest (warning only). Analysis of the 1,200 eligible cases revealed no significant differences in the effects of the treatments. According to interviews and one official measure (the commission of subsequent violence against *any* victim), however, short arrest had a substantial initial (thirty-day) deterrent effect relative to the warning-only group. The deterrent effect dissipated over a longer follow-up period. These data highlight the importance of both multiple measures of recidivism and an adequate follow-up period to determine treatment effects. Based on police reports to the local shelter's hotline of all probable cause domestic violence cases, the short-arrest group consistently showed a significantly higher rate of long term recidivism than did the warning-only group.¹⁰⁵ The authors considered this official measure to be the most comprehensive. Thus, the findings obtained from both the Omaha and Milwaukee experiments fail to support those of the Minneapolis study.

¹⁰² FRANKLYN W. DUNFORD ET AL., *THE OMAHA DOMESTIC POLICE EXPERIMENT: FINAL REPORT* (1989).

¹⁰³ *Id.*; Franklyn W. Dunford, *System-Initiated Warrants for Suspects of Misdemeanor Domestic Assault: A Pilot Study*, 7 JUST. Q. 631, 642-43 (1990).

¹⁰⁴ DUNFORD ET AL., *supra* note 102; Franklyn W. Dunford et al., *The Role of Arrest in Domestic Assault: The Omaha Police Experiment*, 28 CRIMINOLOGY 183 (1990).

¹⁰⁵ Lawrence W. Sherman et al., *From Initial Deterrence to Long Term Escalation: Short-Custody Arrest for Poverty Ghetto Domestic Violence*, 29 CRIMINOLOGY 821 (1991).

III. METHODS

A. RESEARCH LOCATION

Charlotte is the largest urban area between Washington, D.C. and Atlanta, Georgia. Located on the border between North and South Carolina, the city covers an estimated 160 square miles. The population in 1986 was 352,070.¹⁰⁶ The 1990 population estimates place 390,000 people within the city. The ratio of whites to blacks within the city is about 2:1 (67.4% white versus 31.1% black, based on census data).

The Charlotte Police force has over 1,000 employees.¹⁰⁷ Civilians account for approximately twenty percent of the workforce.¹⁰⁸ The sworn officers are divided into patrol, investigative, and administrative divisions, the patrol division constituting over seventy-five percent of the sworn personnel.¹⁰⁹ The majority (sixty-six percent) of the Charlotte patrol officers are white males.¹¹⁰ The remainder comprise black males (seventeen percent), white females (twelve percent), and black females (five percent).¹¹¹ More than fifty percent of the patrol officers have been on the force less than three years.¹¹²

B. RESEARCH DESIGN

The Charlotte experiment investigated the effectiveness of three police responses to spouse abuse: (a) advising and possibly separating the couple; (b) issuing a citation to the offender (an order requiring the offender to appear in court to answer specific charges); and, (c) arresting the offender.

North Carolina law gives a police officer the legal authority to arrest a spouse abuser for a misdemeanor offense committed in the officer's presence. The officer has similar authority with regard to misdemeanors committed out of the officer's presence, provided that the officer has probable cause to believe that (a) the offender has committed a misdemeanor, *and* that (b) the officer either (i) will not be apprehended unless immediately arrested, or (ii) might cause physical injury to himself or others, or damage to property, unless

¹⁰⁶ U.S. BUREAU OF THE CENSUS, COUNTY & CITY DATA BOOK 690 (1988).

¹⁰⁷ This information was acquired by the authors from internal Charlotte Police Department documents and data.

¹⁰⁸ *Id.*

¹⁰⁹ *Id.*

¹¹⁰ *Id.*

¹¹¹ *Id.*

¹¹² *Id.*

immediately arrested.¹¹³

The experiment utilized the entire patrol force and operated citywide twenty-four hours a day. Cases that met specified eligibility criteria were randomly assigned to one of the three treatments, and these cases were followed for a period of at least six months to discern whether recidivism had occurred. Measures of recidivism were obtained through use of both official police records and victim interviews. The research design called for victims to be interviewed shortly after the presenting incident and again six months after the presenting incident.

C. ELIGIBILITY CRITERIA

In order for a call for police assistance to be included in the experiment, it had to meet a number of eligibility criteria that were determined by legal requirements, policy judgments, and matters of research design.

The overriding legal criterion was that eligible cases must have been classified as misdemeanor offenses. The necessity for this criterion is that a spouse abuse incident must have been such that any of the three randomly assigned treatments *could have been* assigned but that none was *required*. If, for example, the responding officers determined that no offense had been committed, arresting the offender was not a viable option. If a felony had been committed, or if the suspect was subject to an outstanding warrant or a restraining order, an arrest was mandatory, and neither of the other two treatments would have been options. The requirement that eligible cases fall within the misdemeanor range of incidents ensured the selection of cases in which police were empowered but not required to make an arrest.

Other eligibility criteria were determined by policy judgments made by the Charlotte Police Department and the research staff. As a matter of policy we decided to focus only on the female victims of male offenders. We utilized a relatively literal interpretation of the concept of spouse and included only heterosexual spouse (married, divorced, separated) and spouse-like (cohabitant, ex-cohabitant) relationships. Other family relationships such as parent-child and sibling relationships were excluded. Non-cohabiting boyfriend-girlfriend relationships were excluded because they lacked the living together component of a spousal relationship. Similarly, cases in which either the victim or the offender was under the age of eight-

¹¹³ N.C. GEN. STAT. § 15A-401(b)(1-2)(1991).

een were excluded since special research instruments and parental approval procedures would have been required.

Policy judgments of a different nature were involved in the decision to exclude three additional types of cases. Major emphasis was placed on both victim and officer safety. The research was designed so that the project should not pose any additional danger either to the victim or to the responding officers. Consequently, cases in which the victim insisted on the arrest of the offender, cases where the suspect threatened or assaulted the officer, and cases where the officers believed the suspect posed imminent danger to the victim were excluded from the experiment. Officers remained free to make arrests in these situations without being restrained by any features of the research design.

The final eligibility criterion arose directly from the research design. Since it was not possible for the responding officers to deliver all of the treatment responses if either the victim or the suspect was not present at the scene, the absence of either made the case ineligible for inclusion in the experiment. While cases selected for the experiment do not represent all spouse abuse calls that the police receive, they do represent cases in which police have the discretionary power to make warrantless arrests.

D. RANDOMIZATION PROCEDURES

The procedures for the random assignment of treatments utilized the police department's Computer Assisted Dispatch system. When any call for service was received at the police department the complaint-taker brought up a format on the computer that is stamped with the time when the person makes the initial contact. The time field is a five digit number representing the cumulative seconds at that time for that day and was utilized to generate the random treatment assignments. Dividing the time field by three and adding one to the remainder produced a digit of one, two, or three, which represented the code for the assigned treatment response. This procedure is based on the time a call is received and was not subject to manipulation. The time stamp occurred automatically before the telecommunication operator was informed of the reason for the call.¹¹⁴ No problems were encountered in implementing the randomization procedures. By removing the process from human

¹¹⁴ As a back-up system in the event the computer was down when a treatment code was requested, sealed envelopes with random assignments in them were kept by dispatch. During the life of the experiment only five envelopes were utilized, with the verification process confirming that all treatments had been correctly assigned.

decision-making, this aspect of the experiment was carried out exactly as designed.

Random assignment procedures are employed to obtain equivalence among the cases in the different experimental groups. This ensures that any differences observed in the groups prior to the administration of treatments occur by chance. Differences between the groups after the treatments may then be attributable to treatment effect. Examination of the background characteristics of the cases in the different treatments made possible a high level of confidence in the integrity of the randomization process. Comparisons using twenty-nine characteristics which might have been responsible for an unknown bias in random assignment to treatment categories showed that in twenty-eight of the twenty-nine comparisons there were no differences between the three groups that were statistically significant at the .05 level.¹¹⁵

E. THE TREATMENTS

Two features were common to all three treatments. First, the responding officers were to attempt to calm matters down and restore order. Second, each victim was to be given a Victim Information card which provided basic details about the availability of local resources that could be of assistance, namely the Victim Assistance Program and the Battered Women's Shelter. In addition, general police procedures allowed officers to transport a person to another location.

