Issues in the Measurement of Victimization

The task at hand

Conceptual issues

Measurement issues

Procedural issues

Assessment

National Criminal Justice Reference Service

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Abstract

This volume summarizes 15 years of research on methodological issues in the measurement of criminal victimization by means of population surveys. The report reviews some factors of crime which affect our ability to measure it accurately, including the relative infrequency of criminal victimization, the skewed distribution of victimization in the population, and the fugitive nature of crime. The first chapter reviews specific measurement problems: furtive character of crime. The second chapter reviews methodological issues: limited distribution of knowledge of incidents, forgetting or inaccurate recall of events, and differential productivity of survey respondents. The next chapter reviews three procedural issues which affect estimates of victimization rates: problems of panel bias and attrition, differences between telephone and in-person interviews, and interviewer effects. The final chapter summarizes the current state of the art in this area and discusses possible future developments in victimization survey methodology.
introduction

This volume presents an extensive overview of 25 years of methodological development in refining the methods by which criminal victimization can be measured through survey interviews. Both in the United States and abroad, there has been a great deal of research on methods for employing sample surveys to gauge the incidence of crime. This report attempts to synthesize these efforts, to report on the nature of the problems facing those conducting victimization surveys, and to assess the current state of the art. It examines the conceptualization of victimization implicit in current survey methods, the measurement techniques employed in conducting surveys to gather data on the crime experiences of the public. The review takes a critical stance with regard to many of these concepts and methods. However, this detailed criticism is possible only because those responsible for the surveys have paid a great deal of attention to methodological aspects of their work. The Bureau of Justice Statistics (BJS) and the U.S. Census Bureau have sponsored a number of investigations of victim survey methodology, and they continue to test the limitations of the data which their current survey produces. Researchers and data users of victimization data.

This volume is aimed at interested and relatively well-informed crime researchers and users of victimization data. It is not a "how-to-do-it" text, but a synthesis of a substantial body of methodological research on the reliability and validity of victimization data. It is intended to inform researchers and data users of the strengths and weaknesses of these data, and, where weaknesses are apparent, to pinpoint some specific pitfalls to avoid in using the data. This report also notes a number of areas in which further methodological research is needed. There is increasing interest in criminal justice statistics with national scope, and attention to these continuing problems may speed the development of a useful Federal statutory program.

Methodological features

The specific focus of this volume is the methodological features of the National Crime Survey. This survey program, sponsored by BJS and conducted by the U.S. Census Bureau, is our primary source of data on patterns of criminal victimization, and the only ongoing victimization survey which is national in scope. (For details on the National Crime Survey see: Garofalo and Hindelang, 1977.) However, there has been a great deal of innovative methodological research conducted by States and municipalities, and abroad, and many of these contributions will also be reviewed here. The findings of all of those investigations have been remarkably consistent, suggesting that the problems inherent in measuring the extent of criminal victimization through sample surveys involve fundamental social and psychological issues.

Some of the consistency may reflect the fact that all of the surveys have focused on the same issue—victimization. The next chapter examines some aspects of crime itself and how they shape the nature of the survey enterprise. These include the relative infrequency of serious victimization, the skewed distribution of victimization in the population, and the covert nature of criminal action. Chapter 3 explores certain conceptual issues related to how surveys operationalize the concept of criminal victimization. It criticizes the events orientation of such surveys, the characterization of crime as a discrete rather than an occasionally continuous social process, and the abstraction of events from their social context.

Chapter 4 reviews the impact of human factors on the accurate recall of events from the past. These contribute to the selective forgetting or inaccurate recall of those incidents. Chapter 5 examines problems which are procedural in character, including panel bias and attrition, interviewer effects, and decisions regarding interviewing procedures. Chapter 6 concludes with an assessment of current survey practices, a critique of data on the incidence of criminal assault, and some notes on future methodological developments in this area.
The task at hand

Many methodological and procedural hurdles to measuring victimization can be overcome. However, attempts to measure the frequency of crime through general population surveys face certain intractable difficulties stemming from the nature of crime. These difficulties ultimately limit our ability to use survey methods in gathering data on offenses. Three facts about crime are important in this regard. First, it is relatively infrequent, especially in its most serious and violent forms; it is evenly distributed; and most criminals do their best to avoid detection.

Frequency of crime

Despite the large numbers which fill the columns of the FBI's Uniform Crime Report, one of the most important aspects of crime from the point of view of individual citizens is that it strikes infrequently. As any reasonably well-informed person will tell you, thefts, muggings, and other acts of violence are much more likely to occur to someone else. The infrequency of serious crime in the general population has important implications for the way surveys are conducted and the types of questions asked. For example, interviews with people in Scandinavian countries are designed to obtain reliable information only on the more serious and violent forms of crime, and many of the respondents are not asked about their experiences with less serious offenses like pocket picking, but no truly serious crime is ever reported.

The problem is even more extreme in countries with lower crime rates. It is relatively infrequent, especially in its most serious and violent forms; it is evenly distributed; and most criminals do their best to avoid detection.

The infrequency of serious crime in the general population has important implications for victimization surveys. This was a major concern when the Crime Commission first considered conducting a victimization survey. Official statistics for the mid-1960's suggested, for example, that there were about 180 robberies of all kinds (including crime against businesses) for every 100,000 persons in the population. Given the apparently low frequency of such incidents, very large survey samples would be required to collect reliable information on many kinds of serious crime. All surveys are subject to sampling error, and, if victimization rates are relatively low, sampling error may tend to mask very substantial variations in estimates of those rates. Sampling error may also make it difficult to examine differences in victimization among population groups, because sampling variation may be greater than true differences among them. Those who favored the surveys were convinced that the "dark figure" of unreported crime was large enough that a national sample of 10,000 households would uncover enough victims for analysis. The truth lay somewhere in between. While the survey collected data on property crimes which were common enough to make estimates of their incidence in the population, the number of victims of personal crimes who were located was very small.

The problem is more extreme in countries with lower crime rates. Recent victim surveys have been used to make estimates of the frequency of events problem and the many other measurement issues involved here, it is extremely difficult to produce accurate estimates of the victimization rate even when large samples are brought to bear on the subject.

