PREVENTING REPEAT INCIDENTS OF FAMILY VIOLENCE: A RANDOMIZED FIELD TEST OF A SECOND RESPONDER PROGRAM IN REDLANDS, CA

Robert C. Davis
David Weisburd
Edwin E. Hamilton

August 20, 2007

ACKNOWLEDGEMENTS

The Redlands Police Department made this research possible. Their cooperation throughout the project ensured the integrity of the research design and the intervention being evaluated. Often, in our experience, research using true experimental designs runs into myriad implementation problems, from overrides of experimental assignments to slower-than-expected intake rates. None of these occurred in the Redlands study, primarily due to the commitment of RPD staff to evidence-based policing and the goals of this work.

ABSTRACT

Recent evaluations of "second response" programs for domestic violence victims have cast doubt on their effectiveness. These programs are designed to educate and empower victims who have reported incidents of domestic abuse to the police. The program model involves a social worker or specially trained domestic violence police officer going to the homes of victims who have reported domestic abuse some time after the initial patrol response to the call for service. The second responder talks with victims about the nature of domestic violence, helps them develop a safety plan, and informs them about help available for counseling needs, relocation, civil legal assistance, restraining orders, and other social services.

This field test, conducted with the cooperation of the Redlands, CA, Police Department, sought to vary one of the parameters thought to affect the impact of second response programs. Victims who called the Redlands police with a domestic abuse complaint were randomly assigned (a) to receive a second response within 24 hours, (b) to receive a second response within seven days, or (c) to receive no second response. A check of police records and surveys with victims six months after the initial complaint was called did not indicate any reduction in new abuse resulting from any second response condition. The current findings, coupled with earlier research results, strongly suggest that second response programs are at best ineffective in reducing the potential for new abuse and at worst may increase the likelihood of new abusive incidents. Implications for criminal justice policy are discussed.

INTRODUCTION

It is no longer assumed that the initial patrol response to domestic incidents—especially those incidents where no arrest is made—is sufficient in and of itself to protect victims from recurrence of abuse. Domestic violence experts have come to realize that effective solutions to domestic violence must involve efforts to educate victims about their options and connect them with counseling, relocation, civil legal assistance, and other services that can lessen dependence on the abuser. In recent years, a number of programs have been developed in which social workers ("second responders") visit homes in which domestic incidents were recently reported to the police in order to help them find long-term solutions to recurring abuse (Dean, Lumb, Proctor, Klopovic, Hyatt, & Hamby, 2000; Mickish, 2002). While these programs rapidly gained in popularity in the United States, the evidence regarding their effectiveness is mixed. Although some research has indicated that second responder programs can prevent repeat victimization, the most rigorous studies have suggested that these programs may actually increase the odds of abuse recurring. In this context, the importance of rigorous, theoretically based, empirical study is especially important (McCord, 2003).

The Redlands, California, field trial assessed one parameter of second response programs that might account for the variation in research findings. Based on a supposition that victims may be especially receptive to crime prevention opportunities immediately following victimization (Davis & Smith, 1994; Anderson, Chenery, & Pease; 1995), the Redlands study set out to test the question of whether more efficacious outcomes would be gained the closer that a second response occurs to the actual domestic violence event. Accordingly, the field test included three levels of timing of a second

responder intervention: immediate, delayed, or none. The study employed a randomized experimental design. Such designs, when properly designed and implemented are generally agreed to provide the highest level of confidence in drawing policy conclusions (Boruch, Victor, & Cecil, 2000; Campbell & Boruch, 1975; Cook & Campbell, 1979; Farrington, 1983; Feder & Boruch, 2000; Shadish, Cook, & Campbell, 2002; Weisburd, 2003).

From the study, we hoped to identify whether there are versions of this intervention that are likely to reduce continuing abuse and whether there are versions that have no effect or actually increase abuse. We hoped that the results would affect how criminal justice planners, victim service providers, and law enforcement agencies design and implement these programs.

Literature Review

One of the most promising areas of research in modern criminology is work on repeat victimization. For the past twenty-five years, victimization surveys have noted that a small percentage of the population experiences a relatively large proportion of all crime, and that one of the strongest predictors of victimization that researchers have isolated is being a victim on an earlier occasion (Outlaw & Ruback, 2002; Sorenson, Siegel, Golding, & Stein, 1991; Hindelang, Gottfredson, & Garafolo, 1978). Reports by the Canada Solicitor General (1988), the National Board for Crime Prevention (1994), and others have shown that sexual assault survivors stand as much as a thirty-five times greater chance of revictimization than non-victims (Messman-Moore & Long, 2000; Gold, Sinclair, & Balge, 1999; Muehlenhard, Highby, & Lee, 1998; Collins, 1998),

robbery victims a nine times greater chance, and residential burglary victims a four times greater risk (Palmer, Holmes, & Hollin, 2002; Budd, 1999; Bowers, Hirschfield, & Johnson, 1998; Robinson, 1998).

The risk of revictimization is greatest in the period soon after the previous victimization for crimes as diverse as school crime, residential burglary, bias crime, domestic violence, auto crimes, neighbor disputes, and retail crimes (Farrell, Sousa, & Weisel, 2002; Farrell & Pease, 1993). In domestic violence cases, for example, the risk of revictimization is highest within the first eleven days and declines thereafter (Lloyd, Farrell, and Pease, 1994). These studies support the notion of event dependency in repeat victimization; that is, there is something about being victimized that increases the risk of another victimization. For example, burglars may note additional items worth coming back for upon visiting a house for the first time (Clarke, Perkins, and Smith, 2001).

Despite the fact that U.S. police have developed “hot spots” models for responding to crime problems (Sherman & Weisburd, 1995; Weisburd & Braga, 2003), the British have capitalized more directly on the practical implications of repeat victimization. British criminologists and law enforcement administrators have realized that if being victimized once is a good predictor of who will be victimized in the future, then it makes sense to concentrate crime prevention efforts on persons who report victimization to the authorities (Farrell & Pease, 1993). This is seen as an efficient use of police resources: "By pointing to the most probable times and places of future offenses, repeat victimization also helps identify the times and places where offenders may be found and apprehended. There is potential for the development of a symbiotic relationship between crime prevention and offender detection...." (National Board for Crime

Prevention, 1994, page 2). The British also recognize that any program incorporating a problem-solving approach to policing should pay special attention to repeat victims, who contribute disproportionately to an area's crime statistics, especially in high-crime areas (Farrell & Sousa, 2001; Trickett, Osborne, Seymour, & Pease, 1992). Indeed, a recent study in England found that all police forces surveyed had a repeat victimization strategy (Farrell, Edmunds, & Hobbs, 2000).

This approach is reinforced by the fact that people are likely to be especially receptive to crime prevention opportunities immediately following victimization. There is a "window of opportunity" during the first weeks after a crime during which victims feel vulnerable and are willing to seriously consider behavioral and lifestyle changes (Davis & Smith, 1994; Anderson, Chenery, & Pease, 1995). There is good reason to believe that crime prevention and victim support would be most successful for victims promptly after their victimization.

In their work on repeat victimization, the British have pursued a model of interaction between research and practice (Laycock, 2001; Anderson, et al., 1995; Farrell & Pease, 1993; Farrell, 1995). In this country, researchers and public officials have also begun to recognize the potential benefits of working with repeat victims, as evidenced by the increase in the number of programs initiated around the country, as well as a recent National Research Council conference on crime prevention that featured a panel on repeat victimization and the Justice Department’s Office for Victims of Crime inclusion of repeat victimization in its national evaluation plan.

