

# Some Unexpected Effects of a Police Service for Victims

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*This article examines the impact of a special police program for crime victims. Police officers recontacted recent victims by telephone. The officers were to find out whether they needed any assistance, refer them to support services, offer advice and information, gather any new information on the case, and indicate the department's continuing concern for their plight. Participating victims also were sent crime prevention materials. The program was evaluated using an experimental design. Victims were randomly assigned to treatment and control groups, and only the former were contacted. Then, follow-up interviews were conducted with all victims in order to assess the impact of the program.*

In the past decade there has been an awakening of interest in the fate of crime victims. Many programs have been developed to provide ameliorative services for victims (these are summarized in Waller, 1982; Cronin and Bourque, 1981). Although there have been some studies of the needs of victims (Friedman et al., 1982) and their contacts with service programs (Elias, 1984), there have been few *experiments* probing the impact of victim assistance. Many of the consequences of victimization, including fear, dissatisfaction with the police, personal withdrawal, and flight from the neighborhood, also are rooted in other social, economic, and experiential factors. Those, in turn, are likely to be related to the capacity of people to seek out services on their own, or to the decisional criteria of service-delivery personnel seeking "suitable" clients. Thus it is likely that only true experiments, which break the ties between those factors and program participation, will provide credible

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evidence of program effects. This report describes an experimental evaluation of a modest program for crime victims conducted by the Houston Police Department. Over a ten-month period, victims were assigned randomly to treatment and control conditions, and only the former were served. This selective treatment was justified ethically and politically by the fact that the program was new and could not be large enough to serve everyone, often a useful basis to appeal successfully for random assignment. Personal interviews were conducted with 351 participants and nonparticipants to gather evaluation data. The results were clear: The program generally had no measured benefits for participants. In fact, among specific groups, including Hispanics and Cambodian and Vietnamese refugees, there was some evidence that the program had negative consequences.

### *ASSUMPTIONS UNDERLYING THE PROGRAM*

Victims traditionally have been the "forgotten participants" in the criminal justice system, valued only for their role in (a) reporting crimes when they occur, and (b) appearing in courts as witnesses. However, studies of the police have highlighted the extent to which their function often is one of dealing with victims' problems rather than "fighting crime" (Sherman, 1986). Police officers who respond to calls represent the sole contact that the majority of victims have with the criminal justice system, for many crimes are never solved and many do not even warrant a follow-up visit from a detective. Patrol officers are the primary link between victims and the state, and any attempt to expand programs for crime victims inevitably will depend upon the cooperation, if not active assistance, of the police. There is some correlational evidence concerning the consequences of how police deal with victims, but few evaluations of the effectiveness of police programs for victims. Research on victims' relationships with the police suggest the matter is complex. Surveys indicate that most people have a favorable opinion of the police, and even victims are positive about them shortly after their experience. However, once they come into contact with the police, the satisfaction of many erodes (Shapland, Willmore, and Duff, 1985; Schneider, Burkhart, and Wilson, 1976).

Victims appear to want information, recognition, advice, support, protection, and reassurance, and they often do not get it from the police.

Victims feel frustrated by a lack of feedback about progress in their case or its probable disposition (Kelly, 1982). They know very little about police or court procedures, and may have unrealistic expectations about the capacity of the police to solve their case. Several studies indicate that victims have little knowledge about programs available for them or where to turn for assistance with practical problems (Elias, 1984; Ziegenhagen, 1976). Victims need advice on what to do, assistance with pressing problems, and sympathy. Shapland (1984) found that "caring and supportive attitudes [on the part of police] were the main subject for victim praise."

### *THE PROGRAM*

The Houston program was designed to meet these needs, operate in a resource-efficient manner, and be evaluated in a rigorous fashion. It involved police officers recontacting recent crime victims by telephone. One of their tasks was to indicate the department's continuing concern for the victim's plight. The officers were to find out whether victims needed any assistance. Before calling, the contacting officers systematically reviewed case reports for clues concerning potential problems; they then conducted the interview following questionnaires developed for each major type of offense. The officers were prepared to offer advice and information on filing insurance claims, and to answer questions about the progress of the case. The calls potentially could facilitate a two-way exchange of information between victims and the police. The officers took reports of any new information that had turned up about the case. In addition, the calls provided citizens an opportunity to report threats of retaliation, other crimes, or even "nonpolice" problems with things such as municipal services. The contacting officers had on hand an inventory of community resources and public and private agencies to which victims could be referred when appropriate. They also had a number of crime prevention brochures, which were mailed to virtually all of the contacted victims. Finally, the Task Force planning the program felt it would help reassure victims that the police were around, aware of their problems, and providing continuous protection.