The relative infrequency of victimization by serious crime has significant implications for the utility of the data generated by the crime surveys and for the applicability of the method to criminal justice research. The National Crime Survey originally was designed to produce estimates of personal crime victimization rates for the United States as a whole and to date the data have only been used for national statistics. The data were analyzed for different cities, and the range within which we are 95 percent certain the true value must fall. The size of these intervals represents only the potential effect of sampling error.
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level analyses as well. Some researchers (e.g., Booth et al., 1977; Skolnick et al., 1979) have used published estimates of victimization to set standards of performance. For example, surveys conducted to evaluate the Seattle Community Crime Prevention Program produced an estimate of 4.9 per 1,000 households in the first round of interviewing and 2.0 in the second; these interviews all were concentrated in only five census tracts and (3) the survey examined the incidence of the most frequent major crime (rape). (Clelland et al., 1977). Studies that focus on personal crimes or small subgroups of the population are limited to an even greater extent by the laws of sampling.

Distribution of crime

Not everyone shares equally the burden of crime, and the highest risk groups often are relatively small. They also can be the most difficult to locate. One problem of victimization surveys is to identify high-risk populations and their particular problem areas. Because these groups constitute disproportionately in the overall victimization rate, the proportion of total data collected from these high-risk populations is relatively small. Researchers interested in the effects of crime on fear and behavior also could benefit from detailed data on such groups as minority-group males, junior citizens, and middle-class families moving back into America's inner cities. Be-cause these groups are relatively small, general population surveys will not represent everyone in the popula- tion proportionately. The U.S. Population Census of 1970, for example, appears to have underestimated black males aged 20 to 24 by 18 percent, and the National Crime Survey for 1974 reached only 66 percent of the targeted figures for that group (National Research Council, 1976:table 6).

While crime is relatively infrequent in the general population, this is not the case among certain subgroups. Crime is speci-fically segregated. For example, in 1970 the households of the reported robbers in the United States were concentrated in 23 cities which hosted only 15 percent of the nation's population (Robgen, 1979). Within these crime centers was another concentration. In general, differences in the distribution of crimes within cities are even greater than differences among cities. As a result, a rela-tively small proportion of the population is exposed to extremely high levels of risk, especially from violent crime. People from these groups contribute disproportionately to the total count of victims.

The relatively extreme spatial concentration of violent crime, especially victimizations of violent crime, poses a challenge to many surveys. Most surveys involve area prob-ability samples in which successively

<table>
<thead>
<tr>
<th>Type of crime</th>
<th>Interpersonal violence</th>
<th>Robbery</th>
<th>Personal theft</th>
<th>Burglary</th>
<th>Larceny</th>
<th>Auto theft</th>
</tr>
</thead>
<tbody>
<tr>
<td>Woman</td>
<td>3.2</td>
<td>3.9</td>
<td>46.8</td>
<td>94.8</td>
<td>95.7</td>
<td>92.4</td>
</tr>
<tr>
<td>Percent who do not know relation to offender</td>
<td>3.2</td>
<td>3.6</td>
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</tbody>
</table>

Source: Compiled by the author from all regular and special sources cited in the text. Crime categories overlap so table is not as complete as it could be. Author's computation. Note that this data was weighted to reflect the narrowest possible fashion as one

The difficulty is that factors associated with the victimization rate exist relatively little to rene-phantom offenses could be substantial. Researchers interested in the effects of crime on fear and behavior also could benefit from detailed data on such groups as minority-group males, junior citizens, and middle-class families moving back into America's inner cities. Be-cause these groups are relatively small, general population surveys will not represent everyone in the popula- tion proportionately. The U.S. Population Census of 1970, for example, appears to have underestimated black males aged 20 to 24 by 18 percent, and the National Crime Survey for 1974 reached only 66 percent of the targeted figures for that group (National Research Council, 1976:table 6).

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Chapter 3

Conceptual issues

The form taken by victimization surveys reflects decisions which were made concerning the nature of crime and the utility of various ways of knowing about it. In the National Crime Survey, victimizations are conceptualized as discrete incidents, with a beginning and an end, and clearly bounded in space and time. As a result, the survey does not measure well continuing processes which are not so clearly delineated and which resemble enduring conditions more than discrete events. Further, victim surveys usually are oriented toward incidents rather than victims. They only measure events that can be uniquely described, thus ignoring classes of crimes for which victimization is quite prevalent even though the frequency of individual incidents is unknown. Finally, victimization surveys attempt to vary from their social context and define criminality without reference to the assessment of those directly involved in the incident. It is not clear that this is desirable, and there is evidence that participants in the surveys impose such criteria on the data in any event. All of these conceptual positions are reflected in the manner in which the concept of victimization is operationalized in surveys.

Operationalization of victimization

Operationalization is the translation of concepts being measured into terms workable in light of the measurement methodology to be employed. In the victimization surveys, this takes the form of specific questionnaire items which are designed to elicit reports of crimes from their victims. Those questions define the concepts being measured in concrete terms. BOS's survey efforts rely on the criminal code as the source for the definition of criminal events. The questionnaire items parallel guidelines developed by the FBI for its crime reporting system in identifying components of events which are used to signal the occurrence of a crime. Respondents in the survey are not simply asked if they have been victimized. If we ask people what bothers them, we will generate a great deal of data on the incidence of street urchins, demonstrators, noisy neighbors, and other things which lie outside the preview of the criminal law beyond the capacity of the police to handle even in an informal manner. Rather, respondents are asked to report on their participation in specific events. The occurrence of a criminal incident is indicated when they recall that they experienced or observed things resembling key elements of crimes. At the heart of the survey instrument is the "incident screen." A series of questions is asked of each respondent, including:

Did anyone beat you up, attack you, or hit you with something such as a rock or bottle?

Were you knifed, shot at, or attacked with some other weapon by anyone at all?

Property crimes are probed by questions concerning the actual theft of property, or the observation of specific evidence that someone had attempted to steal something. These items serve as "memory jogs." Each is designed to assist respondents in scanning their memories for crimes occurring within the survey's reference period. In the National Crime Survey, 17 items are employed to elicit "yes" responses from anyone who was a victim of one of these incidents. Eleven questions which ask of all respondents are reproduced in figure 2. An additional six questions about household crimes such as burglary are administered to an adult informant in each household. At the end of the screening questionnaire, two "catch-all" questions (items 47 and 48 here) are included to stimulate the recall of any incident that has been overlooked. Respondents are asked if they had called the police about anything else that they thought was a crime, or if something had happened to them which they could have reported to the police. However, only reports of incidents which could be classified as rape, robbery, assault, burglary, or theft are retained for analysis.

