# THE OMAHA DOMESTIC VIOLENCE POLICE EXPERIMENT*

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Table of Contents

# INTRODUCTION

In what has come to be known as a landmark study, the Minneapolis Domestic Violence Experiment (Sherman and Berk 1984a, 1984b) was conducted to assess the effects of different police responses to individuals apprehended for domestic assault. The authors reported that:

> "arrest was the most effective of three standard methods police use to reduce domestic violence. The other police methods--attempting to counsel both parties or sending assailants away--were found to be considerably less effective in deterring future violence in the cases examined." (1984b, p. 1)

Sherman and Berk specified "arrest and initial incarceration alone", as deterring continued domestic assault and recommended that the police adopt arrest as the favored response to domestic assault <u>on the basis</u> of its deterrent power. These findings and recommendations came at a time when advocacy for increased sensitivity to women's rights was strong and pressure was mounting to change the social service approach to domestic violence that had dominated law enforcement and court policy over the preceding two decades (Morash, 1986). Sherman and Berk's recommendations were uniquely appealing for the times and were received by many women's advocates and law enforcement administrators as justification for change (Cohn and Sherman, 1987; Sherman and Cohn, 1989).

The overwhelming reaction of the research community to the Minneapolis Experiment, with its recommendation for presumptory arrests in cases of misdemeanor domestic assault, was a call for additional studies to corroborate its conclusions (Binder and Meeker, 1988; Lempert, 1989; Williams and Hawkins, 1989). The Omaha Domestic Violence Police Experiment was conceived and designed, along with five other projects[1] funded by the National Institute of Justice, to determine if the findings reported for the Minneapolis Domestic Violence Experiment could be

[1] Dade County, Florida; Atlanta, Georgia; Charlotte, North Carolina; Milwaukee, Wisconsin; Colorado Springs, Colorado.

replicated elsewhere (National Institute of Justice, 1986). This report describes the results of that effort in Omaha.

AN OVERVIEW OF THE MINNEAPOLIS EXPERIMENT

By way of general overview and before details of the Omaha Study are presented, a brief description of the Minneapolis Experiment is offered as taken from the first two reports describing the Minneapolis Experiment (Sherman and Burke 1984a; 1984b). The major purpose of this review is to describe the research that was to be replicated in Omaha.

The design of the Minneapolis Domestic Violence experiment called for the police to randomly assign suspected offenders/couples to one of three experimental conditions: arrest, separation, or some form of advice/counseling. A six-month follow-up period was used to measure and compare the prevalence of domestic violence subsequent to each of the three police interventions.

The Minneapolis design applied only to simple domestic assaults (misdemeanors) when both the suspect and the victim were present when the police arrived. The experiment included only those cases in which police were empowered (but not required) to make arrests under Minnesota State law; the police officer must have had probable cause to believe that a suspect had assaulted the victim within four hours of police contact (but police need not have witnessed the assault). Cases of life-threatening or severe injury, usually labelled as a felony (aggravated assault), were excluded from the study by design.

The experiment began on March 17, 1981 and ran until August 1, 1982 producing 330 case reports. All but one of the 34 officers assigned to two Minneapolis precincts with high rates of domestic violence agreed to participate in the study for one year. Cases in which suspects attempted to assault a police officer, when victims persistently demanded an arrest or when both parties were injured were excluded from the experiment. By the third or fourth month of the study it was determined that many of the officers were not turning in cases. In an attempt to increase the number of cases in the experiment 18 additional officers were recruited to participate in the study. This occurred during the ninth month.

The design called for each officer to carry a pad of report forms, color coded for the three different police actions. Each time the officers encountered a situation that fit the experiment's criteria they were to take whatever action was indicated by the report form on the top of the pad. The forms were numbered and arranged in random order for each officer. As a check on the randomization process the staff was to log in reports filled out by the officer referring each case in the order in which they were received and make sure that the sequence corresponded to the original assignment of treatments.

In an effort to maximize victim rapport, a predominantly female, minority research staff was employed to contact the victims for a detailed face-to-face initial interview, to be followed by telephone follow-up interviews every two weeks for 24 weeks. The interviews were designed primarily to measure the frequency and seriousness of victimizations caused by suspects <u>after</u> police intervention. The research staff also collected criminal justice reports of domestic violence that mentioned the suspects name during the six-month follow-up period.

Two kinds of outcome measures were used. One was an officially recorded "failure" of the offender to survive a six-month follow-up period without a police report for further domestic violence. The second outcome measure came from interviews with victims in which they were asked if there had been a repeat incident with the same suspect "broadly defined to include an actual assault, threatened assault or property damage (Sherman and Berk, 1984a p. 266)." Both of these outcomes were used as binary variables (i.e., repeat incident or not) and official police information was used to assess the amount of time elapsed from the treatment to either a failure or the end of the follow-up period. Three analyses were performed: the first using a linear probability model, the second using a logit formulation and the third using a proportional hazard approach. The binary outcome was employed for the linear probability and logit analyses while the time-to-failure was used in the proportional hazard method. The authors, reporting on their findings using police records of contact for domestic violence, indicated that the "separation" treatment produced the highest recidivism, "arrest" produced the lowest, with the impact of "advise" statistically indistinguishable from the other two effects. Results of the analyses on the victim-report data determined that arrest still produced the lowest recidivism but advice produced the highest.

## THE OMAHA RESEARCH DESIGN

Omaha is a city of approximately 400,000 inhabitants 10 percent of whom are black and 2 percent of hispanic origin (U.S. Department of Commerce, 1983). The city is split into three sectors (South, West, North) for police purposes. In concert with Chief Robert Wadman of the Omaha Police Division and after surveying 911 dispatch records, it was determined that approximately sixty percent of all disturbance calls were reported during the hours of "C" shift. On this basis the decision was made to limit the replication experiment in Omaha to eligible domestic assaults coming to the attention of the police throughout the city (all three sectors) during the hours of "C" Shift[2] (4:00 p.m. to 12:00 midnight). As a result, no segment of the city (e.g., social economic status or ethnic groups) would be excluded from participation in the experiment by the research design and the majority of domestic violence calls would be captured by the study.

Following the design of the Minneapolis Experiment, police calls for domestic violence found to be eligible for the study were randomly assigned to "arrest", "separation" or "mediation" for all instances in which both victims and suspects

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[2] "D" Shift (8:00 p.m. to 4:00 a.m.) was also used until it was disbanded approximately 6 months into the experiment.

were present when the police arrived. As an extension to the Minneapolis study, eligible cases in which suspects were absent when the police arrived (more than 40% of the time) were randomly assigned to a "warrant" or to a "no warrant" treatment group.[3] To be eligible for either of the two experiments, the following conditions applied: 1) probable cause for an arrest for misdemeanor assault must have existed, 2) the case must have involved a clearly identifiable victim and suspect, 3) both parties to the assault must have been of age (18 or older), 4) both parties must have lived together sometime during the year preceding the assault, and in the offender present experiment, 5) neither party to the offense could have an arrest warrant on file. Cases for which the police had no legal authority to make an arrest (i.e., no probable cause to believe that an assault had occurred) were excluded from the experiment as were more serious cases (i.e., felony cases). A review of the kinds of cases found eligible for the experiments is helpful.

The data of Table 1 were taken from the Domestic Violence Report forms filled out by the police for every case entered into the two Omaha Experiments. These data describe the presenting offense for cases referred to the studies as reported by the officers responding to calls for assistance. After determining demographics for victims and suspects, police officers were asked to report if suspects and victims had been using alcohol or drugs prior to their arrival. Such assessments were not based on standardized nor scientific tests, rather, upon the opinions of police officers resulting from their interactions with both parties to disturbances. Police officers were much less likely to report victims/suspects as using drugs compared to alcohol. This was probably due to less drug use among cohabitant couples in conflict than alcohol, although the difficulty in detecting

[3] The two Omaha Experiments will be referred to throughout the report as the replication experiment and the offender absent experiment respectively.

Table 1
Responding Officer Reports of the Presenting Incident
Treatment As Randomly Assigned

| | Replication Experiment | | | | | | Offender Absent Experiment | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Mediation N=115 | | Separation N=106 | | Arrest N=109 | | Warrant N=111 | | No Warrant N=136 | |
| | N | % | N | % | N | % | N | % | N | % |
| Alcohol Involved - Suspects | 65 | 56.5 | 60 | 56.6 | 62 | 56.9 | 61 | 55.0 | 62 | 45.6 |
| Alcohol Involved - Victims | 39 | 33.9 | 33 | 31.1 | 31 | 28.4 | 25 | 22.5 | 28 | 20.6 |
| Drugs Involved - Suspects | 3 | 2.6 | 3 | 2.8 | 5 | 4.6 | 14 | 12.6 | 14 | 10.3 |
| Drugs Involved - Victims | 1 | .9 | 1 | .9 | 0 | -- | 0 | -- | 0 | -- |
| Officer Reports of Victim Injuries | | | | | | | | | | |
| Scratched, cut | 23 | 20.0 | 28 | 26.4 | 28 | 25.7 | 26 | 23.4 | 38 | 27.9 |
| Hit, slapped | 34 | 29.6 | 42 | 39.6 | 35 | 32.1 | 38 | 34.2 | 62 | 45.6 |
| Bruised, soreness | 48 | 41.7 | 48 | 45.3 | 54 | 49.5 | 61 | 55.0 | 79 | 58.1 |
| Knocked down | 8 | 7.0 | 4 | 3.8 | 5 | 4.6 | 9 | 8.1 | 8 | 5.9 |
| Choked | 4 | 3.5 | 4 | 3.8 | 5 | 4.6 | 11 | 9.9 | 7 | 5.1 |
| Stabbed | 0 | -- | 0 | -- | 1 | .9 | 1 | .9 | 0 | -- |
| No injury mentioned | 9 | 7.8 | 4 | 3.8 | 6 | 5.5 | 7 | 6.3 | 3 | 2.2 |
| No injury | 10 | 8.7 | 9 | 8.5 | 6 | 5.5 | 6 | 5.4 | 5 | 3.7 |
| Other | 26 | 22.6 | 24 | 22.6 | 25 | 22.9 | 41 | 36.9 | 39 | 28.7 |
| Office Reports of Suspect Injuries | | | | | | | | | | |
| Scratched, cut | 10 | 8.7 | 15 | 14.2 | 14 | 12.8 | NA | | NA | |
| Hit,slapped | 2 | 1.7 | 1 | .9 | 1 | .9 | NA | | NA | |
| Bruised, soreness | 1 | .9 | 5 | 4.7 | 4 | 3.7 | NA | | NA | |
| Knocked down | 0 | -- | 0 | -- | 0 | -- | NA | | NA | |
| Choked | 0 | -- | 0 | -- | 0 | -- | NA | | NA | |
| Stabbed | 0 | -- | 0 | -- | 0 | -- | NA | | NA | |
| No injury mentioned | 3 | 2.6 | 2 | 1.9 | 1 | .9 | NA | | NA | |
| No injury | 83 | 72.2 | 71 | 67.0 | 75 | 68.8 | NA | | NA | |
| Other | 3 | 2.6 | 4 | 3.8 | 5 | 4.6 | NA | | NA | |
| Responding Officer's Description of Incident Coded for Probable Cause | | | | | | | | | | |
| No physical assault mentioned | 18 | 15.6 | 8 | 7.5 | 10 | 9.2 | 4 | 3.6 | 7 | 5.1 |
| Assault-no physical evidence mentioned | 20 | 17.4 | 16 | 15.1 | 19 | 17.4 | 11 | 9.9 | 17 | 12.5 |
| Probable cause for misdemeanor assault mentioned | 70 | 60.9 | 76 | 71.7 | 76 | 69.7 | 87 | 78.4 | 105 | 77.2 |
| Probable cause of felony assault-weapon mentioned | 1 | .9 | 3 | 2.8 | 2 | 1.8 | 2 | 1.8 | 4 | 2.9 |
| Missing cases | 6 | 5.2 | 3 | 2.8 | 2 | 1.8 | 7 | 6.3 | 3 | 2.2 |
| Responding Officer's Estimated Time at Scene | | | | | | | | | | |
| Mean (minutes) | 84 | | 85 | | 66 | | 49 | | 51 | |
| 30 minutes or less | 55% | | 57% | | 60% | | 63% | | 65% | |
| 1 hour or less | 90% | | 86% | | 85% | | 89% | | 88% | |
| Missing cases | 26 | | 36 | | 27 | | 36 | | 56 | |

drug use may have also contributed to the infrequency with which it was reported. On the basis of officer reports, suspects were using alcohol in 54 percent of the cases found eligible for the study and 27 percent of the victims were reported using alcohol. It was also determined that the most frequently reported injuries recorded for victims were complaints for soreness and bruises (50%) followed by reports of being hit or slapped (37%). Only 6 percent of the officers specifically specified that no injuries were sustained by victims. The reports of injuries to suspects were limited, almost exclusively, to scratches and cuts with the majority of the police reporting no injuries sustained by suspects (69%). As an additional assessment for the seriousness of the presenting offense, the investigators coded police descriptions of the presenting offense for probable cause for an arrest. It must be noted that officers sometimes failed to report injuries on the Domestic Violence Report form. Nevertheless, seventy-two percent of the cases were coded as containing probable cause for an arrest for misdemeanor assault. No mention of an assault was made for 8 percent of the cases coded for probable cause. It is evident that the majority of cases finding their way into the Omaha experiments involved visible physical injury and represented fairly serious cases of misdemeanor assault.

After responding officers determined eligibility, they contacted (by radio or telephone) the Information Unit of the Omaha Police Division and gave the civilian operators the dates and times of the calls, names and birth dates of the victims and suspects, and their own police identification numbers. Treatments were then assigned as the result of a computer generated randomization program initiated by the Information Unit operators. In this manner eligibility decisions always preceded requests for randomized dispositions and permanent and protected records of the particulars of each transaction were recorded and stored within the computer.

DESCRIPTION OF THE EXPERIMENTS

In February of 1986 all of the command officers and patrolmen assigned to "C" and "D" shifts were trained during a succession of 3-day training sessions about the rationale, contents and mechanics of the experiments. At each shift change thereafter, officers new to "C" shift were similarly trained. Training was conducted by project staff, and both police and city attorney personnel. The project was presented as an official program of the department; officers were instructed to participate as a matter of Division policy.

A total of 194 officers were ultimately assigned to the participating shifts and received training on the methods and procedures of the experiment (see Table 2). Of that number, 31 (16%) failed to refer at least one case to the study. It is noteworthy that of the 31 officers in this category, 27 were in a position to make referrals to the project for three months or less. Notwithstanding the relatively high overall level of (at least nominal) officer participation, 61 (31%) of the officers accounted for approximately 75 percent of the referrals. What appears to be a somewhat skewed referral distribution is mitigated, in part, by a differential in referral rates by area of the city. The police officers working the West Sector, a homogeneously middle and upper class section of the city were responsible for 7.4 percent of the referrals to the project, while those deployed in the overwhelmingly working and lower class North Sector, made 66.5 percent of the referrals. One area of the city thus provided two-thirds of the referrals to the project and all of the officers with high referral rates worked in busy districts within that area.[4] The South Sector included downtown Omaha and what used to be the City of South Omaha, a primarily working class area with pockets of

[4] After extensive field observations (in excess of 60 full shift ride-alongs) and conversations with emergency/police dispatchers, the authors concluded that the geographical distribution of referrals to the experiment represents a fairly accurate reflection of the general level of police activity in the city.

Table 2

Number of Eligible Cases Referred to the Omaha Experiments
by Officers Eligible to Make Referrals

| | Number of Referrals | Number of Officers | Total Number of Referrals | Percent of Referrals |
|---|---|---|---|---|
| | 0 | 31 | 0 | 0 |
| | 1 | 29 | 29 | 2.8 |
| | 2 | 27 | 54 | 5.2 |
| | 3 | 20 | 60 | 5.8 |
| | 4 | 17 | 68 | 6.7 |
| | 5 | 9 | 45 | 4.4 |
| | 6-10 | 28 | 213* | 20.6 |
| | 11-15 | 22 | 276* | 26.7 |
| | 16-25 | 7 | 137* | 13.3 |
| | 26 | 2 | 52 | 5.0 |
| | 31 | 1 | 31 | 3.0 |
| | 67 | 1 | 67 | 6.5 |
| Total | | 194 | 1032 | 100 |

* Based on actual frequency of referral.

both lower and middle class neighborhoods. The officers of South Omaha made 26.1 percent of the referrals to the experiment. Inasmuch as all officers during the hours of the experiments were eligible to make referrals to the experiments (i.e., participation was not limited to a specialized unit of officers extensively trained and monitored to handle domestic violence calls), and 84 percent of the officers in the field during the hours of the experiment did make referrals to the experiments from all areas of the city, the external validity of the experiments is strengthened. The fact that the experimental treatments were applied by a large majority of officers as they engaged in the activities routine to working the streets of the city of Omaha, is fairly good evidence that the findings from the Omaha experiments can be generalized to the total city; at least for those hours represented by the experiments.

Although the uneven referral rates are of some concern since they could affect the external validity or generalizability of the experiment, a related concern involved the extent to which random assignment produced equivalent experimental groups within sectors (internal validity). A comparison of referrals by sector provided strong evidence for equivalency (see Appendix A).

