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The National Crime Survey: Working Papers

Volume I: Current and Historical Perspectives

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Validating reports of victimization

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The National Crime Survey: Working Papers

Volume I: Current and Historical Perspectives

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Abstract

Selections are presented pertaining to the objectives and design of the National Crime Survey (NCS), accounts of the the design, and a discussion of conceptual issues associated with measuring victimization. Examples of problems and prospects for using NCS data are also presented. The National Crime Survey, sponsored by the Bureau of Justice Statistics, is a complex survey having a wide range of applications for administrators, planners, and policymakers at all levels of government and in the private sector. On a staggered schedule, a large national sample (nearly 132,000 people) is interviewed two times a year for 3 years about crimes suffered during the previous 6 months. Established in 1973, the survey is designed to measure the levels of criminal victimization of persons and households for the crimes of rape, robbery, assault, burglary, motor vehicle theft, and larceny. The survey distinguishes between crimes reported to the police and those not reported to the police. The survey also collects the circumstances surrounding the crimes, which can be used to predict what groups of people are more likely than others to be crime victims.

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Preface

The National Crime Survey is a Federal statistical program established in the early 1970's by the Law Enforcement Assistance Administration to measure the annual levels of victimization from criminal activity in the United States. The survey is designed to measure the levels of criminal victimization of persons and households for the crimes of rape, robbery, assault, burglary, motor vehicle theft, and larceny. The National Crime Survey was developed for the Law Enforcement Assistance Administration by personnel detailed from the Bureau of the Census and is now administered by the Bureau under an interagency agreement. The program was transferred from the Law Enforcement Assistance Administration to the Bureau of Justice Statistics in December 1979.*

The National Crime Survey is a complex social survey having a wide range of applications for administrators, planners, and policymakers at all levels of government and in the private sector. Recent user studies have indicated, however, that the potential of victimization surveys has not been fully realized. The Bureau of Justice Statistics has therefore commissioned a series of monographs to expand the public's understanding and use of victimization surveys.

Two of the volumes, prepared under the general title *The National Crime Survey: Working Papers*, record much of the conceptual development and research activity that preceded the establishment of the current National Crime Survey design. They also provide the user with information suitable for developing applications and interpretations of National Crime Survey statistics. These volumes provide documentation on a range of methodological subjects pertaining to the National Crime Survey design and questionnaire.

Many of the documents found in these volumes were not intended for broad dissemination. The majority of the source materials are conference papers, interoffice and interagency memoranda, and reports prepared by Bureau of the Census and Law Enforcement Assistance Administration personnel, consultants, contractors, and grantees. They were directed at relatively narrow questions and limited audiences.

*Most of the papers in this volume were prepared during the period that the National Crime Survey was sponsored by the Law Enforcement Assistance Administration. Readers interested in current information about the program should contact the Bureau of Justice Statistics.

The principal editorial task involved in preparing these volumes was to provide a continuity of thought and analysis among the separate papers and to retain the ideas and expressions of the individual authors while editing them for style and format and removing some redundant material. The ideas and opinions expressed in these papers are those of the authors and do not necessarily represent either the position or policies of the United States Department of Justice or of the editors. It is important also to keep in mind that many of the papers were written some years ago and conclusions and interpretations made at the time may well be viewed differently today.

Volume I: Current and Historical Perspectives presents selections pertaining to the objectives of the National Crime Survey and its design, the early methodological and organizational steps establishing the design, conceptual issues associated with measuring victimization, and examples of problems and prospects for using National Crime Survey data.

Volume II: Methodological Studies contains a series of technical papers on methodological issues associated with the survey. These topics include the issues of memory failure, recall bias, classification of victimization events, sample design and coverage problems, response effects, and consequences of telephone versus in-person interviewing.

Our selection among the many documents available for inclusion in these volumes was guided by several considerations. Unpublished documents and materials published in relatively inaccessible places were given high priority for inclusion. Nevertheless, some relatively easy-to-obtain material has been included for the sake of continuity and completeness.

During the early years of the National Crime Survey program, national victimization surveys also included commercial establishments, and special surveys were conducted in 26 cities. The commercial and city surveys no longer are being conducted and are not likely to be duplicated in the near future. Documents pertaining to these special surveys have not been included in these volumes.

The editors wish to thank the many contributors whose work became the basis for these volumes. In addition, we wish to acknowledge the contributions of Robert J. Breitenbach, Ronald J. Leffler, Richard L. Roberts, and Marlene B. Simon, who assisted us in selecting these materials and preparing them for publication.

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The development of the National Crime Survey

Introduction

The victimization surveys conducted for the Law Enforcement Assistance Administration (LEAA) and now for the Bureau of Justice Statistics (BJS) trace their origin in the research and development effort of the President's Commission on Law Enforcement and Administration of Justice, commonly known as the Crime Commission. The Crime Commission supported several efforts to develop more reliable information on the distribution of crime in American society. The Nation's primary source of information about crime, the FBI's Uniform Crime Reporting System, provided only limited information concerning victims, and there was widespread concern that those statistics, which recorded only crime known to the police, did not accurately portray the true state of affairs regarding the volume and type of crimes.

During the mid-1960's, the Crime Commission sponsored methodological research on the measurement of victimization and attitudes toward crime, and they funded the National Opinion Research Center (NORC) to conduct a major survey designed to produce estimates of the true national victimization rate for common personal and household crimes. Although there were a number of difficulties with the NORC survey, it convincingly demonstrated that the volume of crime was much greater than official statistics of the time had indicated. As a result of these research efforts, the Crime Commission recommended that a National Criminal Justice Statistics Center be established to gather data on a continuing basis on crime and the costs of crime.

Another significant event in the history of the crime survey was a series of conferences held between December 1967 and March 1968 by the Census Bureau, to assess data needs of the criminal justice system. These conferences, attended by a wide array of criminal justice practitioners, identified a broad range of data needs, including the need for victimization surveys.

After the Congress established LEAA in 1968, a Statistics Division was established within the new organization to encourage the development of new sources of data on crime and criminal justice. Many of the staff in this new division came from the Census Bureau, and they were heavily influenced by the experience of the Crime Commission and the recommendations of the data-needs conferences. Because of their technical orientation, they understood the need for extensive preliminary research and feasibility testing in the development of the new social indicators that would flow from continuing victimization surveys. Beginning in 1969, LEAA fielded a series of preliminary field studies of the measurement of victimization, and made a host of decisions regarding the form such surveys would take. This development effort was undertaken in close cooperation with the Census Bureau and with expert consultants, many of whom continue to be involved in victimization research.

The first selection in this chapter is an excerpt from a paper by Richard Dodge and Anthony Turner of the Census Bureau. Both played major roles in the development of the crime surveys. Their paper outlines survey development efforts prior to mid-1971. These developments included:

a. choosing a rotating panel design with bounded interviews similar to the Current Population Survey;
b. choosing an ideal timeframe for asking respondents about their experiences;
c. assessing the reliability of crime recall;
d. weighing the advantages of interviewing an informant about incidents affecting others in his or her household, rather than individually interviewing everyone in the household;
e. determining a suitable lower limit for the age of eligible respondents;
f. exploring the advantage of mail or telephone interviewing in comparison with in-person methods;
g. developing a suitable questionnaire content and structure.

Following this paper is a chronology complete through 1979 of important milestones in the evolution of the National Crime Survey (NCS) program. It describes all of the major yield tests conducted in conjunction with the development of the surveys and summarizes their findings. These findings also are elaborated in two other volumes in this series, *Issues in the Measurement of Victimization* and *Volume II: Methodological Studies.* The chronology also indicates the scheduling of the city victimization surveys and other significant events in the history of the program.

The final selection in this chapter presents LEAA's statement of the goals of the NCS program. The program is intended to facilitate:

a. the creation of an ongoing social indicator measuring crime;
b. research and development on methodological issues in the measurement of crime;
c. research on issues of national concern involving crime and the criminal justice system;
d. local research and planning efforts;
e. studies of vulnerability and susceptibility to crime, multiple victimization, and other specific crime problems of population groups.

Methodological foundations for establishing a national survey of victimization*

by RICHARD W. DODGE AND ANTHONY G. TURNER

One of the primary responsibilities of the Statistics Division of LEAA is to provide timely statistical data on crime and its impact on society. Available statistics show counts of crimes that have been reported by citizens to the police and that the police, in turn, have reported in their statistics. However, evidence indicates that a significant volume of crimes committed against citizens never become known to the police. In addition, administrative statistics cannot provide the demographic and socioeconomic framework essential to understanding the broader impact of crime.

The Statistics Division of LEAA hopes to provide such data by establishing the NCS, which will be operated as a continuous national survey, administered by the Bureau of the Census to general probability samples of households, businesses, and institutions.

The core questions of the NCS will provide measures of the incidence of serious crime and the effect on its victims. Data available from the survey will include national estimates of crime events, the number of victims, the economic cost of crimes, multiple victimizations, characteristics of offenders, and victim-offender relationships. These data will be published to display the socioeconomic and demographic distribution of crimes and victims, as well as the geographic distribution (that is, national and regional data), and data for some of the very large cities and states.

In its initial stages, for reasons to be described later in the paper, the NCS will limit its focus to various forms of theft and interpersonal assaultive behavior. Later, as survey techniques are sufficiently developed and refined, we anticipate including the measurement of other types of crime.

In planning for a national survey to measure victim experiences, a host of methodological problems must be addressed, evaluated, and documented. Since early 1970, the Bureau of the Census has launched a broad series of pilot studies for LEAA to ascertain the feasibility of measuring the total incidence of major crimes through the use of survey techniques.

Earlier attempts by other researchers were not only very promising in showing the analytical value of victim surveys, but were also invaluable as pioneering efforts from the standpoint of suggesting several methodological questions for the Census Bureau and LEAA to address in their pilot tests. The only national survey ever undertaken was the NORC study of 1966. Criticism of this study pointed up the need to conduct further research on the differences in the amount of crime as estimated from questionnaires where the respondent reports for himself and from questionnaires where the respondent reports for others in the household.

Other surveys conducted for the Crime Commission during the mid-1960's were localized rather than national in scope. These studies, too, were useful in suggesting methodological problem areas, such as:

(1) What is the extent and nature of memory failure for victims of crime?
(2) What is the optimum length of the reference period for recalling crimes?
(3) What is the optimum mode of phrasing questions to avoid legal jargon for the answering public, yet to elicit responses that can be properly coded according to established standards for purposes of categorizing crimes?

This paper is devoted to a discussion of the methods tests conducted by the Census Bureau and LEAA to focus on the aforementioned problem areas. In addition, we will also touch upon the topics of questionnaire format, use of telephone and mail survey techniques, and the use of business records to assess commercial victimization. Some of the results are presented, although a number of methodological inquiries are still in varying stages of completion and data for them are not yet available.

Victim recall, telescoping, and other technical problems addressed through reverse record-check studies

A crucial issue in planning for a national household survey of victimization is the ability of respondents to recall incidents of victimization befalling them or other household members. Thorough study of this problem, and the related subject of telescoping, is needed in order to establish the optimum reference period to be used in the survey. Cost considerations become a significant element in this determination when it is recognized that cutting the reference period in half, from 6 months to 3, for example, necessitates a doubling of the sample size to achieve the same degree of reliability. Sample size is an especially critical parameter in setting up a crime incident survey since most major crimes, such as rape, robbery, or aggravated assault, are statistically rare phenomena. The recall problem has been more thoroughly studied by LEAA and the Census Bureau than any of the other methodological problems being considered here. The studies have taken the form of a series of reverse record checks with samples of known victims drawn from police-maintained offense records. To date, these tests have been conducted in Washington, D.C. (March 1970), Baltimore, Maryland (July 1970), and San Jose, California (January 1971). The San Jose test took place at the same time as the Pilot Cities Victimization Survey, conducted in both San Jose and Dayton, Ohio, which was designed to gather data on crime incidence from a general population sample.

There are certain difficulties in using police records as sources of samples. Only cases reported to the police are included. This leaves unstudied the large number of crimes that are not reported to the police and thus leaves unknown the degree to which recall problems for nonreported crimes differ from those that can be studied. A further problem in the use of police records involves sample selection. Our experience has been that although offense reports are public records, we have not been able to select a sample directly but have had to supply specifications to others. In general, the samples were quite satisfactory for our purposes, but errors in selection occurred that reduced the effective sample size. The most common of these were cases where the victim did not reside in the local metropolitan area or where the crime selected was directed against a commercial establishment or a person acting in a commercial capacity. Crime victims seem to be more elusive than the general population, especially victims of personal crimes, and we have had great difficulty in locating our respondents. Only through exhaustive interviewer efforts were we able to achieve response rates in the three tests to date varying from 63 to 69 percent. This, of course, is separate from the ability or willingness of respondents to report crimes of which they were the victims, once they have been located.

On the positive side, the advantages of using police records as a source for testing victim recall seem to us compelling. They provide a readily available sample of victims that, because victimization is a low-incidence phenomenon, would be costly to identify in any other way. And, most importantly, they permit a direct comparison of a respondent report in a household interview situation, some time after the event, with the actual official report of the same event made when memory failure was at a minimum. Recognizing that the offense report is not the entire truth of the matter, it nonetheless provides at the very least an anchor in time, not otherwise available, to which subsequent reports can be compared with a high degree of confidence.

The three pretests using samples of known victims had other purposes besides studying recall. The content of the questions that were designed to screen for incidents, the order in which they were asked, and specific question wording were modified each time as a result of field experience. In Washington, D.C., and Baltimore, victims were selected from four major crime groups—robbery, assault, burglary, and larceny. Cases of homicide and auto theft were not included because they are fairly well reported and not difficult to conceptualize. (In addition, victims of murder pose an obvious interviewing problem.) Questions on theft of automobiles and other motor vehicles were included although no such cases were sampled from the police records. This was done to distinguish motor vehicle theft from other kinds of larcenies. Rape was excluded from the first two tests because of the sensitivity of the issue. In San Jose, however, a sample of rape cases (one-half the size of the samples for the other crimes) was selected for interview. The screen questions that had been used previously to elicit reports of assaults were left essentially the same to see if they would elicit reports of rape. More explicit wording was rejected as not appropriate for a Federal agency to use and likely to be offensive to respondents.

In addition, revealed as a byproduct of these tests was the problem of classification of crimes. Various inconsistencies were noted between the police classifications and those made as a result of the personal interviews. To some extent, these variations brought to light defects in the questionnaires, which were subsequently corrected. Nevertheless, in the great majority of cases, there was sufficient detail obtained in the interview to enable a match to be made to the corresponding offense report.

The principal conclusions to emerge so far from these tests are these:

- If the objective is to determine whether a crime occurred, as opposed to placing it in a more accurate timeframe, then a 12-month reference period is as good as one of 6 months. This should be qualified by mentioning that two of these tests were anchored on the calendar year so that the furthest limit was one of the most salient of dates—New Year's Day. The recall bias that derives from time telescoping can be largely corrected by providing interviews with bounding information, that is, the record of incidents from the previous interview. The plans for the National Crime Survey contemplate a substantial degree of overlap in sample addresses from one collection period to the next—in the neighborhood of 75 to 80 percent.

- To the extent that it is desirable to place an incident in a specific timeframe, greater accuracy is obtained from a shorter reference period. Thus, a 6-month reference period is better than 12, and a 3-month period is better than 6. As was mentioned earlier, cost constraints become increasingly important as the time reference is shortened.

Beyond the ability to locate and interview respondents is the probability of the respondent's recalling a specific act of victimization, which was determined in these studies by matching a respondent report with an incident selected from police records. This probability was very high for crimes involving theft of property (80 to 85 percent). With respect to personal crimes, robbery was well reported (75 percent and above), but rape and assault were less so (66 2/3 percent and 50 percent, respectively). An important factor in the recall rates for cases of personal victimization is the relationship of the offender and victim. Recall rates vary directly with the nature of that relationship; that is, when victim and offender are strangers, recall rates are high (75 percent in San Jose). Acquaintance, and even more kinship, results in lower reporting rates, as low as 22 percent for relatives in San Jose. Since assaults are more likely to occur between people who are at least known to each other, if not related, we would expect recall rates for assaults to be low. Robberies, on the other hand, tend to occur between strangers (70 percent of the cases selected in San Jose) and, thus, recall rates are correspondingly high.

At the moment our conclusion is, when considered in connection with a continuing survey, that a 6-month reference period is better than a 12-month period for producing calendar-year data and for obtaining earlier and more timely results. With a 6-month rolling reference period, some data could theoretically be available after 12 months—assuming bounded interviews—and the data would be "centered" 3 months ago. For a 12-month reference period, 18 months would be required before data, comparably reliable, would be available and they would be centered 6 months ago. As was mentioned above, the sample size for a 6-month reference period is twice that for a 12-month period.

It is to be expected that any statistics that purport to measure the incidence of crime would inevitably be compared

*Excerpted from paper presented at the Annual Meetings of the American Statistical Association, August 1971.

with crimes known to and reported by the police, issued regularly in the FBI's Uniform Crime Reports (UCR). For the victim surveys, therefore, considerable effort has been expended in developing the instruments so that certain major crimes elicited can be classified in accordance with the definitions used by UCR. This has been done in order to make comparisons between UCR and victim survey results meaningful. On the other hand, it should be noted that tabulation plans call for presenting victim-event data in sufficient detail to permit analysts who so desire to describe crimes in ways that may depart from the constraints imposed by UCR definitions.

Successive improvements in the survey questionnaires used in the three pretests have been made to the extent that we now feel our ability to classify crimes according to UCR standards cannot likely be improved further. We feel that any remaining inconsistencies that may show up between police and survey classifications would be due largely to normal response errors, legal differences in the definitions of crime from one jurisdiction to another, and variable police practices in recording crimes.

Screening for incidents

In designing survey instruments for the various pretests and for the regular surveys to follow, it was decided to screen for all relevant incidents before obtaining details of any one incident. This was based on some experiences from previous surveys and also from our a priori judgment that better results would be obtained by letting the respondent remain in the incident-centered context while a series of specific questions attempted to elicit reports of victimization. This procedure has a very practical aspect, as noted by Biderman and Reiss, in that it takes advantage of the respondent's interest and freshness to establish the general victimization profile before proceeding to the specifics. The procedure of obtaining complete information about each incident at the time it is first mentioned runs the risk of boring or tiring the respondent, who can easily "forget" to report additional incidents. The screening procedure as adopted also has the added advantage of informing the interviewer of the total victimization picture so that he or she may be better able to assist the respondent in disentangling the facts of two similar larceny incidents, for example.

The content of the screening questionnaire itself poses crucial methodological problems. We have adopted what may be characterized as an approach somewhere between a brief screen consisting of perhaps one question concerned with each of the types of crimes in which we are interested and the alternative of compiling a lengthy list of very specific questions with which to bombard the respondent, explicitly mentioning a multitude of examples of the kinds of property that might have been stolen or the kinds of situations in which he might have been the victim of a personal crime.

We feel that the current version of the screen, while subject to further improvement, is a satisfactory compromise that achieves a reasonable measure of completeness of coverage without losing the respondent's attention. After each pretest we have modified the screen questions in order to overcome defects that have become evident. In the most recent version of the questionnaire, we have added two "catchall" questions to the end of the screen in a final effort to elicit incidents that the more specific questions have not brought out. These questions ask the respondent if he called the police to report something that happened to him that he thought was a crime and, second, if anything else happened to him that he thought was a crime but did not report to the police. As would be expected, these questions resulted in many reports of crimes other than those that are the focus of our studies—for example, vandalism, peeping toms, etc.—and also reports of noncrimes. However, they have also yielded descriptions of events that appear to qualify as one of the five major crimes. We use the word "appear" because the interviewer was asked to write as complete a description of the incident as possible, but did not fill out a detailed incident report form. In a number of cases, the description of the event was too sketchy to permit conclusive determination of what kind of crime had occurred.

In a nationwide experimental survey conducted in July 1971 and utilizing the Census Bureau's Quarterly Household Survey (QHS), interviewers were instructed to fill out an incident report on each situtation where the crime reported in the two catchall questions seemed to qualify as one that should have been mentioned in response to one of the earlier screen questions. We do not, as yet, have any results from this modification in procedure, but we do have some evidence from the surveys conducted in January 1971 on the kinds of events reported in these two final screen questions.

In the San Jose police sample, somewhat fewer than 3 percent of the successfully matched incidents were reported in the catchall questions. However, there were a number of other reports of one of the five crimes that did not match the selected sample cases. Larcenies and assaults were most frequently picked up as a result of these additional probes. A hand tally of responses to these questions in the Pilot Cities Survey indicated that as many as 5 percent of all incidents that qualified as one of the five crimes were reported in these two catchall questions.

Self-respondent versus household respondent

Another methodological problem of significance in establishing a National Crime Survey is the choice of the respondent in a household. The most economical approach is to interview any responsible adult who is home when the interviewer calls—which means that the respondent will often be a person who does not have an outside job or attend school. These respondents would report for themselves and all other eligible household members. For crimes where the entire household can be considered the victim (i.e., burglary, auto theft, etc.), this procedure may produce satisfactory results. However, for those crimes where a person is the victim, there is evidence from the surveys conducted for the President's Commission

on Law Enforcement and Administration of Justice that the household respondent reports other household members less frequently as victims than himself or herself, even though these persons are more likely to be exposed to crimes of this kind.

Interviewing all eligible household members individually is obviously a more expensive method. Less expensive would be the randomized predesignation of household members based on household size. This has serious implications on the overall effective sample size, however, since for a fixed cost it results in a sample size that is about 40 percent as large as if all household members had been included through the use of a household respondent. The decision as to which method to use has to balance the cost of the designated respondent procedure against the bias implicit in the household respondent approach.

A direct test of this problem was built into the Pilot Cities Victimization Survey. The sample households were divided equally in advance into those where a household respondent would be asked to report for all household members 16 years old and above, and those where each qualified household member would be interviewed individually.

At this time, only preliminary results are available based on hand tallies of raw data that have not been edited or weighted to allow for oversampling in the poverty areas of both cities. It is not known what effect, if any, editing and weighting will have on this comparison. The raw data indicate that the self-respondent households reported more incidents of crime than did those where the most available person responded for everyone. Although the interviewed households were almost equally divided, the self-respondent households reported 57 percent of all crimes. In addition, there was a tendency for certain crimes to be more frequently reported by persons in self-respondent households than the relative totals for all incidents would lead one to suspect. Petty larceny and assault were the principal examples of this. We would conjecture that petty larcenies are the most easily forgotten of all these crimes, but are likely to be better reported when each household member is interviewed, including the owner of the particular item that was stolen. Assaults, on the other hand, may not be "forgotten" so much as they may not always be known to other family members, because of embarrassment; or if they occurred between family members or friends, they may be edited out by the respondent. Whatever the reason, the involvement of all family members as respondents has a better chance of bringing out these reports, especially if the interviews are conducted separately.

In contrast to petty larceny and assault, auto theft was reported at about the same rate, regardless of the interview method involved. However, it should be pointed out that even in those households where everyone eligible was personally interviewed, certain screening questions were asked only once in the household and were asked of the first person interviewed, the equivalent of the household respondent in the other procedure. The screen questions that were deemed to fall into the category of household crimes that were to be asked only once were those concerned with burglary, larceny of household goods left outside, and theft of a motor vehicle or part of a motor vehicle. We would expect, therefore, that no significant difference would occur in the reporting rate for these crimes between the two procedures. If differences should appear, as in kinds of larcenies, they might be attributable to another household member volunteering such information during the course of the interview, having been reminded of a household crime during the course of the individual screen questions. Obviously, the distinction between household and individual crimes is somewhat arbitrary and respondents cannot be expected to sort their reality out as neatly as researchers would like.

There is also a "fatigue" factor associated with the use of a household respondent who has to report for all household members. We have adopted the rule that once the household screen questions have been asked, the individual screen questions must be asked about each household member in turn. Many respondents, especially when there are a number of other eligible household members, rapidly become conditioned and say something to the effect that the answer is "no" for everyone else, too. Interviewers find it difficult, under these circumstances, to follow the correct procedures and ask all questions, in turn, for each person—especially if it risks antagonizing the respondent. And, even if they persist, it is likely that the respondent, having decided that the answers are all "no," will not give any further thought to the matter. Our feeling is that this is a compromise procedure and, although it annoys some respondents, it probably evokes further reports of victimization that we would otherwise miss altogether.

Age of respondent

A problem that we feel is related to the type of respondent is that of the appropriate minimum age. The LEAA surveys (as of 1971) use age 16 as the minimum age for which victim data are sought. Sixteen is the age used to designate the lower end of the labor force. The decision as to what age is appropriate for the study of crime victims is, to some extent, arbitrary. Serious crimes can and do occur to younger people (robberies of newsboys, to cite a well-known example). On the other hand, threats, fights, and other events that would qualify, at least at the field collection stage, as crimes are common occurrences for many youth. Are these crimes of sufficient significance to warrant increased costs in the field only to be subsequently winnowed out at the processing stage?

To gain some insight into this problem, an experiment was conducted in five major cities in conjunction with the July 1971 QHS of Victims of Crime. In New York, Chicago, Los Angeles, Detroit, and Washington, D.C., interviewers were instructed to obtain information for all household members 12 years and above. Since all these interviews used a household respondent, we have not studied the problems of interviewing these young people themselves. Nevertheless, we expect to accumulate a body of useful information on this age group that will have a bearing on the selection of the type of respondent for the NCS. (Note: the NCS currently involves personal interviews with respondents 14 years of age and older, and quizzes older informants about those aged 12 and 13.)

Mail feasibility test

Mail as an alternate data collection technique offers obvious economies. If the expensive process of screening for instances of victimization could be conducted by mail, field costs could be cut drastically. Our assumption is that the details of reported incidents would then be collected by personal interviews. For the moment, at least, we feel that mail would not be appropriate as an initial contact, but could be utilized in a sample design that provided for multiple interviews over time with persons residing at designated addresses.

As previously noted, preparations for the inauguration of the NCS have included the use of the Census Bureau's QHS as a vehicle for testing questionnaire design and for collecting preliminary national data. The sample design of the QHS enabled us to conduct a mail feasibility test to run parallel with the personal interview survey in July 1971. The QHS sample is divided into six groups, each of which constitutes a national sample of approximately 3,000 occupied households. Each quarter a new group enters the sample and an old one completes its stay. The crime victim survey is being added to the QHS every 6 months. Thus, in the July 1971 survey, two-thirds of the addresses had been in sample for the previous survey in January. The other one-third, which had left the sample since January, was used for the mail test.

A mail questionnaire was designed containing a letter from the Director of the Census Bureau on the front and the screening questions, plus a few demographic items, on the inside. These questionnaires were mailed to coincide with the start of the regular personal interviewing for the July QHS. In August, a sample of nonrespondents to the mailing phase was followed up in the field. At the same time, interviewers were to collect details of incidents reported on the mail-screening questionnaire. For all addresses in the sample in January, interviewers were supplied with information as to their earlier report—either a brief summary of any incidents reported or an indication that there were no incidents or that the household was not interviewed in January. This bounding information was to be used only when incidents reported in July appeared to be duplications of those reported in January. One-half the households reporting incidents were designated for interview by personal visit, while the other half were to be obtained, insofar as possible, by telephone.

A comparison of the incident reporting rates for the mail survey with those obtained by personal interviews will indicate whether, or to what extent, mail can be used in collecting these kinds of data. The results of this experiment will be available some time next spring.

Recommendations for future methods tests

In the course of working with the various tests efforts to date, a number of methodological studies suggested themselves for the future. Some such studies might be undertaken prior to the establishment of the NCS, others in conjunction with the panel, and still others independently of the panel. Some of the possible methods tests under consideration are as follows:

1. A test of the effects on reporting frequencies under varying reference periods (e.g., within the past 3 months, within the past 6 months, within the past year), utilizing a general population sample with a multiple split-sample approach.
2. A test of whether a randomized-response technique is better than conventional questioning methods for eliciting reports of assaults (and perhaps rapes and robberies).
3. An experiment designed to compare the categories into which various police agencies would classify crimes on the basis of data elements determined from an interview survey.
4. A test of whether proxy-respondent reporting of crimes is different in amount and type from self-respondent reporting, utilizing a sample of known crimes from police files.
5. A test of whether the measure of change in crime incidence between two periods differs by type of respondent (self versus proxy).

We end this progress report on a tentative note. That is to say, we feel we have made a beginning in studying the methodological foundations for establishing a recurring National Crime Survey, but, in so doing, we recognize that much remains to be learned.

A chronology of National Crime Survey developments*

December 1969–February 1970

Preliminary planning discussions were held between Census Bureau and LEAA personnel (George Hall, Anthony Turner, and Dawn Nelson of LEAA; Daniel Levine, Richard Dodge, Sol Helfand, Ruth Asin, and Paul Shapiro of the Census Bureau). Advice and comments on proposed study, procedures, and questionnaire content and wording were solicited from consultants Albert Biderman, Albert Reiss, Marvin Wolfgang, and Ronald Beattie.

March 1970

The Washington, D.C., reverse record-check pretest of about 500 known victims of the crimes of assault, robbery, burglary, and larceny, selected from police files, was conducted to obtain information on reference period, recall ability, telescoping, and questioning procedures.

Major findings:

1. Assault victims had the highest noninterview rate, mostly because they could not be located.
2. Victim recall rates were higher for robbery and burglary than for larceny, and much higher than for assault.
3. Recall rates were slightly higher for crimes occurring 3 months prior to the interview, and were about the same for crimes occurring 6 and 11 months prior.
4. Accuracy of recall in terms of reporting correct month of occurrence was better for incidents occurring 3 months prior to the interview than 6 months, and better for those occurring 6 months prior than 11 months.
5. Forward telescoping occurred slightly more with a 12-month reference period than with a 6-month period.
6. Problems in crime classification related to definitions, question wording, and question order were identified.
7. Areas of procedural difficulties were identified, such as duplicate reporting and "on-the-job" multiple victims.
8. Several specific screening questions were much better for eliciting crimes than two general screeners.

*Source: U.S. Bureau of the Census, Crime Surveys Branch. Updated by the editors.

April 1970

A pretest of about 500 randomly selected businesses and other organizations was conducted in Cleveland and Akron, Ohio, to study recordkeeping systems for crimes of robbery, burglary, larceny (including shoplifting, employee theft, and bad checks), vandalism, auto theft, arson, and riot.

Major findings:

1. Victimization other than robbery and burglary could not be adequately measured.
2. Recordkeeping or lack thereof was not related to size or kind of business.

July 1970

The Baltimore, Maryland, reverse record-check pretest of about 500 known victims of crime was conducted to obtain additional information on optimum reference period and problems of victim recall ability, and to test improvements in crime classification and questionnaire design.

Major findings:

1. Assault victims again had the highest overall noninterview rate, and the largest proportion of "unable- to-locate" noninterviews.
2. The recall rate for burglary was slightly higher than for larceny and robbery, and all three were much higher than the assault recall rate. Recall of aggravated assaults was slightly better than simple assaults.
3. Recall rates were slightly higher for crimes that occurred in April (3 months prior to interview) than for crimes that occurred in January (6 months prior).
4. Accuracy of recall was best for burglaries and worst for assaults, and slightly better for crimes that occurred in January than in April (for all crimes except robbery).
5. Questionnaire improvement resulted in more accurate crime classification, though a few remaining weaknesses were identified.
6. Many additional questionnaire improvements for the future were indicated.

January 1971

A third reverse record-check study of about 600 known victims of crime selected from each of the previous 12 months was conducted in San Jose, California, to continue examination of recall ability for the purpose of determining optimum reference period. The crime of rape was tested for the first time.

Major findings:

1. Noninterview rates varied only modestly by type of crime, the highest rate for robbery and lowest for burglary.
2. Recall rates were higher for property crimes of burglary and larceny than for violent crimes of assault, rape, and robbery. Assault again had the lowest reporting rate.
3. The recall rate for crimes occurring within the previous 6 months was about the same as for crimes occurring within the previous 12 months.
4. Accuracy or recall was better for the previous 6 months than for 12 months.
5. Violent crimes involving strangers were reported better than those involving persons known to each other (but not related), which in turn were reported better than those involving relatives.
6. Police and survey classification of type of crime agreed to a large degree.
7. Survey dollar-loss data indicated losses greater than police assessment.

Household victimization surveys were conducted in San Jose, California, and Dayton, Ohio, using a probability sample of about 5,500 households in each area and a reference period of 12 months. A major purpose of these surveys, besides providing victimization data for program purposes, was to test survey instruments and processing developed to date on a general population sample. Another purpose was to begin development of procedures for large-scale clerical and computer data processing.

A major methodological study of the use of self-respondents versus a household respondent was built into this effort. Results demonstrated conclusively that the self-respondent method produced substantially greater reporting of incidents, particularly petty larceny and assault.

Additional information on optimum reference period was obtained by comparing estimated number of incidents from the first 6 months of 1970 to the last 6 months. The figures show a much greater proportion reported in the last half, indicating a combined effect of memory failure and telescoping.

Commercial victimization surveys were also conducted in San Jose and Dayton using an area sample of about 2,500 businesses. The major purposes of this effort were similar to those of the household surveys.

An additional 360 cases of known commercial victims were selected from Dayton police records and interviewed during the survey to test recall. Results indicated that the ability to recall correct month of occurrence decreased as elapsed time from incident increased.

The first nationwide household victimization survey was conducted as a supplement to the QHS, with sample size of 15,000 occupied housing units, and used a 12-month reference period. Similar supplements were conducted at 6-month intervals through July 1972, using a 6-month reference period, and provided the forum for further development of questionnaires, instructions, field procedures, clerical and computer processing procedures, and tabulations. They also served as the vehicle for further methodological studies and measurements. These surveys, though national in scope, were experimental only, and were not intended to produce substantive results for publication.

Data collected for 1970 in QHS were tabulated comparing estimated number of incidents for the first 6 months of 1970 against the last 6 months. Nearly 80 percent more personal crimes and 55 percent more property crimes were reported as occurring in the second half than in the first half of the year.

July 1971

The victimization supplement to QHS was conducted for the second time, using information from January interviews to bound July interviews for the four rotation groups that had been in the sample in January. Interviews in the other two rotation groups were unbounded. Higher victimization rates were obtained from unbounded interviews than from bounded interviews, indicating again that this technique may effectively control forward telescoping.

A test of screening for victimization by mail was also conducted in July 1971, by using the two QHS rotation groups retired since January, with personal followup of victims and nonrespondents. Higher victimization rates were obtained by personal interview than by mail, particularly for strong-arm robbery and aggravated assault.

Another experiment conducted in conjunction with July 1971, QHS in five major cities was designed to assess victimization rates for children 12 to 15. The lower age limit for previous victimization data collection had been 16, but information collected in this study demonstrated conclusively that 12- to 15-year-olds had substantial victimization rates.

January 1972

The third QHS victimization survey was conducted, incorporating various improvements in forms and instructions.

July 1972

The final QHS victimization survey was conducted.

The National Crime Survey was launched in the field utilizing a statistical design that was jointly developed by the Census Bureau and LEAA. The survey covers a general probability sample of households (NCS) and commercial establishments (Commercial Victimization Survey). The two major components of the survey are:

1. A national sample of 72,000 households and 15,000 businesses, one-sixth of which are interviewed each month and again at 6-month intervals, with rotation. Bounded data for persons 12 and over, households, and businesses are collected by self-response for persons 14 and over and by proxy for children 12 to 13, for a 6-month reference period, and are tabulated by calendar quarter of crime occurrence.

2. A sample of 12,000 households and 2,000 commercial establishments in each of 26 large central cities, interviewed yearly in groups of 5, 8, and 13 over a 3- to 4-month period in 1 year and reinterviewed 2 to 3 years later. Unbounded data for persons 12 and over, households, and businesses are collected by self-response for persons 14 and over, and by proxy for children 12 to 13, using a 12-month reference period, and are tabulated for that 12-month period.

July–November 1972

Cities Surveys were conducted in eight Impact Cities: Atlanta, Baltimore, Cleveland, Dallas, Denver, Newark, Portland (Oregon), and St. Louis.

July–December 1972

Approximately one-half of the national sample was introduced, the remainder not being ready at the time. All interviews were unbounded, and the information obtained was primarily used in bounding subsequent interviews.

January–April 1973

Cities Surveys were conducted in Chicago, Detroit, Los Angeles, New York, and Philadelphia.

January–June 1973

The remaining half of the national sample was introduced for the initial unbounded interview. In the repeating half sample, bounded interviews were conducted and eventually were to be the only interviews conducted during this period from which tabulations were produced for publication by LEAA.

Special tallies comparing bounded with unbounded data show victimization rates approximately 35 percent higher from unbounded data.

July–December 1973

Bounded interviews were conducted in the entire national sample.

January–April 1974

Surveys were conducted in the following 13 cities: Boston, Buffalo, Cincinnati, Houston, Miami, Milwaukee, Minneapolis, New Orleans, Oakland, Pittsburgh, San Diego, San Francisco, and Washington, D.C.

A test of interviewing 12- and 13-year-olds for themselves instead of by proxy (as is the general procedure) was conducted in San Francisco. Victimization rates appeared to be slightly higher by self-response, but the numbers were very small.

January–June 1974

Rotation started in the national sample, with the first incoming rotation group coming into sample during this period. Initial unbounded interviews from incoming rotations are used only for providing information with which to bound subsequent interviews and are not included in any victimization estimates.

July–December 1974

The first outgoing rotation group exited the sample.

January–April 1975

Surveys were conducted again in the cities originally interviewed in 1973—Chicago, Detroit, Los Angeles, New York, and Philadelphia.

February–June 1975

Surveys were conducted again in the eight Impact Cities first interviewed in 1972—Atlanta, Baltimore, Cleveland, Dallas, Denver, Newark, Portland (Oregon), and St. Louis.

July 1975

Expansion of Commercial Victimization Survey began.

Spring 1974–July 1976

National Academy of Sciences reviewed NCS developments. During this period, the National Research Council of the Academy conducted a thorough review of the sample design, survey procedures, questionnaire, data analysis, and reporting activities of LEAA and the Census Bureau. Its report, *Surveying Crime*, was published in 1976.

July 1976–June 1977

The Census Bureau conducted the "Maximum Personal Visit Maximum Telephone Interview" experiment. During this period, randomly selected subsamples of each monthly panel were interviewed by one of those methods whenever possible. The data were analyzed to explore possible artifacts associated with mode of interview.

Major findings:

1. A sizable segment of the samples could not be reached by telephone.

2. Generally victimization rates were higher among those interviewed in person, but only for 1 of 13 crime types were significantly different.

February 1977–December 1977

Marvin Wolfgang added a major research supplement to the NCS. Respondents were given descriptions of selected offenses and asked to rate their seriousness.

Major findings:

1. Respondents can give meaningful magnitude estimation ratings of crime seriousness in a mass survey context.

2. Seriousness scores were calculated for 204 representative offense types, ranging from very insignificant crimes to property, political, and corporate violations, and serious personal crimes.

September 1977

Data collection for the National Commercial Victimization Survey was suspended.

October 1977

Congressional hearings were held concerning the possible suspension of the NCS program. Nine witnesses were called to testify, and eight reports were entered into the record. The survey continued in full operation without interruption after these hearings.

February 1978

The Statistics Division of LEAA sponsored a 3-day conference of researchers and government personnel in Leesburg, Virginia, to consider the conceptual and methodological status of the NCS. The conference report describes an agenda for research and development in victimization surveys.

January 1979

The Statistics Division of LEAA released a Request for Proposal (RFP) for the redesign of the NCS. The contract was awarded to a consortium of organizations headed by the Bureau of Social Science Research in September 1979.

Current objectives for the National Crime Survey Program*

by the LAW ENFORCEMENT ASSISTANCE ADMINISTRATION

Based on a careful review of the National Academy of Sciences report, responses of persons who have been asked to comment on an earlier version of these objectives, consultation with the Office for Improvements in the Administration of Justice, and assessment of current LEAA needs, the following is considered to be an appropriate statement of the objectives of a publicly funded nationwide statistical series on victimization:

- To provide trend data that will serve as a set of continuous and comparable national social indic tors for the rate of victimization for selected crimes of violence and crimes of theft and for other factors related to crime and victimization in support of national criminal justice policy and decisionmaking and in support of informed public discussion.

- To conduct a program of conceptual and methodological research that will improve the victimization surveys in response to the National Academy of Sciences evaluation, including refinements of measurement, survey techniques, and questionnaire design.

- To exploit the depth and richness of currently available victimization data through analytical research on issues of public concern and of consequence to the development of national, State, and local criminal justice policy and legislation, with broad dissemination of findings.

- To assist State and local government efforts to improve the administration of criminal justice through (a) promotion of analysis of national data to understand local implications; (b) provision of national guidance on the feasibility, conduct, and utility of local victimization surveys; and (c) provision of a limited set of subnational social indicators derived from the national survey.

- To expand the current victimization survey to include assessment of vulnerability and susceptibility to crime of various segments of the population, and to explore governmental and private approaches for reducing the opportunity for criminal acts and the risk of victimization.

- To examine, through the longitudinal component of the survey, those factors associated with repeated or multiple victimizations to discover appropriate means of reducing such victimizations or minimizing their consequences.

- To use the ongoing national survey to obtain additional information on crime and criminal justice issues through supplemental questionnaires.

The list of objectives has intentionally been entitled "current," first to convey that some earlier objectives have been reviewed and discarded, and second that these objectives are subject to modification as the needs of the criminal justice system and policy concerns of the Department of Justice and LEAA subsequently may dictate.

Some objectives of the National Crime Survey that have been cited in earlier documents have proven undesirable on a cost/benefit basis or are simply unworkable. The goal of obtaining quarterly victimization data has been discarded because it is exceedingly expensive in its implementation and because the state of the art in criminal justice intervention strategies does not permit responses to changes in victimization on a quarterly basis.

The assumption once held that victimization data could be used to evaluate local crime-reduction programs has proved false. Before-and-after victimization surveys measure only a small set of possible consequences of criminal justice programs and they are not able to isolate noncriminal justice program influences.

