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The Deterrent Effect of Arrest in Domestic Violence: Differentiating Between Victim and Perpetrator Response

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I. INTRODUCTION

The policies concerning arrest in domestic violence cases have changed markedly over the last fifteen years. In the 1970s, the general policy of police intervention in domestic disputes was to avoid arrest as much as possible, to calm down the parties and to mediate the conflict.¹ Today, all U.S. states allow warrantless arrests in domestic misdemeanors² and several states mandate arrest. Even in states with no mandatory arrest policy, many police departments prefer to arrest in domestic violence cases. In addition, many states require their police departments to state their policies concerning response to domestic violence.³ These changes in policy are the result of several factors. First, feminist groups have made the issue of domestic violence visible. In addition, there have been numerous highly publicized lawsuits against police departments for negligent police action in domestic violence cases.⁴ However, the Supreme Court decision

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1. See, e.g., WAYNE R. LAFAVE, ARREST: THE DECISION TO TAKE A SUSPECT INTO CUSTODY 50, 121 (1965) (describing the practices in the 1960s); see also Joan Zorza, *The Criminal Law of Misdemeanor Domestic Violence, 1970-1990*, 83 J. CRIM. L. & CRIMINOLOGY 46, 48 (1992) (quoting the police training materials of the seventies); DEL MARTIN, BATTERED WIVES 94 (1981).

2. See Cheryl Hanna, *No Right to Choose: Mandated Victim Participation in Domestic Violence Prosecutions*, 109 HARV. L. REV. 1850, 1859 (1996).

3. See, e.g., WIS. STAT. § 968.075 (2000). Wisconsin, in addition to having a mandatory arrest law, requires a policy statement of all police departments. See *id.*

4. *Bruno v. Codd*, 396 N.Y.S.2d 974 (1977), a class action lawsuit against the New York police department and its officers, ended in settlements. The most influential case was *Thurman v. City of Torrington*, 595 F. Supp. 1521 (D. Conn. 1984), in which the jury

*DeShaney v. Winnebago County Department of Social Services*⁵ represents a setback for domestic violence victims seeking legal redress against police departments. Prior to the *DeShaney* decision, domestic violence victims could sue individual police officers for violating their substantive due process rights when the police failed to protect them. *DeShaney* seriously curtailed these rights. Because of the Court's decision, a domestic violence victim must fit into a very narrow class of exceptions in order to make out a substantive due process claim.⁶ There are still, however, several grounds on which a civil rights action for police negligence may be successful, especially if it is based on sex discrimination or a special relationship.

Change has also been brought about by empirical research. In 1984, Professor Sherman and his group conducted the first empirical and experimental study on the effects of arrest on domestic violence in Minneapolis.⁷ The results, which were widely published and had a permanent policy impact, showed that arrest deters recidivism.⁸

The authors, however, cautioned against hasty conclusions and called for more research.⁹ Replicate studies were conducted in the late 1980s in Omaha (NE),¹⁰ Milwaukee (WI),¹¹ Charlotte (NC),¹² Colorado Springs

rewarded the plaintiff over two million dollars in damages for flagrant negligence in police actions that failed to protect Mrs. Thurman from the violence of her ex-husband. See generally *Developments in the Law: Legal Responses to Domestic Violence*, 106 HARV. L. REV. 1498 (1993) [hereinafter *Developments*].

5. 489 U.S. 189 (1989).

6. See J. David Hirschel & Ira W. Hutchison, III., *Female Spouse Abuse and the Police Response: The Charlotte, North Carolina Experiment*, 83 J. CRIM. L. & CRIMINOLOGY 73, 85-86 (1992); Laura S. Harper, *Battered Women Suing Police for Failure to Intervene: Viable Legal Avenues After DeShaney v. Winnebago County Department of Social Services*, 75 CORNELL L. REV. 1393 (1990). See generally *Developments*, *supra* note 4, at 1500.

7. See Lawrence W. Sherman & Richard A. Berk, *The Minneapolis Domestic Violence Experiment*, 1 POLICE FOUND. REP. 1 (1984) [hereinafter Sherman & Berk, *Minneapolis Domestic Violence*]; see also Lawrence W. Sherman & Richard A. Berk, *The Specific Deterrent Effects of Arrest for Domestic Assault*, 49 AM. SOC. REV. 261 (1984) [hereinafter Sherman & Berk, *Specific Deterrent Effects*]; LAWRENCE W. SHERMAN, *POLICING DOMESTIC VIOLENCE: EXPERIMENTS AND DILEMMAS* (1992) [hereinafter SHERMAN, *POLICING DOMESTIC VIOLENCE*].

8. Because the study became public the same year the influential Thurman case was decided, its impact is difficult to assess. The change in arrest policies of big police departments in the mid-eighties was documented in ELLEN G. COHN & LAWRENCE W. SHERMAN, *POLICE POLICY ON DOMESTIC VIOLENCE, 1986: A NATIONAL SURVEY* (1987). See generally *Thurman*, 595 F. Supp. 1521.

9. See Sherman & Berk, *Minneapolis Domestic Violence*, *supra* note 7, at 1.

10. See generally Franklyn W. Dunford et al., *The Omaha Domestic Violence Police Experiment: Final Report to the National Institute of Justice, Technical Report*, INST. OF BEHAV. SCI., U. OF COLORADO (1989) [hereinafter Dunford et al., *Omaha Domestic Violence*]; Franklyn W. Dunford et al., *The Role of Arrest in Domestic Assault: The Omaha Police Experiment*, 28 CRIMINOLOGY 183 (1990) [hereinafter Dunford et al., *Role of Arrest*]; Franklyn W. Dunford, *The Measurement of Recidivism in Cases of Spouse Assault*, 83 J. CRIM. L. & CRIMINOLOGY 120 (1992) [hereinafter Dunford, *Measurement of Recidivism*].

11. See generally SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7. The research was originally reported in Lawrence W. Sherman & Douglas A. Smith, *Crime, Punishment*

(CO)¹³ and Miami Dade (FL).¹⁴ The results did not replicate those of the original Minneapolis study. The deterrent effects of arrest were much more ambiguous. Because the conclusions of these studies are still often invoked both in scholarly literature and everyday discussion with practitioners,¹⁵ their critical review is still timely.

Theoretically, these studies relied on theories of social control and deviance and therefore, focused on the effects of arrest on the suspect.¹⁶ According to the social control theory, those suspects who have the most to lose—a job, marriage or social respect—are the ones most sensitive to the deterrent effect of arrest.¹⁷ Sherman's study revealed empirical support for the theory concerning employment, but not marriage.¹⁸ The deviance or labeling theory, in contrast, suggests that arrest may bolster the labeling of deviant persons and thus increase future violence.

Reading these studies, I became concerned that they do not seriously consider the effects of arrest on victims' behavior. Contemporary criminological research addresses the role victims and other people play in reporting crime to the police.¹⁹ This factor should also be taken into consideration when analyzing reports of domestic violence. To put it simply, different methods of police intervention in domestic violence cases may have different effects on victims' willingness to call the police when

and Stake in Conformity: Legal and Extralegal Control of Domestic Violence, 57 AM. SOC. REV. 680 (1992). See also Lawrence W. Sherman et al., *From Initial Deterrence to Long-Term Escalation: The Effects of Short-Custody Arrest for Poverty Ghetto Domestic Violence*, 29 CRIMINOLOGY 821 (1991) [hereinafter Sherman et al., *Initial Deterrence*]; Lawrence W. Sherman & Janell D. Schmidt, *The Variable Effects of Arrest on Criminal Careers: The Milwaukee Domestic Violence Experiment*, 83 J. CRIM. L. & CRIMINOLOGY 137 (1992) [hereinafter Sherman & Schmidt, *Variable Effects of Arrest*].

