A QUASI-EXPERIMENTAL ANALYSIS OF PERSONAL VICTIMIZATION REPORTING BY HOUSEHOLD RESPONDENTS IN THE NATIONAL CRIME SURVEY (1)

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Introduction

The National Crime Survey (NCS) has collected data on crime victimization since 1972 from a continuing, rotating-panel survey of U.S. housing units. Interviews are attempted once every six months over a period of three years with all of-age occupants of each in-sample unit. (U.S. Department of Justice, 1981: Appendix III.) A program for redesign of the NCS begun in 1979 is directed towards a reexamination of its current design and data structures. Here, we report results of the program's inquiry into NCS procedures for data collection, and for crime event classification and counting, that involve distinctions between "Personal" and "Household" Crimes.

We will show in this paper that rates of victimizations treated by the NCS as crimes against an individual person are increased greatly when respondents (R) are designated as household respondent (HR) to answer questions about their household, including questions about those victimizations that the NCS treats as crimes against the household organization. (2)

Our major purpose is to call attention to serious misinterpretations of NCS data which can, and have, resulted from the different procedures used with HR and those Rs not so designated (NHR). These procedures elicit information about victimizations and attribute them to a person or to the household, collectively. Some ready correctives to these procedural biases are suggested to reduce some of the resulting error, as are fuller remedies that require major conceptual and procedural changes of the survey. This paper also exemplifies quasi-experimental use of the longitudinal data afforded by a panel design, such as the NCS, for the identification of procedural bias in surveys.

Direct And Constructed Survey Indicators

A primitive form of survey indicator operationalization employs identical words and concepts (constructs) for the questions put to Rs and the statistical indicator yielded. As surveys become progressively more sophisticated and the use of their data more refined, such direct indicators come to be progressively less tolerable. Concepts must rather be operationalized indirectly by indicators constructed from answers to more complex questions, more questions, or both. The concepts "crime," "victimization," "victim," and those of particular categories and subcategories of crime pertinent to the NCS, prove unamenable to simple, direct operationalization (Biderman et al., 1966; Biderman and Reiss, 1967). The current NCS design is affected, however, by residues of thought and procedure from survey practice that attempts direct operationalization of indicator concepts.

The designers of the NCS "...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 R, explicitly mentioning a multitude of examples of the kinds of property that might have been stolen or the kinds of situation in which he might have been the victim of a personal crime." (Dodge and Turner, 1971:10). A Crime Incident Report, administered after the screen questions, contains the detailed questioning necessary for establishing that a victimization mentioned in response to screen questions is within the survey's scope and for classifying the crime for counting and analytic purposes. Crime counts and classifications are based exclusively on the data from incident reports. Screen questions aimed at one type of crime may yield incident reports that come to be classified as some other type of crime.

Treatment of Households and Individuals

NCS interviewers are told to designate as HR that member of the household who is "most knowledgeable...the one who appears to know--who could be reasonably expected to know--the answers to the household questions, including the screener questions" (U.S. Bureau of the Census, 1975: Section D[1],5-1). All members of the household 14 years of age and older are given the Individual Screen Questions (with persons 12 and 13 years of age proxy interviewed). HR always is a dual R, answering both the household and personal screeners. HR may also serve as the proxy R for any members aged 12-13 in the household or older incapacitated members who may be interviewed by proxy.

The designers of the NCS realized that some household victimizations would be yielded by Individual Screen Questions and by Rs who were given only those questions. The NCS provides for accepting such victimizations, although the designers assumed that few of these incidents would not also be reported by a HR. We are more concerned here that Household Screen Questions may also yield many incidents that come to be classified as personal crimes. Rs who were not HR would be "bombarded" by fewer cues that might elicit recollections of personal victimizations.