Other features differentiated the three treatments. The advise/separate treatment required that the officers attempt to help the couple solve their immediate problem, possibly referring them to an appropriate social service agency or asking one of them to leave the residence for a period of time.¹¹⁶ The arrest treatment required that the suspect be arrested, handcuffed, and transported to the local jail for an appearance before a magistrate. The citation treatment required that the officers issue the offender a standard citation and explain the required court appearance to both the victim and the offender.

¹¹⁵ J. DAVID HIRSCHER ET AL., CHARLOTTE SPOUSE ASSAULT REPLICATION PROJECT: FINAL REPORT 35-39 (1991).

¹¹⁶ The advise/separate treatment does not provide for a mandatory separation of the couples. North Carolina law does not empower officers to force a separation unless an arrest is made.

IV. FINDINGS

This section, which describes the findings of the Charlotte study, is divided into two parts. In the first part we examine the flow of calls received by the Charlotte Police Department during the study period. We determine the percentage of calls that constituted probable and non-probable cause cases, the percentage of probable cause cases in which on-scene arrest was mandatory, discretionary, or not feasible, and the percentage of probable cause cases that constituted eligible cases for the experimental part of our study. This examination provides an understanding of the broader context in which the experiment was conducted and aids in the interpretation of the significance of those results. In the second part of this section we present the experimental findings, focusing in particular on the prevalence, incidence, and time to failure of recidivism as measured by both arrests and victim reports of abuse.

A. ANALYSIS OF CALLS RECEIVED

1. Overview of Calls Received

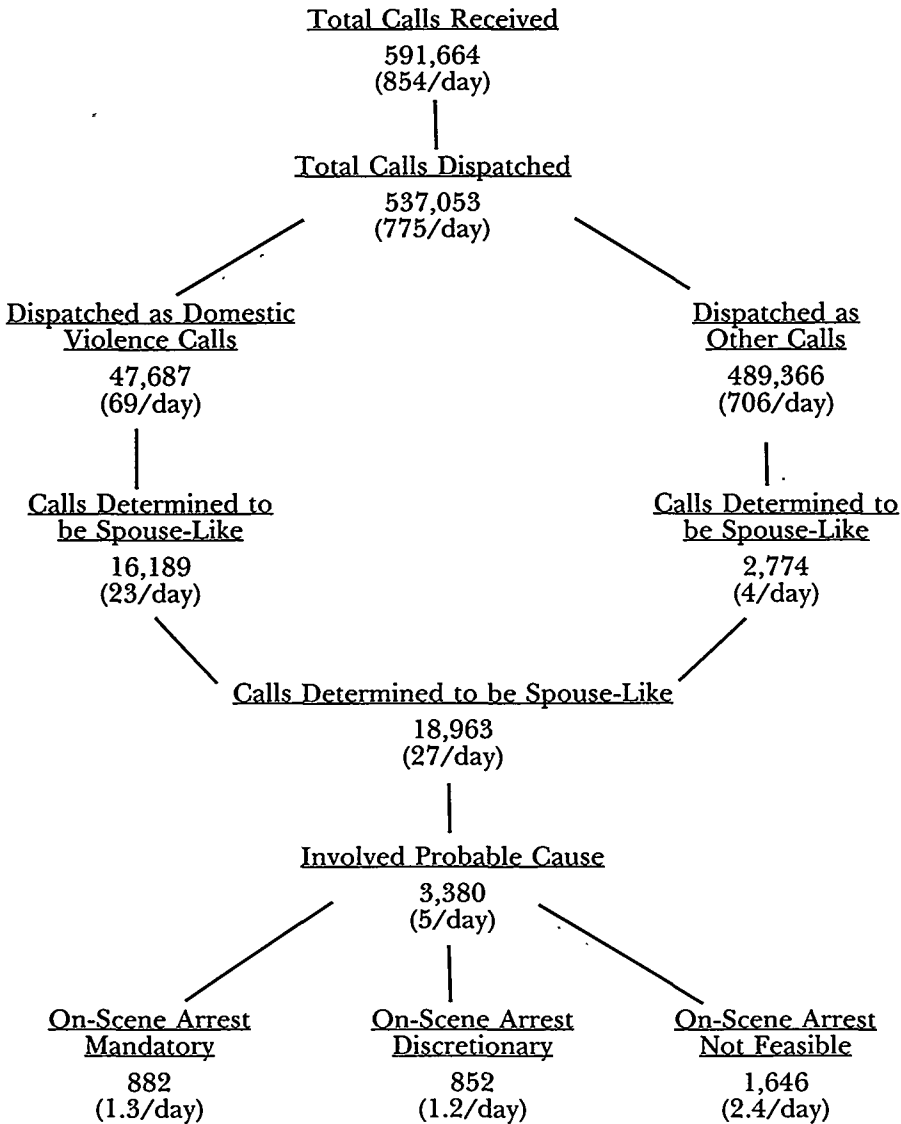
The data presented in Figure 1 provide a description of the flow of cases received by the police department during the ninety-nine week period from August 8, 1987 through June 30, 1989.¹¹⁷ As can be seen, 591,664 calls for assistance were received, 537,053 of which, or 90.8%, resulted in an officer being dispatched to the scene. A total of 47,687 (8.9%) of the calls in which an officer was dispatched were received under the domestic violence (10-91) code. Naturally, not all of these calls turned out to involve a domestic situation. Conversely, a number of calls dispatched on other codes (in particular 10-90, the assault code) turned out to involve a domestic situation.

A total of 18,963 cases were determined at the scene to be spouse-like situations. This represents 3.2% of calls received and

¹¹⁷ Prior to the inception of the research, the police department routinely collected basic information on calls for service, and on both probable cause and non-probable cause cases. However, much of this information was not computerized and none of it was presented utilizing the cohabitant and ex-cohabitant relationship categories employed in this research study. As a consequence, the Charlotte Police Department agreed to use two additional forms for the project. One, the Domestic Violence Supplement Report, was completed for all spouse-like cases in which there was probable cause to believe that a crime had been committed. As its title indicates, this form was used as a supplement to the existing offense report. The second, the Domestic Violence Miscellaneous Incident form, was completed in all non-probable cause domestic violence cases. These included both spouse-like and non spouse-like (e.g. parent-child, brother-sister) cases. Through use of these forms and call for service data detailed information was obtained on all domestic violence calls received by the Charlotte Police Department.

FIGURE I

**FLOW OF CALLS FOR ASSISTANCE RECEIVED BY THE CHARLOTTE
POLICE DEPARTMENT, AUGUST 8, 1987 THROUGH JUNE 30, 1989**



3.5% of calls dispatched. Of the 18,963 spouse-like cases, 15,583 (82.2%) involved situations where there was no probable cause to believe that a crime had been committed. Of the 3,380 probable cause cases, 882 (26.1%) resulted in a mandatory arrest. On-the-scene arrest was discretionary in 852 cases (25.2%) and not feasible in 1,646 cases (48.7%).

2. *Non-Probable Cause Spouse-Like Cases*

The above analysis clearly indicates that arrest is not an option in the vast majority (82.2%) of the spouse-like cases, since the responding officers cannot determine probable cause. To obtain a more thorough understanding of these cases, data were collected on a sample of the 15,583 non-probable cause spouse-like cases. These cases were by definition less serious situations with no discernible victims. Officers were often unable to provide much information other than the kind of situation they encountered and the action they took.

These additional data were collected from August 8, 1987 through January 3, 1989. In a large number of the cases we did not learn the disputants' relationship. As can be seen from Table 1, the 8,916 cases for which such information was obtained were evenly split between married couples (41.9%) and cohabitants (40.0%). In approximately two-fifths (41.0%) of the situations police could do little upon arrival at the scene. In these cases, either there was no apparent problem (21.3%) or the complainant had left the scene (19.7%). Most problems encountered were described either as a shouting match between the disputants (32.9%) or a drinking or drug problem (20.3%). After arriving on the scene the officers generally took no action (48.4%) or simply calmed things down (32.1%). Thus, the vast majority of the spouse-like calls were situations involving minor problems requiring little or no police action.