12. See generally J. DAVID HIRSCHSEL ET AL., *CHARLOTTE SPOUSE ASSAULT REPLICATION PROJECT: FINAL REPORT FOR THE NATIONAL INSTITUTE OF JUSTICE* (1991) [hereinafter HIRSCHSEL ET AL., *CHARLOTTE SPOUSE ASSAULT*]; Hirschel & Hutchison, *supra* note 6, at 73.

13. See generally Richard A. Berk et al., *A Bayesian Analysis of the Colorado Springs Spouse Abuse Experiment*, 83 J. CRIM. L. & CRIMINOLOGY 170 (1992).

14. See generally Anthony M. Pate & Edwin E. Hamilton, *Formal and Informal Deterrents to Domestic Violence: The Dade County Spouse Assault Experiment*, 57 AM. SOC. REV. 691 (1992); Hubert Williams, *Spouse Abuse Research Raises New Questions about Police Response to Domestic Violence*, POLICE FOUND. REP. 5 (1992).

15. See, e.g., Linda G. Mills, *Intuition and Insight: A New Job Description for the Battered Woman's Prosecutor and Other More Modest Proposals*, 7 UCLA WOMEN'S L.J. 183, 189 (1997); Barbara Fedders, Note, *Lobbying for Mandatory-Arrest Policies: Race, Class, and the Politics of the Battered Women's Movement*, 23 N.Y.U. REV. L. & SOC. CHANGE 281, 291 (1997). In addition, judges, prosecutors and defense lawyers often referred to these empirical studies when the author of this Article made field observations and interviews in county courts in Madison and Milwaukee, Wisconsin in 1998.

16. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 160.

17. See Sherman & Smith, *supra* note 11, at 685. See generally Kirk R. Williams & Richard Hawkins, *The Meaning of Arrest for Wife Assault*, 27 CRIMINOLOGY 163 (1989).

18. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 181-84; Sherman & Smith, *supra* note 11, at 685-86; Williams, *supra* note 14, at 5.

19. See *infra* Section II.

they are repeatedly victimized.

The above mentioned empirical studies could help to support this hypothesis. In these experimental studies, domestic violence calls were responded to in three different ways. In the first group the police made an arrest, in the second group they gave a warning or advice to the parties and in the third group the method of intervention varied depending on the study. The effect of intervention was measured after six months by three factors: (1) victim interviews; (2) the number of subsequent arrests and (3) the number of calls or other reports to the police.

The authors of the empirical studies used the number of subsequent arrests or calls to the police, which I will refer to as the "official data," as the primary measure for repeat violence. I will argue that victim interviews reflect repeat violence more adequately than these official data. The official data reflect not only the level of violence but also the victim's willingness to report it. Consequently, after arrest, victim interviews may show less repeat violence than after other types of interventions, but the official data may show more. A likely explanation for this pattern is that arrest deters violence and encourages victims to call the police.

In this Article, the results of the empirical studies are reanalyzed, taking victim impact into account. In four out of six studies, the relationship between victim interview data and official data indicates that this hypothesis may be correct. Since the empirical studies do not consider the effects of arrest on the victim's willingness to report, the data are not easily analyzed from this vantagepoint. These methodological problems are discussed. Notwithstanding this, I argue that reexamining the studies shows that victim impact should be taken into account when researching the effects of arrest in domestic violence.

II. VICTIMS AS AGENTS AND REPORTING OF CRIME

A. VICTIMS AS AGENTS

The empirical research's failure to consider the impact on victims may be due to our conceptions of agency and victimization. Victims' reactions to police intervention may have been easily overlooked because the victim/agent dichotomy in domestic violence is particularly stigmatizing, stereotypical and gendered.²⁰ Even feminist theories of power and male domination, which have been helpful in explaining the undeniably gendered nature of domestic violence, sometimes obscure the agency of victims.²¹ For example, theories of learned helplessness and "battered wife

20. See generally Martha R. Mahoney, *Legal Images of Battered Women: Redefining the Issue of Separation*, 90 MICH. L. REV. 1 (1991).

21. See generally Martha Mahoney, *Women's Lives, Violence, and Agency*, in *THE PUBLIC NATURE OF PRIVATE VIOLENCE* (Martha Fineman & Roxanne Mykitiuk eds., 1994).

syndrome"²² have increased awareness of the serious psychological consequences of domestic violence, but at the same time have denied agency to victims. Recent empirical work has challenged the notion that victims of domestic violence are helpless, passive and incompetent.²³ Such research shows that battered women use a number of survival and protection tactics, ranging from choices of how and when to speak to decisions of leaving and calling the police.²⁴ The victim/agent dichotomy has also been challenged in recent theory²⁵ and has gradually given way to seeing the victim as capable of reasoned action.

Some authors have paid attention to the possible effect arrest may have on victims' behavior. Grant Bowman suggests that arrest may empower the victim by giving her time to arrange for safety.²⁶ It has also been argued that arrest affirms the victim's rights²⁷ or gives the victim a sign that there are forces to help her.²⁸ Also, arrest protects the victim from immediate danger and violence more effectively than other intervention. Therefore, we would expect arrest to encourage the victim to call the police in repeat victimization.²⁹ According to this hypothesis, victims whose perpetrators are not arrested experience police intervention as futile and are

22. See, e.g., LENORE WALKER, *THE BATTERED WOMAN* (1979); LENORE WALKER, *THE BATTERED WOMAN SYNDROME* (1984).

23. See, e.g., LINDA GORDON, *HEROES OF THEIR OWN LIVES: THE POLITICS AND HISTORY OF FAMILY VIOLENCE* (1989).

24. See Karla Fischer et al., *The Culture of Battering and the Role of Mediation in Domestic Violence Cases*, 46 SMU L. REV. 2117, 2136 (1993).

25. See Mahoney, *supra* note 20. See generally EDWARD W. GONDOLF & ELLEN R. FISCHER, *BATTERED WOMEN AS SURVIVORS: AN ALTERNATIVE TO TREATING LEARNED HELPLESSNESS* (1988). This critique is related to the feminist philosophy of subjectivity that challenges the notion of isolated, autonomous and gendered subjectivity in Western philosophy. See SEYLA BENHABIB, *SITUATING THE SELF: GENDER, COMMUNITY AND POSTMODERNISM IN CONTEMPORARY ETHICS* 142 (1992). See generally NGAIRE NAFFINE, *LAW AND THE SEXES: EXPLORATIONS IN FEMINIST JURISPRUDENCE* (1990). For a psychoanalytical point of view, see generally Robin West, *Jurisprudence and Gender*, 55 U. CHI. L. REV. 1 (1988).

26. See Cynthia Grant Bowman, *The Arrest Experiments: A Feminist Critique*, 83 J. CRIM. L. & CRIMINOLOGY 201, 204 (1992); SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 278.

27. See Bowman, *supra* note 26, at 204.

28. See Marion Wanless, Note, *Mandatory Arrest: A Step Toward Eradicating Domestic Violence, But is it Enough?*, 1996 U. ILL. L. REV. 533, 549 (1996).