One of the greatest challenges faced when implementing random assignment in field settings is the monitoring and identification of all violations of randomized outcomes (Dunford, 1989). While researchers may not be able to prevent violations of randomly designated treatments (e.g., arresting when treatment is randomized to mediate), they should be able to insure that such violations will not go undetected when they occur. Because there were some violations of the randomly assigned treatment dispositions, four treatment classifications were possible: Treatment as Assigned (TA); Treatment as Officially Recorded (TR); Treatment Immediately Delivered (TID); and Treatment Ultimately Delivered (TUD). The first measure, Treatment as Assigned, was the treatment form that was randomized by computer and communicated to officers in the field via the Information Unit of the Omaha Police Division: arrest, separate, mediate, warrant or no warrant. The treatment variable used for the majority of analyses presented in this paper, for both offender present and absent experiments, is Treatment as Assigned. The second measure, Treatment as Recorded, is treatment as recorded by responding officers on the Domestic Violence Report forms. After receiving a randomized treatment via the Information Unit, officers recorded the dispositions on the Domestic Violence Report form, along with other relevant information, and forwarded the reports to the project. When the 577 Treatments as Assigned were compared with the Treatments as Recorded on the Domestic Violence Report forms, four discrepancies were found. Because this measure of treatment was nearly identical to Treatment as Assigned it was not used for any substantive analyses in this report. The third measure, Treatment as Immediately Delivered, reflects estimates of the initial treatment that was actually delivered at the scene of an eligible case.[5] This measure was determined by asking victims about treatments delivered to suspects and by reviewing what police officers wrote on the Domestic Violence Report form about the treatment delivered and comparing the two. Three different rules were used for this (TID) classification of treatment. First, when officers were able to deliver <u>at least part</u> of the assigned treatment, such cases were considered to be delivered as assigned even though the ultimate outcome may have been different. Second, when victims reported the police as delivering treatments that were different from those assigned, the cases were defined as misdelivered and classified as reported by victims, even though a good case could be made that misdelivery may not have occurred. For example, eight cases that were randomly assigned to mediation were

[5] The operationalization of treatment as delivered (TID, TUD) as described here represents the authors best estimates, based upon available information, of what actually happened, but are not the only ways that treatment could have been defined.

recalled by victims as involving separation and were thus reclassified as separation for the Treatment as Immediately Delivered classification. However, because so many officers advised couples, as a part of mediation, to get away from each other for a "cooling off" period, victims could have easily confused separation with mediation. The third rule for classifying Treatment as Immediately Delivered involved cases where there was evidence that officers violated the intent of the assigned treatment; i.e., that treatment was misdelivered. There were 7 cases that could be defined as definite misdeliveries. These were primarily cases in which suspects were arrested when they were assigned to non-arrest treatments or when officers failed to arrest when they were directed to do so. In each case explanations for the violations of random assignment were noted on the Domestic Violence Report forms. As noted above, many of the discrepancies between Treatment as Assigned and Treatment Immediately Delivered may have involved differences in perceptions of what happened rather than in any real differences, while others were clear misdeliveries. The remaining discrepancies were anomalies. For example, one suspect randomly assigned to receive a warrant, returned while the police were still on site and was arrested.

The final treatment category consisted of Treatment as Ultimately Delivered. This classification was determined by comparing treatments as assigned with what victims reported as ultimately happening, what police officers recorded on the Domestic Violence Report Forms as ultimately happening and comparing Arrest and Warrant treatments with official records of police, prosecuting attorney and court actions. If, for example, in the course of delivering Mediation a suspect assaulted an officer and was arrested, the case was defined as follows: Treatment as Assigned = Mediation; Treatment as Immediately Delivered = Mediation; Treatment as Ultimately Delivered = Arrest. Also, if the assigned treatment was an Arrest or a Warrant and no official records could be found that an arrest was made or that a warrant was issued, the case was classified on the Treatment as Ultimately Delivered measure as Mediation and No Warrant respectively. If, however, suspects believed that a warrant was to be issued for their arrests, which was the case for 40 percent of the warrants for which no records could be found that warrants were ever issued, the warrant classification for Treatment as Ultimately Delivered was maintained. Treatment as Ultimately Delivered is problematic in that the absence of an officially recorded arrest or warrant is not foolproof evidence that an arrest was not made or a warrant was not issued. Several cases were found, for example, in which no records of arrests were found in the police record bureau even though the cases were found to have been officially recorded as "booked" into the jail. Conversely, cases were found in the police record bureau that were not found in the "jail" booking records. Also, suspects were frequently told that warrants were going to be issued for their arrests for cases in which no records of warrants could be found, further complicating the definition of outcome. Treatment as Ultimately Delivered was, for these reasons, not used to assess outcome for this paper.[6]

Given the number of ways that treatment can be defined, which of the four definitions of treatment described here most usefully serves our research interests; i.e., informs us about the effects of treatment? Treatment as randomly assigned is clearly the most useful of the treatment classifications from an experimental perspective, but is substantively useful only if randomly assigned treatment categories can be determined to have substantive integrity; that is, that the treatments actually delivered in connection with each assigned treatment category were consistently similar within categories and consistently different

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[6] It should be noted that all of the analyses conducted for Treatment as Assigned for the Omaha <u>replication</u> study were repeated for Treatment as Ultimately Delivered with quite similar results. The conclusions based on the former were not changed by the latter.

between categories. It is the opinion of the investigators that treatment as assigned, notwithstanding the few violations that pose a threat to its integrity, is the best of the four treatment classification schemes for a number of reasons. First, it is the result of random assignment with all of the advantages of a true experimental design. To use any other treatment classification would, in effect, require abandoning the experimental aspects of the research. Second, although a number of instances in which the misapplication of treatment may have occurred have been cited, most of them are based on circumstantial evidence. When treatment is conceptualized as what agents of law enforcement officially report they did, rather than on what may have transpired after cases leave their control or what victims think they did, the number of discrepancies between treatments assigned and delivered is very small for both of the Omaha experiments (Replication = 7 out of 330, Offender Absent = 8 out of 247). Third, if the goal of both the Minneapolis and the Omaha experiments was to determine the merit of police policy involving different interventions in domestic disputes as actually implemented in the field, testing for differences between Treatments as Assigned more accurately reflect actual practice than do any of the other classifications. While officers are attempting to mediate a dispute, for example, some suspects will become belligerent and abusive and will be arrested. In departments with policies for warranting suspects for domestic assaults when suspects are gone when the police arrive, the prosecuting attorney will most certainly fail to file on some portion of the requests for warrants. Violations made in the delivery of Treatments as Assigned can be expected to repeat themselves in the typical day-to-day activity of the police and thus may be appropriately included in experimentation designed to impact police policy decisions. Finally, the number of indisputable violations to Treatment as Assigned are sufficiently few that the alternative treatment classifications are not very attractive.

As a check on the misapplication of treatment, Table 3 presents the disparities between Treatments as Assigned and Treatments as Delivered.[7] Within the replication experiment, 95 percent of the cases assigned to an arrest received an arrest, 92 percent of those assigned to be separated were separated, and 89 percent of the mediation cases were mediated, for an overall treatment delivered as assigned rate of 92 percent. When mediate and separate were collapsed and made into an "informal" or nonpunitive treatment category (Binder and Meeker, 1988) to eventually be used to compare with a "formal" or punitive treatment (arrest) group, the overall delivered as assigned rate was 97 percent. Within the offender absent experiment, 96 percent of the warrants and 97 percent of the no warrants were <u>initially</u> delivered as assigned.[8] This level of misapplication is quite small and does not affect the power of the test for pure cases. Apart from the "power" issue, the point at which the level of misapplication jeopardizes the internal validity of an experimental design is not a fixed rule about which there is consensus, but is left to individual judgement. Given the relatively small number of misapplications, the suspicion that many of the cases conservatively defined as

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7 Treatment as Delivered will refer to Treatment as Immediately Delivered from this point on unless noted otherwise.

8 That is, responding officers received the randomized dispositions from the Information Unit, informed the victims of the action to be taken, and in the case of randomized warrant treatments, submitted the relevant information to Research and Planning where the the proper paperwork was prepared and sent to the City Prosecuting Attorney's Office for review and the issuance of a warrant. At this point warrants were not issued for 24 (22%) of the cases assigned to receive a warrant. The potential difficulty associated with the failure of the Prosecuting Attorney's Office to deliver the treatments assigned is somewhat lessened by the fact that about half of the interviewed victims whose assailants were not ultimately issued warrants reported that the suspects had been told that warrants for their arrest would be issued. Suspects thus may have assumed that a warrant had been issued and may have reacted accordingly. More importantly, because the Omaha experiment was designed to assess the effects of police action, and because the police officers involved in the experiment delivered the randomized treatments that they were assigned to deliver, and because the real world is populated with District and Prosecuting Attorneys who routinely decide <u>against</u> prosecuting some portion of the cases referred by the police, the decision was made to consider the 24 cases noted here to have been treated as assigned for the Treatment As Immediately Delivered analyses.

Table 3

Random Assignment as Assigned Compared to
As Delivered - Replication and Offender Absent Experiments

| ASSIGNED | Disposition | | AS DELIVERED Mediate | Separate | Arrest | Warrant | No Warrant | Total |
|---|---|---|---|---|---|---|---|---|
| | Mediate* | N | 102 | 8 | 2 | -- | 3 | 115 |
| | | % | 89 | 7 | 2 | | 2 | 100 |
| | Separate | N | 5 | 98 | 3 | -- | -- | 106 |
| | | % | 5 | 92 | 3 | | | 100 |
| AS ASSIGNED | Arrest | N | 3 | 2 | 104 | -- | -- | 109 |
| | | % | 3 | 2 | 95 | | | 100 |
| | Warrant** | N | 1 | -- | 2 | 107*** | 1 | 111 |
| | | % | 1 | | 2 | 96 | 1 | 100 |
| | No Warrant | N | 1 | 2 | -- | 1 | 132 | 136 |
| | | % | 1 | 1 | | 1 | 97 | 100 |

* Replication Prevalence of Misapplication: Chi-square=3.502 DF=2 p=.174
** Offender Absent Prevalence of Misapplication: Chi-square= .086 DF=1 p=.770
*** Twenty-four cases assigned to receive a warrant were not issued warrants by the prosecuting attorney's office.

misapplications may not in actuality have been misapplied, and the provision to collapse mediation and separation into one nonpunitive treatment class group for some analyses, the misapplication rates shown in Table 3 were judged by project staff to be acceptable (i.e., did not provide a serious threat to internal validity) and subs t attention was focused upon the distribution of the misapplied cases. When the prevalence of misapplied cases was compared by treatment groups for the two experiments (following Glass and Stanley, 1970), the null hypothesis that misapplications were independent of treatment assignments, could not be rejected.

The purpose of random assignment within experiments is to insure the equivalence of experimental groups within probability limits at the point of assignment. One check on how well random assignment worked involved a comparison of the randomly assigned treatment groups for each of the experiments on variables thought to have some relevance for an assignment bias (e.g., ethnicity, SES, employment status, level of violence, prior arrest history, etc.; see Appendix B). Six (6.2%) of the 96 comparisons for the replication experiment proved to be statistically different at probability levels of .10 or less (about the number that would be expected to be different by chance), and the differences did not consistently favor any one of the treatment groups. The same analysis for the offender absent experiment found ten (10.4%) of the comparisons to be statistically different and, counterintuitively, the differences favored the No Warrant experimental group as having slightly more serious cases than the Warrant group. The differences found were assumed to be the result of sampling error and not manipulation, reasoning that if random assignment had been deliberately manipulated the expectation would be for those cases characterized by more serious or prolonged abuses to have been assigned to receive a warrant rather than to a no warrant disposition, since in the latter the police simply took the victim's complaint and departed.[9]

An additional assessment for bias involved comparing respondents who were interviewed with those not interviewed on arrest recidivism. The prevalence of rearrest was 10.6 percent for those interviewed and 10.4 percent for those not interviewed for the replication experiment and the average number of rearrests was .126 and .104 respectively. These differences were not statistically significant. Nearly identical results were obtained with the same analyses for the offender absent experiment. The prevalence of rearrest was 8.7 percent for those interviewed and 10.0 for those not interviewed and the average number of rearrests was .116 and .100 respectively.

Two types of outcome measures were included in the research design. The first was official recidivism outcome measured by new arrests and complaints for any crimes committed by the suspect against the victim as found in official police records.[10] The second was a victim report of three forms of repeated violence; 1) fear of injury, 2) pushing-hitting and 3) physical injury. Both types of outcome measures provide for assessing differential treatment effects on subsequent conflict. The design called for interviewing victims two times over a 6 month follow-up period; the initial interview at the end of the first week after the

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[9] The authors are fairly confident that random assignment worked the way it was supposed to work. After extensive and ongoing reviews of the procedures used to randomly assign cases to different treatments and after watching and talking to officers about those procedures throughout the sampling period, no evidence was ever found that the procedures were manipulated.

[10] Arrest recidivism was defined two ways: when suspects were rearrested for crimes against victims and when suspect-victim pairs came back into the experiment as repeat cases in which there was probable cause for an arrest for misdemeanor assault as documented on the Domestic Violence Reports.

presenting offense, and the second 6 months later.[11] All of the interviewers were female and were matched on ethnicity to victim respondents. Almost all of the interviews were conducted in the homes of respondents and always in absolute privacy. Twenty percent of the sampled victims did not complete initial interviews in the replication experiment and 16 percent of the victims from the offender absent sample did not complete initial interviews (see Table 4). When participation was dichotomized and compared (Chi Square) by treatment groups for each of the experiments to determine if rates were disproportionate to any particular treatment, no statistically significant differences were found. The overall completion rate at the 6 month interview (including initial losses) was 73 percent for the replication experiment and 79 percent for the offender absent experiment. Again, losses were not disproportionate for any particular treatment group for either of the experiments. Of the 32 cases lost from the first to the second interview, all but 3 (91%) were lost because victims had moved and could not be located.

TREATMENT CONTENT

Apart from instructing officers to advise suspects assigned to separation to stay away from victims for a minimum of eight hours, no attempt was made to standardize treatments. This was done both to replicate the procedures of the Minneapolis Experiment and to simulate typical police activity with regard to the police handling of domestic assault cases. In addition to determining if the Minneapolis findings apply to Omaha, the goal was to test the differential effectiveness of what police routinely do when responding to domestic assaults.

[11] Twelve month follow-up interviews were conducted. However, due to funding limitations 12 month interviews and record searches were conducted during the data analysis and write up periods of the current grant and were thus not available to be included in this report.

Table 4

Interview Completion Information
Initial and 6 Month Follow-up Interviews

Offender Present

| Disposition | Initial Interviews Completed N | % | Initial Interviews Refused N | % | Initial Interviews Not Located N | % | Initial Non-English Speaking N | % | Initial Eligible But Not Interviewed N | % | Total Cases | Eligible Follow-up Interviews Completed N | %[a] | Eligible Follow-up Interviews Refused N | % | Eligible Follow-up Interviews Not Located N | % | Total Interviews Completed N | %[b] |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Mediate | 91 | 79.1 | 17 | 14.8 | 7 | 6.1 | — | | — | | 115 | 85 | 93.4 | 0 | 0 | 6 | 6.6 | 85 | 73.9 |
| Separate | 89 | 84.0 | 8 | 7.5 | 8 | 7.5 | 1 | .9 | — | | 106 | 80 | 89.9 | 1 | 1.1 | 8 | 9.0 | 80 | 75.5 |
| Arrest | 83 | 76.1 | 12 | 11.0 | 12 | 11.0 | 1 | .9 | 1 | .9 | 109 | 77 | 92.8 | 1 | 1.2 | 5 | 6.0 | 77 | 70.6 |
| Total | 263 | 79.7 | 37 | 11.2 | 27 | 8.2 | 2 | .6 | 1 | .3 | 330 | 242 | 92.0 | 2 | .8 | 19 | 7.2 | 242 | 73.3 |

Offender Absent

| Disposition | Initial Interviews Completed N | % | Initial Interviews Refused N | % | Initial Interviews Not Located N | % | Total Cases | Eligible Follow-up Interviews Completed N | %[a] | Eligible Follow-up Interview Refused N | % | Eligible Follow-up Interview Not Located N | % | Total Interviews Completed N | %[b] |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Warrant | 91 | 82.0 | 13 | 11.7 | 7 | 6.3 | 111 | 84 | 92.3 | 1 | 1 | 6 | 6.6 | 84 | 75.7 |
| No Warrant | 116 | 85.3 | 9 | 6.6 | 11 | 8.1 | 136 | 112 | 96.6 | — | — | 4 | 3.4 | 112 | 82.4 |
| Total | 207 | 83.8 | 22 | 8.9 | 18 | 7.3 | 247 | 196 | 94.7 | 1 | .5 | 10 | 4.8 | 196 | 79.4 |

The following are the results of tests for differences in the prevalence of completed initial and 6 month follow-up interviews by disposition.

Initial

Offender Present Chi-Square = 2.064 DF = 2 p = .356
Offender Absent Chi-Square = .494 DF = 1 p = .482

6-Month Follow-up

Offender Present Chi-Square = .671 DF = 2 p = .715
Offender Absent Chi-Square = 1.663 DF = 1 p = .197

a. The proportion of cases with initial interviews completing 6 month follow-up interviews.
b. The proportion of cases with initial and 6 month follow-up interviews.

Precise treatment content, as a result, is not easily provided. The sources of descriptive information about treatment content include police records of police actions (Domestic Violence Report) and victim reports of what the police did. Such reports were not always definitive. The reports the police provided were brief, involving demographic information, a short check list of action taken and a narrative of the presenting offense and injuries. Victim reports were problematic in other ways. First, not all victims were interviewed (80% completed an initial interview). Second, victims were not always sure of what happened during, and as a result of, police interventions. Standard police practice involves separating victims and suspects for interrogation while at the scene of a crime so that victims sometimes made uninformed assumptions about the treatment content given to suspects. When the interviewers asked victim respondents about the action taken by the police (treatment), the interviewers were instructed to follow up on the treatment that the victims reported as being received rather than the treatment that was assigned. It was not practical, it was reasoned, to ask respondents questions about treatments that were not perceived as being delivered.[12]

The substance of treatment is probably best described by respondents who reported themselves or their assailants as receiving a given treatment, irrespective of the treatments randomly assigned. However, when the proportion of victims randomly assigned to a treatment and reporting on that treatment is known, the extent to which characteristics of that treatment are delivered as assigned can

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[12] The difference between a mediate and a separate, for example was not always clear. Sometimes the police would mediate a dispute and leave, after which the suspect would leave of his/her own accord and the victim would report separation as the police disposition. Sometimes the police would effect a separation by sending the suspect away only to have him/her return shortly after the departure of the police after which the victim would report that all the police did was to talk to them. Finally, the difference between warrant and no warrant treatments were not always apparent to victims who, for whatever reason (stress, injury, turmoil, alcohol, etc.), were not paying close attention to the details of police interventions.

be assessed. The following descriptions of treatment content were obtained from police reports and from interviews with victims who reported on treatments.

