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The National Crime Survey: Working Papers

Volume II: Methodological Studies

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Abstract

This volume contains a series of technical papers on methodological issues associated with the National Crime Survey (NCS). Topics include memory failure, recall bias, classification of victimization events, sample design and coverage problems, response effects, and consequences of telephone versus in-person interviewing. 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 123,000 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 detailed information about the victims, the crimes, and 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 (NCS), a Federal statistical program established in the early 1970's, is sponsored by the Bureau of Justice Statistics [formerly the Law Enforcement Assistance Administration (LEAA)] to measure the annual levels of victimization from criminal activity in the United States. Data collection is conducted by the Bureau of the Census under an interagency agreement. The program was transferred from LEAA to the Bureau of Justice Statistics (BJS) in December 1979.* 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.

NCS 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 NCS design. They also provide the user with information suitable for developing applications and interpretations of NCS statistics. These volumes provide documentation on a range of methodological subjects pertaining to the NCS 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 LEAA 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 NCS was sponsored by LEAA. 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 might well be viewed differently today.

Volume I: Current and Historical Perspectives presents selections pertaining to the objectives of the NCS and its design, accounts of the early methodological and organizational steps establishing the design, a discussion of conceptual issues associated with measuring victimization, and examples of problems and prospects for using NCS 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 NCS 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 except in cases where they provide information relevant to the national survey.

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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Series and multiple victimization

Introduction

Since the beginning of the National Crime Survey (NCS) in the early 1970's, the Bureau of Justice Statistics (formerly the Law Enforcement Assistance Administration), through interagency agreement with the Bureau of the Census and by grants and contracts with nongovernmental groups, has supported continuing studies of the survey's design and methodology. The results of these studies have been distributed mostly within BJS and the Bureau in order to improve the management and operation of NCS. Some findings have received wider dissemination at meetings of professional and scholarly societies, but most are known to only a small group of professionals in government and the research community. Most documents published in this volume are therefore intra- and interagency memoranda that have not received wide distribution and comment.

This chapter is devoted to the problem of series victimization, a methodological artifact unique to victimization surveys. Series victimization reports occur because the respondent has experienced more than two victimizations of a similar type within the 6-month recall period and cannot recall the details. Consequently, the respondent is unable to report each incident separately, as normal NCS interview procedures require. An example of a series incident is a woman who repeatedly experiences physical threats or abuse from her spouse, to the point that she cannot distinguish the details of each separate incident.

Multiple victimization occurs whenever more than one incident is recorded during the 6-month recall period. The definition of a series incident requires that three or more nondiscrete incidents of a similar type be present. Thus, series incident reporting is a byproduct of the current method of recording incidents (completing a separate report for each discrete incident) and may be viewed as a special case of multiple victimization.

The recording of series incidents has posed a troublesome problem for counting incidents and computing victimization rates. Current practice excludes series victimization from total counts.

In the first selection of this chapter, Richard W. Dodge defines the problem of series incident reporting, proposes a method for including series incident counts in national reports, and recommends methodological study of series reports. The next two selections report on subsequent research on series incidents based on interview schedules, and provide some understanding of the conditions under which series reporting most likely occurs.

The final selection by Albert J. Reiss, Jr., summarizes some major findings derived from an analysis of NCS data tapes from 1972 to 1975 comparing series and nonseries reporting.

Series victimization—what is to be done?*

by Richard W. Dodge

Series victimizations have been, to date, the unwanted offspring of the NCS. We have been reluctant to acknowledge their existence, but on the other hand we recognize that they will not fade away, so we have compromised by alluding to them in prefaces to reports and by discussing them briefly in statistical appendixes. My feeling is that we should now confront the issue directly so that we can make a better assessment of the relationship between series victimizations and those regular victimizations that are the basis for the data in our reports. Similar concern has been expressed by others who work with the data. Before proposing a plan of action, some background may be in order.

In preparing for the collection of data for the NCS, it was recognized that there were situations involving multiple crimes that ran together in the victim's mind so that it was difficult, if not impossible, to separate the details of each. However, specific reference to series crimes did not appear on a victimization questionnaire until the July 1971 Quarterly Housing Survey.[1] Under current procedures, a series crime is defined as consisting of a minimum of three incidents which are very similar in detail and among which the respondent is unable to distinguish so that they can be recorded on separate incident report forms. The interviewer indicates by checking precoded boxes on the questionnaire during what season (or seasons) of the year the incidents occurred and the approximate number of incidents in each series (3–4, 5–10, or 11+). The balance of the incident report is then completed for the most recent incident. A limited number of tabulations are prepared for series victimizations, utilizing the same processing procedures as the regular data, but based on the month in which the data were collected rather than the month(s) in which the incidents occurred. This means that when both the collection year and the data year are considered together, the months of occurrence overlap about 71 percent of the time for a given calendar year. The closest correspondence between the two periods would be achieved by using the second, third, and fourth collection quarters of one year and the first quarter of the following year. Even here, the common months of occurrence total 87.5 percent, leaving 1 month in 8 not comparable. It is this comparability problem that makes it difficult to judge the impact of series crimes on the regular data. One part of the proposal presented in the next section is designed to address this problem by making possible the combination of series crimes with the bulk of the data.

The seriousness of the series issue can be generally documented by noting that weighted series victimizations amounted to about 5 percent of all personal and household victimizations in 1973.[2] However, this substantially understates the situation when it is recalled that a minimum of three incidents must have occurred in each series; often the number is a good deal higher. An examination of specific crimes suggests that series victimizations are more likely to be concentrated in two crimes, assault and household larceny, and in the less serious forms of these crimes, i.e., simple assault and larcenies where the value of the loss was under $50 or unknown. Thus, we are left with a body of data that is too significant to be ignored, but which is presently relegated to the technical appendix.

Another part of the problem is that series crimes probably cover a range of situations which are quite dissimilar. Some series crimes may be the product of an inexperienced interviewer's failure to probe sufficiently. A different interviewer confronting a similar situation may be able to obtain the necessary number of incident reports. In one case, a series crime is created and set aside from regular data; in the other, a potential series crime is avoided and the details of each incident are incorporated in the tabulations. There is a suggestion that greater experience on the part of interviewers may result in fewer series victimizations being accepted because the number of incidents reported in the national survey declined 30 percent between collection year 1973–74. Data from the first three quarters of 1975, however, indicate that the number of series victimizations may be stabilizing at or near 1974 levels. This does not mean that ways cannot be found to reduce the number of series victimizations still further. At the other end of the spectrum, there are legitimate series victimizations which cannot possibly be separated into discrete episodes. These are cases where being a victim is almost a condition of existence, by virtue of the nature of a person's job (police officer) or the location of a person's residence. Part of the research effort on series incidents will be to provide hard data on these types of incidents and to suggest how, if at all, they should be incorporated with the other crime data.

This proposal is basically directed to the notion that we need to know a good deal more about the series data before we can decide how to relate it to the regular, non-series data. A first step would be to make it possible to tabulate the series data with the regular data by allocating series victimizations to a specific month. This can be done by utilizing the item on the Incident Report (1b) where the interviewer records the season or seasons when the incidents occurred, and the reference period for the particular month of interview. A specific allocation scheme is set forth in the attachment to this memorandum. Once the month of occurrence has been allocated, rate tabulations could be produced for annual 1974 data for series victimizations alone and in combination with the regular data which would measure more directly than before the impact of series crimes. Because these tables would have many more victim characteristics than the existing series tables, especially for personal crimes, they would provide more insight into who the victims of series victimizations are. Producing the regular tables for series victimizations would not be as useful since details are collected concerning the most recent incident, which may or may not be typical of the series.

Interviewers write narrative summaries of each incident at the end of the incident report. A systematic examination of these summaries for a sample of series crimes might be instructive.

Ultimately, however, it seems that some form of field effort should be undertaken. This might take the form of an intensive followup of series victimizations by supervisors or senior interviewers. There are approximately 100 series victimizations reported each month in the national sample. If it is felt that continuing households should not be used, the followup could be restricted to the outgoing rotation group (although this would entail additional time to accumulate sufficient data). A questionnaire should probably be developed to gather this additional information. It is possible that a carefully devised sequence of questions might permit a respondent to disentangle the details of incidents so that separate reports could be recorded. Eventually, these questions might become part of the regular collection procedure. The following are indicative of the kinds of questions that might be answered by such a field activity: What proportion of series victimizations are due to inarticulate respondents and/or to interviewers who fail to probe sufficiently? How similar are series incidents? Do they occur in the same places? Are the offenders the same? If theft is involved, are the amounts of loss roughly comparable? Are the same kinds of crime involved in each series, or are respondents simply reporting what happened in the majority of cases? In collecting details of the most recent incident, are we getting the typical incident instead?

At the conclusion of any research into the series problem, we will have to contend with how to incorporate series data with the regular crime data. Should the data be weighted by the best estimate of the number of incidents in each series? If a policeman reports that he is threatened every day he is on duty, do we compute a factor reflecting the number of working days in the reference period or does this give undue weight to a rather special situation? There are undoubtedly other questions that could be explored, which will occur as planning moves forward. The purpose of this memorandum has been to present the case for making a beginning.

*Excerpted from: Richard W. Dodge, "Series victimization—what is to be done?" Washington, D.C.: U.S. Bureau of the Census memorandum, October 31, 1975.

[1]The problem was identified by others engaged in developmental work in the field of victimization surveys. For example, see Albert D. Biderman et al., "Report on a Pilot Study in the District of Columbia on Victimization and Attitudes Toward Law Enforcement and Administration of Justice." Washington, D.C., 1967, pp. 70-1.

[2]This figure is based on the 87.5-percent overlap comparison and counts each series of victimization as one.

Attachment:
Proposed allocation scheme for series incidents

The principal obstacle to combining series incidents with regular crimes is that the specific month (or months) of occurrence of the series of incidents is not obtained by the interviewer. Instead, the season of the year in which the incidents occurred, defined in 3-month periods, is indicated—up to a maximum of three seasons for any 6-month reference period. This makes it impossible to process the series crimes into the data-quarter and data-year formats used for the regular crime data. At present, the series data are tabulated quarterly and annually according to when they were collected, rather than when the incidents occurred. The only way to gauge the impact of the series crimes directly, under present field procedures, would be to devise a scheme to allocate series incidents to specific months of the year. A proposal to achieve this allocation follows.

No answer in Item 1b—This is not a substantial problem (only about one-half of 1 percent of the series incidents had no time span indicated), but it should be handled first. To do so, all series incidents should be ordered by collection month, and by control number within each month. Blanks can then be assigned the entry of the preceding case.

Good entries in Item 1b—To allocate series incidents for 1 data year, the 17 collection months that make up that year must be treated individually. For each month, the allocation scheme utilizes the information in Item 1b on season of occurrence in relation to the 6 months in the reference period. Depending on the month, either one or two boxes will be inapplicable for the particular reference period and can be ignored. Otherwise, the various possible combinations of responses to 1b should be set up as a matrix and the specific series crimes tallied into the appropriate category. To illustrate, the reference period for interviews obtained in January 1975 was July–December 1974. The possible entries in Item 1b with months falling in the reference period are:[3]

box 2 only	July–August
box 3 only	September-November
box 4 only	December
boxes 2 + 3	July-November
boxes 3 + 4	September-December
boxes 2, 3, + 4 (include 2 + 4)	July–December

If each incident is to be counted only once, then the specific months can be assigned in sequence within each category, either starting with the most recent month and working backward, or vice versa. In 10 of the 17 collection months that comprise the data year, anywhere from 1 to 5 of the reference months will fall in either the preceding or following year. All cases allocated to these months would, of course, be excluded by the processing.

Since each series crime contains, by definition, a minimum of three incidents, a more accurate estimate of the impact of these crimes would be obtained by counting each incident three times. The allocation procedure could be similar to the one described above, with the basic pattern utilizing 3-month sequences, where possible, in a rotating pattern—i.e., July, August, September; August, September, October; etc.

[3]Entries in boxes 2 and 4 is a possible, but unlikely combination because a substantial gap between incidents should make it possible for them to be separated. Any instances of this type should be added to the 2, 3, + 4 category.

A preliminary inquiry into series victimizations*

by RICHARD W. DODGE

The problem of series crimes—those incidents of such frequent occurrence that respondents are unable to provide details for each incident separately—has been recognized from the outset of victimization surveying. Recently, the Panel for the Evaluation of Crime Surveys of the National Academy of Sciences recommended more thorough study of series offenses, so that ways might be found to include them with the regular (or nonseries) crimes. At the present time, multiple victimizations befalling a single victim are incorporated into the survey tabulations as long as each event can be separately recalled and reported. However, series incidents, which are a special case of multiple victimization, cannot easily be added to the regular victimization data because of the lack of precision regarding the number of events in the series and when each one occurred. In order to make progress in merging series incidents with the bulk of the victimization data, it is necessary to know more about the nature of these crimes than can be obtained from the relatively small number of tabulations now provided from series data.[1]

A first step is to look at individual questionnaires and especially at interviewer summaries, to obtain more information on what kinds of incidents are being reported under the label of series crimes. This report is based on an examination of all the series incidents reported in the NCS for the collection month of September 1975. There were 96 series incidents reported in 91 households that month (three households reported two series incidents and one household reported three). The great majority of series reports were obtained in personal interviews (88 out of 96) rather than by telephone. There were five proxy respondents, all for children under 14 years of age.

The distribution of the series incidents by type of crime and number of incidents in the series is shown in table 1. One quarter

1. Number of incidents in series by type of crime (September 1975)*

Incidents	Total	3-4	5-10	11+	NA, DK
Robbery:					
With injury	1	1	0	0	0
Without injury	5	2	2	1	0
Assault:					
Aggravated	6	1	4	1	0
Simple	12	5	5	2	0
Personal larceny without contact	22	13	6	2	1
Burglary:					
Forcible entry	4	1	2	0	1
Unlawful entry	11	7	4	0	0
Attempted forcible entry	5	4	1	0	0
Household larceny	30	17	8	3	2
Total	96	51	32	9	4

*This is the only table in this report that is duplicated by series tables now produced. Results for the 3rd quarter of 1975 show that slightly less than half the series crimes fell into the 3-4 category.

2. Season of occurrence of series victimization, by type of crime (September 1975)

Type of crime	Total	Spring only	Summer only	Spring and summer	NA
Crimes of violence	24	2	5	14	3
Personal theft	22	7	5	8	2
Household crimes	50	12	24	13	1
Total	96	21	34	35	6

of the total were classified as violent crimes and these tended to be the less serious forms—5 out of 6 robberies were without injury and 12 of 18 assaults were simple, rather than aggravated. Series incidents for unlawful entry and attempted forcible entry together were four times as prevalent as those for the more serious form of burglary, forcible entry. Rape, personal larceny with contact, and motor vehicle theft were not reported as series crimes during this particular month. A slight majority of series victimizations consisted of three to four incidents. Assaults were more likely than the other types of crimes to be reported as having greater numbers of incidents per series, two-thirds falling in the five-or-more-times categories. In almost one-fourth of the series reports, the specific number of incidents was recorded by the interviewer, even though not specifically asked for, suggesting that it might be advisable to ask for the number of incidents directly, instead of using class intervals as at present.

Series incidents can occur in any or all months of a 6-month reference period. Because the specific dates of occurrence of these incidents are often vague in respondents' minds, it was decided at the outset to group the months of occurrence into seasons. For this purpose, the seasons were divided into four 3-month periods, winter being December, January, and February, etc. Since the interviews in this study were conducted in September, only two seasons, spring and summer, were involved. As can be seen in table 2, slightly over one-third of the series victimizations occurred in both spring and summer and nearly as many occurred only in the summer months. Within three broad categories of types of crime, sharp differences emerge. The majority of crimes of violence were reported to have taken place in both spring and summer, whereas personal theft and especially household crimes as a group were reported to have occurred in one season or the other, but mostly during the summer months. It is not possible to determine from this sample whether the preponderance of household crimes occurring in the summer only is a function of seasonality or of a tendency to telescope events forward within the reference period. As in the case of the number of events in a series, there was evidence that some respondents could have provided the specific months of occurrence, if they had been asked, especially when only three or four incidents were involved.

An attempt was made to classify series incidents into categories that might be more indicative of the kinds of events involved. An examination of the data for the two crimes of violence suggested three specific categories, plus one for "other" situations. These were crimes occurring in the course of one's job and those classified as domestic altercations, both of which involved assaults only, and incidents for crimes of violence directed against adolescents. Table 3 shows that assaults in the line of duty were the most common series incidents for crimes of violence. These same crimes

*Richard W. Dodge, "A Preliminary Inquiry into Series Victimizations," Washington, D.C.: U.S. Bureau of the Census memorandum, 1977.

[1]An inspection of existing series tabulations and a special run of series data through the regular table programs indicates that whites, especially in the younger age groups, are more likely than blacks to report series victimizations. Members of the Armed Forces also are more prone than the average to report themselves as victims of series crimes.

3. Nature of series victimizations for violent crimes by number of incidents (September 1975)

Incidents	Total	3-4	5-10	11+
Assault:				
Job related	10	2	6*	2
Domestic altercations	6	2	3	1
Adolescents	2	1	1	0
Other	1	1	0	0
Robbery:				
Adolescents	3	2	0	1
Other	2	1	1	0
Total	24	9	11	4

*One of these incidents was apparently misclassified in the survey as a robbery. The series occurred in a grocery store and the offenders were trying to steal groceries and threatened a store employee in the process. Question 13f was marked "yes"—that something belonging to the victim (employee) was taken—which accounts for its classification as a robbery.

4. Nature of series victimizations for personal larcenies by number of incidents (September 1975)

Incidents	Total	3-4	5-10	11+	NA, DK
In school	9	6	3	0	0
On the job	6	1	3	2	0
From motor vehicles	4	4	0	0	0
Other	3	2	0	0	1
Total	22	13	6	2	1

5. Nature of series victimizations for household crimes, by number of incidents (September 1975)

Incidents	Total	3-4	5-10	11+	NA, DK
Burglary:					
Home—stranger	13	7	6	0	0
Home—nonstranger	2	1	1	0	0
Garage	5	4	0	0	1
Household larceny:					
Inside home—stranger	4	3	1	0	0
Inside home—nonstranger	10	4	4	0	2
Vicinity of home	6	2	2	2	0
Motor vehicle—gasoline	7	5	1	1	0
Motor vehicle—other	3	3	0	0	0
Total	50	29	15	3	3

6. Number of other incidents in households with one series victimization (September 1975)

Incidents	Total*	Number of series households with other incidents: None	1	2	3	4	5
Robbery	6	3	1	1	1	0	0
Assault	16	10	3	3	0	0	0
Personal larceny	21	13	2	4	1	0	1
Burglary	17	7	5	3	1	1	0
Household larceny	27	15	8	2	1	1	0
Total	87	48	19	13	4	2	1

*Excludes four households which had more than one series victimization.

were especially likely to occur five or more times. Assaults stemming from domestic altercations occupied an intermediate position, while the remaining cases clustered at the lower end of the frequency scale.

Series thefts are best classified in terms of where the events took place. Thefts away from home (personal larceny without contact) most commonly occurred to school children and, secondarily, in job-related situations (table 4). The latter tended to be concentrated in the five-or-more-times-per-series categories. Thefts from motor vehicles constituted another clear-cut category.

Burglary incidents occurring in the home and those incidents of household larceny not involving motor vehicle theft were more or less evenly divided between those reported as having occurred three or four times and those with five or more incidents (table 5). By contrast, incidents of burglary of garages and larcenies from motor vehicles near the home were more likely to fall in the lowest category. Most thefts from motor vehicles in the vicinity of the home involved siphoning gasoline from the tank.

The relationship between series incidents and other incidents occurring in the same household was also examined (table 6). In 48 of the 91 households in the sample, there were no additional incidents—the series incident being the only one reported in the reference period. In 19 households, one other incident also occurred during the reference period and, in another 13, there were two additional incidents. Seven households reported three or more incidents. The four households which reported more than one series victimization have been excluded from table 6 to avoid double counting. The one household with three series victimizations had no other reported nonseries incidents. In each of the households with two series victimizations there were an additional two incidents reported during the reference period. To have included these nine series victimizations in table 6 would have increased the totals in the categories of two and three additional victimizations by three and six, respectively. Although we have no evidence on the incidence of multiple victimizations in the national survey, the figures cited above suggest that series victims may be more prone to victimization than the average, particularly when it is considered that each series victimization represents a minimum of three incidents.

Table 7 depicts each of the additional victimizations in broad categories by type of crime and compares them with the classification of the series crime. In this table, the multiple series households have been included in order to arrive at an overall pattern of relationships. Thus, the nine series incidents in these households appear both as "other" incidents and as series incidents in the table. There were 94 other victimizations in the 43 households which experienced additional incidents—more than 2 per household. The evidence of association between series and other crimes within the same household is not clear-cut, although there is a suggestion that victims of robbery and assault as series crimes are more likely to be victims of other incidents of personal crime. In the 20 additional incidents reported by the households with robbery and/or assault as series crimes, the same person was also a victim of a personal crime (violence or theft) in 10 instances. Households that were victims of series burglary and/or household larceny were also more likely to have experienced other household crimes—in 30 of 53 incidents. Personal larceny without contact series victimizations followed a different pattern—very few of the additional crimes occurred to the series victim, but were about equally divided between personal crimes occurring to other household members and household crimes.

7. Relationship between series incidents and other incidents occurring in the same households (September 1975)

		Other incidents							
			Personal crimes				Household crimes		
			Not involving series victim		Involving series victim				
	Total series with other incidents*	Total	Violent	Theft	Violent	Theft	Burglary	Larceny	Motor vehicle theft
Robbery	3	6	0	1	2	1	1	1	0
Assault	8	14	0	2	4	3	5	0	0
Personal larceny	9	21	6	3	0	2	5	5	0
Burglary	13	25	1	2	5	2	6	8	1
Household larceny	15	28	1	1	6	5	4	11	0
Total	48	94	8	9	17	13	21	25	1

*Occurring in 43 households.

Multiple victimizations

Questionnaires involving multiple victimizations of the same individual or household were also examined for the third collection quarter of 1975. These incidents differed from those in series in that the respondent was able to report the details of each incident that befell him. For direct comparison with series crimes, multiple victimizations were initially defined as consisting of a minimum of three incidents which could be classified into one of the broad categories used in the NCS. Thus, a respondent who reported two unlawful entries and one forcible entry was considered to have been a multiple victim of burglary. In addition, the multiple crimes, whether personal or against the household, had to be reported by the same respondent.

On this basis, multiple victimizations occurred in only four of the crimes measured by the survey: assault, personal larceny without contact, burglary, and household larceny. There were 25 multiple victimizations (in 23 households) involving 82 separate incidents taking place in the same time period when 96 series victimizations were reported. There was no overlap between households reporting series victimizations and those with multiple incidents as defined above. In contrast to series crimes, multiple victimizations were composed exclusively of three or four incidents. There were 18 involving three incidents and 7 involving four.

In terms of when they occurred, 60 of the 82 specific incidents took place in the second half of the reference period, with 31 occurring in August alone, the month preceding the interview. When each multiple victimization is distributed on the same basis as the series crimes, by season, only 3 of 25 occurred in the spring, with the remainder about equally divided between those spanning spring and summer and those taking place only in the summer months. This is similar to the distribution of the series incidents, although the latter had a somewhat larger proportion reported in the spring months.

It proved to be more difficult to classify multiple victimizations into specific categories according to the nature of the events than to do so with series victimizations. This was presumably due to the fact that each incident was recalled individually by the respondent, who was able to do so, in part at least, because there were distinctive aspects to each event which facilitated separate recall. Also, there were no multiple victimizations involving five or more incidents, whereas nearly one-half of the series victimizations were of that magnitude or more. For series victimizations, details were collected for only the most recent incident so that one had to assume that the other incidents were basically the same unless the interviewer's summary provided evidence to the contrary.

There were five assault multiple victimizations of which two were domestic altercations, both occurring in the same household. The offender was the household head who attacked his wife on several occasions and threatened their son during the same incidents. Two other assaults were job-related, although in one of these there was a combination of assault occurring on the job and unrelated events. In the other assault multiple victimization, there were four unrelated incidents with different offenders in each instance. In 13 of the 17 incidents involved in these assault multiple victimizations the victim was at least casually acquainted with the offender.

Two of the four burglary multiple victimizations involved the taking of substantial items from respondents' apartments--i.e., television sets, radios, and stereo equipment. The other two were attempted forcible entry incidents, and in one of these the respondent also reported a forcible entry where nothing was taken.

The most common target of noncontact theft, which is a combination of household larceny and personal larceny without contact, was the contents of motor vehicles. There were 11 multiple victimizations where objects were taken from motor vehicles. In five of these cases, cars were involved exclusively, whereas six concerned cars in combination with other items, such as bicycles, lawn mowers, and potted plants. Even when each incident in a multiple victimization was directed against a car, the specific items usually varied. In fact, there was only one instance of a theft of the same item each time—gasoline from a car. Four multiple victimizations occurred in varying degrees in schools—two exclusively so, and the other two in combination with other locations. There was one household larceny that did not involve a motor vehicle, but consisted of articles taken from the yard. In all of these theft incidents the diversity of the items taken, or occasionally the differing locations, apparently aided the victim in recalling each incident separately.

As to the question of the tendency of multiple victimization households to be victims of other incidents, the results are similar to those for series households. Eleven of 23 households with multiple victimizations reported no other incidents. In 10 households there were 18 additional incidents, but 6 of these households reported only 1 extra incident so that 4 households accounted for 12 additional incidents. In the two households that each reported two sets of multiple victimizations, there was a total of seven or more incidents. As in series victimizations, a small minority of households appeared to be especially vulnerable to victimization—in this case, 5 households reported a total of 41 incidents.

If we broaden the definition of multiple victimization to include any combination of three or more incidents reported by the same respondent, there were 25 additional qualifying multiple victimizations, involving 82 separate incidents, reported in the month under examination. In 19 cases, the incidents all involved noncontact crimes (personal larceny, household larceny, burglary, or motor vehicle theft); the remaining 6 consisted of combinations of contact (assault and robbery) and noncontact crimes. (One of these households also contained a multiple victimization for burglary, as defined earlier.) There were no cases involving contact crimes exclusively. These multiple victimizations consisted predominantly of 3 incidents—in 20 of the 25 households. In two households respondents were victimized four times during the reference period; in two more, five incidents each were reported; and in the double multiple victimization household, there were seven incidents. The pattern of seasonality was very similar to that for the more rigorously defined multiple victimizations: the majority were reported to have occurred in the June-to-August quarter rather than in the one preceding.

For the 70 noncontact crimes, the most common object of victimization was the motor vehicle; in 21 cases (reported in 13 households), parts of motor vehicles or their contents were stolen or attempts were made to do so; in another 8 incidents, automobiles themselves were involved. In seven situations, thefts were directed against bicycles. There were 12 instances where various forms of burglary were reported but nothing was taken. The remaining 22 cases involved a great variety of objects that were either stolen or attempts were made to do so. In 9 of the 12 incidents where contact occurred between victim and offender, the latter was a stranger. Two of the other incidents involved the spouse of the same victim. This contrasts with the preponderance of nonstranger relationships when multiple victimizations were more narrowly defined.

In 18 of the 25 households, there were no incidents reported by other household members. (In the one household with two multiple victimizations, all seven incidents were reported by the same respondent.) There was one additional incident in six households and one household reported two other victimizations.

Conclusions

What conclusions can be drawn from this initial attempt to look at series crimes and at multiple victimizations which were reported separately? The examination of the individual questionnaires suggests that some incidents were classified as series as a convenience by the interviewer. Raising the minimum number acceptable to five, for example, would have eliminated slightly over half the cases in this sample. Whether it would have that much effect in actual practice is problematical, but it seems likely that the numbers would be reduced by at least one-third. Asking for the specific number of incidents in a series and the month of occurrence apparently would be productive of answers in a number of cases and this could be incorporated into the questionnaire at any time. The effort required by the respondent to provide these answers might enable him to go further toward separating the details of each incident sufficiently so that individual incident reports could be filled. It is difficult to say definitively that there is a tendency for respondents to report series incidents more readily for events which are more distant from the date of interview as speculated by the National Academy of Sciences.[2] In fact, the present data suggest the opposite—as more series events were reported in the summer only as compared with spring—but this may be affected by seasonal factors.

The evidence from this sample indicates that series incidents can be classified into categories that reflect the nature of the events, i.e., assaults in the line of duty, thefts on the job, etc. More cases might suggest different classifications, especially for household crimes. What this sample cannot do is indicate whether these incidents would be classified into the same type of crime as the degree of specificity increases—that is, are assaults in the line of duty all attempted assaults without a weapon or do some involve physical injury? Would additional probing reveal that an attempted robbery with a weapon was involved? Because details are collected only for the most recent event, we cannot be sure that if all the evidence were in that each specific episode would be classified the same way. A study of this sample of series questionnaires leaves one with the impression that there is a variety of experience that is being lost in the present series designation. Whether further probing could get at this potential difference is not certain. What is certain is that multiple victimizations where individual episodes *can* be recalled by the victim are classified in the greatest detail possible. If series crimes cannot be broken down this way, there will be problems in combining these crimes with the regular data. Beyond this is the question of whether all series crimes should be added in to achieve a total count. Should crimes that stem directly from the nature of one's job be treated like other crimes that fall into the same category? For example, if a policeman reports that he is the victim of verbal assault every day he is on duty, should this be added into the total of all attempted assaults without a weapon? No further research is needed to demonstrate that cases similar to this have occurred and will continue to occur.

Multiple victimizations, whether defined as consisting of the same kinds of crime or as combinations of different crimes, were generally composed of fewer incidents than series crimes and thus were easier to recall separately. Even when multiple victimizations were classified as the same type of crime, there was more likelihood of variety among the incidents as, for example, in the kinds of articles stolen in noncontact crimes. However, the distribution by season did not differ significantly from that reported for series incidents.

The next stage in the investigation of series incidents will be an examination of a larger sample of questionnaires for a different period of the year to gain further insights into the nature of these crimes. Eventually, a supplemental set of questions will be developed and field tested in order to obtain sufficient information about series crimes so that substantially more of them can be incorporated with the regular crime data.

[2] *Surveying Crime*, Panel for the Evaluation of Crime Surveys, National Academy of Sciences, Washington, D.C., 1976, p. 78.

Patterns of personal series incidents in the National Crime Survey*

by Richard W. Dodge and Harold R. Lentzner

One of the major unresolved problems of the NCS is how to treat crime incidents that occur in a series. These are events which happen with such frequency that respondents are unable, even with interviewer probing, to provide details for each incident separately. This is not a new problem, but has been recognized since the beginning of victimization surveying. The National Academy of Sciences study of the NCS recommended both that existing data be examined for clues as to how series crimes might be combined with data for regular (or nonseries) crimes, and also that new ways be explored for understanding changes in the reporting of series over time.[1] Extensive work on this latter point has already been undertaken by Albert Reiss and his colleagues at Yale.[2] By joining records from different reporting periods, they were able to construct a longitudinal file which enabled them to examine the pattern of crime incidents at sample addresses over time—for the seven periods over a span of 3 years when addresses are eligible for a crime survey interview. It should be emphasized that this was a longitudinal file of what happened at specific addresses, not a history of what transpired for particular households. In the NCS, households that move are not followed; instead, interviews are obtained from the new occupants at the sample address.

Examination of series incidents from a longitudinal perspective has the potential for illuminating the nature of series incidents by providing data on their persistence over time and their relationships to other victimizations in the same or different reference periods, particularly to other multiple victimizations that are reported separately. Reiss and his associates discovered, among other things, that series incidents are of comparatively short duration—that is to say, there is relatively little reporting of series incidents in subsequent interviews; that roughly three-fourths of persons or households reporting series incidents in a given interview report no victimization at all during the next interview; that multiple reporting of series events is less common than for regular crimes that are reported separately; and that the proportion of movers is greater among victims of series incidents than for other victims.

Perhaps the most interesting question raised by these findings is the largely one-time nature of series incidents. On the surface it seems puzzling that a person or household that suffered a minimum of three incidents in one 6-month period would report no victimizations at all in the next interview.

In an effort to investigate these discontinuities, the basic survey documents were examined for a sample of addresses that were in the NCS for the normal stay of seven periods.[3] These documents are the control cards for each household that lived at an address during the 3 years it was in the NCS sample and the questionnaires containing reports for crimes classified by the interviewer as series incidents. The control cards provide a roster of the household members throughout the period, as well as a brief description of each incident that was reported to the interviewers. The questionnaires contain a narrative summary of the incident, which often provides amplification of the events contained in the survey record. This is especially true of series incidents because the questionnaire gathers details for only the most recent incident in the series, once the number of events in the entire series of incidents and the season or seasons when they occurred have been obtained. Many interviewers use the summary as it was intended, to characterize the entire series of incidents, noting special features of these phenomena that are not captured on the computer record at present.

The sample upon which this report is based was selected from six panel-rotations, those entering the NCS sample in the months of January, February, March, July, August, and September 1974. Any household residing at a sample address that experienced a series incident at any one of the times that the address was eligible for an interview was selected for this study. The entire computer record for all households living at the address was extracted, as well as the household control cards for each address and all questionnaires containing series incidents. Because questionnaire booklets for all incidents reported by household members during a given interview period are kept together, numerous regular (or nonseries) incident questionnaires were also included. A total of 664 addresses were identified as reporting 812 series incidents of all types.[4]

This exploratory report concentrates on the incidents involving the violent personal crimes of rape, robbery, and assault. There were 205 of these incidents—3 cases of rape, 20 robberies, and 182 assaults.[5]

The additional data on the control cards and questionnaires enabled us to classify these 205 incidents into categories that were more indicative of the kinds of events involved. Four groups were identified—one where the series of incidents apparently was directly related to the nature of the job the victim held (i.e., police officer, bus driver, etc.). The second group constituted cases of domestic violence, including both intrafamilial altercations and those between persons who were well acquainted but not related to one another, such as friends and neighbors. There was also a distinct category of violence between children, usually occurring in school, on the school grounds or going to and from school.[6] Finally, there was a miscellaneous category which consisted of incidents involving adults which could not be assigned to either the job or domestic violence categories. These usually involved persons who were not well acquainted with one another, although there were six situations which occurred on the job but did not relate to the nature of the job.

A comparison of the overall relationships among these four groups of personal series crimes will be presented in a series of seven tables.[7] Following this, each group will be separately analyzed in an attempt to identify the specific problems that each presents. The paper will conclude with some suggestions for future lines of inquiry into the nature of series crimes and will raise some questions about their possible incorporation into the regular body of crime data.

Table 8 distributes the four groups by major type of violent crime. Assault is clearly the dominant type of crime, comprising 100 percent of the job-related crimes. In addition, simple assault is considerably more frequent than aggravated assault; but there is little difference in overall terms between the two categories of robbery. Nearly one-half of the series incidents consisted of three or four events. There appears to be considerable variation among the four kinds of series incidents in this regard, but the differences are not statistically significant (table 9). Table 10 demonstrates what Reiss and his associates have previously shown; namely, that series crimes tend to be nonrepetitive. This is generally true irrespective of the nature of the event. About three-quarters of the respondents victimized by violent series crimes reported no other series crime (violent or theft) during their stay in the NCS sample, another 21 percent reported two sets of series crimes, with a miniscule number reporting three or more. For violent series crimes alone, only 13 percent of the respondents reported any other violent series crime in a subsequent interview. Table 11 shows that personal series crimes tend to be reported near the beginning of the household's stay in the NCS sample, i.e., 69 percent in either the first or second reporting periods—as contrasted with 4 percent in the seventh interview. As Table 12 suggests, the distribution of the number of times a particular household remains in the sample is more even. Although the differences in the percentages for time in sample do not exceed sampling variability, they do imply a contrast with the number of the interview in which the series crimes are reported—a finding also noted by Reiss.

8. Personal series incidents: Percent distribution of incidents, by type of crime and classification of incident

Type of crime	Total		Job related	Domestic violence	Children	Miscellaneous
	100%	(205)	100% (77)	100% (49)	100% (42)	100% (37)
Rape	1	(3)	0	4	0	3
Robbery	10	(20)	0	6	24	19
With injury	4	(8)	0	6	5	8
Without injury	6	(12)	0	0	19	11
Assault	89	(182)	100	90	76	78
Aggravated	19	(39)	19	22	10	24
Simple	70	(143)	81	67	67	54

Note: Percents may not add to detail shown because of rounding. Numbers in parentheses refer to number of cases in a cell.

9. Personal series incidents: Percent distribution of the number of incidents per series, by classification of incident

Number of incidents per series	Total		Job related	Domestic violence	Children	Miscellaneous
	100%	(205)	100% (77)	100% (49)	100% (42)	100% (37)
3-4	43	(88)	26	51	43	68
5-10	29	(60)	38	22	31	19
11 or more	22	(45)	30	27	14	8
Don't know	6	(12)	6	0	12	5

Note: Numbers in parentheses refer to number of cases in a cell.

10. Victims of personal series incidents: Percent distribution of the number of series per victim, by classification of victim*

Number of series per victim	Total		Job related	Domestic violence	Children	Miscellaneous
	100%	(169)	100% (52)**	100% (46)	100% (37)	100% (34)
One	76	(128)	60	91	78	76
Two	21	(35)	33	4	22	24
Three	2	(3)	2	4	0	0
Four	1	(2)	4	0	0	0
Five	0	(0)	0	0	0	0
Six or more	1	(1)	2	0	0	0

Note: Percents may not add to 100 because of rounding. Numbers in parentheses refer to number of cases in a cell.

*Includes 16 series involving theft reported by 14 victims.
**Includes one victim who also reported a domestic violence series.

The NCS employs the principle known as bounding to control the tendency of respondents to bring forward into the reference period events that occurred earlier. Operationally, this means that the first interview at a sample address in the incoming rotation group is a bounding interview, used to establish a frame of reference, but the data collected are not incorporated into the estimates of victimization. There are other situations, however, in which a household or respondent can be in an unbounded status during the second through the seventh time an address is in the sample. The most common of these is where a household moves into a sample location during one of these subsequent interview periods. Other examples include units which were not interviewed in the previous reporting period, new construction

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* Excerpted with editorial modification from a paper prepared for the annual meetings of the American Statistical Association, San Diego, California, 1978.

[1] *Surveying Crime*, Bettye K. E. Penick, Editor, Panel for the Evaluation of Crime Surveys, Committee on National Statistics, National Academy of Sciences (Washington, D. C., 1976), p. 88ff.

[2] For example, Albert J. Reiss, Jr., *Victim Reporting of Series and Nonseries Incidents Over Time*, Analytical Studies in Victimization by Crime, Data Report #6 (Yale University, June 1977) (unpublished).

[3] Exceptions occur when addresses in new construction segments are added to the sample, units are added or eliminated through conversion of existing units, units are demolished, etc.

[4] There were an estimated 19,034 nonseries incidents reported by these same households out of a total of about 70,000 interviewed households.

[5] Two hundred and eleven violent series incidents were identified, but six cases were eliminated because either the questionnaire and/or the control card was not located or the data were insufficient to classify the incident. We use the term personal series as equivalent to the total of these three violent crimes.

[6] Unfortunately, the present questionnaire does not identify incidents taking place on school grounds or between school and home, although this information is often supplied in the interviewer summaries.

[7] Due to the small sample size, many of the differences shown in these tables do not exceed the 90-percent confidence level which is the minimum customarily employed in Census Bureau standard error tests. Statements which are based on comparisons that fall below this level are so indicated.

11. Personal series incidents: Percent distribution of the number of the interview in which the incident was reported, by classification of incident

Interview in which series incident reported	Total		Job related	Domestic violence	Children	Miscellaneous
	100%	(205)	100% (77)	100% (49)	100% (42)	100% (37)
First	53	(108)	48	59	38	70
Second	16	(33)	19	16	17	8
Third	8	(16)	8	4	12	8
Fourth	7	(14)	5	8	12	3
Fifth	6	(13)	8	4	10	3
Sixth	6	(13)	10	4	2	5
Seventh	4	(8)	1	4	10	3

Note: Percents may not add to 100 because of rounding. Numbers in parentheses refer to number of cases in a cell.

12. Victims of personal series incidents: Percent distribution of the number of times the victim's household was in sample, by classification of victim

Number of times victim's household was in sample	Total		Job related	Domestic violence	Children	Miscellaneous
	100%	(169)	100% (52)*	100% (46)	100% (37)	100% (34)
One	21	(36)	15	24	16	32
Two	11	(18)	8	17	11	6
Three	11	(18)	15	15	3	6
Four	9	(15)	13	9	5	6
Five	3	(5)	2	0	8	3
Six	5	(9)	6	4	5	6
Seven	40	(68)	40	30	51	41

Note: Percents may not add to 100 because of rounding. Numbers in parentheses refer to number of cases in a cell.

*Includes one victim who also reported a domestic violence series.

13. Personal series incidents: Percent distribution of the bounding status of the incident by classification of incident

Bounding status of the incident	Total		Job related	Domestic violence	Children	Miscellaneous
	100%	(205)	100% (77)	100% (49)	100% (42)	100% (37)
Bounded	44	(91)	48	35	62	30
Unbounded	56	(114)	52	65	38	70
Incoming rotation group	24	(49)	17	27	29	30
All other	32	(65)	35	39	10	41

Note: Percents may not add to detail shown because of rounding.

units which are periodically added to the sample, or extra units discovered by interviewers when, for example, a single family home is converted into apartments. In addition, individual persons can be added to households or be unavailable for interview at a given period. Table 13 indicates that there are a substantial number of unbounded interviews with the victims of violent personal crimes. Although sampling error might account for apparent differences in table 13, the data suggest that unbounded incidents reported subsequent to the initial interview are an important component of personal series crimes.

Table 14 depicts the patterns of other crime incidents for each respondent who experienced one or more personal series incidents. Approximately one-third of the total reported no other incidents during the period their household was in the NCS. For about one-half of the cases there were either nonseries thefts only reported or a combination of nonseries thefts and crimes of violence. Only 14 of 169 respondents reported nonviolent series crimes.

Series incidents relating to the victim's job

Each of the special categories of personal series incidents has characteristics that set it off from the other groups, which suggests that different ways may have to be developed to probe more fully into the surrounding circumstances. The 52 victims of job-related series incidents can be classified according to the following categories:

- Law enforcement: police officers, narcotics agents, security guards, etc. (23)
- Institutional: social workers, nurses, parole officers, teacher's aids, etc. (9)
- Recreation: bartenders, managers of fast food restaurants, etc. (7)
- Transportation: bus drivers, railroad employees (6)
- Union organizers (2)
- Miscellaneous (5)

The great majority of the offenders in these incidents were strangers to their victims. This was true in 52 of the 77 incidents that were classified as job-related. Even though the relationship between victim and offender is based on the most recent incident, there is no reason to believe that this preponderance of stranger offenders would vary if each incident could be described separately. Exceptions to this relationship were more frequently found in institutional settings where social workers or parole officers might be at least casually acquainted with the offender. Although the evidence is fragmentary, there is a suggestion that the offender in these types of series

14. Victims of personal series incidents: Percent distribution of other crimes occurring to victims by classification of victim*

Other crimes occurring to victims of personal series incidents	Total		Job related	Domestic violence	Children	Miscellaneous
	100%	(169)	100% (52)**	100% (46)	100% (37)	100% (34)
No other incidents	31	(52)	31	35	32	24
Nonviolent series only	3	(5)	2	2	5	3
Nonseries	61	(103)	63	59	59	62
Violent only	11	(19)	10	7	16	15
Theft only	30	(51)	29	37	24	29
Combination	20	(33)	25	15	19	18
Nonviolent series and nonseries	5	(9)	4	4	3	12

Note: Percents may not add to 100 because of rounding. Numbers in parentheses refer to number of cases in a cell.

*Does not include any household crimes reported by other household members.

**Includes one victim who also reported a domestic violence series.

incidents is more likely to be the same person than in the more anonymous relationships involving police officers or bus drivers.

The job-related personal series victimizations had the greatest number of multiple series of the four groups under consideration—77 incidents for 52 respondents.[8] A substantial number of nonseries or regular incidents were also experienced by these 52 individuals, but these incidents were not evenly distributed. Seventeen persons reported no single incidents in addition to the personal series, although there was one instance of a series theft. Twenty respondents suffered a total of 50 incidents of violent crime, and all but a handful of these were job-related. There were also 70 reports of thefts experienced by 29 persons. A substantial amount of overlap occurred in these 2 categories with 14 persons being victimized at least once by both theft and violence, including one individual who also reported 2 series thefts. The concentration of nonseries victimizations is illustrated by the fact that 6 persons accounted for 32 of the 50 violent crimes, and 7 persons reported themselves as victims in 37 of the 70 thefts.

Nearly half of the victims occupied jobs in the law enforcement area, and here one might expect more than an average of approximately two series incidents per respondent (44 incidents experienced by 23 respondents). There were cases where such respondents reported either series or nonseries offenses during each reporting period. The most notable example was a police officer in a small northeastern city who reported 6 series crimes and 10 related nonseries crimes during his 7 times in sample. Other police officers reported offenses each time in sample, but their stay was limited to two or three periods. However, there were only two cases in which police officers reported three separate events of assault during a specific reference period—the equivalent of the minimum reporting number for a series of crimes. In contrast, there were police officers who reported series or related nonseries incidents on only a few of the total number of interviews in which they participated. For example, in one of the largest cities of the country, a police officer reported no incidents in two of the four times the household was in the sample. Overall, the 23 law enforcement respondents were interviewed a total of 94 times and reported no violent crimes on 44 of these occasions.

Throughout this report it should be kept in mind that there may be other law enforcement officials who as NCS respondents did not report a single series crime. While it is conceivable that police officers in small communities may experience substantially fewer victimizations than their big city counterparts, or none at all, this examination of microrecords suggests that the apparent underreporting among law enforcement respondents may extend beyond those who reported a series incident at some time during their stay in the sample.

Another case in point involved an employee of a private bus company who was in the sample six times. For four of these times his occupation was listed as bus driver. He reported no personal incidents related to his job until the last time he was interviewed when he was credited with one series incident and five related nonseries assaults. (With four additional incidents of larceny in the same reference period, it is perhaps understandable that he was not interviewed on the final occasion of the household's inclusion in the NCS.) What accounts for this explosion of crime reporting? Were his routes changed so that he was given a less desirable area? In this particular interview, he was described as a school bus driver; perhaps he drove charter buses previously. Or should this be ascribed to interviewer variance because the interviewer who conducted this particular interview had not been assigned to that household previously? These are necessarily conjectures which serve to underline the fact that we need to know considerably more than we do at present about the circumstances surrounding a report of a series of incidents.

A final note about the law enforcement component of this special sample. Reiss noted a higher proportion of police at the scene of the incident for series crimes known to the police than for nonseries crimes.[9] Our data suggest a possible explanation for this difference. In the first place, a greater proportion of series than of nonseries crimes are crimes of violence. Nearly one-fourth (44 out of 205) of the personal series incidents in our sample were reported by police officers or others in law enforcement occupations. We assume, without having the specific evidence in hand, that police officers make up a substantially smaller proportion of victims of nonseries crimes. In 24 of these 44 cases, "police on scene" was given in response to the question of who reported the incident to the police. Thus the disproportionate

[8]There were also four theft series victimizations reported by three of these respondents.

[9]Reiss, op. cit., p. 38.

tionate number of series victims who are police personnel or are in related occupations may account for the difference in this item.[10]

Obtaining a more accurate understanding of the extent of victimization of persons whose jobs make them more likely to experience multiple victimizations does not solve the problem of how to present such data. If a police officer or a bus driver is subjected to situations that meet the classification criteria for NCS crimes, but with such frequency that separate incident reports cannot be filled, how are these crimes to be treated in relation to the majority of crimes where separate reports are obtained? No matter how much we perfect the instrument to measure crime victimization, we will be left with a residue, perhaps smaller than at present, of incidents that cannot easily be amalgamated with nonseries crimes.

Series incidents involving domestic violence

Domestic series incidents are differentiated from other personal incidents primarily by the closeness of the relationship between victim and offender. As shown in table 15, a great majority of offenders in this sample were actually related to their victims, while most of the remaining offenders had developed firm social relationships as neighbors or good friends. Roughly half of all domestic series incidents and about four of every five violent series incidents occurring between relatives involved spouses or ex-spouses. In all of these cases the offending partner was the husband or ex-husband and the victim was the wife or ex-wife. While it was often difficult, if not impossible, to determine exact marital status at time of incident, it was frequently apparent by time of interview that the marriage had ended or was in the process of ending. At the time 17 of the 27 spouse-abuse incidents were reported to NCS interviewers, the victims were separated or divorced and living apart from their husbands.

15. Victim-offender relationships—Domestic series incidents

Type of relationship	Number of domestic series incidents
Relatives	35
Spouse or exspouse	27
Other	8
Nonrelatives	14
Neighbors	5
Boyfriend, date, etc.	5
Others	4

Regarding offender continuity in domestic series incidents, it should be obvious, given such a large proportion of spouse-abuse cases, that the majority of incidents were committed by the same offender. There were, however, some exceptions; in several crimes where the offenders were identified as neighbors, there was evidence that different persons committed the individual acts. Hence, it may be concluded from this small sample that for most but not all domestic incidents, data on offender characteristics could easily be obtained for each victimization in the series.

One notable feature of this group of series incidents is the rarity of multiple series victimization, either within a particular interview or across interview periods. Only two victims reported more than one domestic series; one respondent charged her ex-husband with two series incidents during the same 6-month period, while another reported being assaulted by her spouse in two consecutive periods.

Why were there so few multiple series incidents reported in the sample? Research in the field has shown that in certain domestic situations violence is a regularly occurring phenomenon. One possible explanation is that additional attacks occurred but were recorded as discrete events. The record shows that in a number of situations this was the case. In most of these households there was one additional related incident, often reported during the same or the following interview. In other households, however,a number of related crimes were reported. To illustrate, one respondent accused her husband of committing five crimes over a 6-month period; a series of burglaries, a larceny, and an armed robbery in addition to two violent series incidents! In another case involving attacks by neighbors, the series victim, his wife, and a child all reported separate but related assaults during the same interview. One victim of domestic series abuse reported a multiple nonseries victimization (three assaults committed by the same offender) in the next interview period.

These situations were the exception and not the rule. Victims of series domestic violence, spouses and others, usually experienced no other related incidents during their time in sample. As for their overall experience with nonseries incidents, 17 victims of domestic violence experienced no additional crime, whereas the remaining 30 were involved in 18 violent incidents and 51 thefts. Roughly one-third of the victims in the latter group reported at least one violent crime and one theft.

Another possible explanation, particularly relevant to intrafamilial violence, is that while there may be recurring acts of violence, they go unreported because they take place before or after the household is in sample. The likelihood of this transpiring is enhanced by the fact that, as previously mentioned, many households are not in sample the full 3 years. In fact, over half of those households reporting intrafamilial series violence were in the sample fewer than 4 times, and 13 households left immediately after the period in which the series incident was reported.