29. See Lisa G. Lerman, *The Decontextualization of Domestic Violence*, 83 J. CRIM. L. & CRIMINOLOGY 217, 233 (1992). This is consistent with the information crime surveys provide on why crime is not reported to the police. Some reasons given by victims reflect whether they experienced the crime as a minor incident or private matter. Often, however, the answers reflect how victims expect the police will or can act to protect their interests. See MICHAEL J. HINDELANG, *CRIMINAL VICTIMIZATION IN EIGHT AMERICAN CITIES: A DESCRIPTIVE ANALYSIS OF COMMON THEFT AND ASSAULT* 390 (1976); JAN VAN DIJK ET AL., *EXPERIENCES OF CRIME ACROSS THE WORLD: KEY FINDINGS FROM THE 1989 INTERNATIONAL CRIME SURVEY* 69 (1991); KEITH A. BOTTOMLEY, *CRIMINOLOGY IN FOCUS: PAST TRENDS AND FUTURE PROSPECTS* 27 (1979). See generally Leslie W. Kennedy, *Going it Alone: Unreported Crime and Individual Self-Help*, 16 J. CRIM. JUST. 403 (1988).

less likely to contact the police in the future.

On the other hand, it has been questioned whether arrest has an empowering effect. The victim may experience arrest more as an authoritarian outside intervention than her own empowering choice.³⁰ Furthermore, arrest may discourage the victim from calling the police if the arrest hurts her economically by, for example, causing her partner to lose his job or a week's salary, or by leading to divorce and a subsequent loss of lifestyle.³¹ It has even been suggested that arrest, rather than decreasing violence, increases the quantity or severity of attacks,³² making the victim less likely to call the police.

The hypothesis put forward in this Article rests on the assumption that arrest empowers and protects victims, at least in the short run, and therefore, encourages calling the police in cases of repeat victimization. Miller and Krull have found that repeat victimization varies in different victim groups.³³ Similarly, changes in willingness to call the police probably vary between different victim groups, depending on what other resources are available to the victim, how dependent the victim is on the perpetrator and the victim's social status and integration. The hypothesis here is based on the assumption that victims call the police only when under real and serious threat. Therefore, I expect the overall willingness of victims to call the police to increase as a consequence of an arrest.

The empirical studies give an opportunity to test this hypothesis. In reality, the victim's reaction is based on what she expects the police to do, not only on what the police actually do. The victim's expectations of what the police will do can be based on a number of factors other than her own experiences of police intervention. In the empirical studies, however, the initial intervention, that is arrest, warning or separation, was randomly assigned. In all other respects, the groups were treated the same. Thus, differences in victim response should result from the different methods of intervention.

An additional problem is that not only victims themselves call the police. According to empirical studies, the percentage of calls made by those other than the victim vary from one-third to two-thirds.³⁴ Many of these calls are made by children, yet other family members and neighbors

30. See Wanless, *supra* note 28, at 548.

31. See Bowman, *supra* note 26, at 204.

32. See Wanless, *supra* note 28, at 549.

33. See JoAnn Miller & Amy C. Krull, *Controlling Domestic Violence: Victim Resources and Police Intervention*, in *OUT OF THE DARKNESS: CONTEMPORARY PERSPECTIVES ON FAMILY VIOLENCE* 236 (Glenda Kaufman Kantor & Jana L. Jasinski eds., 1997).

34. In Milwaukee, 55% of the calls to the police were made by the victim, 42% in Omaha and 34% in Colorado Springs. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 326; see also Miller & Krull, *supra* note 33, at 242. Another study reports that three-quarters of the calls reporting domestic violence were made by the victim. See PATRICK A. LANGAN & CHRISTOPHER A. INNES, *PREVENTING DOMESTIC VIOLENCE AGAINST WOMEN* 4 (1986).

also make them. Thus, the term "victim response" includes calls from other people. One could expect that other people are more likely to call the police in cases of repeat violence if they are satisfied with the effects of the initial intervention. In particular, family members are probably motivated by similar concerns as victims, and therefore equally sensitive to changes in police action.

B. REPORTING OF CRIME

We have relatively little empirical information on crime victims' reporting patterns. However, the little we have suggests that a sensitivity to changes in reporting is essential in measuring the overall amount of crime. There have also been remarkable changes in reporting domestic crime to the police during the past twenty years, which shows that changes in policies can affect victims' reporting patterns. It would make sense that the experience of each domestic violence victim is important.

Crime statistics and victim surveys show that only a fraction of crime is ever reported to the police. The relationship between "real" crime and officially reported crime has proven to be complicated.³⁵ Institutional theories assume that, instead of reflecting a "real" crime rate, official crime statistics reflect how law enforcement institutions operate and define deviant behavior as crime.³⁶ Studies on the relationship between crime reported to the police and crime reported in surveys show that this relationship varies considerably between different crimes. For example, serious violent crime and car theft are reported to the police more often than petty crimes and crimes among intimates.³⁷ Domestic assault is probably one of the most under-reported crimes both in crime statistics and in crime surveys.³⁸

35. The shift from realist theories developed by Adolphe Quetelet, which assume a constant relationship between "real" crime and reported crime, to institutional theories is described in KEITH A. BOTTOMLEY & CLIVE COLEMAN, *UNDERSTANDING CRIME RATES* 6-15 (1981). A fuller account of the history of measurement of crime is found in THORSTEN SELLIN, *MEASUREMENT OF DELINQUENCY* 9 (1964). Illustrative estimates about the relationship between officially recorded crime and hidden crime are still frequently made. The estimated level of reported crime usually varies between one-third and two-thirds. See STEVEN BOX, *DEVIANCE, REALITY AND SOCIETY* 27 (1981) (estimating two-thirds); *THE PROBLEM OF CRIME: CRIME, ORDER AND SOCIAL CONTROL* 22 (John Muncie & Eugene McLaughlin eds., 1996) (estimating one-third).

36. See generally John Kitsuse & Aaron Cicourel, *A Note on the Uses of Official Statistics*, 11 *SOC. PROB.* 131 (1963). See also AARON CICOUREL, *THE SOCIAL ORGANIZATION OF JUVENILE JUSTICE* 24 (1976). Several institutional studies have argued that law enforcement responds in such a way that statistics emphasize crime in the working and lower classes. See BOX, *supra* note 35, at 5; *THE PROBLEM OF CRIME*, *supra* note 35, at 28; Robert J. Sampson, *Effects of Socioeconomic Context on Official Reaction to Juvenile Delinquency*, 51 *AM. SOC. REV.* 884 (1986).

37. See HINDELANG, *supra* note 29, at 358; ROBERT M. O'BRIEN, *CRIME AND VICTIMIZATION DATA: LAW AND CRIMINAL JUSTICE SERIES*, VOL. 42, 85 (1985); VAN DIJK ET AL., *supra* note 29, at 55.

38. Many studies on female victimization have discovered a remarkable amount of

The relationship between crime reported in victim surveys and police data has changed over time. While the Uniform Crime Reports showed a steady increase in reported crime in the 1980s, no equivalent trend was manifested in the National Crime Surveys. This change was most notable concerning rape and aggravated assault.³⁹ Changing policies in the handling of sexual and domestic crime have impacted the attitudes of the police, leading to an increase in the recording of these offenses. Also, victims' willingness to report cases of domestic assault and sexual crimes to the police has increased.⁴⁰

In the experimental studies discussed in this Article, the police were instructed to record all contacts from the victims. Therefore, the differences in official data should not reflect different recording patterns, but a difference either in actual violence or in victims' reporting patterns.

However, victim interviews are not without drawbacks in measuring "real" crime. Victims do not report all victimization in surveys, especially if the perpetrator is known to them,⁴¹ and they did not report all violence in the victim interviews in the empirical studies.⁴² Nonetheless, victim interviews are still the most reliable source to gauge the effect of police intervention.⁴³ In the empirical studies, victim interviews revealed more incidents of repeat violence than the official data. This simple fact is reason enough to consider victim interviews more reliable in measuring repeat violence than the official data.

unreported domestic crime. See Elizabeth Stanko, *Hidden Violence Against Women*, in VICTIMS OF CRIME: A NEW DEAL 40 (Mike Maguire & John Pointing eds., 1988); Mike Maguire, *Crime Statistics, Pattern and Trends*, in THE OXFORD HANDBOOK OF CRIMINOLOGY 168 (1994) (giving an overview); O'BRIEN, *supra* note 37, at 102; BOTTOMLEY & COLEMAN, *supra* note 35, at 67. See generally DIANA E. H. RUSSELL, RAPE IN MARRIAGE (1990).