3. *Probable Cause Spouse-Like Cases*

As noted above, Figure 1 reveals that, of the 18,963 calls determined to involve spouse-like situations, 3,380 (17.8%) were cases in which there was probable cause to believe that a crime had been committed. Among the probable cause spouse-like cases, 882 (26.1%) were cases in which an on-the-scene arrest was mandatory, 852 (25.2%) were cases in which an on-the-scene arrest was discretionary, and 1,646 (48.7%) were cases in which an on-the-scene arrest was not feasible. The third category consisted primarily of cases in which the offender had left the scene (1,437, 87.3%) and

TABLE 1
PROFILE OF NON-PROBABLE CAUSE SPOUSE-LIKE CASES

N=8,916

Disputant Relationship	Number	Percentage
Married	3735	41.9
Separated	692	7.8
Divorced	248	2.8
Cohabitants	3571	40.0
Ex-Cohabitants	670	7.5
Disputant Race		
Black/Black	4445	73.8
White/White	1443	23.9
White/Black	91	1.5
Other	47	0.8
Situation Encountered*		
Gone on Arrival	1753	19.7
No Apparent Problem	1900	21.3
Shouting Match	2937	32.9
Drinking/Drug Problem	1814	20.3
Other (e.g. property disputes marital problems, eviction, problems with children)	1347	15.1
Disposition*		
Calmed Things Down	2865	32.1
Transported Male	296	3.3
Transported Female	344	3.9
Transported Other	36	0.4
No Action Taken	4312	48.4
Other (e.g. counseled/advised, one or both parties left)	1480	16.6

* More than one response may have been given.

cases in which officers determined that warrantless arrest was improper (215, 13.1%).¹¹⁸

When an on-the-scene arrest was mandatory it was generally because the victim had insisted on the offender's arrest (603, 68.4%, of the cases) or because the officers believed arrest was necessary to ensure the victim's safety (182, 20.6%, of the cases). An on-the-

¹¹⁸ The percentages total to more than 100% because more than one reason may have been given as to why an on-the-scene arrest was mandatory or not feasible. If an arrest was mandatory (e.g. the offender was subject to a restraining order), but was not feasible (e.g. the offender had left the scene), an on-the-scene arrest was deemed not feasible since a warrant would have to be issued for the offender's arrest.

scene arrest was mandatory in thirty-six (4.1%) cases because the offender was subject to an outstanding warrant and in twelve (1.4%) cases because the offender was subject to a restraining order. An on-the-scene arrest was not feasible in sixteen cases where the offender was subject to either an outstanding warrant or a restraining order but had left the scene.

The vast majority of probable cause cases (over 90%: see Table 2) were offenses committed by male offenders against female victims with whom they were either married (40.6%) or cohabiting (38.5%). In 95.5% of the cases, the offense was classified as an offense against the person, typically some form of assault. In about seventy-five percent of the cases the offender inflicted some type of injury upon the victim (most commonly bruises). In about twenty percent of the cases the offender caused property damage.

A comparison of salient victim, offender, and incident characteristics reveals some interesting differences between cases in which on-the-scene arrests were mandatory, discretionary, or not feasible. As an examination of Table 2 indicates, on-the-scene arrests were less likely to be feasible in cases involving victims and offenders (1) who were not living together, (2) who were either maritally separated or ex-cohabitants, (3) who showed no use of alcohol or drugs at the time, (4) where no physical injury was apparent, and (5) where property offenses (e.g. criminal trespass, larceny) were more often committed. Cases in which arrest was mandatory, on the other hand, were most likely to involve offenders who had been drinking or taking drugs. Finally, cases in which arrest was discretionary most differed from the other two categories in that they were most likely to involve male victims and female offenders. This may indicate that female offenders were less likely than their male counterparts to pose further danger to their victims or to leave the scene.

TABLE 2
PROFILE OF PROBABLE CAUSE CASES BY ARREST CATEGORY

	On-Scene Arrest Mandatory		On-Scene Arrest Discretionary		On-Scene Arrest Not Feasible		Total	
	N	%	N	%	N	%	N	%
VICTIM INFORMATION								
<u>Sex</u>								
Male	45	5.1	155	18.2	42	2.6	242	7.2
Female	837	94.9	697	81.8	1604	97.4	3138	92.8
Chi Square = 214.217 $p = .000^1$ Cramers v = .252								
<u>Race</u>								
Black	620	70.8	592	69.9	1185	72.5	2397	71.4
White	252	28.8	245	28.9	436	26.7	933	27.8
Other	4	0.5	10	1.2	13	0.8	27	0.8
Chi Square = 4.829 $p = .305$								
<u>Marital Status</u>								
Married	380	43.1	404	47.4	587	35.7	1371	40.6
Separated	40	4.5	17	2.0	152	9.2	209	6.2
Divorced	13	1.5	8	0.9	24	1.5	45	1.3
Cohabitant	349	39.6	354	41.5	597	36.3	1300	38.5
Ex-Cohabitant	100	11.3	69	8.1	286	17.4	455	13.5
Chi Square = 119.479 $p = .000$ Cramers v = .188								
<u>Living with Offender</u>								
Yes	700	79.4	720	84.5	1082	65.7	2502	74.0
No	182	20.6	132	15.5	564	34.3	878	26.0
Chi Square = 120.591 $p = .000$ Cramers v = .189								
<u>Alcohol/Drug Use</u>								
Impaired	84	9.7	104	12.3	128	7.9	316	9.5
Apparent Use	174	20.1	165	19.5	238	14.8	577	17.4
No Apparent Use	609	70.2	575	68.1	1245	77.3	2429	73.1
Chi Square = 31.065 $p = .000$ Cramers v = .097								
<u>Injury</u>								
None	200	23.2	173	20.6	491	30.4	864	26.1
Gunshot Wound	1	0.1	0	0.0	3	0.2	4	0.1
Knife Wound	18	2.1	32	3.8	23	1.4	73	2.2
Broken Bone	14	1.6	10	1.2	16	1.0	40	1.2
Bruises	570	66.2	554	65.9	979	60.7	2103	63.4
Other	58	6.7	72	8.6	102	6.3	232	7.0
Chi Square = 49.787 $p = .000$ Cramers v = .087								

¹ Measures of association are given when p is equal to or $< .05$.

Table 2: (continued)

	On-Scene Arrest Mandatory		On-Scene Arrest Discretionary		On-Scene Arrest Not Feasible		Total	
	N	%	N	%	N	%	N	%
<u>Medical Treatment</u>								
None	724	86.1	690	83.7	1358	87.4	2772	86.1
Hospitalized	8	1.0	6	0.7	2	0.1	16	0.5
Treated and Released	51	6.1	60	7.3	112	7.2	233	6.9
Refused Treatment	58	6.9	68	8.3	81	5.2	207	6.4
	Chi Square = 18.803		$p=.005$		Cramers v = .054			
<u>OFFENDER INFORMATION</u>								
<u>Sex</u>								
Male	843	95.6	700	88.2	1607	97.6	3150	93.2
Female	39	4.4	152	17.8	39	2.4	230	6.8
	Chi Square = 222.575		$p=.000$		Cramers v = .257			
<u>Race</u>								
Black	642	72.8	600	70.4	1223	74.3	2465	72.9
White	225	25.5	228	26.8	387	23.5	840	24.9
Other	15	1.7	24	2.8	36	2.2	75	2.2
	Chi Square = 6.202		$p=.185$					
<u>Alcohol/Drug Use</u>								
Impaired	249	29.0	202	24.3	310	22.4	761	24.7
Apparent Use	289	33.6	228	27.4	385	27.8	902	29.3
No Apparent Use	321	37.4	402	48.3	690	49.8	1413	45.9
	Chi Square = 36.357		$p=.000$		Cramers v = .077			
<u>OFFENSE INFORMATION</u>								
<u>Crime Against the Person</u>								
	846	95.9	832	97.7	1550	94.2	3228	95.5
<u>Crime Against Property</u>								
	36	4.1	20	2.3	96	5.8	152	4.5
	Chi Square = 16.355		$p=.000$		Cramers v = .070			
<u>Property Damage</u>								
Yes	171	19.7	174	20.6	341	20.8	686	20.5
No	698	80.3	670	79.4	1297	79.2	2665	79.5
	Chi Square = .468		$p=.791$					

B. EXPERIMENTAL RESULTS*1. Caseflow and Officer Participation*

Training was conducted in June, 1987. The actual field experiment began on June 13, 1987 with a pretest phase that lasted until August 7, 1987. Between the inception of the field test on August 8, 1987 and June 30, 1989, the project received 686 eligible cases at the rate of almost exactly one case per day.¹¹⁹ It took 252 officers to produce the 686 eligible cases, of whom 116 officers contributed one case, forty-eight officers two cases, twenty-one officers three cases, twenty-one officers four cases, and forty-six officers five or more cases. The top three contributors produced fifty-four eligible cases, 7.8% of the total number.