The program was thus intended to have the following measurable outcomes:

- victims' satisfaction with the quality of police service would be enhanced
- victims would take more positive measures to protect their homes from revictimization
- victims' fear of crime would be reduced
- victims' commitment to their neighborhood as a place to live would increase

An important appeal of this program was its simplicity and low cost. It gave the department a chance to "reach out and touch" victims without changing the organization or challenging the traditional work styles of patrol officers on the street. The program thus seemed readily implementable.

### *EVALUATION DESIGN AND MEASURES*

The evaluation was an experiment in which victims were randomly assigned to treatment and control groups in order to equate them on factors other than their treatment status. An examination of case assignments revealed that their grouping was unrelated to the social background of victims, the extent of their injury, victim-offender relationships, weapon use, and numerous other features of the incidents.

Although the program dealt with victims of all kinds, the evaluation was confined to a subset: Houston residents thirteen and older who were victimized by conventional personal or household crimes. Victims of rape and the survivors of homicide victims were served by other programs, and so were excluded. The treatment group included 235 victims; there were 250 cases in the control category. The recontact office reached 93% of the treatment cases by telephone; the others received a letter from the program director. Due to administrative problems involved in extracting incident records from the department's computer, only 15% of victims could be contacted within seven days of the crime, 45% were contacted within two weeks, and 82% were contacted within one month of the time the crime occurred.

As the evaluation period grew to a close, personal interviews were conducted with victims in both treatment and control groups. For incidents (such as burglary) in which the household rather than particular individuals could be considered the victim, the original

complainant was the target of the recontact call and the evaluation interview. The interviewers were blind to the experiment. They knew the sample consisted of persons who had been victimized, and that the police department was the source of sample names and addresses. If respondents asked how they got their names, they were to be told that interviews were being conducted "with persons who had contacted the Houston police." A check-box on the questionnaire indicated that 43% of those interviewed were given that information. The first visits of the interviewers were preceded by an advance letter from the Mayor of Houston introducing the survey. Every effort was made to locate sample victims, even if they had moved. A large number of follow-up contacts was required in each case, and none could be abandoned without authorization by the survey supervisor. Validation checks were made of one-third of each interviewer's completed cases.

In the end, interviews were completed with 74% of treatment cases and 70% of control cases; one interview could not be independently verified, leaving 176 completed control cases and 175 completed treatment cases. There were few outright refusals (4%). More often, interviewers were unable to find anyone at an address after many visits, the victim had moved out of the metropolitan area, or the victim spoke neither English nor Spanish. In the aggregate, and when examined by type of crime, this completion rate compares favorably with U.S. Census Bureau surveys of victims selected from police files (Turner, 1981; Dodge, 1981; Murphy and Dodge, 1981). Comparisons of a number of measures gleaned from police incident reports for all of the cases indicated no significant differences between the original sample and the completed sample.

As discussed above, there were four hypothesized consequences of the program: satisfaction with the quality of police service would be enhanced, victims would take more positive measures to protect their homes from revictimization, commitment to their neighborhood as a place to live would increase, and fear of crime would be reduced. These consequences were measured using multiple-item scales. The questions were written to provide heterogeneous coverage of the concepts of interest, and most of the scales employed questions with a variety of response formats. The scales were single-factored, with reliabilities (Cronbach's alpha) of around .70. There were three measures of fear of crime: one assessed *fear of personal attack*, one the perceived *extent of local personal crime*, and one the extent of *local property crime*. There was one five-item measure of *satisfaction with police services*, a three-

item measure of *neighborhood satisfaction*, and an index that counted the recent adoption of up to six *household crime prevention actions*. (More details on the scales can be found in Annan, 1985.)

## FINDINGS

The data were analyzed in three ways. Mean differences between treatment and control groups were calculated; those mean differences were adjusted for measured covariates; and hypotheses about treatment-covariate interaction were tested. Only the last analysis revealed any interpretable program effects.

The first look at the data found only one marginally significant ( $p = .05$ ) difference between treatment and control group means on these six scale scores. In contrast to the control group, those in the recontact group were likely to think that property crime was a *larger* problem in their area. An analysis of mean differences of the individual items making up the various scales pointed to the same conclusion and revealed numerous but individually nonsignificant differences also pointing in the direction of higher levels of fear and perceptions of crime problems among victims in the treatment category. This was counter to the program hypothesis.

