

4 Methodological Issues in the Study of Victimization*

WESLEY G. SKOGAN

INTRODUCTION

Victimization surveys collect data on criminal incidents through interviews with their participants. This use of self-reports of past events raises important measurement issues. Participants in a victimization survey are more akin to *observers* than to respondents in traditional opinion surveys. We assume that people may or may not have been involved in events which have inter-subjective meaning, about which independent observers could agree. The task of interviewers is to elicit accurate reports of those occurrences. Because the survey gathers data on events external to the individual, and those events presumably have a reality apart from their description to an interviewer, the standard of accuracy in victimization research is the match between the reality of an incident and its description. This

* Some of this material appeared previously in *Issues in the Measurement of Victimization*. Preparation of that report was supported by Grant No. 78-SS-AX-0045, awarded to the Center for Urban Affairs, Northwestern University by the Bureau of Justice Statistics, US Department of Justice under the Omnibus Crime Control and Safe Streets Act of 1968, as amended. The revision of those materials was supported by Grant No. 81-IJ-CX-0069 by the National Institute of Justice, US Department of Justice, and by the Max-Planck Society. Marlene B. Simon and Paul J. Lavrakas, along with several anonymous reviewers, contributed helpful comments on an earlier version. Points of view or opinions stated in this document are those of the author and do not necessarily represent the official position or policies of the US Department of Justice, or the Max-Planck Society.

match will be problematic under the best of circumstances. The problem is exacerbated by the nature of crime, conceptual ambiguity surrounding the definition of criminal incidents, and a host of human processes affecting the accurate recall and description of things which occurred in the past. As a result, data on victimization may seem extraordinarily fragile, overly dependent upon subtle variations in the manner in which it is gathered. In 1966 the Bureau of Social Science Research conducted the first investigation of victimization survey techniques. Its report concluded:

Our survey method is heavily dependent upon the ability and motivation of the respondent to remember events and report them in the interview situation. In our pretest and survey experience, we have found that the quality of the reports of victimization that are elicited by our interviews depends to a considerable degree upon how the task of remembering and reporting is structured by the interview schedule and, presumably, by the way in which the interviewer uses it. (Biderman *et al.*, 1967: 52)

As with the data on crime gathered by police, reports of victimization reflect both the distribution of events and our procedures for eliciting those reports.

Most of what we know about measurement problems in victimization surveys comes from three kinds of research. The first methodological research technique is analytic; it involves carefully examining the results of a victimization survey to infer the impact of various methodological features of the study on the data. The second technique is experimental; it involves varying specific survey methods across parallel samples and then comparing the resulting estimates of victimization rate or other aspects of the data. The third method is criterion validation; it depends on the existence of some alternative record of a crime which we can assume is accurate and we can compare to the results of an interview with the victim. Each of these techniques has made an important contribution to our understanding of the nature of error in measures of victimization.

MEASUREMENT ISSUES

Beginning with the work of the Crime Commission, there has been a great deal of research on specific techniques and strategies for

improving the quality of victimization data. In addition, we can call on a substantial body of methodological research in related fields which confront problems similar to those plaguing victimization studies. These include investigations of the quality of data gathered in surveys of unemployment, household expenditures, and health.

These studies suggest that retrospective reports of experiences with crime are clouded by four kinds of error. Each reflects fundamental human processes, and affects social measurements of all kinds. Errors in the measurement of victimization may be due to (1) ignorance of events, (2) forgetting (or not telling), (3) inaccurate or incomplete recall (or lying), and (4) differential interview productivity. (In addition, there are a host of procedural problems and decisions about survey techniques, which seriously affect the data; they will be considered in the next section). Respondents sometimes do not know of things about which we quiz them. They also might have forgotten about them, a fallibility which in practice we cannot distinguish from their deliberately not telling us about them. Respondents may also either inadvertently or malevolently tell us something that is incorrect. Finally, some people are better respondents than others: they more readily grasp the nature of the task presented them; they work harder at it; and they tire of the demands of a survey less rapidly. All of those factors conspire to shape the volume and character of reports of victimization, sometimes independently and sometimes in conjunction with the true distribution of criminal incidents.

Knowledge of Incidents

The problem of respondents not knowing things which a measurement technique assumes they have knowledge of can have significant implications for the design of victimization studies. The commercial surveys conducted for LEAA were limited in scope to burglary and robbery, deliberately avoiding the difficulties involved in gathering incidence figures for shoplifting and employee theft. It was assumed that a large proportion of those offenses went undetected by their victims. Individuals also may not recognize that an incident is a crime; this has limited the utility of surveys for studying offenses such as fraud. People also seem to exclude broad ranges of their experience as lying outside of the purview of the criminal law. Respondents in the national survey conducted for the Crime Commission were encouraged to volunteer reports of victimization for crimes not

explicitly covered in the interview. However, Ennis (1967) notes that few respondents mentioned ordinance violations, housing discrimination, illegal treatment by government agencies, or other such offenses.

The bulk of research concerning the problem of lack of knowledge on the part of respondents has focused on proxy interview procedures. In early surveys it was assumed that crimes were salient events that would be widely discussed, at least among members of a victim's household. Therefore it was assumed that it would be possible to conduct a victimization survey by interviewing just one adult in a household, asking him or her about the experiences of each household member. This procedure would seem to generate victimization data for a large number of individuals at low cost. Subsequent analysis of data gathered in this fashion indicates the method is inadequate. In Biderman's Washington, DC, study the correlation between household size and the number of incidents reported was even negative, rather than positive, in sign. The same pattern of underrecall for persons other than the respondent has been found in surveys overseas. In a survey in Stuttgart, Stephan (1976) questioned residents of 741 households. In some he interviewed all members of the family directly, while in others he interviewed only heads of households and asked them to report on victimization of other members of their family. Direct personal interviews proved to be almost 50% more productive of victimization reports.

The apparent unreliability of household informants as sources of data about the experiences of others led LEAA to fund an experiment in San Jose and Dayton. In half of the households interviewed (the sample was 11 000 households in each city) a "chance respondent" was interviewed, and in the other half every resident 16 years of age and older was quizzed. Differences between the estimates of victimization rates produced by the two methods were substantial; the ratio was 1.7 to 1 for rape, 2.1 to 1 for strong armed robbery, and 2.2 to 1 for attempted robbery, in favor of the self-response technique (Kalish, 1974:37). As a result of this experiment LEAA decided to adopt complete-enumeration procedures for city and national surveys, despite the substantially greater cost this entails.

Forgetting and Not Telling of Incidents

There is a tendency for victim-respondents to fail to report information about incidents which have occurred and about which they should

have been knowledgeable. We can observe examples of non-recall in methodological studies employing each of the three research techniques described at the outset. For instance, the way in which an interview is structured affects the frequency with which instances of criminal victimization are recalled. Experiments reveal that when respondents have to work harder at their assigned recall task, or when the task is organized so that they easily can learn how to reduce their workload, they will respond by restricting the amount of information they contribute to the survey. Second, record checks indicate that victim recall can be highly selective. Respondents seem to edit incidents which may be embarrassing or may be considered "none of the government's business," even when they previously were reported to the police. Finally, victimization rates analyzed monthly or quarterly over the length of the survey's recall period typically indicate that few incidents occur in the most distant months, although other evidence suggests that crime was just as frequent then. In each case the observed variations in victimization rates are artifacts of the method employed to gather the data rather than reflection of the distribution of the true rate of crime. There are three general sources of non-response which correspond to the examples given above: respondent load and fatigue, purposeful suppression of valid responses, and forgetting.

Load and Fatigue

The effects of workload factors were first noted in the Bureau of Social Science Research's pretest of victimization survey methods. They experimented with two procedures for conducting interviews. In the first, respondents were given flash cards describing criminal incidents. If they indicated that they had been involved in such an event, a detailed incident report form was completed for it at that time. The other procedure involved asking respondents to give "yes or no" answers to a complete checklist of offense descriptions before filling out incident report forms for each positive response. The first procedure clearly linked a positive response with a lengthy respondent task, while the latter did not allow the respondent to become test wise until it was too late. Not surprisingly, the second mode elicited 2½ times as many reports of incidents as the first (Biderman *et al.*, 1967).

The current screening procedure used in the National Crime Survey reflects this experience. By deferring the introduction of incident forms until the completion of the incident checklist, it may encourage more complete recall. However, there may still be a tendency for respondents to suppress reports of victimization in order to speed the interview, a disposition that presumably would be greater in surveys with 12- rather than 6-month reference periods. Surveys that employ a household informant entail a considerably heavier respondent burden. Biderman (1973) speculates that once respondents have manifested their co-operativeness by recalling a victimization, there is less pressure in the interview situation to remember others, because the interviewer has been "satisfied." Personal interviews are social interactions. Interviewers ask for people's time, and they can offer little in return. Respondents may reciprocate by offering a little to the interviewer and then stopping. This may explain the surprisingly slight incidence of multiple victimization documented above. Given the average number of victimizations in the population, statistically we should find fewer nonvictims and more multiple victims than currently are uncovered in surveys (Sparks *et al.*, 1977). Fatigue, impatience with the repetitiveness of the incident screen, and other factors may account in part for the observed distribution. This is likely to be more common among poorly motivated respondents, those who find interviews taxing or incomprehensible, and those who find few social rewards in chatting with someone from the US Census Bureau. Biderman (1973) speculates that such persons may be more likely to be victimized by crime as well.