2. Assigned and Delivered Treatments

Of the 686 eligible cases, 573 (83.5%) were delivered as assigned (see Table 3). The remaining 113 (16.5%) of the cases were misassigned. These misassignments were delivered by eighty-four officers, and were not equally distributed across the three treatments. The misassignment rate for the arrest treatment was 9.1%, for the advise/separate treatment 12.8%, and for the citation treatment 26.7%. Clearly, implementation of the citation treatment as randomized was not as high as for the other two treatments. Misassignments were of four types: advise/separate treatments delivered as arrests (twenty-eight cases), citations delivered as arrests (forty-three cases), citations delivered as advise/separate (twenty cases), and arrests delivered as advise/separate (seventeen cases).¹²⁰ The movement from less severe to more severe treatment is partly due to the fact that the most common reason given for misassignment was "escalation of imminent danger" (fifty-two cases).

¹¹⁹ The discrepancy between the number of eligible cases (686) and the number of cases in which an on-the-scene arrest was discretionary (852) arises because cases involving either male victims or female offenders were not eligible for the experiment.

¹²⁰ The treatment delivered in two misassigned cases resulted in a criminal summons. In one case, the suspect left before the responding officer could issue a citation. The other case involved an officer who had received an advise/separate treatment and transported the victim to the magistrate's office.

TABLE 3
MISASSIGNMENT OF TREATMENTS

	Randomized Treatment			Total
	Arrest	Citation	Advise/Separate	
<u>Treatment Delivered</u>				
Arrest	200	43	28	271
Citation	3	176	0	179
Advise/Separate	17	20	197	234
Criminal Summons	0	1	1	2
Total	220	240	226	686

$\chi^2=812.430$, d.f.=6, $p.<.001$, Cramer's $V.=0.770$

3. Arrest Recidivism: Definition and Operationalization

Arrest recidivism can be measured in a number of different ways. Decisions have to be made regarding the type of offense, the type of victim-offender relationship, and the time period to be included in the definition. Arrest recidivism can encompass, for example, any subsequent arrest or only a subsequent arrest for a violent crime. It can include any offense committed against any victim or only offenses committed against a specified victim. Finally, the follow-up period can include any specified amount of time.

In this study we defined arrest recidivism as

Any arrest for any subsequent offense by the same offender against the same victim committed within six months of the presenting incident.

Although most of the offenses committed by offenders were assaults against the person, this operational definition is not limited to such assaults. The definition includes a variety of other offenses such as criminal trespass and damage to property. This operational definition does not include a subsequent arrest made on the basis of an offense committed during the presenting incident.¹²¹ Likewise, arrests for procedural matters are not included as arrest recidivism.¹²²

The 686 eligible cases involved 650 different offenders necessi-

¹²¹ Thus, for example, if after an advise/separate treatment had been assigned and delivered, the victim swore out a warrant and had the offender arrested for an offense committed during the presenting incident, this would not be included as arrest recidivism.

¹²² Thus, if after a citation treatment had been assigned and delivered, and a subsequent order for the offender's arrest was issued and served based on his failure to appear for court proceedings as mandated by the citation, this would not count as arrest recidivism.

tating the acquisition of a total of 650 offender criminal histories. The police department maintains computerized records of all arrests made within city limits. Hard copies of all arrest, offense, and supplemental reports are kept by date of offense. Certified Police Information Network operators conducted computerized record searches on all offenders involved in eligible cases. In each of the thirty-six cases where an offender re-entered the experiment with a new offense, these incidents were processed as repeat offenses rather than as new cases.

The analysis of main effects focuses on three aspects of arrest recidivism: prevalence, incidence, and time to failure. Prevalence of failure concentrates on whether a subject failed and is defined as the percentage of offenders that had at least one failure according to the definition of recidivism used above. Incidence focuses on the number of times a subject fails and is defined as the mean number of failures per case within a given treatment group. Time to failure concerns the specified six-month follow-up period and examines when the offender recidivates. All primary analyses were conducted on treatments as assigned.

4. Arrest Recidivism: Prevalence and Incidence

The prevalence and incidence of arrest recidivism for the three treatments during the six months after the presenting incidents are reported in Table 4.

TABLE 4
PREVALENCE AND INCIDENCE OF ARREST RECIDIVISM, BY
TREATMENT^a

Number of Subsequent Arrests	Treatment Assigned			
	Arrest	Citation	Advise/Separate	Total
0	175	181	187	543
1	36	33	24	93
2	2	7	1	10
3	1	1	0	2
4	0	2	0	2
Total Failures	39	43	25	107
Total Cases	214	224	212	650
Prevalence ^b	18.2	19.2	11.8	16.5
Incidence ^c	.201	.259	.123	.195

^a As measured six months subsequent to presenting incident

^b $\chi^2=5.063$, d.f.=2, $p=.080$

^c F ratio=4.211, d.f.(1)=2, d.f.(2)=647, $p=.015$

Examination of Table 4 indicates an overall prevalence rate of 16.5, a prevalence rate of 18.2 for the arrest treatment, 11.8 for advise/separate, and 19.2 for the citation treatment. There are no statistically significant differences among the three treatments which are significant at the .05 level. Based on prevalence, arrest is no better at deterring failure than the other two treatments.¹²³

Incidence is defined as the average number of failures per case within a given group. As shown in Table 4, the incidence rate of arrest recidivism for the total sample was .195. Examination of the data in this table shows an incidence rate of .201 for those in the arrest treatment, .123 for offenders in the advise/separate treatment, and .259 for those in the citation treatment. Analysis of variance based on the number of subsequent arrests produced an overall F ratio significant at the .05 level. However, Scheffé Multiple Range Comparisons yielded significance at the .05 level only for the advise/separate-citation comparison (see Table 5).¹²⁴ Thus, in com-

¹²³ The vast majority (76.4%) of the crimes for which offenders were rearrested were for assaults against the victim. There were no significant differences between the three treatment groups in the types of offenses for which rearrests took place.

¹²⁴ The authors recognize that, given the distribution of these data, some might argue that analysis of variance is not the most appropriate statistical technique. Consequently these data were also analyzed using χ^2 . To reduce the number of cells with expected

paring rates of failure as measured by incidence, arrest is apparently more effective than the citation treatment but apparently less effective than the advise/separate treatment. However, arrest is not significantly more or less effective than these other two treatments at statistically acceptable levels.

TABLE 5
INCIDENCE OF ARREST RECIDIVISM, SCHEFFÉ PAIRWISE
COMPARISONS OF TREATMENT GROUPS

<u>Descriptive Statistics</u>				
	<u>Arrest</u>	<u>Citation</u>	<u>Advise/Separate</u>	
	mean = 0.201	mean = 0.259	mean = 0.123	
	N = 214	N = 224	N = 212	
<u>Pairwise Comparisons</u>				
<u>Comparison</u>	<u>Contrast</u>	<u>Significance</u>		
Advise/Separate v. Citation	0.136	$p. < .05$		
Advise/Separate v. Arrest	0.078	$p. > .05$		
Citation v. Arrest	0.058	$p. > .05$		
<u>Combined Comparisons</u>				
Critical L-Ratio = +/- 2.449 ^a				
<u>Groups Contrasted</u>	<u>Contrast</u>	<u>Std. Error of Contrast</u>	<u>L-Ratio</u>	<u>p.</u>
Adv/Sep + Cit v. Arrest	-0.010	0.041	-0.248	> .05
Adv/Sep v. Cit + Arrest	+ 0.107	0.041	+ 2.610	< .05

^a = Critical L = SQRT [(g-1) (Critical F)] where g = the number of treatment groups and the Critical F is the F-value required to reject the hypothesis that all group means are equal with 2 and 647 degrees of freedom at the 0.05 alpha error probability level.

