In Guenther Kaiser, Helmut Kury and Hans-Jorg Albrecht (eds)
Victims and Criminal Justice, vol. 3
Freiburg: Max Planck Institute, 1991, 97-114

The Impact of Victim Service Programs

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Contents

Since the early 1970s, there has been an enormous growth in the services routinely available for crime victims in the United States. These services take a number of forms. Some organizations emphasize crisis intervention; they provide aid to individuals who are experiencing temporary difficulty in adjusting to post-victimization stress. Victims also may need assistance in dealing with the more mundane consequences of victimization. They may need emergency shelter, clothing, food, or cash. In addition, many assistance programs help victims locate public or private agencies that can provide specific kinds of aid, such as helping them deal with government bureaucracies and serving as "advocates" to represent victim's interests. Programs that are closely allied with the police or prosecution offices frequently facilitate the cooperation of victims with the criminal justice system. Finally, because victims frequently are **re-victimized** at a high rate, some service programs are involved in crime prevention efforts (for a detailed discussion, see *Davis & Henley* 1990).

Despite the growing availability of victim services, we know surprisingly little about their effectiveness. Although there have been many descriptions of victim support programs, there are few systematic evaluations of the extent to which they meet client needs. This chapter describes a study of victims' needs that addresses this question. It is based on data collected from interviews with 470 victims of robbery, assault, and burglary in four American cities: Lexington (Kentucky), Evanston (Illinois), Tucson (Arizona), and Rochester (New York). In each city we had the cooperation of the principal local victim assistance program; they opened their files and allowed us to draw samples of their clients. In cities where the victim service agency did not have the names of all crime victims on file, we also selected samples from police files. In each city, we were able to sample victims who had been served by the programs and others who had no significant contact with them.

1. Evaluating Victim Programs

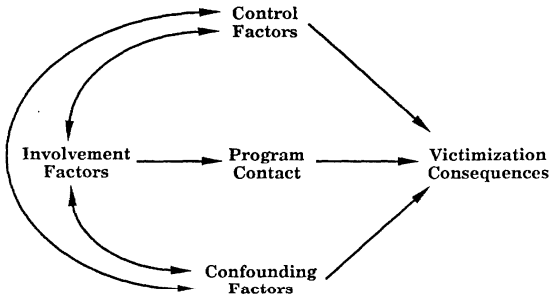
The best evaluations of crime victim services have been "quasi-experimental." That is, they compared samples of victims who were served in some way with others who were not, to identify differences between them on measures of crime-related stress, psychological adjustment, fear of crime, and other criterion factors. These evaluations typically matched the two groups along one or more dimensions (for example, by comparing victims of similar kinds of crime who are of the same sex), and most also employ multiple regression and use statistical controls for other factors influencing the consequences of victimization.

For example, *Davis, Smith and Bauer* (1988) conducted a quasi-experimental evaluation of the impact of training crime victims in crime prevention techniques. Their earlier research indicated that more than 20 percent of New York City crime victims were **revictimized** within four months. In response, they divided victims into two matched groups. One group received the standard assistance provided by the New York City Victim Services Agency, while the other group received additional crime prevention training. They evaluated the effectiveness of this training with a follow-up survey measuring the victimization experiences of the two groups over the next 12 months, and found that the training program had no measurable effect.

However, there are a number of limitations of this research design. These limitations make it difficult to come to any firm conclusions about the effectiveness of victim programs, even though most quasi-experimental studies point to the conclusion that they do not have much of an impact. Most important, there are **selection biases** that are not accounted for. Victims who receive treatment often differ from those who do not on a number of dimensions, and those factors could account for any apparent effects of the program. For example, *Smith, Cook and Harrell* (1985) suggest that the police are more likely to refer highly distraught victims to service programs, and note that court-related services usually go only to victims whose cases are prosecuted, and these typically are among the most serious. The nature of victimization also **precludes the use of a pretest**, an evaluation tool that can overcome some of these problems. In addition, there may be **imitation of treatment**. That is, some victims receive assistance from the police, other agencies, family members, and friends, that closely resembles that given by service programs. This is likely to close the gap between "treated" and "untreated" victims in an evaluation. Studies of the impact of victimization indicate that there is **spontaneous recovery** among victims; over time the consequences of crime diminish in the minds of victims of many kinds of crime. Healing with the passage of time can cloud the apparent effects of treatment when groups are not carefully matched.