The concept of the National Crime Survey as an instrument for calibrating the UCR ignores the different conceptual bases of each, which, while enhancing the findings of the other, are not sufficiently congruent to permit revising the data from one source solely on the basis of the data from the other.

Perhaps most important, the objective of providing subnational data for States or metropolitan areas by means of either an expanded national sample to reach Standard Metropolitan Statistical Areas (SMSA's), as recommended by the National Academy, or through separate city surveys (which the Academy recommended that LEAA discontinue) is suspended as an objective for fiscal years 1978–79 for two reasons: first, the Academy states that an "...objective of producing operating intelligence for jurisdictions is inconsistent with the original purposes of NCS..." and second, even should we disagree with that view, the cost involved in expanding the sample size precludes further work on subnational areas in fiscal years 1978–79. The limited subnational data now referenced in the fourth objective would be derived from categorizing data from the national sample by type of area (urban, suburban, rural) and by characteristics of the neighborhood. In fiscal years 1980–82, funding levels permitting, the sample size could be expanded to achieve specific subnational data objectives that may be formulated at that time.

*Excerpted from a memorandum from James Gregg, Acting Administrator of LEAA, to Peter F. Flaherty, Deputy Attorney General, entered as testimony before the Committee on the Judiciary, U.S. House of Representatives, on October 13, 1977. It appears in full in the congressional document "Suspension of the National Crime Survey," 95th Congress, 1st Session, October 13, 1977, pp. 67-69.

Chapter 2

Validating reports of victimization

Introduction

The seven selections in this chapter each concern the validity of reports of victimization gathered in survey interviews. The initial planning studies conducted by the Law Enforcement Assistance Administration (LEAA) used a technique called a "reverse record check" to validate reports of victimization. This technique involved sampling victims of crime from a record system, in this case from police files, and interviewing them using the most current version of the survey questionnaire. Information from the two sources was compared to establish the ability of the survey instrument to recover descriptions of instances in victimization known to police.

The first of these record checks was conducted in Washington, D.C., in March 1970. This pretest had three objectives: to determine the length of the recall period to ask respondents about, to measure error in the recall of the dates on which incidents occurred, and to explore the use of broad questions rather than specific ones to spark the victim's memory of criminal events. This test employed a sample of 600 persons identified by records of the Metropolitan Police Department of the District of Columbia. They were victims of assault, burglary, robbery, and theft.

The pretest indicated that using a 6-month reference period resulted in more accurate recall than did questions about incidents during the previous 12 months. There was substantial error in the recall of the dates of incidents, with events that occurred before the beginning of the reference periods being moved forward in time and telescoped into the reference period. The pretest also revealed problems of questionnaire design.

The next recall study presented was conducted 4 months later, in Baltimore, Maryland. The goal of this test was to study improvements in the questionnaire, to examine problems in the classification of crimes, and to again investigate the optimum length of the recall period. Five hundred victims of crimes were sampled from police records. As in Washington, D.C., there was some difficulty in locating and interviewing many of them. Over one-half of the victims of assault selected for the sample were not found. This test revealed that recall varied substantially by type of crime. Recall of burglary was relatively complete, while recall of assaults was poor.

The last major record check conducted by LEAA took place in San Jose, California. This was the most sophisticated study, and tested what was considered the final version of the questionnaire. Victims of rape were included in the record sample for the first time. There was a concerted effort to insure that crimes reported in the survey could be classified in the correct analytic category. As a result of this test, a 6-month reference period was adopted for the National Crime Survey (NCS), a decision that has had great cost implications. In general, the shorter the reference period for a retrospective survey, the larger the sample must be to have relatively low levels of sampling error.

The San Jose record check was conducted in conjunction with a larger field test of victim-survey methods. Large-scale population surveys were carried out in San Jose and Dayton, Ohio, to test procedures that had been developed using specialized populations. This pretest also investigated one remaining data-collection option, the use of proxy informants to report on the victimization experiences of themselves and others in their household, rather than conducting interviews with everyone in the household. The experiment clearly indicated that the self-response approach was superior. Again, this finding has the effect of significantly increasing the number of interviews required for the National Crime Survey, and, thus, survey costs also.

The next selection, written by Albert D. Biderman, is excerpted from a series of memos concerning these pretests. He was acting as a consultant to the Census Bureau, reviewing the planning and execution of the tests. In these memos he reviews the methodological issues that the tests addressed and comments on the way in which they were resolved. He is particularly critical of decisions regarding the reference period of the survey and of the design of the "incident screen" section of the questionnaire.

The final contributions in this chapter describe record checks that were conducted by researchers outside of the Census Bureau. Albert Biderman reports on a study of injury victims. He sampled hospital injury cases and interviewed patients to find those who were the victims of crime. The study was designed to shed some light on the recall of assaults, which in previous record-check studies had been very poor. He concludes that a survey that gathered reports of criminal incidents within the context of a larger focus on the incidence of personal harms would produce data of great social interest and higher recall accuracy. He also pinpoints many problems in conducting surveys using samples of injury victims, and concludes that they alone are not a cost-effective mechanism for producing estimates of victimization rates for injury-producing crimes.

In the final selection Anne Schneider reports on a record check conducted in Portland, Oregon, in which crimes described by victims in a survey later were tracked through police files. This "forward" record check revealed patterns of forgetting and mistaken recall that are in many ways similar to those suggested by other studies. She found that errors in recall are largely random with respect to the characteristics of victims or incidents. However, there was a considerable mismatch between some incident descriptions garnered from the two sources, especially in the reported race of offenders.

The Washington, D.C., recall study*

by RICHARD W. DODGE

The Law Enforcement Assistance Administration (LEAA) was established by the Omnibus Crime Control and Safe Streets Act of 1968 as part of the Department of Justice and was authorized, as one of its functions, to develop statistical information on crime and criminal justice. A major effort in this regard will be directed toward the production of much-needed victimization data by means of nationwide sample surveys, to be undertaken by the Bureau of the Census under the sponsorship of LEAA.

As part of this project, the Demographic Surveys Division has begun work on the development of a national household survey designed to produce data on personal victimization. Previous studies have revealed problems in gathering such statistics that must be resolved before a major nationwide study can be undertaken. The first pretest, conducted in Washington, D.C., in March 1970, examined three of these problems. It was designed (1) to determine the most effective reference period about which to question the respondent to gain the fullest and most reliable information, (2) to measure the degree of forward telescoping, i.e., the tendency of the respondent to advance an incident occurring outside the reference period into that period when questioned, and (3) to explore the possibility of identifying incidents by a few broad general questions as opposed to a series of more specific probing questions. This is a report of that initial pretest.

Pretest design and field problems

The pretest employed a reverse record-check technique. With the complete cooperation of the Washington, D.C., Metropolitan Police Department, the victim respondents to be interviewed were identified and the dates of their victimization established from police records. The information given in the interview was then checked against that contained in the police report.

The original pretest design called for 600 personal interviews with victims of crime—150 victims of each of four crimes (assault, burglary, larceny, and robbery). These 150 cases were, in turn, to be selected from five different time periods. Three of these periods were chosen to test the accuracy of respondent recall directly and consisted of cases occurring 3, 6, and 11 months prior to the pretest. The other two were selected to measure the amount of forward telescoping and included incidents that occurred 7 or 8 months and 13 or 14 months earlier. The screening questionnaire was designed in two versions, one with a reference period of the preceding 6 months, the other with one of the last 12 months.

Since the initial police reports on crimes are public records, the selected reports were photocopied for Census Bureau use. Because the files also contain some confidential material, however, Census Bureau employees were not permitted to select the sample cases. This task was undertaken by Police Department clerical employees, in addition to their other duties. Two complications arose from this procedure. First, not all the cases were drawn according to specifications; some of those received involved burglaries, larcenies, and robberies committed against business establishments rather than against individuals. Further, a few additional cases involved complaints filed by persons living at too great a distance from the Washington, D.C., area to be reached easily for interview. Therefore, although more than 600 cases were actually selected, the combination of business crimes and out-of-scope addresses reduced the usable number to about 480. As it turned out, however, we would have had great difficulty in handling a workload of the size originally specified, because of interviewer problems to be discussed later.

Secondly, as noted above, the case selection activity was conducted by the police whenever their regular work allowed time. Since the police records in the District of Columbia are filed chronologically, selection was by the individual months we had specified. Because of the press of time, the selected cases were delivered and, in turn, assigned to interviewers on a flow basis. As a consequence, interviewers, working with their later assignments, often found themselves in the same neighborhood and even on the same streets they had previously worked with earlier cases.

Although initial contact with the police was made well over a month prior to the pretest, it was not foreseen that the initial exchange of letters and the subsequent arrangements would be such a slow process. Nor was there any advance indication that Census Bureau employees would not be permitted to select the cases directly.

These problems led to the conclusion that selection of the sample should be given top priority in planning future pretests involving the use of official records. Once the decision is made, sample selection can be taking place concurrently with the preparatory activities, such as questionnaire design, and can be finished with ample time to spare. As a means of eliminating selection of business crimes, greater emphasis should be placed on this point in early conversations, and detailed written specifications should be left with the police representatives.

Questionnaire design

The pretest questionnaire consisted of two parts—a series of screen questions designed to elicit specific incidents of assault, robbery, burglary, larceny, and auto theft, and five different incident sheets designed to obtain details of each of these victimizations. Auto theft was included in the pretest questionnaire, even though no cases were selected, so that it could be distinguished from other property crimes. Two versions of the screen were used, the only difference being the reference periods of 6 and 12 months. The screen contained two broad questions about property crimes and personal crimes, respectively, plus a series of probing questions intended to jog the respondent's memory by mentioning specific situations and examples. Depending upon the responses to these probing questions, the appropriate incident sheet was completed for each incident mentioned. Since the main purpose of this pretest was to determine the ability of victims to recall criminal incidents and the dates of these incidents, questions on other details of the crime were kept to a minimum. Problems encountered with this questionnaire will be discussed later in this report.

*Excerpted from: Richard W. Dodge. "Victim Recall of Crime." Washington, D.C.: U.S. Bureau of the Census, 1970 (unpublished memorandum).

Locating respondents

Interviewers were provided with the name of the victim and his or her address at the time of the crime; these were taken from the police record. As expected, many of these addresses (some as old as 14 months) were no longer current; in fact, 30 percent of the persons in the sample had moved, indicating greater transiency than that for the general population. Efforts to locate those who had moved included speaking with new occupants, with other family members, with resident managers, and with the local post office. Without doubt, the most valuable lead in this location process was the victim's place of work and work telephone which, fortunately, was available on many of the police reports.

The designated victim respondents received no advance word that they were to be contacted. Proxy respondents were permitted only as a last resort; they comprised only a handful of the total number of interviews. Relatively few interviews were completed on the first few days; some respondents had moved, and many more worked. This initial low production was, in many cases, due to the interviewers' not receiving all available information on the respondents. When supplied with working hours, telephone numbers, and place of work, the interviewers were able to schedule visits when they were likely to find the respondent home.

Once the telephone began to be used in the survey, part of the pretest design was forfeit, in that the respondent was forewarned that he or she was to be interviewed. Although some earlier surveys had indicated that persons contacted by phone were more likely to refuse than persons contacted personally, this did not prove to be the case in this pretest. However, over the telephone, people were more cautious and demanded a fuller explanation of the survey, its sponsor, and its purpose; in many cases it was necessary to reveal that the names had been obtained from the District of Columbia police records. This advance contact appeared in no way to bias the outcome; no respondent gave any sign of preparing for the interview.

Unless, for some reason, the element of surprise is absolutely essential, there appears to be no reason why the respondent should not be contacted directly, either by letter or by telephone, to arrange for the interview. The rate of completed interviews climbed appreciably when this advance notice was permitted, with no apparent adverse effects on the information obtained. A briefer and more straightforward introduction would probably be adequate if this direct approach to the respondent were adopted. Although a suggested introduction was supplied to the interviewers, they frequently shortened it, or otherwise modified it, by giving a lengthy introduction of their own. Perhaps a standard, even memorized, short introduction should be insisted upon. In addition, all relevant information regarding working hours, place of work, and telephone numbers should be provided to the interviewer. This information permits the interviewer to make and keep specific appointments, using other time to locate missing respondents and arrange for additional appointments.

Conducting interviews

The interviews were conducted by three current program interviewers with extensive past experience, three new interviewers recruited locally, and assorted staff personnel. The experienced interviewers and staff personnel appeared to master the interview content and technique quickly, due to previous interview experience and training for other surveys. The local inexperienced interviewers were, quite naturally, uneasy and overwhelmed at first by the interview situation. By the end of the pretest period, they were conducting interviews much more smoothly and efficiently. Their greatest shortcoming was a lack of initiative in locating respondents. They were simply unaware of the various resources available to them and the best means of using these resources to locate a respondent who had moved.

Since this pretest consisted of many cases to be covered in a short period of time, high production per interviewer was essential. In any similar future situations, the use of experienced interviewers only would be advisable. If it is necessary to use inexperienced interviewers, a more extensive training program should be designed exclusively for them, a program that would cover not only the specifics of the one particular survey to be done, but also general interviewing techniques and training in followup and locating skills. Additionally, they should be observed for several days prior to working on their own.

Results of the pretest

Interviewing for the initial pretest of the victims of crime survey was completed during the first 3 weeks of March 1970. Of a sample of 484 victims of crime assigned to the field, interviews were conducted with 326 respondents, resulting in a completed interview rate of 67 percent. This rate ranged from a low 55 percent for assault cases to a high of 77 percent for burglaries. Each interview took an average of about 14 minutes to complete. Only eight interviews took over half an hour.

Most of the victims of crime in the sample for whom interviews were not completed were so classified either they had moved out of the Washington, D.C., area or because they could not be located. A comprehensive analysis of all noninterviews by reason for noninterview, type of crime, and date of crime indicates that the failure to reach selected victims varied by type of crime. The hypothesis could be ventured that the difference in response rates perhaps reflects a difference in the usual victim of each type of crime. Specifically, in over half of the assault noninterview cases, the respondent could not be located, suggesting that victims of this strictly personal crime may tend to be more transient than victims of the three property crimes of burglary, larceny, and robbery, where noninterview reasons were more widely distributed among the various categories.

The major purpose of the pretest was to obtain information on the ability of victims of crime to recall the date of their victimization. Of 226 completed interviews, where the incident in question occurred within the 6- or 12-month period inquired about, 81 percent of the respondents (or 183) actually recalled the specific incident (to the best of our ability to judge a proper match, based on a comparison of the details of the respondent's report and the police report). Using the base of completed interviews in each time group, 62 percent of the respondents reported the crime in the correct month; 73 percent when it occurred 3 months ago, 60 percent when it occurred 6 months ago, and 49 percent when the crime happened 11 months ago. In addition, other respondents recalled that the crime had taken place, but did not identify the proper month of occurrence. That proportion of the total also increased with the length of the reference period—13 percent for cases only 3 months old, 18 percent for those 6 months in the past, and 29 percent for those which occurred 11 months ago. Though the numbers are generally too small to permit any valid conclusions to be drawn, they appear to indicate that victim recall may be better for incidents of robbery and burglary than for incidents of larceny, and much better than for assaults.

Using a different base, only those who recalled the crime, 77 percent placed the incident in the proper month according to the date that appeared on the police report. Where the incident occurred 3 months ago, 85 percent of the respondents placed it in the correct month; where it occurred 6 months ago, 77 percent did so; and where it occurred 11 months ago, 63 percent did so. These data are illustrated in table 1.

This pretest was also designed to obtain information on another problem, the forward telescoping tendencies of victims of crime. For this purpose, crimes occurring in July or August 1969 and in January or February 1969, were selected from police records. The victims of these earlier crimes were interviewed using the standard 6- or 12-month questionnaire, respectively, to determine whether or not they would tend to move the date of the crime forward in their memories to fit it within the time period about which they were queried. The results of this forward telescoping test appear in table 2 by type of crime. The data show that 17 percent of the respondents did indeed move the date forward from July or August to fit within the 6-month period beginning in September, and that 21 percent recalled crimes actually occurring in January or February as happening within the 12-month period starting in March.

A number of other tabulations of the pretest data were made by Census Bureau staff. With respect to whether respondents who recalled the specific incident also recalled reporting it to the police, it was discovered that only one respondent did not recall that the police had been notified. Table 3 shows who reported crimes to the police, for all incidents that were brought out in the interviews, by type of crime. It can be seen that almost 75 percent of all crimes were reported to the police by the victim.

The staff also compared the police classification of the type of crime with the interview classification of the same crime. Except for robberies, the police classification and the way a crime was reported in the interview generally agree. The robbery incidents apparently present a major problem for the classification of crimes in the interview situation. This confusion is particularly acute where purse-snatching incidents are concerned. However, some of the problems with the robbery classification arose from the design of the screen questionnaire. Since the larceny questions preceded the robbery question, most purse snatchings, either with or without force, were picked up as larcenies. A question on the larceny incident sheet determined if force was involved, but only if the incident was a purse snatch. Also preceding the robbery question were the assault questions that asked about threats with a weapon. Several victims of armed robbery responded affirmatively at that point, when robbery and not assault was the goal of the criminal. Finally, the situation was complicated by the fact that the District of Columbia police report all purse snatchings as robberies, even those where force is not involved. Apparently, at least four factors need to be considered in attempting to resolve this problem:

- the way the victim views the crime;
- the way the interviewer views the crime when the situation is not clear-cut as to the proper procedure;
- the order and wording of the questionnaire; and
- the definition of the crime itself.

Future questionnaire modification should also insure that multiple crimes, e.g., robbery-assault and burglary-assault, will be identified.

Table 1. Correct recall of month of incident, by length of recall period

Reported to police	Number recalled incident	Number recalled incident in correct month	Percent recalled incident in correct month
3 months ago—December 1969	74	63	85.1
6 months ago—September 1969	60	46	76.7
11 months ago—April 1969	49	31	63.3
Total	183	140	76.5

Table 2. Degree of forward telescoping, by date and type of crime

Date of crime	Victims' recollection of date of crime	Total	Type of crime: Assault	Burglary	Larceny	Robbery
July-Aug., 1969	Recalled correctly or did not recall	43 (83%)	10 (83%)	10 (67%)	17 (100%)	6 (75%)
	Telescoped forward	9 (17%)	2 (17%)	5 (33%)	0	2 (25%)
	Total	52	12	15	17	8
Jan.-Feb., 1969	Recalled correctly or did not recall	38 (79%)	8 (73%)	11 (85%)	9 (82%)	10 (77%)
	Telescoped forward	10 (21%)	3 (27%)	2 (15%)	2 (18%)	3 (23%)
	Total	48	11	13	11	13
Total	Recalled correctly or did not recall	81 (81%)	18 (78%)	21 (75%)	26 (93%)	16 (76%)
	Telescoped forward	19 (19%)	5 (22%)	7 (25%)	2 (7%)	5 (24%)
	Total	100	23	28	28	21

Table 3. Person reporting crime to police*

Crime	Total	Number of cases where crime reported by: Victim	Relative	Other
Burglary	136	102 (75%)	12 (9%)	22 (16%)
Larceny	117	95 (81%)	8 (7%)	14 (12%)
Robbery	30	25 (83%)	--	5 (17%)
Auto theft	17	11 (65%)	1 (6%)	5 (29%)
Assault	72	42 (58%)	6 (8%)	24 (33%)
Total	372	275 (74%)	27 (7%)	70 (19%)

*Based on reports by victims, classified by type of crime as reported in the interview.

The difficulty in classifying crimes is only one of several problems that became apparent during the course of the pretest. Another problem was that a single incident (for example, a robbery) was fairly frequently picked up in the screening section as two or more incidents (say, a larceny and assault) and was not always resolved in the course of the interview. Also, it was generally felt by those who took part in the pretest as interviewers or observers that the screen questions were a little too repetitious and complicated. A few policemen and teachers fell into the sample as assault victims; these assaults had taken place in the course of their work. While apparently these respondents considered the incidents serious enough at the time to file a police report, they were not able to single out these incidents in the interview. Rather, their responses were along the lines of "I get threatened nearly every day." Future pretests and the main survey will have to decide how to handle these "victims in the line of duty."

Assault cases apparently present a special problem for criminal victimization studies. The significantly low interview-completion rate for assault cases was noted above. Moreover, even when interviews were obtained in these cases, significantly fewer assault victims actually recalled the specific assault incident for which we had a police report, as compared with victims of the other crimes. In reviewing the police reports of these incidents, it became apparent that many of these "memory lapse" cases were fights, family altercations, or other situations in which the attacker was known to the victim. One was probably considered an accident by those involved, and two were "victimizations in the line of duty." A number of explanations suggest themselves: respondents may not have conceived of these incidents as legitimate assaults; such occurrences may not be so unusual as to be salient events in the victim's life; or some respondents may not have wished to mention family quarrels to an interviewer.

Two general questions were included to determine if they alone would be adequate to identify incidents; the pretest demonstrated rather decisively that these questions were ineffective. The specific probing screen questions elicited 145 more incidents than did the two general screen questions; only 38 of these additional incidents were attempted burglaries and auto thefts that the general screen was not designed to elicit.

Conclusion

The major focus of this pretest was to determine the optimum recall period for which persons can report specific incidents of victimization. Perhaps not surprisingly, the results indicate that the shortest (i.e, 3-month) period proved to be the best both in terms of the percent of those respondents who could recall the incident and those who could place it in the correct month. A certain amount of forward telescoping was also in evidence, but, unlike the incidents that are forgotten, this tendency can be controlled if a bounded interview technique is adopted, whereby information obtained in a previous interview is used to remind the respondent of incidents reported as occurring in an earlier reference period in order to prevent duplicate reports in later periods.

The pretest also demonstrated that the screening questions need further development; there was virtually unanimous agreement among interviewers and observers that these questions were long and repetitious. At the same time, the two general questions were not sufficient to bring out all the reported incidents. In addition, the sequence of questions influenced the reporting of particular kinds of crimes, especially so in the case of robberies that frequently were reported as larcenies. Thus, revision of the screen should attempt to devise a briefer series of questions that would be sufficiently probing to bring out the maximum number of reports of victimization. In working on this revision, consideration should be given to whether it is desirable to maintain the present attempt to distinguish among the various crimes—distinctions that are not so apparent to respondents. In any event, the emphasis on the next pretest should be placed on these kinds of problems, even though further experimentation with recall might be desirable.

The Baltimore recall study*

by LINDA R. MURPHY and RICHARD W. DODGE

The Demographic Surveys Division of the Bureau of the Census, under the sponsorship of the Law Enforcement Assistance Administration of the Department of Justice, is in the process of developing a household survey designed to produce national estimates of personal victimization. Several problems in gathering victim data have been identified in earlier studies and a series of pretests has been planned by the Bureau to study these problems before a major nationwide survey is undertaken.

The first pretest was conducted in Washington, D.C., in March 1970 and was designed to produce basic data on the recall ability of victims of crime, including the most effective reference period and the degree of forward telescoping. The second pretest, which is the subject of this report, was conducted in Baltimore, Maryland, in July 1970. This pretest was designed to study improvements in questionnaire design and problems of recall and classification of different crimes that were identified in the initial pretest, as well as to obtain additional information on the optimum recall period.

Pretest design

As in the initial study, the second pretest employed a reverse record-check technique. With the cooperation of the Baltimore City Police Department, the sample of victim respondents (together with the dates and other selected details of their victimization) was selected from police crime reports. In order to determine the ability of the victim to recall the particular incident, the information given in the interview was compared with that obtained from the police records.

The original design called for 500 sample cases of victims of crime—150 victims each of assault and robbery and 100 victims each of burglary and larceny. A larger number of assault and robbery victims was selected because the initial pretest indicated that greater problems of recall and classification were associated with these crimes. These 500 cases were, in turn, to be selected equally from two different time periods—crimes that occurred 6 months and 3 months prior to the second pretest, i.e., in January and April. These two periods were selected because the Washington, D.C., pretest showed that while the shorter time period produced more accurate recall, the difference was not very great, so that further testing of recall seemed desirable.

The screen questionnaire, modified in wording and question order as a result of the initial pretest, employed a reference period beginning January 1, 1970, or approximately 6 months prior to interviewing. Alternate methods for obtaining details of any crimes mentioned in the screen were tested in the Baltimore pretest. In one-half of the cases, details were to be collected on improved versions of the five specific incident sheets used in the Washington pretest—assault, burglary, larceny, auto theft (a special subcategory of larceny), and robbery. The appropriate incident sheet to be filled in was determined by responses obtained from the victim to the specific screen questions. In the other half of the cases, details of each crime mentioned by the respondent were to be collected on a consolidated general incident sheet. It was felt that the general incident sheet would simplify the collection of detailed information; would lighten the interviewer's burden, since he or she wouldn't have to keep as many incident sheets on hand; and would, furthermore, ensure that all the questions necessary for classification of type of crime would be asked for each incident recalled by the victim.

Sample selection

Computer listings were obtained from the Baltimore City Police Department containing complaint numbers of all assaults, burglaries, larcenies, and robberies that occurred in Baltimore in April and January of this year; and a random sample of complaint numbers was selected from these listings. Out-of-scope cases, where the victim was a commercial establishment or where the victim lived outside the immediate vicinity of Baltimore or was under 18 years of age, could not be identified on the listings. Therefore, a preliminary sample three times as large as necessary was selected.

The final sample selection was done in the Central Records Division at the Baltimore City Police Department headquarters. The police reports of the crimes originally selected were scanned individually and out-of-scope cases eliminated, until the appropriate number of in-scope cases was found. As a result of this procedure, the sample, although not a probability one in the strict sense, was fairly representative in that a cross section of cases was chosen from all police precincts. Since the Baltimore City Police would not allow microfilm copies to be made of their police reports, only a few selected details of each crime were hand-copied by Census Bureau and LEAA staff members, to be used later in the matching operation.

Questionnaire design

The pretest questionnaire consisted of two parts: a series of probing, specific screen questions designed to elicit mention of incidents of assault, burglary, larceny, auto theft, robbery, and attempts to commit any of these crimes; and either general or specific incident sheets (described above) designed to collect details of any incidents mentioned in the screen.

The screen questions were much the same as those used in the initial pretest, though several improvements in wording and order were made. However, two screen questions included in the Washington, D.C., pretest, asking about property crimes and crimes of violence in general, were eliminated because it had been found that many more incidents were elicited with the probing screen questions. Two different catchall questions were added to the screen used in the second pretest in an attempt to elicit mention of crimes selected from police reports that were not brought out by the specific probing screen questions. These questions were asked at the very end of the interview, after all incident sheets had been completed. They asked about any kind of crime committed against the victim respondent that had or had not been reported to the police.

Questions in the detailed incident sheets were included for three purposes: to aid in matching incidents mentioned in the interviews with those selected from police records, to permit classification of crimes according to FBI Uniform Crime Report definitions, and to test questions that will eventually be used to produce statistics in such areas as offender characteristics and multiple victimization.

Prior to administering the questionnaire, the interviewers were to fill in selected items of a Current Population Survey Control Card for the household. There was some difficulty encountered in using a document not specifically designed for the crime survey.

Beyond this obvious problem, however, the interviewers in Baltimore found that a number of respondents resented being asked for such personal information, which appeared to them to be unnecessary and irrelevant to the purpose of the survey. An explanation of the need for this information should be made available for interviewers to give to the respondent whenever it is required.

Locating respondents

Interviewers were provided with information from the police records to enable them to locate the designated victim respondent quickly and efficiently. In addition to name and address, they were given his or her home telephone number, place of employment or school, business telephone number, occupation, and working hours, whenever this information was available. Even though telephone number and place of work were available, interviewers were urged to make every effort to interview the respondent in person and at home. Furthermore, it was impressed upon the interviewers that this was a designated respondent survey and that proxy respondents were to be interviewed only as a last resort—i.e., if the alternative was no interview at all. Interviews with proxy respondents and telephone interviews comprised only a small proportion of the total number of interviews.

The persons selected for the survey were given no advance notice of their inclusion in the sample. Interviewers were provided with a very brief, straightforward introduction that simply asked for the respondent's cooperation in testing a questionnaire for the Department of Justice that was intended to measure the amounts and kinds of crime of which people are victims. In general, the interviewers found this introduction to be sufficient, although in telephoning ahead for an appointment they often found that a more detailed explanation was required.

Training

Interviewing began on Monday, July 27, and was expected to be completed, for the most part, by the end of that week, leaving only a small amount of followup work for the next week. Since the interviews were to be conducted by experienced current-program interviewers, it was felt that half a day's training would be sufficient. The brief training period was also preferred because each interviewer was assigned a rather heavy workload to be completed in a small amount of time. The training consisted mainly of an informal discussion of procedures and questionnaire content, followed by two mock interviews.

It became apparent as interviewing began, however, that the training had not been sufficient to familiarize the interviewers thoroughly with the questionnaire content and procedures before they began the field work. Although almost all of the interviewers appeared to master the interview very quickly after beginning work, some confusion persisted throughout the pretest period on such matters as skip instructions in the questionnaire, the use of specific incident sheets, etc. It is felt that a longer, more thorough, and more structured training is needed in the future.

Furthermore, since a large number of different types of criminal situations are commonly covered in the interview, not to mention the many uncommon situations mentioned by respondents, it is felt that future training sessions should include more mock interviews.

Results of the pretest

Interviewing for the second pretest of the victims-of-crime survey continued a full week beyond the expected completion date and lasted a total of 2 weeks, ending August 7, 1970. Much of the interviewing was done during the second week, whereas the plan had been to use that period for cleanup work. The major problem was the unexpectedly great difficulty in locating a large number of the selected victims of crime. Fully 20 percent finally could not be located, even though the interviewers possessed substantial information about the respondents that was expected to enable them to locate almost everybody. Undoubtedly, without this information, the proportion would have been even larger.

Interviews were completed for 362 respondents out of a sample of 527 victims of crime, for a completed interview rate of 69 percent. This rate ranged from a low of 63 percent for assault cases to a high of 78 percent for larcenies. The interview took an average of about 20 minutes to complete, regardless of whether general or specific incident sheets were used. An average of 1.3 incident sheets were filled per case.

Of the total of 165 noninterviews, well over half (63 percent) of the victims could not be located. Victims of assault had the highest proportion of "unable-to-locate" noninterviews, with 76 percent; and victims of larceny had the lowest, with 52 percent. These results provide further support for the hypothesis ventured in the report of the initial pretest, that victims of such strictly personal crimes as assault may tend to be more transient than victims of property crimes. However, it is not clear whether any of this transiency results from the victimization experience.

One of the purposes of this second pretest was to obtain additional information on the ability of victims to recall incidents of crime, and particularly their ability to recall the date of such incidents accurately. As indicated in table 4, of 362 completed interviews, 242 respondents, or 67 percent, recalled the particular incident that was selected from police records. This results from our judgment as to what constituted a proper match, based on a comparison of details obtained in the interviews and details taken from police reports. For a few cases, where the match was doubtful, Baltimore City police records were consulted again when it was felt they might contain additional information that could confirm or refute the match.

*Excerpted from: Linda R. Yost (Murphy) and Richard W. Dodge, "Report on the Household Survey of Victims of Crime: The Second Pretest, Baltimore, Md." Washington, D.C.: U.S. Bureau of the Census, 1970 (unpublished memorandum).

Table 4. Recall of incidents, by type of crime

Type of crime	Number of completed interviews	Number of recalled incidents	Recall rate (percent of interviews)
Assault	99	36	36
April	47	19	40
January	52	17	33
Burglary	77	66	86
April	40	33	82
January	37	33	89
Larceny	83	62	75
April	43	35	81
January	40	27	68
Robbery	103	78	76
April	49	38	78
January	54	40	74
All crimes	362	242	67
April	179	125	69
January	183	117	64

The recall rate ranged from a high of 86 percent for burglaries to a low of only 36 percent for assaults.

The recall rate for all four types of crime was only slightly higher for crimes that occurred in April (69 percent) than for crimes that occurred in January (64 percent). If the assault cases are subtracted from the total, the overall recall rate becomes 78 percent, with the proportions for April and January being 80 percent and 77 percent, respectively.

As for accuracy of recall, 57 percent of the victims of all crimes who recalled the particular crime were able to recall it as occurring within the correct month. This excludes 12 cases in which the incident was elicited by catchall questions or simply mentioned in an interviewer's note, and the date of the crime was, therefore, not ascertained. Victims of assault were least accurate, with 48 percent placing the incident in the correct month; and victims of burglary were most accurate, with 61 percent giving the month correctly. These figures on recall, by type and month of crime, are contained in table 5. Almost 90 percent of the respondents recalled the crime as occurring within 2 months of the actual month. It is interesting to note that respondents who did recall the incident were more accurate in their remembrance of crimes that occurred in January than for April crimes. Robbery was the only type of crime for which this situation was reversed. It is possible that the month of January is a more salient month in people's memories than most other months.

Seventeen proxy respondents were interviewed in the Baltimore pretest, and while this number is too small to permit any valid comparison to be made with the self-respondents, the general pattern of recall appeared to be similar. Compared with 67 percent recall for all respondents, 65 percent of the proxy respondents recalled the particular incident. The average number of incidents mentioned per proxy respondent was 1, compared with 1.3 for all respondents. Major research in the use of household respondents for reporting crime is planned for the near future.

Table 5. Accuracy of recall, by type and date of crime

Type of crime	Total number recalled incident and gave data	Correct month	1 month difference	2 months difference	3 or more months difference	Within 2-month span including correct month	Series (7-month span including correct month)
Assault							
April	15 (100%)	6 (40%)	2 (13%)	5 (33%)	---	2 (13%)	---
January	10 (100%)	6 (60%)	3 (30%)	1 (10%)	---	---	---
Total	25 (100%)	12 (48%)	5 (20%)	6 (24%)	---	2 (8%)	---
Burglary							
April	33 (100%)	15 (45%)	14 (42%)	---	---	1 (3%)	---
January	33 (100%)	25 (76%)	3 (9%)	---	3 (9%)	---	---
Total	66 (100%)	40 (61%)	17 (26%)	---	3 (5%)	1 (2%)	---
Larceny							
April	34 (100%)	18 (53%)	7 (21%)	6 (18%)	1 (3%)	1 (3%)	1 (3%)
January	27 (100%)	17 (63%)	2 (7%)	5 (19%)	2 (7%)	---	---
Total	61 (100%)	35 (57%)	9 (15%)	11 (18%)	3 (5%)	1 (2%)	1 (2%)
Robbery							
April	38 (100%)	23 (61%)	9 (24%)	4 (11%)	---	1 (3%)	---
January	40 (100%)	20 (50%)	11 (28%)	2 (5%)	4 (10%)	---	---
Total	78 (100%)	43 (55%)	20 (26%)	6 (8%)	4 (5%)	1 (1%)	---
All crimes							
April	120 (100%)	62 (52%)	32 (27%)	15 (13%)	1 (1%)	5 (4%)	1 (1%)
January	110 (100%)	68 (62%)	19 (17%)	8 (7%)	9 (8%)	---	---
Total	230 (100%)	130 (57%)	51 (22%)	23 (10%)	10 (4%)	5 (2%)	1 (.4%)

Table 6. Recall of assaults, by type

	Total	Simple	Aggravated
Recalled	36	18 (33%)	18 (41%)
Not recalled	63	37 (67%)	26 (59%)
	99	55	44

Assault cases evidently pose special problems for criminal victimization studies using victims as the data source. In the first place, in both pretests, assault victims were much more difficult to find and interview than were victims of the other crimes. Secondly, when interviewed, assault victims were much less likely to report the incident that caused their inclusion in the sample. Victims of aggravated assault recalled the incident to a greater degree than victims of simple assault, but the difference is not very great, as shown in table 6.

An examination of the location of the assault does not provide any conclusive evidence on the recall problem. Aggravated assaults taking place within dwellings were more likely to be recalled than those occurring elsewhere (on the street, in taverns, etc.), 50 percent as compared with 31 percent. On the other hand, simple assaults occurring on the street, in taverns, etc., were slightly better recalled than those in dwellings (37 percent versus 30 percent).

The absence of offender information for unrecalled assaults prevents any examination of whether the offender was known to the victim, which is undoubtedly an important variable.

One of the major problems uncovered in the initial pretest in Washington, D.C., was that of classifying the reported crimes. It was found that significant differences existed between the police classification of crimes and the classification resulting from the interview. In the Washington, D.C., pretest, the interview classification was based on responses to the screen questions that indicated the specific incident sheet to be filled and not on information contained in the incident sheet, which was often inadequate to permit reclassification.

The classification problem was complicated by the order of the screen questions used in Washington, D.C., which tended to encourage reporting of robberies as either larcenies or assaults because questions on these two crimes preceded those for robberies.

Several improvements were made in the Baltimore pretest that permitted more accurate crime classification and, therefore, a better comparison with police classification. The order of the screen questions was changed so that robbery questions were asked prior to larceny and assault questions, and enough questions were added to each specific incident sheet to permit, in most cases, the accurate classification according to the FBI Uniform Crime Report definitions of each incident for which an incident sheet was filled. The only problem encountered with the specific incident sheets used in Baltimore was that they did not handle attempted robberies or larcenies very well, because respondents were not asked specifically if the offender had attempted to take something from them. Separate questions on attempts will be included in future questionnaires.

The results of these improvements are shown in table 7, which compares the Baltimore City police classification of all recalled incidents with the Census Bureau staff's classification according to the Uniform Crime Report definition. This table presumably reflects true differences in classification, and it can be seen that the differences are fairly small on the whole. The most significant differences are the 13 percent of cases classified by the Baltimore City police as larceny that were classified as burglary by the Census Bureau, and the 17 percent of robberies that were classified by the Bureau as larcenies.

In several of the latter cases, the respondent had indicated in the screen that he had been robbed by force or threat, but in the incident sheet denied that any force or threat of harm was used by the offender. Therefore, the incident was classified as larceny, since the decision was made to accept information obtained in the detailed questioning as correct in the event of conflict with information obtained in the screen. It is possible, however, that the respondent may have misunderstood the somewhat complicated question on the incident sheet as to use of force or threat and that the incident was actually a robbery and not a larceny. In future questionnaires, the respondent will be asked about actual use of force and threats separately.

In addition to the incidents that matched the police reports, an extra 233 criminal victimizations were elicited by the screen, and incident sheets were completed. Of these, 139, or 60 percent, had been reported to the police. This proportion varies by type of crime, ranging from 35 percent of larcenies to 84 percent of robberies. The number of additional incidents elicited by the screen questions by type of crime and whether

Table 7. Classification of recalled incidents

Classification by Baltimore City police	Total completed interviews	Not recalled	Classification according to UCR by the Census Bureau: Total	Assault	Burglary	Larceny	Robbery	Other	Unable to classify
Assault	99 (100%)	63 (64%)	36 (100%)	29 (81%)			3 (8%)	3 (8%)	1 (3%)
Burglary	77 (100%)	11 (14%)	66 (100%)		65 (98%)	1 (2%)			
Larceny	83 (100%)	21 (25%)	62 (100%)		8 (13%)	54 (87%)			
Robbery	103 (100%)	25 (24%)	78 (100%)	1 (1%)		13 (17%)	64 (82%)		
Total	362 (100%)	120 (33%)	242 (100%)	30	73	68	67	3	1

or not they were reported to police is given in table 8.

Tallies were also made of offender information for all assault and robbery incidents mentioned. These data are shown in table 9. For robberies, 82 percent of the offenders were strangers to the victim, while this was the case in only 41 percent of the assaults. In 93 percent of the robberies, compared with 78 percent of the assaults, the offender was male. More than half (63 percent) of the assault offenders were 21 years of age or older, while 20 percent were 17 through 20. The robbery offenders were younger in comparison—41 percent were 21 or over and 30 percent were 17 through 20.

Multiple victimization data were also tallied for all incidents of robbery and assault. The number of other persons besides the selected victims who were robbed or harmed is given in table 10. More than one person was victimized in only 12 percent of the cases.

A number of dollar-loss tallies were also made for the property crimes of larceny, burglary, and robbery. The average dollar loss for each type of crime, as reported in the interview, is compared with the average loss shown on the police reports in table 11. For those cases in which the particular incident was not recalled, only the police-reported average loss is given.

The number of cases in which the dollar loss reported in the interview was higher, lower, or the same as the loss appearing on the police report is given in table 12.

The catchall questions at the end of the screen questionnaire elicited 50 additional crimes not reported previously. Thirty-eight percent of the entries in those questions were classifiable as assaults (family disputes, arguments, etc.). Various other complaints such as vandalism, noisy neighbors, and so forth, accounted for an additional 48 percent. Eleven of these incidents (nine of which were assaults) were judged probable matches with police reports in the sample and, therefore, as recalled incidents. In addition, four incidents of crime were mentioned in interviewer notes on the questionnaire, two of which were assaults that were considered probable matches. Entries in the catchall items are more common if the two are asked immediately following the screen questions or if only one incident is recorded. In fact, 82 percent of all incidents mentioned in these two items were recorded when less than two incidents were reported. This seems to suggest that if multiple incident sheets have been filled out, then the respondent has reported all crimes occurring to him or her before the catchall questions are asked.

Table 8. Additional incidents, by type of crime and whether reported to police

Type of crime	Total	Reported to police	Not reported to police	Don't know
Assault	28	13 (46%)	14 (50%)	1 (4%)
Auto theft	9	6 (67%)	3 (33%)	--
Burglary	96	66 (69%)	30 (31%)	--
Larceny	60	21 (35%)	39 (65%)	--
Robbery	31	26 (84%)	5 (16%)	--
Other	9	7 (78%)	2 (22%)	--
Total	233	139 (60%)	93 (40%)	1 (4%)

Table 9. Offender characteristics for robbery and assault cases

	Total	Assault	Robbery
Relationship to victim			
Known	42 (28%)	28 (57%)	14 (14%)
Relative	12 (8%)	11 (22%)	1 (1%)
Stranger	101 (68%)	20 (41%)	81 (82%)
Don't know	1 (1%)	---	1 (1%)
NA	4 (3%)	1 (2%)	3 (3%)
Total	148(100%)	49(100%)	99(100%)
Sex			
Male	130 (83%)	38 (78%)	92 (93%)
Female	9	8 (16%)	1 (1%)
Both	3	---	3 (3%)
Don't know	1	1 (2%)	---
NA	5	2 (4%)	3 (3%)
Total	148(100%)	49(100%)	99(100%)
Age			
Under 12	4	3 (6%)	1 (1%)
12–16	19 (13%)	2 (4%)	17 (17%)
17–20	40 (27%)	10 (20%)	30 (30%)
21 or over	72 (49%)	31 (63%)	41 (41%)
12–20*	3	1 (2%)	2 (2%)
17 or over*	2	---	2 (2%)
Don't know	4	1 (2%)	3 (3%)
NA	4	1 (2%)	3 (3%)
Total	148(100%)	49(100%)	99(100%)

*Ages of offenders fell into two categories.

desirability of using one general incident sheet instead of several specific sheets. On the whole, it was felt that the questionnaire was a great improvement over the original used in Washington, D.C. The reordering of screen questions prevented most of the duplication of events experienced earlier, though in a few cases there was a tendency to obtain responses to both burglary and attempted burglary questions. This duplication was invariably reconciled in the interview, but the addition of the phrase "other than incidents already mentioned" to the attempt question in the future should halt the tendency altogether. Two screen questions remained that were felt to be too long and complicated; these have been revised.

