

COLORADO SPRINGS
DOMESTIC VIOLENCE EXPER.
FINAL REPORT

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Colorado spring's spouse assault
replication project final report

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EXECUTIVE SUMMARY

The Colorado Springs Spouse Assault Replication Project was conducted by the Colorado Springs Police Department under a grant from the National Institute of Justice.* The Colorado Springs Police Department (CSPD) is a department of 400 sworn officers and 200 civilians serving a population of 280,000. Prior to undertaking this project, the CSPD had not been involved in research on this scale.

When the results of the Minneapolis study of police alternatives to domestic violence were released in April, 1984, domestic violence was a topic that the CSPD and community groups were concerned about locally. During the same period that the National Institute of Justice (NIJ) was actively seeking police departments and researchers to be involved in a multi-site replication of the Minneapolis study, the CSPD formed a committee to discuss domestic violence issues and develop appropriate departmental policies and procedures. The committee found that the data available to them was limited and did not provide a satisfactory basis for making policy decisions about police intervention in domestic violence incidents. As a result, participation in the replication of the Minneapolis study became attractive as a means to obtaining detailed data, and hopefully, results that would indicate directions for future policy. In the fall of 1985, the Colorado Springs Police Department began a serious effort to

* This project was supported under NIJ grant 86-IJ-CX-0045.

become one of the replication sites. Late in 1986, with funding secured, the CSPD embarked on what came to be known simple as the Colorado Springs Domestic Violence Project.

Design.

The multi-site experiment sponsored by NIJ, the Spouse Assault Replication Program (SARP), was carried out in Milwaukee, Omaha, Miami/Dade County, Atlanta, and Charlotte, as well in Colorado Springs. At each site the design was tailored to local conditions and questions of interest but was constructed around common data elements that could be compared across sites. In all sites, the core of the experiment was to test the effect of arrest and other dispositions on the frequency of subsequent violent domestic incidents.

In the Colorado Springs experiment, an incident was eligible for inclusion in the study if it met the following criteria:

- 1) no felony had been committed;
- 2) there was probable cause to believe that one of four misdemeanor offenses had been committed--third degree assault, harassment, false imprisonment, or menacing;
- 3) at or before the time of the incident the suspect and victim were involved in a conjugal relationship with each other;

- 4) the suspect was 18 years of age or older;
- 5) the suspect was present at the scene when the officers arrived;
- 6) the suspect did not have an outstanding warrant or a restraining order against him;
- 7) the victim was a resident of Colorado Springs; and
- 8) the victim did not insist on arrest.

Once eligibility was established, one of four randomly selected alternatives was to be imposed:

- 1) restoring order only;
- 2) issuing an emergency protection order only;
- 3) issuing an emergency protection order and having the suspect go immediately to a counseling session with a trained therapist; or
- 4) issuing an emergency protection order and arresting the suspect--where arrest was defined as physically taking the suspect into custody and transporting him to the jail.

The dispositions were to be announced to the officers by the dispatchers via telephone or radio. As originally designed, a list of randomly selected alternatives was given to the communications center daily. Dispatchers were instructed to assign each new eligible incident the next disposition on the list and inform the officer at the scene of the assignment. In order to insure the integrity of the process this procedure was modified by

replacing the lists with sealed numbered envelopes that each contained a randomly selected disposition.

It was possible for an incident to be rendered ineligible even after a disposition had been assigned. A case could be declared ineligible and removed from the experiment after a disposition had been assigned for such reasons as:

- 1) arresting the suspect for an offense not related to the domestic incident (e.g., for possession of drugs);
- 2) arresting the suspect for the officers' or victim's immediate safety; or
- 3) the incident being a repeat call in the project, that is, the parties involved having already been involved in an incident that had been assigned a disposition through the project.

The outcome variable--reoccurrence of a domestic violence incident within six months of the original incident--was to be measured in two ways:

- 1) by the numbers of incidents in which a suspect was arrested for a subsequent offense as recorded in official arrested records; and
- 2) by the numbers of incidents reported by victims during interviews conducted by the project staff.

Because there were four different dispositions to be tested, the target sample size was 1600 incidents.

The Field Experiment

A pre-test was conducted between March 26, 1987, and continued through May 10, 1987. After some modifications were made, the full experiment was launched on June 28, 1987. Case flow into the project was slow. In order to increase case flow the number of officers worked in the project was expanded.

Initially the project operated with fewer than 30 officers who had been specially trained for the project. Between July of 1987 and February of 1988, all remaining patrol officers were brought into the project and given abbreviated training in domestic violence and project procedures. Later, reserve officers were brought into the project to ride with regular patrol officers in order to provide the minimum two officer response without having to send two units.

A total of 1658 cases had a randomized disposition imposed. Of 1191 of these, the randomly selected disposition was imposed. In 315 cases, a different disposition from the one assigned was imposed. In 31 cases, there were questions as to assignment, imposition, or eligibility. In 121 cases, the procedures in place to screen out repeat calls in the project failed and the same victim and suspect received a second project disposition.

Seventeen percent of the cases failed to receive the assigned disposition. Many, though not all, of these 217 cases received a stronger intervention than assigned. Ignoring the dispositions outside of the four experimental alternatives, all of the assignments to restore order that had a different alternative imposed had a stronger alternative (either EPO or arrest) imposed. Of those assigned but not receiving EPO, (again ignoring dispositions outside the experimental design), 24 received a weaker intervention (restore order) and 32 received a stronger alternative -- arrest. There were 68 cases assigned to counseling that received a different project disposition. Thirty-eight of these were weaker interventions - 19 EPO's and 19 restore orders - and 30 were arrests. Sixty-five cases assigned to arrest received a weaker intervention.

Threat or injury to the victim or hostile behavior toward the officers were characteristics associated with larger numbers of arrests imposed than assigned, especially when the original assignments were to counseling or restore order.

Data Collection

In addition to information collected at the initial scene by the officers responding to the incident, the project collected information from a variety of other sources.

- 1) The therapists who conducted the counseling sessions filled out a form on each session held.

- 2) Trained interviewers working directly for the project, conducted interviews with victims within days of the incident and six months later. A systematically selected subset of victims were given interim follow-up interviews during the six month period as well.
- 3) A small amount of data was collected on the progress of project cases in the courts following arrest.
- 4) Finally, CSPD arrest records and victim records were checked for all victims and all suspects to obtain information on prior arrests and prior domestic incidents in which either the present victim or present suspect has been a victim.

Outcome

The results of the preliminary analysis raise more questions than they answer. Using victim reports of subsequent incidents that involved hitting, slapping, hurting, or detaining couples assigned to the arrest disposition (whether or not it was actually imposed) had lower rates of recidivism. However, when data from official arrest records are used to measure failure, no differences are found among the treatments assigned.

Victim reports of new incidents and official arrest records do not match well. While it is to be expected that not all incidents reported by victims in the interviews resulted in an arrest, it would be expected that most incidents that resulted in

an arrest would be recalled by the victims and reported to the interviewer. In fact, about three-quarters of the new incidents resulting in arrest were not reported by the victims during the interviews.

While further analyses of the data may help to explain these results, the preliminary findings cannot be interpreted as providing a clear indication of what law enforcement policy should be in responding to domestic violence incidents.

Chapter 1

From Policy Questions to a Research Commitment

Description of the Community

Colorado Springs, the second largest city in Colorado, is located 70 miles south of Denver at the edge of the Rocky Mountains. Nestled between low bluffs to the east and massive Pikes Peak to the west, the city was originally founded as a resort community for the wealthy. After the first of several military bases was established in the area in 1942, the city experienced rapid population growth, changing from a town of 30,000 to a city of over 215,000 by 1980. A few years into the eighties the growth began to slow and by the end of the decade the population had stabilized, at least temporarily, at around 280,000. During the years the Domestic Violence Project was operating in the field, 1987-89, the city grew less than two percent.

The city's rapid growth has been without the heavy industrialization that has fostered the growth of many other cities. Colorado Springs owes its growth primarily to the proliferation of military establishments and the resulting attraction of clean, high-tech industries to the area. Following the establishment of Fort (then Camp) Carson in 1942, Peterson Field and Ent Air Force Base were also located in the area. In the 1950's the North American Air Defense Command and Combat Operations Center were

located inside Cheyenne Mountain in Colorado Springs and the U.S. Air Force Academy was built just to the north of the city. More recently the area has seen the arrival of the Consolidated Space Operations Center, the U. S. Space Command, Falcon Air Force Base, the SDI National Test Facility, and the United States Air Force Space Command. Electronics manufacturers and aerospace/defense firms followed the military, giving the city the nickname "Silicon Mountain." The Chamber of Commerce lists nearly 100 aerospace/defense firms and contractors ranging in size from 1 to 3650 employees.

As of September, 1988 there were 35,000 active duty military personnel in the area.¹ Of the ten largest employers of civilians in the area, six are government or other public entities including the military installations, the city government, the city's largest school district, and the city owned hospital. Of the remaining four, two are electronics firms, one is a private hospital, and one is a mail-order retailer.² The city is also home to a number of non-profit organizations, among them the Olympic Training Center and the U. S. Olympic Committee.

Due in part to the heavy military presence in the area, the local population is relatively young, in spite of the fact the Colorado Springs is popular as a retirement community. Forty-two percent of the area's population is under age 25; sixty-two percent is

under age 35. Eighty-four percent of the metropolitan population is white, 6% is black, 8% is Hispanic, and 2% is Asian, Native American, or other.³

According to the 1980 census, 82% of the population 25 years old or older were high school graduates. The median number of years of school completed was 12.9, with 22% of the population 25 or older having completed four or more years of college.

For a city with its population, Colorado Springs has a very large land area and is thus not densely settled. Most of the city population lives within the 140 square miles that comprised the city's area before the latest annexation. In 1988, an annexation on the city's eastern boundary brought the total land area to 178 square miles. Many city residents live in single family housing while others live in complexes of townhouses or two or three story apartment buildings. Few apartment buildings are over three stories.

The cost of living in Colorado Springs is fairly reasonable. Housing, groceries, utilities, transportation, and miscellaneous goods and services are all below average according to the American Chamber of Commerce Cost of Living Index, while the cost of health care is above average. The Index gave Colorado Springs a composite rating of 93.0 for the 1st quarter of 1989. (One hundred is the inter-city average.) The median household income in 1988 was \$28,995.⁴

With the city's low population density and the availability of affordable, relatively new, housing (most of the city's housing stock is less than forty years old), even the least affluent areas of the city contain attractive places to live. The median price for a new house in Colorado Springs in 1988 was \$94,600, while the median price for a resold house was \$77,975. According to figures released by Coldwell Banker in January of 1989, a house of approximately 2,000 square feet with three bedrooms, two baths, a family room, and a two car garage "in a neighborhood typical for a corporate transferee" could be bought for \$77,667. Rents are similarly reasonable. Average rents in multi-family buildings in 1988 were \$231 for a studio, \$268 for a one bedroom, \$332 for a two bedroom, and \$436 for a three bedroom unit. The average residential monthly utility bill for gas, electricity, water, and wastewater treatment in 1988 was \$102.27.⁵

The city owns or manages about one thousand public housing units including those for senior citizens. Public housing is scattered throughout the city and includes single family dwellings, duplexes, four-plexes, and a few small apartment complexes--none over 19 units. In addition, the city subsidizes an additional thousand units on the private market.

Colorado Springs crime rates tend to be lower than that of other cities its size, perhaps due to the low density. Comparing crime in Colorado Springs to that of the eight other cities between 250,000 and 299,999 in population⁶ (as reported in the 1988

Uniform Crime Reports) showed that Colorado Springs had rates significantly lower than the average of the other eight cities for five of the seven index crimes. The overall rate of property crime was 93% of the average rate for the other cities while the rate of violent crime was only 55%. Colorado Springs' homicide rate was only 3.6 per 100,000 compared to an average rate of 12.3 for the other cities, or only about 30% of that average.⁷

The Colorado Springs Police Department.

The Colorado Springs Police Department, along with other city departments, has had to grow to provide services to an ever larger community. The department's growth, however, has not kept pace with the population increases or the geographical expansion of the city. In 1980, the city had 364 sworn officers; in 1989 it had 404. During that time the department civilianized some positions and introduced the taking of some minor case reports over the telephone in an effort to keep its sworn officers available to handle duties which only they could perform. Still, the number of sworn officers in 1989 was only 11% greater than it was in 1980 but by 1989 the officers were serving a population that had grown by 30% and a land area that had grown by 73%. In 1980 there were 3.52 officers per square mile and 1.69 officers per 1000 population. In 1990 there were 2.27 officers per square mile and 1.44 officers per 1000 population.

A recent Bureau of Justice Statistics report on police departments in large cities gives an average of 9.8 officers per square mile and 2.0 officers per 1000 population for cities 250,000 to 350,000 (based on 1987 data).⁸ Colorado Springs has a relatively low ratio of officers to population and a very low ratio of officers to land area.

Of the sworn officers, about three-fourths are assigned to the patrol bureau with some of these assigned to special units within the bureau. At any given time, taking into account officers out sick, at training, or on vacation, there are likely to be eight to twelve officers on a shift (sometimes fewer) in each of the three geographical divisions within the city. Each division contains roughly 93,000 people and 60 square miles. One of the biggest challenges facing the department is to manage its calls for service workload and deploy its officers so that the residents receive a broad range of high quality police services while retaining the ability to respond quickly and effectively to emergencies.

Police Response to Domestic Violence.

There has been much written about the traditional reluctance of the police to intervene in domestic incidents beyond temporarily restoring order. Not only have police been reluctant to take action against the perpetrators, often they have not even been dispatched to the scene of the disturbance.

Testimony from police officials before the U. S. Commission on Civil Rights revealed that some departments respond to only a fraction of the domestic disturbance calls they receive. Where they do respond, the officers often do not make a report, even when assaults have occurred. The Commission found that it is common police practice to separate the parties and refer the victim to the civil courts for any further remedies.⁹

A study of conjugal violence in Marion County, Indiana, (which includes Indianapolis), in 1978, found that "between two-thirds and three-fourths of all calls to police on conjugal fights were 'solved' by the dispatchers."¹⁰ Police were dispatched to a domestic disturbance when, "in the dispatcher's judgment, the fight was so serious that a felony was likely being committed."¹¹ Otherwise, the dispatchers explained to the victim that a complaint could be made at the prosecutor's office. No record was made of the call beyond the tape-recorded conversation between the caller and the dispatcher.

As the Commission on Civil Rights noted, such practices "put the burden of law enforcement squarely upon the assault victim, relieving the police of any obligation to investigate, to collect evidence, or to record the crime."¹² Furthermore, "police emphasis upon civil remedies sometimes results in further deemphasis of the criminal nature of spouse abuse."¹³

In Colorado Springs, all incoming calls for service are assigned a priority from one to four. Those assigned a priority one are serious crime situations in progress requiring immediate action by the nearest available police unit. All fights in progress and all disturbances, including domestics, are dispatched as priority one calls. Thus, issues of police practice in Colorado Springs center on how the police should handle the incident once on the scene of the disturbance.

A 1984 memorandum from the lieutenant in charge of training at the CSPD noted that "officers have been informally trained by peers and supervisors alike to try to talk domestic violence victims out of signing a complaint." But the police are not the only actors in the system. The lieutenant went on to say that training accompanied by specific policy could change officers' attitudes but that cooperation from the courts would be needed for officers actions to be effective. Specifically, the lieutenant cautioned that if an officer arrested a suspect there was a good possibility that he would be released on his own recognizance and return home within the hour, still angry. Clearly, more than police attitudes and procedures are involved.

The Minneapolis Study and CSPD Initiatives.

By the time the results of the Minneapolis study were released in April of 1984, domestic violence was receiving attention on a variety of fronts. Police departments, the CSPD among them, were anxious to respond to community concerns about domestic violence and to take a more active role combating the problem, but they were also interested in developing sound policy and procedures that would minimize their legal liability, insure the safety of the victim, any children in the household, and the officers themselves, and effectively reduce the frequency of repeat incidents. The publication of the Minneapolis results intensified efforts to learn about domestic violence and develop strategies to combat the problem.

In June of 1984, the chief of the Colorado Springs Police Department was invited to attend a meeting of police chiefs potentially interested in replicating the Minneapolis experiment. John Tagert, the chief at the time, was unable to attend but the department was represented at the conference by the Director of Administrative Services, James Vetter. Upon his return Vetter requested that a report be prepared for him on domestic violence in Colorado Springs using available information. Summarizing calls for service data collected for the department's annual reports, the resulting study noted that family disturbances totaled 6.1% of the calls for service in 1983 and consumed 11.9% of the total department calls for service man-hours. In addi-

tion, the report revealed that 27.5% of the homicides from 1981 through 1983 "were a direct result of domestic violence." Finally, the report noted that calls for service for domestic incidents were increasing, even while overall calls for service related to disturbances were decreasing.

In August, partly at the urging of community groups and the Human Relations Commission, the department established a committee to discuss issues relating to domestic violence and develop appropriate departmental policies and procedures. At the first meeting, the Director of Administrative Services reported on the conference he had attended. The committee felt at that time that the CSPD should not participate in the replication. The committee discussed the need for explicit departmental policy and procedures for handling domestic violence incidents and agreed that further analysis of calls for service data would be helpful in determining the scope of the problem. It was noted in the discussions that any policy developed by the police department must be coordinated with the courts and prosecutors and that the availability of space at the jail would have an influence on the decision to arrest.

During the fall of 1984, the committee was briefed by representatives from various local agencies that dealt with the victims of domestic violence. In addition, the committee attempted to assess the volume and character of domestic violence calls by examining calls for service data over Labor Day

weekend. Calls for service data proved to be inadequate. First, not all incidents of domestic violence were identifiable as such from the information received by Communications and recorded in the computer aided dispatch (CAD) system. Second, the committee was interested in more detailed information than could be recorded on the CAD screens. In November of 1984, the Deputy Chief of Patrol directed all patrol officers to collect data on domestic disturbances by filling out a questionnaire on each domestic call handled over a two month period. The survey gathered information on the number, type, and disposition of domestic incidents, the extent of any injuries, the relationship between the suspect and victim, and whether either was connected to the military.

Developing explicit policy and procedures on emergency protection orders (EPO's) was a major focus of the committee's work. (EPO's are temporary restraining orders issued by a judge to protect actual or potential victims of abuse.) A draft policy and procedure was developed by the committee and on December 14, 1984, the chief issued the committee's product as a formal order establishing departmental procedures for obtaining and enforcing emergency protection orders.