Overall, eighty-one victims reported that a mediation treatment had been given. A review of Table 5 reveals some inconsistency across officers with respect to the content of mediation as reported by victims. The most common counsel given in mediation was to advise victims to leave (21%) or to give victims legal advice (17%; how to get a restraining order, what constitutes probable cause for an arrest, etc.). Very little was done in the way of referral or actual counseling. Thirty percent of the victims identifying the treatment as mediation reported that the police did advise them to seek outside help and 24 percent reported that the police told them where to go to get help. According to victims, the presence of the police tended to stop the fights they were having with suspects (77%) and the explanation most frequently given for this effect was that they or the suspects left as a result of the police intervention. Finally, victims reported that the police seldom took sides (17% of the time) when responding to calls for assistance and that when they did it was evenly divided between them and suspects. The mean time spent in the mediation process was estimated by victims to be 23 minutes and the length of time the police were on site was estimated to be 29 minutes.[13] Mediation, as delivered in Omaha, was generally little more than the restoration of order. With few exceptions the police simply calmed the protagonists and then left, doing so as quickly as circumstances permitted. No informed or systematic approach to counseling could be said to describe the mediation delivered. Sixty

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[13] It is interesting to note that victim estimates of the time the police spent on site were much lower than police estimates (sometimes 30 to 40 minutes less) and was lowest for the mediation treatment. Inasmuch as officers usually note their arrival and departure times their estimates may have more face validity. It is certain, however, that most police officers in Omaha lacked formal training in mediation skills and that mediation is most often defined as restoring order and leaving.

Table 5

Type of Counsel That The Police Gave to Mediated Couples as Reported by Victims

| Police Counsel | First Response | | Second Response | |
|---|---|---|---|---|
| Advised victim to leave the premises | 17 | 21% | 6 | 23% |
| Gave victim legal advice | 14 | 17% | 6 | 23% |
| Asked the victim what she/he wanted done | 10 | 12% | 3 | 11% |
| Gave victim advice on services | 9 | 11% | 2 | 8% |
| General talk to both victim and suspects | 8 | 10% | -- | -- |
| Advised both victim and suspect to calm down | 5 | 6% | 1 | 4% |
| Gave advice on marital relationship | 5 | 6% | 4 | 15% |
| Gave personal advice to victim and suspect | 4 | 5% | -- | -- |
| Asked victim if she/he would be alright | 3 | 4% | 3 | 12% |
| Other | 6 | 7% | 1 | 4% |

percent (69 cases) of the 115 cases randomly assigned to mediation reported on mediation.

Ninety-one victims reported on their experiences in the separation treatment. The police achieved separation by asking suspects to leave in 68 percent of the cases and victims to leave in the other 32 percent of the cases. The majority went to a relative's (40%) or to a friend's (16%) to stay. The average length of separation was almost 3 full days (70 hours). Two-thirds (67%) of the victims reported the separation as lasting eight hours or longer while 23 percent were apart for two hours or less. Eighty-seven percent of the victims reported that the presence of the police stopped the trouble they were having with suspects. The reason most often given was that one or the other left as a result of the police intervention. The police were also reported by victims as taking sides 23 percent of the time and that they (victims) were favored two-thirds of the time when the police did so. The mean length of time police were present as estimated by victims was 37 minutes. Given the lack of authority to force people legally living together to separate, the success that officers had in getting people to separate was notable and the average length of time that couples remained apart (3 days) was striking. Seventy-one percent (75 cases) of the 106 victims where cases were randomly assigned to separation answered questions about this disposition.

Ninety-seven victims responded to questions about the arrest treatment. Sixty percent of the total reporting an arrest indicated that they did not want the police to arrest suspects, 65 percent reported that suspects blamed them for the arrests and 21 percent indicated that suspects threatened them because of the arrests. Ninety-three percent of the victims reported that the police presence stopped the violence, two-thirds (66%) of whom cited the arrest as the reason for the restoration of order. This was not surprising given that ninety-five percent of the victims reported that suspects went to jail. Twenty-nine percent of the victims reported the police as taking sides, 78 percent of whom reported the police as taking their (victims') sides. The mean estimated length of time that the police were on site was 37 minutes. The total time in custody (the time from the point of cuffing the suspect at arrest through release) was not available. Time in custody was measured as the period from booking to release. However, the minimum time an arrested person could be in custody, from the point of arrest to the point of booking, was estimated by police officers to be a little over one hour. Less than 20 percent of those booked for the experiment were released from custody within two hours. The average length of time in jail (from the point of booking through release to post bond,[14] as measured by the jail) was 15 hours and 46 minutes for those randomly assigned to arrest. Jail records also indicated that bond amounts assessed against suspects ranged from $50 to $850, most frequently for either $350 (65%) or for $100 (26%). About half (47%) of the suspects were released on bond and the other half (50%) were released after going to court. Three percent received pretrial releases or were transferred to other facilities. An arrest for domestic assault in Omaha was clearly not a trivial issue. Seventy-four percent (75 cases) of the 109 victims whose suspects were randomly assigned to arrest reported on the arrest experience.

Eighty-three victims responded to questions about the warrant treatment. Ninety-two percent of those responding (76 cases) indicated that when the police came they spent time talking to them about the assault and two-thirds (66%) felt that the police issued a warrant to support them. Victims estimated the average length of time responding officers spent on the call involving their assault was 34 minutes. The average length of the time that suspects were estimated by victims to

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[14] Jail records list the time at which a suspect is sent from the jail up to the third floor of the department to post bond as the point of release from jail; which should not be confused with a release from custody. Custody is not terminated until the bonding process is completed.

be away after the assault was two and one-half days and in 32 percent of the cases, suspects had not returned within at least a week of the presenting offense. Thirty percent were gone two hours or less while 48 percent were away for 24 hours or more. Victims also reported that about 50 percent of the suspects knew that a warrant was to be issued for their arrests.[15] In the majority of these instances suspects learned of pending warrants from either the victim (51%) or from a relative-friend (26%). The results of searches of the records of the Prosecuting Attorney's Office revealed that warrants had been issued for 87 (78%) of the 111 suspects randomly assigned to receive warrants. Sixty-eight percent of the cases for which warrants had been issued were arrested on those warrants during the six-month followup period.[16] The Prosecuting Attorney was also found to have decided against issuing warrants for 5 of the cases (4.5%) and no record that warrants had been issued could be found for 19 of the cases (17%). Notwithstanding the lack of evidence for the delivery of warrants for 17 percent of the cases assigned to warrants, the proportion of suspects actually warranted and subsequently arrested was substantial. Seventy-five percent (83 cases) of the 109 victims who were randomly assigned to receive a warrant also reported on the warrant treatment.

One hundred and fifteen victims responded to questions about the <u>no warrant</u> treatment. Eighty-seven percent said that the police talked to them about the incident in which they were assaulted and 92 percent (106 cases) said that the police asked them if they wanted to file a complaint against the suspect. Sixty-seven percent (77 cases) responded affirmatively and the police took a formal

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[15] All warranted suspects eventually should have known that a warrant for their arrest was issued. A letter is sent to suspects at the time warrants are issued by the Prosecuting Attorney's office advising them of the warrants and inviting suspects to come in and resolve the issue.

[16] Ford (1983) reports a 62 percent chance of an arrest resulting from a warrant affidavit for a 6 month follow-up period in Marion County, Indiana.

complaint. Sixty-nine percent (79 cases) reported that they wanted a warrant filed for the suspect's arrest in spite of the fact that the police took no action in this regard. However, the police did as a standard response, advise victims of their right to seek warrants for suspects' arrests through the Prosecuting Attorney's office. The average length of time that suspects were away after assaulting victims was two days, although about one-third (32%) returned within two hours of leaving and one-third (31%) after 24 hours or more. The no warrant treatment consisted, by in large, of taking complaints and "advising warrant." Eighty percent (109 cases) of the 136 victims whose partners were randomly assigned to the no warrant disposition responded to the no warrant treatment questions.

DATA SOURCES

The Omaha experiments are based on three general data sources; victim reports, Domestic Violence Report forms and police and court records.

1. Victim Reports. Victims were interviewed three times over a one year period about prior experiences with domestic violence, the presenting offense and subsequent feelings about and experiences with suspects. Although the information obtained from victims is quite comprehensive, the only victim reported information used in the analyses for this report involved demographic characteristics, a few background measures used as control variables and four outcome measures. Inasmuch as this report focuses upon the replication of Minneapolis, variables used in the Omaha study were limited to the kinds of variables used in the analyses of the Minneapolis Experiment. Additional analyses are currently in progress, however, in which most of the victim reported measures obtained in Omaha are used extensively to test a large number of hypotheses about domestic violence (Dunford and Elliott, 1989). For example, in addition to a variety of background measures for both victims and suspects, a modified Conflict Tactic Scale (Straus, 1979) was included to be used to assess changes in the context of conflict over different time

periods. Detailed data on the particulars of repeated domestic incidents involving injury were also obtained from victims as were a variety of measures thought to be associated with domestic violence (e.g., empowerment, cycles of violence, drug and alcohol use, prior exposure to domestic violence, self-esteem, depression, locus of control, fear).

2. Domestic Violence Reports. When police officers encountered domestic disturbances of any sort they were to fill out a Domestic Violence Report and send it to headquarters at the end of each shift, along with all of their other reports. Although it was impossible to determine how faithful officers were in this regard, Domestic Violence Reports were turned in for all but two of the eligible cases referred to the experiment. These reports contained officer accounts of the presenting offense along with demographic information for victims and suspects.

3. Official Records. The records of the Police Record bureau, the jail, and the court were searched at six and twelve months to determine the incidence of arrests, complaints and warrants for old and new offenses. The date and type of each offense was recorded for each offense record found, along with the results of court actions, when they were known.

OMAHA-MINNEAPOLIS COMPARISONS

Having presented an overview of the Minneapolis Experiment and the details of the Omaha replication, a comparison of the major characteristics of the two are shown in Table 6. All of the data on the Minneapolis Experiment of Table 6 were obtained from Sherman and Berk's original reports (Sherman and Berk, 1984a, 1984b). While the two experiments are quite similar across most of the comparisons specified in Table 6, they differ on a few key issues. Differences in the penalties resulting from court appearances associated with random assignment to arrest (e.g., time spent in jail), differences in the areas of the cities covered

Table 6

A Comparison of Selected Characteristics for the Minneapolis and Omaha Experiments

| | Minneapolis Experiment | Omaha Replication |
|---|---|---|
| 1. Relationship of suspect to victim | | |
| Divorced or separated husband | 3% | 1% |
| Unmarried lover/boyfriend | 45% | 39% |
| Ex-lover/boyfriend | -- | 9% |
| Current husband | 35% | 42% |
| Wife/girlfriend/ex-girlfriend | 2% | 4% |
| Relative, roommate, other | 15% | 5% |
| 2. Type of offenses eligible for the experiments: | | |
| Misdemeanor assault | Yes | Yes |
| Menacing (with probable cause for arrest) | No | Yes |
| 3. Conditions that nullified eligibility: | | |
| Victim insistence on the arrest of suspect | Yes | No |
| Outstanding & verified warrants | Yes | Yes |
| Victim in imminent danger | Yes | Yes |
| Case previously entered into the experiment | No | Yes |
| Male victims | No | No |
| Assault on officers (before random assignment) | Yes | Yes |
| Suspect absent | Yes | Yes[a] |
| Underage (18 or younger) | Yes | Yes |
| 4. Inclusiveness of the Experiments: | | |
| Total city coverage | No[b] | Yes |
| Twenty-four hour coverage | Yes | No[c] |
| All officers on shift involved | No[d] | Yes |
| Every day | Yes | Yes |
| 5. Police information: | | |
| Number of officers eligible to make referrals | 52 | 194 |
| Number of case contributors | | 163 |
| 6. Concentration of refusals: | | |
| 3 officers = | 28% | 12% |
| 7. Eligibility determined temporally prior to knowledge of randomized treatment: | No | Yes |
| 8. Sampling period: | 16½ mos. | 18 mos. |

a A second experiment was conducted in Omaha focusing upon cases in which suspects were absent when the police came.
b Two precincts--until a third precinct was added during the experiment.
c 4 p.m to midnight.
d Specifically trained domestic violence officers only.

| Table 6 (Continued) | Minneapolis Experiment | Omaha Replication |
|---|---|---|
| 9. Sample sizes: | | |
| Mediate | 92 | 115 |
| Separate | 108 | 106 |
| Arrest | 114 | 109 |
| Total | 314[e] | 330[f] |
| Mean number of referrals per month | 18.5 | 18.3 |
| 10. Follow-up Period | 6 mos. | 6 mos.[g] |
| 11. Proportion of cases misapplied | 17.8[h] | 7.9 |
| 12. Interview data: | | |
| Proportion of initial interviews completed | 62% | 80% |
| Proportion of interviews completed after 6 month follow-up | 49% | 73% |
| Face-to-face interviews only | No | Yes |
| Female interviewers | Yes | Yes |
| Interviewer-victims matched ethnically | No | Yes |
| Payments to victims interviewed-all interviews | No | Yes |
| 13. Outcome measures: | | |
| Official arrest for repeated domestic conflict of any sort | Yes | Yes |
| Official complaint reports taken from victims by police officers | Yes | Yes |
| Reports by project staff of police interventions for repeated domestic conflict | Yes | No |
| Victim reports of the number of episodes in which she/he: | | |
| a) Was actually assaulted | Yes | Yes[i] |
| b) Was threatened with assault | Yes | No |
| c) Had property damaged | Yes | No |
| d) Felt in danger of being physically hurt | No | Yes |
| e) Was pushed, hit or hands laid on them | No | Yes |
| f) Was physically injured | No | Yes |
| g) Date of 1st, 2nd, 3rd victim reported repeat episodes with injury | No | Yes |
| Outcome measures disaggregated for analyses | No | Yes |

e The total number of cases reported for Minneapolis is actually 330, but 16 cases were dropped because no treatment was applied or because they were viewed as not belonging in the study. The fact that both the Minneapolis and the Omaha Experiment appear to contain the same number of cases is purely coincidental.

f Repeat cases were not treated as new cases in Omaha as they were in Minneapolis.

g 12-month follow-up measures were also obtained in Omaha, but were not available for this report.

h Because of the variability between definitions of misapplication care must be exercised in the interpretation of these proportions. The rate for Minneapolis was calculated from Table 1, Sherman and Berk, 1984a.

i Victim reports of assault was determined in Omaha on the basis of four different measures (see d through g), each measure providing a different dimension of assault.

Victim reported data are based upon 205 initial interviews in Minneapolis and 263 initial interviews in Omaha.

Table 6 (Continued)

| | Minneapolis Experiment | Omaha Replication |
|---|---|---|
| 14. Proportion of those randomized to the arrest treatment sentenced to jail/probation/fines | 2% | 64% |
| 15. Unemployment | | |
| Victims | 61% | 50% |
| Suspects | 60% | 31% |
| 16. Prior assaults and police involvement | | |
| Victims assaulted by suspect in prior 6 months | 80% | 83%[j] |
| Police intervention in domestic dispute, last 6 months | 60% | unknown |
| Victims reporting the police ever coming to victim's assistance because the suspect was hitting or threatening her/him | unknown | 64% |
| Couple in counseling program at time of initial interviews | 27% | 11% |
| 17. Prior arrests of male suspects: | | |
| Ever arrested for any offense | 59% | 65% |
| Ever arrested on domestic violence statute | 5% | unknown |
| Ever arrested for any offense against victim | unknown | 11% |
| Arrested for any offense against victims in prior 6 months | unknown | 3% |
| 18. Mean age | | |
| Victims | 30 years | 31 years |
| Suspects | 32 years | 31 years |

| | Minneapolis | | Omaha | |
|---|---|---|---|---|
| | Victims | Suspects | Victims | Suspects |
| 19. Education | | | | |
| >high school | 43% | 42% | 34% | 31% |
| high school only | 33% | 36% | 43% | 50% |
| <high school | 24% | 22% | 23% | 19% |
| 20. Ethnicity | | | | |
| White | 57% | 45% | 56% | 50% |
| Black | 23% | 36% | 37% | 43% |
| Hispanic | -- | -- | 3% | 4% |
| Native American | 18% | 16% | 4% | 3% |
| Other | 2% | 3% | .3% | -- |

j Assaults and police involvement were determined in Omaha by victim reports which excludes those not interviewed.

by the experiments, differences in interview completion rates and differences in outcome measures and the way they were aggregated may affect the relevance of two experiments for one another.

FINDINGS, REPLICATION EXPERIMENT

The effects of treatments/dispositions as randomly assigned was examined using each of the five outcome measures (official arrest and complaints and victim reports of repeated violence) obtained during the six month period following the date of entry into the experiment. Data are presented for each comparison and statistical assessments of differences in the prevalence and frequency of repeat offending are made. In comparisons using victim reports, missing cases are included in the analyses to allow for examination of the effect of missing data on the experimental design.[17]

Findings for the two official measures of failure (arrests and complaints) are presented in Table 7. The two are presented separately to facilitate interpretation. Arrest was defined as a repeat arrest for any violation in which the original suspect victimized the original victim or when an original victim and suspect pair came back into the experiment as a repeat eligible case (i.e., a misdemeanor assault). Repeat cases (i.e., cases referred by officers to the study for a second time) were counted like repeat arrests, because eligibility for inclusion in the experiment required the existence of probable cause for an arrest for a misdemeanor assault. Complaints were defined as official reports taken by police officers from the original victims implicating original suspects, as found in the Police Record Bureau.