This procedure for operationalizing victimization implies at least three conceptual decisions concerning the nature of crime: That crimes are discrete events which are bounded in space and time, that they are knowable only as distinct individual instances, and that they can be understood apart from their social context.

Discrete events versus continuous processes

The fundamental unit of analysis in crime surveys is the victimization: an incident involving a victim(s) and an offender(s), which has a beginning, some characteristic activity, and an end. Events that resemble this ideal can be firmly placed in space and time, enabling us to examine day-and-night, public-and-private, and seasonal cycles of criminality. This undoubtedly is a useful way to describe many criminal incidents, including street robberies, store break-ins, and simple thefts. But there are many other kinds of crime (even by the definition employed in the crime surveys) which more accurately may be thought of as continuous processes rather than discrete events. What observers count as discrete incidents may be instances of ongoing disputes, conflicts, or predations. Several crimes which recently have come to the attention of the American public fall into this category, including child abuse, spouse abuse, and robberies of children in school. A commercial crime in this category of "continuous" criminality is price fixing. Because these are more or less enduring conditions rather than discrete events, they are difficult to count in conventional fashion. Incidents in this category share all of the barriers to reliable measurement to be discussed here, in addition to their unacceptability with regard to conventional accounting practices. For example, consider a family in which the father comes home drunk every night, regularly beats his wife, and threatens his
The fundamental unit of analysis in crime surveys is the victimization: an incident involving a victim(s) and an offender(s), which has a beginning, a characteristic activity, and an end. An alternative approach to estimating the magnitude of crime problems of this sort would be to turn from incidence to prevalence measures of victimization. This has a substantial impact on the magnitudes of those estimates. Recent (1978) calculations that including series incidents would increase the estimated number of crimes in the United States by 18 percent. The irony is that those victims who are the worst the crime problem, partially or totally about the targets of crime. For example, in 1958 the Small Business Administration conducted a nationwide victimization survey of business establishments. Businesses were asked to report whether or not they had suffered any financial loss from shoplifting and employee theft and if they had agreed to someone committing those crimes. Black (1969). This survey found that two-thirds of all retail businesses suffered identifying losses and that 31 percent had approved an offender during the previous year. Using its analytic focus the "victim or not" dichotomy, the study then explored the correlates of victimization by those two types of crime. Thus, even in the absence of information on the frequency of specific incidents, a survey such as this could yield important insights into victimization.

<table>
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<tr>
<th>Individual screen questions</th>
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<tbody>
<tr>
<td>1. Did you experience an attempted to steal something that you believed to belong to you?</td>
</tr>
<tr>
<td>2. Were you harmed or threatened during the incident?</td>
</tr>
<tr>
<td>3. Did you report the incident to the police?</td>
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</tbody>
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<table>
<thead>
<tr>
<th>CHECK ITEM 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Yes...</td>
</tr>
<tr>
<td>No...</td>
</tr>
</tbody>
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<tr>
<th>CHECK ITEM 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Yes...</td>
</tr>
<tr>
<td>No...</td>
</tr>
</tbody>
</table>

<table>
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<tr>
<th>CHECK ITEM 3</th>
</tr>
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<tbody>
<tr>
<td>Yes...</td>
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<tr>
<td>No...</td>
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<tr>
<th>CHECK ITEM 4</th>
</tr>
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<tbody>
<tr>
<td>Yes...</td>
</tr>
<tr>
<td>No...</td>
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Events orientation

The events orientation is illustrative of a larger conceptual issue: whether crimes can be recorded in the victim survey only if they are known as discrete events. Other ways to discern crime, in particular, victims who cannot recall the victimization. Therefore, in which similar incidents occurred, they are classified as series. About 100 incidents are recorded every month in the survey, and they make up about 3 percent of all incidents reports (Doyle, 1975). It is difficult to use the data currently available to establish a series incidents to estimate their frequency. In order to determine what kind of victimization rates. The series-incident problem is illustrative of the difficulty of their condition, are not counted as victims at all.

Social meaning of incidents

The major conceptual problem implied by the incidence approach is that we can talk about crime apart from its social meaning. The definition of the event will be interpreted as carelessness, error, or the result of In. Crime is a label which may be officially cited and not after intent is established: this is what crime is. Social definitions differ many objectively similar. In general, when civilians kill police officers, it is a crime, but when po-

<table>
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<th>Figure 2</th>
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Children, who may be tend to modify their responses are asked. Droduced reports that a particular kind of event occurred "many times" or "every week," in an in-depth study of these reports. The largest group was occupationally related violence. It included things such as physical attacks, injuries, and property theft. All of these incidents included assaults. The next largest group was related to personal violence. Causes of this violence are accidents, nonviolent efforts, and other assaults. Names of these incidents included assaults, injuries, and property theft. All of these incidents included assaults. The next largest group was related to personal violence. Causes of this violence are accidents, nonviolent efforts, and other assaults. Names of these incidents included assaults, injuries, and property theft. All of these incidents included assaults. The next largest group was related to personal violence. Causes of this violence are accidents, nonviolent efforts, and other assaults.
will be mixed, and his interpretation of the facts probably will be determined by how he wants to deal with his insurance company. The commercial vicinidty survey, on the other hand, could have recorded the incident as an attempted burglary, especially if the story gained some detail in retelling. Note that every crime measurement technique confronts the same cloudy information base. In that circumstance, social processes will determine the outcome of the investigation.

The importance of the social interpretation of events may be reflected in the major puzzles presented by new data now coming from the National Crime Survey: the apparent low number of victimizations from assaultive violence contributed by higher status respondents. Victimization always have assumed that the bulk of victim of assault comes from the lower reaches of the social ladder. Lower status persons are heavily overrepresented among victim of such crimes that are recorded in police files. Of course, one of the most useful findings of victimization surveys is that education is frequently only weakly and sometimes even positively correlated with reports of victimization by assault. Illustration data from the National Crime Survey are presented in table 3. In this table, rates per thousand for assault are correlated for groups distinguished by the level of education at an educational attainment.

This table makes a number of findings quite obvious. It characterized earlier surveys in the United States and Europe (e.g., as well as surveys in Germany (Stephan, 1977), and data from Australia, Canada, Norway, Sweden, Finland, and Denmark (1976). If it is a methodological artifact, then the data from the "true" assault rate, it is an extremely robust finding.