Another project that began in the committee and was completed by the spring of 1985 was the production of a community resource information card that officers could give to persons involved in

domestic disturbances. Hearing from those working with victims in the community had convinced the committee that the police department needed to take an interest beyond the immediate resolution of the call. Victims needed further assistance that was not within the department's sphere of responsibility to provide, but the department could at least provide the victims with information on resources available to them within the community. The department developed a card for officers to give to victims that listed community agencies and phone numbers.

The results of the survey conducted by the Patrol Bureau were presented to the department in March of 1985. The highlights were as follows: Saturday was the day of the week with the highest concentration of domestic calls, followed by Sunday, Monday, and Friday; most calls to domestic incidents occurred between 8:00 P.M. and midnight; three-fourths of the cases in the survey involved alcohol or drugs; eighty-five percent of the cases did not result in a signed complaint; fully a third of the incidents involved military families; and about half of the cases involved injuries.

The committee was able to use this data and what it had learned from its contacts with community agencies to institute some additional changes in department practices. Specific training in handling incidents of domestic violence was added to the in-service training curriculum, a new form for Emergency Protection orders was created so that the department would have an

appropriate record of the order, the Communications Section improved the information they recorded on dispositions of domestic incidents, the Chaplaincy Corps was trained to provide some crisis intervention in domestic situations, and the department expanded its efforts to coordinate with the Domestic Violence Prevention Center to provide assistance to victims, and with the District Attorney to pursue prosecution of suspects in domestic violence. One other outcome of the committee's work was that EPO information was placed into the on-line hazard file in the Communications Section. Under the new procedure whenever an address associated with an EPO was typed in, a notice appeared on the screen alerting the communications clerk to the fact that there was an item in the hazard file pertaining to the address. An associated screen could then be accessed with a single key stroke and the information on the EPO could be read by the clerk. The dispatcher could then notify the officers before they even arrived at the scene that an EPO was in effect.

The department continued tabulating information on domestic violence calls available through the computer aided dispatch system at intervals throughout 1985. Support gradually grew for department participation in the replication of the Minneapolis study. No one was really satisfied with the data available as a basis for making policy decisions about police intervention in domestic violence incidents. Participation in the study came to be seen as a way for the department to fund a thorough analysis

of domestic violence incidents and, hopefully, to find clear indications of the directions for policy from the project's results. Not long after James D. Munger took over as the new Chief of Police, in early November, 1985, the Director of Administrative Services enlisted the aid of personnel in the department's Crime Analysis Unit to prepare the actual grant application.

NOTES

¹ Colorado Springs Fact Sheet, The Greater Colorado Springs Economic Development Council, August, 1989.

² Gazette Telegraph Answer Book, 1990, Gazette Telegraph, ?? 1990.

³ Figures supplied by the Pikes Peak Area Council of Governments.

⁴ The Fact Sheet, The Greater Colorado Springs Economic Development Council, August, 1989.

⁵ The Fact Sheet, The Greater Colorado Springs Economic Development Council, August, 1989.

⁶ Birmingham, AL, Anaheim, CA, Fresno, CA, Wichita, KS, Saint Paul, MN, Arlington, TX, Corpus Christi, TX, and Norfolk, VA.

⁷ Data from FBI Uniform Crime Reports, 1988, pp. 71-115.

⁸ Reaves, Brian A., Police Departments in Large Cities, 1987. Bureau of Justice Statistics, August, 1989, p.2.

⁹ U. S. Commission on Civil Rights, Under the Rule of Thumb: Battered Women and the Administration of Justice, January, 1982, pp. 14-19.

¹⁰ Ford, David A., "Wife Battery and Criminal Justice: A Study of Victim Decision-Making," Family Relations, 1983, 32(4), p. 465.

¹¹ Ford, 1983, p. 465.

¹² U. S. Commission on Civil Rights, 1982, p. 14.

¹³ U. S. Commission on Civil Rights, 1982, p. 20.

Chapter 2

Preparing to Do Research in a Police Setting

A brief overview of the design.

The Spouse Assault Replication Program (SARP), sponsored by the National Institute of Justice, was conceived as a multi-site experiment in which each site would tailor its design to local conditions and questions of interest while producing certain common data elements that were to be comparable across all sites. The cross-site data would then be incorporated into a cross-site analysis directed by the Program Review Team (PRT), a national panel of professionals in research and criminal justice.

As in the other sites--Milwaukee, Omaha, Miami/Dade County, Atlanta, and Charlotte, the Colorado Springs study involved a field experiment to test the effect of arrest and other dispositions on the frequency of subsequent violent domestic incidents. In the Colorado Springs experiment, an incident was eligible for inclusion if it did not involve a felony and met the following criteria:

- (1) there was probable cause to believe that one of four misdemeanor offenses had been committed--third degree assault, harassment, false imprisonment, or menacing;*

* See page 31 of the grant proposal (Appendix A.) for statutory definitions.

- (2) at or before the time of the incident the suspect and victim were involved in a conjugal relationship with each other;
- (3) the suspect was 18 years of age or older;
- (4) the suspect was present at the scene when the officers arrived;
- (5) the victim was a resident of Colorado Springs; and
- (6) the victim did not insist on arrest.

Once officers determined that an incident met the eligibility criteria, they were to request a disposition from dispatch. Dispatch, in turn, would give the officer one of four randomly imposed dispositions: restore order; issue an emergency protection order; provide immediate counseling to the suspect by a trained therapist; or arrest the suspect. An officer could override the randomly imposed disposition and take a case out of the experiment if he had to arrest the suspect on an outstanding warrant or to insure the safety of the victim or the officer. Information on subsequent incidents within a six month period was to be gathered by the project staff from arrest records and from interviews with the victims.

A major concern of all the sites was the so-called "pipeline study," the component of the project at all sites which would track all domestic disturbances, provide data on which ones were eligible for the experiment and which were not, and which

actually entered the experiment and which did not. In order to test the impact of the alternative imposed under the research design (given the attrition in cases expected with respect to outcome measures), the case count would be a critical variable. Based on an analysis of calls for service during 90 days in the fall of 1986, the Colorado Springs project expected to bring an average of 169 cases a month into the project.*

Organizing and staffing the project.

It was clear from the beginning that the department could not carry out the research alone. For one thing, cooperation from the local judges would be necessary in order for the random imposition of alternatives to work and, particularly, for the EPO process to be manageable, a critical issue since testing the efficacy of the EPO in reducing subsequent violence was a major thrust of the Colorado Springs proposal. For another, the department needed an independent, outside evaluator to analyze the data that would be produced by the project. Also, the department needed therapists to provide the crisis intervention in order to be able to implement the counseling alternative.

The chief judge gave his strong support to the project early in the process which bolstered the department's position. Following

* During the project years, the annual volume of domestic disturbance calls that did not result in a case report was 7862 in 1987, 8549 in 1988, and 9909 in 1989. These figures represent 6% to 7% of calls for service not resulting in case reports. Offenses committed during domestic disturbances cannot be identified in data on crimes reported, but the number of case reports resulting from domestic disturbances is believed to be small.

the evaluator. Dr. Berk had been one of the two researchers involved in the Minneapolis study and the department welcomed his interest in the project. Contracting for counseling services remained to be arranged once the award of the grant was assured.

The Colorado Springs Police Department is organized into three bureaus--the Patrol Bureau, the Investigations Bureau, and the Administration Bureau. Since the Deputy Chief of Administration was the project director of record with NIJ, the project was housed within the Administration Bureau. The deputy chief appointed the crime analyst who had prepared most of the proposal to be the grant manager and oversee the day to day operations of the project. The grant manager, in turn, hired an assistant and a secretary, both from outside the department. These three were the original staff on the project. Later on, other personnel were brought in on a temporary or contract basis to do interviewing, data collection, and data entry.

Over the course of the project, many civilians and sworn officers in the department became involved, some on a full time basis for many months at a time, in coordinating the field experiment within the patrol bureau, providing a liaison between the project staff and patrol officers, checking official records, assisting in locating victims for interviews, and providing technical computer assistance. In addition, the city Information Systems Department provided programming assistance.

In the months preceding the actual field implementation the project staff was concerned with the following: finalizing the design, including settling with the PRT and the other sites on the definitions of common data elements to be included; developing project procedures and data collection instruments; developing and delivering training to communications and patrol personnel who would be involved in the project; contracting for counseling services in order to be able to deliver counseling as one of the randomly imposed alternatives; hiring and training interviewers; and preparing for the collection of criminal history data on suspects and victims and court system "penetration" data on the progress and outcomes of project cases that went to court.

The implementation of the field experiment was dependent on the CSPD Communications Section, which was in the Administration Bureau, and the Patrol Bureau, which was under the command of its own deputy chief. One of the earliest tasks which the project addressed was to get the cooperation of the Communications Section and the Patrol Bureau. Both entities were going to have to assume additional duties associated with the project from which they might not see any immediate benefits. Both would have to commit a certain amount of employee time to training, both would have to commit a certain number of manhours to project tasks once the field experiment was underway, and both would have to handle additional paperwork that was for project purposes only.

With the Chief of the Administration Bureau the director of the project, formal cooperation was assured. Discussions with the Communications Section centered on actual procedures to be used in dispatching trained officers to domestic disturbances during project hours, communicating with officers regarding the eligibility of cases, assigning randomized dispositions and conveying them to the officers, recording information on the call screen, and entering disposition information into the hazard file.

Discussions with the Patrol Bureau initially centered on what officers would be assigned to the project. The resulting plan was to train the twelve officers from the Priority Response Team of the Support Services Unit to be the primary responders in the project. Once trained, these officers would be dispatched to domestic disturbances during project hours. Initially it was not thought necessary to involve supervisors or commanders in the actual project activities. It was expected that the small group of trained officers could operate on their own in the field, turn in the forms they completed, and direct any questions to the project staff.

A Change in Design.

In January, 1987, the project director and project manager met with District Judge Cannon who was a member of the Jail Oversight Committee. The judge was very concerned about the impact of the

experiment on the jail and the court system. The meeting took a different turn, however, when the judge informed the project leaders that he and the other judges were worried about protecting the victim when the police arrested a suspect or imposed the counseling alternative. As the experiment was designed at that time, the possible alternatives were arrest, counseling, EPO, or restore order. As a result of the discussions with the judge, the dispositions were changed to include the issuance of an EPO in all but the restore order alternative. The possibilities then became restore order, issue an EPO, send to counseling and issue an EPO, and arrest and issue an EPO. These were the dispositions actually imposed during the experiment.

Designing the Project Forms

The project forms to be used in the pre-test--the implementation form which the officers would fill out, the interview schedules to be used for the initial interviews with victims, and the forms for collecting criminal history information--were designed first of all to capture data matching the requirements for common data elements as established by the PRT, and secondly to capture data of particular interest in the local situation in Colorado Springs. The implementation form and the interview schedules were drafted by the project staff, reviewed by the evaluator and discussed at the quarterly meetings of all the Spouse Assault Replication Program (SARP) sites.

Deciding on project days and hours.

The various analyses of domestic violence incidents in Colorado Springs had consistently shown Thursday, Friday, Saturday, and Sunday to be the evenings with the highest numbers of incidents. These days were chosen as the days the project would operate. The project hours were established as 1900 hours to 0200 hours.

Training of Patrol Officers.

The Priority Response Team had been chosen to staff the project in the field in part because the project hours spanned portions of two regular patrol shifts and the team worked a special "power shift" which roughly corresponded to the project's hours. The 12 officers received three days (twenty-four hours) of special training which covered the goals of the project, legal issues, officer safety, community resources, and the dynamics of violent domestic situations. The training was conducted by the CSPD training academy, the Domestic Violence Prevention Center, and various professionals from the community.

Selection and training of interviewers.

Two interviewers were hired initially through contacts with the Domestic Violence Prevention Center. They both had been working at the Safe House directly with victims of domestic violence and had experience doing intake interviews there.

Because the interviewers were experienced in working with victims of domestic violence, the training given them by the project staff centered on the administration of the particular interview schedules they would be using.

Contracting for Counseling Services

The June 13, 1986, resubmission of the original proposal described the counseling alternative as the delivery of three hours of crisis counseling by trained therapists to the victim and the assailant at a neutral location to which the officer at the scene would transport them. By January of 1987, due to the requirements of the experiment and cost considerations, the design had been modified to provide counseling to the assailant only. The intention still was to provide immediate crisis counseling, but a backup plan was developed, in case the resources were not available to deliver the crisis counseling, under which the assailant would be asked to sign an agreement that he or she would contact the Domestic Violence Prevention Center within 12 hours to be scheduled for therapy. As part of this back-up plan, the project staff would follow up the next day with the Center to see if the suspect had made an appointment for counseling. As it turned out, it was possible to implement immediate counseling as one disposition in the experiment.

In February a solicitation was mailed to 54 prospective counselors. From the bids received, one organization was

selected to provide the counseling, although the actual contract was not finalized until June.

With an organization selected to provide the counseling, the basic structure for the project's operations were set. Once the project began actual operations, many of the procedures envisioned in the planning stages had to be revised or refined. The operations of the actual experimental phase are described in the next chapter.

Chapter 3

The Field Experiment

The pre-test began on March 26, 1987 and continued through May 10, 1987. Initially, the project operated Thursday through Sunday, from 1900 hours each of those evenings until 0200 hours the following morning. Contrary to the original design which would have restricted the project to the sectors with the highest incidence of domestic disturbance calls, calls from anywhere in the city were eligible to be included in the project. During the pre-test only the arrest, EPO, and restore order alternatives were used because of the expense of the counseling option.

Randomization Procedures

Using SPSS/PC+ V2.0 (copyright SPSS Inc.), the four possible dispositions were arranged in a randomly sequenced list of 2400 alternatives. The sampling feature of SPSS was then used to randomly select 100 alternatives at a time. Each set of 100 was listed in a notebook which was given to the Communications Center. When an officer on the domestic violence team (during the pretest this was limited to the 12 Priority Response Team officers) determined that he had an eligible case, he contacted the Channel 3 dispatcher and requested a disposition. The dispatcher gave the officer the code for the alternative which was next on

the list and recorded the number of the call in the space provided on the list. At the end of the project hours each morning (0200 hours), the list was left with the communications supervisor for the project staff to collect during the day.

Once the assigned disposition was given to the officer, the dispatcher entered the disposition on the call screen. In addition, a hazard was placed on the address. This was a notice that appeared automatically on the call screen if that address was entered into the CAD system on a new call. With a single keystroke, the associated hazard screen could be called up displaying the information on the prior call including what the assigned disposition had been. This procedure was designed to screen out individuals who had already entered the project and had a random disposition assigned as part of the experiment. The hazard notices were removed six months after the date of entry. After that time if the same couple turned up on a new call, they were eligible to be included in the experiment again.

At the beginning of the each shift, the Priority Response Team sergeant provided the Communications Center with the designations of the units that would be available for dispatch to domestic calls. The trained officers were riding in two man cars so only one unit needed to be dispatched to the scene. Only if no unit with trained officers was available was an untrained unit sent. When that did happen, the call was not able to be included in the experiment.

Once a trained unit was at the scene, the officers determined if the call met the criteria for inclusion in the project. During the pre-test the criteria for eligibility were as follows:

the incident had to involve male/female disputants who were married or living together;

the suspect had to be present;

the suspect had to be over 18;

the victim had to be a local resident;

the suspect could not be impaired by drugs/alcohol.

In addition, any one of the following would render the case ineligible:

the incident involved a felony;

the suspect had an outstanding warrant or a restraining order against him;

the officers arrested him on another offense not related to the domestic incident (e.g. the officers found drugs on the suspect);

the officers had to arrest the suspect for their own or the victim's immediate safety;

the call was a repeat call in the project (that is the parties involved had a prior incident in the project, in which a randomized disposition was assigned).

If the call met all the criteria for inclusion, the officers requested that the dispatcher give them a disposition. The dispatcher then gave the officers the next disposition on the randomized list given to the Communications Center by the project. The officers imposed the disposition or overrode it if they had a compelling reason to do so, such as the victim insist-

ing on arrest, the victim being assaulted or threatened in the officers' presence, the suspect assaulting or threatening the officers, or the officers arresting the suspect for another crime that they discovered after they requested the disposition.

Unfortunately, it proved difficult for the trained officers to respond to domestic violence calls because, as Priority Response Team Officers, they were in demand on other types of calls. Fortunately, even before the pre-test began, plans had been made to train additional personnel from among the regular sector officers on the swing and midnight shifts. Within a month of the start of the field test, an additional 17 officers, nine from swing shift and eight from the midnight shift received two days of domestic violence training. A supervisor from the Communications Section also attended the training.

Because of the small numbers of cases that actually were brought into the project in the early weeks, the project moved into seven day a week operation on April 30, 1987 and expanded the hours from 1900-0200 to 1700-0500. The intake portion of the pre-test was concluded on May 10, 1987, with a disturbingly low number of cases brought into the project. Low case flow would turn out to be the major issue that would concern the project staff during the remainder of 1987.

On each case that had entered the project during the pre-test, the officers had filled out an implementation form. The other project processes and data collection instruments needed to be tested, so project staff continued to work with the cases that had been brought in, completing initial and follow-up interviews and collecting criminal history information.

The analysis of the pre-test procedures and the data from the pre-test cases resulted in a number of modifications. (1) It was decided that in order to bring enough cases into the project it would be necessary to operate the project seven days a week from 1600 hours to 0500 hours and to train 15 more officers to respond to domestic violence calls. (2) The eligibility standards for including cases in the project were changed to admit cases involving subjects who had been using alcohol and couples who had a romantic relationship but were not living together. (3) A decision was also made to involve patrol sergeants and lieutenants in the project and to set up training for them. Clearly the last had not been anticipated under the original plan which had only the officers in one special unit involved with the project. Once the project began to involve other patrol officers, it became clear that the support of the first line supervisors and the commanders would be important for the success of the effort.

The decision to allow cases involving intoxicated subjects into the project resulted in a modification to the randomization

procedure. Because it was felt that an intoxicated suspect could not be assigned to the counseling alternative, two random lists of dispositions were constructed: one for use when the suspect was intoxicated and the other for use when he was not. When an officer called for a disposition he had to specify whether he wanted a disposition from the alpha list, which was a randomly ordered list of dispositions excluding counseling, or the bravo list, which was a randomly ordered list using all four dispositions. (The use of two lists was dropped after a few months in favor of having the counselors themselves make a determination as to whether the suspect was too intoxicated to participate in the counseling.)