A review of the prevalence rates and mean frequencies in Table 7 tells the outcome story, by in large, independent of statistical tests: arresting suspects

[17] The extent of missing data in the arrest and complaint data is unknown since the absence of an official record may be due to recording failures or to official actions taken in other police-court jurisdictions.

Table 7

Prevalence and Mean Frequency of Arrest and Complaint Recidivism
6 Months after the Presenting Offense
Replication

| Number of Official Actions | Arrest Recidivism: Mediate | Arrest Recidivism: Separate | Arrest Recidivism: Arrest | Arrest Recidivism: Total | Complaint Recidivism: Mediate | Complaint Recidivism: Separate | Complaint Recidivism: Arrest | Complaint Recidivism: Total |
|---|---|---|---|---|---|---|---|---|
| 0 | 105<br>91.3 | 94<br>88.7 | 96<br>88.1 | 295 | 98<br>85.2 | 87<br>81.1 | 90<br>82.6 | 275 |
| 1 | 9<br>7.8 | 12<br>11.3 | 10<br>9.2 | 31 | 15<br>13.0 | 15<br>14.2 | 12<br>11.0 | 42 |
| 2 | -- | -- | 3<br>2.8 | 3 | 2<br>1.7 | 3<br>2.8 | 5<br>4.6 | 10 |
| 3 | 1<br>.9 | -- | -- | 1 | -- | 1<br>.9 | 1<br>.9 | 2 |
| 4 | -- | -- | -- | | -- | -- | 1<br>.9 | 1 |
| Total | 115 | 106 | 109 | 330 | 115 | 106 | 109 | 330 |
| Prevalence* | 8.7 | 11.3 | 11.9 | | 14.8 | 17.9 | 17.4 | |
| Frequency** | .104 | .113 | .147 | | .165 | .226 | .266 | |

| | Arrest Recidivism | Complaint Recidivism |
|---|---|---|
| | *Chi-Square .700 DF=2 p=.705 | *Chi-Square .460 DF=2 p=.794 |
| | **F=.385 p=.681 | **F=.950 p=.388 |
| **Mediate vs. Separate | t value=.19 DF=217 p=.851 | t value= .94 DF=197 p=.350 |
| **Mediate vs. Arrest | t value=.78 DF=216 p=.435 | t value=1.34 DF=178 p=.183 |
| **Separate vs. Arrest | t value=.66 DF=200 p=.513 | t value=.48 DF=205 p=.635 |

had no more effect in deterring future arrests or complaints (involving the same suspects and victims) than did separating or counseling them. The overall statistical comparisons revealed no significant differences in the prevalence or frequency of offending between treatment groups.

When the crimes associated with the repeat arrests noted in Table 7 were tabulated by offense charge, twenty-seven of the charges (68%) were for assault, eight (20%) were for disorderly conduct, two (5%) were for criminal mischief and one each (2.5%) were for trespass, failing to leave on request and destruction of property. When statistical tests for differences in arrest outcome were limited to the twenty seven repeat assault cases, no differences by treatments were found.

Comparisons of victim reports of repeated violence, as shown in Table 8, also resulted in no statistically significant differences between the treatment groups, although the interpretation of the findings was complicated somewhat by the treatment of missing data. When the analyses presented in Table 8 were repeated without missing data as an outcome (to check the possible effect missing data had on the statistical tests), again there were no statistically significant differences (Table not shown). In comparing the two analyses (with and without missing data) the prevalence and frequency of victim reported injury for the arrest treatment group was slightly lower, but not statistically significant and this difference was not found in comparisons involving the other two failure outcomes. Given the absence of any statistically significant differences, the hypothesis of no differences between groups cannot be rejected. It is thus concluded that victims whose partners were arrested were no less likely to experience repeated violence from that partner than were victims whose partners received a randomized separate or mediate disposition from the police. It is important to note that the data do not favor any specific type of treatment. Repeated domestic violence did not appear to be related to police decisions to arrest suspects, to separate them

Table 8

Victim Reported Outcomes During the 6-Month Followup Period
Replication

| Outcome | Victim Felt Endangered+ | | | | Victim Pushed or Hit+ | | | | Victim Physically Injured+ | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mediate | Separate | Arrest | Total | Mediate | Separate | Arrest | Total | Mediate | Separate | Arrest | Total |
| Yes | 41 | 39 | 44 | 124 | 35 | 34 | 29 | 98 | 23 | 22 | 16 | 61 |
| | 35.7 | 36.8 | 40.4 | | 30.4 | 32.1 | 26.6 | | 20.0 | 20.8 | 14.7 | |
| No | 44 | 41 | 33 | 118 | 50 | 46 | 48 | 144 | 62 | 58 | 61 | 181 |
| | 38.7 | 38.7 | 30.3 | | 43.5 | 43.4 | 44.0 | | 53.9 | 54.7 | 56.0 | |
| Missing | 30 | 26 | 32 | 88 | 30 | 26 | 32 | 88 | 30 | 26 | 32 | 88 |
| | 26.1 | 24.5 | 29.4 | | 26.1 | 24.5 | 29.4 | | 26.1 | 24.5 | 29.4 | |
| Total | 115 | 106 | 109 | 330 | 115 | 106 | 109 | 330 | 115 | 106 | 109 | 330 |
| Frequency** | 2.576 | 1.875 | 2.416 | | 1.482 | 1.750 | 2.104 | | .635 | .800 | .558 | |

| | Victim Felt Endangered+ | Victim Pushed or Hit+ | Victim Physically Injured+ |
|---|---|---|---|
| | Chi-Square 2.220 DF=4 p=.695 | Chi-Square 1.072 DF=4 p=.899 | Chi-Square 1.828 DF=4 p=.767 |
| | **F=.176 p=.838 | **F=.319 p=.727 | **F=.244 p=.784 |
| **Mediate vs. Separate | t value=.54 DF=115 p=.590 | t value= .40 DF=156 p=.689 | t value= .45 DF=129 p=.654 |
| **Mediate vs. Arrest | t value=.12 DF=137 p=.908 | t value= .76 DF=127 p=.452 | t value= .26 DF=151 p=.794 |
| **Separate vs. Arrest | t value=.61 DF=139 p=.545 | t value= .41 DF=140 p=.685 | t value= .62 DF=142 p=.536 |

+Question: "How many times since the event we talked about 6 months ago have you and (Offender) been involved in a fight or disagreement <u>in which you felt</u> that you were in danger of being physically hurt?"

+Question: "How many times since the event we talked about 6 months ago has (Offender) actually pushed, hit or laid hands on you in some way as part of a fight or disagreement you were having?"

+Question: "In how many fights or disagreements were you physically injured (e.g., knocked down, bruised, scratched, cut, choked, bones broken, eyes or teeth injured?"

from victims or to mediate disputes in which they were involved. Furthermore, when similar analyses were conducted controlling for prior arrests, controlling for ethnicity, limiting cases to couples in conjugal relationships at the presenting offense, or to cases in which cohabitants had lived together for the entire six month follow-up period or to cases involving persons who had lived together for at least some time during the follow-up period, the same outcome was observed.[18]

The seven percent misapplication rate reported earlier, prompted the collapsing of the two unofficial police responses (mediate and separate) into one category in order to reduce the effects of misapplication. This procedure also facilitated tests for the effects of arrests (or punishment) versus no arrest (or no punishment) as suggested by Binder and Meeker (1988) for a test of the deterrence hypothesis. The two non-arrest treatments are conceptualized as informal police responses to domestic violence. This "informal" treatment group could then be compared with an arrest group, where arrest is considered a "formal" police response to assault. This conceptualization limits the assumptions made about the content of treatment to the presence or absence of an arrest. Collapsing the data in this way reduced the level of misapplication to three percent, eliminating over 50 percent of the disjunction between treatment assigned and treatment delivered while maintaining the integrity of the experimental design. The results of the comparisons involving formal and informal treatments are shown in Tables 9 and 10 and do not alter any of the conclusions drawn thus far. No statistical tests or consistent trends in the data favored arresting suspects as opposed to not arresting them, for any of the five outcome measures used to assess failure.

The analyses presented thus far include repeat cases as failures and not as new cases. That is, in the Omaha Replication repeat cases found eligible for the

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[18] Analyses of the effect of Treatment as Delivered on each of the five outcome measures outlined above resulted in the same findings as reported for Treatment as Assigned; no statistical differences were found.

Table 9

Prevalence (P) and Mean Frequency ($\bar{X}$) of Arrest and Complaint Recidivism
6-Months After the Presenting Offense
Informal vs. Formal Treatment

Replication

| Number of Official Actions | Arrest Recidivism Informal | Arrest Recidivism Formal | Arrest Recidivism Total | Complaint Recidivism Informal | Complaint Recidivism Formal | Complaint Recidivism Total |
|---|---|---|---|---|---|---|
| 0 | 199<br>90.0 | 96<br>88.1 | 295 | 185<br>83.7 | 90<br>82.6 | 275 |
| 1 | 21<br>9.5 | 10<br>9.2 | 31 | 30<br>13.6 | 12<br>11.0 | 42 |
| 2 | -- | 3<br>2.8 | 3 | 5<br>2.3 | 5<br>4.6 | 10 |
| 3 | 1<br>.5 | -- | 1 | 1<br>.5 | 1<br>.9 | 2 |
| 4 | -- | -- | -- | | 1<br>.9 | 1 |
| Total | 221 | 109 | 330 | 221 | 109 | 330 |
| Prevalence* | 10.0 | 11.9 | | 16.3 | 17.4 | |
| Frequency** | .109 | .147 | | .195 | .266 | |

Arrest Recidivism:
*Chi-Square .299 DF=1 p=.584
**F=.742 p=.390
t value= .81 DF=183 p=.420

Complaint Recidivism:
*Chi-Square .068 DF=1 p=.794
**F=1.224 p=.269
t value=.99 DF=163 p=.324

Table 10

Victim Reported Outcome of Fear of Being Physically Hurt,
of Being Pushed, Hit or Manhandled, or of Being Physically Injured
by the Suspect During the 6-Month Follow-up Period by Treatment

Informal vs. Formal Treatment
Replication

| Prevalence | Victim Felt Endangered+ | | | Victim Pushed or Hit+ | | | Victim Physically Injured+ | | |
|---|---|---|---|---|---|---|---|---|---|
| | Informal | Formal | Total | Informal | Formal | Total | Informal | Formal | Total |
| Yes | 80 | 44 | 124 | 69 | 29 | 98 | 45 | 16 | 61 |
| | 36.2 | 40.4 | | 31.2 | 26.6 | | 20.4 | 14.7 | |
| No | 85 | 33 | 118 | 96 | 48 | 144 | 120 | 61 | 181 |
| | 38.5 | 30.3 | | 43.4 | 44.0 | | 54.3 | 56.0 | |
| Missing | 56 | 32 | 88 | 56 | 32 | 88 | 56 | 32 | 88 |
| | 25.3 | 29.4 | | 25.3 | 29.4 | | 25.3 | 29.4 | |
| Total | 221 | 109 | 330 | 221 | 109 | | 221 | 109 | 330 |
| Frequency** | 2.236 | 2.416 | | 1.612 | 2.104 | | .715 | .558 | |

| | Victim Felt Endangered+ | Victim Pushed or Hit+ | Victim Physically Injured+ |
|---|---|---|---|
| | *Chi-Square 2.148 DF=2 p=.342 | *Chi-Square .972 DF=2 p=.615 | *Chi-Square 1.754 DF=2 p=.416 |
| | **F=.027 p=.869 | **F=.520 p=.472 | **F=.262 p=.609 |
| Informal vs. Formal | t value=.18 DF=193 p=.855 | t value=.63 DF=111 p=.529 | t value=.54 DF=171 p=.589 |

research (i.e., cases in which the police were called a second time and probable cause for a domestic misdemeanor arrest existed) were not treated as new cases, but as failures of cases already entered into the study. In order to better replicate the Minneapolis Experiment in which repeat cases were treated as new cases, the analyses conducted above were replicated on a reconstructed data set in which repeat cases in Omaha were counted not only as failures for originally submitted cases, but as new cases as well. Data from official records lent themselves nicely to these adjustments since all officially recorded actions taken by the police and court contained the dates that each action was taken. Thus, all post-entry violations involving a repeat case could be assigned to the original case or the new case depending upon the date of occurrence; all violations occurring after the date of the newly created case were assigned to the new case.[19] Unfortunately, the only victim reported repeat violence for which dates were obtained were for physical injuries and the analysis treating repeat cases as new cases is limited to official record measures and victim reported injuries.[20]

Analyses of the reconstructed data set are shown in Table 11 and do not alter the previous findings. No consistent patterns of outcome or statistical tests for differences among treatment groups favored arrest as a conflict reducing treatment compared to mediation or separation.

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[19] Most repeat calls for domestic violence found in official records were not eligible as new cases in the study (lacked probable cause, were too serious, occurred on other shifts, etc.) and thus had not returned to the experiment as repeat cases.

[20] Victims were asked to provide the dates of the new arguments or fights they had with the suspect for the first three conflicts in which they were physically injured after the presenting offense. The dates associated with the resulting first three repeat instances of injury accounted for 88.5 percent of all of the physical injuries victims reported for the 6-month follow-up period. Seven victims reported injuries associated with episodes for which dates were not obtained (three for cases mediated and two each for cases separated and arrested). Given the even distribution of cases involving more than three new injuries, the decision was made to use episodes-of-injuries-with-dates for a victim reported measure of failure for the repeat cases as new cases analyses.

Table 11

Prevalence and Mean Frequency of Arrest, Complaint and Victim Reported
Injury Recidivism 6-Months after the Presenting Offense
Repeat Cases as New Cases
Replication

| Number of Official Actions | Arrest Recidivism Mediate | Separate | Arrest | Total | Complaint Recidivism Mediate | Separate | Arrest | Total | Outcome | Victim Physically Injured+ Mediate | Separate | Arrest | Total |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 107 | 96 | 100 | 303 | 97 | 87 | 93 | 277 | Yes | 21 | 24 | 18 | 63 |
| | 91.5 | 87.3 | 87.7 | | 82.9 | 79.1 | 81.6 | | | 17.9 | 21.8 | 15.8 | |
| 1 | 9 | 14 | 13 | 36 | 18 | 19 | 17 | 54 | No | 66 | 60 | 61 | 187 |
| | 7.7 | 12.7 | 11.4 | | 15.4 | 17.3 | 14.9 | | | 56.4 | 54.5 | 53.5 | |
| 2 | 1 | -- | 1 | 2 | 2 | 3 | 3 | 8 | Missing | 30 | 26 | 35 | 91 |
| | .9 | | .9 | | 1.7 | 2.7 | 2.6 | | | 25.6 | 23.6 | 30.7 | |
| 3 | -- | -- | -- | | -- | 1 | -- | 1 | Total | 117 | 110 | 114 | 341 |
| | | | | | | .9 | | | | | | | |
| 4 | -- | -- | -- | | -- | -- | 1 | 1 | Frequency** | .471 | .774 | .595 | |
| | | | | | | | .9 | | | | | | |
| Total | 117 | 110 | 114 | 341 | 117 | 110 | 114 | 341 | | | | | |
| Prevalence* | 8.5 | 12.7 | 12.3 | | 17.1 | 20.9 | 18.4 | | | | | | |
| Frequency** | .094 | .127 | .132 | | .188 | .254 | .237 | | | | | | |

| | Arrest Recidivism | Complaint Recidivism | Victim Physically Injured+ |
|---|---|---|---|
| | *Chi-Square 1.224 DF=2 p=.542 | *Chi-Square .555 DF=2 p=.758 | *Chi-Square 2.339 DF=4 p=.674 |
| | **F=.421 p=.657 | **F=.492 p=.612 | **F=.444 p=.640 |
| **Mediate vs. Separate | t value=.76 DF=223 p=.446 | t value=1.01 DF=207 p=.315 | t value=.91 DF= 19 p=.366 |
| **Mediate vs. Arrest | t value=.83 DF=224 p=.407 | t value= .72 DF=208 p=.473 | t value=.47 DF=135 p=.641 |
| **Separate vs. Arrest | t value=.09 DF=222 p=.927 | t value= .23 DF=222 p=.815 | t value=.48 DF=151 p=.633 |

TOTAL RECIDIVISM, REPLICATION EXPERIMENT

While it can be assumed that arrest records most typically represent a subset of the behaviors for which people can be arrested, it cannot be assumed for the Omaha experiment that victims always reported (when interviewed) the abusive behavior for which their cohabitant partners were arrested. At issue is whether all official arrests in Omaha were captured by or reflected in victim reports of abuse suffered at the hands of suspects. If they are not, it would be possible to construct a new measure of total recidivism which would reflect <u>any</u> recidivism, official or victim-reported.

To assess the overlap between arrest records for assault and victim reports of repeated violence for the six month follow-up period, all three measures of victim reports of violence were aggregated for prevalence (fear of injury, pushing-hitting, physical injury) and were then crosstabulated with the prevalence of official arrests of suspects for violence directed towards victims (see Table 12). Eighty-three percent of the arrests for violence appear to be captured by victim reports of new violence (or fear of new violence), at least this is the case for a prevalence measure of repeated violence. Also, arrests were found for only one percent of the suspects for whom no victim reports of new violence were recorded. It appears that the victim report measure of prevalence captures nearly all cases that could be classified as repeat cases based upon the records of the Omaha Police Record Bureau. Of interest as well, is the finding that arrests were found in only 4 percent of the cases for whom some violence (or fear of violence) was reported by the victim. Very little of the violent and abusive behavior associated with domestic violence appears to be captured by official arrest records.