Conclusion

One of the main purposes of the second pretest in Baltimore was to study the effectiveness of improvements in the questionnaire, as well as to test the

Table 10. Multiple victimization (for all robbery and assault incidents)

			Number of other persons robbed or harmed				
Type of incident	Total	None	1	2	3	Don't know	Not applicable
Assault	49 (100%)	42 (93%)	3 (7%)	1 (2%)	1 (2%)	1 (2%)	1 (2%)
Robbery	99 (100%)	83 (83%)	10 (10%)	2 (2%)	1 (1%)	---	3 (3%)
Total	148 (100%)	125 (84%)	13 (9%)	3 (2%)	2 (1%)	1 (.7%)	4 (3%)

Table 11. Average dollar loss

	Larceny		Burglary		Robbery	
	Interview	Police report	Interview	Police report	Interview	Police report
Recalled incidents	$107	$79	$267	$323	$45	$55
Incidents not recalled	---	55	---	85	---	124*

*This figure is considerably affected by one loss amount of $1,530. Excluding this loss, the average is $63.

Table 12. Comparison of dollar loss: Interview with police report

	Total	Larceny	Burglary	Robbery
Interview higher	91 (44%)	30 (48%)	33 (50%)	28 (36%)
Police higher	47 (23%)	15 (24%)	10 (15%)	22 (28%)
Same amount	49 (24%)	13 (21%)	10 (15%)	26 (33%)
Not determined	19 (9%)	4 (6%)	13 (20%)	2 (3%)
Total	206 (100%)	62 (100%)	66 (100%)	78 (100%)

The two general questions asked at the conclusion of the interview were found to be useful in eliciting mention of additional incidents not reported in the initial screening; but since the date of occurrence and other details of these incidents were not ascertained, it was frequently difficult to determine a match with the crime selected from police records. As for the efficiency of the general incident sheet, it undoubtedly simplified procedures for the interviewer, as well as the later classification operation. Most of the interviews, however, indicated that they experienced no great difficulty in carrying and shuffling about the five specific incident sheets, and actually preferred using them to collect details of crimes. They felt that the question on the specific incident sheets often seemed more relevant to the particular crime than did questions on the general sheet. In addition, the general incident sheet required more complicated skip patterns and was, therefore, more difficult to use in the interview. The problem with using specific incident sheets, however, is that crimes are not always correctly classified by the screen and, therefore, additional questions are needed on the incident sheets to permit accurate classification. Thus, the ability to distinguish between larcenies and burglaries requires additional questions to be added to each of the incident sheets, resulting in almost identical questions for both. The same thing is true of robbery and assault incident sheets. It is suggested that the most efficient method of collecting details of crimes would be to compromise and use two general incident sheets—one for the property crimes of larceny and burglary and one for the violent personal crimes of assault and robbery.

One finding of the Baltimore pretest that causes concern is the very low proportion of assault incidents recalled by respondents. In addition to the difficulty of locating designated victims of assault, only 36 percent of those who were interviewed were able (or willing) to recall the specific incident. This compares with recall rates of 75 to 86 percent for the other three crimes. Although assaults were the most poorly recalled of the crimes in the Washington, D.C., pretest, the discrepancy between them and the other crimes were not as great. The hypothesis could be advanced that the unrecalled assaults, whether forgotten or purposely held back, basically involve family altercations.

More research on this problem seems needed, including some experience with a general population sample.

A final matter that deserves further study is that of the most desirable recall period for reporting incidents of victimization. The Baltimore test did not demonstrate conclusively the superiority of the 3-month recall over that for 6 months. As was mentioned earlier, the fact that January 1 is an easily recalled date for most people may have improved their ability to remember events occurring 6 months prior to the interview date. Because the recall period is a crucial element in conducting victimization surveys, another record-check study is planned. This test will be designed to include cases selected from each month over a 12-month period. In addition to providing further data on recall, it is hoped that this study will clarify further the special problem of assaults.

The San Jose recall study*

by ANTHONY G. TURNER

In January 1971, a personal interview survey of known victims of crime was conducted in Santa Clara County, California, by the Bureau of the Census, under LEAA sponsorship and design specifications.

The survey utilized a probability sample of 620 persons who were known to have been victims of specified crimes during 1970. The sample was selected from offense reports maintained by the San Jose Police Department. The offense records chosen were for personal (as opposed to commercial) victims of the crimes of robbery, assault, rape, burglary, and larceny.

The basic purpose of the survey was to continue examination of memory bias related to victim recall. Earlier studies of recall ability were undertaken in Washington, D.C., and Baltimore. More specifically, the aim of the San Jose survey was to examine recall acumen to assist in determining an optimum reference period for the forthcoming National Crime Survey victim surveys. Results from the Washington, D.C., and Baltimore studies provided important and fairly conclusive insights into the magnitude of the telescoping phenomenon and the extent of bias in relying on a victim to recall the crime incident in the same month it actually occurred.

Evidence from neither the Washington, D.C., test nor the Baltimore test, however, was conclusive regarding the degree to which known victims could place an event within the proper reference period, irrespective of whether the precise month was remembered. The inconclusive nature of the evidence from those two tests was due to their experimental designs, which are intended to address other methodological questions.

Besides the problem of reference period, other methodological objectives served by the San Jose Reverse Record Check study included the refinement of the questionnaire; the efficacy of measuring rape incidence through the victim approach; and continuing analysis of: (1) reasons for inaccuracies in survey reporting, and (2) the success of classifying survey-determined crimes into legal categories.

Reference period

In designing the study, the principal question facing us was, "Does the erosion of memory due to the passage of time significantly affect the number of crime incidents reported by victims in an interview situation?" The answer, of course, could have an enormous impact on the design of a continuing panel survey to measure crime incidence through the use of general population sampling. If, for example, we could determine that a reference period of 6 months ago is not appreciably different from one of 3 months ago in terms of recall failure, then we would opt for a 6-month reference period since the required sample size for a given degree of reliability would be reduced by one-half. (The length of the reference period is especially crucial for crime incidence surveys inasmuch as the rarity of the phenomenon—in a statistical sense—requires large sample sizes for reliable measurement.) By extension, an analogous statement can be made about a 12-month reference period versus a 6-month period.

The preceding discussion implies that in an ongoing survey it is requisite for the victim to recall an event as being within the reference period, but is not at all essential for him to remember the precise date or month of occurrence. Short of total memory failure, the only bias emerging from this approach is telescoping (the mnemonic phenomenon of reporting an event as occurring within a given reference period when in fact it occurred in some prior time interval). This can be largely corrected with bounded interviews when a continuing panel operation is utilized. A bounded interview technique will correct for telescoping bias in those identical survey units that are in overlap panels from one interview to the next. The technique does not apply to survey units in replacement panels or to nonidentical units in overlapping panels. The total overlapping units in the National Crime survey will likely be about 75 to 80 percent.

The chief concern addressed, then, in the San Jose Reverse Record Check was to examine the extent of total memory failure. Analysis of the results includes differential assessment by type of crime and whether extenuating circumstances are correlated with faulty memories or purposive nonreporting.

Rape measurement

One of the more difficult methodological considerations in designing a victimization survey is the problem of measuring the incidence of rape. Historically, there has been a great deal of reluctance to pose, in an interview setting, a question of the genre, "Were you raped at any time during the past____months?"

An inquiry phrased in such indelicate terms would likely promote public charges of the unbridled insensitivity of government snoopers as well as congressional outrage. It appeared quite plausible, however, that a measurement of rape incidence could be made from a survey interview without blatant question wording of the form "Were you raped . . .?"

In the course of developing the survey instruments through feasibility tests in Washington, D.C., and Baltimore, one of the question sequences asks, ". . . were you knifed, shot at or attacked in some other way by anyone at all?" Affirmative responses are followed up with further questioning to determine the nature of the attack. It is possible, of course, that rape victims would respond affirmatively to this question, and probably with considerably less embarrassment than one phrased in less subtle terms.

Classification of crimes

It is to be expected that any statistics that purport to measure the incidence of crime would inevitably be compared with crimes known to and reported by the police, and issued regularly in the FBI's Uniform Crime Reports (UCR). For the victim surveys, therefore, considerable effort has been expended in developing the instruments so that crimes elicited can be classified in accordance with the definitions used by UCR. This has been done in order to make comparisons between UCR and victim survey results meaningful.

On the other hand, much attention has been given to the very real problem of constructing interview questions in such a way as to trigger the respondent's memory properly concerning the event without burdening his mind with legal labels for crimes. It should also be noted that tabulation plans call for presenting victim-event data in sufficient detail to permit analysts who so desire to describe crime in ways that may depart from the constraints imposed by UCR definitions.

A third objective, therefore, to be addressed by the San Jose Reverse Record Check was a continuation of the examination of whether the instrument itself can be coded to conform to FBI definitions for crimes. This problem was addressed previously in both the Washington, D.C., and Baltimore tests.

Study design

With the cooperation of the San Jose City Police Department and the assistance of Robert Cushman and his associates of the Santa Clara criminal justice pilot program, a probability sample of personal (as opposed to commercial) victims of crimes was selected from the offense reports in the police files. The victims were chosen to provide uniform representation over each of the 12 months of 1970 for each of five types of crime—burglary, robbery, larceny, assault, and rape. Except for rape, a systematic selection of an expected 12 offense reports was chosen from each month of calendar year 1970 for each type of crime. For rape, six offense reports were selected from each month.

Excluded as being out of scope were cases where the victim was younger than 16 years old and cases where the victim was either a commercial establishment or the person victimized was acting in a commercial capacity (for example, a store clerk who was held up for the cash register receipts). Also excluded was any victim whose home address at the time the event was reported to the police was outside Santa Clara County.

The expected and actual distribution of sample cases is given in table 13.

Table 13. Expected and actual number of sample cases, by type of crime

Offense	Sample size: Expected, Per month	Expected, Total	Total actually selected
Total	54	648	620
Robbery	12	144	136
Assault	12	144	137
Rape	6	72	72
Burglary	12	144	142
Larceny	12	144	133

Personal interviews were attempted with the 620 named victims during January 1971 by Bureau of the Census interviewers.

The interviewers were not told that the names of the respondents had been taken from offense reports maintained by the police department. This procedure was necessary to avoid an obvious bias when testing recall ability.

It should be noted that the San Jose Reverse Record Check was conducted in conjunction with a larger survey of victimization, which utilized a general probability sample of about 5,000 households selected throughout Santa Clara County. In the larger survey a split-sample technique was employed. Half the households were interviewed with a household respondent screener, whereby a single responsible member of the household reported for all members. The households in the remaining half-sample had a self-respondent approach, where each household member reported for herself or himself. In the reverse record check, only the self-respondent technique was used. That survey is the subject of a separate report to be prepared when the results are tabulated. The interviewers who had been hired for the larger survey conducted the Reverse Record Check Study. The same questionnaire forms were also employed for the two studies.

Three basic questionnaires were utilized. The first—the so-called screener—consists of a number of questions designed to elicit a simple yes or no answer regarding personal or household victim incidents. Respondents were asked to answer in terms of events that occurred to them "during 1970, that is, between January 1 and December 31 of last year." The crimes covered by the screener were the five aforementioned ones, plus auto theft. (Auto theft was included to distinguish it from other kinds of larcenies.) The screener also provided basic demographic data and contained several attitudinal questions about crime.

For persons with affirmative responses to the portion of the screener dealing with crime incidents, a second questionnaire was administered depending on the type of crime. Under one procedure a questionnaire relating to personal violent crimes was used. With the other procedure a questionnaire relating to theft of property was used. Both supplementary questionnaires were to obtain a large amount of detail about the event—month, time, and place of occurrence; property damage; injuries suffered; time lost from work; characteristics of offender; amount and type of property loss; and whether police, insurance companies, or other officials were notified.

Victims were interviewed in their homes or place of work. Those who had moved were followed up, where possible, unless they had left Santa Clara County. Completed questionnaires were compared against the offense reports by Washington, D.C., research staff to match up the proper incidents (many respondents reported incidents other than the ones that were sampled from the police files).

The rate of response in the San Jose Reverse Record Check was 63.5 percent. Of the noninterview cases, the large majority—76 percent— were persons who could not be located. Another 11 percent of the noninterview cases had moved from the area; the remaining 13 percent were not interviewed for other reasons, including refusals and persons who were never available. By type of crime the interview completion rate showed fairly modest variation, ranging from 73 percent for burglary to 59 percent for robbery.

Results—reference period

The data collected in the San Jose Reverse Record Check were tabulated in a variety of ways for purposes of analyzing the reliability of various reference periods. Table 14 shows the extent to which cases sampled from police records were reported in the survey as occurring during the reference period—that is, within the past 12 months, or during 1970.

*Excerpted from: Law Enforcement Assistance Administration "San Jose Methods Test of Known Crime Victims," Washington, D.C.: U.S. Department of Justice, Government Printing Office No. 1972 0-462-102, 1972.

Table 14. "Cases sampled from police records by whether reported in survey "within past 12 months," by type of crime

Type of crime	Total police case interviewed	Reported to interviewer as "within past 12 months"* Total	Percent
All crimes	394	292	74.1
Violent	206	129	62.6
Assault	81	39	48.1
Rape	45	30	66.7
Robbery	80	61	76.3
Property	188	162	86.2
Burglary	104	94	90.3
Larceny	84	68	81.0

*Literally, the question-wording of the interview document was "during 1970, that is, between January 1 and December 31 of last year."

Table 15. Cases sampled from police records by time period, by whether reported in survey interview within same period, all crimes (unweighted sample tallies)

Reported to police	Total	Reported to interviewer within same period Total	Percent
Within past month	36	24	66.7
Within past 3 months	101	70	69.3
Within past 6 months	201	135	67.2
Within past 9 months	304	202	66.4
Within past 12 months	394	265	67.3*

*Includes only those cases for which month was reported in interview. Compare with 74.1 percent shown in table 14.

One of the most noteworthy findings of the survey is that about three-fourths of the incidents for which the victim was interviewed resulted in mention of the event by the victim to the survey interviewer. The property crimes of burglary and larceny were reported with 86 percent recall, significantly greater than the 63 percent recall for the violent crimes of assault, rape, and robbery.

Again, as with Washington, D.C., and Baltimore, the crimes reported least often were those of assault—48 percent in San Jose. (A discussion of the characteristics of cases not reported is presented later in this report.)

Emphasis should be placed on the fact that the survey results show a 74-percent recall rate when the inquiry is for "the past 12 months." The experiment did not tell us what the recall expectation would be if varying recall periods had been used. Future methodological studies could be designed to address this question more rigorously.

It is possible, however, to gain some additional insights about reference periods by examining the San Jose data in other ways. Though the survey asked about crimes occurring during 1970, respondents were also asked to provide the month of occurrence, where possible. Results were tabulated to show the extent to which respondents were able to place events properly as occurring within the past month, the past 3 months, etc. These results are shown in table 15.

The figures in table 15 were computed from unweighted tallies. Those figures do not reflect adjustments that may be due to differential sample sizes by type of crime (the expected sample size for each was n; for rape, it was n/2). Nor do the figures in table 15 reflect an adjustment for varying response rates by type of crime. However, weighting adjustments of the type described above, in fact, have little effect upon these estimates.

There were a total of 27 cases reported in the survey interview for which the date (month) could not be recalled by the respondent. These cases were properly recalled as occurring "within the past 12 months," and account for the difference of 74.1 percent shown in table 14 and 67.3 percent shown in table 15.

Of the 27 cases mentioned, 13 actually occurred during the last 6 months of 1970. If we assume these 13 cases would have been reported if the interview document had been worded to ask about events occurring "during the last 6 months," then 74 percent of the cases for that reference period would have been recalled. Similarly, for a 3-month reference period, the figure would be 74 percent. The assumption cited is tenable if we make the further assumption that the only cases that would not be reported under such circumstances would be those "telescoped" to an earlier (more distant) time period.

It is clear on the basis of these results that a reference period of 12 months is basically as reliable as the other reference period shown, as long as recall of the precise month of the occurrence is not a criterion for consideration. Indeed, if recall ability within the reference period were the only criterion for choosing the optimum period for a continuing survey, we would naturally be led to choose a 12-month reference period because of the implications on the number of interviews required to achieve a given level of reliability.

The proposed plans for the National Crime Survey, however, call for a rotating sample of some 60,000 households to be interviewed at the rate of 10,000 a month, using a rolling reference period of 6 months. In effect, the procedure calls for each 10,000-household subset to be interviewed about events occurring during the previous 6 months; so that the January panel would be interviewed about the preceding July–December period, the February panel about the August–January period, etc. This procedure will ultimately permit a moving index of crime to be estimated, say semiannually, based on

Table 16. Cases sampled from police records by time period, by whether reported in survey interview during the same period

Reported to police	Total	Reported to interviewer during same period Total*	Percent
1-6 months ago	201	135	67.2
1-3 months ago	101	70	69.3
1 month ago	36	24	66.7
2 months ago	34	19	55.9
3 months ago	31	17	54.8
4-6 months ago	100	50	50.0
4 months ago	32	12	37.5
5 months ago	32	9	28.1
6 months ago	36	14	38.9
7-12 months ago	193	103	53.4
7-9 months ago	103	47	45.6
7 months ago	36	13	36.1
8 months ago	33	11	33.3
9 months ago	34	11	32.4
10-12 months ago	90	27	30.0
10 months ago	29	10	34.5
11 months ago	27	3	11.1
12 months ago	34	13	38.2

*Note subtotals do not add to totals. Though a respondent may have failed to recall the exact month, his error may still have placed the event within the same 3-month or 6-month period that it occurred.

60,000 interviewed households. Such an index could be constructed, theoretically, after the first 6 months of data were compiled and would be "centered 3 months ago."

Alternatively, a 12-month reference period would produce mathematical equivalency in terms of sampling variance with 30,000 interviews spread uniformly over the first 6 months. The moving index, however, would be less timely, centering 6 months ago rather than 3 months ago.

Moreover, in addition to moving averages there will be data produced relating to a specific time period, most likely calendar year. For this purpose it will be requisite to have the month or quarter of occurrence of an event reported, as accurately as possible, by the respondent. Results of the San Jose study indicate that the period of occurrence is more likely to be recalled for events occurring within the previous 6 months than for events occurring 7 to 12 months ago, i.e., 67 percent versus 53 percent. On a month-by-month basis, however, there is very little to choose from after the first 3 months. Cases of 1 month ago have reporting accuracy of 67 percent; 2 and 3 months ago are about 55 percent accurate. After that, 4 or more months ago averages around 33 percent correct reporting. See table 16.

Results—measurement of rape

The San Jose study was the first attempt in the series of Census Bureau-LEAA feasibility tests to determine whether the instruments developed to date could successfully elicit mention of rape attacks by known victims.

In evaluating the results, it should be observed first that the completed interview rate for rape victims selected from the police files was as good as for all crimes as a whole (62.5 percent versus 63.5 percent). Neither of the other violent crimes surveyed (robbery or assault) had completed interview rates higher than that for rape.

For those rape victims for whom it was possible to obtain an interview, two-thirds of them (30 out of 45 cases) reported the incident in the survey test. Though on the face of it this ratio of reporting leaves something to be desired, it is interesting to note that rape victims appear more likely to mention (or remember) the incidents in a survey atmosphere than victims of assault. About one-half the interviewed assault victims reported the events during the survey interview.

Five of the "rape" victims, though mentioning the incident in the interview, reported the kind of details that caused the event to be classified in the test as an assault. There is no way of determining whether these five cases were misclassified by the police or whether, alternatively, the victims may have edited the details for the interviewer's benefit—either through shame or embarrassment or through memory failure.

It is worth noting that all five cases were attempted rapes according to police standards. This suggests that the survey instrument needs further refinement to clear up ambiguities between aggravated assaults and attempted rapes in the classifications. Further analysis of the unreported cases reveals that only 4 of the 15 were stranger-to-stranger attacks, according to the police offense reports (actually one of the four cases had a blank entry for offender on the police form). The remaining 11 cases all involved an alleged offender who was known by the victim.

Examining the offender-victim relationship by whether the event was reported in the interview shows that 84 percent of the rape attacks by strangers were reported compared to 54 percent of the rape attacks by known assailants. These figures are summarized in table 17.

Table 17. Relationship of victim-offender in rape cases, by whether reported in interview

Relationship of offender to victim*	Total inter-viewed	Percent reporting incident in interview
All cases	45	66.7
Relative	0	
Known	24	54.2
Stranger	19	84.2
No entry	2	50.0

*As determined from police offense report.

Comparison of victim-offender relationship by whether reported in interview

To gain further insight into some of the factors that may be related to reporting incidents in an interview, an analysis of the victim-offender relationship versus the reporting habits was made. Information on the police form was available to permit tallies of the relationship between the victim and the alleged offender for violent crimes. No tally was made of the property crimes in this regard largely because personal confrontation between victim and offender rarely occurs during the commission of the crime.

The results indicate that stranger-to-stranger confrontations are more salient than those involving persons who know or are related to each other. Violent crimes involving strangers were reported in the interview 75 percent of the time; those involving relatives were reported only 22 percent of the time; and those involving persons who knew each other (not kin) were reported with 58 percent frequency. These results are displayed in table 18.

Of the cases not reported in the survey, two of every three were incidents where the victim and the assailant were related or otherwise known to each other. See table 19.

In setting up the study design, assault and robbery cases were each sampled so that their overall sample size was twice that of the rape cases. For this reason, when examining the results shown in table 18 or in table 19, it is more appropriate to use the weighted figures than the unweighted ones. There are no important differences, however, in the two sets of figures.

Table 18. Police sample cases interviewed by victim-offender relationship, by whether incident was reported in interview

Victim-offender relationship and reporting status	Assault	Rape	Robbery	Total all 3	Total weighted (percent)*
Total cases	81	45	80	206	
Proportion reporting incident (percent)	48.1	66.7	76.3	63.1	63.7
Offender a relative	18			18	
Proportion reporting incident (percent)	22.2			22.2	22.2
Offender known	38	24	16	78	
Proportion reporting incident (percent)	81.6	54.2	68.9	57.7	56.9
Offender a stranger	24	19	56	99	
Proportion reporting incident (percent)	54.2	84.2	80.4	74.7	76.3
No entry for offender	1	2	8	11	
Proportion reporting incident (percent)	100.0	50.0	62.5	63.6	61.5

*Recomputed to adjust for differential expected sample size by type of crime—size of sample for rape was n/2; for robbery and assault, the sample size was each n.

Table 19. Incidents not reported in interview, by victim-offender relationship

Incidents by type of offender	Unweighted Number	Unweighted Percent	Weighted* (percent)
Total incidents not reported	76	100	100
Offender status			
Relative	14	18	15
Known	33	44	48
Stranger	25	33	31
Not recorded	4	5	6

*See footnote in table 18.

Table 20. Proportion of crimes classified identically between police and survey schemes, assuming police as standard

Type of crime according to police classification	Total	Classified same in survey: Number	Classified same in survey: Percent of total
Total	292	245	84
Assault	39	33	85
Burglary	94	91	97
Larceny	68	56	82
Robbery	61	54	89
Rape	30	24	80

Classification of crimes

One of the very important methodological analyses of the San Jose study was a comparison of the reported crimes as classified by the police versus the classification from the interview procedure. There are several variants that have a bearing on inconsistencies that may occur between the two classification schemes. Among them are the following:

(1) The survey instrument may be inadequately constructed.
(2) Individual police departments may not conform perfectly to reporting standards established for Uniform Crime Reports.
(3) The details of an event that lead to classification in the survey may be poorly remembered or purposely altered by the respondent when interviewed.
(4) The details of an event that lead to classification in police records may not be communicated cogently by the victim to the police officer.
(5) The police officer may not properly record the details on the offense report.
(6) Interviewer variance may introduce errors.

In the San Jose study, it is not clear to what degree the above-mentioned variants were operating. Only the first of the six points, however, is subject to improvement through modification of the survey instrument. Improvements in the question construction were made following the feasibility test conducted in Washington, D.C., and again following the Baltimore study.

A classification of the types of crime, according to police reports, and the proportion of those that were classified similarly (if recalled at all) are presented in table 20.

These figures assume the police classification to be the standard and show the proportion of cases that were classified into the same categories through the survey procedures. The reverse position—the assumption that the survey classification is standard—would also be interesting to examine. To do so, however, requires weighting the data to reflect differential selection rates for the crimes sampled (the crimes measured do not occur in the general population of crime acts with equal frequencies; in 1970, for example, fewer than 200 rapes occurred in San Jose compared to several thousand burglaries, according to police reports.) The variance due to these differential weighting factors by type of crime is so large that the reweighted results cannot be meaningfully analyzed. A useful study in the future would be one carefully designed to measure the degree to which police classify crimes according to the victim survey definitions, assuming the latter as the standard.

In general, it is clear from table 20 that for most police-determined offenses, the probability that the event would be classified the same way through the survey route is fairly high. (Again note that the converse has not been conclusively determined; see preceding paragraph.)

An attempt was made to provide a separate analysis of petty versus grand larceny in terms of police-survey classification practices. Traditionally, victim surveys have produced dollar-amount losses in crimes of theft that exceed the amounts recorded in police statistics (cf. the Washington, D.C., and Baltimore test results). This phenomenon would appear to have serious implications on the survey-determined larcenies, as to whether they can be properly classified as grand or petty—i.e., above or below $50.

In the San Jose study, the results were inconclusive for two reasons. The number of petty larcenies included in the test was too few to analyze reliably; and a fairly large percentage of the larceny cases contained no information on dollar loss from either the survey results, the police report, or both. In general, the survey results produced loss amounts that exceeded the police assessment. For those cases for which determination of dollar loss was available from both sources (police and survey), the median value as reported in the survey was about 40 percent higher than the police determination for grand larceny and burglary, and about 80 percent for robbery. For petty larceny, the median values were the same, but these results are based on only 10 cases. These data are presented in table 21.

Table 21. Median dollar loss comparison, by crime

Type of crime (police classification)	Median loss reported by: Survey (1)	Median loss reported by: Police (2)	Percent difference (1)-(2) (2)
Larceny, total	$200	$152	31.6
$50 or over	340	240	41.7
Under $50*	22	22.50	-2.2
Burglary	379	270	40.4
Robbery	42	23	82.6

*Based on only 10 cases.

Summary and recommendations

The major conclusions yielded by this study are as follows:

(1) A reference period of 12 months is not worse than one of 6 months for simply assessing whether a crime occurred.
(2) To place an occurrence in a specific timeframe (month or quarter), respondents are more accurate with a 12-month reference period than a 12-month reference period.
(3) Police-known victims of most crimes reported the incident in the interview a high percentage of the time, except assault victims and rape victims. Their reporting rates were about one-half and two-thirds, respectively.
(4) For cases of personal victimization that were not reported in the survey interview, two-thirds involved incidents where the victim and the assailant were related or otherwise known to each other. On the other hand, stranger-to-stranger confrontations were reported in three of every four cases.
(5) Our ability to classify crimes according to UCR criteria is fairly accurate. Only minor modifications are suggested for the survey instrument for future efforts in terms of refining the classification procedures.

In light of conclusions (1) and (2) above when considered in connection with a continuing survey, a 6-month reference period is better than a 12-month period for producing calendar-year data and for obtaining earlier and more timely results. With a 6-month rolling reference period, some data could theoretically be available after 12 months—assuming bounded interviews—and the data would be centered 3 months ago. For a 12-month reference period, 18 months would be required before data, comparably reliable, would be available, and they would be centered 6 months ago. The sample size, however, for a 6-month reference period is twice that for a 12-month period.

In the course of working with the San Jose data, as well as the Washington, D.C., and Baltimore data, a number of methodological studies suggested themselves for the future. Some such studies might be undertaken prior to the establishment of the National Crime Survey, others in conjunction with the survey, and still others independently of the survey. A listing of possible methods tests follows:

(1) A test of the effects on reporting frequencies under varying reference periods (e.g., within the past 3 months, within the past 6 months, within the past year), utilizing a general population sample with a multiple split-sample approach.
(2) A test of whether the Warner randomized response technique is better than conventional questioning methods for eliciting reports of assaults (perhaps rapes and robberies also).
(3) An experiment designed to compare the categories into which various police agencies would classify crimes on the basis of data elements determined from an interview survey.
(4) A test of whether proxy-respondent reporting of crimes is different in amount and type from self-respondent reporting, utilizing a sample of known crimes from police files.
(5) A test of whether the measure of change in crime incidence between two periods differs by type of respondent (self versus proxy).

The Dayton-San Jose methods test*

by CAROL B. KALISH

The Dayton-San Jose Pilot Survey of Victimization, conducted during January and February 1971, was the first joint effort by LEAA and the Bureau of the Census to apply their victimological research methods to a general population sample. Before this survey was undertaken, several smaller scale validation studies had been completed. The principal overall purpose of the validation studies was to develop the survey instruments through alternative questionnaire designs administered to known crime victims selected from police offense reports. Therefore, a major technical purpose of the Dayton-San Jose survey was to test the survey instruments on a sample of the general population.

The survey universe consisted of the urbanized areas of Santa Clara County, California, and Montgomery County, Ohio; the data, therefore, cover not only the central cities of Dayton and San Jose but also the surrounding highly urban territory. Personal interviews were used for a probability sample of approximately 5,500 households and more than 1,000 businesses in each of the two areas. Field interviewing was carried out simultaneously in both areas.

In the household sector, the housing units selected for the Dayton-San Jose survey were located in the 1970 Decennial Census Address Coding Guide areas of the Montgomery County portion of the Dayton, Ohio, Standard Metropolitan Statistical Area (SMSA) and the San Jose, California, SMSA. The Address Coding Guide area created for each of these SMSA's corresponds to the city delivery area of the postal service. Approximately 95 percent of the population in both SMSA's is included in the Address Coding Guide area.

This survey was the first attempt to try out on a general population sample the survey methods developed in smaller studies. The basic methodological objectives of the Dayton-San Jose survey were:

- to examine varying respondent techniques,
- to determine the degree of cooperation of a general sample of people in a survey of this type,
- to pilot-test a questionnaire pertaining to citizen attitudes about crime and the fear of crime, and
- to examine the problem of optimum length of the recall period through use of a general sample. In addition, there was some experimenting with telephone interviews.

Table 22. Estimates of 1970 incidents by respondent method —Dayton and San Jose combined

Type of incident	Respondent techniques: Self	Household	Ratio of self to household
Strong-arm robbery	1,307	621	2.10
Armed robbery	994	845	1.18
Robbery attempts	2,140	974	2.20
Aggravated assault	2,273	1,489	1.53
Simple assault	6,094	4,928	1.24
Attempted assault	12,441	7,195	1.73
Rape and attempted rape	484	399	1.21

One of the most significant technical features examined in this survey was the question of who makes the most valid respondent for personal crimes: each household member responding for himself or herself (self-respondent) or a chance respondent in each household responding for all members of the household (household respondent).

A controlled experiment was designed into the survey to answer this question. A split-sample approach was used in which a random half of the 11,000 designated sample households in Dayton and San Jose combined were given the self-respondent treatment, and the remaining random half-sample was given the household respondent treatment. The results, shown in table 22, reveal a substantially greater reporting of incidents with the self-respondent method. The pattern is consistent for each type of crime, although the magnitude of the ratio varies.

Another important technical feature of the survey was examining the length of the recall period. The effects of the choice of the reference period on recall have been documented fairly extensively from LEAA/Bureau of the Census reverse record checks (i.e., studies comparing survey-derived information with police offense reports, where the sample unit is the named victim in police files). A general principle that can be inferred from these recall studies is that the accuracy of survey-derived incidence data increases as the length of the recall period decreases. In other words, asking respondents to report incidents a day old produces less bias from memory failure than asking them to report incidents a week old. Memory bias, however, is not the only design parameter to consider in choosing an optimum reference period. Sample size plays a major role, as well. For example, the longer the recall period used, the smaller the sample size required to produce data at a given degree of reliability. Thus, whereas sampling errors decrease with an increasing length of reference period, nonsampling errors (in the form of respondent memory failure) increase.

As a result of these considerations, i.e., balancing cost against precision and accuracy, and in view of future LEAA/Bureau of the Census victimization surveys, the reference period was narrowed to a choice between 6 months and 12 months. Respondents were asked to report the month in which each victimization took place. Results were tabulated, comparing the estimated number of incidents for the first 6 months of 1970 with the estimated number of incidents for the last 6 months. (See table 23.)

*Excerpted from Appendix 1 of *Crimes and Victims: A Report on the Dayton-San Jose Pilot Survey of Victimization.* Washington, D.C.: National Criminal Justice Information and Statistics Service, Law Enforcement Assistance Administration, 1974.

Table 23. Estimates of 1970 incidents by respondent method, by when occurring—Dayton and San Jose combined

Type of incident	Self respondent: First 6 months of 1970	Self: Last 6 months of 1970	Self: Ratio last 6 to first 6	Household respondent: First 6 months of 1970	Household: Last 6 months of 1970	Household: Ratio last 6 to first 6
Strong-arm robbery	544	762	1.40	210	410	1.95
Armed robbery	438	556	1.27	218	627	2.88
Robbery attempts	957	1,184	1.24	408	567	1.39
Aggravated assault	1,066	1,207	1.13	808	681	.84
Simple assault	2,688	3,406	1.27	2,190	2,738	1.25
Attempted assault	4,892	7,549	1.54	2,689	4,507	1.68
Rape and attempted rape	86	394	4.35	67	332	4.95

Independent evidence suggests that there is very little seasonal variation between the two halves of a calendar year. The survey figures, however, show a dramatic difference between the estimates by time of year reported. This pattern persists for both self-respondents and household respondents, although the disparity is greater for the latter. These figures reflect the joint effect of greater memory fading in the earlier months and telescoping of incidents from the first 6-month period into the second 6-month period.

As a result of the Dayton-San Jose survey findings, a number of methodological refinements were being made in the statistical procedures of the National Crime Survey (NCS). Therefore, comparisons between data in this report and those forthcoming from the NCS should also be made with caution.

Basically, the NCS uses a nationwide sample of individuals, households, and businesses representative of the country as a whole. The same sample is interviewed twice a year about experiences with crime in the period since the last interview. The data are aggregated and published four times a year. This statistically sophisticated approach provides a reliable empirical measure of changes in the extent and nature of crimes of theft and violence. As an adjunct to this nationwide quarterly report, separate reports will be published for the central city in 13 major metropolitan centers each year, reporting on at least 26 major cities in the first 2 years of reports.

Though the Dayton-San Jose report is in many respects prototypical of reports to be issued in connection with the National Crime Survey, there are several major differences between the two. Some of the more important differences are presented here.

1. NCS data are collected using the self-respondent technique entirely. The Dayton-San Jose project used a household respondent method in half the survey units, which resulted in understating the incidence of crime.

2. The personal survey universe for the NCS is persons 12 or older. In the Dayton-San Jose study the universe consisted of persons 16 and older.

3. The basic unit of analysis for socioeconomic distributions and the associated rates of victimization in the NCS is the victim, rather than persons victimized one or more times. A person (or household) can be a victim several times in a given period of time; consequently, there are more victims than there are persons (or households) victimized. The rates of victimization will be correspondingly higher in NCS reports than in the Dayton-San Jose survey. Also, counts of victims in each of the crime categories are additive, unlike counts of persons victimized one or more times.

4. For Dayton-San Jose the household, rather than the person, was designated as the victim of noncommercial larceny events, irrespective of where the victimization took place or who was actually affected. This designation meant that household crimes included not only those larcenies occurring at home, but also such "personal" victimizations as the theft of personal belongings from school or athletic lockers, restaurant coatrooms, one's office, and so forth. For the NCS these personal larceny events are ascribed to the person actually victimized, so that his relevant demographic characteristics can be analyzed. Household larcenies—those actually occurring at home—will be counted as household crimes. With this procedure, the household victimization rates for the NCS will be decidedly lower than if the Dayton-San Jose procedure was used; but personal victimization rates will not be correspondingly higher.

5. A major procedural difference between the NCS and the Dayton-San Jose survey is the questionnaire format. The latter survey utilized multiple forms in the household sector and the commercial sector. In the household sector, a "screener" was used along with separate incident forms for personal crimes and for property crimes.

For the NCS a single questionnaire is in use for the household sector; all the pertinent information for a given respondent is recorded on a single form. It is not expected that this changeover will have any important substantive effect. Quality control is improved, however, not only with greater ease of administration by interviewers in the field, but also in the subsequent handling of survey forms in office processing.

Notes on the methodological development of the National Crime Survey*

by ALBERT D. BIDERMAN

The NCS instruments

The decision to focus the National Crime Survey on the index crime classes is a policy decision that is not well justified by saying the comparison is inevitable. It would be less inevitable if the policy of trying for the greatest comparability had not been adopted. An equally good or better logical case could be made for the propositions that (1) the two kinds of data are inherently comparable only in an extremely loose way or, in any strict sense, they are incomparable; and (2) a better use would be one which shuns comparisons and aims the survey precisely at that which UCR reads most imperfectly, such as nonindex offenses including, notably, petty larceny, vandalism, simple assault, and frauds.

I still think that the individual screen questions are encumbered too much by trying to have them resemble UCR categories rather than have them follow what is found to be, through experimentation, the most productive cues for mental association to criminal events. It is sufficient in the screen to determine that someone tried to hurt or harm the person in order for the interviewer to move to an incident report. That kind of screen question should be posed without encumbering it with the specifics of weaponry. These specific cues about weapons should be used separately since they are also valuable in jogging memories, but so too do the words "hit, strike, bite, and scratch." So the how's and where's of harms should figure in the screen not in accordance with the logic of crime classifications, but as they are most productive for stimulating recall regarding criminal events. Rule 1 should be: It is the report form that serves the function of classification, not the screener.

The survey will be built around a specific cueing screener strategy. The basic psychological idea here is to stimulate the respondent into thinking about concrete life situations rather than abstractly about "crimes" or types of crimes. Experience in previous studies and in the LEAA-Census Bureau pretests leaves no doubt that the former is a far more effective strategy.

Replies to several of the screener questions can be "yes" for a given incident quite validly. It is clear from the Baltimore test report and some of the interviewer instructions that there was some intention in the design for each screener item to identify one and only one incident. There is inadequate recognition that the screener design accepts the possibility of redundancy as a minor sacrifice to omissions. The screener items have two functions: to identify incidents by as many cues as can be feasibly used, and to identify the crucial features of the incident that must be followed up by administering a particular form. It takes information not now automatically gotten from the screener to determine (sometimes the respondent won't know) if various things that are mentioned as having happened in response to the screener are parts of the same incident or are to be treated as such. Other questions have to be asked if the interviewer is going to determine if various things mentioned in the screener are all parts of one incident. In our study we used the question "When did that happen?" as I recall, which could produce the "Same as the other thing I mentioned" response.

We still have considerable problems remaining in the instrument for dealing with respondents for whom the more abstract and complex kinds of cues work poorly, as evidenced in poor results on assaults. Of the two major failings, the major one is disturbing the psychological set of searching the mind for a concrete experience by continually asking about plural classes (abstractions) of experience with the "How many times?" question. The other one is the failure to capitalize on linkages of recall to concrete places in which offenses occur commonly. All that was needed here was suitable questions asking about "at work," and the other kinds of places.

Followup probes for multiple occurrences of similar events would probably be better with single-event focus, rather than "how many times." Ask, for example: "Was there some other time since January that someone tried to break in. . .?" If the probe also elicits an incident, then follow up again with "Any other time that this happened?" If the respondent balks because of a series type of phenomenon ("It keeps happening all the time"), a special provision for incident recording is needed. It is important to get an idea of how often things occur that don't fit the conventional offense unit concept.

The device of asking how many times a particular class of incident occurred must be pursued differently if multiple victimization of the same respondent by the kind of events suggested by a particular screener probe is to be fully developed.

Interviewer instruction and training materials must allow the interviewer to encourage the respondent to attempt to recall each event separately and deliberately during screener administration. I would ask after every screener "yes":

> "Did that kind of thing happen to you only once or more than once during the last 6 months?" Then ask, "When was the first time that happened?" Then, "When was the next time that happened?" Then, "Was there another time that happened?"

and continue until all incidents suggested by that screener question have been covered. It is extremely important to avoid an instrument that would have a bias against the hypothesis of differential risk or proneness to victimization of individuals. Our best guess is that the tendency of the interview is to make it less likely that the second, third, and "nth" instances of victimization of any type are mentioned than the first, but that an individual who has been victimized once in a certain way is more likely to have a second such incident than a random respondent is likely to have had any such incidents. A special effort should be made to avoid having the necessary device of treating "series of crimes" in a special way interfere with getting reports of many discrete incidents from a victimization-prone respondent.

The assault victims' difficulties in Baltimore and Washington, D.C., reflect a combination of circumstances making them problem cases. In much higher proportion than other respondents, I venture that they are transient, single, multiple-victimization prone, have low language facility, and are likely to have fewer of whatever motives there are that make for cooperativeness in surveys. I suspect the matter of abstract language ability—ability to deal with the interview as an intellectual task—is itself a problem. The questions should be made as unformidable as possible. There is a lot that can be done to lower the "Flesch" score of questions: breaking compound sentences in two, avoiding passive voice, using the most everyday term rather than the more middle-class one, etc.