There are at least two competing explanations of this phenomenon. One is that it is an artifact of differential productivity in interview situations. This will be discussed in detail below. The other explanation is that respondents of different classes apply differing interpretations to certain situations. If the "true" distribution of violences in victimological traditions have assumed, those who are unaccustomed to physical abuse may find it more memorable. Whatever it is that education may be a condition of daily life may seem to others a brush with criminal violence. Because crime by its nature involves impulsive motives and the impulsiveness of definitions upon events by observers, differences in what respondents remember or think about events and their descriptions of past events raises important measurement and data analysis questions. The victimization survey are more aware to observers than they are to traditional opinion surveys. We assume that victim have been involved in events which include some subjective meaning about which illegal substances are used. The task of interviewers is to elicit accurate descriptions of those occurrences.

Surveys tapping the hopes and fears or voting intentions of the citizenry strive for reliability in measurement. Because those surveys are probing internal states, researchers primarily are concerned that the reported state of those states are reliable and not highly dependent upon a particular set of conditions. Victimization surveys strive for a greater synthesis, that of validity. Because the surveys gather data on events external to the individual, and these events presumably have a reality aspect, we must know of events from their description to an interview, the standard of accuracy in vicinidty research is the match between the reality of an incident and its description.

This match is problematic under the best of circumstances. The problem is exacerbated by the nature of crime, conceptual disagreement 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 from the National Crime Survey are collected in a variety of forms. The following data are the "true" assault rate: 8.3 per 1000.

Evidence from victimization surveys do not always support this interpretation of the data. For example, if differences in assault rates by social classes were strongly influenced by differential interpretations of events, then interviewers, we expect the positive education-assault relationship to be stronger for more trivial events; they presumably are more amenable to differential interpretations. It is not, likewise, we would expect alternative measures of social standing, such as family income, to exhibit similarity effectively protest. They do not: assault drops poorly in frequency with increasing income (Stephan, 1978, table 3).

On the other hand, Stephan (1975), reports that "sensitive forms of crime would be greater among residents of Dustin than those of large American cities because crime is much less common in Germany. Therefore, Germany has a pattern of victim reports of assaultive events. He uses this hypothesis by victimization surveys in industrialized nations. He finds this hypothesis by victimization surveys in industrialized nations. He finds this hypothesis to be true. However, we know of events about which we quiz them.

Still, respondents do not distinguish from their deliberately false statements. Finally, some people are better respondents than others: they work harder at it; and they tire of the demands of the survey less rapidly. All of these factors combine to shape the volume and character of reports of victimization, sometimes independently and sometimes in conjunction with other distribution of criminal incidents.

Methodological research techniques

Most of what we know about measurement issues 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 rates 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 can be traced or can be compared 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 the National Crime Survey and related efforts. The substance of these contributions will be discussed in this and in the next section.

Respondents sometimes do not know about things which have gone wrong. They also might have forgotten about them, a failing which in practice we cannot distinguish from their deliberately false statements. Respondents may also either inadvertently or maliciously lie about 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 the survey less rapidly. All of these factors combine to shape the volume and character of reports of victimization, sometimes independently and sometimes in conjunction with other distribution of criminal incidents.

Analytic methods

A simple measure of data gathered in the National Crime Survey is often revealing of substantial methodological questions. Strange for more trivial assessment, the data collusion reveals that reports of events are more frequent in months nearer to the date of the interview. In January 1971 a segment of the National Crime Survey was asked about crime experiences for 1970. Eighty percent of the...
the personal crimes that were recalled by victims took place by day—were remem-
bered during the last 6 months of the period (Turner, 1973b). The same “busi-
ing” of events in more recent months has been observed in Germany (Schwind et al.,
1975) and England (Sparks et al., 1973). This does not seem to reflect the true re-
view of crime.
Internal analyses of victimization data also point to weaknesses in reports gath-
ered by the police. The data strongly suggest that the report of violent crimes in the
Census Bureau’s investigations were widely confirmed to the power and accu-
ricacy of incident reporting. Schneider’s study was concerned with the interview-
the recall of crimes involves many perceived amounts of victimization. The pro-
nontrol of those additional questions on
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the police and the victim to recast events.

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the conviction (Turner, 1972a). Further,

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are reporting on events which occurred

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than those on file with the police. Thus, we have no reading of the accuracy with which unprompted 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 (1970) has suggested interviewing people who talked with the victims about their experiences and comparing those recollections with descriptions by vic­ tims who did not see the police, in an at­ tempt to validate the recall of unprompted crimes. This approach might have the advantage of knowledge of the "true" victimization event but loses much of its analytic power. There has been quite enough critical or innova­ tive research with regard to that criterion, however.

The final problem with record checks is the apparent universal tendency for vic­ tims not to know the full story of a victimization. In every study of this type a substantial proportion of victims are unable or unwilling to file complaints because they lack the knowledge to do so, which lends them great analytic power. There has not been enough critical or innova­ tive research with regard to that criterion, however.

None of the record checks conducted by the U.S. Census Bureau has matched its usual standard for interview completion. In the city of San Jose a victimization survey of the general population enjoyed a 97 percent completion rate. As part of this project the same census workers re-interviewed a random sample of 741 households. In some cities victimization rates found by the two methods were the same, that is, the ratio was 1.1 to 1 for rape, 2.1 to 1 for strong-armed robbery, and 3.5 to 1 for a variety of other crimes such as assault, burglary, theft, and larceny. If the respondents were asked to report all crimes in both complete and censored surveys, there would be a tendency for respondents to suppress crimes which are not committed against persons or which will be discovered by the police. Finally, victimization rates that are determined by survey methods in the same general area of the country, in the same period of time, are often subject to a national victimization survey of the general population enjoyed crimes.

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Personal Surveys entail a considerably heavier burden. Biderman (1973) speculates that one reason respondents have manifested that cooperation 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 interaction interviews. 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 explains 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 screens, and other factors may account in part for the observed distribution. This is likely to be more common among poorly motivated respondents, who feel little interest in interviews taking or incomprehensible, and those who find few social rewards in chatting with someone from the U.S. 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 corroborated by cross-checking primarily from record checks based on reports given in the incident screens and other file records. In the San Jose methodological study, a comparison above, evidence was reported that respondents who were known victims of the crime were less likely to report the crime and in incidents where the victim knows the offender. In those cases it was the eventual victim, rather than the apparent offender, who first initiated the event. Other causes may be encouraged or facilitated, if not caused, by citizens before. Biderman’s (1973) survey in Washington, D.C., dealt in passing with this problem. There, 25 percent of all victims agreed that they were neglected or had done something foolish which contrived their plight. Victims feel culpability may be less likely to report their experience even if at an interview.