The implementation form was revised following the pre-test as were the interview forms. Modifications to the forms were limited by the requirements of the core analysis and comparability across sites.

The field experiment resumed on June 28, 1987 with fewer than 30 trained officers. Partly as a result of the small number of officers participating, the case flow remained low, even with the change in eligibility requirements. In an attempt to increase commitment to the project, a luncheon for trained officers was held in mid-July to discuss project procedures and answer questions. Attendance was lower than expected, probably due to the work and sleep schedules of the officers involved. Officers who

did attend were paid time and a half. With the resumption of the project in June, the participating officers were supposed to fill out a domestic violence implementation form on every domestic incident to which they responded, not just those that they found eligible for the project. With only a small group of officers participating, it was expected that the data collected on ineligible calls would be small since it would be limited to cases to which those officers responded. Even so, the number of forms turned in to the project was a small fraction of the number of domestic disturbance calls to which the trained officers responded.

There were two strategies used to try to solve the problem. One was to continue to train additional officers to staff the project in the field. The other was to improve the performance of the trained officers by enlisting the support of their sergeants and commanders.

Up to this point the training of officers for the project had required that they be released from duty for two or three days to attend training. It was not feasible to take additional officers off the street for that length of time for domestic violence training. Instead, it was decided to forego the general training on domestic violence and briefly cover the objectives of the experiment, the procedures officers needed to follow when responding to potential project calls, and the instructions for filling out the implementation form. Throughout the summer and

fall, one-on-one training of this type was given to selected officers by the project manager or the most experienced of the trained officers.

On August 6th a dinner meeting of patrol sergeants and lieutenants was held to acquaint them with details of the project's operation and to enlist their aid in improving the flow of cases and paperwork into the project. As a result of the meeting, the project began sending notices to the swing and midnight shift lieutenants listing domestic violence calls with missing paperwork and the names of the officers who had been sent on the calls.

In order to be able to follow up with officers who did not turn in required forms, it was necessary to identify which calls qualified as domestic violence calls and, therefore, should have a form filled out. Patrol officers were asked to notify dispatch when a call that was dispatched as a domestic disturbance turned out not to be a domestic violence call so that the dispatcher could record that information on the call screen and the project staff would not expect a form for that call.

During the Project Review Team's visit to Colorado Springs in October, 1987, the team members expressed concern about the integrity of the randomization process because the dispatchers were working from open lists and could see what other

dispositions were listed. The PRT recommended changing the procedure to a blind one where each disposition would be sealed inside a sequentially numbered envelope. The dispatcher would then open the next envelope and give the officer the disposition without being able to know what dispositions were in the envelopes that followed. The new procedure was implemented in November. At the end of November, the case count was still low. The project manager recommended that all swing and midnight officers be trained in project procedures and that the patrol sergeants become even more active in the project. These recommendations were passed on to the chief by the project director. One of the patrol lieutenants was appointed to be the liaison between the Patrol Bureau and the project. In addition, one of the trained patrol officers was assigned to the project on a full time basis to coordinate the project activities that affected patrol officers and to provide training and assistance to the officers.

During December of 1987 and January of 1988, written procedures (SOP's) were developed for officers on the project and for dispatchers. In January and February all patrol officers received training in the domestic violence project procedures. Officers who had already attended domestic violence training were required to attend this training as well. The training was conducted at the CSPD training academy in two hour sessions held each Tuesday during January and part of February.

At this time, also, a weekly case flow report was implemented. The report showed the number of domestic calls dispatched during project hours for the week, the number of these that involved spouse or spouse-like situations, and the number answered by project officers. The report also showed the number of calls answered by project officers which proved to be unfounded, the number of forms that should have been completed, the number actually completed, the number of calls found eligible, and the number of dispositions requested. The report also presented a table showing dispositions assigned and actually imposed for the week. For ineligible cases, the report included figures on reasons for ineligibility.

On February 20th, 1988, a new code was established for domestic violence calls coming into the Communications Center. The old code, 2408, was used for domestic disturbances but also for any disturbance which did not fit one of the other categories (bar/cafe/business disturbance, crowd gathering, disturbance outside, drunk disturbance). The new code, 2424, was used only for domestic disturbances. Having a more accurate way to identify domestic calls received by the Communications Center made it easier to track calls that were potential project cases.

With all patrol officers participating in the project, motivation posed a different problem than it did when only a small group of officers was involved. Several incentives were introduced to encourage officers to bring cases into the project. Boards

graphing the number of dispositions imposed by each officer were put up at the sub-stations where the officers reported for duty. The department also began issuing certificates to officers for the successful imposition of project dispositions. For each disposition imposed, the officers were sent a certificate. For imposing ten dispositions a certificate signed by the chief of police and the mayor was presented to the officer by the deputy chief or captain of patrol at line-up. A larger certificate, also signed by the chief and the mayor, was presented to any officer imposing 25 dispositions. A plaque was presented to an officer imposing 30 dispositions. On a few occasions the chief of police made the presentations at meetings of patrol supervisors. In addition, officers were able to get academic credit through the local community college for participation in the project.

During the summer of 1988, one other strategy was implemented to bolster the project's efficiency in the field. Each swing and midnight shift, one or two units were designated as domestic violence cars. These were staffed with two officers each so that a single unit could be dispatched without the need to send a second car immediately. The domestic violence cars were available to respond to domestic calls in any zone in the city. If necessary, they could be dispatched to assist on other priority one calls but generally other officers would handle paperwork and follow-up on those calls. In order to maximize the number of units on the street and still maintain the domestic violence

cars, reserve officers were trained in project procedures and paired with regular patrol officers to staff the domestic violence cars whenever possible.

Chapter 4

Randomization

There are at least four sequential analyses that should be done to address the issue of random assignment implementation. First, it is essential to determine whether the fraction of the experimental subjects in each of the treatment groups approximately corresponds to the designed proportions. A failure to be reasonably close to the design targets would indicate that the random assignment was not properly implemented. Second, it is essential to estimate the number of misassignments. A large number is worrisome because it sets the stage for potential biases in post-test comparisons between treatment groups. Third, it is essential to determine whether there is any pattern to the misassignments. That is, is there any evidence that subjects are being systematically shifted from certain assigned treatments to other delivered treatments? Fourth, if there are a substantial number of misassignments, it is essential to estimate which kinds of experimental subjects are being shifted from one assigned treatment to another delivered treatment. It is only in this manner that one can gain some understanding of any potential biases in estimates of treatment impact (and also be well on the way to developing statistical adjustments that can sometimes make the comparisons more "fair"). In this chapter, only the first two questions will be considered in any depth.

Treatment Implementation: Assigned Versus Imposed Treatments

The data to be used came from the Colorado Springs experiment's implementation forms.¹ At the scene of each intervention (or immediately upon leaving the scene) each participating police officer filled out a form which briefly indicated what had happened. Among the pieces of information recorded were the eligibility of the case (entries 54, 55, and 59), the treatment assigned at random (entry 60) and the treatment imposed (entry 61). The analysis here will concentrate on eligible cases and then compare the treatment assigned to the treatment delivered. In addition to the four original treatments to be assigned (arrest, counseling, EPO, restore order), there is an added "other" category, and in addition to the four original treatments to be delivered, there is an added "other" category and an added "arrest for other crime" category.

Four distinct files of cases were constructed: 1) cases in which the assigned and delivered treatments were clearly the same; 2) cases in which either the assigned and delivered treatments were not the same or in which it was not fully clear if they were; 3) cases in which it could not be plausibly determined if the assigned and delivered treatments were the same; and 4) cases in which a given offender and victim were given a second exposure to the treatment because of a new offense. The relevant cross tabulation for each is shown in Tables 4.1 through Table 4.4. All four tables are combined in Table 4.5 which became the focus

of our statistical analysis (since ideally, all of these cases should have been exposed to the treatment randomly assigned).²

TABLE 4.1: FILE I CASES

		TREATMENT IMPOSED				
		Arrest	Counseling	EPO	Restore	TOTAL
T A						
R S	Arrest	325	0	0	0	325
E S						
A I	Counseling	0	282	0	0	282
T G						
M N	EPO	0	0	322	0	322
E E						
N D	Restore	0	0	0	262	262
T						
	TOTAL	325	282	322	262	1191

TABLE 4.2: FILE II CASES

		TREATMENT IMPOSED						
		Other	Arrest	Counseling	EPO	Restore	Arrest- other	TOTAL
T A	Arrest	0	17	1	20	39	3	80
R S								
E S	Counseling	1	28	14	13	17	1	74
A I								
T G	EPO	0	31	0	18	21	5	75
M N								
E E	Restore	1	28	0	34	21	2	86
N D								
T								
TOTAL		2	104	15	85	98	11	315

TABLE 4.3: FILE III CASES

		TREATMENT IMPOSED						
		Other	Arrest	Counseling	EPO	Restore	Arrest- other	TOTAL
T A	Other	1	0	0	0	0	0	1
R S								
E S	Arrest	2	0	0	1	1	0	4
A I								
T G	Counseling	1	0	2	0	0	1	4
M N								
E E	EPO	1	5	0	1	1	0	8
N D								
T	Restore	8	1	0	1	3	1	14
TOTAL		13	6	2	3	5	2	31

TABLE 4.4: FILE IV CASES

		TREATMENT IMPOSED					TOTAL
		Arrest	Counseling	EPO	Restore	Arrest- other	
T R E A T M E N D T	Arrest	25	0	1	2	0	28
	Counseling	2	20	6	2	0	30
	EPO	3	0	32	2	1	38
	Restore	3	0	0	22	0	25
	TOTAL	33	20	39	28	1	121

TABLE 4.5: TOTAL CASES

		TREATMENT IMPOSED						TOTAL
		Other	Arrest	Counseling	EPO	Restore	Arrest- other	
T R E A T M E N T	Other	1	0	0	0	0	0	1
	Arrest	2	367	1	22	42	3	437
	Counseling	2	30	318	19	19	2	390
	EPO	1	39	0	373	24	6	443
	Restore	9	32	0	35	308	3	387
TOTAL		15	468	319	449	393	14	1658

chi-squared (Treatments Assigned) = 6.46, p-value = .09 (Df=3)
 chi-squared (Treatments Assigned) = 6.40, p-value = .01 (Df=1)
 chi-squared (Marginal Homogeneity) = 65.36, p-value = .00 (Df=3)
 chi-squared (Symmetry) = 73.26, p-value = .00 (Df=6)

If the random assignment were properly implemented, the distribution of cases along the right margin of Table 4.5 should reflect the original research design, and all of the cases within the table should fall on the main diagonal (representing instances when the assigned treatment was the same as the imposed treatment). Clearly, this is not the situation. First, one case was randomly assigned to "other," although this probably represents a recording error. Second, even ignoring the "other" category, the cases are not distributed equally down the right margin. On its face, it seems as if the counseling and restore order treatments were less common than the arrest and EPO treatments. Third, 14 offenders were arrested for crimes other than the instant offense ("Arrest-other"). In effect, for these 14 the experiment was aborted. Finally, for 292 cases (17 percent), the assigned and imposed treatments are not the same. It is also important to stress, however, that some of the counts are a bit uncertain because of recording problems in the data.

What can one make of this pattern? On an optimistic note, the 17 percent misassignment rate compares favorably with the misassignment rate in the Minneapolis experiment, and the biases introduced there do not seem to have been terribly serious (although it is likely that the impact of the arrest treatment was underestimated).³ However, it is difficult to say more without an analysis of Table 4.5.

The significance test results reported at the bottom of Table 4.5 provide an initial sense of how "real" the problems are. The first two Chi-squared figures test the hypothesis that the cases are equally distributed along the vertical margin (i.e., that the probability of assignment was the same for each of the treatments). Even after excluding the one anomalous case of "other," however, one's conclusions depend on how the analysis is undertaken. The PRT recommended (personal communication) a test with all four cells explicitly represented. Then, the p-value is .09. At the .05 level, the null hypothesis of a rectangular distribution is not rejected. This is, of course, a comforting result. Yet, if the two cells with the smallest numbers are compared to the two cells with the largest numbers the p-value is .01. At the .05 level, the null hypothesis of a rectangular distribution is rejected. We favor the second test, since it incorporates a priori information that the crisis counseling intervention was by design used less frequently early in the life of the experiment. Under these circumstances, moreover, the rejection of the null hypothesis is not necessarily problematic; the random assignment may still be effectively valid.

The third test at the bottom of Table 4.5 evaluates the hypothesis that the two marginal distributions are the same. If the experiment was properly implemented the number of people assigned to each treatment should be the same as the number of people receiving each treatment. However, a Stuart-Maxwell test

for marginal homogeneity rejects the null hypothesis decisively. Thus, something "real" is happening in the translation of the assigned treatment to the imposed treatment.

Exploring this "translation" further, it is clear that any answers to be found lie in the off-diagonal (misassignment) cells. For example, if the misassignments were selected by the equivalent of a simple random sample (a good thing for interpreting the experimental results), the pattern of numbers above the main diagonal would be the same as the pattern of numbers below the main diagonal. Unfortunately, a McNemar test of table symmetry was applied, and the null hypothesis of symmetry was decisively rejected. Fortunately, with only a 17 percent misassignment rate, the possible design biases are probably not very large in any cell.

Some Background on the Victims and Suspects

For the Colorado Springs experiment to be useful for policy, the experimental subjects must be a reasonably representative cross-section of people in the community. Some biographical information was collected on the experiment's implementation forms, and we turn to those data now.

Table 4.6 shows not just the racial breakdowns for the victims (the bottom margin of the table), but the breakdowns for each treatment assigned. While the majority of victims were white, there is clearly a healthy mix of ethnic groups represented. The mix is also about what one would expect for the population of

family violence victims in Colorado Springs. There is also no evidence that the ethnic distribution varies by assigned treatment. The null hypothesis of no association (reported at the bottom of the table) was not rejected. Table 4.7 shows that the story is virtually the same for the race of the suspect.

Tables 4.8 and 4.9 undertake parallel analyses for the gender of the victim and suspect. The vast majority of victims are women and the vast majority of offenders are men, certainly no surprise. And there is no association between the treatment assigned and gender in either case.

TABLE 4.6
VICTIM'S RACE BY ASSIGNED TREATMENT

		Other	Asian	Black	White	Hispanic	Amer.Ind.	TOTAL
T R E A T M E N T S I G N E D	Other	0% (0)	0% (0)	100% (1)	0% (0)	0% (0)	0% (0)	100% (1)
	Arrest	1.4% (6)	3.7% (16)	24.9% (109)	55.8% (244)	14.0% (61)	0.2% (1)	100% (437)
	Counseling	0.5% (2)	1.3% (5)	24.9% (97)	58.7% (229)	14.4% (56)	0.2% (1)	100% (390)
	EPO	1.3% (6)	2.5% (11)	22.8% (101)	62.1% (275)	11.1% (49)	0.2% (1)	100% (443)
	Restore	0.8% (3)	3.1% (12)	27.9% (108)	58.1% (225)	10.1% (39)	0% (0)	100% (387)
	TOTAL	1.0% (17)	2.6% (44)	25.1% (416)	58.7% (973)	12.4% (205)	0.2% (3)	100% (1658)

chi-squared (Association). = 18.96, p-value = 0.52 (Df=20)

TABLE 4.7
SUSPECTS RACE BY ASSIGNED TREATMENT

		Other	Asian	Black	White	Hispanic	Amer.Ind.	TOTAL
T R E A T M E N T I N G S T A T E	Other	0% (0)	0% (0)	100% (1)	0% (0)	0% (0)	0% (0)	100% (1)
	Arrest	1.1% (5)	2.1% (9)	30.0% (131)	49.9% (218)	16.7% (73)	0.2% (1)	100% (437)
	Counseling	0.5% (2)	0.8% (3)	30.3% (118)	53.1% (207)	15.1% (59)	0.2% (1)	100% (390)
	EPO	1.1% (5)	0.7% (3)	30.3% (134)	56.4% (250)	11.5% (51)	0% (0)	100% (443)
	Restore	1.5% (6)	1.0% (4)	33.1% (128)	53.5% (207)	10.9% (42)	0% (0)	100% (387)
	TOTAL	1.1% (18)	1.1% (19)	30.9% (512)	53.2% (882)	13.6% (225)	0.1% (2)	100% (1658)

Chi-squared (Association) = 20.74, p-value = 0.41 (Df=20)

TABLE 4.8
SUSPECTS SEX BY ASSIGNED TREATMENT

		NA	Male	Female	TOTAL
T R E A T M E N T I N G D	Other	0% (0)	100% (1)	0% (0)	100% (1)
	Arrest	0.2% (1)	87.2% (381)	12.6% (55)	100% (437)
	Counseling	0% (0)	90.0% (351)	10.0% (39)	100% (390)
	EPO	0% (0)	91.7% (406)	8.3% (37)	100% (443)
	Restore	0.5% (2)	85.6% (331)	13.9% (54)	100% (387)
	TOTAL	0.2% (3)	88.7% (1470)	11.1% (185)	100% (1658)

Chi-squared (Association) = 12.27, p-value = .14 (Df=8)

TABLE 4.9
VICTIM'S SEX BY ASSIGNED TREATMENT

		NA	Male	Female	TOTAL
T R E A T M E N T I N G S	Other	0% (0)	0% (0)	100% (1)	100% (1)
	Arrest	0.2% (1)	12.6% (55)	87.2% (381)	100% (437)
	Counseling	0% (0)	9.5% (37)	90.5% (353)	100% (390)
	EPO	0% (0)	8.3% (37)	91.7% (406)	100% (443)
	Restore	0.3% (1)	13.9% (54)	85.8% (332)	100% (387)
	TOTAL	0.1% (2)	11.0% (183)	88.9% (1473)	100% (1658)

Chi-squared (Association) = 10.89, p-value = .21 (Df=8)

Tables 4.10 and 4.11 address similar issues for the occupation of the victim and suspect.. Keeping in mind that occupation was determined by the officer at the scene (not through victim interviews), perhaps the most important finding is that about a quarter of the suspects were in the military. In addition, the proportions for high status occupations are small, perhaps because of the actual distribution of family violence in the community and/or perhaps because family violence in more affluent households is less likely to be reported to the police. In any case, the occupational distributions in tables 4.10 and 4.11 are about the same for each intervention (see the Chi-squared tests reported at the bottom of each table).