Research by Davis & Smith (1994) found that crime prevention programs directed at victims were successful in increasing precautionary behaviors, and suggested that such

programs could result in less re-victimization. The New York field test consisted of 191 recent victims of robbery, burglary, and non-sexual assault who were divided into two groups using a quasi-experimental design. One group received traditional crisis counseling while the other received instruction in crime prevention and was offered free upgrades of home security hardware. Relative to the crisis counseling group, victims assigned to the crime prevention training were significantly more likely to believe that the crime could have been avoided, had significantly greater knowledge of crime prevention principles, and were significantly more likely to engage in precautionary behaviors. Victims who had experienced the crime prevention training had a 33 percent lower rate of re-victimization than controls over the next twelve months. However, the sample was small and the difference only attained marginal statistical significance.

Intervening to Prevent Repeat Domestic Violence

The earliest program that worked with victims to prevent repeat incidents of domestic violence was begun in New York City in the mid-1980s. The New York Housing Police Department and Victim Services (now Safe Horizon) began the Domestic Violence Intervention Education Project (DVIEP) as a response to family violence hot spots in New York City. DVIEP crisis response teams, each consisting of a police officer and a social worker, were dispatched to follow up on the initial police response to domestic complaints.

The teams provided victims with information on services and legal options and warned perpetrators (when they were present) of legal consequences of continued abuse. The intent was to empower victims and to increase the social and personal costs to perpetrators of abusive behavior. Contrary to common myth, careers of batterers are

often short or very sporadic (Feld & Straus, 1989; Maxwell, Garner, & Fagan, 2001; Langan & Innes, 1986; Quigley & Leonard, 1996). For those batterers who do not desist or reduce their abusive behavior over a period of time, Fagan (1989) argues that social or legal sanctions and victim actions that raise the personal or social costs to the batterer may promote a reduction or cessation in abuse. The DVIEP program was expected to promote desistance by empowering women to leave the relationship, demand change under threat of leaving, or inflict shame on the abuser. Moreover, the mere physical presence of a police officer was expected to directly stigmatize those abusers who were present at the time of the home visit.

The second responder model pioneered in New York is an early example of now-popular coordinated approaches to domestic violence that advocates argue hold the best hope of reducing recidivism in households experiencing domestic violence (Hart, 1992). Elements of the approach developed in New York were widely replicated in both England and the U.S. For example, one English program offered victims wearable alarms linked to the police, access to counseling by victim caseworkers, and community meetings designed to raise awareness of domestic violence and the police role (Lloyd, et al., 1994). In the U.S., a program in DuPage County, Illinois, combined a tough law enforcement approach to domestic violence with advocacy for the victims of violence. The advocates offered support, gave women information about the legal system, and informed them about further counseling and advocacy services that were available (Weisz, Tolman, & Bennett, 1995).

The availability of VAWA funds and the stipulation that jurisdictions develop a coordinated response to domestic violence have encouraged the promotion of the New

York model in the U.S. Given these initiatives, it is surprising to note that, until the 1990s, little research had been conducted to examine the impact of such programs on subsequent victimization or willingness to report. The limited and preliminary research conducted on the British multidisciplinary programs to reduce repeat domestic violence suggested that they are effective in reducing repeat calls to the police (Kelly, 1999; Hanmer, Griffiths, & Jerwood, 1999), although designs have been weak.

Some research in the U.S. also found support for a beneficial effect of second responder programs on repeat domestic abuse. A study in Portland, OR, assessed the effect of enhanced evidence collection in domestic incidents and attempts to empower victims by providing a follow-up to the patrol response (Jolin, Feyerherm, Fountain, and Friedman, 1999). The researchers randomly assigned domestic violence incidents in which an arrest had been made to one of two conditions. In the experimental condition, the police were instructed via a checklist to make extra efforts at collecting evidence from the crime scene, and domestic violence officers making a second response to the scene of the incident gave victims information about the criminal justice process, gave them safety planning information, and referred them to social services. The control cases did not get the enhanced evidence collection or the second response. The researchers found statistically significant (although not large) differences in case filings, convictions, and sentencing in favor of the experimental cases. Moreover, in experimental cases, victims were more likely to call the police again, yet less likely to report new abuse in an interview with research staff six months after the initial incident. The pattern of results suggested that victims who received the intervention experienced less new abuse than victims in the control condition, but had greater confidence in the police and hence were

more likely to call them when future problems arose. While the methodology of the study was rigorous, it unfortunately confounded second response intervention with enhanced collection of evidence at the scene of the domestic incident.

The Police Foundation conducted an evaluation of a second responder program in Richmond, Virginia, (Greenspan, Weisburd, Lane, Ready, and Crossen-Powell, 2003). In the Richmond program, second responders were summoned to the scene as soon as police considered it safe for them to intervene and provided links to emergency food and shelter assistance; advised victims about legal remedies; and helped them develop safety plans. The study did not include an analysis of rearrest data, but victims who received the intervention reported significantly less victimization (threats or physical harm) relative to controls when surveyed six months later. The Richmond study used a quasi-experimental design in which cases receiving second responder services in two Richmond precincts were compared with domestic violence misdemeanors in two other precincts not participating in the second responder program. However, the design was weakened substantially by the fact that officers in the targeted precincts only summoned second responders in a small proportion of cases, making it unclear whether the cases selected for the intervention were truly comparable to control cases.

The New York Experiments

Two randomized field trials of second responder programs were conducted in New York City public housing projects. Both tested the same intervention model: persons who reported family violence to the police were randomly assigned to receive or not to receive a follow-up visit from a domestic violence police officer and a social worker. This follow-up visit was not immediate, as is the case with most second responder programs, but occurred an average of two weeks later. Both field tests included a second experimental treatment. Public housing units included in the studies were randomly assigned to receive or not receive education about domestic violence through brochures, posters, and public meetings.

The sampling frame for the first experiment (hereafter referred to as the "DVIEP study") was households in designated public housing units in Manhattan where someone had called the police in response to a family violence incident (this could be violence between romantic intimates, sibling violence, elder abuse, or other forms of violence between persons related or living under the same roof). The incidents were minor in nature (only 7 percent of the incidents resulted in arrests and just 14 percent of victims reported any form of injury). Four hundred and thirty-five victims were randomly assigned to receive a home visit as a follow-up to the patrol response. The control group received only the initial police patrol response. Additional calls for police services were tracked for both groups over the next six months.

At the end of the tracking period, researchers interviewed victims to ask about new abuse, about satisfaction with the police response, and about victims' knowledge and use of social services. Interviews were completed with 72 percent of the sample. This

unusually high success rate for a domestic violence criminal justice sample was due, in part, to the low level of transience among New Yorkers living in public housing.

According to law enforcement records, households that received either the home visit intervention or public education about domestic violence were more likely to call the police during the subsequent six months than households that did not receive the interventions. Yet, according to victim survey data, there were no differences between the two groups in abuse during the six months following the trigger incident.

In the literature on the effectiveness of arrest on curbing violence, victim reports and calls to the police usually are both treated as imperfect indicators measuring an underlying construct of actual violence. However, the two measures clearly are not synonymous. Many victimizations—and especially many family violence victimizations—are not reported to the police (Straus & Gelles, 1990; Harris & Associates, 1979; Dutton, 1995). In a 1997 Criminology paper, Davis and Taylor (1997) interpreted this pattern of results to mean that the experimental interventions did not affect actual violence levels but did increase victims' confidence in the police and made victims more willing to report violence when it occurred. Indeed, that explanation is consistent with theory on which DVIEP was based; program administrators had hoped that victims who received the intervention would call the police more often because they would gain confidence that the police would help.

A second experimental investigation (Davis & Medina, 2001) of the same interventions was conducted several years later, this time using a sample of 402 public housing residents who had reported elder abuse incidents to the police. Like the cases in the first field test, incidents in this study (hereafter referred to as the “elder abuse” study)

were also relatively minor (5 percent of the abusers were arrested, just 4 percent of victims reported any injuries, and in only 22 percent of the cases was a crime alleged to have occurred).