39. See Robert M. O'Brien, *Comparing Detrended UCR and NCS Crime Rates Over Time: 1973-1986*, 18 J. CRIM. JUST. 229, 233-35 (1990).

40. The number of domestic violence calls has soared since the introduction of mandatory arrest policies and other changes concerning domestic violence. Wanless reports that the number of domestic calls almost doubled during the first four or five years of mandatory arrest in New Jersey and Iowa. See Wanless, *supra* note 28, at 550. In Milwaukee, police reports of domestic violence almost doubled after the mandatory arrest law came into force in 1989. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 119. For other studies showing that a change in the response of the criminal justice system produces a change in the number of crimes reported to the police, see Maguire, *supra* note 38, at 167.

41. See Wesley G. Skogan, *Issues in the Measurement of Victimization*, in U.S. DEP'T OF JUST., BJS REP. 16 (1981); see also O'BRIEN, *supra* note 37, at 49.

42. See Richard A. Berk et al., *The Deterrence Effect of Arrest in Incidents of Domestic Violence: A Bayesian Analysis of Four Field Experiments*, 57 AM. SOC. REV. 698, 700 (1992) [hereinafter Berk et al., *Deterrent Effect*]; see also Richard A. Berk et al., *Colorado Springs Spouse Assault Replication Project Final Report* 129 (1991) (unpublished manuscript) [hereinafter Berk et al., *Colorado Springs Spouse Assault*].

43. See Miller & Krull, *supra* note 33.

III. AN OVERVIEW OF THE STUDIES

The six studies⁴⁴ discussed in this Article present an exceptional and outstanding example of empirical criminological research. Despite the same basic methodological design, the detailed methodological choices of the studies differ to some extent.⁴⁵ The samples, though not always selected in the same way, give a consistent picture of the domestic violence police encounter. Both victims and suspects had a higher rate of unemployment than the general population of the area,⁴⁶ African-Americans were over-represented⁴⁷ and many suspects had previous criminal records.⁴⁸ Marriage rate varied in the different samples,⁴⁹ but most cases included couples living together at the time of the incident.⁵⁰ Alcohol consumption was prevalent, more so among suspects than victims.⁵¹

Only misdemeanor assaults, and in some studies other misdemeanors, were included in the studies so that the police could use discretion in making arrests.⁵² Felonies were excluded from the studies because police have no discretion in these matters. In most of the studies, cases in which

44. See *supra* notes 7, 10-14.

45. See Joel Garner et al., *Published Findings from the Spouse Assault Replication Program: A Critical Review*, 11 J. QUANTITATIVE CRIMINOLOGY 3, 9 (1995).

46. In the Minneapolis study, for example, 60% of both victims and suspects were unemployed, while the unemployment rate of the city was 5%. See Sherman & Berk, *Specific Deterrent Effects*, *supra* note 7, at 263. In Milwaukee, 47% of the suspects were unemployed, whereas in Charlotte the unemployment rate was estimated to be between 22% to 30%. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 141; MILLER & KRULL, *supra* note 33, at 237.

47. In the Milwaukee study, 75% of the suspects were African-American. In the Charlotte study, African-Americans made up 70% of the suspects. In other samples, the proportion was 31 to 43%. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 141. In the Minneapolis study, 57% of the victims and 45% of the suspects were white. See Sherman & Berk, *Specific Deterrent Effects*, *supra* note 7, at 263.

48. The Minneapolis and Omaha studies give previous arrest rates of 57%, see Sherman & Berk, *Specific Deterrent Effects*, *supra* note 7, at 263, and 65%, see Dunford et al., *Omaha Domestic Violence*, *supra* note 10, at 31, respectively. In Charlotte, 70% had a criminal record. See HIRSCHER ET AL., *CHARLOTTE SPOUSE ASSAULT*, *supra* note 12, at 37. Previous arrests for domestic violence, however, were less common. See *id.*

49. Milwaukee had the lowest marriage rate at 30%, and Miami Dade and Colorado Springs the highest at 79% and 69%, respectively. Others had rates of 35 to 50%. See Sherman, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 141.

50. In Milwaukee, for example, 90% were cohabiting at that time. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 344.

51. In Dade County, police officers suspected that 70% of suspects used drugs or alcohol. See Pate & Hamilton, *supra* note 14, at 693. In Milwaukee, 42% of suspects were intoxicated. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 344. In Omaha, 70% of suspects and 30% of victims were under the influence of either alcohol or drugs. See Dunford et al., *Omaha Domestic Violence*, *supra* note 10, at 6. In Charlotte, more than half of the suspects and one-third of the victims were intoxicated. See HIRSCHER ET AL., *CHARLOTTE SPOUSE ASSAULT*, *supra* note 12, at 35-36.

52. See Dunford et al., *Omaha Domestic Violence*, *supra* note 10, at 29. Harassment was included in the Colorado Springs experiment, forming the majority of the crimes, and menacing was included in the Omaha study. See *id.*

the victim demanded an arrest were also excluded. Moreover, the victim and the suspect had to be present at the scene when the police arrived to make the arrest feasible.⁵³

Some studies analyzed intervention-as-assigned⁵⁴ and others analyzed intervention-as-delivered. In Minneapolis, Charlotte and Colorado Springs, intervention methods could not always be delivered as assigned. If intervention-as-assigned could not be delivered, it usually meant that arrest was used in cases assigned to informal intervention.⁵⁵ It may be assumed that in those cases either the suspect's behavior toward the police—threat, assault, etc.—warranted arrest, or the police deemed the initial crime or the actual threat toward the victim as serious enough to warrant an arrest. Since the incidents in arrest groups were probably more serious than in others, we would expect increased recidivism with arrests if it were measured on intervention-as-delivered.

While these differences in research design make comparisons of prevalence or frequency among the research sites practically impossible, they negligibly impact my reexamination of the studies. In this Article, I compare the victim interview data with the official data in each study, using the victim interview data to measure a change in the level of violence and the official data to measure a possible change in the level of reporting. As in the original studies, relative differences between arrest groups and non-arrest groups are compared, not the actual levels of violence. If, for example, victim interviews reveal less violence but the official data indicate more calls to the police when the suspect was arrested as compared with no arrest, the study suggests that arrest deters violence and encourages the victim to call the police. For the purpose of this analysis, the relationship between victim data and official data is crucial, not the actual levels of violence.

The studies provide information about changes in violence and in victims' reporting behavior at three different points after the initial incident. First, some of the studies provide information about the effect of arrest on victims' safety immediately after the incident. Second, all of the studies provide victim interview data and official data about subsequent incidences of violence six months after the initial incident. Third, some of the studies report the official measures of recidivism after one year or more.

The official data on repeat violence includes both the number of calls

53. See Sherman & Berk, *Specific Deterrent Effects*, *supra* note 7, at 262-63.

54. In the Milwaukee and Omaha studies, intervention-as-assigned and intervention-as-delivered was almost identical, whereas in the Colorado Springs and Charlotte studies there were differences. See Dunford et al., *Omaha Domestic Violence*, *supra* note 10, at 11.