In order to examine more comprehensively the effect of the advise/separate treatment, and to control to some extent for misassignments of treatment (38.1% of the misassignments were citations delivered as arrests), analysis was undertaken of the informal versus the formal treatment responses. This task was accomplished through the use of pairwise Scheffé comparisons of the informal (advise/separate) versus the formal (citation and arrest) treatment

counts of less than 5 the categories of 2, 3, and 4 subsequent arrests were collapsed into a single category of 1 or more arrests. The resulting 3 by 3 table produced a χ^2 of 12.028 (df=4, $p=.017$). Utilizing χ^2 , results significant at the .05 level were likewise obtained for the comparison between the advise/separate and citation categories ($\chi^2 = 8.559$ (df=2, $p=.014$)) and the combined arrest-citation and separate/advise categories ($\chi^2=6.916$ (df=2, $p=.031$)).

responses. The Scheffé approach allows the combining of treatment groups for the purpose of comparing the aggregated group to the other treatment groups.¹²⁵

Analysis indicated that the informal response better deterred subsequent abuse, as can be observed in Table 5. Also, the contrast between arrest and non-arrest (advise/separate and citation combined) was not statistically significant at the 0.05 level.

5. *Prevalence and Incidence Re-examined*

The primary analyses of official recidivism utilized treatment as assigned as the independent variable. Analysis was performed on the 650 couples that produced eligible cases. In this subsection we examine the results in several ways. First, we take into account race, prior record, and other such variables. Second, we conduct the analyses on sample sizes that are theoretically important, but different from the basic sample of 650. Finally, we utilize treatment as delivered as the independent variable.

a. Analyses on Treatment as Assigned Taking into Account Race, Prior Record, and Other Variables

This analysis takes into account such offender-related variables as race, age, employment status, prior record and victim-suspect relationship. Initial examination of the association between these variables and arrest recidivism indicated that the strongest predictors of recidivism were measures of prior criminal activity, such as possession of a local (felony or misdemeanor) record, possession of a state (felony) record, and number of prior non-traffic arrests within the preceding five years. Further, while prior criminal activity was associated with recidivism, other offender-related variables, such as race, age, marital and employment status, were not.¹²⁶ Moreover, knowledge of an offender's prior criminal activity produced only a modest contribution to predicting correctly an offender's probability of recidivating.¹²⁷

¹²⁵ JOSEPH L. FLEISS, *THE DESIGN AND ANALYSIS OF CLINICAL EXPERIMENTS* (1986).

¹²⁶ Attempts to build a logistic model indicated that with the best measures of prior criminal activity in the model, no other variables exceeded a .1 level of significance cut-off for their addition to the model.

¹²⁷ Goodman and Kruskal's Tau was .050 ($p = .000$) for the association between possession of local arrest record and recidivism, .035 ($p = .000$) for non traffic arrest within preceding five years and recidivism, and .029 ($p = .00002$) for possession of state felony record and recidivism.

b. Analyses Based on Samples Other Than the Basic Sample

As noted above, all primary analyses of arrest recidivism were conducted on a sample size of 650. While the authors consider this sample size the most appropriate for the analyses undertaken, they acknowledge that arguments may be raised in favor of other sample sizes.

First, instead of treating repeat cases as treatment failures, they might have been counted as new cases, although this violates the assumption of independence. The thirty-six repeat cases in this study would raise the sample size to 686. Second, cases that entered the experiment during the last six months of operation might have been excluded from analyses of arrest recidivism, since operational procedures changed on June 30, 1989 when the last eligible case was collected. Excluding the cases that entered the experiment in the last six months reduces the sample size to 513. Third, analysis might have been conducted solely on cases that were delivered the assigned treatment. A total of 545 of the 650 eligible cases were delivered the treatment they had been assigned.

Analysis of the delivered-as-assigned cases ($N=545$) and of repeat cases treated as new cases ($N = 686$) produced results that were insignificant at the .05 level. Neither of these analytical approaches showed statistically significant differences in arrest recidivism between the three treatment groups.

Analysis of all cases except those that entered the experiment during the last six months ($N = 513$) yielded results similar to those obtained by the primary analysis of arrest recidivism. This analysis produced findings of significance at the .05 level only for analysis of variance conducted on the incidence rates. As in our primary analysis, the Scheffé comparison test indicated that the significant difference existed between the advise/separate and the citation treatments. The former more effectively deterred abuse.

c. Analyses Based on Treatment as Delivered

Primary analyses were performed on treatments as assigned. This yielded the most unambiguous results relative to statistical standards. Since not all of the treatments were delivered as assigned, we considered it worthwhile to examine the results obtained by utilizing treatment as delivered as the independent variable. Prevalence and incidence analyses conducted utilizing treatment as delivered as the independent variable yielded results that were insignificant at the .05 level.

6. *Arrest Recidivism: Time to Failure*

Prevalence and incidence measures are not sensitive to significant fluctuations in the relative effectiveness of the three treatments that may occur during the six month period. It may be the case, for example, that arrest has an immediate deterrent effect (e.g. one month) but in the long term (e.g. six months) has no greater deterrent effect than the other two treatments. In order to identify such time fluctuations in deterrence, survival analysis was conducted on the data.

The survival experiences of the different treatment groups are presented in Figure 2. As can be observed from an examination of this graph, at no time is the arrest treatment more effective in deterring subsequent abuse than the other two treatments.