Once again, law enforcement records for these households were tracked for the next six months. As in the first experiment, the investigators were successful in interviewing more than 70 percent of victims in the sample at the end of the six month tracking period. Also, as in the first experiment, it was found that victims who received the home visit intervention called the police sooner and more often than controls. Survey results showed that victims who received both home visits and public education were significantly more likely to report new abuse relative to those who received neither home visits nor public education.

The two New York studies were consistent in the finding that households that were assigned to receive a home visit called the police more frequently over the next six months than households that were assigned to a control condition. In the first study, where we did not observe differences in abuse reported on victim surveys, the results seemed to indicate that the interventions did not affect actual abuse but encouraged victims to call the police when abuse occurred. However, in the second (elder abuse) study, where not only was more abuse reported to the police but more abuse was reported on victim surveys by those who had received both interventions, the finding is more troubling. Since victim surveys are widely accepted as an indication of true incident rates, the results suggest that the interventions actually may have increased abuse, not just the reporting of abuse. (The findings from the studies are summarized in Davis, Maxwell, and Taylor, 2006.)

Going into these studies, it had been assumed that the effects of the interventions would be to empower victims through information about their situation, available services, and legal options. The program logic model posited that new abuse would decline as victims extracted themselves from self-defeating relationships or worked with social services and criminal justice staff to develop strategies to end the abuse while staying in the relationship. However, researchers in the New York studies found no evidence that those who received the interventions were more likely to avail themselves of social or legal services, so the intervention could not have worked—at least not in the way intended.

McCord (2003) cautioned criminal justice practitioners that even well-intended, well-planned, and adequately executed programs provide no guarantee for efficacy and can in fact cause harm. There is some precedent for iatrogenic outcomes resulting from attempts to intervene with victims of intimate partner violence. For example, Ford (1991) reported results from an experiment that batterers who were prosecuted to conviction were significantly angrier than men whose cases were diverted or dropped. Harrell (1991) reported that a larger proportion of men assigned to batterer intervention programs committed new abuse compared to men assigned to a control group. Finally, Sherman (1992) reports that among unemployed spouse abusers living in underclass areas, arrest may increase the annual frequency of reported violence (see McCord, 2003, for other prevention programs found harmful).

Need for the Research

Since the 1980s, the second responder model has become a common enhancement to the police patrol response to domestic violence, often funded by federal dollars. The intervention has usually been justified on the basis of reducing repeat incidents of violence. But the New York experiments are disturbing because they suggest that the intervention may be having the opposite effect.

The New York field tests were more rigorous than earlier studies that found beneficial effects of second responder programs, and it is often the case when a new intervention is introduced that initial positive evaluation findings are replaced by unfavorable findings as evaluation designs become stronger (Davis and Taylor, 1999). However, it also is possible that the New York and Richmond studies came to different conclusions because the interventions examined in the studies differed in key respects. The New York experiments examined a program in which the second response typically occurred a number of days after the initial patrol response, while the second responders studied in the Richmond and Portland evaluations typically showed up while the responding officers were still on the scene.

The Redlands second responder field test was designed to determine whether the timing of the intervention is a key determinant of its effects. In this study, we compared conditions in which the second response occurred within twenty-four hours, within seven days, or not at all. From these trials, we hoped to obtain reliable results about second responder program models that lessen the odds of continuing abuse and those that may enhance the odds.

METHOD

Design Overview

With the cooperation of the Redlands, CA, Police Department, we conducted a randomized experiment in which: (a) second responders were dispatched to the crime scene within 24 hours (n=75); (b) second responders visited victims' homes one week after the call for service (n=77); or (c) no second response occurred (n=148). This design yielded a statistical power level greater than .80 for a moderate to small difference between the treatment and control groups (Cohen, 1988).[1]

The Study Site: Redlands, CA

The study was conducted in Redlands, California. Redlands is nestled at the foot of the mountains and edge of the desert in the East Valley region of southwest San Bernardino County. A community of just over 70,000 and growing, Redlands was built on the citrus industry, and its rich heritage is evident in the historic buildings and locations throughout the city. As the economy evolved away from agriculture, Redlands has struck a balance between being a bedroom community to the Los Angeles region and a strong local business environment, most notably as the home of ESRI, the world's leading geographic information software producer. Culture, business, and government in

[1] Statistical power for this experiment will vary depending upon the analysis strategy we use. Assuming a .05 significance level and a two tailed test, if we combine the treatment groups and compare treatment overall to absence of treatment, we would expect a statistical power level of greater than .80 even if the standardized effect size is close to .30 (generally considered a close to small effect, see Cohen, 1988). In the case of examination of each of the two experimental categories with the control categories separately, an effect size of greater than .36 would be necessary to reach this same threshold of statistical power. This is still less than the standard of .50 that Cohen (1988) defines as a moderate effect size.

Redlands are supported by a strong tradition of education, both through the public schools and the University of Redlands.

The Redlands Police Department (RPD), a force of 90 sworn officers, is known for its progressive and innovative approach to policing. For the past decade, the department has pursued a crime prevention strategy based on both risk and protective factors. The RPD seeks to build and support a strong community through controlling crime before it occurs and pursuing opportunities to engage youth in activities that reduce the risk of delinquency. The RPD is committed to innovative use of technology and analysis and a data-driven policing model. Redlands hosted the Community Mapping, Planning, and Analysis for Safety Strategies (COMPASS) initiative that brought regional crime mapping to the East Valley. Working closely with ESRI and the University of Redlands, the police department has actively supported efforts and research that support evidence-based policing. Parallel Justice initiatives are actively pursued by RPD, and have resulted in the realignment of police resources in support of victims and survivors of crime in the community. RPD has shown special leadership in dealing with offenders re-entering the community. Participating in a state pilot effort of GPS monitoring of parolees, RPD conducted analysis that led to the first arrest of a sex offender using real-time field GPS data in California. RPD also coordinates an effort with local churches to provide mentors and other assistance for offenders coming out of prison.

Case Assignment

Beginning January 1, 2005, and continuing through December 3, 2005, incidents reported to the Redlands Police Department were reviewed each morning by a research

assistant to determine whether the incidents involved intimate partners. Cases were determined to be eligible if the incident was coded as a misdemeanor or felony battery of a spouse or intimate partner. The research assistant also developed a computer program to read through the free-text case descriptions and flag additional cases that contained key words such as “domestic” or “abuse”. When cases were identified by the computer software, the research assistant read over the case to determine if it did, in fact, involve intimate partner violence.

Once a case was identified, its Redlands police domestic report (DR) number was entered on the next available line in a sequential project log along with the victim’s name. Each line of the log contained a pre-determined treatment group assignment, created using a computer random number generator. The logs were checked by the principal investigator once a week to determine whether the DR numbers on completed lines in the log were sequential; if the numbers did follow in sequence, this provided assurance that no efforts were being made by members of the Redlands police to circumvent the random assignment process in an effort to ensure that second responses were made in specific cases. This procedure provided a good check on the integrity of the random assignment process. Periodic visits were also made by project staff to supervise the process and discuss any problems with the research assistant and Redlands police staff.

Prior to the start of case intake, procedures were worked out with the Redlands police to handle overrides of the experimental assignments. Cases assigned to the control condition where police staff felt there was high potential for serious continued violence, and therefore a home visit was essential, were presented to a lieutenant for consideration. If the lieutenant concurred that there was serious and immediate danger to the victim, the

experimental assignment was overridden and an immediate home visit conducted. In the final sample of 308 cases, there were 75 cases assigned to the 24-hour second response condition, 77 cases assigned to the seven-day second response condition, 148 cases assigned to the control condition, and eight cases that had been assigned to the control conditions but reassigned to receive an immediate second response as the result of a supervisor's override.