Lying and Not Telling

The evidence that respondents may be lying, or deliberately suppressing reports of events of which they have full knowledge, is inferential. It comes primarily from record checks based on reports of incidents sampled from police files. In the San Jose methodological study described above, evidence emerged that known victims were neglecting to describe particular events. The relationship between the victim and the offender as recorded by the police seemed to play an important role in the recall of those events in subsequent interviews. Incidents in which the victim and the offender were related to one

another were reported in the survey only 22.2% of the time. The recall rate rose sharply when the relationship between the parties was more tenuous. For events involving strangers the recall rate was 76%. Two-thirds of the personal victimizations that were not recalled involved at least an acquaintance between the parties, while three-quarters of all "stranger" crimes were recalled. Eleven of the fifteen known rapes which went unmentioned involved non-strangers.

Almost an identical pattern was uncovered in a record-check study of the validity of survey reports of assault conducted by Statistics Canada. They found that 71% of stranger assaults were recalled, but only 56% of "known party" assaults and 29% of related-party were recalled (Catlin and Murray, 1979: table R). Those figures are extraordinarily similar to findings from the San Jose record check. There are competing explanations for this phenomenon. Victims may not remember disputes which arise within kinship or friendship circles as readily as they remember events involving strangers. Or, such disputes may not register as the kind of incidents that the interviewer is looking for – they may not be construed as crimes. People may think that to be a "crime" violence must involve strangers. However, these alternatives seem unlikely, for these incidents all were "founded" by the San Jose police.

It may be that persons who have been victimized by someone they know frequently may not think it is any of the interviewer's business. Or, the survey may raise again the memory of a painful situation, one which victims may not wish to recall. Although these all were incidents which came to the attention of the police, the victim may not have been the party who called them; many crimes are reported to police by friends, relatives, and bystanders, and the offended party may not wish to spread the story even further. Finally, in related-party cases the question of who is to blame and who is the real victim is not always clear, and the role of the person being interviewed might not always withstand close scrutiny. It is possible that an interview with any of the participants in these affairs could have recorded what appeared to be a victimization.

Victims who are themselves culpable may also be motivated to suppress information about criminal incidents. Research on crime indicates that "victim precipitation" is a common phenomenon in violent crime and in incidents where the victim knows the offender. In those incidents it is the eventual victim, rather than apparent offender, who first initiated the event. Other crimes may be encour-

aged or facilitated, if not caused, by citizen behavior. Biderman's (1967) survey in Washington, DC, dealt in passing with this problem. There, 25% of all victims agreed that they were negligent or had done something foolish which contributed to their plight. Victims who feel culpable may be less likely to report their experiences later in an interview.

Forgetting

Most research on nonrecall has focused on what is assumed to be true forgetting. The problem has been described variously as "time-dependent error" and "memory decay", for it appears that the difficulty is one of remembering incidents from the more distant past. At one time it was assumed that crimes were very memorable events; it was planned to use retrospective surveys of the general population to reconstruct an historical time series for victimization rates, using interviews with a life-long reference period. Pretest quickly demonstrated the futility of that enterprise. Rather than being readily memorable, Biderman *et al.* (1967:31) found:

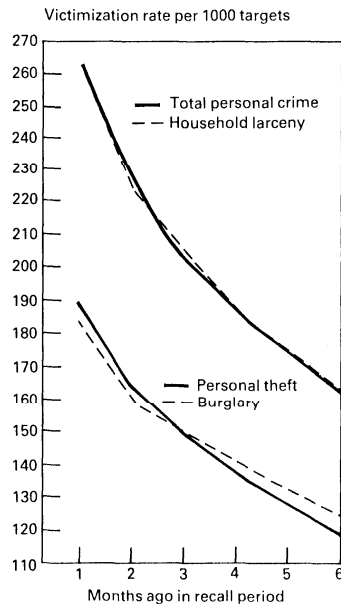
In practice, most respondents seemed to find it difficult to remember incidents of victimization other than recent cases. ... People reported hours, days, and even weeks later that incidents they had not remembered at the time they were interviewed had come to mind subsequently.

In the Washington, DC, survey, respondents were asked to recall the "worst crime that has ever happened to you." They recalled a total of 260 incidents in response, only 108 of which occurred more than 2 years previously, and only 60 of which happened 6 or more years in the past (Biderman *et al.*, 1967:41). Biderman *et al.* (1967:40) noted:

Respondents have to do a great deal of thinking and slow reflection before they can remember even fairly serious crimes of which they were victims some time ago – even when these older incidents are far more consequential than recent ones.

The National Crime Survey now inquires only about what has happened "in the last 6 months."

The fact that victims forget about their experiences with the passage of time also has serious implications for the accuracy of estimates of victimization rate based on surveys. This is illustrated in Figure 4.1 which shows how different estimates of victimization would be if they were calculated on the basis of crime which were described by happening 1 month ago, 2 months ago, etc. If we used crimes described as occurring only 1 month ago, we would find that the national rate of victimization from personal theft was 189 per



SOURCE: Data from Woltman, Bushery and Carstenson, 1975: table 2.

FIGURE 4.1 Rates of victimization reported for months in recall period

1000, and for all personal crime 261 per 1000. However, with increasing lengths of recall those estimates would have dropped sharply. Based on incidents recalled for the sixth month before the interview, the corresponding rates would drop to 119 per 1000 for personal theft and 162 for all personal crimes.

As we shall see below, not all of this gradient can be attributed to the forgetting of past incidents. It is also shaped by forward telescoping. However, decreases in rates for personal crime and burglary of nearly 100 incidents per 1000 over a 6-month recall period clearly signal trouble. As we saw with regard to the 1971 Quarterly Household Survey, the problem is even more extreme in 12- as opposed to 6-month recall periods, and this doubtless affects the yearly victimization estimates produced in the city surveys conducted between 1972 and 1975.

Similar declines in recall with the passage of time can be observed in data from record-check studies. In record checks, samples of cases of different "ages" are drawn from police files. Interviews with victims are employed to determine if those from the more distant past are less likely to be recalled. Record checks are more definitive studies of the forgetting problem because other factors which affect the distribution of data such as that in Figure 4.1 are not present. Table 4.1 summarizes the findings of the San Jose record check. It indicates the proportion of incidents that was remembered by victims in light of the number of months of recall they required. As Table 4.1 indicates, recall was relatively high for cases from 1 to 3 months in the past, but it hovered around only 50% for those from 4 to 9 months in the past, and then dropped below one-third for those from nearly 1 year in the past. As the author of the San Jose report noted, based on

TABLE 4.1. Record-check recall, by months of recall demanded

Months between interview and incident	% recalled	(N)
1-3	69	(101)
4-6	50	(100)
7-9	46	(103)
10-12	30	(90)

SOURCE: Turner, 1972a:8.

this criterion" ... there is very little to choose from after the first three months." (Turner, 1972a:8).

Declining rates of recall with the passage of time also were noted in earlier US Census Bureau record checks in Washington, DC, and Baltimore, although patterns in the Washington study were less clear-cut (Dodge, 1970). In Baltimore, levels of recall were much higher than in San Jose, averaging 81%, but evidenced a steady decline with passing months (Yost and Dodge, 1970). On the other hand, Sparks *et al.* (1977) found high rates of recall (averaging 92%) and only a slight decline in that rate over a 10-month period.

Inaccurate or Incomplete Recall of Incidents

The failure of respondents to share information about events which apparently did involve them is not the only type of error encountered in data gathered in interviews on crime victims. Information which is volunteered may be incorrect or at least different from that gathered on the same incident from other, presumably more reliable, sources. Victims make mistakes: they may inaccurately recall the amount lost in a crime or the exact date of the incident. In their London study, Sparks *et al.* (1977) compared the month in which victims placed incidents with police information on the same offenses. They found that only 55% recalled the month of the offense with accuracy. Victims may also deliberately misconstrue their role in a crime, the value of a stolen object, or the identity of an offender. This may be common in crimes that involve close victim-offender relationships, victim complicity, or victim precipitation. The police often suspect the motives of complainants, and so might survey interviewers. Record-check comparisons of archival and interview data indicate that at least two types of recall error present a serious methodological problem: temporal telescoping and misreporting. There can be a great deal of disagreement, some of which appears to be time dependent, about the characteristics of offenses and offenders between these two data sources.

Telescoping

The issue of temporal telescoping has received a great deal of attention, because it has profound implications for survey design and

cost. In an early study, Gray (1955) conducted a record check of reports of sick leave by British civil servants. He found that few forgot completely that they had taken leave, but that there was a substantial tendency for them to err in recalling when they took it. Neter and Waksberg (1964) investigated telescoping problems in self-reports of household repairs. They found that recall error was predominantly in the forward direction, moving events closer to the date of the interview. They also discovered that major expenditures – which presumably would be more memorable – were more likely than minor expenses to be telescoped forward. Because minor expenditures also were more likely to be forgotten, the error structure of the US Census Bureau's self-report data on household repairs was very complex.