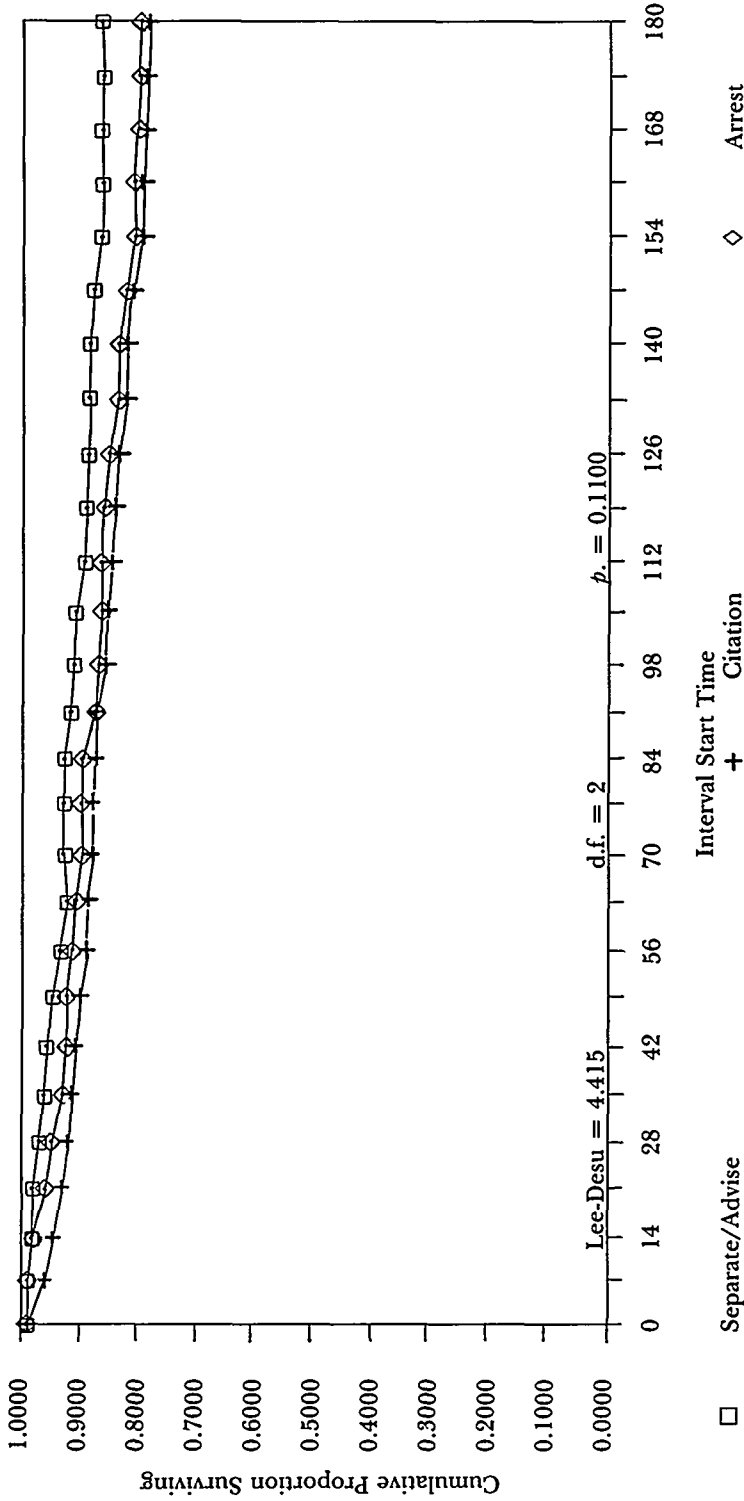
7. *Secondary Arrest Recidivism*

Recidivism results based on arrest of the same offender have been narrowly and specifically defined under the category of primary arrest recidivism. However, different definitions of the dependent measure are possible which lead to varying results and interpretations. In order to assess treatment effects on recidivism more thoroughly, we investigate alternative operational definitions of arrest recidivism.

Four different operational definitions are examined, all of which utilize the six-month follow-up period and all of which incorporate an arrest or rearrest of the same offender. The first of these four measures is similar to primary arrest recidivism but includes arrests which are related to the presenting incident, such as where the couple receives the advise treatment at the presenting incident but where the victim secures a warrant for the offender's arrest for that same incident the following day. The next operational definition of secondary arrest recidivism allows a subsequent arrest of the offender for any crime of violence against any person. Here, we eliminate the criterion that the incident must involve the original victim from the presenting incident. The third approach to secondary arrest recidivism allows crimes against property in addition to crimes of violence. The fourth operational definition is all-encompassing: any subsequent arrest of the offender. This last dependent measure incorporates arrest for spouse abuse, assaults against others, property damage, and drug related crimes.

As can be seen from Table 6, there is no evidence that the prevalence of arrest or rearrest of spouse abusers is significantly influ-

FIGURE 2
TIME TO FAILURE FOR ARREST RECIDIVISM BY THE THREE TREATMENTS



□ Separate/Advise
+ Interval Start Time
◇ Arrest

enced by police response regardless of the particular definition of arrest.

Different conceptualizations of arrest, however, produce strikingly different *p.* values. In addition, they lead to markedly different conclusions about the *amount* of recidivism.

In the data presented here, the recidivism rate using the broadest definition is twice the rate using the primary and more specific definition (32.6% vs. 16.5%). Approximately one-third of the male offenders were arrested or rearrested at least once within six months of the presenting incidents. As these data suggest, however, this fact does not necessarily mean an arrest for another assault against the same female victim.

The operational definition chosen also affects the relative effectiveness of the three treatments in deterring recidivism. When the broadest definition of arrest recidivism is utilized, the citation treatment produces the lowest rate of recidivism. When we use the narrowest definition of arrest recidivism, however, the citation treatment produces the highest rate of recidivism. None of these treatment differences though are statistically different at the .05 level.

The different operational definitions of arrest do not, however, produce equally insignificant results. Indeed, the *p.* values vary considerably. Our measure of primary arrest recidivism yields a *p.* value of .080, while our broadest definition of secondary arrest recidivism yields a *p.* value of .67. In general, the narrower the definition of arrest recidivism, the lower the *p.* value.

The importance of selecting a suitable operational definition of arrest cannot be overemphasized. The theoretical framework of this study for assessing treatment effects necessitated excluding arrests for so called victimless crimes, such as the unlawful possession of drugs, and arrests for offenses against victims other than those in the presenting incidents. Since we attempted to measure the effectiveness of different treatment responses in deterring subsequent abuse against spouses and cohabitants, it was not logical to count as failures subsequent arrests for "victimless" crimes or for offenses committed against other victims, such as employers or strangers. Likewise, the importance of thoroughly investigating the details of each arrest even against the same victim cannot be overly stressed. Simply taking a rap sheet at face value without examining case reports may lead to the erroneous inclusion of arrests on procedural matters as arrests for subsequent offenses.

TABLE 6
SPOUSE ABUSE RECIDIVISM RATES FOR DIFFERENT DEFINITIONS OF
ARREST RECIDIVISM BY TREATMENT

	Total 650*	Treatment Assigned			Chi-Sq.	<i>p.</i>
		Adv./Sep. 212	Cit. 224	Arr. 214		
<u>Primary Arrest Recidivism</u>						
Any subsequent arrest except one relating to the presenting incident involving the same victim and the same offender	107 (16.5)	25 (11.8)	43 (19.2)	39 (18.2)	5.06	0.08
<u>Secondary Arrest Recidivism</u>						
Any subsequent arrest of the same offender involving the same victim as in the presenting incident	128 (19.7)	35 (16.5)	47 (20.1)	46 (21.5)	2.03	0.36
Any subsequent arrest of the same offender for any crime of violence against any person	113 (17.4)	30 (14.2)	41 (18.3)	42 (19.6)	2.42	0.30
Any subsequent arrest of the same offender for any crime of violence against any person or property	155 (23.8)	46 (21.7)	54 (24.1)	55 (25.7)	0.95	0.62
Any subsequent arrest for any offense	212 (32.6)	71 (33.5)	68 (30.4)	73 (34.1)	0.81	0.67

* The numbers total by row, the percentages, which are in parentheses, are calculated as a percentage of the total number of cases that received a particular treatment.

8. Abuse Recidivism

The research design called for victims to be interviewed twice:

first, shortly after the presenting incident; and again six months after the presenting incident.

The initial interview focused on episodes of abuse which occurred between the time of the presenting incident and the interview. The six month interview included only incidents occurring since the time of the first interview. More specifically, each victim was asked about six types of victimization, namely whether the offender had: (1) threatened to hurt her; (2) actually hurt or tried to hurt her; (3) threatened to hurt any member of the family; (4) actually hurt or tried to hurt any member of the family; (5) threatened to damage property; and, (6) actually damaged any property. Victims were asked to estimate how often each type of victimization had occurred and were posed more detailed questions on the first and most recent occurrence of victimization.

A total of 686 eligible cases were obtained during the course of the experiment. Forty of these were excluded from the interview process because they involved repeat victims who had already been assigned interviews ($N=36$) or misapplications of treatment (cases where it was discovered after the responding officers had carried out the mandated treatment that either the victim or offender was under age eighteen; $N=4$). Initial interviews were obtained in 419 of the 646 cases assigned for interview for an assigned interview completion rate of sixty-five percent.¹²⁸

Six month interviews were obtained from 324 victims for an assigned six month victim interview rate of eighty-three percent. The completion of 324 six month interviews produces an overall assigned interview rate (both initial and six month) of 50.2%. While this is lower than expected or desired, the generalizability of the results to a larger pool of eligible cases is believed to be sound.

Analysis of the victim interview data produced no statistically significant differences between the three treatments for any of the

¹²⁸ Since victim interviews were not obtained in a sizable minority of the eligible cases, the generalizability of findings based upon the victim interviews could be affected by the extent to which cases where interviews were obtained differ from those where interviews were not obtained. To test whether any significant differences existed between these two groups of cases, relevant victim, offender, and offense characteristics were compared. Few differences significant at the .05 level were observed between cases in the two groups. Victims who were interviewed did not differ significantly from those who were not interviewed on such key background variables as race, age, marital status, employment status, and living arrangements, nor on any of the offense related variables examined. In only two areas were differences noted that were significant at the .05 level. Cases that produced interviews were less likely to have victims who (based on police reports) were under the influence of alcohol or drugs at the time of the presenting incident. In addition, interviewed cases were more likely to have offenders with prior state felony records.