Notwithstanding the finding that victim reports of repeated violence incorporated nearly all official arrests in Omaha, the overlap was not complete. A new composite measure of the prevalence of repeat offending was thus created

Table 12

Officially Recorded Repeat Arrests of Suspects for Assaults on Victims
by Victim Reports of Repeated Violence

Replication

| Repeat Arrests for Assaults | | Victim Reports of Repeated Violence: No | Yes | Total |
|---|---|---|---|---|
| No | N | 183 | 243 | 426 |
| | % | 43.0 | 57.0 | 100 |
| | % | 98.9 | 96.0 | |
| Yes | N | 2 | 10 | 12 |
| | % | 16.7 | 83.3 | 100 |
| | % | 1.1 | 4.0 | |
| Total | N | 185 | 253 | 438 |
| | % | 100 | 100 | |

consisting of all three victim reported outcome measures (fear of injury, pushing/shoving, physical injury) plus official arrests and complaints. Treatment groups were then compared using this total recidivism composite. As shown in Table 13, no statistically significant differences between treatment groups were found. When the experimental conditions mediation and separation were collapsed (Informal) and compared with arrest (Formal), again no statistically significant differences were found (Table 13). The results of the analysis of the total recidivism for the offender present experiment were consistent with those reported earlier. Arresting suspects of misdemeanor domestic assault in Omaha did not appear to be any more effective in reducing the prevalence of subsequent domestic conflict than did either of the other two experimental treatments whether outcome measures were used independently or were combined.[21]

Before leaving the discussion of the replication findings, a comment on the power of the tests employed is in order. Because the null hypothesis of no difference in outcome between treatment groups was not rejected in the Omaha replication of the Minneapolis Experiment the question of statistical power becomes an issue (Cohen, 1977). The question is, did the statistical tests have sufficient power to detect substantial differences between groups if such differences existed? To examine this issue, power analyses for the Omaha replication were based on a null hypothesis of no differences in the prevalence of repeat offending between those cases randomly assigned to arrest treatment and those randomly

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[21] As additional checks on the effects of experimental treatments, additional composite measures of the prevalence of recidivism were developed based on the three victim reported measures of recidivism and the two official measures of recidivism. These measures were aggregated a number of different ways and were compared by experimental treatments. For example, the three measures of victim reports were combined with arrests for assaults and with complaints for assaults; victim reported measures for hitting and for injury were combined with assault arrests; and the hit and injury measures were combined. Altogether seven different combinations of the five failure measures were created and analyzed, with and without missing data, as tests for the effects of randomized treatments. No statistically significant differences were found.

Table 13

The Prevalence of Total Recidivism* for the 6-Month Follow-up Period
Replication

| | Total Recidivism | | | |
|---|---|---|---|---|
| Treatment | Mediate | Separate | Arrest | Total |
| None | 35<br>30.4 | 33<br>31.1 | 25<br>22.9 | 93 |
| One or More | 50<br>43.5 | 47<br>44.3 | 52<br>47.7 | 149 |
| Missing | 30<br>26.1 | 26<br>24.5 | 32<br>29.4 | 88 |
| Total | 115 | 106 | 109 | 330 |

Chi Square 2.336 DF = 4 p = .674

| | Informal | Formal | Total |
|---|---|---|---|
| None | 68<br>30.8 | 25<br>22.9 | 93 |
| One or More | 97<br>43.9 | 52<br>47.7 | 149 |
| Missing | 56<br>25.3 | 32<br>29.4 | 88 |
| Total | 221 | 109 | 330 |

Chi Square 2.267 DF = 2 p = .322

*Fear of Injury + Pushed-Hit + Physical Injury + Arrests + Complaints assigned to non-arrest treatments. Because tests for power are in part a function of decisions about how much difference must exist between groups before a difference is acknowledged, the decision was made to set the difference at 10 percent, so that differences in recidivism must exceed 9 percent before a difference would be considered significant. This decision was made early during the development of the research design and was set relatively high to insure substantive differences before the null hypothesis could be rejected. Assessments for a Chi Square test of differences in the prevalence of rearrests for any new crimes against victims by suspects, with non-arrest and arrest group sizes of 221 and 109, alpha = .10 and a difference in proportion of 10 percent with $P_1 = .15$ and $P_2 = .05$ (proportions were selected to match anticipated recidivism rates), produced a power of .81. That is, the probability of rejecting the hypothesis of no difference when a substantial difference exists was .81. The same analysis for the prevalence of reinjury for a difference in proportion of 10 percent ($P_1$=.20; $P_2$=.10) yielded a power of .80. Assessments for power for F-tests for treatment differences in the mean frequency of arrest recidivism were also conducted. Given a sample of 330 cases with an approximately equal distribution of cases between three treatment groups (arrest, separation, mediation), Cohen's effect size set at .20 (representing .2 the standard deviation of the group means), the power of the test was .95. Thus, the probability of not rejecting the null hypothesis that the means of the three treatment groups came from the same population (i.e., are not different) when this hypothesis is in fact false, is .05. Similar analyses for differences in the mean frequency of victim reported reinjury produced a power of .88. Thus, in general, the failure to find statistical differences does not appear to be the result of a lack of statistical power.

TIME TO FAILURE, REPLICATION EXPERIMENT

Notwithstanding an inability to find differences in the prevalence and frequency of repeat offending between treatment groups six months after the presenting offense, it is still possible that one treatment may delay repeated instances of conflict longer than other treatments. If, for example, the Minneapolis Experiment (Sherman and Berk, 1984a) finding that arrest delayed recidivism for significantly longer periods of time compared to other treatments could be replicated, it would have policy implications quite independent of the earlier failure to replicate the Minneapolis prevalence outcomes in Omaha.

The analytical approach used in the time to failure analysis involved a simple non-parametric life table and survival analysis procedure employing the Kaplan Meier (1958) product-limit estimate of the survival distribution. As implemented, the procedure (Dixon et al., 1985) calculated the number of days to failure for arrest, complaints and victim reported injuries using the date of the presenting offense as the start point and the date of the first failure (arrest, complaint or injury) per case as the point of failure. The survival curves were then compared for equality over time, using the Mantel-Cox (1966) test for differences. The results of these procedures are illustrated in Figures 1 through 3.

The survival curves plotted for each of the three outcome measures reveal no consistent differences between groups and the results of the statistical tests indicate that the hypothesis of no differences in time to failure should not be rejected. There were no real differences between the treatment survival curves and when the small differences that did appear were reviewed, the results were inconsistent--arrest treatment tended to fail earliest when rearrest was the outcome, latest when victim reported injury was the outcome and in between the other two when complaint was the outcome.[22]

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22 When survival analyses were completed for Total Failure (repeat arrests + repeat complaints + victim reported reinjury) and for Total Assault (repeat arrests for assault + repeat complaints for assault + victim reported reinjury) no statistically significant differences in the survival curves were found. Nor was arrest favored in a visual comparison of the curves.

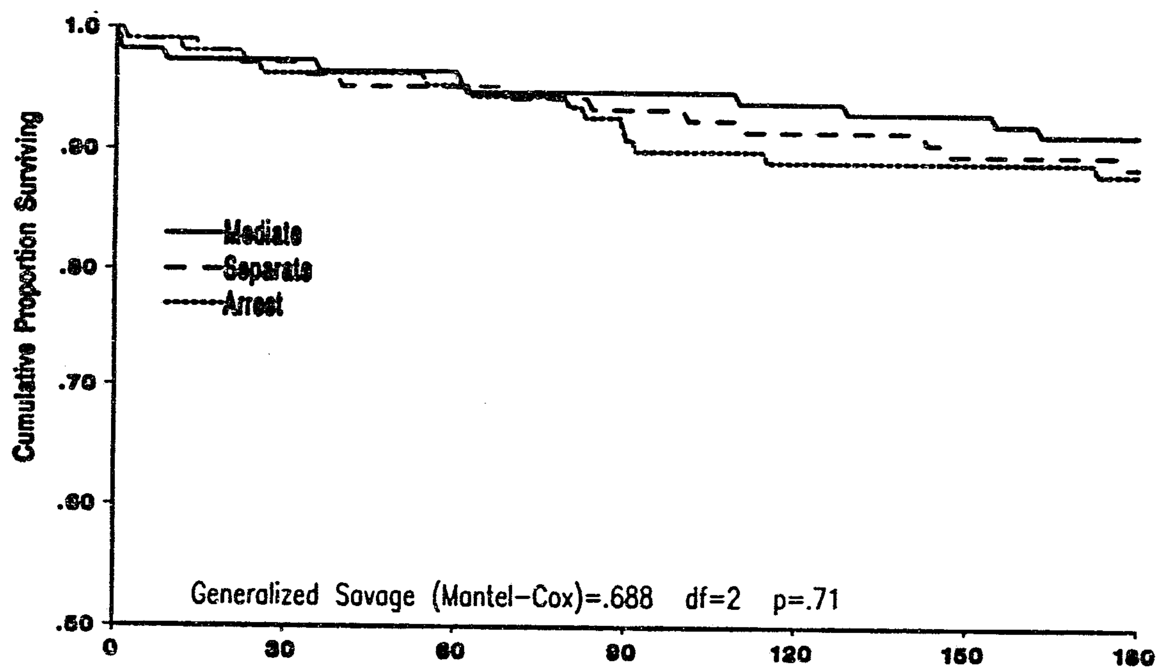


**Figure 1: Survival Functions for Arrest by Treatment Group, Replication Experiment, Original Sample**

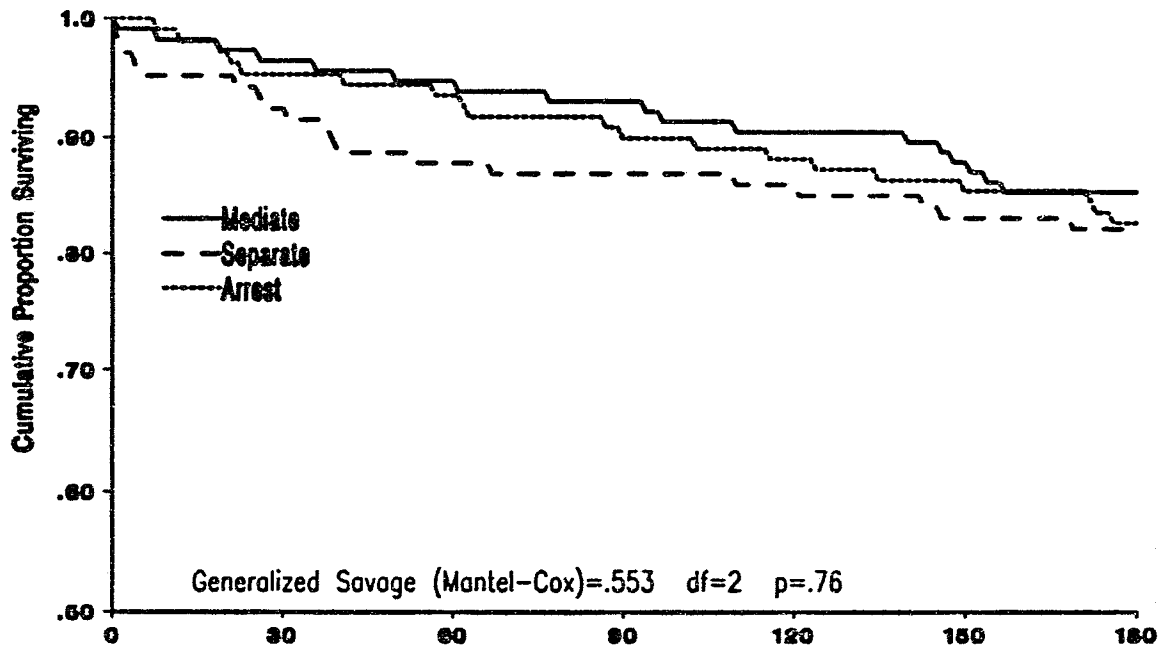


**Figure 2: Survival Functions for Complaint by Treatment Group, Replication Experiment, Original Sample**

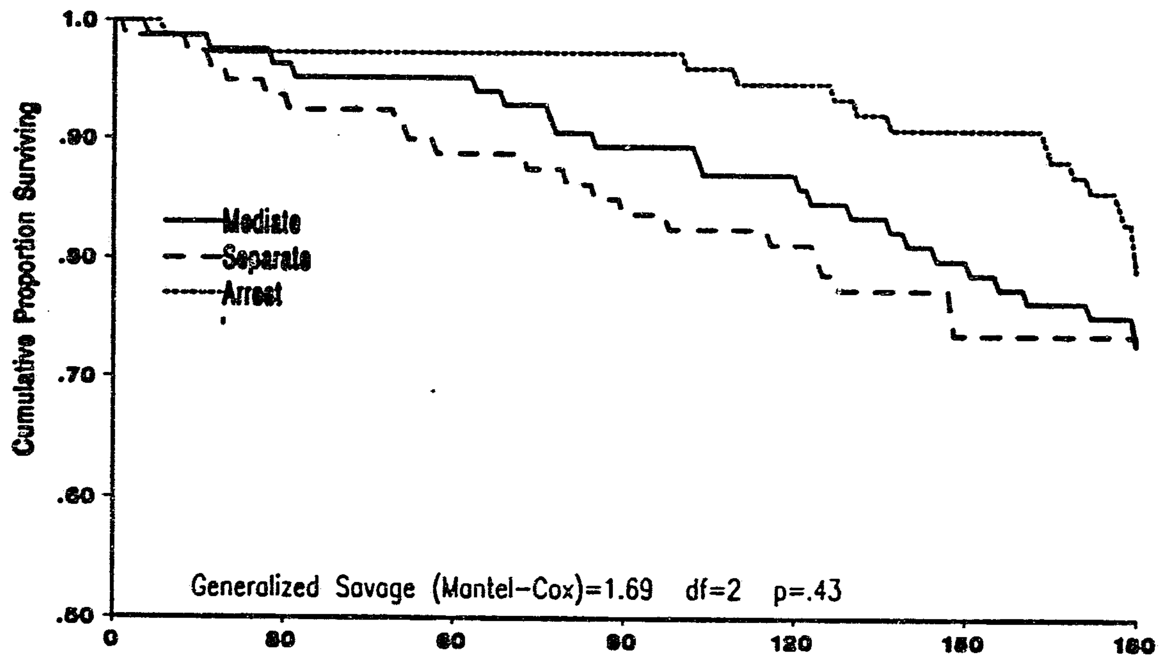


**Figure 3: Survival Functions for Victim-Reported Injury by Treatment Group, Replication Experiment, Original Sample**

Right hand censoring is an obvious problem for the survival analysis in the sense that a repeat conflict had not occurred for all cases by the end point of the analysis. Only eleven percent of the suspects in the replication experiment were rearrested for a crime against the original victim during the six-month follow-up period. It is noted, however, that the failures were evenly spread across the three treatment groups (Mediate=10, Separate=12, Arrest=13). The magnitude of the right hand censoring problem was similar for the other outcome measures (17% of the suspects had at least one official complaint filed against them and 18 percent of the victims reported that suspects had physically injured them in the interim 6 month period). Given the paucity and distribution of repeat cases found for the treatment groups and the obvious lack of substantive or statistical differences, the continued use of more elaborate and complex life table analyses were deemed unwarranted. After six months at risk, no one treatment group could be described as requiring more time to fail than any other treatment group.

FINDINGS, OFFENDER ABSENT EXPERIMENT

The analyses used to examine the outcomes of the offender absent experiment (warrant, no warrant) were identical to those outlined above for the replication experiment. Cases randomized to a warrant treatment were compared with those randomized to a no warrant treatment six months after the randomization took place. The results of these comparisons are shown in Tables 14 and 15. The most notable findings associated with these comparisons is the consistency with which those assigned to receive a warrant scored lower on both prevalence and frequency of repeat offending irrespective of the outcome measure used. Suspects randomly assigned to the no warrant treatment were always found to have substantively higher rates of repeated conflict compared to suspects assigned to the warrant treatment. Prevalence and frequency comparisons involving the official measures of recidivism (arrests and complaints) were all either statistically different or approached

Table 14

Prevalence and Mean Frequency of Arrest and Complaint Recidivism
6 Months after the Presenting Offense
Offender Absent

| Number of Official Actions | Arrest Recidivism Warrant | Arrest Recidivism No Warrant | Arrest Recidivism Total | Complaint Recidivism Warrant | Complaint Recidivism No Warrant | Complaint Recidivism Total |
|---|---|---|---|---|---|---|
| 0 | 105<br>94.6 | 120<br>88.2 | 225 | 95<br>85.6 | 106<br>77.9 | 201 |
| 1 | 5<br>4.5 | 13<br>9.6 | 18 | 11<br>9.9 | 21<br>15.4 | 32 |
| 2 | 1<br>.9 | 2<br>1.5 | 3 | 4<br>3.6 | 5<br>3.7 | 9 |
| 3 | -- | -- | -- | 1<br>.9 | 3<br>2.2 | 4 |
| 4 | -- | 1<br>.7 | 1 | -- | -- | -- |
| 6 | -- | -- | -- | -- | 1<br>.7 | 1 |
| Total | 111 | 136 | 247 | 111 | 136 | 247 |
| Prevalence* | 5.4 | 11.8 | | 14.4 | 22.1 | |
| Frequency** | .063 | .154 | | .198 | .338 | |

Arrest Recidivism:
*Chi-Square 3.046 DF=1 p=.081
**F=2.953 p=.087
**t value=1.81 DF=219 p=.071

Complaint Recidivism:
*Chi-Square 2.357 DF=1 p=.125
**F=2.445 p=.119
**t value=1.63 DF=236 p=.105

Table 15

Victim Reported Outcomes During the 6-Month Followup Period
Offender Absent

| | Victim Felt Endangered+ | | | Victim Pushed or Hit+ | | | Victim Physically Injured+ | | |
|---|---|---|---|---|---|---|---|---|---|
| Outcome* | Warrant | No Warrant | Total | Warrant | No Warrant | Total | Warrant | No Warrant | Total |
| Yes | 41 | 65 | 106 | 29 | 53 | 82 | 18 | 41 | 59 |
| | 36.9 | 47.8 | | 26.1 | 39.0 | | 16.2 | 30.1 | |
| No | 43 | 47 | 90 | 55 | 59 | 114 | 66 | 71 | 137 |
| | 38.7 | 34.6 | | 49.5 | 43.4 | | 59.6 | 52.2 | |
| Missing | 27 | 24 | 51 | 27 | 24 | 51 | 27 | 24 | 51 |
| | 24.3 | 17.6 | | 24.3 | 17.6 | | 24.3 | 17.6 | |
| Total | 111 | 136 | 247 | 111 | 136 | 247 | 111 | 136 | 247 |
| Frequency** | 3.560 | 5.348 | | 2.774 | 3.420 | | 1.476 | 1.991 | |

*Chi-Square 3.292 DF=2 p=.193
**F=.611 p=.436
**Warrant vs. No Warrant t value=.81 DF=194 p=.418

+Question: "How many times since the event we talked about 6 months ago have you and (Offender) been involved in a fight or disagreement <u>in which you felt</u> that you were in danger of being physically hurt?"