It should be pointed out, however, that longevity in the sample did not guarantee a more complete victimization history. There were a number of cases involving victims in households in the sample six or seven times who reported only one or two series or related nonseries crimes.

The case history of one of these victims is particularly revealing and suggests another possible explanation for the seemingly isolated nature of domestic series victimization. Only after the respondent had been interviewed five times did she report a series of violent beatings by her husband and acknowledge that the attacks had taken place on a regular basis for many years. Thus, through 2 years of interviewing the respondent had failed to report a steady stream of violent attacks committed by her spouse. It is entirely possible that many more incidents of domestic violence, series and nonseries, go unreported because of fear of reprisal, embarrassment, lack of interviewer-respondent rapport, or other reasons.

Finally, the uncommonness of multiple series victimization might, in part, result from the fact that many of those who publicly acknowledge the existence of domestic discord appear to take steps to prevent any further occurrence. As noted earlier, a number of victims of spouse abuse were separated or divorced at the time of interview, and were reporting on conditions which existed before the breakup of the marriage. In addition, information obtained from the NCS control card and incident summaries also shows that victims of other types of domestic series violence may have prevented further abuse by moving to another location or obtaining assistance from law enforcement authorities. Altogether 10 victims of series abuse appeared to have acted in a positive manner to thwart any further attacks.

Perhaps the greatest problem faced with regard to further work in the area of domestic violence is the sensitivity of the subject matter. Many respondents, particularly those who continue to live under the threat of attack, are too embarrassed or frightened to talk about the problem in their own homes. Even reporting an attack to an NCS interviewer, in some cases, is an act of personal courage. Further probing, either at the time the incident is reported or in subsequent interviews, may very well alienate or even endanger some respondents.

Series incidents between children

Another group of readily identifiable victims of personal series crimes are children. We have limited this group to those situations where the offenders were also children, although in two instances involving multiple offenders the oldest were above 20 years of age. There were 37 individual victims and 42 series incidents, with 5 persons suffering 2 personal series victimizations during their period in the sample. The great majority of incidents (30 out of 42) either took place inside school, on the school grounds, or on the way to and from school. The ages of the victims tended toward the lower end of the spectrum: 21 were 12 or 13 years old and thus their incidents were obtained from proxies, 11 more were 14, and only 10 were 15 or older. The relationship between victims and offenders by age, sex, and race for each of the 42 incidents is shown in table 16. The age comparison is less precise than for the other two characteristics because respondents were asked to assign offenders to age categories. Thus, if the respondent's age was included within any part of the range of the offender's ages, the age was considered to be the "same." With these qualifications, children involved in series crimes tended to be of the same age and sex, although the relationship by race was more evenly divided. The offenders in series crimes between juveniles were about evenly divided between those who were described as strangers and those considered to be casual acquaintances. In only 5 of the 40 incidents where this relationship was reported did the respondent indicate that the offender was well known.

16. Relationship between victims and offenders in personal series crimes, by selected characteristics

Relationship	Age	Sex	Race
Victim-offender characteristic:			
Same	30	34	22
Different	8	5	17
Offender characteristic unknown	4	3	3

Multiple personal series victimization for this group is a relatively rare phenomenon, and four of the five instances occurred in only one case—four cases of violent threats during the same period when the respondent suffered his only series victimization. Interestingly, there were no reports of victimization for anyone in this household during the remaining two periods the family lived at that address. Verified cases of series involving extortion of lunch money from school children were only reported twice. In one case the situation was resolved after police were notified; in the other, the household moved before the next interview. Both of these victims were 12 years old; if the bulk of such activity occurs to younger children, then the survey cannot measure it at present. Another possible reason for underreporting, if it does occur, is that 12- and 13-year-olds may be ashamed or afraid to tell their parents who, of course, are proxy respondents for children of these ages.

There is very little evidence to explain the largely one-series phenomenon exhibited by these respondents. Aside from the lunch money problem that was resolved after police were notified, one child was transferred to a different school and another, after enduring a series of threats, turned on his assailant and administered a beating which presumably curtailed that particular line of activity. In a number of cases, the household moved away so that the subsequent history was not known; in many more cases it was known and very little, if anything, occurred. Part of the explanation might be that as a child grows older, the concept of what constitutes a reportable offense is redefined in his mind. As noted earlier, most of these victims were 14 or under. Older children may be better able to cope with potentially threatening situations and avoid them or, if the confrontations occur, do not consider themselves to have been the victims of violent crimes.

One issue that has bothered many is that of the triviality of some of the reported crimes in the NCS. This is more than an issue affecting series crimes, of course, but a number of these, as reported in interviewer summaries, underline the desirability of examining more closely the conceptual boundaries of crime. Does being pushed into a snowbank or being verbally threatened for dating a particular boy fall within these limits? The fact that only 10 of the 42 series incidents were reported to the police (although many more were reported to school authorities) is worth noting in this context.

Victims of personal series crimes also report nonseries crimes and series crimes involving theft. As was noted earlier for job-related victimizations, these additional crimes tend to be concentrated rather than evenly distributed. Of the 37 young victims of personal series crimes, 12 reported no other incidents at all, 2 were victims of a series of thefts and no other incidents, 14 respondents were also victimized in 24 individual incidents of violence, and 17 persons reported a total of 38 separate thefts. Among those reporting nonseries incidents, there were eight persons who experienced at least one of each general type of crime, including one victim who also experienced a series of thefts. As with the personal series crimes, many of the nonseries crimes were school-related—12 violent crimes reported by 8 persons and 23 theft

[10]The "true" percent of "police on scene" is perhaps even higher. In our sample there were nine cases where a household member reported the incident to the police, which could have meant the victim in some cases. Also, six respondents interpreted the question whether the incident was reported to the police differently, although their responses were equivalent to "police on scene." They said, in effect, that the incident was not reported to the police, because it didn't have to be since they were the police.

crimes victimizing 14 youngsters. In addition, the three series thefts took place in school settings.

Miscellaneous series incidents

Of the series incidents making up the miscellaneous category, 15 were "street crimes," i.e., predatory robberies and assaults committed by strangers; 6 involved coworkers; and 5 were assaults or attempted assaults by acquaintances in a social setting, such as an encounter in a bar or on a playground. The 11 remaining incidents could not be categorized.
Although these incidents were characterized by a variety of situational experiences, the victims tended to be alike. Specifically, 27 of the 34 victims were males and 20 were in their teens or twenties. Older victims were relatively uncommon; only four persons age 50 or older reported a miscellaneous series incident.

The relationship between victim and offender was not characterized by the degree of intimacy evident in domestic series incidents but neither did it approach the anonymity of job-related offenses. In approximately half of the crimes the offenders were either strangers or were known by sight only. And although the evidence is sketchy, there is little reason to believe that in most of these incidents the events in series were committed by the same individual or individuals. Persons known to the victim committed 17 of these series crimes; quite often they were known through casual social contacts or from work. In some cases the same individual was responsible for all attacks, whereas in others, for example a series of threatened assaults involving a supervisor and a number of his employees, different individuals were involved.

As was true for domestic and juvenile violence, examples of multiple-series incidents were quite rare. The 37 incidents were carried out against 34 victims, with 3 victims each suffering 2 crimes. It should be noted that each of these victims reported both series as having occurred during the same 6-month period. Thus, there were no cases of multiple miscellaneous series occurring in two or more reference periods, nor was there any series overlap, i.e., victims reporting other types of violent series incidents along with a miscellaneous series either in the same or different periods. Five victims of miscellaneous series reported one additional nonviolent series involving theft or attempted theft, but there was no evidence that the two types of series events were linked. Finally, one victim reported multiple nonseries victimizations—three attempted robberies—during the same reference period as the series incident.

Understandably, it is even more difficult to attempt to explain the short-term nature of series victimizations for this diverse collection of crimes than for others. One might hypothesize that the 12 persons who were the victims of the 14 series involving predatory robbery or stranger assault took steps to reduce their vulnerability, but the NCS record does not provide such information. In one of the six series incidents involving coworkers, the respondent reported that the offender was fired for incompatability; in three others the victim was no longer associated with the firm where the incident took place, but there was no evidence that this change was in any way related to the series incident. Household mobility made it impossible to track the subsequent history of a number of victims of miscellaneous series incidents. Victims of miscellaneous series incidents reported a total of 67 discrete nonseries crimes, 25 personal acts of violence (13 respondents) and 45 thefts (20 respondents). Eight persons experienced at least one of each major type, whereas nine others reported no individual incidents at all.

Conclusion

This exploratory examination of violent personal incidents occurring in a series, though revealing a wide diversity of situations has, at the same time, suggested certain commonalities that may provide a basis for further scrutiny of such incidents. Three broad categories have been distinguished—cases relating to the victim's job, cases involving young children, and those classified as domestic violence. Unfortunately, the residue consists of incidents that have much less in common. Nonetheless, we feel this exercise has provided insights, not available elsewhere, as to what additional kinds of information need to be obtained in order to better understand the nature of series incidents. One consequence of developing and administering supplemental inquiries for victims of series crimes may be that the number of incidents classified as series can be substantially reduced, possibly by as much as one-third. This will still leave the perplexing problem of the "true" series cases and how they should be presented in relation to the regular crimes in the NCS.

One of the most interesting aspects of the series issue is the general lack of repetitiveness of personal series crimes. This study has made some tentative suggestions (at least in a few cases) as to why this may be so, but definitive answers are still not possible. For job-related crimes, although they have more repeat series than any other type, intuition suggests that there is substantial undercounting. Much of this lack of duplication may have a reasonable explanation, but, if so, more probing inquiries are needed to discover the reasons.

Other problems with the series concept have been noted before, and these have been confirmed by this investigation. Interviewer variation, a problem in all surveys, appears to be evident in certain sequences when a change of interviewers brings forth a flood of crime reports where before there was apparent crime-free serenity or reveals the existence of domestic violence of long standing. It is also evident that some interviewers are not applying the series concept properly. This is supported by the narrative summaries where a series of incidents is said to have included events both with and without weapons, for example, or where the interviewer reports the most memorable incident, rather than the most recent. Indeed, in some cases with law enforcement respondents, separate reports are filled for especially noteworthy incidents and the rest lumped together as one series incident. Perhaps, under present conditions, this is not a bad approach. The incidents involving children, especially, raise the issue of what constitutes a crime in the NCS. If the minimum age is ever lowered, this problem of triviality will become more compelling. Contributing to response error are such other matters as respondents who learn that denying incidents contributes to shorter interviews, and the sensitive nature of domestic incidents which are undoubtedly grossly underreported but which may be improved upon with better questions and/or more thorough interviewer training. These are not exclusively problems of series crimes, of course, but their impact is perhaps greater in this area.

A slightly revised version of the NCS questionnaire is scheduled to be introduced in January 1979. There will be changes in the questions which ask about the number of incidents and the seasons when they occurred. Interviewers will first ask for the number of incidents in the series and record the exact number of incidents or the respondent's best estimate. In well over half of the series questionnaires examined so far, the specific number appears either in the screening questions, on the control card, or in the interviewer summary or other note space on the questionnaire. The second change will require that the number of incidents be allocated by the quarter of the year in which they occurred. The effect of these modifications will be to facilitate the incorporation of series data, as presently collected, with the regular NCS crimes.

In the long run, it seems that special questionnaires need to be devised to probe more fully into each report of a series. This could be accomplished by providing interviewers with a special supplement which would be administered whenever a series incident is reported. At the experimental stage, it might be preferable to identify these cases in each of the 12 regional offices and have a supervisor or senior interviewer return to the household. In fact, it might be desirable to consider this or some similar arrangement as a permanent procedure, rather than leaving the entire determination of what constitutes a series crime as decentralized as it is at present. In the current sample, there are about 100 series cases reported each month nationwide, so that no single office would have more than 10 or 12.

The design of such a questionnaire will not be easy because, as this investigation has implied, probing questions that are appropriate for some respondents will be inappropriate for others. Persons whose series incident is job-related need to be queried more about their job and the circumstances surrounding it, whether the particular tasks they do have been altered, and whether the location or time of day has undergone change. These questions would obviously be irrelevant for young children where the main concerns are the relationship with their assailants, whether or not the offenders are always the same persons, and what, if anything, the victims do to avoid repetition of such events. Outside of changing jobs, the former category of series victims has less ability to control these matters. Where domestic violence is involved, the characteristics of the offender can be collected once and be applicable to all incidents, but the sensitive nature of the situation and the steps respondents take to reduce risk, and when they take these steps, become especially important considerations. One desirable outcome of such an in-depth inquiry might be to reduce further the number of series reports by enabling respondents to sort out the details of each incident so that individual incident reports can be filled. Where series crimes are too numerous for that, the classification may be improved so that different types of crime are not commingled in one report.

We would also consider it desirable to investigate more thoroughly the reasons for the lack of continuity of series incidents from one reference period to the next. This could be restricted to households in the sample for the last interview. Interviewers would be provided with information about any series incidents in the previous interview, but would only utilize it if a series incident was not reported on the return visit. Again, it might be preferable for the office to identify such cases from filled questionnaires and assign a supervisor to conduct a reconciliation interview which would seek to ascertain the reasons for the absence of a series report. Because a significant proportion of series households move before the next interview, it would be necessary to attempt a followup of a subsample of movers in any such experiment.

This report has deliberately raised more questions about personal series incidents than provided answers. In the present state of knowledge of series crimes, this is entirely appropriate and, in fact, a necessary prelude to further research into this problem. We have yet to examine the quantitatively larger area of series thefts, although the documents for this investigation are now available. Once this is done, we believe that the focus of activity should shift from a preoccupation with what has been collected in the past to the development and testing of a new collection instrument.

Summary of series and nonseries incident reporting, 1972–75*

by ALBERT J. REISS, JR.

Some of the main features of series and nonseries incident reporting in the NCS from 1972 to 1975 are explored with the object of determining technical problems in survey reporting of victimization by crime. Comparisons also are made between series and nonseries victimizations for offense and personal and social characteristics of victimization. The major findings of this report are summarized below.

1. There has been a decline in the annual series incident reporting from 1972 to 1975. For the first full year of the survey, series reporting rates were higher than they have been in any month since that time. It appears that the series reporting rate may stabilize somewhat below 3 percent of all reported incidents if current NCS procedures continue to be followed.

2. Several survey design factors appear to determine victim reporting rates.

- The interviewer procedures followed may affect series reporting rates. The sharp decline in the percentage of incidents that are series since the first year of the survey is consistent with Census Bureau reports that interview practices were altered during the first year to reduce series reporting rates. However, these practices may not account for the smaller decline during the second year or lead to a compensating increase in multiple nonseries incident reporting.
- Personal interviews produce higher rates of series and nonseries victimization per 1,000 interviews than do telephone interviews.
- There are substantial effects of both interview and bounding status on incident reporting rates.

For every interview period, the nonseries and series incident rates per 1,000 interviews are greater for personal than telephone bounded and unbounded interviews, demonstrating a clear effect of type of interview independent of the bounding effect. The highest reporting rates for both series and nonseries incidents occur in unbounded personal interviews. Unbounded telephone interviews produce the next highest reporting rates followed by that for bounded personal interviews with bounded telephone interviews having the lowest rates. These differences hold for all interview reporting periods.

3. Although series victimizations are reported for all major types of crime, both actual and attempted, they are disproportionally concentrated and underrepresented in some personal and household crimes when compared with nonseries victimizations.

- Among robbery victimizations, robberies and attempts to rob without a weapon comprise a substantially larger proportion of the series than of the nonseries incidents.
- Assaults are proportionally more of the series than of the nonseries victimizations. However, the difference occurs for only selected types of assaults: serious assault with a weapon but without theft, minor assaults, all attempted assaults.
- Purse snatching and pocket picking occur proportionally less often among series than nonseries incidents.
- Theft and attempted theft of motor or other vehicles is infrequently reported as series in comparison with the distribution for nonseries incidents.
- In the aggregate, attempted crimes are more likely to be reported as series incidents than are actual crimes. This is true for all types of crime other than purse snatching, larceny, and burglary among the major crimes.
- In the aggregate, crimes without theft are twice as likely to be reported as a series as are crimes with theft.
- The highest series reporting rate occurs for attempted crimes without theft, where 1 in 10 incidents are reported as series incidents.
- The theft status of a crime is more important in series victimization reporting than is its success in completion. Crimes without theft, whether actual or attempted, are more likely to be reported as series victimizations than crimes with theft, whether actual or attempted.

Overall, series reporting then is disproportionally concentrated in a few selected types of crime incidents that do not involve victimization by theft, particularly in the crime of attempted assault.

4. The amount of series victimization reported also varies among the types of crime incidents.

- The percent of series incidents with 11 or more reported victimizations is greater for crimes involving contact with persons than for those without contact.
- Reports of attempted series crimes, except for assaults, involve on the average a smaller number of series incidents than do reports of actual series crimes.
- Series incidents without theft have a larger average number of reported incidents than do those with theft.
- Series incidents without theft are, on the average, larger both for the actual and attempted crimes than are actual and attempted series incidents with theft.

5. Adding estimates of the number of incidents involved in series victimization to those for nonseries victimization would increase the total number of victim incidents by 18 percent. Series victimization generally makes a greater contribution to the victimization rate of crimes that involve contact with persons than those without contact.

6. Errors in the reporting of the occurrence of events and their correct placement in time (commonly referred to as recall bias) are evident in both series and nonseries reports of incidents for both interview periods and reference periods of the occurrence of events. There is a disproportionate number of both series and nonseries crime incidents within the reference month nearest to that of interview, but the bias is roughly equal for series and nonseries incidents.

7. The report of victimization within any period of time in the NCS is a function of the length of time a person or a household has been in sample.

- Among persons reporting only a single series or nonseries victimization, their reporting is disproportionally concentrated within the first interview period. Series victimized persons are more likely than nonseries victimized persons to disproportionally concentrate their reports of victimization in their first interview. The difference between series and nonseries reporting increases with number of interviews. Moreover, among nonseries victims, after the first interview, the reporting of nonseries victimization tends to be equally concentrated in successive interview periods regardless of the number of interviews. This first interview effect is only partly attributable to unbounding.
- Series victimized persons over time show a propensity to nonseries victimization as well. Over time, persons who experience one or more series incidents will also experience a substantial number of nonseries incidents, though most will not be the same kind as those reported in the last series incident. There is, however, a class of persons who are exclusively series victimized over time; that class is 54 percent of persons who ever report a series incident and 46 percent of households.

8. The reporting of series as compared with nonseries victimization varies somewhat with the personal and social characteristics of the victim.

- More of the series than of the nonseries victimizations are reported by whites, males, 12- to 15-year-olds, the never married, and persons with less than a high school education.
- For assault with theft and larceny with and without contact, a substantially greater proportion of the series than of the nonseries victimizations are reported by 12- to 15-year-olds regardless of race and sex.
- The rate of nonseries victimization per 1,000 interviews is greatest for whites and blacks (but not other nonwhites) who are college educated. Among victims of assault, college educated males have the highest rates of victimization reported per 1,000 interviews and the rates are highest for all college educated reporting victimizations by larceny without contact. Generally, high school graduates have the lowest rates of victimization per 1,000 interviews, with college educated the highest, and those with less than high school education intermediate rates, but the pattern is not altogether consistent among the types of crime against persons. Despite the inconsistencies, it is possible that the college educated generally are more responsive to the survey procedure, reporting more accurately their victim experiences.

9. On first reporting any series victimization, one-fourth of all persons and 29 percent of all households also report some other nonseries victimization. This represents a substantial multiple victimization among persons already reporting high multiple victimization as series victimization.

10. Repeat victimization 6 months following the first report of victimization is relatively uncommon for series victimized persons and households and for multiple nonseries victims.

- Seventy-six percent of series victimized persons and 73 percent of series victimized households reported no series or nonseries victimization 6 months later.
- Seventy-five percent of persons reporting three or more nonseries victimizations and 67 percent of households reporting three or more nonseries victimizations reported no series or nonseries victimization 6 months later.

11. Among repeat series victims, there is a high persistence of victimization by the same type of crime, either by series or nonseries incident reporting.

- When persons first report series victimization and subsequently report nonseries victimization by the same type of crime, there is a substantial reduction in the amount of nonseries victimization reported for that type of crime. Only 6 percent of persons first reporting series victimization and subsequently reporting nonseries victimization for that type of crime report three or more separate crime incidents.
- It is relatively uncommon for persons who report a series victimization to subsequently report series victimization by some other type of crime. Repeat series victimization is thus largely limited to the same type of crime.
- Among repeat victims first reporting series victimization, about one in four persons report nonseries victimization by some other type of crime 6 months later.

12. The patterns of repeat series and nonseries victimization for households first reporting series household victimization are similar to those for persons.

- Among series victimized households reporting repeat victimization 6 months later, 65 percent report victimization by the same type of crime, either series or nonseries. This is only somewhat below the 72 percent reported for persons.
- There is some variation by type of crime. Series household larceny shows the greatest propensity to repeat victimization (73 percent) by the same type of crime, followed by series burglary (59 percent); series motor vehicle theft shows relatively little propensity to repeat vicimization by the same type of crime (20 percent). Series motor vehicle theft may be easier to prevent than other crimes against households since it is a relatively uncommon event.
- When households first report series victimization and subsequently report nonseries victimization, there is a substantial reduction in the amount of nonseries victimization reported. Only 7 percent of households first reporting series victimization report three or more separate incidents 6 months later.
- Households originally reporting series household larceny are the most likely to report repeat victimization by nonseries incidents (56 percent) for the same type of crime originally reported as a series incident. Thus victims of both personal and household larceny show the greatest tendency to report repeat victimization by nonseries incidents for the same type of crime originally reported as a series incident.

13. The substantial reduction in the amount of victimization following first report of series victimization suggests that series respondents probably overestimate the number of incidents in a series, though such overestimation may be greater for reports of 5 or more incidents in a series than for estimates of 3 to 4 incidents.

14. The propensity of persons and households to move is affected by both personal and household victimization. Where there is high multiple victimization of the household and its members within a 6-month period, a majority of households move.

*Excerpted from Albert J. Reiss, Jr., "Summary for Victim Reporting of Series and Nonseries Incidents Over Time," Technical Report #3 (April 1977) and Data Report #6 (June 1977), produced under Analytical Studies in Victimization by Crime (LEAA Grants No. 75–55–99–6013 and 77–55–99–6012).

- A person's residential mobility often cannot be undertaken independent from that of the common household. Of the persons first reporting victimization, 24 percent moved during the 6 months following their first report of victimization. Among the persons moving, only 25 percent moved while their household stayed so that 74 percent of all victimized persons moved as part of a household move.
- Of the households where one or more members reported victimization of the person, 86 percent were households where there was no household victimization; 12 percent were households reporting two or more household victimizations; and 2 percent were households reporting three or more.
- A substantial proportion of persons first reporting victimization come from households with a high victimization rate, since 12 percent, or one in nine, come from households with three or more personal and household victimizations in a 6-month period.
- Among households where one or more members report personal victimization by crime, the higher the level of household victimization, the more likely a household is to move within the next 6 months. Only 16 percent of the households where a member reports personal victimization and the household reports no victimization moved within the next 6 months, but 35 percent of those reporting three or more victimizations move within the next 6 months.

Chapter 2

The maximum personal visit/ maximum telephone interview experiment

Introduction

The present method of interviewing used by the National Crime Survey (NCS)—primary use of personal visit interviewing with secondary use of telephone interviewing—is a relatively expensive survey methodology. The cost advantage of telephone interviewing is generally recognized, but several questions concerning the quality of the information obtained by this method, especially where sensitive questions concerning victimization are involved, are still a matter of study. The Law Enforcement Assistance Administration, in an effort to find alternative methods to collect victimization information, commissioned the Bureau of the Census to explore the relative advantages of the telephone and personal visit interview methods to determine whether the former might be applicable to victimization surveys.

During June 1976 to July 1977, as part of the regular NCS field operation, the Bureau undertook a controlled experiment to determine the relative effects of maximum telephone and maximum personal visit interviewing on the reporting of personal and household victimizations. The three papers reproduced in part in this chapter provide a description of the experimental design and a summary of the findings.

The first selection by H. F. Woltman and J. M. Bushery describes in detail the methodology employed in the experiment and provides an analysis of the effects of the two experimental groups—maximum personal visit interviewing and maximum telephone interviewing—in comparison to the regular NCS procedures on victimization reporting. The second selection by A. G. Turner focuses on a comparison of the two experimental groups and is primarily concerned with the question of whether the two methods of interviewing had an effect on victimization reporting. The Turner analysis is based on a special subpopulation constructed to adjust for procedures used when conducting the experiment. The final selection by J. Linebarger reports on the findings of a quality control recheck study designed to identify interviewer errors in the implementation of the experiment.

Results of the NCS maximum personal visit/maximum telephone interview experiment*

by HENRY F. WOLTMAN and JOHN M. BUSHERY

Introduction

Objectives of the experiment

The purpose of this research is to assess the differences in reporting criminal victimizations due to interviewing respondents using three different interview procedures:

- a method which maximizes the use of personal visit interviews, using the current field organization
- a method which maximizes the use of telephone interviews using the current field organization
- the current NCS interview procedure, which includes a mixture of personal visit interviews and telephone interviews.

It must be emphasized that the maximum telephone interview procedure was designed for use within the existing field organization and normal survey procedures. No special training in telephone interviewing techniques was provided; however, a memorandum containing some technical advice on telephone interviewing was sent to the NCS interviewers at the beginning of the experiment (NCS National Interviewer's Memorandum No. 76–7). Thus, the evaluation of this experimental procedure can only be used to determine whether or not, within the constraints of existing field procedures, telephone interviews can be used to replace personal visit interviews.

In the analysis, it has been assumed that if two experimental procedures elicit reports of victimizations at different rates, the lower victimization rate is an underestimate. That is, the higher the estimated victimization rate is for a specific type of crime, the less biased that estimate is.

Description of the data analyzed

The data on which these analyses are based were collected from three subsamples of the NCS sample. Two systematic subsamples of NCS segments, each subsample comprising one-twelfth of each monthly NCS sample, were selected for this experiment.

In one subsample the use of personal visit (PV) interviews was to be maximized. This subsample is hereafter referred to as the "PV group." Another subsample in which the use of telephone interviews was to be maximized, is hereafter referred to as the "telephone group." Each of these subsamples contains one-twelfth the number of NCS segments in the full NCS sample. The third subsample consists of the remaining five-sixths of the full NCS sample. This subsample was interviewed using the standard interview procedure for the NCS and serves as a control for this experiment.

Data were collected by interviewing each sample household twice in the period July 1976–June 1977.[1] Thus a household in the first panel of the sample was interviewed in July 1976, and again in January 1977. Victimization counts and rates were obtained by combining the data from all four collection quarters during which the experiment was conducted. The level of these estimates is approximately equal to the level of annual estimates in the NCS. The victimization rates in this report represent a sort of "moving average" annual rate, representing crimes occurring during six overlapping 12-month time periods: January–December 1976, February 1976–January 1977, March 1976–February 1977, April 1976–March 1977, May 1976–April 1977, and June 1976–May 1977. For example, households in panel 1 of the sample were interviewed in July 1976 and January 1977 and provided data for the estimates involving crimes that occurred during the period from January–December 1976.

The NCS-weighting operation involving all stages of the normal NCS estimation procedure was performed separately for each of these groups, so that three independent sets of estimates of victimization counts and rates at the national level could be obtained. This weighting operation included corrections for the different sample sizes of the three groups.

The analyses in this report deal with all households in the NCS sample and the findings of this report can be interpreted as indicating the results to be expected if either of the experimental interview procedures were used to replace the standard NCS procedure. A related report by the Statistical Research Division[2] deals only with households interviewed during the previous enumeration. That is, previous noninterviews, replacement households, and new construction households have been excluded. Such an analysis provides a more direct measure of the difference to be expected between telephone interviewing and personal visit interviewing when there are no operational reasons to restrict application of either procedure. It can perhaps be considered a more "pure" comparison of maximum telephone versus maximum personal visit interviewing.

Data involving noninterview status by age and race for each of the designated interview procedures have also been examined in an effort to determine whether either experimental interview procedure may be more or less effective in obtaining interviews with specific subgroups of the population.

Description of methods of interviewing used for the experiment

Two experimental groups and a control group were selected for this experiment; however, only households in returning rotation groups were eligible for the experiment. All households in incoming rotation groups were interviewed using the standard NCS procedure. This was done for several reasons:

- There is no information available concerning such households; NCS control cards must be completed and telephone interviews are difficult to perform without names and addresses, and information about telephone numbers and availability.
- A telephone interview is not necessarily the most desirable way to introduce a household to the survey.
- The incoming rotation groups are used only to provide a "bound" for subsequent interviews and, as a result, are not used to produce estimates. Data obtained from these rotation groups would therefore not be available unless special processing were used.

Under the maximum personal visit procedure, each person belonging to a household in the PV group was required to be interviewed by personal visit.[3]

This differs from the standard NCS procedure which requires only that the household respondent must be interviewed in a personal visit. Any other respondents available for interview at that time are also interviewed. Call-backs for persons not interviewed during the initial contact of the enumeration period may be carried out by telephone or in a personal visit, according to the judgment of the interviewer. In the PV group, on the other hand, telephone interviews were allowed only as a last resort to avoid noninterviews.

Under the maximum telephone interview procedure all interviews were to be conducted by telephone, including the interview of the household respondent. Only households which had been interviewed during the previous enumeration—6 months prior—were eligible. Households not interviewed in the previous enumeration were to be interviewed using a procedure essentially similar to the standard NCS procedure. Ineligible households included replacement households for movers, households at addresses added to update the sampling frame for new construction, and households which were previously type A or B noninterviews. In these households, the household respondent and any other available persons were to be interviewed by telephone if at all possible. Other households not to be interviewed by telephone included households with no telephone and households that had indicated telephone interviews were unacceptable.[4]

Households in the control group were interviewed following the standard NCS procedure, the mixture of personal visit and telephone interviews, which has been described above. Note that none of the callbacks in the control group were required to be by telephone.[5]

Reliability of the estimates

Variances of estimates presented in this report

The response rates and noninterview rates shown in tables 18–24 are estimates based on the three subsamples used in this study. An approximate variance estimate for each of these estimates can be computed as

$$\mathrm{Var}(P) = \frac{P(100-P)(DEF)}{N}$$

where P is the estimated response rate or noninterview rate, N is the number of sample households or persons on which the rate is based, and DEF is the design effect.[6]

The estimates of primary interest are the differences between the victimization rates elicited by the standard NCS interview procedure and those elicited by the two experimental procedures. Since the samples for all three procedures are approximately uncorrelated, the variance of the difference between the victimization rate for the control group and the rate for either experimental group can be written

$$\mathrm{Var}(\bar{V}_C - \bar{V}_E) \doteq \mathrm{Var}(\bar{V}_C) + \mathrm{Var}(\bar{V}_E)$$

where $\bar{V}_C$ is the victimization rate of the control group and $\bar{V}_E$ is the corresponding rate for the experimental group in the comparison.

The variances of these estimated victimization rates can be computed as follows:

$$\mathrm{Var}(\bar{V}_C) \doteq [1.2(b)(\bar{V}_C)(1000-\bar{V}_C)]/N_C$$

$$\mathrm{Var}(\bar{V}_E) \doteq [1.2(b)(\bar{V}_E)(1000-\bar{V}_E)]/N_E$$

where b represents the inverse of the sampling fraction for the full NCS sample, corrected for design effects. The value used for b (=1718) was obtained from the April 1, 1977, memorandum, "Generalized Standard Errors for Annual Data from the National Crime Survey (NCS)—National Sample." The terms N_C and N_E represent the size, as estimated from the control group and the experimental group, of the subgroup for which the victimization rates were estimated. Note that an estimated victimization rate close to zero would result in an estimated variance close to zero. This would probably cause a substantial understatement of the width of the true confidence interval.

Confidence intervals of the differences between victimization rates

A 95-percent confidence interval has been constructed for each contrast between victimization rates for the control group and each experimental group. Each of the tables dealing with crimes against persons contains 23 different contrasts between the control group and the PV group and 23 contrasts between the control group and the telephone group (except for the more detailed tables: 26, 31, 41, and 42). Although the 23 contrasts between the control group and each experimental group are not independent, one would expect, on the average, that for each table about 1.15 of the 23 confidence intervals around the contrasts between the control group and an experimental group would not include the average value of the difference between victimization rates that would be obtained from repeated replications of this experiment.[7]

Similarly, each of the tables dealing with household crimes contains 13 contrasts between the control group and each experimental group. For household crimes, one would expect, on the average, about 0.65 of an erroneous confidence interval in each table, or, in other words, a "per batch" error rate of 65 percent.

In order to ensure a 5-percent "per batch" error rate, it is sufficient to require each "comparisonwise" error rate to be $5/M$ percent, when there are M comparisons per batch. To achieve a 5-percent "per batch" error rate for crimes against persons, the "comparisonwise" error rate must be held at $5/23 = 0.217$ percent. This would of course result in substantially broader confi-

*U.S. Bureau of the Census memorandum, December 9, 1977.

[1]Households leaving the sample in July–December 1976 and households entering the sample in January–June 1977 were interviewed only once.

[2]Editors' note: See the next selection in this volume entitled "An Experiment to Compare Three Interview Procedures in the National Crime Survey."

[3]Proxy interviews were conducted for persons 12 to 14 years old in all three groups, but the designated interview procedure was still to be used in these cases.

[4]During the first interview of the experiment (conducted in July-December 1976), an attempt was made to persuade respondents in these households to allow subsequent interviews to be conducted by telephone.

[5]In January and February 1977 the severe weather and snow necessitated more telephone interviews in the control group and PV group than would have ordinarily been allowed. In some areas the entire household was interviewed by telephone. For example, in Buffalo no personal visits could be made because travel was restricted by the local government.

[6]For the type A noninterview rates, and rates based on households, the value of $DEF = 1.2$ has been used. This value was obtained in the CPS intraclass correlation study but should apply to the NCS because of the similarity of the two sample designs. The design effect for the type Z noninterview rates has been estimated to be $DEF = 1.4$.

[7]For those readers for whom the inherent dependence of the comparisons makes this statement problematic, we suggest reading pages 86-87 of the paper by John Tukey: "The Problem of Multiple Comparisons." Here error rates per comparison, per batch, and batchwise are discussed. Following the rationale presented by Tukey, if we consider the 23 comparisons for crimes against persons between the control group and an experimental group as one "batch" and if the experiment were repeated 100 times, a 5-percent comparisonwise error rate would result in about 115 erroneous confidence intervals in these 100 batches. The "per batch" error rate would be 115 percent.

17. Coverage ratios for the NCS maximum personal visit-maximum telephone interview experiment

Age	Total population age 12 and over			Whites			Nonwhites		
	Procedure			Procedure			Procedure		
	Standard NCS	Maximum personal visit	Maximum telephone	Standard NCS	Maximum personal visit	Maximum telephone	Standard NCS	Maximum personal visit	Maximum telephone
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
12-13	1.014	0.969	0.989	1.025	0.999	1.008	0.958	0.809	0.888
14-15	0.992	0.985	0.918	1.009	1.000	0.944	0.900	0.905	0.776
16-17	0.999	0.925	0.947	1.011	0.957	0.987	0.929	0.749	0.728
18-19	0.931	0.799	0.781	0.950	0.835	0.796	0.824	0.594	0.693
20-21	0.874	0.778	0.743	0.893	0.802	0.768	0.761	0.635	0.592
22-24	0.943	0.869	0.829	0.961	0.894	0.840	0.830	0.908	0.755
25-29	0.963	0.882	0.856	0.978	0.899	0.871	0.862	0.765	0.759
30-34	0.928	0.908	0.915	0.944	0.932	0.939	0.819	0.735	0.748
35-39	0.953	0.900	0.942	0.966	0.907	0.968	0.861	0.852	0.755
40-44	0.958	0.847	0.912	0.964	0.863	0.919	0.919	0.731	0.867
45-49	0.962	0.906	0.917	0.973	0.933	0.922	0.877	0.705	0.879
50-54	0.928	0.914	0.947	0.932	0.940	0.949	0.900	0.702	0.923
55-59	0.943	0.958	0.954	0.942	0.979	0.954	0.958	0.769	0.959
60-61	0.992	0.862	1.056	0.992	0.886	1.090	0.990	0.630	0.720
62-64	0.970	0.851	0.933	0.979	0.851	0.931	0.886	0.853	0.948
65-69	0.903	0.857	0.881	0.913	0.858	0.885	0.824	0.847	0.850
70-74	0.962	0.956	0.996	0.959	0.993	0.989	0.985	0.548	1.071
75+	0.935	0.911	0.985	0.942	0.956	0.987	0.866	0.455	0.972
Overall ages	0.951	0.896	0.909	0.961	0.917	0.923	0.877	0.751	0.809
Corrected for apparent sampling bias	0.945	0.933	0.928	0.955	0.955	0.942	0.872	0.782	0.826

dence intervals than are given in this report. A similar statement can be made concerning contrasts for crimes against households. Because of the large "per batch" error rates involved in this analysis, isolated instances of a statistically significant difference between victimization rates usually have not been given special attention.

Response rates and nonresponse rates by interview procedure

Undercoverage

It appears that both experimental interview procedures were subject to slightly more undercoverage problems than the standard NCS interview procedure. Table 17 contains the coverage ratios, by race, obtained under the three interview procedures. The coverage ratio is the ratio of the sample estimate of the population in a given age-race-sex cell (using all stages of estimation except the adjustment to the independent age-race-sex counts) to the independent control count for the same subgroup of the population. A coverage ratio smaller than unity indicates undercoverage.

For most age categories, the coverage ratios of the experimental groups are smaller than the corresponding coverage ratios of the control group. In addition, it appears that for all three groups undercoverage is more of a problem among nonwhites than among whites.

Some of the differential undercoverage in the two experimental groups, relative to the control group, can be accounted for by considering the number of households designated for each experimental procedure [see table 19, columns (2), (5), and (8)]. The sampling fraction for each experimental group should have been 0.08333, but the fraction for the PV group was only 0.08005 and the fraction for the telephone group was only 0.08161. When the apparent sampling biases are taken into account, the corrected overall coverage ratios for the total population 12+ control group, the PV group, and the telephone group are 0.945, 0.933, and 0.928, respectively, and the differences are not as great as indicated in table 17. However, there still may be differences in coverage among the three interview procedures.

We can only hypothesize about the reason for these differences. One seemingly reasonable explanation may be that the relative inflexibility of the two experimental procedures vis-a-vis the current procedure results in a loss of coverage. Interviewers

18. Type of respondent by type of interview

Type of interview and proxy status	Control group			PV group			Telephone group		
	Number	Percent	Percent of interviewed	Number	Percent	Percent of interviewed	Number	Percent	Percent of interviewed
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Total persons in interviewed households	234,075	100.0	—	22,285	100.0	—	22,697	100.0	—
Persons interviewed	229,659	98.1	100.0	21,845	98.0	100.0	22,151	97.6	100.0
Self-response	213,976	91.4	93.2	20,309	91.1	93.0	20,485	90.3	92.5
Personal visit	179,764	76.8	78.3	21,041	94.4	96.3*	4,335	19.1	19.6
Self-response	165,227	70.6	71.9	19,541	87.7	89.5	3,998	17.6	18.0
Proxy	14,537	6.2	6.3	1,500	6.7	6.9	337	1.5	1.5
Telephone	49,895	21.3	21.7	804	3.6	3.7	17,816	78.5	80.4**
Self-response	48,749	20.8	21.2	768	3.4	3.5	16,487	72.6	74.4
Proxy	1,146	0.5	0.5	36	0.2	0.2	1,329	5.9	6.0

* Percent of persons interviewed by personal visit in PV group.
** Percent of persons interviewed by telephone in telephone group.

19. Household status and eligibility for maximum telephone interview procedure

Household status	Control group			PV group			Telephone group		
	Number	Percent	Percent of interviewed	Number	Percent	Percent of interviewed	Number	Percent	Percent of interviewed
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Total designated for procedure	124,612	100.0	—	11,899	100.0	—	12,131	100.0	—
Total interviewed	102,554	82.3	100.0	9,897	83.2	100.0	10,010	82.5	100.0
Same household as previous interview	88,702	71.2	86.5	8,574	72.1	86.6	8,852	73.0	88.4
Replacement household	9,806	7.9	9.6	991	8.3	10.0	831	6.9	8.3
Previous noninterview or new construction	3,862	3.1	3.8	313	2.6	3.2	318	2.6	3.2
NA for household status	184	0.1	0.2	19	0.2	0.2	9	0.1	0.1
Noninterview	22,058	17.7	—	2,002	16.8	—	2,121	17.5	—

faced with completing their assignment while adhering to the experimental procedures may be more prone to "forget" to interview some household members. Adoption of either experimental procedure must be accompanied by an awareness of the potential for increased undercoverage.

Proportion of interviews performed using the designated interview procedure

Almost all persons in the PV group were interviewed by personal visit—96.3 percent. On the other hand, only 80.4 percent of the interviews of persons in the telephone group were conducted by telephone [see table 18, columns (7) and (10)]. This relatively low proportion of interviews conducted using the designated interview procedure can be explained by the fact that only 88.4 percent of the households in the telephone group were interviewed during the previous enumeration. As can be seen, most of the 11.6 percent of households not interviewed during the previous enumeration 6 months earlier were replacement households [see table 19, column (10)].

A total of 102,554 household interviews[8] were conducted over the course of the experiment, using the standard NCS procedure. Of these, 98.8 percent were conducted by personal visit and 1.2 percent by telephone. Under the maximum personal visit procedure, 99.6 percent of the household interviews were carried out using personal visits and 0.4 percent using the telephone. In the telephone group, as expected, more household interviews were carried out using the telephone, 74.8 percent, with only 25.2 percent conducted in personal visits [see table 20, column (3)].

Note that for nonwhites in the telephone group only about 62 percent of the household interviews were carried out over the telephone [see table 20, columns (7) and (9)]. In addition, only about 67 percent of the interviews of persons among nonwhites in the telephone group were carried out by telephone [see table 21, column (7)]. For whites these figures were about 76 to 80 percent, respectively [see table 20, column (5) and table 21, column (7)]. This may indicate that nonwhite respondents have a greater preference for the face-to-face format of personal visit interviews, or that proportionately fewer nonwhite households were eligible to be interviewed by telephone.[9] Certainly another factor contributing to the difference is that proportionately fewer nonwhite households have telephones.

20. Type of interview conducted with household respondent, by race of head of household

Type of interview conducted with household respondent	Race of head of household							
	Total		White		Black		Other	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Control group—Total	102,554	100.0	91,170	100.0	10,240	100.0	1,144	100.0
Personal visit	101,360	98.8	90,097	98.8	10,131	98.9	1,132	99.0
Telephone	1,194	1.2	1,073	1.2	109	1.1	12	1.0
PV group—Total	9,897	100.0	8,888	100.0	902	100.0	107	100.0
Personal visit	9,856	99.6	8,858	99.7	891	98.8	107	100.0
Telephone	41	0.4	30	0.3	11	1.2	0	0.0
Telephone group—Total	10,010	100.0	8,942	100.0	966	100.0	102	100.0
Personal visit	2,521	25.2	2,114	23.6	371	38.4	36	35.3
Telephone	7,489	74.8	6,828	76.4	595	61.6	66	64.7

Proportion of self-respondents under each interview procedure

Tests conducted as part of the methodological design phase of the NCS have shown that self-response produces more reports of victimizations than does the use of a household respondent. For this reason, self-response was to be maximized under the two experimental interview procedures as is required under the standard NCS interview procedure. All three interview procedures included a provision for proxy interviews, but such interviews were allowed only when the respondent was expected to be absent from the household through the end of the interview period, or when the respondent was physically or mentally incapable of responding for himself.

The proportion of interviews completed using self-response does not vary substantially among the three interview procedures. Under the standard NCS procedure 93.2 percent of the interviews of persons were completed using self-response. Under the maximum personal visit and telephone interview procedures 93.0 and 92.5 percent of the interviews of persons were completed using self-response [see table 18, columns (4), (7), and (10)].

Noninterview rates

Counts of noninterviewed households—type A noninterview rates. The proportions of housing units designated for either experimental interview procedure and subsequently classified as type A, B, or C noninterview do not vary appreciably from the corresponding proportions in the control group. In particular, the most important household noninterview rate, the type A noninterview rate,[10] does not differ substantially between the control group and either of the experimental groups.

The control group experienced a type A noninterview rate of 4.2 percent, while the PV group and the telephone group had type A rates of 4.1 percent and 3.9 percent, respectively [see table 22, columns (2), (5), and (8)].

The distribution of the type A noninterviews by reason for the noninterview reveals that the proportion of type A noninterviews due to "no one at home" was higher for the control group than for the PV group—16.9 percent versus 13.1 percent [see table 23, columns (3) and (5)]. There is marginal evidence that the proportion of type A noninterviews due to refusals was larger in the PV group than in the control group—61.2 percent versus 56.2 percent.[11]

There were also some differences between the control group and the telephone group in the reasons for type A noninterviews. Only 3.7 percent of the type A's in the telephone group were due to "other" reasons, compared with 6.3 percent in the control group. It is possible that the telephone group had relatively few type A noninterviews due to "other" reasons because the "other" reasons for type A noninterviews include quarantined households and impassable roads. Obviously such situations pose no obstacle to a telephone interview.

[8]For this report, "household interview" has been defined to mean only the interview of the household respondent, including the administration of the household and individual screen questions.

[9]There may have been more movers, new construction and/or previous noninterviews among nonwhites than among whites.

[10]A type A noninterview occurs when the interviewer fails to conduct an interview at an occupied housing unit, usually because the respondent refused to be interviewed, no one was at home, or the household members were temporarily absent when the interviewer visited (for example, on vacation).

[11]This difference is not significant at the 5-percent level, but is significant at the 10-percent level.

21. Type of interview conducted, by specified interview procedure

Race of respondent and type of interview conducted	Control group		PV group		Telephone group	
	Number	Percent	Number	Percent	Number	Percent
[1]	[2]	[3]	[4]	[5]	[6]	[7]
Total persons in interviewed households	234,075	100.0	22,285	100.0	22,697	100.0
Personal visit	179,764	76.8	21,041	94.4	4,335	19.1
Telephone	49,895	21.3	804	3.6	17,816	78.5
Type-Z noninterview (and rate)	4,416	1.9	440	2.0	546	2.4
White	207,554	100.0	19,969	100.0	20,224	100.0
Personal visit	158,946	76.6	18,881	94.6	3,600	17.8
Telephone	44,948	21.7	708	3.5	16,154	79.9
Type-Z noninterview (and rate)	3,660	1.8	380	1.9	470	2.3
Black and other	26,521	100.0	2,316	100.0	2,473	100.0
Personal visit	20,818	78.5	2,160	93.3	735	29.7
Telephone	4,947	18.7	96	4.1	1,662	67.2
Type-Z noninterview (and rate)	756	2.9	60	2.6	76	3.1

22. Noninterview rates

	Control group			PV group			Telephone group		
	Number	Percent	Percent of noninterview	Number	Percent	Percent of noninterview	Number	Percent	Percent of noninterview
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Total households designated for procedure	124,612	100.0	——	11,899	100.0	——	12,131	100.0	——
Interviewed households	102,554	82.3	——	9,897	83.2	——	10,010	82.5	——
Noninterviewed households	22,058	17.7	100.0	2,002	16.8	100.0	2,121	17.5	100.0
Type A	4,466	3.6	20.2	428	3.6	21.4	403	3.3	19.0
Type B	16,671	13.4	75.6	1,500	12.6	74.9	1,615	13.3	76.1
Type C	921	0.7	4.2	74	0.6	3.7	103	0.8	4.9
Type-A noninterview rate*	4.2%			4.1%			3.9%		

* Computed as: Rate = Type-A noninterview/(Interview + Type-A noninterview).

23. Distribution of type-A noninterviews, by reason

Reason for type-A noninterview	Control group		PV group		Telephone group	
	Number	Percent	Number	Percent	Number	Percent
[1]	[2]	[3]	[4]	[5]	[6]	[7]
Total type A	4,466	100.0	428	100.0	403	100.0
No one at home	756	16.9	56	13.1	60	14.9
Temporarily absent	915	20.5	80	18.7	84	20.8
Refused	2,512	56.2	262	61.2	244	60.5
Other	283	6.3	30	7.0	15	3.7

Counts of noninterviewed persons within interviewed households—type Z noninterview rates. The type Z noninterview rate[12] experienced using the maximum telephone interview procedure was higher than the type Z noninterview rates obtained using the standard NCS procedure and the maximum personal visit interview procedure. In the telephone group the type Z noninterview rate was 2.4 percent while in the control group and the PV group the type Z rates were 1.9 and 2.0 percent, respectively [see table 21, columns (3), (5), and (7)].

An examination of type Z rates by several age categories fails to provide conclusive evidence that the type Z rate of the telephone group was higher than the rate for the control group or the PV group in any specific category. However, in every age category the type Z noninterview rate of the telephone group was always higher (though not significantly higher) than the type Z rates of the other two groups (see table 24).

[12]A type Z noninterview occurs when no interview can be obtained for a person in an interviewed household.

24. Type of interview conducted, by specified interview procedure for several age groups

Age group and type of interview conducted	Control group		PV group		Telephone group	
	Number	Percent	Number	Percent	Number	Percent
[1]	[2]	[3]	[4]	[5]	[6]	[7]
12–15	22,952	100.0	2,268	100.0	2,192	100.0
Personal visit	18,028	78.5	2,178	96.0	393	17.9
Telephone	4,676	20.4	65	2.9	1,771	80.8
Type-Z noninterview	248	1.1	25	1.1	28	1.3
16–19	22,718	100.0	2,066	100.0	2,088	100.0
Personal visit	13,911	61.2	1,853	89.7	343	16.4
Telephone	8,178	36.0	150	7.3	1,656	79.3
Type-Z noninterview	629	2.8	63	3.0	89	4.3
20–24	25,187	100.0	2,378	100.0	2,242	100.0
Personal visit	18,091	71.8	2,169	91.2	719	32.1
Telephone	6,152	24.4	112	4.7	1,419	63.3
Type-Z noninterview	944	3.7	97	4.1	104	4.6
25–34	43,117	100.0	4,101	100.0	4,094	100.0
Personal visit	33,076	76.7	3,879	94.6	951	23.2
Telephone	9,309	21.6	150	3.7	3,064	74.8
Type-Z noninterview	732	1.7	72	1.8	79	1.9
35–49	47,471	100.0	4,432	100.0	4,657	100.0
Personal visit	35,207	74.2	4,151	93.7	774	16.6
Telephone	11,262	23.7	180	4.1	3,761	80.8
Type-Z noninterview	1,002	2.1	101	2.3	122	2.6
50–64	43,230	100.0	4,186	100.0	4,428	100.0
Personal visit	34,159	79.0	4,004	95.7	616	13.9
Telephone	8,408	19.4	116	2.8	3,716	83.9
Type-Z noninterview	663	1.5	66	1.6	96	2.2
65+	29,400	100.0	2,854	100.0	2,996	100.0
Personal visit	27,292	92.8	2,807	98.4	539	18.0
Telephone	1,910	6.5	31	1.1	2,429	81.1
Type-Z noninterview	198	0.7	16	0.6	28	0.9

Differences in victimization rates for crimes against persons, by interview procedure

Total population 12+

There is no evidence that for the total population crimes against persons were reported at different rates under the standard NCS interview procedure and the maximum personal visit interview procedure. None of the differences for the major crime categories were statistically significant.