Household Crimes As Analytic Concepts

The concept "Household Crime," is important to the victimization survey method both from the standpoint of collection methodology and from the standpoint of the analytic measures and units. As the development of the NCS proceeded, however, the analytic distinctions between individual and household crimes departed from those that were applied to collection, e.g., the crime types figuring in the household screen questions were no longer exclusively types treated as "Household Crimes" in analysis. Furthermore, neither the collection nor the analysis set of "Household Crimes" were fully congruent with the concept of a "crime affecting the household as a whole." Household Crimes in NCS analyses, nonetheless, are those attributed to the household as a whole and published victimization rates for these crimes are computed on the base of the U.S. population of households, employing household sample weights. Causal and interpretive analyses of these types of crime and descriptive cross-tabulations employ household characteristics. NCS rates for personal victimizations, on the other hand, are computed on a base of the U.S. population of persons 12 years of age and older, employing person sample weights.

Screen Cues And Classifying Rules

There is imperfect consonance of the types of crime defined by the NCS as Household Crimes for analytic purposes (see Glossary of Terms, U.S. Department of Justice, 1981:107-108) and the referents of the Household Screen Questions (see Appendix II, Survey Instruments, U.S. Department of Justice, 1981:90). Of the six

crime-oriented Household Screen Questions, five are attempts to operationalize directly crimes treated as Household Crimes by the NCS. These questions deal with burglary, actual or attempted, thefts of property from the home or from a temporary residence, and motor vehicle theft. The last of the Household Screen Questions, however, asks about thefts of parts from motor vehicles. These thefts are treated as NCS Household Crimes only if the theft took place while the vehicle was "at home," but the screen question is not qualified as to the place of theft. While the other five questions are more closely restricted to Household Crime definitions in the necessary conditions they incorporate, they do not specify sufficient conditions. The screen questions addressed to residential burglary and to motor vehicle theft come closest. Yet, even in these two cases, if an assault against a household member occurred in the same incident, NCS priority classification rules treat the report as a Personal Crime. For the other three Household Screen Questions, facts beyond those addressed by the question must be established by the Crime Incident Report to determine whether the crime is to be treated as Household or Personal. Not all of the classification criteria are embodied in the wording of any of the questions. For various circumstances, interviewers must also draw upon instructions from an elaborate Interviewer's Manual (U.S. Department of Justice, 1975). Examples of the most general criterion frequently applicable to distinguishing Household from Personal Crimes--whether a property crime occurred "at or near the home"--illustrate how special circumstances may govern the classifications.

To be classed as a Household Crime, a theft has to occur on residential premises occupied by the R's household or, in the case of single-family residences, on the street immediately adjacent to the residence. What would be a Household Crime if a bicycle was stolen from the yard of single-family occupants becomes a crime against the individual owner of the bicycle if it is in the shared yard of a multiple family residence or if it were stolen from any other place. Indeed, if a bicycle belongs to a child under 12-years-of- age, there would be no NCS incident in the latter case but there would be a Household Crime in the former, in that any property stolen "from the home" is defined by the NCS as household property.

Thefts "away from home" without victim-offender contact are treated as Personal Crimes, regardless of whether the property taken may be regarded as belonging to one individual or the household organization. A number of rules apply to cases in which more than one member of the household mentions such an incident, but in many instances, if not most, only the report of the first R mentioning the incident will be taken and that R will be treated as the victim. The HR is always the first household member interviewed. The more R there are in a household, the greater the possibility that a victimization mentioned by a NHR will be deleted as duplicative of a HR report.

Cognitive Differences

Evidence from survey research supports a conclusion that cues and additional questions on the same or related topics increases productivity of responses (Biderman et. al., 1966; Murphy, 1976). Since HR are given more cues, one would expect they would report more and different kinds of victimizations. The specific cues given only to HRs in the NCS not only may yield mentions of Personal Crimes during administration of the household screen, itself, but they may readily continue in working memory to evoke associations and recall when the HR is asked the individual screen questions (Biderman and Moore, 1980). Household Screen Questions, for example, are the only ones to suggest explicitly victimizations at a hotel or while on vacation, family outing, or business travel, or thefts of bicycles.