Treatment

For designated incidents, a team of officers, including a trained female domestic violence detective, visited households within either twenty-four hours or seven days of a domestic complaint. The visits generally typically lasted 30-45 minutes, depending on the victim's receptiveness to assistance. The goals of home visits were to ensure that the victim had information about and access to resources and services, to answer any questions they had about the complaint or the justice process, and to encourage a sense of trust in the police and the criminal justice system as a whole.

A written protocol guided the officer or officers making home visits. The visits began by the officer talking to the victim about the recent incident and any immediate safety concerns that she had. The officer discussed with the victim the nature of domestic violence and the very real possibility that the incident she experienced would recur if no action was taken. The officer tried to make the victim understand that the police department took the matter seriously and was there to assist her. She also asked the victim a series of questions about her relationship with the abuser, history of abuse, and the presence of children and weapons in the home.

Once preliminaries were taken care of, the second response officer tried to ensure that the victim had information about resources and services; offered practical assistance; worked with the victim to develop a safety plan; and instructed the victim in how to document future abusive or stalking behaviors. Before leaving, the officer provided the victim with a written description of local resources to assist domestic violence victims, including housing relocation, counseling, domestic violence shelters, medical help, civil legal assistance, information about the criminal justice process, aid in applying for an order of relief, and emergency financial assistance.

In cases where the complainant was not home in two tries, literature was left and/or phone contact made with the household. In-person contact was made with the victim in 84 percent of households in the 1\one–day and seven-day conditions. It might be argued that since not everyone assigned to receive a second response in fact received the full strength treatment, the internal validity of the study was reduced. However, we and other evaluators have argued (Davis and Smith, 1994b; Gartin, 1995) that the fact that an intended criminal justice intervention is not always actually delivered does not reflect a weakness of the experiment. The test was of a public policy intervention—a program to make *reasonable efforts* to conduct follow-up home visits within time and budgetary constraints. Only in a perfect world would every household have received the intended follow-up visit. Researching such a system might tell us about whether home visits work in theory, but would not inform us about a public policy which attempts to conduct home visits.

Collecting Rearrest Data from Police Files

Six months after the reporting date of the last incident in the study, Redlands Police crime analysis officers wrote a software program to search their database to determine if any new incidents had been reported. The search returned any cases associated with the same victim in the trigger incident. For any new incidents identified, information was collected on the date, charge, and identity if the perpetrator.

Interview Procedures

Six months following the trigger incident, research staff attempted to interview victims about any new incidents of abuse that might have occurred. These attempts were made by telephone, with each victim receiving at least five attempts spread out over daytime, evening, and weekend hours. In cases where the victim could not be reached by phone, an incentive letter was sent to the victim's home, offering a $50 stipend to call our research offices. We took several steps to protect victims from possible retaliation from the abuser. When making phone calls, no messages were left if no one answered. If someone other than the victim answered and wanted to know why we wanted to speak to the victim, we told them that we were conducting a "women's health study" in cooperation with the City of Redlands. Incentive letters similarly stated that we were interested in the victim's responses for a women's health study. In the handful of cases with male victims, we said we were conducting a "men's health study."

We ran into serious problems reaching victims for telephone interviews. In fact, the final interview rate by phone was just 14 percent, even including sending incentive letters that offered an increased stipend for completing the interview. We were able to interview just twenty-two victims who were taken into the sample during the first seven

months of intake. From this initial group, we found 83 numbers disconnected or incorrect, 33 numbers with no answer after five attempts (spread out during days, nights, and weekends), ten numbers with caller ID block, and nine cases with no number available from police records. The incentive letters offering $50 for victims to call in led only to an additional seven interviews.

As a result of the poor rate of success for the first half of the sample, we decided to try going to the homes of a subsample of victims to see whether this method would lead to better success. We believed it would because we felt that our lack of success by phone was due to victims having their phones disconnected or to victims' use of caller ID to screen out calls from unrecognized numbers. A pilot test of home visits produced twenty-two interviews out of fifty households tried. With this success, NIJ agreed to provide some additional funding to conduct home visits for all victims not yet interviewed. With the additional funds, we made two home visit attempts for each victim not yet interviewed–one during the day and one during evening hours.

The home visits raised our interview success rate dramatically (see Table 1). We achieved a 41 percent interview success rate overall: Without the home visits, the success rate would have been just 19 percent. The most common reason for not obtaining interviews was that victims had moved without a forwarding address. These cases accounted for nearly one-quarter of all cases in the sample.

Table 1: Results of Interview Attempts

Interviewed	41%
-- By phone	13%
-- By incentive letter	6%
-- By home visit	22%
Refused	18%
Moved	24%
No contact	16%
Total	100% (N=300)

Characteristics of those who completed interviews

Table 2 compares characteristics of cases in which interviews were competed with those where they were not completed. There were no statistically significant or even substantial differences between the two groups in terms of prior domestic police reports[2], victim gender, victim age, or perpetrator age. However, there was a significant

2 Following convention in studies of criminal justice interventions for domestic violence cases, we gathered information only on prior domestic incidents. Research has shown that prior domestic incidents are most closely related to the propensity to commit future domestic abuse.

difference in terms of charge; victims who were interviewed were more likely to be involved in non-violent incidents relative to those not interviewed.[3]

Table 2: Characteristics of Cases Where Interviews Were Completed

Charge (% violent)	71%	83%	.03
Prior DV	0.38	0.48	ns
Victim gender (% female)	85%	81%	ns
Victim age	35.0	33.9	ns
Perpetrator age	35.2	35.5	ns

Finally, there were no meaningful differences in the proportion of interview completion according to treatment group. The rate of completion was 41 percent for the one-day response group; 44 percent for the seven-day response group; and 40 percent for the control group.

3 This might mean that recidivism using the victim interview sample might result in slight underestimates of the true rate of new abuse.

RESULTS

Case Characteristics

We had access to information on charge, prior domestic incidents, victim gender, victim age, and perpetrator age to define all cases included in the sample. Table 3 indicates that a large majority of cases involved violent acts. Fifty-three percent of cases involved charges of misdemeanor battery, 23 percent felony spousal assault, 1 percent assault with a weapon, and 1 percent sexual assaults. Twenty-one percent of the cases involved non-violent charges, most commonly vandalism, violation of a restraining order, threats, or harassing phone calls.

Table 3: Complaint Charges

Battery	53%
Spousal assault	23%
Assault with weapon	1%
Sexual assault	1%
Vandalism	5%
Violation of restraining order	5%
Harassing phone calls	4%
Threats	4%
Burglary/larceny	1%
Disable phone line	1%
Other	3%

Three in four perpetrators did not have a history of abuse against the victim with the Redlands Police Department. Sixteen percent had one prior incident on file, 4 percent had two incidents, 3 percent had three incidents, and 1 percent had four or more incidents.

Eighty-two percent of the victims were female, while 18 percent were male. The median age for victims was 33 years and, for perpetrators, 35 years.

We had additional information on characteristics of victims and cases from those victims interviewed. Slightly more than one in three victims (34 percent) said that they were married to the perpetrator and they were living together. Twenty-four percent said that they were living with the perpetrator but not married. Twenty-one percent of victims said that they were romantically involved with the perpetrator but not living with him, and the same proportion said that they were divorced or separated from the perpetrator. Nearly half of victims (46 percent) said that they had been involved with the perpetrator for more than five years and another 39 percent said that the relationship had been ongoing for at least one year. Just 14 percent reported that their relationships had begun within the past year.