Although telescoping is found in many retrospective surveys, it is not altogether clear why it takes place. It may partly be due to the demand characteristics of a victimization survey. In this case, the "demand" to produce an incident occurs because most respondents have not been victims of most of the crimes covered in the interview. The long incident screen questionnaire produces a succession of "no" responses, and respondents may feel the interviewers are "disappointed" by their lack of productivity. In this situation, the temptation to give the interviewer some false but apparently satisfying information may be overpowering, especially when a familiar but slightly out-of-bounds incident comes to mind (Biderman, 1970). There is also some evidence that frequent and recurring events are telescoped more often, for there is a greater likelihood that the respondent will become confused about their dates (Sudman and Bradburn, 1974). The "interview demand" hypothesis does not explain, however, continued forward telescoping even within the reference point for a survey, a phenomenon noted by Neter and Waksberg (1964) and in all the victimization record checks.

For purposes of making accurate estimates of victimization rates for a calendar period, any disposition by respondents to draw into the reference period events which took place before (or after) is more threatening than errors in time placement within the period. The more threatening phenomenon is known as "external telescoping." Various survey techniques have been developed to deal with this problem. One solution has been to "bound" surveys conducted for estimation purposes by an earlier interview. The bounding interview, which takes place at the beginning of the reference period, gathers reports of prior incidents and serves as a benchmark for the ensuing

timespan. Interviews conducted at the conclusion of the reference period presumably are then protected from forward telescoping. In addition, incident reports gathered in the initial interview can be used to screen later interviews to eliminate duplications. Another aid to recall is to shorten the length of the reference period and to locate its terminal point as close in time as possible to the date of the interview. This increases recall accuracy (the demand for details about temporally distant events is eliminated) and limits the scope for backward telescoping. The trade-off, of course, is cost. Finally, external telescoping can be reduced by "bounding" the beginning of the reference period with a salient date. During interview pretests for the National Crime Survey it became apparent that people had difficulty locating events in time because of the absence of salient reference points. They appeared to remember incidents which occurred in January more frequently than many other months because they "came just after the first of the year" (Yost and Dodge, 1970). Interviews which refer to reference periods with natural boundaries marking their beginning and end seem to be more satisfactory.

The effects of external telescoping on victimization rate estimates can be considerable. In the 1970 Washington, DC, record check, for example, some individuals were selected for interviewing because police files indicated that they had been victimized 7 months before. They were asked only about their experiences during the "past 6 months." About 15% of those out-of-bounds incidents were pushed forward into the reference period. Over 20% of a sample of 13 month-old cases were incorrectly placed within a 12-month reference period by another group of victims (Dodge, 1970). In July 1971, the issue was investigated experimentally. A victimization instrument was administered to 18000 participants in the US Census Bureau's Quarterly Household Survey, 12000 of whom had been interviewed about crime in January of the same year. The survey asked about their experiences "in the past 6 months." In every crime category, the 6000 respondents whose interviews were unbounded reported more incidents than those who had been questioned before. The ratio of unbounded to bounded reports ranged from 1.2 to 1 for burglary to 1.9 to 1 for robbery. This was roughly the same magnitude of error due to telescoping in Neter and Waksberg's (1964) comparison of bounded and unbounded reports of household repairs: Their unbounded interviews yielded 40% more reports of expenditures.

The design of the National Crime Survey facilitates comparisons between bounded and unbounded interviews on a continuous basis.

There are differences between estimates of the national victimization rate for several crimes based on interviews conducted for repeat, bounded samples and new, unbounded samples for a reference period. New, unbounded households entering the survey report more instances of victimization than those which were already part of the sample used for estimation purposes; in the aggregate, the difference in rates was about 33%, a very substantial discrepancy attributable to this single methodological difference. In this regard it is also important to note that the city victimization samples interviewed by the US Census Bureau were unbounded. The interviews conducted in 26 cities between 1972 and 1975 employed 12-month unbounded reference periods. In eight of the cities, the reference period also did not refer to a calendar year (January through December), which probably further reduces the quality of the data. We do not know enough about the consequences of this to predict its impact on other measures. If more serious incidents were telescoped into the reference period (Reiss, 1978) while less serious ones were more rapidly forgotten (Neter and Waksberg, 1964), the relative mix of crimes as well as rates of victimization would be affected. On the other hand, external telescoping should have proportionally less of an impact on reports gathered for a 12-month period than it does on the unbounded components of the National Crime Survey with its 6-month reference period. Respondent fatigue and forgetting should be greater over the longer span, however.

In addition to reconceptualizing bounding procedures, research on the telescoping process should focus on internal telescoping effects and on the correlates of telescoping itself. We know little about *why* events are telescoped or about their differential misplacement in time. In her record check in Portland, Schneider (1977) examined the kinds of events which were most severely moved about in time. Her survey employed a 12-month reference period. On the average, matched incidents were pulled forward within the period by 2.2 months. Forty-nine per cent of all incidents were placed in the wrong month by their victims. She found a weak tendency for more trivial incidents to be telescoped forward more often, and for events which occurred more distantly in the past to be pulled forward more frequently. Also, crimes in which the victim reported resisting the offender were often misplaced in time. However, the tendency to move events forward in time was not related to the age, race, sex, or educational level of respondents. Telescoping within a reference period presents analytic difficulties, for it impedes our understanding

of the timing and sequencing of criminal incidents. Even within the 6-month reference period currently employed in the National Crime Survey, survey incidents apparently are being pulled forward in dramatic fashion. Twenty-eight per cent of all incidents now are being placed in the first month of any recall period, four times as many as in any last month (Reiss, 1978). This destroys the utility of the data for examining issues such as the sequencing of multiple victimizations or the impact of recent experiences with crime on victim's willingness to resist another attack or to report ensuing incidents to the police. Without accurate data on the temporal placement of incidents we cannot link them in causal fashion to other events, such as quitting a job, moving to another address, installing a crime-prevention device, or getting a divorce. To document the causes and consequences of crime at the micro-level we need accurate data on the relative time placement of many events in people's lives, including victimization.

Other Sources of Measurement Error

Research on inaccurate recall has focused almost exclusively on the time placement of individual incidents. However, there is reason to suspect that victims are likely to recall inaccurately other aspects of events. Record checks which match significant characteristics of incidents between police files and victim interviews would shed a great deal of light on the general reliability of the data collected in the surveys. The only record check of the characteristics of incidents that has been made by the US Census Bureau focused on differences in estimates of dollar losses between victims and the Washington, DC, police. That comparison revealed that citizens made substantially higher estimates of value of their stolen and damaged property than did the police. Three-fourths of the loss estimates gathered in interviews were higher than those recorded by the police, often by 50 to 100%. On the other hand, there was no indication that these differences were time dependent or that the dollar amount of a loss affected the accuracy of its recall (Turner, 1970). In the Portland record check, Schneider (1977) compared police and interview data on a variety of incident attributes. She found that survey estimates of loss and seriousness consistently were higher than police figures. Victims were much more likely than the police to mention that weapons were involved in a case. Police reports and victims also disagreed much of the time on the race of the offender and, as noted

above, on victim-offender relationships. Victims also reported substantially longer response time by the police than official records indicated. On the other hand, there was a good match for such factors as the age and sex of suspects and the number of offenders involved in the incidents. Interestingly, these mismatches were not consistently related to the passage of time. Some incidents were from twelve months in the past, yet none of the errors in those comparisons (scored as measures of the difference between victim and police reports) was time dependent. Also, the passage of time was not related to the tendency of the victims to give "don't know" responses to questions about their experiences. Only knowledge of the date of the incident seemed to fade with time. It would seem that the criterion of accuracy employed in survey pretest record checks was the most stringent of choices.

Differential Productivity of Respondents

Research on survey methodology indicates that respondents differ in their willingness or ability to adopt a productive role during an interview (Sudman and Bradburn, 1974). In general, more highly educated respondents are more co-operative, more at ease in interview situations, and more able to recall the details of events. Those factors may affect the accuracy with which victimizations are recalled during interviews.

As I noted above, it is assumed that most forms of criminal victimization are more frequent among lower status persons. However, surveys conducted for the Crime Commission found victimization to be positively related to measures of social class. The strongest social class correlate of victimization was education. College-educated respondents recalled victimizations at a higher rate than did others. This surprising pattern may be due to differing definitions of "victimization" and attendant variations in the probability that events will be recalled in an interview. Or, that significant negative associations between social position and victimization may be masked in survey findings by greater interview productivity among more highly educated and testwise respondents. Higher levels of education (but not income) measure entry into a "test and measurement culture" in which surveys, questionnaires, and opinion polls are recognizable features of life. In addition, more educated respondents may enjoy greater verbal fluency of the kind necessary for conducting a

bureaucratic encounter, and they may generally be more inclined to trust the stated intentions of inquiring government agents. Interviews with such respondents should be less perfunctory, involve greater task comprehension, and elicit more effort in completing the task than those with less comprehending or less able respondents.