*Chi-Square 4.861 DF=2 p=.088
**F=.157 p=.693
t value=.41 DF=194 p=.683

+Question: "How many times since the event we talked about 6 months ago has (Offender) actually pushed, hit or laid hands on you in some way as part of a fight or disagreement you were having?"

*Chi-Square 6.865 DF=2 p=.032
**F=.158 p=.692
t value=.41 DF=192 p=.683

+Question: "In how many fights or disagreements were you physically injured (e.g., knocked down, bruised, scratched, cut, choked, bones broken, eyes or teeth injured?"

statistical significance. Those not warranted for assaulting victims were twice as likely to be subsequently arrested for an offense against the same victims as were those warranted, and similar differences in prevalence and frequency of offending were found for complaint comparisons.[23] The evidence for the deterrent effect of a warrant found in the official rearrest data are supported, in part, by victim reports of repeated violence. While two of the three prevalence comparisons for victim reports were statistically significant, the strongest effect is observed for the prevalence of repeated injury. This finding is consistent with the comparisons involving repeat arrests and may be so since episodes involving physical injury are those most likely to meet the probable cause requirements for arrest; i.e., this victim reported measure may be the closest parallel to the arrest measure. The differences in the frequency of repeat offending for the victim reported outcome measures are very small and statistically nonsignificant.

When controls were initiated and comparative analyses were repeated limiting the sample to respondents in conjugal relationships, to couples living together for the entire six month follow-up period and to couples living together for any portion of the follow-up period, the direction of the differences always favored the warrant treatment (i.e., less recidivism). Interestingly, while eight of the nine prevalence comparisons for victim reports of repeated conflict were found to be statistically significant using the controls specified, no statistically significant differences were found for prevalence comparisons based on the arrest outcome measure--a departure from the experimental finding.

To more accurately parallel the data base of the Minneapolis Experiment, the analyses for main effects of the experimental treatments reported here were

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[23] When the repeat arrests identified in Table 14 were analyzed for content by charges, 16 (57.1%) were for assaults, 6 (21.1%) were for disorderly conduct, two were for trespass and one each was for harassment, theft, failing to obey a court order or giving false information.

duplicated on a reconstructed data set in which cases coming back into the study as eligible repeat cases were treated as new cases (see Table 16). The procedures used for this analysis were described earlier for the replication experiment. While the direction of the differences emerging from the repeat cases as new cases analyses parallel those found for the original data set and the prevalence measure for victim reports of physical injury was statistically significant, comparisons for repeat arrests and complaints failed to emerge as statistically different in this analysis. It is difficult to assess the importance of this difference in findings using the reconstructed data set, given the inability to use the two other victim reports of failure (fear of injury and hitting-shoving) for the repeat case as new case comparisons.

TOTAL RECIDIVISM, OFFENDER ABSENT EXPERIMENT

Following the methodology described for the replication experiment, seven different composites of outcome were developed to assess the prevalence of recidivism. When the experimental treatments for the offender absent study were compared on total recidivism (all victim and official reported failure), findings were consistent with those reported earlier based upon single outcome measures (see Table 17). That is, cases for which the police petitioned for warrants were less likely to engage in repeated conflict over a six month follow-up period than were cases for which the police simply took reports of repeated incidents and advised victims of their rights. Similar findings were observed in five out of six of the composite measures used as outcomes for the offender absent study. The use of composite measures to assess the effects of treatment on the prevalence of recidivism showed consistent statistically significant differences which favored warrants compared to no warrants as a method of reducing domestic conflict when offenders were absent.

Table 16

Prevalence and Mean Frequency of Arrest and Complaint Recidivism
6 Months after the Presenting Offense

Repeat Cases as New Cases
Offender Absent

| Number of Official Actions | Arrest Recidivism Warrant | No Warrant | Total | Complaint Recidivism Warrant | No Warrant | Total | Outcome | Victim Physically Injured+ Warrant | No Warrant | Total |
|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 113<br>92.6 | 123<br>87.9 | 236 | 100<br>82.0 | 107<br>76.4 | 207 | Yes | 21<br>17.2 | 42<br>30.0 | 146 |
| 1 | 8<br>6.6 | 15<br>10.7 | 23 | 16<br>13.1 | 26<br>18.6 | 42 | No | 72<br>59.0 | 74<br>52.9 | 63 |
| 2 | 1<br>.8 | 1<br>.7 | 2 | 5<br>4.1 | 4<br>2.9 | 9 | Missing | 29<br>23.8 | 24<br>17.1 | 53 |
| 3 | — | 1<br>.7 | 1 | 1<br>.8 | 2<br>1.4 | 3 | Total | 122<br>1.409 | 140<br>1.914 | 262 |
| 5 | — | — | — | — | 1<br>.7 | 1 | | | | |
| Total | 122 | 140 | 262 | 122 | 140 | 262 | | | | |
| Prevalence* | 7.4 | 12.1 | | 18.0 | 23.6 | | | | | |
| Frequency** | .082 | .143 | | .238 | .321 | | | 1.409 | 1.914 | |

| | Arrest Recidivism | Complaint Recidivism | Victim Physically Injured+ |
|---|---|---|---|
| | *Chi-Square 1.656 DF=1 p=.198 | *Chi-Square 1.206 DF=1 p=.272 | *Chi-Square 6.860 DF=2 p=.032 |
| | **F=1.730 p=.190 | **F=1.094 p=.297 | **F=.174 p=.677 |
| **Warrant vs. No Warrant | t value=1.35 DF=251 p=.180 | t value=1.06 DF=258 p=.289 | t value=.43 DF=.207 p=.669 |

Table 17

The Prevalence of Total Recidivism* for the 6-Month Follow-up Period
Offender Absent

| | Total Recidivism | | |
|---|---|---|---|
| Outcome | Warrant | No Warrant | Total |
| None | 34<br>30.6 | 32<br>23.5 | 66 |
| One or More | 50<br>45.0 | 80<br>58.8 | 130 |
| Missing | 27<br>24.3 | 24<br>17.6 | 51 |
| Total | 111 | 136 | 247 |

Chi Square 4.678  DF = 2  p = .096

*Fear of Injury + Pushed-Hit + Physical Injury + Arrests + Complaints

TIME TO FAILURE ANALYSIS, OFFENDER ABSENT EXPERIMENT

Time to failure was assessed calculating the number of days to rearrest, new complaints, or victim reported injuries using the date of the original presenting offense as a starting point, and the date of the first failure for each of the three outcome measures as ending points. The survival curves were subsequently compared for differences and the results are shown in Figures 4 through 6. The plots for each of the outcome measures revealed a consistent pattern across all three measures and statistically significant differences ($p \leq .10$) were found in two of the three comparisons, all of which favored the warrant treatment. These data suggest that the time required to fail, as measured by repeat arrests or complaints for domestic violence or for victim reports of new injuries sustained in domestic incidents, was significantly longer for those assigned to warrant treatments than it was for those assigned to non-warrant treatments.[24]

Although there was some inconsistency across the outcome measures of the offender absent experiment, the findings are provocative and suggest that system initiated warrants may have an effect in reducing or delaying future domestic conflicts. These data are clearly different from those found for the replication experiment. They suggest that a pending arrest may be more of a deterrent than an actual arrest. The data indicate that when the police initiated warrants for suspects who were absent when they responded to calls for assistance in domestic cases, warranted suspects were less likely to engage in subsequent conflicts with cohabitants than were those for whom no warrants were sought. Further, the data suggest that law enforcement initiated warrants for missing suspects significantly extended the time to new conflict compared to instances in which victims were

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[24] When survival analyses were completed for Total Failure (repeat arrests + repeat complaints + victim reported reinjury) and for Total Assault (repeat arrests for assault + repeat complaints for assault + victim reported reinjury) strong statistically significant differences were found which favored the Warrant Treatment as prolonging time to failure.

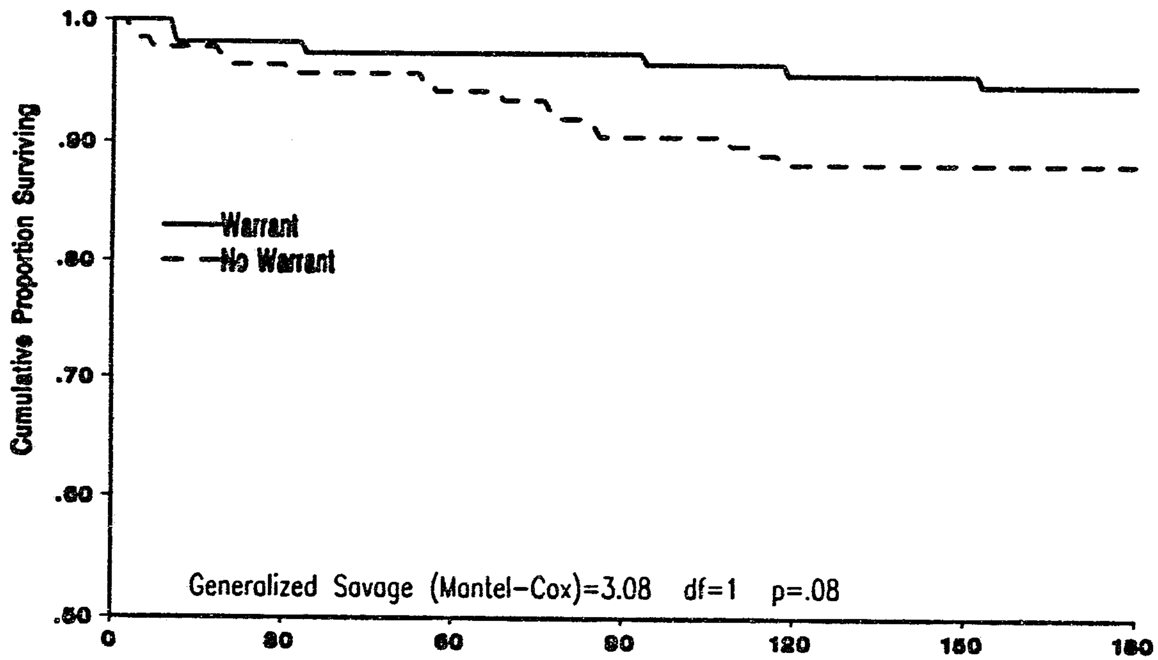


**Figure 4: Survival Functions for Arrest by Treatment Group, Offender Absent, Original Sample**

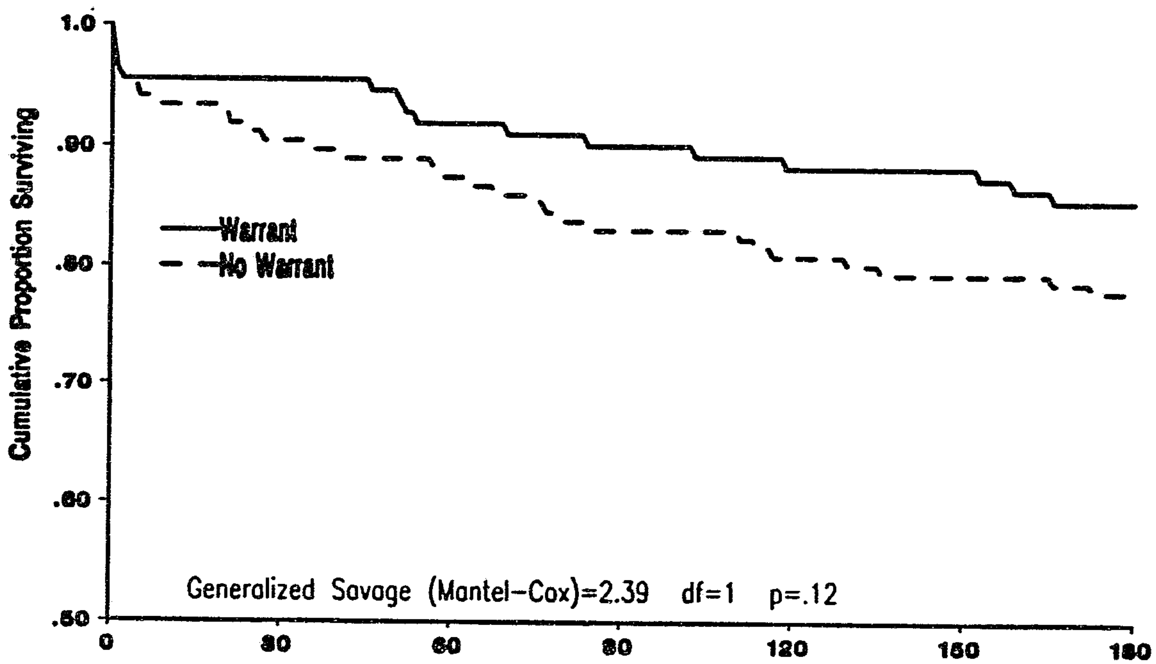


**Figure 5: Survival Functions for Complaint by Treatment Group, Offender Absent, Original Sample**

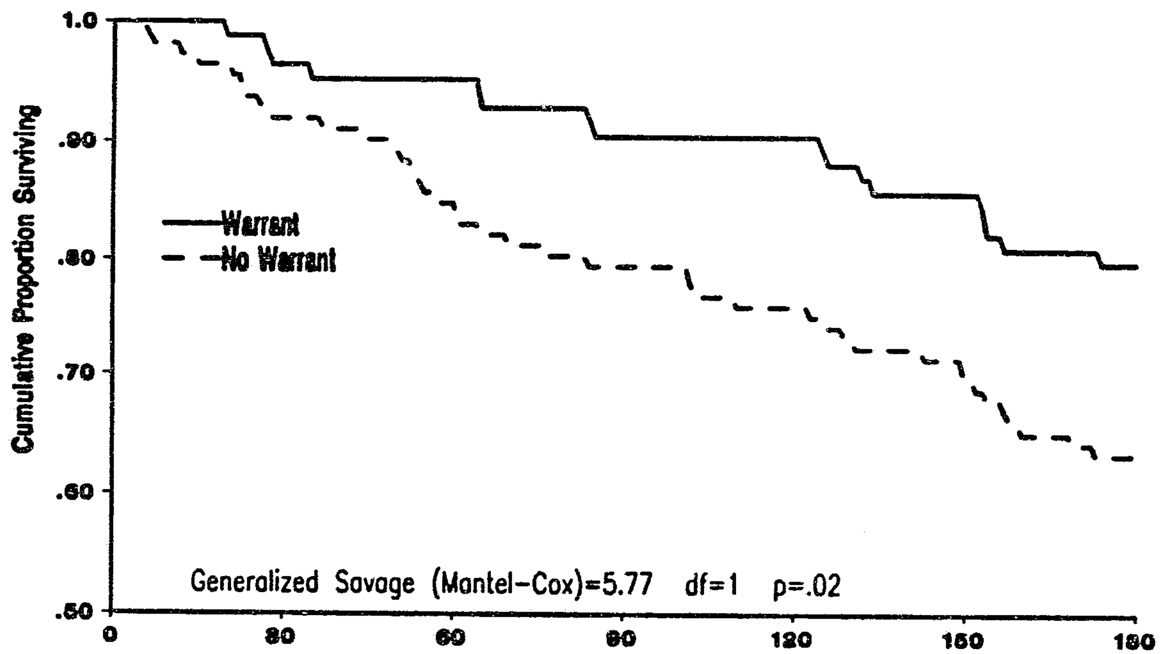


**Figure 6: Survival Functions for Victim-Reported Injury by Treatment Group, Offender Absent, Original Sample**

simply advised of their right to seek a warrant for the suspect's arrest and were left on their own to do so.

DISCUSSION

The generalizability of the results of the research conducted in Omaha beyond Omaha must await the outcomes of the five other research efforts currently funded by the National Institute of Justice to replicate the Minneapolis Experiment. Since the results from all of these studies are not yet available, what follows applies only to Omaha and only to the types of cases defined as eligible during the hours of the experiment. Furthermore, although a serious attempt was made to replicate the Minneapolis Experiment in Omaha, comparisons of the details of two experiments reveal a number of significant differences. Whether or not these differences account for the differences in the findings of the two experiments is uncertain.

Given the strength of the experimental design used in Omaha and the absence of any evidence that the design was manipulated in any significant way, the inability to replicate findings associated with the Minneapolis Experiment calls into question any generalization of the Minneapolis findings to other sites. First, arrest in Omaha, <u>by itself</u>, did not appear to deter subsequent domestic conflict any more than separating or mediating those in conflict, i.e., arrest and the immediate period of custody associated with arrest, was not the deterrent to continued domestic conflict that was expected. If the Omaha findings should be replicated on the other five sites conducting experiments on this issue, policy based on the presumptory arrest recommendation coming out of the Minneapolis Experiment should be reconsidered. Second, while arrest, by itself, did not act as a deterrent to continued domestic conflict for the misdemeanor domestic assault cases coming to the attention of the Omaha police, neither did it increase continued domestic conflict between parties to an arrest for assault. That is, victim reported measures of repeated conflict, which are measures of behavior (as opposed to arrest and complaint data which are measures of official police reaction to known violations of the law), clearly did not indicate that victims whose partners were arrested were at greater risk of subsequent conflict than were those whose partners were handled informally (mediated or separated) by the police. Arrest, therefore did not appear to place victims in greater danger of increased conflict than did separation or mediation. It would appear that what the police did in Omaha after responding to cases of misdemeanor domestic assault (arrest, separate, mediate) neither helped nor hurt victims in terms of subsequent conflict.