55. For methodological differences, see generally Patrick R. Gartin, *Dealing with Design Failures in Randomized Field Experiments: Analytic Issues Regarding the Evaluation of Treatment Effects*, 32 J. RES. IN CRIME & DELINQ. 432 (1995).

or other reports to the police and the number of subsequent arrests. The reports indicate the actual level of reporting of domestic crime. These data are sensitive to possible changes in patterns of reporting.⁵⁶ Therefore, in this Article, the number of hotline calls or police reports is used as a primary measure of victims' reporting to the police.

The number of arrests provides information about the official response to repeat violence. As such, it is dependent on victims' responses. Yet, I would argue, the number of arrests is not as sensitive to changes in victim responses as police reports, since arrest is likely to occur in serious cases⁵⁷ that may lead to contact with the police regardless of victims' earlier experiences with the police.

IV. TRACES OF VICTIM IMPACT IN THE EMPIRICAL STUDIES

A. SHORT-TERM DETERRENT EFFECT OF ARREST

Initial victim interviews were conducted approximately one month after the initial incident took place. According to these first interviews, repeat violence in the arrest group was lower than in other groups. Arrest showed a deterrent effect and protected victims.

In Omaha, Milwaukee and Minneapolis, the victims were asked whether police action stopped quarrels or fights. In Omaha, 77% of victims assigned to the mediation group, 87% of the separation group and 93% of the arrest group answered yes.⁵⁸ In Milwaukee, victims reported that they were attacked immediately after the police left in 7% of cases in which a warning was given. If the suspect was arrested, he attacked the victim when they met again in 1.7% of the full-arrest cases and 2.2% of the short-arrest cases.⁵⁹ The small number of cases in Minneapolis makes comparison difficult. However, the incidence of new violence within twenty-four hours was very low, 1.4%, in arrest and advice groups, and higher, 5.6%, in the group in which the parties had been separated. The prevalence of a new quarrel in the separated group was almost four times higher, 11%, than in the arrest group, 3%.⁶⁰ This result is not surprising, since there is no guarantee that the suspect will stay away for the required

56. See generally Lerman, *supra* note 29.

57. Of course, the situation is different if a mandatory arrest policy is rigorously applied, but such a policy was not followed in the experimental studies.

58. See Dunford et al., *Omaha Domestic Violence*, *supra* note 10, at 22-24. That arrest did not always stop violence is probably explained by analysis based on intervention-as-assigned (not intervention-as-delivered). Ninety-five percent of suspects in the arrest group actually went to jail, and 20% were released within two hours, making continuation of the conflict possible in the arrest group as well. See *id.*

59. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 350.

60. See Sherman & Berk, *Specific Deterrent Effects*, *supra* note 7, at 268.

time limit.⁶¹

The deterrent effect generally prevailed for a month up to the initial interview. In Milwaukee and Metro Dade, those not arrested were much more likely to repeat than those arrested.⁶² In Charlotte, however, recidivism in all three groups was about the same.⁶³

Only the Milwaukee study reports official data up to the initial interviews. No statistically significant differences between the groups in the rate of arrests or hotline calls from the same victim were found.⁶⁴ Thus, lesser violence to the victims in the arrest group produced reports to the police at the same level as in other groups.

B. AFTER SIX MONTHS

1. Omaha: Victims and Official Data Tell a Different Story

In the Omaha experiment, the cases were assigned to three different types of intervention: arrest, separation and mediation. Detention times following arrest varied from the very short, less than two hours, to longer, with an average of sixteen hours. Separation was achieved by persuading either the victim or the suspect to leave, with the average length of separation being three days. Advice to leave the scene was given as part of mediation and was followed in some cases.⁶⁵ Thus, the mediation and separation groups were not necessarily distinct. Prosecution followed arrest and almost two-thirds of those perpetrators received a sentence.⁶⁶

The victim interviews showed the deterrent effect of arrest. The prevalence of repeat violence was 27% in the arrest group, compared with 30 to 32% in the two other groups. Serious repeat violence leading to injury was observed in 15% of arrest cases and in 20 to 21% of others.⁶⁷

The official data showed about the same amount of recidivism in arrest and separation groups but less in the mediation group. When mediation and separation were combined into one group, referred to as informal intervention, the rate of repeat arrests was almost the same after arrest, 12%, and informal intervention, 10%. The frequency of complaints was

61. Required separation was eight hours in the Minneapolis study. According to this study, the reunion in the separation group took place within one day in 57% of the separation cases. *See id.*

62. *See* SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 351; *see also* Williams, *supra* note 14, at 2.

63. *See* HIRSCHER ET AL., *CHARLOTTE SPOUSE ASSAULT*, *supra* note 12, at 121-22.

64. *See* SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 351.

65. *See* Dunford et al., *Role of Arrest*, *supra* note 10, at 190; *see also* Dunford et al., *Omaha Domestic Violence*, *supra* note 10, at 22.

66. *See* Dunford et al., *Omaha Domestic Violence*, *supra* note 10, at 31.

67. *See* Dunford et al., *Role of Arrest*, *supra* note 10, at 197; Dunford et al., *Omaha Domestic Violence*, *supra* note 10, at 35. The percentages include cases in which interview data is missing. Actually, 37 to 44% of interviewed victims reported repeat violence. *See id.*

higher in the arrest group.⁶⁸ These results are consistent with the hypothesis that victims in the arrest group experienced less repeat violence than those in the informal intervention group, but were more likely to call the police in cases of repeat violence.

2. Different Stories Repeated in Charlotte

The Charlotte study emphasized subsequent prosecution of cases. Therefore, besides arrest and advice/separation, citation was also an alternative method of intervention. In the citation group, the suspect received an order to appear in court to answer charges at a later date.⁶⁹ For the arrest group, the length of detainment varied, with the median being nine hours. Because all arrests were included in the same intervention group, no conclusions about the effect of the length of detainment can be made. Both arrests and citations were followed by prosecutorial decision-making. About two-thirds of cases in each group were dismissed without further proceedings. Beyond that, the indictment was sometimes deferred. About one-fifth of the suspects were found guilty in each group. A sentence was usually suspended, and only four suspects spent some time in jail in addition to the initial arrest.⁷⁰

In all groups, but particularly in the advice/separation group, the police used traditional mediation techniques, such as discussing family problems, advising marriage counseling and asking the suspect or the victim to leave.⁷¹ Data from the advice and separation groups were combined, and in the advice/separation group the police advised the suspect or the victim to leave the scene.⁷²

The sample was collected with a careful disqualification procedure. This resulted in fewer than half of the domestic cases being accepted into the sample. For example, the victims were asked whether they wanted an arrest, and if so, the case was excluded. Furthermore, cases in which the safety of the victim required arrest were excluded.⁷³

The victim interviews conducted six months after the alternative intervention showed no significant difference between the groups. There was more repeat violence in the citation group, 65%, but no difference between the arrest and separation/advice groups, 59% and 60%, respectively.⁷⁴ The official measure of recidivism was the number of

68. *See* Dunford et al., *Role of Arrest*, *supra* note 10, at 200. Rates of new complaints were almost the same, 17% and 16%, respectively. *See id.*

69. *See* HIRSCHER ET AL., *CHARLOTTE SPOUSE ASSAULT*, *supra* note 12, at 12.

70. *See id.* at 147-52.

71. *See id.* at 17, 62-75; Joan Zorza, *Must We Stop Arresting Batterers?: Analysis and Policy Implications of New Police Domestic Violence Studies*, 28 *NEW ENG. L. REV.* 929, 952 (1994).

72. *See* SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 131.

73. *See* HIRSCHER ET AL., *CHARLOTTE SPOUSE ASSAULT*, *supra* note 12, at 25.