Forgotten

Most research on cross-checks is 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 in remembering incidents from the more distant past. As one time was assumed that crimes were very memorable events, it was proposed to use the retrospective surveys of the general population to reconstruct an historical time series for victimization rates, using interviews with a life-long reference period. Protocols quickly demonstrated the feasibility of that enterprise. Rather than being readily memorable, Biderman et al. (1972:31) found:

In practice, most respondents seemed to be doing well to remember incidents which had occurred some time ago. 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.

Table 4: Patterns of record-check response

<table>
<thead>
<tr>
<th>Incident characteristics</th>
<th>Percent recalled</th>
<th>Number of cases</th>
</tr>
</thead>
<tbody>
<tr>
<td>Offender a stranger</td>
<td>76.3</td>
<td>99</td>
</tr>
<tr>
<td>Offender known</td>
<td>56.9</td>
<td>78</td>
</tr>
<tr>
<td>Offender related</td>
<td>22.2</td>
<td>18</td>
</tr>
<tr>
<td>(Total cases)</td>
<td>(65.7%)</td>
<td>(206)</td>
</tr>
<tr>
<td>Assault-total</td>
<td>48</td>
<td>51</td>
</tr>
<tr>
<td>Assault by strangers</td>
<td>54</td>
<td>24</td>
</tr>
<tr>
<td>Rape-total</td>
<td>87</td>
<td>45</td>
</tr>
<tr>
<td>Rape by stranger</td>
<td>84</td>
<td>19</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Percent</th>
<th>Number of cases</th>
</tr>
</thead>
</table>

strangers the recall rate was 76 percent. Two-thirds of the personal variations 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 rapes which were mentioned 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 foundough 71 percent of stranger assaults were recalled, but only 56 percent of "known" (or "known" assaults and 39 percent of related-\-assaults were recalled (Cottle and Murray, 1979:Table 8). Those figures are essentially similar to findings from the San Jose record check.

There are competing explanations for this phenomenon. Victimization may not member disputes which arise within kinship or friendship circles as readily as they remember events involving strangers—the data in table 4 may reflect true forgetting. 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 alternative accounts and for these incidents all were "snowballed" 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 wish to retrieve. In all, the arguments are not to say that what the police seem to the apparent offense of the victim is assumed to be the real victim, rather than the apparent offender, who first initiated the event. Other causes may be encouraged or facilitated, if not caused, by citizens before. Biderman’s (1973) survey in Washington, D.C., dealt in passing with this problem. There, 25 percent of all victims agreed that they were neglected or had done something foolish which contrived their plight. Victims feel culpability may be less likely to report their experience even if at an interview.

The fact that victims forget about their experiences with the passage of time also has serious implications for the accuracy of victimization estimates produced in the city surveys conducted between 1972 and 1975.

Table 5: Recall of incidents

<table>
<thead>
<tr>
<th>Months ago in recall period</th>
<th>Total personal crime</th>
<th>Household larceny</th>
<th>Personal theft</th>
<th>Burglary</th>
</tr>
</thead>
<tbody>
<tr>
<td>0-6</td>
<td>95</td>
<td>55 (10)</td>
<td>75 (10)</td>
<td>30 (10)</td>
</tr>
<tr>
<td>7-12</td>
<td>90</td>
<td>53 (10)</td>
<td>46 (10)</td>
<td>19 (10)</td>
</tr>
<tr>
<td>13-18</td>
<td>82</td>
<td>50 (10)</td>
<td>41 (10)</td>
<td>13 (10)</td>
</tr>
<tr>
<td>19-24</td>
<td>78</td>
<td>46 (10)</td>
<td>36 (10)</td>
<td>12 (10)</td>
</tr>
<tr>
<td>25-30</td>
<td>72</td>
<td>41 (10)</td>
<td>33 (10)</td>
<td>9 (10)</td>
</tr>
<tr>
<td>31-36</td>
<td>66</td>
<td>34 (10)</td>
<td>28 (10)</td>
<td>7 (10)</td>
</tr>
<tr>
<td>37-42</td>
<td>60</td>
<td>27 (10)</td>
<td>23 (10)</td>
<td>5 (10)</td>
</tr>
<tr>
<td>43-48</td>
<td>54</td>
<td>20 (10)</td>
<td>19 (10)</td>
<td>4 (10)</td>
</tr>
<tr>
<td>49-54</td>
<td>47</td>
<td>16 (10)</td>
<td>16 (10)</td>
<td>3 (10)</td>
</tr>
<tr>
<td>55-60</td>
<td>40</td>
<td>11 (10)</td>
<td>12 (10)</td>
<td>2 (10)</td>
</tr>
<tr>
<td>61-66</td>
<td>34</td>
<td>8 (10)</td>
<td>9 (10)</td>
<td>1 (10)</td>
</tr>
<tr>
<td>67-72</td>
<td>28</td>
<td>5 (10)</td>
<td>6 (10)</td>
<td>1 (10)</td>
</tr>
<tr>
<td>73-78</td>
<td>22</td>
<td>3 (10)</td>
<td>4 (10)</td>
<td>0 (10)</td>
</tr>
<tr>
<td>79-84</td>
<td>16</td>
<td>2 (10)</td>
<td>3 (10)</td>
<td>0 (10)</td>
</tr>
<tr>
<td>85-90</td>
<td>10</td>
<td>1 (10)</td>
<td>2 (10)</td>
<td>0 (10)</td>
</tr>
</tbody>
</table>


Respondents have to do a great deal of pointing and slow reflection before they can remember even fairly serious crimes of which they were victims some time ago—even when those 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."
Conflicting evidence