There is little evidence supporting either of these explanations for a positive education-assault victimization relationship. In England, Sparks *et al.* (1977) found that among upper class respondents victimizations which were recalled were more likely to be trivial ones, or attempted rather than successful crimes. Similar findings have been reported for Germany (Stephan, 1967) and the United States (Biderman *et al.*, 1967). In the National Crime Survey those proportions fluctuate considerably among those with lower levels of educational attainment, but are by far the highest for those with college training. In data collected during the first 6 months of 1977, 63 % of all college-educated assault victims fell in the "attempted assault without a weapon" category; for everyone else that figure was 49% (author's computation). The only other evidence that differences in the ability of victims to complete the interview task are affected by education was reported by Reiss (1978). He found that less educated respondents were more likely to recall incidents that fell into the "series" category, which is composed of crimes for which discrete details could not be remembered. On the other hand, Schneider (1977) found in her record check that education was not related to any tendency of self-identified victims to give "don't know" responses or to systematic differences between police reports and interview data on incidents. Based on this evidence, it seems that productivity effects are more likely to be of the "recalled or not" variety and thus at work only in the screen section of the survey instrument. It remains unclear why nonrecall error should be distinct from errors in the detailed incident descriptions gathered in the incident report section of the instrument.

PROCEDURAL ISSUES

Telephone Versus Personal Interviews

While the National Crime Survey is described as a personal-interview study, a substantial proportion of interviews are conducted via the telephone. In NCS, contact with a sample household is initially

established by a personal visit by an interviewer. During this visit an interviewer lists each household member; at that time he or she also interviews all available respondents. However, the interviewer exercises discretion about whether to complete the remaining interrogations by other personal visits or by telephone, and is to choose the easiest and most cost effective method (US Census Bureau, 1979).

We do not have a reliable reading of the consequences of this procedure. Some comparisons of the results of interviews conducted personally and by telephone indicate that there are few differences between them. Comparisons of parallel surveys that have been conducted using the two methods sometimes indicate a similar equivalency, but sometimes favor one of the interviewing modes. No truly definitive experiment has been conducted detaining the consequences of mode of interview for data on victimization. Research on related topics also provides no clear lesson for victimization surveys. There are reasons to suspect that telephone interviews may be less productive than those conducted in person, and there are counterarguments which support both conclusions.

It is widely argued that surveys of the general population achieve higher completion rates when interviews are conducted in person. Because the US Census Bureau pursues a mixed-mode data collection strategy to pursue individual noncompletions, it is impossible to talk about the relative effectiveness of each in the crime surveys. In its Health Interview experiments, the Survey Research Center of the University of Michigan found a 10% difference between both completion rates and refusal rates which favored the in-person strategy (Cannell *et al.*, 1979). On the other hand, the Center achieved virtually identical results in another study of the two techniques (Groves, 1979). Those who favor personal interviews also argue that "data are better" when collected in that way because of the greater rapport that can develop between interviewer and respondent. Also, in intimate settings, interviewers can supply more verbal and nonverbal cues to shape respondent behavior, and both parties may be more satisfied with the emotional rewards of face-to-face contact. Comparisons by mode of interview indicate that respondents and interviewers are less satisfied with telephone interviews (Groves, 1979; Cannell *et al.*, 1979). Respondents tend to supply less detail in response to open-ended questions given over the telephone (Groves, 1979). They also are more likely to evidence response-set bias, using the same verbal category in answer to a string of questions more frequently when interviews are conducted by telephone (Groves,

1979). It also seems that respondents in telephone surveys are less certain of the sponsorship of those studies or of the use to which the data will be put. Rodgers (1976) and Groves (1979) both found they were less likely than those being interviewed in person to supply sensitive personal information such as family income. In the Groves study, telephone respondents also were more likely to report that they felt "uneasy" discussing selected topics.

Vigorous arguments can be made in support of telephone surveys as well. Some have argued that telephone interviews may be more productive because they are anonymous. On the telephone it may be possible to be more candid and matter-of-fact about embarrassing issues, and it may be easier for respondents to admit less desirable behavior. In a record check of the two modes of interviewing, Rodgers (1976) found that telephone reports of whether or not respondents were registered to vote were more accurate. On the other hand, Groves (1979) found no difference between the two approaches in terms of the social desirability of responses to various measures. Telephone interviews may often be more discrete, for other members of a household usually cannot hear the questions asked. Because the work is conducted at a central site, telephone interviewing can be better supervised than can field visits. As a result, interviews may be more standardized. Rogers (1976) found that interviewer styles were more uniform and interviewer effects on data were less pronounced over the telephone, and that across two waves of interviews responses by members of her telephone panel were more consistent. Interviewers are undeniably safer in telephone surveys, often a significant concern. Groves (1979) also found that completion rates for urban areas were higher for telephone than in-person surveys.

Evidence on the relative validity of data gathered in each is important, for the mixed-mode data collection strategy employed in the crime surveys is distributed in a decidedly non-random fashion. During the first few years of the National Crime Survey about 25% of all interviews were conducted over the telephone (National Research Council, 1976), and persons who were interviewed by telephone were more likely than others to be young, male, and black. All other evidence suggests that these are among the most likely groups to be victimized. If telephone interviews are not as productive of reports of victimization as those conducted in person, the resulting rate estimates will be severely affected.

There have been four major studies of the problem in the context of measuring victimization. While they came to somewhat different conclusions, the best of them supports the use of the telephone. In the first study, the results of interviews with households in which maximum effort was made to employ only personal visits were compared with those in which telephones were used whenever feasible. No major difference between victimization reports from the two samples were apparent (Turner, 1977; Woltman and Bushery, 1977b). In another analysis, Tuchfarber and Klecka (1976) contrasted the results of parallel victimization surveys. They uncovered more victimizations over the telephone than the US Census Bureau measured in person. However, Reiss (1978) analysed several years of national data organized in panel fashion and controlling for a host of other factors – found that telephones were 50% less productive than personal interviews.

The results of this research can only be labeled inconclusive. Rodgers (1976), Groves (1979), Klecka and Tuchfarber (1974), and others agree that there were few differences in relationships between variables gathered in differing fashion. The problem seems to be one of threats to the precision of estimates of the number of victimizations and other objects of interest for the population as a whole. In 1966 Biderman *et al.* (1967) attempted to use the telephone to gather victimization data, but quickly abandoned the technique as inadequate. Since most recent and systematic evidence on the issue is ambiguous, additional research should be conducted if only because of the very large contingent of telephone respondents in the current crime panel. In the US Census Bureau project, many persons in each "experimental" condition actually were interviewed by another mode. In the Klecka and Tuchfarber (1978) study two different organizations had conducted the parallel surveys and different sampling frames were employed for each, leaving room for a host of differences between the surveys in addition to the way in which the interviews were conducted. Reiss' data are correlational and suffer from a lack of random assignment of mode of interview. Clearly an experiment is called for in which samples of individuals would be randomly assigned to groups and interviewed in different ways, in conjunction with a record check to provide an independent reading of what their responses "should be."

This design was employed by Statistics Canada in a major methodological study of the validity of survey reports of victimization

(Catlin and Murray, 1979). They sampled 1525 crime victims from the files of the Edmonton, Alberta, police department. Victims were randomly assigned into two groups, one to be interviewed only by telephone and the other only in person. An additional random sample of adults was drawn from the city directory and mingled with the two victim samples in order to disguise the true purpose of the survey from the interviewers. This insured that some respondents would have no victimizations to report. Parallel surveys were then conducted to assess the completion rates, cost, and accuracy of recall associated with each survey method. Statistics Canada found that telephone interviews were significantly less expensive to conduct – including an allocation of administrative expenses and other overhead costs, telephone interviews cost 70% less than in person interviews. Telephone interviews also were as successful as in-person efforts at reaching respondents; the completion rates of the two parallel surveys were quite similar. Finally, there was virtually no difference between the two surveys in the proportion of known victimizations which were successfully registered in the interviews. The in-person interviews recovered 64% of the criterion incidents, and the telephone interviews 63%. As a result of this experiment, Statistics Canada employed telephone interviews in its large-scale 1979 study of criminal victimization in Vancouver, British Columbia, and in its 1982 surveys of five major cities.

Interviewer Effects

In addition to panel artifacts and biases related to mode of interviewing, differences among survey interviewers in the way they carry out their task also shape resulting data. Interviewer effects are but one of several sources of “correlated response variance” (Bailar, 1976). These effects manifest themselves as variance on indicators which is shared among respondents who were quizzed by the same interviewer. The effects of interviewers can be quite substantial, especially when survey personnel have had comparatively little training and are only minimally supervised. Interviewer effects reached epidemic proportions in the 1960 Census of Population, a technical rationale for accepting a cost-cutting move to self-enumeration by use of a mail survey in the 1970 Census. In the city victimization surveys conducted by the US Census Bureau, interviewer effects were comparable in magnitude to sampling error. For example, for Baltimore it is

necessary to multiply estimates of sampling variance by 1.60 to calculate confidence intervals which take account of both sampling and interviewer variance. The estimated rate for all victimizations there was 110 per 1000, with a sampling-error range (with 95% confidence) around that estimate from 40 to 180 per 1000. Taking into account the effects of correlated response variance extended that range to from 20 to 200 per 1000. Those differences become even more extreme when we examine particular crimes (Bailar *et al.*, 1977).