74. *See* Hirschel & Hutchison, *supra* note 6, at 111; HIRSCHER ET AL., *CHARLOTTE*

subsequent arrests. Subsequent arrests were lower in the advice/separation group than in the arrest and citation groups. Arrests involving both the same victim and any victim were counted, and the result was the same.⁷⁵

The results support my hypothesis. If we compare arrest group with the advice/separation group, victim interviews indicated that recidivism occurred at the same level in both groups, but it was more often reported to the police in the arrest group. In the citation group, victims reported more violence in the interviews, and the rate of arrests was higher than in the advice/separation group. It is possible that not only arrest but also other legal intervention from the criminal justice system may encourage the victim to call the police when repeatedly victimized.

This result is confirmed by comparing those cases where interviewed victims report recidivism. A new arrest was made in 34% of the cases in which arrest was the first intervention. Only 31% of the citation group repeat offenders and 25% of the advice group repeat offenders were arrested.⁷⁶ It is possible that the police intervention was different because the subsequent act of violence was more or less dangerous or because the police attitude was affected by the previous arrest/citation. However, the study reports no such difference and, therefore, the possibility that this result reflects a difference in victim response should not be dismissed. Victims who have experienced one arrest are more likely to believe that a call to the police will result in arrest, and thus more likely to make this call.

SPOUSE ASSAULT, *supra* note 12, at 130. The percentages in the text represent those cases in which interviews were completed. The researchers repeated the analyses using different measures of repeat violence, with the same result: no significant difference between the groups. Insignificant differences varied using different measures. The interpretation of these numbers is strained by three problems. First, a smaller portion of victims were interviewed in the advice/separation group than in other groups, either because they disappeared or were impaired due to drugs or alcohol. As Zorza points out, some domestic abuse victims turn to drugs or alcohol to cope with the abuse. See Zorza, *supra* note 71, at 952. Therefore, it is possible that the disappeared cases are repeat violence cases and the actual amount of violence in the advice/separation group is higher. See HIRSCHTEL ET AL., CHARLOTTE SPOUSE ASSAULT, *supra* note 12, at 85, 130; Hirschtel & Hutchison, *supra* note 6, at 110 n.128. Secondly, the cases that were assigned for arrest were different from the others. There was more alcohol use, more injuries caused to victims and more previous criminality. While the researchers maintain that these differences are not significant, see HIRSCHTEL ET AL., CHARLOTTE SPOUSE ASSAULT, *supra* note 12, at 34-40, they all point in the same direction—more serious cases. Thirdly, analyses were made based on intervention-as-assigned, even though several cases assigned to advice/separation and citations were treated with arrest. See *id.* at 58. If actual arrest had some deterrent effect, some of that effect would deter cases in the two other groups. All these cautions suggest that the “almost no-deterrence result” of this study may be due partly to research design.

75. See HIRSCHTEL ET AL., CHARLOTTE SPOUSE ASSAULT, *supra* note 12, at 115; Hirschtel & Hutchison, *supra* note 6, at 109.

76. See HIRSCHTEL ET AL., CHARLOTTE SPOUSE ASSAULT, *supra* note 12, at 141-42 (Table 11-2).

3. Different Stories Repeated in Miami Dade

The Miami experiment provides the most convincing support for the deterrent effect of arrest. In this experiment, cases were assigned to four groups: (1) arrest and counseling; (2) arrest; (3) advice and counseling and (4) advice alone by the police. Since counseling in the first and third groups did not appear to impact the effect of intervention, results were presented as either arrest or non-arrest interventions. The duration of detention varied, with an average time of fifteen hours.⁷⁷

According to victim interviews, the prevalence of repeat violence was lower in the arrest group, 15%, than in the non-arrest group, 27%. In addition, the number of assaults was smaller in the arrest group.⁷⁸ Official domestic violence continuation reports⁷⁹ indicated almost the same level of repeat violence for each group. Arrests were made more often in the arrest group, 3.8%, than in the non-arrest group, 1.1%. Again, this is consistent with my hypothesis.

4. Colorado Springs: The Victims' Stories are Not Taken Seriously

The Colorado Springs experiment differs from the other studies in two ways. First, a quarter of the suspects overall, or one-third of employed suspects, were employed by the military, where domestic violence may be more common.⁸⁰ A criminal record can be particularly harmful for somebody in the military, and it is therefore possible that victims in this sample may be more likely to “hide” repeat violence regardless of intervention type. Secondly, in addition to assaults, the study also included menacing and harassment.⁸¹ Methods of intervention were: (1) arrest and emergency protection order; (2) emergency protection order and immediate counseling for the suspect; (3) emergency protection order alone and (4) restoring order at the scene.⁸²

The victim interviews showed deterrent effect for both the arrest group and the emergency protection order only group. Victims reported a lower prevalence of repeat violence in the arrest group.⁸³ The victim interview data, however, was dismissed by the authors. They stated, “In brief, we are

77. See Pate & Hamilton, *supra* note 14, at 693.

78. See Williams, *supra* note 14, at 4.

79. Prepared in the follow-up by specialized police officers, the continuation reports contain information about continued violence reported to the police after the incident. Though content and source of information is not clearly explained in the published report, it presumably contains information about all repeat incidents that have in some way been reported to the police. See *id.* at 3-5.

80. See Zorza, *supra* note 71, at 957-58.

81. See Berk et al., *Deterrent Effect*, *supra* note 42, at 700.

82. See Berk et al., *supra* note 13, at 174. There was a remarkable crossover: In 18% of the cases, intervention was not delivered as assigned. Most often, counseling was upgraded to other treatments. See *id.* at 177-78.

83. See Berk et al., *Colorado Springs Spouse Assault*, *supra* note 42, at 116-18.

not taking the apparent intervention effect seriously."⁸⁴

The official data of repeat arrest rate was about the same in all cases.⁸⁵ The emergency protection order, with or without counseling, produced a slightly lower arrest rate than other methods of intervention, and arrest produced a slightly lower repeat arrest rate than restoring order at the scene.⁸⁶ Colorado Springs data is consistent with my hypothesis. The victim interviews reveal less violence in the arrest group, while the official data yields almost the same rate for all methods of intervention.

5. Minneapolis and Milwaukee Studies

The original Minneapolis study and the Milwaukee study, on the other hand, do not support my hypothesis. In Minneapolis, the results speak persuasively for the deterrent effect of arrest. Cases were randomly assigned to three groups. In one group the suspect was arrested, in another the suspect was sent away from the scene for eight hours and in the third parties were counseled. Recidivism in the arrest group, measured by both victim interviews and official data, was about half of what it was in the other two groups for the following six months.⁸⁷

The deterrent effect was found despite background factors such as employment, criminal history or time in jail.⁸⁸ The results are even more convincing when the fact that several of the cases from the separation and advice groups were moved to the arrest group is taken into account.⁸⁹ Because the removed cases were more serious or included offenses other than domestic crimes, such as police resistance, one would expect this shift to increase recidivism in the arrest group. Nonetheless, the arrest group still scored lower in repeat offenses.

While the Minneapolis study does not support my hypothesis, there is some indication that victim impact might differ according to intervention. Comparing the advice and separation groups, the victim interviews state more violence in the advice group and official reports state more violence in the separation group.⁹⁰ This suggests that separation encouraged more calls to the police than a mere visit and advice on the spot.

Similarly, the differences between intervention groups in the Milwaukee study do not support my hypothesis. There may be reasons for

84. Berk et al., *supra* note 13, at 197.

85. See Berk et al., *supra* note 13, at 183 (Table 3).

86. See Berk et al., *Deterrent Effect*, *supra* note 42, at 701.

87. According to victim interviews, recidivism was 19% in the arrest group, 33% in the separation group and 37% in the advice group. According to the official data, it was 10% in arrest, 24% in separation and 19% in the advice group. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 278.