Analyses of Respondent Status Effects

Hindelang, Gottfredson and Garofalo (1978: 142-143) on finding that HR had a greater likelihood of "personal victimization" than did other Rs, even with controls for age, sex, marital status and whether or not a household crime was also reported for the particular household, ventured that this might be because NHR "received fewer stimulus questions" and included Rs interviewed by proxy. Initially, the Redesign Program employed a cross-sectional analysis to determine what respondent status rate differences remained when proxy interviews, household size, age, and type of crime were controlled (Cantor, 1981). This analysis suggested that personal larceny rates for HR average more than twice those for NHR with comparable characteristics and assaultive crime rates averaging about 15% greater for HR.

Despite the strength of these results, two important known procedural influences on victimization reporting could not be examined by cross-sectional analyses--the bounding of interviews to control "telescoping" and the presumed tendency for incident rates to decline for panels at successive times in sample. HR are disproportionately often members of smaller households. Smaller households, in turn, tend to have greater mobility and to be in sample for fewer successive interviews than larger ones. Although the control for household size, therefore, removed some bounding and time-in-sample effects from our analysis, there are good reasons to suppose that even within households, HR are likely to be interviewed on more successive occasions than NHR. Longitudinal data can be helpful in dealing precisely with these two effects as well as for avoiding possibilities that personal characteristics other than those captured by control variables are responsible for rate differences.

The Quasi-Experiment

A quasi-experiment was constructed from the NCS longitudinal panel file developed by Reiss (1977) for NCS interviews conducted from 1972 through 1975. The number of crime reports given by the same persons who in successive interviews had one and then the other of HR and NHR statuses can be compared with each other as well as with reports of Rs from the same households whose HR or NHR status remained constant at successive interviews. By limiting the analysis to households with more than one R and whose composition remained constant across the first three times-in-sample, the effects of both time-in-sample and of the presence or absence of a bounding interview can be observed. The first interview is universally not bounded by a previous interview; the second and third interviews for these cases will have been bounded in all the quasi-experimental cases by an interview with Personal Screen Questions. The design permits estimation of the effects of whether or not the bounding interview also included Household Screen Questions. All 12-13-year-old Rs and any others interviewed by proxy were eliminated. Households were also deleted if there were any interview non-completions for members in the accepted age range. The number of Rs in the household is controlled in all our analyses. One-R households, which are excluded from our design, and mobile and young persons, who are variously totally excluded or highly underrepresented, moreover, have much higher victimization rates than does the population retained for the quasi-experiment. The magnitudes of the effects observed, therefore, cannot be generalized to the entire NCS sample.

Unweighted data are employed, in that our immediate interest resides in the experimental population, rather than in estimates of rates for the total U.S. population.

Our quasi-experiment is feasible because interviewers designate a different R as HR at successive interviews. In our selected population, 66% of Rs have been HR for at least one of the three interviews. Table 1 shows a grouping of Rs into the eight possible sequences of respondent status (H=HR; N=NHR) at the first three times-in-sample.

Table 1. Percent of quasi-experimental population in respondent-status sequence groups for the first three interviews, by number of respondents in household.

Sequence			Respondents in Household 2	3	4+	All Households
H(1)	H(2)	H(3)	22.8	13.6	8.1	18.2
N(1)	H(2)	H(3)	9.1	5.8	4.0	7.5
H(1)	N(2)	N(3)	9.1	7.5	6.5	8.3
N(1)	N(2)	N(3)	22.8	45.2	59.3	34.3
H(1)	H(2)	N(3)	9.2	6.3	4.7	7.8
N(1)	H(2)	N(3)	8.8	7.6	6.4	8.1
H(1)	N(2)	H(3)	8.8	5.9	3.9	7.3
N(1)	N(2)	H(3)	9.2	8.0	7.2	8.6
	Total		100	100	100	100
	N		(8702)	(33801)	(25643)	(147146)

Personal Larceny Without Contact is treated separately from crimes with contact because the latter have much greater congruity with operationalization of household versus personal crime distinctions in NCS instrumentation and classificatory procedures than do the former. The "with contact" category includes assaultive crimes and Personal Larceny With Contact: i.e., pocketpicking and purse-snatching. The effects of changes in respondent status on rates of reporting of thefts without contact from the second to third interviews are discussed first, since first interview rates are inflated by the absence of a bounding interview. (1)

Personal Crimes Without Contact:--It is apparent in Table 2 that a sequence H(2)-N(3) is always associated with extremely large decreases in rates of Personal Crime Without Contact, and the reverse sequence, N(2)-H(3), with similarly large increases. In only one of the 12 relevant comparisons is the rate when R is HR less than twice that when NHR. The differences are about 300% or more in the largest households.