Evidence about the relation between incidences of forgery and forgetting is mixed. One of the first investigations was done as part of a study on a subject not related to victimization, the accuracy of reports. The results of that study were applied to the present analysis. Forgetting was found to be both forward and backward in time and to be more memorable- were more likely to be remembered, the error of report was of the same magnitude. In general, non-recall was more memorable in a crime, the value of a victim-offender relationship. This may be common in crimes that involve close victim-offender relationships, victim complicity, or victim precipitation. The police often suspect that monthly survey rates for reported crimes or unreported crimes deviated from official figures for specific crimes in the same fashion, indicating that there was no relationship between the police and the victim or criminal, and that the incident was not related to age, race, residential mobility, sex, social class, or the state of the situation. The other hand, because most incidents in the London study were recorded, there was little evidence to be inaccurate. Evidence about the relation between incidences of forgery and forgetting is mixed. One of the first investigations was done as part of a study on a subject not related to victimization, the accuracy of reports. The results of that study were applied to the present analysis. Forgetting was found to be both forward and backward in time and to be more memorable—were more likely to be remembered, the error of report was of the same magnitude. 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The other hand, because most incidents in the London study were recorded, there was little evidence to be inaccurate.
Telescoping within a reference period is much more difficult than controlling telescoping within a reference period. It is possible to increase the accuracy with which respondents remember events by asking them in the construction of their own reference periods. In the English Study by Sparks et al. (1977), the author was able to ask questions about respondents’ recent activities. Landlords were asked about “things which you did in the past year,” including vacations, major illnesses, births, marriages, and job changes. Interviews were solicited for each of these events, which are usually highly salient in the minds of the participants. When victimization information was gathered later in the interview, the incidents were put within the context of these events. It would be easier, for example, that an assault took place between a holiday and a marriage of a daughter. This appeared to increase the accuracy with which the dates of criminal incidents were elicited, for the known victim subgroups in the survey selected in the same fashion as those in the record check but exhibiting telescoping that American victim samples. The effects of telescopic victimization on crime rates are difficult to measure but are considerable. In the 1970 Washington, D.C., repeat survey, for example, the subset of respondents who in the initial interview were selected for interviewing because the last victimization had occurred during the “past 6 months.” About 15 percent of these out-of-bound incidents were pulled forward into the reference period. Over 20 percent of a sample of 13 murders had occurred within a 12-month reference period by respondents who had entered a new address during the past 6 months. In the same study, 12 of 100 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 6,000 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 property damage. This roughly the same magnitude of error that telescoping in Nye and Weisberg’s (1964) comparison of bounded and unbounded reports of household repairs: their unbounded interviews yielded 40 percent more reports of expenditures. The design of the National Crime Survey enables us to make comparisons between bounded and unbounded interviews on the same subjects and to report on differences between estimates of the reference period victimization rate for several crimes based on interviews conducted for repeat, bounded samples and new, unbounded interviews. Table 6 reports on the differences between estimates of victimization rate for several crimes based on interviews conducted for repeat, bounded samples and new, unbounded samples. The data are for the period January to June, 1973. In every case, the new, unbounded interviews reported 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 percent, a very substantial difference attributable to this single methodological difference. It should be noted that the data presented in Table 6 actually understate the impact of telescoping on victimization. Many interviews in the “bounded” column in Table 6 were in fact unbounded. In theory, individuals who are interviewed for the first time are questioned to establish an initial reference period and to gather data for use later to eliminate duplicate reports of events. In practice, these are more complex. The U.S. Census Bureau conceives of its survey sample as a sample of addresses. Thus it treats an address as bounded when at least one person there has been interviewed. As a result, many respondents are asked about crime in the past 6 months of a loss affected the accuracy of its recall. For example, she found that survey estimates of loss and property values consistently were higher than police figures. Victimization reports were more likely than police reports to mention small offenses. Police reports and victimization data on a variety of incident attributes. She found that survey estimates of loss and property values consistently were higher than police figures. Victimization reports were more likely than police reports to mention small offenses. Police reports and victimization data on a variety of incident attributes. She found that survey estimates of loss and property values consistently were higher than police figures. 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Other factors contributing to the omission of offenses during an interview may not be time dependent. Record checks strongly suggest that incidents involving close relationships between the parties are withheld from interviewers.

months in the past, yet none of the error 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 criteria for accuracy employed in the survey pretests suggested the most stringent of checks was the most erroneous of choices.

Differential productivity of respondents

Research on general survey methodology indicates that respondents also differ in their willingness or ability to adopt a productive role during an interview (Sadun and Bradford, 1976). In general, more highly educated respondents are more cooperative, 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 we 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 recall victimizations at a higher rate than did other respondents; this pattern may be due to differing definitions of victimization and systemic variations in the probability that events will be recalled in an interview. On the other hand, researchers suspect that significant negative associations between social position and victimization are masked in the survey findings by greater interview productivity among more highly educated and less-risk respondents. Further support for the notion that not (in)correct measure entry into a "rational theoretical culture" in which surveys, questionnaires, and opinion polls are strongly favored is 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 requiring 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 interviewers. There is also evidence that the 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 "serious" category, which is composed of crimes for which discrete details could not be remembered. On the other hand, Schieller (1977) found in her record check that education was not related to any tendency for victims to give "don't know" responses to systematic differences between police reports and interview data on incidents. Based on this evidence, it appears that productivity differences may be accounted for only by the "recall and not" variety and thus at work only in the recollection section of the interview instrument. It remains unclear whether nonrecall error ever should be divided from errors in the recollection section descriptions gathered in the incident report section of the instrument.

Summary

Conceptual and measurement decisions have had a substantial impact on the volume and nature of crime in America revealed by victimization surveys. Between 1973 and 1978 trends for the major categories of offenses measured by the National Crime Survey were flat, revealing little increase in crime over that 6-year period (U.S. Department of Justice, 1979). This stability highlights the importance of conceptual and measurement problems for they have had a substantial impact on the apparent level of crime in all the events of the 1970s. The use of proxy respondents serves to deprive the apparent extent of victimization. When people are asked to recall events for others as well as for themselves, their own experiences predominate. In addition, using a household informant places a taxing burden on a respondent, and she may fatigue or become an important factor shaping his or her productivity. Proxy effects also may account for the sharp increase in victimization rates currently recorded between the ages of 13 and 14, the point when the National Crime Survey shifts from proxy to self-reports for youth. In addition, proxy respondents are questioned to gather data about people whom U.S. Census Bureau interviewers cannot arrange to interview individually. Under many circumstances these hard-to-reach household members may be persons whose lifestyle would lead them to expect more frequent self-reports of victimization (Widom et al., 1978).

There is also evidence of considerable variability in the ability to recall victimization data which is reflected in nonresponse and part in error due to forward telescoping. As shown in Figure 4, telescoping is based on one record-check that education was not related to any tendency for victims to give "don't know" responses to systematic differences between police reports and interview data on incidents. Based on this evidence, it appears that productivity differences may be accounted for only by the "recall and not" variety and thus at work only in the recollection section of the interview instrument. It remains unclear whether nonrecall error ever should be divided from errors in the recollection section descriptions gathered in the incident report section of the instrument.