Finally, figures 4.1 and 4.2 show, via boxplots, the age distributions for victims and suspects for each of the four interventions assigned ("other" dropped). Both victims and suspects are on the average (medians) about 30 years of age, although suspects are typically a bit older than victims.⁴ And once again, there is no association between biography and treatment. One-way analyses of variance on both Figures (separately) failed to reject the null hypothesis of identical means.

To summarize, the marginals on some key biographical variables hold no surprises. The subjects include a good mix of people in

TABLE 4.10
VICTIM'S OCCUPATION BY ASSIGNED TREATMENT

<u>OCCUPATION</u>	<u>TREATMENT</u>					TOTAL
	Other	Arrest	Counseling	EPO	Restore	
NA	100% (1)	34.5% (151)	23.9% (93)	31.3% (139)	29.2% (113)	30.0% (497)
Housewife	0% (0)	7.3% (32)	9.0% (35)	9.5% (42)	14.2% (55)	10.0% (164)
Military	0% (0)	7.3% (32)	7.4% (29)	7.5% (33)	4.9% (19)	6.8% (113)
Professional/ Management	0% (0)	5.5% (24)	2.3% (9)	5.0% (22)	4.4% (17)	4.3% (72)
Skilled/ Technician	0% (0)	14.9% (65)	19.2% (75)	13.5% (60)	16.8% (65)	16.0% (265)
Unskilled	0% (0)	18.5% (81)	24.6% (96)	18.3% (81)	18.6% (72)	19.9% (330)
Student	0% (0)	1.4% (6)	1.5% (6)	2.5% (11)	1.8% (7)	1.8% (30)
Unemployed	0% (0)	10.1% (44)	11.3% (44)	11.7% (52)	9.0% (35)	10.6% (175)
Disabled	0% (0)	0% (0)	0.3% (1)	0% (0)	0.3% (1)	0.1% (2)
Retired	0% (0)	0.5% (2)	0.5% (2)	0.7% (3)	0.8% (3)	0.5% (10)
TOTAL	100% (1)	100% (437)	100% (390)	100% (443)	100% (387)	100% (1658)

Chi-squared (Association) = 45.97, p-value = 0.23 (Df=36)

TABLE 4.11
SUSPECT'S OCCUPATION BY ASSIGNED TREATMENT

<u>OCCUPATION</u>	<u>TREATMENT</u>					TOTAL
	Other	Arrest	Counseling	EPO	Restore	
NA	100% (1)	22.7% (99)	17.2% (67)	20.5% (91)	22.0% (85)	20.7% (343)
Housewife	0% (0)	0.9% (4)	1.0% (4)	0.5% (2)	1.8% (7)	1.0% (17)
Military	0% (0)	22.0% (96)	24.6% (96)	25.5% (113)	25.3% (98)	24.3% (403)
Professional/ Management	0% (0)	3.0% (13)	2.6% (10)	2.7% (12)	4.1% (16)	3.1% (51)
Skilled/ Technician	0% (0)	18.1% (79)	18.7% (73)	21.7% (96)	15.5% (60)	18.6% (308)
Unskilled	0% (0)	20.8% (91)	17.9% (70)	18.1% (80)	19.9% (77)	19.2% (318)
Student	0% (0)	1.4% (6)	3.3% (13)	0.7% (3)	1.6% (6)	1.6% (28)
Unemployed	0% (0)	9.8% (43)	11.8% (46)	8.1% (36)	8.0% (31)	9.4% (156)
Disabled	0% (0)	1.1% (5)	0.3% (1)	1.1% (5)	0.5% (2)	0.8% (13)
Retired	0% (0)	0.2% (1)	2.6% (10)	1.1% (5)	1.3% (5)	1.3% (21)
TOTAL	100% (1)	100% (437)	100% (390)	100% (443)	100% (387)	100% (1658)

Chi-squared (Association) = 45.43, p-value = 0.13 (Df=36)

FIGURE 4.1

Victim's Age by Treatment

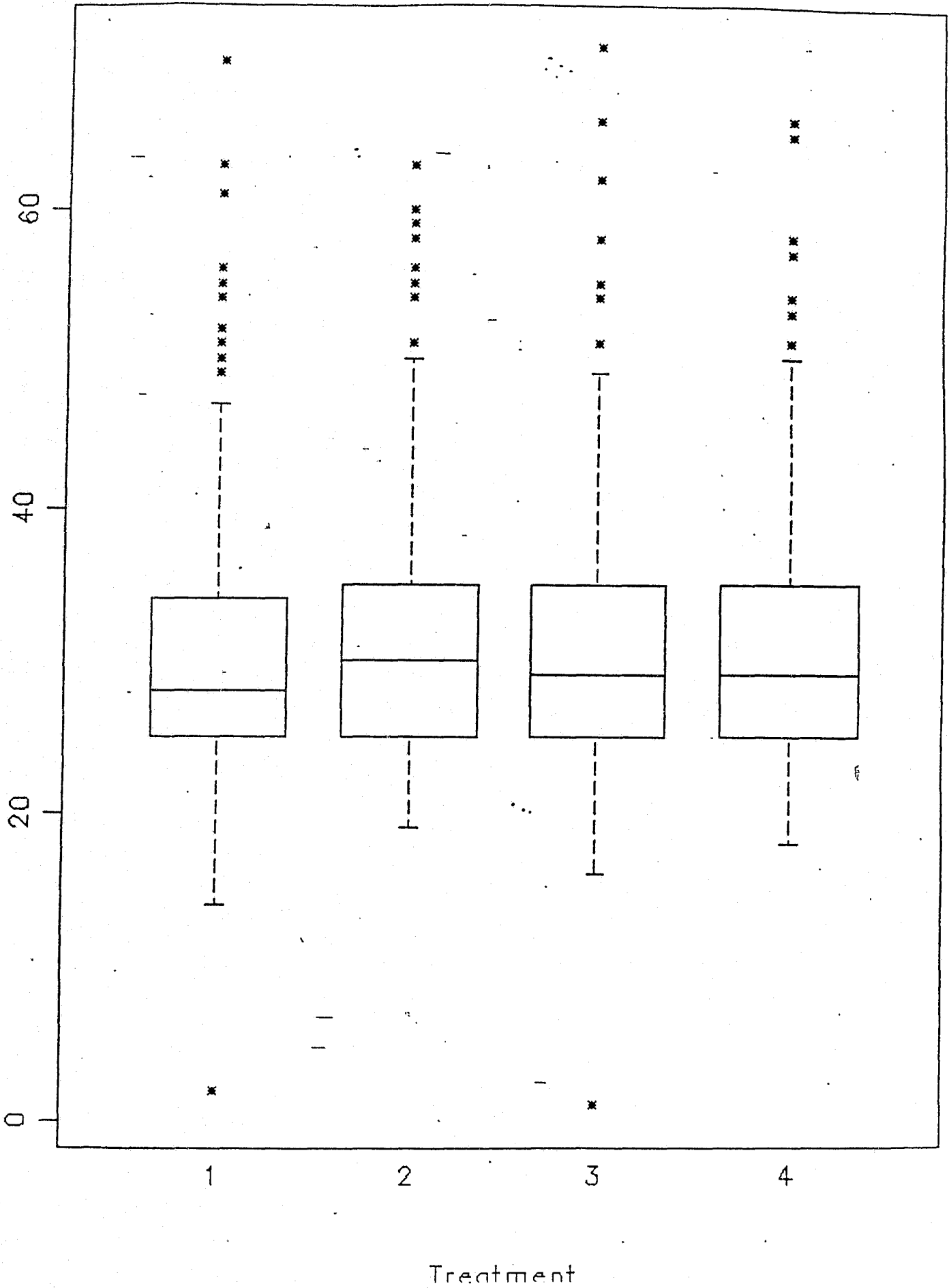
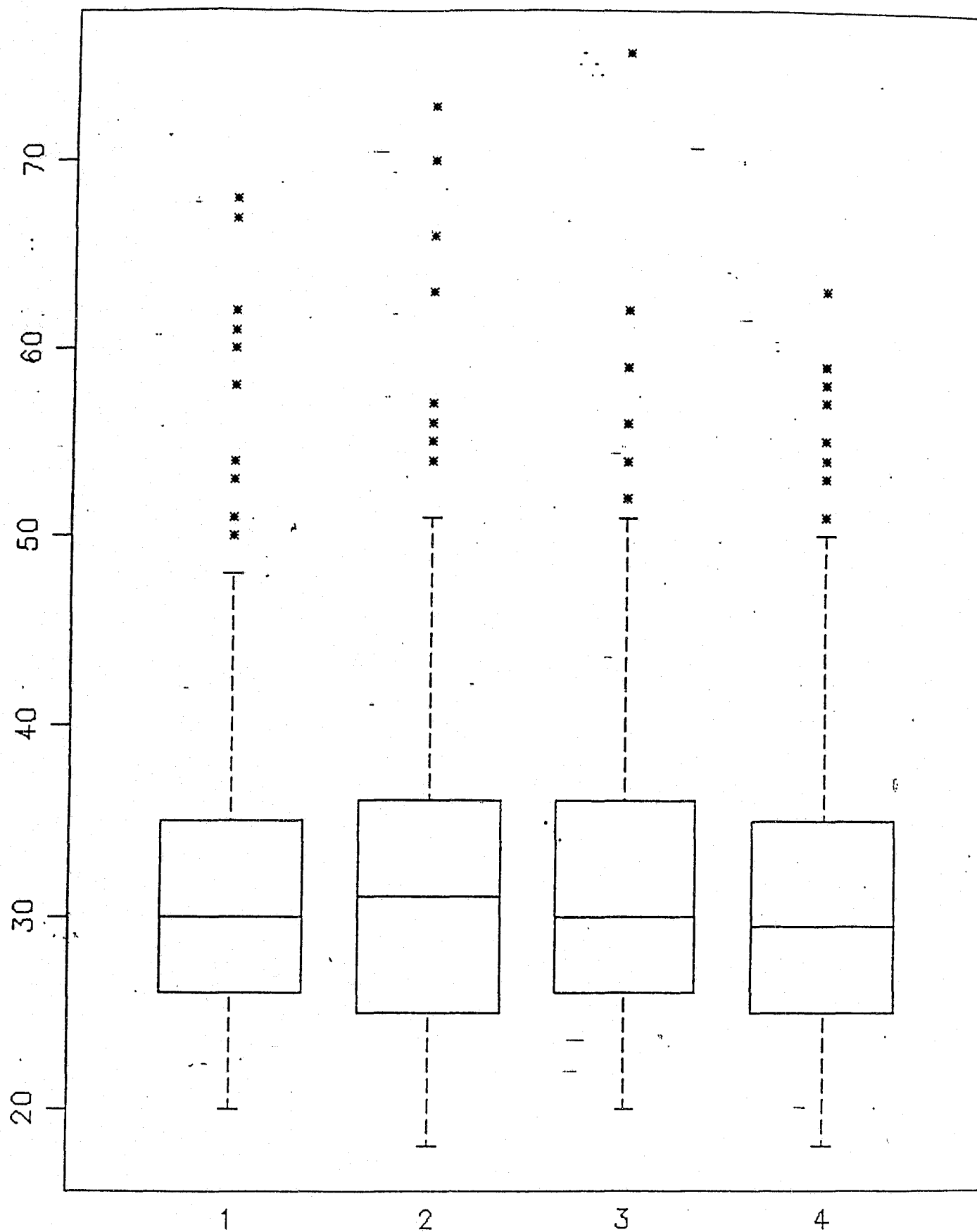


FIGURE 4.2

Suspect's Age by Treatment



roughly the proportions one might expect for families in Colorado Springs experiencing family violence. Moreover, there does not seem to be any association between these key biographical variables and the treatments assigned. We stress, however, that the absence of such association provides no information about biases that may have been introduced when, for 17 percent of the cases, the assigned treatment is not the same as the imposed treatment.

Tables 4.12 through 4.15 compared assigned and imposed treatments by place of occurrence, suspect's behavior toward officers, suspect's action against the victim, and weapon involvement. Threat or injury to the victim and a hostile behavior toward the officers by the suspect are associated with a substantially larger number of arrests imposed than assigned. These factors are associated with a smaller number of counseling and restore order alternatives imposed than assigned. The implementation of EPO assignments seems relatively unaffected by threat or injury to the victim or by hostile behavior toward the officers.

TABLE 4.12

ASSIGNED AND IMPOSED TREATMENT BY PLACE OF OCCURRENCE

<u>Treatment</u>	<u>PLACE OF OCCURRENCE</u>						<u>TOTAL</u>
	<u>Single Family</u>	<u>Multi Family</u>	<u>Public Housing</u>	<u>Bar/ Rest.</u>	<u>Other</u>	<u>Nothing Reported</u>	
Arrest							
Assigned	145	257	3	7	21	4	437
Imposed	169	264	4	8	20	3	468
Counseling							
Assigned	167	196	3	5	16	3	390
Imposed	139	163	3	4	8	2	319
EPO							
Assigned	173	239	3	8	18	2	443
Imposed	176	244	3	5	19	2	449
Restore Order							
Assigned	129	231	2	5	17	3	387
Imposed	122	239	1	8	21	2	393
Other							
Assigned	0	0	0	0	0	1	1
Imposed	4	7	0	0	0	4	15
Arrest other Offense							
Assigned	-	-	-	-	-	-	-
Imposed	4	6	0	0	4	0	14
Total							
Assigned	614	923	11	25	72	13	1658
Imposed	614	923	11	25	72	13	1658

TABLE 4.13

**ASSIGNED AND IMPOSED TREATMENT BY SUSPECT BEHAVIOR
TOWARD OFFICER AT ARRIVAL**

<u>Treatment</u>	<u>SUSPECT BEHAVIOR</u>					<u>Undetermined/ Not Reported</u>	<u>TOTAL</u>
	<u>Coop- erative</u>	<u>Detached</u>	<u>Uncoop- erative</u>	<u>Hostile</u>			
Arrest							
Assigned	267	30	53	46		41	437
Imposed	264	32	66	73		33	468
Counseling							
Assigned	244	25	53	32		36	390
Imposed	216	21	38	23		21	319
EPO							
Assigned	265	36	57	50		35	443
Imposed	266	41	57	45		40	449
Restore Order							
Assigned	220	34	40	48		45	387
Imposed	237	31	39	30		56	393
Other							
Assigned	0	0	0	0		1	1
Imposed	8	0	0	1		6	15
Arrest other Offense							
Assigned	-	-	-	-		-	-
Imposed	5	0	3	4		2	14
Total							
Assigned	996	125	203	176		158	1658
Imposed	996	125	203	176		158	1658

TABLE 4.14

**ASSIGNED AND IMPOSED TREATMENTS BY SUSPECT'S
ACTION AGAINST VICTIM**

SUSPECT'S ACTIONS AGAINST VICTIM*

<u>Treatment</u>	<u>Victim Threatened</u>	<u>Victim Harrassed</u>	<u>Victim Restrained</u>	<u>Victim Injured</u>
Arrest				
Assigned	95	242	61	207
Imposed	128	262	76	253
Counseling				
Assigned	109	239	55	174
Imposed	87	203	44	174
EPO				
Assigned	123	263	61	190
Imposed	124	269	60	189
Restore Order				
Assigned	100	241	44	162
Imposed	80	235	40	140
Other				
Assigned	0	0	0	0
Imposed	3	6	1	2
Arrest for Other Offense				
Assigned	-	-	-	-
Imposed	5	10	0	2

*These actions are not mutually exclusive. On the implementation form officers were asked to check all which applied.

TABLE 4.15

ASSIGNED AND IMPOSED TREATMENT BY WEAPON INVOLVEMENT

<u>Treatment</u>	<u>WEAPONS</u>						<u>TOTAL</u>
	<u>None</u>	<u>Physical Force</u>	<u>Knife</u>	<u>Club</u>	<u>Firearm</u>	<u>Nothing Reported</u>	
Arrest							
Assigned	97	296	14	4	1	25	437
Imposed	90	331	16	7	3	21	468
Counseling							
Assigned	97	259	16	7	2	9	390
Imposed	79	219	13	6	2	0	319
EPO							
Assigned	121	284	15	6	6	11	443
Imposed	120	290	16	6	6	11	449
Restore Order							
Assigned	91	255	15	5	6	15	387
Imposed	109	244	14	3	4	19	393
Other							
Assigned	0	0	0	0	0	1	1
Imposed	3	3	1	0	0	8	15
Arrest other Offense							
Assigned	-	-	-	-	-	-	-
Imposed	5	7	0	0	0	2	14
Total							
Assigned	406	1094	60	22	15	61	1658
Imposed	406	1094	60	22	15	61	1658

NOTES

1 These are, of course, the only data in which there are virtually no missing cases and also the only data collected at the time of the experiment's implementation.

2 At least two of the tables have samples that are too small for analysis.

3 Richard A. Berk, Gordon K. Smyth, and Lawrence W. Sherman, "When Random Assignment Fails: Some Lessons from the Minneapolis Spouse Abuse Experiment," Journal of Quantitative Criminology, Vol. 4, No. 3, 1988, pages 209-223.

4 The two ages near zero in Figure 4.1 are clearly reporting errors of some sort, but they do not affect the overall story shown in the boxplots.

Chapter 5

The Arrest and Restore Order Alternatives

Prior to the project the alternatives available to officers responding to domestic disturbance situations were to restore order and leave (often after encouraging one of the parties to leave voluntarily), seek an emergency protection order, take an intoxicated party to the detoxification center, or arrest the suspect. The choice was driven by concern for the victim's safety and by the desire not to have the original responding officer or another officer have to return to the scene. If they did have to go back a second time, the second call often resulted in an arrest.

During the project the officers were presented with four possible options. The counseling alternative was available only during the project itself. Issuing an EPO, while available as an alternative prior to the project, was handled through some special procedures during the project period. Because of the special nature of these two alternatives, they are described in detail in chapters of their own. The restore order alternative and the arrest alternative are described below.

Restore Order

When officers arrived at the scene on any domestic call their first goal was to restore order, regardless of what other action was to be taken. Under most circumstances, two officers were

dispatched on a domestic call. For safety reasons, officers tried to wait until there were two of them at the scene before approaching the disturbance.

Once at the scene of the disturbance itself, usually inside a residence, the first thing officers would do is to separate the parties from each other and from others in the household. If it was not possible to have the parties to the disturbance in separate rooms, the officers would try to have them facing away from each other and towards an officer. This would allow the officers to maintain eye contact with each other while talking with and being able to control the party in front of them. This procedure helped to insure the safety of both officers.

