SOME RESPONSE EFFECTS OF THE NATIONAL CRIME SURVEY

Robert G. Lehnen, Indiana University and Albert J. Reiss, Jr., Yale University

The use of complex surveys to record and estimate socially relevant information is increasingly common among government and industry. Among the newest of large scale sample surveys is the National Crime Survey (NCS) a survey of victimization sponsored by the Law Enforcement Assistance Administration and conducted by the Bureau of the Census through interagency agreement. NCS estimates the rate of criminal victimization from the serious crimes of rape, robbery, assault, burglary, larceny, and motor vehicle theft. The survey was designed during the period 1970-1972, and has been operational since July, 1972. The primary concern of the early days of NCS was with the design of a suitable survey instrument and with traditional statistical objectives such as sampling efficiency and point estimation.

As experience with the current NCS design accumulates, attention now focuses on the variation introduced into NCS estimates from non-sampling error. A recent report on the methodological aspects of NCS, Surveying Crime, (National Research Council, 1976), focuses substantial attention on this problem. Response error is one source of non-sampling error that received considerable attention, (NRC, 1976: 62-80).

In an earlier paper, we explored various features of the NCS panel field survey design as sources of non-sampling error. We specifically examined variation in the number of prior interviews, the number of victimizations previously reported, and the medium of interview (whether in-person or by telephone) as sources of error on survey reporting of victimizations (Lehnen and Reiss, 1978). We extend this discussion in this paper by examining some of the conceptual, methodological, and analytical difficulties inherent in the NCS design and explore their implications for acquiring a suitable estimate of the degree to which response errors affect current estimates of victimization rates.

Basic Design of the NCS

The basic sampling design of the NCS is a stratified multistage cluster sample divided into six rotation groups. Each rotation group is a systematic one-sixth sample of the eligible housing units as well as a one-sixth sample interview each month (Bureau of the Census, 1976: (1)1-2). Not until January 1, 1977 was a complete rotation design reached where each rotation group is interviewed once every six months for three years, or seven interviews.

The rotation group sampling design was selected to stabilize the estimates of victimization from one period to the next, to bound the reporting of victimizations for any given period of time, and to reduce the cost of the field survey. Only six of the seven interviews in a balanced design are used for estimation since the initial interview is used only to bound the period of victimization reporting, i.e., to eliminate duplicate reports of crime incidents beyond the six month recall period.

By design, then, each successive interview is to be bounded by the previous one to reduce error arising from telescoping incidents that occur before, into the period of estimation. In executing the survey design, only about 82 percent of all household and person interviews are actually bounded; from 17 to 19 percent, depending upon the interview period, are actually unbounded (Reiss, 1977: 8). These unbounded interviews are primarily from persons and households that move into a housing unit previously in sample. But unbounded interviews also come from other sources. Some are from persons or households that are in-sample but not interviewed in the preceding six months. A small proportion are from housing units added to update sampling lists.

Bounding status has a significant impact on the estimated victimization rate. The amount of error that is introduced by the inclusion of unbound interviews in estimating the victimization rate is probably considerable since only two-thirds of all reported crime victimizations come from the 82 percent of all bounded interviews (Reiss, 1977: 9). The higher rate of victimization reported in unbounded interviews is traceable in large part to the combined effect of persons and households moving into locations having substantially higher victimization rates than do non-movers and the error arising from telescoping victimizations into unbounded interviews. Clearly, the current Bureau of the Census procedures for estimating victimization rates over-estimates the rates by including substantial numbers of victimizations from actual unbounded interviews.

The basic data collection instrument of the NCS is the Basic Screen Questionnaire that is administered to a household respondent and all members 14 years of age and older.[1] This questionnaire includes questions about household and respondent attributes and "screen questions" to identify whether the household or respondent has been victimized. Whenever any screen question identifies a potential victim of crime, the interviewer administers the Crime Incident Report for each incident identified. The Crime Incident Report is a description of the crime event and

[1] Information about each household member aged 12 and 13 is obtained by a proxy respondent who is either the household respondent or some other knowledgable household member. Proxies are also obtained when a particular respondent is physically or mentally unable to answer the individual questions or if a household member 14 or older is temporarily absent and is not expected to return before the ennumeration closeout date (Bureau of the Census, 1976: (1) 5-2).

any action taken by the respondent or others. In the victimization survey, a respondent is naive about the Crime Incident Report until at least one incident is reported on the Basic Screen Questionnaire.