By contrast, Rs whose status remains unchanged display much less change in rates of reporting for these crimes. The T(2) and T(3) rates for two groups who have the same respondent status at all three times-in-sample are quite stable.

Thefts of vehicle parts:--Such high association of HR status with Personal Crimes Without Contact suggested the presence of some gross procedural effect. Asking a specific question about thefts of vehicle parts only in the household screen was particularly suspect because it involves a common event "away from home." Therefore, we examined the contribution of such thefts to the rates that were shown in Table 2. Overall, for interviews with HRs, about 37% of the no-contact personal thefts involve motor vehicle parts; for interviews with NHRs, only about 7.5% do. Since HRs are specifically asked about these thefts we expect them to report proportionally more than do NHR, including reporting more when HR has an NHR status. Because Personal Crimes of theft, moreover, constitute so large a proportion of all Personal Crimes, all analyses of aggregate "personal victimization" are greatly distorted by the assignment of the motor-vehicle-parts thefts to HRs as victim. We are not able to perform a similar separate analysis for thefts of bicycles, for Personal Crimes at hotels and other temporary lodgings, or other specific instances in which we suspect HRs are much more likely than NHRs to recall or to be assigned a victimization.

Table 2. T(2) and T(3) rates for personal crimes without contact+ by three-interview respondent-status sequence and by number of respondents in household.

Sequence			Respondents in Household 2 Interview # 2	2 Interview # 3	3 Interview # 2	3 Interview # 3	4+ Interview # 2	4+ Interview # 3
H(1)	H(2)	H(3)	36.6 (20038)	34.3	44.2 (4592)	46.6	42.3 (2082)	48.5
N(1)	H(2)	H(3)	45.1 (8019)	39.3	60.9 (1970)	49.2	42.2 (1019)	62.8
H(1)	N(2)	N(3)	23.7 (8019)	20.6	23.6 (2541)	23.2	22.2 (1664)	26.4
N(1)	N(2)	N(3)	26.7 (20038)	25.9	59.3 (15288)	51.4	56.8 (15208)	48.4
H(1)	H(2)	N(3)	38.4 (8092)	17.3	46.5 (2130)	16.4	54.2 (1199)	17.5
N(1)	H(2)	N(3)	46.7 (7702)	19.9	54.4 (2575)	33.8	75.2 (1636)	26.3
H(1)	N(2)	H(3)	16.4 (7702)	41.5	21.5 (2004)	44.9	18.2 (991)	69.6
N(1)	N(2)	H(3)	23.0 (8092)	46.1	28.5 (2701)	71.8	27.1 (1844)	71.6
	Total		32.0 (87702)	30.4	48.8 (33801)	46.2	50.2 (25643)	47.2

+ Data are for 1972-1975 collection period and computed with unweighted data. Rates are per 1,000 respondents (denominator in parentheses).

In a separate analysis not presented here we removed thefts of vehicle parts from the analysis shown in Table 2 to assess the HR bias on all other theft without contact. The pattern observed in Table 2 remains for those who change status from T(2) to T(3); only the magnitude of the differences is somewhat reduced. The highest victimization rates, however, are not consistently those for HR cells. Rs in the larger households who are NHR for all three interviews have by far the highest rates. Comparing groups in the same status at a particular interview, Personal Crime rates now vary directly with the number of times that group is NHR over the three interviews. This is true for both NHR and HR rates; for example, at T(2) for each household size, rates when NHR are highest for the NNN sequence group, lowest for the HNH group, with the HNN group having intermediate NHR rates. For contact as well as no-contact crimes, we observed subsequently that rates as either HR or NHR vary directly with the number of times R is NHR over the three interviews. The chance of being HR reflects such factors as age and time away from residence which are associated with crime vulnerability.