The failure to replicate the Minneapolis findings will undoubtedly cast some doubt upon the wisdom of a mandatory or even a presumptory arrest policy for cases of misdemeanor domestic assault. At this point, we are in the awkward position of having conflicting results from two experiments and no clear, unambiguous direction from the available research on this issue. Fortunately, the results from additional replications will soon be available and will hopefully provide a clearer picture of the effects (if any) of different types of police responses to misdemeanor domestic assaults.

Notwithstanding the unequivocal need to await the findings from each of the five other replications of the Minneapolis Experiment before considering the generalizability of findings beyond the sites involved, a discussion of the policy implications of the Omaha police experiments for the City of Omaha seems appropriate and is offered to city officials for their consideration. Having stated experimental findings based upon sound scientific methodology and procedures, what follows is our conjecture regarding their application to policy in Omaha. The two should not be confused.

1. Non-Arrest as Policy. Since arresting suspects is expensive and conflicts/ assaults do not appear to increase when arrests are not made, one response to these

data might be a recommendation to effect informal dispositions (separate or mediate) in cases of misdemeanor domestic assaults in Omaha. In this manner the costs associated with taking officers out of service, transporting suspects, bookings, jail, etc. would be avoided. A significant problem with this approach, however, is that it seems ethically inappropriate, it violates the recommendations of the Attorney General's Task Force on Family Violence (U.S. Department of Justice, 1984) and it may be illegal (Thurman v. Torrington, 1985; Berliner, 1989; Barbieri, 1989; Dawson; 1989) to patently ignore the rights of victims. What can be said to justify a legal system wherein victims have little protection from violent behavior that is against the law? That the issue is complex and not given to simplistic solutions is a given (Zimring, 1987; Mederer and Gelles, 1989); that domestic assault should be ignored when known to the authorities is not (Goolkasian, 1986).

2. Non-Mandatory Arrest Policy. An alternative policy is one which encourages arrest when probable cause for arrest exists, but does not mandate it. Our own experience in Omaha and that of others (Ford, 1983) suggests that the police prefer to arrest when probable cause for an arrest exists and that the failure to do so in cases of domestic assault is often due to the belief that the justice system does not follow through with prosecution and sanctions in cases where arrests for misdemeanor domestic assault have been made (Steinman, 1988). If the recommendations noted by Goolkasian (1986) for coordination among the criminal justice agencies of Omaha (the Police Division, the Prosecuting Attorney's Office, the court) were followed and misdemeanor domestic assaults were vigorously prosecuted, and if Omaha administrators would promote arrests for misdemeanor spouse assault and clarify the liability problems caused when officers fail to protect victims of domestic assault (Steinman, 1988), the police would be more likely to arrest in most cases of misdemeanor domestic assault.

The wisdom of adopting a mandatory arrest policy for cases of misdemeanor domestic assault, in the absence of data that supports such a policy, is also of concern. There is reason to believe that presumptive arrests may rob victims of their discretionary power and deprive them of an enormous source of empowerment. Mederer and Gelles (1989) have made the argument that mandatory arrest and prosecution policies in cases of domestic assault do not allow those battered to drop charges against their assailants. They suggest that mandatory arrest and prosecution, rather than empower victims, may actually serve to disempower them. Instead of controlling their own destinies, victims of domestic assault may be put "again" in the position of having their fates determined by others; and "In the case of mandatory arrest or prosecution, the 'others' are primarily male police officers and prosecutors" (Mederer and Gelles, 1989, p. 32). Ford (1983) makes a similar argument suggesting that victims often use the criminal justice process as a means to negotiate for their own security with suspects. "For example, she (the victim) could negotiate a settlement with the man (suspect) under which she would not prosecute if he left her alone. By the time the case came to trial the process had already worked for her and contact with the man in court would be destructive" (1983, p. 469).

A policy that encourages, but does not mandate arrest may be useful from several points of view. First, it would allow officers in Omaha to respond to the wishes of victims who do not want, for a variety of reasons, suspects arrested. The rationale cited earlier for this option is provocative and requires further investigation. Second, when an arrest is seen as an entry point into a coordinated criminal justice system rather than an end point, it may shift the burden of deterrence from a single official police intervention (arrest) to a sequence of other interventions, each of which may have some salutary effect. This view recognizes that suspects chronically involved in domestic violence most frequently do not admit to having a problem in this regard (Adams, 1988; Holtzworth-Munroe, 1988; Sonkin et al., 1985; Shields and Hanneke, 1983), are not easily treated (Hamberger and Hastings, 1988) and do not seek help voluntarily (Roberts, 1984) to deal with such problems and thus might require sustained long term interventions to change their ways. It supports arrest in domestic assault instances in which probable cause for an arrest is present and when victims support the arrest of suspects, not because arrest is a panacea for deterring domestic violence, but because of the penalties and the leverage that an arrest implicitly facilitates. Such a policy combines, as Mederer and Gelles (1989) suggest, compassion and control. The consequence of arrest for a domestic assault potentially involves criminal charges, a court conviction and a sentence that mandates (most typically as a condition of a partially or wholly suspended sentence) penalties and interventions that are specifically developed to impact domestic conflict and violence. While there is very limited data available on the effectiveness of such interventions (Elliott, 1989; Guerney et al., 1987) city officials may wish to try to identify, document and fully use effective services if/when they exist.

It should be made clear again, that the selection of either, or some variation, of the above policies is probably dependent upon the ends desired. If cost is the sole criteria, a policy of mediating or separating couples in conflict would not be inconsistent with the findings from the Omaha replication. If, however, there is an interest in trying to use the weight of the criminal justice system in Omaha to impact the incidence of domestic violence, the adoption of a policy that encourages, but does not mandate arrest for misdemeanor domestic assaults would also be consistent with the Omaha findings. It is important to remember that neither of the policies discussed here are enjoined by the findings for the Omaha experiment. The only definitive conclusions that can be drawn from these data is that arrest did not act (comparatively) as a deterrent to future conflict among those apprehended for misdemeanor domestic assault in the Omaha sample, nor did arrest increase the probability of subsequent conflict.

The policy considerations described here are also consistent with the offender absent findings. The results of the offender absent experiment suggest that a warrant may have had some power to discourage and/or delay repeat offending in Omaha. The dynamics of the process whereby issuing a warrant when suspects are missing deters subsequent conflict remains to be investigated. If replicated, however, these data would suggest that system initiated warrants, when supported by victims, may have several advantages: One, leaving before the police arrive would no longer represent a simple avenue for suspects to use to escape the legal consequences of their illegal assaultive behavior. When misdemeanor domestic violence is viewed as a civil matter rather than a criminal violation, as it was in Omaha, the response among law enforcement representatives has been to take a complaint and to advise victims of their rights to obtain legal protection (warrants, restraining orders, etc.) in instances where suspects have left the scene before their arrival. Two, the burden of filing a warrant for the arrest of suspects where probable cause exists, would be lifted from those least often prepared to effect such action (the victim) and placed upon those (the justice system) most capable of taking this action. Historically a $25 fee has been required in Omaha before representatives of the prosecuting attorney's office would accept a citizen request for a warrant. Dealing with the bureaucracy of the justice system is likely to intimidate all but the most determined and aggressive victims in the best of circumstances, and is even more likely to do so when the system inhibits spontaneous access (see Ford, 1983 for an excellent description of the sometimes precarious and inhospitable nature of the justice system to victims of domestic violence). Third, system initiated warrants would communicate the message that given probable cause and the mere consent of victims, suspects accused of domestic assault would be aggressively pursued and prosecuted by the state, independent of the conditions traditionally imposed upon victims. Fourth, a warrant might hold some deterrent power, in and of itself, that may function to reduce continued domestic violence. Finally, the issuance of a warrant potentially provides for the same leverage that an arrest contains as discussed above. That is, at the point at which an individual is arrested on a warrant for domestic assault the potential for assessing penalties and mandating treatment emerges.

Overall then, the adoption of a policy encouraging arrests or system initiated warrants over other police dispositions in instances of domestic misdemeanor assault might be appropriate in a coordinated Omaha justice system. It is clear, however, that arrest, by itself, was not effective in reducing or preventing continuing domestic conflict in Omaha, and that a dependence upon arrest to reduce such conflict is unwarranted, perhaps erroneous and even counterproductive.

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## CASES

Thurman v. City of Torrington, 595 F. Supp. 1521; Thurman v. City of Torrington, USDC No. H-84-120, June 25, 1985.

# APPENDIX A

Appendix A

Referrals of Eligible Cases by Police Patrol Sectors

| Disposition | West | North | South | Total |
|---|---|---|---|---|
| Mediate | 10 | 74 | 24 | 108 |
| | 9.3 | 68.5 | 22.2 | |
| Separate | 9 | 62 | 25 | 96 |
| | 9.4 | 64.6 | 26.0 | |
| Arrest | 5 | 66 | 31 | 102 |
| | 4.9 | 64.7 | 30.4 | |
| Warrant | 5 | 67 | 25 | 97 |
| | 5.2 | 69.1 | 25.8 | |
| No Warrant | 10 | 82 | 33 | 125 |
| | 8.0 | 65.6 | 26.4 | |
| Total | 39 | 351 | 138 | 528 |
| % | 7.4 | 66.5 | 26.1 | |

Chi-Square = 4.226 DF = 8 p = .836
District of Occurrence was missing for 49 (8%) cases.

# APPENDIX B

Table 1

Tests for Differences of Means* by Disposition for Selected Self-Reported Items at the First Interview** Replication

| | Mediate % | Separate % | Arrest % | Chi Square | p |
|---|---|---|---|---|---|
| Ethnicity of Victim | | | | | |
| White | 64.0 | 63.3 | 56.6 | | |
| Black | 31.9 | 32.2 | 33.7 | | |
| Hispanic | 3.3 | 2.2 | 3.6 | | |
| American Indian | 2.2 | 2.2 | 6.0 | | |
| Oriental | 1.1 | -- | -- | 5.061 | .751 |
| Victim SES (Hollingshead 1 = High SES) | | | | | |
| 1 | 4.3 | 4.5 | 7.5 | | |
| 2 | 10.6 | 9.1 | 10.0 | | |
| 3 | 17.0 | 9.1 | 10.0 | | |
| 4 | 12.8 | 13.6 | 25.0 | | |
| 5 | 23.4 | 25.0 | 25.0 | | |
| 6 | 17.0 | 20.5 | 15.0 | | |
| 7 | 10.6 | 13.6 | 5.0 | | |
| 8 | 4.3 | 4.5 | 2.5 | 6.558 | .950 |
| Monthly Take-Home Pay of Victims Reporting Employment | | | | | |
| Less than $300 | 6.5 | 18.2 | 9.8 | | |
| $300 to $600 | 34.8 | 36.4 | 41.5 | | |
| $600 to $900 | 26.1 | 27.3 | 26.8 | | |
| $900 to $1500 | 21.7 | 15.9 | 22.0 | | |
| $1500 to $2000 | 10.9 | -- | -- | | |
| Over $2000 | -- | 2.3 | -- | 14.844 | .138 |
| Victim Employed | 51.6 | 49.4 | 48.2 | .215 | .898 |
| Sex of Victim | | | | | |
| Female | 91.2 | 96.6 | 97.6 | 4.470 | .107 |
| Victim Suspect Living Together at Incident | 80.2 | 76.4 | 80.7 | .592 | .744 |
| Victim Suspect Living Together at First Interview | 54.9 | 52.8 | 53.0 | .100 | .951 |
| Victim Receiving Public Assistance | 30.0 | 43.8 | 35.4 | 3.747 | .154 |
| Ethnicity of Suspect | | | | | |
| White | 54.9 | 55.1 | 55.4 | | |
| Black | 37.4 | 38.2 | 37.3 | | |
| Hispanic | 4.4 | 4.5 | 4.8 | | |
| American Indian | 3.3 | 2.2 | 2.4 | .247 | 1.000 |

*Yates correction for continuity applied.
**Unless otherwise indicated all percentages are for affirmative responses to items.

Table 1 (Continued)

| | Mediate | Separate | Arrest | Chi Square | p |
|---|---|---|---|---|---|
| Suspect SES If Employed (Hollingshead 1 = High SES) | | | | | |
| 1 | 4.4 | 6.7 | 3.8 | | |
| 2 | 13.2 | 8.3 | 9.6 | | |
| 3 | 30.9 | 26.7 | 36.5 | | |
| 4 | 29.4 | 26.7 | 15.4 | | |
| 5 | 5.9 | 6.7 | 11.5 | | |
| 6 | 8.8 | 10.0 | 3.8 | | |
| 7 | 5.9 | 6.7 | 11.5 | | |
| 8 | -- | 8.3 | 7.7 | | |
| 9 | 1.5 | -- | -- | 16.088 | .447 |
| Monthly Take-Home Pay of Suspects Reporting Employment | | | | | |
| Less Than $300 | 2.9 | 3.3 | 3.8 | | |
| $300 to $600 | 27.9 | 21.7 | 17.0 | | |
| $600 to $900 | 22.1 | 23.3 | 28.3 | | |
| $900 to $1500 | 35.3 | 25.0 | 22.6 | | |
| $1500 to $2000 | 2.9 | 13.3 | 11.3 | | |
| Over $2000 | 1.5 | -- | 1.9 | | |
| Don't Know | 7.4 | 13.3 | 15.1 | 11.432 | .492 |
| Suspect Employed | 79.1 | 67.4 | 63.9 | 5.156 | .076 |
| Suspect Unemployed During the Last 6 Months | 39.7 | 30.0 | 30.2 | 1.758 | .415 |
| Suspect Receiving Public Assistance | 13.6 | 9.3 | 6.2 | 2.694 | .260 |
| Victim Dependent Upon Suspect-Finance | | | | | |
| Very to Totally Dependent | 27.5 | 16.9 | 16.9 | | |
| Moderate to Somewhat Dependent | 25.3 | 37.1 | 38.6 | | |
| Not at All Dependent | 47.3 | 46.1 | 44.6 | 6.146 | .188 |
| Who Called the Police | | | | | |
| Victim | 47.8 | 57.3 | 39.8 | | |
| Suspect | 4.4 | 4.5 | 2.4 | | |
| Other Family Member | 21.1 | 16.9 | 21.7 | | |
| Neighbor | 15.6 | 7.9 | 18.1 | | |
| Friend | 2.2 | 1.1 | 7.2 | | |
| Hospital Personnel | -- | 2.2 | 1.2 | | |
| Relation Living with Victim | 4.4 | 1.1 | 2.4 | | |
| An Official | -- | 1.1 | -- | | |
| Victim Doesn't Know | 3.3 | 5.6 | 6.0 | | |
| Other | 1.1 | 2.2 | 1.2 | 19.827 | .343 |
| Verbal Arguments Before the Fight | 86.8 | 87.6 | 89.2 | .228 | .892 |
| Who Started the Argument | | | | | |
| Offender | 60.8 | 62.3 | 54.8 | | |
| Victim | 15.2 | 10.4 | 13.7 | | |
| Both | 24.1 | 27.3 | 31.5 | 1.881 | .758 |

Table 1 (Continued)

| | Mediate | Separate | Arrest | Chi Square | p |
|---|---|---|---|---|---|
| Who Most Aggressive | | | | | |
| Offender | 68.4 | 71.8 | 68.9 | | |
| Victim | 13.9 | 16.7 | 13.5 | | |
| Both | 17.7 | 11.5 | 17.6 | 1.622 | .805 |
| Description of the Presenting Offense: | | | | | |
| Suspect Berated Victim | 93.4 | 94.4 | 91.6 | .548 | .760 |
| Suspect Damaged Something of Victim's | 46.2 | 42.0 | 37.3 | 1.382 | .501 |
| Suspect Threatened to Throw Something at Victim | 19.8 | 12.4 | 16.9 | 1.835 | .399 |
| Suspect Threatened to Hit Victim | 49.5 | 47.2 | 54.2 | .878 | .644 |
| Suspect Threw Something at Victim | 28.6 | 20.2 | 18.1 | 3.127 | .209 |
| Suspect Pushed, Shoved, Slapped Victim | 86.8 | 87.6 | 85.5 | .166 | .920 |
| Suspect Tried to Hit Victim with Something | 18.9 | 22.5 | 18.1 | .602 | .740 |
| Suspect Hit Victim with Something | 18.7 | 19.1 | 16.9 | .161 | .923 |
| Suspect Bit or Kicked Victim | 20.9 | 13.5 | 20.5 | 2.054 | .358 |
| Suspect Hit Victim with Fist | 29.6 | 51.7 | 61.4 | 8.384 | .015 |
| Suspect Beat up Victim | 31.9 | 44.9 | 42.2 | 3.567 | .168 |
| Suspect Threatened Victim with Knife | 7.7 | 9.0 | 6.0 | .539 | .764 |
| Suspect Threatened Victim with Gun | 6.6 | 6.7 | 2.4 | 2.045 | .360 |
| Suspect Cut or Stabbed Victim | 3.3 | 3.4 | 3.6 | .014 | .993 |
| Suspect Shot Victim | -- | 1.1 | -- | 1.962 | .375 |
| Suspect Threatened to Kill Victim | 26.4 | 24.7 | 25.3 | .067 | .967 |
| Suspect Tried to Kill Victim | 18.7 | 18.0 | 12.0 | 1.657 | .437 |
| Victim Berated Suspect | 72.5 | 64.0 | 77.1 | 3.705 | .157 |
| Victim Damaged Something of Suspect's | 5.5 | 5.6 | 4.8 | .062 | .969 |
| Victim Threatened to Damage Something of Suspect's | 11.0 | 7.9 | 8.4 | .599 | .741 |
| Victim Threatened to Hit Suspect | 26.4 | 16.9 | 15.7 | 3.862 | .145 |
| Victim Threw Something at Suspect | 12.1 | 5.6 | 9.6 | 2.310 | .315 |
| Victim Pushed, Shoved, Slapped Suspect | 49.5 | 52.8 | 49.4 | .270 | .874 |
| Victim Tried to Hit Suspect with Something | 17.6 | 12.4 | 15.7 | .971 | .615 |
| Victim Hit Suspect with Something | 9.9 | 13.5 | 6.0 | 2.682 | .262 |
| Victim Bit or Kicked Suspect | 12.1 | 12.4 | 10.8 | .107 | .948 |
| Victim Hit Suspect with Fist | 19.8 | 25.8 | 26.5 | 1.341 | .511 |
| Victim Beat up Suspect | 4.4 | 4.5 | 3.6 | .099 | .952 |
| Victim Threatened Suspect with Knife | 6.6 | 4.5 | 6.0 | .392 | .822 |
| Victim Threatened Suspect with Gun | 1.1 | -- | -- | 1.897 | .387 |
| Victim Cut or Stabbed Suspect | 1.1 | 1.1 | 2.4 | .640 | .726 |
| Victim Shot Suspect | -- | -- | -- | -- | |
| Victim Threatened to Kill Suspect | 2.2 | 6.7 | 2.4 | 3.183 | .204 |
| Victim Tried to Kill Suspect | 1.1 | 1.1 | -- | .930 | .628 |
| Victim Was Physically Injured | 75.8 | 82.0 | 72.3 | 2.359 | .307 |
| Victim Was Knocked Down | 43.5 | 57.5 | 43.3 | 3.723 | .155 |
| Victim Was Bruised-Scratched | 84.1 | 82.2 | 78.3 | .723 | .697 |
| Victim Was Cut-Bleeding | 40.6 | 46.6 | 40.0 | .753 | .686 |
| Victim Was Knocked Unconscious | 2.9 | 6.8 | 6.7 | 1.322 | .516 |
| Victim Had Broken Bones | 5.8 | 13.7 | 13.3 | 2.807 | .246 |
| Victim Had Head Injuries | 31.9 | 31.5 | 40.0 | 1.297 | .523 |
| Victim Was Choked | 1.4 | 6.8 | 1.7 | 3.918 | .141 |
| Victim Had Hair Pulled | 8.7 | 4.1 | 5.0 | 1.470 | .479 |