All of this suggests that in the absence of randomization it is difficult to assess the contribution that service programs might make to victim recovery. This chapter describes a somewhat different approach to non-experimental evaluation than the matching-and-regression model that characterizes past evaluations of victim programs. This approach attempts to statistically model the confounding problems identified above, using a structural equations design.

FIGURE 1
A Model of Program Impact



A general outline of this approach is sketched in Figure 1, which indicates that we divided the problem into five classes of measures. The dependent variable in the analysis is the **consequences** of victimization, which will be represented by a single factor score. The analysis model includes a treatment indicator, which will be a dichotomous measure of whether victims had **contact** with their local victim services program. Another set of factors are potentially **confounding**; that is, they could be related both to involvement in the program and the consequences of victimization, making it difficult to decide between the two effects. Figure 1 also allows for factors that affect the consequences of victimization and thus need to be **controlled**, but which are not potentially confounding. Finally, Figure 1 includes factors that explain why some victims are **involved** in the program, but that are not linked directly to the consequences of crime (these are some of the "selection factors" described above). The two-headed arrows included in Figure 1 specify that factors may be correlated with each other without implying causality. Note that the model sketched in Figure 1 specifies which linkages should be statistically significant (by the single-headed arrows) and which should not; these hypotheses enable us to test the over-all "fit" of the model to data, as well as estimate the strength of all of the arrows shown in Figure 1.

2. Concepts and Measures

2.1 Impact of Victimization

The four-city study gathered information on the consequences of crime for 470 crime victims. Responses to three questions were used to gauge the crime's **impact on everyday life** at the time of the followup interview. Respondents were asked about how much difficulty they were having in their relationships with family members, how hard it was for them to "get on with life normally," and if the incident still kept them from going places or doing things that they wanted to do. Two other questions measured the continuing **emotional impact** of victimization; respondents were asked how upset and how frustrated they still were when they thought about their experience. In addition, all respondents were asked three **fear of crime** questions to judge how unsafe they felt in their neighborhood, how often they thought about personal crime as they left their homes, and how worried they were about being burglarized.

These three measures of the continuing consequences of victimization could be examined separately, but they also were correlated with one another. Not surprisingly, victim's fear, continuing frustration, and the residual impact of their earlier experience on their daily lives were related. Because the measures were so highly correlated, a single factor score was calculated which combined their common variance; it was correlated +.74 with the measure of fear of crime, +.79 with everyday impact, and +.80 with emotional impact. Victims with a high score on this factor were more upset and frustrated, more fearful of being victimized, and found it harder to lead a normal life. This single measure of the general consequences of victimization will be used throughout the report.

2.2 Program Contact

The purpose of this chapter is to examine differences between victims that may be attributable to the victim program. We operationally defined having "program contact" as recalling receiving at least a telephone call or a personal visit from program staff. Noncontact cases were persons who had no such contacts; their only encounter with the program might have been a form letter or telephone message left with a third party.

By this measure, 34 percent of the victims we interviewed recalled having any contact with their local victim services program. When they had, they were largely pleased with the quality of the service they received; 80 percent ranked themselves as "very satisfied" or "satisfied" with the help they got. They received a mixture of practical assistance (31 percent) and counseling advice (15 percent), and 38 percent got both. However, another 16 percent indicated they received "no help at all," and 29 percent indicated that they had at least one problem that the agency could not help them with.