Once the parties were separated, officers could get each person's side of the story and determine what had occurred. During this interview process officers were also able to determine who was the suspect and who was the victim. During the process of calming the suspect and victim, the officers discussed referral options with them.

The form that the officers had to complete on project cases contained a section where officers were to check off the actions they took at the scene. Officers often used this section of the form as a checklist for themselves while at the scene. The list of actions from the form is as follows:

calm things down;
give personal advice on how disputants could get along with one another;
provide information about legal rights;
recommend someone go for legal assistance;
provide information on women's shelters or support groups;
help victim contact victim support or shelter program;
recommend or refer to family counseling;
refer to victim/witness program.

During the project the restore order alternative was sometimes imposed in situations where officers would have chosen another alternative if it had been up to them. In these instances, the officers sometimes stayed on the scene a long time trying to make sure that the situation was under control. When the situation was especially volatile, officers would try to play down the seriousness of the situation in order to make restoring order and leaving seem like a reasonable alternative to the victim and to the officers themselves.

Arrest

When the disposition to be imposed was arrest, the officers were faced with a different situation from one in which they were only supposed to restore order. In order to set the stage for an arrest, officers would stress the seriousness of the situation to both parties and emphasize that the law had been broken.

There were three important differences between a typical misdemeanor arrest in a domestic situation and an arrest during the project. The first was that under normal circumstances arrests were only made when the victim was willing to sign a complaint or an offense was committed in the officer's presence. During the project, when the arrest disposition was assigned, it was not necessary for the victim to sign a complaint. The second difference was that normally, if a summons was issued and the suspect was cooperative, no EPO was sought. During the project, an EPO was always issued when an arrest was made. The third difference was that a typical misdemeanor arrest involved serving the suspect with a summons and releasing him. During the project, all arrests were "in custody" arrests. This means that the suspect was removed from the premises by the officers and taken to the jail for booking. Most often, parties arrested during the project spent thirty minutes to about two hours at the jail before being released on bail or on their own recognizance.

Arrest was imposed in 468 cases in the project. The suspect was handcuffed in 122 cases, but handcuffed in front of the victim in only 67.

Chapter 6

The EPO

While in field experiments it is not possible to achieve the controlled conditions of a laboratory, it is desirable for as many outside factors as possible to remain static during the course of the experiment. This experiment was undertaken because of heightened interest in domestic violence and the interest of law enforcement professionals and many others in developing strategies to combat it. Throughout the experiment, there was considerable activity going on outside the police department to develop services for victims and abusers, strengthen laws, and encourage a broader role by criminal justice agencies. During the course of this project, legislation was first passed and then repealed establishing domestic violence as a discreet offense. Locally, services for victims and abusers continued to evolve throughout the life of the project. A local Domestic Violence Coalition was established with members from the judiciary, the legal community, private and public social service agencies, and law enforcement. The Coalition worked to improve and coordinate the community's response to the domestic violence problem. Potentially, all these activities had an impact on the context in which the experiment was operating. Efforts were made to try to keep practices that directly affected the experiment constant over the life of the field portion of the project. Nowhere was

this more of a challenge than with the issuing of Emergency Protection Orders by the court.

Emergency Protection Orders (EPO's) are temporary restraining orders issued by a county or district judge for the purpose of protecting actual or potential abuse victims. During normal court hours, the victim must apply directly to the court for the order. Outside normal court hours--that is, evenings, weekends, and holidays--an emergency protection order may be approved verbally by the on-call judge who will indicate what the provisions of the order are to be. An EPO is good until 4:30 p.m. the next court business day. The victim (the protected person) may apply to the court between 1 p.m. and 3 p.m. of the next court business day to have the order extended, modified, or terminated. Violation of an EPO is a violation of a court order and is an offense separate from any other offense involved such as trespass, harassment, or assault.

The first procedural problem arose during the pre-test. Apparently it had been a practice in the past for officers to call the dispatcher to contact the on-call judge when a telephone was not available to the officers at the scene. The dispatcher would place the call to the judge, relay the required information, and try to obtain the EPO. It was determined that this procedure was not correct legally. While the officers could relay the information through another sworn law enforcement officer, legally they could not relay the information through the

civilian dispatcher. With the concurrence of the chief judge, the commander of the Communications Section issued a memorandum to patrol officers clarifying this point and giving new instructions on procedures to follow when a telephone was not available. Under the new procedures, the officers were directed to call, via radio, their shift commander, sergeant, or another officer with access to a telephone and relay the information for the EPO to them. The receiving officer would then call the judge and obtain the EPO. This procedure remained unchanged during the remainder of the experiment.

Under department procedures on EPO's established in 1984, an officer must request an Emergency Protection Order whenever a suspect is physically arrested for a criminal offense arising from a domestic abuse incident. In all other cases in which an officer believes that an adult or emancipated minor is in "immediate and present danger of domestic abuse," the officer is to seek an EPO.¹ Had the project implemented the original design under which a suspect would have been arrested without the issuance of an EPO, the design requirements would have violated specific department policy. As it was, the imposition of the restore order alternative was sometimes in conflict with the department directive to seek an EPO when the victim was in danger. According to the experimental procedures, if an officer felt strongly that he could not apply the restore order alternative, he was to declare the case ineligible and not request a

disposition. There may have been some cases in which the officer requested a disposition, gambling that he would not be assigned the restore order alternative, only to override it when he did receive it in order to provide some protection for the victim.

Emergency Protection Orders are meaningless if not enforced. Because under the project there were many more EPO's issued than under normal conditions, there were many questions from officers about enforcing EPO's. On January 12, 1988 an amendment to the 1984 order on EPO procedures was issued providing additional details on the procedures to be followed when an EPO was violated. Under these procedures when an officer believes that a subject has violated an EPO previously served and still active he is to do one of the following:

- o arrest the violator and take him before the issuing judge if the court is in session;
- o arrest the violator and book him (or her) into the jail for contempt of court;
- o arrest the violator for a criminal offense committed in conjunction with the violation of the EPO and document the violation of the EPO in the offense report;
- o make an offense report of the violation listing the State of Colorado as the victim if the violator is gone prior to the officer's arrival or the officer uses his discretion to remove rather than arrest;
- o have a formal pick-up made if the violator is gone prior to the officer's arrival and the officer feels that an arrest is appropriate.²

The procedures clearly state that "under no circumstances shall a 'Sworn Member' complete a summons and complaint for the violation of an Emergency Protection Order." The procedures also note that

per order of the Chief Judge, any person booked into the jail for violation of an EPO will be held without bail until the next court business day.

In the original proposal the four alternatives to be tested were restore order, EPO, counseling, and arrest. It was in response to the judges' concerns for the safety of victims when the suspect was taken to counseling or arrested that the design was changed to have EPO's issued along with the imposition of counseling or arrest. That substantially increased the burden on the on-call judges who had to approve each EPO.

By May of 1988 the volume of requests for EPO's was overwhelming the on-call judges. From January through April of 1988 an average of 111 EPO's per month were filed with the court. Reversing their previous position, the judges felt that the issuing of an EPO was unnecessary when a suspect was arrested and held without bond until the next business day. In a meeting with CSPD officials on June 3rd the judge attending was made aware of how important it was to maintain the same procedures for the imposition of the various alternatives throughout the experiment. Changing the arrest disposition from arrest and EPO to arrest without EPO midway in the experiment would jeopardize the success of the project. Several alternatives were discussed, most of which involved making provisions, statutory if necessary, to

authorize designated court or law enforcement personnel to issue EPO's with various controls and judicial review.

At the June en banc meeting of the Fourth Judicial District and El Paso County Court judges a tentative decision was made to terminate completely the issuance of emergency protection orders. The problem confronting the judges was simply that they were receiving so many requests for EPO's at night that they could not get enough rest to be able to perform effectively in court during the day. But terminating issuing EPO's would not only jeopardize the project, it would eliminate an important tool for protecting victims of domestic violence in the community.

When the judges gave July 1st as the date on which they would stop issuing EPO's the Domestic Violence Prevention Center and various community groups voiced their concerns to the police department. One of the CSPD captains met with the Chief Judge of the Fourth Judicial District on June 24th to discuss the various possibilities. The chief judge did acknowledge that complete cessation of EPO issuance was against existing statute. He further stated that he had notified the Chief Judge of the Colorado Supreme Court of the action the local judges intended to take but had not received any response. The Fourth District's Chief Judge expressed his continued support of the project and did say that the judges had delayed the termination of EPO issuance until August 1st which would give the judges an opportunity to discuss it again at their July meeting.

In response to the possibility of cutting off EPO's, the Chief of the Colorado Springs Police Department wrote to the Chief Judge of the Supreme Court of Colorado requesting his intervention. The Chief Judge advised both the local chief judge and the police chief to work towards a mutually satisfactory agreement.³

On August 10th, the Chief Judge of the Fourth Judicial District issued Chief Judge Order 88-10 which prescribed procedures under which peace officers were authorized to issue Emergency Protection Orders between the hours of 5 p.m. and 7.am. Monday through Friday and all day weekends and holidays without calling the on-duty judge. Copies of all EPO's so issued were to be delivered to the Judicial Administrator no later than 10 a.m. the next working day so that the on-duty judicial officer could review them.⁴ On August 17, 1988 a further refinement of EPO issuing procedures by the chief judge was communicated to patrol officers through a CSPD memorandum. Officers were directed to strike out "Witness the Honorable Judge" on the EPO document and replace it with "Chief Judge Order 88-10," and to put the name of the commander or his designee on the line where the name of the issuing judge had appeared before.⁵

On August 18th, before any officers had issued EPO's under Chief Judge Order 88-10, the deputy chief of administration at the CSPD issued a bulletin suspending the issuing of EPO's by officers without a judge's approval. Officers were directed to contact the on-call judge to approve EPO's prior to issuance. The reason

given for the suspension of officers' issuing EPO's on their own authority was to obtain legal review of the judicial order authorizing the direct issuance of EPO's by officers (Chief Judge Order 88-10).⁶ In fact, a representative from the City Attorney's Office had already contacted CSPD commanders and told them to stop the experiment pending the outcome of the legal review only to be immediately overridden by the chief of police. On August 23rd, the city attorney notified the chief of police of action taken by the city council regarding emergency protection orders. The council adopted the recommendation of the City Attorney's Office that the on-call judge must ratify any EPO's issued during the night by officers and that officers must consult the on-duty judge before taking any action under an EPO issued on the officers' own authority and not yet ratified by a judge. Since no EPO's were issued under Chief Judge Order 88-10, the council's action had no effect on actual procedures, but the council's and city attorney's actions demonstrate how quickly such issues can become further complicated by additional actors. Fortunately, throughout the period of confusion over how EPO's were going to be handled certain judges made themselves available to take officers' calls requesting EPO's, thus keeping these events from interfering with the imposition of EPO's in the experiment.

On August 25th the city agreed to furnish a cellular phone for the use of on-call judges responding to requests for EPO's for

the duration of the CSPD's domestic violence project and at the expense of the project. While this did not solve the problem of the judge on call having his sleep interrupted frequently, it did increase the chances that the designated on-call judge could be reached successfully and that the other judges would remain undisturbed.

On September 9th a memorandum was issued by the Fourth Judicial District to all law enforcement agencies (not just the CSPD) informing them that a central number had been established that would provide a link to the on-call judge without having to call his or her residence number.⁷ The "link" was, in fact, the cellular phone. When the CSPD bulletin was issued September 19th on contacting on-call judges it directed officers to first call the number of the cellular phone, and then to resort to normal paging procedures if they failed to make contact. Use of the cellular phone eliminated one step in the old process--contacting dispatch to determine which judge was on call--and was therefore more efficient from the officers' point of view. With the cellular phone in, use the issuance of EPO's proceeded smoothly for the remaining months of the field experiment.

The problems with the EPO's demonstrate the difficulties of operating field experiments. They also raise the question of the practical problems of widespread EPO use. EPO use in Colorado Springs dropped off sharply after the end of the project. If EPO's were sought on a regular basis at the rate they were during

the field experiment the present system in Colorado Springs would have difficulty handling the workload.

NOTES

- ¹ General order issued 12-14-84, manual reference L 1400.5, manual number L 1460-L 1460.20.
- ² General order issued 1-12-88, manual number L 1460.20.
- ³ Letter from Chief James D. Munger to the Chief Justice of the Supreme Court of Colorado, July 18, 1990, and responding letter from Chief Justice Joseph R. Quinn to Chief Munger July 29, 1990.
- ⁴ Chief Judge Order 88-10, issued by Chief Judge Donald Campbell, Fourth Judicial District, August 10, 1988.
- ⁵ Memorandum from Capt. William C. Thiede to all sworn officers, August 17, 1987.
- ⁶ CSPD Bulletin, Serial No.: 528-88(A), approved by James G. Vetter and issued 8/18/88.
- ⁷ Memorandum from Doug Haxton, Officer of the Court Administrator, Fourth Judicial District of Colorado, to all law enforcement agencies, September 9, 1988.

Chapter 7

Counseling

The counseling alternative deserves a chapter of its own because any findings with respect to the impact of this alternative on the subsequent behavior of abusers would be difficult to interpret without a thorough understanding of the procedures and content of the counseling alternative. The structure, procedures for imposition, and content of the counseling are described in some detail in the following pages.

The Structure of the Counseling Alternative

The organization which provided the counseling was selected through a competitive bid process. The therapists associated with the organization had specific experience in counseling abusers and victims of domestic violence and were well qualified to provide the counseling sessions envisioned under the research design. Of the original ten therapists who began working with the project, six continued throughout most of the project.

When the main part of the project started at the end of June, 1987, the plan called for an initial counseling session with the abuser immediately following police intervention at the scene and a subsequent group session to which a subset of the abusers would be assigned. Because of the low case flow into the project, it

quickly became clear that there were not enough abusers receiving the counseling alternative to organize group sessions within a reasonable time period. The plans for group sessions were dropped without a single group ever meeting. An individual second counseling session was substituted for the group session in the design. Until November of 1988 abusers were systematically selected for the second counseling session. All first counseling sessions were recorded in a notebook in consecutive order. If a session had an even number the abuser was scheduled for a second session. If it had an odd number, no second session was scheduled. In November of 1988, the selection process was dropped and all abusers were scheduled for second counseling sessions. Since the turnout for second sessions was only moderate, scheduling all abusers for second sessions was intended to increase the number of second sessions held. This would allow for an analysis of possible outcome differences between abusers attending a single session and those attending two.

First sessions were held in a private office at police department headquarters. If abusers were sober they were allowed to travel on their own to headquarters. Otherwise, they were transported by officers. Second sessions were held at the offices of the organization providing the counselors.

Procedures for Implementing the Counseling Alternative

As discussed in Chapter 3, the procedures for the imposition of the counseling alternative changed several times in the early months of the project. However, the content of the counseling did not change, except for very minor refinements, during the life of the project.

Because of the cost of implementation, the counseling alternative was excluded from the pretest conducted in the spring of 1987. When the main experiment was begun in June of that year, all four alternatives were available. Under the design implemented in the pretest, only suspects not impaired by alcohol or drugs were eligible to be included in the experiment. That was changed for the start-up of the main experiment to a design under which impaired suspects were eligible for inclusion in the experiment but not eligible to have the counseling alternative imposed. For several months, two lists were used for imposing randomized dispositions--a randomly ordered list of the four alternatives for suspects unimpaired by drugs or alcohol and a randomly ordered list of the three alternatives that did not involve counseling for suspects under the effects of drugs or alcohol (usually alcohol). Early in the fall of 1987, the two lists were abandoned in favor of a single randomized list of all four alternatives. The decision as to whether the suspect was able to participate in counseling was left to the counselor. If a suspect was judged too intoxicated to participate, the counselors were to schedule an appointment for a first session at police

headquarters for the following day. This procedure was followed for the remainder of the project.

In the spring of 1988, the project purchased two breath sensors for determining the blood alcohol level of suspects who appeared for initial counseling sessions. From then on, all suspects, whether suspected of being intoxicated or not, were asked to take the test and their blood alcohol levels were recorded by the counselors. However, the decision as to whether a suspect was able to participate in counseling remained a subjective one on the part of the counselors. Since the case flow into the project was very light until the winter of 1988, most cases were handled under the final set of project procedures. The only exception to this was that only half of the abusers receiving counseling were scheduled for a second session until the fall of 1988.

The following description of the procedures for imposing and carrying out the counseling alternative applies to the final procedures. When the dispatcher gave an officer the code for the counseling disposition, the officer then notified the suspect that he (or she) was being offered counseling. At that time the officer also initiated procedures to obtain an Emergency Protection Order and requested the dispatcher to page the therapist who was on call on that day. If the suspect refused to go to counseling, he was served the EPO, escorted from the premises if he was required to leave under the terms of the EPO, and released

unless it was necessary for the officers to transport him to detox or another location.

If the suspect was not affected by alcohol, the officer could decide to allow him to travel to headquarters on his own, in which case the officer notified the front desk officer that the suspect was en route and provided a description of the person. If alcohol was a factor, the officer was required to transport the suspect to police headquarters.

Once the suspect arrived at headquarters, he waited in the lobby for the therapist to arrive. (If an officer transported the suspect, he was free to leave as soon as he had delivered the suspect to the lobby.) If dispatch failed to make contact with a therapist within twenty minutes, or if a therapist who had been contacted failed to show up within 30-45 minutes after the suspect's arrival, the front desk officer would notify the suspect and tell him that he could leave. (Actually, the suspect was free to leave at anytime since his participation in the counseling was voluntary.)

Once the therapist arrived, he escorted the suspect (now the client in the counseling) to the office used for the counseling. The therapist determined whether or not he could proceed with counseling at that time. From May of 1988 on, he asked the client to take the breath sensor test and recorded the blood alcohol level. If the client was too intoxicated to participate

in counseling or if client was uncooperative, the therapist reminded the client that an Emergency Protection Order was in effect and discussed housing options with him. There was a phone in the office that could be used to arrange a place to sleep (a friend or family member's residence, a hotel, the Red Cross Shelter, etc.). The counselor also helped the client arrange transportation to his destination. If transportation could not be arranged and the person had a blood alcohol level of .05 or greater (legal intoxication) the counselor notified the front desk officer and the person was not allowed to leave on his own. If the offender was not legally intoxicated he was allowed to leave on his own from the lobby.