Table 1 (Continued)

| | Mediate | Separate | Arrest | Chi Square/ F Ratio | p |
|---|---|---|---|---|---|
| Description of the Presenting Offense: | | | | | |
| Victim Was Hit, Kicked | 7.2 | 11.0 | 10.0 | .609 | .738 |
| Victim Bruised-Twisted-Sprained | 5.8 | 4.1 | 1.7 | 1.446 | .485 |
| Victim Had Other Injury | 1.4 | 6.8 | 1.7 | 3.918 | .141 |
| Victim Went to Doctor or Emergency Room | 11.8 | 27.4 | 19.7 | 5.418 | .067 |
| Victim Use of Alcohol was Cause | 5.5 | 3.4 | 2.4 | 1.198 | .549 |
| Suspect Use of Alcohol was Cause | 38.5 | 31.5 | 37.3 | 1.097 | .558 |
| Use of Alcohol by Both was Cause | 6.6 | 6.7 | 7.2 | .030 | .985 |
| Ever Description of Conflict: | | | | | |
| Suspect Ever Put Victim Down | 92.3 | 97.8 | 96.4 | 3.314 | .191 |
| Suspect Ever Tied Victim Up | 2.2 | 3.4 | 2.4 | .269 | .874 |
| Suspect Ever Locked Victim in Room-House | 11.0 | 16.9 | 13.3 | 1.329 | .514 |
| Suspect Ever Locked Victim Out of House | 39.6 | 40.4 | 44.6 | .505 | .777 |
| Suspect Ever Damaged Victim's Things | 68.1 | 67.4 | 63.9 | .403 | .818 |
| Suspect Ever Threatened to Throw Things at Victim | 36.3 | 34.8 | 37.3 | .119 | .942 |
| Suspect Ever Threatened to Hit Victim | 65.9 | 62.9 | 72.3 | 1.764 | .414 |
| Suspect Ever Threw Something at Victim | 44.0 | 36.4 | 36.1 | 1.482 | .477 |
| Suspect Ever Pushed, Grabbed, Shoved Victim | 91.2 | 92.1 | 90.4 | .170 | .919 |
| Suspect Ever Tried to Hit Victim With Something | 29.7 | 30.3 | 26.8 | .285 | .867 |
| Suspect Ever Choked Victim | 42.9 | 47.2 | 50.6 | 1.056 | .590 |
| Suspect Ever Hit Victim With Something | 30.8 | 23.6 | 25.3 | 1.293 | .524 |
| Suspect Ever Bit or Kicked Victim | 36.3 | 29.2 | 33.7 | 1.034 | .596 |
| Suspect Ever Hit Victim with Fist | 57.1 | 65.2 | 78.3 | 8.852 | .012 |
| Suspect Ever Beat up Victim | 51.6 | 51.7 | 63.9 | 3.417 | .181 |
| Suspect Ever Threatened Victim with Knife | 20.9 | 10.2 | 14.5 | 3.985 | .136 |
| Suspect Ever Threatened with Gun | 15.4 | 11.2 | 14.5 | .716 | .699 |
| Suspect Ever Threatened to Kill Victim | 41.8 | 24.7 | 42.2 | 7.583 | .023 |
| Suspect Ever Cut or Stabbed Victim | 5.5 | 6.7 | 9.8 | 1.217 | .544 |
| Suspect Ever Shot Victim | 1.1 | -- | -- | 1.897 | .387 |
| Suspect Ever Tried to Kill Victim | 17.6 | 20.2 | 22.9 | .761 | .684 |
| Other Times Victim Could Have Called Police | 62.2 | 64.0 | 59.8 | .335 | .846 |
| Suspect Ever Arrested Prior to Presenting Offense | 62.6 | 56.6 | 73.4 | 6.800 | .033 |
| Number of Times Suspect Ever Arrested Prior to Presenting Offense | 2.696 | 2.368 | 3.028 | .751 | .473 |
| Suspect Ever Arrested for a Crime Against the Victim Prior to the Presenting Offense | 8.7 | 10.4 | 13.8 | 1.523 | .467 |
| Number of Times Suspect Was Ever Arrested for a Crime Against the Victim Prior the Presenting Offense | .122 | .132 | .202 | .876 | .417 |
| Suspect Arrested for a Crime Against the Victim During the 6-Months Prior to the Presenting Offense | 3.5 | .9 | 4.6 | 2.549 | .280 |
| Number of Times Suspect was Arrested for a Crime Against the Victim During the 6-Months Prior to the Presenting Offense | .035 | .009 | .073 | 1.804 | .166 |

Table 2 (Continued)

| | Warrant % | No Warrant % | Chi Square | p |
|---|---|---|---|---|
| Suspect SES If Employed (Hollingshead 1 = High SES) | | | | |
| 1 | 9.3 | 3.1 | | |
| 2 | 14.8 | 21.9 | | |
| 3 | 42.6 | 28.1 | | |
| 4 | 18.5 | 25.0 | | |
| 5 | 1.9 | 3.1 | | |
| 6 | 7.4 | 10.9 | | |
| 7 | 1.9 | 4.7 | | |
| 9 | 3.7 | 3.1 | 6.266 | .509 |
| Monthly Take-Home Pay of Suspects Reporting Employment | | | | |
| Less Than $300 | 9.3 | 5.8 | | |
| $300 to $600 | 18.5 | 17.4 | | |
| $600 to $900 | 16.7 | 18.8 | | |
| $900 to $1500 | 38.9 | 29.0 | | |
| $1500 to $2000 | 3.7 | 4.3 | | |
| Over $2000 | 1.9 | 5.8 | | |
| Don't Know | 11.1 | 18.8 | 3.852 | .697 |
| Suspect Employed | 65.1 | 64.4 | 0.000 | 1.000 |
| Suspect Unemployed During the Last 6 Months | 33.3 | 39.1 | 0.224 | .636 |
| Suspect Receiving Public Assistance | 4.7 | 15.7 | 6.834 | .033 |
| Victim Dependent Upon Suspect-Finance | | | | |
| Very to Totally Dependent | 14.3 | 14.7 | | |
| Moderate to Somewhat Dependent | 27.5 | 29.3 | | |
| Not at All Dependent | 58.2 | 56.0 | .109 | .947 |
| Who Called the Police | | | | |
| Victim | 52.2 | 56.0 | | |
| Suspect | 3.3 | 2.6 | | |
| Other Family Member | 17.4 | 12.9 | | |
| Neighbor | 9.8 | 12.1 | | |
| Friend | 8.7 | 4.3 | | |
| Hospital Personnel | 2.2 | 7.8 | | |
| Relation Living with Victim | 2.2 | 0.9 | | |
| An Official | 1.1 | -- | | |
| Victim Doesn't Know | 1.1 | 1.7 | | |
| Other | 2.2 | 1.7 | 7.825 | .552 |
| Verbal Arguments Before the Fight | 79.1 | 89.7 | 3.656 | .056 |
| Who Started the Argument | | | | |
| Offender | 70.4 | 70.9 | | |
| Victim | 12.7 | 9.7 | | |
| Both | 16.9 | 19.4 | 0.485 | .785 |

Table 2 (Continued)

| | Mediate | Separate % | Arrest % | Chi Square/ F Ratio | p |
|---|---|---|---|---|---|
| Description of the Presenting Offense: | | | | | |
| Victim Was Hit, Kicked | | 10.5 | 8.0 | 0.098 | .754 |
| Victim Bruised-Twisted-Sprained | | 7.9 | 5.0 | 0.222 | .637 |
| Victim Had Other Injury | | 1.3 | 3.0 | 0.054 | .816 |
| Victim Went to Doctor or Emergency Room | | 35.5 | 42.0 | 0.512 | .474 |
| Victim Use of Alcohol was Cause | | 2.2 | 4.3 | 0.200 | .655 |
| Suspect Use of Alcohol was Cause | | 31.9 | 24.1 | 1.164 | .281 |
| Use of Alcohol by Both was Cause | | 4.4 | 3.4 | 0.000 | 1.000 |
| Ever Description of Conflict: | | | | | |
| Suspect Ever Put Victim Down | | 90.1 | 96.6 | 2.584 | .108 |
| Suspect Ever Tied Victim Up | | 2.2 | 3.4 | 0.013 | .908 |
| Suspect Ever Locked Victim in Room-House | | 18.7 | 16.4 | 0.062 | .803 |
| Suspect Ever Locked Victim Out of House | | 34.1 | 31.9 | 0.033 | .857 |
| Suspect Ever Damaged Victim's Things | | 63.7 | 65.5 | 0.014 | .905 |
| Suspect Ever Threatened to Throw Things at Victim | | 39.3 | 38.8 | 0.000 | 1.000 |
| Suspect Ever Threatened to Hit Victim | | 68.5 | 81.9 | 4.233 | .040 |
| Suspect Ever Threw Something at Victim | | 40.7 | 42.2 | 0.008 | .930 |
| Suspect Ever Pushed, Grabbed, Shoved Victim | | 86.8 | 94.8 | 3.178 | .075 |
| Suspect Ever Tried to Hit Victim With Something | | 35.2 | 37.1 | 0.019 | .891 |
| Suspect Ever Choked Victim | | 50.5 | 56.9 | 0.592 | .442 |
| Suspect Ever Hit Victim With Something | | 27.5 | 33.6 | 0.638 | .425 |
| Suspect Ever Bit or Kicked Victim | | 39.6 | 51.7 | 2.564 | .109 |
| Suspect Ever Hit Victim with Fist | | 71.4 | 68.1 | 0.132 | .716 |
| Suspect Ever Beat up Victim | | 67.0 | 65.5 | 0.006 | .936 |
| Suspect Ever Threatened Victim with Knife | | 14.3 | 25.0 | 2.987 | .084 |
| Suspect Ever Threatened with Gun | | 13.2 | 13.8 | 0.000 | 1.000 |
| Suspect Ever Threatened to Kill Victim | | 49.5 | 51.7 | 0.034 | .854 |
| Suspect Ever Cut or Stabbed Victim | | 13.2 | 10.3 | 0.172 | .678 |
| Suspect Ever Shot Victim | | -- | -- | -- | -- |
| Suspect Ever Tried to Kill Victim | | 27.5 | 26.7 | 0.000 | 1.000 |
| Other Times Victim Could Have Called Police | | 66.7 | 74.1 | 1.032 | .310 |
| Suspect Ever Arrested Prior to Presenting Offense | | 56.8 | 64.7 | 1.625 | .202 |
| Number of Times Suspect Ever Arrested Prior to Presenting Offense | | 2.514 | 3.250 | 1.177 | .184 |
| Suspect Ever Arrested for a Crime Against the Victim Prior to the Presenting Offense | | 8.1 | 11.0 | .595 | .441 |
| Number of Times Suspect Was Ever Arrested for a Crime Against the Victim Prior the Presenting Offense | | .117 | .132 | .077 | .782 |
| Suspect Arrested for a Crime Against the Victim During the 6-Months Prior to the Presenting Offense | | 2.7 | .7 | 1.485 | .223 |
| Number of Times Suspect was Arrested for a Crime Against the Victim During the 6-Months Prior to the Presenting Offense | | .027 | .004 | 1.482 | .224 |

Table 2 (Continued)

| | Warrant % | No Warrant % | Chi Square | p |
|---|---|---|---|---|
| **Who Most Aggressive** | | | | |
| Offender | 75.0 | 76.0 | | |
| Victim | 15.3 | 17.3 | | |
| Both | 9.7 | 6.7 | 0.590 | .744 |
| **Description of the Presenting Offense:** | | | | |
| Suspect Berated Victim | 85.7 | 94.8 | 3.965 | .046 |
| Suspect Damaged Something of Victim's | 47.3 | 42.6 | 0.275 | .600 |
| Suspect Threatened to Throw Something at Victim | 24.2 | 22.6 | 0.010 | .922 |
| Suspect Threatened to Hit Victim | 57.1 | 62.6 | 0.426 | .514 |
| Suspect Threw Something at Victim | 24.2 | 18.3 | .748 | .387 |
| Suspect Pushed, Shoved, Slapped Victim | 80.2 | 91.3 | 4.413 | .036 |
| Suspect Tried to Hit Victim with Something | 26.4 | 25.2 | 0.001 | .978 |
| Suspect Hit Victim with Something | 16.5 | 21.7 | 0.592 | .442 |
| Suspect Bit or Kicked Victim | 24.2 | 36.5 | 3.062 | .080 |
| Suspect Hit Victim with Fist | 57.1 | 57.4 | 0.000 | 1.000 |
| Suspect Beat up Victim | 52.7 | 51.3 | 0.004 | .948 |
| Suspect Threatened Victim with Knife | 4.4 | 12.7 | 2.941 | .086 |
| Suspect Threatened Victim with Cun | 4.4 | 5.2 | 0.000 | 1.000 |
| Suspect Cut or Stabbed Victim | 8.8 | 5.2 | 0.538 | .463 |
| Suspect Shot Victim | 3.3 | 0.9 | 0.556 | .456 |
| Suspect Threatened to Kill Victim | 30.8 | 30.4 | 0.000 | 1.000 |
| Suspect Tried to Kill Victim | 14.3 | 20.0 | .788 | .375 |
| Victim Berated Suspect | 63.7 | 69.6 | 0.539 | .463 |
| Victim Damaged Something of Suspect's | 3.3 | 7.0 | 0.719 | .396 |
| Victim Threatened to Damage Something of Suspect's | 11.0 | 13.0 | 0.055 | .815 |
| Victim Threatened to Hit Suspect | 15.4 | 26.1 | 2.856 | .091 |
| Victim Threw Something at Suspect | 8.8 | 13.0 | 0.547 | .460 |
| Victim Pushed, Shoved, Slapped Suspect | 41.8 | 49.6 | 0.952 | .329 |
| Victim Tried to Hit Suspect with Something | 13.2 | 20.0 | 1.224 | .269 |
| Victim Hit Suspect with Something | 12.1 | 13.0 | 0.000 | 1.000 |
| Victim Bit or Kicked Suspect | 16.5 | 25.2 | 1.816 | .178 |
| Victim Hit Suspect with Fist | 26.4 | 26.1 | 0.000 | 1.000 |
| Victim Beat up Suspect | 3.3 | 2.6 | 0.000 | 1.000 |
| Victim Threatened Suspect with Knife | 8.8 | 8.7 | 0.000 | 1.000 |
| Victim Threatened Suspect with Gun | 4.4 | 3.5 | 0.000 | 1.000 |
| Victim Cut or Stabbed Suspect | 1.1 | -- | 0.010 | .906 |
| Victim Shot Suspect | -- | -- | -- | -- |
| Victim Threatened to Kill Suspect | 9.9 | 11.3 | 0.010 | .921 |
| Victim Tried to Kill Suspect | -- | 3.5 | 1.660 | .198 |
| Victim Was Physically Injured | 83.5 | 86.2 | 0.117 | .732 |
| Victim Was Knocked Down | 59.2 | 53.0 | 0.447 | .504 |
| Victim Was Bruised-Scratched | 82.9 | 78.0 | 0.378 | .538 |
| Victim Was Cut-Bleeding | 40.8 | 43.0 | 0.020 | .889 |
| Victim Was Knocked Unconscious | 9.2 | 8.0 | 0.000 | .990 |
| Victim Had Broken Bones | 3.9 | 6.0 | 0.071 | .790 |
| Victim Had Head Injuries | 40.8 | 37.0 | 0.126 | .722 |
| Victim Was Choked | 6.6 | 4.0 | 0.180 | .672 |
| Victim Had Hair Pulled | 3.9 | 2.0 | 0.098 | .755 |