Our Crime Commission study mentions poor recall of victimization being expected for the type of unreported incident where the victim sees nothing whatever he can do about it (except cry over the spilt milk). No pattern of actions follows upon the event that reinforces its psychological impact and provides additional concrete anchors in experience for recalling it.

This is unlike the case where the police come around and take a report, where there is a pending court case, etc. The data from San Jose do not permit tests of the functions of some reinforcing postvictimization events for recall; for example, was there recovery, or detective bureau investigation?

But this would add only a little to what is very imperfect validity testing. And operating in a somewhat different direction would be the psychological effects of closure or nonclosure on recall that have been a popular subject of psychological investigation.

Another kind of test that might be made on unreported offenses by reporting period would be to take victimization data fr[illegible] survey on all multivictim events [illegible] determine what difference time makes in whether only one, or more than one, victim of the same offense reports it in an interview.

I don't think techniques using random-response methods are worth using for the victim series. Apart from their statistical inefficiency, I think their use will create more respondent resistance and psychological noise in the interview situation than it will eliminate. I think that the strength of the interview (and particularly, the interview as an institution) depends upon the creation of the atmosphere that it is the most natural thing in the world for the respondent not to be defensive and to be leveling—an atmosphere that the random response methods thoroughly disturb. I know of no controlled study comparing the "take" from such methods with blunt asking, however; so from this standpoint I would like to see you undertake a trial that, my guess is, will discredit the random response method on psychological grounds. The Census Bureau should have a special stake in presuming the interviewee's cooperation, forthrightness, and confidence that his anonymity is safeguarded.

The use of mail methods

Although I agree that the personal interview approach in the large will yield far better results than a mail device, I think that the potentialities of both mail and leave-with questionnaires should not be dismissed as lightly as they have been. The Quarterly Household Survey mailback experiment comparing victimization rates from mail and personal interviews is extremely encouraging with regard to potentialities of the use of mail modes. I think there is ample justification in these data for making a substantial investment in their analysis to determine whether mail can be used satisfactorily with certain classes of respondents who can be identified from fact-sheet characteristics. It is altogether possible that mail may even prove more satisfactory than personal household-informant interviews for household victimization data, for example. There is a good possibility that the mail method may prove satisfactory if the mail responses are accepted only from respondents reporting above a certain level of education or where there is a certain pattern of household composition. The mail technique seems fairly satisfactory for those victimization classes in which victims do not concentrate in low education categories—much or all of the difference between mail and personal modes may be due to low-education respondents. It might also be possible to use the mail method with a followup only of respondents who report zero victimization, and still achieve rates not sufficiently lower than from personal screeners to justify the sacrifice of the economies of the mail method. Even male versus female may be a useful discriminator for determining whether a followup of a mail return is necessary. The assumption in the San Jose report that panel respondents eventually will be deterred by learning that an affirmative screener response results in an interviewer followup is simply an assumption. Can we conclude there is a general disposition of respondents to avoid such followups? There is as much reason to anticipate that the fact the Bureau of the Census takes these responses seriously will tend to encourage, rather than discourage, conscientious completion of screeners on this topic by mail. Another quite feasible possibility that might be tried is to allow interviewers, or interviewers and editors, to nominate respondents who they expect would be good mail candidates. Certainly, the possibilities of the combined mode procedure for the panel should not be dismissed lightly for a survey as extremely costly as is the NCS.

Reference period issues

In the San Jose report a statement of a conclusion reached in record-check studies appears that is strongly contradicted by data in the report on number of incidents reported by nature and length of the reference period. I refer to the statement "a 12-month reference period is no worse than 6 months (for simply ascertaining whether an incident occurred) so long as the exact month of the occurrence is not requisite." Considering table 16 in the San Jose recall report, we find that a much higher number of incidents is reported for the last 6 months than the first 6 months, when people are asked about a year's period, and that the evidence on the effects of the control of telescoping by bounding indicates that only a fraction of this excess seems due to forward telescoping. Recency effects are still very much there. I think the prose used to present the results and the selection of tabular presentations slight the effects recency would have on estimates. The case for a 6-month reference period is stronger than given here. The effect of recency on the most important indicator—annual estimated victimization rate—is the most important reason for using a brief reference period. This criterion should receive far more em-

*Drawn by the editors from four memoranda prepared by Albert Biderman for the Law Enforcement Assistance Administration between June 27, 1970, and March 7, 1973.

phasis in the discussion, relative to accurate placement within fractions of the chosen reference period.

It is of great importance that interview methods, recording format, and analysis criteria be developed that allow for the count of incidents that the respondent, with some specified degree of self-confidence, can report occurred not longer ago than the beginning of the reference period, even though he or she is unwilling to hazard a guess (or guesses incorrectly) with regard to the specific month during that period in which it occurred. In an earlier comment, I suggested that 1-month class intervals made for a somewhat conservative statement about recall accuracy. While a month involves asking the respondents just to hit the barn door of time, they may be really aiming at something at the very edge of the door, and anyway, the real interest is in their hitting anywhere on the whole barn. Dating within a named month is a much more stringent criterion for most dates of occurrence than being able to date within plus or minus N days of the actual event, even when N is considerably smaller than 30 days. The pretest should be analyzed from this standpoint, which will be more or less automatic in a rolling panel interview procedure—i.e., where respondent's anchor is anything that occurred since the last interview.

I still do not think that the issue of reference-period effects on the completeness and accuracy of incident reporting has been adequately investigated for a well-informed cost-benefit decision regarding the optimum reference period to be used. There are two partially invalid assumptions evidenced in the report by Turner that have clouded consideration of this question. First of all, the statement in the San Jose recall study report that a sample of 10,000 units for a reference period of 3 months is equivalent in terms of the reliability of the estimates to a sample of 5,000 units for a reference period of 6 months is incorrect to the extent that the universe in which we are interested is not that of individuals but of crime incidents. The evidence in this report, for example, suggests that because of the combined effects of telescoping and recall (and, as we shall indicate, some other possible effects as well), an unbounded interview with a 6-month reference period may actually be worse in its reliability for various offense classes than a 3-month unbounded interview, and perhaps worse if there is close analysis of the available data for the overall victimization estimate. A definite statement cannot be made about the cost-benefit of a 3-month reference period, since such data are not available here. There is reason to expect that the N's of incidents from a 3-month reference period would be somewhat greater than half those from a 6-month, and for some types of incidents, very much greater. Further, since N does not affect reliability linearly in probability computations, the issue of what reference periods are equivalent to various sample sizes is more complicated than indicated here.

A concrete significance of these considerations for the NCS is how to handle and treat the interviews with new entrants into the panel. This first interview could be conducted solely for the purpose of bounding with the results discarded; or the results could be accepted and weighted with estimates of the amount of telescoping that has taken place. The resulting data would be of questionable value for many purposes. But another alternative would be to have a very brief reference period, say 3 months, were it to be established that this length of reference period is not highly affected by invalid recall, even when unbounded. An even more appropriate procedure would be to employ a reference period of, say, 6 months and retain for analysis only the data for incidents of the last 3 months on the basis of the assumption (which can be investigated experimentally) that telescoping affects almost exclusively incidents from outside of the reference period, their being telescoped into the first portion of it. However, the issue of how the first interview is to be treated also puts into question whether correct cost-benefit considerations have been applied in choosing the reference period for any of the successive interviews with panel participants. Perhaps a 3-month, 3-month, 6-month, 6-month pattern might be employed or even a 3-month, 3-month, 6-month, 1-year pattern of reference periods in successive interviews if, for example, it is found that respondent training takes place that improves accuracy of response in successive interviews.

Another assumption I find questionable in the report that may have affected the consideration given to the appropriate choice of reference period is that the only two important effects on the completeness of incidents reported by respondents (as measured by the N's of incidents in tables here that compare results using different reference periods) are "memory fading" and "telescoping." Another possible factor affecting these results is that to which we refer by such terms as "respondent productivity," "respondent fatigue," or "perfunctory cooperativeness." This type of factor is important in determining how many incidents are reported by respondents affected by multiple incidents of victimization and particularly by victimization-prone respondents who potentially are able to report a great many incidents. There may also be an interviewer factor of a similar kind operating to limit the amount recorded when there is an extremely burdensome amount to record. Once a respondent has manifested his cooperativeness with the demands of the interview by describing some victimization, the psychic need to add additional incidents is reduced. Fatigue, impatience, the feeling of being repetitive as similar kinds of incidents are described, also come into play. The longer the reference period, the greater the potential restriction of multiple-incident mentions from this kind of factor. In the report of the 1966 Washington Pilot Survey and in critiques of the NORC Victimization Survey, we noted the extremely suspicious distribution of number of incidents reported per respondent and interpreted these as possible evidences of "productivity restrictions" by interviewees. There is reason to infer also that such effects may be particularly serious for poorly motivated respondents and those who find the task of the interview difficult and taxing. These very classes of respondents may be those peculiarly subject to victimization of particular types; for example, aggravated assaults. We also noted in an earlier comment on the NCS methodological trials that the distribution of the number of incident reports per respondent showed considerable improvement in the direction of reasonable expectation over earlier work as the instrument was refined and shorter reference periods were used. That is, a longer and fatter inverse pyramid of numbrs reporting from one to N incidents was produced. I suggest that a high-priority analysis of data now available would be to examine the effects of different reference periods and of bounding recall on multiple-incident reporting by respondents. Thus, for example, the data in table 16 that reflect on the significance of the reference period may actually mask the extent to which higher incidence rates would be estimated from interviews using a 6-month period only (the report does not contain data allowing direct comparison of the victimization rates yielded by these two reference periods), in that, with a larger number of incidents eligible to be reported, a higher proportion of them might not get mentioned because of respondents (or interviewers) running out of steam or for other reasons imposing ceilings on their productivity in the interview.

To take another reason for the importance of such an analysis, I have earlier made an inference from a comparison of the ratios of the estimates of incidence yielded by bounded and unbounded procedures to conclude that telescoping accounts only partially for the higher estimates yielded by shorter reference periods. Since the effects of bounding are investigated only with regard to 6-month periods, however, it is quite possible that the effects of bounding on longer or shorter periods might be quite different than these comparisons suggest. In the data in table 16, which compares incidents reported for the first 6 months with those for the last 6 months of the 1-year reference period, it is likely that telescoping affects the first 6 months' estimates primarily, while the "output restriction factor" due to respondent fatigue, etc., might very well be concentrated in the second 6-month estimate. Lower ratios, consequently, would appear than would be reported in comparing the estimates that would be obtained for the first 6 months, had an interview been conducted with that reference period as compared with the estimates for those 6 months from an interview with a 1-year reference period conducted 6 months later. My hunch would be that bounding would only have modest effects in controlling output restriction factors, since my impressions are that the telescoping affects primarily respondents who do not have much to report that actually is eligible for the correct reference period. In other words, the reduced number of incidents reported in the aggregate for the entire sample in the bounded procedure does not imply a reduction in the burdens experienced by those respondents with many incidents available for reporting; that is, those who are most apt to be affected by productivity-restricting factors. They are helped by shorter reference periods, however, to report more. Unbounded short periods, on the other hand, may actually encourage some kinds of forward telescoping.

These considerations can be illuminated only by careful analysis of multiple-incidence reporting, and such an analysis is essential to clarify the cost-benefit issues raised in this methodological report.

Certainly, there is no warrant anywhere in the methodological data for the choice of a 12-month reference period for the cities' samples. The justification given is a non sequitur both because of the sheer mass of evidence of the major effect of reference period on data validity and the significance of seasonal variables for many analyses of victimization data making dating events important. This decision is simply a mistake, lessened in its seriousness only by the fact that the entire cities sample plan is difficult to justify on a cost-benefit basis. It will be of extremely limited utility as presently planned. To the extent that the cities' surveys can be defined only on political and not on scientific grounds, my methodological criticism of this decision may be irrelevant.

phasis in the discussion, relative to accurate placement within fractions of the chosen reference period.

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A social indicator of interpersonal harm*

by ALBERT D. BIDERMAN

Of all crimes, those causing bodily injury are particularly costly, feared, and deplored. They also tend to be relatively inaccessible to current methods of observation and statistical recording. During the last few years, the victimization survey has been widely adopted as a method for recording criminal events that escape official agency attention and recording. Reverse-record tests for the National Crime Survey, however, found the survey method failed to record a large proportion of assaults known to the police. The method was far less successful in gaining valid reports of assaults from known victims than it was for other categories of criminal victimization. These results may be due to: (1) vagaries of victims' memories, (2) their definitions of events as crimes, or (3) their reticence about the circumstances leading to their being assaulted. This report deals with a preliminary exploration of survey strategies that attempt to reduce the effects of all three sources of invalidity.

Strategy

Basically, the strategies explored involve use of radically different approaches to the screening portion of the interview. They will be referred to as "objective, current consequences screening" to differentiate them from the "crime event recall" approach of current victim survey screening methods. From the standpoint of the record-check validity criterion, the "screener" is the most critical step of the interview in that it determines what events, if any, of the respondent's history are reported to the interviewer. The screening approaches we tried represent departures in two key respects: (1) rather than past-tense questions asking the respondent to search his mind to remember events, he initially is asked present-tense questions about things he is experiencing at the time of the interview ("current consequences"), and (2) rather than asking the respondent initially to think about "crimes," he or she is asked first about a broad class of directly perceived phenomena—physiological consequences of events—of which those caused by criminal assaults constitute a subclass defined in part by relatively elusive, complex, nonobjective, and variant criteria.

The recall task in objective, current-consequences screening becomes one of remembering the time and circumstances of the cause of a condition. Events that might not come to a subject's mind when the respondent's task is recalling "crimes" thereby become available for exploration by detailed interviewing to determine whether they meet evidentiary and judgmental criteria for counting them as crimes. The technique also allows consideration of victimizing events that fall in large and shadowy gray areas between the criminal and noncriminal.

Specific approaches

Preliminary explorations of such approaches were undertaken to assess the feasibility of various alternative concrete applications and the utility of the data they might yield. They involved two small-scale field tests in Washington, D.C. The first test "piggy-backed" injury screening questions in a sample survey of households (N=641) with followup questioning of those respondents who said they were currently suffering from a handicap or pain due to an injury (N=96). The second test involved interviews in households of crime-related injury victims who had received ambulance service during a 4-week period, in households of an equal number of noncrime-related ambulance cases, and in neighboring control households (total N=58). Both tests were used for developing and trying out patterns of questioning. The first used brief screening questions that may be employed economically in any continuing large-scale omnibus survey of citizen attitudes and behavior; the latter adhered closely to the screening questioning procedure used in the national Health Interview Survey (HIS). It employed screening questions involving some items of recall of past events for a very brief reference period, as well as questions on existing conditions.

Efficiencies and inefficiencies

The household survey test shed light on the degree to which the efficiencies of an objective, current-consequences approach were great enough to offset its relative inefficiencies. These differences in efficiency affect the required sample sizes, interview length, and analytic complexity required for a survey with given objectives. Relative to past-event recall, current objective consequences screening will reduce data losses from: (1) respondents' failures of recall, (2) the application of overly restrictive ideas of "crime" in the recall task, and (3) the restriction of the interview to a brief reference period. The approach also eliminates from the interview and the analysis events that are of trivial consequence to victims, since the respondent only reports matters that are above a threshold of "current attention." For the proposed approach to be of relative value, these gains must offset the following sources of inefficiency: (1) the loss of data on events that do not still have serious consequences at the time of interview, including all data on attempted crimes and threats, however grave these may be from a legal, moral, or psychic point of view; and (2) the need for complex analysis to estimate the incidence of victimizing events given the variable duration ("mortality") of injury effects. Consideration of the productiveness of the approach varies depending upon the value attached to causes or effects.

Incidence and prevalence of victimization

The current-consequences approach directly yields indicators of the prevalence of harmful effects of crime among a population at a particular time. The survey we conducted, for example, found that about 15 percent of the respondents were currently suffering from handicaps or pain due to an injury. Acts regarded as criminal by the injured person were responsible for 18 percent of these conditions. Many (29 percent) of those with injuries reported they were suffering effects of more than one injury. Very few of the injuries attributed to crime were recent—over one-third of the conditions date back 5 or more years. The data are summarized in figure 1 and table 24.

*Source: Final Grant Report No. 74-55-00-6003 to the Law Enforcement Assistance Administration, 1975.

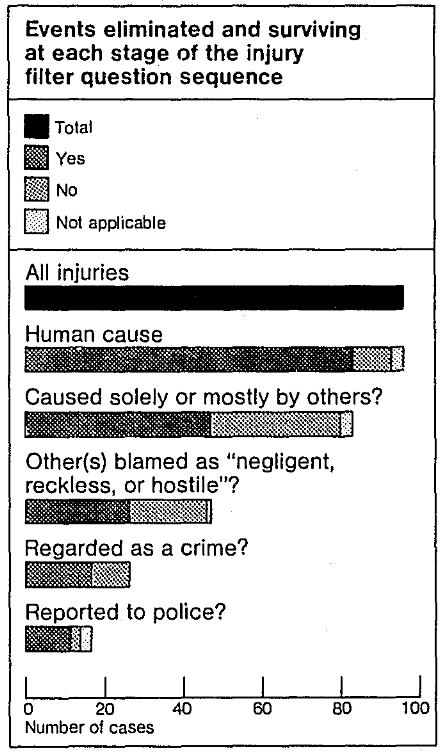

Figure 1

Table 24. Time when injury took place

Less than 3 months ago	3%
3 months but less than 1 year	11%
1 year but less than 2	16%
2-5 years ago	20%
6-10 years ago	18%
More than 10 years	32%
	100% (96)

Such indicators of the prevalence of adverse conditions resulting from crime are of great importance and neglected usefulness. Nonetheless, there has always been much greater interest and attention to indicators of the incidence of crime events than in the prevalence of their effects. The current-consequences approach could provide incidence estimates only given a large number of observations at many time points, if the estimate were to take account of the decay of effects of injuries with short-lived consequences for the victim. (Although there are no available data on the seriousness or duration of injuries from assaults, inferences are possible that most are short-lived.) The sample used in the present test yielded far too few conditions of recent origin to afford a basis for a quantitative 1-year estimate of the incidence of assaults producing injuries. Only 4 of the 17 victims in the present survey who attributed their injuries to crimes had been hurt during the previous 12 months. These results suggest that the method would not be economical for estimating incidence if used alone in a survey. This is true even though it is possible that screening only for current consequences in a survey will yield an equivalently large number of crime events in Washington, D.C., for a 1-year reference period as did the Census Bureau-LEAA Washington victimization survey. The events revealed by the current-consequences approach would doubtless represent crimes of much greater average severity. To contribute estimates of incidence, however, our conclusion is that the approach can be used with cost-effectiveness only in a crime victimization survey that also uses past-event recall screening, or in a survey that has broader objectives than gaining data on crime events. A third possibility would be to apply the method to a sample of injury victims identified by other surveys or listings.

The results of the pilot survey show the importance for the etiology of injury of human agency and of failures of legal and other social controls. Almost half of the injured respondents attributed the harm from which they were suffering to actions of others. One-fourth of injuries from all causes were blamed upon "negligent," "reckless," or "hostile" behavior by other parties; in most of these instances, acts the victim regarded as "criminal." These results indicate that norm violations as a cause of injury merit greater attention than they currently receive in data collection in the health field.

Ambulance victim followup

An ambulance service victim followup test was undertaken for the present study. It combined the objectives of a validity check of injury screening for identifying crime-caused injuries with a trial of the adaptability of the approach to procedures used in one major continuing survey—the Health Interview Survey (HIS).

This followup encountered serious completion difficulties because of apparent inaccuracies in the ambulance records used to identify known victims. Also, information given the respondent concerning the nature and purposes of this followup seemingly aroused much more frequent respondent suspicion and evasiveness than was encountered among injury cases interviewed in the omnibus general population survey. Recipients of ambulance service were concentrated in areas of the city in which survey completions are particularly difficult to achieve. Completion rates were below 50 percent for victims' households. Nonstandard household compositions, furthermore, may have aggravated the nonreporting of morbidity by a household respondent asked about other members of the household. The unreliability of proxy informants is known to be a serious problem with the HIS procedure that was followed in this test. The followup interviews produced injury reports from only 52 percent of the interviewed households in which an injury requiring ambulance service presumably had occurred during the relevant 4-week period. Given this low success rate, no effort was made at detailed matching to determine how many of these reports may have involved some injury other than that which led to selection of the household from the ambulance records. Interview success for assigned cases involving an injury that had been classified by the responding ambulance squad as "crime related" was about equal to that for "not crime-related" cases. Some very recent injuries were reported by "control group" households selected from the same block as the ambulance service cases (3 injuries among the 20 such households interviewed), suggesting an exceptionally high incidence rate for these particular neighborhoods. The data are recorded in table 25.

Since so many of the problems experienced in this test stemmed fom the source of record, it is not definitive as a validity test of the HIS technique. Nonetheless, it does cast some doubt on the usefulness of the HIS procedure for gaining the data desired. These include severe problems in locating, contacting, and gaining the cooperation of precisely those kinds of citizens most prone to serious injury. For those injury cases that are routinely identified in HIS interviewing, the trial indicates that a set of brief, simple followup questions could produce important information on criminal events as causes of injury and, more broadly, on the role of human agents in the etiology of injury.

Table 25. Ambulance case followup: Outcomes of interviews assigned

Interview outcome	Group 1 Households with crime-related injuries	Group 2 Households with noncrime-related injuries	Group 3 Community sample
Completed	42% (21)	36% (17)	50% (20)
Respondent refused	30% (15)	32% (15)	35% (14)
Incorrect address information—no such address or no such resident	14% (7)	21% (10)	10% (4)
No answer	10% (5)	9% (4)	0% (0)
Interviewer refused	4% (2)	2% (1)	5% (2)
Total assigned	100% (50)	100% (47)	100% (40)

Semantic problems

Economizing on interviewing time in the omnibus survey led to compromises of what would have been ideal procedure. The screening questions used deviated somewhat from the rigorous application of the logic of our theory regarding sources of response error in surveys. The respondent was asked to report pain or handicap due to injury. The questions thereby directed the respondent's attention to matters of both present and past—his or her present physical condition and a past cause of the condition that qualified it as being due to an "injury." The logically and psychologically nicer procedure would be to first have the respondent identify any conditions he or she is currently experiencing and then, for each condition, provide information as to its origins.

The screening questions we used also involved the ambiguity in meaning that the word "injury" has in the English language, in that "injury" can refer to both the act that harms and the resulting damage. Additional confusion may enter into the respondent's psychological set because of other semantic baggage carried by the word "injury"—its meanings embrace moral and legal matters (it is etymologically related to "justice"). The differentiation in speech of injuries from such other sources of physiological harm as microorganisms, congenital disorders, or degenerative conditions is imprecise and freighted with complex linguistic survivals.

In the ambulance service followup, where the screening format of the HIS was followed, we retained the words "accident or injury" that are used in the HIS. In common speech, "accident" can imply an event free of fault or harmful intent on the part of an actor. It therefore involves a prejudgment with regard to one crucial concern of the present survey that makes it unsuitable. Used together in the phrase "injury or accident," however, unsuitable implicit meanings of the two terms offset each other.

Presumably, many conditions that are sequelae of injuries are not identified as such by respondents, particularly those with delayed reactions, prolonged low-level effects, or complex interaction with other agencies of morbidity. Our procedure elicits no data concerning complaints of unknown or uncertain origin even where expert examination might have concluded that a contusion, laceration, or other qualifying insult must have been involved.

The HIS procedure we followed involves essentially event recall rather than current condition screening questions. It uses mostly past and past-imperfect tense constructions in its screen questions. The reasoning underlying our recommended approach indeed suggests that the HIS procedure fails to yield reports of some conditions and events that would be yielded by present-imperfect grammar.

Implications for future work

This study was undertaken to evaluate the feasibility of the use of injury screening for the identification of victims of criminal interpersonal violence and, if the approach were found fruitful, to recommend "a full-fledged injury survey" or alternative approaches.

Although this small exploratory effort suggests potential utility for the strategies investigated, the results are not sufficiently definitive to allow recommendations of immediate alterations or supplementations of the National Criminal Victimization Survey. The results do suggest the value of further research exploration of screening for injury and other consequences of crime as approaches in victimization surveys. Some of the avenues we see worth pursuing are of direct and exclusive pertinence to criminal justice statistical endeavors; others involve linking criminal justice to other concerns; and yet others are of such broad methodological or substantive pertinence as to transcend the immediate interest in criminal justice statistics.

The implications of the exploratory work are also separable into those that relate to the general strategy of focusing screening on injuries (or, yet more generally, on the larger, more objectively identifiable classes of harms of which those due to crime form a relatively elusive subset), as contrasted with the more specific approach of restricting the screening to currently existing conditions. Since we have tested only the latter, more restricted approach in a general population survey, we have little basis for determining how productive injury screening would be were it to be used in an event-recall procedure. This merits trial. The current-consequences approach deals with memory-fade as a function of time, but other facilitations of the reporting task might be contributed by recall of past objective consequences. This would be true, presumably, in those cases where the harm is more memorable, less ambiguous, and less threatening for the respondent to remember and mention than is the law violation involved as its cause.

The objective-consequences strategy has substantive as well as procedural significance. It affords a basis for gaining data on phenomena that fall in a gray area—which from the standpoint of given criteria of evidence and judgment involve some degree of ambiguity as to whether they did or did not involve crimes. It is important to develop information on the size of this gray area relative to that which we unambiguously label "crime" and, should it prove large, to develop means for taking account of such phenomena in analyses of the incidence of crime and the significance of its effects.

In the work completed, attention was concentrated on the potential feasibility and usefulness of identifying crime as a cause of current injury conditions. For estimating the sample size requirements for a survey of criminal injury victimization using current-consequences screening, the results of our trial have the following implications (accepting data from Washington as not grossly atypical). A survey of 1,000 adults might be expected to yield approximately 30 (± 10) who possessed one or more injuries they attributed to crimes. For data sufficient for substantially detailed statistical analysis, therefore, one would need to screen a sample including no fewer than 10,000 completed cases. Such a sample would be expected to yield about 200 to 400 persons suffering from injuries due to "criminal" acts. An equivalent number of cases for causes within a 1-year reference period would require roughly four times as large a sample. (Since some proportion of the respondents would be suffering from effects of more than one crime event, the number of events would be greater than the number of victims in the sample.) To identify these cases for detailed interviewing, the survey would have to permit administration of simple screening questions (two to four straightforward questions) to everyone in the sample and then detailed followup questioning to those suffering from any injuries (judging from our results, about one-sixth of the total sample).

Presumably, improvements in the screen questioning techniques are possible that would make the survey at least somewhat more productive of eligible cases than was true in this first trial. On the other hand, some of the injury causes that respondents were willing to label "criminal" in response to a single question would not accord with desired external definitions of "crime" that might be applied to more detailed information from the respondent.

Clearly, it would be wastefully inefficient to undertake a survey devoted exclusively to current injury screening for the purpose of identifying crime victims, particularly so if analyses of incidence of crime rather than prevalence of effects were of primary importance. Although the technique has value, economy requires that it be pursued operationally in conjunction with surveys directed to other purposes or that also use other approaches.

While our results suggest that the strategies explored in these tests have value that merit their consideration for use within surveys oriented exclusively to the generation of crime statistics, a more important implication of the present study is the need for bridging the institutional compartmentalization of statistical systems. From the standpoint of data-collection efficiency, great economies would be realized by pursuing information regarding crime as cause of injury within surveys directed more broadly toward the topic of injury, or even toward health in general. From the standpoint of the meanings and uses data may have, there is also great value from examining crime as a source of harm to physical well-being within the context of inquiries into the topic of physical well-being. The ordinary perspective of crime statistics asks: "What number or proportion of crimes involve injuries to victims?" The methodology pursued here asks "What proportion of injuries involve crimes?" The latter type of question provides a metric for many problems of social evaluation and social policy within the criminal justice field that are not given by the former. Furthermore, it affords a source of information regarding the ways in which criminal justice matters are bound up with those in the realm of health and safety.

In connection with this study, some preliminary discussions were held with representatives of other agencies regarding the feasibility of pursuing some of the criminal justice statistics interest in injury events and other classes of misfortune jointly with other current or prospective data-collection efforts. Such cooperative arrangements merit vigorous pursuit.

The use of objective and current-consequences approaches may also prove valuable for investigating the impact of crime on life domains other than physiological health. Something close to this orientation has already figured in a number of victimization surveys in the form of questioning about residence and neighborhood; for example, questions about actual, intended, or desired changes of residence with followup questioning to determine whether these were provoked by direct victimization. Other domains that could be explored in this fashion are social relations, personal property, working life, and psychological and sexual adjustment. One strategic multipurpose vehicle might be general screening surveys of the impacts of various kinds of severe disruptions in the normal course of life of individuals and families, with followup interviewing of those cases pertinent to interests of specific agencies charged with preventing, offsetting, or compensating for social misfortune.

With regard to the modification or supplementation of National Crime Survey data by use of objective and current-consequences screening strategies, further exploratory study is needed to: (1) establish more reliably and for national samples how productive of data various alternative approaches would be, (2) to improve and validate interviewing and analytic procedures, and (3) to examine the feasibility of applying these strategies to areas other than physical injury.

Of various alternatives we have considered, the following appear to us of most immediate merit:

(1) Validation and instrument development studies using mechanisms such as those of the Consumer Product Safety Commission's National Electronics Injury Surveillance System (NEISS) to identify victims for followup who have suffered from those classes of injury most commonly characteristic of interpersonal violence.

(2) Cumulation of a sufficient number of cases from national samples to establish the order of magnitude of the prevalence of crime-caused injury among the population. This may be accomplished by incorporating items similar to those used for the present test in omnibus national surveys.

(3) A limited special survey to explore question patterns covering a range of crime-caused conditions broader than injuries alone, as a means of determining the more general utility of a consequences-oriented questioning procedure for gaining criminal victimization data. This special survey might well include short reference period recall items as well as current conditions in its screening battery. Data should be developed in sufficient detail to provide a basis for treating analytically events that fall in the gray area between criminal and noncriminal. By identifying the variable factors that determine when victimization is defined and acted on as criminal victimization by victims and others, such a study would provide bases for improvements in both the methodology and the interpretation of crime statistics.

Differences between survey and police information about crime *

by ANNE L. SCHNEIDER

Problems in relying on the Uniform Crime Reports (UCR) or other types of officially recorded crime data to obtain an accurate measurement of crime are relatively well known. The best documented problems include the fact that official crime reports contain no record of the "dark figure of crime" resulting from victims who do not report the offenses. In addition, case studies of many police departments indicate that some proportion of the crimes that are reported by victims are not recorded as crime events. There is some documentation indicating that police departments may, at times, systematically "down-classify" crimes in order to reduce the overall crime index, which includes only the more serious offenses.

Recognition of these and other problems resulted in major efforts in the 1960's to develop alternative methods of measuring crimes through surveys of the general population. After a number of pioneering methodological studies, LEAA and the Census Bureau implemented a series of victimization surveys in the larger cities of the nation, and in 1972 began the National Crime Survey, which is a nationwide rolling interview of randomly selected households.

The first methodological studies used a reverse record-check procedure in which victims' names were drawn from police records of reported crimes. These victims were then interviewed using a victimization survey instrument designed to jog their memories and to elicit details of the crime incident. The major purposes of the early methodological studies were to establish the most efficient length of the recall period (3 months, 6 months, 12 months); to finalize the most effective types of memory-jogging questions; and to establish methods for minimizing problems introduced by the fact that victims tend to telescope into the recall period incidents that actually occurred prior to the earliest month in the desired time span.

These methodological studies focused exclusively on one general type of bias in the victimization data: bias in terms of whether the survey provided an adequate measure of the amount of crime that occurs and/or changes in the amount. Major emphasis was placed on measuring the extent to which victims "forget" incidents that they reported to the police (forget to recall them for the interviewer or, for other reasons, fail to tell the interviewer about them), and the extent to which victims telescope incidents into the recall period from a timeframe prior to the most distant month included in the survey design.

Another question of concern is whether the victimization data provide an accurate portrayal of the types of crimes that occur, the "facts" about the events, the seriousness of the crimes, characteristics of suspects, and so on. As Biderman has pointed out, recalling crime events or the details of them is not an easy task for survey respondents:

> The survey method is dependent upon the recall of the respondent. This can be particularly unreliable when he is asked to recall a past event which had few serious durable consequences for the victim or demands for further action on his part. . . .

Errors in the survey data could be produced by victims' inability to recall correctly the details of the crime, especially if the event occurred several months in the past. Victims may systematically overestimate or underestimate the seriousness of the crime in comparison with information that they provided to the police. Of particular concern is whether certain types of respondents systematically overestimate or underestimate the seriousness of the event. If so, the survey data will provide a biased portrayal of the distribution of the seriousness of crimes. It also is possible that the time lag between the crime event and the interview has an impact on the respondent's memory of the details of the crime that could result in systematic overestimation or underestimation of crime seriousness. If most persons, for example, tend to forget the more serious aspects of the event as the time lag increases, then the seriousness would be underestimated by the survey. If most persons tend to accentuate the degree of seriousness as the time lag increases, then the survey data would overestimate seriousness.

Concern also has been expressed by some that the classification procedures used by the survey could result in biased data. Most studies of the victimization surveys indicate that survey-generated estimates of the amount of crime are considerably above UCR estimates, even when one excludes crime events from the survey data that respondents say were not reported to the police. The magnitude varies from one study to another, but some analyses indicate that the survey estimates of reported crimes are twice as great as police estimates. This phenomenon could be produced by respondents' saying the event was reported when, in fact, it was not. Or, it could be due to police practices of not recording certain types of events or of down-classifying them. Still another alternative is the possibility that survey-crime classifiers are responding to a different set of information than were the police and systematically overclassify the crimes, resulting in a greater number of incidents in the more serious categories than were known to the police.

James Levine, in a highly speculative condemnation of victimization surveys, argues the following:

> Because coders must make decisions solely on the basis of unclear, incomplete accounts of respondents as filtered secondhand by interviewers, they inevitably play a role in determining the amount and kinds of crime ultimately extracted from interviews.... Since there are many marginal cases of criminality that are reported (in the interviews) and few precise coding guidelines, many 'crimes' that emanate from the surveys may be artifacts of the coding process....

The purpose of this report is to compare information given by the victim to the survey interviewer with information given by the same victim to the police at the time the crime occurred. More specifically, the purposes are to compare and analyze differences between survey and police data for a set of 212 matched cases in relation to each of the following:

1. The classification of the crime (using UCR classification rules) and the details of the event that are used to produce the classification;
2. The seriousness of the offense, using the Sellin and Wolfgang index of

*Excerpted from: Anne L. Schneider, "The Portland Forward Records Check of Crime Victims" Final Grant Report from the Institute for Police Analysis (Eugene, OR) to the Law Enforcement Assistance Administration (Grant No. 76-NI-99-0084), 1977.

seriousness and the total amount of dollar loss from the crime;

3. The characteristics of the offenders, as recalled by the victim;

4. The activities of the police, as recalled by the victim compared with police records of their activities; and

5. The behavior and activities of victims and witnesses during the crime.

The analysis will include a presentation of the amount of case-by-case difference and an examination of the correlates of differences between the survey and the police data.

Methodology

The methodology used in the study is a forward record check of crime events reported to interviewers during the 1974 Portland, Oregon, victimization survey. The forward record check involved selecting all of the crime events reported in the Portland survey that occurred within the city limits of Portland and that respondents said were reported to the police. The address of each crime had been coded by street and house number in the original survey data. A search was made of all original police reports for a time period preceding the earliest month of the survey recall period by at least 16 months. If a crime event was found at the proper address, the report was checked against the survey data to determine whether the two events involved the same victim or household. If so, and if the event constituted a "definite match" with the survey data (see definition below), then the search procedures were stopped for that event. If the event did not fit the definite match category and/or if the victim was different from the one in the survey, then the search procedure continued by examining all original police reports involving crime incidents within five square blocks of the location of the survey crime. If no crime events involving the victim or household on the survey were found within five square blocks, the event was classified as a definite "no match." It should be emphasized that the search procedure involved all crime events, regardless of the classification used by the police, for a time period beginning in January 1972 and continuing through September 1974. The earliest month of recall required by the survey was April 1973.

Approximately 16 percent of the crime events mentioned in the survey contained no address precise enough to locate the event in police data. In addition, slightly less than half of the incidents with precise addresses could not be found in the police data. To avoid as much bias in the data as possible, a name search was initiated for all the survey incidents in which the respondent had given at least a last name. There were 89 victims who gave their names, and 103 incidents were reported by these persons to the interviewer. (This is approximately 25 percent of the total number of crime incidents that, according to the victim, had become known to the police.) The name search involved a double-blind procedure in which police department personnel conducted the search and provided us only with the report number of incidents that might be the ones that matched the survey data. These reports were then pulled and compared against the questionnaires. The name search was not very productive. Only 12 incidents were found through the name search that were not also found through the address search alone.

After all the search procedures were finished, the incidents were divided into one of two categories, as a first step in developing the final judgment about whether the police report concerned the same crime reported on the survey.

1. Definite match. A definite match was defined, initially, as a victim and an incident that matched the survey data in virtually all relevant aspects. The rule was that 90 percent or more of the relevant victim/household characteristics should be the same between the survey and the police data. Age should be within a year or two; sex, race, and occupation should be correct; the address of the incident and of the victim should be the same; the phone number should match; and the partial name identifier should match. On the first phase of determining whether a match existed or not, we required that 90 percent or more of the relatively unusual aspects of the event reported in the survey data and on the police form should be the same. Characteristics of the crime itself could not be used (e.g., classification, date, weapon, type of entry, etc.).

2. Definite no-match. A definite no-match decision could be made if there was no record of a crime having occurred at the location (or within five square blocks of it) against a victim who bore any resemblance to any household member in the survey. In addition, an event was considered an unmatched crime if we found reference to the event, but the police had not filled out a separate crime report on it. This happened several times in apartment or boarding-house burglaries. The police filled out a report on the most serious crime and listed the other incidents and their victims in the narrative section of the report. The third type of no-match were the crimes for which the location given by the survey respondent was too vague or was not known at all and the name given by the respondent was not sufficient to use in the name search. Thus, no search could be undertaken for these crimes.

These rules were sufficient to categorize almost all of the incidents either as matches or no-matches. There were, however, 21 survey incidents (4 percent of the total) that could not be categorized either as a match or no-match using these criteria, but decisions on most of these were quite straightforward.

The results of the forward record check are shown in table 26.

The problems in determining whether a police event matched the survey event were far less severe than we had anticipated. Persons who conduct reverse record checks also must determine whether the victim is recalling the same event that was drawn from police files or a different one, but there has been very little discussion of this or of the methodology used to determine whether an event matched or not. Richard Sparks reports that only 4 of 237 events (2 percent) in his London reverse record check did not match the police report closely enough to consider it the same event, but no other authors of reverse-survey studies have discussed the problem or the methodology used to match events.

It should be emphasized that some bias could be introduced into a study by the methods and decisions used to match

Table 26. Results of the forward record check

Category	N	Percent of total	Percent of events for which search was undertaken
Definite match	212	45	53
No search (vague address)	77	16	--
No-match			
No record of victim or event or crime at location of survey incident	160	34	40
Event found; no separate crime report filed by police	6	01	02
Police report of victim or household found but incidents do not match	21	04	05
Total	476		
Total for search			399

Table 27. Classification differences, by type of offense

	Same classification by police		Different classification by police	
	N	Percent	N	Percent
Personal (total)	12	75	4	25
Rape	0	--	1	--
Robbery	2	--	1	--
Assault	10	83	2	17
Property (total)	181	92	15	8
Burglary	106	97	3	3
Larceny	55	82	10	18
Auto theft	18	100	0	0
Miscellaneous	2	--	2	--
Total	193	91	19	9

the crimes. If the rules require too much similarity, then the data will show closer correspondence between the characteristics of the survey event and the police event. If the rules require too little similarity, then apparent differences will be introduced into the data which, in fact, are the result of different crimes having been reported to the interviewer and to the police. As noted above, the task of matching was simpler than we had anticipated, and the use of the "unique-identifiers" approach resulted in clear judgments concerning match or no-match for most of the crimes.

Our findings are summarized in table 27. Ninety-one percent of the incidents were classified into the same major crime category, and 9 percent contained sufficient informational differences to produce a different classification.

It is clear that personal crimes in the 1974 survey were more likely than property crimes to be classified differently. The number is too small to draw definite conclusions, but it is interesting to note that in 25 percent of the personal crimes, the information provided by the victim to the interviewer was sufficiently different than that provided to the police to change the classification.

Results of the Portland tests for property offenses are identical to the comparison of police and survey classification conducted in San Jose. Using police data as the standard, the San Jose survey correctly classified 97 percent of the burglaries and 82 percent of the larcenies. Survey classification of personal offenses was the same as police classification in 85 percent of the cases, whereas the Portland police classified 75 percent of the survey personal crimes into the same categories as the Portland survey. This difference between Portland and San Jose is not statistically significant.

Differences in recovery by seriousness

Two different types of seriousness measures are used to determine whether there is any systematic overestimation or underestimation of seriousness in the survey data, compared with police records of the same events. The analysis also includes an examination of victim and interview characteristics to determine whether certain types of victims, or certain types of situations, result in an overestimation or underestimation of the seriousness of the crime.

The first seriousness scale is a replication of the Sellin and Wolfgang index. Our survey data generally produced slightly higher estimates of crime seriousness than did the police information for the same events. The product-moment correlation between the survey and police seriousness scores is +.63 (R^2=.40).

A considerable portion of the survey's higher estimates of seriousness is produced by two indicators used in the scale. Survey data were more apt to indicate that the offender had a weapon, and the survey data generally provided higher estimates of the amount of loss from the crime.