In contrast, there is evidence that persons interviewed using the maximum telephone procedure reported fewer victimizations than persons interviewed using the standard NCS procedure. The control group reported 129.40 crimes against persons per 1,000 persons 12+ versus only 119.14 per 1,000 reported by the telephone group. The 95-percent confidence interval around this difference indicates that the difference is at least 3 crimes per 1,000 persons and may be as large as 17 crimes per 1,000 persons.

The major source of the difference appears to be crimes of theft without contact[13] (also called personal larceny without contact). The control group reported 94.52 of these crimes per 1,000 persons and the telephone group reported only 84.64, a difference of 9.88 crimes per 1,000 persons [see table 25, columns (2), (4) and (7)].

It appears that the crimes of theft most often underreported in the telephone group were those in which the stolen property was worth less than $25 (see table 26).[14]

[13]In all crimes of theft (personal larceny) without contact the offender is defined to be a stranger. Thus, by definition, no crimes of personal larceny without contact are committed by a nonstranger.

[14]The victimization rates in table 26 are based on estimates of larcenies committed elsewhere (rather than "at home") using a household weight rather than a person weight.

25. Comparison of victimization rates for three interviewing procedures for crimes against all persons age 12 and over

Type of personal crime	Victimization rates per 1,000 persons age 12 and over			Standard NCS procedure versus maximum personal visit procedure		Standard NCS procedure versus maximum telephone procedure	
	Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Difference in victimization rates	95-percent confidence interval	Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	129.40	130.22	119.14	−0.82	−8.39 to 6.75	10.26*	2.96 to 17.56
Crimes of violence	32.00	34.11	31.83	−2.11	−6.18 to 1.96	0.17	−3.77 to 4.11
Robbery	5.97	6.90	6.69	−0.93	−2.78 to 0.92	−0.72	−2.54 to 1.10
Assault	25.20	26.27	24.65	−1.07	−4.66 to 2.52	0.55	−2.94 to 4.04
Aggravated	9.57	11.73	10.12	−2.16	−4.56 to 0.24	−0.55	−2.79 to 1.69
Simple	15.63	14.54	14.53	1.09	−1.61 to 3.79	1.10	−1.60 to 3.80
Crimes of theft	97.41	96.11	87.31	1.30	−5.33 to 7.93	10.10*	3.73 to 16.47
With contact	2.89	3.28	2.67	−0.39	−1.67 to 0.89	0.22	−0.94 to 1.38
Without contact	94.52	92.84	84.64	1.68	−4.85 to 8.21	9.88*	3.60 to 16.16
Crimes committed by strangers							
Total personal crimes	117.63	116.37	106.56	1.26	−5.96 to 8.48	11.07*	4.11 to 18.03
Crimes of violence	20.44	20.36	19.94	0.08	−3.10 to 3.26	0.50	−2.64 to 3.64
Robbery	4.86	5.78	5.60	−0.92	−2.61 to 0.77	−0.74	−2.41 to 0.93
Assault	15.01	13.91	13.35	1.10	−1.54 to 3.74	1.66	−0.93 to 4.25
Aggravated	6.17	6.32	4.87	−0.15	−1.93 to 1.63	1.30	−0.28 to 2.88
Simple	8.84	7.58	8.48	1.26	−0.71 to 3.23	0.36	−1.70 to 2.42
Crimes of theft	97.20	96.00	87.12	1.20	−5.43 to 7.83	10.08*	3.72 to 16.44
With contact	2.67	3.17	2.48	−0.50	−1.76 to 0.76	0.19	−0.93 to 1.31
Without contact	94.52	92.84	84.64	1.68	−4.85 to 8.21	9.88*	3.60 to 16.16
Crimes committed by nonstrangers							
Total personal crimes	11.77	13.86	12.58	−2.09	−4.70 to 0.52	−0.81	−3.31 to 1.69
Crimes of violence	11.56	13.75	12.39	−2.19	−4.79 to 0.41	−0.83	−3.31 to 1.65
Robbery	1.11	1.12	1.09	−0.01	−0.76 to 0.74	0.02	−0.72 to 0.76
Assault	10.19	12.36	11.30	−2.17	−4.64 to 0.30	−1.11	−3.47 to 1.25
Aggravated	3.40	5.40	5.25	−2.00*	−3.62 to −0.38	1.85*	−3.45 to −0.25
Simple	6.79	6.96	6.05	−0.17	−2.04 to 1.70	0.74	−1.01 to 2.49

* Indicates statistical significance at the 5-percent level.

26. Comparison of victimization rates for standard NCS procedure and maximum telephone procedure for personal larceny without contact

Personal larceny without contact, by value	Victimization rates per 1,000 households		Standard NCS procedure versus maximum telephone procedure	
	Standard NCS procedure	Maximum telephone procedure	Difference in victimization rates (per 1,000)	95-percent confidence interval
Total personal larceny without contact	91.81	80.14	11.67*	5.54 to 17.80
Completed	84.49	73.29	11.20*	5.31 to 17.09
Under $50	54.27	44.92	9.35*	4.66 to 14.04
Under $10	22.64	18.86	3.78*	0.70 to 6.86
$10–24	18.34	13.98	4.36*	1.69 to 7.03
$25–49	13.30	12.09	1.21	−1.26 to 3.68
$50 or more	24.77	25.58	−0.81	−4.35 to 2.73
$50–99	10.22	10.19	0.03	−2.23 to 2.29
$100–249	11.59	10.37	1.22	−1.07 to 3.51
$250 or more	5.63	5.02	0.61	−0.99 to 2.21
NA amount	2.78	2.78	0.00	−1.18 to 1.18
Attempted	7.32	6.85	0.47	−1.39 to 2.33

* Indicates statistical significance at the 5-percent level.

27. Comparison of victimization rates for three interviewing procedures for crimes against males

Type of personal crime	Victimization rates per 1,000 persons age 12 and over			Standard NCS procedure versus maximum personal visit procedure		Standard NCS procedure versus maximum telephone procedure	
	Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Difference in victimization rates	95-percent confidence interval	Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	151.49	147.03	132.86	4.46	– 7.09 to 16.01	18.63	7.51 to 29.75
Crimes of violence	42.47	43.34	42.22	– 0.87	– 7.50 to 5.76	0.25	– 6.27 to 6.77
Robbery	8.48	7.91	10.45	0.57	– 2.33 to 3.47	– 1.97	– 5.24 to 1.30
Assault	33.76	35.22	31.77	– 1.46	– 7.46 to 4.54	1.99	– 3.71 to 7.69
Aggravated	14.30	18.25	13.55	– 3.95	– 8.27 to 0.37	0.75	– 3.01 to 4.51
Simple	19.46	16.98	18.22	2.48	– 1.76 to 6.72	1.24	– 3.11 to 5.59
Crimes of Theft	109.02	103.68	90.64	5.34	– 4.61 to 15.29	18.38*	9.00 to 27.76
With contact	2.74	2.94	1.77	– 0.20	– 1.96 to 1.56	0.97	– 0.43 to 2.37
Without contact	106.28	100.74	88.87	5.54	– 4.29 to 15.37	17.41	8.11 to 26.71
Crimes committed by strangers							
Total personal crimes	138.41	132.69	118.57	5.72	– 5.35 to 16.79	19.84*	9.29 to 30.39
Crimes of violence	29.70	29.25	28.12	0.45	– 5.04 to 5.94	1.58	– 3.79 to 6.95
Robbery	7.15	7.51	8.73	– 0.36	– 3.17 to 2.45	– 1.58	– 4.57 to 1.41
Assault	22.37	21.53	19.39	0.84	– 3.90 to 5.58	2.98	– 1.52 to 7.48
Aggravated	10.08	10.22	8.07	– 0.14	– 3.42 to 3.14	2.01	– 0.92 to 4.94
Simple	12.29	11.32	11.32	0.97	– 2.49 to 4.43	0.97	– 2.47 to 4.41
Crimes of Theft	108.71	103.45	90.45	5.26	– 4.68 to 15.20	18.26*	8.89 to 27.63
With contact	2.43	2.72	1.58	– 0.29	– 1.98 to 1.40	0.85	– 0.47 to 2.17
Without contact	106.28	100.74	88.87	5.54	– 4.29 to 15.37	17.41*	8.11 to 26.71
Crimes committed by nonstrangers							
Total personal crimes	13.07	14.33	14.28	– 1.26	– 5.12 to 2.60	– 1.21	– 5.04 to 2.62
Crimes of violence	12.76	14.10	14.10	– 1.34	– 5.17 to 2.49	– 1.34	– 5.15 to 2.47
Robbery	1.33	0.41	1.72	0.92*	0.20 to 1.64	– 0.39	– 1.72 to 0.94
Assault	11.39	13.69	12.38	– 2.30	– 6.06 to 1.46	– 0.99	– 4.56 to 2.58
Aggravated	4.22	8.03	5.48	– 3.81*	– 6.66 to – 0.96	– 1.26	– 3.63 to 1.11
Simple	7.17	5.66	6.90	1.51	– 0.96 to 3.98	0.27	– 2.42 to 2.96

* Indicates statistical significance at the 5-percent level.

Crimes against persons by sex

As was the case for the population as a whole, there is little evidence to indicate that either males or females in the PV group reported crimes against persons at a rate different from their counterparts in the control group [see tables 27 and 28, columns (2), (3), and (5)]. The relationship between the victimization rates reported by males in the control group and telephone group is about the same as for the population as a whole.

Males in the telephone group reported personal larcenies without contact and hence total personal crimes at a lower rate than did males in the control group. The differences between victimization rates for males were almost twice as large as the differences that occurred for the total population. However, the differences between the control group and the telephone group for males were not significantly larger at the 5-percent level than the differences for the total population. Male respondents in the control group reported 106.28 personal larcenies without contact per 1,000 persons versus only 88.87 per 1,000 reported by males in the telephone group, a difference of 17.41 crimes per 1,000 persons compared with a difference of 9.88 per 1,000 for the total population.[15] This resulted in 151.49 total personal crimes reported by males in the control group versus 132.86 per 1,000 reported by males in the telephone group, a difference of 18.63 crimes per 1 persons, compared with a difference c .26 crimes per 1,000 for the total population [see tables 25 and 27, columns (2), (4), and (7)].

[15]The contrasts of the differences between the control group and telephone group for males and the total population are not statistically significant at the 5-percent level. However, for crimes of theft without contact the contrast between the difference for males and the total population is marginally significant (i.e., at the 10-percent level).

28. Comparison of victimization rates for three interviewing procedures for crimes against females

Type of personal crime	Victimization rates per 1,000 persons age 12 and over			Standard NCS procedure versus maximum personal visit procedure		Standard NCS procedure versus maximum telephone procedure	
	Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Difference in victimization rates	95-percent confidence interval	Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	109.11	114.93	106.48	– 5.82	– 15.72 to 4.08	2.63	– 6.97 to 12.23
Crimes of violence	22.38	25.71	22.24	– 3.33	– 8.22 to 1.56	0.14	– 4.45 to 4.73
Robbery	3.67	5.98	3.22	– 2.31	– 4.67 to 0.05	0.45	– 1.32 to 2.22
Assault	17.34	18.11	18.09	– 0.77	– 4.91 to 3.37	– 0.75	– 4.89 to 3.39
Aggravated	5.23	5.79	6.96	– 0.56	– 2.91 to 1.79	– 1.73	– 4.29 to 0.83
Simple	12.10	12.32	11.13	– 0.22	– 3.65 to 3.21	0.97	– 2.31 to 4.25
Crimes of theft	86.74	89.23	84.24	– 2.49	– 11.34 to 6.36	2.50	– 6.15 to 11.15
With contact	3.02	3.58	3.50	– 0.56	– 2.40 to 1.28	– 0.48	– 2.30 to 1.34
Without contact	83.71	85.65	80.74	– 1.94	– 10.63 to 6.75	2.97	– 5.51 to 11.45
Crimes committed by strangers							
Total personal crimes	98.54	101.49	95.47	– 2.95	– 12.33 to 6.43	3.07	– 6.08 to 12.22
Crimes of violence	11.93	12.27	11.43	– 0.34	– 3.76 to 3.08	0.50	– 2.81 to 3.81
Robbery	2.75	4.21	2.71	– 1.46	– 3.44 to 0.52	0.04	– 1.58 to 1.66
Assault	8.25	6.97	7.78	1.28	– 1.33 to 3.89	0.47	– 2.27 to 3.21
Aggravated	2.58	2.78	1.92	– 0.20	– 1.83 to 1.43	0.66	– 0.72 to 2.04
Simple	5.67	4.18	5.86	1.49	– 0.55 to 3.53	– 0.19	– 2.56 to 2.18
Crimes of theft	86.61	89.23	84.05	– 2.62	– 11.47 to 6.23	2.56	– 6.08 to 11.20
With contact	2.90	3.58	3.31	– 0.68	– 2.52 to 1.16	– 0.41	– 2.19 to 1.37
Without contact	83.71	85.65	80.74	– 1.94	– 10.63 to 6.75	2.97	– 5.51 to 11.45
Crimes committed by nonstrangers							
Total personal crimes	10.58	13.43	11.02	– 2.85	– 6.39 to 0.69	– 0.44	– 3.68 to 2.80
Crimes of violence	10.45	13.43	10.81	– 2.98	– 6.52 to 0.56	– 0.36	– 3.57 to 2.85
Robbery	0.92	1.77	0.50	– 0.85	– 2.13 to 0.43	0.42	– 0.30 to 1.14
Assault	9.09	11.15	10.31	– 2.06	– 5.30 to 1.18	– 1.22	– 4.34 to 1.90
Aggravated	2.65	3.01	5.04	– 0.36	– 2.05 to 1.33	– 2.39*	– 4.54 to – 0.24
Simple	6.44	8.14	5.27	– 1.70	– 4.47 to 1.07	1.17	– 1.10 to 3.44

* Indicates statistical significance at the 5-percent level.

There is no evidence that female respondents in the control group and the telephone group reported victimizations at different rates. For only one type of crime—aggravated assault committed by nonstrangers—was the difference in victimization rates statistically significant [see table 28, columns (2), (4), and (7)].

29. Comparison of victimization rates for three interviewing procedures for crimes against whites

Type of personal crime	Victimization rates per 1,000 persons age 12 and over: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	129.43	129.31	119.39	0.12	– 7.94 to 8.18	10.04*	2.24 to 17.84
Crimes of violence	30.71	31.79	31.64	– 1.08	– 5.29 to 3.13	– 0.93	– 5.12 to 3.26
Robbery	5.13	6.02	6.10	– 0.89	– 2.74 to 0.96	– 0.97	– 2.82 to 0.88
Assault	24.82	25.06	25.07	– 0.24	– 3.99 to 3.51	– 0.25	– 4.00 to 3.50
Aggravated	8.96	10.13	9.66	– 1.17	– 3.56 to 1.22	– 0.70	– 3.04 to 1.64
Simple	15.86	14.93	15.41	0.93	– 1.99 to 3.85	0.45	– 2.51 to 3.41
Crimes of theft	98.72	97.52	87.68	1.20	– 5.93 to 8.33	11.04*	4.22 to 17.86
With contact	2.41	3.13	2.15	– 0.72	– 2.05 to 0.61	0.26	– 0.86 to 1.38
Without contact	96.31	94.39	85.53	1.92	– 5.11 to 8.95	10.78*	4.04 to 17.52
Crimes committed by strangers							
Total personal crimes	118.54	117.46	107.29	1.08	– 6.66 to 8.82	11.25*	3.80 to 18.70
Crimes of violence	19.99	20.07	19.71	– 0.08	– 3.45 to 3.29	0.28	– 3.06 to 3.62
Robbery	4.14	4.96	5.10	– 0.82	– 2.50 to 0.86	– 0.96	– 2.65 to 0.73
Assault	15.31	14.57	14.15	0.74	– 2.15 to 3.63	1.16	– 1.68 to 4.00
Aggravated	6.19	6.40	5.02	– 0.21	– 2.12 to 1.70	1.17	– 0.54 to 2.88
Simple	9.12	8.17	9.12	0.95	– 1.22 to 3.12	0.00	– 2.28 to 2.28
Crimes of theft	98.55	97.40	87.58	1.15	– 5.98 to 8.28	10.97*	4.15 to 17.79
With contact	2.25	3.01	2.05	– 0.76	– 2.06 to 0.54	0.20	– 0.89 to 1.29
Without contact	96.31	94.39	85.53	1.92	– 5.11 to 8.95	10.78*	4.04 to 17.52
Crimes committed by nonstrangers							
Total personal crimes	10.89	11.85	12.02	– 0.96	– 3.55 to 1.63	– 1.13	– 3.73 to 1.47
Crimes of violence	10.73	11.73	11.93	– 1.00	– 3.58 to 1.58	– 1.20	– 3.79 to 1.39
Robbery	0.99	1.06	1.00	– 0.07	– 0.85 to 0.71	– 0.01	– 0.77 to 0.75
Assault	9.52	10.49	10.93	– 0.97	– 3.41 to 1.47	– 1.41	– 3.89 to 1.07
Aggravated	2.78	3.73	4.64	– 0.95	– 2.40 to 0.50	– 1.86*	– 3.46 to – 0.26
Simple	6.74	6.75	6.29	– 0.01	– 1.98 to 1.96	0.45	– 1.45 to 2.35

* Indicates statistical significance at the 5-percent level.

Crimes against persons by race

As could be expected, the comparisons between the victimization rates reported by whites in the control group and each experimental group have resulted in the same conclusions as the comparisons for the total population. There is no evidence of a difference between the standard NCS procedure and the maximum personal visit procedure. Crimes of theft without contact, however, appear to be reported at a lower rate under the maximum telephone interview procedure than under the standard NCS interview procedure. At this point it is interesting to note that aggravated assaults committed by nonstrangers were reported at a higher rate by whites in the telephone group than by whites in the control group [see table 29, column (7)].

This was also true for the total population 12+ [see table 25, column (7)] and for females [see table 28, column (7)]. Since this relationship has appeared repeatedly in the analysis, it seems possible that the maximum telephone procedure may result in slightly more reports of aggravated assault by nonstrangers than the standard NCS procedure, particularly among whites and female respondents.

A somewhat different picture emerges from the comparisons for blacks. The data indicate that the maximum personal visit procedure may elicit more reports of crimes against persons committed by nonstrangers, in particular aggravated assault, from blacks than does the standard NCS procedure. Blacks in the control group reported only 8.29 aggravated assaults committed by nonstrangers per 1,000 persons, versus 19.64 per 1,000 reported by blacks in the PV group [see table 30, columns (2), (3), and (5)].

While blacks in the PV group reported some types of crimes against persons at a higher rate than their counterparts in the control group, it appears that exactly the opposite is true of black respondents in the telephone group. Blacks in the telephone group reported aggravated assaults committed by strangers (and hence assaults committed by strangers) at a significantly lower rate than did black respondents in the control group. Black respondents in the control group reported 6.23 aggravated assaults and 12.69 assaults committed by strangers compared with 1.99 and 5.38 of these respective crimes per 1,000 persons reported by blacks in the telephone group [see table 30, columns (2), (4), and (7)]. A more detailed comparison between the control group and the telephone group indicates that attempted assault with a weapon is the specific type of aggravated assault underreported by blacks in the telephone group [see table 31, columns (2), (3), and (4)].

30. Comparison of victimization rates for three interviewing procedures for crimes against blacks

Type of personal crime	Victimization rates per 1,000 persons age 12 and over: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	130.54	146.01	116.92	– 15.47	– 39.47 to 8.53	13.62	– 8.46 to 35.70
Crimes of violence	42.44	56.43	33.63	– 13.99	– 29.57 to 1.59	8.81	– 3.66 to 21.28
Robbery	12.53	13.88	11.45	– 1.35	– 9.30 to 6.60	1.08	– 6.23 to 8.39
Assault	28.51	39.67	21.43	– 11.16	– 24.32 to 2.00	7.08	– 2.97 to 17.13
Aggravated	14.52	26.24	13.03	– 11.72*	– 22.41 to – 1.03	1.49	– 6.31 to 9.29
Simple	13.99	13.43	8.40	0.56	– 7.31 to 8.43	5.59	– 0.83 to 12.01
Crimes of theft	88.09	89.62	83.27	– 1.53	– 21.01 to 17.95	4.82	– 14.12 to 23.76
With contact	6.12	4.92	7.33	1.20	– 3.63 to 6.03	– 1.21	– 7.00 to 4.58
Without contact	81.97	84.70	75.94	– 2.73	– 21.71 to 16.25	6.03	– 12.15 to 24.21
Crimes committed by strangers							
Total personal crimes	111.46	114.05	97.92	– 2.59	– 24.26 to 19.08	13.54	– 6.90 to 33.98
Crimes of violence	24.00	24.43	15.61	– 0.43	– 10.96 to 10.10	8.39	– 0.29 to 17.07
Robbery	10.47	12.10	9.47	– 1.63	– 9.05 to 5.79	1.00	– 5.66 to 7.66
Assault	12.69	10.46	5.38	2.23	– 4.78 to 9.24	7.31*	2.01 to 12.61
Aggravated	6.23	6.61	1.99	– 0.38	– 5.90 to 5.14	4.24*	0.92 to 7.56
Simple	6.46	3.86	3.39	2.60	– 1.76 to 6.96	3.07	– 1.06 to 7.20
Crimes of theft	87.48	89.62	82.33	– 2.14	– 21.61 to 17.33	5.15	– 13.70 to 24.00
With contact	5.51	4.92	6.39	0.59	– 4.21 to 5.39	– 0.88	– 6.30 to 4.54
Without contact	81.97	84.70	75.94	– 2.73	– 21.71 to 16.25	6.03	– 12.15 to 24.21
Crimes committed by nonstrangers							
Total personal crimes	19.08	31.99	18.99	– 12.91*	– 24.70 to – 1.12	0.09	– 9.25 to 9.43
Crimes of violence	18.45	31.99	18.02	– 13.54*	– 25.32 to – 1.76	0.43	– 8.68 to 9.54
Robbery	2.06	1.78	1.98	0.28	– 2.62 to 3.18	0.08	– 2.97 to 3.13
Assault	15.82	29.21	16.05	– 13.39*	– 24.65 to – 2.13	– 0.23	– 8.82 to 8.36
Aggravated	8.29	19.64	11.04	– 11.35*	– 20.57 to – 2.13	– 2.75	– 9.82 to 4.32
Simple	7.53	9.57	5.01	– 2.04	– 8.62 to 4.54	2.52	– 2.42 to 7.46

* Indicates statistical significance at the 5-percent level.

31. Comparison of victimization rates for blacks—assaults committed by strangers—standard NCS procedure versus maximum telephone interview procedure

Type of crime	Victimization rates per 1,000 persons age 12 and over: Standard NCS procedure	Maximum telephone procedure	Difference	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]
Assault	12.69	5.38	7.31*	2.01 to 12.61
Aggravated	6.23	1.99	4.24*	0.92 to 7.56
With injury	1.81	0.75	1.06	– 0.93 to 3.05
Attempted, with weapon	4.42	1.24	3.18*	0.51 to 5.85
Simple assault	6.46	3.39	3.07	– 1.06 to 7.20
With injury	1.08	0.00	1.08	
Attempted, without weapon	5.38	3.39	1.99	– 2.09 to 6.07

* Indicates statistical significance at the 5-percent level.

32. Comparison of victimization rates for three interviewing procedures for crimes against males and females age 12-24

Type of personal crime	Victimization rates per 1,000 persons age 12 and over: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	207.29	205.00	196.90	2.29	−14.26 to 18.84	10.39	−5.94 to 26.72
Crimes of violence	58.85	65.27	56.70	−6.42	−16.50 to 3.66	2.15	−7.35 to 11.65
Robbery	9.18	11.72	9.29	−2.54	−6.91 to 1.83	−0.11	−4.04 to 3.82
Assault	47.67	50.97	46.73	−3.30	−12.29 to 5.69	0.94	−7.72 to 9.60
Aggravated	17.77	25.67	17.99	−7.90*	−14.29 to −1.51	−0.22	−5.67 to 5.23
Simple	29.90	25.30	28.74	4.60	−1.89 to 11.00	1.16	−5.70 to 8.02
Crimes of theft	148.44	139.74	140.19	8.70	−5.54 to 22.94	8.25	−6.02 to 22.52
With contact	3.44	2.66	2.60	0.78	−1.36 to 2.92	0.84	−1.28 to 2.06
Without contact	145.00	137.07	137.59	7.93	−6.19 to 22.05	7.41	−6.74 to 21.56
Crimes committed by strangers							
Total personal crimes	184.40	174.28	169.66	10.12	−5.45 to 25.69	14.74	−0.70 to 30.18
Crimes of violence	36.27	34.90	29.77	1.37	−6.16 to 8.90	6.50	−0.53 to 13.53
Robbery	6.77	8.53	7.37	−1.76	−5.49 to 1.97	−0.60	−4.09 to 2.89
Assault	28.12	24.47	21.72	3.65	−2.72 to 10.02	6.40*	0.35 to 12.45
Aggravated	11.86	14.33	7.31	−2.47	−7.30 to 2.36	4.55*	1.22 to 7.88
Simple	16.26	10.14	14.40	6.12*	1.91 to 10.33	1.86	−3.05 to 6.77
Crimes of theft	148.12	139.38	139.90	8.74	−5.48 to 22.96	8.22	−6.03 to 22.47
With contact	3.12	2.31	2.30	0.81	−1.19 to 2.81	0.82	−1.18 to 2.82
Without contact	145.00	137.07	137.59	7.93	−6.19 to 22.05	7.41	−6.74 to 21.56
Crimes committed by nonstrangers							
Total personal crimes	22.90	30.72	27.23	−7.82*	−14.81 to −0.83	−4.33	−10.96 to 2.30
Crimes of violence	22.58	30.36	26.94	−7.78*	−14.73 to −0.83	−4.36	−10.95 to 2.23
Robbery	2.41	3.19	1.92	−0.78	−3.06 to 1.50	0.49	−1.33 to 2.31
Assault	19.55	26.50	25.01	−6.95*	−13.45 to −0.45	−5.46	−11.80 to 0.88
Aggravated	5.91	11.33	10.67	−5.42*	−9.66 to −1.18	−4.76*	−8.89 to −0.63
Simple	13.64	15.17	14.34	−1.53	−6.52 to 3.46	−0.70	−5.56 to 4.16

* Indicates statistical significance at the 5-percent level.

Crimes against persons by age and sex

There is no evidence to suggest that for persons 25 years old or older the maximum personal visit interview procedure elicits reports of victimizations at a rate different from the standard NCS interview procedure. However, it appears that aggravated assault was reported at a significantly higher rate by persons 12 to 24 years old in the PV group than by their counterparts in the control group. This appears to be true primarily for males in this age group and not for females [see tables 32, 35, and 38, columns (2), (3) and (5)].

More clear-cut differences can be seen between the control group and the telephone group. In particular, crimes of theft without contact were reported at a lower rate by 25- to 49-year-olds, both male and female, and by 12- to 24-year-old males in the telephone group than by their counterparts in the control group [see tables 33, 35, 36, and 39, columns (2), (4), and (7)].

33. Comparison of victimization rates for three interviewing procedures for crimes against males and females age 25-49

Type of personal crime	Victimization rates per 1,000 persons age 12 and over: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	128.43	131.13	113.71	−2.70	−14.91 to 9.51	14.72*	3.20 to 26.24
Crimes of violence	28.95	28.30	30.61	0.65	−5.36 to 6.66	−1.66	−7.87 to 4.55
Robbery	5.24	5.82	6.66	−0.58	−3.32 to 2.16	−1.42	−4.33 to 1.49
Assault	23.17	22.31	23.21	0.86	−4.50 to 6.22	−0.04	−5.48 to 5.40
Aggravated	8.84	8.56	10.14	0.28	−3.06 to 3.62	−1.30	−4.90 to 2.30
Simple	14.33	13.74	13.07	0.59	−3.63 to 4.81	1.26	−2.86 to 5.38
Crimes of theft	99.48	102.83	83.11	−3.35	−14.33 to 7.63	16.37*	6.32 to 26.42
With contact	2.71	3.04	2.42	−0.33	−2.31 to 1.65	0.29	−1.49 to 2.07
Without contact	96.77	99.79	80.69	−3.02	−13.86 to 7.82	16.08*	6.16 to 26.00
Crimes committed by strangers							
Total personal crimes	117.89	121.92	103.11	−4.03	−15.86 to 7.80	14.78*	3.73 to 25.83
Crimes of violence	18.60	19.08	20.26	−0.48	−5.43 to 4.47	−1.66	−6.73 to 3.41
Robbery	4.34	5.41	5.34	−1.07	−3.70 to 1.56	−1.00	−3.61 to 1.61
Assault	13.89	13.68	14.19	0.21	−4.00 to 4.42	−0.30	−4.57 to 3.97
Aggravated	5.49	4.13	5.23	1.36	−1.00 to 3.72	0.26	−2.35 to 2.87
Simple	8.40	9.54	8.96	−1.14	−4.64 to 2.36	−0.56	−3.95 to 2.83
Crimes of theft	99.30	102.83	82.85	−3.53	−14.51 to 7.45	16.45*	6.41 to 26.49
With contact	2.53	3.04	2.15	−0.51	−2.49 to 1.47	0.38	−1.31 to 2.07
Without contact	96.77	99.79	80.69	−3.02	−13.86 to 7.82	16.08*	6.16 to 26.00
Crimes committed by nonstrangers							
Total personal crimes	10.54	9.21	10.61	1.33	−2.15 to 4.81	−0.07	−3.77 to 3.63
Crimes of violence	10.35	9.21	10.34	1.14	−2.34 to 4.62	0.01	−3.64 to 3.66
Robbery	0.90	0.41	1.32	0.49	−0.28 to 1.26	−0.42	−1.71 to 0.87
Assault	9.28	8.63	9.02	0.65	−2.71 to 4.01	0.26	−3.16 to 3.68
Aggravated	3.35	4.43	4.91	−1.08	−3.46 to 1.30	−1.56	−4.05 to 0.93
Simple	5.93	4.20	4.11	1.73	−0.65 to 4.11	1.82	−0.54 to 4.18

* Indicates statistical significance at the 5-percent level.

34. Comparison of victimization rates for three interviewing procedures for crimes against males and females age 50 or over

Type of personal crime	Victimization rates per 1,000 persons age 12 and over: Standard NCS procedure	Victimization rates per 1,000 persons age 12 and over: Maximum personal visit procedure	Victimization rates per 1,000 persons age 12 and over: Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	Standard NCS procedure versus maximum personal visit procedure: 95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	Standard NCS procedure versus maximum telephone procedure: 95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	55.57	56.95	51.49	− 1.38	− 10.70 to 7.94	4.08	− 4.81 to 12.97
Crimes of violence	9.89	11.23	9.55	− 1.34	− 5.56 to 2.88	0.34	− 3.57 to 4.25
Robbery	3.78	3.59	4.23	0.19	− 2.22 to 2.60	− 0.45	− 3.04 to 2.14
Assault	6.06	7.33	5.33	− 1.27	− 4.68 to 2.14	0.73	− 2.21 to 3.67
Aggravated	2.58	2.19	2.59	0.39	− 1.51 to 2.29	− 0.01	− 2.05 to 2.03
Simple	3.48	5.14	2.74	− 1.66	− 4.50 to 1.18	0.74	− 1.38 to 2.86
Crimes of theft	45.68	45.72	41.94	− 0.04	− 8.45 to 8.37	3.74	− 4.33 to 11.81
With contact	2.56	4.16	3.05	− 1.60	− 4.14 to 0.94	− 0.49	− 2.69 to 1.71
Without contact	43.12	41.56	38.89	1.56	− 6.48 to 9.60	4.23	− 3.56 to 12.02
Crimes committed by strangers							
Total personal crimes	53.00	53.62	50.48	− 0.62	− 9.68 to 8.44	2.52	− 6.28 to 11.32
Crimes of violence	7.46	7.90	8.54	− 0.44	− 3.99 to 3.11	− 1.08	− 4.75 to 2.59
Robbery	3.64	3.59	4.23	0.05	− 2.36 to 2.46	− 0.59	− 3.18 to 2.00
Assault	3.76	4.00	4.32	− 0.24	− 2.77 to 2.29	− 0.56	− 3.17 to 2.05
Aggravated	1.54	1.31	2.09	0.23	− 1.24 to 1.70	− 0.55	− 2.36 to 1.26
Simple	2.23	2.69	2.23	− 0.46	− 2.53 to 1.61	0.00	− 1.89 to 1.89
Crimes of theft	45.54	45.72	41.94	− 0.18	− 8.58 to 8.22	3.60	− 4.47 to 11.67
With contact	2.42	4.16	3.05	− 1.74	− 4.28 to 0.80	− 0.63	− 2.82 to 1.56
Without contact	43.12	41.56	38.89	1.56	− 6.48 to 9.60	4.23	− 3.56 to 12.02
Crimes committed by nonstrangers							
Total personal crimes	2.57	3.32	1.01	− 0.75	− 3.04 to 1.54	1.55*	0.20 to 2.92
Crimes of violence	2.43	3.32	1.01	− 0.89	− 3.18 to 1.40	1.42*	0.07 to 2.77
Robbery	0.13	0.00	0.00	0.13	− 0.01 to 0.27	0.13	− 0.01 to 0.27
Assault	2.24	3.32	1.01	− 0.03	− 3.31 to 1.25	1.28	− 0.07 to 2.63
Aggravated	1.04	0.88	0.49	0.16	− 1.04 to 1.36	0.55	− 0.38 to 1.48
Simple	1.25	2.44	0.51	− 1.19	− 3.13 to 0.75	0.74	− 0.22 to 1.70

* Indicates statistical significance at the 5-percent level.

35. Comparison of victimization rates for three interviewing procedures for crimes against males age 12-24

Type of personal crime	Victimization rates per 1,000 persons age 12 and over: Standard NCS procedure	Victimization rates per 1,000 persons age 12 and over: Maximum personal visit procedure	Victimization rates per 1,000 persons age 12 and over: Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	Standard NCS procedure versus maximum personal visit procedure: 95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	Standard NCS procedure versus maximum telephone procedure: 95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	243.04	219.35	207.29	23.69	− 0.49 to 47.87	35.75*	12.12 to 59.38
Crimes of violence	76.87	75.85	72.07	1.02	− 14.40 to 16.44	4.80	− 10.24 to 19.84
Robbery	13.78	11.15	16.38	2.63	− 3.55 to 8.81	− 2.60	− 9.91 to 4.71
Assault	62.53	64.71	55.70	− 2.18	− 16.48 to 12.12	6.83	− 6.54 to 20.20
Aggravated	26.41	39.27	23.02	− 12.86*	− 24.01 to − 1.71	3 39	− 5.40 to 20.25
Simple	36.12	25.43	32.68	10.69*	1.36 to 20.02	3.44	− 6.92 to 13.80
Crimes of theft	166.17	143.50	135.21	22.67*	2.14 to 43.20	30.96*	10.97 to 50.95
With contact	4.20	3.04	1.21	1.16	− 2.10 to 4.42	2.99*	0.76 to 5.22
Without contact	161.97	140.46	134.00	21.51*	1.17 to 41.85	27.97*	8.06 to 47.88
Crimes committed by strangers							
Total personal crimes	217.73	190.21	174.83	27.52*	4.56 to 50.48	42.90*	20.69 to 65.11
Crimes of violence	51.90	47.43	40.21	4.47	− 7.96 to 16.90	11.69*	0.16 to 23.22
Robbery	10.72	9.86	13.05	0.86	− 4.92 to 6.64	− 2.33	− 8.86 to 4.20
Assault	40.70	37.58	27.16	3.12	− 7.99 to 14.23	13.54*	3.91 to 23.17
Aggravated	18.86	22.97	11.26	− 4.11	− 12.76 to 4.54	7.60*	1.30 to 13.90
Simple	21.84	14.61	15.89	7.23*	0.09 to 14.37	5.95	− 1.42 to 13.32
Crimes of theft	165.84	142.77	134.62	23.07*	2.58 to 43.56	31.22*	11.26 to 51.18
With contact	3.87	2.31	0.61	1.56	− 1.32 to 4.44	3.26*	1.52 to 5.00
Without contact	161.97	140.46	134.00	21.51*	1.17 to 41.85	27.97*	8.06 to 47.88
Crimes committed by nonstrangers							
Total personal crimes	25.30	29.15	32.45	− 3.85	− 13.59 to 5.89	− 7.15	− 17.32 to 3.02
Crimes of violence	24.97	28.42	31.86	− 3.45	− 13.07 to 6.17	− 6.89	− 16.98 to 3.20
Robbery	3.06	1.30	3.32	1.76	− 0.46 to 3.98	− 0.26	− 3.58 to 3.06
Assault	21.83	27.13	28.54	− 5.30	− 14.68 to 4.08	− 6.71	− 16.26 to 2.84
Aggravated	7.55	16.30	11.75	− 8.75*	− 15.94 to − 1.56	− 4.20	− 10.35 to 1.95
Simple	14.28	10.83	16.79	3.45	− 2.66 to 9.56	− 2.51	− 9.91 to 4.89

* Indicates statistical significance at the 5-percent level.

36. Comparison of victimization rates for three interviewing procedures for crimes against males age 25–49

Type of personal crime	Victimization rates per 1,000 persons age 12 and over			Standard NCS procedure versus maximum personal visit procedure		Standard NCS procedure versus maximum telephone procedure	
	Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Difference in victimization rates	95-percent confidence interval	Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	141.40	143.22	127.30	− 1.82	− 20.02 to 16.38	14.10	− 3.25 to 31.45
Crimes of violence	36.74	36.89	40.51	− 0.15	− 9.95 to 9.65	− 3.77	− 13.95 to 6.41
Robbery	6.92	6.27	10.59	0.65	− 3.47 to 4.77	− 3.67	− 8.89 to 1.55
Assault	29.73	30.62	29.92	− 0.89	− 9.83 to 8.05	− 0.19	− 9.02 to 8.64
Aggravated	12.44	12.39	13.15	0.05	− 5.70 to 5.80	− 0.71	− 6.60 to 5.18
Simple	17.28	18.24	16.77	− 0.96	− 7.90 to 5.98	0.51	− 6.15 to 7.17
Crimes of theft	104.65	106.32	86.78	− 1.67	− 17.68 to 14.34	17.87*	3.16 to 32.58
With contact	2.08	3.09	2.60	− 1.01	− 3.85 to 1.83	− 0.52	− 3.14 to 2.10
Without contact	102.57	103.24	84.18	− 0.67	− 16.48 to 15.14	18.39*	3.87 to 32.91
Crimes committed by strangers							
Total personal crimes	130.96	132.97	117.56	− 2.01	− 19.65 to 15.63	13.40	− 3.37 to 30.17
Crimes of violence	26.65	26.64	30.78	0.01	− 8.36 to 8.38	− 4.13	− 13.03 to 4.77
Robbery	6.03	6.27	8.85	− 0.24	− 4.34 to 3.86	− 2.82	− 7.60 to 1.96
Assault	20.57	20.38	21.93	0.19	− 7.16 to 7.54	− 1.36	− 8.93 to 6.21
Aggravated	8.79	6.41	8.52	2.38	− 1.84 to 6.60	0.27	− 4.50 to 5.04
Simple	11.78	13.97	13.41	− 2.19	− 8.25 to 3.87	− 1.63	− 7.56 to 4.30
Crimes of theft	104.31	106.32	86.78	− 2.01	− 18.02 to 14.00	17.93*	2.82 to 32.24
With contact	1.74	3.09	2.60	− 1.35	− 4.18 to 1.48	− 0.86	− 3.46 to 1.74
Without contact	102.57	103.24	84.18	− 0.67	− 16.48 to 15.14	18.39*	3.87 to 32.91
Crimes committed by nonstrangers							
Total personal crimes	10.43	10.25	9.74	0.18	− 5.06 to 5.42	0.69	− 4.42 to 5.80
Crimes of violence	10.09	10.25	9.74	− 0.16	− 5.39 to 5.07	0.35	− 4.75 to 5.45
Robbery	0.89	0.00	1.75	0.89*	0.42 to 1.36	− 0.86	− 2.98 to 1.26
Assault	9.16	10.25	7.99	− 1.09	− 6.30 to 4.12	1.17	− 3.47 to 5.81
Aggravated	3.65	5.98	4.63	− 2.33	− 6.27 to 1.61	− 0.98	− 4.46 to 2.50
Simple	5.50	4.27	3.36	1.23	− 2.20 to 4.66	2.14	− 0.94 to 5.22

* Indicates statistical significance at the 5-percent level.

37. Comparison of victimization rates for three interviewing procedures for crimes against males age 50 or over

Type of personal crime	Victimization rates per 1,000 persons age 12 and over			Standard NCS procedure versus maximum personal visit procedure		Standard NCS procedure versus maximum telephone procedure	
	Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Difference in victimization rates	95-percent confidence interval	Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	67.34	75.05	61.70	− 7.71	− 23.48 to 8.06	5.64	− 8.75 to 20.03
Crimes of violence	13.43	17.32	12.98	− 3.89	− 11.65 to 3.87	0.45	− 6.30 to 7.20
Robbery	4.91	6.66	4.00	− 1.75	− 6.58 to 3.08	0.91	− 2.89 to 4.71
Assault	8.47	9.96	8.98	− 1.49	− 7.42 to 4.44	− 0.51	− 6.12 to 5.10
Aggravated	3.86	3.69	4.10	0.17	− 3.48 to 3.82	− 0.24	− 4.04 to 3.56
Simple	4.60	6.27	4.88	− 1.67	− 6.36 to 3.02	− 0.28	− 4.42 to 3.86
Crimes of theft	53.91	57.72	48.72	− 3.81	− 17.79 to 10.17	5.19	− 7.70 to 18.08
With contact	2.06	2.65	1.27	− 0.59	− 3.65 to 2.47	0.79	− 1.39 to 2.97
Without contact	51.84	55.08	47.45	− 3.24	− 16.92 to 10.44	4.39	− 8.33 to 17.11
Crimes committed by strangers							
Total personal crimes	63.77	71.07	60.57	− 7.30	− 22.68 to 8.08	3.20	− 11.05 to 17.45
Crimes of violence	10.12	13.35	11.85	− 3.23	− 10.05 to 3.59	− 1.73	− 8.13 to 4.67
Robbery	4.84	6.66	4.00	− 1.82	− 6.65 to 3.01	0.84	− 2.96 to 4.64
Assault	5.22	5.99	7.85	− 0.77	− 5.38 to 3.84	− 2.63	− 7.81 to 2.55
Aggravated	2.44	1.72	4.10	0.72	− 1.82 to 3.26	− 1.66	− 5.40 to 2.08
Simple	2.77	4.26	3.75	− 1.49	− 5.34 to 2.36	− 0.98	− 4.58 to 2.62
Crimes of theft	53.65	57.72	48.72	− 4.07	− 18.05 to 9.91	4.93	− 7.95 to 17.81
With contact	1.81	2.65	1.27	− 0.84	− 3.89 to 2.21	0.54	− 1.62 to 2.70
Without contact	51.84	55.08	47.45	− 3.24	− 16.92 to 10.44	4.39	− 8.33 to 17.11
Crimes committed by nonstrangers							
Total personal crimes	3.57	3.98	1.13	− 0.41	− 4.18 to 3.36	2.44*	0.25 to 4.63
Crimes of violence	3.31	3.98	1.13	− 0.67	− 4.43 to 3.09	2.18*	0.01 to 4.35
Robbery	0.06	0.00	0.00	0.06	− 0.08 to 0.20	0.06	− 0.08 to 0.20
Assault	3.25	3.98	1.13	− 0.73	− 4.48 to 3.02	2.12	− 0.05 to 4.29
Aggravated	1.42	1.97	0.00	− 0.55	− 3.18 to 2.08	1.42*	0.74 to 2.10
Simple	1.83	2.01	1.13	− 0.18	− 2.86 to 2.50	0.70	− 1.36 to 2.76

* Indicates statistical significance at the 5-percent level.

38. Comparison of victimization rates for three interviewing procedures for crimes against females age 12-24

Type of personal crime	Victimization rates per 1,000 persons age 12 and over			Standard NCS procedure versus maximum personal visit procedure		Standard NCS procedure versus maximum telephone procedure	
	Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Difference in victimization rates	95-percent confidence interval	Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	171.87	190.94	186.50	− 19.07	− 41.65 to 3.51	− 14.63	− 37.15 to 7.89
Crimes of violence	41.00	54.89	41.32	− 13.89*	− 26.88 to − 0.90	− 0.32	− 11.86 to 11.22
Robbery	4.62	12.28	2.21	− 7.66*	− 13.83 to − 1.49	2.41	− 0.44 to 5.26
Assault	32.95	37.50	37.75	− 4.55	− 15.45 to 6.35	− 4.80	− 15.79 to 6.19
Aggravated	9.21	12.33	12.95	− 3.12	− 9.41 to 3.17	− 3.74	− 10.21 to 2.73
Simple	23.74	25.17	24.80	− 1.43	− 10.44 to 7.58	− 1.06	− 10.06 to 7.94
Crimes of theft	130.87	136.05	145.18	− 5.18	− 24.92 to 14.56	− 14.31	− 34.66 to 6.04
With contact	2.69	2.30	4.00	0.39	− 2.39 to 3.17	− 1.31	− 4.92 to 2.30
Without contact	128.18	133.75	141.19	− 5.57	− 25.17 to 14.03	− 13.01	− 33.13 to 7.11
Crimes committed by strangers							
Total personal crimes	151.35	158.68	164.49	− 7.33	− 28.36 to 13.70	− 13.14	− 34.57 to 8.29
Crimes of violence	20.79	22.63	19.31	− 1.84	− 10.38 to 6.70	1.48	− 6.53 to 9.49
Robbery	2.86	7.23	1.68	− 4.37	− 9.12 to 0.38	1.18	− 1.27 to 3.63
Assault	15.64	11.63	16.27	4.01	− 2.27 to 10.29	− 0.63	− 7.95 to 6.69
Aggravated	4.92	5.87	3.35	− 0.95	− 5.32 to 3.42	1.57	− 1.85 to 4.99
Simple	10.72	5.76	12.91	4.96*	0.43 to 9.49	− 2.19	− 8.69 to 4.31
Crimes of theft	130.57	136.05	145.18	− 5.48	− 25.22 to 14.26	− 14.61	− 34.96 to 5.74
With contact	2.39	2.30	4.00	0.09	− 2.68 to 2.86	− 1.61	− 5.20 to 1.98
Without contact	128.18	133.75	141.19	− 5.57	− 25.17 to 14.03	− 13.01	− 33.13 to 7.11
Crimes committed by nonstrangers							
Total personal crimes	20.52	32.26	22.01	− 11.74*	− 21.76 to − 1.72	− 1.49	− 9.97 to 6.99
Crimes of violence	20.21	32.26	22.01	− 12.05*	− 22.07 to − 2.03	− 1.80	− 10.28 to 6.68
Robbery	1.76	5.05	0.53	− 3.29	− 7.25 to 0.67	1.23	− 0.24 to 2.70
Assault	17.31	25.88	21.48	− 8.57	− 17.59 to 0.45	− 4.17	− 12.50 to 4.16
Aggravated	4.29	6.46	9.60	− 2.17	− 6.72 to 2.38	− 5.31	− 10.82 to 0.20
Simple	13.02	19.42	11.88	− 6.40	− 14.24 to 1.44	1.14	− 5.17 to 7.45

* Indicates statistical significance at the 5-percent level.

39. Comparison of victimization rates for three interviewing procedures for crimes against females age 25-49

Type of personal crime	Victimization rates per 1,000 persons age 12 and over			Standard NCS procedure versus maximum personal visit procedure		Standard NCS procedure versus maximum telephone procedure	
	Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Difference in victimization rates	95-percent confidence interval	Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	116.08	119.75	100.89	− 3.67	− 20.03 to 12.69	15.19	− 0.07 to 30.45
Crimes of violence	21.52	20.20	21.26	1.32	− 5.60 to 8.44	0.26	− 7.01 to 7.53
Robbery	3.64	5.39	2.96	− 1.75	− 5.39 to 1.89	0.68	− 2.08 to 3.44
Assault	16.93	14.48	16.88	2.45	− 3.62 to 8.52	0.05	− 6.44 to 6.54
Aggravated	5.42	4.97	7.30	0.45	− 3.11 to 4.01	− 1.88	− 6.12 to 2.36
Simple	11.52	9.51	9.59	2.01	− 2.93 to 6.95	1.93	− 3.02 to 6.88
Crimes of theft	94.56	99.54	79.64	− 4.98	− 20.06 to 10.10	14.92*	1.18 to 28.66
With contact	3.32	2.99	2.24	0.33	− 2.44 to 3.10	1.08	− 1.35 to 3.51
Without contact	91.24	96.55	77.39	− 5.31	− 20.18 to 9.56	13.85*	0.29 to 27.41
Crimes committed by strangers							
Total personal crimes	105.44	111.51	89.46	− 6.07	− 21.92 to 9.78	15.98*	1.50 to 30.46
Crimes of violence	10.92	11.97	10.34	− 1.05	− 6.52 to 4.42	0.58	− 4.53 to 5.69
Robbery	2.74	4.60	2.03	− 1.86	− 5.21 to 1.49	0.71	− 1.59 to 3.01
Assault	7.53	7.37	6.89	0.16	− 4.16 to 4.48	0.64	− 3.54 to 4.82
Aggravated	2.34	1.99	2.12	0.35	− 1.92 to 2.62	0.22	− 2.11 to 2.55
Simple	5.18	5.37	4.77	− 0.19	− 3.87 to 3.49	0.41	− 3.07 to 3.89
Crimes of theft	94.51	99.54	79.13	− 5.03	− 20.10 to 10.04	15.38*	1.68 to 29.08
With contact	3.28	2.99	1.73	0.29	− 2.48 to 3.06	1.55	− 0.63 to 3.73
Without contact	91.24	96.55	77.39	− 5.31	− 20.18 to 9.56	13.85*	0.29 to 27.41
Crimes committed by nonstrangers							
Total personal crimes	10.64	8.23	11.43	2.41	− 2.21 to 7.03	− 0.79	− 6.13 to 4.55
Crimes of violence	10.60	8.23	10.92	2.37	− 2.25 to 6.99	− 0.32	− 5.55 to 4.91
Robbery	0.90	0.79	0.92	0.11	− 1.32 to 1.54	− 0.02	− 1.55 to 1.51
Assault	9.40	7.11	10.00	2.29	− 2.01 to 6.59	− 0.60	− 5.60 to 4.40
Aggravated	3.07	2.98	5.18	0.09	− 2.66 to 2.84	− 2.11	− 5.66 to 1.44
Simple	6.33	4.13	4.82	2.20	− 1.11 to 5.51	1.51	− 2.03 to 5.05

* Indicates statistical significance at the 5-percent level.