According to victims, two in three perpetrators had abused them prior to the sampled incident—a far higher number than reported above for the proportion who had a previous history with the police for abuse against the victim. According to victims, perpetrators had committed an average of two prior acts of abuse against them.

Eight in ten victims had children living in the house. A strikingly high number said that they had completed high school (83 percent) and 45 percent reported taking at least some college courses.

What Happened During Home Visits

According to the officers conducting the second responses, contact was made with the victims at their homes in 84 percent of the cases in which it was attempted. In cases where the home visit attempt was not successful, literature was left with information

about community services. Perpetrators were present in just twenty-one of the home visits, according to officers. That low number was the result of an intentional practice to call ahead to make sure perpetrators were not present when the officers came to the home. But officers were not always able to reach victims by phone ahead of time, so there were some instances in which the perpetrator was there when the officers arrived.

One concern about conducting second response visits was that the visit might trigger anger in the perpetrator. While partners were home during just a handful of visits, nearly half (46 percent) of victims said that their partners were aware that the visit occurred. Of these, approximately one in four (28 percent) reported that their partner had a negative reaction to the visit.

All but three of the victims interviewed said that they talked with the officer about their needs during the home visit. Seventy-eight percent said they and the officer discussed a safety plan during the visit; one in three said that they discussed getting help for their spouse.

All victims contacted were given a packet of information describing local services available for victims. In addition, in 23 cases, officers recorded referral information given in response to victims' expression of interest in a particular resource. The most common of these specific referrals were to counseling programs (14 referrals) or parenting classes (5 referrals). Other types of referrals—including shelters, civil legal assistance, assistance in obtaining a restraining order, and district attorney victim advocates—were given in just one or two cases.

When asked to evaluate the visit, two in three victims said that it was very helpful and another 28 percent felt that the visit was somewhat helpful. Only a small proportion (5 percent) felt that the visit had not been helpful.

Characteristics of cases assigned to treatment versus control groups

Table 4 compares cases assigned to the two intervention groups with those assigned to the control condition. With one exception, there were minimal differences between the treatment groups on all variables. Victims assigned to the one-day response tended to have been in their relationships longer than victims in the seven-day response condition and the control condition. The finding of a single significant difference out of ten tests conducted on pre-treatment characteristics of groups would be expected by chance. We do not believe that this raises concerns about the integrity of the random assignment process.

Table 4: Characteristics of Cases Assigned to Intervention and Control Groups

All cases (N=300)				
Charge (% violent)	75%	74%	83%	ns
Prior DV	0.53	0.34	0.38	ns
Victim gender	81%	86%	81%	ns
Victim age	34.5	33.4	35.4	ns
Perpetrator age	35.4	34.4	36.8	ns
Interviewed cases (N=128)				
Relationship (% married)	37%	32%	34%	ns
Length relationship (5 yrs+)	63%	50%	37%	.02
Prior incidents	8.0	5.1	5.0	ns
Children in home	1.7	2.0	1.5	ns
Victim education (%HS grad)	81%	85%	85%	ns

Recidivism Data from Redlands Police Records

Approximately nine months after the final case was taken into the sample, the Redlands Police Department ran a check of their database to determine whether victims in our sample had been a victim in a new complaint. To eliminate unfounded and non-crime complaints, we examined only those new cases where the RPD assigned a new DR number. For any new cases we found, we coded the date of new incidents, the charge, and whether the perpetrator was the same or a different individual.

Prevalence of new domestic incidents

Table 5 presents the proportion of cases resulting in new incidents according to treatment group. The table shows that the one-day response group generated somewhat more new incidents (32 percent) compared to the seven-day response group (23 percent) or the control group (24 percent). The two second response groups combined yielded a 28 percent rate of new incidents compared to the control group rate of 24 percent. None of these differences approached statistical significance.

We also analyzed separately only those new incidents that were known to involve the same perpetrator as the original incident.[4] Those results, also displayed in Table 5, showed essentially no difference according to treatment groups.

Table 5: Prevalence of New Incidents by Treatment Group

Any subsequent incident reported	32%	23%	28%	24%	.51

4 Ten cases in which the identity of the perpetrator was not known were coded as "same perpetrator" based on the observation that over 90 percent of new incidents where the identity of the perpetrator was known involved the same perpetrator as the original incident.

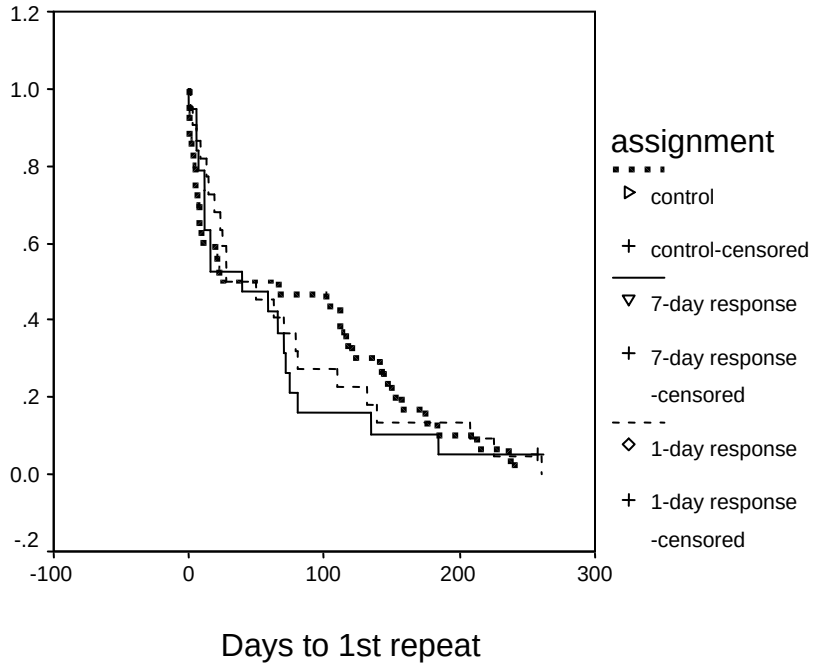
Subsequent incident with same perpetrator	25%	20%	23%	22%	.99

* Significance tests collapse both second response treatments; no significant differences emerged when comparing 1-day and 7-day groups to controls separately, either.

Time to failure

Figure 1 presents survival curves for cases assigned to each of the intervention conditions. The curves represent the proportion of cases in each condition that had not reported a new incident at each time interval across the bottom of the curves. They suggest that the cases assigned to the two second response conditions tended to fail somewhat sooner than control cases.

Figure 1: Survival Functions by Group



The statistics in Table 6 confirm that the cases assigned to the two second response conditions tended to fail somewhat sooner than control cases (mean survival time = 72 days and 59 days for one- and seven-day response groups compared to 79 days for the control group). But the differences were slight and the confidence intervals for the three conditions showed strong overlap. The Breslow statistic, based on a comparison of the number of terminal events with the expected number for each time interval, did not approach statistical significance.

Table 6: Survival Time by Treatment Group

1-day response	71.77	16.42	39.58, 103.96	0.05	.83
7-day response	58.79	15.27	28.86, 88.71	0.07	.80
Control	79.30	14.82	50.25, 108.35		

* Comparison with control cases

Frequency of new incidents

We also examined the frequency of new abusive incidents reported to the police. The mean number of new incidents for the one-day response group was 0.64, compared to 0.42 for the seven-day response group and 0.46 for the control group.

To test for statistical significance of differences between treatment groups in the number of new domestic incidents, we ran a negative binomial model. The model included dummy variables representing the two second response conditions, with the dummy for the control condition omitted. The results, presented in Table 7, are consistent with the findings based on prevalence and time to failure. Cases assigned to the one-day response group had more repeats than cases assigned to the seven-day

response or the control condition but the difference did not rise to the level of statistical significance.