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Conceptual and measurement decisions have had a substantial impact on the volume and nature of crime in America revealed by victimization surveys. Between 1973 and 1978 trends for the major categories of offenses measured by the National Crime Survey were flat, revealing little increase in crime over that 6-year period (U.S. Department of Justice, 1979). This stability highlights the importance of conceptual and measurement problems for they have had a substantial impact on the apparent level of crime in all the events of the 1970s. The use of proxy respondents serves to deprive the apparent extent of victimization. When people are asked to recall events for others as well as for themselves, their own experiences predominate. In addition, using a household informant places a taxing burden on a respondent, and she may fatigue or become an important factor shaping his or her productivity. Proxy effects also may account for the sharp increase in victimization rates currently recorded between the ages of 13 and 14, the point when the National Crime Survey shifts from proxy to self-reports for youth. In addition, proxy respondents are questioned to gather data about people whom U.S. Census Bureau interviewers cannot arrange to interview individually. Under many circumstances these hard-to-reach household members may be persons whose lifestyle would lead them to expect more frequent self-reports of victimization (Widom et al., 1978).

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The results of victimization surveys also are affected by procedural factors. These are difficulties inherent in fielding national retrospective surveys, the manner in which surveys are carried out on a day-to-day basis, and variations in survey procedures which arise in different places. In this chapter we consider three of the most important of these factors: Panel bias and attrition. Second, we will examine some of the consequences of the panel design of the National Crime Survey, including panel bias and attrition. Third, we will explore the implications of variations in modes of interviewing, comparing telephone and in-person methods. Finally, we will review evidence concerning interviewer bias in victimization surveys. There are considerable gaps in our knowledge of some of these topics, but enough data are at hand to suggest that the manner in which the surveys are being conducted has considerable consequences for the picture of crime in America which emerges from the end product.

Panel bias and attrition

The National Crime Survey is a panel study. As a result, the findings are affected by a variety of panel effects that lead to systematic biases in victimization data. Panel biases are artifacts in data attributable to the fact that respondents are interviewed again and again in a survey. These biases may arise because respondents tire of the task and either suppress reports of things the interviewer wants to know or fail to exert the effort necessary to succeed at it. Panel bias also arises when participation in a survey affects the attitudes or behavior of respondents. In one-time, cross-sectional surveys this presents no difficulty if those effects come later, but in panel efforts it will affect subsequent readings of the sample.

Panel bias

Panel effects are ubiquitous. In a review of the topic, Bailar (1975) reports that participation in prior interviews has depressed subsequent reports of household expenditures, repairs, and alterations. In addition, when the reports of new respondents to a survey are compared with those who are being reinterviewed at the same time, it was found that the recall of recent illnesses and of unemployment is lower among experienced survey participants. All of these reports of prior victimizations seem to be attributable to panel fatigue. In Bailar's (1975) study, incoming panel members reported rates of unemployment which were 30 percent higher than those reported by experienced respondents. (However, it may be that these different reports are attributable to telescoping.) In the National Crime Survey, respondents are scheduled to be reinterviewed seven times, at 6-month intervals. This is comparable to the Current Population Survey, in which respondents are to be questioned eight times. Wolman and Bayl (1977a) took advantage of this survey design to compare victimization reports for groups with differing degrees of panel experience who were being interviewed in the same month. They found generally declining rates of reported victimization as the number of times that respondents had been interviewed increased. The largest drop was between the second and third interview, when reports of various personal crimes declined between 4 and 10 percent.

Lehnen and Reiss (1978) tackled the problem using special tabulations for individuals which merged their responses over the number of waves of the panel. They argued that panel participants could have contradictory effects: participants might become fatigued by continued interviewing and restrict their output as a result, or their ability to recall events could be enhanced by this unexpected continuing interest in their experiences on the part of the Government. Their data did not speak unambiguously to these alternatives, however. They found that the number of times respondents were reinterviewed was negatively correlated with reports of victimization. But they included the results of bounding interviews in this analysis and it is not clear how greatly affected the outcome. They also found that respondents perceiving the interview as a test-wise task avoided those respondents as positively related to reporting victimizations in later interviews. We have seen that this could have several interpretations, but it does argue against strong test-wise task avoidance effects in the panel.

There has been much less research on the issue of how participation in a survey affects the subsequent behavior of respondents. Surveys which employ a panel design are particularly threatened by this form of bias. The issue is not that of accurate measurement but that panel participants are no longer representative of the population from which they are drawn if such effects are present. The Center for Political Studies at the University of Michigan conducts a biyearly election study using such a panel, and it has found to its distress that repeated interviewing has increased the proportion of panel members who are registered to vote (Trensay and Rahn, 1975). It confirmed that effect by a record check of voter registration lists. The only evidence on this issue available for the National Crime Survey relies entirely on the reports of participants. Those data indicate a substantial panel effect on one aspect of victim behavior. Experienced respondents are much more likely than first-time respondents to recall that they reported crimes to the police (Murphy and Cowan, 1976).

An important unanswered question is whether or not panel participation affects rates of victimization among panel members. The linkage between participation and victimization would be indirect,
through the possible impact of panel attrition on the adoption of self-protective measures. There is considerable correlational evidence that certain risk-reduction tactics—such as staying home—can reduce individual and household victimization rates (cf. Skeggs and Mas- field, 1980). If repeated questioning about crime or interviews regarding the serendipity of various offers, has had an impact on how well subjects adopt them.

Following 6 months, the surveys may be underestimating the true extent of victimization in the Nation.

Panel attrition

Baseline in panel data also may reflect se- lection attrition in panel membership. Any panel study which extends over a 30-year period inevitably will be threat- ened by the loss of participants. (It was pre- dicted that about 10 percent of the American population moves each year). Even panel studies which vigorously attempt to follow relo- cating families are hard pressed to pre- serve their original sample. The National Crime Survey panel currently is organ- ized as a study of residents of sample addresses. No attempt is made to retain diaries. As a result, a considerable proportion of the people initially interviewed at those addresses do not participate throughout the life of their panel. Those who replace them at those addresses are treated as "replacement households" and represent their dwelling units (in an initial- ly unbounded state) in ensuing rounds representing their dwelling units (in an initial- ly unbounded state) in ensuing rounds.

A problem arises because this replacement policy does not ensure that "replacement" procedure is far from random. In fact, it is not even possible to say how many or proportion of those reporting a victimiza- tion remained in their households or moved to new homes of those reporting a victimiza- tion. The panel replacement policy is one reason to expect that changes in victimization rates over time will be underestimated.