To focus more clearly on the effects of respondent status change, we can neglect the first interview and those Rs in the same status at both T(2) and T(3). The remaining Rs form two groups which, as shown in Table 3, exchange respondent status from T(2) to T(3). The symmetry of the association of personal larceny victimization (excluding vehicle-part thefts) with respondent status is remarkable. The decreases in reporting rates for the HN sequence is almost precisely equal to the increases for the NH sequence within each category of

household size. The magnitudes ot the changes are also impressive--well over 50% for the two smaller household sizes categories and almost 100% for the largest.

Table 3. T(2) and T(3) rates for personal crimes without contact+ by number of respondents in the household for respondents changing respondent status.

Sequence	Respondents in Household: 2 Interview # 2	2 Interview # 3	3 Interview # 2	3 Interview # 3	4+ Interview # 2	4+ Interview # 3
H(2) N(3)	42.5	18.6 (15794)	50.8	25.9 (4305)	66.3	22.6 (2835)
N(2) H(3)	19.8	43.9 (15794)	25.5	60.4 (4305)	24.0	70.9 (2835)

+ See note for Table 2.

With-contact crimes:--Rates for crimes that are free of definitional ambiguities in distinguishing "household" from "personal" victimization, such as are involved in NCS treatments of no-contact thefts, are examined next. Victimizations involving violence, along with purse-snatching and pocketpicking, as acts directly on the person of the victim, can be treated together as unambiguously personal in their definition and operationalization in the NCS. Unlike no-contact thefts, an Incident Report is accepted from each household member for with-contact crimes, even if another member was also victimized in the same incident and reports it. For these "with-contact" crimes, response status effects can be quite safely attributed to cognitive value of additional questioning experienced in HR statuses.

Since the rates for contact crimes are less than half the incidence of the no-contact crimes, small-N variability makes cross- tabular analyses tenuous at this level of disaggregation, so again just the four T(2) and T(3) sequences are displayed (Table 4). With one exception, where there is a change in status, the higher rate is in the HR condition. Although there is quite clearly a respondent status effect, the purely cognitive influence of a HR interview does not increase with-contact victimization rates nearly as much nor as consistently as both procedural and cognitive influences of the HR condition affect no-contact victimization rates. Declines in victimization rates by time in sample for the constant status cells are quite high in all but the largest households. If an equal time-in-sample decrease is predicted for the corresponding cells where household respondent status changes, the table becomes fully consistent with the hypothesis of respondent status effect.

Table 4. Second and third interview rates for personal crimes with contact+ by respondent-status sequence and by number of respondents in household.

Sequence	Respondents in Household: 2 Interview # 2	2 Interview # 3	3 Interview # 2	3 Interview # 3	4+ Interview # 2	4+ Interview # 3	All Households: Interview # 2	All Households: Interview # 3
H(2) H(3)	10.1	7.8 (28057)	9.1	7.2 (6562)	7.1	7.1 (3101)	9.7	7.7 (37720)
N(2) N(3)	11.9	10.6 (28057)	20.1	16.7 (17824)	17.8	18.6 (16372)	15.8	14.5 (62753)
H(2) N(3)	13.8	9.6 (15794)	11.3	8.9 (4705)	16.2	8.1 (2835)	13.6	9.3 (23334)
N(2) H(3)	11.0	10.4 (15794)	9.1	13.2 (4705)	10.9	14.8 (2835)	10.6	11.5 (23334)
Total	11.5	9.5 (87702)	15.2	13.3 (33796)	15.6	15.6 (25643)	13.1	11.4 (147141)

+ See note for Table 2.