Clients were free to leave at any time during the counseling session, but were encouraged to stay. Whether the client refused to participate, left part-way through, or completed the counseling session, the procedures described above for helping the client determine a destination and arrange transportation, and for dealing with intoxicated clients were followed.

Sessions Completed

While the counseling alternative was assigned in 390 cases, it was actually imposed in 319. A total of 318 reports of first counseling sessions were entered into the data set. Of these, three do not show starting or ending times and may not have taken place. Five show a duration of only ten minutes to forty

minutes, suggesting that they may have been cut short by the client's departure. Fifteen have a starting time recorded but no ending time. Some or all of these may have been refusals. A total of 278 first sessions lasted one hour or longer, usually one and a half hours.

One hundred ninety-two second sessions were assigned. It is clear that 124 were completed. Fifty-nine scheduled sessions did not take place. Nine sessions show a starting time but no ending time, leaving it unclear as to whether the client showed up.

The Content of the Counseling

The first session.

The first session was designed as one and a half hours of crisis intervention. The first and most basic goal of the session was to get the client to abide by the terms of the Emergency Protection Order for the period of time it was to be in effect. In addition, the goals were to get the client to recognize that physical violence is wrong and is a problem to work on, to get the client to sign an agreement to use time-outs as a way to avoid violence, control anger, and de-escalate tension, and to get the client to agree to seek further treatment.

At the beginning of the counseling session the client was given a copy of the domestic violence project handout to read and sign. The handout informed the suspect that he had been randomly se-

lected to receive counseling rather than be arrested, that the counselors were not affiliated with the police department, that the discussions with the counselor would remain confidential (but that the police department would be notified as to whether or not he attended the counseling sessions scheduled for him), and that it was to his benefit to participate in the counseling since if there were another violent incident he would probably be arrested. At the bottom of the form were two statements: "I pledge never to allow my anger to go to the point where I forcefully touch another family member, no matter how right I feel I am" and "I pledge to use time-out procedures instead, whenever my partner initiates time-out." The client was asked to sign the form directly underneath the two pledges. One copy was kept by the counselor. A second copy was given to the client.

Next, the client was given a single sheet titled "What is domestic violence?" which provided a definition and examples of "hands on" and "hands off" violence. Fifteen minutes were allocated to the discussion of the information on this page. The client was given a copy to take home.

A one page handout on the cycle of violence was then used as the basis for discussing the stress that the offender was experiencing and helping the individual to understand his own stress/anger/violence rhythms. Fifteen minutes were allocated to this discussion and the client was given a copy of the handout to take home.

The next fifteen minutes of the session were used to help the client put the escalation of his anger into perspective by seeing his feelings and reactions in terms of a scale that ranged from 0 to 10 where 0 was "no anger" and 10 was "extreme anger accompanied by violence." Questions on the form asked the client to describe in detail what he did, what his self-angering thoughts were, what he could have done differently, and what he would do next time. Clients who were only to participate in one session worked with the form in the session and were given a copy to take home. Those scheduled for a second session were asked to complete it at home and bring it to the second session.

The time-out contract was then introduced to the client and he was encouraged to use the anger scale just discussed to identify the characteristics of his own danger zone on the contract. The contract was intended to elicit a commitment from the client to try a "time-out" rather than allowing a situation to escalate into a violent incident. Under the contract the client agreed to ask for a time-out or to acknowledge and abide by his partner's request for a time-out, go to a quiet private place, calm himself down with the help of specific thoughts or activities, and only return when he was in control.

In addition to a copy of the time-out contract itself, (the original was retained by the therapist), a sheet with an explanation of the time-out process was given to the client. The

sheet was addressed to the client's partner and explained the use of the time-out technique.

In addition to the materials already described, each client was given three other handouts, "Power and Control Wheel," "Anger Lessons," and "How to Turn Off the Road to Anger." The "Power and Control Wheel" is a graphic presentation of eight typical methods of exercising control over the partner. "Anger Lessons" is a sheet with seven questions for the abuser to answer about his parents' behavior when they were angry, how they handled conflict, what the abuser thought he learned from his parents about conflict and anger, how he expressed anger as a child, and what he had learned that he would like to change. "How to Turn Off the Road to Anger" gives fifteen concrete thoughts to employ to keep anger under control. Clients scheduled to return for a second session were asked to answer the "Anger Lesson" questions and bring the sheet to the second session. Clients not scheduled to return were encouraged to use all the additional materials as best they could and to consider making an appointment on their own.

The second session.

The chief goal of the second counseling session was to get the client to stop physical abuse of family members for at least two weeks and, hopefully, for ninety days. The subsidiary goals were to get the client to understand what led to the most recent violent episode and to recognize his own pattern of violence

including the stimuli that cause stress and his personal triggers and to get the client to make a commitment to deal with anger in non-violent ways such as employing time-outs, communicating more effectively, using relaxation techniques, and learning to visualize stressful situations and mentally rehearse strategies to cope with them. In addition, the counselors encouraged the clients to seek additional counseling, either through the organization offering the crisis intervention or through other private or public resources in the community.

Much of the second session was structured around four of the items presented to the client as handouts in the first session: "Anger Lessons;" "Anger Scale Self-Examination;" "Power and Control Wheel;" and "How to Turn Off the Road to Anger." If the client had not completed the exercises at home, he could complete them during the session. The second session afforded the time to go into the material in these items in considerably more depth than in the first session. However, no new or different concepts were presented in the second session.

Chapter 8

Interviewing

One of the major undertakings of the project was the interviewing of victims, not once, but a minimum of two and as many as seven times during a six month period. The purpose of the interviews was to collect more information about the suspect and victim than could be gathered by the officer handling the incident and to collect information about domestic violence prior and subsequent to the project incident. Under the original design of the project, all victims were to be interviewed as soon as possible after the incident that received the randomized disposition, interviewed every two weeks for the next three months, and then given a final interview three months later--six months after the project incident. The follow-up interviews were designed to pick up information on repeat incidents while they were still fresh in the victims' minds, but there was some concern that the frequent contact with project staff could actually affect the behavior of victims and suspects and, therefore, could contaminate the experiment. The other sites' designs called for only initial and final interviews. In order to be able to check for the possible effects of frequent contact and to have data comparable to the other sites it was decided that 25% of the victims would receive only initial and final interviews while the remaining 75% would receive follow-up interviews as well.

Design of Interview Schedules

The interview schedules were designed by project staff, primarily the interviewer supervisor. Before the pre-test began, the schedule for the initial interview was tested on a small group of women at the Domestic Violence Prevention Center Safe House. The other interview schedules did not receive a formal test prior to the field experiment. Several revisions were made in the instruments once the experiment was under way. Since these occurred early in the project, the bulk of the interviews were conducted with the final versions of the interview schedules.

There were four versions of the initial interview schedule that were actually used by the interviewers. One was used for fewer than half a dozen interviews. (The data from this version was not keypunched due to the time and costs involved in creating separate data entry screens and a separate code book for so few cases.) Two others were used more extensively but still only accounted for a small percentage (7%) of the 1251 initial interviews completed during the project. The changes from the early versions to the final version were largely in the wording of questions and the order in which they appeared on the schedule. On a few questions, response categories were added to cover responses interviewers reported getting from victims in the early interviews.

The questions asked in the initial interviews covered several topics. The first was the character of the relationship between

the victim and suspect, whether they were married, whether they were living together, how long they had known each other, and how long they had had their present relationship. The victim was then asked about any incidents in the previous six months in which the suspect had threatened her, damaged property, physically restrained her, or hurt her or someone else in the household. Information gathered on these incidents included what types of acts the suspect had engaged in, who they were directed against (for example, who was hit or whose property was damaged), whether there was any injury and the extent, and what, if any, police intervention occurred.

Next the victim was asked about the incident that resulted in the case coming into the project. Again, questions were asked about what the suspect did, to whom he did it, and what the result was, for example, any injuries. More detail was requested about what the police did at the scene in this incident than about past incidents. The victim was also asked how she felt about the police actions and whether she felt safer or less safe as a result.

Even though the time span between the project incident and the initial interview was short, the victim was asked about any subsequent contacts with the suspect and any new incidents which had occurred. Some questions about what had transpired since the police contact at the time of the project incident differed

depending on which alternative had been imposed under the project. For example, if the arrest alternative had been imposed the victim was asked if she had bailed out the suspect, whether she planned to testify against him, and whether she had had contact with the District Attorney's Office or Pre-Trial Services Agency. Information was gathered on the timing and nature of contacts between the victim and suspect since the project incident. The questions about new incidents involving abuse paralleled the questions asked earlier in the interview about past incidents. Finally, questions about the victim's and suspect's occupation and education were asked as well as questions about the age and sex of all children in the household.

There were three versions of the follow-up interviews. As with the initial interviews, few of the changes had to do with the content of the questions. Aside from the changes from one version to the next, the same interview schedule was used for all of the follow-up interviews. That is, the same questions were asked at the second, third, fourth, and fifth follow-up interviews as at the first follow-up. The first follow-up interview was scheduled for two weeks after the initial interview. The second, third, and fourth interviews followed at two week intervals. The fifth interview was scheduled for four weeks after the fourth interview so that it would fall at three months after the project incident. Information was obtained on the extent of contact between the victim and suspect since the initial interview (or since the last follow-up interview if one

or more follow-up interviews had already taken place), and on any new incidents since the last interview, including information on any police intervention.

Two versions of final interviews were used in the field. The questions asked in the final interviews were mostly the same as those asked in the follow-up interviews except that the time period covered was the three months since the last follow-up interview (for 75% of the cases) or the six months after the initial interview (for 25% of the cases). Information was collected on the extent of contact with the suspect, any new incidents of abuse, any police intervention, and the current status of the relationship between the victim and suspect.

Hiring and Training of Interviewers

In the months preceding the pre-test, the decision was made to hire interviewers on a contract basis rather than as project employees. This turned out well for the project, especially since the case flow was low for many months. Initially, two interviewers were brought into the project on a contract basis. Both had been working with victims at the Domestic Violence Prevention Center. As part of their duties they had been conducting intake interviews at the center's Safe House. As a result it was felt that the only training that was needed was instruction in using the various interview schedules that had been developed for the project.

The training consisted of studying the interview schedules and then being tested on them by the interviewer supervisor. Before working on their own, the interviewers also conducted a phone interview with an actual victim with the interviewer supervisor on the phone as well. When additional interviewers were hired later, the original interviewers helped train them. Periodic meetings were held in which problems with the interview schedules, with contacting victims and with getting victims' cooperation were discussed. Some revisions to the interview schedules grew out of the early meetings. The importance of getting completed interviews was stressed throughout the project. The interviewers learned to persevere even under unfavorable conditions such as when the suspect was present during a face-to-face interview with the victim.

Interviewers were paid only for completed interviews. Initially, interviewers were paid \$5.00 per interview for telephone interviews and \$7.50 plus mileage costs for face-to-face interviews. At the end of September, 1987, the contracts with the interviewers were renegotiated and the rates set at \$7.50 per telephone interview and \$20.00 per face-to-face interview. Under the new contracts, the interviewers were not reimbursed for travel costs (mileage) on face-to-face interviews. Victims were never paid for interviews in this project.

All of the interviewers that worked on the project were female. None was hired with specific language abilities in mind. As it

turned out, Spanish speaking interviewers were not needed, as the Hispanic victims spoke English. The language problems that did arise occurred with Asian victims, principally Korean. None of the interviewers spoke Korean or any other Asian language.

Assigning Interviews.

Once the information from the implementation form (the form filled out by the officer in the field) was entered into the computer and the case was determined to meet the project criteria, identifying information was entered into a administrative file. As the data was entered, every fourth case was flagged as a case to receive an initial and final interview only. These same cases were assigned to receive face-to face interviews while the other 75% were assigned to receive telephone interviews. Some victims assigned to receive interviews by telephone were interviewed in person because they did not have telephones or could not be reached by phone, but these were outside the sample deliberately chosen to receive personal interviews. Interviewers were assigned cases for interviews on a systematic basis. The list of interviewers was applied to the list of victims to be interviewed and matched in sequence. However, some interviewers did only face-to-face interviews, some did only telephone interviews, and some did both which complicated the process of assigning interviews. Once a case was assigned to an interviewer, that interviewer did all the interviews on that case unless the interviewer left the project

before all the interviews were completed or an interviewer was not successful in getting the cooperation of a particular victim, in which case a different interviewer was sometimes assigned.

At each stage of the interview process--initial interviews, follow-up interviews, and final interviews--a control sheet was kept for each case which recorded contacts and attempted contacts with victims and information on any leads developed in locating the victim. In addition, a master control sheet was part of each case file which showed what interviews the victim should receive, the date and time of completed interviews, and the current status of the case. A parallel computerized file was actually used for monitoring the interviewing process on a day to day basis.

Conducting the Interviews.

Interviewers were contract workers who worked from their own locations. Telephone interviews were typically conducted from the interviewers' residences, while face-to-face interviews were conducted at the victims' residences or, occasionally, at some other location of a victim's choice. Interviewers came into the office once a week--toward the end of the project they were asked to come in twice a week--to drop off completed interviews and pick up the new list of persons to be interviewed.

Contacting victims could sometimes be difficult. Information gathered by the officer in the field included the home address and phone number (if any) of both the suspect and the victim,

employment information on both, and name, address, and phone number of a contact person. Even so, victims often had no telephone, changed residences, perhaps moved in with friends or relatives, or left the area altogether. Many of the victims did not work outside the home. Telephone and utility listings were likely to be in some one else's name. Inquiries directed to neighbors in the area could be met with some suspicion, especially since the interviewers carried police department identification.

There was considerable discussion during the interview process about the wisdom of identifying the interviewers as representatives of the police department. (The introduction that the telephone interviewers were to read began by saying that they were calling from the Domestic Violence Project of the Colorado Springs Police Department.) In some instances it may have made it less likely that the victim would cooperate while in others the victim apparently welcomed what she perceived as the department's interest in her welfare. Even though the victims were clearly told that the interviews were part of a research project, the fact that the interviewers were connected to the police department may have given the impression that the police were checking on the situation, particularly when the interviews were conducted in person. Interviewers doing follow-up interviews reported that some victims made comments to the effect that they had told the suspect he had to behave because the

interviewer would be back in two weeks. Regardless of instructions, as the project progressed the interviewers avoided identifying themselves as affiliated with the police department to anyone other than the victim herself, and then only if it became necessary.

About sixty to sixty-five percent of the face-to-face interviews were conducted in single family dwellings or duplexes, about thirty to thirty-five percent in apartment complexes, and about five percent at the victims' workplaces or other non-residential locations. Interviewers worked alone and according to their own schedules. Some interviewers did most of their interviews during the daytime; others did most of their interviews in the evening. When the interviewing was done depended on the interviewer's personal schedule and on when the victims could be contacted.

When neither attempts by telephone nor in person were successful in making contact with victims or when contact was made but victims refused to consent to the interviews, letters were sent out asking the victims to contact the project. Letters sent to victims that the project had been unable to contact at all were sent by certified mail. By the final stages of the project the number of victims that the project was still in contact with had dwindled. A strong effort was made to track down victims for whom the project still needed final interviews, including those whose follow-up interviews had not been completed and even a few whose initial interviews had never been done.

Verifications.

Part way through the interviewing process one interviewer was hired specifically to do verifications. With interviewers working on their own it was necessary to have some system of accountability. The verification process consisted of drawing a sample from the interview records and contacting the victims. The victims were asked if they remembered being interviewed and, if so, if they were interviewed by phone or in person. A few questions were then asked that were taken from whichever interview schedule should have been used for that interview. The victim was asked the question and, then, after she answered it, she was asked if she remembered being asked it in the interview. The verification process reduced any temptation for the interviewers to bill the project for interviews they did not actually complete or to bill the project for a face-to-face interview when the it was actually completed over the telephone.

Chapter 9

The Data Set

The data set consists of 114 separate files containing data in fixed length ASCII files. The data come from five sources: implementation forms, counseling sessions, interviews, criminal history and victimization records, and court proceedings. Every case in the project has data from the implementation form and from the check of criminal and victimization records (even if the information is only that no criminal record was found). Cases where the counseling alternative was imposed and the suspect attended counseling have counseling data. In most cases, the victim received an initial and a final interview, so there is data from those interviews. Cases where follow-up interviews were conducted have data on from one to five follow-up interviews. Cases that went to court have court penetration data.

Because of revisions to the data collection instruments it was not always possible to code the data from different versions so that it would fit into a single master file. Thus, where there are multiple versions of the data collection instruments there are usually separate files for each version.

In addition, the cases are categorized according to their status in the project. The 1202 cases in which the officer imposed the

disposition that the dispatcher gave him were placed into a separate file labeled File I. The 315 cases in which the officer had imposed a different disposition from the one the dispatcher gave him were placed in File II. Cases which had more than one response on key variables on the implementation forms were placed in File III. File III was further subdivided into IIIA which consisted of 31 cases where the proper response could be verified, making the case eligible for inclusion in File I or II (usually II), and File IIIB which consisted of two remaining cases with multiple responses. The process of screening out calls involving couples who had already entered the project through a previous case turned out to be imperfect. Cases which involved couples who had re-entered the project within a six month time period (and who, therefore, were given another randomized disposition in error) were placed in File IV. When the data were entered, the cases from each of these four master files were placed in separate computer files. Thus a given computer file contains data for a particular version of a particular data collection instrument taken from cases from one of the four files. Figure I. lists all of the project files by data source, version, and master file.

All records contain the call screen number of the incident and may contain the victim and/or suspect's ID number depending on what information is in a particular file. A unique four digit ID number was assigned to each individual in the project. It was

FIGURE I
PROJECT FILES

MASTER FILE	SOURCE	DISK OR DIRECTORY NAME	FILE NAME	NUMBER OF RECORDS
IMPLEMENTATION FORMS				
I	All versions	4-17-90	ONE.FIX	1202
II	All versions	4-17-90	TWO.FIX	315
III	All versions	4-17-90	THREEA.FIX	31
	All versions	4-17-90	THREEB.FIX	2
IV	All versions	4-17-90	FOUR.FIX*	110

COUNSELING REPORTS				
I	First version	MCOUN2	CNSL3	15
	Second version	MCOUN2	CNSL2	38
	Final version	MCOUN2	CNSL1	198
II	First version	MCOUN2-2**	CNSL3**	2
	Second version	MCOUN2-2	CNSL2	1
	Final version	MCOUN2-2	CNSL1	22
III	First version	MCOUN3-2	CNSL3	1
	Second version	MCOUN3-2	CNSL2	1
	Final version	MCOUN3-2	CNSL1	3
IV	First version	MCOUN4-2	CNSL3	0
	Second version	MCOUN4-2	CNSL2	0
	Final version	MCOUN4-2	CNSL1	21

*All files have the extension .FIX. Due to space considerations the extensions will not be shown on the remaining files.