40. Comparison of victimization rates for three interviewing procedures for crimes against females age 50 and over

Type of personal crime	Victimization rates per 1,000 persons age 12 and over			Standard NCS procedure versus maximum personal visit procedure		Standard NCS procedure versus maximum telephone procedure	
	Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Difference in victimization rates	95-percent confidence interval	Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Crimes committed by strangers or nonstrangers							
Total personal crimes	45.97	42.25	43.03	3.72	−7.22 to 14.66	2.94	−8.10 to 13.98
Crimes of violence	7.00	6.28	6.70	0.72	−3.58 to 5.02	0.30	−4.13 to 4.73
Robbery	2.86	1.10	4.41	1.76	−0.16 to 3.68	−1.55	−5.09 to 1.99
Assault	4.09	5.18	2.30	−1.09	−4.94 to 2.76	1.79	−0.90 to 4.48
Aggravated	1.53	0.97	1.33	0.56	−1.17 to 2.29	0.20	−1.79 to 2.19
Simple	2.56	4.22	0.97	−1.66	−5.11 to 1.79	1.59	−0.22 to 3.40
Crimes of theft	38.97	35.97	36.32	3.00	−7.13 to 13.13	2.65	−7.53 to 12.83
With contact	2.97	5.38	4.53	−2.41	−6.29 to 1.47	−1.56	−5.14 to 2.02
Without contact	36.00	30.58	31.79	5.42	−3.98 to 14.82	4.21	−5.36 to 13.78
Crimes committed by strangers							
Total personal crimes	44.21	39.45	42.12	4.76	−5.84 to 15.36	2.09	−8.83 to 13.01
Crimes of violence	5.29	3.48	5.80	1.81	−1.45 to 5.07	−0.51	−4.61 to 3.59
Robbery	2.67	1.10	4.41	1.57	−0.34 to 3.48	−1.74	−5.27 to 1.79
Assault	2.58	2.39	1.39	0.19	−2.47 to 2.85	1.19	−0.91 to 3.29
Aggravated	0.80	0.97	0.43	−0.17	−1.84 to 1.50	0.37	−0.80 to 1.54
Simple	1.78	1.42	0.97	0.36	−1.70 to 2.42	0.81	−0.94 to 2.56
Crimes of theft	38.91	35.97	36.32	2.94	−7.19 to 13.07	2.59	−7.59 to 12.77
With contact	2.92	5.38	4.53	−2.46	−6.34 to 1.42	−1.61	−5.19 to 1.97
Without contact	36.00	30.58	31.79	5.42	−3.98 to 14.82	4.21	−5.36 to 13.78
Crimes committed by nonstrangers							
Total personal crimes	1.76	2.79	0.90	−1.03	−3.84 to 1.78	0.86	−0.84 to 2.56
Crimes of violence	1.70	2.79	0.90	−1.09	−3.90 to 1.72	0.80	−0.89 to 2.49
Robbery	0.19	0.00	0.00	0.19	−0.04 to 0.42	0.19	−0.04 to 0.42
Assault	1.51	2.79	0.90	−1.28	−4.08 to 1.52	0.61	−1.07 to 2.29
Aggravated	0.73	0.00	0.90	0.73*	0.29 to 1.17	−0.17	−1.78 to 1.44
Simple	0.78	2.79	0.00	−2.01	−4.77 to 0.75	0.78*	0.32 to 1.24

* Indicates statistical significance at the 5-percent level.

41. Differences between victimization rates of blacks and whites, by type of interview procedure

Type of crime	Race of respondent	Victimization rate procedure			Difference of the differences between standard NCS procedure and—	
		Standard NCS	Maximum personal visit	Maximum telephone	Maximum personal visit procedure	Maximum telephone procedure
[1]	[2]	[3]	[4]	[5]	[6]	[7]
Total personal crimes:	Black	130.54	146.01	116.92		
	White	129.43	129.31	119.39		
Difference in rates		1.11	16.70	−2.47	−15.59	3.58
Standard error of difference		3.73	12.37	11.35	12.92	11.95
Crimes of violence:	Black	42.44	56.43	33.63		
	White	30.71	31.79	31.64		
Difference in rates		11.73	24.64	1.99	−12.91	9.74
Standard error of difference		2.20	7.93	6.34	8.23	6.71
Crimes of violence: (Committed by nonstrangers)	Black	18.45	31.99	18.02		
	White	10.73	11.73	11.93		
Difference in rates		7.72	20.26	6.09	−12.54*	1.63
Standard error of difference		1.46	5.98	4.60	6.15	4.82
Crimes of theft:	Black	88.09	89.62	83.27		
	White	98.72	97.52	87.68		
Difference in rates		−10.63	−7.90	−4.41	−2.73	−6.22
Standard error of difference		3.16	10.10	9.78	10.58	10.28

* Indicates statistical significance at the 5-percent level.

Effect of interview procedure on comparisons of victimization rates between population subgroups

Since the victimization rates reported by blacks and whites appear to be affected differently by the three interview procedures used in this study, it is possible that comparisons of victimization rates by age or race would result in different conclusions, depending on the interview procedure used.

For example, for total personal crimes, the standard NCS procedure and the maximum telephone procedure suggest only small differences between the victimization rates for whites and blacks while the maximum personal visit procedure shows the rate for blacks to be higher than the rate for whites—almost 17 per 1,000 persons. In addition, the difference between the victimization rates for blacks and whites for crimes of violence under the maximum personal visit procedure was about double that obtained under the standard NCS procedure—about 24 per 1,000 versus 12 per 1,000. For crimes of violence committed by nonstrangers, the difference between whites and blacks in the PV group was over twice that between blacks and whites in the control group—20.26 crimes per 1,000 persons versus 7.72 per 1,000. This last difference is statistically significant at the 5-percent level, but the other differences are not statistically significant. The telephone group showed almost no difference between the victimization rates for crimes of violence, and about the same difference as the control group for crimes of violence committed by nonstrangers (see table 41).

Even though the standard errors on the differences of the black-white victimization rate differences are so large as to preclude any definite conclusions, these data nevertheless suggest that the standard NCS interview procedure and the maximum telephone interview procedure may tend to compress the differential between victimization rates for blacks and whites.

An examination of the differences between victimization rates reported by persons in broad age groups by interview procedure indicates that the interview procedure probably has a relatively small effect on comparisons of victimization rates between different age groups (see table 42).

42. Differences between victimization rates for age categories, by type of interview procedure

Type of crime	Age Category	Victimization rate procedure: Standard NCS	Maximum personal visit	Maximum telephone	Difference of the differences between standard NCS procedure and—: Maximum personal visit procedure	Maximum telephone procedure
[1]	[2]	[3]	[4]	[5]	[6]	[7]
Age 12–24 versus age 25–49						
Total personal crimes:	12–24	207.29	205.00	196.90		
	25–49	128.43	131.13	113.71		
Difference in rates		78.86	73.87	83.19	4.99	– 4.33
Standard error of difference		3.16	10.00	9.70	10.49	10.20
Crimes of violence:	12–24	58.85	65.27	56.70		
	25–49	28.95	28.30	30.67		
Difference in rates		29.90	36.97	26.09	– 7.07	3.81
Standard error of difference		1.75	5.72	5.52	5.98	5.79
Crimes of theft:	12–24	148.44	139.74	140.19		
	25–49	99.48	102.83	83.11		
Difference in rates		48.96	36.91	57.08	12.05	– 8.12
Standard error of difference		2.79	8.74	8.45	9.17	8.90
Age 12–24 versus age 50+						
Total personal crimes:	12–24	207.29	205.00	196.90		
	50+	55.57	56.95	41.78		
Difference in rates		151.72	148.05	155.12	3.67	– 3.40
Standard error of difference		2.92	9.24	8.84	9.69	9.31
Crimes of violence:	12–24	58.85	65.27	56.70		
	50+	9.89	11.23	7.75		
Difference in rates		49.96	54.04	48.95	– 4.08	1.01
Standard error of difference		1.60	5.34	4.92	5.57	5.17
Crimes of theft:	12–24	148.44	139.74	140.19		
	50+	45.68	45.72	34.03		
Difference in rates		102.76	94.02	106.16	8.74	– 3.40
Standard error of difference		2.59	8.03	7.78	8.44	8.20
Age 25–49 versus age 50+						
Total personal crimes:	25–49	128.43	131.13	113.71		
	50+	55.57	56.95	41.78		
Difference in rates		72.86	74.18	71.93	– 1.32	0.93
Standard error of difference		2.34	7.48	6.81	7.84	7.20
Crimes of violence:	25–49	28.95	28.30	30.61		
	50+	9.89	11.23	7.75		
Difference in rates		19.06	17.07	22.86	1.99	– 3.80
Standard error of difference		1.12	3.58	3.48	3.75	3.66
Crimes of theft:	25–49	99.48	102.83	83.11		
	50+	45.68	45.72	34.03		
Difference in rates		53.80	57.11	49.08	– 3.31	4.72
Standard error of difference		2.11	6.73	6.00	7.05	6.36

43. Comparison of victimization rates for three interviewing procedures for crimes against all households

Type of household crime	Victimization rates per 1,000 households: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates (per 1000)	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Race of head: Total							
Total household crimes	226.86	238.77	225.56	– 11.91	– 26.33 to 2.51	1.30	– 12.92 to 15.52
Burglary	88.25	92.89	83.88	– 4.64	– 14.45 to 5.17	4.37	– 5.08 to 13.82
Forcible entry	29.18	32.43	27.32	– 3.25	– 9.22 to 2.72	1.86	– 3.70 to 7.42
Unlawful entry	38.60	39.57	36.50	– 0.97	– 7.57 to 5.63	2.10	– 4.29 to 8.49
Attempted forcible entry	20.47	20.89	20.06	– 0.42	– 5.26 to 4.42	0.41	– 4.36 to 5.18
Household larceny	122.41	129.04	124.66	– 6.63	– 17.96 to 4.70	– 2.25	– 13.48 to 8.98
Completed	113.51	119.26	115.85	– 5.75	– 16.71 to 5.21	– 2.34	– 13.22 to 8.54
Less than $50	73.37	77.69	71.60	– 4.32	– 13.36 to 4.72	1.77	– 7.01 to 10.55
$50 or more	35.76	36.13	39.44	– 0.37	– 6.69 to 5.95	– 3.68	– 10.27 to 2.91
Attempted	8.90	9.78	8.81	– 0.88	– 4.20 to 2.44	0.09	– 3.09 to 3.27
Motor vehicle theft	16.20	16.84	17.02	– 0.64	– 4.99 to 3.71	– 0.82	– 5.21 to 3.57
Completed	9.90	10.82	10.84	– 0.92	– 4.41 to 2.57	– 0.94	– 4.45 to 2.57
Attempted	6.31	6.02	6.18	0.29	– 2.34 to 2.92	0.13	– 2.54 to 2.80

Differences in victimization rates for crimes against households, by interview procedure

Total households and race of head

There is no evidence to indicate that households interviewed using the maximum personal visit procedure reported crimes against households at a rate different from those interviewed using the standard NCS procedure (see table 43).

An examination of the victimization rates for households with a white head of household also fails to provide evidence that the maximum personal visit procedure is either more or less effective than the standard NCS procedure in eliciting reports of crimes against households (see table 44).

This is not the case for households with a black head of household in the PV group and the control group. Black households in the PV group reported burglaries involving forcible entry, total burglaries, and total household crimes at a significantly higher rate than their counterparts in the control group.

For example, black households in the control group reported only 257.77 total household crimes per 1,000 households versus the 354.11 per 1,000 reported by black households in the PV group. The 95-percent confidence interval indicates that the magnitude of the difference is at least 47 per 1,000 households and could be as much as 147 per 1,000 households [see table 45, columns (2), (3), and (5)].

There is no evidence to indicate that the maximum telephone interview procedure elicits reports of household victimizations at a rate different from the standard NCS procedure. This is true for both households with a white and black head and for both groups combined.

44. Comparison of victimization rates for three interviewing procedures for crimes against households headed by whites

Type of household crime	Victimization rates per 1,000 households: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Race of head: White							
Total household crimes	223.53	225.45	220.03	− 1.92	− 17.00 to 13.16	3.50	− 11.52 to 18.52
Burglary	84.08	83.01	79.32	1.07	− 8.90 to 11.04	4.76	− 5.06 to 14.58
Forcible entry	26.10	24.44	24.29	1.66	− 3.93 to 7.25	1.81	− 3.79 to 7.41
Unlawful entry	38.82	38.38	37.13	0.44	− 6.50 to 7.38	1.69	− 5.17 to 8.55
Attempted forcible entry	19.15	20.20	17.90	− 1.05	− 6.12 to 4.02	1.25	− 3.57 to 6.07
Household larceny	123.57	127.17	122.96	− 3.60	− 15.62 to 8.42	0.61	− 11.29 to 12.51
Completed	114.83	117.75	113.18	− 2.92	− 14.55 to 8.71	1.65	− 9.84 to 13.14
Less than $50	75.17	77.72	68.60	− 2.55	− 12.20 to 7.10	6.57	− 2.62 to 15.76
$50 or more	35.32	34.64	40.22	0.68	− 5.93 to 7.20	− 4.90	− 11.98 to 2.18
Attempted	8.75	9.42	9.78	− 0.67	− 4.15 to 2.81	− 1.03	− 4.58 to 2.52
Motor vehicle thefts	15.88	15.26	17.75	0.62	− 3.81 to 5.05	− 1.87	− 6.63 to 2.89
Completed	9.48	9.87	11.29	− 0.39	− 3.95 to 3.17	− 1.81	− 5.61 to 1.99
Attempted	6.40	5.39	6.46	1.01	− 1.66 to 3.68	− 0.06	− 2.96 to 2.84

45. Comparison of victimization rates for three interviewing procedures for crimes against households headed by blacks

Type of household crime	Victimization rates per 1,000 households: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Race of head: Black							
Total household crimes	257.77	154.11	274.60	− 96.34*	− 145.21 to − 47.47	− 16.83	− 63.03 to 29.37
Burglary	122.96	174.15	127.69	− 51.19*	− 89.78 to − 12.60	− 4.73	− 39.29 to 29.83
Forcible entry	53.53	93.14	52.84	− 39.61*	− 68.97 to − 10.25	0.69	− 22.52 to 23.90
Unlawful entry	38.08	53.61	35.10	− 15.53	− 38.42 to 7.36	2.98	− 16.17 to 22.13
Attempted forcible entry	31.35	27.41	39.75	3.94	− 12.98 to 20.86	− 8.40	− 28.47 to 11.67
Household larceny	115.04	151.56	134.15	− 36.52	− 73.08 to 0.04	− 19.11	− 54.25 to 16.03
Completed	104.11	137.85	134.15	− 33.74	− 68.88 to 1.40	− 30.04	− 65.06 to 4.98
Less than $50	60.62	83.88	89.88	− 23.26	− 51.46 to 4.94	− 29.26*	− 58.50 to − 0.02
$50 or more	39.40	47.56	35.31	− 8.16	− 29.91 to 13.59	4.09	− 15.15 to 23.33
Attempted	10.94	13.71	0.00	− 2.77	− 14.63 to 9.09	− 10.94*	7.68 to 14.20
Motor vehicle theft	19.78	28.40	12.76	− 8.62	− 25.49 to 8.25	7.02	− 4.90 to 18.94
Completed	13.56	16.65	8.28	− 3.09	− 16.15 to 9.97	5.28	− 4.38 to 14.94
Attempted	6.21	11.76	4.48	− 5.55	− 16.41 to 5.31	1.73	− 5.32 to 8.78

* Indicates statistical significance at the 5-percent level.

46. Comparison of victimization rates for three interviewing procedures for crimes against all households owned or being bought

Type of household crime	Victimization rates per 1,000 households: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Race of head: Total							
Total household crimes	198.76	218.81	195.35	− 20.05*	− 37.43 to − 2.67	3.41	− 13.35 to 20.17
Burglary	73.51	83.00	72.68	− 9.49	− 21.07 to 2.09	0.83	− 10.14 to 11.80
Forcible entry	24.10	24.38	22.69	− 0.28	− 6.78 to 6.22	1.41	− 4.90 to 7.72
Unlawful entry	33.29	39.16	34.80	− 5.87	− 14.00 to 2.26	− 1.51	− 9.24 to 6.22
Attempted forcible entry	16.13	19.46	15.20	− 3.33	− 9.11 to 2.45	0.93	− 4.25 to 6.11
Household larceny	111.78	119.82	109.97	− 8.04	− 21.70 to 5.62	1.81	− 11.42 to 15.04
Completed	103.66	112.98	100.46	− 9.32	− 22.63 to 3.99	3.20	− 9.52 to 15.92
Less than $50	66.66	73.47	60.72	− 6.81	− 17.77 to 4.15	5.94	− 4.19 to 16.07
$50 or more	33.40	34.22	34.19	− 0.82	− 8.48 to 6.84	− 0.79	− 8.46 to 6.88
Attempted	8.12	6.84	9.51	1.28	− 2.23 to 4.79	− 1.39	− 5.46 to 2.68
Motor vehicle theft	13.46	15.99	12.69	− 2.53	− 7.78 to 2.72	0.77	− 3.97 to 5.51
Completed	8.37	9.45	7.72	− 1.08	− 5.14 to 2.98	0.65	− 3.06 to 4.36
Attempted	5.09	6.54	4.97	− 1.45	− 4.81 to 1.91	0.12	− 2.85 to 3.09

* Indicates statistical significance at the 5-percent level.

Crimes against households —owned or being bought

The data indicate that homeowners report more household victimizations when the maximum personal visit procedure is used than when the standard NCS procedure is used.

Overall, homeowners in the PV group reported total household crimes at a significantly higher rate than homeowners in the control group—198.76 per 1,000 in the control group versus 218.81 per 1,000 in the PV group [see table 46, columns (2), (3), and (5)].

The evidence is even more conclusive that black homeowners reported more crimes against households when interviewed using the maximum personal visit procedure than when interviewed using the standard NCS procedure. Completed household larceny, unlawful entry, burglary, and total household crimes were all reported at a significantly higher rate by black homeowners in the PV group than by black homeowners in the control group. For example, black homeowners in the PV group reported 373.40 total household crimes, versus 227.85 for black homeowners in the control group, a highly unlikely difference of 145.55 crimes per 1,000 households [see table 48, columns (2), (3), and (5)].

There is no conclusive evidence that either black or white homeowners, or both combined, reported crimes against households at different rates when interviewed using the standard NCS procedure or the maximum telepone procedure [see tables 46-48, columns (2), (4), and 7)].

47. Comparison of victimization rates for three interviewing procedures for crimes against households owned or being bought by whites

Type of household crime	Victimization rates per 1,000 households: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Race of head: White							
Total household crimes	196.89	207.36	192.27	– 10.47	– 28.25 to 7.31	4.62	– 12.80 to 22.04
Burglary	71.97	77.57	70.83	– 5.60	– 17.32 to 6.12	1.14	– 10.20 to 12.48
Forcible entry	22.57	21.25	20.36	1.32	– 5.03 to 7.67	2.21	– 4.06 to 8.48
Unlawful entry	33.92	36.54	35.81	– 2.62	– 10.84 to 5.60	– 1.89	– 10.08 to 6.30
Attempted forcible entry	15.48	19.78	14.65	– 4.30	– 10.36 to 1.76	0.83	– 4.49 to 6.15
Household larceny	111.88	116.15	109.01	– 4.27	– 18.33 to 9.79	2.87	– 10.91 to 16.65
Completed	104.08	109.58	98.93	– 5.50	– 19.20 to 8.20	5.15	– 8.06 to 18.36
Less than $50	67.68	72.94	58.87	– 5.26	– 16.65 to 6.13	8.81	– 1.65 to 19.27
$50 or more	32.90	31.54	34.70	1.36	– 6.33 to 9.05	– 1.80	– 9.86 to 6.26
Attempted	7.80	6.59	10.07	1.21	– 2.38 to 4.80	– 2.27	– 6.63 to 2.09
Motor vehicle theft	13.03	13.63	12.44	– 0.60	– 5.68 to 4.48	0.59	– 4.32 to 5.50
Completed	7.96	8.26	7.39	– 0.30	– 4.27 to 3.67	0.57	– 3.23 to 4.37
Attempted	5.08	5.37	5.05	– 0.29	– 3.49 to 2.91	0.03	– 3.10 to 3.16

48. Comparison of victimization rates for three interviewing procedures for crimes against households owned or being bought by blacks

Type of household crime	Victimization rates per 1,000 households: Standard NCS procedure	Maximum personal visit procedure	Maximum telephone procedure	Standard NCS procedure versus maximum personal visit procedure: Difference in victimization rates	95-percent confidence interval	Standard NCS procedure versus maximum telephone procedure: Difference in victimization rates	95-percent confidence interval
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Race of head: Black							
Total household crimes	227.85	373.40	218.08	– 145.55*	– 220.01 to – 71.09	9.77	– 52.98 to 72.52
Burglary	94.39	157.08	96.73	– 62.69*	– 118.42 to – 6.96	– 2.34	– 47.14 to 42.46
Forcible entry	43.00	65.42	47.57	– 22.42	– 60.34 to 15.50	– 4.57	– 36.72 to 27.58
Unlawful entry	26.99	74.87	26.14	– 47.88*	– 87.68 to – 8.08	0.85	– 23.40 to 25.10
Attempted forcible entry	24.39	16.78	23.03	7.61	– 12.80 to 28.02	1.36	– 21.46 to 24.18
Household larceny	113.50	169.53	104.51	– 56.03	– 113.69 to 1.63	8.99	– 37.59 to 55.57
Completed	101.02	158.78	104.51	– 57.76*	– 113.83 to – 1.69	– 3.49	– 49.84 to 42.86
Less than $50	56.54	80.56	68.64	– 24.02	– 65.85 to 17.81	– 12.10	– 50.15 to 25.95
$50 or more	40.17	70.33	27.66	– 30.16	– 69.22 to 8.90	12.51	– 12.89 to 37.91
Attempted	12.47	10.72	0.00	1.75	– 14.41 to 17.91	12.47*	7.23 to 17.71
Motor vehicle theft	19.95	46.78	16.84	– 26.83	– 58.85 to 5.19	3.11	– 16.58 to 22.80
Completed	14.09	25.02	12.28	– 10.93	– 34.77 to 12.91	1.81	– 15.02 to 18.64
Attempted	5.84	21.76	4.57	– 15.92	– 37.87 to 6.03	1.27	– 9.10 to 11.64

* Indicates statistical significance at the 5-percent level.

Discussion of differences in victimization rates for household crimes—standard NCS procedure versus maximum personal visit procedure

The preceding analysis of household victimization rates in the PV and control groups suggested that there were real differences between the rates, resulting primarily from the substantially higher rates obtained in the PV group among households with a black head and in particular among black homeowners.

This result was unexpected because in eliciting household crimes the two procedures should be almost identical. In both procedures the household screen questions were to be administered in a personal visit interview. Only for the individual screen questions did the two procedures differ.

It seemed unreasonable that the two procedures should produce such different results. The most reasonable explanation for this phenomenon was that more household crimes were reported by nonhousehold respondents in black households in the PV group. This could explain the difference, because in the control group only about 67 percent of the black nonhousehold respondents were interviewed by personal visit, while in the PV group about 93 percent were interviewed by personal visit (see table 49).

If the NCS interviewer did elicit a report of a household crime from a nonhousehold respondent, that incident should have been recorded under the individual or catchall screen question which elicited the report and detailed information about the incident obtained from the nonhousehold respondent. If this were done, and the hypothesis is true, then proportionately more household crimes among black households should have been reported by nonhousehold respondents in the PV group than in the control group and proportionately more of these crimes should have been elicited by individual/catchall screen questions. As can be seen in table 50, there is a difference between the distributions of total household crimes by type of respondents reporting these crimes for black households in the control group and the PV group, but not for total households or white households.

In the control group, 12.4 percent of household crimes among black households were reported by a nonhousehold respondent, compared with 15.9 percent in the PV group.

49. Type of interview conducted with nonhousehold respondents

	Control group: Number	Control group: Percent	PV group: Number	PV group: Percent	Telephone group: Number	Telephone group: Percent
[1]	[2]	[3]	[4]	[5]	[6]	[7]
Total nonhousehold						
Respondents interviewed	127,105	100.0	11,948	100.0	12,141	100.00
Personal visit	78,404	61.7	11,185	93.6	1,814	14.9
Telephone	48,701	38.3	763	6.4	10,327	85.1
Total white nonhousehold						
Respondents	112,724	100.0	10,701	100.0	10,812	100.0
Personal visit	68,849	61.1	10,023	93.7	1,486	13.7
Telephone	43,875	38.9	678	6.3	9,326	86.3
Total black nonhousehold						
Respondents interviewed	12,689	100.0	1,065	100.0	1,119	100.0
Personal visit	8,467	66.7	994	93.3	273	24.4
Telephone	4,222	33.3	71	6.7	846	75.6
Total other nonhousehold						
Respondents interviewed	1,692	100.0	182	100.0	210	100.0
Personal visit	1,088	64.3	168	92.3	55	26.2
Telephone	604	35.7	14	7.7	155	73.8

There is also some evidence that the NCS interviewers may have incorrectly recorded household crime incidents reported by a nonhousehold respondent (and elicited by an individual screen question) under a household screen question and then incorrectly interviewed the household respondent to obtain the details of these incidents. A home study exam included in an NCS interviewer memorandum for September 1976 revealed that of the 560 NCS interviewers and clerks who participated in the exam, 114 (20.4 percent) indicated that household crimes reported by nonhousehold respondents should be recorded under the appropriate household screen question.

This is incorrect since it was not a household screen question that elicited the report from the nonhousehold respondent. In addition to this type of error, 132 (23.6 percent) of the interviewers and clerks indicated that detailed information about a household crime reported by a nonhousehold respondent should be obtained from the household respondent.[16]

Note that either of these errors, or the two combined, would serve to obscure the fact that nonhousehold respondents in the PV group contributed more heavily to the victimization rates for crimes against households. The first error can be detected since crimes reported by nonhousehold respondents in answer to household screen questions are obviously inconsistent. This did in fact occur more often in the PV group than in the control group. This type of error occurred in 5.1 percent of the household crimes among black households in the PV group, compared with only 2.0 percent among the corresponding households in the control group.

Unfortunately, the second type of error cannot be detected, nor can the two errors be detected if they occur simultaneously. The error rates indicated above do serve to demonstrate the fact that such errors are likely to occur and may have occurred more often in the PV group. Thus it seems likely that the estimated differences between household victimization rates among black households in the control group and the PV group represent real differences brought about by the different interview procedures.

[16]There was probably some overlap between the 114 and 132 persons who gave these two incorrect answers, but the extent of this overlap is, unfortunately, not available.

Conclusions

The general findings are as follows:

(1) If the standard NCS interview procedure were replaced by the maximum personal visit procedure, the result would possibly be a modest increase in reported aggravated assaults by blacks. Overall personal victimization rates for the population as a whole would probably not be affected to any practical extent.

(2) For one type of crime, crimes of violence committed by nonstrangers, the

50. Distribution of total household crimes reported in control group and PV group by type of respondent and screen question

Households	Control group			PV group		
	Number	Percent	Weighted count	Number	Percent	Weighted count
Total households						
Total household crimes	11,632	100.0		1,157	100.0	
Reported by household respondents	9,978	85.8		987	85.3	
Household screen	8,635	74.2		844	72.9	
Individual/catchall screen	1,343	11.5		143	12.4	
Reported by nonhousehold respondent	1,654	14.2		170	14.7	
Household screen*	342	2.9		42	3.6	
Individual/catchall screen	1,312	11.3		128	11.1	
Total household screen	8,977	77.2		886	76.6	
Total individual/catchall screen	2,655	22.8		271	23.4	
White households						
Total household crimes	10,205	100.0		992	100.0	
Reported by household respondent	8,732	85.6		847	85.4	
Household screen	7,552	74.0		719	72.5	
Individual/catchall screen	1,180	11.6		128	12.9	
Reported by nonhousehold respondent	1,473	14.4		145	14.6	
Household screen*	315	3.1		34	3.4	
Individual/catchall screen	1,158	11.3		111	11.2	
Total household screen	7,867	77.1		753	75.9	
Total individual/catchall screen	2,338	22.9		239	24.1	
Black households						
Total household crimes	1,318	100.0	2,113,129	157	100.0	2,935,432
Reported by household respondent	1,155	87.6	1,860,848	132	84.1	2,494,436
Household screen	1,004	76.2	1,606,337	117	74.5	2,220,145
Individual/catchall screen	151	11.5	254,511	15	9.6	274,291
Reported by nonhousehold respondent	163	12.4	252,281	25	15.9	440,996
Household screen*	26	2.0	41,372	8	5.1	135,422
Individual/catchall screen	137	10.4	210,909	17	10.8	305,574
Total household screen	1,030	78.1	1,647,709	125	79.6	2,355,567
Total individual/catchall screen	288	21.9	465,420	32	20.4	579,865

* If the interviewing procedures were correctly followed, there should be no household crimes reported in this category.

maximum personal visit procedure detected a difference between the victimization rates of blacks and whites that was significantly larger than the corresponding difference obtained under the standard NCS interview procedure. Comparisons of personal victimization rates by broad age groups were essentially the same for each interview procedure.

(3) It appears that the maximum telephone interview procedure is less effective than the standard NCS procedure in eliciting reports of minor personal larcenies without contact (those in which the loss is less than $25). This seems to be particularly true for males and whites. Although these may be relatively "trivial" crimes, they form a large part of total personal crimes, and any change in these rates would undoubtedly change the rate for total personal crimes.

(4) This experiment indicates that the maximum personal visit interview procedure would produce higher victimization rates for crimes against households than the standard NCS procedure, particularly among households in which the head is black. This result is unexpected, because the maximum personal visit and the standard NCS interview procedures were essentially identical in the interview of the household respondent. The best explanation for this result is that the maximum personal visit procedure elicited substantially more reports of household crimes from respondents other than the household respondent, but that the interviewer, in the interests of "consistency," recorded these incidents under the appropriate household screen question and obtained the detailed account of the crime from the household respondent.

(5) There seems to be little difference between the victimization rates obtained for household crimes elicited by using the maximum telephone interview procedure and the standard NCS procedure.

On the whole, even though it appears that the maximum personal visit interview procedure may elicit more reports of victimizations than the standard NCS procedure for some types of crime, this procedure may not be particularly cost-efficient when one considers the added expense inherent in applying such a procedure over the entire NCS sample. Since most of the added victimizations elicited by this procedure appear to be among black respondents and households with a black head of household, it may be worthwhile to interview black or nonwhite households using the maximum personal visit procedure and white households using the standard NCS procedure or the maximum telephone procedure.

The maximum telephone procedure has been shown to be somewhat deficient in eliciting reports of minor personal larceny without contact and reports of aggravated assault among blacks relative to the standard NCS procedure. It is possible, however, that telephone interview procedures may be revised to correct these deficiencies. Such revisions would undoubtedly make the maximum telephone interview procedure more cost efficient than the standard NCS procedure. We recommend, therefore, that an effort be made to correct these deficiencies in the maximum telephone interview procedure.

It would also be desirable to test the maximum personal visit interview procedure at greater length among nonwhite households to verify that the increased victimization rates for household crimes using the PV procedure do indeed represent real procedural differences and not an experimental anomaly.

An experiment to compare three interview procedures in the National Crime Survey*

by Anthony G. Turner

This report presents some results from a special tabulation prepared from NCS experimental data to compare three interview methods. The data contained herein pertain to what is termed "identical repeat" households taken from interviews collected during the 12-month period covering July 1976-June 1977. The basic experimental results have been reported by Henry Woltman and John Bushery.[1] Though this report is intended to be self-contained, it would be useful for the reader to be familiar with the Woltman and Bushery report. The essential distinction between the two reports is that they deal with different populations.

Study methodology

Before describing the special population from which the data used in this report were produced, a discussion of the research design for the overall study is provided as background. The purpose of the experiment was to compare three methods of conducting NCS interviews in all but the incoming rotation group. The three procedures tested include (a) one in which personal interviewing was used to its maximum potential; (b) one in which telephone interviewing was used to its maximum potential; and (c) one in which the standard NCS procedure, a mix of personal and telephone interviewing, was utilized. The three procedures thus constituted three treatment groups in a three-way controlled experiment consisting of a control group and two experimental groups. The control group contained a random five-sixths of the survey sample each month, and the standard NCS interviewing procedure was employed. The two experimental groups each contained a random one-twelfth of the survey sample; one of the groups was assigned the maximum personal visit procedure and the other, the maximum telephone procedure. The assignment of units to treatment groups was done in such a way that whole segments were assigned to the same group. Interviews were carried out monthly in the NCS for a full year; altogether approximately 120,000 household interviews were obtained, involving over 275,000 persons.

The experiment was restricted to those rotation panels in the NCS that had been previously interviewed. This constraint was imposed because the unbounded interviews obtained from the incoming rotation group are not used in constructing the basic NCS estimates. This means that only addresses that had been previously contacted were the subjects of the experiment. Since telephone numbers are usually obtained in the first interview with a household, there was no need to generate telephone numbers through a special precanvass for those households which fell into the telephone test group.

The basic experiment was concerned with comparing reported victimization rates using three different interview procedures for the universe as described above. Not all persons in maximum personal visit households were actually interviewed in person. Some were interviewed by telephone. Conversely, some persons in maximum telephone households were interviewed by personal visit. Some reasons for conducting telephone interviews in the personal visit test group included those cases where the respondent would refuse the interview unless conducted by telephone and cases where the respondent was never at home after repeated visits and no firm appointment for a personal interview could be made. Permissible reasons for personal visit interviews in the telephone test group included those cases where a telephone was not available, a correct telephone number was not available, or the respondent had indicated in previous contacts that telephone interviews were unacceptable.

A certain class of units was administratively ruled ineligible for the two test procedures, even though they were randomly assigned to those groups. This class of units included replacement households appearing since the last enumeration, new construction units, and households which were nonintervew in the last enumeration. As is the case with the current rules of administering the regular, monthly NCS interviews, these units were first visited and at least one household member interviewed, in person, before telephone callbacks were permitted. The percent of sample units that have one of the statuses referred to above (replacement unit, new construction, or previous noninterview) was 13.3 for the 12-month period.

The data from the full sample reflect estimated differences among the three interview methods under current operational rules, in this case the restriction against new or previous noninterview households from initial telephone interviews. Analysis of the full sample appears to be appropriate, therefore, from the practical viewpoint of assessing mixed mode differences as operationalized using the existing field interviewing structure. For the purposes of more basic research, however, the full sample is less useful. With the full sample, we cannot be sure whether the detected differences among treatments are due to the three tested techniques or to the differential application of them. In the telephone test group, the maximum telephone procedure was not allowed in about 12 percent of the households. Instead, a personal visit procedure was used in which a face-to-face interview was required for at least one household member. Stated another way, personal visit contacts were actually made, by design, in 56 percent of the combined test group households and telephone contacts were made in the remaining 44 percent. The group of respondents represented by the 12 percent is a different population (noninterview last time or mover) than the remaining 88 percent. Treatment differences, as measured by the full sample, therefore, may be confounded by the differential mix of treatments as applied to the two populations. A "purer" measure of treatment differences can be gleaned from the part of the population that was truly eligible for the experiment—that is, households which can be termed "identical repeats."

This report is based on a special set of tabulations, which excludes new construction, replacement, or previous noninterview households from all three treatment groups. Victimization rates were thus produced for the subuniverse of households that were, in effect, eligible to participate in the experimental design. Table 51 reports the proportion of such households in each treatment.

Employing the same logic as above, one may argue that households without telephones should also be excluded from the analysis; the telephone procedure cannot, by definition, be used in such households.

*U.S. Bureau of the Census memorandum, December 1977.

[1]Henry F. Woltman and John M. Bushery, "Results of NCS Maximum Personal Visit—Maximum Telephone Interview Experiment," U.S. Bureau of the Census, December 1977.

.51. Proportion of identical repeat* households by month of interview by treatment**

Month	Treatment group		
	Standard NCS	Maximum personal visit	Maximum telephone
1976			
July	.870	.883	.866
August	.872	.852	.893
September	.858	.859	.884
October	.850	.873	.879
November	.849	.862	.864
December	.837	.843	.877
1977			
January	.854	.854	.858
February	.860	.859	.881
March	.878	.853	.898
April	.886	.901	.895
May	.885	.871	.903
June	.881	.881	.914
Total	.865	.866	.884
Number	102,554	9,897	10,010

* Households that were interviewed in the previous enumeration, (that is, that were not type A or B noninterviews last time) were not replaced by another household at the sample address nor were they added to the current sample as new construction.
** Data from NCS collection period July 1976–77.

It was decided not to exclude these households, however, on the grounds that the presence or absence of a telephone represents real differences in populations that must be dealt with in any survey, while the ambiguous treatment (with respect to type of interview) of replacement, new, or noninterview households is an artifact of the particular survey conditions under which the NCS is normally carried out. Nevertheless, apparent differences between test groups, even among the identical repeat households, could reflect not only differences due to method but actual differences in victim experience between households with and without telephones. An analysis could be directed toward this issue by producing and examining tabulations with nontelephone households excluded.

Before turning to the results, a few words should be said about differences in analytic approaches taken in this report compared with the full sample (FS) report.[2] First, the populations differ, the full sample report representing the entire sample and this report representing the subset of identical repeat households only. Second, specific crimes are studied individually in this report rather than in aggregate. For example, the crimes of robbery and aggravated assault are treated separately and are not combined together or with other types of crimes to form groupings such as "violent crimes." Next, the full sample report was, properly so, concerned with a comparison of the test groups versus the control group, the latter representing current operational procedures. This report, which focuses more directly on the differences between the personal visit and telephone interview groups and is not particularly concerned with comparing either one to standard procedures, excludes the control group. For brevity of presentation, the terms "maximum personal visit procedure" and "maximum telephone interview procedure" are generally shortened to "personal visit" and "telephone," in both the text and the tables.

For purposes of comparison between the two experimental groups, the larger victimization rate is taken to be the more accurate, consistent with the usual convention of assessing procedural differences in victimization measurement. Confidence intervals at the 95-percent level are constructed around the difference in rates between the two procedures. Standard errors were estimated in accordance with the method described in the full sample report. Normal NCS weighting, including ratio estimation to independent controls and an adjustment for nonresponse, was applied in constructing the estimates; the victimization rates are at the same level of magnitude as annual NCS estimates, though they represent a moving annual average.

Summary of results

Noninterview rates

In the discussion for this section the reader should be aware of the semantic distinction between the term "personal interview" and the terms "personal crime" or "personal theft." The former term refers to face-to-face interviews and the latter terms refer to crimes in which there is personal contact[3] between the offender and the victim.

One important indicator for evaluating survey procedures is the nonresponse rate. The type A rate—that is, the proportion of eligible sample households for which an interview should have been obtained but was not—could not be examined directly for the identical repeat households.

It is possible, however, to examine the type Z noninterview rate for the identical repeat households. The type Z rate is the proportion of persons within interviewed households for whom an interview was not obtained. The type Z rate comes about in NCS because of the requirement for self-response from all eligible members of a household (excluding 12- and 13-year-olds). The telephone group type Z nonresponse rate (2.3 percent) is statistically larger than the control group (1.8 percent) but not larger than the personal visit group (1.8 percent).

The study population, identical repeat households, represents 86.7 percent of the full sample of households interviewed during July 1976-June 1977. Since the assignment of sample segments to the three experimental groups was independent of previous interview status, the expected result is that each group should contain the same estimated proportion (.867 percent) of identical repeat households. The telephone test group is statistically larger (at the 5-percent level) than the expected value, .884 versus .867. It is conceivable that a potential interviewer effect may have contributed to the upward bias in the telephone group, though we have no concrete evidence to support the conjecture. One of the interviewer procedures required that telephone group households be verified as existing at the same location as the previous enumeration. The interviewer, in contacting a unit by phone, was to make certain not only that she had the same household as last time but that it lived at the same address. Households that move within the same general geographic area often keep the same telephone number they had previously. Such households would not be eligible for the experiment; instead their replacement households (at the old address) would be. An interviewer, however, could easily assume she had the right address if the same respondent(s) were accessed by the old telephone number. Without verifying the address location, these households would thus be erroneously interviewed in the telephone group and, in fact, counted as identical repeat households. By the nature of the experiment, a bias of this type is not possible in the personal visit group or in the control group.

Type of interview

It is of interest to examine how closely the actual interview mode corresponds to the prescribed treatment, especially for the two test groups. We would want to know what proportion of the telephone treatment respondents received telephone interviews and what proportion of the personal visit treatment respondents received face-to-face interviews.

When we examine these relationships for the identical repeat households (table 52), the proportion of maximum telephone respondents getting personal interviews indicates a sizable segment of the population with whom telephone interviewing cannot be accomplished, even when the procedure is maximized. The 13-percent figure therefore reflects the estimated proportion of the population for which personal interviews must be conducted either because phones are not available to the respondents or because respondents will not accept telephone interviews.

52. Interviewed persons, by type of interview, by month, by treatment for identical repeat households

Month	Standard NCS			Maximum personal visit			Maximum telephone		
	Type of interview			Type of interview			Type of interview		
	Total	Personal	Telephone	Total	Personal	Telephone	Total	Personal	Telephone
1976									
July	16,684	13,180	3,504	1,713	1,664	49	1,670	211	1,459
August	16,818	13,308	3,510	1,508	1,452	56	1,753	273	1,480
September	16,922	13,316	3,606	1,608	1,552	56	1,697	195	1,502
October	16,594	12,968	3,626	1,784	1,735	49	1,658	224	1,434
November	16,907	12,992	3,915	1,569	1,505	64	1,549	231	1,318
December	16,590	12,608	3,982	1,511	1,424	87	1,677	241	1,436
1977									
January	16,606	12,523	4,083	1,618	1,515	103	1,619	163	1,456
February	16,882	13,060	3,822	1,536	1,464	72	1,716	246	1,470
March	17,280	13,267	4,013	1,643	1,612	31	1,729	209	1,520
April	17,359	13,377	3,982	1,776	1,729	47	1,634	183	1,451
May	17,613	13,530	4,083	1,552	1,494	58	1,600	183	1,417
June	17,238	13,475	3,763	1,534	1,490	44	1,710	236	1,474
Total	203,493	157,604	45,889	19,352	18,636	716	20,012	2,595	17,417
Percent	100	77.4	22.6	100	96.3	3.7	100	13	87

Victimization rates for identical repeat households

The remainder of the analysis in this report is concerned with the reporting of victimizations in the survey using the two test procedures—personal visit and telephone. This poses an analytical problem because the sample sizes of the test groups are small. The statistical rarity of victimization thus means that significant differences between the two test procedures are not easy to find by employing the traditional t-test. Nonparametric methods are therefore used in an effort to detect whether there is any fundamental difference between the two test procedures.

The crime victimizations studied have been categorized into 13 non-nested groupings (6 types of personal crime and 7 types of household crime). Each of the 13 types is considered as an independent measure of the two procedures. In truth, the 13 categories are not independent since the estimates were all made from the same sample rather than from 13 independent samples. The response to the various questions on victimization experience are thus positively correlated, and there exists an intraclass correlation among sample units due to the clustering of the sample households. It is assumed, however, that these correlations are close to zero (an assumption which needs empirical verification), and the nonparametric Wilcoxon signed ranks test is used to determine the existence of predominance of one method over the other, even though confidence intervals around the treatment differences may include zero.

Of the 13 major crime categories examined, only personal theft shows a significant difference between the two test procedures (see tables 53 and 54). Personal visit gives a victimization rate which is about 1 percentage point higher than the telephone procedure (or 10 points per 1,000); when the standard error is taken into account, the difference may be as small as about one-tenth of a percentage point or as large as 1.9 percentage points. The evidence appears strong that the preference (in terms of greater reporting) for face-to-face interviews in reporting of personal theft victimizations is not universal but rather is a phenomenon of the male population. There is clearly a significant difference for white males; for black males the point estimate of the difference is as large as for white males but the standard error for blacks is too large to claim a nonzero difference. Whites (both sexes combined) show a larger rate of victimization for personal theft in the personal visit group, but this can be traced to the influence of white males.

None of the remaining 12 crimes shows a statistically significant difference between personal visit and telephone households. Considering only the direction of the difference, however, between the two treatments, it is seen that the personal visit procedure gives a higher victimization rate for 11 of the 13 victimization types. This result, by the Wilcoxon signed ranks test, is significant at approximately the 1-percent level of confidence. We would conclude, therefore, that it is improbable that the observed differences in victimization rates would occur if, in fact, the underlying rates were the same for both treatments. No conclusions, other than the ones regarding personal theft, can be drawn with respect to the magnitude of the differences, however.

The Wilcoxon test can be applied further in order to pinpoint whether the preference for personal visit interview is manifested among population subgroups. Table 55 summarizes the tests made. From table 55 it can be judged that the overall preference for face-to-face interview is concentrated in the white population in general and, particularly, in personal crimes among white

[2] See footnote 1.

[3] An exception is personal crimes of theft. These do not involve victim-offender contact but rather involve the theft of personal items away from the home.

53. Comparison of telephone and personal interview procedures for persons age 12 and over: Personal crimes

Type of personal crime	Victimization rate per 1,000 persons		Rate difference	
	Telephone	Personal visit	Personal–telephone	95-percent confidence interval
Stranger-to-stranger aggravated assault	4.32	5.50	1.18	– 1.11 to 3.47
Stranger-to-stranger simple assault	6.80	6.83	– 0.42	– 3.07 to 2.23
Nonstranger-to-nonstranger aggravated assault	3.17	3.31	0.14	– 1.72 to 2.00
Nonstranger-to-nonstranger simple assault	3.92	5.63	1.71	– 0.55 to 3.97
Robbery	4.90	5.32	0.42	– 1.92 to 2.76
Personal theft	76.33	86.10	9.77*	0.82 to 18.72
Estimated population (000's)	155,443	152,157		

*Indicates statistical significance at the 5-percent level.

54. Comparison of telephone and personal interview procedures for total households

(Rates for identical repeat households for collection period July 1976–June 1977)

Type of household victimization	Victimization rate per 1,000 persons		Rate difference	
	Telephone	Personal visit	Personal–telephone	95-percent confidence interval
Forcible entry burglary	22.23	24.15	1.92	– 5.61 to 9.45
Unlawful entry burglary	31.43	35.66	4.23	– 4.78 to 13.24
Attempted burglary	16.63	19.75	3.12	– 3.57 to 9.81
Larceny under $50	63.42	67.11	3.69	– 8.67 to 16.05
Larceny $50 or more	33.35	30.00	– 3.35	– 12.11 to 5.41
Completed motor vehicle theft	7.31	8.70	1.39	– 3.07 to 5.85
Attempted motor vehicle theft	5.36	5.98	0.62	– 3.14 to 4.38
Estimated households (000's)	66,151	65,609		

victims. There is no clear-cut evidence that household crimes are better reported under the maximum personal visit procedure. Further, the male population is likely to report more victimizations in a personal interview than by phone. These results are consistent with the findings for personal theft.

Summary

In attempting to evaluate two survey procedures, several indicators are compared for the restricted set of households that were genuinely exposed to the experimental treatments. The type A noninterview rates could not be computed for the identical repeat households. For the full sample, however, they show virtually identical values for the personal visit group and the telephone group. The type Z rates for the restricted universe do not show a statistical difference between the telephone and personal visit groups. The telephone group, however, has a higher type Z nonresponse than the standard NCS procedure, whereas the personal visit group is about the same as the standard NCS. A second indicator, size of household, shows comparable values between the two test groups with the telephone procedure appearing to give slightly better within-household coverage.

Perhaps the key statistics in assessing the quality of the two procedures are the victimization rates. Personal theft is the only crime of 13 examined where one procedure (face-to-face interviews) yielded a statistically larger victim rate. This may be partly an artifact of the small sample

55. Summary of Wilcoxon signed ranks test comparisons

Population group	Type of crime	Number of comparisons = n	Wilcoxon test statistic*	Level of significance
Total	All	13	12.5	.01
White	All	13	16	.025
Black	All	13	31.5	.20
Total	Personal	12	14	.05
		12	20.5	.10
Male	Personal	12	13	.025
Female	Personal	12	39	.50
White	Personal	12	13	.025
Black	Personal	12	34	.40
Total	Households	14	31	.10
		14	44.5	.40

* The test statistic equals the sum of the ranks assigned to those paired differences (PV_I, T_I) where T_I (telephone victim rate for the Ith crime) exceeds PV_I (personal visit rate for the Ith crime). So, when PV_I is larger than T_I, define R_I to be equal to zero; when T_I is larger, R_I = absolute rank assigned to the pair. The test statistic is then equal to $\sum_{I}^{n} R_I$. The first line in the table is interpreted to mean, therefore, that we can be 99 percent confident of the finding that the personal visit procedure produces a larger victimization rate, for all crime in general, than the telephone procedure, as long as we assume that the 13 crime comparisons made are virtually independent.

sizes. Personal theft is the crime with the greatest frequency of occurrence; it is not surprising that if statistical differences exist between the two test procedures, they are likely to be detected for this crime. The maximum estimated difference between the two procedures for personal theft is only about 2 victimizations per 100 reports. White males, in particular, manifest a propensity for more victim reports of personal theft in a face-to-face interview than in a telephone interview.

For victimization reporting in general, a nonparametric test provides evidence that the maximum face-to-face interview procedure is better than the maximum telephone interview procedure; that it appears to be a phenomenon of the white population in reporting personal crimes, as opposed to household crimes; and that males are the chief subgroup contributing to the difference. The test used, however, does not permit any inferences to be made about the size of the differences between the two procedures.

Viewing all the evidence together, it is very difficult to discard the telephone procedure as a promising interview mode. Insofar as the response rates and within-household coverage rates are concerned, the telephone procedure appears to be at least as good as the personal visit procedure, with the possible exception that the type Z noninterview rate may be higher with the telephone procedure. As for victimization rates, while the available evidence does not favor telephone interviews, neither is there a universal superiority exhibited by personal visit interviews.