The sources of interviewer effects are numerous. Interviewers differ in how they interpret individual survey items and in their understanding of the purpose of the enterprise. Some probe for detailed comments more vigorously than do others, and some interviewers readily accept “don’t know” and other nonresponses. Interviewers also differ in how they interpret and record responses to questions and how they explain individual items to respondents who do not understand them. Often they do not link the verbal and nonverbal cues they give respondents to any productive effort on the respondent’s part, thus rewarding unacceptable task behavior (Canell *et al.*, 1979).

Examination of the types of incidents for which interviewer effects are most substantial suggests that they involve the particularly sensitive topics probed by the victimization surveys. The most systematic analysis of those effects indicates that they are greatest for crimes in the “assaultive violence without theft” category – that is, for rapes, intrafamilial disputes, and public brawling (Bailey *et al.*, 1978). Dodge and Lentzner (1978) noted that reports of series incidents often are first recorded when a new interviewer takes responsibility for a household. Presumably some interviewers are better than others at eliciting reports of “conditions” rather than events, while others more quickly tire of attempting to untangle vague or complex incidents.

Precise estimates of the magnitude of interviewer artifacts in the data are based on “interpenetrated sample” research. In each of the eight cities studies by the US Census Bureau in 1975, interviewers were assigned batches of 80 sample households. A portion of these were randomly assigned from a pool of households distributed between pairs of interviewers. Then, comparisons were made in the data collected from these households, examining contrasts among interviewers. The analytic question was, How much of the observed variance in reports of victimization could be attributed to interview-

ers rather than to sampling variance and the true distribution of crime (Bailey *et al.*, 1978)?

There was considerable disparity in reports of victimization among interviewers, between interviewers assigned to the same supervisor, and across cities. Interviewer effects were most extreme in Newark, where it is necessary to multiply estimates of sampling variance by 2.4 to take this additional source of error into account. Interviewer effects were most extreme for assaults and petty theft. Hearteningly, they were not linked to the attributes of the respondents themselves (Bailey *et al.*, 1978). As a result, such effects will have fewer consequences for tabulations of relationships in the data. Also, the impact of interviewer variance on a set of data goes down as the number of interviewers in a study increases and the average number of respondents each one deals with decreases. Thus interviewer effects are much less significant in National Crime Survey (Bailar *et al.*, 1977). Conversely, centralized telephone surveys typically employ fewer interviewers, and the impact of differences among them will thus be more substantial.

SOME RESEARCH ISSUES

Because they refer in principle to objectively observable events, methodological criticisms of victimization surveys impose a strict standard upon the data, that of criterion validity. Validity is a question of the relationship between two distinct measures of the same variable; if different nonsurvey measurement procedures identify (in this case) the same events or victims we are more confident that the data are not artifactual, generated by the measurement process itself. However, few of the measures commonly employed in survey research have any known validity. Survey data which are good by standard of the profession usually display, at most, internal consistency, and are related in expected ways to benchmark attributes or attitudes of the respondents. The validity of self-reports even of simple behaviors is often quite low: claims about having voted often are inflated by 10 or 20% (Traugott and Katosh, 1979; Clausen, 1968; Weiss, 1968). In one study 47% of the sample misrecalled whether or not they gave money to a Community Chest drive (Cahalan, 1968). Any socially approved behavior will be claimed by more persons than actually practice it. Biderman and Reiss (1967) summarized a study which reported that 30% of a sample of persons

known to have visited a doctor within 2 weeks prior to the interview failed to report the event, and that 7% of a sample of recently hospitalized persons exhibited similar lapses in memory. Forward and backward telescoping affect the report of vacation and sick leave and self-reports of household expenditures.

The most useful position on all of this is that data always contain errors. Data are indicators of the relative distribution of some conceptual variable across a population. The numbers themselves only partially reflect that distribution (their true score component). They also are partially artifacts of the measurement method (their method component), and they are clouded by random noise from a variety of sources. In dealing with data the questions always are: Is the error component of the data truly random with respect to the relationships I am investigating? Am I being led astray by misinterpretations encouraged by method effects? The more we know about a set of data, the more confident we can be when we answer these questions.

One of the largest problem areas in the measurement of victimization is assault. While the complexity of victimization survey data demands that we interpret all of them with care, the methodological shortcomings of the enterprise seem most to affect reports of interpersonal violence. We have seen in record checks that many assaults are not picked up in personal interviews, even when they have already been reported to the police. In the Baltimore method test, only 36% of all assaults were recovered in the interviews, and less than one-half of those were placed in the correct month by their victims (Yost and Dodge, 1970). In San Jose, 48% of the victims of assault recalled the event, but that percentage dropped to 22% among those who were victimized by acquaintances or members of their own family (Turner, 1972a). It was apparent in the pretests that the interview process was not eliciting thorough accounts of interpersonal violence and that the problem was acute in the case of non-stranger assault. We also have seen the unexpected relation between education and reports of assault victimization, a relationship which leads us to suspect that more educated respondents are most likely to remember such offenses, to define them as crimes, or to cooperate in their reconstruction in interviews. Series victimizations, which ordinarily are excluded when data from the National Crime Survey are analysed for official reports, almost all involve assaultive violence. This leads to the severe undercounting of assaults and greatly reduces the apparent frequency of multiple violent victimiza-

tion. Because series offenses are more likely to be reported by less educated respondents, they further cloud the relation between education and victimization violence. Interviewer effects also hit hardest at events in this category, overwhelmingly in the direction of undercounting them (Bailar *et al.*, 1977; Graham, 1974). Finally, panel attrition also probably disproportionately reflects assault victimization. Especially when the incidents involve neighbors or related parties, violent assault should propel victims to seek refuge in other domiciles. These assault-linked shortfalls in the data seem to be reflected in a number of puzzling aspects of victimization research. They undoubtedly account for the high proportion of assaults attributed to strangers in the national panel and in the city studies. Across the 26 cities where surveys have been conducted, an average of 70% of all interpersonal violence was attributed to strangers; in the 1973 national data the figure was 60% (author's computation). There are several other method artifacts which shape data on aspects of assault victimization. Method artifacts probably account for the fact that in the National Crime Survey data victim-offender relationships do not appear to affect the rate at which victimizations are reported to the police. This is a surprising – and puzzling – finding. Given the low proportion of violence within close interpersonal networks which surfaces in the survey data, that which does is probably of such a character that it is also readily reported to the police. Second, the relative dearth of non-stranger offenses in the data undoubtedly increases the proportion of assaults and rapes which was reported to have involved victims and offenders of different races. To the extent to which people are likely to know or live near persons of the same race, the underestimation of violence among acquaintances will skew the data in favor of interracial crime. Given the potentially unsettling social consequences of high levels of interracial crime in America, artificially high reports of that rate are unwelcome. Error in the measurement of interpersonal violence which is related to the differential productivity of respondents may account for the observation that blacks recall far fewer reports of minor assault than do whites. The most trivial form of violence in the crime survey is “attempted assault without a weapon,” which includes incidents which resulted in no injury and in which no weapon was brandished. That a crime even occurred is inferential, and most of these events may better be described as threatening encounters. There is no particular reason to expect blacks to experience fewer of these episodes than whites; in fact, given what we know about class- and

culture-linked youthful exuberance it is more plausible to expect the opposite. Yet in the 1973 data, fully 47% of all the assaultive violence recalled by whites fell in this category, while only 31% of the assaults reported by blacks were trivial. Almost 60% of all black assault victimizations were categorized as aggravated assault (involving serious injury or the use of a weapon) while only 37% of all attacks on whites were aggravated. This is highly unlikely. Much more plausible is the hypothesis that blacks reported fewer of their less threatening encounters, while whites dredged up everything in memory. Further investigations of the correlates of respondent productivity are required before we can make any confident statements based upon much of the data on assault.

The hypothesis that the experiences of blacks are substantially undercounted in the victim data account for one of the most anomalous patterns appearing in the city data when the data are used in aggregate form. At the city level, using the 26 surveyed communities as the units of analysis, we find that one of the best predictors of rates of violence is “% white.” The higher the proportion of the population which is white, the higher the rate of interpersonal violence. Because data on race are highly correlated at the city level with other social indicators, including those measuring the extent of poverty, educational failure, and the quality of life, we also find that high rates of interpersonal violence are positively associated with good housing, low population density, high income, and high levels of formal education. This is quite unlikely. It seems rather that white communities were represented by samples of white respondents, and that they produced more exhaustive reports of events and a more thorough recounting of essentially trivial events.

There are also problems inherent in the three techniques which have been used in the past to conduct methodological research on victimization. An ideal research design to tackle the problem of better measuring assault has not yet been fielded.