Table 7: Frequency of Abuse by Treatment (Negative Binomial Regression Models)

Constant	-0.78	0.19	-4.01	.00
1-day response	.33	0.32	1.04	.29
7-day response	-0.10	0.33	-0.30	.76

* Coefficients represent deviations from control group
Model chi-square = 129.29, df=1, p = .000

Outcome Measures Based on Victim Interviews

Victims in the second response groups were somewhat more likely to report having seen the abuser since the original incident. Ninety-seven percent of those assigned to the one-day response reported having seen the abuser compared to 91 percent of those assigned to the seven-day response condition and 84 percent of those assigned to the control condition. This difference did not approach statistical significance in a test run combining both second response conditions (p=.14).

Table 8 presents differences between the treatment groups in new abusive incidents. On all of the measures—from physical abuse to threats to controlling abuse to total abuse—victims assigned to the second response conditions were somewhat more likely than those assigned to the control condition to respond affirmatively. The

difference was slight for incidents of physical abuse (9 percent versus 7 percent), but more substantial when all forms of abuse were taken into account (45 percent versus 31 percent). However, none of the differences reached statistical significance.

One factor that may have been related to the likelihood of new abuse in the second response groups was the abuser finding out about the home visit. In 19 percent of the 26 cases where the victim said that the perpetrator was aware of the home visit, victims reported subsequent physical abuse. In contrast, among cases where victims were sure that the abuser was *not* aware of the visit, just 5 percent reported subsequent physical abuse. This may suggest a negative effect of the second response visits, but it may also just mean that victims who were having trouble with their partner were more likely to use the visit as a means of discouraging him from committing new abuse. We observed that among six cases where victims said that the perpetrator was home during the second response, none reported new abuse of any kind.

Table 8: Prevalence of New Abusive Incidents by Treatment Group

Any physical abuse reported	13%	6%	9%	7%	.44
Any threats reported	26%	12%	19%	12%	.22
Any controlling abuse reported	42%	41%	42%	31%	.26
Any abuse of any kind reported	42%	47%	45%	31%	.14

* Significance tests collapse both second response treatments; no significant differences emerged when comparing 1-day and 7-day groups to controls separately, either.

Finally, victims were, overall, quite satisfied with the treatment they received from the Redlands police (see Table 9). Sixty-four percent stated that they were very satisfied, 19 percent were somewhat satisfied, and just 17 percent were dissatisfied. No differences were observed according to treatment condition (p-=.60).

Table 9: Satisfaction with Police Response by Treatment Group

Proportion satisfied with treatment	84%	79%	81%	86%	.60

* Significance tests collapse both second response treatments; no significant differences emerged when comparing 1-day and 7-day groups to controls separately, either.

DISCUSSION

We began this investigation as a result of serious concerns about the efficacy of second response programs. A study by the Police Foundation (Greenspan, et al 2003) and a Portland, OR, study by Jolin, et al (1999) both had suggested that second response programs reduce future abuse as measured by victim reports in surveys. But there were difficulties interpreting the results of both studies. The Police Foundation study was a quasi-experiment, and it was clear that the cases chosen for a second response by the Richmond Police Department were a small fraction of all cases eligible according to the study's criteria; how the cases were assigned was not known or not reported by the researchers. The Jolin study randomly assigned cases to treatments, but confounded second responses with collection of additional evidence by the police that resulted in more case filings, more convictions, and tougher sentences. It is unknown whether it was the second response or the enhanced criminal justice outcomes that were responsible for the observed decrease in subsequent abuse reported by victims.

The New York series of experiments were specifically designed to test the effects of second response programs using true experimental designs, the "gold standard" in research methodologies. A pooled analysis conducted by Davis, et al (2006) reanalyzed data from three separate field experiments, each testing the same intervention on somewhat different populations. The pooled analyses consistently indicated that the interventions were associated with an increase in reporting of new abusive incidents to authorities and to research interviewers.

The Redlands study was designed as a test of the second responder model with a population not from a major metropolitan area. It also varied the timing of the response,

one factor thought to potentially affect the efficacy of the intervention. The study yielded no evidence that the intervention helped reduce the potential for subsequent abuse. Not one of the seven principal outcomes measured (prevalence and frequency of new abuse; time to failure; survey measures of physical abuse, threats, and controlling abuse; and satisfaction with the police response) showed a reduction in abuse in favor of the groups that received the second response.

In fact, the evidence suggests—although certainly not definitively—that the intervention increased abusive incidents. The difference in the prevalence of any abusive incident reported on the survey was substantial (14 percentage points higher for those assigned to the second response conditions than for controls) although not statistically reliable with the available sample size. On all seven principal measures of new abuse, second response cases performed worse than controls.

It would be difficult to argue that the failure of the field test to yield positive results was caused by poor implementation of the program. In a very high proportion of households assigned to receive a second response, face-to-face interviews were conducted with the victim. There were checks on the integrity of the intervention, including completion of a check-list by the officers conducting the second responses to indicate that all key areas were touched upon in their discussion with victims. Members of the research staff also conducted ride-alongs at several points during the study to ensure that the protocol was being followed. Finally, an overwhelming proportion of victims indicated that they found the visits helpful.

We cannot say for certain why the field test did not yield more positive results. We have some indication that intimate partners who found out about the intervention

were more likely to commit new abuse. If so, then any beneficial effects that the intervention might have in educating victims or encouraging them to seek help may have been offset by a hostile response to the intervention by abusers. Both Fagan (1989) and Sherman (1992) warn that criminal sanctions may incite more abuse, especially among the chronic abusers or those with low stakes in conformity. In the same vein, Ford (1991) reports results from an experiment that batterers who were prosecuted to conviction were significantly angrier than men whose cases were diverted or dropped.

The results of this field test when considered in the light of the results of the New York experiments should send up a strong caution signal to those funding and those implementing second response programs. The best available evidence suggests that these programs are at best ineffective and at worst may place victims in greater harm. Since much of the funding for initiating these programs came from the Justice Department, we recommend that DOJ undertake a wider field test in multiple cities. The field tests would test variations of the second response, especially including different ways to deal with abusers —trying to keep contact with the victim confidential, warning the abuser of consequences if additional abuse occurs, or offering the abuser support for dealing with his abuse issues. With the evidence accumulated to-date, we cannot pretend ignorance about the effects of second response programs. A series of field tests designed in the way suggested could point the way forward from the untenable position we are now in where well-intentioned services may place victims at risk.

On a more general note, the experience with second responder programs highlights the importance of ethical considerations when developing programs to intervene in domestic abuse situations or other attempts to improve the criminal justice

system. Any new program has an explicit or implicit logic model that prescribes how the intervention is expected to have beneficial effects. But these logic models are not always right. It is not enough to design something that *should* create positive outcomes: It is also necessary to verify empirically that the logic model is correct and the program really does have the benefits that were intended.

Evaluations have often produced evidence that criminal justice programs do not produce the results intended, or that they even produce results opposite to what planners intended. A recent evaluation of a parole reentry program showed that the program not only failed to reduce recidivism among participants but actually increased the rate at which parolees committed new crimes (Wilson and Davis, 2006). McCord (2003) reviews a number of evaluations of delinquency prevention programs that, in fact, increased delinquent behaviors.

This means that evaluation of new program models is critically important. McCord (2003) argued that the potential adverse effects of social programs ought to be taken as seriously as the side effects of drugs are considered in the pharmaceutical field. This is not an easy prescription. There are good arguments in favor of not evaluating new programs since these programs often have not effectively implemented the intrevention model. In such cases, process evaluation is often more appropriate than impact evaluation. Moreover, when evaluation studies are done, it is typically the case that the findings are positive, often due to weak designs that do not rigorously test the counterfactual, i.e. “How would outcomes be different if the program did not exist?” Often, later, more rigorous research finds that what seemed to be a successful model does not actually produce the social good that was hoped for (Davis and Taylor, 1999).