Analysis of NCS recheck results*

by JOHN LINEBARGER

Summary

This memorandum presents the results of the recheck of the procedures used in the NCS maximum telephone/maximum personal visit experiment. The main purpose for taking the recheck samples was to estimate the rate of interviewer nonadherence to experimental procedures on a monthly basis. Interviewer nonadherence to procedures is defined as cases in which the interview method as recorded by the interviewer is different from the interview method stated in the recheck response.

The results of the recheck indicate no flagrant violations of procedures on the part of the interviewers. For all 12 months examined, no error rates were significantly higher than 2.5 percent. However, when procedures were violated, they tended to be of the form where a personal interview was conducted when a telephone interview was marked by the interviewer as having been conducted.

56. Error rates for maximum personal visits and maximum telephone groups by month of interview

Month	Max PV		Max T	
	Estimated error rate	95-percent confidence limits	Estimated error rate	95-percent confidence limits
[1]	[2]	[3]	[4]	[5]
1976				
July	.016	.005, .028	.032	.017, .046
August	.013	.003, .024	.040	.022, .058
September	.019	.009, .030	.041	.022, .060
October	.025	.013, .037	.066	.035, .097
November	.029	.010, .048	.039	.021, .061
December	.028	.009, .047	.035	.020, .050
1977				
January	.034	.014, .054	.029	.013, .045
February	.042	.023, .061	.004	.000, .008
March	.042	.022, .062	.025	.011, .039
April	.011	.001, .021	.059	.034, .084
May	.015	.003, .027	.049	.029, .069
June	.028	.012, .044	.023	.010, .036
Overall	.024	.018, .030	.038	.032, .044

Methodology used in this study

The sample of households was selected by each regional office via a preprinted sample selection sheet. The designated sample households were then contacted by telephone and the respondents in the household were asked if they had been interviewed originally in person or by telephone. This contact constituted the "recheck response." Cases where the original NCS interview had been by proxy were not included in the analysis.

Two test groups were involved—the maximum personal visit group (MAX PV) and maximum telephone group (MAX T). In each test group, the NCS interview was supposed to be conducted by the indicated "maximum" method, with certain limited exceptions. The original specifications called for approximately 480 households (around 240 per test treatment) to be selected each month for the recheck.

Not all persons from each household selected for the sample were included in the analysis presented below. A person was excluded from analysis for any of the following reasons;

(1) The person contacted in the recheck said that she or he or other respondent had not been interviewed originally (NI);

(2) The person contacted in the recheck said that he or she or other respondent had been interviewed originally by proxy (P);

(3) No recheck response was obtained from the respondent or was obtained by proxy, along with other circumstances which warranted excluding a case from analysis (O). The majority of these cases were due to respondents not at home, telephones disconnected, and telephone numbers changed.

Because some of the excluded persons made up entire households, the average number of households analyzed each month was 228 rather than 240.

Results

Differences between MAX PV and MAX T error rates, by test group

The rate of interviewer nonadherence within each experimental test group will be referred to as the "error rate" and is of the form $A-B$ where:

A = number of persons in the sample within a given test group for whom the recheck response differs from the interviewer's record.

B = number of persons in the sample for whom data were obtained on the method of interview, within a given test group.

These error rate estimates along with the 95-percent confidence limits are presented by month in table 56.

The notes pertaining to the recheck responses from the regional offices from July to November cast some doubt as to the respondent's ability to remember the exact method of interview (particularly if they had been part of the panel for a long time). This uncertainty triggered concern that the interviewer error rates were overstated. Hence, in December the recheck interviewers were instructed to persist until they received positive assurance as to the actual method of interview. The notes pertaining to the December recheck responses showed that the interviewers' records were clearly incorrect in about 50 percent of the difference cases. It was hard to discern from the other 50 percent of the notes on difference cases whether the interviewer's record or the recheck response was incorrect. Because all difference cases were counted against the interviewer, the error rates for both test groups may be overstated.

Table 57 examines whether the monthly error rates of each test group are significantly (S) or not significantly (NS) different at the .05 level. The table indicates that the error rates were significantly different at the .05 level in 5 of the 12 months examined (September, October, February, April, and May) as well as overall. The error rate within the MAX PV segment was higher only in February. Hence, these results indicate that the rates of interviewer nonadherence to procedures were more likely to be higher within the MAX T test group than within the MAX PV test group.

*U.S. Bureau of the Census memorandum, January 18, 1978.

57. Tests of significance for the difference in error rates by month of interview

Month	PV Error rate	T Error rate	Significant difference at .05 level
[1]	[2]	[3]	[4]
1976			
July	.016	.032	NS
August	.013	.040	NS
September	.019	.041	S [$PV < T$]
October	.025	.066	S [$PV < T$]
November	.029	.041	NS
December	.028	.035	NS
1977			
January	.034	.029	NS
February	.042	.004	S [$PV > T$]
March	.042	.025	NS
April	.011	.059	S [$PV < T$]
May	.015	.049	S [$PV < T$]
June	.028	.023	NS
Overall	.024	.038	S [$PV < T$]

58. Tests of significance for the difference in personal visit and telephone error rates by month of interview

Month	PV error rate (combined test groups)	T error rate (combined test groups)	Significant difference at <.05 level
[1]	[2]	[3]	[4]
1976			
July	.015	.035	S [$PV < T$]
August	.021	.034	NS
September	.019	.042	S [$PV < T$]
October	.029	.064	S [$PV < T$]
November	.031	.037	NS
December	.028	.036	NS
1977			
January	.031	.032	NS
February	.031	.013	NS
March	.047	.024	NS
April	.027	.044	NS
May	.017	.047	S [$PV < T$]
June	.023	.028	NS
Overall	.031	.037	NS

It should be noted that at the .05 level none of the error rates was significantly higher than 2.5 percent in any month for either test group. This result is encouraging since 2.5 percent is a reasonably small error rate.

Differences between personal visit interview and telephone interview error rates, independent of test group

As noted previously, there were limited exceptions where the interviewer recorded using the "nonmaximum" method for the test group. Therefore, it is necessary to combine test groups (MAX PV and MAX T) to examine whether interviewers used the other method more often when they recorded the interview as telephone, or when they recorded the interview method as personal visit. The monthly error rates presented in table 58 are defined as follows:

$$\text{(1) PV error rate} = \frac{\text{Number of cases from both test groups where interviewer recorded interview method as a personal visit, but used telephone to recheck}}{\text{Number of cases from both test groups where interviewer recorded interview method as personal visit}}$$

$$\text{(2) T error rate} = \frac{\text{Number of cases from both test groups where interviewer recorded method as telephone, but used personal visit according to recheck}}{\text{Number of cases from both test groups where interviewer recorded interview method as telephone}}$$

One can see from table 58 that there were 4 months in which the personal interview error rates and telephone error rates were significantly different at the .05 level (July, September, October, and May). In these months, the T error rates were higher than the PV error rates. A sign test on the monthly differences provides additional evidence at the .05 level that the T error rate tended to be larger than the PV error rate.

Thus, it is possible that (1) interviewers for the NCS survey found it more convenient to conduct a personal interview than a telephone interview; (2) the respondents' memories were biased toward personal interview because each household in NCS is required to be visited in person for the initial interview; or (3) both of the above reasons apply.

Chapter 3

Nonsampling errors in the National Crime Survey

Introduction

Sample surveys are subject to both sampling and nonsampling errors. The causes of nonsampling errors are not fully understood; however, they are present in every survey and have received increasing attention from researchers. The Bureau of the Census, with the encouragement of the Bureau of Justice Statistics, has sought to identify the types of nonsampling error in the National Crime Survey (NCS) and to calculate the effects of these errors on estimates of victimization rates. The six papers in this chapter discuss several sources of nonsampling error present in NCS. Some are unique to NCS, but most are inherent to sample survey designs in general.

The first paper by Dorcas Graham identifies the items in the NCS questionnaire most subject to response error. The Graham study is based on a test-retest methodology involving the verification of interview data through a reinterview and reconciliation process.

The second paper by Richard Dodge examines the final classification of victimizations by the screen questions used to capture the events. Dodge finds that most screen questions are responsible for the largest percentage of the type of victimizations they were designed to capture, but he recommends that the screen questions for household victimizations be tested by asking them of all respondents.

Respondents in the NCS who are 12 and 13 years old are interviewed by proxy, usually a parent. Anthony Turner's paper reports on a split-half experimental study designed to estimate the effects of person versus proxy interviewing on victimization reporting.

Between 1972 and 1975 the Bureau of the Census, with LEAA funding, conducted a special set of city surveys'' in 26 separate metropolitan areas. Although the design of the city surveys differed from the NCS household panel, they used a similar questionnaire and included supplemental attitudinal questions on crime-related topics. The report by Charles Cowan, Linda Murphy, and Judy Wiener summarizes the findings of a study designed to determine the effects of supplemental attitudinal questions on victimization reporting.

An analysis of completed questionnaires for reports of ''personal larceny without contact'' victimizations by Richard Dodge examines whether there is error in the interpretation and classification of this type of victimization. Dodge's study suggests that respondents may not fully understand the distinctions involved in the definition of that type of crime and that interviewers appear to make numerous mistakes in recording information pertaining to this type of victimization.

The final paper in this chapter by Carol Spivey examines errors in the reporting of household social-demographic ''Neighborhood Characteristics.'' Spivey examines the reasons why some households do not have neighborhood characteristic information attached to the interview record and identifies several sources of demographic change associated with the underreporting

Response errors in the National Crime Survey: July 1974–June 1976*

by Dorcas Graham

Introduction

This report focuses on the effects of errors arising from the erroneous reporting of circumstances surrounding criminal acts on the quality of data collected in the NCS. Data required for the assessment of accuracy of reporting were obtained by conducting a reinterview survey (described in Appendix A) in a subsample of households. This report succeeds a similar report of March 1975, which provided an evaluation of response errors for data collected from November 1972 through June 1974. Because of the limited respondent universe for the reinterview survey prior to January 1977, a period of 18 to 24 months was required to cumulate enough data to produce reliable measures of response error.

The accuracy of survey results can be evaluated by determining the extent to which repeated measures of the same concept yield the same responses. The method used is to compare the results of the two interviews and compute response error measures. The two measures of response error discussed in this analysis are the index of inconsistent response (a measure of gross error or response variability) and the net difference rate (a measure of net error or bias). The effect of response error on the quality of data for a particular category is reflected by the levels of net and gross errors associated with that category. Appendix B of this report contains a more detailed definition of these measures.

Since less than half of the categories had enough cases to compute reliable measures, only a limited evaluation of variability between responses for multiple-response items on the two interviews is given in this report. Table 59 shows the indexes of inconsistent response for 28 multiple-response categories. Approximately one-third of the categories indicated moderate levels of inconsistent reporting. Indexes in the 20-50 range are defined as "moderate." Comparison of the 28 indexes shown in the tables with results for November 1972–June 1974 did not reveal any significant differences between the estimated indexes. For the 22 single-response items shown in table 60, the individual indexes revealed that responses to identical questions on the two interviews for most of the categories tended to be highly correlated. The level of inconsistency was for the most part similar to past results.

Thirteen of the 28 multiple-response categories shown in table 59 showed evidence of bias. For the single-response items, there were five that showed evidence of bias in the original survey distributions. Since the two "don't know" categories in

59. Response error measures for items with multiple responses

Item number	Before reconciliation: L-fold index of inconsistent response: Sample estimate	Before reconciliation: L-fold index of inconsistent response: 95-percent confidence limits	After reconciliation: Percent in class (reinterview estimate)	After reconciliation: Net difference: Sample estimate	After reconciliation: Net difference: 95-percent confidence limits
6c. Any evidence offender(s) forced way in building?					
Broken lock or window	14.6	9.4 to 22.8	20.0	1.5	−0.2 to 3.1
Forced door or window	20.6	14.3 to 29.9	19.1	2.7	−0.9 to 4.6
Slashed screen	27.4	16.4 to 45.7	5.9	0.5	−0.8 to 1.8
Other	40.8	28.7 to 58.1	8.2	3.0	−0.8 to 5.1
7a. Did the person have a weapon?					
Gun	6.7	2.2 to 20.3	9.1	0.8	−0.6 to 2.
Other weapon	12.2	5.1 to 28.9	8.8	−0.4	−2.4 to 1.
7d. How were you threatened?					
Verbal threat of attack	23.0	11.5 to 46.0	67.1	1.2	−4.8 to 7.2
Weapon present or threatened with weapon	22.5	11.3 to 45.0	26.8	6.1	+0.1 to 14.
7e. What actually happened?					
Something taken without permission	0.0	0.0 to 7.3	50.4	0.0	−3.6 to 3.6
Forcible entry of house	10.7	4.1 to 28.0	21.1	0.9	−2.8 to 4.6
7f. How did the person attack you?					
Hit, slapped, knocked down	3.4	0.6 to 19.6	53.3	0.0	−6.7 to 6.7
Grabbed, held, tripped, jumped, pushed	20.5	9.3 to 45.5	38.3	+1.7	−3.0 to 9.7
8a. What were the injuries you suffered, if any?					
None	7.1	1.9 to 26.5	34.4	0.0	−6.6 to 6.6
Bruises, black eye, cuts, scratches, swellings	6.6	1.7 to 24.5	50.8	3.3	−2.4 to 12.2
13c. What did they try to take?					
Part of car	15.6	7.4 to 32.6	28.6	3.8	−1.2 to 8.8
Don't know	16.2	7.3 to 35.9	22.8	−1.0	−5.6 to 3.7
13e. What did happen?					
Attacked	5.9	2.3 to 15.5	14.9	1.2	−0.4 to 3.5
Threatened with harm	18.9	12.1 to 29.4	28.6	0.4	−2.5 to 3.3
Attempted to break into house or garage	6.4	3.0 to 13.4	30.2	1.6	−0.5 to 3.6
Harrassed, argument, abusive language	12.5	6.5 to 24.0	16.1	0.4	−0.7 to 2.3
Damaged or destroyed property	14.3	6.5 to 31.8	8.6	0.0	−1.8 to 1.8
Other	20.6	13.0 to 32.4	22.4	0.4	−2.5 to 3.3
13f. What was taken?					
Only cash taken	27.6	19.4 to 39.2	4.0	1.8	1.0 to 2.8
Purse	34.1	21.6 to 53.7	1.7	1.3	0.6 to 2.1
Wallet	18.9	11.5 to 31.0	3.1	1.0	0.4 to 1.7
Car	20.0	12.7 to 31.5	3.4	1.5	0.8 to 2.4
Part of car	14.5	11.0 to 19.1	17.5	2.2	1.2 to 3.2
Other	11.7	8.9 to 15.3	71.1	1.0	0.1 to 1.9

*U.S. Bureau of the Census memorandum, August 10, 1977.

60. Response error measures for single-response items

Item number and category	Before reconciliation: Index of inconsistent response: Sample estimate	Before reconciliation: Index of inconsistent response: 95-percent confidence limits	After reconciliation: Percent in class (reinterview estimate)	After reconciliation: Net difference: Sample estimate	After reconciliation: Net difference: 95-percent confidence limits
Check Item					
A. Is this incident report for a series of crimes?					
No	13.6	7.8 to 23.5	NA	NA	NA
Yes	13.6	7.8 to 23.5	NA	NA	NA
1. In what month did incident happen?					
L-fold index	15.6	13.8 to 17.7			
2. About what time did this (the most recent incident) happen?					
L-fold index	20.0	17.8 to 22.6			
Don't know	26.3	21.9 to 31.6	15.8	0.7	−0.1 to 1.5
6 a.m. to 6 p.m. (daytime)	12.3	9.0 to 15.2	35.8	−0.6	−1.3 to 0.2
6 p.m. to midnight (night)	17.2	14.1 to 21.1	25.0	−0.1	−1.1 to 0.8
Midnight to 6 a.m.	16.3	12.9 to 20.7	17.3	−0.3	−1.0 to 0.4
Don't know	44.8	36.9 to 54.4	6.1	0.3	−0.4 to 1.0
3b. Did it happen inside the limits of a city, town, village, etc.?					
Yes	12.9	9.7 to 17.1	14.4	−0.6	−1.2 to 0.0
No	12.9	9.7 to 17.1	85.6	0.6	0.0 to 1.2
4. Where did the incident take place?					
L-fold index	8.9	7.4 to 10.8			
At or in own home/apt., garage or other building on property	4.3	2.9 to 6.2	29.8	−0.3	−0.9 to 0.2
At or in vacation home, hotel/motel	22.2	11.6 to 42.8	1.2	*	*
Inside commercial building such as store, restaurant, bank, gas station, public conveyance or station	10.6	6.6 to 16.9	5.4	0.3	−0.1 to 0.8
Inside office, factory or warehouse	*	*	0.5	*	*
Near own home, yard, sidewalk, driveway, carport	8.8	6.8 to 11.5	28.3	0.1	−0.6 to 0.8
On the street, in a park, field, playground, school grounds, or parking lot	9.4	7.2 to 12.2	26.0	−0.3	−1.0 to 0.4
Inside school	3.0	1.6 to 7.9	4.5	−0.1	−0.3 to 0.2
Other	23.2	16.4 to 32.8	4.3	−0.1	−0.5 to 0.3
5a. Were you a customer, employee, or owner?					
L-fold index	2.3	0.4 to 13.6			
Customer	2.5	0.4 to 14.4	60.0	1.2	−2.2 to 6.8
Employee	2.6	0.4 to 14.9	36.5	−1.2	−6.8 to 2.1
Owner	*	*	2.4	*	*
Other	*	*	1.2	*	*
5b. Did the person(s) steal or try to steal anything belonging to store, office, factory, etc.?					
L-fold index	24.1	15.2 to 41.1			
Yes	*	*	15.2	*	*
No	26.0	14.3 to 47.1	60.8	−5.1	−13.2 to 0.6
Don't know	15.5	6.5 to 36.8	24.0	2.5	−1.8 to 9.4
6a. Did the offender(s) live there or have a right to be there?					
L-fold index	24.0	18.4 to 31.6			
Yes	8.0	4.0 to 16.0	12.4	0.5	−0.6 to 1.5
No	25.0	18.5 to 33.6	77.0	−2.1	−3.3 to 0.3
Don't know	36.8	26.9 to 50.2	10.6	1.6	0.2 to 3.2
6b. Did the offender(s) get in or just try to get in the building?					
L-fold index	5.4	2.8 to 10.5			
Actually got in	3.1	1.3 to 7.5	73.8	2.6	−0.5 to 1.5
Just tried to get in	5.5	2.8 to 11.1	23.2	−0.5	−1.9 to 0.4
Don't know	*	*	3.0	*	*
6d. How did offender(s) get in/try to get in?					
L-fold index	9.6	5.2 to 17.9			
Through unlocked door or window	7.2	3.2 to 15.9	58.4	−0.6	−3.0 to 1.8
Had key	*	*	10.2	*	*
Don't know	10.5	4.4 to 25.0	16.3	0.0	−2.1 to 2.1
Other	12.8	5.8 to 28.4	15.1	1.2	−0.9 to 4.5
7b. Did the person(s) hit you, knock you down, or actually attack you in any way?					
Yes	4.1	1.6 to 10.8	24.2	−0.4	−2.2 to 0.7
No	4.1	1.6 to 10.8	75.8	0.4	−0.7 to 2.2
7c. Did the person(s) threaten you with harm?					
Yes	9.5	4.9 to 18.3	42.8	−1.0	−3.4 to 1.3
No	9.5	4.9 to 18.3	57.1	1.0	−1.3 to 3.4
11. Was the crime committed by only one or more than one person?					
L-fold index	8.4	4.8 to 14.9			
Only one	7.2	3.8 to 14.0	59.0	0.4	−1.2 to 2.0
Don't know	11.1	5.0 to 24.4	11.6	0.8	−0.6 to 3.0
More than one	8.5	4.4 to 16.4	29.3	−1.2	−3.6 to 0.4
11a. Was this person male or female?					
L-fold index	0.0	0.0 to 9.5			
Male	0.0	0.0 to 9.6	83.0	0.0	−2.7 to 2.7
Female	0.0	0.0 to 10.3	15.6	0.0	−2.7 to 2.7
Don't know	*	*	1.4	*	*
12a. Were you the only person there besides the offender(s)?					
Yes	10.4	6.0 to 18.0	38.9	2.4	−0.3 to 5.3
No	10.4	6.0 to 18.0	61.1	−2.4	−5.3 to −0.3
13a. Was something stolen or taken without permission that belonged to you or others in the household?					
Yes	3.9	2.6 to 6.0	76.3	−0.4	−0.9 to 0.1
No	3.9	2.6 to 6.0	23.7	0.4	−0.1 to 0.9

continued on next page

* Too few sample cases in category to compute reliable measures of response error. NA Item was not reconciled; therefore, net difference rate could not be computed.

60. Response error measures for single-response items—(continued)

Item number and category	Before reconciliation: Index of inconsistent response, Sample estimate	Before reconciliation: Index of inconsistent response, 95-percent confidence limits	After reconciliation: Percent in class (reinterview estimate)	After reconciliation: Net difference, Sample estimate	After reconciliation: Net difference, 95-percent confidence limits
13b. Did the person(s) attempt to take something that belonged to you or others in the household?					
Yes	9.7	5.8 to 16.3	29.5	1.4	−0.5 to 3.3
No	9.7	5.8 to 16.3	70.5	−1.4	−3.3 to 0.5
16a. Was all or part of the stolen money or property recovered, not counting anything received from insurance?					
L-fold index	12.5	8.6 to 18.2			
None	11.5	7.7 to 17.1	89.4	0.9	0.1 to 1.6
All	11.1	6.5 to 18.8	5.7	−0.3	−0.8 to 0.3
Part	16.4	10.1 to 26.4	4.9	−0.6	−1.2 to 0.0
17a. Was there any insurance against theft?					
L-fold index	19.1	15.7 to 23.3			
No	16.5	13.3 to 20.5	68.4	0.2	0.1 to 1.4
Don't know	41.2	29.1 to 22.5	2.8	0.4	−0.3 to 1.0
Yes	18.2	14.7 to 22.5	28.8	−0.5	−1.8 to 0.7
17b. Was this loss reported to an insurance company?					
L-fold index	4.8	2.3 to 10.1			
Yes	3.5	1.4 to 8.3	53.0	0.0	−1.2 to 1.2
No	4.9	2.3 to 10.2	47.0	−0.3	−1.7 to 1.0
17c. Was any of this loss recovered through insurance?					
L-fold index	17.5	9.9 to 31.0			
Not yet settled	19.7	10.6 to 36.7	19.2	2.0	−1.3 to 5.4
Yes	11.1	5.3 to 23.2	74.0	−1.4	−4.4 to 1.7
No	•	•	6.8	•	•
20a. Were police informed of this incident?					
L-fold index	9.3	7.4 to 11.7			
No	7.0	5.4 to 9.3	61.6	0.2	−0.4 to 0.8
Don't know	•	•	0.3	•	•
Yes—Who told them?					
Household member	7.5	5.7 to 9.9	33.2	0.0	−0.7 to 0.7
Someone else	22.1	15.7 to 32.2	3.8	−0.1	−0.5 to 0.4
Police on scene	•	•	1.1	•	•

* Too few sample cases in category to compute reliable measures of response error. NA Item was not reconciled; therefore, net difference rate could not be computed.

item 2 (time occurred—table 60) were moderately inconsistent in the past we recommend that they be revised. This would reduce the possiblility for errors which we suspect are inadvertent interviewer errors in marking the correct "don't know" category rather than real changes between responses on the two interviews.

Summary of results for the index of inconsistent response

General guidelines for interpreting the index of inconsistent response computed for the individual categories and the L-fold indexes are: indexes below 20 are low; indexes from 20 through 50 are moderate; and those over 50 are high, indicating that improvements are needed in the method used to collect these data or that the category concepts themselves are ambiguous.

There were 14 items for which the respondents' answers could be classified in more than one category. For analytical purposes, each category of the multiple responses was treated as a separate distribution and original and reinterview responses were compared in a 2 x 2 classification system using the two classes "mentioned category" and "did not mention category."

Tables 59 and 60 are divided into two sections (before and after reconciliation) with each section showing a different response-error measure. The "before reconciliation" section shows the L-fold index and individual indexes for each category of an item. When there are only two categories in a distribution, the index for both categories and the L-fold index are identical. Therefore, the numbers in the L-fold column in table 59 also represent the estimated index for categories which were mentioned and also categories which were not mentioned by the respondents. The L-fold index, which is a weighted average of the individual categories, provides a measure of the amount of inconsistency in the entire distribution.

Fourteen multiple-response items were asked on the reinterview survey. Separate response-error measures were computed for each of the 78 categories; however, only 28 categories had enough cases to yield reliable measures of response error. About two-thirds of these categories had low levels of inconsistency and the remaining one-third were in the moderately inconsistent range. These occurred in items 6c, 7d, 7f, 13e, and 13f (see table 59 for specific categories affected). The level of inconsistency in reporting for multiple-response items was about the same as that measured for November 1972–June 1974 for all categories except 13e (purse taken), which showed a significantly higher level of inconsistency.

Of the 44 single-response items (only one category could be marked per item) asked on reinterview, only 22 items had enough cases to compute reliable measures. Three of the L-fold indexes, items 2, 5b, and 6a, were in the moderately inconsistent range with the remaining items exhibiting low levels of disagreement. However, indexes for individual categories of some of the distributions indicated some problems with response disagreement. The items and categories affected are listed below. Four of the categories affected were "don't know" categories.

For both of the "don't know" categories in Item 2, approximately one-half of the response differences were caused by a shift between the two "don't know" categories. We suspect that most of the differences for these cases may be attributed to interviewer uncertainty as to which "don't know" category to mark rather than to any real change in responses. For the remaining half of the "don't know" responses, the shifts in responses were generally from a "don't know" response on the original survey to a specific response on the reinterview survey. We recommend that the second "don't know" category be replaced with a comparable category which could decrease the possibility for errors in categorizing "don't know" responses—for example, "sometime between 6 p.m. and 6 a.m."

Net difference rate

The "after reconciliation" section of tables 59 and 60 shows net difference rates and their 95-percent confidence intervals which indicate if systematic errors in reporting have introduced bias into the distribution; that is, it is very likely that there is a bias in a particular category of a distribution when the 95-percent confidence interval of the net difference does not include zero as a possible value. However, this conclusion is subject to a 5-percent chance of an estimate of bias lying outside the limits indicated. The sign of the limits of the interval indicate the direction of the bias.

The after-reconciliation section also indicates the proportion of persons in the category according to the reinterview. The sum of these values (percent in class and net difference) equals the proportion of persons in the category, according to the original survey. For example, the after-reconciliation entry for Item 6a shows that according to the reinterview, in 77 percent of the crime incidents the offender did not live there or have a right to be there. The estimated net difference rate for this category is −2.1 percent, indicating that the original survey underestimated the number of persons in this category. Therefore, in the original survey 74.9 (77.0−2.1) percent of the crime incidents involved offenders who did not live there or have a right to be there.

If the two interviews were truly independent, the difference between the original and reinterview estimates of the proportion of responses in a category should not be greater than that expected to result from sampling error. However, as indicated by the following net difference rates, some differences beyond those due to sampling error did occur for various categories. Most of the categories indicated that the original survey overstated the number of cases in the categories. All of the possible response categories for Item 13f (what was taken) showed evidence of overstatement in the original survey. Reinterview results indicated that some fairly small biases exist in the original survey distributions.

Item	Category	Sample estimate	95 percent confidence limits
2. What time did the incident happen?	Don't know whether day or night	26.3[1]	21.9 to 31.6
	At night—don't know time	44.8[1]	36.9 to 54.4
4. Where did the incident occur?	At or in vacation home, hotel/motel	22.8	11.6 to 42.8
	Other	23.2	16.4 to 32.8
5b. Did the person steal/try to steal from a store, office, factory?	No	26.0	14.3 to 47.1
6a. Did the offender live there or have a right to be there?	No	25.0	18.5 to 33.6
	Don't know	36.8	26.9 to 50.2
17a. Was there any insurance against theft?	Don't know	41.2[1]	29.1 to 58.3
20a. Who informed the police of this incident?	Someone else	22.1	15.7 to 32.2

[1]Categories which also had estimates of inconsistency in the 20–50 range in the report published for data collected from November 1972–June 1974.

Item	Category	Sample estimate	95 percent confidence limits
6a. Did offender live there or have a right to be there?	No	− 2.1[2]	−3.8 to −0.3
	Don't know	+ 1.6[2]	+0.2 to +3.2
12a. Were you the only person there besides the offender?	Yes	+ 2.4	+0.3 to +5.3
	No	− 2.4	−5.3 to −0.3
16a. Was all or part of the stolen money or property recovered?	None	+ 0.9	+0.1 to +1.6
6c. Any evidence offender forced way into building?	Other	+ 3.0	+0.8 to +5.1
7d. How were you threatened?	Weapon present or threatened with weapon	+ 6.1	+0.1 to +14.5
13f. What was taken?	Mentioned only cash taken	+ 1.8	+1.0 to +2.8
	Mentioned purse	+ 1.3	+0.6 to +2.1
	Mentioned wallet	+ 1.0	+0.4 to +1.7
	Mentioned car	+ 1.5	+0.8 to +2.4
	Mentioned part of car	+ 2.2	+1.2 to +3.2
	Mentioned other	+ 1.0[2]	+0.1 to +1.9

[2]Categories also showed evidence of bias in the same direction in the November 1972–June 1974 analysis of response errors.

Appendix A:

Description of reinterview survey

The reinterview survey, which is conducted monthly, involves the systematic selection of a subsample of interviewers (one-sixth of the interviewers and one-third of their workload) for the purpose of checking their work in the areas of coverage and content. In coverage reinterview, the supervisor repeats the listing by recanvassing each designated segment and checks the living quarters found against the list originally prepared by the interviewer. Also, the number of persons in each household is checked against the listing prepared by the interviewer.

The origin of coverage errors may be summarized as follows:

1. The erroneous omission of units may arise from the failure to canvass the land area of a segment thoroughly, from overlooking units in a multiunit structure, or from mistakenly regarding two or more units as a single unit. The erroneous omission of persons could arise from erroneous omission of units, from failure to list all usual residents on the control card, mistakenly regarding a household as vacant, and from mistakenly regarding a household member as a nonmember because of some misunderstanding about the definition of usual residence.

2. The erroneous inclusion of units could arise from listing units located outside segment boundaries, from mistakenly regarding a single unit as two or more units, and occasionally from the dishonest fabrication of units. The erroneous inclusion of persons could arise from the erroneous inclusion of units, from mistakenly regarding vacant units or units temporarily occupied by nonresidents as occupied units, and from mistakenly regarding nonmembers of households as members and placing these persons on the household roster.

Content reinterview involves verifying certain control card items and reasking a subset of the questions on the original survey questionnaire. The original household respondent is the only person eligible to answer the household screen questions on reinterview. In addition, a respondent is randomly chosen from each selected household to answer individual screen questions for himself or herself only. Beginning in January 1977, the NCS reinterview included all persons originally interviewed in selected households. For any incidents reported in the screen questions, a crime incident report, which contains a subset of the items on the original, is completed.

At the conclusion of a reinterview, the reinterviewer uses a reconciliation form on which the original responses for all persons 12+ years of age who were interviewed on the original survey have been transcribed. The reinterviewer checks (1) that all incidents were reported in both interviews, and (2) that details reported in both interviews are the same. If an incident was not reported in both interviews the respondent is asked to explain why it was not reported and this is noted on the reconciliation form. From the explanations given by the respondents, the reinterviewer determines whether the crime incident was reported in error or omitted in error on the original or reinterview surveys. If a crime incident was reported on both interviews, the individual items on the crime incident report are compared and when differences are found, the reinterviewer questions the respondent to determine which response is correct. The correct response is then recorded on the reconciliation form. (All errors charged to the interviewer are summarized on the summary report of interviewer performance. Performance of individual interviewers is rated in the areas checked and errors discussed with the interviewers.)

Performance standards are specified in an effort to control the quality of work and thus the accuracy of survey results. Interviewers whose performance is below the specified standards are detected and corrective measures for improving performance are taken. Information obtained in the reinterview survey is regarded as being of higher quality than that obtained in the initial interview because it is obtained by a permanent staff of more experienced interviewers. Therefore, reinterview data is used to provide a standard for measuring the quality of coverage of persons and units and reliability of original survey data.

However, the reinterview technique applies the same methods as used originally in conducting the coverage and content checks and is thus subject to the weaknesses inherent in the methods. All of the deficiencies in the interview and coverage methods cannot be eliminated by repeating the process in intensified form. The reinterview errors in coverage and content are correlated with errors in the original survey, producing biased estimates of error in the direction of understatement.

Appendix B:

Response error

Sample surveys are subject to sampling errors and nonsampling errors. Nonsampling errors may be introduced either in the collection of data or in their processing. Nonsampling errors introduced during the course of data collection are referred to as response error.

Response error results in the assignment of a person to an incorrect category in a classification system. For example, if a person's response belongs to the category "car stolen," a response error results from the assignment of that person's response to the category "other motor vehicle stolen." Such errors affect the data in at least three ways: (1) the errors may create instability in the estimates derived from the survey; (2) the errors may introduce bias into the estimates; and (3) data relationships may become distorted.

Response errors may be due to the questionnaire design, the interviewing approach, the characteristics of the question, respondent attitudes, or other causes. These errors may be accidental, introduced purposely, arise from the lack of information, or be the result of other factors. A respondent may misunderstand a question and give an improper answer unintentionally, or may respond incorrectly because of a belief that certain answers are more acceptable, or for some other reason. Similarly, the interviewer may affect the accuracy of responses by the way the questions were asked, interpreted, and recorded.

The understanding and measurement of response errors are essential for three main purposes:

(1) To improve the assessments of the accuracy of data provided by a particular survey and to determine how much weight can be given those data in procedures for making conclusions, decisions, or actions in which the data are inputs;

(2) To determine how to improve the measurement techniques used in the survey and the accuracy of the estimates produced from the survey; and

(3) To contribute to the accumulation of information on response error for use in future surveys.

The meaning of response variation and response bias

Response error is composed of response variance and response bias. In reinterview each person or housing unit is viewed as having a population of responses to a specific question which can be generated by independent repetition of the same survey procedure under the same general conditions. The initial survey obtains one of these responses while the reinterview obtains a second; the two responses are assumed to have been selected randomly from the population of responses and are compared to produce estimates of the average trial-to-trial response variability commonly referred to as simple response variance. The total variance of responses for a population equals the average simple response variance for the persons in the population (i.e., the variance between trials for the same person) plus the sampling variance (i.e., the variance between persons).

The difference between the expected number of persons in a class based on initial survey responses and the "true" number of persons in that class based on reinterview responses after reconciliation is defined as response bias. The reinterview results after reconciliation are used to define a "true" value, even though these results may also vary over repeated trials. In practice, one cannot provide unbiased estimates of simple response variance and response bias in the original survey, but one can estimate them from a sample reinterview survey.

Measurements of response variation and bias

Matching information from the original with reinterview results and reconciled results for identical persons provides information necessary to compute indexes of response variance and bias. The following diagram is one way in which the results of the original and reinterview survey may be compared for a given category.

	Original survey		
Reinterview survey	In category	Not in category	Total
In category	a	b	$a + b$
Not in category	c	d	$c + d$
Total	$a + c$	$b + d$	$n = a + b + c + d$

In this table the cells denoted by a and d represent cases where the original and reinterview responses agreed and b and c cells represent cases where responses differed. The quality of data collected for a particular category of a classification system is reflected by the levels of gross and net variation associated with that category. The values of a, b, c, and d are the components of the indexes of gross and net error. A description of the indexes follows.

The index of inconsistent response

The total or gross response variation affecting the tabulated figure for any given category of an item is equal to the number of cases which moved into the category plus the number of cases which moved out of that category between the interview and reinterview. In terms of the diagram, the gross variation is equal to $b + c$. The values of a and d are not components of variance, since they represent cases where the original and reinterview responses agreed. If everyone reported identically in both the original and reinterview, then $b + c$ would be equal to zero. In this circumstance, there would be no variability in response; this leads intuitively to the fact that the simple response variance is zero.

It is difficult to compare raw measures of gross error because their magnitudes depend on the proportion of the population in the given category. An index less subject to this defect is preferable so that in interpreting the results one can compare across characteristics to determine which are most affected. The measure is called the index of inconsistent response and is defined in notation as $\hat{I}$. This index may be interpreted as that proportion of the total variance (including sample variance) accounted for by response variance. In the notation of the diagram we have defined the response variance as the total number of differences in response between the original and reinterview which is equal to $b + c$. There are several reasonable estimates of the total variance. However, we use as an estimate of the total variance an average value based on the two surveys.

The general form of the total variance is $1/n\,(p_1q_2 + p_2q_1)$.

In the notation of the diagram,

$p_1 = a + c =$ number of persons classified as having the characteristics in the original survey

$p_2 = a + b =$ number of persons classified as having the characteristic in the reinterview

$q_1 = b + d =$ number of persons classified as not having the characteristic in the original survey

$q_2 = c + d =$ number of persons classified as not having the characteristics in the reinterview.

Therefore, for this application the total variance can be represented as $1/n[(a+c)(c+d) + (a+b)(b+d)]$.

The purpose of pairing p_1 with q_2 is to prevent the erratic and meaningless fluctuations of the index of inconsistent response for some questions when the values of p_1 and p_2 may differ substantially.

As $\hat{I}$ becomes larger, a greater proportion of the total variance is due to variability in responses. This accounts for the name of the index, since as $\hat{I}$ increases, the item shows greater inconsistency in response.

The net difference rate

The net difference rate, on the other hand, for a particular category is the difference between the proportion of persons classified as having a specified characteristic in the original survey and the proportion classified in that category in reinterview. In terms of the diagram, the net difference rate is defined as $c - b$ divided by n. Therefore, when b and c are equal, regardless of the number in each cell, the response bias is zero. Of course when the number of persons in b and c is large relative to n and at the same time b and c are equal, the level of simple response variance will be high while the amount of response bias is still zero.

It should be pointed out that the estimates of response error derived from these measures tend to be understated. This is because the method of reinterview, in application, does not meet its theoretical objectives. One reason for this is conditioning of responses. Independent repetitions of the original survey procedures under the same general conditions would be required to make proper estimates of response error. This is not achievable, since the first response must be regarded as conditioning subsequent responses. If the respondent answered the question on the original interview, the answer in the reinterview may be made based upon the original reply rather than an independent attempt to answer the question.

Sampling variability

The measures of response error presented in this report are based on a sample and are, therefore, subject to sampling variability. A 95-percent confidence interval has been constructed and is shown in the tables for each of the estimated response-error measures. If all possible samples were selected, each of these surveyed under essentially the same general conditions, and an estimate and its estimated standard error were calculated from each sample, then approximately 95 percent of the intervals from two standard errors below the estimate to two standard errors above the estimate would include the average value of the estimates of all possible samples. The average value of all possible samples may or may not be contained in any particular computed intervals. But for a particular sample, one can say with specified confidence that the average of all possible samples is included in the constructed intervals. These confidence intervals have been estimated from the sample results and provide a rough approximation on the extent of sampling error associated with each estimate. Due to the assumptions made in estimating the sampling errors, these confidence intervals would be expected to understate the actual sampling variability for the estimated response errors.

In computing the indexes, cutoff points were established for which they are not computed. If the confidence interval is too wide, the information supplied is of marginal value. No index is computed if the total number of in-category determinations is less than 40. Note that in meeting the "40" criterion, a case put in-category in both trials would count as "2."

Comparison of victimizations as reported on the screen questions with their final classification: 1976*

by Richard W. Dodge

This report compares the screen questions that elicited reports of victimizations occurring in 1976 with the final classification of these victimizations based on data from the incident report. Similar reports were prepared earlier, covering incidents that took place in 1974 and 1975.

The main conclusion from the previous reports was that the screen questions that were designed to probe for particular crimes produced the bulk of the reports that were eventually classified into those same crime categories. The 1976 data confirm these results. The method for determining the correspondence between screen question and final crime classification was to designate "key" screening questions which could reasonably be considered as probing for a particular type of crime or crimes. In the case of questions dealing with theft, the two general "catchall" questions (numbers 45 and 46—see figure 1) were somewhat arbitrarily assigned to the two most common forms of theft, personal larceny without contact and household larceny. Table 61 shows that the bulk of reports for each crime came from the group of key questions, although the proportion was only slightly over half for robbery. On the other hand, one question produced 83 percent of the reported motor vehicle thefts, a different question stimulated 71 percent of personal larcenies with contact, and two questions evoked 79 percent of reported burglaries. Table 62

*U.S. Bureau of the Census memorandum, December 22, 1977.

62. National Crime Survey: Percent of major crimes elicited by key screening questions, 1974, 1975, and 1976

Type of crime	1974	1975	1976
Rape	75.8	77.0	83.3
Robbery	51.6	55.8	51.6
Assault	91.2	92.7	92.1
Personal larceny with contact	70.6	71.9	70.8
Personal larceny without contact	95.6*	93.8	93.6
Burglary	75.7	77.9	78.8
Household larceny	95.6*	93.5	93.4
Motor vehicle theft	86.6	85.4	82.6

* Personal larceny without contact and household larceny were not separated in the 1974 tables.

61. National crime survey: Percent of major crimes elicited by specific screen questions, 1976[1]

Question number	Total crimes	Rape	Assault	Personal larceny: With contact	Personal larceny: Without contact	Robbery	Burglary	Household larceny	Motor vehicle theft
29	10.5	5.6	0.7	0.5	0.1	2.4	57.3*	2.0	0.3
30	3.9	1.9	0.4	0.3	(Z)	0.1	21.5*	0.6	0.4
31	12.4	—	0.1	0.3	1.8	0.2	3.3	45.6*	1.4
32	1.8	—	—	0.5	2.9*	0.6	2.5	0.5	0.4
34	3.2	—	—	—	0.7	1.7	0.3	0.6	82.6*
35	15.5	0.9	(Z)	—	25.9*	0.9	0.8	19.8*	4.4
Total household screen questions	47.3	8.3	1.1	1.5	31.5	6.0	85.8	69.1	89.2
36	2.2	—	—	70.8*	2.3	11.2	0.1	0.2	—
37	1.0	1.9	0.7	2.8	0.1	30.1	(Z)	(Z)	0.1
38	1.0	3.7	2.1	5.4	0.3	21.4	0.1	—	—
39	3.5	14.8*	29.2*	—	(Z)	10.2	(Z)	—	—
40	0.9	1.9*	8.1*	—	(Z)	1.6	(Z)	—	—
41	5.0	22.2*	42.4*	0.3	0.1	9.4	(Z)	—	0.1
42	1.6	44.4*	12.3*	—	0.1	3.1	0.1	(Z)	—
43	8.7	—	(Z)	0.3	16.2*	0.7	0.4	8.4*	0.4
44	13.7	—	0.1	10.5	31.5*	1.9	2.1	1.6	0.7
45	10.8	—	(Z)	5.4	12.8*	1.0	8.5	16.3*	3.9
46	3.1	0.9	0.2	2.1	4.3*	1.6	1.9	3.4*	4.6
Total individual screen questions	51.5	89.8	95.3	97.4	67.7	92.4	13.1	29.9	9.8
47	0.6	0.9	2.5	0.5	0.3	1.2	0.5	0.4	0.2
48	0.5	0.9	1.0	0.8	0.3	0.5	0.4	0.4	0.4
NA	0.2	—	0.1	—	0.2	—	0.3	0.1	0.4
Percent from key questions	—	83.3	92.1	70.8	93.6	51.6	78.8	93.4	82.6

* Indicates key questions designed to elicit reports of the specific crime.
1 Data based on unweighted tallies.
(Z) Less than 0.05.

indicates that the proportion of crimes elicited from the key questions has been fairly stable over the 3-year period.

As in the past, the generally worded "catchall" questions made significant contributions in 1976 to the total for the various crimes. For example, questions 45 and 46, which probe for theft, provided 20 percent of all household larcenies and 17 percent of personal larcenies without contact. Even the general catchall questions, 47 and 48, although generating only about 1 percent of the total of all measured crimes, were the source of about 4 percent of the assaults reported in the survey. Whatever list of specific screening questions is contemplated for a revised questionnaire, the use of such general probes to give a respondent a further opportunity to recall a particular victimization experience should be given serious consideration.

Probably the most important question arising from these reports is whether the procedure of asking the battery of household screen questions (numbers 29 through 35) only once in each household is justified. This procedure was adopted on the assumption that any knowledgeable household member, 18 and over (usually the first household member interviewed), could provide information on the crimes that were essentially directed against the household as a group rather than against an individual member. In 1976, 13 percent of burglaries, 30 percent of household larcenies, and 10 percent of motor vehicle thefts were first reported on the individual screen questions. It is assumed that the household respondent would have recalled these crimes from the more explicit wording of questions 29–35, especially for burglary and motor vehicle theft, so that these household crimes were provided by household members who were only asked the individual screening questions. The question naturally arises as to the effect on recall if all respondents had been asked about objects taken from a garage or left in the yard, or about a part removed from a car. It is not clear that the "knowledgeable" respondent is completely informed about (or remembers) items illegally removed from an unlocked garage or taken from a family member on a business trip.

HOUSEHOLD SCREEN QUESTIONS

29. Now I'd like to ask some questions about crime. They refer only to the last 6 months – between _____ 1, 197__ and _____, 197__. During the last 6 months, did anyone break into or somehow illegally get into your (apartment/home), garage, or another building on your property? ☐ Yes – How many times? ☐ No

30. (Other than the incident(s) just mentioned) Did you find a door jammed, a lock forced, or any other signs of an ATTEMPTED break in? ☐ Yes – How many times? ☐ No

31. Was anything at all stolen that is kept outside your home, or happened to be left out, such as a bicycle, a garden hose, or lawn furniture? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No

32. Did anyone take something belonging to you or to any member of this household, from a place where you or they were temporarily staying, such as a friend's or relative's home, a hotel or motel, or a vacation home? ☐ Yes – How many times? ☐ No

33. What was the total number of motor vehicles (cars, trucks, etc.) owned by you or any other member of this household during the last 6 months? (932) ☐ None – *SKIP* to 36 ☐ 1 ☐ 2 ☐ 3 ☐ 4 or more

34. Did anyone steal, TRY to steal, or use (it/any of them) without permission? ☐ Yes – How many times? ☐ No

35. Did anyone steal or TRY to steal parts attached to (it/any of them), such as a battery, hubcaps, tape-deck, etc.? ☐ Yes – How many times? ☐ No

INDIVIDUAL SCREEN QUESTIONS

36. The following questions refer only to things that happened to YOU during the last 6 months – between _____ 1, 197__ and _____, 197__. Did you have your (pocket picked/purse snatched)? ☐ Yes – How many times? ☐ No

37. Did anyone take something (else) directly from you by using force, such as by a stickup, mugging or threat? ☐ Yes – How many times? ☐ No

38. Did anyone TRY to rob you by using force or threatening to harm you? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No

39. Did anyone beat you up, attack you or hit you with something, such as a rock or bottle? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No

40. Were you knifed, shot at, or attacked with some other weapon by anyone at all? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No

41. Did anyone THREATEN to beat you up or THREATEN you with a knife, gun, or some other weapon, NOT including telephone threats? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No

42. Did anyone TRY to attack you in some other way? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No

43. During the last 6 months, did anyone steal things that belonged to you from inside ANY car or truck, such as packages or clothing? ☐ Yes – How many times? ☐ No

44. Was anything stolen from you while you were away from home, for instance at work, in a theater or restaurant, or while traveling? ☐ Yes – How many times? ☐ No

45. (Other than any incidents you've already mentioned) was anything (else) at all stolen from you during the last 6 months? ☐ Yes – How many times? ☐ No

46. Did you find any evidence that someone ATTEMPTED to steal something that belonged to you? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No

47. Did you call the police during the last 6 months to report something that happened to YOU which you thought was a crime? (Do not count any calls made to the police concerning the incidents you have just told me about.) ☐ No – *SKIP* to 48 ☐ Yes – What happened? (936)

CHECK ITEM C: Look at 47. Was HH member 12+ attacked or threatened, or was something stolen or an attempt made to steal something that belonged to him? ☐ Yes – How many times? ☐ No

48. Did anything happen to YOU during the last 6 months which you thought was a crime, but did NOT report to the police? (other than any incidents already mentioned) ☐ No – *SKIP* to *Check Item E* ☐ Yes – What happened? (939)

CHECK ITEM D: Look at 48. Was HH member 12+ attacked or threatened, or was something stolen or an attempt made to steal something that belonged to him? ☐ Yes – How many times? ☐ No

CHECK ITEM E: Do any of the screen questions contain any entries for "How many times?" ☐ No – *Interview next HH member. End interview if last respondent, and fill Item 12 on cover page.* ☐ Yes – *Fill Crime Incident Reports.*

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Figure 1

The fact that an incident is ultimately classified as a "household" crime does not mean that it is so perceived by a household member or even that he or she is aware of it. The concept of the separate household screener deserves to be tested at the time that revision of the NCS questionnaire is undertaken.

Report on 12- and 13-year-old interviewing experiment*

by ANTHONY G. TURNER

One of the methodological issues raised in the early stages of the NCS was the problem of whether 12- and 13-year-old respondents should answer for themselves. Arguments against interviewing 12- and 13-year-olds directly were that children of those ages possibly were too young to give responsible and reliable answers; and that asking sensitive questions about victimization might be distasteful to the child or the parents, and thereby result in a loss of rapport. Arguments for interviewing the child directly were that the actual victim of a crime would have a better recollection of the incident, and children do not always tell their parents everything that happens to them.

The current procedure on NCS is to interview 12- and 13-year-olds by proxy (usually a parent), but an experiment was conducted in San Francisco during the 1974 cities sample interviewing in an effort to determine if victimization rates differed between the two procedures of self versus proxy respondent and, if they differed, where the major differences lay.

The design of the experiment was simple: the interviewers in San Francisco were instructed in half the sample units to interview 12- and 13-year-olds directly, and in the other half, to interview a proxy for them. Using this procedure, approximately 570 interviews were processed with 12- and 13-year-olds, 53.5 percent of which were self-response interviews.

The small sample size, the large design effect, and the low frequency of victimization make it impossible to reach any definite conclusions, but the data are very suggestive of possible effects that need to be researched in more depth.

The design effect (the percentage increase in the variance expected under simple random sampling) calculated for several crimes ranged from +21.7 percent to +32.4 percent. This estimated increase in the variance made it impossible to conduct any testing using the Chi-square distribution, reducing the analysis to a pairwise comparison of rates of victimization between the two interviewing procedures. Furthermore, only nine items had sufficiently large frequencies to be of interest. Table 63 presents personal victimizations for each category of crime by type of interview and table 64 presents personal victim-

*U.S. Bureau of Census memorandum, April 8, 1976.