The next analysis employed multiple regression to bring to bear more of what is known about the concerns of victims. In a true experiment with a large sample, mean differences between groups (or the lack thereof) may be persuasive evidence of program effects. However, there are other important factors that affect most of the outcome measures outlined above, including age, gender, race, and the nature of the victimization experience. These sources of naturally occurring (as opposed to experimentally induced) variation in the outcome measures "disturb" the measures (Judd and Kenny, 1981), and one can improve the efficiency of an estimate of the impact of the treatment by controlling for them. If other known sources of variation are accounted for, the remaining variation in outcomes that might be related to the intervention can be estimated more accurately.

The list of those control factors was a long one. The measures all were drawn from police incident reports filled out at the time of the crime; thus they could not have been affected by the ensuing treatment, one of the assumptions of this analysis model. There were five indicators of

crime seriousness (covering loss, weapons present, and injury), and measures of incident location, victim-offender relationships, other recent victimizations, acquaintance with victims, neighborhood ties, other contacts with the police, and nine indicators of personal attributes.

Controlling for the linear effects of these 27 indicators had only one effect—it enhanced the significance of the previously identified and unanticipated impact of the program on perceptions of the extent of local personal crime problems. Why that should have been the case remained a mystery. Otherwise, controlling for other factors, which in past research have been correlated with the outcome measures, did not clarify any program effects.

The third analysis asked the question, “Did the intervention appear to have a (larger) effect upon particular kinds of clients?” Often a program or policy may prove more relevant or useful for some groups of people and less for others. This may be linked to measured differences in who they are or the nature of their problems. These subject-specific effects may be detected as treatment-covariate interactions.

Often hypotheses about subgroup interaction effects are sufficiently numerous, unanticipated, or so ill-formulated that they are not built into the evaluation design, but instead are explored only after the fact. That was the case here. There were no special strata used in assigning cases to treatment or control conditions in order to ensure a balance of cases for particular client subgroups, and the number of victims who fall into some hypothetically important categories sometimes is very small. Rather, the program officers tried to deal with all the (treatment group) victims from their district. However, in light of past research, the nature of the program as it evolved, and the problem it addressed, suggested several plausible hypotheses about “who it might help” among the larger pool of victims recontacted by the police in Houston.

- *Culture*. The recontact program was conducted entirely in English, and the effect of the police calling recently immigrant Asian and Hispanic victims may have been as bewildering as reassuring. Some victims also may not find the extra attention of the police as reassuring as do others. Blacks, and immigrants from some countries, historically have had troubled relations with the police, and may fall into this category. Hypothesis: Recontact had less beneficial effects upon blacks, Hispanics, and Asians.
- *Seriousness*. One important feature of the recontact program was that it involved victims of almost all manner of crimes. There were many measurable differences in the level of seriousness of the incidents, some of

- which may not have had any measurable consequences. Hypothesis: The program had more beneficial effects upon the victims of serious offenses.
- *Vulnerability.* Previous research on fear of crime suggests that certain kinds of people feel particularly vulnerable to crime, either because of their limited capacity to defend themselves or their fear of the physical injury consequences of victimization (Skogan and Maxfield, 1981). Two vulnerable groups are women and the elderly. Hypothesis: The program had more beneficial effects for vulnerable victims.
  - *Supporters.* One finding of research on victims is that those who are not surrounded by networks of supporters are more afraid of crime (Friedman et al., 1982). For example, people who live alone may have no one to share their concerns with, and if they are victimized they may not have anyone to take care of them. They may feel at risk even at home if they are often alone. People who do not know their neighbors may rely more upon the police and social agencies for reassurance and support, and need the attention of “official” supporters. Hypothesis: The program will have more beneficial outcomes for isolated victims.
  - *Extent of Victimization.* While everyone in the evaluation sample had been victimized at least once, the questionnaire identified other recent experiences with crime. Those interviewed described an average of 3.1 victimizations in the past 10-12 months; 26% of the respondents recalled seven or more incidents during that period. In addition, many were “vicariously” victimized—that is, they knew other people in the area who had been victimized. Both direct and vicarious victimization are correlates of fear of crime and prevention behaviors, and those in this group may be a subset in need of reassurance. Hypothesis: Recontact had beneficial consequences for more frequently victimized victims.