Bounding Effects

Because first interviews are unbounded, the quasi-experiment compares rates for groups in this wholly unbounded condition with those after a previous HR interview (fully bounded) and after a previous NR interview (partially bounded). Comparing the relative magnitudes of the effects of respondent status and bounding may be instructive in that control of telescoping by a bounding interview has been accepted as of such great importance to the NCS as to merit the expense of conducting an initial interview not employed for data purposes.

Tables 5 and 6 show first and second interview rates, respectively, for Personal Crimes Without Contact and those With Contact. It is apparent that respondent status is far more important than interview bounding for the former and almost as important as bounding for the latter crime types. For the no-contact thefts, the decline in rate for the HN sequence is almost twice the declines observed for the stable sequences--HH or NN--while the NH sequence shows an increase in rates almost as large as the HN decrease. For with-contact victimizations, the NH sequence involves a decline two-thirds again as large as that for the stable respondent statuses, and the NH sequence a decline half that for the stable sequences. Earlier, we observed modest T(2)-T(3) decreases in mean rates for the whole population in the case of no-contact crimes. For with-contact offenses, quite large total rate fall-offs are observed between T(1) and T(2). For the stable sequences, the time-in-sample decrease is about 20% for those who are

Table 5. T(1) and T(2) rates for personal crimes without contact+ by respondent-status sequence and by number of respondents in household.

Sequence	Respondents in Household: 2 Interview # 1	2 Interview # 2	3 Interview # 1	3 Interview # 2	4+ Interview # 1	4+ Interview # 2	All Households: Interview # 1	All Households: Interview # 2
H(1) H(2)	54.8	37.1 (28130)	55.3	44.9 (6722)	70.1	46.6 (3281)	56.2	39.3 (38133)
N(1) N(2)	36.7	25.6 (28130)	81.9	54.7 (17989)	80.8	53.6 (17052)	61.5	41.5 (63171)
H(1) N(2)	63.5	20.1 (15721)	71.5	22.7 (4545)	91.2	20.7 (2655)	68.3	20.7 (22921)
N(1) H(2)	28.1	45.9 (15721)	38.5	57.2 (4545)	36.9	62.5 (2655)	31.2	50.1 (22921)
Total	45.8	32.0 (87702)	69.4	48.8 (33801)	75.9	50.2 (25643)	56.5	39.0 (147146)

+ See note for Table 2.

Table 6. T(1) and T(2) rates for personal crimes with contact+ by respondent-status sequence and by number of respondents in household.

Sequence	Respondents in Household: 2 Interview # 1	2 Interview # 2	3 Interview # 1	3 Interview # 2	4+ Interview # 1	4+ Interview # 2	All Households: Interview # 1	All Households: Interview # 2
H(1) H(2)	15.3	9.6 (28130)	11.5	7.7 (6722)	11.6	7.6 (3281)	14.3	9.1 (38133)
N(1) N(2)	19.6	12.7 (28130)	33.2	20.3 (17989)	33.3	18.2 (17052)	27.2	16.4 (63171)
H(1) N(2)	18.4	9.4 (15721)	15.6	7.9 (4545)	18.5	7.2 (2655)	17.8	8.9 (22921)
N(1) H(2)	16.4	14.3 (15721)	16.3	13.4 (4545)	21.5	16.2 (2655)	17.0	14.3 (22921)
Total	17.4	11.4 (87702)	24.3	15.2 (33801)	27.7	15.5 (25643)	20.8	13.0 (147146)

+ See note for Table 2.

HR both times and 7.4% for those who are NHR at both. For the NH sequence, T(1)-T(2) declines are expected because of the change from unbounded to bounded interviewing and from "panel effect." If the panel effect is about equal between T(1) and T(2) and between T(2) and T(3), then the remaining effects of respondent status change would have about the same magnitudes as the observed difference between the bounded and unbounded condition.

Utility of Quasi-Experimental Approaches

The foregoing analyses illustrate the utility of longitudinal quasi-experiments in panel surveys for the exploration of methodological issues. Another problem being approached similarly is the effects of proxy interviewing of 13-year-olds by comparing responses for the same individuals prior to and after they are first self-interviewed after their 14th birthday (Reiss, 1982).