Response Errors

The phrase "response error" is sufficiently vague to warrant clarification. We use the phrase response error to include all sources of variation introduced into the observation of victimization phenomena by the social mechanism used to measure, record, and process the information. Most response errors arise from the interaction of respondent and interviewer within the behavior context of an "interview." Variation, for example, may be induced into the observation because of status differences between interviewer and respondent or because of the medium of interview -- by telephone, in person, or by self administration. Although these factors may contribute to overall error in estimating victimization rates from sample surveys, one attribute of the NCS design, its repeated measurement of the same respondent, appears to be a principal source of response error (Lehnen and Reiss, 1978).

Our previous research suggests that reporting of incidents declines substantially as a function of the number of interviews, or in other words, because respondent "resistance" or "fatigue" is present. It is difficult to give a precise estimate of the response effect of repeated measurement because there are two other competing hypotheses that adequately explain this finding. One alternative explanation derives from the problems associated with estimating the victimization rate based on reports of respondents who remain or come in as replacements in the survey versus those who leave. The other explanation suggests that the fall-off in reporting reflects actual changes in victimization experiences.

While both hypotheses are discussed below, explanations of non-sampling sources of variation in survey estimates of crime victimization must also be viewed within the perspective of the victimization survey as a socially organized means of knowing about crime. Generally speaking there are two opposing views about estimating the crime rate. On the one hand, there are the realists who contend that there is an actual amount of crime that takes place in any period of time and that it is knowable. Aligned against them are the institutionalists -- or legal realists -- who contend that crime is known only by some institutionally prescribed criteria and agency actions based on them. Crime is what the police, prosecutors, or courts do. Neither posture seems a reasonable one since we can know only by institutionally and socially organized means of knowing (Biderman & Reiss, 1967: 2). There is, then, no way of knowing a "true rate" of crime. Our understanding of the occurrence of crime is enhanced, however, by comparing institutionally organized means of estimating crime rates and their error structure.

The victimization survey is but one institutionally organized means for estimating the crime rate. Ultimately its utility must be assessed by comparing its accuracy and coverage with those of other institutionally organized means, e.g., offender surveys of offending, or official agency statistics such as those derived by procedures for acquiring information and reporting offenses known to the police. The test of the hypotheses we propose is limited, of necessity, to a test within the confines of the victim survey. But one or two examples may serve to illustrate possible limits of the survey means of estimating crime rates.

The victim survey is a count of household and person victimizations not a count of offenses or events and their verification. Moreover only an event against persons can give more than one victim. The design of the survey requires the recall of victimizations as point-in-time events. This means that not only is there error associated with recall, but that respondents may be unable to recall, unwilling to report, or actually do not experience some events as discrete point-in-time events. The conceptualization and reporting of some of these victimizations as "series" events of victimization, however, derives in part from the socially organized means of knowing in the victimization survey -- by means designed to recall specific crime incidents and their occurrence. When the respondent is unable to recall experiences with crime in terms of details that separate their recall as discrete or point-in-time events, the reports are classified as series victimizations but the respondent is asked to estimate the number of discrete events, making no allowance for the possibility of conceptualizing them as chronic or continuing events.

To illustrate with the second example, the victim survey relies upon assumptions that some events are household victimizations while others are person victimizations; the former can be elicited from any, but need be from only one member of household, while the latter must be elicited from each person as an experience of victimization. By contrast, police agency statistics on crime events begin with their mobilization to situations and their potential reporters. Offenders as well as victims and any complainants or witnesses become multiple sources about some events. On the average the law enforcement agency survey takes place closer to the time of occurrence of the event than does the victimization survey.

Methodological Problems and NCS

A number of methodological problems affect response to the victim survey. Although this list is potentially quite long, two problems specific to the NCS deserve particular attention: (1) problems arising from the repeated measures design, and (2) problems arising from the treatment of series incidents.

The first and probably most troublesome is the mover-stayer replacement problem. A housing unit is the basic sampling unit of the NCS so that repeated measures of crime victimization

are obtained for whichever members of a household or whichever household is resident in the sample housing unit at the time the survey interview is taken. Because persons and households move from sampled housing units, the initial sample receiving the bounding interview changes dramatically by the time it reaches full maturity at the seventh interview. The change can be observed indirectly by the differences in reporting of incidents between the first-timers and the seventh-timers. Although there is the possibility that observed changes in incident reporting represent adaptations in behavior which result in lower changes of victimization for the stayers, one must suspect that movers are different from stayers in their risk of victimization and that the decision to move must be associated directly or indirectly with victimization experience. Reiss (1977a) found that residential mobility increases with both the seriousness and amount of victimization by crime. Persons reporting four of more victimizations within a six month period were three times as likely to move as were those reporting only a single victimization.