63. Personal victimization by type of interview: Crimes against persons

	Frequencies			Percent victimization				
	Total	SR	Proxy	Total	SR	Proxy	Difference	Total of difference
Control totals	16,783	8,985	7,797	—	—	—	—	—
Total victims	2,425	1,384	1,041	14.449	15.403	13.351	+2.052	.62
Assaultive violence	1,554	932	622	9.259	10.373	7.977	+2.396	.87
With theft	212	124	88	1.263	1.380	1.129	•	—
Rape	0	0	0	0	0	0	0	0
Attempted rape	0	0	0	0	0	0	0	0
Serious assault	29	29	0	.173	.323	0	•	—
With weapon	29	29	0	.173	.323	0	•	—
No weapon	0	0	0	0	0	0	0	0
Minor assault	182	94	88	1.084	1.046	1.129	•	—
Without theft	1,342	808	534	7.996	8.993	6.849	+2.144	.84
Rape	0	0	0	0	0	0	0	0
Attempted rape	28	28	0	.167	.312	0	•	—
Serious assault	181	120	61	1.078	1.336	.782	•	—
With weapon	181	120	61	1.078	1.336	.782	•	—
No weapon	0	0	0	0	0	0	0	0
Attempted assault with weapon	119	62	57	.709	.690	.731	•	—
Minor assault	325	238	87	1.936	2.649	1.116	•	•
Attempted assault no weapon	688	360	329	4.099	4.007	4.220	−.213	−.112

* The absolute value of the difference is less than 1.00.

64. Personal victimizations reported to police— Base of rates = frequency of victimization

	Frequencies			Percent victimization				
	Total	SR	Proxy	Total	SR	Proxy	Difference	Total of difference
Control totals	16,783	8,985	7,797	—	—	—	—	—
Total victims	602	398	204	24.285	28.757	19.597	+9.16	.866
Assaultive violence	425	307	118	27.349	32.940	18.971	+13.97	1.06
With theft	154	94	60	•	•	•	•	•
Rape	0	0	0	—	—	—	—	—
Attempted rape	0	0	0	—	—	—	—	—
Serious assault	0	0	0	•	•	•	•	•
With weapon	0	0	0	•	•	•	•	•
No weapon	0	0	0	•	•	•	•	•
Minor assault	154	94	60	•	•	•	•	•
Without theft	271	213	58	•	•	•	•	•
Rape	0	0	0	—	—	—	—	—
Attempted rape	0	0	0	•	•	•	•	•
Serious assault	30	30	0	•	•	•	•	•
With weapon	30	30	0	•	•	•	•	•
No weapon	0	0	0	—	—	—	—	—
Attempted assault with weapon	0	0	0	—	—	—	—	—
Minor assault	63	63	0	•	•	•	•	•
Attempted assault no weapon	179	121	58	•	•	•	•	•

* Insufficient cases to estimate the statistic.

izations reported to the police for each category of crime by type of interview. The base of the rates of victimization in table 64 is equal to the frequency of victimization reported by type of crime in table 63. None of the differences reported in table 63 is greater than 2.5 percent, but it is interesting to note that for the most part, victimization rates for self-respondents are higher for types of crimes involving personal assault, whereas self-respondent rates are lower than proxy rates when assault is not involved. T-tests show that significance levels for all of the differences presented are greater than 0.1. The same observations may be made regarding table 64, where there are only two categories (total victimization and assaultive violence) with sufficient frequencies to be of interest.

To conclude, it is impossible to make any recommendations regarding the mode of interview for 12- and 13-year-olds because of the lack of power available in the experiment. The one interesting point in the results is the reversal between the two techniques in yielding higher reporting rates for assaults versus nonassaults. If it is feasible to repeat the experiment, it may be better to conduct the experiment on a much larger sample.

Effects of supplemental questions on victimization estimates*

by CHARLES D. COWAN, LINDA R. MURPHY, and JUDY WIENER

Introduction

From 1972 through 1975, the Bureau of the Census conducted a series of surveys, the National Crime Surveys, sponsored by the Law Enforcement Assistance Administration (LEAA), in 26 U.S. cities. In each of the NCS cities surveyed a sample of 12,000 housing units was selected. Within each household, self-response interviews were conducted with each household member 14 or older to determine the extent and nature of their personal crime victimization experience during the preceding 12-month period. Personal crimes covered by NCS include rape, assault, robbery, personal larceny with contact (pocket picking and purse snatching), and personal larceny without contact, as well as attempts at any of these. Proxy interviews were conducted for 12- and 13-year-old household members and also for older members under limited circumstances. A household respondent was similarly asked about household crime victimization experience in the previous year. Household crimes covered by NCS include burglary, household larceny, motor vehicle theft, and attempts at any of these.

In addition to the victimization questions, a supplemental series of attitude questions was asked of respondents age 16 or older (strictly by self-response) in a random subsample of one-half of the interviewed units—i.e., about 5,000 interviewed households and 10,000 interviewed persons in each city. For each respondent, the survey procedure called for administering the attitude supplement prior to the basic victimization questionnaire in a deliberate attempt to avoid unduly influencing attitude responses. This was considered necessary because the supplement dealt with respondent attitudes, opinions, and behavior patterns in regard to crime and fear of crime—e.g., why a respondent moved to a particular neighborhood; whether the neighborhood was safe during the day or night; whether crime was increasing or decreasing there; what the respondent thought were the chances of personally being attacked or robbed; and whether, in the respondent's opinion, the local police were doing a good job. It also dealt with more general questions, such as whether crime throughout the United States was increasing or decreasing, and whether newspapers and television accurately reported the seriousness of crime.

*Excerpted from a paper presented to the annual meeting of the American Statistical Association, Washington, D.C., 1979.

Study design

Data were taken from NCS cities surveys conducted in 1974, primarily for the purpose of broadening the base of the overall investigation with the addition of 13 more cities—Boston, Buffalo, Cincinnati, Houston, Miami, Milwaukee, Minneapolis, New Orleans, Oakland, Pittsburgh, San Diego, San Francisco, and Washington, D.C. As in all other NCS cities surveys, the sample size was 12,000 housing units, half of which were slated to receive the attitude supplement, again administered before the crime victimization questions.

A set of special tabulations were produced for each half-sample: the subsample receiving the attitude questions and that receiving just the basic victimization interview. For all cities involved, identical processing, weighting, and tabulation procedures were used. Standard errors were obtained by interpolation from those calculated by the Bureau of the Census and published in *Criminal Victimization Surveys in 13 American Cities* by LEAA.

In this second study we went beyond looking just at differences in total personal crime victimization rates and total property crime victimization rates. We examined differences by detailed type of crime, crime event characteristics (such as amount of loss, victim-offender relationship, whether reported to police, amount of medical expense), and victim/household characteristics.

Personal crimes of violence

The analysis indicates that, for all but two cities, significantly more personal crimes of violence (rape, robbery, assault, and attempts at these) were reported by persons in the supplement sample than by persons not in the supplement sample. In the other two cities, a higher victimization rate was also reported, even though the difference was not significant (see table 65). We can conclude that inclusion of the attitude supplement had an overall effect on survey results for personal crimes of violence.

Detailed type of crime

Looking at more detailed types of crime, rape and robbery show no detectable difference between the two rates using the t-test, while assault rates were significantly higher for persons receiving the supplement. However, 11 of the 13 differences for robbery are positive, so the sign test would indicate that perhaps robbery also was affected by administration of the supplement. Following this line of reasoning, the difference within assault is apparently due to simple assault rather than aggravated assault. A paired t-test does not allow rejection of the hypothesis that simple assault rate differences are equal to aggravated assault rate differences, but because 11 of 13 of the simple assault rate differences are larger than the aggravated assault rate differences, the sign test suggests that the supplement had a greater effect on simple assaults than aggravated assaults.

Similarly, looking at the differences for attempted assaults, differences in rates for attempted assaults without a weapon were larger than differences for attempted assaults with a weapon in 10 of 13 cities, although the paired t-test again does not prove to be significant. This same type of analysis is not as informative for robbery. Although a case can be made, using the sign test, that the supplement led to higher reporting of robberies, no claims can be made about detailed categories of robbery.

Crime event characteristics

One reason to look at crime event characteristics is to try to attach some measure of saliency or relative importance to the crime, since such factors may well be related to differential recall and reporting. The first variable to investigate is the victim-offender relationship. Examining crimes committed by a stranger, respondents in the supplement reported 7.72 more crimes per 1,000 persons than persons not in the supplement, a significant difference. For crimes committed by an offender who was not a stranger, 11 of 13 city differences were positive, indicating by the sign test that the supplement may have led to increased reporting of crimes committed by nonstrangers. The t-test did not prove to be significant here, nor was it significant when the differences between rates for crimes committed by strangers were compared with rate differences of nonstranger

65. Summary of rates for in and out of supplement samples

(Rates averaged across 13 cities. Rates per 1,000 persons)

Type of crime	Victimization rate		Standard errors		Z of difference	Number of positive differences +
	In	Out	In	Out		
Crimes of violence	59.01	48.27	2.49	2.26	3.194 **	13
Rape and attempted rape	2.33	2.04	.57	.50	.382	8
Robbery	19.90	16.23	1.47	1.32	1.858	11
Robbery/attempted robbery with injury	5.97	5.01	.83	.75	.858	10
Serious assault	3.26	2.68	.64	.57	.677	12
Minor assault	2.71	2.77	.58	.59	−.073	7
Robbery without injury	7.67	7.36	.92	.90	.241	7
Attempted robbery without injury	6.26	4.89	.84	.75	1.217	10
Assault	36.77	28.87	1.98	1.76	2.982**	13
Aggravated assault	16.13	13.16	1.31	1.18	1.685	13
With injury	4.90	4.62	.74	.73	.239	8
Attempted assault with weapon	11.23	8.53	1.10	.98	1.833**	13
Simple assault	20.65	15.71	1.50	1.31	2.481*	13
With injury	4.71	4.53	.74	.73	.173	7
Attempted assault without weapon	15.94	11.18	1.32	1.11	2.760	13
Crimes of theft	112.70	97.56	3.30	3.10	3.344	13
Personal larceny with contact	10.86	10.36	1.11	1.08	.323	8
Purse snatching	3.02	2.89	.63	.62	.147	6
Attempted purse snatching	1.58	1.57	.42	.43	.017	7
Pocket picking	6.26	5.91	.85	.85	.291	8
Personal larceny without contact	101.84	87.19	3.16	2.96	3.384	13
Household crimes	504.49	447.68	7.83	7.77	5.150**	13
Burglary	136.46	124.85	5.51	5.33	1.514	13
Forcible entry	53.00	48.67	3.68	3.53	.849	12
Nothing taken	9.05	9.05	1.57	1.57	.000	6
Property damage	6.82	7.19	1.37	1.40	−.189	7
No property damage	2.23	1.86	.78	.70	.353	8
Something taken	43.94	39.62	3.38	3.22	.925	12
Unlawful entry without force	46.11	44.55	3.48	3.43	.319	9
Attempted forcible entry	37.36	31.63	3.13	2.90	1.345	12
Larceny	332.05	289.98	7.47	7.23	4.047	13
Under $50	194.22	167.46	6.33	5.98	3.073	13
$50 or more	99.18	91.13	4.81	4.69	1.196	10
NA amount	9.32	8.32	1.62	1.49	.455	9
Attempted larceny	29.33	23.08	2.80	2.54	1.653	11
Auto theft	35.98	32.84	3.06	2.93	.741	10
Theft of car	21.46	20.98	2.39	2.34	.149	7
Theft of other vehicle	2.33	1.77	.82	.69	.523	7
Attempted theft of car	11.71	9.66	1.84	1.73	.812	12
Attempted theft of other vehicle	.48	.46	.36	.35	.040	10

* Significant at the .05 level.
** Significant at the .01 level.
\+ Probabilities of positive differences for the sign test:

	p
All 13 differences positive	.0001
12 of 13 differences positive	.0016
11 of 13 differences positive	.0095
10 of 13 differences positive	.0349
9 of 13 differences positive	.0873
8 of 13 differences positive	.1571
7 of 13 differences positive	.2095

crimes. However, for the latter comparison, 11 of 13 cities did have positive differences of the differences, again indicating (by means of the sign test) that the supplement may have been a greater stimulus to the reporting of crimes committed by strangers than by nonstrangers.

In comparing crimes which were not reported to police with those which were, victimization rates for crimes not reported to police were higher for supplement respondents in 12 of 13 cities. Conversely, victimization rates for crimes reported to police were lower for supplement respondents in 11 of 13 cities. The change in direction leads to a different test to determine if the change is significant. A two-way classification can be established:

Crimes not reported to police	Crimes reported to police		
	# differences		
# differences	Positive	Negative	Total
Positive	1	11	12
Negative	1	0	1
Total	2	11	13

This cross classification can be tested using the McNemar Test for Significance of Changes. Using the table of binomial probabilities, one can reject the hypothesis that there is no change in the reporting behavior dependent on whether the crime was reported to police at the .0224 level of significance. As this reversal is significant, it is interesting to note that administration of the supplement had the reverse effect from that previously noted for crimes reported to police. It appears that the supplement actually depresses reporting of crimes that have previously been reported to police, whereas it stimulates reporting of crimes not previously reported to police. This result will be referred to later in the discussion of the saliency of some crimes. Finally, the differences between the supplement and nonsupplement samples were examined by month of occurrence. Although some differences were significantly different for individual cities, none of the summary statistics for any month were significantly different using the t-test. Using the sign test, however, all months except May, August, and September showed significantly higher reporting for the supplement half sample.

Victim characteristics

Differences within selected victim characteristics were examined to determine whether any particular subgroups may have been disproportionately affected by the administration of the attitude supplement. Examining differences first by age, in the 13 cities combined, persons in the 16- to 19-year old-age group who were in the supplement sample reported an average of 37.93 more personal crimes of violence per 1,000 persons than the 16- to 19-year-old persons not receiving the supplement. The sign test indicates the same thing, with positive differences in all 13 cities, and indicates similar results for the 20- to 24-year-old age group and the 25- to 34-year-old age category, with 12 and 13 cities, respectively, showing positive differences. The remaining age groups show mixed results, with no indication of half-sample differences for the 35- to 49-year-old age group, 11 of 13 cities showing positive differences in the 50- to 64-year-old group, and no indication of a difference in the 65-or-over age group. A regression run fitting the ratio of the in-supplement to out-of-supplement rates to the midpoints of the age categories shows a decline in the relative difference between the samples as age increases. The regression equation is:

$$\frac{\text{Violence Victimization Rate—In}}{\text{Violence Victimization Rate—Out}} = 1.4536 \underset{(.0757)}{-} .0049 \underset{(.0017)}{\text{Age}}$$

$n = 78$ (6 age categories × 13 cities)
$R^2 = .0955$

The t-ratio, $-.0049/.0017 = -2.8824$, is significant for the negative slope, indicating a decline in the relative difference.

Personal crimes of theft

In 11 cities, the reported victimization rates for personal crimes of theft (pocket picking, purse snatching, personal larceny without contact, and attempts at these) are significantly higher for persons receiving the supplement than for persons not receiving the supplement. In the other two cities, a higher victimization rate was reported, but the difference was not significant (see table 65). We can conclude that just as for personal crimes of violence, the inclusion of the attitude supplement had an effect on survey results for personal crimes of theft.

Detailed type of crime

Of the detailed types of crime, only personal larceny without contact, which averages over all cities a higher rate of 14.64 crimes for every 1,000 persons, showed significant differences. In contrast, crimes of personal larceny with contact (pocket picking and purse snatching combined) resulted in only slightly higher victimization rates across all cities, which also was not significant. Examination of the particular categories, purse snatching, attempted purse snatching, and pocket picking, show basically the same results as for total personal larceny with contact. Furthermore, the sign test gives no indication of any difference between the supplement and nonsupplement samples for any categories of crime other than personal larceny without contact, mentioned above.

Crime event characteristics

The analysis of personal crimes of theft is analagous to that of crimes of violence in the preceding section. Crimes were first examined by whether they were reported to the police. Personal crimes of theft which were not reported to the police were recalled in the interview at a slightly higher rate by persons who were given the supplement, while supplement respondents recalled fewer crimes of theft which were reported to police. In the former case, 10 of 13 cities had higher supplement rates for crimes not reported to police, significant at the .0922 confidence level (of marginal acceptance under Census Bureau norms). In the latter case, 9 of 13 cities had lower supplement rates for crimes reported to police. Again (as in the section on crimes of violence), a two way classification can be established.

Crimes reported to police	Crimes not reported to police		
	# differences		
# differences	Positive	Negative	Total
Positive	1	3	4
Negative	9	0	9
Total	10	3	13

In this case, however, the results are not according to McNemar's Test for Significance of Changes. The hypothesis being tested is that the increased reporting of crimes that were not reported to police by respondents in the supplement sample suffers a reversal for crimes that were reported to police. The nine cities where this is true are apparently counterbalanced by Milwaukee, Pittsburgh, and San Diego, where the opposite is true, namely that crimes of theft not reported to the police are underreported on the survey relative to those not in the supplement whereas crimes of theft reported to the police are reported more frequently in the supplement sample.

Looking at total loss, for each category of dollar loss due to theft and/or damage loss, there was no detectable difference between respondents receiving the supplement and those not receiving the supplement. Finally, looking at month of occurrence, the sign test indicates that except for October, each month the persons receiving the supplement reported higher victimization rates. The t-test for all cities combined, however, does not show any significant differences, as the differences, though mostly positive, are not of sufficient magnitude to satisfy the stricter assumptions of the t-test. It should be noted that although there is a pattern in reporting of crimes by month of occurrence, this pattern is consistent for both in- and out-of-supplement samples, and so no trend is observed in the differences between the two samples. This was also true of personal crimes of violence.

Victim characteristics

For race, in all cities whites who were given the supplement reported a greater personal theft rate than whites who did not receive the supplement. The difference was significant in 9 of the 13 cities, as was the weighted average of all the cities (15.80 crimes per 1,000 persons). Although blacks averaged differences of 9.44 more crimes per 1,000 persons respectively, the differences were only significant in one or two cities, and in some cities persons not receiving the supplement reported slightly greater rates. Further, a t-test of the difference between the differences for whites and blacks was not

significant, indicating that there is no reason to suspect that the supplement affected either group to a greater extent.

By age, persons under 65 years of age receiving the supplement reported a slightly higher personal theft rate than those not receiving the supplement using the sign test, while persons above 65 showed no detectable difference as only 6 of the 13 cities showed positive differences. Combining all cities, the difference between rates decreased with age. Unlike the previous section where the gap between samples was demonstrated to be decreasing, significance tests on the regression coefficients for this regression cannot reject the hypothesis that there is no interaction of age with the supplement.

Household crimes

The t-test shows that the difference in victimization rates for total household crimes of burglary, larceny, motor vehicle theft, and attempts at any of these, is statistically significant at the 99-percent confidence level for 12 of the 13 cities, and at the 90-percent level for the other city (see table 65). In each of the cities a higher victimization rate was reported for households responding to the supplement than for those not responding. Therefore, we can conclude that inclusion of the supplement increased reporting of household crimes in addition to increases reported earlier for other types of crime.

Detailed type of crime

Looking at the more detailed types of household crimes, however, one finds that only for larceny, completed and attempted, is the t-statistic above the critical value for all cities. Attempted larceny was reported at an average rate difference across all cities of 6.25, significantly lower than the average rate difference of 34.13 per 1,000 households for completed larceny. Similarly, on the average, no detectable difference in victimization rates was found for burglaries where nothing was taken, as compared to an average of 4.32 more burglaries per 1,000 households where something was taken. In the latter case, the reporting of burglaries does seem to have been increased by the administration of the supplement, as differences are positive for 12 of the 13 cities (although the 4.32 difference itself is not significant using the t-test). For forcible entry with nothing taken, only 6 of 13 cities hold a positive difference, which is not significant by the sign test, nor are the differences of the differences between something and nothing taken.

Crime event characteristics

As in the previous two sections covering personal crimes, the sign test indicates that the victimization rate for household crimes not reported to the police was greater for households in the supplement sample than for households which were not. The sign test also shows for household crimes that were reported to police, households receiving the supplement had lower household crime victimization rates than those that did not receive the supplement. A two-way classification will again be used to test for an interaction.

Crimes reported to police	Crimes not reported to police		
	# differences		
# differences	Positive	Negative	Total
Positive	1	2	3
Negative	9	1	10
Total	10	3	13

Again McNemar's Test rejects the hypothesis that no interaction exists between the administration of the supplement and whether the crime was reported to the police.

Considering total loss due to theft and/or damages as another indicator of saliency, a somewhat unusual finding emerges. For household crimes with a total loss of less than $50, households receiving the attitude supplement reported slightly higher victimization rates (1.24 victimizations per 1,000 households) than those not in the supplement sample. As 10 out of 13 cities had positive differences for less-than-$50 loss, this result is marginally significant by the sign test. For losses of $50 or more, the reverse is true with a net difference of −1.47 victimizations per 1,000 households, and 10 of 13 cities with negative differences, yielding lower victimization reporting for the supplement sample. A t-test of the difference of the differences between the two half-samples [1.24 −(−1.47) = 2.71] is not significant, but McNemar's Test for the Significance of Changes indicates a reversal took place, significant at the .0654 confidence level.

Crimes with loss ≥$50	Crimes with loss <$50		
	# differences		
# differences	Positive	Negative	Total
Positive	1	2	3
Negative	9	1	10
Total	10	3	13

Again, there seems to be an interaction between administration of the supplement and amount of loss incurred.

Like the sections on personal theft and personal crimes of violence, the differences in rates of reporting household crime varied by month in which the crime was reported to have occurred. For the average of the 13 cities, the total household victimization rate was significantly higher for people receiving the supplement for the months of July and November. Other months all showed a higher household victimization rate for households in the supplement sample, and the average differences across all cities were positive, though not significantly different, but the sign test indicates that for all months a difference existed, with respondents to the supplement reporting higher victimization rates.

Household characteristics

Results for race of head for household crimes differ from results for personal crimes of violence. For the personal crimes, only whites showed a significant difference in reporting, and there was no detectable difference in the test between whites and blacks to see if they reacted differently to the supplement. For household crimes both whites and blacks show significant differences in the response to the supplement. For both groups, higher numbers of victimizations were reported for the supplement half-sample. The difference of the differences is not significant, however, so there is no reason to suspect that blacks and whites reacted differently to the supplement.

Conclusion

The original study cited earlier suggested three possible explanations for the observed differences between subsamples in victimization recall or reporting to survey interviewers:

1. Improved respondent recall and reporting of events occurring within the reference period due to memory stimulation.

2. Increased respondent desire to be accommodating, resulting in a greater amount of exaggeration or fabrication.

3. Increased telescoping of events actually occurring prior to the beginning of the reference period because of memory stimulation and/or accommodation.

Several of the findings of this more detailed study tend to indicate that the effect of administering the attitude supplement is to increase respondent reporting of less serious victimizations—e.g., simple assault, those committed by strangers, those not reported to police, loss under $50. One view of this result is that one would expect, if the effect were due to exaggeration or fabrication (the second hypothesis), that respondents would have chosen more important events with which to be accommodating, thus refuting hypothesis number two. Another view is that respondents would only exaggerate less serious crimes, as more serious crimes may get the respondent too deeply involved in the interview, and the respondent would prefer to avoid getting into anything "heavy." This latter explanation would tend to support hypothesis number two. Probably the only way to actually test this hypothesis is to conduct a reverse record check with a subsample of a broad spectrum of crimes. So no determination is made regarding hypothesis number two, though the predominant view is that victimization surveys suffer more from underreporting than overreporting by respondents.

Our findings do not support the third hypothesis. Higher victimization rates for the supplement sample would have been expected in the earlier part of the reference period, had telescoping from without been the explanation. But our findings (based mainly on the sign test) indicate higher rates throughout the year, with no discernible pattern. There are indicators based on an analysis of the NCS-National Sample that unbounded data are subject to forward external telescoping, which causes disproportionately higher victimization reports in the early months of the reference period. A regression of the relative difference between total victimizations from the returning and incoming rotations in the NCS-National Sample by month of report in the reference period shows that the relative difference goes from negative to positive, increasing linearly as the reported month of occurrence gets closer in time to the month of interview.[1] The regression equation is:

$$100 \times \frac{\text{Returning rotations}}{\text{Incoming rotations}} = -.39 + .09\,(\text{Month})$$
$$(.04)\quad(.009)$$
$$n = 8 \quad R^2 = .965$$

Because of this relationship in the NCS-National Sample, and because no such relationship exists in the analysis of the cities sample data, we conclude that telescoping is not a predominant factor.

Assuming, though, that we are correct in our conclusion of better reporting of crimes occurring within the reference period, we feel it may be an oversimplification to attribute the effect to just "memory stimulation." A good deal of the evidence from this examination indicates that the increased incident reporting is for those kinds of events that are more common—e.g., simple assault, attempted assault without a weapon, personal larceny without contact, household larceny (particularly completed household larceny and those with loss under $50), burglary involving actual theft—and by those population subgroups that generally have higher victimization rates—younger persons, white persons, and males. These results could be due to simple memory stimulation—i.e., those with a larger pool of events from which to recall more when their memories are stimulated by a device such as the attitude interview. However, it may also be the case that some types of respondents may be more productive when stimulated than others. A multivariate look at the data would develop a model which could simultaneously examine differences in rates by crime characteristics and by demographic characteristics. Such a model may allow us to disentangle productivity from simple recall.

[1] It should be noted that in the NCS-National Sample, returning rotations are bounded by earlier interviews and incoming rotations are unbounded, being first time interviews, and so are used only for bounding purposes.

Examination of incident reports involving personal larceny without contact*

by Richard W. Dodge

This report is an outcome of tabulating incident and victimization data from the National Sample using the revised crime stub. One of the major changes in this stub is that the household crime of larceny away from home is retabulated as the personal crime of larceny without contact.

Weighted totals for personal crimes in the incident tables are lower than those in the victimization tables for two reasons. One is because incidents in the household survey which involve both a personal victimization and a crime against a commercial establishment are not counted as personal incidents because they are considered to be represented in the independent commercial victimization survey sample. To count them as incidents in both surveys would be double counting, so the incidents are eliminated from the household survey, but the details of the personal victimization are included. The other difference in incident and victimization totals involves situations where more than one person is victimized in the same incident. The details of each victimization are recorded for all victims in the sample, but the incident is counted only once. This is accomplished by reducing the weight of the incident by the reciprocal of the total number of victims.

In examining the totals for incidents and victimizations for personal larceny without contact, the difference seemed larger than one might expect. Under what circumstances are several persons victimized in the same crime when there is no direct contact between victim and offender? What kinds of incidents involved a break-in or a loss to a commercial firm and a simultaneous loss to an individual, again in his absence? To gain some insight into this matter, a listing was prepared for all personal larcenies without contact for one data quarter (Quarter 1 of 1975). Cases involving both multiple victimization and a commercial incident were identified separately. Questionnaires were pulled for a 10-percent sample of all personal larcenies without contact, with all cases of multiple victimization and/or commercial theft being selected with certainty. There were 67 cases where a personal crime occurred in conjunction with a commercial incident and 10 involving multiple victimizations, but none involving both. This report presents the results of a detailed examination of these questionnaires. The availability of the documents also provided an opportunity to look at 26 incidents that are classified as attempted larcenies. Since by definition there was no direct contact between victim and offender in these situations, there has to be evidence that an attempt was made to steal something. An examination of the questionnaires would provide examples of the nature of such evidence.

The key element in this analysis was the interviewer's written summary of the incident which frequently provided information beyond that recorded in responses to the specific questions. Many of these summaries were very helpful in gaining insight into the circumstances surrounding the crime; others were so brief that they added nothing to what was known from the questionnaire itself. Although these summaries are used in the clerical edit of the documents, my impression is that not many changes are made in specific questions because of them. Their primary value appears to be in providing insights as to how well the questionnaire is eliciting the details of incidents so that they may be properly classified, and in identifying problem areas in the questionnaire itself or in its administration by interviewers.

An examination of the 67 cases classified as personal larceny without contact and involving a commercial incident suggests that there may be a misunderstanding on the part of some interviewers in marking item 5b on the incident report. This item reads, "Did the person steal or try to steal anything belonging to the store, restaurant, office, factory, etc.?" If the response is "yes," the incident is not counted as an incident in the household survey; if the answer is "no" or "don't know" it is counted. The only direct evidence to confirm or refute the response in 5b is the interviewer's summary. In 17 of the 67 cases examined, the written description explicitly indicated that the incident involved both commercial and personal property, e.g., a loss of a personal calculator during a burglary of a law firm, a respondent's precision tools taken during the burglary of the business where he worked, etc. In 22 incidents, commercial burglary or robbery was not mentioned in the summary, but the circumstances were such that a simultaneous commercial crime was possible. All but 2 of these incidents occurred at the respondent's place of work and 12 happened during the day when access to the premises was relatively open. Even if one assumes that in all these incidents, something was taken (or attempted to be taken) that belonged to the business, there were another 21 situations where it seems on the whole unlikely that a commercial incident involving burglary or robbery also occurred. These were situations where the location of the incident was a place of general public access. In these circumstances the more likely of the commercial crimes measured by the survey would be robbery, but the description of the event made no mention of a simultaneous crime directed against the establishment. In most of these 21 cases, the combination of commercial robbery and the reported personal crime seems highly improbable. Examples include theft of a jacket from a hospital waiting room, loss of a case of liquor at an airport, a book stolen from a seat on an airplane, shoes taken from the entrance to a bowling alley, and rings taken from a bench at the YWCA.

If a commercial crime occurred at all, it most probably would have been larceny, which is not measured in the commercial victimization survey. If this was the crime referred to by respondents in answer to question 5b, then the entire incident would not be represented in either survey. The other possibility is that some respondents do not listen carefully to 5b and miss the qualifying phrase "anything belonging to the store, restaurant, office, factory, etc." Whatever the reason, it is apparent that some incidents are being incorrectly eliminated from the household survey.

Most of the remaining cases dealt with special situations which were classified correctly under the system now in use, but which raise questions that need to be considered in any revision of procedures and concepts. The first concerns unrecognizable businesses which are, by their nature, not covered in the Commercial Victimization Survey. At present, although no separate counts of unrecognizable business crimes are obtained, incident reports are filled and the crimes are classified according to the current criteria. In the majority of cases, since unrecognizable businesses operate out of the respondent's home, the skip pattern avoids the questions that determine whether a commercial incident also was involved. However, there were three cases involving personal larceny without contact where the theft from the unrecognizable business occurred away from the respondent's home inside a commercial building or office. Question 5b was marked "yes," indicating that the incident also involved the theft of items belonging to a business—either the respondent's own or the place where the articles from the respondent's business were temporarily located. Two cases involved persons exhibiting items from their own businesses—in one case, antiques, in the other, paintings—in a public hall. It seems unlikely that a theft occurred simultaneously against the places where the exhibitions were being held, so that the respondents were probably interpreting question 5b as referring to their businesses. In the other case, a person who ran a janitorial service out of his home lost a dewaxing machine which was taken from the "office" of the business, which was either in the respondent's home or in another building on his property. However, he apparently considered the office as a separate entity, so the location of the crime was marked "office" in question 4 and question 5b was marked "yes." In all three of these situations, the incidents were eliminated from the household survey, and, as unrecognizable businesses, they were not represented in the commercial survey.

There is another set of circumstances—of which two cases were discovered in this investigation—where an incident is eliminated from the household survey and is also not counted in the commercial survey. This involves incidents occurring in places that are out-of-scope for CVS—in the two cases observed, in a municipal firehouse and a state highway department garage. In both situations, the respondents lost personal possessions, although it was not clear whether something belonging to the fire department was also stolen. However, to the extent that simultaneous thefts occurred, the incidents would not be represented in either survey.

Another case involved the stealing of a company-owned tool which the respondent had to pay for since he was responsible for the tools and the company had no theft insurance. The specific question on the screen, number 44, does not have the phrase "that belonged to you," but the appropriate question on the incident report, 13a, does. The interviewer's manual does not explicitly cover this situation, but it does stress that items belonging to a recognizable business should not be included on the incident report.

The final special instance illustrates a loophole in the present classification criteria which results in some personal larcenies with contact being classified as larcenies without contact. This occurs when an attempt is made to take a wallet or money in a person's immediate possession. The classification scheme only allows for an attempt at taking a purse, so that identical situations involving wallets, for example, end up as personal larceny without contact. In the case examined, a man tried to take a wallet from a women's purse, but she retaliated by hitting him and he fled.[1] If he had tried to take the entire purse, the incident would have been personal larceny with contact. This differential treatment does not apply to completed thefts of these objects; they are all personal larcenies with contact—as long as no force or threat of force is involved.

The evidence from this examination of personal larceny without contact suggests that some interviewers do not understand the necessary circumstances that must obtain for this crime to occur in conjunction with a commercial crime.[2] (Presumably, this confusion may occur with other crimes measured in the household survey, although with direct contact crimes, the fact of the victim's being present may make the situation clearer.) While it is not possible to eliminate all duplication of incidents between the household and commercial surveys, more could probably be done to make clear that the double counting we wish to avoid is that concerning commercial burglary and robbery. Even in these two crimes there may be borderline cases where it is not clear whether both a personal and a commercial crime occurred.

As mentioned earlier, there were 10 cases classified as personal larceny without contact where other persons were also victimized. From an examination of the questionnaires, five of the incidents were correctly classified, three probably should be personal larceny with contact (although only one of these could be traced to interviewer error), and two were borderline cases.

The legitimate cases involved guests losing valuables at a party or in some place of public accommodation, such as a sports arena or a bar. In two other cases, the classification was correct in terms of the criteria used to classify crimes, but the facts indicated personal larceny with contact. In one, a box of candy was snatched out of a child's hands and in the other both cash and credit cards were taken from a man's pocket. Since neither "purse" nor "wallet" was checked in 13f, the crimes ended up as larcenies without contact. (If cash only had been stolen in the latter case, the skip pattern would have directed the interviewer to 14c where it would have been determined that the cash was on his person and the combination of cash and another item stolen skips over this item.) In the other case, a boy's wallet was stolen from him on a school bus, but tossed back to him when the offenders discovered that there was no money in it; however, the interviewer did not mark the wallet as stolen. In the same incident a bus card was taken from the boy's pocket; but, since it was not a wallet, purse, or cash, the incident was classified as larceny without contact. One borderline case involved an interpretation of the phrase "on your person." A wallet was taken from a shoulder purse which the victim was wearing during an evening at a bar. However, the wallet was not marked as being on the victim's person. In the other case, it is questionable whether a crime occurred, but if it did, it should have been personal larceny with contact. A gang of boys tried to take money from the respondent and two of his friends as they were lying on the school lawn. No threat was indicated in question 7c and the gang left when the potential victims refused to give them money. The summary implied that there was a verbal demand involving no physical contact or explicit threat. However, since money, and not a purse, was involved, the attempt became larceny without contact. An explicit threat would result in the incident being classified as an attempted robbery. This set of circumstances raises the question of whether a verbal demand with perhaps an implied threat as perceived by the respondent constitutes a classifiable crime.

These cases also illustrate a problem in the consistency edit when the respondent does not know whether any other persons (or how many) were robbed at the same time because of the nature of the theft. In the

*U.S. Bureau of the Census memorandum, June 22, 1976.

[1]This incident occurred in a store and was also recorded as a commercial incident.

[2]There was also some evidence that the concept was not entirely understood by those responsible for the clerical edit. There were three cases where the clerical edit changed a "no" in question 5b to "yes" on the basis of no discernible facts or evidence anywhere on the questionnaire.

case of the man who lost money and credit cards, presumably to a pickpocket, he could not answer questions 12a (Were you the only person there besides the offender?) and 12b (How many of these persons, not counting yourself, were robbed, harmed, or threatened?) so the interviewer wrote in "don't know" for both questions. The clerical edit requires that these entries be lined out. The respondent, since he was traveling in a city away from home, was able to state definitely that no other household members were involved (question 12c). The consistency edit requires that if 12c is "No" and 12b is blank, that a "1" be entered in 12b. By this means, the incident became one with multiple victims, which would seem to be stretching the probable facts in the matter.

Attempted larcenies without contact are a very small proportion of all larcenies, because the evidence of an attempt is not always readily apparent. There were 26 cases on the listing which were classified as attempted larcenies without contact. In 19 of these, there was evidence that was persuasive to the respondents that an attempt had been made to steal something. In 16 cases, a motor vehicle was involved and the evidence—a raised hood, glove compartment ransacked, battery cables disconnected—led the respondent to assume that the intent was to steal a part of the car, rather than the car itself—and that vandalism was not the motive. Two other cases involved tampering with school lockers and in one instance the contents of a suitcase had been disarranged but nothing was missing. However, in five other cases, the facts, supplemented by the interviewer's summary, would suggest that the incident should have been personal larceny with contact. Two of these have already been discussed in connection with commercial incidents and those involving more than one victim. The other three involved attempts to take money or a wallet from the person which, under the present system, are larcenies without contact. Another case involved a boy who resisted a schoolmate's attempt to steal his watch. The summary mentioned that he was threatened, but this was not marked in 7c; since there was no threat, the crime became larceny without contact, rather than larceny with contact or attempted robbery. The final case involved a girl whose rings were stolen by an acquaintance before school and were returned to the victim when another girl forced the offender to do so. Presumably, this was a completed rather than an attempted larceny. This raises the question of how long the offender must possess an article before the theft is considered completed. In an example cited earlier, a wallet was taken on a school bus and returned when it was found to contain no money. The interviewer did not consider this a theft. In one of the attempted larcenies, a schoolmate grabbed another's wallet and the victim immediately grabbed it back; since it was considered an attempt and the object was not a purse, it was classed as a noncontact larceny. If it had been considered a completed theft, even though immediately returned, it would have been larceny with contact.

The foregoing analysis of personal larceny without contact cases has pointed out some areas where possible problems exist in interpreting concepts and in the system for classifying crimes. Revision of interviewer instructions, edit procedures, and the classification scheme would help to overcome some of these problems; others may have to await revisions in the questionnaire. Necessarily, such a report emphasizes shortcomings in data collection and processing. It should be kept in mind that only about 2 percent of the personal larcenies without contact recorded in the first data quarter of 1975 involved a simultaneous commercial incident. Nonetheless, such an examination provides a way of monitoring the entire program to spot potential difficulties that could not be caught in any other way. I would recommend that a regular professional review of a sample of questionnaires be instituted as a quality control measure and that it cover all the crimes included in the NCS.

National Crime Survey household records without neighborhood characteristics: Collection quarters 1-4, 1976*

by Carol Spivey

When the NCS Complete File was matched to the Neighborhood Characteristics File, 11.7 percent of the NCS records for the fourth quarter of 1976 had no matching neighborhood data. Figures for this and previous quarters are shown in table 66.

Two factors account for nearly all missing neighborhood data—new construction segments and combined ("X"-suffixed) Enumeration Districts (EDs). The bulk of units without neighborhood characteristics are in areas newly constructed since the 1970 Decennial Census. (Neighborhood characteristics were developed from the decennial census.) As can be expected, the further away in time from 1970, the larger the proportion of new construction in the sample. For example, new construction in the third quarter of 1972 represented 4.6 percent of the NCS file, in the fourth quarter of 1974 it was 9.1 percent, and by the fourth quarter of 1976 it was 10.8.

A relatively constant proportion of the file, slightly less than 1.0 percent, have no neighborhood data because of an "X"-suffixed ED number. An ED has about 700 housing units or about 2,000 people. When the decennial census counts for an ED indicated that it was larger, that ED was partitioned. Each part of the ED received the same number but a different alphabetic suffix, starting with a B. Later when sampling for the NCS, two or more of these suffixes were sometimes combined to make the ED mappable. Such combinations were given an "X"-suffix and could no longer be matched to the neighborhood characteristics file.

The remainder of the file without neighborhood characteristics data, between 0.02 and 0.14 percent, are mismatched for other reasons, often because NCS data has a blank ED number.

66. No neighborhood characteristics data matched to NCS data
(Collection quarter data)

		Total		"X" segments		New construction		Other	
Year-quarter	Size of file	Number	Percent	Number	Percent	Number	Percent	Number	Percent
1972-3rd	16,621	935	5.6	169	1.0	763	4.6	3	0.02
-4th	16,788	1,047	6.2	152	0.9	873	5.2	22	0.13
1973-1st	37,552	2,448	6.5	344	0.9	2,098	5.6	12	0.02
-2nd	37,413	2,837	7.6	339	0.9	2,447	6.5	51	0.14
-3rd	36,998	2,903	7.8	337	0.9	2,554	6.9	12	0.03
-4th	36,775	3,394	9.2	319	0.9	3,028	8.2	47	0.12
1974-1st	33,010	3,016	9.1	281	0.8	2,723	8.2	12	0.06
-2nd	32,830	3,387	10.3	284	0.9	3,064	9.3	39	0.12
-3rd	32,928	3,301	10.0	262	0.8	3,029	9.2	10	0.03
-4th	36,234	3,610	10.0	278	0.8	3,314	9.1	18	0.03
1975-1st	36,439	3,477	9.5	254	0.7	3,210	8.8	13	0.03
-2nd	36,333	3,739	10.3	283	0.8	3,435	9.4	21	0.06
-3rd	36,463	3,635	10.0	245	0.7	3,370	9.2	20	0.05
-4th	36,599	3,926	10.7	312	0.8	3,593	9.8	21	0.06
1976-1st	36,683	3,903	10.6	218	0.6	3,664	10.0	21	0.06
-2nd	36,765	4,124	11.2	307	0.8	3,792	10.3	25	0.07
-3rd	36,952	4,104	11.1	204	0.6	3,884	10.5	25	0.07
-4th	37,142	4,342	11.7	305	0.8	4,011	10.8	26	0.07

*U.S. Bureau of the Census memorandum, January 27, 1977.

Chapter 4

Nonsampling errors associated with the rotating panel design

Introduction

The papers presented in Chapter 4 are devoted to the special problems associated with memory bias in the recall of criminal victimizations and with the special features of the National Crime Survey (NCS) design used to compensate for these recall problems. These papers differ from those found in Chapter 3 because they are specifically devoted to discussing the relationship between memory bias and the rotating, panel features of the NCS design, whereas the papers in Chapter 3 discuss problems common to many sample surveys.

The first paper in this chapter, by Anthony G. Turner, outlines the principal features of the NCS rotating panel design as they relate to the problems of controlling memory bias. Among other problems, Turner discusses "telescoping" of victimization reports and shows how "bounding" and other techniques are used to control and estimate memory bias.

The second paper, by Linda R. Murphy and Charles D. Cowan, compares victimization rates for unbounded and bounded interviews. Their study demonstrates a higher report of victimizations for unbounded interviews, which the authors suggest is evidence of the magnitude of over-reporting resulting from telescoping.

The third study by Henry Woltman, John Bushery, and Larry Carstensen takes another approach toward estimating the effects of memory bias by examining the "fall off" in reporting as a function of time from interview to reported incident.

The fourth paper by Henry Woltman and Glenn Cadek considers the relationship between characteristics of the victimization and other characteristics of the incident. They report that the time between the interview and the occurrence of the incident appears to be more associated with victimization reporting than the social characteristics of the victim such as sex, age, and race or the characteristics of the incident such as the presence or absence of a weapon or whether the victim knew the offender.

The final paper, by Henry Woltman and John Bushery, summarizes the findings of a study of panel bias directed toward estimating the fall-off in reporting of victimizations associated with time in sample.

The effect of memory bias on the design of the National Crime Survey*

by ANTHONY G. TURNER

Introduction and purpose

This paper is devoted to the general topic of recall length and memory bias, subjects which are of key concern to survey practitioners involved in the design of retrospective surveys. The survey discussed in this paper is the NCS and the central theme of the discussion will be to demonstrate how problems associated with respondent recall have had very direct impact on the statistical design of NCS. None of the data presented here is original in the sense that they have not been reported elsewhere. Rather, all of the data shown have been abstracted from prior studies or reports to address the current topic. Consistent with the objectives of this conference, we focus primarily on the efforts of statisticians at the Bureau of the Census. However, this is not to suggest that others who have studied victim survey-related memory bias issues have not made far-reaching contributions.

Memory bias—relation to NCS

There are two fundamental types of bias arising from faulty memories on the part of respondents that affect the results of retrospective survey data. The first is simply memory failure, where the event being elicited is totally forgotten. One might imagine that for NCS, memory failure is not a large problem. After all, how could someone forget that he or she has been criminally assaulted or held up? The evidence is abundant, however, that criminal victimizations, even very serious ones, are not always recalled, particularly if the event occurred long ago.[1]

The second type of bias is memory distortion or, rather, partial memory failure. This occurs when an event is remembered but details surrounding the event are hazily recalled or misremembered. In NCS, for example, a respondent may remember being robbed but forget how much money was taken, whether two or three offenders were involved, or other details. A particularly troublesome type of partial memory failure in most retrospective surveys is the respondent tendency known as telescoping. Telescoping occurs when an event is rightfully recalled, but it is remembered as occurring earlier or later than its actual occurrence. In the NCS, for example, a respondent may report a rape attempt in the 6-month reference period, when it actually occurred 8 months ago.[2] A bias such as this is external telescoping, that is, when an event is erroneously telescoped into the reference period. Telescoping within a reference period can also occur, and we refer to this condition as internal telescoping.

Though there has been a considerable body of documentation relating to recall bias in NCS, there is not a great deal known about the causes of memory decay. It is plausible to speculate, for example, that the seriousness of a victimization would affect its saliency in the respondent's memory. One could reason that the more trivial an event, the less likely it is to be remembered and reported on in the survey. It is conceivable that particularly heinous crimes are psychologically repressed by the victim and thus would not be subject to being tapped in a victimization survey. Moreover, there remains the question as to what degree forgetfulness can be attributed to the characteristics of the respondents. Is memory decay for crime-reporting related to a respondent's age, educational attainment, or to some other personal characteristic?

We should not overlook, however, that there are a number of other factors which contribute to potential reporting biases in the crime survey. These factors are not associated with memory per se, but unfortunately it is not always possible to disentangle their contribution to total reporting error from the contribution which is attributable to memory bias. One such reporting error is purposeful suppression. A victim may decide not to report a victimization to our survey interviewer for several reasons: the offender was the victim's spouse, the victim may have precipitated the attack or otherwise felt partly responsible (e.g., being drunk on the street), the victim is embarrassed to admit the victimization to a government interviewer, etc.

A related type of purposeful suppression comes about due to respondent conditioning. Respondents are interviewed every 6 months in this survey, and it is quite possible for a respondent to realize and to take advantage of the fact that the fewer victimizations which are mentioned, the shorter the interview.[3]

Another nonmemory-related factor that can distort the overall results is the problem of misconceptualization. Respondents very quickly realize, in the course of the interview, that they are to report instances of "crimes" by which they have been victimized. Depending upon the respondents' preconceptions about "what the government really wants," they may not, for example, report an intrafamilial assault, on the grounds that, at least in their own mind, the attack was not a "crime." Though the questionnaire is constructed to try to avoid such pitfalls, it is of course not perfect.

Finally, another source of reporting error is the use of a proxy respondent. Here, the proxy can fail to mention a relevant victimization of another household member, either because he or she is unaware of it altogether or, though aware, does not have as vivid a memory of the details as the victim. Bias due to this phenomenon is minimized, however, through the use of self-response in NCS.

Memory bias—effect on NCS design

Our knowledge about memory bias in NCS, particularly its causes, is far from complete. Furthermore, there is a need for additional study, under properly controlled experimentation, to separate memory factors from other types of reporting errors. Yet the evidence is clear that recall bias exists for victimization survey measurement, as it does for virtually any type of retrospective survey inquiry.

The question could then be legitimately raised, "granted that memory biases can adversely affect victim survey statistics, what, if any, design features are built into NCS to minimize or otherwise take account of their impact?" We have identified at least five major survey design features of NCS that are directly linked to the problem of recall, and a sixth which can be partially associated with recall bias.

*A paper presented at the Census Bureau-American Marketing Association Conference, Arlington, Virginia (October 1976).

[1]See Anthony G. Turner, "The San Jose recall study"; Richard W. Dodge, "The Washington, D.C. recall study"; Linda R. Yost and Richard W. Dodge, "The Baltimore recall study"; and Richard W. Dodge and Anthony G. Turner, "Methodological foundations for establishing a national survey of victimization," in Robert G. Lehnen and Wesley G. Skogan (editors), *The National Crime Survey: Working Papers, Volume I: Current and Historical Perspectives*. See also the paper by H. Woltman, J. Bushery, and L. Carstensen reprinted in this chapter.

[2]See the papers by H. Woltman, J. Bushery, and L. Carstensen and by L. R. Murphy and C. D. Cowan reprinted in this chapter.

[3]See the paper by H. Woltman and J. Bushery reprinted in this chapter.

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1 OF 2

One of the principal survey design questions for a retrospective survey is the length of the recall period. The answer is often a function of a number of predetermined constraints, including budget and the specified frequency of data production. Tabulated results from NCS data itself indicate very strikingly that the length of the recall period affects the survey estimates. Victimization rates decrease with an increasing recall period, ranging from 1 to 6 months.

The results shown in table 67 demonstrate both memory fading and internal telescoping. Such dramatic results were naturally not available at the time the NCS was being designed. The question of recall, however, was addressed through a series of reverse record studies that were undertaken in preparation for the NCS. These studies all involved surveying known victims of certain crimes, selected from police records. The upshot was that the findings convinced the statisticians responsible for the design of the NCS that memory failure could not be taken lightly and that choosing the length of the recall period for NCS was serious business indeed. If the budget for the survey had been unlimited, a reference period of 1 month would not have been inconceivable, given the very substantial losses to be expected with longer periods. But decisions about survey design are rarely made on technical grounds alone, and ultimately it was decided to use a 6-month reference period for NCS (the viable alternatives were 3 months and 12 months).

67. Victimization rate by length of recall period for victimizations reported as occurring in June 1973 through September 1974*

Type of crime	Victimization rate by accumulated length of recall period					
	1 month	2 months	3 months	4 months	5 months	6 months
Total personal crimes	261.61	227.67	202.59	186.68	174.23	161.87
Crimes of violence	72.46	59.60	53.69	49.56	45.85	42.55
Assault	58.91	47.34	42.37	38.53	35.49	32.54
Personal theft	189.15	163.07	148.90	137.13	128.38	119.32
Total household crimes	485.18	416.29	382.91	355.57	334.18	311.85
Burglary	185.44	160.32	149.42	140.06	132.16	123.62
Household larceny	264.43	224.06	203.56	187.21	174.90	162.43
Motor vehicle theft	35.30	31.92	29.93	28.29	27.12	25.80

*Personal crimes—per 1,000 persons age 12 and over interviewed.
Household crimes—per 1,000 households interviewed.