2.3 Confounding Factors

These are factors that **could** be simultaneously related both to involvement with a service program **and** to the consequences of victimization. As a result, it is difficult to tell whether the relationship that we observe between program participation and the consequences of victimization is due to the actual impact of the program, or if the relationship is really due to the concurrent impact of other variables. For example, *Smith, Cook and Harrell* (1985) found that police spent more time and effort helping victims who were evidencing severe consequences of victimization, and that they were also more likely to refer them to a service program. As a result, any effect of the police in assisting victims to adjust to their new situation could be mistaken for a positive effect of the program. At the same time, the tendency of police to refer more severely impacted victims for treatment might make the program appear to be ineffective, for it is very likely that these victims would still be worse off than nonparticipants at the point when the program was evaluated.

In our four city study, we examined the possibly confounding role of a number of important factors. The first was the amount of disruption that victimization had caused in our respondents lives, at the time of the crime. This was measured along two dimensions: the impact of crime on their daily lives at the time of the incident, and the emotional impact of the incident as they recalled it later during the interview. These were the same as the measures of the disruption and emotional state of victims at the time of the interview, as described earlier. The **everyday impact** of victimization was assessed by questions about the difficulty it caused for relationships with family members and with "getting on with your life normally," and if the incident kept them from going places or doing things that they wanted to do at the time. The **emotional impact** of the incident was assessed by questions about how upset they were at the time of the crime, and how frustrated they were by the situation. Presumably, victims who were more

upset and whose lives were more disrupted would be more likely to seek - or be referred to - a service program, and would also be more likely to report being disrupted and distraught at the time of the interview.

Table 1: Scale Score Correlations with Victimization Consequences and Program Contact

	Correlation with:	
	Adverse consequences	Program contact
Potential confounds		
Everyday impact	.59 **	.07
Emotional impact	.36 **	.05
Social support	.32 **	-.07
Other victimization	.21 **	.07
Incident seriousness	.32 **	.05
Support by police	-.14 **	.09 *
Race (nonwhites)	-.03	.05
Length of residence	-.13 **	-.05
Employed	.11 **	-.06
Sex (female)	.21 **	.09 *
Age	.03	.11 **
Crime a robbery	-.05	-.02
Crime an assault	.04	.04
Elapsed time in days	-.07	.07
Control factors		
Life stress	.32 **	.01
Internal locus	-.31 **	-.02
Involvement factors		
Police referral index	-.04	.36 **
Information from others	.01	.41 **

Note: one tailed significance test *>.05 **>.01.

Table 1 presents the correlation between summary scale scores combining responses to the individual questions noted earlier and our measure of the consequences of victimization. Both the impact of victimization on their everyday life and the extent of emotional impact recalled by these victims were significantly related to the continuing consequences of victimization at the time of the interview. On the other hand, for this sample of victims there was no significant correlation between those measures and whether they came into contact with their local services program. This runs counter to *Golding et al.'s* (1988) finding that the use of services is mediated primarily by victim distress, but that study included only female victims of sexual assault, a crime that is not represented in our data.

Table 1 also presents those correlations for a three-item indicator of the extent of the **personal network** supporting each respondent, measured by how difficult respondents thought it would be to borrow money, get a ride if they needed it, and get help from someone to solve their problems. We hypothesized that victims with extensive personal support networks would be less likely to avail themselves of program services, and would also be less affected by victimization in the months following the incident. In addition, Table 1 summarizes the apparent impact of a two-item index of how often these victims had experienced **other crimes** during the past two years. We hypothesized that repeated victims would be more likely to seek assistance, and would also be more impacted by crime. Likewise, we hypothesized that victims of **serious** crime would be more likely to receive assistance, as well as be more affected by its consequences. This was measured in a variety of ways in the follow-up interview, but most of the variation in other serious measures was represented by a single question asking respondents to rank their experience from "very serious" to "not at all serious." Finally, we hypothesized that victims who got more supportive attention from the **police** would report fewer continuing difficulties, and would be more likely to be placed in contact with a service program. This was measured by combining responses to three questions that asked all victims about how helpful and sympathetic the police were, and how satisfied they were with the way the police responded. Table 1 indicates that this index was indeed related to a higher probability of coming into contact with a program, and it also was related to lower levels of current distress. *Smith et al.* (1985) suggested that the police are more attentive to victims in extreme distress, and that controlling for other factors these victims would be more upset than others even some months after the event, but that was not the case in this instance.