The quasi-experimental method is particularly important for the NCS in that there is no other data source on unregistered crime that can serve for criterion purposes. Only the NCS mechanism itself can afford sufficient data for tests of the measurement properties of procedural alternatives. Victimization incidence is so low that even the full 1972-1975 NCS data file employed here proves barely adequate to sustain analyses even at the relatively high level of aggregation of the present inquiry. Although a number of split-sample methodological tests and experiments have been conducted for the NCS, the costs of such exercises limit them severely in number, scope and refinement. The costs are not only monetary. Experiments using the survey itself introduce additional complexities for analysts into what already are formidably complex data files. The very nature of a continuing, rotating panel design creates, moreover, a system that is tightly integrated across its entire panel structure, cross- sectionally and across time. There is no way to isolate totally an experimental treatment to keep it from disturbing the balance of rotation groups provided by the sample design. It can take three years to expunge all effects on current data of any alteration of procedure for any part of the NCS sample.

A proposal has been made to allocate, permanently and systematically, 5% of the NCS sample for controlled experimentation. But that sample is insufficient, for example, for inquiries directed at highly specific forms of crimes, such as rape, for which annual incidence rates are less than one per 1,000 persons. The requirement will remain for using the entire NCS, itself, for learning about its accuracy and validity.

Remedies for Respondent Status Biases

Currently, extensive use is being made of NCS data in research studies which explore personal correlates of victimization. We have shown that much of the variation of personal victimization rates results from systematically variable procedural treatment of NCS Rs. The consequence of the respondent status bias in the NCS is usually to depress the magnitudes of observed differences and the confidence with which null hypotheses are rejected in multivariate analyses. This is not true of all variables and combinations, however. For example, the atypically high personal crime rates for persons living alone, such as college students or divorced men, will be procedurally elevated yet further over those for persons with like characteristics living with others. This capacity of the NCS to generate misunderstanding of phenomena because of procedural treatment of Rs calls for immediate remedy as well as more fundamental longer term rectification. Immediate remedies are those that the Bureau of Justice Statistics and individual users can take with the existing data files. The more fundamental remedies require changes in the collection procedures and the conceptual structure on which they are based.

Immediate partial remedies: The predominant respondent status bias stems from merging most thefts of motor vehicle parts (those occurring "away from home") into the personal larceny category. Immediate steps should be taken to reclassify all such thefts as a separate category of household crime, precisely as thefts of the whole vehicle are treated.

Even removing these thefts still leaves Personal Larcenies Without Contact radically subject to respondent status bias. Existing data afford no basis for exhaustively subdividing the class into household and personal components. The entire class should be identified as a mixed class of household and personal victimizations in official published NCS tabulations. It should be grouped with the Household rather than the Personal Crimes and rates for the class computed on a household rather than a person base. Certain refined subclasses can be constructed nonetheless using specific place-of-offense codes (e.g. workplaces, school grounds, etc.) that can safely be treated as personal victimizations of R for reporting or research purposes.

The definition of Household Crimes should be altered for closer congruence with both Personal Larceny Without Contact and Household Crime classes. These categories should be identified as victimizations that can be associated reliably only with a household and not necessarily with any specifically affected member of it.

Remedies requiring design changes:--Remedies vary in how radically we choose to alter the current system and in the costs involved, whether of money, time-series continuity, or file complexity.

The least radical remedy preserves intact the current designated HR collection and screen procedure while altering the conceptual definitions of Household Crimes and Person Crimes. Household Crimes would be more cleanly defined as those involving (a) trespass of the residential premises and/or (b) thefts of property regarded by the R as belonging to the entire household, or (c) thefts of property which is under the responsible guardianship of the principal persons of the household for the household's use or for use by children of the household. Wherever an individual's personal property was affected, a crime incident report would be taken for that person, even if another report was taken for a Household Crime component of that same incident. Additional information would have to be collected in the incident reports to permit these discriminations. The information collected on each incident would have to be sufficient to allow translation into the units and categories of the present NCS in order to preserve series continuity.