This argues for an increased commitment to conducting rigorous and early evaluations of new criminal justice programs. Although the obstacles and the costs are great, by failing to gather reliable information early on, we run the risk that programs that are ineffective or worse will be replicated and multiply. Once there is an entrenched constituency for a particular type of program, the chances that changes will be made based on empirical evidence quickly become nil.

This has clearly been the case with Duluth-style batterer intervention programs. During the past two decades, advocates of the Duluth model have dominated the conversation on batterer intervention with the result that other forms of batterer intervention have all but disappeared from the landscape. Although every federally-funded evaluation of batterer intervention programs has supported the null hypothesis (i.e., that these programs have no effect on recidivism), there has been no discernable movement away from this model for batterer programs (Jackson, Feder, Forde, Davis, Maxwell, & Taylor, 2003).

Good intentions are at the core of any new criminal justice reform program. But good intentions are not enough. It is also important that we find out if the assumptions behind new programs translate to positive outcomes. If they do not, then there should be a presumed obligation to modify the program in ways that make it more efficacious. There is an irony at work here: the field of medicine must prove the efficacy of new products or new treatments before they become widely used. Researchers must demonstrate that their studies will not bring unintended negative consequences to people who participate as subjects. Yet, we act as if good intentions are enough when subjecting individuals to new criminal justice interventions.

In a better world, this would be different:.We would have an obligation to design rigorous research that would confirm or deny the assumptions that were inherent in the logic models of new programs. With that research in-hand, we would then make informed decisions about how to best intervene in people's lives in ways that were constructive and avoided any significant possibility of causing harm instead of good. Fortunately, there are an increasing number of police departments that, like the Redlands Police Department, have an interest in subjecting new techniques to rigorous testing and implementing evidence-based solutions.

REFERENCES

Allison, Paul D. (1984). *Event history analysis: Regression for longitudinal event data.* Beverly Hills, CA: Sage Publications.

Anderson, D., Chenery, S. & Pease, K. (1995). *Biting back, tackling repeat burglary and car crime* (Crime Detection & Prevention Series Paper 58). London: Home Office.

Boruch, R. F., Victor, T. & Cecil, J. S. (2000). Resolving ethical and legal problems in Randomized experiments. *Crime and Delinquency,* 46(3), 330-353.

Bowers, K.J., Hirschfield, A., & Johnson, S.D. (1998). Victimization revisited: a case study of non-residential repeat burglary in Merseyside. *British Journal of Criminology*, 38(3), 429-452.

Budd, T. (1999). *Burglary of domestic dwellings: findings from the British Crime Survey*. London, UK: U.K. Home Office

Campbell, D. P. & Boruch, R. F. (1975). Making the case for randomized assignment to treatments by considering the alternatives: Six ways in which quasi-experimental evaluations in compensatory education tend to underestimate effects. pp. 195-296 in C.A. Bennett & A. A. Lumsdaine (Eds.), *Evaluation and Experiment: Some Critical Issues in Assessing Social Programs.* New York: Academic Press.

Canada Solicitor General, (1988). *Multiple victimization, Canadian urban victimization survey bulletin* (No. 10). Ottawa: Ministry of the Solicitor General.

Clarke, R., E. Perkins, and D. Smith, Jr. (2001). Explaining repeat residential burglaries: An analysis of property stolen. In G. Farrell and K. Pease (eds.), *Repeat Victimization. Crime Prevention Studies,* Vol. 12. Monsey, New York: Criminal Justice Press.

Cohen, J. (1988). *Statistical power analysis for the behavioral sciences.* Second Edition. Hillsdale, NJ: Lawrence Erlbaum Associates.

Collins, M.E. (1998). Factors influencing sexual victimization and revictimization in a sample of adolescent mothers. *Journal of Interpersonal Violence,* 13(1), 3-24.

Cook, T. D. & Campbell, D.T. (1979). *Quasi-experimentation: Design and analysis issues for field settings.* Boston, MA: Houghton Mifflin Company.

Davis, R.C. & Medina, J. (2001). Results from an elder abuse prevention experiment

In New York City. *National Institute of Justice Research in Brief*, Washington, DC: U.S. Department of Justice.

Davis, R.C. & Smith, B. (1994). Teaching victims crime prevention skills: Can individuals lower their risk of crime? *Criminal Justice Review*, 19, 56-68.

Davis, R.C. & B.Taylor. (1999) Does batterer treatment reduce violence? A synthesis of the literature. *Women and Criminal Justice, 10*, 69-93.

Davis, R.C. & Taylor, B. (1997). Evaluating a proactive police response to domestic violence: The results of a randomized experiment. *Criminology*, 35, 307-333.

Davis, R.C., Maxwell, C. & Taylor, B. (2006). Preventing repeat incidents of family violence: Analysis of data from three field experiments. *Journal of Experimental Criminology, 2*, 183-210.

Dean, C. W., Lumb, R., Proctor, K., Klopovic, J., Hyatt, A. & Hamby, R. (2000). *Social Work and Police Partnership: A Summons to the Village Strategies and Effective Practices.* (A Report of the Charlotte-Mecklenburg Police Department and the University of North Carolina at Charlotte). Raleigh, NC: North Carolina Governor's Crime Commission.

Dutton, D.G. (1995). *The Domestic Assault of Women: Psychological and Criminal Justice Perspectives*. Boston, MA: Allyn and Bacon.

Fagan, J. A. (1989). Cessation of family violence: Deterrence and dissuasion. In L. Ohlin & M. Tonry (Eds.), *Family Violence* (Vol. 11). Crime and justice: an annual review of research. Chicago: University of Chicago Press.

Farrell, G. (1995). Preventing repeat victimization. In M. Tonry & D.P. Farrington (Eds.), *Building a safer society: Strategic approaches to crime prevention*. Chicago: University of Chicago Press.

Farrell, G., Edmunds, A., & Hobbs, L., et. al. (2000). *RV snapshot: UK policing and repeat victimisation*. London: Home Office.

Farrell, G. & Pease, K. (1993). *Once bitten, twice bitten: Repeat victimization and its implications for crime prevention* (Crime Prevention Unit Paper 46). London: Home Office.

Farrell, G. & Sousa, W. (2001). Repeat victimization and hot spots: the overlap and its implications for crime control and problem-oriented policing. pp. 221-240 in G. Farrell & K. Pease (Eds.), *Repeat Victimization (Crime Prevention Studies, vol. 12).* Monsey, NY: Criminal Justice Press.

Farrell, G., Sousa, W., & Weisel, D. (2002). The time window effect in the measurement of repeat victimization: a methodology for its examination and an empirical study. pp. 15-27 in N. Tilley (ed.), *Analysis for Crime Prevention*. Monsey, NY: Criminal Justice Press.

Farrington, D. P. (1983). Randomized experiments on crime and justice. In M. Tonry & N. Morris (Eds.), *Crime and Justice – An Annual Review of Research.* (pp. 257-308).

Feder, L. & Boruch, R. F. (2000). Need for experiments in criminal justice settings. *Crime and Delinquency, 46(3), 291-294.*

Feld, S. L., & Straus, M. (1989, February). Escalation and desistance of wife assault in marriage. *Criminology, 27*(1), 141-161.

Ford, D. A. (1991). Prosecution as a victim power source: A note on empowering women in violent conjugal relationships. *Law & Society Review, 25,* 313-334.

Gartin, P. R. (1995). Dealing with design failures in randomized field experiments: Analytic issues regarding the evaluation of treatment effects. *Journal of Research in Crime and Delinquency, 32*(4), 425-445.