To test these hypotheses, a regression analysis was conducted that took into account the main effects of treatment and group membership, and then tested the significance of an additional treatment-covariate interaction term. The results clarified the initial, counterhypothetical findings. There was a significant relationship between being Hispanic or Asian, *plus* being recontacted, on *all three* scales measuring fear and perceptions of crime. In each case, the apparent impact of the program was to increase levels of fear among Hispanic and Asian victims. This treatment-covariate interaction was robust. When entered in a regression analysis with the other 27 covariates, this interaction measure was still significantly related to fear of personal victimization ( $p < .01$ ) and concern about local property crime ( $p < .03$ ). A detailed look at the scale scores (presented in Table 1) told the same story. In the group that was recontacted by the police, Hispanics were more fearful of personal

victimization and perceived more problems with personal and property crime in their area of the city. They were also less likely to be satisfied with the neighborhood as a place to live if they had been called, but the difference was not significant. None of these differences could be seen for whites or blacks. There were not enough Asians in the sample to support a statistical analysis, but the four in the treatment group were much more likely than the three Asians in the control group to be fearful, worry about personal and property crime, and to be dissatisfied with where they were living.

There are only limited data to probe *why* the recontact program had this effect. It was not that Hispanics and Asians soured on the police; in fact, for both groups general evaluations of police service were higher (but not significantly) in the treatment category (see Table 1). One clue may be found in interviewer ratings of each respondent's facility with English. Virtually all of the victims whose English was rated "fair" or "poor" rather than "good" were in these two groups, and an analysis of mean outcome scores (also shown in Table 1) indicates that victims who were rated only fair or poor in their facility with English were more likely than others to perceive area personal crime problems and to be dissatisfied with their neighborhood. Linguistic differences on other outcome measures were not significant, but several also pointed to unexpected program consequences. Using the facility-with-English measure in a regression analysis and in a treatment-covariate interaction test did not account for all of the Hispanic/Asian effect, however. Language may be only part of the problem, perhaps because the measure does not capture all of the cross-cultural communication problems that it points to.

## CONCLUSION

The apparently negative effect of Houston's callback program upon Hispanic and (perhaps) Asian victims may yield a useful lesson about the importance of sensitivity to cultural differences in the implementation of police programs.

The program effects revealed by mean differences between treatment and control-group members were weak, as well as inexplicable. They were more significant when extraneous sources of variation were taken into account using multiple regression, but remained difficult to

**TABLE 1: Outcome Scores by Race and Facility with English**

	Fear of Area Personal Victimization		Perceived Area Personal Crime Problems		Concern About Area Property Crime Problems		Satisfaction with Area		Evaluations of Police		Number of Cases	
	Control	Treatment (Sigf)	Control	Treatment (Sigf)	Control	Treatment (Sigf)	Control	Treatment (Sigf)	Control	Treatment (Sigf)	Control	Treatment
Race												
Black	1.70	1.54 (.25)	1.45	1.55 (.25)	2.15	2.26 (.25)	2.12	2.13 (.50)	3.11	2.98 (.25)	43	45
White	1.69	1.67 (.50)	1.66	1.69 (.40)	2.17	2.07 (.25)	2.15	2.14 (.50)	3.28	3.29 (.50)	79	85
Hispanic	1.51	1.86 (.005)	1.53	1.84 (.025)	2.01	2.34 (.01)	2.40	2.18 (.10)	3.16	3.23 (.40)	44	38
English												
Good	1.68	1.68 (.50)	1.60	1.67 (.25)	2.19	2.19 (.50)	2.15	2.16 (.50)	3.17	3.20 (.40)	147	163
Fair or Poor <sup>a</sup>	1.44	1.58 (.25)	1.39	1.94 (.005)	1.72	2.05 (.10)	2.64	2.28 (.05)	3.24	3.48 (.25)	29	12

a. Based on interviewer rating. "Poor" includes interviews conducted in Spanish. One-tailed small sample t-tests of significance.

explain. Formulating and testing explicit hypotheses concerning treatment-covariate interaction proved more fruitful. The evaluation data cannot pinpoint the precise source of the negative impact. Victims with poor facility in English may not have understood what was being said to them by recontact officers. Or, they may have understood the words but interpreted the intent of the program differently than did victims from different cultural backgrounds. Nonetheless, this analysis of the evaluation data did identify an important shortcoming in the program, and pointed in the direction of the problem.

While the recontact program fielded by the Houston Police Department was simple, cheap, and did not have to face any hard implementation questions, it probably was "too little and too late" to have demonstrable effects upon the fears and perceptions of crime victims. Because of the typically long period of time between the incidents and the calls, victims with serious problems would have to have solved them on their own. The department responded to the linguistic problems revealed by the evaluation by making Spanish-speaking officers available to the program. However, the program's contacts with victims were fleeting, and the callback officers had little concrete help to offer them. It would be a mistake to extend the conclusions of this research to "victim services" in general, for many programs offer much more rapid and extensive contact and support. However, the *method* by which the Houston effort was evaluated holds real promise for documenting the effects of such programs.

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