

METHODOLOGICAL FOUNDATIONS FOR ESTABLISHING A NATIONAL SURVEY OF VICTIMIZATION

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

The Statistics Division of LEAA hopes to provide such data by establishing a National Crime Survey Panel 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 Crime Panel 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 the number 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 socio-economic 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 Crime Panel 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 crimes.

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 they were invaluable as pioneering efforts from the standpoint of suggesting several methodological questions for Census and LEAA to address in their pilot tests. The only national survey ever undertaken was the National Opinion Research Center 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 National Crime Commission during the mid-60'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 which 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 Census 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, though 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 Census 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 in 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 incidents 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 which 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 which 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 which, 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, 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 and Baltimore, victims of four major crimes were selected--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 by-product 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:

- a. If the objective is to determine whether a crime occurred, as opposed to placing it in a more accurate time frame, 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 which derives from time telescoping can be largely corrected by providing interviewers with bounding information, that is, the record of incidents from the previous interview. The plans for the National Crime Panel contemplate a substantial degree of overlap in sample addresses from one collection period to the next--in the neighborhood of 75-80 percent.
- b. To the extent that it is desirable to place an incident in a specific time frame, 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.
- c. 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 (2/3 and 1/2, 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, result 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 it 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 which purport to measure the incidence of crime would inevitably be compared with crimes known to and reported by the police, issued regularly in the FBI's Uniform Crime Reports. 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 which 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 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 a "middle way" between a brief screen consisting of, say, 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 which 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 "catch-all" 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 which he thought was a crime, and, second, if anything else happened to him which 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 which are the focus of our studies--for example, vandalism,

peeping toms, etc.--and also reports of non-crimes. However, they have also yielded descriptions of events which 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 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, interviewers were instructed to fill an incident report on each situation where the crime reported in the two catch-all 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 catch-all questions. However, there were a number of other reports of one of the five crimes which 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 Surveys indicated that as many as 5 percent of all incidents that qualified as one of the 5 crimes were reported in these two catch-all questions.

Self-Respondent vs. Household Respondent

Another methodological problem of significance in establishing a National Crime Panel 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 more often than not be the housewife. This respondent would report for himself 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 he reports himself, 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 pre-designation 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 which 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 himself and all other 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 which 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 for himself, 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 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 some 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, that 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 be giving 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 which we would otherwise miss altogether. (See Reference 3.)

Age of Respondent

A problem which we feel is related to the type of respondent is that of the appropriate minimum age. The LEAA surveys to date have used age 16 as the minimum age for which victim data are sought. Sixteen is the age now 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 Quarterly Household Survey of Victims of Crime. In New York, Chicago, Los Angeles, Detroit, and Washington, 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 which will have a bearing on the selection of the type of respondent for the National Crime Survey.

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 National Crime Panel have included the use of the Census Bureau's Quarterly Household Survey 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 six 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, an indication that there were no incidents or that the household was not interviewed 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 sometime next spring.

Commercial and Other Institutional Victimization

In addition to the methods testing that has been going on in the household sector for crime measurement, some work has also been undertaken to assess the feasibility of using crime victimization surveys in commercial establishments and

other institutions.

In late spring of 1970 a commercial victimization pretest was conducted for LEAA by the Census Bureau. The survey took place in Cleveland and Akron, Ohio, with a general probability sample of about 500 business establishments. The methodology employed for the Cleveland-Akron test could be the subject of a paper in its own right. Briefly, however, the objectives were to determine the degree to which businesses keep written records of crime incidence and their losses due to crime, to test questionnaire wording and format, and to examine alternative reference periods for recalling crimes.

One of the findings of the Cleveland-Akron experiment indicates that it is not feasible to rely on existing written records maintained by businesses for estimating crime incidence. Only about half the businesses that were crime victims stated they kept written records of those crimes. Curiously, a higher proportion of non-victims stated they would keep records if victimized.

Another significant methodological finding was that commercial establishments have very little documentation on the amount of inventory shrinkage due to employee theft or to shoplifting, both of which are forms of larceny.

The evidence on reference period matches the findings of other researchers as well as other Census-LEAA efforts, namely that proportionately more incidents are reported for a recent period than for a distant one; and more so than can reasonably be accounted for by seasonal fluctuation. Additional information on the problem of reference period will be available, however, from a carefully designed reverse record check study in Dayton, Ohio. This study used a sample of several hundred known commercial victims taken from police reports. These victims were subsequently followed up for personal interview. The results are being compiled and should be available before the end of 1971.

Besides the commercial victimization methods tests, we have also conducted some research on record-keeping practices in governmental institutions and offices. There again, the findings indicate that except for public school systems, government organizations do not keep adequate records for survey uses. Another experiment is underway to test the feasibility of using a diary approach for recording crimes among a sample of government offices.

Recommendations for Future Methods Tests

In the course of working with the various test 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 National Crime Panel, 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 the Warner 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).
- (6) Further exploratory work associated with the measurement problem of assessing the amount of certain types of commercial crime, such as employee theft, shoplifting, shipping fraud, embezzlement, vandalism, arson, and bomb damage.

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 panel, but in so doing, we recognize that much remains to be learned.

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