Two other sets of potentially confounding factors are described in Table 1. The first is a set of demographic factors describing the backgrounds

of victims interviewed in the four-city study. (Other demographic factors were included in the survey, but they were not correlated in any substantial way with either program contact or the consequences measure.) As suggested by past research, victims with long-term roots in the community (measured by length of residence) are more likely to report fewer continuing consequences of victimization, and women are more likely to report more. Those who were employed were more likely than others to report adverse effects of their experience, but this difference disappears when age and sex are taken into account. Women and older persons were more likely to come into contact with their local victim service program, while men are more likely to recover quickly from the effects of victimization (*Resick* 1987). Race is included in the multivariate analysis described below because past research suggests that nonwhites are more likely to be impacted by victimization. Similarly, variables identifying victims of assault and robbery (in contrast to burglary) were included because past research suggests that personal victimization should have more of an effect on the kinds of adverse consequences examined in this analysis.

Finally, Table 1 examines the relationship between the consequences measure, program involvement, and the amount of **time that passed** between each victimization and the date on which the interview was conducted. Studies of the impact of victimization indicate that the passage of time is one of the best healers; they frequently find that over time the consequences of crime diminish, albeit at rates that differ for various kinds of offenses and victims. This is often called "spontaneous recovery," and it is obviously an important factor to control in a correlational study. As Table 1 indicates, elapsed time was negatively related to current consequences in our four-city sample, although the relationship was not quite statistically significant (p.=06). Table 1 also reveals that victims who were interviewed after a longer interval of time were **more** likely to recall having contact with the program. This was an artifact of our sampling procedures, for the correlation between program involvement and elapsed time varied considerably across cities. It was strongest in Tucson, where we sampled victims who had contact with the local service program from their files, and added to this an additional list of victims from police files. Because the police had the names of many victims who were not contacted by the program, we satisfied our sampling quota of non-contact cases without delving too far into the past. To satisfy our quota of contact cases we had to select victims from as long as 11 months in the past. As a result, there was a substantial positive correlation (=.34) between program involvement and elapsed time in Tucson. As a result, elapsed time is among the "confounding" variables, and it (along with the city of residence of each respondent) will be included in the statistical analysis described below.

2.4 Involvement Factors

These are factors that help explain why people are involved in the program, but that are **not** likely to be related to the longer-term consequences of victimization. They are included in the analysis to help account for "selection biases"; i.e., systematic differences between program participants and non-participants. In the four-city study we focused on the ways that people heard about, or were referred to, the programs. Victims were asked if the police had told them about the availability of services, or if police had given them any brochures or information about their rights; the two were related strongly, so this analysis employs a summary measure of **referral by police** that combines responses to the two questions. In other parts of the questionnaire, victims were asked if a prosecutor had told them about the program, if they had heard about it from friends or a family member, if they had heard about it from another victim, or if they had seen something in the media about the program. Responses to these questions are summarized in an index of the number of different places respondents **heard or were told** about victim services.

Table 1 presents the correlation between each of these indices, the adverse consequences of crime, and the extent of program contact for crime victims in four cities. Both police referral and information from others were significantly related to program contact, but not to the adverse consequences of victimization.