Gold S.R., Sinclair, B.B., & Balge, K.A. (1999). Risk of sexual revictimization: a theoretical model. *Aggression and Violent Behavior,* 4(4), pp. 457-470.

Greenspan, R., Weisburd, D., Lane, E., Ready, J., Crossen-Powell, S., & Booth, W.C. (2003). *The Richmond/Police Foundation domestic violence partnership.* Washington, DC: Police Foundation.

Hanmer, J., Griffiths, S., & Jerwood, D. (1999*). Arresting evidence: Domestic violence and repeat victimisation* (Police Series Research Paper 104). London: Home Office.

Harrell, A. (1991). *Evaluation of court-ordered treatment for domestic violence offenders* (Final Report Supported by the State Justice Institute (#90-12l-E-089), Under Subcontract from the Institute for Social Analysis). Washington, DC: Urban Institute.

Harris, Louis, and Associates (1979). *A Survey of Spousal Abuse Against Women in Kentucky*. New York: Harris and Associates.

Hart, Barbara (1992). *State codes on domestic violence: Analysis, commentary, and recommendations.* Reno, NV: National Council of Juvenile and Family Court Judges.

Hindelang, M., Gottfredson, M., & Garafolo, J. (1978). *Victims of personal crime: An empirical foundation for a theory of personal victimization*. Cambridge, MA: Ballinger Publishing Company.

Jackson, S., Feder, L., Forde, D.R., Davis, R.C., Maxwell, C.D., & Taylor, B.G. (2003). *Batterer intervention programs: Where do we go from here?* Washington, D.C.: National Institute of Justice.

Jolin, A., Feyerherm, W., Fountain, R., & Friedman, S. (1999). *Beyond arrest: The Portland, Oregon domestic violence experiment final report.* Washington, D.C.: National Institute of Justice.

Kelly, L. (1999). *Domestic violence matters: An evaluation of a development project* (Research Findings No. 91). London: Home Office.

Langan, P. A., & Innes, C. A. (1986). *Preventing Domestic Violence Against Women* (Special Report). Washington D.C.: U.S. Government Printing Office.

Laycock, G. (2001). Hypothesis-based research: the repeat victimization story. *Criminal Justice: The International Journal of Policy and Practice*, 1, 59-82.

Lloyd, S., Farrell, G. & Pease, K. (1994). *Preventing repeated domestic violence: A demonstration project on Merseyside* (Crime Prevention Unit Paper 49). London: Home Office.

Matthews, R., Pease, C., & Pease, K. (2001). Repeated bank robbery: theme and variations. pp. 153-164 in G. Farrell & K. Pease (Eds.), *Repeat Victimization (Crime Prevention Studies, vol. 12).* Monsey, NY: Criminal Justice Press.

Maxwell, C.D., Garner, J.H., & Fagan, J.A. (2001). The effects of arrest on intimate Violence: New evidence from the spouse assault replication program. *National Institute of Justice Research in Brief*, Washington, DC: U.S. Department of Justice.

McCord, J. (2003). Cures that Harm: Unanticipated Outcomes of Crime Prevention Programs. *The Annals of the American Academy of Political and Social Science*, 587 (1).

Messman-Moore, T.L., & Long, P.J. (2000). Child sexual abuse and revictimization in the form of adult sexual abuse, adult physical abuse, and adult psychological maltreatment. *Journal of Interpersonal Violence*, 15(5), 489-502.

Mickish, J. E. (2002). Domestic Violence. pp. 77-118 in B. D. Byers & J. E. Hendricks (Eds.), *Crisis Intervention in Criminal Justice/Social Services*, Third Edition. Springfield, IL: Charles C. Thomas

Muehlenhard, C.L., Highby, B.J., & Lee, R.S., et al. (1998). The sexual revictimization of women and men sexually abused as children: a review of the literature. *Annual Review of Sex Research,* 9, 177-223.

National Board for Crime Prevention (1994). *Wise after the event: Tackling repeat victimization*. London: Home Office.

Orchowsky, S. J. (1999). Evaluation of a Coordinated Community Response to Domestic Violence: The Alexandria Domestic Violence Intervention Project – Final Report. (Applied Research Associates). Washington, D.C.: U.S. Department of Justice.

Outlaw, M & Ruback, B. (2002). Repeat and multiple victimizations: the role of individual and contextual factors. *Violence and Victims, 17*, 187-204.

Palmer, E., Holmes, A., & Hollin, C. (2002). Investigating burglars' decisions: Factors influencing target choice, method of entry, reasons for offending, and repeat victimization. *Security Journal*, *15*, 7-18.

Police Research Group. (1998). *Repeat victimisation: taking stock* (Crime Detection and Prevention Series Paper, #90). London, UK: Home Office.

Polvi, N., Looman, T., Humphries, C. & Pease, K. (1991). The time of repeat burglary victimization. *British Journal of Criminology,* 31(4), 411-414.

Quigley, B. M., & Leonard, K. E. (1996). Desistance of husband aggression in the early years of marriage. *Violence and Victims, 11*(4), 355-370.

Robinson, M.B. (1998). Burglary revictimization: the time period of heightened risk *British Journal of Criminology*, 38(1), pp. 78-87.

Shadish, W. J., Cook, T. D. & Campbell, D. T. (2002). *Experimental and Quasi-Experimental Designs for Generalized Causal Inference.* Boston: Houghton Mifflin Co.

Sherman, L. W. (1992). *Policing domestic violence: Experiments and dilemmas.* New York: Free Press.

Sherman, L., & Weisburd, D. (1995). General deterrent effects of police patrol in crime hot spots: A randomized controlled trial. *Justice Quarterly,* 12, 625-648.

Sorenson, S.B., Siegel, J.M., Golding, J.M., & Stein, J.A. (1991). Repeat sexual victimization. Violence and Victims, 6 (4), 299-301.

Straus, M. & Gelles, R. (1990). *Physical Violence in American Families: Risk Factors and Adaptions to Violence in 8,145 Families.* New Brunswick:

Transaction Publishers.

Straus, M., Hamby, S., Boney-McCoy, S., & Sugarman, D. (1996). The revised conflict Tactics scale (CTS2); Development and preliminary data. *Journal of Family Issues, 17,* 283-316.

Titus, R. & Gover, A. R. (2001). Personal fraud: the victims and the scams. Pp. 133-151 in G. Farrell & K. Pease (Eds.), *Repeat Victimization (Crime Prevention Studies, vol. 12).* Monsey, NY: Criminal Justice Press.

Trickett, A., Osborne, D., Seymour, J., Pease, K. (1992). What is different about high crime areas?. British Journal of Criminology, 32, 81-90.

Weisburd, D. (2003). *Ethical practice and evaluation of interventions in crime and justice: The moral imperative for randomized trials.* [Evaluation Review]. 27(3), 336-354.

Weisburd, D. & Braga, A. A., (2003). Hot Spots Policing. In H. Kury & J. Obergfell-Fuchs (Eds.), *Crime Prevention: New Approaches.* Mainz, Germany: Weisser Ring.

Weisburd, D. & Lum, C. (2004). The diffusion of computerized crime mapping in policing: Linking research to practice. *Police Practice Research,* (forthcoming).

Wilson, J.A. & R.C. Davis (2006). Project Greenlight: Smoothing the Path from Prison to Home. Criminology and Public Policy, 5, 303-338.

Weisz, Arlene N., Richard M. Tolman, and Larry Bennett (1995). *Effects of services to Battered women on completed prosecutions and levels of police intervention.* Paper presented at the Fourth International Family Violence Conference, Durham, NH.

Zorza, J. (1994). Must we stop arresting batterers?: analysis and policy implications of new police domestic violence studies. *New England Law Rev*., 28, 929-90.