A simple examination of data gathered in a victimization survey often is revealing of substantial methodological problems. For example, comparison of victimization rate estimates by month or quarter across the length of the recall period of a survey always reveals that reports of offenses are more frequent in months closer in time to the date of the interview. In January 1971 a segment of the sample for the US Census Bureau's Quarterly Household Survey was asked about crime experiences for 1970. Of the personal crimes that were recalled by victims 80% took place (as they were remembered) during the last

6 months of the period (Turner, 1972b). The same "bunching" of events in more recent months has been observed in Germany (Schwind *et al.*, 1977). This does not reflect the true distribution of crime.

However, a great failing of inferential analysis of existing data is that we can only guess what might be error and we can only infer what the causes of error are. We recognize the excessive clustering of crime because we generally understand the seasonal pattern of offenses, and we presume that it occurs because victims forget more easily incidents which happened further in the past (actually, that is only partially responsible for the observed clustering). Many people have been puzzled by the positive relationship between education and assault rates revealed by the National Crime Survey. The data seem wrong; on the other hand, we collected the data because we thought that the base of our knowledge of victimization was inadequate. We often do not know enough about crime to recognize to what extent an observed distribution is affected by methodological factors.

The second widely used technique for exploring methodological quirks in victimization data is the parallel sample experiment. Alternative forms of a questionnaire can be administered, or different procedures can be utilized across groups, to explore the consequences for the data that are collected. Strictly speaking, this is an experiment only when assignment of membership in those groups is random. Perhaps the best example of an experiment supporting the development of the National Crime Survey was that conducted in 1971 in Dayton and in San Jose. The issue being explored was the relative advantage of gathering victimization data through interviews with every member of a sample household as opposed to interviewing a household informant. In each city half of a household sample was completely interviewed, while in the remainder only an informant was questioned. Not unexpectedly, the former procedure produced more reports of victimization (Kalish, 1974).

The experimental nature of these investigations lends a great deal of credibility to their findings, due to the power of random assignment. There are weaknesses in this experimental approach, however. The criterion by which the "better" method or procedure is to be chosen as a result of these studies is unclear, and in the end the decision always depends on an argument based upon other information. It has usually been assumed that "more is better," that the procedure which produces the largest number of victimizations is more accurate. Tuchfarber and Klecka (1976) argued that their

telephone survey procedure was better than parallel personal interviews because they uncovered more reports of victimization. However, this is not an unambiguous criterion. In addition to the absence of the criterion, these studies are limited by the large number of interviews which are required to test conclusively the effects of methods or procedures. The relative infrequency of crime means that either very large samples or very large method effects must be involved if a cross-sample difference is to be significant. In a proxy interview study conducted as part of the San Francisco City Victimization Survey, only 570 persons 12 or 13 years of age lived in the 9778 households in the sample (Cowan, 1976). There were differences in rates of personal victimization reported directly by youthful respondents rather than by their parents. However, the interviewing experiment was far from definitive due to the large sampling errors involved.

A third procedure for identifying methodological weaknesses in report of victimization is the record check. The US Census Bureau conducted three record checks while developing the procedures employed in the National Crime Survey (Yost and Dodge, 1970; Dodge, 1970; Turner, 1972a). In each study, samples of incidents were drawn from police files, and interviewers were dispatched to quiz victims named therein. The data gathered in these interviews were compared to the official records. Two questions were examined: "Did the victim recall the incident for the interviewer?" and, "Did the victim accurately identify the month in which it occurred?" Record checks thus documented the recovery power of the survey and the validity of the dating of incidents. They were used to test successive improvements in the survey's instruments and the length of the recall period that respondents could be expected to report upon accurately. (These record checks are reviewed in detail in Sparks *et al.*, 1977, and Hindelang, 1976). The record-check approach to the validation of reports of victimization is potentially a powerful tool for methodological research. However, the credibility of the findings depends in large measure on three assumptions: that the record employed in the comparison contains the correct view of the event, that the findings of record check studies can be extended to cases in which no record was generated, and that problems in fielding such studies do not influence their findings. The first difficulty with record-check validation is the assumption that a police file is a useful criterion for judging the veracity of victims' reports of their experiences. It has been assumed that the detail employed in the US

Census Bureau's validation studies – the month in which the incident occurred – was correctly reflected in police records. While this seems to be reasonable, the assumption that the police and victim necessarily would classify an incident into the same analytic category, or would interpret the event in similar fashion when making their respective reports, probably is not. There can be pressure on both the police and the victim to recast events. The record check in San Jose suggests that attempted rapes and assaults were particularly prone to differential classification (Turner, 1972a). Further, Schneider's (1977) record comparison found a great deal of disagreement between the record of the Portland police and victim descriptions of key elements of event. For example, on the question of whether or not the victim and offender were known to one another prior to the incident, the two reports agreed only 56% of the time. From the victim's point of view it also seemed that the Portland police were prone to classify assaults in less serious, Part 2 categories. In neither case is it clear that the police were correct in recording the crime. It should be noted that data from police records and police decisions regarding an incident may reflect information gathered from a variety of sources other than the victim, including their own observations and reports of witnesses. This is another reason why details about events drawn from the two sources may not always be in agreement.

A second difficulty with the record-check approach to validation of incident reports is that it is limited to incidents which somehow came to the attention of a recordkeeper. The victimization surveys themselves suggest that only 50% of all serious crimes are reported to the police. Reported crimes are systematically different from unreported incidents, principally in terms of their seriousness (Skogan, 1976a). In general, record checks have been conducted only on crimes which are more serious and which are reported, investigated and recorded by the police. Those crimes certainly should be most vividly remembered by victims. Biderman (1971) has noted that we should expect:

... poor recall of victimization for the type of unreported incident where the victim sees nothing whatever he can do about it (except cry over spilt milk). No pattern of actions follow upon the event that reinforce its psychological impact and provide additional concrete anchors in experience for recalling it.

Woltman and Cadek (1977) report that the "memory decay" curve apparent in data from the National Crime Survey is not as steep for

reported as for unreported incidents, or for those which are more as opposed to less serious. All of this suggests that record checks conducted to date probably overestimate the aggregate accuracy of the report of victimization gathered in the surveys. There also do not seem to have been any record-check validations of victimization reports which have utilized reports other than those on file with the police. Thus we have no reading of the accuracy with which unreported crime is recalled in the National Crime Survey, despite the fact that the gathering of such data is one of the major goals of the project. Reiss (1977b) has suggested interviewing people who talked with the victims about their experiences and comparing those recollections with descriptions by victims who did not call the police, in an attempt to validate the recall of unreported crimes. Samples could also be drawn of households to which the police were dispatched, but where they did not write up an incident report. Record-check studies have the advantage of some alternate knowledge of the "true score" under investigation, which lends them great analytic power. There has not been enough critical or innovative research with regard to that criterion, however.

The final problem with record checks is the apparently universal tendency for victims to be hard to reach for interviewing. In every study of this type a substantial proportion of victims sampled from police files cannot be found or refuse to be interviewed. As a result, we are uncertain of the generalizability of the findings of these studies to the larger and apparently more transient victim population. None of the record checks conducted by the US Census Bureau has matched its usual standard for interview completions. In the city of San Jose a victimization survey of the general population enjoyed a 97 percent completion rate. As part of that project, a sample of victims was selected from police files, and their addresses were imbedded in the general sample. In that special victim group interviews were completed for only 63.5%. The bulk of the non-interviews (76%) was with people who simply could not be located; an additional 11% of victims moved from the city, and 13% refused to be interviewed or were never available (Turner, 1972a). This completion rate was the lowest of all the record checks conducted by the US Census Bureau, although the figures were only slightly higher (about 68%) in studies conducted earlier in Baltimore and Washington, DC. In London, Sparks *et al.* (1977) had even worse luck; they could find only 43% of their known victim sample, and 8% refused to cooperate. These low completion rates are not surprising. It is prosecutor's lore that the first response of many victims of crime is to

arrange an unlisted telephone number or to move to a new address. In the National Crime Survey people who recently have moved report higher rates of victimization than those who have not, and a substantial proportion of those reporting multiple or series victimizations moved to another address prior to the next wave of interviews (Reiss, 1978; Lehnen and Reiss, 1978). In addition, many victims (and witnesses) give false addresses to the police in order to avoid further involvement in a case or retaliation by their antagonist. It is unclear how generalizable findings based on those who remain accessible are to all crime victims.