A second feature of the NCS survey design linked to the issue of memory bias is the matter of sample size, which, in turn, is very closely tied to the length of the reference period. The overall sample size for a survey is generally a function of the statistics being measured and the reliability which is sought in their measurement. Where retrospective data are involved, recall acumen also becomes an important determinant in sample size. For example, in the NCS one objective is to produce annual counts of victimizations. The sample size for NCS was determined to be 60,000 interviewed units. If recall failure were not an issue in NCS then the survey could conceivably be administered once a year, in which case the 60,000 sample units would be asked to report victimizations for the preceding 12 months. Instead, the survey is conducted twice yearly with the same respondents, since memory loss is so great after 6 months. The chief point here is that there are 120,000 interviews, an effective doubling of the required sample size—a phenomenon, thus directly attributable to recall bias.

A third important feature of the NCS design that takes account of memory bias—in this case, partial memory failure—is the procedure of bounding the survey interviews. In a rotating panel design like the NCS it is possible to partially control for forward telescoping into the reference period (external telescoping) by reminding the respondent of incidents he or she reported in the previous interview in order to disallow them from being reported in the current reference period. This practice is known as bounding the interviews. In some of the experimental work conducted prior to the start of the NCS, it was learned that interviews which are unbounded produce considerably higher estimates of victimization than interviews which are bounded.

Memory bias from telescoping into the reference period can be effectively controlled with the use of bounded interviews. As a consequence, a design feature that was instituted for NCS was to exclude the data collected from incoming rotation groups in the production of the survey estimates for publication, because these data are unbounded. This is not a trivial decision since one-seventh of the total interviews conducted each month are from an incoming rotation group. Subsequent analysis of these NCS data, comparing unbounded victimization estimates from incoming rotation groups to bounded victimization estimates from continuing rotation groups, supports the decision to exclude the unbounded interviews from the published statistics.[4]

The "rolling reference period" design feature partially controls for internal telescoping, and represents a fourth design parameter of NCS associated with memory factors. The rolling reference period arises because the sample design is administered by having each one-sixth of the sample persons report incidents for the last 6 months, so that interviewees contacted in January report for the preceding July–December, those contacted in February report for the preceding August–January, etc. The survey estimates are thus produced in such a manner that one-sixth of the observations that make up the quarterly estimate are taken from respondent reports of crimes occurring 6 months ago, another one-sixth are from crimes reported as occurring 5 months ago, and similarly one-sixth come from each of 4-month-ago reports, 3-month-ago reports, 2-month-ago reports, and finally for reports within the last month. This procedure, in effect, means that equal weight is given to the observations over all the months of the reference period, even when we know that greater memory fading affects the more distant observations (refer again to table 67).

A fifth design feature of NCS, in which concern about recall played a part, was in the construction of the questionnaire itself. The victimization-related sections of the

[4]See the analysis by L. R. Murphy and C. D. Cowan reprinted in this chapter.

Effects of bounding on telescoping in the National Crime Survey*

by Linda R. Murphy and Charles D. Cowan

Introduction

In a general population sample survey calling for respondent recall of events experienced prior to the interview, the type of memory error known as telescoping is of major concern. Telescoping is the tendency of the respondent to report events as occurring either earlier or later than they actually occurred. An event being reported as occurring earlier than it actually occurred is backward telescoping, whereas forward telescoping is reporting an event as occurring later. Further, both backward and forward telescoping can be either internal to the survey's reference period, or external. Internal telescoping occurs when the respondent correctly places an event within the reference period, but misinforms on the precise day, week, or month of occurrence. External telescoping occurs when the respondent erroneously places an event into the reference period. Telescoping is an important technical issue in a panel survey involving recall for two reasons. First, depending upon the magnitude, nature, and direction, uncontrolled telescoping can result in serious response biases in survey estimates for a given time period; and second, various procedural efforts to control telescoping have a major impact on survey design and cost.

In an effort to control forward external telescoping, an interviewing procedure called bounding was instigated for the purpose of minimizing the shifting of reports of crimes into the NCS reference period. Thus, bounding is a procedure utilized to prevent the reporting of the same incidents in consecutive reference periods by eliminating reports of incidents that were also reported during the previous interview. The initial interviews at addresses in incoming rotation groups are used to bound subsequent interviews; they are not used to produce the estimates of victimizations. This is a very costly feature of the NCS design, since the data from incoming rotation groups are therefore not used in tabulating results for publication.

The primary focus of this paper is to examine the effectiveness of the procedural and design features of NCS related to bounding, in controlling forward external telescoping, using data, for the first time, from NCS itself. This will be done by comparing estimates of victimizations based on bounded data from returning rotation groups with estimates based on unbounded data from incoming rotation groups. This estimate of the bounding effect using NCS data can provide a basis for reevaluating the cost-effectiveness of this aspect of the survey design, though in this paper we provide only a bivariate description of the data.

A second issue addressed in this paper is variation in forward external telescoping, by means of examining differential effects of bounding by demographic and socioeconomic characteristics of respondents, as well as by characteristics of the incidents of victimization. We believe this is a critical issue to investigate in victimization surveys. If there are no significant differences in telescoping for either certain classes of respondents or for certain classes of events, then relationships and patterns would be unaffected by the inclusion of unbounded data in producing survey estimates, though levels of victimization would be affected. Thus it may be less crucial to maintain the bounded aspects of the NCS. On the other hand, if there are significant differences in telescoping by certain respondents or for certain incidents, then relationships and patterns would be distorted by including unbounded data in the survey estimates.

Two broad questions suggest themselves. First, do some groups of respondents telescope events more than others? Second, are some types of incidents telescoped more than other types? There are two feasible but opposing hypotheses related to differential telescoping by type of incident. One hypothesis is that the more important, more serious, or more salient events are telescoped forward to a greater degree than the less important, perhaps because the less important are more likely to be completely forgotten. The second hypothesis is that the less important, less serious, or less salient events are telescoped forward to a greater degree because the month of occurrence is less accurately recalled and therefore subject to greater recall bias.

One final aspect of the bounded design in NCS to be discussed in this paper is the extent of actual bounding of interviews among households, and within households among persons, in repeat rotation groups.

*A paper presented to the annual meeting of the American Statistical Association (August 1976), Boston, Massachusetts.

Comparison of victimization rates

Two estimates, total personal victimizations[1] and total property victimizations,[2] are of primary interest in this paper. For each, we are comparing the total bounded victimization rate obtained from the returning rotation groups with the unbounded victimization rate estimated from the incoming rotation groups. The rates for each sample are produced using identical processing, weighting, and tabulation procedures, with appropriate adjustments to account for the fact that the incoming rotation group is approximately one-sixth the size of the bounded sample.

Tables 68 and 69 show the bounded and unbounded rates for total personal and total property victimizations for data quarters I-74 through I-75,[3] plus a z-test of the difference between the rates. All rates reported in the tables are victimizations per 1,000 people or households. Tables 68-74 indicate for each of the data quarters under analysis (column 1): the victimization rates estimated for the population from the bounded and unbounded samples (columns 2 and 3); the difference between the unbounded and bounded rates divided by the bounded rate and expressed as a percent (column 4); the standard errors associated with the two rates (columns 5 and 6); the standard error of the absolute difference between the rates (column 7); and the z-statistic testing whether the difference between the bounded rates is significantly greater than zero. The test is calculated as the ratio of the absolute difference between the unbounded and bounded victimization rates to the square root of the sum of the squared standard errors associated with each rate.[4] The standard errors used were published in the 1973 Advance Report, *Criminal Victimization in the United States* by the Law Enforcement Assistance Administration (LEAA). Because of the large sample sizes the z-statistic approximates the normal distribution, and can be used in conjunction with a table of normal areas and ordinates to determine the level of significance of the test.

68. Total personal victimization rates for bounded and unbounded samples in NCS
(Rate per 1,000 persons)

Data Quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	7.89	11.30	43.219	0.268	0.834	0.876	3.892
II/74	8.90	12.31	38.315	0.285	0.871	0.916	3.721
III/74	9.38	14.88	58.635	0.292	0.958	1.002	5.491
IV/74	9.74	13.29	36.448	0.297	0.904	0.952	3.731
I/75	8.55	12.17	42.339	0.275	0.864	0.906	3.994

69. Total property victimization rates for bounded and unbounded samples in NCS
(Rates per 1,000 households)

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	302.77	138.75	35.010	1.247	3.407	3.628	9.917
II/74	304.89	149.97	44.077	1.249	3.502	3.718	12.339
III/74	314.99	156.65	36.229	1.292	3.548	3.776	11.033
IV/74	319.00	168.96	41.035	1.308	3.647	3.874	12.689
I/75	202.75	147.16	43.221	1.225	3.453	3.664	12.120

70. Comparison of bounded and unbounded personal victimization rates for various types of crimes
(Rates per 1,000 persons)

A—Type of crime: Assaultive violence—Without theft

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	5.36	8.12	51.493	0.215	0.689	0.721	3.827
II/74	6.67	9.09	36.282	0.243	0.739	0.778	3.112
III/74	6.67	10.75	61.169	0.241	0.812	0.847	4.817
IV/74	6.76	8.83	30.621	0.242	0.724	0.764	2.711
I/75	5.93	8.36	40.978	0.222	0.699	0.734	3.311

B—Type of crime: Personal theft—Without assault

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	1.91	2.48	29.843	0.107	0.391	0.405	1.407
II/74	1.63	2.62	60.736	0.106	0.397	0.411	2.410
III/74	2.04	3.44	68.627	0.104	0.431	0.444	3.155
IV/74	2.33	3.52	51.073	0.102	0.434	0.446	2.667
I/75	2.01	2.88	43.284	0.100	0.407	0.420	2.074

The test being performed is a one-tailed z-test, because the procedure of bounding as applied in NCS would only eliminate reporting of victimizations in two consecutive quarters. There is never a chance that victimizations would be added to the reports because of bounding. The null hypothesis, formally stated, is "there is no difference between bounded and unbounded victimization rates." The alternative hypothesis, formally stated, is that "unbounded victimization rates are greater than bounded rates." A z-value of greater than 1.64 means that we can be sure 95 out of 100 times that the estimated differences are greater than zero, and thus are not due to sampling variation; similarly, a value greater than 1.28 means that 90 out of 100 times, the results will not be due to sampling variation (except in the comparison of victimizations reported to the police, table 74, which is a two-tailed test, with z-values of 1.96 and 1.64, respectively).

The z-values clearly show that there are statistically significant differences in the bounded and unbounded personal crime victimization rates for each quarter (table 68), demonstrating that bounding does eliminate a significant number of duplicate victimization reports. The same is true for bounded and unbounded property crime victimization rates (table 69). The unbounded personal victimization rates average 43.8 percent higher than the bounded rates, ranging from 36.4 percent to 58.6 percent. The unbounded property victimization rates average 39.9 percent higher than the bounded rates, and range from 35.0 percent to 44.1 percent.

The following analysis again makes use of testing the difference between bounded and unbounded rates, and represents a preliminary look at the data. A more detailed analysis of the effects of bounding on telescoping for subestimates is planned as more data are collected. The first comparisons we make are by type of crime. Rates of assaultive violence without theft are 44 percent higher on average in the unbounded sample than in the bounded sample, and rates for personal theft without assault are 51 percent higher on average in the unbounded sample (tables 70a and 70b). But comparing the rates quarter by quarter, there is no clear-cut pattern showing that unbounded rates are uniformly higher for the one type of crime over the other. The same result can be found comparing burglaries with larcenies (tables 71a and 71b). The unbounded rates for burglaries and larcenies are, on the average, 40 percent higher than the bounded rates. But in some quarters the relative difference for burglaries is significantly ($\alpha < .10$) larger than for larcenies, and in other quarters it is smaller. Telescoping does not seem to consistently affect rates for one major type of crime more than another.

71. Comparison of bounded and unbounded property victimization rates for various types of crimes
(Rates per 1,000 households)

A—Type of crime: Burglary

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	19.23	27.38	42.382	0.567	1.598	1.696	4.807
II/74	22.60	33.34	47.522	0.612	1.760	1.864	5.763
III/74	26.85	36.62	36.387	0.694	1.837	1.954	5.001
IV/74	23.89	31.40	31.436	0.623	1.698	1.809	4.152
I/75	20.65	29.13	41.065	0.581	1.634	1.734	4.890

B—Type of crime: Larceny

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	79.15	105.36	33.089	1.110	3.080	3.274	7.999
II/74	77.13	111.93	45.119	1.094	3.141	3.326	10.464
III/74	83.34	113.44	36.117	1.124	3.144	3.339	9.014
IV/74	90.78	129.74	42.917	1.163	3.295	3.494	11.151
I/75	77.60	112.30	44.716	1.085	3.124	3.307	10.494

72. Comparison of bounded and unbounded property victimization rates for various types of crimes
(Rates per 1,000 households)

A—Type of crime: Burglary—Entry

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	15.08	19.89	31.897	0.503	1.362	1.451	3.314
II/74	17.85	25.28	41.625	0.545	1.532	1.626	4.569
III/74	20.94	27.71	32.330	0.587	1.598	1.703	3.976
IV/74	18.77	24.25	29.196	0.554	1.492	1.592	3.443
I/75	16.09	20.43	26.973	0.512	1.369	1.462	2.969

B—Type of crime: Burglary—Attempted entry

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	4.15	7.49	80.482	0.282	0.856	0.901	3.708
II/74	4.75	8.06	69.684	0.301	0.880	0.930	3.559
III/74	5.92	8.91	50.507	0.327	0.915	0.971	3.078
IV/74	5.12	7.15	39.648	0.307	0.833	0.888	2.287
I/75	4.56	8.70	90.789	0.289	0.903	0.949	4.365

For subgroups of burglary, however, telescoping is much more prevalent for attempted entries than for actual entries. The unbounded sample rates for burglary/actual entry are, on the average, only 32 percent higher than the bounded rates, whereas the unbounded sample rates for burglary/attempted entry are an average 66 percent higher than the bounded rates (tables 72a and 72b). The same is true when

[1]Personal crimes encompass completed and attempted assault, including rape and robbery.
[2]Property crimes encompass completed and attempted burglary, larceny, and auto theft.
[3]Data quarters I–74 through I–75 refer to data from interviews conducted during January–March 1974 through January–March 1975.
[4] $\frac{r_u - r_b}{\sqrt{S_b^2 + S_u^2}}$

73. Comparison of bounded and unbounded property victimization rates for various types of crimes
(Rates per 1,000 households)

A—Type of crime: Larceny—Completed

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	73.98	97.72	32.090	1.078	2.990	3.179	7.469
II/74	72.57	105.22	44.991	1.065	3.072	3.252	10.040
III/74	77.50	104.87	35.316	1.089	3.058	3.246	8.432
IV/74	85.42	121.49	42.227	1.132	3.216	3.409	10.580
I/75	72.48	104.34	43.957	1.053	3.043	3.221	9.893

B—Type of crime: Larceny—Attempted

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	5.17	7.62	47.389	0.316	0.862	0.918	2.670
II/74	4.56	6.71	47.149	0.295	0.817	0.869	2.475
III/74	5.84	8.57	46.747	0.325	0.900	0.957	2.853
IV/74	5.36	8.25	53.918	0.313	0.884	0.938	3.082
I/75	5.12	7.96	55.469	0.307	0.870	0.923	3.078

comparing completed and attempted larcenies. Again the relative difference is higher for attempted than completed crimes. The average relative difference for the five quarters for attempted larcenies was 50 percent, while for completed larcenies it was only 40 percent (tables 73a and 73b). So it is apparent that telescoping does have a differential effect on the rates of various subcategories of crimes.

There also seem to be some differences in telescoping for property crimes by demographic characteristics of households. The relative difference between bounded and unbounded rates for property crimes reported by one-person households is rather low, only 20 percent higher for the unbounded sample on average. This relative difference increases as the number of persons in the household increases, rising to a 51-percent greater reporting rate in the unbounded sample for households having six or more persons (tables 74a-d).

Another factor which may indicate saliency or importance, and thus influence telescoping, is whether or not the victimization was reported to the police. In four of the five quarters examined, a significantly larger proportion of the property victimizations were not reported to the police in the unbounded sample (table 75).

Two additional factors, total loss suffered from property victimizations and from personal victimizations and whether or not the offender was a stranger to the victim, were included in our analysis as possible indicators of saliency or importance. However, we found no consistent pattern associated with either of these variables over the five data quarters examined.

The figures presented in these tables report the degree to which telescoping occurs. At present, it is safe to conclude that telescoping would have a significant effect on victimization rates if the interviews were not bounded. Beyond that, we can point out that some crimes are telescoped to a greater degree than others, either according to the type of crime or the circumstances, or because of the demographic characteristics of the household. We do not have valid empirical information about why these factors affect telescoping.

Qualifications to comparisons

Three qualifications should be noted with regard to the preceding analysis comparing bounded with unbounded data from NCS. The first qualification is that since respondents are interviewed every 6 months, the data quarters are not independent of one another, because there is some overlap of respondents from one data quarter to the next. Second, all data from returning rotation groups are considered and treated as bounded for purposes of the preceding analysis in this paper. However, since NCS uses a probability sample of addresses rather than designated households or persons, not all of the interviews conducted in returning rotation groups are subject to the actual interviewing procedure of bounding. For interviews in a household to actually be bounded, the identical household must have been interviewed during the previous enumeration period. Therefore, interviews in replacement households, and households that were noninterview or not in sample the previous period, are actually unbounded. However, data from these unbounded interviews are included with data from the bounded interviews because they are in returning rotation groups, and their exclusion may bias the sample.

The unbounded households in returning rotation groups comprise a sizable portion of the interviewed sample (see table 76), averaging 13.3 percent over the five collection quarters, I–74 through I–75. Of these unbounded households, an average of 9.6 percent are replacement households, and 3.7 percent were previously noninterview or not in sample. These unbounded households contribute disproportionally more victimizations than do the actually bounded households. Though bounded households make up about 86 percent of the interviews, they contribute only 76 percent of the victimizations, while unbounded households, which comprise only 13 percent of the interviews, contribute 24 percent of the victimizations. This translates into a reporting rate of about 79 percent more victimizations from unbounded households than one would expect from their proportion of the sample.

Even more striking in terms of contributing victimizations is the difference between types of unbounded households. Households that were previously noninterview or not in sample, while making up 4 percent of the interviews, contribute almost 6 percent of the victimizations. But replacement households, which primarily represent movers and make up about 10 percent of the interviews, contribute an average of nearly 18 percent of the victimizations, or 92 percent more than their expected proportion. Recalling the overall difference of about a 40-percent higher victimization rate for unbounded, incoming rotation groups than for bounded, returning rotation groups, these figures appear to indicate that something more than merely the lack of bounding may be related to the disproportionate reporting of victimizations among replacement households. It is conceivable that they actually experience victimization more frequently than nonmovers for reasons associated with their mobility. Perhaps they move to get away from crime. At any rate, this question appears to warrant further investigation.

Admittedly, the set of data used in the preceding discussion of unbounded data within returning rotation groups is somewhat lacking in refinement, being based on unweighted counts. However, the stability

74. Comparison of bounded and unbounded property victimization rates by number of persons in household
(Rate per 1,000 households)

A—Number of persons in household: 1 person

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	58.87	64.93	10.294	2.119	6.003	6.366	0.952
II/74	62.20	76.44	22.894	2.166	6.397	6.754	2.108
III/74	66.58	82.49	23.896	2.215	6.515	6.881	2.312
IV/74	69.15	83.62	20.926	2.248	6.516	6.893	2.099
I/75	58.37	72.01	23.368	2.061	6.099	6.438	2.119

B—Number of persons in household: 2 to 3 persons

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	89.09	121.93	36.862	1.621	4.783	5.050	6.503
II/74	91.72	131.01	42.837	1.637	4.916	5.182	7.582
III/74	103.25	133.00	28.814	1.720	4.927	5.218	5.701
IV/74	104.40	134.25	28.592	1.723	4.933	5.225	5.713
I/75	87.89	125.66	42.974	1.588	4.799	5.055	7.472

C—Number of persons in household: 4 to 5 persons

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	140.65	186.12	32.328	2.847	8.067	8.555	5.315
II/74	138.87	200.83	44.617	2.825	8.289	8.757	7.075
III/74	156.23	217.86	39.448	2.945	8.557	9.049	6.811
IV/74	163.47	268.20	64.067	2.992	9.271	9.742	10.750
I/75	141.86	219.25	54.554	2.830	8.504	8.963	8.635

D—Number of persons in household: 6 or more persons

Data quarter	Victimization rate			Standard errors			
	Bounded	Unbounded	% difference	Bounded	Unbounded	Difference	z of difference
I/74	189.88	282.68	48.873	5.890	16.658	17.669	5.252
II/74	188.39	303.44	61.070	5.886	17.084	18.070	6.367
III/74	197.13	320.15	62.406	6.029	17.512	18.520	6.642
IV/74	226.72	324.76	43.243	6.422	17.631	18.764	5.225
I/75	202.24	281.56	39.221	6.103	16.928	17.995	4.408

75. Comparison of total property victimizations not reported to police for bounded and unbounded samples in NCS

Data Quarter	Percent victimizations not reported to police: Bounded	Un-bounded	Difference	Standard errors: Bounded	Un-bounded	Difference	z of difference
I/74	70.672	72.575	1.903	0.6000	0.4778	0.7671	2.481
II/74	66.954	68.769	1.815	0.6128	0.4779	0.7771	2.335
III/74	65.504	65.132	−0.372	0.5824	0.4857	0.7583	−0.491
IV/74	68.884	71.546	2.662	0.5481	0.4394	0.7024	3.790
I/75	67.343	71.339	3.996	0.6097	0.4661	0.7674	5.207

* A two-tailed test of significance was used. A test statistic $|z| < 1.96$ means that one is 95-percent certain that the difference is not the result of sampling error. A test statistic $1.64 < |z| < 1.96$ means that one can be 90-percent certain—but not 95-percent certain—that the difference is not the result of sampling error.

76. Extent of household correspondence between enumeration periods*

Household status	Collection quarter: I–74	II–74	III–74	IV–74	I–75
Same household as last enumeration: Bounded					
Percent of interviewed households	85.85	87.92	86.55	85.18	86.38
Percent of victimizations	74.44	77.11	75.84	73.16	75.11
Not same household as last enumeration: Unbounded					
Total					
Percent of interviewed households	13.87	11.78	12.98	14.58	13.38
Percent of victimizations	25.56	22.89	24.16	26.84	24.89
Ratio**	1.8428	1.9431	1.8613	1.8409	1.8602
Replacement household					
Percent of interviewed households	9.85	8.27	9.37	11.08	9.59
Percent of victimizations	19.27	16.56	18.09	21.42	18.28
Ratio**	1.9563	2.0024	1.9306	1.9332	1.9062
Previous noninterview or not in sample					
Percent of interviewed households	4.02	3.51	3.61	3.50	3.79
Percent of victimizations	6.14	6.17	5.70	5.26	6.33
Ratio**	1.5274	1.7578	1.5789	1.5029	1.6702
Not ascertained					
Percent of interviewed households	0.27	0.30	0.47	0.23	0.25
Percent of victimizations	0.15	0.16	0.37	0.16	0.28

* All data in this table based on unweighted counts.

** $\frac{\text{Percent victimizations from unbounded households}}{\text{Percent of interviewed households that are unbounded within returning rotation groups}}$

77. Extent of person correspondence between enumeration periods within corresponding households*

Interviewed persons	Collection quarter: I–74	II–74	III–74	IV–74	I–75
Percent matched to previous interviews: Bounded	94.85	95.07	94.56	95.57	94.45
Percent nonmatch to previous interview: Unbounded	5.15	4.93	5.44	5.43	5.55
Percent bounded persons reporting 1+ victimizations	7.95	7.79	7.50	8.19	8.21
Percent unbounded persons reporting 1+ victimizations	11.17	10.37	9.48	16.52	11.18
Ratio unbounded to bounded reporting 1+ victimizations	1.4050	1.3312	1.2640	1.2845	1.3617

* All data in this table based on unweighted counts.

of the patterns is apparent and provides evidence that the effect of bounding is understated in comparisons of data between incoming and returning rotation groups, since the returning groups include a substantial amount of unbounded data.

The third qualification is that even within actually bounded households, some interviews with individual household members are unbounded, either because the person is new to the household since the prior enumeration period, or because the person was previously noninterview. A special computer match of interviewed persons in collection quarters I–74 through I–75 with files for previous enumeration periods was performed for the purpose of determining correspondence and bounding of individuals within bounded households. Results of that operation indicate an average of about 95 percent bounded individual interviews (see table 77). Again, this pattern is quite stable over time, and again a difference in reporting victimizations between bounded and unbounded interviews is evident. An average of 7.9 percent of the bounded persons, and 10.5 percent of the unbounded persons reported one or more victimizations. These data also appear to provide evidence that the bounding effect is understated in comparisons of incoming with returning rotation group data.

Conclusion

The data presented in this paper strongly support the conclusion that NCS bounding procedures and design effectively reduce the memory bias of forward external telescoping. Our results, comparing bounded with unbounded sample data, are consistent with results from similar comparisons in the area of consumer expenditures.[5] In that study, however, Neter and Waksberg point out that telescoping effects are compounded with conditioning effects in comparisons between unbounded data based on first interviews and bounded data based on second or later interviews. Evidence from the expenditure study and also from a study of NCS panel bias[6] suggests that conditioning probably accounts for a much smaller portion of the observed differences in NCS than does telescoping.

Further, we can conclude that some variation in telescoping is associated with characteristics of victimization events. Our analysis indicated that telescoping was present for all major types of crimes, but in no discernible pattern. However, it did indicate a greater degree of telescoping for the subcategories of attempted larceny and attempted burglary than for the completed crimes. It also indicated a larger proportion of victimizations not reported to police in the unbounded sample than in the bounded. These results, considered alone, could be interpreted as evidence that the less serious, less important, or less salient events are more subject to the recall bias of forward telescoping than the more serious ones. The finding of no pattern of association with total loss or victim-offender relationship does not support this interpretation. Therefore, we can only conclude that some characteristics of events appear to be related to differential forward external telescoping.

Finally, our evidence also indicates that some variation in telescoping is associated with household characteristics, but hardly any telescoping can be explained by respondent characteristics. Most of the differences found in our analysis of demographic variables, including age, sex, race, education, tenure, and income, were tenuous at best.

[5] John Neter and Joseph Waksberg, U.S. Bureau of the Census, "Response Errors in the Collection of Expenditures Data by Household Interviews: An Experimental Study," Technical Paper No. 11.

[6] See the paper by H. Woltman and J. Bushery reprinted in this chapter.

Recall bias and telescoping in the National Crime Survey*

by Henry Woltman, John Bushery, and Larry Carstensen

This memorandum describes the work we have done concerning recall bias and telescoping as they affect the data collected in the NCS. We also recommend further experimental studies needed to more fully evaluate these effects. In reviewing the results, the following definitions as they relate to the reporting of crime incidents in the NCS will be helpful to the reader:

1. Recall bias: an incident is not reported because it is forgotten by the respondent.

2. Telescoping: the incident is reported but the respondent misremembers the date (month) of occurrence.

3. "Bounded" reference periods: the respondent is reminded of the incidents he reported during his previous interview and those incidents are not allowed to be reported as occurring in the present reference period.

4. "Unbounded" reference periods: no control, other than the respondent's own memory, is exercised over telescoping of incidents into the reference period.

5. "Partially bounded" reference periods: the monthly periods within a bounded reference period are partially bounded when no telescoping is allowed into the bounded period but telescoping between months within the bounded period is not controlled.

6. Internal and external telescoping: "internal" is the name given to telescoping (forward or backward) within a bounded reference period; "external" refers to telescoping into the reference period from without and is prevented by bounding.

78. Data compilation chart for table 79

Month of interview	Reported month of occurrence: June 1973	July	August	September	October	November	December	January 1974	February	March	April	May	June	July	August	September
1973																
July	1															
August	2	1														
September	3	2	1													
October	4	3	2	1												
November	5	4	3	2	1											
December	6	5	4	3	2	1										
1974																
January		6	5	4	3	2	1									
February			6	5	4	3	2	1								
March				6	5	4	3	2	1							
April					6	5	4	3	2							
May						6	5	4	3	1						
June							6	5	4	2	1					
July								6	5	3	2	1				
August									6	4	3	2	1			
September										5	4	3	2	1		
October										6	5	4	3	2	1	
November											6	5	4	3	2	1
December												6	5	4	3	2
1975																
January													6	5	4	3
February														6	5	4
March															6	5

Note: Data collected for periods marked 1 are included in "1" rates of table 79;
Data marked 1 or 2 are included in "2" rates of table 79, etc.

Introduction

The quarterly estimates of victimizations produced by the NCS are made up of data obtained from interviews covering a 6-month bounded reference period. For any particular quarter, each month of the quarter is represented by victimizations reported as occurring in that month from interviews that took place 1, 2, 3, 4, 5, or 6 months after the reported month of occurrence. Thus, any memory failure on the part of the respondent can introduce a bias into the estimate because the weighting factors that are used are related to sample size rather than to length of recall period (we shall use the "length of recall period" to mean the time lag between the reported month of occurrence and the month of interview). We attempt to provide estimates of the recall loss associated with the NCS measurement procedures so that inferences can be made as to the potential magnitude of bias in the current estimates. Unweighted counts of victimizations obtained from samples J01, J02, J03, and J04 covering the months of occurrence June 1973–September 1974 were used for this study. The data have been combined so that the rate for each type of crime can be examined in terms of the accumulated length of recall period. The compilation chart is shown in table 78. The victimizations reported as occurring 1 month prior to the month of interview (i.e., all of the points labeled 1 in table 78) divided by the average number of interviewed persons 12+ or households (depending on the type of crime) for these points, provide the rates shown in the 1-month column of table 79. The victimizations reported as occurring 1 or 2 months prior to the month of interview (i.e., points labeled 1 or 2) were divided by the average of the interviewed persons 12+ or households for the interview months corresponding to the points labeled 1 or 2 for each of the 16 months of occurrence to get the rates shown in the 2-months column of table 79, etc. If there were no errors in reporting the month of occurrence (i.e., no internal telescoping within the 6-month bounded reference period) and the losses due to recall were constant for each reference period from 1 to 6 months, then the expected value of the victimization rates shown in table 79 would be equal. As is evident, however, the victimization rates are different and it is the dropping off of these rates as the accumulated recall period increases from 1 to 6 months that we are attempting to evaluate and interpret.

*U.S. Bureau of the Census memorandum, September 23, 1975.

79. Victimization rate by length of recall period for victimizations reported as occurring in June 1973 through September 1974*

Type of crime	Victimization rate by accumulated length of recall period: 1 month	2 months	3 months	4 months	5 months	6 months
Total personal crimes	261.61	227.67	202.59	186.68	174.23	161.87
Crimes of violence	72.46	59.60	53.69	49.56	45.85	42.55
Assault	58.91	47.34	42.37	38.53	35.49	32.54
Personal theft	189.15	163.07	148.90	137.13	128.38	119.32
Total household crimes	485.18	416.29	382.91	355.57	334.18	311.85
Burglary	185.44	160.32	149.42	140.06	132.16	123.62
Household larceny	264.43	224.06	203.56	187.21	174.90	162.43
Motor vehicle theft	35.30	31.92	29.93	28.29	27.12	25.80

* Personal crimes—per 1,000 persons age 12 and over interviewed.
Household crimes—per 1,000 households interviewed.

Discussion of results

Sudman and Bradburn[1] propose an exponential model to describe the relative error caused by recall bias. The model is as follows.

$$r_o = ae^{-b_1 t} \tag{1}$$

where r_o is the fraction of all events reported ($1 - r_o$ equals the relative error), a is a nontime-related parameter measuring mainly the original knowledge and/or the impact of the event on the respondent, b_1 is the time related parameter measuring the memory failure of the respondent, and t is the length of the reference in units of time (weeks, months, etc.).

To describe the telescoping effect, the following logarithmic model is proposed

$$r_t = \frac{\log b_2 t}{t} \tag{2}$$

where r_t is the relative error caused by telescoping, b_2 is the telescoping parameter, and t is again the length of the reference period in units of time.

It can be seen that on the basis of these models, as t increases, the relative error caused by memory failure, $1 - r_o$, will increase and that caused by telescoping, r_t, will decrease. In order to evaluate the constants of these models, independent validating data, such as record checks, are needed. Anticipating a situation where validation data are not available, Sudman and Bradburn give the following ratio model relating the levels of reporting for two reference periods of different length, t_2 and t_1:

$$R\left(\frac{t_2}{t_1}\right) = \left(\frac{t_1}{t_2}\right)\left(\frac{t_2 + \log b_2 t_2}{t_1 + \log b_2 t_1}\right) e^{-b_1(t_2 - t_1)} \tag{3}$$

Equation (3) implies that there is some optimum time where the understatement caused by memory failure would balance the overstatement caused by telescoping. This model assumes that the telescoping is external to the reference period. In our case, the NCS, we assume that we prevent external telescoping by the bounding process and that internal telescoping does not affect the overall level (the average) of reporting, but only the distribution within the months of the bounded period. Thus this model is not applicable to our data.

However, a method to remove the internal telescoping effect in the NCS data so that any variation in the victimization rate by the length of the recall period will be the result of memory failure can be developed on the basis of the telescoping model as follows.

Define:

$$R_i = 1 + \frac{\log b_2 i}{i}$$

as one plus the relative error caused by telescoping into the i^{th} accumulated recall period.

$$Q_i = \frac{V_{Unb_i}}{V_{PB_i}}$$

as the ratio of victimization rates derived from observed unbounded (V_{Unb}) and observed partially bounded (V_{PB}) data for the i^{th} accumulated recall period.

$$P_i = \frac{V_{PB_i}}{V_{B_i}}$$

as the ratio of victimization rates derived from partially bounded and bounded (V_B) data for the i^{th} accumulated recall period. Note that Q_6 by definition is equal to unbounded to bounded data since the NCS uses a 6-month bounded recall period and that P_6 is by definition equal to one since the NCS uses a bounded 6-month recall period.

Since the R_i's are a measure of the telescoping effect, they also can be considered as a ratio of V_{Unb_i}/V_{B_i}. With this interpretation we can estimate the P_i's as follows:

If we divide R_i by Q_i we get

$$\frac{R_i}{Q_i} = \frac{V_{Unb_i}/V_{B_i}}{V_{Unb_i}/V_{PB_i}} = \frac{V_{PB_i}}{V_{B_i}} = P_i$$

Therefore, assuming equation (2) is valid for victimization data, the P_i values can be used to remove the internal telescoping effect. That is, the victimization rate for a bounded reference period of length i months is estimated by $V_{B_i} = V_{PB_i}/P_i$

where V_{PB_i} is the rate obtained from the NCS given in table 79.

The unbounded data for sample JO2 for the interview months January–June 1973, along with the corresponding bounded data from sample JO1 for the same interview months, were used to estimate the telescoping parameter b_2 (see table 80). The estimated parameter b_2 was then used to compute the R_i values defined above (see table 81). The NCS data for the interview months January–June 1973 were also used to compute victimization rates for accumulated recall periods of 1 to 5 months. These rates and similar rates obtained from the unbounded sample JO2 were used to compute the Q_i values (see table 82).

Taking a 1-month recall period for total personal crimes (TPC), the values in these tables can be interpreted as follows. For an unbounded 1-month recall period we expect to observe 106-percent [$100(R_1 - 1)$] higher victimization rate than for a 1-month bounded period based on the telescoping model and estimated b_2 parameter (see table 80). Similarly, we observe only

[1]S. Sudman and N.M. Bradburn, "Effects of Time and Memory Factors on Response in Surveys," *Journal of the American Statistical Association* 68, No. 344, pp. 805–815.

80. Comparison of victimization rates based on bounded and unbounded data*

Category	Bounded victimization rate (J01)	Unbounded victimization rate (J02)	Relative error (J02 − J01)/J01	Estimated b_2
Total personal crimes	64.30	83.95	31%	11.35
Crimes of violence	16.40	22.73	39%	34.69
Assault	12.15	17.11	41%	46.97
Personal theft	47.91	61.22	28%	7.75
Total household crimes	103.16	137.58	33%	16.74
Burglary	42.79	57.10	33%	16.91
Household larceny	51.41	68.68	34%	17.27
Motor vehicle theft	8.96	11.80	32%	13.34

* Rate represents number of victimizations reported as occurring in 6 months prior to interview for interview months January–June 1973 per 1,000 interviewed household or persons age 12 and over.

81. Theoretical telescoping factors comparing unbounded to bounded victimization rates by length of recall period

Category	R_1	R_2	R_3	R_4	R_5	R_6
Total personal crimes	2.06	1.68	1.51	1.41	1.35	1.31
Crimes of violence	2.54	1.92	1.67	1.54	1.45	1.39
Assault	2.67	1.99	1.72	1.57	1.47	1.41
Personal theft	1.89	1.60	1.46	1.37	1.32	1.28
Total household crimes	2.22	1.76	1.57	1.46	1.38	1.33
Burglary	2.23	1.76	1.57	1.46	1.39	1.33
Household larceny	2.24	1.77	1.57	1.46	1.39	1.34
Motor vehicle theft	2.13	1.71	1.53	1.43	1.36	1.32

82. Observed ratios comparing unbounded to partially bounded victimization rates

Category	Q_1	Q_2	Q_3	Q_4	Q_5	Q_6
Total personal crimes	1.14	1.14	1.18	1.23	1.22	1.31
Crimes of violence	1.26	1.27	1.27	1.31	1.33	1.39
Assault	1.40	1.32	1.30	1.32	1.35	1.41
Personal theft	1.08	1.10	1.15	1.21	1.24	1.28
Total household crimes	1.14	1.18	1.20	1.22	1.27	1.33
Burglary	1.30	1.23	1.24	1.24	1.29	1.35
Household larceny	1.05	1.14	1.16	1.18	1.27	1.34
Motor vehicle theft	0.98	1.20	1.25	1.30	1.28	1.32

83. Theoretical ratios of partially bounded to bounded victimization rates

Category	P_1	P_2	P_3	P_4	P_5	P_6
Total personal crimes	1.80	1.47	1.28	1.15	1.07	1.0
Crimes of violence	2.02	1.51	1.32	1.18	1.09	1.0
Assault	1.91	1.51	1.32	1.19	1.09	1.0
Personal theft	1.74	1.45	1.27	1.14	1.06	1.0
Total household crimes	1.96	1.49	1.31	1.20	1.09	1.0
Burglary	1.71	1.43	1.26	1.18	1.07	1.0
Household larceny	2.13	1.55	1.35	1.24	1.09	1.0
Motor vehicle theft	1.17	1.43	1.22	1.10	1.07	1.0

a 14-percent $[100(Q_1 - 1)]$ higher victimization rate in a 1-month unbounded recall period when compared to a 1-month partially bounded recall period (see table 81). Here the 1-month recall period is partially bounded as a result of the use of a 6-month bounded recall period in the NCS. Dividing the values in table 81 by the corresponding values in table 82 gives the estimated values for P_i which are found in table 83.

Applying these factors to the victimization rates in table 79 obtain the calculated victimization rates shown in table 84 (the observed rates of table 79 are repeated for comparison purposes). These calculated rates are estimates after adjusting for the effects of internal telescoping.

The adjusted victimization rates show a slight increase with time. We are inclined to disregard this slight incline and treat the data as being level—that is, exhibiting no change in memory failure over recall periods ranging from 1 to 6 months. This conclusion results from the fact that closer analysis of the NCS data indicates that what we have previously defined as bounded data (i.e., the data from sample JO1 for interview months January–June 1973) does in fact contain a considerable amount of unbounded data. We speculate that data obtained from as many as 25 percent of the interviewed households was unbounded. This results from the fact that about 10 percent of segments interviewed in these months were in sample for the first time as well as the fact that another 14 percent of the households in sample for the first time 6 months earlier now contained a new household, were not interviewed 6 months earlier, or were newly constructed units in sample for the first time during January–June 1973. Thus, the victimization rates denoted earlier as relating to partially bounded recall periods are overstated; the Q_i values are understated (since $Q_i = V_{iUnb}/V_{PBi}$); and the P_i values are overstated (since $P_i = R_i/Q_i$). This conclusion in turn implies that the calculated victimization rates shown in table 84, which presumably reflect the rates after the internal telescoping effects have been removed, are understated.

It is also probable that the understatement is different by the length of the reference period since we would expect the unbounded data to have a differential effect on

84. Theoretical victimization rate after removing "telescoping effect" compared to observed rate for months of occurrence—June 1973 to September 1974
(Rates shown per 1,000 interviewed persons age 12 and over or households)

Category	Reference period: 1-month Obs	1-month Calc	2-month Obs	2-month Calc	3-month Obs	3-month Calc	4-month Obs	4-month Calc	5-month Obs	5-month Calc	6-month Obs	6-month Calc
Total personal crimes	261.61	145.43	227.67	155.14	202.59	157.92	186.68	162.46	174.23	163.18	161.87	161.87
Crimes of violence	72.46	35.96	59.60	39.51	53.69	40.79	49.56	42.13	45.85	42.17	42.55	42.55
Assault	58.91	30.87	47.34	31.39	42.37	32.01	38.53	32.47	35.49	32.54	32.54	32.54
Personal theft	189.15	108.54	163.07	112.27	148.90	117.21	137.13	120.40	128.38	121.02	119.32	119.32
Total household crimes	485.18	248.17	416.29	278.98	382.91	293.38	355.57	296.78	334.18	306.73	311.85	311.85
Burglary	185.44	108.24	160.32	111.83	149.42	118.17	140.06	119.18	132.16	123.21	123.62	123.62
Household larceny	264.43	124.23	224.06	144.56	203.56	150.35	187.21	151.53	174.90	159.83	162.43	162.43
Motor vehicle theft	35.30	16.24	31.92	22.33	29.93	24.48	28.29	25.85	27.12	25.37	25.80	25.80

each month of the 6-month recall period. Assuming the underlying basis of the telescoping model is correct—that is, that the relative error due to telescoping decreases with increasing length of the reference period—we would expect the relative error in the Q_i value to decrease with increasing length of the reference period.

The hypothesis that the adjusted data is level is consistent with the San Jose pretest data. In that study the reporting level fell to approximately 67 percent of the actual occurrences for a recall period of 1 month, and no additional recall loss was observed for periods of recall up to 6 months. However, the San Jose pretest data relates to crimes already reported to the police, and we would expect that reporting the crime to the police would reinforce the memory process. The lack of any data to validate our assumption that the logarithmic model, equation (2), is valid suggests the need to gather additional data in order to more fully evaluate the effects of recall bias and internal telescoping.

Are memory biases in the National Crime Survey associated with the characteristics of the criminal incident?*

by HENRY WOLTMAN and GLENN CADEK

Objectives

Previous analysis of the NCS data has shown that the combined effect of memory biases—specifically those of recall of a criminal event (recall bias) and misplacing of the event in time (telescoping)—can have a substantial impact on the estimated victimization rate.[1] However, in addition to estimating the number of criminal victimizations, a major aim of the NCS is the isolation of personal characteristics which are related to victimization experiences.[2] To the extent that memory biases are associated with the characteristics of the criminal event, the usefulness of the NCS data for both purposes will be lessened.

This report provides the results of an analysis of the NCS data for evidence of association between the reported characteristics of reported incidents and the combined effects of recall bias and telescoping within the 6-month reference period. Specifically, we examine the distribution of incidents by the number of months prior to the month of interview they were reported to have occurred (1-6) by the reported characteristics of the incident. Statistically significant differences in these distributions provide evidence of differential effects of memory biases, recall biases, and telescoping.

Methodology

For the data under study, the distribution of criminal events across each of the 6 months of recall would be uniformly distributed if there were no memory biases.[3] If there were no memory biases, for any particular month of occurrence one would expect the same number of incidents to be reported as occurring in that month for interviews taking place 1, 2, 3, 4, 5, or 6 months after the month of occurrence.

85. Number (and percent) of personal incidents by reported month of occurrence prior to month of interview and whether reported to police

Was incident reported?	1st month	2nd month	3rd month	4th month	5th month	6th month	Total
Total personal crimes							
Yes	2255.36 (.230)	1783.25 (.182)	1669.48 (.170)	1506.96 (.154)	1137.59 (.140)	1220.76 (.124)	9812.4
No	6921.64 (.287)	4664.74 (.194)	3977.73 (.165)	3356.8 (.139)	2931.54 (.122)	2224.38 (.092)	24076.83
Total crimes of violence							
Yes	913.46 (.267)	633.66 (.185)	555.45 (.162)	498.3 (.145)	436.55 (.127)	389.76 (.114)	3427.18
No	1302.03 (.305)	802.15 (.188)	705.1 (.165)	575.75 (.135)	509.92 (.120)	371.16 (.087)	4266.11
Assault							
Yes	699.14 (.288)	467.09 (.192)	386.93 (.159)	346.91 (.143)	293.89 (.121)	234.7 (.097)	2428.66
No	1073.26 (.322)	619.23 (.186)	551.94 (.165)	424.29 (.127)	394.32 (.118)	272 (.082)	3335.94
Crimes of theft (Personal)							
Yes	1435 (.222)	1149.6 (.177)	1113.83 (.172)	1008.66 (.156)	940.03 (.145)	831 (.128)	6428.12
No	5619.6 (.284)	3862.08 (.195)	3271.62 (.165)	2781.10 (.140)	2421.62 (.122)	1853.21 (.094)	19800.18

86. Total number (and percent) of household incidents by reported month of occurrence prior to month of interview and whether reported to police

Was incident reported?	1st month	2nd month	3rd month	4th month	5th month	6th month	Total
Total household crimes							
Yes	2342.29 (.213)	1919.03 (.171)	1878.03 (.171)	1750.5 (.159)	1684.2 (.153)	1439.6 (.131)	11013.65
No	5112.91 (.288)	3472.67 (.195)	2970.1 (.167)	2431.16 (.137)	2108.6 (.119)	1675. (.094)	17770.44
Larceny (Household)							
Yes	837.37 (.217)	677.11 (.176)	639 (.166)	618 (.160)	581 (.151)	502 (.130)	3854.48
No	3208.41 (.289)	2199.17 (.198)	1844.6	1611.66	1309.1 (.118)	1023 (.092)	11095.94
Burglary							
Yes	1134.58 (.206)	944.92 (.171)	960.7 (.174)	886.5 (.161)	849.7 (.206)	733.6 (.133)	5510
No	1717.5 (.289)	1139.5 (.192)	991.5 (.167)	808.83 (.136)	709.5 (.119)	579 (.097)	5945.85
Motor vehicle theft							
Yes	470.33 (.269)	297 (.170)	278.33 (.159)	246 (.141)	253.5 (.145)	204 (.117)	1749.16
No	187 (.257)	134 (.184)	134 (.184)	111 (.152)	90 (.123)	73 (.100)	729

In fact, the NCS is designed such that one can tabulate data by each month of recall across any number of months of occurrence. Since the NCS data are collected for the 6 months prior to the month of interview, an estimate for each month of occurrence can be made by each month of recall. These data can then be cumulated across months of occurrence to increase the reliability of the comparisons.

To summarize, we have cumulated the unweighted[4] number of reported incidents by month of recall for the months of occurrence June 1973–June 1975 and expressed the number of incidents reported for each month of recall as a percent of all incidents reported for this time period. If there were no memory biases, we would expect (disregarding sampling error) that one-sixth of the total incidents would have been reported as occurring in each of the 6-month recalls.

A z-statistic on the difference between the percents was calculated for each month of recall to test for statistically significant differences in the distributions by the characteristics of the incident—for example, whether or not the incident was reported to the police. See Appendix A for a discussion of the variance estimate used in the z-statistic.

For each type of crime and characteristic of the incident shown in this report, six z-statistics, one for each month of recall, are shown. In order to carry out the comparisons at a conservative overall or "experimentwise" significance level of α so that the probability of making no false claims of significance for all comparisons is $1\text{-}\alpha$, a simple multiple comparison procedure was employed. This procedure is generally known as the "Bonferroni" method and consists of carrying out multiple z-tests but reducing the significance for each test (comparison) from α to (α/m) where m is the number of prespecified comparisons of interest.[5] In our case, we let $m=6$ since we are interested in six comparisons, one for each month of recall. Using this test procedure, any comparison is declared significant if the z-statistic is less than -2.64 or greater than 2.64 and we have at least a 95-percent chance of making no false claims of significance among the six comparisons.

Results

Our analysis of the NCS national sample data by whether or not the incident was reported to the police indicates that there is in fact a statistically significant difference at the 5-percent level between the proportion of incidents reported versus those not reported to the police for one or more of the six-month-of-recall comparisons for total personal crimes, crimes of violence, personal theft, total household crimes, household larceny, and burglary[6] (see tables 85 and 86). Table 89 summarizes the z-statistic for the estimated differences between the proportion of all incidents reported by each month of recall. Significant differences at the 5-percent "experimentwise" level have been asterisked. Notice that for total personal crimes and personal theft, there is a significant difference between the proportion of incidents reported to the police versus the proportion not reported for 1, 5, and 6 months of recall. For total household crimes, household larceny, and burglary, there is a significant difference for 1, 4, 5, and 6 months of recall. In no case were significant differences detected for 3 months of recall.

Thus, our conclusion is that for these crimes, there is a differential effect of memory biases (recall and telescoping) for incidents reported versus those not reported to the police. The distributions of several other characteristics of the incident were also tabulated from crimes of violence and assault (see tables 87 and 88). These included characteristics of incidents involving a single offender—sex, race, and age, whether or not a weapon was used in the incident, and the victim's relationship to the offender (stranger versus nonstranger).