88. See *id.* at 278.

89. This crossover occurred in about 18% of the advice group cases and 23% of the separation group cases. See *id.* at 274.

90. See *supra* note 87.

these discrepancies related to the design of the study, but it is impossible to estimate their impact. The three alternative group interventions were short arrest, approximately three hours, full arrest, approximately eleven hours, and warning. In the warning group, the parties were told that if the police needed to return one of them would be arrested,⁹¹ thus discouraging victims from contacting the police.⁹²

At the six-month interview, victims reported recidivism in 35% of full-arrest cases, 30% of short-arrest cases and 31% of warning cases.⁹³ Reports to the police showed the same rate of repeat violence against the same victims in each group.⁹⁴ Recidivism against any victim was slightly higher in the short-arrest group than in the two other groups.

In the Milwaukee study, the researchers collected official data of recidivism both before and after the randomly assigned method of intervention. Reports of domestic violence and arrests increased in all groups after the assigned method of intervention.⁹⁵ Interestingly, the increase followed quite different patterns in different groups. Reports to the police, reflecting the willingness of victims and their kin to report crime to the police, increased most in the short-arrest group and least in the warning group. It is possible that this pattern is dependent on the impact of arrest on victims. The victims in the warning group may have been discouraged from calling the police, since the police warned that they would arrest *either* the victim or the perpetrator the next time they were called.

Increases were high in repeat arrests and in reports to police in the short-arrest group. In the full-arrest group, both measures increased by approximately 60%. In the warning group, reports to police increased the least, 45%, whereas repeat arrests increased substantially, 82%. Since arrest suggests that the case is serious, this may mean that victims in the warning group called the police only in serious cases. Taking into account that according to victim interviews the prevalence of repeat violence was about the same in the short-arrest group as in the warning group, it is possible that arrest encouraged victims to call the police.

C. DOES LONG-TERM ESCALATION EXIST?

The author of the Minneapolis and Milwaukee studies has stressed that the Milwaukee data shows escalation of violence in the short-arrest group

91. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 335.

92. See Zorza, *supra* note 71, at 966.

93. See Sherman & Schmidt, *Variable Effects of Arrest*, *supra* note 11, at 154.

94. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 353; Sherman et al., *Initial Deterrence*, *supra* note 11, at 836.

95. The increase in reports is mainly explained by the new arrest policy that was introduced in Milwaukee shortly before the experiment was carried out in 1987 to 1988. The domestic violence cases reported by the police during this time soared from 7,000 in 1986 to 8,700 in 1988. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 119.

over time.⁹⁶ During the second year after the initial intervention, the frequency of officially recorded any-victim violence was twice as high in the short-arrest group as in the warning group.⁹⁷ Since we cannot compare that information with victim-reported violence, it is equally possible that victims were more willing to call the police after they had experienced an arrest of the perpetrator.

Only the Omaha study has both victim interviews and official data one year after intervention. Victim interviews reported repeat violence and injuries at the same rate in arrest cases as in other cases.⁹⁸ Official data showed that more perpetrators were arrested and more complaints were made in the arrest group.⁹⁹ This again is consistent with my hypothesis that this difference more likely results from a change in victims' patterns of calling police than from a change in actual violence.

V. DISCUSSION

The immediate effect of arrest is unambiguous—arrest protects the victim,¹⁰⁰ and the deterrent effect lasts at least one month following the arrest.¹⁰¹ The strength of the effect varied in different studies, but generally the level of victims who were safe during the first month was five to ten percentage points higher after arrest than after other methods of intervention. With the exception of the Milwaukee study, victim interviews taken after six months show that arrest deters repeat violence.

After six months, according to the official data, the level of complaints to police following intervention is either the same in all groups or higher in the arrest group. The only exception is the original Minneapolis study, in which based on both victim interviews and official data, repeat violence was lowest in the arrest group.¹⁰²

Assuming that victim interviews give a more accurate picture of the level of violence than the official data, four out of six empirical studies

96. See *id.* at 188.

97. See *id.* at 352.

98. Zorza has calculated the prevalence of repeat violence and injuries to those victims who gave interviews. There was no difference in physical injury among victims whose perpetrators were either arrested or not arrested. Victims were hit or pushed slightly more often in the arrest group, 53% versus 51% in the non-arrest group. See Zorza, *supra* note 71, at 988.

99. See Dunford, *Measurement of Recidivism*, *supra* note 10, at 124-27.

100. See Lisa A. Frisch, *Research that Succeeds, the Policies that Fail*, 83 J. CRIM. L. & CRIMINOLOGY 209, 212 (1992).

101. One exception is Charlotte, where no difference exists between the groups.

102. Garner, Fagan and Maxwell have also paid attention to the discrepancy between the results of victim interviews and official data. Basing their analysis on any distinguishable effect after six months, they report deterrence according to victim interviews in five out of six studies and according to official data in three studies. See Garner et al., *supra* note 45, at 12. In Table 1, I interpret very little differences as the same level, whereas Garner, Fagan and Maxwell interpret even a slight difference as deterrent/escalation.

examined are consistent with the hypothesis presented in this Article. In these four studies, victims who experienced an arrest of the perpetrator suffered less repeat violence than victims whose perpetrators were treated through other interventions. Yet, victims who experienced arrest of the perpetrator called the police as often or more often than victims in other intervention groups. A logical explanation is that victims who experienced an arrest of the perpetrator were encouraged to call the police when subsequently assaulted by the same perpetrator.

TABLE 1: EFFECT OF ARREST IN EXPERIMENTAL STUDIES

	Victim Interviews	Complaints	New Arrests
Minneapolis	Deterrence	Deterrence	Deterrence
Milwaukee	No deterrence	Same	Same
Omaha	Deterrence	Same	Slightly more in arrest group
Charlotte	Same	-	More in arrest group
Colorado Springs	Deterrence	-	Same
Miami	Deterrence	Same	More in arrest group

EXPLANATIONS:

Deterrence = Measure of repeat violence is lower in the arrest group than in other treatment groups.

Same = Measure of repeat violence is almost the same in all intervention groups.

More in arrest group = Measure of repeat violence is higher in the arrest group.

The results of the four studies suggest that this hypothesis is worth serious consideration. However, reexamination of these studies alone can not validate this hypothesis. Both the original empirical studies and my reexamination of them are strained by methodological problems that limit the conclusions that can be drawn. First, two of six studies do not support this hypothesis. This may depend on differences in the research design or demographic factors of the sites. For example, Milwaukee had the highest proportion of African-American suspects, probably an important factor in assessing how arrest affects both suspects' and victims' behavior.

Secondly, due to the research design, the available data are neither sufficient nor suitable for assessing how and to what extent arrest affected victims' choices of whether to call the police in cases of repeat violence. The studies did not collect data about victims' situations and the data

collected was not fully reported. The effect of arrest might vary between different victim groups, depending on the other resources available to the victim, how dependent the victim is on the perpetrator and the victim's social status and integration. To assess the factors, more information is needed. While the studies reported the employment status of the victims as compared to that of the perpetrators,¹⁰³ they did not consider whether the victims worked in their homes as full-time mothers. This is an important factor in assessing the vulnerability of the victim.¹⁰⁴ Miller and Krull's analysis of victim interview data suggests that unemployed victims are revictimized more than employed victims.¹⁰⁵ Further, the relationship between the victim and the perpetrator is not consistently recorded. While marital status is reported, the number of cohabiting couples is not reported in a systematic way. Only one study reports how many couples were still living together when the six-month interview was conducted. Due to these deficiencies, how different victim variables affect victims' willingness to report subsequent victimization is impossible to assess.