2.5 Control Factors

These are factors that effect the consequences of victimization, but that probably are not related to program contact. Controlling for them increases the plausibility of the over-all model by increasing the explained variance in the consequences measure. This analysis includes two such indicators. One is an index of **personal locus of control**. This was measured by responses to two questions about the extent to which respondents thought that "what happens is one's doing," and "people have control over their own lives." A high score on this measure identifys respondents who felt they had greater personal control over their fate. *Norris* and *Scheer* (1989) found that victims with an internal locus of control were more likely to seek the assistance of mental health professionals, but the role of police and others in referring victims to official service programs could cancel out the effects of such psychological orientations in many circumstances. Surveys in these four cities indicated that locus of control was not linked to program contact, although victims with a high personal locus of control were less likely to report continuing adverse consequences. Likewise, victims who were experiencing a great deal of **life stress** at the time of the incident were more likely to feel the continuing consequences of that crime. Life stress was measured by combining responses to questions about whether family members or friends had been ill or died; if the respondents had been ill or injured some other way near the time of the crime; if they had lost their job or had financial problems; if they were divorced or had problems with their mate; and if a member of their family had been arrested or was on drugs.

3. The Sample and the Survey

The focus of this study was on victim's needs, where they sought help, and the kinds of assistance that they received. Thus it was necessary to interview some victims who had received assistance from service programs, and others who perhaps only received help from their families or friends, or did not receive any aid at all. In each city we planned to complete 60 interviews with victims served by the programs we were studying and 60 interviews with victims not served by the programs. We further divided each planned sample between robbery victims, assault victims, and burglary victims. We knew from experience that crime victims can be difficult to track down for subsequent interviews: they are wary of strangers, and many move or change their telephone number as a result of their experience. Because we would not be able to find all of our sample victims, we oversampled from program and police files. At each site we had to tailor our selection plan to fit the record systems that we encountered there. Because the interviews were conducted by telephone, we included in our samples only victims whose telephone numbers were recorded in program or police files. Our decision to conduct telephone interviews was cost-driven; we recognized that eliminating victims without telephones led to some sample bias, but the cost and logistical difficulties of conducting personal interviews in four different cities was prohibitive.

The survey was conducted by 12 experienced interviewers during the early Summer of 1989. They attempted to contact victims at 1026 sample telephone numbers. Of these, 345 (34 percent) were no longer working numbers, had been changed to unlisted numbers, or whoever answered claimed that the respondent had moved. This verified our expectation that victims often attempt to break their connection with the circumstances that led to their victimization. In another 128 cases (12 percent) no one ever answered the phone, the respondent was never home, or we only reached an automatic answering machine. Victims refused to be interviewed or

denied that they had been a victim only 8 percent of the time. In all, we completed interviews with 470 (46 percent) of the victims that we had originally selected. This is significantly higher than some comparable studies of crime victims (see *Friedman et al.* 1982), but lower than response rates achieved in personal interviews with victims by the US Census Bureau (see *Skogan* 1981). It is quite close to the 45 percent completion rate obtained for a survey of known victims in London (*Sparks et al.* 1977). Response rates did not vary much by city, ranging from 41 to 48 percent.

The distribution of completed interviews was remarkably similar across the four cities. Among respondents, the percentage of robbery victims ranged from 50 to 54 percent, assault victims from 25 to 33 percent, and burglary victims from 13 to 24 percent. Overall, 38 percent of respondents were nonwhites, 58 percent were women, 12 percent were over 60 years of age, and 30 percent were quite poor - they reported household incomes of less than US $10,000 per year. These demographic factors varied by city, reflecting differences in the economic and social makeup of each community. The average elapsed time between the day of the crime incident and the day that we interviewed the victim was 5.4 months.

4. Findings

The analysis followed the model sketched in Figure 1. Figure 1 described a network of interrelated variables that can be represented by a series of structural equations. These equations are estimated simultaneously, so that the separate effects of factors with two dependent variables - for example, the effects of incident seriousness on both program contact and the adverse consequences of victimization - can be calculated at the same time. All of these computations were done using LISREL 7.13.