REFERENCES

- Abt Associates (1977), *Victimization in Joliet and Peoria, Illinois* (Cambridge, Mass: Abt Associates).
- Australian Bureau of Statistics (1975), *General social survey - crime victims. Canberra* (A.B.S.#) Catalogue no. 4105.0. 22 June 1979 (May).
- Bailar, Barbara A. (1975), "The Effects of Rotation Group Bias on Estimates from Panel Surveys," *Journal of the American Statistical Association*, 70 (Mar.) 23-30.
- Bailar, Barbara A. (1976), "Some Sources of Error and Their effect on Census Statistics," *Demography*, 13 (May) 273-86.
- Bailar, Barbara A., Bailey, Leory and Stevens, Joyce (1977), "Measures of Interviewer Bias and Variance," *Journal of Marketing Research*, 14 (Aug.) 337-43.
- Bailey, Leroy, Moore, Thomas F. and Bailar, Barbara A. (1978), "An Interviewer Variance Study for the Eight Impact Cities of the National Crime Survey Cities Sample," *Journal of the American Statistical Association*, 73 (Mar.) 23-30.
- Balvanz, Bill (1979), "The Effects of the National Survey of Crime Severity on the Victimization Rates Determined from the National Crime Survey," Washington, DC: Demographic Surveys Division, US Census Bureau, memorandum, 15 Oct.
- Biderman, Albert D. (1967), "Surveys of Population Samples for Estimating Crime Incidence," *Annals of the American Academy of Political and Social Science*, 374 (Nov.) 16-33.
- Biderman, Albert D., and Reiss, Jr, Albert J., (1967), "On exploring the 'dark figure' of crime," *Annals of the American Academy of Political and Social Science*, 374 (Nov.) 1-15.
- Biderman, Albert D., Johnson, Louise A., McIntyre Jennie, and Weir Adrienne W., (1967) *Report on a Pilot Study in the District of Columbia on Victimization and Attitudes Toward Law Enforcement*, US President's Commission on Law Enforcement and Administration of Justice, Field Survey I. (Washington, DC: US Government Printing Office).
- Biderman, Albert D. (1970a), "Memos Concerning National Crime Survey Developments (Washington, DC: Bureau of Social Science Research, memoranda) 9 Apr. and 10 June.
- Biderman, Albert D. (1970b), "Time Distortions of Victimization and Mnemonic Effects (Washington, DC: Bureau of Social Science Research, memorandum).
- Biderman, Albert D. (1971), "Memo Concerning National Crime Survey Developments (Washington DC: Bureau of Social Science Research, memorandum).
- Biderman, Albert D. (1973), "Memo Concerning National Crime Survey Developments (Washington DC: Bureau of Social Science Research, memorandum, Mar.).
- Biderman, Albert D. (1975), *A Social Indicator of Interpersonal Harm* (Washington, DC: Bureau of Social Science Research).
- Booth, Alan, Johnson, David R., and Choldin Harvey M., (1977), "Correlates of City Crime Rates: Victimization Survey Versus Official Statistics," *Social Problems*, vol. 25, 187-97.
- Buckhout, Robert (1974), "Eyewitness Testimony," *Scientific American*, vol. 231 (Dec.) 23-31.
- Cahalan, Don (1968), "Correlates of Respondent Accuracy in the Denver Validity Survey," *Public Opinion Quarterly*, vol. 32 (Winter) 607-21.
- Cannell, Charles F., Oksenberg, Lois, and Converse, Jean M. (1979), *Experiments in Interviewing Techniques: Field Experiments in Health Reporting, 1971-77* (Ann Arbor: Institute for Social Research, University of Michigan).
- Catlin, Gary, and Murray, Susan (1979), *Report on Canadian Victimization Survey Methodological pretests* (Ottawa: Statistics Canada).
- Cirel, Paul, Evans, Patricia, McGillis, Daniel, and Whitcomb, Debra, (1977), *Community Crime Prevention Program - Seattle, Washington* (Washington, DC: US Government Printing Office).
- Clausen, Aage R. (1968), "Response Validity: Vote Report," *Public Opinion Quarterly* 32 (Winter) 588-606.
- Corrado, Raymond R., Glackman, William, and Roesch, Ronald (1979), *Interim Report One: Extent and Distribution of Victimization* (Burnaby, BC: Department of Criminology, Simon Fraser University).
- Cowan, Charles D. (1976), "Twelve and Thirteen-Year-old Interviewing Experiment" Washington, DC: Statistical Research Division, US Census Bureau, memorandum, 8 Apr.
- Cowan, Charles D., Murphy, Linda R., and Wiener, Judy (1979), "Effects of Supplemental Questions on Victimization Estimates from the National Crime Survey," paper presented at the Annual Meeting of the American Statistical Association, Washington, DC.
- Curtis, Lynn A. (1974), *Criminal Violence: National Patterns and Behavior* (Lexington, Mass: Lexington Books).
- De Neufville, Janet (1975), *Social Indicators and Public Policy* (New York: Elsevier).
- Dodge, Richard W. (1970), "Victim Recall Pretest - Washington, DC" (Washington, DC: US Census Bureau, memorandum, 10 June).
- Dodge, Richard W., and Turner, Anthony G. (1971), "Methodological Foundations for Establishing a National Survey of Victimization," paper

- presented at the Annual Meeting of the American Statistical Association, Aug.
- Dodge, Richard W. (1975), "Series Victimization: What Is To Be Done?" (Washington, DC: Crime Statistics Analysis Staff, US Census Bureau, memorandum, 31 Oct.)
- Dodge, Richard W., Lentzner, Harold, and Shenk, Frederick (1976) "Crime in the United States: a Report on the National Crime Survey," in Wesley G. Skogan (ed.), *Sample Surveys of the Victims of Crime* (Cambridge, Mass: Ballinger) pp. 1-26.
- Dodge, Richard W. (1977a), "Analysis of Screen Questions on the National Crime Survey" (Washington DC: Crime Statistics Analysis Staff, U.S. Census Bureau, memorandum, 22 Dec).
- Dodge, Richard W. (1977b), "A Preliminary Inquiry into Series Victimization," (Washington, DC: Crime Statistics Analysis Staff, US Census Bureau, memorandum, July).
- Dodge, Richard W., and Lentzner, Harold R. (1978), "Patterns of Personal Series Incidents in the National Crime Survey," paper presented at the Annual Meeting of the American Statistical Association, San Diego, Calif., 14-17 Aug.
- Ennis, Philip (1967), *Criminal Victimization in the United States: a Report of a National Survey*, US President's Commission on Law Enforcement and Administration of Justice, Field Survey II (Washington, DC: US Government Printing Office).
- Garofalo, James, and Hindelang, Michael J. (1977), *An introduction to the National Crime Survey* (Washington, DC: National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration, US Department of Justice).
- Gottfredson, Michael R., and Hindelang, Michael J. (1977), "A Consideration of Memory decay and Telescoping Biases in Victimization Surveys," *Journal of Criminal Justice*, vol. 5, 202-16.
- Graham, Dorcas (1974), "Reasons for Differences in the Number of Crime Incidents Reported on the Original and Reinterview Survey by Type of Crime, November 1972 to June 1975 (Washington, DC: Statistical Methods Division, US Census Bureau, memorandum, 30 Oct.).
- Gray, Percy G. (1955), "The Memory Factor in Social Surveys," *Journal of the American Statistical Association*, vol. 50 (June) 344-63.
- Groves, Robert M. (1977), "A Comparison of National Telephone and Personal Interview Surveys: Some Response and Nonresponse Differences," paper presented at the Annual Meeting of the American Association for Public Opinion Research.
- Groves, Robert M. (1979), "Actors and Questions in Telephone and Personal Interview Surveys," *Public Opinion Quarterly*, vol. 43 (Summer) 190-205.
- Hindelang, Michael J. (1976), *Criminal Victimization in Eight American Cities* (Cambridge, Mass.: Ballinger).
- Hindelang, Michael J. (1978), "Race and Involvement in Crimes," *American Sociological Review*, vol. 43 (Feb.) 93-109.
- Hindelang, Michael J., Gottfredson, Michael R., and James Garofalo (1978), *Victims of Personal Crime* (Cambridge, Mass: Ballinger).