Thirdly, victim interview data was incomplete because not all victims were reached for interviews. For instance, in the Minneapolis study, 62% of the victims were reached for immediate interviews and 49% for the interviews six months after the incident.¹⁰⁶ In Charlotte, the respective portions were 65% and 50%.¹⁰⁷ In Omaha, initial interviews were conducted in 80% of cases and six-month follow-up interviews in 73%.¹⁰⁸ This was the main reason victim interview data was partly dismissed by the authors.

In prevalence studies, victim surveys are usually treated as more accurate measures of "real" crime than police statistics, because victims reveal more crime than statistics and criminological research has not discovered any constant ratio between crime reported and crime experienced. Also in the empirical studies, victims reported more crimes in interviews than they had reported to the police.¹⁰⁹ By using the official data

103. In Minneapolis, 60% of both were unemployed. In Omaha, 50% of victims and 31% of suspects were unemployed. See Dunford et al., *Role of Arrest*, *supra* note 10, at 194. In Charlotte, one-third of victims and one-fifth of suspects were unemployed. See HIRSCHTEL ET AL., CHARLOTTE SPOUSE ASSAULT, *supra* note 12, at 26-37.

104. The published results of the Colorado Springs experiment state that in addition to the 10% who were unemployed, 30% of victims reported no occupation. See Berk et al., *supra* note 13, at 175. Only the unpublished report reveals that one-third of those with no occupation were housewives. See Berk et al., *Colorado Springs Spouse Assault*, *supra* note 39, at 55.

105. See MILLER & KRULL, *supra* note 33, at 246.

106. See Sherman & Berk, *Specific Deterrent Effects*, *supra* note 7, at 265.

107. See Hirschel & Hutchison, *supra* note 6, at 110.

108. See Dunford et al., *Role of Arrest*, *supra* note 10, at 189.

109. The Colorado Springs study is the one exception. The authors questioned the victim's ability to recall domestic violence incidents in the interviews. See Berk et al., *supra* note 13, at 197. With this one exception, victims recalled more incidents in the interviews than were reported to the police. It is impossible to know if there was a

as the measure of subsequent crime, the researchers necessarily assumed that the ratio between experienced crime and reported crime was unaffected by the victims' experience of police intervention. This is a bold assumption, but it is not impossible.

When different intervention groups are compared, a crucial question is whether the interview samples are biased against some forms of intervention. In the empirical studies, different statistical tests were done to find if there was a possible bias in the interviewed cases. No such differences in dropout rates,¹¹⁰ variable interventions or background variables were found that would have made interviewed cases qualitatively different from the rest of the sample.¹¹¹ Yet, Berk has suggested that the victim interview data are qualitatively different from the official data because victims whose perpetrators are most likely to be deterred by arrest might be over-represented in the interview data. Most likely, deterred perpetrators are employed or others with a "stake in conformity" for whom an arrest may cause the most unfortunate consequences. Because of the bias, victim interviews may overestimate the beneficial effect of arrest.¹¹²

In Colorado Springs, these effects were studied by comparing the effect of arrest on employed and unemployed suspects. There were more calls to the police in the unemployed group.¹¹³ While the victim interviews showed no difference between employed and unemployed suspects when intervention-as-assigned was used, it is possible that the difference in the official data—increased calls to the police—is a result of the increased willingness of victims to call the police in cases of repeat violence after arrest. While it is clear that there is a difference between employed and unemployed perpetrators, we can not know whether this difference in calls to police is due to a difference in the level of repeat violence, in the victims' response, or both.

Finally, the official data usually include domestic violence against any victim, not only against the same victim who was involved when the initial intervention was used. Thus, even if the original victims report less violence in the interviews, it is possible that the perpetrators have only shifted to new relationships and continued their violent behavior against new partners. There is, however, no indication that this happened more

difference in the way the interviews were carried out. It is also possible that the victims were particularly reluctant to report violence to the interviewers in Colorado Springs because many of them were connected to the military and might have felt that it was a particularly sensitive issue about which to talk.

110. See Dunford et al., *Role of Arrest*, *supra* note 10, at 198.

111. See Sherman & Berk, *Specific Deterrent Effects*, *supra* note 7, at 265; Dunford et al., *Role of Arrest*, *supra* note 10, at 188-89; HIRSCHTEL ET AL., CHARLOTTE SPOUSE ASSAULT, *supra* note 12, at 34-40; Hirschel & Hutchison, *supra* note 6, at 110 n.128.

112. Berk et al., *supra* note 13, at 197; Lawrence W. Sherman, *The Influence of Criminology on Criminal Law: Evaluating Arrests for Misdemeanor Domestic Violence*, 83 J. CRIM. L. & CRIMINOLOGY 1, 31 (1992).

113. See Berk et al., *supra* note 13, at 188-92.

frequently in those cases in which arrest took place. In Charlotte, where a distinction between arrests involving same victim and any victim was made, there was no difference.¹¹⁴

To sum up, the methodological problems encountered do not show that victim interview data was biased against arrest groups. It is possible that it was, but we do not know that, nor do we know how it would be biased. Therefore, we have to ask which is a more serious methodological problem, to disregard victim interview data and possible effect on the victims' behavior or use the data notwithstanding a possible bias. For me, the answer is clear.

VI. CONCLUSION

Overall, the empirical studies support my hypothesis that arrest deters violence and encourages victims to call the police. This explanation of the data is also consistent with the overall increase in reporting domestic violence to the police since the criminal justice system started taking these crimes seriously.¹¹⁵ Of course, overall awareness of domestic crime and public policies, such as mandatory arrest laws, affect victims' behavior differently than individual experiences of arrest. These studies suggest, however, that individual experiences with arrest affect victims' willingness to report domestic violence.

In his discussion of policy recommendations based on these studies, Professor Sherman emphasized a choice between the long-term escalation effect of violence and immediate protection.¹¹⁶ Conclusions about long-term escalation were solely based on official data, which can be influenced by victim impact. Therefore, the fear of long-term escalation may be unfounded.

Also, the conclusion that arrest increases violence among unemployed perpetrators is suspect because it is based on official data. Victim interviews show that there is a deterrent effect among unemployed suspects. While there is a difference between employed and unemployed perpetrators, this may be a consequence of both different levels of violence and different responses from the victims. Therefore, recommendations for police practice in domestic violence should not be based on assumptions of increased violence in the long-term or among unemployed perpetrators. Instead, policy recommendations should be based on other considerations, such as victim safety immediately after the arrest, justice and proportionality. According to these studies, the short-term effect of the arrest was clear, it protects the victim. This conclusion is far less controversial than any conclusions about the long-term effects.

114. See Hirschel & Hutchison, *supra* note 6, at 109.

115. See generally *supra* note 39 and 40.

116. See SHERMAN, *POLICING DOMESTIC VIOLENCE*, *supra* note 7, at 205.

This Article shows that we still do not know enough about the effects of arrest. Reexamining the empirical studies shows that arrest probably encourages victims to report subsequent violence to the police, as well as decreases repeat violence. I think that this interpretation of the results is more sound than those interpretations that only focus on the perpetrator.

The most important point of this Article is methodological. An analysis of the effects of police intervention in domestic violence must be based on both perpetrator and victim response. In interpreting the empirical studies, we must ask: which is a more serious methodological problem, not to recognize possible victim impact and use official data as measurement of repeat violence, or to use victim interview data without being able to control for a possible bias in interview drop-out? Since no bias has been found, using the victim interview data to measure repeat violence is a lesser problem. Of course, neither solution is satisfactory for future research and therefore, victim impact must be considered an integral part of future research designs.