Comparison of survey and police information on amount of loss is shown in table 28. In every type of comparison, the survey estimates are higher than those provided by the police, even though the correlation coefficients between logged estimates of loss are rather high. The implication is that either the survey respondents systematically overestimate the amount of loss or the police underestimate it. In the auto-theft category, there were many police reports that contained no value at all for the stolen car, and this greatly inflated the difference between survey and police estimates of loss. The average loss from burglaries is considerably higher than one might have anticipated, primarily because of one incident that involved a loss of more than 14,000 according to both the police and the survey information.

Further analysis was undertaken to determine if there are any particular types of people who are more inclined to make errors in estimating the seriousness of the crime, or if there are any particular types of persons who are more likely than others to overestimate (or underestimate) the seriousness of the event. In addition, we were interested in determining whether the time lag between when the crime actually occurred and when the survey interview took place had an impact on the respondent's propensity to make errors or to overestimate or underestimate the seriousness of the crime. The results of the analysis are shown in table 29.

Forward telescoping refers to the survey respondent's tendency to pull events forward in time, placing them closer to the date of the survey interview than they should have been. As shown, individuals who are more apt to make forward-telescoping errors are neither more nor less likely to overestimate the seriousness of the crime or overestimate the dollar loss. Further, the differences between survey and police information on seriousness and dollar loss are neither greater nor less for these persons. (Net difference in seriousness and dollar loss refers to the absolute value of the difference between the survey and the police data.) One of the implications of this finding is that persons who overestimate crime seriousness to the interviewer (or from whom the police underestimate the seriousness) do not tend to pull the events forward in time to any greater extent than others. If the opposite were true, then survey victimization data would be biased because of the convergence of individuals' pulling events into the recall period who are more apt to overestimate (or underestimate) their seriousness.

Net telescoping refers to the absolute amount of error made by the survey respondent in recalling the month when the crime took place. Persons who make more errors in the date are not more inclined to have overestimated the seriousness, nor did we find more absolute

Table 28. Survey and police estimates of loss from crime

	Percent of cases with no loss indicated		Average dollar loss		Average loss excluding "no loss" category		Median loss	
	Survey	Police	Survey	Police	Survey	Police	Survey	Police
Burglary	19	21	$548	$412	$680	$522	$300	$155
Larceny	12	14	126	96	143	112	100	75
Auto theft	10	56	662	186	736	419	500	260
All incidents	16	21	411	281	488	357		
Burglary:	r=.81*							
Larceny:	r=.77							
Auto theft:	r=.60							
All cases:	r=.82							

*Correlation coefficients derived from logged dollar values given on the police and survey forms.

Table 29. Correlates of overestimating and underestimating crime seriousness

	Overestimates		Net differences	
	Seriousness scale	Dollar loss	Seriousness scale	Dollar loss
Forward telescoping	.00	-.07	.06	-.09
Net telescoping	.00	-.09	.01	-.14
Time lag from crime to survey interview	-.01	-.09	.01	-.14
Race (0=black; 1=white)	.00	-.01	.01	-.01
Sex (0=female; 1=male)	.00	-.05	.05	.00
Education	.01	-.10	.03	-.00
Age	-.02	.07	.03	.04
Positive attitude toward police	.00	.00	-.01	-.04

differences in the amount of seriousness reported to the interviewer compared with the police for these persons. The correlation of -.14 between the amount of telescoping and the amount of difference in dollar loss is close to statistical significance, however, and the possibility must be left open that persons who are inclined to make one type of error (in the date) are slightly more apt to make other errors in their reports to the interviewer or the police.

The time lag refers to the amount of time that elapsed between the crime and when the interview took place. If longer time periods result in the survey respondent's providing different information to the interviewer compared to that given to the police right after the crime, then positive correlations should be found between the time lag and the net differences in seriousness scales and the net difference in amount of loss. There is no correlation with absolute difference in the seriousness scale, and the correlation with differences in dollar loss is in the opposite direction from what we anticipated. Thus, it appears that the delay between the event and the survey interview does not produce distortions in the information concerning loss or seriousness. Furthermore, the weak and insignificant correlations between time lag and overestimates of seriousness suggest that respondents do

not systematically accentuate the seriousness of the crime as time passes, nor do they systematically distort the event in such a way as to recall it as being less serious than the event recorded by the police.

Four indicators of respondent socioeconomic characteristics were correlated with the direction and amount of differences between police and survey information concerning crime seriousness. These data indicate that race, sex, educational level, and age are not related to the type of differences nor to the absolute amount of differences.

The original survey data included several questions designed to tap the respondent's attitudes toward the police. These were correlated with the amount and direction of differences to determine whether persons who hold more positive attitudes differ in any systematic way concerning the direction or magnitude of differences between the information they provided the interviewer and the information recorded by the police. The type and amount of differences between survey and police data are not related to the respondent's attitude toward the police.

Differences in recovery and characteristics of suspects

Each respondent to the survey was asked whether he or she knew how many persons were involved in the crime; the age, race, and sex of the suspects; and whether the person(s) was a stranger or was known to the victim. Similar information was pulled from the original police reports for each of the matched incidents.

Race of suspect

Both the police and the survey data indicated that 28 of the offenses were committed by whites, but the two sources of information agreed that a white person was a suspect in 13 incidents and disagreed on the others. Survey data indicated that 31 incidents involved a black suspect, whereas the police records showed that black persons were suspected in 25 incidents. There were 129 crimes for which neither the survey nor the police data contained any information about a suspect (61 percent of the total). The data are detailed in table 30.

Table 30. Race of suspect

	Police classification				
	White	Black	Other	Unknown	Total
Survey classification					
White	15	3	2	8	28
Black	1	13	0	17	31
Other	1	1	0	2	4
Unknown	11	8	1	129	149
Total	28	25	3	156	212

Total agreement: 157/212 = 74%

Agreement excluding unknown category: 28/83 = 34%

The total amount of agreement between police and survey data consists of the number of incidents on which both agreed on the race of the suspect or agreed that the suspect was unknown. The two sources agreed on 74 percent of the incidents and disagreed on 26 percent. Clearly, the greatest amount of agreement, in absolute terms, is that the race of the suspect was unknown (129 cases). If these are excluded, the agreement between police and survey data concerning racial characteristics of suspects is only 34 percent.

Concern has been expressed by some persons that victimization survey data may not be an accurate reflection of racial characteristics of offenders if victims project racial bias or prejudice into their perception of who committed the crime. The data show that there were 31 black persons suspected by the victims, but more than half of the police reports on these incidents (58 percent) indicated that the suspect was white or had unknown racial characteristics. Of the suspects identified in the survey as white, 46 percent were recorded as unknown, black, or "other" in the police data. Although the number of cases is very small, the data indicated that these victims slightly overestimated the number of incidents involving black suspects in comparison with police estimates of whether the suspect was white or black.

A similar phenomenon is found when one examines survey responses concerning racial characteristics of persons that the police record data show as unknown. Of these cases, there were 27 for which survey respondents claimed to have information on the racial characteristics. Eight (30 percent) were characterized as white, compared with 70 percent as either black or Hispanic. Police data compared with survey "unknowns" do not show this pattern. There were 149 cases of unknown suspects according to survey respondents; of these, the police records contained racial information on 20, with more than half (55 percent) being characterized as white and 45 percent being characterized as black or other.

Additional analysis of the data shows that black victims, rather than white victims, are primarily responsible for overidentification of suspects as black when police data contain no information on racial characteristics of the suspects. For white victims, there were 20 cases in which the police did not record any information on race of the suspect. The white victims told the interviewer that 12 of these (60 percent) were white and 8 (40 percent) were black. Black and Hispanic victims provided information on [illegible] cases that the police said involved an unknown suspect, and the victims indicated that five of the seven were black rather than white.

The data presented indicate that victims have a very slight tendency to suspect blacks when the police data indicate the suspect is unknown, but there is no evidence at all that this is due to white victims projecting racial bias into their identification of suspects. Black persons may "oversuspect" blacks even more than whites do.

Offender known or stranger

It is widely suspected that victimization surveys underestimate the proportion of incidents committed by persons known to the victim. This phenomenon could be produced by the greater saliency of stranger-perpetrated incidents and a corresponding inability of victims to remember offenses committed by persons they know. It could be due to the victims being reluctant to tell the interviewer about incidents committed by friends, acquaintances, or household members. Another possibility, and the only one that can be examined with the matched incident set, is that victims report the crime to the interviewer but do not provide accurate information concerning the fact that they knew who the offender was. The data in table 31 do not show any support for this possibility, however. If we assume that the police records are correct with regard to whether or not the suspect is known to the victim, then the survey elicited the correct response in 13 of the 25 cases (52 percent) that the police said involved persons known to the victim. The survey elicited the correct response in 25 of the 43 cases that police data show involved a stranger (58 percent). The differences in survey inaccuracies are not sufficiently great to conclude that victims intentionally fail to tell the interviewer that they were acquainted with the suspect.

Table 31. Offender known or stranger

Survey classification	Police classification: Stranger	Known	No data	Total
Stranger	25	6	16	47
Known	2	13	9	24
No data	16	6	119	147
Total	43	25	144	212

Total agreement 157/212 = 74%

Agreement excluding no-data category 38/93 = 41%

Other characteristics of suspects

The victimization data did not differ much from police records in terms of the average age of suspects, the number of offenders, or the sex of offenders. The average age, from both sources of data, was between 18 and 19 years and both sources indicated that approximately 30 percent of the suspects were known to be male (most of the others were unknown). There were no misclassifications of sex of offender in that none of the females identified in the survey (or by the police) was classified as male by the police (or in the survey) and none of the males was classified as female. (See table 32.)

The major conclusion to be drawn is that the survey and police data generally provide very similar aggregate portrayals of the characteristics of offenders even though there is substantial case-by-case disagreement between the two sources. The implication of this is that either source of information would be adequate to describe characteristics of offenders. However, if one wished to analyze correlates of offender characteristics, there are two problems. The first is that one or the other source of data contains considerable case-by-case error (or both have considerable error) that could produce different results for the analysis, depending upon which data set was used. If the error is random, then the strength of association would be diminished but the results should be the same regardless of whether one conducted the analysis on survey data or on police data.

Table 32. Other characteristics of suspects

	Survey	Police
Age of suspect* ($\bar{x}$)	18.2	18.7
Number of suspects ($\bar{x}$)	1.9	2.8
Percent of all incidents with male identified as suspect	30	30

*This includes estimated age of youngest and oldest suspects.

Correlates of errors

Correlation coefficients describing the strength of relationship between selected independent variables and the error in information about offender characteristics are shown in table 33. Of particular concern to those who conduct victimization surveys is the question of whether the time lag between when the crime took place and when the interview was conducted contributes to the amount of error.

It is reasonable to propose that survey-generated information about offenders involved in incidents that occurred more recently in the recall period will be more similar to the police information. The problem of memory decay and distortion in the recall of factual events tends to be a function of time between the event and when the data about the event were collected. To test this possibility, the time lag between interview and the actual date of the crime was correlated with the amount of error in recollection about offender characteristics. The negative relationship indicates that Portland victims in the matched incident set have a slight tendency to produce more accurate information about events that occurred during the more distant months. These relationships are not statistically significant, however, and generalizations cannot be made to a broader population.

The data in table 33 also indicate that persons who are more apt to make errors in recalling the date of the event (net telescoping) are neither more nor less apt to make errors about the characteristics of offenders. In addition, incidents that are forward-telescoped are not more apt to have errors in information about the suspects.

Another proposition that was tested concerns whether the survey and police information about offenders is more accurate (e.g., more similar) for serious crimes than for less serious ones. One might propose that most of the differences between survey and police information are attributable to the victims' inability to correctly recall information about suspects during the interview situation and that the tendency is most marked for trivial rather than serious crimes. The latter, being more salient,

Table 33. The correlates of measurement error

Characteristics of victims	Correlates of absolute amount of differences in offender characteristics*: Race N=36	Offender stranger or known N=46	Number of offenders N=43	Age of youngest N=35	Age of oldest N=14	Correlates of direction of differences in recollection about offender characteristics**: Race as white N=36	Offender as stranger N=46	Number of offenders N=43	Age of youngest N=35	Age of oldest N=14
Forward telescoping	-.16	-.20	-.17	.16	-.25	-.05	-.12	-.10	-.08	-.14
Net telescoping	-.16	-.19	-.06	.08	-.17	-.13	-.06	.07	-.06	-.04
Time lag from incident to interview	-.12	-.21	-.02	.13	-.33	-.11	-.08	-.07	.10	-.28
Seriousness of crime (survey estimate)	-.30[a]	.03	-.08	-.05	-.03	-.08	.13	.07	-.02	.02
Seriousness of crime (police estimate)	-.20	.03	-.07	.09	.07	-.06	.13	.02	-.23	-.00
Race (0=black; 1=white)	.15	-.06	-.06	.11	--	.06	-.12	-.14	-.02	--
Sex (0=female; 1=male)	.35[a]	-.19	-.09	.13	-.21	.06	-.19	.04	-.18	-.28
Education	.21	-.06	.10	-.08	-.54[a]	.04	.01	-.15	-.06	-.50[b]
Age	-.00	.19	-.02	.26	.45	-.06	.07	-.13	.06	.51[b]
Positive attitudes toward police		.09	.02	-.16	.29		-.09	-.13	.07	-.08

*Positive correlations mean that a higher score on the characteristic is related to greater error (differences) between the survey and the police data. The letter a indicates the correlation coefficient is significant at the 0.5 level.

**Positive correlations mean that higher scores on the characteristic listed on the left are related to the survey data "over-reporting" or the police data "under-reporting") the characteristics of offenders listed across the the top. The letter b means the correlation is significant at the 0.5 level

should be recalled with greater precision. The data in table 33 provide very weak support for the proposition. The seriousness of the crime as measured from the survey data and from the police data is correlated with each type of error, but only 1 of the 10 correlations is significant. The negative relationship of -.30 between survey estimates of crime seriousness and errors in race of the suspect indicates that more serious crimes tend to be characterized by fewer errors. Characteristics of the offender generally are not correlated with the amount of error, but there are two exceptions to the pattern. Crime incidents involving men are characterized by more differences between police and survey information about the race of the suspect. And incidents involving persons with higher educational levels tend to show fewer differences between police and survey information about the age of the oldest offender in multiple-suspect crimes. It should be emphasized, however, that we tested 50 different relationships. Using the .05 significance level, one would expect to find two or three "significant" correlations by chance alone. Thus, substantive significance should not be attributed to the significant correlations in table 33 unless they are replicated in other studies.

The data in table 33 also show correlation coefficients between selected independent variables and the direction of differences in survey and police information. There are two major purposes for examining correlates of the direction of the differences in police and survey data concerning offender characteristics. The first is to determine whether the time lag between the crime and the interview data is associated with memory distortion concerning offender characteristics. As shown, there are no significant correlations indicating that survey information about suspects does not become distorted as a function of time lag.

The second major purpose is to determine whether certain characteristics of the incident or the offender are associated with systematic differences between police and survey information. This is of interest to persons who may be using survey data to test propositions involving offender types and any of the independent variables shown in the table. For example, one might test the proposition that younger offenders commit less serious crimes than older offenders. If this were tested and a significant correlation obtained from survey data, one would have to consider the possibility that victims underestimate the seriousness of an offense if it is committed by a younger person or, conversely, that victims overestimate the age of the offenders as a direct function of the seriousness of the crime. Another example would be a study in which the researcher used survey data to test the proposition that younger victims are more apt to be involved in crimes perpetrated by younger offenders. If support were found for the proposition, one would have to consider whether or not victims tend to distort the age of suspected offenders to be closer to their own age.

There are almost no statistically significant correlations between the direction of differences and characteristics of the victim or crime incident. The two statistically significant correlations in table 33 (at the .05 level) would be expected on the basis of chance since there are 50 correlations in that part of the table.

Table 34. Multivariate analysis of differences between police and survey data (standardized beta coefficients)

	Absolute amount of error (differences) in relation to:								
Predictor variables	Dollar loss N=154	Seriousness N=202	Police activities N=202	Presence of witness N=135	Victim activities N=133	Race of offender N=34	Age of youngest or only N=33	Stranger or known N=42	Number of offenders N=40
Absolute value of differences									
Time lag from crime to interview	-.09	.06	.11	-.10	.03	.06	.13	-.08	.05
Seriousness of crime (survey)	X	X	-.01	.10	-.20*	-.05	.18	-.19	-.11
Telescoping, absolute value	-.06	-.05	-.19*	.11	.19	-.20	.13	-.15	-.13
Age	.01	.02	-.04	.14	.02	.02	.36*	.14	-.05
R^2	.02	.00	.02	.03	.09	.03	.13	.11	.02
Direction of differences									
Time lag from crime to interview	-.06	.07	.02	-.21*	-.08	-.04	.27	-.04	-.26
Seriousness of crime (survey)	X	X	-.08	.02	.00	.09	.00	-.19	.19
Telescoping, absolute value	-.03	-.06	.00	.06	.12	-.12	-.22	-.05	.26
Age	.05	-.03	-.03	-.11	-.04	-.15	.07	.03	-.13
R^2	.01	.00	.01	.05	.01	.05	.05	.05	.09

Note: Survey seriousness is not independent of the dependent variables because of the variables used to develop the seriousness score, and thus it was not included in these multiple regression analyses.
*$P<.05$

Police error or survey error?

It is impossible to make any definitive determination of whether the differences between survey and police data concerning any of the characteristics examined in this section of the report are attributable mainly to the survey or mainly to the police. When differences exist, it is possible that the respondent provided different information to the police than to the survey interviewer, but it is also possible that the recording of information introduced some of the error.

Some indirect information can be developed, however, concerning the extent to which memory distortion or decay produces errors in the survey data. We used four procedures to test this possibility, all of them based on assumptions concerning the nature of memory decay and distortion:

1. It is reasonable to assume that respondents forget and/or distort information as a function of the time lag between the event and the interview. Thus, correlations between time lag and either the amount or direction of error are an indication of survey error rather than police error.

2. It also is reasonable to assume that respondents are better able to recall accurate information about serious crimes than about trivial ones. Thus, correlations between seriousness and the amount or direction of error would be evidence that the survey data are the source of the error.

3. There have been some studies that suggest that memory decay and distortion are more pronounced and occur more rapidly for older persons. If this is the case for crime information, then correlations between age and the amount or direction of error would indicate that some proportion of the differences between survey and police data is attributable to the survey.

4. There is one type of error that is known to exist in the survey rather than in the police data. Errors in recalling the date of the event (telescoping) are attributable almost entirely to the survey. If we assume that persons who make one type of error also tend to make other types of errors, then a correlation between telescoping and the other kinds of error would indicate that some portion of the erroneous information is directly attributable to the survey.

In contrast to these assumptions, errors that correlate with socioeconomic characteristics of respondents such as race, sex, or educational level cannot be attributed either to the survey or to the police data. If persons with some characteristics tend to make more errors than others, it probably is due to the social interaction effects either with the police officer or the interviewer.

To test the propositions, multiple regression analysis was used to determine the amount of variance in the dependent variables that could be explained by the time lag, seriousness, age of respondent, and telescoping. The results are shown in table 34.

The very small and generally nonsignificant values of the standardized beta coefficients in the table, as well as the small amount of combined explanatory power of all the predictor variables, justify a conclusion that neither the amount nor direction of errors can be attributed to memory loss. The implication is that differences between survey and police data are generally random differences rather than systematic errors in the survey data produced by memory decay.

Chapter 3

Conceptual and methodological issues

Introduction

In this chapter, three contributors discuss in various ways the difficulty of deciding what and whom to count in victimization research. In the first selection, Albert Biderman calls for a social accounting approach to the measurement of individual harms. The selection emphasizes the difficulty in determining the "criminality" of many of the misfortunes that befall individual citizens. He advocates beginning with instances of harms, like personal injuries, and tracing their genesis. This approach would lead us to assess incidents in terms of fault, a concept emphasizing the role of contributing factors and precipitating events in establishing the proper classification of events.

The second contribution, by Richard F. Sparks, recommends new approaches to measuring risk of victimization. He reviews traditional uses of crime rates in research and evaluation and the various ways in which they have been calculated. He points out problems in both the numerators and denominators of those measures and argues that they are particularly unsuited for one of the most important applications of such data—indicating how likely it is that something will happen to someone. He calls for the use of opportunity-based rates, which more closely approximate the risks facing potential targets of crime.

In the final section Stephen Fienberg illustrates that the question of what and whom to count is not an easy one and that crime statistics of necessity impose great simplicity on otherwise complex events. He illustrates these complexities by a hypothetical example, which highlights the differences in accounts between the FBI's Uniform Crime Reports and the National Crime Survey (NCS).

When does interpersonal violence become crime? —theory and methods for statistical surveys*

by ALBERT D. BIDERMAN

This paper deals with some theoretical and methodological problems of developing statistical knowledge of differential access to criminal law. For simplicity, it focuses on the conditions under which the criminal law determines responses to an event involving someone injuring another physically.

Attention to the problems of access to law, when the concern has been with criminal law at all, has been most often concerned with law that disadvantaged members of society could well do with less of, rather than more. An extensive theme in the sociology of law is that the repressive power of the criminal law is largely reserved for the poor and powerless. It is they who are the almost exclusive subjects of intimidational and promiscuous forms of law enforcement and judicial penalty and who have poor recourse to legal protections of due process against the criminal law apparatus of the state.

Recently, however, the limited role played by the apparatus of the criminal law in protecting the persons and property of the socially disadvantaged has become a popular public issue. The poor, particularly, are the most frequent victims of the "common" or "predatory" crimes of person against person, it is maintained, but it is also their communities that are served least well by police, prosecutors, and courts. The failures of the state to provide reasonable protection for the lives and property of those who already are socially disadvantaged in many other ways, to be sure, has been a theme particularly favored by ideological conservatives. It has been a convenient way of countering attacks against their proposals for strengthening the agencies of repressive justice. But it has been a powerful argument well grounded in fact. It has been an argument that liberals and leftists currently seem able to neutralize only by its partial incorporation into their own agenda. In the United States, research documentation now supports calls for more equitable allocation of police services to less wealthy areas of cities and for according criminal complaints of the poor the same credence and attention given those of the well-to-do. The theme has been taken up by minority and women's activist organizations, with rape being the focus of the latter's complaints regarding discriminatory law enforcement. Minority activists maintain that the hazards to the victim of being further victimized by law enforcement currently make it injudicious to be a complainant.

The problems with which we will be concerned here go beyond those arising when citizens seek access to law enforcement to deter or punish harms, only to have their approach rejected by officialdom. The criminal law and the apparatus for its enforcement may not be seen by citizenry as having any relevance whatsoever to particular classes of situations in which harms are inflicted and suffered. This can be true in the individual case or collectively, as in demands of neighborhoods for intensified police service. Although in theory the burden of the detection and prosecution of violations of the criminal law is on the state, in fact it rarely acts in cases of the victimization of persons except when it is prompted to do so by initiatives taken by citizens. When they fail to do so, events that may be crimes fail to become matters of official action and record.

Victimization surveys

The recognition that official agencies respond selectively and do not record or process many events that can be legitimately classed as crimes has been one of the justifications for instituting a large-scale victimization survey in the United States. The results of such surveys may illuminate the access to law enforcement open to various elements of the population. These surveys should provide data relevant to the question of the circumstances making agencies of criminal justice responsive and effective, or unresponsive and ineffective. Data from such surveys are particularly essential if we are to learn about events that are never recorded at all and that leave no traces in those official records that have been the main sources of data for studies of the criminal justice system.

Another purpose of victimization surveys is to provide information on the access of law—that is, on events not dealt with by official agencies because they are not reported to them by citizens. Such surveys have sought to elucidate reasons for the nonreporting of crimes by victims. These reasons include the time and trouble that can be occasioned by making things a matter of official attention, a sense of the uselessness of so doing, and sympathy for or fear of reprisal from the offender, among other factors. Each of these impediments to mobilizing the official system may be regarded as an impediment to the operation of criminal law as a means for controlling social harms.

Attention to the victims of crimes predated the development of the victimization survey method. Victimology developed as an empirical field in criminology using data from officially registered events or interviews with those officially identified as criminals or crime victims. To the extent that it informs us of the characteristics of those who fall victims of particular kinds of criminal acts, research on victimology helps illuminate the demand side of our questions regarding access to criminal law. A particular emphasis of victimology, however, has been to consider the victim as belonging on the independent side of causal equations of criminology. We will consider limitations of the use of data from official records for answering questions about the causal relations of victims to crimes and suggest the utility of the criminal victimization survey method for fulfilling the logic of the tasks set out for victimology. We will then examine limitations of the logic and method of these surveys and suggest the need for a method going beyond the criminal victimization survey, if we are to have knowledge of the differential application of law.

Victimology and victimization surveys

By calling into question the attribution of the cause of crime primarily to "the offender" or "the criminal," and by transferring attention to the "victim" as a cause, criminological victimology implicitly involves a questioning of the very definitions of "crime." I refer both to the definitions that are applied in everyday life by the immediate participants in relevant social events and those used by agencies of social control.

Research on victimology has usually been based upon cases officially defined as "criminal." Victimology has sought to correct for the "one-eyed" search for causes of crimes in characteristics and behavior of the injurer and the relative obliviousness that exists to crime-causing characteristics or behavior of the injured. One important train of thought and research in victimology has been especially interested in "non-innocent" victims—those who act in such a way as to share culpability with the offender in bringing about the crime. In that they are dealing with cases in which the official system has defined the event as a crime, and therefore the offender as guilty ("causative? ") of the crime, such researchers have an explicit or implicit critical thrust against the official system for its neglect of the victim's role in crime.

But the official system is not oblivious to contributory acts of the injured. It screens out of its system of attention and action many social incidents in which one person feels he or she has been harmed criminally by some other(s). Complainants are "cooled out" informally by officialdom, and various proportions of their complaints are formally adjudged "unfounded." Judgments by personnel of the official system regarding causative involvements of the complainants figure in this process, as they may go to the defense or excuse of the alleged offender or as they affect the credibility of the testimony of the complainant.

There are yet prior filterings. Insofar as interpersonal victimization is concerned, the official system is in most instances loathe to proceed without the cooperation of the victim as complainant, even in the case of incidents that come to official attention independently of victim reports. In deciding whether to mobilize the official system, victims apply their own conceptions of whether the act indeed was "criminal," whether it should be made a matter for official attention, and whether the official system would be likely to act sufficiently in accordance with the victim's view and desires were a complaint made. The injured party's view of his or her own role in the event, including the relation to the offender and contributory acts, can enter into his or her decision.

When it studies officially recorded crimes, therefore, victimology removes from its ken a large proportion of all events in which it is clear that Party A suffered harm from an act of Party B, but where judgments by the victim or by official personnel concerning the victim-offender relationship suggested that the event should not be made a subject of official action. These grounds for excluding events from the criminal justice process include all of the classes of judgment that are the central objectives of victimology, whether these be the relations of culpabilities of the "victim" to those of the "offender" of Mendelsohn's victim typology; the risk-predisposing factors of a psychological, social, or biological type to which von Hentig attended; particularistic relations of "victim" to "offender," such as have figured, for example, in Wolfgang's 1958 study of murder victims; or the inversions of customary judgments that figure in the victim-cum-offender theories of neo-Marxist or black power radicals. When only recorded crimes are studied, there is excluded, of course, the special class of victim specified by Reckless as the "non-reporting victim." Victimological research that is based exclusively on officially recorded offenses thereby may be excluding most of the social phenomena with which it is particularly concerned.

To give just a few examples under the various classes mentioned above: Where persons occupy a position in social space that makes them extremely frequent victims of a given type of offense, they tend to refrain from reporting such offenses to the police—if for no other reason than that the burdens of such reporting and followup actions may be intolerably great. This is true of some residents and proprietors of extremely high-crime areas. There may even be reasons to suspect some degree of inverse function between the likelihood of being victimized by a specific class of offense and the disposition to report that type of offense to the police.

It is well known that high proportions of certain kinds of victimization occur to persons occupying deviant social roles—prostitutes, homosexuals, and alcoholics, for example—bulk high in urban police files as victims of robbery, extortion, and assault, but they contribute even more, presumably, to the "dark figure" of unreported crime.

One reason for nonreporting is a victim's belief that officialdom will not find his or her otherwise unsupported complaint credible in the face of countertestimony of the offender. This is presumably often the case where the offender is of much higher social status than the victim. Sexual assaults against domestic servants by their employers are a case in point.

The ideological inversion of victim and offender roles, to take another variant of this victimology perspective, has become a particularly frequent theme in the recent literature of dissent, including that of "radical sociology." The ordinary presupposition of this type of orientation to "exploiter-class"-victim culpability and offender innocence assumes political powerlessness on the lower-class offender's part and the existence of a "class justice" that proceeds against the lower-class offender with disregard for "objective social realities." The unrecorded crimes, from this point of view, are the routine acts of "social exploitation and oppression," with only the retaliatory, compensatory, or revolutionary acts of the "oppressed classes" figuring in recorded criminality.

Particularistic relations of offenders and victims also are a common source of the nonreporting of offenses—for example, offenses of one member of a family against another or those among close friends. Often, such relations keep an act, which would be regarded as criminal if committed by a stranger, from being defined as criminal at all by the parties involved.

Victim sharing of culpability for the criminal episode is a particularly obvious instance in which victims would be unlikely to report an offense or to assist in the criminal justice process.

Without proceeding to illustrate each of the classes of cases, we can also call attention to the fact that the independent knowledge by the official system of the occurrence of an offense and an independent disposition on its part to proceed to act upon it tend to vary with the victim's disposition to seek official attention and action.

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*Excerpted with minor editorial changes from a paper prepared for the Access to Law Conference of the Research Committee on the Sociology of Law, International Sociological Association, Cambridge, England, September 1973.

In sum, the foregoing factors affect the likelihood of crime's being recorded by official agencies so that it varies inversely with the degree to which that crime is of interest to the field of victimology. Victimological research that proceeds on the basis of data of officially recorded crimes, therefore, would seem ill-suited for illuminating precisely those questions with which victimology of whatever brand is most concerned.

There are, to be sure, some factors operating in the other direction—that is, to make official recording more likely for subclasses of incidents that derive from some patterned victim-offender relationship than where the offender's choice of a target is more random. This is because the likelihood of the detection and accurate identification of the offender will vary directly with the intensity, distinctiveness, and duration of the offender-victim relationship in question. Although, for some purposes, victimology can make use of crime data in which nothing is known about the offender, the victim is more likely to be identified and recorded as such to the extent that he knows the offender and can lead authorities to him. With the exception of only a few studies, this has meant that the identity of the offender has become known because of an arrest. This is particularly true of statistical studies of victim-offender relationships, since law enforcement agencies rarely process statistical data on offender characteristics, even where an identifiable person is suspect, when no subject is apprehended. Only the exceptional study has gone to police investigatory reports to examine such limited categorical data furnished by witnesses or complainants as the race, sex, or number of offenders involved in a given incident. In these ways, the data available to victimological study are disproportionately loaded with cases in proportion to the particular significance of certain patterned offender-victim relationships to the system.

What manner of sample, then, can victimology have of criminal events, given the double-edged biases in the data available to it? No crime involving a victim is totally independent of acts or traits of the victim in relation to those of the offender, even if these be no more than those resulting in an intersection of the time-space trajectories of the victim's person or property and the reach of the offender. It is difficult to think of any general class of these victim characteristics that does not affect the likelihood of crime definition and recording as well as crime occurrence. The selective nature of official data affects the three varieties of interest that are variously displayed by different students of victimology:

1. Scientific; that is, an interest solely in the causal association of victim and offender acts or characteristics.
2. Social engineering; those concerned directly with measures to reduce the hazards of victimization, including efforts to increase the chances of offender detection and prosecution.
3. Legal and moral; those concerned with more accurate and just assignments of responsibility, blame, fault, guilt, culpability, mitigation, etc., including those entailing fundamental alterations of systems of law and justice.

A generalized measure of interpersonal violence

If we consider one of the fundamental social purposes that victimization surveys seek to serve (and, indeed, that form much of the demand for crime statistics) an even more fundamental virtue of the methodological orientation proposed here is suggested. Crime statistics have always been looked to as a crucial indicator of the state of a society—as a "moral measure" or global "social indicator." But perhaps a social indicator that shuns premature moral measurement may serve this purpose better than one restricted to crimes. We can illuminate this proposed indicator by considering a social-functional orientation to law.

We will postulate—not at all unconventionally—that law has two functions: (1) to keep the activities of people from hurting other people, individually or collectively and directly or indirectly, and (2) to balance things again where A does harm B, although "getting even" sometimes involves retribution rather than restoration or compensation. The legal institutions are not the sole ones having such functions, since this is precisely what much of social life and many social institutions are all about. Legal institutions, indeed, come into play where other forms of social control and regulation have failed to operate. But where other means of social regulation, formal or informal, fail so that the acts of one may injure another, we look to law to forbid and deter the act and to provide recompense if it occurs.

In some theoretical utopia, behavior would be so regulated that no one would ever harm anyone else. We can use this utopia as an ideal model, in the scientific sense, as the ideal gas or the frictionless machine is used. The statistic proposed here is a measure of deviations from this theoretical ideal. An appropriate means of measurement can be developed readily if we restrict the specific measure with which we will deal to one of physical (bodily) harms involving a direct relationship between a human causal agent and the person injured. There are possibilities of extending the same rationale to property and psychic harms and to more indirect connections between the causal agent—"person responsible"—and the harm. These will not be discussed here because of their many complexities.

Interviewing on injuries, such as that conducted by The National Health Interviewing Survey in the United States, with minor extensions of the questioning to permit the identification and appropriate classifications of all injuries resulting directly from some human agency, would make possible a comprehensive series on social or interpersonal violence in the broadest sense.

In its raw form, this would be a totally amoral measure. Its scope would not be restricted by judgments with regard to fault, guilt, blameworthiness, legality, legitimacy, willfulness, negligence, intent, sanity, or competence, etc., of the parties to the event. Its causality principle would be simply that of the connection of the activities of others to the injury of the person affected. Thus, that large class in the present series on injuries contributed by motor vehicle injuries would be a major component of the proposed one. So would the torn leg of the third baseman spiked by the sliding base runner; the split head of the felon clubbed by the policeman or the pedestrian mugged by the felon; the dinner guest's lap into which the hostess spilled the scalding soup; and the injuries resulting from an extravagantly ardent embrace.

The illustrations we have given have been selected because they represent a certain simplicity in identifying them as cases of interpersonal violence. We will discuss some instances that will present greater definitional and operational complexities. But first, the illustrations of our concept and procedure by these simpler cases can be noted. Each of them represents a relationship of A+B; e.g., A's spikes cut B's shins; A's club breaks B's head, etc. The directionality is not necessarily identical to cause in a moral sense. Our spilled soup case might have involved a situation in which all concerned viewed the accident as the guest's fault, not the hostess's—the first bumped the latter's arm. Despite this, a careful observer might have noted the hostess's lack of prudence as contributory. We also have not asked about intention— did the base runner try to hurt the third baseman? Nor have we dealt with the legitimacy of the act or its motive—was an embrace an act of love or lust; the victim a wife, lover, or stranger; the act love or rape?

Crime statistics must omit vast gray areas because of ambiguities of decision with regard to such matters. We are proposing a measurement that would fill a gap between crime statistics—that restrict possibilities of valuable inference about social events by asking extremely difficult questions about quintessentially human and social aspects of phenomena—and published health statistics, that ask nothing at all about them. Considered purely from the standpoint of medical etiology, the latter are deficient, because without information on the human agency in accidents, little is known about them. The detailed *International Classification of Diseases* uses categories of the causes of injuries that recognize this fact.

Our interest in the proposed data is that of a social indicator rather than a medical one, however. From this standpoint, it would seem useful to identify those conditions of life in which the acts of parties cause physical injury to others. Many of the strictures of formal and informal social controls—of law, custom, etiquette, and "decency"— attempt to constrain behavior on the basis of presumptions about the hazards that acts pose to others. The ultimate logic of the indicator we are proposing is that of a measure of departures from the Golden Rule (within the restricted sphere of bodily injury).

For the diagnostic uses of social indicators, there would appear to be advantages to a source of data that separates the relatively objective and consensual aspects of physical misfortunes from those that involve variable judgmental elements. When injuries are divided into those arising from crimes and those arising from accidents, a variety of presumptions are made with regard to cause or responsibility and, hence, remedy. The movement toward "no-fault" automobile insurance manifests a shift in social practice analogous in its fundamental perception to that we are proposing here for a social statistic —namely, that causes are always multiple, often obscure, and particularly so when they involve elements of moral and legal judgment. The highway safety field provides another illustration through the efforts of professionals in the area to substitute the term "crashes" for "accidents" in general usage to avoid the etiologic presumptions suggested by the word "accident." Social efficiency can be served by independent responses to objective measures of harms and risks, leaving matters of motive, prudence, intention, and legitimacy to separate agencies.

The measurements proposed here could valuably be related to other data (both external and simultaneously collected) by which the relationship injuries to various mechanisms of social control could be examined. In that our original data will be based on self-reports, we can determine the victim's perceptions of whether the event was "pure accident" or due to an unnecessary or unwarranted failure of some form of social control—those demanding prudence, morality, adherence to law, etc. Cases falling in the latter category—where there is reliance on some behavior-regulating system and where it fails to function effectively—can yield indicators of the effectiveness of social controls.

From aggregate analysis of the injury data being proposed, suggestions may emerge regarding "needs" for controls, other than those seen as applicable by the persons affected. These may relate either to events that fall in the class those affected define as "pure accidents" or classes in which various "failures" of a control system are seen by victims as causal, but where a more general and readily controllable agency can be found operative by analysis of aggregate data.

Social perception of the magnitude of hazard itself is one of the determinants by which collective decisions are reached regarding the need to bring a behavior or a situational class within the realm of formal or informal social regulation or social insurance and compensation. In order to establish sound actuarial knowledge of the magnitude of hazards various types of social situations present, the data employed should be phenomenologically comprehensive and phenomenologically analyzable.

The collection of the proposed data on interpersonal violence would involve problems of theoretical and operational definition of greater complexity and arbitrariness than the illustrations given above might suggest. From the standpoint of a ready extension of crime data, the illustrations we have chosen serve well to suggest a distinction regarding the directness and immediacy of the relation of act to injury—acts such as pushing (ICD E887), striking, cutting, shooting, or stabbing (ICD E960,966). For uses with the broader theoretical relevance we have suggested in the discussion above, however, the operational definitions of human agency could be extended. Examples of such extension would be injuries suffered by hotel guests from a fire caused by one of their number's smoking in bed. Once we admit fires, however, the problems of unknown or ambiguously known origins appear, as well as those of assigning multiple causes as between, say, human and technological agencies. For example, do we attribute cause of a fire to a leaking gas valve or to someone who should have insured that it was not leaking?

A generalized version of the model on which the foregoing approach is based would encompass all forms of social harm. Measures of departures from a theoretical ideal social control system in which no activities of any person harm any other person might serve as social indicators of the effectiveness of social control and compensation.

We will not at this time go beyond indicating the existence of many such problems in the theoretical and operational implement of the basic suggestion being advanced here, with only the additional remark that their solution depends upon decisions regarding the uses one ultimately wishes to make of the statistics.

Measuring crime rates and opportunities for crime*

by Richard F. Sparks

A statistic very commonly used by criminologists when describing or attempting to explain criminal behavior is the crime rate—e.g., the number of crimes known to the police per 100,000 of the resident population. In recent years, with the development of victimization surveying, similar use has been made of victimization rate—e.g., the number of victimizations reported per 1,000 persons, households, or commercial establishments. It will be argued here that rates such as these need very careful interpretation, and that for many purposes they may be extremely misleading. In particular, it will be argued that in the calculation of such rates it is generally desirable to take account of opportunities for committing the illegal acts in question.

Purposes for which crime rates are calculated

Before considering some statistical and conceptual properties of crime and/or victimization rates, it is necessary to review briefly the reasons why these statistics are used as measures of criminal behavior or its consequences, and more generally the reasons why it has been thought important to measure crime or victimization in particular times or places. Such measurement appears to have had four main objectives:

(1) Historically, the first purpose for collecting statistics on crime and criminals appears to have been the measurement of the "moral health" of nations, cities, etc. The name given by the earliest demographers and statisticians to their measures of crime—Moralstatistik, statistique morale—gives a sufficient indication of this objective; if the numbers of crimes or criminals increased, then in some sense the moral "health" of the nation would be growing worse. Something of this concern appears to linger on in popular interpretations of crime statistics: a rising crime rate is seen as an indication of increased depravity or decreased probity, or as a sign of a usually ill-defined "social pathology." Somewhat similarly, Taylor, Walton, and Young have argued that from a radical perspective crime statistics can be used as an "examination of the extent of compliance in industrial society (in quite the same way . . . as it is possible to use statistics on strikes as an index of dissensus in direct class relations at the workplace)."

*Excerpted from a paper presented at the Annual Meeting of the American Society of Criminology, Atlanta, Georgia, November 1977.

(2) A second purpose for measuring crime has been the evaluation of the effectiveness of the machinery of social control. Jeremy Bentham was one of the first to urge that accurate measurement of crime was a necessary adjunct for the legislator; he urged the collection of statistics on convictions and prisoners as "a kind of political barometer, by which the effects of every legislative operation relative to the subject may be indicated and made palpable."

(3) A third reason for measuring crime is the estimation of the risk of becoming a victim. This concern is present, though often implicit, in contemporary efforts to develop "social indicators." As victimization surveying has developed over the past decade, the assessment of risk has become increasingly prominent; indeed, it appears to have been one of the main objectives of the National Crime Surveys.

(4) Finally, the measurement of crime has been a necessary preliminary to the development and testing of criminological theories. Typically the testing of such theories has involved comparisons of crime rates in different places or types of place (for example, cities versus suburbs), or over time, or attempts at correlating changes in candidate independent or explanatory variables with changes in crime rates.

The calculation of crime and victimization rates

A simple rate, like a crime or victimization rate, is a function of only two elements: (1) a number of acts, events, situations, etc., that occur in a given place and time period; and (2) a number of persons or other elements present in the same place and time period. Thus a crime rate R_c is typically defined by

$$R_c = \frac{kC}{P}$$

where C is the number of crimes committed, P is the number of persons available to commit crimes, and k is a number chosen either to give a convenient rate or a conventional base, e.g., 100,000 persons. Thus a rate R_c has a natural verbal translation: "For every P persons, C crimes were committed."