- Jacob, Herbert (1975), "Crimes, Victims and Statistics: Some Words of Caution" unpublished manuscript (Evanston, Ill.: Department of Political Science, Northwestern University).
- Kalish, Carol B. (1974), *Crimes and Victims: a Report on the Dayton-San Jose Pilot Survey of Victimization* (Washington, DC: National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration, US Department of Justice).
- Klecka, William R., and Tuchfarber, Alfred J. (1974), "The Efficiency Biases, and Problems of Random Digit Dialing," Paper presented at the Annual Meeting of the American Association for Public Opinion Research (June).
- Klecka, William R., and Tuchfarber, Alfred J. (1978), "Random Digit Dialing: a Comparison to Personal Surveys," *Public Opinion Quarterly*, vol. 42 (Spring) 105-14.
- Lehnen, Robert G., and Reiss, Albert J. Jr (1978), "Some Response Effects in the National Crime Survey," *Victimology*, vol. 3, nos. 1-2, 110-24.
- Martin, Elizabeth (1978), "A Twist on the Heisenberg Principle - or How Crime Affects Its Measurement," unpublished paper (Chapel Hill, NC: Institute for Research in Social Science, University of North Carolina).
- Murphy, Linda R., and Cowan, Charles D. (1976), "Effects of Bounding on Telescoping in the National Crime Survey," paper presented at the Annual Meeting of the American Statistical Association, Boston, Mass. (23-26 Aug.)
- National Research Council (1976), *Surveying Crime: Report of the Panel for the Evaluation of Crime Surveys* (Washington, DC: National Academy of Sciences).
- Neter, John, and Wakeberg, Joseph (1964), "A Study of Response Errors in Expenditures Data from Household Interviews," *Journal of the American Statistical Association*, vol. 59 (Mar.) 17-55.
- Reiss, Albert J., Jr (1969), *Field Survey: Appendix A to Crime Against Small Business* (Washington, DC: US Government Printing Office).
- Reiss, Albert J. Jr (1977a), *Final Report for Analytical Studies of Victimization Using National Crime Survey Panel Data* (New Haven, Conn.: Institution for Policy Studies, Yale University).
- Reiss, Albert J., Jr (1977b), *A Note on Optimal Reference Period of Recall* (New Haven, Conn.: Institution for Policy Studies, Yale University, memorandum).
- Reiss, Albert J., Jr (1978), *Final Report for Analytical Studies of Victimization by Crime Using National Crime Survey Panel Data* (New Haven, Conn.: Institution for Policy Studies, Yale University).
- Research Triangle Institute (1977), *Analysis of the Utility and Benefits of the National Crime Survey* (Research Triangle Park, NC: Research Triangle Institute).
- Rodgers, Theresa F. (1976), "Interviews by Telephone and In-Person: Quality of Responses and Field Performance," *Public Opinion Quarterly*, vol. 40 (Spring) 51-65.
- Schneider, Anne L. (1976), "Victimization Surveys and Criminal Justice System Evaluation," in Wesley G. Skogan (ed.), *Sample Surveys of the Victims of Crime* (Cambridge, Mass.: Ballinger) pp. 135-50.

- Schneider, Anne L. (1977). *The Portland Forward Records Check of Crime Victims: Final Report* (Eugene, Oreg.: Institute for Policy Analysis).
- Schwind, Hans-Dieter, Ahlborn, Wilfried, Eger, Hans, Jany, Ulrich, Pudel, Volker, and Weiss, Rudiger (1975). *Dunkelfeldforschung in Göttingen 1973/1974* (Wiesbaden: BKA-Forschungsreihe).
- Shichor, David, Decker, David L., and O'Brien Robert, M (1979). "Population Density and Criminal Victimization," *Criminology*, vol. 17 (Aug.) 184-93.
- Skogan, Wesley G. (1974). "The Validity of Official Crime Statistics: an Empirical Investigation," *Social Science Quarterly*, vol. 55 (June) 25-38.
- Skogan, Wesley G. (1975). "Measurement Problems in Official and Survey Crime Rates," *Journal of Criminal Justice*, vol. 3 (Spring) 17-32.
- Skogan, Wesley G. (1976a). "Citizen Reporting of Crime: Some National Panel data," *Criminology*, vol. 13 (Feb.) 535-49.
- Skogan, Wesley G. (1976b). "Crime and Crime Rates," in Wesley G. Skogan (ed.) *Sample Surveys of the Victims of Crime* (Cambridge, Mass.: Ballinger) pp. 105-20.
- Skogan, Wesley G., and Klecka, William R. (1977). *The Fear of Crime* (Washington, DC: American Political Science Association).
- Skogan, Wesley G. (1978). *The Northwestern University Center for Urban Affairs Random Digit Dialing Telephone Survey* (Evanston, Ill.: Center for Urban Affairs, Northwestern University).
- Skogan, Wesley G. (1979). "Crime in Contemporary America," in Hugh Graham and Ted Robert Gurr (eds.), *Violence in America*, 2nd edn (Beverly Hills, Calif.: Sage Publication) ch. 14.
- Skogan, Wesley G. and Maxfield, Michael G. (1980). *Coping with Crime: Victimization, Fear and Reactions to Crime in Three American cities* (Evanston, Ill. Center for Urban Affairs, Northwestern University).
- Small Business Administration (1969). *Crime Against Small Business*, a Report of the Small Business Administration Transmitted to the Select Committee on Small Business, United States Senate (Washington, DC: US Government Printing Office).
- Sparks, Richard, Hazel G. Genn, and David J. Dodd (1977). *Surveying Victims* (New York: John Wiley).
- Statistics Canada (1979). *Greater Vancouver Crime Survey Documentation Package* 4 vols (Ottawa, Special Surveys Group, Statistics Canada).
- Steinmetz, Carl (1979). "An Empirically Tested Analysis of Victimization Risks," paper presented at the Third International Symposium on Victimology (Münster, Germany, 2-8 Sept.).
- Stephan, Egon (1975). "Die ergebnisse der Stuttgarter opferbefragung unter berücksichtigung vergleichbarer Amerikanischer daten," *Kriminalstatistik*, vol. 5, 201-6.
- Stephan, Egon (1976). "Die Stuttgarter Opferbefragung (Wiesbaden, Germany: Bundeskriminalamt).
- Stephan, Egon (n.d.). "Report on Two Studies of the Perception of Crime and Crime Control from the Victim's Point of View," unpublished manuscript. (Freiburg, Germany: Max-Planck-Institute)
- Sudman, Seymour, and Bradburn, Norman M. (1974). *Response Effects in Surveys: a Review and Synthesis* (Chicago, Ill.: Aldine).

- Traugott, Michael W., and Katosh, John P. (1979). "Response Validity in Surveys of Voting Behavior," *Public Opinion Quarterly*, vol. 43 (Fall) 359-77.
- Tuchfarber, Alfred, and Klecka, William R. (1976) *Random Digit Dialing: Lowering the Cost of Victimization Surveys* (Washington, DC: The Police Foundation).
- Turner, Anthony G. (1970). *Personal Victimization Pretest: Evaluation of Findings* (Washington, DC: Statistical Research Center, Law Enforcement Assistance Administration, memorandum, 17 Apr.).
- Turner, Anthony G. (1972a). *The San Jose Methods Test of Known Crime Victims* (Washington, DC: National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration, US Department of Justice).
- Turner, Anthony G. (1972b). "Methodological Issues in the Development of the National Crime Survey Panel: Partial findings" (Washington, DC: National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration, US Department of Justice, memorandum, Dec.).
- Turner, Anthony G., and Dodge, Richard W. (1972). "Surveys of Personal and Organizational Victimization," paper prepared for the Symposium on Studies of Public Experience, Knowledge and Opinion of Crime and Justice (Washington, DC, Mar.).
- Turner, Anthony G. (1977). "An Experiment to Compare Three Interview Procedures in the National Crime Survey," (Washington, DC: Statistical Research Division, US Census Bureau, memorandum, Mar.).
- US Census Bureau (1979). "Survey Documentation: National Crime Survey Central Cities Sample, 1974" (Washington DC: US Census Bureau, memorandum, June).
- US Department of Justice (1975) *Criminal Victimization Surveys in 13 American Cities* (Washington, DC: National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration).
- US Department of Justice (1979a). *Criminal Victimization in the United States 1976* (Washington, DC: National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration).
- US Department of Justice (1979b) *Criminal Victimization in the United States: Summary Findings of 1977-78 Changes in Crime and of Trends since 1973*. Washington, DC National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration).
- Weiss, Carol M. (1968). "Validity of Welfare Mother's Interview Responses" *Public Opinion Quarterly* 32 (Winter) 622-33.
- Wolf, Preben (1976a). "On Individual Victims of Certain Crimes in Four Scandinavian Countries 1970/74: a Comparative Study" paper presented at the Second International Symposium on Victimology (Boston. 5-11 Sept.).
- Wolf, Preben (1976b). *Note on Some of the Methods applied in the Scandinavian Victim Surveys 1970-74* (København: Sociologisk Institut, København Universitet).
- Wolfgang, Marvin E. (1978). National survey of crime severity. Grant Report to LEAA-NCJISS, 9 Nov.

- Woltman, Henry F., Bushery, John, and Carstensen, Larry (1975) "Recall Bias and Telescoping in the National Crime Survey" (Washington, DC: Statistical Methods Division, US Census Bureau, memorandum, 23 Sept.).
- Woltman, Henry F., and Cadec, Glenn (1977), "Are Memory Biases in the National Crime Survey Associated with the Characteristics of the Criminal Incident?" (Washington, DC: Statistical Methods Division, US Census Bureau memorandum, 4 Apr.).
- Woltman, Henry F., and Bushery, John M. (1977a), "Update of the National Crime Survey Panel bias Study" (Washington, DC: Statistical Methods Division, US Census Bureau, memorandum, 11 July).
- Woltman, Henry F., and Bushery, John M. (1977b) "Results of the National Crime Survey Maximum Personal Visit, Maximum Telephone Interview Experiment" (Washington, DC Statistical Methods Division, US Census Bureau, memorandum, 9 Dec.).
- Yost, Linda R., and Dodge, Richard W. (1970), "Household Survey of Victims of Crime: Second Pretest - Baltimore, Maryland" (Washington, DC: US Census Bureau, memorandum, 30 Nov.).
- Zimring, Franklin (1972), "The Medium Is the Message: Firearm Caliber as a Determinant of Death from Assault" *Journal of Legal Studies*, vol. 1 (Jan.) 97-123.