TABLE 7
VICTIM REPORTED RECIDIVISM, BY TREATMENT^a

	Treatment Assigned			Total
	Arrest	Citation	Advise/Separate	
Number of Incidents of Recidivism				
0	46	43	41	130
1	14	18	10	42
2	2	12	8	22
3	12	6	13	31
4	3	9	3	15
5 or more	35	36	27	98
Total Failures	66	81	61	208
Total N Interviewed	112	124	102	338
Prevalence ^b	58.9	65.3	59.8	61.5
Incidence ^c	2.152	2.226	2.078	2.157

^a Based on initial and six month interviews combined

^b $\chi^2=1.202$, d.f. = 2, $p=.548$

^c F ratio=.132, d.f.(1)=2, d.f.(2)=335, $p=.875$

six measures of recidivism examined in either the initial or the six month interviews.¹²⁹ These analyses of victim reported recidivism focused on the six measures of recidivism taken individually. Since it was possible for a victim to report the occurrence of more than one type of repeat incident, it is conceivable that a small number of cases with different types of repeat incidents was unduly affecting the overall picture of victim reported recidivism obtained by examining the six measures separately.

The desirability of constructing a composite measure of recidivism is underscored by the fact that high levels of recidivism were reported in both the initial and the six month interviews for only two of the six measures of subsequent abusive incidents: *offender threatened victim*, and *offender hurt, or tried to hurt victim*. A composite measure of recidivism was obtained by summing the responses to the screen questions for the six types of victimization; i.e., any occurrence of any of the six measures of abusive incidents. Analyses based on these *composite* measures of rates of recidivism again showed no significant differences between the three treatments,

¹²⁹ HIRSCHTEL ET AL., *supra* note 115, at 121-24.

either for the initial or for the six month interviews.¹³⁰

In order to obtain a complete accounting for all acts of recidivism for each case for the full follow-up period, data obtained from the initial interviews were combined with data from the six month interviews.¹³¹

Table 7 presents the prevalence and incidence rates of total victim reported recidivism for the cumulative-composite initial and six month interviews. Consistent with the findings reported thus far on victim reported recidivism, there are no significant differences between the three treatments in subsequent victim reported incidents. The pattern of statistically insignificant findings was maintained when treatment categories were combined.

9. *Synthesis of Arrest and Abuse Recidivism*

The effects of the three treatments employed in this experiment have been examined through the use of two different outcome measures: an official measure of recidivism obtained from police arrest records, and a self-report measure obtained from victim interviews. The two measures each have their respective strengths and weaknesses.

The utilization of an official measure of recidivism, such as subsequent arrest, allows for all subjects in a sample to be tracked with relative ease for a given follow-up period. Thus, there is little case attrition when such official measures are employed. Moreover, the acts of recidivism reported in police arrest records are validated by an independent third party, namely the responding police officers, and have both their occurrence and the time of their occurrence recorded in official reports. On the negative side, however, the basis of the arrest needs to be thoroughly understood (as discussed above) and, only a limited percentage of acts of recidivism are captured by police arrest records. The police are called in a limited percentage of subsequent incidents of abuse, and furthermore only a certain percentage of those incidents are likely to result in the arrest of the offender.

Victim interviews can capture a far wider range of abusive acts committed against victims. However, this can be achieved only for a certain percentage of the sample since it is unlikely that all of the victims will be interviewed. Moreover, information given by victims

¹³⁰ *Id.* at 127-28.

¹³¹ Since fourteen initial interviews were conducted at or around the six month mark, these fourteen cases were added to the 324 cases for which six month interviews were obtained to produce a final sample size of 338.

is not generally validated by an independent third party, and as a consequence is subject to the inadequacies inherent in any survey research endeavor. Such inadequacies include dependence upon respondent definition of situations, and problems arising out of inadequate respondent memory, including such factors as both forward and backward telescoping.

Thus far the results of the outcome analyses conducted on official arrest records and victim interviews have been reported separately. The most notable differences between the results obtained through use of the two data sources have been the far higher prevalence and incidence rates of recidivism reported in the victim interviews. These higher rates may be attributed to the broader definition of recidivism employed in the victim interviews and the factors discussed above which limit the amount of recidivism revealed by arrest records.

TABLE 8:
ARREST RECIDIVISM CROSSTABULATED BY VICTIM REPORTED
RECIDIVISM¹³²

	Arrest Recidivism		
	Yes	No	Total
Victim Reported Recidivism			
Yes	60 (56.1%)	141 (26.0%)	201
No	5 (4.7%)	132 (24.3%)	137
Not Interviewed	42 (39.2%)	270 (49.7%)	312
Total	107	543	650

$$x^2=44.598, \text{ d.f.}=2, p.<.001$$

It is important to examine whether there is a strong agreement

¹³² The absence of treatment effect with the combined arrest and victim reported approach is further confirmed by log-linear analysis. Both a saturated model and a reduced model restricted to the three statistically significant effects (arrest recidivism, victim reported recidivism, and the product of the two) produce log-likelihood chi squares that indicate that the observed and expected frequencies in Table 9 are not significantly different. Treatment assigned, as either a nested or a main effect, is, therefore, not helpful in arriving at an understanding of frequency distributions within the cells of the three dimensional table (Table 9). Just as treatment assigned did not appear to affect the prevalence or frequency of recidivism, or the length of time to failure, it does not appear to affect the distribution of cases in the cells in Table 9. For further details see HIRSCHTEL ET AL., *supra* note 115, at 139-145.

between those cases reported as failures in the official records and those reported as failures in the victim interviews. In Table 8 the prevalence of arrest recidivism is cross-tabulated with the prevalence of victim reported recidivism. There is strong agreement between the two data sources, with statistical association significant beyond the .001 level. There are only five cases in which there was a report of official recidivism, but no victim reported recidivism. The total of 141 cases in which there was victim reported recidivism, but no report of official recidivism, is within the range of what might reasonably have been expected given the definitional and reporting issues discussed above. When this analysis was repeated controlling for treatment as assigned, similar results were obtained. There was consistency among the results obtained for the three treatments individually (Table 9), and these results paralleled those reported for the combined treatments.

Examination of arrest recidivism and victim reported recidivism both separately and together produce the conclusion that treatment assigned does not affect the likelihood of subsequent abuse on the part of the offender. Thus, arrest is neither more nor less effective than the other two treatments at deterring subsequent abuse.

TABLE 9
ARREST AND VICTIM-REPORTED RECIDIVISM, BY TREATMENT
ASSIGNED

	Treatment Assigned								
	Arrest ^a			Citation ^b			Advise/Separate ^c		
	Arrest Recidivism	Arrest Recidivism	Arrest Recidivism	Arrest Recidivism	Arrest Recidivism	Arrest Recidivism	Arrest Recidivism	Arrest Recidivism	Arrest Recidivism
Victim Reported Recidivism	Yes	No	Total	Yes	No	Total	Yes	No	Total
Yes	22 (56.4) ^d	43 (24.6)	65	23 (53.5)	52 (28.7)	75	15 (60.0)	46 (24.6)	61
No	1 (2.6)	46 (26.3)	47	2 (4.6)	47 (26.0)	49	2 (8.0)	39 (20.9)	41
Not Interviewed	16 (41.0)	86 (49.1)	102	18 (41.9)	82 (45.3)	100	8 (32.0)	102 (54.5)	110
Total	39	175	214	43	181	224	25	187	212

^a $\chi^2 = 19.256$, d.f.=2, $p < .001$

^b $\chi^2 = 13.671$, d.f.=2, $p = .001$

^c $\chi^2 = 13.649$, d.f.=2, $p = .001$

^d Percentages in parentheses

V. SUMMARY AND DISCUSSION

The results of the Charlotte experiment are decisive and unambiguous, and indicate that arrest of misdemeanor spouse abusers is neither substantively nor statistically a more effective deterrent to repeat abuse than either of the other two police responses examined in this location. Based on thorough analysis of data from official police records of rearrest, as well as from intensive interviews with victims of abuse, there is no evidence that arrest is a more effective deterrent to subsequent abuse. This conclusion remains regardless of the measure of recidivism utilized — prevalence, incidence, or time to failure. If either victim interview or official police data showed arrest to be significantly better at deterring abuse, then a case could be made on that basis; but neither data set supports this conclusion. Were there a pattern discerned in the data which showed arrest to be generally more effective, even if not at statistically significant levels, it would be tempting to come to a positive conclusion about the deterrent benefits of arrest. However, there is no pattern in that direction. We thus have no choice but to conclude, based on the Charlotte data, that arrest had no more of a significant deterrent value than did the other two police responses.