A victimization rate R_v is typically defined in a similar fashion, but with two differences. First, the numerator of the right-hand side contains the number of victimizations rather than the number of crimes; depending on the definitions of these two things, and the "counting rules" used for each, they need not be identical. Second, the denominator is typically the number of persons, organizations, etc., capable of being victims. Thus the current National Crime Surveys, for example, compute commercial victimization rates to a base of (nongovernmental) recognizable businesses; no account is taken of the population of persons able to rob or burgle those businesses.

Rates of this kind measure the incidence of crime or victimization, since their numerators contain (essentially) numbers of events. But analogous rates can be constructed that measure the prevalence of crime-committing or being a victim; for such rates the numerator is usually the number of persons (organizations, etc.) who had committed one or more crimes, or been a victim on one or more occasions, in a given time period. Since a single offender may commit more than one offense in a given time period, or a person or organization may be a victim on more than one occasion, incidence and prevalence rates are not necessarily identical. (Compare death rates, where the number of deaths is necessarily identical with the number of persons who die.)*

Crime and victimization rates raise a number of well-known problems of measurement. Typically we are interested in the numbers of crimes that are actually committed; but statistical series like the Uniform Crime Reports of course give only the number of crimes "known to the police." Similarly, victimization surveys aim to measure the number of victimizations that

*In the case of phenomena that have some temporal duration (e.g., diseases) a further distinction is sometimes drawn between "point-prevalence" rates and "period-prevalence" rates. Crime and victimization prevalence rates are of the latter type, i.e., they give the percentage of the population at risk who were criminals or victims within a time period such as a year.

actually occur; what they get instead is the number of victimizations correctly recalled by survey respondents, reported to interviewers, etc. Each rate thus has, in its numerator, a "dark figure" of incidents that are not counted. Similar problems can also occur with the denominators of these rates, e.g., through underenumeration in a census; typically, however, these are much less serious. Having noted these problems of measurement, I shall from now on ignore them; my interest is in the interpretation of crime and/or victimization rates, and not with the accuracy of the counts of incidents or persons that they may involve.

The first of these problems of interpretation is a purely statistical one. It is obvious that a rate defined as in the equation for crime rate given earlier is a kind of average; it is in fact a function of the arithmetic mean number of crimes committed per person. But such an average, taken over the whole of a population, clearly need not represent the experience of any individual or subgroup within that population. A death rate for the whole of a population—sometimes called a "crude" death rate—may conceal considerable variations in the incidence of death in various subgroups of that population. For this reason it is customary to calculate separate rates for subgroups whose experience is known to be different; e.g., age-specific or race-sex-specific rates, or rates associated with different causes of death as well as with different populations. Such rates make possible between-group comparisons; for instance the risk of dying of heart disease at age 15, compared with the risk at age 75. Moreover, if the subgroups used to calculate such "specific" rates are reasonably homogeneous with respect to the phenomenon being measured, the resulting rates will not be very misleading as within-group descriptions of experience or risk. For example, if every white male age 21 on his last birthday had an approximately equal chance of contracting smallpox by his 22nd birthday, an age-specific rate of infection of smallpox would give an accurate measure of risk to each individual, though of course it would not describe any individual's actual experience (since either he catches smallpox or he does not). The same thing is true for

Table 35. Distribution of victimization incidents in three Inner London areas in 1972, and expected numbers based on Poisson distribution (all three areas)

	Area								
Number of incidents	Brixton No.	%	Hackney No.	%	Kensington No.	%	Total No.	%	Expected numbers (λ = 1.07)
None	101	56	104	58	93	51	298	55	187
1	40	22	40	22	40	22	120	22	200
2	13	7	11	6	32	17	56	10	107
3	14	8	18	10	8	4	40	7	38
4	6	3	2	1	3	2	11	2	10
5	2	1	3	2	1	1	6	1	2
6+	6	3	1	1	7	4	14	3	1
Totals	182	100	179	100	184	100	545	100	(545)
Total number of incidents	208		151		223		582		
Average number of incidents	1.14		84		1.21		1.07		

phenomena like crime or victimization, which can involve the same individual more than once in any noninfinitesimal time period. If every member of a given subgroup were to commit or suffer (say) exactly two crimes per year, then the resulting rate would necessarily reflect each individual's experience in that year. Though this is of course unlikely to happen, a crime rate would still not be too misleading, provided that the within-group variance was small, relative to the subgroup mean (i.e., in proportion as the coefficient of variation approached zero). Finally, even though the within-group variance was considerable, the rate might not be too misleading, provided that the distribution was approximately normal (or more generally was symmetrical about its mean).

It seems clear that this is generally not the case, however, either for crimes committed or victimizations experienced. Data from a number of studies to date strongly suggest that the frequency distributions of crime-related events are typically extremely skewed, with the great majority of the population having no crimes of victimizations in a given time period, and at the other extreme a small proportion of the population having a great many. It follows that a crime- or victimization-incidence rate will be an extremely misleading descriptor of the group's experience, or of the risk of crime or victimization.

This point can be illustrated with data taken from a victimization survey which I conducted in three Inner London areas in 1973. Table 35 gives the numbers of respondents reporting 0, 1, 2, . . . incidents of victimization of various types as having happened within the survey reference period (approximately the calendar year 1972), together with sample victimization rates per person. It will be seen that for two of the three areas, and for the sample as a whole, the total victimization rate is in excess of 1.0 per person. A naive interpretation of these rates might suggest that everyone in the sample was a victim at least once in the year, or, alternatively, that the risk of victimization in those areas was about 100 percent, i.e., virtual certainty. Yet, as the table shows, over half of the respondents in each area reported no incidents at all.

Similar findings have emerged from a number of other victimization surveys done in recent years, and the same general picture appears to be emerging from the NCS surveys. In the commercial victimization survey conducted in Houston, Texas, for example, the aggregate rate for robbery and burglary was 1.278 incidents per establishment; yet nearly 60 percent of the businesses surveyed reported no incidents at all as having occurred within the 1-year reference period. In the case of the NCS surveys this problem is especially serious, since in the Law Enforcement Assistance Administration's (LEAA) published reports to date on these surveys, the victimization rate is virtually the only statistic used.

Given a skew distribution of the kind disclosed by table 35, the victimization rate might still have some readily interpretable meaning in terms of victimization experience and/or risk, if the occurrence of multiple victimization were approximately random, i.e., if it more or less conformed to a Poisson distribution with the overall mean as a transition rate. As the right-hand column of table 35 shows, however, this is not the case: the numbers reporting no victimization at all, and the numbers reporting several incidents, are both greater than would be predicted by a Poisson distribution. Compound distributions—based on assumptions of "contagion" or increasing probability of victimization or of heterogeneity of "proneness" to victimization—give a somewhat better fit to data like those of table 35; so does the skew distribution first described by Yule and discussed by Simon, which is based on slightly different assumptions. Using the London survey data, an attempt was made to identify empirically subgroups of the sample with different mean rates of victimization, for whom the distributions of incidents would be adequately described by separate simple Poisson processes. Unfortunately, this attempt was unsuccessful. No set of criteria—based on attributes such as age, sex, race, or social class, either singly or in combination—could be found by which the sample could be subdivided into groups in which the frequency distribution of multiple victimization was no greater than would be expected by chance.

In summary, in the present state of our knowledge, even specific subgroup rates are apt to be extremely misleading as descriptors of the experience of, or the risk of, victimization. A prevalence measure, such as the percentage who are victimized on one or more occasions in a given time period, is somewhat less misleading. But such a measure completely masks the extreme cases of multiple victimization that occur; if this is to be avoided, the full frequency distribution must be presented.

Though the evidence is much less complete, it appears that the committing of crimes is distributed in a similar fashion. Carr-Hill found that convictions for crimes of violence among adult males in England and Wales displayed a distribution not unlike that of table 35: most of the population had no convictions, while a small proportion had many. As with victimization, a crime rate based on such a distribution would be extremely misleading; it would greatly overstate the involvement in crime of the majority, while of course understating the activity of the "crime-prone" minority.

Opportunities and rates

The concept of opportunity is familiar in criminology, chiefly through the work of Merton and Cloward and Ohlin.* Though seldom explicitly referred to, the concept has also played a part in many less elaborate attempts to explain variations in crime rates over time or place. Thus, for example, it has often been noted that there are well-marked seasonal variations in observed patterns of crime, with crimes of violence typically being more common in the summer months and crimes such as burglary being more common in the winter; a common explanation for such findings is that social interaction is greater in the summer, thus providing greater opportunity for interpersonal violence, whereas longer hours of darkness in the winter months provide greater opportunity for undetected entry into others' property.

More recently, a few researchers have explicitly considered the relationship between crime and opportunities for it. Before considering these approaches, however, we need to examine the relationship between an opportunity for committing a crime and the commission of crime itself. It is clear that as a matter of ordinary language, the existence of an opportunity for a crime to be committed is a logically necessary condition of that crime's occurring. That is, if we are prepared to assert that an opportunity to commit a theft at a particular time and place did not exist, then we should normally be compelled to say that no theft did in fact take place. Thus it is a necessary truth, and not merely a very well-confirmed hypothesis, that no motor cars were stolen in the United States (or anywhere else) in the year 1850; that no room air-conditioners were stolen in the year 1900; that no color television sets were stolen in 1930; and that no credit-card frauds were committed in 1940. The opportunities for those crimes simply did not exist in those years.

*Though it is interesting to note that, in general, both Merton and Cloward and Ohlin tended to regard legitimate and illegitimate opportunities as alternatives, and thus as mutually exclusive: either one obtained a legitimate job or he joined the rackets. But—at least in Western industrial societies—the major opportunities for illegitimate gain open to most people involve theft of some kind from their places of employment; thus legitimate and illegitimate opportunity structures are intimately connected.

The proposition that changes in opportunities to commit crime will lead to changes in the numbers of crimes actually committed appears to be a hypothesis—to involve a contingent matter of fact, and not a truth of logic. But the matter is more complicated than that. Certainly it is not necessarily true that if the amount of stealable property increases, the number of thefts will (ceteris paribus) increase. However, a decrease in the quantum of stealable property, social interaction, etc., may of necessity lead to a decrease in thefts, assaults, etc., if it results in some individuals who formerly had opportunities to commit these acts no longer having them. Thus, suppose that in a time period t_1 every member of a population of N persons has some opportunities to steal; a further increase in opportunities in that population may lead to more thefts, or it may not. Suppose at t_2 the number of opportunities for theft is reduced, so that k individuals are completely without opportunities (so that thefts can only be committed by $N - k$ members of the population); all other things being equal, the number of thefts will necessarily decrease. Evidently if we are comparing numbers of thefts committed at t_1 and t_2 we must take account of changes in opportunities between those two periods; and this is so whether t_1 precedes t_2 or follows it.

It follows that for any of the four objectives of measurement mentioned above, opportunities for crime need to be taken into account in calculating crime rates

for comparisons across time or place. Thus, if the crime rate is to be used as an indicator of social morality, probity, violence-proneness, collective wickedness, etc., it is in effect being interpreted as an average tendency in the population to behave in certain illegal ways; but a person's actually behaving in those ways presupposes that he or she has the opportunity to do so. Suppose that we associate with each person in the population a tendency to steal, assault others, etc.; borrowing a bit of economists' jargon, we might speak of a propensity to steal, assault, defraud, etc. Such a propensity can be defined as the conditional probability that an individual will steal, assault, etc., given the opportunity to do so. The evidence discussed earlier suggests that the distribution of this propensity in the population will be skewed rather than normal. The unconditional probability that an individual will steal, *p(T)*, would then be given by the product of this conditional probability or propensity, and the probability $p\,(O)$ of the necessary opportunity:

$$p\,(T)=p\,(T|O)\cdot p\,(O)$$

But $p\,(O) = 0$ by definition, when no opportunities exist; thus $p(T)$ will also necessarily be zero under those conditions.* Thus if the average (or marginal) propensity to steal remains constant in a population between t_1 and t_2, but opportunities for thefts increase, the probability of theft (and probably the numbers of thefts committed) will increase; but that is not an indication that the population is becoming any more dishonest. In somewhat old-fashioned language, we might describe such a situation by saying that the number of temptations had increased (so that some who formerly had no such temptations now had them), not that people were becoming more susceptible to such temptations as existed.

*Note that the unconditional probability $p\,(T)$ can serve as a transition rate in a Poisson process leading to actual thefts. The number of thefts occurring would in this case be a random variable depending in part on "theft-proneness" and in part on chance factors; models such as those of Greenwood and Yule fit this situation. But this propensity or proneness is not identical with the average number of thefts actually committed. This point has been neglected in some recent attempts at modeling criminal behavior. Shinnar refers to the average number of crimes committed (which he designates by λ) as a random variable; but since he deals constantly in the expectation of this variable, i.e., the mean, he often seems to assume that it is the same for every member of the population of "criminals."

Similar considerations apply if the crime rate is used as a measure of the effectiveness of the system of social control, or as a dependent variable in a criminological theory. In each case, what the crime rate is supposed to measure is (approximately) the tendency of the population to behave in certain illegal ways, under specified control arrangements (e.g., a particular set of penalties) or specified social-structural or other conditions (e.g., a given level of unemployment, status integration, or relative deprivation). Plainly, variations in opportunities must be controlled for, if changes in crime rates are to be interpreted correctly: a sharp decrease in opportunities for crime, for example, could be expected to lead to a decrease in criminal behavior independently of any changes in presumed causal factors or the social control system, merely because it was no longer possible for some people to commit crimes that they would otherwise have committed.

Finally, variations in opportunity must be taken into account in assessing the risk of victimization. If, for example, the number of cars in use doubles while the number of car thefts only goes up by 50 percent, then the average risk of a car's being stolen has declined; similarly, if people cease to go out of their houses at night, their risk of being assaulted in the street at night obviously declines.

One of the first researchers to take into account variations in opportunity was Sarah Boggs, in her study of urban crime patterns. Boggs noted that:

> Environmental opportunities for crime vary from neighborhood to neighborhood. Depending on the activities pursued in different sections of the city, the availability of such targets as safes, cash registers, dispensing machines, people, and their possessions varies in amount and kind. These differing environmental opportunities should be reflected in the occurrence rates (of crime).

Boggs noted correctly that a consequence of population-standardized crime rates was the production of "spuriously high" crime rates for central business districts, which contain relatively small resident populations but large amounts of merchandise, parked cars, and so on. Accordingly, she constructed "crime-specific" rates using denominators that could reflect opportunities for the type of crime in question: a business-residential land-use ratio for commercial burglary and robbery, the amount of street space available for parking, for car theft, and so on. Rank correlations between these rates and rates standardized for population only, across 128 census tracts in St. Louis, were very high for highway robbery, residential burglary, rape, homicide, and aggravated assault; but they were low and in some cases negative for crimes against business, e.g., $\tau = -.230$ in the case of nonresidential daytime burglary.

It is important to note, however, that standardization for variations in opportunities is not an alternative to standardization for the size of the population available to commit crimes (as Boggs' analysis suggests). Instead, both the population of potential offenders and the stock of available opportunities should be reflected in the denominator of a crime rate. Thus, an opportunity-standardized rate R_c^* would be defined by

$$R_c^* = \frac{kC}{p\,(O)} = \frac{Rc}{O}$$

where O stands for opportunities for the type of crime in question, and k is a constant chosen to give a convenient rate or base, e.g., numbers of thefts of cars per 1,000 persons per 1,000 cars available to steal. (Note that R_c^* is defined as zero when O is zero, since in that case C is necessarily zero as well.) If we write $R_{c_1}^*$ for the opportunity-standardized rate in t_1, and $R_{c_2}^*$ for the similar rate in t_2, then $R_{c_2}^* < R_{c_1}^*$ if $(C_2/C_1) < (O_2/O_1)$; and if $O_2 = O_1$, then

$$\frac{R_{c_2}^* - R_{c_1}^*}{R_{c_1}^*} = \frac{R_{c_2} - R_{c_1}}{R_{c_1}}$$

—i.e., if opportunities remain unchanged between t_1 and t_2, they can be ignored in calculating changes in the crime rate.

Defining and measuring opportunities for crime

What constitutes an opportunity for the commission of a crime naturally depends on the type of crime, and satisfactory definition and measurement of opportunities can in some cases be very difficult. In the cases of crimes of violence, opportunities are presumably created by contacts or interactions between persons. As is well known, in a population of N persons, the maximum number of different two-person contacts that can occur is $N(N-1)/2$; this was the base used by Boggs in calculating "crime-specific" rates of homicide and aggravated assault. To be even approximately satisfactory, however, such standardization would need to take into account the nature and duration of interactions between persons, e.g., across different types of communities or in given interpersonal relationships. Thus some years ago Svalastoga analyzed a small sample of homicide cases in Denmark; he found, as have most other researchers on homicide, that the majority involved family members and that strangers accounted for only 12 percent of the cases studied. On the basis of a small survey of students, plus some admitted guesswork, Svalastoga estimated that a Danish person might have contacts with relatives, acquaintances, and strangers in the ratios $4 \cdot 10 : 4 \cdot 10^3 : 4 \cdot 10^6$; on this basis, he calculated that the probability of being killed by an acquaintance was some 3,000 times greater than the probability of being killed by a stranger, and that the probability of being killed by a family member was some 600,000 times greater. These "probabilities" assume, of course, that the numbers and types of contacts are on the average the same within each of these three groups, which is improbable to say the least. Nonetheless, the general logic of this approach seems to be correct; for the purpose of explaining the social, spatial, or temporal distribution of violent crime, as well as for assessing the risk of it, some account needs to be taken of the distribution of opportunities (i.e., interactions between persons) that are a logically necessary condition of such victimization.

In the case of crimes against property, the choice of an adequate base for calculating an opportunity-standardized rate can also be problematic. One approach is simply to use the stock of stealable goods. But for other types of theft, the matter is less clear. Thus, Gould in analyzing bank robberies and burglaries used data on amounts of cash and coin in banks. It might be argued, however, that the number of banking offices is a better measure of opportunity than the amounts of cash that are contained in those banking offices. (Data from the *Statistical Abstracts* show that the amount of cash and coin in banks increased about five times in the period studied by Gould; the number of banking offices increased by only about 70 percent.) Similarly, should one use the number of supermarkets and/or department stores as a base for shoplifting, or the value of those stores' inventories? The answer would seem to be that it depends on the purpose for which rates are being calculated. If the objective is the assessment of risk, then the number of institutions (stores, banks, etc.) would usually be more appropriate; if the objective is the explanation of observed patterns of theft, then the stocks of available goods might be preferred.

In the case of thefts from individuals and/or households, the choice of an appropriate base is even more complicated. In some places, estimates are available for the stocks of consumer goods owned by individuals and/or unincorporated businesses. In the United States, data are available from the *Statistical Abstracts*, and from a variety of trade publications such as *Merchandising Week*, on most types of durable consumer goods. Typically, these data are for production, shipment, or sales of such goods, though estimates of stocks can be derived from them if assumptions are made about average life (or average "stealable" life). Where figures for estimated stocks of such goods are available, they almost invariably disclose massive increases over the past three decades, usually far greater than the increases in burglaries and larcenies recorded in the Uniform Crime Reports. Thus, according to estimates based on market research by a television network, only 9 percent of all American households had a television set in 1950; by 1974 the figure was over 94 percent, with over two-thirds of those households having color television and about two-thirds having more than one set. (Similarly, trade sources estimate that the total number of radio sets in use in the United States increased by nearly 3 1/2 times in the period 1950–74).

Within the past few years, there has evidently been an even more rapid increase in ownership of such things as stereo equipment, tape recorders and cassette players, hand-held calculators, and CB radios. The result has been to increase substantially the quantity of personal disposable property available to be stolen, and thus the opportunities for theft. In the case of television sets, for example, the figures just quoted mean that in 1950 a burglar or thief had less than 1 chance in 10 of finding a television set in an American household chosen at random; by 1974 he would have had difficulty in not finding one, and in two houses out of every three could have had a choice of sets (or the chance of a color set) to steal. Over the same period—and for exactly the same reason—the chance of any particular television set's being stolen has almost certainly decreased sharply.

It does not seem worthwhile to try to estimate, with greater precision or in greater detail, changes in opportunities for theft until more data are available on the amounts and types of property that are stolen; unfortunately, very little information on this subject is now routinely collected. Since 1974, data have been collected (though not published) by the FBI in supplementary returns from police forces under the Uniform Crime Reporting program; but these returns do not give detailed information about types of stolen property, since they distinguish only between cash, motor vehicles, and "other" property, and in any case apply only to crimes that have been reported to (and recorded by) the police. The same thing is true, however, of the National Crime Surveys. The Crime Incident Forms (NCS-2 and NCS-4) now being used in these surveys contain questions pertaining to the value of allegedly stolen property; but they too are coded to distinguish only between thefts of cash, motor vehicles and accessories, and "other" property. Given a more detailed breakdown of the types of things that are stolen— either from individuals, households, or various types of business organizations—it would, in principle, be possible to estimate the stocks of property from which those thefts occurred; if this were done, it would be possible to estimate rates of theft relative to opportunities for theft, either cross-sectionally (e.g., among different social classes) or over time.

Opportunities and criminological explanations

Thus far, this paper has been concerned with the effects of variations in opportunities for crime—for example, changes in the stock of personal disposable property, in the case of theft—on the interpretation of the crime rate. It has been argued that, given the purposes for which we commonly measure crime, it is appropriate to standardize crime rates for opportunities. I can see no argument against this kind of standardization, which would not apply with equal force to standardization for changes in the population available to commit crimes.

It remains to be considered, in conclusion, whether opportunity factors can or should figure as separate independent variables in an explanation of criminal behavior or variations in crime rates. Recent papers by Gould and his associates appear to treat changes in the stocks of one type of property—cars—in precisely this way. Thus Gould writes that "the availability of property influences the amount of theft against it," and he refers to this as a "causal sequence." He goes on to speculate that "property crime is not only related to the availability of property, but . . . this relationship is itself structured by the relative scarcity or abundance of the property being stolen." He also notes that changes in patterns of car theft, and in availability of stealable cars, parallel an apparent change in the population of car thieves, who (according to arrest data) are now much more likely to be juvenile or adolescent "joyriders" than they were in the 1930's or 1940's. (Gould also notes that similar changes appear to have taken place among bank robbers, with "professional" robbers having largely been replaced by inept amateurs.)

Gould suggests, then, that (at least as far as car theft is concerned) the period from the early 1930's to the early 1940's was "a period of economic scarcity" in which car theft was mainly an activity of "professional" thieves; and that the years after about 1942 were "marked by abundance," and the emergence of juvenile "non-professional" thieves. Inspection of the (graphed) data that Gould presents, however, suggests a rather different picture. From his graph of car registrations and car thefts, it appears that:

- Car registrations rose in the years 1933–41, though the increase would admittedly be less if increases in population were taken into account.
- Cars were possibly relatively scarce in the years 1941–45—there was a decline of about 16 percent, from about 35 million, to about 30 million.
- After 1945, the stock of cars registered rose steadily but the numbers of car thefts fell, until about 1950.
- After 1950, the increase in car thefts roughly paralleled the increase in cars registered.

It is evident that none of these changes in stocks of stealable cars and in car thefts in any way necessitates the shift that appears to have taken place, from "professional" to "amateur" thieves; this change is quite independent. Moreover, the relationship between cash and coin in banks and numbers of bank robberies is "somewhat different"; to the extent that there has been a somewhat similar shift in the population of robbers, this is not paralleled by changes in the amounts of cash and coin available to steal. Using the concepts outlined earlier in this paper, we could in fact describe the situation relating to car theft in the following ways:

- In the years 1933–41, the number of cars registered rose fairly steadily, while the numbers of thefts of cars declined; the rate R_c^* of car thefts standardized for opportunities fell very sharply. Quite probably this could have been because, though it was successively easier to steal a car (there were more of them), it was also easier to obtain one legitimately. In the years 1941–45, when no new cars were manufactured and the stock of stealable cars declined, the numbers of thefts (and the theft rate R_c^* rose; because cars became relatively scarce, the opportunities for obtaining them legitimately also decreased.
- In the years 1945–50, the numbers of cars (and thus of opportunities for car theft) rose again; car thefts fell, so the car theft rate R_c^* fell even more sharply; again this could have been because cars were easier to obtain legitimately.
- Finally, in the years 1951–65, the numbers of cars (and of opportunities for car theft) rose steadily; so did the numbers of thefts, so that the car theft rates R_c^* remained about unchanged.

These temporal patterns are evidently compatible with many different combinations of professional and amateur car thieves, and different participation rates of each.

In a later paper, Mansfield, Gould, and Namenwirth expand on Gould's earlier work (and incidentally standardize both car ownership and car theft for changes in population, as Gould has not). They attempt, using data from four countries, to test a model according to which thefts of cars (and, by implication, other stealable property) are determined by the interaction of "professional" and "amateur" demand for stolen vehicles, and the supply of available vehicles. The authors admit that they are only able to carry out a partial test of their model; in particular, they have no data on the relative magnitudes of "professional" and "amateur" demand for stolen vehicles, nor on the shapes of the respective demand curves. Their paper illustrates two ways in which an opportunity factor—e.g., the supply of stealable goods—may be incorporated into an explanatory theory. But it is important to note that it is not opportunities as such—in the sense described in this paper—that figure in such a theory. Instead, it is the relative scarcity or abundance of goods, which may affect participation in theft in, broadly speaking, one of two ways: (1) by affecting motivation to steal, e.g., by making it easier to obtain goods legitimately or conversely by increasing relative deprivation; and (2) by leading to changes in social control measures (in the broadest sense of that term, including measures for the protection of stealable property). Mansfield, Gould, and Namenwirth do mention both of these, but they make no attempt to operationalize or measure either one. The first of these kinds of explanation would seem to require (for example) some evidence about the distribution of stealable property, as well as the quantity of it; it might also require consideration of the value of that property since (under certain conditions) an increase in supply relative to demand may lead to a decrease in price. Thus, for example, if cars are available, opportunities for car theft will (ceteris paribus) increase; but it should also be easier to obtain cars legitimately. In this case we should expect a decrease in R_c^*, such as that which occurred in the years 1933–41 and 1945–50. The second line of explanation might involve showing, for example, that when property is relatively abundant, people are less likely to protect it against theft or to report thefts to the police. But an increase in the stock of goods—and thus in opportunities to acquire those goods legitimately—can occur together with an increase in protective measures; an example would be a law requiring steering-column locks to be fitted to all cars. In this case this number of thefts would be expected to fall, for two distinct reasons: first because demand for stolen cars fell, and secondly because of a decrease in opportunities.

One case in which it may be useful to treat changes in opportunities independently is the case in which they function as an intervening variable, helping to explain an observed relationship between crime or victimization and some other variable. Thus, several victimization surveys have found a negative association between age and victimization, especially for violent crimes such as assault. A possible explanation for this finding is that older people tend to go out less often, especially at night; they are thus less at risk of (certain sorts of) victimization. (What is oppportunity from the offender's point of view, of course, is risk from the victim's.) If this factor is taken into account, the zero-order association between age and victimization may disappear, or at least be reduced. In our London victimization survey, for example, the zero-order Gamma between age and victimization was −.42; between age and the number of nights per week the respondent went out, Gamma = −.18, between nights out and victimization, Gamma = +.20. The partial Gamma between age and victimization, controlling for nights out, was reduced to −.29. A reasonable interpretation of these findings is that a part of the older respondents' lower victimization rate was due simply to the fact that they were less often at risk. A similar analysis is possible for some of the National Crime Survey data, since the NCS-6 "attitude" questionnaire administered to half of the respondents in the city-level surveys contains a question (Q.8a) asking "How often do you go out in the evening for entertainment?"; so far as I know, however, these data have not been analyzed from this point of view.

There seem, therefore, to be some instances in which variations in opportunities for crime may help to explain variations in crime rates. In general, however, it seems to me that such explanations are likely to be relatively simple and uninteresting, in the great majority of cases. It is seldom useful to point out that cars could not be stolen before cars were invented; and it is not, in general, illuminating to point out that a person who never goes out of his or her house will never get assaulted or robbed in the street. Though relatively trivial in themselves, variations in opportunity may nonetheless obscure the effects of more important theoretical variables. To avoid this, the procedure of standardizing for opportunities, as outlined in this paper, seems to me appropriate. This procedure is in fact analogous to the "method of residues" proposed some years ago by Coleman. Coleman noted that a great deal of effort had gone into finding a "law of social gravity" to the effect that (say) the amount of travel between two cities is directly proportional to the product of their populations and inversely proportional to the distance between them. Such a "law" has the unfortunate defect that it does not fit available data on intercity travel. It has the even more serious defect that, where it does fit, it is uninteresting. By standardizing for populations and distance, Coleman suggested, one could calculate for any pair of cities a "residue" that is the difference between observed intercity travel and the amount expected on the basis of population and distance alone. Examination of these residues might then reveal more interesting effects that would otherwise be obscured.

The same approach is useful, it seems to me, in relation to crime and victimization. The number of homicides in New York is greater than the number in (say) New Orleans; but the homicide rate standardized for population is higher in New Orleans. The number of car thefts in the United States in 1965 was greater than the number of car thefts in 1945; but the theft rate standardized for population and the stock of stealable cars was smaller. Only by removing the sociologically trivial effects of population and opportunities can more interesting and important effects be seen.

Deciding what and whom to count*

by Stephen E. Fienberg

Criminal incidents are events or social encounters involving one or more offenders and one or more victims, in one or more locations for specified periods of time. The duration of a single criminal incident may be 10 minutes, an hour, a day, a week, or even a month. Nonetheless, when put into a larger timeframe, a criminal event is quite profitably viewed as the realization of a point process distributed over time and space. What complicates the modeling of a large number of crimes is the interpenetrating social networks linking offenders and victims, both within a single incident and across several incidents, and giving rise to multiple offending and multiple victimization. Reiss has described some of the impact of such networks and associated group structures on crime rates with special attention to the implications for measuring the effects of deterrence and incapacitation. The stochastic structure of criminal social networks and the resulting lack of independence of criminal incidents also have potentially important implications for both the design and analysis of victimization surveys.

How one records crime is a function of one's perspective. A single criminal incident or social encounter can involve one or more offenders, one or more victims or possibly no victims at all, and multiple violations of the law leading to multiple indictments of a single offender or several offenders who have participated in the event. There may even be mutual offending and victimization, e.g., in cases of assault. Thus a particular configuration of crimes aggregated over a given time period may well look dramatically different when viewed from the perspective of offense rates as opposed to victimization rates, and neither set of rates is likely to reveal the true nature of the criminal events that have taken place.

*Excerpted with minor editorial changes from "Victimization and the National Crime Survey: Problems of Design and Analysis," a paper presented at the Second Survey Sampling Symposium at the University of North Carolina, April 1977.

A single hypothetical example can illustrate the complexity associated with criminal incidents and the manner in which they are recorded. A young couple living in the household of the woman's parents in Stamford, Connecticut, go to New York City on December 31 to celebrate New Year's Eve. They park their car in a lot on the east side of Manhattan and have a leisurely dinner at a nearby restaurant. After dinner when they return to their car, they are accosted by five young males just outside the parking lot and are taken into an adjacent alleyway, at approximately 11 p.m. One of the youths threatens the couple with a revolver, and the other four take turns raping the woman. When the woman resists, one of the youths assaults her with a knife, and then he also assaults the man. Following the acts of rape, the youths take the woman's purse and the man's wallet, and they appear to flee. It is now about 1 a.m., January 1. The couple have to travel several blocks to report the incident to the police. When they finally return to the parking lot with a police officer at 3 a.m., they discover that their automobile is missing. A week later three young males are stopped by the police in Newark, New Jersey, driving the couple's car through a red stoplight and the males are arrested.

The incident just described involved five offenders, two victims, three arrests, and numerous offenses including forcible rape, robbery, aggravated assault, and motor vehicle theft. It spanned several hours (and 2 calendar years!) and took place in at least two locations. How would it be classified by various recording systems?

Let us begin with the police record of the event as it is transmitted to the FBI for use in its Uniform Crime Reports (UCR). In a multiple-offense situation, the police classify each offense, and then locate the offense that is highest on the list of what is known as Part I Offenses (the ranking is criminal homicide, forcible rape, robbery, aggravated assault, burglary, larcency-theft, and motor vehicle theft). The highest offense is entered and the others are ignored. Multiple offenses need to be separated in time and place to lead to multiple entries in the UCR. The exception to this rule involves crimes against the person (criminal homicide, forcible rape, and aggravated assault) where one offense is entered for each victim. Thus the UCR record will contain one offense of forcible rape (against the woman) and one offense of aggravated assault (against the man). Had the youths only robbed but not assaulted the man, there would only be one offense entered. These offenses would be recorded by the New York City police, and I am unclear as to which date (and thus which year) they will be attributed to. The UCR record will also show that the offense(s) have been cleared (i.e., "resolved") by the arrest of the three youths in New Jersey. Although this event led to one or two UCR offenses, it might well lead to the prosecution of the five youths on up to a total of 5 counts of rape, 10 counts of aggravated assault and robbery, and 5 counts of motor vehicle theft.

Suppose now that the couple's household is chosen as part of the NCS so that the event will also be recorded from the victim's perspective. Both the man and woman would be interviewed separately and the NCS would record two victimizations in December: one for the woman "assaultive violence with theft—rape," one for the man "assaultive violence with theft—serious assault with weapon." Even if the man had only been robbed but not assaulted, there would still be two victimizations recorded (as compared with a single offense). Moreover, because of the separation of household victimizations from individual victimizations, when the woman's father reports the household victimizations, he may well report the theft of the car separately, and the month of victimization may be given as January, and thus it could go into a separate calendar year.

In summary, our single criminal incident involving five offenders and two victims leads to one or two offenses recorded in New York and two or three victimizations recorded in Connecticut. The perspectives are clearly different, and so too are the records of the event.

Because a large proportion of criminal incidents are never reported to the police, the discrepancy between all criminal offenses and those reported to the police has been described by Biderman and Reiss as the "dark figure" of crime, and one of the original purposes of victimization surveys was "to bring more of the dark figure to statistical light." Biderman and Reiss go on to note:

> In exploring the dark figure of crime, the primary question is not how much of it becomes revealed but rather what will be the selective properties of any particular innovation for its illumination. As in many other problems of scientific observation, the use of approaches and apparatuses with different properties of error has been a means of approaching truer approximations of phenomena that are difficult to measure.

Any set of crime statistics, including those of the survey, involves some evaluative, institutional processing of people's reports. Concepts, definitions, quantitative models, and theories must be adjusted to the fact that the data are not some objectively observable universe of "criminal acts," but rather those events defined, captured, and processed as such by some institutional mechanism.

Much controversy has centered on the comparability of police statistics on offense rates and NCS survey statistics on victimization rates, but the utility (or lack thereof) of the NCS data for such comparisons should not obscure the richness of information about victimization available in the NCS. It is for this reason that the NCS data must be collected and organized in a manner that will make it amenable to standard forms of statistical analysis. Otherwise the rich veins of information on such topics as high-risk segments of the population and multiple victimization, or the way that deviance is perceived and dealt with in various social contexts, may never be mined.

Chapter 4

Uses of the crime survey

Introduction

The selections in this chapter discuss the uses of victimization data. The first contribution summarizes a longer report to the Law Enforcement Assistance Administration (LEAA) prepared by the Research Triangle Institute (RTI) on the application of victimization surveys to scientific and policy problems. The report identifies a number of potential National Crime Survey (NCS) "user communities," and reviews the results of interviews with representatives of these groups regarding the utilization and potential utility of the surveys. The RTI report identifies six major uses for victimization data: policy analysis, scientific research, social indicators, planning and management, evaluation, and teaching. The report indicates that in most of these areas knowledge of potential applications for the data and the actual use of victimization materials have been growing. Some users have employed the results of the city and national NCS surveys, while others have adopted victimization survey methods to gather data pertinent to local needs.

The selection by Fred Shenk and William McInerney reviews some research applications of victimization data and discusses at length some important limits on the potential uses of the NCS. Data from that survey have been used extensively to describe the distribution of victimization in various population groups, and to examine the detailed characteristics of incidents. The authors identify several difficulties with the data that limit their utility. First, the infrequent nature of crime means that there are few reports of victimization within specific population subgroups, making it difficult to do a detailed analysis of victims of most types of offenses. Second, people have difficulty reconstructing frequent and interrelated events in survey interviews, and a large number of crimes are lost from sight because they are hard to count individually. Third, the nature of the NCS samples and the Census Bureau's rules concerning the confidentiality of data conspire to limit the availability of reliable victimization data for small geographical areas. Finally, the data on characteristics of respondents' neighborhoods that have been released on many of the Census Bureau's public use tapes are seriously flawed.

The final contribution in this section reviews the hazards of attempting to compare crime data gathered in the NCS and the Uniform Crime Reporting (UCR) System. There are many reasons why the two are not comparable, some definitional, some methodological, and some arising from differences in the scope or coverage of the two sets of data.

Analysis of the utility and benefits of the National Crime Survey*

by PHILIP S. MCMULLAN, JR., JAMES J. COLLINS, JR., ROBERT GANDOSSY, JOAN GUTMANN LENSKI

This report contains the results of a study to determine the present and potential utility and benefits of surveys of the victims of crime in the United States. The study is especially concerned with the National Crime Survey and its potential for contributing to public and private criminal justice decisionmaking.

A chronological perspective on the program reveals the gradual development of a very large and complex national data series. The NCS program began in 1970 with prestudies in a few cities, but no data from the NCS were available to users until mid-1974. Evidence accumulated in this study shows that there were few uses of these data or of knowledge derived from them until 1976. Substantial increases in both frequency of use and analytical depth of use occurred in 1977 and are projected for 1978.

Victimization survey results are used most often in academic research supported directly or indirectly by LEAA, but significant uses in policy research are also documented. Knowledgeable victimization data users are found in congressional subcommittees, Federal executive offices, national associations, research and service firms, State legislative and planning offices, local criminal justice agencies, and academic institutions.

After examining the history of the NCS program and case studies of past uses, it is hypothesized that the program will experience a continued rapid growth in use for a number of years. This hypothesis is examined for each of the significant user communities. From this examination, it is concluded that the potential benefits of the program to public and private decisions are substantial enough to recommend continuation of the NCS and to support improvement in both the survey methodology and the system of knowledge dissemination.

Evidence for the study was obtained through several methods, including personal interviews with 45 legislative and executive branch staff members and administrators. Evidence was also obtained in personal visits to the offices of 17 potential users in associations, foundations, and research institutes in the Washington, D.C. area. Telephone conversations were held with 47 NCS-using researchers, and visits were made to interview five others who were more directly involved in NCS methodology and scientific analysis. Also, telephone calls were made to 40 State and local agencies and 9 miscellaneous groups that were thought to be current or potential users. Finally, a review of a more than 250-item bibliography provided additional evidence for the analysis. The following sections summarize the findings from the collected and analyzed evidence.

Table 36. Type of use classifications

Type	Definition
Scientific research	Use in research such as that involving tests of (criminological) theories of deterrence, changes in the type of crime over time, societal reaction to fear of crime, and the relationship of crime to the social structure and economic conditions. Most of social science research use falls in this general category.
Policy research	Use in applied research specifically designed to assist in a policy decision rather than just to advance scientific knowledge. Usually performed by legislative or executive staff, consulting agency, or policy research institute. Studies to predict the effects of policies are considered to be policy research studies rather than planning or evaluation.
Social indicator	Use of data for their characteristics as quantifiable measures reflecting the magnitude or extent of social change. As specifically related to victimization, the measures might be rates, quantities, change rates, trends, or risk levels. Analysis to prepare social indicators is included, but scientific research that may produce better indicators is not.
Planning and administration	Use in the selection and administering of appropriate steps to carry out the policies set by the decisionmakers.
Evaluation	Evaluation measures the efficiency, effectiveness, or efficacy of the implemented plans.
Teaching	Use in a classroom exercise is similar to social-indicator use, but this distinction is useful in assessing academic uses.

Applications of NCS data: type of use

In the report by the National Research Council, *Surveying Crime*, the need for a continuing series of victimization surveys is discussed under three headings: (1) victimization survey as a social indicator, (2) executive and legislative uses of victimization surveys, and (3) scientific utility of victimization surveys. Others have proposed that victimization data should have utility for planning and administration and for evaluation of programs and projects. This study has accepted and modified the type of use categories of *Surveying Crime* to provide continuity between the study, and additional categories have been added for the proposed uses not covered in that National Academy of Science (NAS) report. The categories are defined in table 36.

The scientific research and social indicator uses are defined in table 36 in much the same way that they are described in *Surveying Crime*. Executive and legislative use is not a unique type of use in the table. A legislative or executive use may involve nonspecific review of NCS tables and graphs to find situations that may need public policy attention. This will be classed as a social indicator use.

Legislators and executives may also direct staff or consultant attention to specific issues that involve victimization data use. These will be classed as policy research uses. The executive administrator may need statistics for planning to implement policy and for administering the resulting plan. If NCS data were used in such activities, the type class would be planning and administration. Finally, NCS data have been proposed for use in evaluating both the national impact of policy and in a single evaluation-type class.

*Excerpted with editorial revisions from a grant report by the Research Triangle Institute for the National Criminal Justice Information and Statistics Service, LEAA, 1978.

Applications of NCS data by user communities

The Congress

The interviews with congressional counsel and staff disclosed a relatively limited use of NCS data but generally strong support for their potential. Eight of the 10 House and Senate committees interviewed had made some use of NCS data. The Senate Judiciary interviews disclosed no more than routine use of NCS, but there was fair-to-strong NCS support. The upcoming Senate debate on victim compensation is a specific potential use reported, and several other potential uses were less specifically described in the interviews. However, the availability of a reliable social indication of crime was reported to be the most important reason for NCS continuation.

In the subcommittees of the House Judiciary, the persons interviewed reported specific experiences in the use of NCS data or publications. Their experiences are related to victim compensation, gun control, crime and unemployment, and general social indicator use.