Theoretically, this design assumes that changes in housing occupancy and the victim proneness of residents have no effect on estimates of the aggregate crime rate at any point in time, even under the condition of complete turn-over of residents at all sampled housing units from one estimation period to the next. Apart from assuming that in the aggregate household that move into vacated housing units have victimization rates similar to those that left, the design assumes that the crime opportunities associated with locations of housing units will have the same effect in the aggregate on those who move in as on those who move out. This is so because the design hold constant crime opportunities in-so-far as they are associated with housing units and their residential location while allowing those associated with residents to vary.

A longitudinal design alternative to the present one might follow residents of households as they move. Theoretically, this design assumes that some persons or households change their risk of victimization by moving to locations with different crime opportunities and/or that residents who move have different propensities for victimization than those who stay or replace them. By following residents, crime opportunities associated with a particular set of residential locations are allowed to vary while the risks of victimization associated with particular persons and their household remains more or less constant for their time in sample. Indeed this design allows for the possibility that over time even if all originally sampled housing units are replaced, the crime rate can be accurately estimated.

Yet neither design permits us to separate the effect of crime opportunities associated with residential location from those related to victim proneness and changes in residence. As a minimum, a design must examine the effects associated with both replacement and moving households. The current design of the NCS unfortunately permits us to compare only the rates of move-in with move-out households at different points in time. But, replacement households, families that move into the sampled housing units, are not similar to the movers with respect to reporting victimizations. The exact nature of these differences are now being estimated, but the difficulties of classifying respondents into "mover," "stayer," or "replacement" prevent a more complete discussion here.

A second aspect of the repeated measures design is the household composition problem. Unlike the mover-stayer problem where the entire household membership changes, the household composition problem arises from partial changes in membership. One can assume that the composition of the household affects its risk potential, and that a household with several teenaged male residents, a victim prone group, is more at risk than a household composed of an elderly couple. During the course of the three years that a household remains in the sample, major changes in composition can occur. In the case of the household with teenagers, they are likely to take up a new residence for employment, education, or marital reasons, thus drastically changing the victimization potential of the household at a sample housing unit. It is unlikely that these changes are offsetting, in the sense that other households have teenagers who come of age to offset the "move-outs." If so, then one can expect some miscounting of victimization, probably in the direction of undercounting, as a rotation group matures. In sum a second artifact of the repeated measures design, changing household composition, probably affects estimates.

A third aspect of the repeated measures design is the multiple exposure to stimuli problem. In another research paper (Lehnen and Reiss, 1978) we estimated the effect of repeated exposure of the same interview instrument on reports of victimization. This research suggested that there is a substantial decline in reporting victimization as a function of the number of previous exposures to the questionnaire. This finding though consistent with other research on panel or repeated measurement designs, is compromised by the failure to allow for the mover-stayer-replacement effects described above. Thus, the estimated decline in reporting may be due entirely to changes in sample composition, changes in respondents[2] victimization experience, or a "fatigue" effect, as we expected.

In addition to the repeated measures aspect of NCS, the other unique source of response error in NCS is the problem of "series" reports. Ideally, the NCS questionnaire should record each victimization incident as a discrete event; practically respondents sometimes find it difficult to recall the discrete events, especially where there are

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[2] Housing units are removed from the sample when demolished; periodically new housing units are added to enable persons occupying housing constructed after 1970 to be properly represented in the survey (LEAA, 1977: 97-98).

multiple occurrences of a similar nature. A teenager who routinely has his lunch money stolen will have difficulty recalling specific events. A wife who is often assaulted by her husband may not remember the exact details of each beating. In these situations, NCS interviewers record a "series" reported, defined as an incident report for 3 or more similar incidents-assaults, thefts, and so on. The interviewer records the similarities of the events and obtains an estimate of the number of occurrences.

Besides the technical aspect of how series are counted in the published estimates of victimization by crime -- they are not -- the main issue is variability in use of series reporting by interviewers. There is some suggestion that some interviewers are more prone to use series reports and that the content on some series reports could have been recorded as separate incidents. Aside from these difficulties, Reiss (1977b) has discovered that series reports usually appear during the first interviews administered to a rotation group, and that respondents who initially reported series incidents are unlikely to report any other incidents, series or otherwise, during the remainder of their interview experience. This finding suggests either that respondents are no longer being victimized or that they have altered their response behavior, a matter requiring further inquiry.

Conclusion

We have called attention to a number of non-sampling errors that are response effects arising from the current NCS design. Although we have called attention to both further inquiry and changes in design that will permit estimates of these sources of error, it seems clear that a total survey approach where the precise estimation of each of these non-sampling sources of error is built into the design is what is required. The time has come for survey analysts to recognize that the sampling sources of error may be trivial compared with those generated by the design.

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