A broadened remedy entails adding some specific cues to the Individual Screen Questions (for instance, thefts of motor vehicle parts and bicycles, and incidents occurring at vacation places and temporary lodgings) that now figure only in the Household Screen Questions.

More radical remedy involves abandoning the single selectively-designated HR. A randomized HR designation procedure would have some virtues of reducing the systematic character of the respondent status bias and improving knowledge about it (Reiss, 1967: Biderman et al. , 1967). The size-of-household component of the bias would remain, however, along with cognitive effects of differential cueing.

Administering questions addressed to both Household and Personal Crimes equally to all Rs in each household appears to be the most direct remedy for respondent status bias in the NCS. Indeed, our analysis of the logic and strategies of cueing suggests that there is no close correspondence of conventional crime categories, nor the division into household and personal as major classes, with the concepts that should be embodied in individual screen cues. Yet, abandoning the designated HR presents problems in addition to the costs of much duplicative interviewing. Insofar as interests reside in counting incidents as well as victimizations, there is a problem of identifying the same incidents from inconsistent descriptions and datings by different Rs in a household. Moreover, a uniform screening procedure accentuates rather than reduces the bias associated with the number of Rs in each household. The more heads in any household that we put to the task of recalling and recounting crimes that truly affect that household as a whole, the more exhaustive will be the reporting of such crimes for the household. It is, of course, altogether possible that improvements in screening will so reduce underreporting that (1 + N) heads will not be that much better than 1, and any increment of incidents over that from a single R will be almost totally increments of incidents peculiarly affecting the specific Rs who report them. These incremental incidents will be conceptually appropriate for treatment as events varying as a function of household size. To the degree that we fall short of ideally exhaustive screening, however, implementing a completely uniform screening procedure will create the need for learning about, and adjusting for, the "more heads" bias in treating household crimes.

It requires more than equating the screen questions for all Rs in a household to make their treatment uniform in all important respects. If one HR is designated both to complete the household screen and to supply the "control card" information and other data about characteristics of the household, nonuniformity would still be present in such respects as total interview burden, pre-screening interview warm-up, and the presentation of cues about places and roles which facilitate recall of victimization. There appears to be no readily feasible means to eliminate all such nonuniform aspects of treatment except by interviewing just one randomly selected R in each household. Respondent burden and cueing effects can be reduced somewhat for HR by separating the task of providing household information (and of proxy interviews, if that feature is retained) from that of answering the household screen. When the household information is secured from any eligible R under current rules, the HR who is to answer the household screen can be randomly designated. Random designation can also help identify the degree to which the providing of household data affects incident reporting. Single-R households cannot be subject to such treatment as part of an ongoing survey, however.

The most radical alternative involves abandoning the household versus personal crime distinction, conceptually as well as operationally. A purely individualistic concept of crime victimization can find some theoretical support and is the orientation to crime victimization most readily and coherently operationalizable by a personal interview survey. Were one to make the NCS exclusively a self-report survey, as now it is often somewhat erroneously termed, each R would be asked to report events by which that R felt victimized. The manner and degree to which the event victimized the R could be ascertained. We could still collect such information as R is able to give on the victimization of others, singly or collectively, in that same event, including the degree to which R's experienced victimization was in part or in whole due to his identification with those others. Household and personal victimization would thus be treated operationally as they are, both in theory and in personal experience, as nonexclusive phenomena.

FOOTNOTES

(1) This research was supported in part by contracts J-LEAA-015-79 and J-JSIA-004-82 with the Bureau of Justice Statistics, U.S. Department of Justice.

(2) The analyses presented below will, for simplicity, discuss straightforward displays of for each respondent status sequence at two successive interviews (Tij). The data were analysed also by a multiplicative logit model which allowed apportioning Tij rate variation to time in sample, sequence group mean Tij rates, respondents per household, and change in respondent status (Biderman, Cantor and Reiss, 1982). The results of the logit analysis were in all respects consistent with each of the findings reported here.

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