**Note that different files have identical names but are stored in different disk or sub-directories. The unique name of a given file consists of the disk or sub-directory name together with the file name.

MASTER FILE	SOURCE	DISK OR DIRECTORY NAME	FILE NAME	NUMBER OF RECORDS
INITIAL INTERVIEWS				
I	Version A	INTA1	INL-1	1
		INTA1	INL-2	continued*
		INTA1	INL-3	continued
	Version B	INTB-1	INT1-1	48
		INTB-1	INT2-2	continued
	Version C	INTC1	INT-1	953
		INTC1	INT-2	continued
		INTC1	INT-3	continued
II	Version A	INTA2	INL-1	4
		INTA2	INL-2	continued
		INTA2	INL-3	continued
	Version B	INTB-2	INT1-1	26
		INTB-2	INT2-2	continued
	Version C	INTC2	INT-1	193
		INTC2	INT-2	continued
		INTC2	INT-3	continued
III	Version A	INTA3	INL-1	1
		INTA3	INL-2	continued
		INTA3	INL-3	continued
	Version B	INTB-3	INT1-1	4
		INTB-3	INT2-2	continued
	Version C	INTC3	INT-1	16
		INTC3	INT-2	continued
		INTC3	INT-3	continued
IV	Version A	--	--	--
	Version B	--	--	--
	Version C	INTC4	INT-1	8
		INTC4	INT-2	continued
		INTC4	INT-3	continued

*Where the record was too long to fit into a single file, multiple files were used. These are designated in the records column by the word "continued."

MASTER FILE	SOURCE	DISK OR DIRECTORY NAME	FILE NAME	NUMBER OF RECORDS
FOLLOW-UP INTERVIEWS				
I	Version B (oldest version)	MFOLUP1	FLP	20
	Version A (second version)	MFOLLO1	FUP-1	101
	Version C (final version)	MFUPONE MFUPONE	FUP1-1A FUP1-1B	2992 continued
II	Version B	MFOLUP2	FLP	12
	Version A	MFOLLO2	FUP-1	39
	Version C	MFUPTWO	FUP1-1A FUP2-2A	596 continued
III	Version B	MFOLUP3	FLP	3
	Version A	MFOLLO3	FUP-1	3
	Version C	MFUPTHRE	FUP1-1A FUP1-1B	69 continued
IV	Version B	--	--	--
	Version A	--	--	--
	Version C	MUPFOUR MUPFOUR	FUP1-1A FUP1-1B	10 continued
FINAL INTERVIEWS				
I	First version	--	FINAL1	6
	Final version	-- --	FINAL1-1 FINAL1-2	846 continued
II	First version	--	--	--
	Final version	-- --	FINAL2-1 FINAL2-2	207 continued

MASTER FILE	SOURCE	DISK OR DIRECTORY NAME	FILE NAME	NUMBER OF RECORDS
III	First version	--	--	--
	Final version	--	FINAL3-1	20
		--	FINAL3-2	continued
IV	First version	--	--	--
	Final version	--	FINAL4-1	5
		--	FINAL4-2	continued

CRIMINAL HISTORY AND
VICTIMIZATION CHECKS

I	Initial suspect check: summary	CHJ FILE ONE	SUSP	1202
	Initial victim check: summary		VICT	1202
	Suspect six month recheck: summary		SUS6	1202
	Victim six month recheck: summary		VIC6	1202
	Initial suspect check: charges	CHJ FILE ONE VICT & CHARGES	SUS1	4024
	Initial suspect check: victimization		SUS2	1211
	Initial victim check: charges		VIC1	2339
	Initial victim check: victimization		VIC2	1271
	Suspect six month recheck: charges		SU61	2028
	Suspect six month recheck: victimization		SU61	1233
	Victim six month recheck: charges		VI61	1423
	Victim six month recheck: victimization		VI62	1483

MASTER FILE	SOURCE	DISK OR DIRECTORY NAME	FILE NAME	NUMBER OF RECORDS
II	Initial suspect check: summary	CHJ FILE TWO	SUSP	313
	Initial victim check: summary		VICT	313
	Suspect six month recheck: summary		SUS6	313
	Victim six month recheck: summary		VIC6	313
	Initial suspect check: charges	CHJ FILE TWO VICT & CHARGES	SUS1	1364
	Initial suspect check: victimization		SUS2	316
	Initial victim check: charges		VIC1	527
	Initial victim check: victimization		VIC2	327
	Suspect six month recheck: charges		SU61	643
	Suspect six month recheck: victimization		SU62	318
	Victim six month recheck: charges		VI61	399
	Victim six month recheck: victimization		VI62	388
III	Initial suspect check: summary	CHJ FILE THREE	SUSP	33
	Initial victim check: summary		VICT	33
	Suspect six month recheck: summary		SUS6	33
	Victim six month recheck: summary		VIC6	33

MASTER FILE	SOURCE	DISK OR DIRECTORY NAME	FILE NAME	NUMBER OF RECORDS
	Initial suspect check: charges	CHJ FILE THREE VICT & CHARGES	SUS1	59
	Initial suspect check: victimization		SUS2	33
	Initial victim check: charges		VIC1	40
	Initial victim check: victimization		VIC2	35
	Suspect six month recheck: charges		SU61	51
	Suspect six month recheck: victimization		SU62	32
	Victim six month recheck: charges		VI61	38
	Victim six month recheck: victimization		VI62	37

IV All file four cases have criminal and victimization history checks and rechecks with the original project case.

COURT PENETRATION FORMS				
I	Part A	MCRC1-2	CRCA	246
	Part B	MCRC1-2	CRCB	230
	Part C	MCRC1-2	CRCC	206
II	Part A	MCRC2-2	CRCA	74
	Part B	MCRC2-2	CRCB	66
	Part C	MCRC2-2	CRCC	57
III	Part A	MCRC3-2	CRCA	3
	Part B	MCRC3-2	CRCB	3
	Part C	MCRC3-2	CRCC	2
IV	Part A	MCRC4-2	CRCA	17
	Part B	MCRC4-2	CRCB	17
	Part C	MCRC4-2	CRCC	13

possible for a suspect or victim to appear in the project several times for a number of reasons. An individual could appear as a victim in one case and a suspect in another, as a victim or suspect in several cases with different partners, as a participant in a new case with the same partner after six months had elapsed, or as a participant in a repeat case within six months that failed to be screened out at the randomization stage. To keep track of individuals in the project, a fifth and sixth digit were added to the four digit identification number. The fifth digit indicated how many times the person had appeared in the project as a suspect. The sixth digit indicated how many times the person had appeared as a victim. For example, if a suspect came into the project for the first time and was given an ID number of 1234, his full ID number for that case would be 123410. If he came into the project again, his ID number for the second case would be 123420. Similarly, a victim's number would end with the digit "1" for the first case, the digit "2" for the second case, etc. The ID number in the most recent case involving an individual would show the total number of times the person had appeared in the project as a suspect and as a victim.

Implementation Files.

Implementation files contain the information the officer recorded on the implementation form at the time of the incident. This includes such information on the incident as the date and time and location, nature of the offense, demographic information on

the individuals involved, details of the incident, disposition assigned by the dispatcher, and the disposition imposed by the officer. When the officer imposed a different disposition from the one assigned the reasons were given on the form. The form also included information on the arrest, if there was one, on who left the premises under an EPO, and on what specific things the officer did at the scene.

Counseling Files.

The counseling files contain data on the session or sessions attended by the suspect under the crisis intervention disposition (usually referred to by the shorter name "counseling"). This data came from forms filled out at the end of a session by the therapist conducting the session. The forms recorded what items were covered in the session(s) (see Chapter 5 for a description of the items) and the therapist's assessment of the suspect's potential for committing subsequent violent acts. Forms from the later months of the study also include the blood alcohol level of the suspect.

Interviews.

Of the 1550 cases in the project (excluding the repeat cases in File IV), 1246 (80%) received initial interviews. Final interviews were completed in 1079 cases of the cases in Files I through III (70%). A total of 3835 follow-up interviews were completed. If five follow-up interviews had been done on each

of the cases that fell in the 75% targeted for follow-up interviews, around 5800 follow-up interviews would have been done. The number actually completed is about two-thirds of that. It was unusual for a case to have all five follow-up interviews completed, but a fairly high percentage had one or more follow-up interviews done. A description of the content of the various interview schedules is found in Chapter 6.

Criminal History and Victimization Checks.

A criminal history and victimization check was done for every individual who came into the project. The checks were done through the Identification and Records Section of the CSPD and were limited to local (City of Colorado Springs) records. Police department permanent and temporary employees with the proper clearances conducted the checks. Computerized records were utilized to determine if the person had an adult criminal record (called a "CHJ" for the criminal history jacket that contains the hard copy record) and to determine if the person had been the victim in any prior domestic cases. Initially, information was collected from the computerized and hard copy files on the dates and specific charges on the person's record, whether the same suspect or victim was involved as in the project incident, and the dispositions of past cases. This information was put into the project data files in two ways. The summary data taken from the first part of the criminal history and victimization forms was put into the main part of each file. The files containing the information on the suspects were named SUSP and the files

containing the information on the victims were named VICT. In addition, each individual has two other records. SUS1 contains an actual listing of charges, dates, victims, and dispositions involving that suspect as a suspect. SUS2 contains a listing of charges, dates, suspects, and dispositions involving that suspect as a victim. Similarly, VIC1 contains information on charges involving that victim as a suspect, while VIC2 contains information on charges involving that victim as a victim. The data included in the initial criminal history and victimization checks covered the period up to and including the project incident.

The records were rechecked to gather data on any new charges or victimizations during the six months following the project incident. The data collected paralleled that of the initial files described above. This data was put into files called SUS6, SU61, and SU62 for suspects, and VIC6, VI61, VI62 for victims.

Court Penetration Files.

In order to gather information on the court proceedings involving project cases, the project hired a staff person to work at the court facility, attending hearings and recording information for the project. Three data collection forms were used to gather information on court proceedings. There was an initial report, a 30-day follow-up, and a further follow-up. Data from these forms were entered into files named CRCA, CRCB, and CRCC respectively.

The data in CRCA includes information on the advisement, including information on release conditions if the suspect was released. CRCB contains information on any pre-trial conference held and the outcome of that conference as well as information on any new charges which may have been brought. CRCC contains trial and disposition information.

Chapter 10

Analysis

It is not at all apparent how the outcome variable(s) should be defined. For the official data, one could define a "failure" as a repeat violation involving the same offender and same victim. Or a failure could be defined as a repeat violation involving the same offender and a new victim. And/or the crimes represented in a failure could be broadened to include other person crimes.

The options are even more varied for the interview data from victims. Which kinds of incidents should be included and should they be analyzed separately or in aggregate form? And if in aggregate form, how should the offenses be weighted? For example, does one battery equal five threats? Finally, how should failures across the two data sources be treated? Should there be separate analyses for each or should the failures be aggregated?

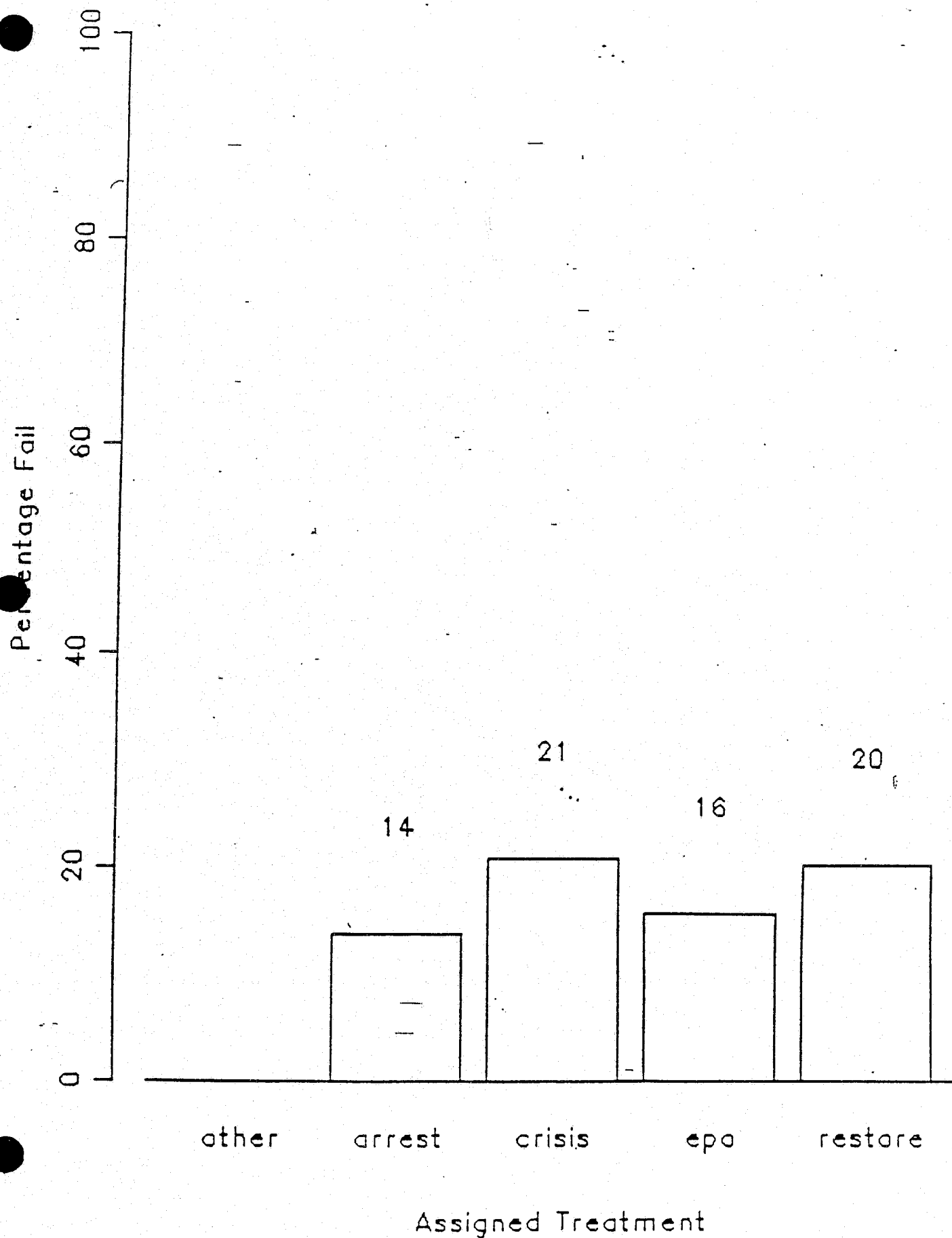
We follow in the footsteps of the Minneapolis analysis in order to enhance comparability.

A "failure" in the official data will be initially defined as a new domestic violation, regardless of who the victim is (coded 1-0)¹, and regardless of the length of the followup period. A "failure" in the victim interview data will be defined as **any**

subsequent incident of completed violence by the original offender, including hitting, slapping, hurting, or "detaining" and related property damage (coded 1-0). We exclude (for now) threats, and attempted assaults, because they are more difficult for the police to verify and, therefore, less likely to appear in the official statistics. We also exclude (for now) attacks and attempted attacks on other family members because they are quite rare and may be totally unrelated to problems between the original victim and offender.

Figure 10.1 shows the percentage of offenders failing according to **victim** reports, broken down by the **assigned** treatments. Clearly, the failure rate is lowest for the group randomly assigned to arrest (11 percent), but the difference between the recidivism for arrest and the recidivism for any other intervention depends on which comparisons are made. Moreover, the role of change needs to be considered.

Victim's Reports of Various Incidents



Before looking at the data, a decision was made to initially focus significance tests on comparisons between arrests and the other three randomly assigned interventions.² This was done by specifying two logistic regressions in which for one, "arrest" was the **only** treatment variable **included** and in which for the other, "arrest" was the **only** treatment variable **excluded**.

As Table 10.1 shows, when the arrest intervention was the only treatment variable included (as a 1-0 binary variable) as an explanatory variable, its regression coefficient was $-.44$, which translates into an odds multiplier of about $.64$. That is, compared to the other three interventions combined, the odds that an offender will commit a new act of domestic violence (as defined above) is cut by a multiplicative factor of about two-thirds. This effect is statistically significant at the $.05$ level (P -value = $.025$, one tailed test, $N=907$).

As Table 10.2 shows, when the arrest intervention was the only treatment variable excluded as an explanatory variable (i.e., the other three treatment variables were included as 1-0 binary variables), the counseling intervention and the restore order intervention were shown to be worse than arrest at the $.05$ level (P -values equal to $.023$ and $.012$ respectively for one tailed tests). The null hypothesis could not be rejected for the contrast between "arrest" and "EPO" alone. In short, the critical

contrast seems to be between the ore successful arrest intervention and the less successful counseling and restore order interventions.