Table 2 summarizes the results of the analysis. To simplify Table 2, only significant linkages ($t > 2.0$) are reported. In total, the model sketched in Figure 1 explained 66 percent of the variance in all of the measures. With 4 degrees of freedom and a chi-square of 2.51, a p value of .643 indicates that the test of the hypothesis that the residuals from the hypothesized model are not significant can be accepted. In fact, no individual residual from the model was significantly different from zero. The adjusted goodness of fit index was 0.97, exceeding the standard of 0.95 recommended by *Joreskog and Sorbom* (1986).

Table 2: Significant Regression Coefficients for Victimization Consequences and Program Contact

	Coefficient for:	
	Adverse consequences	**Program contact**
Potential confounds		
Everyday impact	.400	
Emotional impact	.150	
Social support	.112	
Other victimization	.133	
Incident seriousness	.108	
Support by police		
Race (nonwhites)	-.089	
Length of residence		
Employed		
Sex (female)	.095	
Age		.121
Crime a robbery		
Crime an assault	-.104	
Control factors		
Life stress	.087	--
Internal locus	-.131	--
Involvement factors		
Police referral index	--	.326
Information from others	--	.371
Program contact	(-.015) (not sigf.)	--

Note: Coefficients p>.05 are excluded. Coefficients are net of all other factors in the model. The analysis also included dummy variables for the cities.

The principal finding reported in Table 2 is that there was no statistically reliable link between program contact and the adverse consequences of victimization. The sign of the coefficient associated with program contact was in the hypothesized direction - those with program contact reported fewer adverse consequences at the time of the follow-up interview - but the effect was far from significant.

In addition, the analysis indicates that controlling for other factors, nonwhite residents of these four cities were somewhat less likely than whites to report continuing consequences of victimization. Women were more likely than men to score at the upper end of this measure. Those facing multiple forms of stress in their lives at the time of the crime were also more likely to feel its continuing consequences, while those with a high internal locus of control were less so. Not surprisingly, the list of potential confounding factors gauging the seriousness and impact of the crime at the time continued to predict who felt those consequences most severely. Although it is not shown in Table 2, both program involvement measures (referral by the police and others) were linked to contact with local service programs, but were not directly related to the consequences of victimization.

The principal conclusion drawn here is that a structural equations approach to non-experimental evaluation did not succeed in isolating any ameliorative effects of contact with these four victim service programs. However, there are several reasons to be cautious about this finding. First, there doubtless were confounding factors that were not measured in the survey and included in the analysis. These could suppress any true effects of program involvement (by being positively associated with both program contact and adverse consequences), or they could account for the (insignif icant) negative relationship reported here (by being positively associated with contact and negatively related to consequences). *Davis* (1987a) provides a useful inventory of potentially confounding factors that were not included in this research, including psychological depression and the extent to which victims blame themselves or others for their fate.

Measurement error is a second major limitation of this analysis. Most of the constructs examined here were measured using multiple survey items evidencing acceptable levels of reliability (measured by Cronbach's alpha), but there doubtless was considerable error in the accuracy with which the resulting scale scores represented people's actual experiences and views. Any attempt to "control" for a factor that is measured with error leads to "underadjustment bias" (*Judd & Kenny* 1981); for example, with error in the indicator of the emotional impact of the crime at the time it happened

(which was a recall measure) we would not sufficiently account for its link to our respondents' current levels of emotional distress, even though its effect is seemingly accounted for in the analysis.

5. Toward Randomized Experiments?

This chapter has described how an analysis using structural equations might be used to examine the impact of a non-experimental evaluation of services for crime victims. It found no clear evidence that contact with service programs in four American cities had a significant effect on the intermediate-term consequences of victimization. However, the absence of randomization limits the confidence that we have in this conclusion, because the analysis did not include measures of all potentially confounding factors or the forces that brought victims into contact with the programs. Because error in the measurement of the constructs that were included in the analysis inevitably led us to "underadjust" statistically, even counfounding factors that were included were not fully accounted for.