As we have demonstrated, different measures of recidivism do produce very different rates; however it would be unrealistic and naive to propose a standard approach for defining and operationalizing arrest recidivism. Concepts and their measurement should be user-friendly. Given the diversity of users in this case, any standardized definition is not viable. Nonetheless, we can propose a number of variables relevant to recidivism which, if properly addressed in data dissemination, will facilitate understanding and comparisons across studies. First, elapsed time is always a parameter and needs to be specified. In the present study of spouse abuse arrest recidivism we used a six month time frame. While arrest data for many of our cases extended beyond this time frame, there was not sufficient data for all cases and we wished to avoid right-hand censoring. Second, behaviorally-based operationalizations of recidivism should be as specific as possible. This study used additional abusive incidents which led to the arrest or rearrest of the offender (subsequent to the presenting incident). In the majority of subsequent incidents there had been an additional physical assault. However, a minority of incidents involved threats or property damage rather than physical assault. Thus, the results are open to potential criticism for incorporating diverse behaviors. Third, where arrest or rearrest of an offender constitutes the primary mechanism for measuring recid-

ivism rates, it is crucial to delineate the basis of the arrest. As this study has shown, apparent recidivism rates may vary by 200% contingent on what is included in the arrest. Thus, the difference in recidivism rates obtained from two measures of arrest recidivism may be almost as large as that obtained between arrest and self-reported data. Researchers, and policy makers, should be able to answer the question, "when is an arrest an arrest?" If the mere fact of arrest is sufficient information for purposes of a particular study then this is a moot question. However, in most cases both researchers and policy makers are interested in some aspect of behavior such as substance abuse, domestic violence, or homicide. Finally, as others have noted, utilizing official measures—arrest, conviction, incarceration—are inadequate indicators of the full extent of return to previous behavior. There is little risk in a fresh reminder that official measures consistently fail to capture the full amount of recidivistic behavior.

It is perhaps particularly important for those investigating victim oriented crimes not to lose sight of the deficiencies of official measures of recidivism. Our concern here has been focused on spouse abuse. Legal statutes in every state prescribe general parameters for establishing probable cause, and many states have pro-arrest provisions in domestic violence. However, as we have shown elsewhere¹³³ there is considerable variation in what pro-arrest policies mean in actual practice. Moreover, even in those departments which have strong pro-arrest policies for spouse abuse there is always the variable of officer interpretation of the incident and enforcement of the policy. It does us all well to cast not a caustic eye but a critical one toward recidivism measures based on arrest data.

Examination of the victim interview data in this study reveals alarmingly high levels of repeat incidents of spouse abuse, confirming that the scope of the problem is far greater than police data indicate. As presented earlier, 61.5% of women have experienced another abusive incident within six months. Official records, those based on rearrest by police, show predictably lower prevalence and incidence rates of recidivism than do interview data. Rearrest rates are an extremely conservative measure of recidivism, as they are a conservative measure of spouse abuse in general. Based on police data, repeat incidents are the exception rather than the rule. Based on interviews, however, repeat incidents are the rule rather than the exception, with the *majority* of women who were interviewed having

¹³³ HIRSCHEL ET AL., *supra* note 115.

experienced at least one more abusive incident since the original presenting incident six months earlier.

The apparent discrepancy between police data and interview data is easy to explain. First, a significant percentage of abusive incidents that occur are not reported to the police. Second, some of the abusive incidents reported in the initial or six-month interview are relatively minor and do not legally qualify as crimes, i.e., there is an absence of probable cause or the act committed does not constitute a criminal offense. In some cases this may explain why the police were not called. A large percentage of the spouse abuse calls the police did receive during the period the experiment was being conducted were determined by the responding officers to be situations in which there was not sufficient probable cause to believe that a crime had been committed. It is not that police are unique in their perceptions. As much literature shows, many women do not report incidents because they themselves do not perceive them as crimes.

The data do not support the hypothesis that the treatments differ in deterring abuse. This is significant in that while complementing the results of similar studies conducted in Omaha¹³⁴ and Milwaukee¹³⁵ the results are not in agreement with the earlier Minneapolis study that found arrest to be a more effective deterrent than the other treatments.¹³⁶ The results of the Charlotte and Omaha studies suggest that there is not adequate support for a mandatory or presumptive arrest policy based on specific deterrence. The hope that arrest alone could contribute to the solution of this serious problem is unfulfilled.

Since there was reason to believe arrest to be a successful deterrent (based, for example, on the results of the Minneapolis experiment or the theory of "empowerment" of the victim), we can only speculate on why it was not so in this experiment. First, the majority (69.4%) of male offenders in our sample have previous criminal histories so arrest is neither a new nor an unusual experience. Many of them have already spent significant time in jail. Second, for many of the couples in this research abuse is chronic, rather than an occasional occurrence. For offenders who have criminal histories, or for those who are chronic abusers, it is unrealistic to think that arrest will have much impact. Third, arrest alone, which was the focal point of this research, may not constitute as strong a societal response as perceived. The fact is that "time in jail" is minimal. In

¹³⁴ DUNFORD ET AL., *supra* note 102.

¹³⁵ Sherman et al., *supra* note 105.

¹³⁶ Sherman & Berk, *supra* note 97.

this study the median time between the presenting incident and release from jail for arrested offenders was 9.4 hours with 27.5% of them spending four hours or less in jail.¹³⁷ Arrest with rapid release may simply not mean very much, particularly when the offenders have been arrested before. Fourth, although not technically part of the scope of this project, some information was gathered on the processing of offenders through the criminal justice system. Our data confirm the belief that it is very rare for a spouse abuser to be found guilty and to be ordered to spend any significant time in jail. In only 35.5% of the cases in which the citation or arrest responses were delivered was the offender prosecuted, and in less than one percent of the cases did the offender spend time in jail beyond the initial arrest. Fifth, as jails become more crowded, and as the public learns that even felons are receiving community based punishments and early releases from correctional institutions, it does not take much imagination to conclude that premium jail space will not be used on misdemeanor spouse abusers.

We conclude that arrest is not a significant deterrent for *misdemeanor* spouse assault. We have no way of knowing if arrest would be more of a deterrent for felony spouse assault, or for lower levels of abuse which do not now satisfy the criteria for misdemeanor arrest. As other studies report their findings, and as analyses are continued on data from the multiple experimental studies, there is little doubt that statistical associations will be found between arrest and other variables. Given the hundreds of variables investigated and the thousands of individuals represented in the six studies funded by the National Institute of Justice, there is a very high probability that specific relationships between arrest and other variables will be identified. It remains for cross-site analyses to be conducted to confirm or call into question specific associations in order to avoid premature conclusions about the effect or lack of effect of arrest on deterring spouse abuse.

Questions concerning the appropriate societal response to spouse assault and the role of the police in this response are not answered by this research. There is little doubt that misdemeanor spouse assault has been added to the list of actions that subordinate family privacy considerations to the greater public interest in reducing this kind of behavior. Further, there is no doubt that the police will continue to be involved in spouse abuse situations since they are the only agency available in all areas at all hours of the day and night. Defining spouse assault as criminal is a requisite step in

¹³⁷ HIRSCHEL ET AL., *supra* note 115, at 151.

strengthening the social norms. It places the police in a lynch pin role, connecting the offender and victim with other social, community and criminal justice resources through arrest or referral.

Despite the failure of arrest to have a demonstrated deterrent effect, and despite the inadequacies of the present criminal justice system, arrest may still constitute a viable and appropriate response for the police to pursue in many spouse abuse situations. Even though arrest has not been shown to have a particular deterrent value, and even if arrest may not have much of a punitive value, it may still constitute a more conscionable choice than non-arrest. Not to arrest may communicate to men that abuse is not serious and to women the message that they are on their own. It may communicate to children, who very often witness abuse of their mothers, that the abuse of women is tolerated, if not legitimated. It may communicate to the public at large that a level of violence which is unacceptable when inflicted by a stranger is acceptable when inflicted by an intimate. It is imperative that we recognize the seriousness of spouse abuse and employ measures, however imperfect, to reduce it, even if we do not yet know how to achieve a dramatic reduction in its occurrence.