TABLE 10.1

Logistic Regression for Victim's
Reports of Various Subsequent Incidents
(Arrest Only included)

<u>Variable</u>	<u>Coefficient</u>	<u>P-Value</u>
Constant	-1.63	.000
Arrest	-0.44	.025 (one tailed)

TABLE 10.2

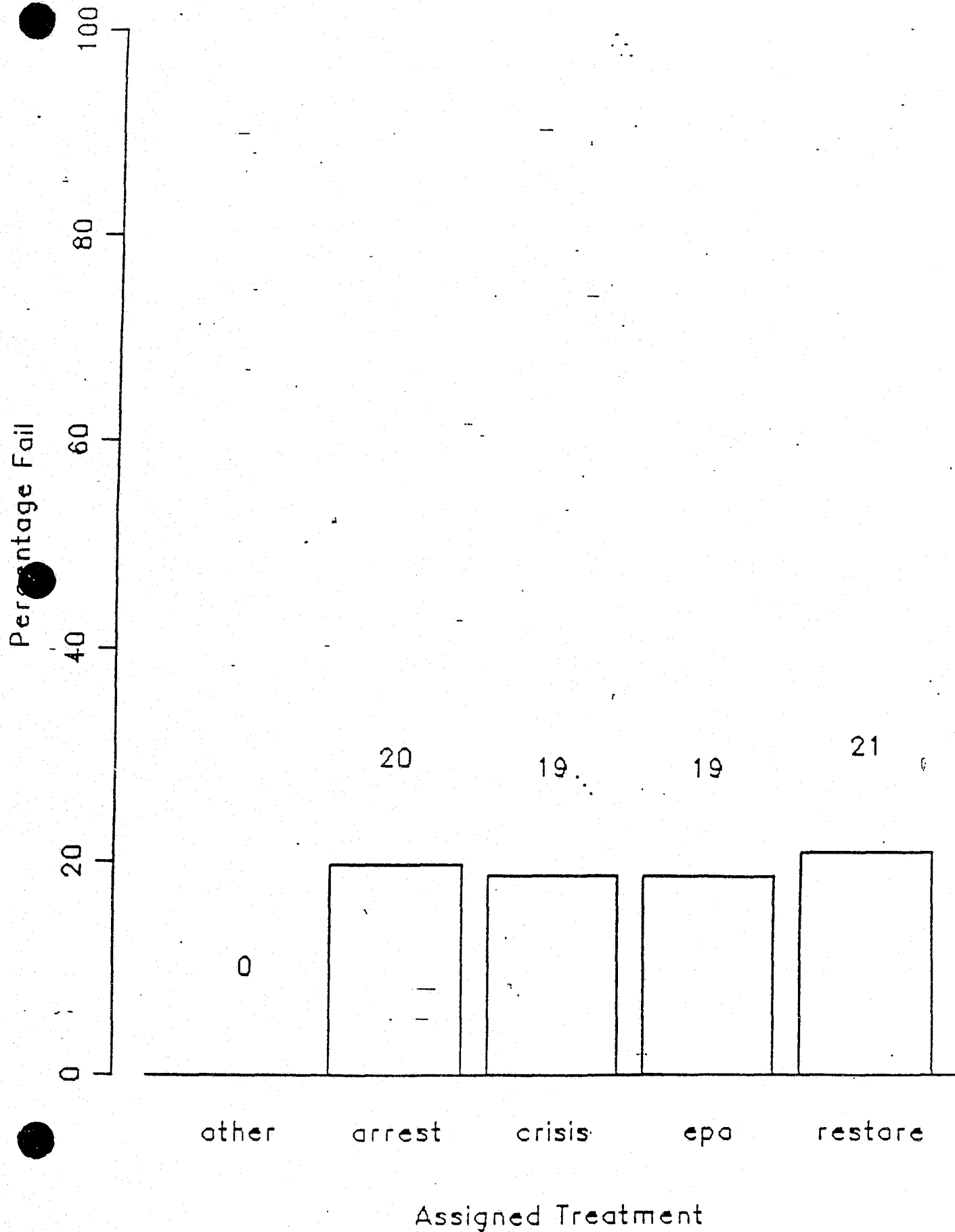
Logistic Regression for Victim's
Reports of Various Subsequent Incidents
(Arrest Only deleted)

<u>Variable</u>	<u>Coefficient</u>	<u>P-Value</u>
Constant	-2.07	.000
Counseling	0.55	.023 (one tailed)
EPO	0.18	.259 (one tailed)
Restore	0.60	.012 (one tailed)

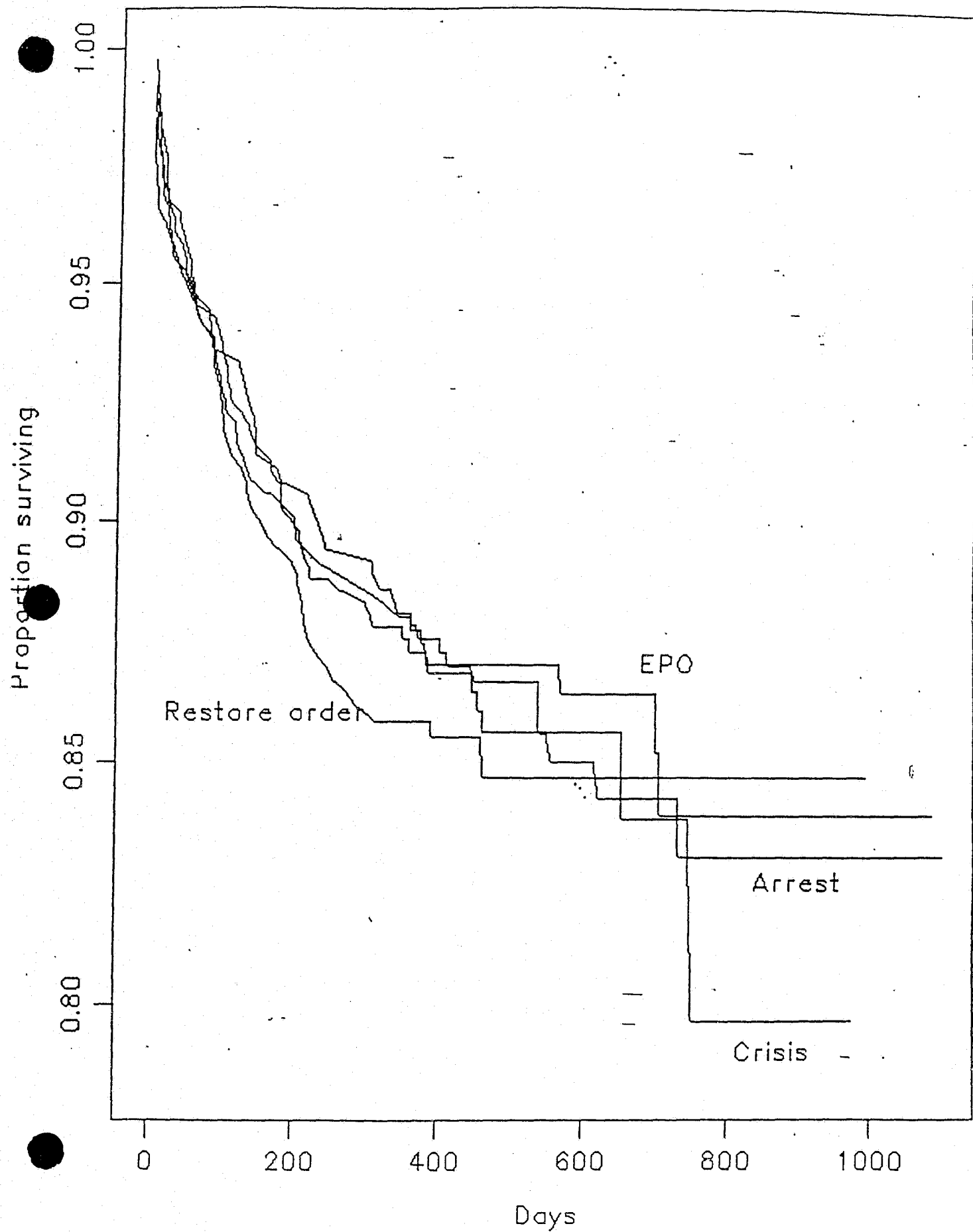
The restore order treatment was the least intrusive and least punitive intervention while the arrest intervention was arguably the most intrusive and punitive. Consequently, the arrest/restore comparison is broadly consistent with some interpretations of the Minneapolis findings. The arrest-counseling comparison is more difficult to explain, in part because counseling included an emergency order of protection. One possibility is that counseling was seen by at least some offenders as a significant dilution of conventional criminal justice sanctions, so that even the potential impact of an emergency order of protection was lost. These and other possibilities will need to be explored further in data analyses to come (which were not requested as part of the Core Analysis).³

Unfortunately, the picture becomes substantially more complicated when the official data are examined. Figure 10.2 shows the failure rates for the randomly assigned treatments, and clearly, not much is going on.⁴ Figure 10.3 shows the survival curves for the four treatments and again (keeping the scale in mind), the differences between the treatment effects are very small. Table 10.3 and 10.4 show logistic regressions, consistent with those estimated for the victim data (N=1658), which confirm that what differences do appear are easily the result of chance (through random assignment). In short, when the official data are used to measure failure, the arrest treatment is no better (but no worse) than the other three treatments.

Police Reports



Survival Curves for All Treatments



Given the very small number of multiple failures among the suspects, it should not be surprising that the same story emerges when the number of failure is considered (same victim or different victim).⁵ The means for the four treatment groups differ little from the simple percentages of failures. Statistical inference, however, may be more problematic because the probability of an initial failure differs dramatically from the probability of a second failure, given a first failure. That is, the failures are not independent. A Poisson distribution, therefore, cannot be assumed in the finite sample case and whether one can rely on asymptotic normality with the sample sizes we have is unclear.

TABLE 10.3

Logistic Regression for Official
Reports of Various Subsequent Incidents
(Arrest Only Included)

<u>Variable</u>	<u>Coefficient</u>	<u>P-Value</u>
Constant	-1.42	.000
Arrest	-0.02	.454 (one tailed)

TABLE 10.4

Logistic Regression for Official
Reports of Various Subsequent Incidents
(Arrest Only deleted)

<u>Variable</u>	<u>Coefficient</u>	<u>P-Value</u>
Constant	-1.41	.000
Counseling	-0.06	.363 (one tailed)
EPO	-0.06	.361 (one tailed)
Restore	0.08	.328 (one tailed)

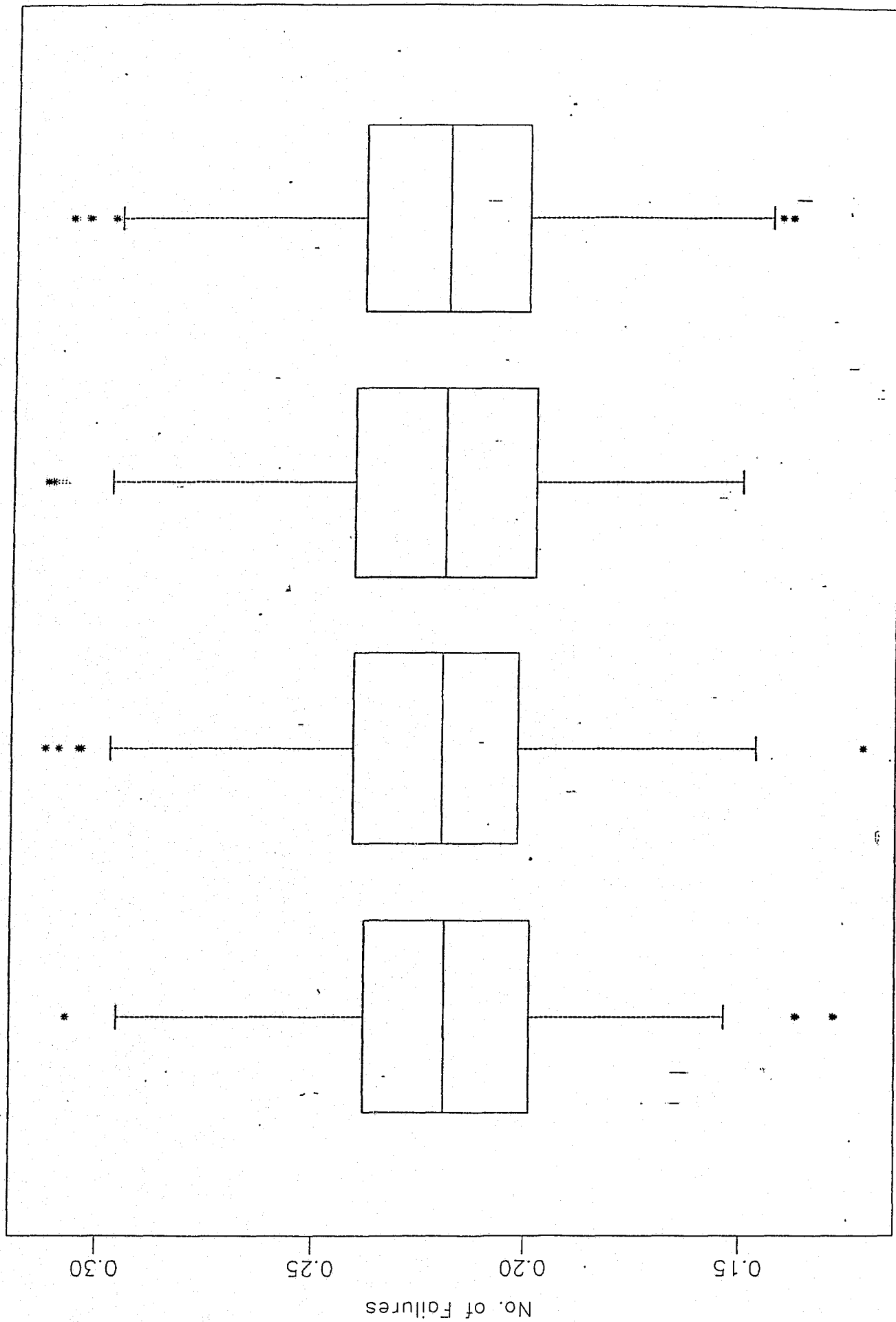
In response to these concerns, randomization tests were done. We are not fully satisfied with this option since it assumes that the number of failures for each suspect is fixed.⁶ This is clearly false and suggests that the amount of stochastic variation is substantially underestimated. Fortunately, it probably does not matter.

Figure 10.4 shows boxplots for the four simulated sampling distributions produced by the randomization test (one for each treatment). Clearly, the sampling distributions substantially overlap. If there were more stochastic variation, it would only serve to make the sampling distributions even less distinguishable from one another. In short, there is no evidence of any treatment effect.

What might account for the different findings across the two data sets? First, the interview respondents are a subset of the full set of victims participating in the study. Whether the subset differs in important ways from the full sample remains to be explored. If so, statistical adjustments will be required before the treatment effects can be properly interpreted (and those are hardly a panacea).

Second, the disparities in findings from the two datasets may reflect coding errors or computation errors that we have made. We have been checking our work regularly, however, and, at this

Sampling Distributions for the Four Treatments



Arrest Crisis EPO Restore

FIGURE 10.4

point, are fairly confident that at least the constructed variables are sound. But more checking needs to be done.

Third, perhaps the differences stem from how the victimization is defined. From the interviews, all one can know is whether the initial victim experienced a second offense from the same offender. The official data allows for new victims to be counted. Exploring this issue would take us far beyond the requested Core Analysis and available resources. But, since the vast majority of failures involved the same victim, we suspect that whether failure includes or excludes incidents with new victims makes no substantive difference. We should stress, however, these are just the sorts of computations for which we have found problems in the data, and much work lies ahead.

Finally, there are troubling disparities between the two failure variables. Table 10.5 cross-classifies victims⁷ by whether there was a new incident reported on the 6-month interview and on the offender rap sheet (**within 6 months** and with the **same** victim, to enhance comparability). Ideally all of the cases should have fallen on the main diagonal. Clearly, this does not happen. Although the association is positive (odds ratio = 2.5), and not easily explained by chance ($P\text{-value} < .05$), nearly 20 percent of the cases are not on the main diagonal. In other words, it is common to find failures reported by victims that do not appear in the official records and failures reported in the official records that do not appear in the victim accounts.

**TABLE 10.5: VICTIM DEFINED FAILURE BY
RAP SHEET DEFINED FAILURE**

RAP SHEET DEFINED

V D I E C F T I I N M E D	No Fail	91.5% (698)	8.5% (65)	85.4% (763)
	Fail	81.5% (106)	18.5% (34)	14.6% (130)
	TOTAL	90.0% (804)	10.0% (89)	100% (893)

Chi-squared (Association) = 11.15, p-value = .00 (Df=3)

Some of the off-diagonal cases represent legitimate differences in the kinds of events captured by the two instruments. For example, an assault reported to us by the victim may not have been reported to the police. However, some of the differences probably stem from measurement difficulties. In particular, sixty-five failures were recorded on the official records, but not reported by victims. That is, about three-quarters of the failures reported in the official data were **not** reported by victims! This is very troubling because one would have expected the incidents in which the police were called would have been the very incidents victims remembered.

Moreover, when the data from the two week followups are compared to the data from the single six month followup, there are discrepancies, **even for single respondents**. That is, what people say week to week does not necessarily correspond to what they say at the end of six months.

These and other problems are no surprise, reflecting long standing concerns of the Colorado Springs effort, expressed a number of times to the PRT. They also reflect well-known themes in literature on human memory and recall, especially as applied to the reconstruction of personal experiences from the past (i.e., "episodic" memory).⁸ Indeed, these concerns were one of the reasons why the multiple followups were permitted to begin with. In any case, an in-depth analysis of potential measurement

artifacts should have the highest priority, and none of our findings can be taken seriously until that work is completed.

Conclusions

It is probably fair to say that the experiment in Colorado Springs was implemented about as well as the experiment in Minneapolis. Given the many obstacles in mounting large scale field experiments, this is a major accomplishment. We have demonstrated once more that large scale fields experiments are feasible, even for social interventions.

At this point, however, no definitive substantive conclusions can be drawn. There is certainly a suggestion that arresting wife battery offenders was the most effective intervention implemented, and a stronger case that an arrest was at least no worse than any of the other interventions. But, for the reasons addressed above, any firm conclusions about what works best are premature.

NOTES

1 A preliminary analysis of the official data suggests that incidents involving the same offender but a new victim are extremely rare.

2 The question of which comparisons to make was briefly discussed in a meeting of all of the sites with the PRT, but no clear recommendations were forthcoming.

3 We have also done preliminary analysis of the victim data using the imposed, rather than the assigned treatments as explanatory variables. The arrest intervention still performs best, but comparisons are significantly complicated by two non-randomly assigned treatments: upgrades to arrest and "other." Proper analysis of these data are well beyond the scope of the Core Analysis and will not be considered here. Suffice it to say, that in broad terms, the story looks to be much the same. Of course, with so few misassignments, this should not be surprising.

4 The "other" can be ignored because of its sample size.

5 We arrive at slightly different counts starting from scratch using the Rap Sheets than the counts reported in data provided by the CSPD. We are still trying to reconcile these differences. But, we do not believe that these problems make any substantive difference.

6 In reaching this conclusion we were aided enormously by conversations with Robert Weiss, William Mason, and David Freedman.

7 More precisely, the table includes victims for whom we have a completed 6-month followup (final) from their first entry into the study. The total number of 6-month followup interviews is 907, including the 14 repeats.

8 See, for example, E. Tulving's Elements of Episodic Memory, Oxford University Press, 1983.