In this light, it clearly is appropriate to recommend the use of randomized experiments to evaluate victim programs. Experiments should be employed when there is a reasonable chance that a program will have measurable effects, both in principle (because it is a good program) and after less expensive correlational studies suggest that the effects are likely to be substantial. However, it is also clear that there are a number of good reasons why randomized experiments have been so few in number. Many of these were illustrated in a randomized evaluation of three alternative forms of service for victims, which was conducted by the New York City Victim Services Agency (VSA) during the mid-1980s (*Davis* 1987a; 1987b). First, VSA faced the ethical dilemma of deliberately withholding services that they already were providing crime victims, so that the experiment could have a true control group. VSA did so by conducting the experiment in an area of New York which they did not regularly serve, so that persons in all of the experimental groups received "extra" service. However, not all jurisdictions may have this kind of discretion, or large pools of victims who are not being served at all. The VSA experiment also had to deal with the fact that its services were being provided by a highly professional staff. Staff members who were entrusted with actually implementing the experimental treatments strongly believed in the services that they already were providing, and they challenged the potential effectiveness of some of the alternatives proposed by evaluators. They also believed strongly in the individualization of treatment, so that the services they provided were keyed to the needs of

each client. This, coupled with the decentralized nature of human services in general, ensured that there would be considerable "heterogeneity of treatment" (*Cook & Campbell* 1979) within the various experimental categories.

As the VSA experiment proceeded, many victims (about 30 percent of those who completed an initial interview) dropped out or could not be located for followup interviews. This is to be expected, for crime victims in American inner cities often face numerous, burdensome, and fear-provoking personal problems. Hence, selective attrition made it difficult to generalize the results of the study. Experiments in the "real world" rarely resemble the neat examples presented in textbooks. In addition, it turned out that most of those in the two experimental counseling programs attended only one or two counseling sessions. This limited extent of program contact was not much different from regular VSA programs, which emphasize providing victims with material assistance (eg, emergency loans, food coupons, home repairs) and some crisis counseling. However, the experimental counseling focused on the effects of new and more intensive techniques, and the weak treatment "dosage" victims actually received was unlikely to produce much of a change in their lives.

Similarly, *Skogan and Wycoff* (1987) conducted a randomized experiment evaluating the impact of a police service for victims. It involved calling them by telephone to provide advice, information, and assistance in locating any help that they needed. The evaluation concluded that this kind of intervention was simply too weak to make much of a difference in victim's lives. On the other hand, victims in the four cities described here met with or talked to staff members from the local victim services agency an average of five times, but there still was no statistical evidence that it had any effect.

Evaluations of other kinds of human services also suggest that we should be modest in our expectations even about good programs that target the emotional and fear-provoking consequences of victimization. For example, there is little evidence that crisis counseling in the fields of suicide prevention, acute psychiatric crises, and surgery has much effect (*Auerbach & Kilmann* 1978). Only by combining the results of hundreds of discrete studies is it possible to detect clear effects of long-term therapy for psychological problems (*Smith, Glass, & Miller* 1980). There is considerable **correlational** evidence that variations in how the police handle victims affects their recovery (see *Skogan* 1990), but the only randomized experiment examining this issue could not find any impact of programs that trained police officers in how to deal with crime victims (*Lurigio & Rosenbaum* 1989). Instead, Davis (1987b) recommends that we should no longer focus on trying to demonstrate the effects of victim programs on the fears and

psychological recovery of most victims (he sets aside victims of rape and other particularly traumatic crimes, where there is evidence that they do not recover spontaneously). He recommends that research should focus on the practical problems that victims face, and on the best ways to provide them with housing, protection, assistance in dealing with bureaucracies, and financial aid. In the aggregate, practical problems account for the vast bulk of victims' needs (*Mayhew* 1984), and meeting them may be the kind of service that government agencies are most effective at delivering.

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