In order to interpret the evidence of congressional use of NCS, it is necessary to examine the process by which Congress gathers evidence and the extent to which there is a capability to use NCS. As explained to RTI by those interviewed, the usual process is an advocacy proceeding in which each side gathers as much evidence as possible with which to advocate its position. Evidence is gathered primarily by lawyers with the assistance of consultants and literature researchers. If quantitative crime analyses are needed, the research brokers on the committee staff attempt to obtain crime analyses from the FBI or from LEAA. If the required analyses cannot be obtained from the Department of Justice, experts in the field in question will be called to consult and possibly to testify.

According to academic researchers, informal networks of researchers and congressional staff members may facilitate the flow of information and opinion from research to legislative policy. The researchers in the informal network are asked to testify when their research helps the advocated position of the committee staff. However, in order to avoid an untenable position, the staff research broker will try to determine the evidence against the advocated position.

In the process described above, congressional staff members seldom have the time or the inclination to perform in-depth quantitative analyses. The staff research broker tries to find completed studies about the subject from which pertinent evidence can be extracted. Executive branch agencies such as LEAA may be called upon for help, but these agencies seldom have policy research analysts available to assist. This usually leaves the congressional staff with the options of settling for aggregate data from reports such as the NCS publications or of depending upon the testimony of favored academic researchers. These are the options that have been available to Congress for NCS uses, and this helps to explain the limited type, level, and frequency of congressional uses.

At present, only the staffs of the House Committee on Crime and the House Select Committee on Aging have gone beyond routine use or simple interpretations of NCS publications. Only these two have obtained sufficient experience with NCS to understand its limitations and to express constructive criticism and specific needs. Other committee staffs express strong support for NCS because of a general concern that Congress too often legislates with inadequate information. Several Senate committees anticipate analytical assistance that is not likely to be forthcoming from the NCS program, as presently organized, or expect the NCS to serve functions for which it may be inappropriate, such as evaluating the national impact of juvenile legislation or victim compensation. All congressional staff respondents agree on the need for a reliable social indicator of crime to avoid total dependence on the Uniform Crime Reports.

It is difficult to determine whether victimization data can have a more significant general impact without specific indicators of trends, risks, and economic costs. It is also not rational to forecast more widespread policy research use with the limited policy research capabilities available to congressional committees. Without an increase in the general analytical capabilities available to congressional committees, NCS congressional utility may increase moderately through informal communication networks now operating. However, the data limitations of current NCS publications can frustrate potential users and may have a negative effect on NCS support. If there were better products and an improved analytical support system between Census Bureau data collectors and congressional research brokers, there should be accelerated use, greater utility, and benefits through more rational legislative decisions.

The executive branch

LEAA. Few of the persons interviewed at LEAA were performing functions that called for the analysis of detailed victimization data. However, each had a concern for information that might be derived from the NCS by others.

The seven LEAA offices are routine users of NCS publications, reading new reports to observe any trends that may signal a change in national crime patterns. The data are sometimes extracted for use in public statements, and several interpretive uses by LEAA personnel were found in congressional hearings on crime and the elderly, and juvenile justice and deliquency. LEAA also receives feedback from Congress on the need for additional information. Specific requests were made by the Senate Special Committee on Aging and the House Select Committee on Aging. Detailed discussion of the need to retain the survey and to modify its methodology were recorded by the Subcommittee on Crime. Other committees are anticipating LEAA assistance in using NCS for victim compensation legislation and juvenile crime analyses.

LEAA obtains indirect utility from NCS through its funding of research and planning. Not all efforts at NCS use to date have been beneficial, but there are some successes that hold promise for greater future program use. In addition to its support of the NCS program, LEAA funds a number of local and State victim surveys through block grants or research programs. The State survey results appear to provide little useful feedback to LEAA, but the local survey results are beginning to have program relevance. Victim-witness assistance in Tucson, antiburglary in Portland and Seattle, police performance in Cincinnati and San Diego, and elderly protection in Chicago are examples of local evaluation efforts that make use of both local and NCS victim data. All may someday influence LEAA programming as the Seattle Community Crime Prevention Program (CCPP) has done by becoming an exemplary project.

The persons interviewed at LEAA range from fair to strong in their extent of support of NCS. Much more had been expected of the NCS, particularly from the 26 surveyed cities. The national survey has been frustrating because of its lack of timeliness, and the NCS evaluation has led some to question the survey's validity as a social indicator. Others contend that scientific research using NCS has not yet provided output that has programmatic implications for LEAA. Despite these past and current frustrations, most of the respondents expect increased use of NCS in their programs when the methodological and procedural problems are resolved.

Department of Justice. Some of those interviewed in the Department of Justice had extensive experience with or an above-average understanding of NCS. They are strong supporters of NCS and are concerned with the policy research needs of the Office for the Improvement in the Administration of Justice (OIAJ). The NCS is supported for its long-range value as a social indicator and its more immediate utility for current policy studies. Victim compensation and gun control are issues already addressed, and policy studies using NCS data on burglary and robbery are underway. The OIAJ is assisted by grantees from policy research institutes such as those at Stanford, Yale, and Duke universities.

The other persons interviewed in the Department of Justice have an interest in the utility of NCS because of either budgetary interests or general interest in reliable crime statistics. Extensive direct use of NCS outside of OIAJ does not seem likely since all persons to whom RTI was referred were interviewed, and none were significant potential users.

Other Federal government agencies. The respondents from the U.S. Bureau of Census, Department of Commerce, were selected because of their past or present participation in the NCS program. They provide historical information, referrals to possible users, and opinions about the potential utility of NCS. They have used NCS data in preparing NCS and professional publications.

The remaining 8 offices and 11 persons in Federal agencies were varied in their interest and level of understanding about NCS. The Bureau of Domestic Business, Department of Commerce, provided respondents who have used commercial survey data from NCS. They have found the data to be limited but helpful in their program on crime in business. Several of their publications have made interpretive use of the data. They are not particularly concerned that the commercial NCS survey was terminated because they have not fully analyzed the data already collected. If it were to be restarted, they would like to suggest additions to the crimes now covered.

Strong support for the NCS program was found in the Administrative Office of the Courts, but this support is for more reliable crime statistics in general rather than because of a specific need of this office. In the Office of Management and Budget there is an interest in reliable crime statistics such as NCS might provide, but there are reservations about NCS validity and utility. The questions about validity are the result of the NAS evaluation. The reservations about utility refer to present NCS products and the difficulty of using them because they are either much too aggregated or much too detailed for policy applications.

The Administration on Aging was not familiar with NCS but was aware that some statistical program had shown crime against the elderly to be less than previously believed. The National Institute on Drug Abuse was familiar with NCS publications and had been in contact with LEAA about future NCS data needs. The Alcohol, Firearms, and Tobacco Office was unaware of the NCS data on weapons used in crime and planned to inquire further about them.

Most of the analytical uses of NCS as a social indicator by these executive branch agencies are publications by LEAA of the results of national or city victimization surveys. The other social-indicator uses are annual social indicator publications of the Census Bureau and a special Census Bureau publication concerning generally accepted myths about crime that are refuted by NCS findings.

The scientific research uses are concerned with the methodology of NCS. The policy research uses vary in subject matter and level of use from routine use in testimony to creative use in policy research for the NCS program itself. The interpretive and analytical uses in policy research involve aging, commercial crime, robbery, guns, victim compensation, juveniles, and statistical policy. However, these uses do not signify a widespread familiarity and acceptance of NCS in Department of Justice and other executive department agencies. The analytical uses are all by the senior economic adviser to the department or his consultants. The interpretive policy research uses are concentrated in aging and commercial crime issues. No examples of NCS use in planning and administration or in evaluation were uncovered in this user group.

Associations and research-service organizations

All of the persons interviewed in this user group were referred from legislative or executive interviews. This user group includes the associations of criminal justice professionals and local officials concerned with the criminal justice system. It also includes several Washington-based organizations that assist executive and legislative agencies in their use of crime statistics.

The overall frequency or use among Washington, D.C., associations and research service organizations is not high, but the persons interviewed were generally supportive of the continuation of the NCS program. They assume that the methodology will be changed as needed, and several hope for more attention to explaining crime in metropolitan areas. Very few of those interviewed are potential users of the NCS knowledge in more than interpretive levels of use, but such uses should increase. Past use of the data has been relatively light, but the interest in future use is somewhat stronger and support for the program is generally good. Because these associations do not maintain analytical staffs, the prospects for in-depth analyses are poor. However, the continuing and growing use of NCS knowledge in interpretive studies is expected.

Municipalities

The earliest of the cities to use the NCS data for more than routine review were the two interviewed Impact Cities, Atlanta and Denver. Beginning in 1972 with LEAA support, each Impact City except Baltimore organized a crime analysis team (CAT) to provide analysis for the annual plans of the local criminal justice planning agencies. In 1974, each of seven Impact Cities prepared special reports on victimization in their city and submitted them to the Criminal Justice Research Center (CJRC) for review and incorporation into an overall victimization report for the Impact Cities.

The Denver CAT used NCS data for Denver as well as other NCS data in each annual criminal justice report since NCS data became available. The most intense use was in the first victimization report for Impact Cities. It was based on detailed microfilm data for the first NCS survey of Denver. The second NCS survey was analyzed for changes and used in the annual planning exercise, but use was limited because of National Criminal Justice Information and Statistics Service (NCJISS) rules prohibiting release of detailed data until the NCJISS report was released. Final release was much too late to have an impact on the Denver crime plans in the years when it may have been useful. In 1976, Denver conducted a limited local victim survey to evaluate a neighborhood anticrime program. The results were ambiguous because of the small number of incidents uncovered in the before-and-after surveys. Denver analysts caution against use of victimization studies for evaluation unless the project is large enough to justify large samples. Random Digit Dialing may provide a method that they can afford to use in later evaluation attempts. In 1978 they will participate with a number of other agencies in a metropolitan areawide survey of attitudes toward public services, and a crime incident survey is being included. They look forward to trend analysis using the two earlier NCS surveys and the 1978 local survey.

The Atlanta CAT has continued to use the NCS since its first Impact Cities report on victimization in 1974. Although originally a Metropolitan Area CAT with 18 staff members, they are now part of the mayor's office and have only 9 members.

One example of detailed analysis by the Atlanta CAT was related to the national debate on crime and the elderly. In 1976 the city's criminal justice council debated the need for a special program to protect the elderly against the crime of burglary. An antiburglary program was already planned for all citizens, but some consideration was given to a special program for the elderly because of news stories that reported high victimization rates and fear of crime among the elderly. The Atlanta CAT found few incidents among official police records, and they then examined the NCS data for 1972. They found a low rate of burglary and a low overall victimization rate for the elderly there as well. Data from the 1975 survey were requested from NCJISS, but only tabulations from the 1972 survey were released to them. A review of detailed attitude data from 1972 was used to conclude that elderly fear crime more than do other age groups, but that fear is usually related to general crime fear rather than specific neighborhood crime fear or actual victimization. Changes from 1972 to 1976 could not be measured because of NCJISS rules on data release. As a result of the study, the CAT did not recommend special programs for the elderly. They recommended additional Atlanta victim studies to define more specifically the elderly crime issue.

A growing number of cities also are conducting or intend to conduct a locally administered survey of victims. Some of these were initiated after the President's Crime Commission and before the NCS surveys were initiated. Others have been initiated because the NCS spurred interest in victimization but was not considered usable for the specific city. In still others, there was interest in having victimization data for evaluation of specific crime prevention programs. The quality of these studies varies widely, and the CJRC staff has tried to provide technical assistance to a number of cities to improve the quality of the studies. The monograph series from LEAA includes one specifically designed to explain the state of the art to those who are considering their own surveys. Telephone interviews were conducted by RTI with several local survey cities to find evidence of past or potential use of NCS. The cities contacted were Louisville; Tucson; Seattle; and Lakewood, Colorado.

Seattle has completed an exemplary project for the National Institute of Law Enforcement and Criminal Justice (NILECJ) that involved a comprehensive burglary reduction plan. The exemplary project is a CCPP to help people recognize their vulnerability to burglary and to help them remove or reduce their risk. The program evaluation is reported to show that the CCPP was successful in reducing the burglary victimization of program participants, and the results were validated through three different types of victim surveys. Reporting of burglary to police increased from 51 to 76 percent after the comprehensive burglary reduction plan was initiated; thus, UCR data would have had limited value for the evaluation. Victimization results estimate a decline of between 48 and 61 percent as a result of the program.

Evaluation of the CCPP was one impressive example among many uses of local victim surveys in Seattle, where victim surveys have become an accepted tool for planning and police performance evaluation. In the burglary program example, victim surveys were used in three different ways: households in treatment and control neighborhoods were surveyed as part of the program, citywide surveys conducted for the broader planning purposes provided a citywide benchmark and measure of relative change, and a simple Random Digit Dialing survey was used to validate the measurement of number of incidents and the reporting to the police of burglaries. National data are used in Seattle whenever studies require data about rare crimes, such as rape or robbery. The local surveys provide an aggregate count and national data provide an estimate of distribution by age, race, sex, or income. With neighborhood characteristics on national data, more use is possible. Seattle supports continuing the national survey and has a third local survey underway.

There is some evidence of growing use of victimization statistics at the local level, but the more extensive users have depended upon the availability of supplementary data from local surveys. Examples of use are typically greater in the cities that have initiated some type of local victim survey. Tucson and Seattle were not surveyed by NCS, but they are higher rated users of the National survey than are NCS cities without a local survey. There are several interpretations that may be made of this, based on the interviews:

(1) Cities or metropolitan areas that acquire competent analysts of crime and the criminal justice system will soon discover the shortcomings of reported crime statistics for most of the analyses to be attempted. The analysts will want age-group information, victim-reporting information, attitudes, or costs of victimization in order to carry out their analyses.

(2) Crime analysts or planners that use national NCS data or victim data from other cities in support of a policy position or proposed program may have difficulty in selling their position to the local council or crime commission. Such bodies typically consider their cities to be different until shown otherwise.

(3) City analysts who dig into the NCS data even for their own city will find that the data frequently falls short of answering specific needs, and they will want a local survey tailored to such specific needs.

(4) After attempting a local survey, local analysts will be much wiser in the use of victim surveys and the cost limitations of increasing their size for increased sensitivity. The national survey will then be better understood and its use as a supplement to the local survey will be more likely.

This interpretation suggests that a LEAA policy of strong support of local victim surveys when help is requested could lead to greater local use of NCS data as well. However, the system for providing such support is not available now and would require careful planning. There is a significant possibility that LEAA could financially support poorly planned and administered local surveys that damage NCS acceptability. However, Census Bureau victim surveys in cities also failed to realize their potential utility. The choice of city rather than county or Standard Metropolitan Statistical Area (SMSA) for the sample unit highlights a failure to determine local agency needs, and the NCS tabulations supplied by the Census Bureau to the cities shows insensitivity to the local agency users. These are a few of the system problems to be solved before local agencies can be assisted effectively by NCS.

Each year after 1973 one or more additional cities began to use victimization data. Several reasons for projections of further gradual growth in use are:

(1) Cities that have increased their levels of use only in response to LEAA guidelines are not committed users and may decrease use of their own NCS city data as it becomes more out of date.

(2) Other NCS cities are limited by their lack of skilled crime analysts. They express interest in working with NCS data more than they have in the past, but they cannot make effective use of the products provided by the Census Bureau and LEAA. If they could obtain special tabulations from the Census Bureau or some other source to meet their special needs of the year, their use could be expected to increase. Neither the tabulation available from the Bureau nor the computer tapes from DUALabs are of any use to the analytically unskilled criminal justice planners.

Without improvements in the products offered, interest by these cities will soon decline. If they find help in making use of their own city's NCS data, their interest may grow in both national data and in having another local survey performed for their city.

(3) Cities with experienced crime analysts who have used local surveys, NCS city survey, and national surveys are likely to continue and expand uses of victimization data. Their uses may be better examples than those published by LEAA. As these uses become well known by other cities with capable analysts, there may be a significant growth in NCS utility. However, there was little evidence that city crime analysts talk to each other except at data-use workshops. These workshops, presented by the CJRC, were well received, but could be further improved by the specific experiences that a few selected cities can share now or in a few years.

Thus, there are factors working both for and against the growing utility of NCS in the cities. Those factors working for greater local utilization can be further encouraged by LEAA, but this will require a clear determination that local utilization has a high priority, not only in the Statistics Division but throughout all of LEAA. If such priority is not given, there will be a temporary continued use of the NCS city reports, a long-term occasional use of NCS results in a few cities, and some expanding use of NCS-supported research products by city analysts who were exposed to NCS during their academic careers. A LEAA commitment to use of victimization data by local agencies will require that LEAA staff or grantees: (1) learn how to use city data from experienced local analysts, (2) provide workshops or other forums for exchange of this information, and (3) support or conduct additional local surveys that are designed to meet the specific needs of the cities in which the survey is to be conducted. If the local user is with a regional planning unit, the survey must provide regional data, and national data for comparison must be presented in compar-

able disaggregations. If there is a need to evaluate a police district treatment program, LEAA or the local unit must take a sample of sufficient size to be sensitive to change in the treatment district and in any control districts. If LEAA continues to fund locally planned surveys, they are strongly advised to insure that the city obtains a professionally designed and administered survey. Technical advice on how to obtain valid results at the lowest cost is needed. This level of technical assistance is not feasible for the NCJISS Statistics Division and the Bureau of the Census as they are now staffed and organized.

States

The utility of the NCS to the States is not considered to be great by the interviewed State criminal justice planners and analysts. Use of the data for victim compensation studies is an exception to the general finding, and it suggests that NCS data in an appropriate issue-related format will have greater utility than currently available NCS documents and printouts. Use in crime analysis at the State level was increased by the LEAA requirement that NCS data be used in comprehensive plans, but the rated level of such uses is low and unlikely to increase without changes in the NCS program. The reasons for this interpretation are:

(1) There are no surveys of States, and the State disaggregations of national surveys are not intended to be representative of the populations of the individual States.

(2) State planners and statistical analysts are reluctant to use the NCS data in support of programs for the State when the data are not specific to and representative of that State.

(3) The LEAA monograph on victim compensation is the only policy use of victimization data available to serve as an example for the State's Statistical Analysis Center (SAC) or the Statistical Planning Agency (SPA).

(4) Most of the funds distributed by the SPA's parent organization go to operating criminal justice agencies; and official statistics of police, courts, and corrections are directly relevant and appropriate to this type of program planning.

(5) The UCR data are available on computers in many States, and there is appropriate software for special analyses and tabulations; NCS data are both difficult to use and not possible to disaggregate geographically.

(6) The attitude questions that were used by NCS were not the questions of prime interest to State planners, and attitude questions are no longer included in NCS surveys.

The primary reason that the State SPA makes little use of the NCS data is that the SPA has little incentive to use the data other than to abide by the LEAA guidelines. There is little evidence that either NCS or UCR data are used directly in setting priorities for criminal justice expenditures, and there is much evidence that attitudes and opinions about crime and the criminal justice system are more important politically. Crime statistics are not expected to influence legislative actions or executive allocations unless they demonstrate a dramatic trend that changes attitudes and opinions. Present NCS data are much too late compared to UCR data and much too difficult to interpret in a time series to serve this important social indicator function for a State.

The other possible use of NCS at a State level is to supplement UCR in the better understanding of crime and its causes or costs. There is a little evidence that the research community is learning how to do this, but no evidence was found that the State crime analysts are prepared for such a high level of analysis. Over 100 documents were obtained from 18 States in an earlier RTI study, and additional documents from State sources were obtained for this study. The analyses contained in these documents do not evidence a high level of analytical skill, and there is evidence that some SAC groups do not have an understanding of statistics or probability. There is a good possibility that NCS data will be misinterpreted if they are used more extensively.

Academic and research institutions

The academic and nonacademic researchers were selected from one or more of the following sources:

- A known core of NCS research users that has played an important role in the historical development of NCS surveys and analytical studies.
- Researchers who have expressed interest in NCS by attending workshops, purchasing tapes, or making inquiries to LEAA, CJRC, Census Bureau, or DUALabs.
- Researchers who appeared prominent in the relevant literature or were referred by other researchers during interviews.

The uses of NCS knowledge in the academic and nonacademic research institutions are no greater and no less than might be expected, given the history of the NCS program. A group of 14 experienced scientific and policy researchers with early involvement in the program are prepared to use the data creatively. Another group of 12 researchers includes competent analysts who are expanding the potential for significant descriptive analyses. A third academic group of 14 has less direct interest in or experience with NCS knowledge but generally supported the program.

Future uses of NCS knowledge by the academic research community are expected to grow significantly as accessibility improves and the experienced user community expands. After 1978, the efforts of LEAA to expand use of NCS knowledge through the University of Michigan's Inter-University Consortium for Political and Social Research (ICPSR) computer archives should have a positive effect on raising the level of use for scientific research and, possibly, for policy research. The ICPSR archives will serve the needs of academic researchers, educators, and others who have postponed use because of DUALab cost and NCS publication data limitations. The expanded uses are not all expected to be creative or policy-relevant in the next few years. There must be a period of learning by both students and educators following the increase in accessibility. Some of the more significant potential uses must also follow the completion of anticipated methodological improvements. These in turn may permit statistical analyses that will provide even better data packages for explanatory analyses and theory development.

The frequency of publications using NCS data

The literature review for this study covered approximately 250 journal articles, books, legislative reports, LEAA publications, research reports, comprehensive plans, and other documents. In all, 179 documents that used victimization data in some form were identified. We traced uses back to 1967 when victimization data were first used by the President's Commission in assessing crime reporting and explaining the need for a supplement to official police statistics. Crime Commission Data (CCD) were the only data referenced until 1974, when NCS data were first published.

In 1975 there was a beginning of use of NCS publication by scientific and policy researchers, but the level was generally routine or interpretive. Work had begun with Census Bureau tapes at OIAJ and CJRC, but very few results had surfaced in published documents.

In 1976 the number of NCS uses observed was up to 41, a substantial increase over the 16 listed for 1975. Old CCD studies on crime reporting were updated with the newly available NCS publications or Census Bureau tapes. The debate on crime and the elderly was revised after the NCS findings on the age distribution of victims. Victim compensation deliberations were aided by three analytical studies that used NCS publications or Census Bureau tapes. LEAA released four more NCS publications, and significant reviews of NCS methodology were reported. Finally, the State of Texas released its first reports on its statewide survey of victimizations and attitudes. A variety of crime-specific subjects appear, and use was made in plans.

The year 1977 saw another increase to 57 uses. The uses are similar to those in 1976 with emphasis on scientific research at the analysis level, but more uses in policy research are noted. Use in planning or evaluation remains limited.

The last year shown is 1978 with 37 uses observed through mid-1978, when the literature review was completed. Our interviews have disclosed a large number of other papers, articles, and reports that were not available for review but are being documented in 1978. Also, the ICPSR training sessions were not completed until the summer of 1978, and these may result in additional documented uses in 1978.

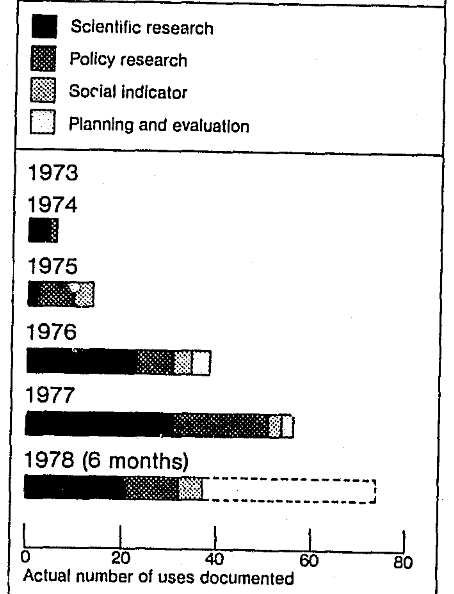
The graph in figure 2 includes all of the uses of data from the NCS program, but excludes CCD uses. Scientific research, policy research, social indicators, and planning and evaluation uses are shown separately for each year. The overall frequency is seen to increase each year but with the rate of increase lower in 1977 than in previous years. If the uses in 1978 are a doubling of those recorded for the first half year, growth would appear steady for 3 years after accelerated growth in 1973–76. Scientific research comprises more than half of the uses in 1976–78, but policy research uses have also played a major role in the growth in use over the period. Social-indicator uses appear constant by comparison with scientific and policy research, and planning and evaluation use is evident only in 1976–77. With so few uses to date, the future importance of the four types of use cannot be projected with any confidence from the figure.

NCS uses include 106 that are primarily scientific research, 81 that are primarily policy research, and 62 that are primarily presentation of descriptive social indicators. If our evaluation of potential uses proves correct, the NCS data use will continue to emphasize scientific research. Policy research should follow in frequency of use, as it has in the past, and social-indicator uses should increase as NCS knowledge gains wider acceptance outside the academic community.

Announced LEAA plans for the future development of the NCS program have been used in making the following forecasts of patterns of NCS use after 1978:

(1) There will be an accelerated growth for several more years in uses by the academic research community because of the LEAA/Department of Justice (DOJ) decision to continue the full NCS program, the planned initiation of a methodological research program by LEAA, and the increased accessibility of academic researchers and educators to detailed NCS data. Such uses will not appear in the literature for several more years, but drafts will become available throughout 1979. Initially, interpretive uses and descriptive analyses will appear, followed by more frequent creative uses as experience grows.

Documented uses of NCS for all categories of use



Note: The highest ranking category is given priority when two or more categories exist. When two categories of equal rank are highest, the category is randomly assigned from one of these. Dotted line indicates projected figure.

Figure 2

(2) Without a methodological research program with a significant statistical analysis component, the scientific uses of NCS data would soon level off as the limitations of the available data were fully understood. However, a successful research program will result in continued growth in frequency and significance of uses.

(3) Social-indicator use will have a more gradual growth as NCS knowledge spreads outside the research community. Methodological improvements in data collection and in statistical analysis will produce better indicators of the risk of victimization over time and increase public interest in the NCS data series.

(4) Increased public interest will be reflected in increased governmental interest and in the need for policy research using NCS knowledge. Use of NCS data will then accelerate in nonacademic institutions and in legislative and executive agencies.

(5) Planning and administrative uses will not become significant unless the NCS program becomes much larger and better oriented geographically to political and administrative jurisdictions.

(6) Evaluation use of victimization data will grow in the cities that can carry out local victimization surveys. Evaluation use at State and national levels will not be feasible.

Summary of findings and conclusions

Growing uses and utility:

(1) Given the gradual pace at which the NCS program has moved toward providing data widely available outside the Bureau of the Census, the amount and nature of use of the NCS have developed in a natural and predictable manner. This gradual pace should not be considered abnormal for a large and complex data series being collected nationally by the Bureau of the Census.

(2) There is evidence that the overall use of NCS knowledge is growing at an accelerating pace as steps taken in recent years by the Statistics Division of LEAA to increase NCS knowledge distribution have had their impact.

(3) It is projected that the use of NCS information will grow at an accelerated pace for a number of years if appropriate modifications are made to NCS production and distribution methods.

Variety in the types of use:

(1) The most frequent substantive use of the data to date has been by the academic community performing scientific rather than policy research. Much of this research initially involved methodological inquiry and comparisons of NCS data with UCR data. More recent research has begun to test theories of the correlates of crime and to develop new hypotheses that may influence congressional debate and LEAA programs at a later date. Growth in this more recent research use of the NCS was evident from the literature review and the nationwide telephone interviews.

(2) There is sufficient evidence of past and potential use of the NCS in policy research to conclude that this use will also grow in the near future.

(3) The NCS aggregate data have been used as a social indicator of crime by many who have received the NCJISS documents. This use has been shallow to date because the interpretations by the Census Bureau do not project trends or postulate causes, and the data are highly aggregated. The full value of the NCS as a social indicator will have to follow the further development of the scientific research uses. These researchers are developing better social indicators than the simple cross-sectional tabulation of incidents per 1,000 persons used presently. The benefit of these social indicators will be to change the conceptions of crime in the Congress and by the public in general.

(4) A national household survey such as the NCS has little use as a tool for detailed planning and evaluation. Census Bureau restrictions on the disaggregation of NCS data limits their use by local planners. The NCS also provides little or no planning input to the program areas of LEAA that do not focus on victims in the social system in general. Since LEAA's mission emphasizes improvement of the criminal justice system, data to describe persons and events within the criminal justice system are currently greatly needed to define LEAA problems and estimate the effects of attempted solutions. However, the need for NCS data for planning does not appear to be great within LEAA program offices.

(5) Although victimization data may be potentially useful as performance measures in evaluating local programs, the NCS does not collect data at the appropriate time or level of detail for such use.

Problems with methodology, validity, and acceptability:

(1) It is the general impression of some potential NCS users and many of the strong supporters of official police statistics that the present NCS methodology produces unacceptable estimates. The National Academy of Sciences review and other criticisms of NCS methodology have caused some concern about the validity of the survey. Experienced users believe that needed methodological changes can be made without destroying the entire value of previously collected data. The academy recommendations are intended to improve the utility of NCS products as well as the validity and reliability of the collected data.

(2) Knowledgeable users fully expect that there will be methodological changes throughout the history of the NCS, as there are in all national series. They support such improvements.

(3) A few users with urban constituents would prefer to have NCS data collection concentrated in one or a few large urban areas. These urban researchers and analysts have concentrated their experiences in a few of the cities in which NCS has completed surveys. They are concerned that household-based surveys of central cities cannot be compared directly with official statistics that include tourists and commuters.

Relevance of findings to LEAA policy for NCS:

(1) The evidence is strong that the NCS is a program with past utility and potential benefits; in the opinion of many in legislative, executive, and academic roles, its termination would represent a tremendous loss.

(2) The findings of this study provide strong support for the continuation of the survey, but not necessarily the full survey now in operation. If maintaining the full survey would prohibit the carrying out of needed analytical and methodological research to enhance the utility of the survey in the future, most of those interviewed would choose a smaller sample to the alternative. However, the reduction of the sample size would be a serious loss to several academic researchers who are presently hard-pressed to find sufficient incidents for study. Longitudinal studies would be stretched out in time, and there would be a loss of sensitivity to changes in the annual victimization rates.

(3) The NCS program lacks clarity in its objectives, particularly with respect to priorities among potential user groups. Attempts to focus equally upon victimization data needs at national, State, and local levels can overwhelm the resources of the NCS program. Attempts to fill the specific needs of both current policy issues and long-range theoretical research can lead to inadequate data for either purpose. It is not yet possible to resolve these priorities by measuring societal benefits from each alternative use, but the consequences of use by each significant user group are explored in this study.

Analytic limitations of the National Crime Survey*

by FRED SHENK and WILLIAM MCINERNEY

Important progress has been made in understanding crime through the use of victimization surveys. These surveys, which gather data from victims on the extent and impact of crime, contribute to the function of baseline statistics for the criminal justice community. Estimates on the extent of crime are tabulated by the National Crime Survey (NCS) for crimes against individuals age 12 and over and for households. For individuals, personal crimes of violence and theft are measured. The household offenses of burglary, larceny, and motor vehicle theft are included as well. In addition to enabling estimation of the extent to which such crimes occur, the survey also permits examination of the characteristics of victims and supplies detailed information concerning the circumstances surrounding the victimization.

In providing these estimates, the NCS contributes useful baseline information for the criminal justice community. However, in applying these data to criminal justice problem solving, analysts should be aware of the capabilities and limitations of the surveys. These capabilities and limitations are conditioned by the concepts and methodology that were intrinsic to the development of the NCS. While a complete review of the analytic strengths and weaknesses of the survey would be too extensive, this paper selectively examines some of the analytic boundaries that may be encountered in using NCS data. Although the importance of methodological considerations that contributed to these boundaries is recognized, greater attention will be paid to the impact of these limits on criminal justice applications. Also, in examining the potential of NCS data, further insights will be gained concerning the kinds of baseline information provided by the survey.

This paper draws directly from the research experiences of the Crime Statistics Analysis Staff of the Bureau of the Census. Since 1974 this staff, in conjunction with the National Criminal Justice Information and Statistics Service (NCJISS) of LEAA, has prepared the official descriptive reports originating from the NCS. This experience has provided valuable insights into the utility of the NCS as a source of information about the phenomenon of crime.

For organizational purposes, two main topics will be examined. First, the general capability of the NCS to provide detailed information for applied primary and secondary analysis will be considered. Second, a review of the NCS-generated data on the geographic distribution of victimization will be undertaken. Specific attention will be given to how the neighborhood-characteristics data set, now a part of the NCS's data files, functions with regard to an understanding of areal victimization patterns.

Current analysis

The Crime Statistics Analysis Staff's publication efforts have been directed in the main to organizing the wealth of information produced by the survey into descriptive reports that detail the national incidence and dispersion of crime as well as assess numerous victim and crime characteristic interrelationships. Examples of topics that have been addressed in the series of annual reports include the following:

- personal, household, and commercial crime rates;
- personal, household, and commercial victim characteristics;
- victim-offender relationships;
- offender characteristics;
- crime characteristics, including time of occurrence, weapons used, victim injury, time lost from work, number of offenders, and victim self-protection measures;
- estimates of crime rates for rural, suburban, and urban areas; and
- rates of reporting crimes to the police and reasons for not reporting.

The approach to the voluminous amount of data available has been to analyze numerous two- and three variable crosstabulations, utilizing hypothesis-testing techniques to distinguish differences in crime rates and percent distributions that achieve appropriate statistical levels of confidence (either the 90- or 95-percent confidence level). This type of description provides a baseline instance, by referring to the publications, crime analysts can find the answers to questions of interest such as: (1) whether blacks are less likely than whites to report crimes to the police; (2) whether elderly persons are more probable victims of crime than younger ones; or (3) how often household burglars use force to gain entry to homes.

Implications of sample size and standard error

The major advantage of working with the NCS as a source of descriptive data about crime is its large sample and accompanying small standard errors for hypothesis testing. Every 6 months about 136,000 respondents are interviewed. Because crime is a statistically rare event, the number of interviews conducted each year vastly exceeds the number of crime incidents uncovered, which average about 31,000 each year.

The crime of assault provides a good example of the reliability of the estimates associated with a survey sample of this size. For 1975, the estimated rate of assault per 1,000 persons age 12 and over was 25.1. The standard error of this statistic at 95 percent confidence level was about 1 per 1,000. When testing for differences in crime rates or victim- and crime-characteristic distributions for large subsamples, small standard errors such as this one make rejection of null hypotheses rather easy.

While the NCS crime estimates are statistically very reliable for large units of analysis, there are a number of problems that the survey's users will encounter if they attempt to more thoroughly examine complex patterns of variable relationships or issues involving small subsamples. Specifically, investigation of such issues as domestic violence, juvenile victims, or reporting of crime to the police by minority groups is difficult because of considerations of sample size and standard error. As the analyst classifies data into smaller and smaller cells, there is the danger that the number of cases for analysis will become too small to provide a reliable data base. While any particular lower limit of cases for analysis is arbitrary, the minimum standard used for the NCS publications is 10 unweighted cases, which is equivalent to about 10,000 weighted cases. Estimates based on fewer cases are considered statistically unreliable and are not reported. Also, because standard errors fluctuate in part with sample size, subclassification of data may lead to a ballooning of standard errors to the degree that rejection of null hypotheses becomes very difficult, even though estimated differences between crime rates of percent distributions appear quite large. Standard errors of 15 and 20 percentage points at the 95-percent confidence level are not at all uncommon when examining limited subsamples.

An example of the analytical limitations encountered with small subpopulations is provided by the data in table 37. This table, taken from a preliminary analysis of domestic violence, indicates the number of unweighted as well as weighted wife-abuse cases recorded by the NCS for 1973-76. Although 121,460 unweighted incident records were available which a female reported abuse by her spouse or ex-spouse, or only 0.4 percent of the total number of records.

Table 37. Personal crimes of violence: Number of victimizations in which a male victimized his female spouse or ex-spouse, by type of crime, 1973-76

Type of crime	Unweighted data	Weighted data	Confidence level*
Crimes of violence	481	616,949	523,971-709,927
Rape	10**	12,981	2,293-23,669
Robbery	39	48,388	30,248-66,528
Robbery with injury	27	34,317	19,041-49,593
Robbery without injury	12	14,071	4,289-23,853
Assault	432	555,580	467,358-643,802
Aggravated assault	137	183,146	132,504-233,788
Simple assault	295	372,434	300,210-44,658

Note: Data are for the United States.
* Has 95 percent probability of including value being estimated.
**Estimate based on 10 or fewer sample cases, is statistically unreliable.

Examining table 37, it is apparent that aggregation over time was necessitated by the weakness of the data for any 1-year period. Yet, this summary table indicates that cases of one crime, rape (and conservatively a second, personal robbery without injury), are so infrequent that they are statistically unreliable, even with aggregated data. Certainly, cross-classification of these data by basic demographic variables such as age, race, or income would produce additional cells with unreliable estimates, making meaningful analysis nearly impossible.

Perhaps the more frustrating problem the analyst encounters with small subgroups is the size of the associated standard errors. As an example, the size of the standard error of the estimate for aggravated assault is from 132,504 to 233,788 at the 95-percent confidence level. With a standard error this large, few meaningful differences between victim or crime characteristic variables would be identifiable, and of course the standard errors would be even larger with variable cross-classification.

The above examples should serve to illustrate some of the difficulties inherent in using the NCS as a source of applied data, despite the size of the samples. As sociologists, criminologists, and political scientists with an interest in crime statistics and their applications to crime problems, we are fortunate to have available a survey with the wealth of detail and reliability provided by the NCS. However, even with the generous sample and large number of variables derived from a sophisticated questionnaire, users must first evaluate, on a topic-by-topic basis, the utility of the NCS for their applications. If NCS data are to be used for detailed descriptive or explanative purposes, an expanded sample would be necessary, but at this time is most unlikely. At present, annual surveys that replicate results can serve to increase confidence in findings stemming from small subsamples. Also, aggregation of annual data may provide a partial solution to small data bases for some subject of analysis.

The problem of nondiscrete crime events

The attempt to count crime events accurately presupposes in part that crime events are discrete. One of the more interesting findings of the NCS has been that a large body of criminal events exists that is very difficult if not impossible to identify separately. These nondiscrete events are called "series" crimes.

The determination of whether crime events are recorded as series crimes is made by the NCS interviewer, within predefined guidelines. These are: (1) the victimizations must be very similar in detail, (2) there must be at least three victimizations in the series, and (3) the respondent must not be able to recall dates and other event details well enough to report them separately. If all of these conditions are met, the interviewer records the nondiscrete events as a series crime and, in addition, records the range of the estimated number of victimizations, the season or seasons of occurrence, and characteristics of the last victimization in the series.

The current practice is to exclude series crimes from NCS crime-rate estimates because of the difficulties in counting the exact number of victimizations and in identifying the month of occurrence, as is the procedure for the discrete data. The body of data in the NCS publications therefore consists of discrete single crimes committed against each person or household and discrete multiple crimes committed against each person or household. Of interest to potential users of NCS reports and data is the fact that identification of the number of discrete multiple crimes and of persons who have been victimized more than one time for any data reporting period, such as one-quarter of a year or one full year, has not yet been accomplished for the reports. The principal problem with this task is that one or more occupants of a housing unit may change from one 6-month reference period to another, which complicates matching crimes reported for the housing unit with individuals for past reference periods.

The exclusion of series crimes from the NCS crime estimates presents at least three problems for survey users. The first is that the published estimated

*Excerpted with editorial revisions from "Issues Arising from Applications of the National Crime Survey," a paper presented at the Annual Meeting of the Southwestern Political Science Association, Houston, Texas, 1978.

Table 38. Personal and household crimes: Number of series crimes and percent distribution of series crimes compared with victimizations not in series, by type of crime, 1976

Type of crime	Number of series crimes	Percent distribution	Number of regular victimizations	Percent distribution
Crimes against persons	933,000	100.0	22,118,000	100.0
Crimes of violence	503,000	53.9	5,599,000	25.3
Rape	11,000	1.1	145,000	0.7
Robbery	47,000	5.0	1,111,000	5.0
Robbery and attempted robbery with injury	19,000	2.1	361,000	1.6
Robbery and attempted robbery without injury	27,000	2.9	750,000	3.4
Assault	446,000	47.8	4,344,000	19.6
Aggravated assault	109,000	11.7	1,695,000	7.7
with injury	33,000	3.5	589,000	2.7
Attempted assault with weapon	76,000	8.2	1,107,000	5.0
Simple assault	337,000	36.1	2,648,000	12.0
with injury	54,000	5.7	692,000	3.1
Attempted assault without weapon	283,000	30.3	1,957,000	8.8
Crimes of theft	430,000	46.1	16,519,000	74.7
Personal larceny with contact	8,000*	0.8	497,000	2.2
Personal larceny without contact	423,000	45.3	16,022,000	72.4
Crimes against households	667,000	100.0	17,199,000	100.0
Burglary	230,000	34.5	6,663,000	38.7
Forcible entry	88,000	13.1	2,277,000	13.2
Unlawful entry without force	105,000	15.8	2,827,000	16.4
Attempted forcible entry	37,000	5.6	1,560,000	9.1
Household larceny	429,000	64.3	9,301,000	54.1
Less than $50	281,000	42.1	5,602,000	32.6
$50 or more	89,000	13.3	2,745,000	16.0
Amount not available	33,000	4.9	299,000	1.7
Attempted larceny	27,000	4.0	655,000	3.8
Motor vehicle theft	8,000*	1.1	1,235,000	7.2
Completed theft	1,000*	0.2	760,000	4.4
Attempted theft	6,000*	0.9	475,000	2.8

Note: Detail may not add to total shown because of rounding. Data are for the United States. Series victimization are for the period April 1976 through March 1977.

*Estimate, based on 10 or fewer sample cases, is statistically unreliable.

