

Preventing Repeat Incidents of Family Violence: A Randomized Field Test of a Second Responder Program in Redlands, California

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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 (e.g. see 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.

A study in Richmond, Virginia, by the Police Foundation (Greenspan, Weisburd, Lane, Ready, and Crossen-Powell, 2003) and a Portland, Oregon, study by Jolin, Feyerherm, Fountain, and Friedman (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.

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A series of studies in New York 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, Maxwell, and Taylor (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 not only to authorities, but also to research interviewers. The New York field tests suggested that second response programs might actually increase the likelihood of new abuse.

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 and Smith, 1994; Anderson, Chenery, and 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 (see, e.g., Boruch, Victor, and Cecil, 2000; Campbell and Boruch, 1975; Cook and Campbell, 1979; Farrington, 1983; Feder and Boruch, 2000; Shadish, Cook, and 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.

METHOD

The study was conducted in Redlands, California, a community of 70,000 at the foot of the mountains and edge of the desert in the East Valley region of southwest San Bernardino County. With the cooperation of the Redlands, California, Police Department (RPD), we conducted a randomized experiment in which households that reported a domestic incident to the police were assigned to one of three experimental conditions: (a) second responders were dispatched to the crime scene within twenty-four 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).

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.

For cases assigned to one of the two, second response treatments, 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 typically lasted 30-45 minutes. 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. 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.

Cases assigned to the control condition received the same patrol response as cases assigned to the two experimental conditions but they did not get a second response. 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.

Six months after the reporting date of the last incident in the study, the RPD database was searched 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 of the perpetrator.

At the same time, 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 the research offices. Finally, for cases in which victims still had not been reached, we made two home visit attempts—one during the day and one during evening hours.

The combination of methods resulted in an interview success rate of 41 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. There were no significant differences in the proportion of victims interviewed according to assigned treatment.

RESULTS

Recidivism Data from Redlands Police Records

Table 1 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.¹ Those results, also displayed in Table 1, showed essentially no difference according to treatment groups.

Table 1: Prevalence of New Incidents by Treatment Group

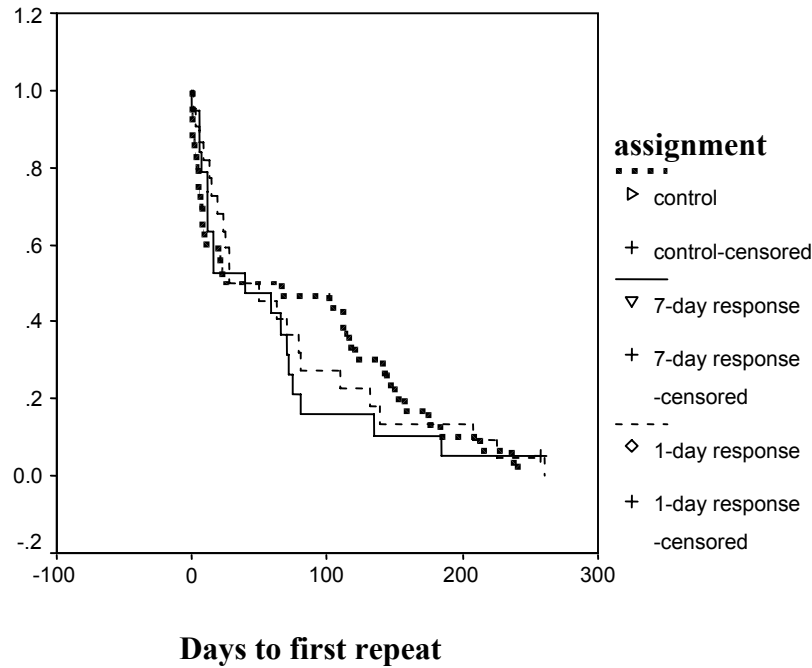
	1-day Response %	7-day Response %	Combined %	Control %	Sign*
Any subsequent incident reported	32	23	28	24	.51
Subsequent incident with same perpetrator	25	20	23	22	.99

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

Essentially the same results were obtained in analyzing survival data, as shown in Figure 1. The survival curves in the figure 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. Again, however, the results did not approach statistical significance.

¹ 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.

Figure 1: Survival Functions by Group



The same pattern was repeated when examining 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. A negative binomial model did not indicate that these differences were statistically reliable (see Table 2).

Table 2: Frequency of Abuse by Treatment (Negative Binomial Regression Models)

	Coefficient*	Standard error	b/std. error	Sign.
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 3 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.

Table 3: Prevalence of New Abusive Incidents by Treatment Group

	1-day Response %	7-day Response %	Combined %	Control %	Sign*
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.

DISCUSSION

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 checklist 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 indications 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 U.S. Justice Department (DOJ), 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.

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.

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