Telephone versus personal interviews

While the National Crime Survey and the victimization surveys conducted in 36 cities are personal interview studies, in each case a substantial proportion of in- terviews was conducted via telephone. In the National Crime Survey, contact with a sample household is initially estab- lished by the personal visit of a survey in- viewer. During this visit the interviewer in- terviews 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 to choose the easiest and most cost- effective method (U.S. Census Bureau, 1979). We do not have a reliable reading of the consequences of this procedure.

Vigorous arguments can be made in sup- port 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 casual 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, Rod- gers (1976) found that telephone reports of incident timing and source of information to vote were more accurate. On the other hand, the use of telephone interviews may diminish the impact of the sample selection process. In its Health Survey experiment, the survey was designed in such a way that the sample Mul- han found a difference between responses via telephone and in-person interview, while that which favored the in-person strategy (Campbell et al., 1979). On the other hand, the Census achieved virtually identical results in research of two tech- niques (Grove, 1979). Those who favor personal interviews also argue that the data are "better" when collected in that way because of the greater rapport that can develop between interviewer and re- spondent. Also, in adequate settings, in- terviewers can supply more verbal and interpretive help to shape respondent be- havior, and both parties may be more satisfied with the emotional rewards of the interview. Comparison by mode of interview indicates that respond- ents and interviewers are less satisfied with telephone interviews (Grove, 1979). Cannon et al. (1976) reports that supply less recall is report to open- ended questions given over the telephone (Grove, 1979). They also are more likely to conduct personal interviews. Evidence on the relative validity of data gathered in each way is important, for the selection of collecting strategy employed in the crime surveys is distributed in a deliberately nonrandom fashion. Dur- ing the first few years of the National Crime Survey, about 50 percent of all in- terviews were conducted by telephone (Klecka and Tuchfarber, 1979). It also is apparent that respondents in telephone surveys are less accurate in their recollection of the facts as well as of the way in which the data were collected. This is particularly true for questions about police contact or experiences with the criminal justice system. 

Interviewer effects

In addition to panel artifacts and biases related to mode of interviewing, differ- ences are found in how the various interviewers go about their way. They carry out their tasks also that shape the picture for the outcome of victimization. Interviewer effects are not all but one of several sources of "correlated responses." Billie (1977). These effects manifest themselves as variance in outcomes which is shared among re-
Assessment

Current state of the art

The National Crime Survey shoulders a difficult task, that of measuring the extent of a complex social process. Most sample surveys confine themselves to more manageable topics: they elicit information about simple behaviors. The seemingly straightforward concept such as unemployment suggests the difficulty of probing such phenomena. It took decades of research effort to arrive at the current (and still controversial) procedure for examining labor force participation, and in the process researchers were forced to drop all efforts to see the concept to interpretations by respondents of their own status. People's assessments of their own status, whether or not they are employed, are subject to various interpretations (Cannell et al., 1979).

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 interview. Some probe for detailed comments, others for general assessments. The experience of the Bureau of the Census. In the city victimization surveys, the most systematic systematization of the interviewers' behavior (Cannell et al., 1979). Dodge and Lentzner (1978) noted that errors in interview productivity and interviewer effects are a less serious problem in those areas as well. Thus, despite all of their difficulties, the data generated by the crime victimization surveys have enormous potential for clarifying many issues in criminology.

The largest problem area remains the data on assault. While the complexity of victimization surveys data demands that we interpret all the data with care, the methodological shortcomings of the enterprise all too often 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 percent of all assaults were recovered in the interviews, and less than one-third of those were placed in the correct month by their victims (Yost and D'Andrea, 1970). In San Jose, 48 percent of the victims of assaults recalled the event, but that percentage dropped to 32 per cent among those who were victimized by acquaintances or members of their own families (Turner, 1972a). It was apparent in the index interviews that the interview procedure was not eliciting thorough accounts of

...
interpersonal violence and that the problem was acute in the case of nonstranger 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 and report assault.

These assault-linked shortfalls in the data do not correspond with estimates at events in this category, overwhelm almost all involve assaultive violence. Error in the measurement of interpersonal violence which is related to the differential productivity of respondents may account for the observation that blacks report far fewer reports of minor assaults than do whites. The most trivial form of violence in the crime survey is a "temporal assault without a weapon," which constitutes more than 40 percent of all injury and in which no weapon was used. Thus, a crime ever occurred is inferred, and most of these events may be described as threatening events. By the same token, we may infer that more educated respondents may less report events of experience with crime.

Improved interviewer techniques may assist less educated respondents to improve the recall task. This research agenda for the National Crime Survey concerns the optimal length of the recall period. A shorter period may lead to less nonrecollection. If the problem could be to shorten the recall period, and thus increase the proportion of them that will require other strategies for granting respondents more time to respond.

Interpersonal violence

The phenomenon with which is most often associated with what is known as the dynamics of interpersonal violence. The evidence from cross-national victim surveys, which are represented for only about 25 percent of all urban murders. Homosexuality and assault are multiple victimization, differing primarily in their...
By their nature we will never be able to discern victim proneness. We will never be able to understand the concept of crime-specific victim proneness. This is beyond excessive reference periods. This is beyond the current definition of a bounded interview, which has little to do with the social-psychological process of role-taking which it was designed to protect against. Selective panel attrition appears to affect estimates of the level of victimization made from the survey, and limits the utility of the data for studying one extreme reaction to crime, namely, moving elsewhere.

Finally, there needs to be a great deal of research on the implications of telephone interviews for the National Crime Survey, both for the data as they are currently collected and for the future organization of the program. As the cost of conducting the survey mounts, these inevitably will be pressure to convert to a telephone survey. Many local victimization surveys are now conducted by telephone (see Abt Associates, 1977; Bogdan, 1978; Statistics Canada, 1979). We know little about the implications of the use of the telephone for undercoverage, nonresponse, and response error. We should be prepared to speak to the costs and benefits of the use of telephone interviews at the national level.

These and many other issues are now being considered by the U.S. Census Bureau and the Bureau of Justice Statistics. A large-scale project to redesign much of the National Crime Survey has been under way since 1973, directed by Albert Biderman of the Bureau of Social Science Research, Inc., in conjunction with researchers and users of victimization data from throughout the nation. The research to be conducted by this group should illuminate many aspects of the data which have already been collected in the national and city surveys, as well as serve as a model for conducting future victimization surveys.

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