Table 39. Personal and household crime: Series crimes by the number of victimizations in series, 1976

Type of crime	Total number of series crimes	3-4 series victimizations	5-10 series victimizations	11 or more series victimizations	Don't know/ not applicable
Crimes against persons	933,280	493,670 (52.9)	267,750 (28.7)	123,420 (13.2)	48,460 (5.2)
Household crimes	667,320	368,550 (55.2)	191,630 (28.7)	55,660 (8.3)	51,480 (7.7)

Note: Detail may not add to total shown because of rounding. Data are for the United States.

number of victimizations, and therefore estimated crime rates, is reduced. In 1976, about 933,000 personal series crimes and 667,000 household series crimes were measured by the NCS, as indicated in table 38. Table 39 indicates that for personal series crimes, 53 percent involved 3 or 4 victimizations and a surprising 13 percent involved 11 or more. For household series crimes, about 55 percent consisted of three or four victimizations. Based on these estimates, and using the lower value for each of the three ranges, well over 4 million personal victimizations and 2 million household victimizations have not been included in the crime level estimates of 22,118,000 personal and 17,199,000 household victimizations for 1976. Since the number of victimizations is reduced, crime-rate estimates are necessarily also lowered. However, from an analytical perspective, the fact that the NCS now underestimates the true level of crime in the nation is probably not very damaging as long as the underestimate is realized by users. And perhaps of more importance is the bringing to light by the NCS of much of the dark figure of heretofore unreported crime.

In addition to understating the level of victimization, the exclusion of series crimes also probably influences the relative distribution of crime by type. Table 38 compares the percent distributions of series and nonseries crimes for 1976. Assaults, particularly attempted assaults without a weapon, and household larcenies under $50 are overrepresented as nondiscrete crimes. This pattern affects the ratio of nonseries personal assault to other violent crimes as well as that for crimes of violence to personal crimes of theft, and the ratio of nonseries household larceny to household burglary and motor vehicle theft. Analysts working with relative distributions or ratios of offenses should be aware that these effects are present in the current crime estimates.

The third problem that series crimes present to analysts is the probable distortion of victim and crime-event relationships for nonseries data because of unequal distributions of series crimes between victim subgroup and crime characteristic categories. As one example of this likelihood, the cross-

Table 40. Personal crimes of violence: Percent distribution of series crimes and regular violent victimizations by selected characteristics of victims, 1976

Victim characteristics	Regular victimizations	Series crimes
Victim relation to offender	5,599,000	503,000
Stranger	64.2	48.7
Nonstranger	35.7	51.3
Victim race*		
White	84.9	90.8
Black	15.1	9.2
Victim sex		
Male	63.1	62.6
Female	36.9	37.4
Victim age		
12-19	34.9	33.3
20-34	43.0	48.0
35-49	12.3	12.0
50-64	6.9	4.9
65 and over	3.0	1.7**

Note: Detail may not add to total shown because of rounding.

Data are for the United States.

*Excludes victims of races other than white or black.

**Estimate, based on 10 or fewer sample cases, is statistically unreliable.

tabulations in table 40 indicate that overrepresentation of violent series crimes most likely existed (at the 95-percent confidence level) in the 1976 data set for offenses committed by persons known to the victim as well as for white victims. As another example, in a preliminary review of the distribution of series crimes from a subsample of NCS questionnaires by Richard Dodge it was found that one violent crime commonly reported in series was assault in the line of duty, such as attacks on police officers or security guards. Domestic friction crimes also ranked high, which would have implications for analysis of this topic and the area of nonstranger crime generally. It is unclear whether inclusion of series crimes in official estimates would significantly alter relationships already documented in the NCS reports, but until more detailed information is available, analysts probably should consider at least conceptually the consequences for applications of NCS data. Subsequent examination of individual variables for possible distortions resulting from exclusion of series crimes is recommended.

A number of problems will have to be overcome before series data can be integrated with the more reliable discrete data. By definition, series victimizations are less clearly remembered by respondents. Since detailed information is collected only about the most recent victimization in the series, nothing is known about the others. Also, because the exact number of victimizations in a series is unknown, some form of weighting would be required for integration with the discrete data. Finally, the two types of data are not now recorded in the same units of time. Eventually, interview schedule redesign for series crimes may ameliorate some of these problems.

Although partial success may be achieved in reducing the level of nondiscrete crimes recorded by the NCS, it has been suggested that the logic of treating all criminal victimizations as separate incidents is inapplicable in some instances. A child who gives up his lunch money every day to a larger child, or a policeman who incurs verbal abuse or physical assault every day while on duty, probably lives in a continuing state of extortion or abuse. Whether events such as these should be defined as individual victimizations is speculative. Crime analysts might be advised also to consider this problem in relation to crime data sources other than the NCS, since the problem extends to all crime-reporting systems.

Subnational crime estimates

In considering potential applications of the NCS, the issue arises as to the capability of the survey to provide areal victimization data on other than a national level. There has been an ever-increasing need by all sectors of the criminal justice community for area-specific data that will contribute to research and planning. Although the NCS was primarily designed to provide national estimates on crime, a limited amount of information is available on subnational victimization patterns. Such information may be especially useful to practitioners who are unable to conduct their own victim surveys. The three main sources of subnational data currently available will be discussed in the next sections: State-level estimates, SMSA data, and data on neighborhood characteristics.

Victimization data for States

Perhaps the most recent and least known development in areal data from the NCS has been the production of selected State-level estimates. Microfilm tabulations and accompanying documentation manuals exist for 1974-76 for the 10 largest U.S. States. States for which data are available include California, Florida, Illinois, Massachusetts, Michigan, New Jersey, New York, Ohio, Pennsylvania, and Texas. These data provide State-level crime statistics that are comparable to the national statistics published yearly. Official reports are now being prepared analyzing this State-level data.

Victimization data for Standard Metropolitan Statistical Areas

The most frequently used areal data available from the NCS are SMSA estimates included as a standard part of annual reports on the survey. By focusing on the type of locality in which the victim lives, aggregate patterns of crime against central city, suburban, and nonmetropolitan residents can be examined and compared. Metropolitan areas (central city and surrounding suburban counties) also can be categorized with respect to the size of the population of their central city: 50,000 to 249,999; 1/4 to 1/2 million; 1/2 to 1 million; and 1 million or more. Note, however, that the data reflect the type of locality in which the victim who reported a crime lives, not the location where the incident occurred. Such conceptualization may be inappropriate for analysts who wish detailed information on the location of the incident. While not a part of the published reports, data tapes from the survey allow for examination of whether or not incidents occurred in the same locale as the victim's place of residence. This information allows users to explore questions such as the extent to which suburbanites are victimized in the central city of the metropolitan area where they live and the extent to which persons are victimized outside of their SMSA of residence.

Neighborhood characteristics data and the NCS

While the state and aggregated SMSA data may be useful for certain criminal justice practitioners, their utility for many other users is problematic. Limitations of sampling error and possible violations of confidentiality complicate the disaggregation of SMSA's and the disaggregation of counties or local jurisdictions within States. Without such data, specific local victimization patterns cannot be examined. Because the NCS was primarily designed to provide national data, analysts seeking to use the survey must be aware that certain geographical data may not be available for their particular research needs. However, based on certain recommendations, attempts have been made to increase the applicability of the NCS by the inclusion of neighborhood characteristics on the survey's data tapes. However, caution is warranted concerning the analytical capability provided by these characteristics.

Developed from a 15-percent sample of the 1970 Census, the Neighborhood Characteristics Public Use Sample is a set of 34 population and housing characteristics. These characteristics represent demographic and residential indicators of the environment in which a sampled household is located. Data for these variables are presented in ratio form with a range from .00 to .99, e.g., ratio of population 65 years and over to total population in the neighborhood.

When using neighborhood characteristics it is important to note what the concept of "neighborhood" means. To preserve confidentiality, neighborhoods are not census tracts, minor civil divisions, or other units for which Census Bureau data are published. Rather, neighborhoods are usually contiguous, computer-aggregated enumeration districts (ED's) or block groups with a population minimum of 4,000. Enumeration districts are administrative divisions set up by the Census Bureau to take the census in areas where door-to-door enumeration was used, averaging 800 population. Block groups are groups of city blocks, averaging 1,000 population, which are the equivalent of ED's in the city mail delivery areas of the 145 SMSA's where the census was taken by mail in 1970. Neither socioeconomic nor demographic data were used in forming neighborhoods. Similarly, maps were not used in constructing neighborhoods. Therefore, while neighborhoods rarely cross county lines, they may straddle a meaningful social boundary such as an urban freeway.

Table 41. Household burglary: Victimization rates, by ratio of young adults (ages 16 to 21) to total population in neighborhoods, 1973

(Rate per 1,000 households)

Neighborhood ratios for young adults	Number of households	Burglary rate*
.00-.09	31,408,797	79.27
.10-.19	32,317,245	92.82
.20-.29	1,040,040	91.28
Above .30	1,066,935	138.08

Note: Data are for the United States.

*Burglaries of households at vacation homes not included.

The neighborhood characteristics have been matched on a household-by-household basis to the NCS sample for data years 1973-76. From this procedure, each household record in the NCS data file has an attached set of neighborhood characteristics that provide information about the neighborhood in which the housing unit is located. However, because neighborhoods were constructed from 1970 Census Bureau data, housing units constructed since that year are without neighborhood characteristics identifiers.

Neighborhood characteristics and areal victimization patterns

Table 41 is a typical example of how neighborhood characteristics may be used to investigate victimization patterns. This table shows the distribution of household burglary rates for 1973 by the ratio of young adults (ages 16 to 21) living in neighborhoods. Interestingly, households in neighborhoods with the highest ratio of young adults also had the highest rate for household burglary. Although no data for specific locales are provided by these characteristics, it is assumed that applicable generalizations concerning area-specific patterns can be gained. Subsequent development of neighborhood typologies may further provide valuable information for local officials concerning the more common areal patterns of victimization.

Analytic limitations when using neighborhood characteristics

While the potential is great for using neighborhood characteristics to help answer generalizable, area-specific questions of interest, there are a number of analytic problems that users must initially recognize. By matching neighborhood characteristics with the locality of households and not the place of occurrence of incidents, analytic efforts investigating the neighborhood correlates of personal crimes are suspect because personal victimizations can occur almost anywhere. Although the survey is able to differentiate between personal incidents that take place at home or away from home, the away-from-home designations do not lend themselves to the interpretation of whether the event took place in the victim's own neighborhood. As table 42 shows, about four-fifths of all personal crimes of violence for 1975 occurred outside the immediate vicinity of the respondents' homes, limiting the utility of examining the neighborhood environment in which incidents occurred. Moreover, selecting only personal incidents that take place at home could bias analysis because at-home personal crimes are different in nature than away-from-home personal crimes.

The situation is somewhat different for household crimes. Since household burglary and household larceny occur at or near the place of residence, neighborhood characteristics data should provide reliable environmental indicators for these crimes. (A very small percentage of burglaries do occur at vacation homes.) However, the household crime of motor vehicle theft can occur anywhere, and therefore only motor vehicle thefts that occur at or near the victim's place of residence would be suitable for analysis using neighborhood indicators.

Unfortunately, there is an additional problem with these data that also taints

Table 42. Selected personal crimes: Percent distribution of incidents, by type of crime and place of occurrence, 1975

Type of crime	Total	Inside own home	Near own home	Away from own home: Total	Inside nonresidential building	Inside school	On street or in park, playground, schoolground, and parking lot	Elsewhere
Crimes of violence	100.0	11.6	9.5	79.0	14.6*	5.8	47.6	11.0
Rape	100.0	22.1	5.3*	72.1	4.8*	0.0*	54.3	13.0
Robbery	100.0	11.1	7.7	81.3	8.5	5.9	59.9	7.0
Robbery with injury	100.0	11.4	8.9	79.7	5.6	2.5*	62.9	8.7
Robbery without injury	100.0	11.0	7.0	81.8	9.8	7.5	58.4	6.1
Assault	100.0	11.3	10.2	78.6	16.7	6.0	43.9	12.0
Aggravated assault	100.0	11.0	10.8	78.2	14.4	3.1	48.3	12.4
Simple assault	100.0	11.4	9.8	78.7	18.0	7.6	41.4	11.7
Personal larceny with contact	100.0	2.5	3.5	94.1	36.6	8.5	41.0	8.0

Note: Detail may not add to total shown because of rounding. Data are for the United States.

*Estimate, based on 10 or fewer sample cases, is statistically unreliable.

their usefulness, even for household burglary and larceny. Specifically, the NCS's national sample is drawn from various forms of Census Bureau address listings. Because the recall period for the survey is 6 months, at any given enumeration there are a certain number of new residents since the last interview. Data compiled from such replacement households have special ramifications when using neighborhood characteristics, because household incidents experienced by new residents potentially occurred in another locale. If such incidents did occur prior to the change in residence, then neighborhood characteristics are not applicable as indicators of the area in which the incident took place. Since 19.4 percent of household burglaries for 1973 were reported from replacement households, the impact of such errors could be significant. At present, there is no procedure for knowing exactly how many household burglaries reported from replacement households actually occurred prior to a change in neighborhood of residence. Furthermore, any decision to exclude replacement households from analysis could have an adverse effect on validity, because a segment of the population (movers) with different experiences from the rest (nonmovers) would be omitted. Analysts using household incidents in conjunction with neighborhood characteristics must be conscious of the bias or error introduced by replacement households.

There are further issues that must be considered when using neighborhood characteristics, not the least of which is the validity of the artificial computer selection process used to form neighborhoods. However, the issues of place of occurrence of personal crimes and motor vehicle theft and error produced by replacement households limit the analytic potential of using neighborhood characteristics to gain insights into areal victimization patterns. Without more accurate measures of the exact geographic place of occurrence of victimizations, neighborhood characteristics only imperfectly achieve the function of providing more detailed area-specific crime data. It appears that if the NCS is to provide usable, area-specific victimization data, especially on a neighborhood scale, greater attention must be paid to developing techniques that define the specific place of occurrence of crimes.

Conclusion

This paper has attempted to inform users of NCS victimization statistics about the survey as a source of baseline information on crime. Therefore, the issues discussed have been concerned, in the narrow sense, with a few practical limitations on data analysis and, from the broad perspective, with the utility of NCS data for criminal justice problem solving. This review has focused upon the manner in which data are now analyzed for publication and how they may contribute to wider research applications.

The NCS represents significant progress in the understanding of crime. Although the survey does not presently provide data to answer all questions concerning crime, and cannot be expected to, new and important findings have come from analysis of the wealth of victimization data collected. Furthermore, the available baseline information provided by the NCS is a major development in the creation of a comprehensive statistical base that documents the trends and patterns of criminal activity. Efforts that would improve the quality of NCS data should further increase its value to the criminal justice community.

The comparability of victimization data and official statistics on crime*

by DUALABS, INC.

Although it is possible to compare NCS data to local police reports, such comparisons are not straightforward and the results must be interpreted with caution. The following discussion addresses the problem of constructing comparisons of victimizations identified in the National Crime Survey Cities Samples with those officially known to the local police.

The logical elements of the comparison must be clearly defined. What elements are required as a matter of definition to classify an incident as a particular type of crime? When and where did the victimization occur and where was it reported? Police crime reports and the National Crime Survey publications both contain certain types of summary statistics, but comparability cannot be established on this level. Instead, some fraction of the crime reflected in official police reports must be extracted and matched conceptually with a small part of the incidents covered in one of the survey cities. The detailed type of crime codes in the NCS files make this matching possible.

Most official police reports are compiled annually, some on a calendar- and some on a fiscal-year basis. However, interviewing for the NCS Cities Samples typically goes on over a 2- to 3-month period, with the result that reported victimizations may be distributed over a 15-month period. A further problem arises from the fact that reports to the police are generally made at the time of occurrence; reports in the survey are retrospective and involve a certain degree of forward telescoping. Therefore, crimes correctly reported to the police in the previous year may be incorrectly reported to the NCS interviewer as belonging to the current year. As a general rule, the best comparison can be made with respect to the time of occurrence for the 6-month period immediately preceding the first month of interviews. This time segment will be the most recent period common to all persons interviewed, and in many cases, local police reports will show figures for the same 6-month period. One potentially serious qualification to this rule is the factor of seasonality as it relates to certain types of crimes (such as household burglary during July and August vacations).

Because the National Crime Surveys include victimizations for persons who recently moved into the city that occurred at the respondent's previous place of residence, and victimizations for all respondents that occurred while away from home, including outside of the city of residence, comparison with police reports will require place-of-occurrence adjustments. Official police reports reflect only crimes that occurred within the police jurisdiction, the corporate limits of the city. The other side of this issue is that police reports include reports made by nonresident victims (out-of-town visitors). The residence of the victim must be considered in relation to the place of occurrence for creation of local data or comparisons with data from other sources.

Adjustment for differences in definitions of crime categories between the NCS and UCR can be made. The NCS files identify each of the elements that constitute a UCR-defined crime. The NCS crime categories corresponding to the UCR counterparts are shown in table 43.

Series victimizations create another restriction when comparing NCS data with local police reports. Data on series victimizations fail to meet the test of incidents separate and distinct in the recollection of the respondent. The same factors that cause incidents to be treated as series may operate to keep them out of official police reports. On the other hand, the possibility that separate incidents of a series victimization are included in UCR reports cannot be ignored. Certainly the analyst should look at the series reports and consider the possibility that they are meaningful incidents deficient primarily in their time reference. Analysts may wish to consider evolving unique editing schemes for series reports related to the user's analytic problem.

Table 43. Comparison of classification schemes between Uniform Crime Report and National Crime Survey for crimes against persons

UCR category	NCS category
Rape	Rape with theft Attempted rape with theft Rape without theft Attempted rape without theft
Aggravated assault	Serious assault without theft Attempted assault with weapon, without theft
Armed robbery	Serious assault with theft with weapon Robbery, no assault, with weapon Attempted robbery, no assault, with weapon
Unarmed robbery	Serious assault, no weapon, with theft Minor assault[1] with theft Robbery, no assault, no weapon Attempted robbery, no assault, no weapon
Simple assault	Minor assault[1] Attempted assault, no weapon, without theft
Larceny[2]	Purse snatch without force Attempted purse snatch without force Pocket picking

[1]Minor is defined to exclude weapons; presence of weapon automatically classifies assault as serious by NCS rules.
[2]UCR definition of larceny includes many more types of offenses than the personal confrontation crimes.

Comparisons with the UCR are affected by the fact that the NCS covers only those victimizations for the population 12 years and older, while the UCR data cover all persons.

Finally, although there is substantial nonreporting to the police of crimes that are reported to interviewers in the NCS, it is likely that some crimes are reported to the police but not mentioned in the NCS interviews.

Comparisons between cities

As noted earlier, the NCS data cover victimizations experienced by city residents whether their victimization(s) occurred in or out of the city. Therefore, in one sense the full extent of crime within the city will not be evident because victimizations to nonresidents are not reported. On the other hand, victimizations to residents occurring outside the city are reported. It follows that cities that attract sizable tourist populations such as New York; Washington, D.C.; and Miami may have a higher rate of crime reported to police than those that remain fairly stable. This same logic also applies to cities having a high commuter influx.

Local variation in the definition of crime must also be considered. For example, a 16-year-old living in New York City may not consider a minor assault worthy of reporting, while the counterpart living in Portland, Oregon might consider the same incident quite serious. What is serious to people in one area may not be serious to people in another.

Users could also compare victimizations for the population of the United States to the victimizations of residents of the 26 central cities. While this type of comparison is possible, major problems are: (1) the periodic nature of the central city surveys; (2) the reference period for interviews in the city surveys is 1 year and the reference period for the national survey interviews is 6 months; and (3) the city survey interviews are unbounded, whereas the national survey interviews are bounded.

*Excerpted with minor editorial revisions from *Handbook and Guide to tape files*. Arlington, Va.: DUALabs, Inc. 1976.

Chapter 5

The future of crime surveys

Introduction

Chapter 5 considers the future of victimization surveys. The first selection summarizes some conclusions of the report of the National Research Council of the National Academy of Sciences, which the Law Enforcement Assistance Administration (LEAA) commissioned to review the victimization program. The report argued for the development of social indicators of crime that detail the extent of injury and financial loss attributable to crime and describe their distribution in the social structure. The academy further stressed a need for measures of citizen attitudes, fear of crime, and satisfaction with the operation of the criminal justice system. It emphasized that understanding why measures of crime and fear rise and fall is as important as documenting their oscillation and urged inclusion of measures of explanatory conditions in the survey. The council's report also reviewed some of the policy and scientific uses of such a series. Victimization data should help policymakers to rank the relative priority of crime and other issues on the policy agenda, while they should assist researchers in winnowing through generations of criminological theories.

The second selection is an excerpt of a speech by Benjamin Renshaw, acting director of the Bureau of Justice Statistics, concerning policies affecting the redesign of the National Crime Survey (NCS). During the 1979–81 period, BJS has commissioned the Census Bureau and a research consortium of individuals and major academic research centers to redesign the National Crime Survey. This paper summarizes the principal objectives of the redesign program and discusses several methodological and organizational constraints that shape that effort.

The need for a continuing series of victimization surveys*

by the NATIONAL RESEARCH COUNCIL

The victimization survey as a social indicator

In the decade since the first victimization surveys were carried out for the President's Commission, substantial progress has been made in the United States and in other countries toward the goal of providing a wide range of social indicators—that is, quantitative time-series data, analogous to economic indicators, that reflect social change, the accomplishment of specific social goals, and the magnitude of social problems or concerns. A continuing series of victimization surveys could provide a range of social indicators.

In suggesting the use of victimization surveys to provide social indicators, we do not envisage mere counts of crimes or victimizations, nor just aggregate rates of victimizations. Instead, the ideal series also should monitor the impact of crime in both personal and social terms. For example, how many persons are injured, in various degrees, as a result of violent crime, and what are the individual and social costs of such injuries? What are the risks of this kind of injury for different sectors of American society, and how are those risks related to other risks of injury? What is the direct personal cost of theft, in any given year, and how does it compare with the cost in other years and to other forms of loss? What is the distribution of criminal victimization of various kinds in the social structure, and how are changes in that distribution related to other social changes?

A continuing national victimization survey would at first probably provide only indicators of the objective effects of crime on the community. But in time, the surveys should produce data on subjective effects as well. A growing body of surveys has, in recent years, attempted to measure perceptions, expectations, beliefs, attitudes, and values, on the assumption that the quality of life is in the eye of the beholder. A fairly consistent finding of these researchers, anticipated to some extent by earlier survey data on the fear of crime, has been that people's subjective perceptions of their own welfare, in this case, their feeling of freedom from crime and/or satisfaction with the workings of the criminal justice system, are not related in any simple or straightforward way to the objective facts of their experience nor to the real risks of crime that they confront. A continuing national victimization survey could thus provide, in a very literal sense, a measure of "domestic tranquility" and could help to relate that sense of tranquility, or its absence, to the relevant facts of social life.

The production of social indicators relating to crime need not involve any particular value premise; in particular, it is not, per se, to imply that an increase in crime, of the kinds measured by victim surveys, necessarily means society has changed for the worse. Analysis may show the change is attributable to a change in the population composition, to increases in wealth, and/or to a shift from activities of equal or greater harmfulness to those types of activities registered as crimes of victimization.

A continuing series of victimization surveys, carefully designed and validated in the ways described elsewhere in this report, could help to fill in the details of American life. It could help to illuminate our society's concepts of crime and the moral order, and it could help to provide a factual foundation for a reassessment of that moral order.

Executive and legislative uses of victimization surveys

For most of the past dozen years, crime has been seen as a serious social problem in the United States and, as such, has been an important political issue. Many of the types of crime or disturbances that caused the most concern in that period—urban and campus riots, assassinations, violent political protest, the Pandora's box known as "Watergate"—did not, of course, require a victimization survey for their investigation. But in addition to those dramatic incidents, there was a general concern about more traditional forms of lawbreaking; in particular, "street crime" and other violence committed by strangers. This concern is reflected in the legislative origins of the NCS.

It can be forcefully argued that this concern is unrealistic. Evidence from a variety of sources, including the NCS and other victimization surveys, suggests that for the majority of Americans, crime of the type surveyed in the NCS is not, in fact, an important personal problem—compared with issues such as inflation, unemployment, educational costs, or race and sex discrimination. What cannot be denied is that public concern about crime is real. Crime is thus likely to remain an important fact of political life.

As to the utility of a continuing series of national victimization surveys for the executive and legislative, it is conceivable that it would reside largely in showing what could not be done about the crime problem, as well as showing, of course, more clearly what that problem is. The existence of such a series would mean that political decisionmakers no longer had to rely solely on the Uniform Crime Reports (UCR) or on other administrative statistics for information on the level of crime. In addition, the victimization series would provide a wealth of information about the distribution and social consequences of crime, which could never be obtained from police statistics. Such a series could thus provide a much more rational basis for expenditures on the criminal justice system than has ever been available. It also could provide data relevant to a wide range of more specific issues, such as gun control and compensation for victims of crime. And, by exploring public attitudes concerning crime and the criminal justice system, as well as the relationship of those attitudes to the experience of victimization, the surveys could help to dispel the ignorance, misunderstanding, and irrational fear that now so often characterize public debate and discussion of crime.

The scientific utility of victimization surveys

For the social analyst, a continuing series of victimization surveys at a national level could provide a rich resource of data. Each survey in such a series could be used as a cross-sectional testing ground for criminological theories (if victimization data were to be supplemented with other behavioral and attitudinal data). In addition, if the survey were a continuing one providing annual data over a period of years, it could be used along with other time-series data in longitudinal studies. Finally, if the series used a panel design, it would be possible to use it to study the consequences of criminal victimization. A continuing series of victimization surveys could yield data for testing theories about societal reaction to crime. It would probably have little to say about the microsociology of interpersonal violence and nothing whatever to say about victimless crime. But the NCS has already pointed to the existence of some criminological phenomena—such as series victimization—for which new theoretical approaches may be needed.

More importantly, the victimization survey makes possible for the first time an adequate test of a whole range of social theories which have attempted to relate crime to the social structure, to culture, to class and class conflict, to economic conditions, or to deterrence. Until now, the only possible test of many of these theories has been official statistics such as the UCR. But, leaving aside their other characteristics, such statistics are a function not only of crime, but also of the working of the system of social control: They thus confound the relationships that theorists have wished to isolate for study. Victimization surveys, which can provide separate measures of crime and of societal response to it, can overcome this limitation. A continuing national survey would thus open the way to an extensive program of retesting of discarded theories and a reexamination of many received truths.

Origins of the present NCS objectives

The original impetus for the NCS came from the President's Commission on Law Enforcement and Administration of Justice, which in 1966 commissioned the first victimization surveys ever carried out. The commission was aware that official statistics of crime in the United States were unsatisfactory, in part because of offenses that were never reported to the police and in part because of wide variations in the recording by police of offenses that were reported to them or known by them. Reviewing the findings of its own surveys, the commission expressed its belief that "the [victimization] survey technique has a great untapped potential as a method for providing additional information about the nature and extent of our crime problem. . . ." (p. 22). The commission also pointed out that:

> What is needed to answer questions about the volume and trend of crime satisfactorily are a number of different crime indicators showing trends over a period of time to supplement the improved reporting by police agencies. The commission experimented with the development of public surveys of victims of crime and feels this can become a useful supplementary yardstick (p. 31).

Agreement with this statement, coupled with the Commission's criticism of the UCR, appears to have established a primary general goal for the NCS: namely, the provision of a "supplementary yardstick" that would merely "calibrate" the UCR. By implication, it seems to have been generally assumed that such calibration would make it possible to use police statistics (in particular, the UCR) as a basis for inferences about some "true" volume of crime. For example, if it could be shown that only 10 percent of all thefts reported to interviewers were recorded in the UCR, then the true level of theft could be obtained simply by multiplying the UCR figure by a factor of 10.

The panel believes that this emphasis on correcting police statistics has been extreme and that it has seriously restricted the possible uses of the victimization survey as a method of studying crime and societal reaction to it.

It can be argued that official statistics on crime, whether compiled by the police, the courts, or any other administrative agency, can never provide a definitive measure of crime. For one thing, such statistics necessarily exclude a great many types of crime. Many of the defects of the UCR to which the President's Commission pointed refer to the limited scope of those statistics: for example, to the fact that the UCR does not include the great bulk of so-called organized crime (gambling, drug trafficking) or "white-collar" crime such as price-fixing, tax evasion, consumer fraud, and political corruption. Even with respect to those types of crime that they include, however, official statistics are necessarily an imperfect measure. This is so because they are the outcome of a complex series of social and organizational processes, varying over time and place, each one of which almost certainly introduces substantial systematic biases into the statistics. Thus, in order to be recorded in the UCR, a crime must (at a minimum) be perceived by the victim or by someone else; it must be defined as a crime by the victim or observer; and, it must in some way become known to the police, it must be defined by the police as a crime, and it must be recorded by the police. At each step in the process, some crimes are excluded (and, perhaps, some noncrimes are included); and it is clear that those crimes that are finally included among administrative statistics are very unlikely to be representative of, or easily related to, the total number of events that might carry legal sanctions within or across jurisdictional boundaries.

This is not, of course, an argument against the compilation, by the police or indeed any other agency, of statistics relating to crime as defined by that agency. Many more statistics are collected now by the police than are published, and these statistics have many operational and administrative uses, even if they are not perfect indicators of the volume of crime in the United States. Nor is it to say that victimization surveys like the NCS have no role to play in complementing or supplementing police or other official agency statistics. It is to say that for most types of offenses, police statistics never can be expected to provide valid measures of the "true" levels of crime in the United States, no matter how much they might be supplemented (or calibrated) by victimization surveys. It follows that such supplementation should not necessarily be the primary objective of the NCS in the future.

Description and explanation

One way to characterize the objectives and utility of a survey like the NCS—and thus to improve its design—would be to consider the possible users of the data produced by the survey. In this instance, this would mean distinguishing between routine or special publications

*Excerpted with editorial modification from Chapters 8 and 9 of Surveying Crime. Washington, D.C.: National Research Council, National Academy of Sciences, 1976 (ed. Bettye K. Penick).

by the Census Bureau and LEAA on the one hand, and public-use data tapes on the other; or, one might distinguish among the possible needs of legislators, law enforcement and criminal justice planning personnel, the general public, and academic or institutional researchers.

A different, and perhaps more fundamental, distinction can be made: This is the distinction, drawn by some students of research methodology, between (a) surveys that are intended merely to measure or describe certain phenomena, and (b) surveys that are intended to explain or analyze those phenomena. Briefly, the main aim of surveys in the first of these two categories is simply to provide information on particular attributes of a population: to discover, for example, how many people were unemployed last week, how much people spend on food or entertainment, or how many people intend to vote for Candidate X. If, as is usual, such a survey is based on a sample, the main inferences made in analyzing the data involve the estimation of population parameters (for the attributes under investigation). A survey that aims at analysis or explanation, on the other hand, is intended to discover and make understandable relationships among factors such as employment, expenditure, and voting behavior, or between those factors and others that may influence them. Analysis of the sample data in the latter case typically involves computation of functions relating two or more variables and not simply the production of univariate population estimates. Moreover, the sample may be chosen to depart from uniform sampling probabilities, in accordance with an experimental design or with analytic goals that require more than the simple proportional representation of subgroups within the population of interest.

These two sets of survey objectives—the descriptive and the analytic—are not, of course, mutually exclusive. Even the simplest of "fact-finding" surveys will usually collect some other information about respondents—for instance, demographic data such as the age and sex of respondents. These data can be regarded as independent variables and can be analyzed in relation to data on, for example, unemployment or education. Such an analysis, by accounting for some of the observed population variance in the latter measures, can have some explanatory force. Perhaps it would be more accurate to consider description and analysis as the endpoints on a continuum, with most actual surveys falling in between the two. But the two goals, nonetheless, may have radically different implications, not only for sample design and methods of data tabulation, but also for the choice of variables to be investigated and for ways in which those variables are measured.

Where, on this continuum, should the NCS in its present form be placed? There can be no doubt that the NCS, as it is at present being conducted, is primarily descriptive rather than analytic in character. An earlier document produced by the forerunner to National Criminal Justice Information and Statistics Service (NCJISS), for example, states that the primary purpose of a national victimization survey would be "to measure the annual change in crime incidence for a limited set of major crimes and to characterize some of the socioeconomic aspects of both the reported events and their victims." The same document referred to providing "a reliable statistical series on the amount of dangerous crime in the United States and the rate of victim experience. . . ." In a similar vein, Turner and Dodge quote the President's (1967) Crime Commission as claiming, "Statistical indicators as comprehensive as the ones reported by the federal government in labor and agricultural statistics could be achieved with victimization survey findings used in combination with data from the Uniform Crime Reports."

Many things follow from this. Consider, for example, the current NCS design for the production of quarterly data: an emphasis that appears to have dictated the choice of a 6-month reference for the NCS, though empirical evidence on recall and telescoping does not clearly justify the cost of that choice. The production of quarterly data is understandable if the aim of the survey is merely to measure criminal victimization, but is difficult to understand from an explanatory or analytic point of view except, perhaps, in connection with questions concerning seasonal variation. Similarly, the sampling design of the NCS may be reasonable if the objective of the survey is solely to measure the general population's victimization experience; but, for the purpose of accounting for variation in experiences with phenomena as rare as the crimes surveyed in the NCS, a very different sample design—possibly with differential sampling among strata determined by variables known or believed to be related to victimization— might be more efficient. Again, the choice of victimization rates per thousand persons as the main statistic may be defended perhaps from a purely descriptive point of view; although, as argued earlier, it is in fact severely narrow even for that aim. By itself, the rate makes little sense if what is sought is an explanation of the distribution of criminal victimization.

At a more basic level, the objectives of a survey, and the relative emphasis on measurement or description versus analysis or explanation, will determine the questions to be asked of survey respondents. A descriptive orientation can be clearly seen in the extremely limited number of independent or explanatory variables on which the NCS now collects information. If we reflect on the major sociological theories offered as explanations of crime over the past half century—from "ecological" and culture-conflict theories, through differential association, anomie, and subcultural theory, to the most recent adaptations of economic theory to criminal behavior—it is clear that there are few, if any, variables now incorporated in the NCS questionnaires that bear on those theories, except through crude and extremely uncertain post hoc "operational" definition. To be sure, most of those theories have paid scant attention to the possible roles of victims or potential victims in the causation of crime; in any case, it is unlikely that an elaborate program of surveys like the NCS could ever be justified solely on the basis of testing abstract and sometimes recondite academic theories.

There are a number of important factors of general explanatory values, however, on which the NCS could easily obtain valuable information that is not now available elsewhere. Most of these factors can be subsumed under the related concepts of vulnerability and risk. Some examples can be found in Reppetto's recent study of residential burglary.* In addition to obtaining, from Boston police records, residential robbery and burglary rates, Reppetto collected data on two sets of factors that were intended to explain those rates. The first, which he called "environmental factors," included attributes of neighborhoods, such as geographic location in relation to the town center, median income, predominant housing type, racial composition, size of youth population, and burglary rates in surrounding areas. In addition, he obtained data on attributes of particular households and dwellings within neighborhoods, such as daytime occupancy of the house, type of structure, security practices, and ease of access to the building. Some of the first group of factors may be studied in the NCS when data files containing the Census Bureau neighborhood characteristics are available, though even then it appears that such things as the income level and victimization rates of surrounding areas may be unavailable. Most of the items in Reppetto's second category are not collected by the NCS at present, although they could be.

Analogous factors relating to vulnerability and risk can easily be thought of for other crimes, such as robbery and assault. We acknowledged earlier that it might be difficult to obtain information about certain prior associations between victims and offenders. Nonetheless, the social location of many incidents surely could be determined with more precision than is now the case. Did a robbery or assault occur in a place of public resort, such as a bar, restaurant, or theater? Did it occur in a private locale, such as the victim's own home or the home of a friend? Did it occur on a public thoroughfare, or on public transportation, such as a bus or subway? Did it occur at the victim's place of employment? None of these questions can now be answered by the NCS. Yet distinctions of this kind could do much to clarify the degrees of risk attaching to locales characterized by different degrees of privacy or protection, and—together with data on different groups' access to such locales—could do much to explain variations in victimization in the general population.

We believe that an assessment of risk should be a primary objective of the national victimization survey, and we offer in historical support the following statement from the President's 1966 Commission:

> The Commission believes that there is a clear public responsibility to keep citizens fully informed of the facts about (violent) crime so they will have facts to go on when they decide what the risks are and what kinds and amounts of precautionary measures they should take (p. 52).

Victimization surveys are concerned not only with crime but also with societal reaction to it. Here, too, there are many important issues awaiting explanatory research. Why do some people report (some) crimes, whereas others do not? To what extent is a victim's decision to call the police "incident-specific," rather than being a consequence of his or her general attitudes toward the police and other agencies of social control? What are the factors that lead some social groups to define certain sorts of behavior as criminal (or at least as deviant), while other groups regard those same forms of behavior as permissible or even mandatory? To what extent are people's attitudes toward the police, the courts, and the rest of the criminal justice system a consequence of their contacts with that system as victims of crime?

Finally, there is a wide range of issues that could be explored by a survey like the NCS, concerning the impact of crime on the community. Why, for example, does there appear to be little, if any, relationship between expressed concern about crime and direct experience of victimization? To what extent is fear of crime a consequence of physical and/or social vulnerability (e.g., old age, forced reliance on public transportation) and to what extent does it flow from the images of crime presented by the media of mass communication?

*Thomas A. Reppetto. *Residential Crime*. Cambridge, Mass.: Ballinger Publishing Co., 1974.

A managerial perspective on the redesign of the National Crime Survey*

by Benjamin H. Renshaw

Efforts to alleviate the trauma, injury, expense, and inconvenience of being a victim of crime has been a major focus for the Law Enforcement Assistance Administration (LEAA) and its intergovernmental delivery system in the last 5 years. Programs to assist the victims such as crisis handling, social service referrals, special aid associated with particular classes of victims, and State victim compensation programs have all emerged. At the national level, Congress is considering national victim assistance and compensation legislation; as an example, the Senate recently passed a bill to provide $30 million in aid to victims of spouse and child abuse.

It is interesting that this concern and resulting legislation appear to be more an effect than a cause of the National Crime Survey—which refers to the national survey of victimizations initiated in 1972 by LEAA and conducted by the Bureau of the Census to gain an understanding of the incidence and impact of crime. If a national statistical series has indeed focused concern on the plight of victims, this is a vitally important benefit of the enterprise.

Moreover it is important to understand that these service/assistance programs and the victimization series both have been undertaken under the statutory umbrella of a law intended to improve systems for the administration of justice at State and local levels. This fact has implications for the redesign and reform of the NCS and for the policy guidelines under which that redesign will proceed.

The issue or problem addressed by this paper is the management of a total reexamination of the National Crime Survey in the context of the policy issues, legislative interests, and program development needs of the Department of Justice. We are not seeking an "ideal" or "optimum" survey; there is no theory for an optimum sampling design. We are seeking methods of acquiring victimization data that take cognizance of benefits and uses, sources of error, present and potential funding levels of the sponsoring agency, and staffing constraints.

The NCS redesign will have to be undertaken in the context of our current statute and to meet the statistical policy needs of the Department of Justice and the Law Enforcement Assistance Administration. My formulation of these policy guidelines follows; clearly these policy rules are subject to review and change within LEAA as the work on the major procurement proceeds.

1. Release and analysis of victimization data must be considered in conjunction with the Federal Bureau of Investigation's Uniform Crime Reports. It seems both possible and essential that aggregate statistics from these two series be presented as part of an overall national report on crime for a common time period.

2. Derivative from the above, victimization data derived from the national household sample will be reported and released for each calendar year within 9 to 10 months of the close of the year for which the report is being made. Data for time periods of less than a year, such as quarterly estimates, are not a requirement for policy-based research, and their collection demonstrably and sharply increases costs.

3. Any methods or instrument used for collection of victimization data must take into consideration what the National Academy of Sciences report called the independent variable problem. Data on risks associated with victimization and lifestyle issues must be dealt with if the enormous policy utility of the NCS data is to be extracted. Currently the victimization data tells us who is victimized but not why nor the efficacy of things people do to avoid and prevent victimization.

4. Work on reexamination of the NCS should proceed on the assumption that there will be no quantum or even major increases in the LEAA staff dedicated to the management of the victimization data collection, analysis, research, and local technical assistance efforts. Initial recommended appropriation levels for the proposed Bureau of Justice Statistics suggests that these staffing constraints will hold well into the 1980's.

5. All policy, program, and research objectives stipulated by LEAA as the sponsoring agency shall be met. If no single design, method, or alternative for acquiring such data can bear the burden of multiple objectives, then separate but coordinated alternatives must be developed. Consideration may have to be given to a cross-sectional design for annual reporting and a longitudinal design to get data on the incidence and impact of victimizations.

6. Substantive objectives, in addition to those already mentioned, will include (a) annual reporting on a national basis of levels and changes for major crime types, data on the attributes of crime, and factors related to the victimization experience; and (b) subnational estimates of the same phenomena for a range of larger States, SMSA's, and other subnational areas that may be identified.

7. Methodological "givens" include the following: (a) any survey design used shall be sufficiently flexible to permit the incorporation of short-term policy and attitude questions on either a national or subnational basis; and (b) all research and design work and products with relation to victimization alternatives shall be available for release by LEAA, through the National Criminal Justice Reference Service and other means, subject only to LEAA's privacy, security, and confidentiality restrictions. The NCS redesign work precludes one definitive test of the data and must be conducted in an open and iterative fashion; rapid release of the data is imperative to facilitate secondary analysis. LEAA, as an example, intends to enforce basis standards for preparation and release of data tapes.

*Excerpted with editorial modification from a paper presented to the annual meeting of the American Statistical Association, San Diego, Calif., 1978. It is published in full in the proceedings of that meeting.

Organizational tensions in the management of statistical enterprises

There are two critical tensions in the management of any statistical enterprise which have a bearing on the future of the National Crime Survey.

On one side there is the absolute need to maintain the objectivity and the integrity of the data series, but without isolating the statistical staff from the policy apparatus of the Department of Justice that plays a critical role in perpetuating the series.

The second tension is for the Department of Justice and LEAA staff concerned with the victimization series to be informed by the best available statistical talent without shifting the fundamental policy responsibility of elected and appointed officials for policy direction to that advisory body.

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