For crimes of violence, statistically significant "experimentwise" differences were

87. Total number (and percent) of total personal incidents by reported month of occurrence prior to month of interview and selected characteristics

		1st month	2nd month	3rd month	4th month	5th month	6th month	Total
Race								
	White	970.17 (.297)	612.12 (.188)	538.25 (.165)	442.29 (.135)	418.45 (.128)	283.09 (.097)	3261.37
	Black/other	479.07 (.302)	277.63 (.175)	249.6 (.157)	229.98 (.145)	186.04 (.117)	162.74 (.103)	1587.96
Age								
	Under 21	556.25 (.321)	325.97 (.188)	294.29 (.170)	211.30 (.122)	206.66 (.119)	140.74 (.081)	1735.20
	21 or over	880.97 (.287)	556.58 (.181)	494.52 (.161)	456.64 (.149)	385.16 (.125)	300.41 (.098)	3074.28
Sex								
	Male	1295.47 (.301)	772.92 (.180)	712.08 (.165)	594.02 (.138)	526.5 (.122)	403.83 (.094)	4804.82
	Female	166.03 (.278)	130.97 (.220)	85.79 (.144)	83.76 (.140)	78.99 (.132)	51 (.085)	596.54
Weapon								
	No weapon	1276.97 (.311)	761.73 (.185)	661.84 (.161)	546.15 (.133)	479.8 (.117)	384.22 (.093)	4110.71
	Some weapon	779.18 (.262)	551.98 (.186)	493.45 (.166)	436.55 (.147)	388.61 (.131)	323.13 (.109)	2972.9
Relationship								
	Stranger	1875 (.277)	1273 (.188)	1141 (.168)	979 (.144)	831 (.123)	680 (.100)	6779
	Nonstranger	785 (.319)	462 (.188)	366 (.149)	331 (.135)	291 (.118)	222 (.090)	2457

88. Total number (and percent) of assault incidents by reported month of occurrence prior to month of interview

		1st month	2nd month	3rd month	4th month	5th month	6th month	Total
Race								
	White	885.34 (.311)	540. (.189)	460.88 (.162)	378.95 (.133)	349.45 (.123)	235.35 (.083)	2849.97
	Black/other	365.72 (.331)	198.99 (.159)	148.55 (.134)	148.55 (.134)	121.44 (.110)	94.21 (.085)	1104.58
Age								
	Under 21	486.91 (.346)	266.53 (.190)	229.09 (.163)	165.21 (.118)	153.41 (.109)	104.79 (.075)	1405.94
	21 or over	753.13 (.299)	465.26 (.185)	406.41 (.161)	362.46 (.144)	311.3 (.124)	221.42 (.088)	2519.98
Sex								
	Male	1100.12 (.320)	621.65 (.181)	565.98 (.165)	454.85 (.132)	402.4 (.117)	294.56 (.086)	3439.56
	Female	157.2 (.287)	124.47 (.227)	76.59 (.140)	77.66 (.142)	70.49 (.129)	41 (.075)	547.41
Weapon								
	No weapon	1073.28 (.323)	608.57 (.183)	567.33 (.171)	431.34 (.130)	369.86 (.111)	276.53 (.083)	3326.91
	Total with weapon	564.09 (.184)	402.4 (.192)	344.04 (.164)	283.79 (.136)	268.88 (.129)	199.12 (.095)	2092.32
Relationship								
	Stranger	1420 (.296)	902 (.188)	813 (.170)	657 (.137)	569 (.119)	433 (.090)	4794
	Nonstranger	720 (.324)	428 (.193)	331 (.149)	296 (.133)	261 (.118)	185 (.083)	2221

*U.S. Bureau of Census memorandum, April 4, 1977.

[1]See the selection by H. Woltman, J. Bushery, and L. Carstensen in this chapter.

[2]See R. W. Dodge and A. G. Turner, "Methodological foundations for establishing a national survey of victimization," in Robert G. Lehnen and Wesley G. Skogan (editors), *The National Crime Survey: Working Papers, Volume I: Current and Historical Perspectives*.

[3]Theoretically, a uniform distribution could also result if there were no misreporting of the month of occurrence (i.e., no telescoping) and the proportion of criminal incidents not recalled was constant for each month of recall.

[4]The incident data have been weighted to reflect the probability of selection of the incident. All remaining stages of estimation, inflation by the reciprocal of the probability of selection of the person or housing unit, noninterview adjustments, and ratio estimation, are not reflected in the figures.

[5]O. J. Dunn, "Multiple Comparisons Among Means," *Journal of the American Statistical Association* 56, No. 263, pp. 52-64.

[6]The NCS cities sample and national sample data are not strictly comparable since the cities survey data are collected using a 12-month unbounded reference period while the national sample data is collected using a 6-month bounded reference period.

89. Z-statistic for the estimated difference between the proportion of incidents reported for selected characteristics of the incident by month of recall

	Month of recall					
	1	2	3	4	5	6
Was incident reported to police? (Table 85)						
Total personal crimes	−9.073*	−2.094	0.892	2.689*	3.675*	6.801*
Total crimes of violence	−3.036*	−0.285	−0.307	1.061	0.843	3.128*
Assault	−2.249	0.516	−0.510	1.385	0.260	1.600
Crimes of theft (personal)	−8.304*	−2.573	1.022	2.418	3.741*	6.068*
Household crimes (Table 86)						
Total household crimes	−10.941*	−3.410*	0.561	3.849*	6.148*	7.066*
Larceny (Household)	−6.848*	−2.357	−0.050	2.692*	3.783*	4.719*
Burglary	−7.802*	−2.110	0.814	2.816*	4.089*	4.510*
Motor vehicle theft	0.482	−0.624	−1.108	−0.558	1.093	0.920
Total crimes of violence (Table 87)						
Race (white, black, other)	−0.291	0.854	0.536	−0.729	0.879	−1.425
Age (under 21, 21 or over)	1.990	0.474	0.634	−2.141	−0.512	−1.592
Sex (male, female)	0.934	−1.810	1.135	−0.129	−0.556	0.549
Type of weapon (no weapon, total with weapon)	3.638*	−0.032	−0.453	−1.354	−1.427	−1.693
Victim's relationship (stranger, nonstranger)	−3.280*	−0.022	1.891	0.994	0.451	1.209
Assault (Table 88)						
Race (white, black, other)	−0.999	0.552	0.167	−0.102	0.915	−0.223
Age (under 21, 21, or over)	−2.464	0.309	0.110	−1.928	−1.104	−1.203
Sex (male, female)	1.267	−1.983	1.240	−0.488	−0.623	0.713
Type of weapon (no weapon, total with weapon)	2.458	−0.698	0.476	−0.512	−1.541	−1.221
Victim's relationship (stranger, nonstranger)	−1.949	−0.375	1.836	0.358	0.118	0.813

*Using the multiple comparison procedure described in the text, the difference is considered statistically significant at the 5-percent "experimentwise" level if the z-statistic is less than −2.64 or greater than 2.64. All statistically significant differences have been asterisked (*). As defined in appendix A, the z-statistic is

$$\frac{(P_{1j} - P_{2j})}{(\text{Var } P_{1j} + \text{Var } P_{2j})^{1/2}},$$

where P_{1j} represents the proportion of incidents having characteristic 1 which were reported in the j^{th} month of recall ($j = 1, 2, \ldots, 6$). P_{2j} is similarly defined for incidents having characteristic 2. For example, characteristic 1 may represent incidents reported to the police while characteristic 2 represents incidents not reported to the police.

detected between the proportions of incidents reported in the first month of recall for incidents involving a weapon versus those involving no weapon and those involving a stranger versus those involving a nonstranger. For assault, no statistically significant "experimentwise" differences were detected (see table 89).

Conclusions

These data clearly indicate that the memory biases of recall and telescoping are related to certain characteristics of the incident. However, apart from the mere fact that this is so, the important question is to what extent these differences bias analysis of the NCS data.

In order to answer this question we must recognize that since the NCS estimates are, for the most, based on bounded data, any telescoping of incidents within the 6-month reference has no pronounced biasing effect on the survey estimates. This results because one-sixth of the observations used for a month of occurrence estimate are taken from respondent reports of crimes occurring 5 months ago, and similarly one-sixth from each of 4 months ago, 3 months ago, 2 months ago, and 1 month ago. This means that equal weight is given to the observations over all months of the reference period. Thus, if the differences by month of recall cited above were due entirely to differential net forward telescoping within the reference period, which implies that the amount of recall bias is the same for incidents having different characteristics, we could conclude at least that the household survey technique does not produce differentially valid estimates of the level and nature of criminal victimization (for the types of incidents studied).

If only differential net forward telescoping within the 6-month reference period is operating, then the reason the proportion of incidents not reported to the police is greatest in the first month of recall is simply that more of such incidents which actually occurred 2, 3, 4, 5, and 6 months ago are reported as occurring 1 month ago. That is, the respondent telescopes them forward in time. The more that are reported as occurring in the earlier months of the reference period (1, 2, and 3 months ago), the fewer that can be reported in the later months (4, 5, and 6 months ago). Thus, there is an expected decrease in the proportion of such incidents by month of recall. To the extent the telescoping phenomenon operates differentially for crimes reported versus those not reported to the police, one would expect to observe a different distribution of incidents by month of recall. For this particular characteristic, one might expect more accurate reporting of the month the incident occurred (i.e., less telescoping) if that incident had also been reported to the police. Such a hypothesis would be consistent with the observed data if only telescoping were operating.

On the other hand, if the differences were due entirely to differential recall, then we conclude that the household survey technique does produce differentially valid estimates of the level and nature of criminal victimization—either of these being a finding of extreme importance.

Certainly the research data on household victimization surveying would not support either extreme. Rather it is almost certain that both memory loss and telescoping within the 6-month reference period are associated with the household survey technique and the relationship between the two in the case of the NCS national survey data is, at present, unknown. As such, any definitive analysis of the association of the characteristics of reported incidents and recall bias alone will have to await the results of research directed at the disentangling of recall bias and telescoping within the NCS reference period.

Appendix: Development of the test statistic

A z-test was used to detect statistically significant differences in the two distributions. The test statistic was of the form

$$z = \frac{p_{1j} - p_{2j}}{\sqrt{(\text{Var}_{p_{1j}} + \text{Var}_{p_{2j}})}}$$

$j = 1,2,3,4,5$ or 6

where p_{ij} is the percent of incidents or victimizations reported in the j^{th} month of recall for the i^{th} population.

The value of p_{ij} [$i = 1$ or 2] can be determined from the 2×6 contingency table that displays our data (number of incidents or number of victimizations).

	Months of recall						
Population	1	2	3	4	5	6	Totals
1	O_{11}	O_{12}	O_{13}	O_{14}	O_{15}	O_{16}	n_1
2	O_{21}	O_{22}	O_{23}	O_{24}	O_{25}	O_{26}	n_2
Totals	C_1	C_2	C_3	C_4	C_5	C_6	n

In this table O_{ij} is the number of incidents or victimizations observed for the i^{th} population for the j^{th} month of recall. The total number of incidents or vicitimizations for population 1 is n_1; the total for population 2 is n_2 and $n = n_1 + n_2$. The total number of observations in the j^{th} month of recall is C_j. We now define

$$p_{ij} = \frac{O_{ij}}{n_i} \left(\sum_j p_{ij} = 1\right).$$

The hypothesis tested by the z-statistic is

$$p_{1j} = p_{2j} \text{ for } j = 1, 2, 3, 4, 5 \text{ or } 6.$$

Making the assumptions that

$$V^2_{\hat{o}_{ij}} = a + \frac{b}{O_{ij}} \qquad (1)$$

where a and b are parameters associated with the particular type of vicitimization or incident. Here V^2 denotes the relvariance and

$$V^2_{\left(\frac{O_{ij}}{n_i}\right)} = V^2_{\hat{o}_{ij}} - V^2_{n_i} = b\left(\frac{1}{O_{ij}} - \frac{1}{n_i}\right) \qquad (2)$$

we have that an estimate of $\text{Var}\left(\frac{O_{ij}}{n_i}\right)$ approximated by

$$\left(\frac{O_{ii}}{n_i}\right)^2 b\left(\frac{1}{O_{ii}} - \frac{1}{n_i}\right)$$

The approximate value of b was obtained from the generalized variance estimates calculated for annual 1974 incident estimates. For personal incidents b has the value 1.52, for household incidents b is 1.76, and for personal victimizations b equals 1.45.

Summary of results from the National Crime Survey panel bias study*

by HENRY WOLTMAN and JOHN BUSHERY

Introduction

Our original study of panel bias in the NCS made use of data collected from July 1973 through March 1975.[1] This report makes use of data collected from July 1973 through December 1976.

As part of the panel bias study, we developed a model for the panel bias effect which relied on a panel bias parameter $\alpha_{t,t+1}$ defined by the equation

$$E(\bar{v}_{qm,t+1}) = (1 - \alpha_{t,t+1})\, E(\bar{v}_{qm,t}) \qquad (1)$$

where

$E(\bar{v}_{qm,t}\ )$ = expected victimization rate for a panel interviewed for the t^{th} time in month m, used to make the estimate for quarter of occurrence q.

$E(\bar{v}_{qm,t+1})$ = corresponding expected victimization rate for a panel interviewed for the $(t+1)^{th}$ time.

and $\alpha_{t,t+1}$ = panel bias parameter between panels interviewed for the t^{th} and $(t+1)^{th}$ times. It is assumed that the parameter is constant for all m. The α term is thus the relative difference between the expected victimization rates.

Originally we were able to obtain estimates of $\alpha_{2,3}$; $\alpha_{3,4}$; $\alpha_{4,5}$; and $\alpha_{5,6}$ only. We can now obtain estimates for $\alpha_{6,7}$; $\alpha_{7,8}$; and $\alpha_{8,9}$ as well. Panels were interviewed for the eighth and ninth times only as a consequence of initiating the NCS sample rotation; in the future, each panel will be interviewed seven times. The estimates of $\alpha_{2,3}$; $\alpha_{3,4}$; $\alpha_{4,5}$; and $\alpha_{5,6}$ can now be made with increased precision. The sample size has at least doubled for all estimates except that of $\alpha_{4,5}$, which has increased by 80 percent.

Summary of results

Estimates of $\alpha_{t,t+1}$

The panel bias parameter between panels interviewed t and $(t+1)$ times is usually positive, suggesting that in general, the victimization rate for panels interviewed $(t+1)$ times is lower than the corresponding rate for panels interviewed only t times.

90. Estimated differences in victimization rates for panels interviewed for the t^{th} and $(t+1)^{st}$ time—Crimes against persons

(Rates expressed as number of victimizations reported as occurring in the 6 months prior to the month of interview, per 1,000 persons age 12 and over)

Type of crime	Victimization Rate: Times in sample		Difference between victimization rates: Absolute (per 1,000)	Percent*	95-percent confidence interval on percent difference $100 \times (\alpha_{t,t+1})$
	2	3		(2 times as base)	
a. Total personal crimes	67.21	62.37	4.84	7.2%	3.7 to 10.7**
Crimes of violence	17.81	16.34	1.47	8.3	1.6 to 15.1**
Assault	13.90	12.81	1.09	7.8	0.1 to 15.6**
Personal theft	49.40	46.03	3.37	6.8	2.8 to 10.8**
	3	4		(3 times as base)	
b. Total personal crimes	64.74	62.72	2.02	3.1	−0.9 to 7.1
Crimes of violence	16.40	15.17	1.23	7.5	−0.5 to 15.5
Assault	12.79	11.65	1.14	8.9	−0.1 to 17.9
Personal theft	48.35	47.54	0.81	1.7	−3.1 to 6.5
	4	5		(4 times as base)	
c. Total personal crimes	61.28	60.32	0.96	1.6	−3.2 to 6.4
Crimes of violence	16.00	15.66	0.34	2.1	−7.7 to 11.9
Assault	12.20	11.87	0.33	2.7	−8.3 to 13.7
Personal theft	45.28	44.66	0.62	1.4	−4.4 to 7.2
	5	6		(5 times as base)	
d. Total personal crimes	64.92	65.18	−0.26	−0.4	−6.2 to 5.4
Crimes of violence	16.67	15.85	0.82	4.9	−6.4 to 16.2
Assault	13.02	11.67	1.35	10.4	−1.9 to 22.7
Personal theft	48.25	49.32	−1.07	−2.2	−9.2 to 4.8
	6	7		(6 times as base)	
e. Total personal crimes	63.36	57.04	6.32	10.0	2.3 to 17.8**
Crimes of violence	17.24	14.98	2.26	13.1	−1.7 to 27.9
Assault	12.96	10.98	1.98	15.3	−1.5 to 32.1
Personal theft	46.12	42.06	4.06	8.8	−0.7 to 18.3
	7	8		(7 times as base)	
f. Total personal crimes	62.29	57.81	4.48	7.2	−2.6 to 17.0
Crimes of violence	14.29	13.38	0.91	6.4	−14.6 to 27.4
Assault	11.46	9.73	1.73	15.1	−6.9 to 37.1
Personal theft	48.00	44.44	3.56	7.4	−3.9 to 18.7
	8	9		(8 times as base)	
g. Total personal crimes	57.57	53.20	4.37	7.6	−6.7 to 21.9
Crimes of violence	17.05	12.59	4.46	26.2	3.2 to 49.2**
Assault	13.27	10.11	3.16	23.8	−2.7 to 50.3
Personal theft	40.52	40.61	−0.09	−0.2	−18.5 to 18.1

* Percent differences = $100 \times (\alpha_{t,t+1})$.
** Indicates percent difference is significantly different from zero at 95-percent confidence level.

*Excerpted from U.S. Bureau of the Census memorandum, July 11, 1977.

[1]Henry Woltman and John Bushery, "A panel bias study in the National Crime Survey," paper presented at the annual meetings of the American Statistical Association, (August 1975), Atlanta, Georgia.

For all of the major categories of crimes against persons, the victimization rates for panels interviewed for the third time are significantly lower than the corresponding rates for panels interviewed for the second time (see entry A in table 90). For example, the 95-percent confidence interval for total personal crimes indicates that the rate for panels in sample for the third time is at least 4 percent and at most 10 percent lower than the rate for panels in sample for the second time. Statistically significant differences between victimization rates do not occur for the majority of the other panel comparisons for t and $(t+1)$ times in sample. However, the relative (percent) difference in victimization rates is positive for almost all of these panel comparisons (see entries B–G in table 90).[2]

There is also evidence for total household crimes and for household larceny that the panel bias parameter is positive between panels interviewed for the second time and the third time—indicating that, for these crimes as well, the victimization rates are lower for panels interviewed three times than for panels interviewed two times (see entry A in table 91).

Just as for personal crimes, there are no statistically significant differences for the other panel comparisons. However, in most cases, the estimate of the panel bias parameter ($\alpha_{t,t+1}$) is again positive (see entries B–G in table 91).

Estimates of $\alpha_{2,t}$

The data indicate that the panel bias parameter for panels interviewed for the second time and the t^{th} time is positive for the major personal crime categories (see entries A–E in table 92).

For crimes against households there is also clear evidence of a positive panel bias parameter for total household crimes and household larceny crimes. For the other types of household crimes the estimates of $\alpha_{2,t}$, although not statistically significant, are nevertheless usually positive (see entries A–E in table 93).

[2]Compare the victimization rates shown in entries A–G of table 90 for panels in sample for the third time. These rates are different (62.37 and 64.74, respectively) because each refers to a different time period, so the two rates could have different expected values. This statement applies to similar situations in all of table 90, entries A–E, and entries A–G of table 91. In table 92, entries A–E, and table 93, entries A–E, the estimated victimization rates for panels in sample for the second time also vary for the two reasons given above.

91. Estimated differences in victimization rates for panels interviewed for the t^{th} and $(t+1)^{st}$ time—Crimes against households

(Rates expressed as number of victimizations reported as occurring in the 6 months prior to the month of interview, per 1,000 households)

Type of crime	Victimization rate: Times in sample		Difference between victimization rates: Absolute (per 1,000)	Percent	95-percent confidence interval on percent difference $100 \times (\alpha_{t,t+1})$
	2	3		(2 times as base)	
a. Total household crimes	121.94	115.06	6.88	5.6%	1.4 to 9.9**
Burglary	47.83	46.43	1.40	2.9	−4.4 to 10.21
Household larceny	64.59	59.75	4.84	7.5	1.8 to 13.3**
Motor vehicle theft	9.52	8.89	0.63	6.6	−9.2 to 22.4
	3	4		(3 times as base)	
b. Total household crimes	114.44	115.98	−1.54	−1.3	−6.3 to 3.7
Burglary	44.55	46.17	−1.62	−3.6	−12.1 to 4.9
Household larceny	60.28	60.85	−0.57	−0.9	−8.2 to 6.4
Motor vehicle theft	9.61	8.97	0.64	6.7	−10.8 to 24.2
	4	5		(4 times as base)	
c. Total household crimes	119.13	115.34	3.79	3.2	−2.3 to 8.7
Burglary	47.31	44.97	2.34	4.9	−4.1 to 13.9
Household larceny	62.85	62.27	0.58	0.9	−7.1 to 8.9
Motor vehicle theft	8.98	8.10	0.88	9.8	−10.5 to 30.1
	5	6		(5 times as base)	
d. Total household crimes	114.37	112.14	2.23	1.9	−5.1 to 8.9
Burglary	45.76	42.66	3.10	6.8	−4.2 to 17.8
Household larceny	59.39	60.02	−0.63	−1.1	−11.4 to 9.2
Motor vehicle theft	9.21	9.46	−0.25	−2.7	−29.5 to 24.1
	6	7		(6 times as base)	
e. Total household crimes	118.85	115.72	3.13	2.6	−6.9 to 12.1
Burglary	46.83	45.43	1.40	3.0	−12.8 to 18.8
Household larceny	62.15	60.61	1.54	2.5	−11.3 to 16.3
Motor vehicle theft	9.86	9.68	0.18	1.8	−33.7 to 37.3
	7	8		(7 times as base)	
f. Total household crimes	107.31	104.40	2.91	5.0	−9.8 to 15.2
Burglary	44.17	39.85	4.32	9.8	−9.2 to 28.8
Household larceny	54.30	56.77	−2.47	−4.5	−23.5 to 14.5
Motor vehicle theft	8.83	7.77	1.06	12.0	−30.3 to 54.3
	8	9		(8 times as base)	
g. Total household crimes	116.16	112.28	3.88	3.3	−13.2 to 19.8
Burglary	44.65	44.64	0.01	0.0	−28.5 to 28.5
Household larceny	63.22	58.87	4.35	6.9	−15.6 to 29.4
Motor vehicle theft	8.30	8.77	−0.47	−5.7	−76.0 to 64.6

*Percent differences = $100 \times (\alpha_{2,t})$
**Indicates percent difference is significantly different from zero at 95-percent confidence level.

Perhaps more importantly, for total personal and household crimes, the point estimates of the panel bias parameters ($\alpha_{2,t}$) suggest even more strongly than did the estimates in tables 90 and 91 that fewer and fewer crimes are reported each time a panel is interviewed. There is no point in the "aging" process at which panel bias reaches a maximum and tends to remain constant thereafter (at least for the major crime categories and a design involving seven visits).

Simultaneous comparison of victimization rates for several panels

In the original study comparisons were limited to those panels interviewed t and $t+1$ times. At that time, no data were available such that several time-in-sample panels could be compared for the same time period. Now such data are available. Panels in sample for the second, third, fourth, fifth and seventh times were interviewed in the period January–June 1976. Table 94 illustrates the decrease in victimization rates that occurs as panels age. Since the purpose of table 94 is merely to illustrate for a single time period the decrease in victimization rates reported by panels as they age and because these estimates were obtained from relatively small samples, no relative differences between the victimization rates of panels have been estimated and no standard errors have been presented.

One can obtain some idea of the bias introduced into the NCS estimates by repeated interviewing if one compares the victimization rates for all five of the panels combined with the rates for the panel interviewed for the second time. The absolute and relative differences between these two sets of victimization rates illustrate the effect of panel bias on the NCS estimates (see table 94).

Effect of time-in-sample on noninterview rates

It appears that the type A noninterview rate increases slightly with the number of times a panel has been interviewed. This rate steadily increases from a low of about 3.9 percent for a panel interviewed for the second time to 4.6 percent for a panel interviewed for the seventh time (see table 94).

On the other hand, the type Z noninterview rate appears to be unaffected by the number of times a panel has been interviewed, since there is no substantial increase in the type Z rate as the panel ages.

92. Estimated differences in victimization rates for panels interviewed for the second time and the t^{th} time—Crimes against persons

(Rates expressed as number of victimizations reported as occurring in the 6 months prior to the month of interview, per 1,000 persons age 12 and over)

Type of crime	Victimization rate: Times in sample		Difference between victimization rates: Absolute (per 1,000)	Percent* (2 times as base)	95-percent confidence interval on percent difference $100 \times (\alpha_{2,t})$
	2	3			
a. Total personal crimes	67.21	62.37	4.84	7.2%	3.7 to 10.7**
Crimes of violence	17.18	16.34	1.47	8.3	1.6 to 15.1**
Assault	13.90	12.81	1.09	7.8	0.1 to 15.6**
Personal theft	49.40	46.03	3.37	6.8	2.8 to 10.8**
	2	4			
b. Total personal crimes	70.95	62.01	8.94	12.6	7.6 to 17.6**
Crimes of violence	18.16	15.24	2.92	16.1	6.1 to 26.1**
Assault	14.32	11.85	2.47	17.2	6.2 to 28.2**
Personal theft	52.79	46.77	6.02	11.4	5.4 to 17.4**
	2	5			
c. Total personal crimes	71.62	62.52	9.10	12.7	6.7 to 18.7**
Crimes of violence	18.16	16.00	2.16	11.9	– 0.6 to 24.4
Assault	14.28	12.00	2.28	16.0	2.3 to 29.8**
Personal theft	53.46	46.52	6.94	13.0	5.8 to 20.3**
	2	6			
d. Total personal crimes	76.48	66.46	10.02	13.1	6.1 to 20.1**
Crimes of violence	20.38	15.22	5.16	25.3	12.8 to 37.8**
Assault	15.34	10.98	4.36	28.4	14.4 to 42.4**
Personal theft	56.10	51.23	4.87	8.7	– 0.1 to 17.5
	2	7			
e. Total personal crimes	69.59	57.04	12.55	18.0	10.3 to 25.8**
Crimes of violence	18.15	14.98	3.17	17.5	2.0 to 33.0**
Assault	14.42	10.98	3.44	23.9	7.7 to 40.2**
Personal theft	51.44	42.06	9.38	18.2	9.2 to 27.2**

*Percent differences = $100 \times (\alpha_{2,t})$
**Indicates percent difference is significantly different from zero at 95-percent confidence level.

Conclusions

Since for many types of crime there is evidence of panel bias, it is clear from the standpoint of reducing the bias on estimates of level that panels should be retained in sample for a shorter period of time; that is, fewer interviews should be conducted before a panel is rotated out of sample. Research is currently underway to determine the optimum length of time to retain a panel in sample taking into consideration the cost factors as well as the impact on estimates of change. The results of the research will be presented in a subsequent report.[3]

[3]See Henry Woltman and John Bushery, "Results of a study to determine the optimum time to retain a panel in sample," in chapter 5 of this volume.

93. Estimated differences in victimization rates for panels interviewed for the second time and the t^{th} time—Crimes against households

(Rates expressed as number of victimizations reported as occurring in the 6 months prior to the month of interview, per 1,000 households)

Type of crime	Victimization rate: Times in sample		Difference between victimization rates: Absolute (per 1,000)	Percent*	95-percent confidence interval on percent difference $100 \times (\alpha_{2,t})$
	2	3			
a. Total household crimes	121.94	115.06	6.88	5.6%	1.4 to 9.9**
Burglary	47.83	46.43	1.40	2.9	– 4.4 to 10.2
Household larceny	64.59	59.75	4.84	7.5	1.8 to 13.3**
Motor vehicle theft	9.52	8.89	0.63	6.6	– 9.2 to 22.4
	2	4			
b. Total household crimes	128.17	118.64	9.53	7.4	1.2 to 13.7**
Burglary	48.24	47.03	1.21	2.5	– 8.5 to 13.5
Household larceny	70.65	62.78	7.87	11.1	2.6 to 19.6**
Motor vehicle theft	9.27	8.83	0.44	4.7	– 20.6 to 30.0**
	2	5			
c. Total household crimes	125.81	114.88	10.93	8.7	1.0 to 16.5**
Burglary	47.40	43.64	3.76	7.9	– 5.1 to 20.9
Household larceny	69.10	63.96	5.14	7.4	– 3.4 to 18.2
Motor vehicle theft	9.32	7.28	2.04	21.9	– 4.6 to 48.4
	2	6			
d. Total household crimes	119.67	108.81	10.86	9.1	– 0.4 to 18.6
Burglary	48.11	43.41	4.70	9.8	– 5.7 to 25.3
Household larceny	62.08	56.72	5.36	8.6	– 5.2 to 22.4
Motor vehicle theft	9.48	8.68	0.80	8.4	– 27.9 to 44.7
	2	7			
e. Total household crimes	132.93	115.72	17.21	12.9	3.7 to 22.2**
Burglary	49.96	45.43	4.53	9.1	– 7.2 to 25.4
Household larceny	73.79	60.61	13.18	17.9	5.7 to 30.2**
Motor vehicle theft	9.17	9.68	– 0.51	– 5.6	– 49.1 to 37.9

*Percent differences = $100 \times (\alpha_{2,t})$
**Indicates percent difference is significantly different from zero at 95% confidence level.

94. Victimization rates for persons and households interviewed in the period January–June 1976 (per 1,000 persons age 12 and over or 1,000 households)

Type of crime	Times in sample*: 2	3	4	5	6**	7	Combined rate	Difference between rate for two times and combined rate: Absolute	Percent
Total personal crimes	73.92	69.00	65.65	64.85	—	62.29	67.12	6.80	9.2
Crimes of violence	18.11	17.11	16.75	16.04	—	14.29	16.46	1.65	9.1
Assault	14.19	13.89	12.39	11.57	—	11.46	12.69	1.50	10.6
Personal theft	55.81	51.90	48.90	48.81	—	48.00	50.66	5.15	9.2
Total household crimes	118.51	119.16	118.01	112.86	—	107.31	115.15	3.36	2.8
Burglary	44.38	44.10	48.09	39.73	—	44.17	44.10	0.28	0.6
Household larceny	64.35	65.14	61.62	65.60	—	54.30	62.19	2.16	3.4
Motor vehicle theft	9.78	9.92	8.30	7.53	—	8.83	8.87	0.91	9.3
Persons interviewed	22,416	22,390	22,923	22,823	—	22,605	113,157		
Households interviewed	9,915	9,978	10,126	10,092	—	10,074	50,185		
Type-Z noninterview rate	1.77	1.90	1.78	1.79	—	1.82	1.81		
Type-A noninterview rate	3.91	4.21	4.34	4.47	—	4.60	4.31		

* Data for each time-in-sample category was obtained from two rotation groups.
** There were no rotation groups in sample for the sixth time during this period.

Chapter 5

Response rate analysis

Introduction

Both this chapter and the next contain several rather brief and technical selections which were memoranda or sections of memoranda. They have been included since they have been important in the history of the National Crime Survey (NCS) and they document a concern for improving the operation of the NCS. This chapter reflects some of the efforts of the Bureau of the Census to improve the accuracy of the victimization survey by its attention to response rates and sample coverage.

The paper by Lawrence T. Love and Anthony G. Turner examines the noninterview rates of NCS and other Census Bureau household surveys and reviews techniques which may contribute to the relatively high completion rates of these surveys. John Bushery's paper examines the effects of time-in-sample on the type of noninterview. The paper by Cynthia Wilder and Masato Asanuma examines the coverage ratios of NCS of 68 age-sex-color categories in comparison to the Current Population Survey (CPS). The fourth paper in in this group, by Irene Montie, David Bateman, Michael Tenebaum, and John Bushery, reports on a study of the effectiveness of control card procedures in the NCS and CPS for identifying eligible respondents in the household.

Respondent availability and response rates*

by LAWRENCE T. LOVE and ANTHONY G. TURNER

There is increasing concern among survey researchers, particularly in the private sector, over their ability to secure the cooperation of the citizenry in carrying out an ever-growing number of household surveys. The question arises as to what has been happening to response rates in social surveys over the past few years, and specifically whether the public is opting to refuse to participate in surveys more so now than in the past.

Since the Census Bureau is the principal data collection agency for the Federal Government, examining the Bureau's experience with public cooperation in its surveys could be illuminating. We have chosen to look at this issue by focusing on five household surveys which the Bureau of the Census conducts. Each of these surveys is characterized by certain elements of commonality: participation in each is voluntary, the principal mode of collection is by personal interview, the respondent universe consists of either households or individuals, the data produced are national in scope (though local data are also produced in some instances), and the surveys are large-scale programs with sizable samples and complex designs. The surveys were also chosen because of their differences—they differ in the nature and scope of the information collected and in the length and frequency of the interviews. Two of the surveys are continuing, monthly collection efforts which have been carried on by the Census Bureau for many years—the Current Population Survey and the Health Interview Survey. The other three are surveys which have come about during the decade of the 1970's—the NCS, the Consumer Expenditures Survey, and the Annual Housing Survey. The general discussion in this paper will refer to our examination of response rates for all five surveys, but only data from the NCS will be presented. For purposes of this dicussion, nonresponse is defined as failure to secure an interview for a unit which is eligible for interview. The Bureau collects information about other types of noninterviews, such as vacant households and other sampled units otherwise out-of-scope for the survey, but these noninterview types are excluded from this presentation. Where available, we will present data specifically on refusal rates. Refusals are particularly germane to the issue of citizen cooperation. Other important nonresponse categories included in the overall rates are lack of respondent availability and vacation households. To avoid dealing with the effects of seasonality in response rates, we have assembled the available data in terms of annual averages or totals.

Table 95 displays the nonresponse rates for the NCS from 1972 to 1975. Nonresponse rates for households have been 4 percent or less on an annual basis since 1973. In the NCS, as in the Current Population Survey, there does appear to be a gradual increase in refusals which has been counterbalanced by an improvement in response rates for the category, "respondent unavailable," though the cumulative evidence to date is too thin to assert a trend.

Nonresponse for sampled persons eligible for interview within interviewed households has been very low, generally under 1.5 percent. We believe that one reason for this is that once the interviewer gains access to the household, it is fairly easy to secure an interview with the individual members by, for example, scheduling a convenient time for callbacks for those who are not at home at the time. In addition, many of these callbacks are completed by telephone interview. The NCS panel design requires that repeat interviews be conducted with occupants of the same housing units every 6 months. Further collection experience will have to be gained before we can assess the effects of repeat observations on nonresponse over the long term.

The Bureau of the Census generally has had considerable success in achieving high response rates for the NCS as well as for the other personal interview household surveys. The question arises as to what do we attribute these response rates which are consistently higher than 90 percent. First, of course, the Bureau of the Census has very elaborate procedures for keeping nonresponse to a minimum. These procedures will be discussed later in this paper.

Aside from our own zeal in followup and other field procedures, however, public reaction to the Census Bureau's surveys may have been a significant factor. Though we do not have any quantifiable evidence, various experts around the Bureau of the Census have advanced several reasons for the past citizen cooperation in our surveys. Some of these reasons are as follows: First, the Bureau of the Census is known by the citizenry and its work has not been suspect in the past. Second, there is the possibility that citizens have felt it was their implicit duty to cooperate with the Government representative even though most of our nondecennial surveys are voluntary and the respondents are informed of this fact. Third, the importance of many of our survey results are demonstrated to the public, frequently by headlined newspaper articles (for example, the monthly unemployment statistics).

Aside from the public's perception of the value of Census Bureau surveys, a number of steps are taken to help attain high response rates:

- During interviewer training, we stress the importance of obtaining public cooperation and explain ways of answering the questions put forth by reluctant respondents. The latter are often discussed during periodic refresher group training sessions by experienced interviewers, many of whom have a special talent for eliciting respondent cooperation. New interviewers in particular can profit by such exposure. Beyond the training, however, we continually stress the importance of obtaining high response rates and we routinely compare (and report) individual interviewer response rates with established standards.
- Periodically, each interviewer is observed while working and subsamples of assigned households are reinterviewed by supervisors. These measures also provide further information that can be used to help individual interviewer performance in the area of gaining respondent cooperation.
- Advance letters are sent when possible to the occupants of our sample addresses informing them about the nature and purpose of the survey and telling them how the information they provide is to be used.
- Studies are conducted to establish the optimum times at which interviewers can expect to find an eligible respondent at home. With more multiple-worker households and more people holding more than one job, the probability of finding the requisite person at home decreases and varies from survey to survey. The most recent study which addresses the issue of optimum hours of the day for conducting interviews is described in the Census Bureau's Working Paper No. 37 entitled "Who's Home When," issued in 1973.

*Excerpted from a paper presented to the annual meeting of the American Statistical Association, Atlanta, Georgia, August 1975.

The paper provides approximations of the best times for finding a respondent at home for a wide variety of ages, sex, and racial/ethnic groups. This kind of information can be of great help to interviewers in determining the most productive survey work hours.

- Because the time constraints of many of our interview periods are so rigid we encourage interviewers to determine the best time to visit households that are to be interviewed more than once, and gather information by telephone from designated respondents who were not at home at the time of the interviewer's visit.
- Interviewers are also required to notify their supervisors about each household refusing to participate in our surveys. These households are subsequently contacted by a regional office staff member either by personal visit, by telephone, or by correspondence. The households are given additional information about the survey and how the data will be used, and the importance of their cooperation is emphasized. If the staff member writes to the households, he informs them that a named interviewer will call again on a specified date and he gives them the regional office telephone number which they may call if they would like to know more about the survey.

We do not have precise figures to show how response rates can be improved by these actions: indeed, some do not lend themselves to definitive analysis. We simply regard them as necessary to the successful conduct of our surveys.

The Census Bureau has some indication that public resistance to household surveys is increasing slightly. The Current Population Survey and the Health Interview Survey are experiencing somewhat higher refusal rates at present than was encountered during the 1960's. Until now, we have been looking at the response rates, but another aspect of our household surveys that we have not mentioned deals with the issue of difficult respondents. This is less easily quantified. We are getting, from our survey managers, an increasing number of reports that the interviewer's job has required more and more "salesmanship" during recent years because the public has become more cautious about the Government. How much public distrust of the Federal Government has filtered down to Census Bureau household surveys is difficult to measure but we do find that the "in-house" interview time has increased in the 1970's and that interviewer fatigue and disillusionment seem to be contributing to an increasing interviewer turnover rate.

We have also found that respondents are becoming increasingly more difficult to find at home (the influx of women into the labor force contributed to this problem); this can pose serious problems for household surveys unless special attention is paid to interviewer work habits. On balance, however, the response rate which household surveys achieve is probably more sensitive to the characteristics of the interview itself than to a lack of cooperation.

First, complex and lengthy interviews make the attainment of high response rates more difficult. We believe, for example, that the Consumer Expenditures Surveys clearly encountered more resistance than other surveys because of the length of the interview and detailed questions.

Second, the number of times a household is interviewed affects cooperation. The Current Population Survey and the quarterly Consumer Expenditures Survey involve eight and five interviews per unit, respectively, and both have higher refusal rates than does the Health Interview Survey. The NCS panel has not been conducted long enough to determine the effects of repeated interviews on its response rate.

Third, the time allotted for data collection combined with the experience of the interviewing staff has an important influence on response rates. The amount of time interviewers need to spend with reluctant respondents depends, in part, on interviewer experience. The amount of time needed to find people at home depends both on the amount of time available for callbacks and on whether or not repeated interviews afford interviewers with an opportunity to establish the best time to call.

95. Nonresponse rates for the National Crime Survey

Category	Year I* (1972)		Year II (1973)		Year III (1974)		Year IV (1975)	
	Number of units	Percent	Number of units	Percent	Number of units	Percent	Number of units	Percent
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Sample households eligible for interview	30,083	100.0	135,264	100.0	122,691	100.0	67,586	100.0
Response	28,673	95.3	130,351	96.4	118,348	96.5	64,904	96.0
Nonresponse	1,410	4.7	4,913	3.6	4,343	3.5	2,682	4.0
Refusal	NA	NA	1,975	1.5	2,163	1.8	NA	NA
Respondent unavailable	NA	NA	2,561	1.9	1,907	1.6	NA	NA
Other	NA	NA	377	0.3	273	0.2	NA	NA
Sample persons** eligible for interview within interviewed households	67,324	100.0	303,413	100.0	272,648	100.0	149,502	100.0
Response	66,356	98.6	299,659	98.8	268,928	98.6	147,126	98.4
Nonresponse	968	1.4	3,754	1.2	3,720	1.4	2,376	1.6

Note: The NCS is conducted monthly, in about 10,000 households, to provide data on the extent and nature of criminal victimizations, focusing on crimes of violence and common theft. Detailed information includes socioeconomic condition and demographic makeup of the victim population and circumstances surrounding the criminal incidents. The sample utilizes a rotating panel design, with repeat interviews in the same units every 6 months.
* Survey started in July 1972; counts are for the last 6 months. Sample for 1972 was about 5,000 households per month.
** Information about certain crimes (generally, those involving personal confrontation) is collected through a self-respondent approach, that is, all adult individuals in the sample household are interviewed for themselves. Within interviewed households individual members may not respond for various reasons.
NA = Not available.

National Crime Survey noninterview rates by time-in-sample*

by JOHN M. BUSHERY

Introduction

This report examines the relationship between particular kinds of type A noninterviews (as defined by the reason for the noninterview) and the length of time the panel-rotation is retained in sample.

Four reasons for type A noninterviews are recorded:

(1) No one home—the interviewer did not find any respondents at home, despite repeated visits and/or telephone calls.

(2) Temporarily absent—the interviewer determined that all household members would be absent during the interview period, for example, on vacation.

(3) Refused—the household member contacted by the interviewer refused to allow an interview to be conducted.

(4) Other—this is a catchall category and includes such circumstances as impassable roads and quarantined households.

Of these four reasons for type A noninterviews, refusal by the respondent is most prevalent and is also the one most likely to be influenced by repeated interviewing of the same household. Thus, this analysis will concentrate on the relationship between the refusal rate and time-in-sample.

Comparisons of these noninterview rates will be made between panel-rotations in sample for the second time and in panel-rotations in sample for the third, fourth, fifth, sixth, and seventh times. Ideally, an analysis of noninterview rates by time-in-sample should use the noninterview rates of panel-rotations in sample for the first time as the base for the comparisons. However, this analysis has used the rates of panel-rotations in sample for the second time as the base because first-time panel-rotations are not included in the regular NCS data files which were used in this analysis.[1]

In addition, a separate analysis of the relationship between the refusal rate and time-in-sample will be performed using three data sets representing a range of time-in-sample categories from the second time in sample through the ninth time in sample.

Table 96 shows the NCS samples and rotation groups used in the noninterview rate comparisons. The three data sets used to examine the relationship between the refusal rate and time-in-sample were obtained in interviews conducted from July through December 1975, from January through June 1976, and from July through December 1976.

96. Composition and collection periods of data used for time-in-sample comparisons

Time-in-sample comparison	Number of times in sample	Composition sample:rotation	Number of times in sample	Composition sample:rotation	Time period of data collection	
2 versus 3	2	JO2:1,2,3,4,5,6 JO3/4:2 3 4 5	3	JO1:1,2,3,4,5,6 JO3/4:1 2 3 4	July–Dec. Jan.–June July–Dec. Jan.–June July–Dec.	1973 1975 1975 1976 1976
2 versus 4	2	JO3/4:3 4 5	4	JO3/4:1 2 3	July–Dec. Jan.–June July–Dec.	1975 1976 1976
2 versus 5	2	JO3/4:4 5	5	JO3/4:1 2	Jan.–June July–Dec.	1976 1976
2 versus 6	2	JO3/4:2	6	JO1:3,4,5,6	Jan.–June	1975
2 versus 7	2	JO3/4:3	7	JO1:4,5,6	July–Dec.	1975

Development of the estimates

The type A noninterview rate is defined as

$$R_A = \frac{N_A}{N_A + N_I} \times 100$$

where N_A = the number of households with a type A noninterview;
N_I = the number of households interviewed.

The rate for each of the four kinds of type A noninterview can be computed as shown below for the refusal rate

$$R_{\text{refusal}} = \frac{N_{\text{refusal}}}{N_A + N_I} \times 100$$

where N_{refusal} = the number of households which refused to be interviewed.

The difference between the noninterview rates of panel-rotations in sample for the second time and the t-th time is given by

$$\Delta(2,t) = R(2) - R(t)$$

where $R(2)$ is the rate for panels in sample for the second time and $R(t)$ is the rate for panels in sample for the t-th time (with $t=3,4,5,6,7$).

Reliability of the estimates

In this report, the variance of a noninterview rate has been estimated as follows:

$$\text{Var}(R) = 1.2(R)(100-R)/N$$

where $N = N_A + N_I$ as defined above and the number 1.2 represents an approximate design effect for type A noninterviews.

The estimated variance of the difference between the rates of panel-rotations in sample for the second time and the t-th time has been computed as

$$\text{Var}(\Delta(2,t)) = \text{Var}(R(2)) + \text{Var}(R(t))$$

Note that we have assumed the correlation between the two noninterview rates to be zero. However, this may not always be true; in fact, there may be at least some positive correlations. Consequently, estimated variances may be overstated to some extent.

Summary of results

Refusal rates

Table 97 provides evidence that panels retained in sample more than three times experience higher refusal rates than panels in sample for the second time. In each of these comparisons, a one-sided z-test reveals that the refusal rate for panels in sample for the fourth, fifth, sixth, or seventh times is higher than that for panels in sample for the second time.

*U.S. Bureau of the Census memorandum, May 19, 1978.

[1]Data from first-time panel-rotations are available in some special data files used in studying the effects of bounding in the NCS.

97. Comparison between type-A noninterview rates for panel-rotations in sample by reason for type-A noninterview

Two versus three times in sample:

Interview status	Noninterview rate (percent): 2 times in sample	3 times in sample	Difference in rates*	Standard error of difference
[1]	[2]	[3]	[4]	[5]
Total households	75,707	75,450		
Type A-noninterview rate	3.70	3.82	−0.12	0.11
No one home	0.85	0.88	−0.03	0.04
Temporarily absent	0.91	0.86	0.05	0.04
Refused	1.66	1.80	−0.13	0.08
Other	0.28	0.28	0.00	0.03

Two versus four times in sample:

Interview status	Noninterview rate (percent): 2 times in sample	3 times in sample	Difference in rates	Standard error of difference
[1]	[2]	[3]	[4]	[5]
Total households	31,604	31,662		
Type-A noninterview rate	4.11	4.11	0.01	0.17
No one home	0.88	0.78	0.10	0.08
Temporarily absent	0.96	0.83	0.13	0.08
Refused	1.96	2.26	−0.30**	0.13
Other	.31	.23	0.08**	0.04

Two versus five times in sample:

Interview status	Noninterview rate (percent): 2 times in sample	5 times in sample	Difference in rates	Standard error of difference
[1]	[2]	[3]	[4]	[5]
Total households	21,145	21,129		
Type-A noninterview rate	4.11	4.46	−0.35	0.21
No one home	0.84	0.74	0.10	0.09
Temporarily absent	0.95	0.86	0.08	0.10
Refused	2.01	2.55	−0.54**	0.16
Other	0.31	0.31	0.00	0.06

Two versus six times in sample:

Interview status	Noninterview rate (percent): 2 times in sample	6 times in sample	Difference in rates	Standard error of difference
[1]	[2]	[3]	[4]	[5]
Total households	10,376	20,826		
Type-A noninterview rate	3.44	4.33	−0.89**	0.25
No one home	0.85	0.80	0.05	0.12
Temporarily absent	0.74	0.82	−0.07	0.11
Refused	1.55	2.42	−0.86**	0.18
Other	0.30	0.30	0.00	0.07

Two versus seven times in sample:

Interview status	Noninterview rate (percent): 2 times in sample	7 times in sample	Difference in rates	Standard error of difference
[1]	[2]	[3]	[4]	[5]
Total households	10,459	15,783		
Type-A noninterview rate	4.12	4.46	−0.34	0.28
No one home	0.96	0.92	0.04	0.13
Temporarily absent	0.98	0.75	0.23	0.13
Refused	1.86	2.52	−0.66**	0.20
Other	0.32	0.27	0.05	0.07

* Due to rounding, the difference between rates shown in columns [2] and [3] may not agree exactly with the difference shown in column [4].
** Significant at the 5-percent level.

It appears that there may be an increasing relationship between the length of time a panel has been retained in sample and the refusal rate. Table 98 shows that for the five time-in-sample comparisons, the magnitude of the difference between the refusal rate for panels in sample for the second time and panels in sample for the t-th time tends in general to increase with t.

Unfortunately the data in table 98 are not amenable to an analysis of the relationship between time-in-sample and the refusal rate for two reasons. In the first place these data are correlated to some extent, and in the second place, each time-in-sample comparison involves a different calendar time period. Each of these factors could confound our analysis. However, the problems of correlated data and of comparisons over different time periods can be avoided by analyzing the refusal rates for rotation groups interviewed within a given 6-month period. This solution has one shortcoming for the data used in this analysis: there is no 6-month time period for which the complete range of time-in-sample categories (from two through seven times in sample) is available.[2]

For this reason, three different data sets collected in July–December 1975, January–June 1976, and July–December 1976 have been analyzed. Each data set includes

[2]The data were collected in January 1977 and later include all six time-in-sample categories from two through seven times in sample.

most of the time-in-sample categories of interest, but lacks one of the categories. The first data set does not provide noninterview or refusal rates for panel-rotations in sample for the fifth time. The second data set provides no estimates for panel-rotations in sample for the sixth time and the third data set provides no estimates for panel-rotations in sample for the seventh time.

Since for each data set the missing time-in-sample category is different, the three data sets should provide an adequate picture of the relationship between time-in-sample and the refusal rate over the full spectrum of time-in-sample categories. However, no comparisons between data sets can be made if the complications of correlation among time-in-sample categories are to be avoided.

Panel-rotations in sample for more than seven times have been included in this analysis since a tendency for the refusal rate to increase as time-in-sample increases should continue beyond the seventh time in sample.

Two tests have been performed to detect evidence of an upward trend in the refusal rate. In the first test, the Spearman rank correlation coefficient (r_s) has been computed for each data set [see table 99, column (6)]. The Spearman rank correlation coefficient requires no assumptions about the distribution of the refusal rate and can be used in a nonparametric test of the null hypothesis that there is no association between the refusal rate and time-in-sample against the alternative hypothesis that there is a monotonically increasing (though not necessarily linear) relationship between the refusal rate and time-in-sample.

Thus if chance alone were operating, the probability of obtaining a value of r_s as large as or larger than the $r_s = 0.90$ computed from the first data set, which has five time-in-sample categories, would be 5/120 or 4.2 percent. Similarly the probability of obtaining values of r_s as large as or larger than those computed from the second and third data sets would be less than 1 percent.

This is conclusive evidence that there is a positive correlation between the number of times a panel-rotation is in sample and the level of the refusal rate.

Another test used in this analysis is due to Bartholomew. In this test the null hypothesis

$H_o : R_2 = R_3 = \ldots = R_{max}$

is tested against the alternative

$H_2 : R_2 \leq R_3 \leq \ldots \leq R_{max}$

98. Difference in refusal rates and type-A noninterview rates by time in sample

Time-in-sample comparison	Difference in refusal rates	Standard error	Difference in type-A rates	Standard error
2 versus 3	−0.13	0.08	−0.12	0.11
2 versus 4	−0.30*	0.13	0.01	0.17
2 versus 5	−0.54*	0.16	−0.35	0.21
2 versus 6	−0.86*	0.18	−0.89*	0.25
2 versus 7	−0.66*	0.20	−0.34*	0.28

* Significant at the 5-percent level.

99. Values of the Spearman rank correlation coefficient for type-A noninterview rates by reason for noninterview

Data collections period	Critical value of r_s	Total Type A*	No one home**	Temporarily absent**	Refused*	Other**	Total for nonrefused**
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
1975							
July–December	0.90	+0.50	−0.60	−0.30	+0.90***	−0.40	−0.70
1976							
January–June	0.83	+1.00***	+0.66	−0.66	+1.00***	−0.60	−0.31
July–December	0.71	+0.79***	0.00	−0.18	+0.96***	+0.14	−0.14

* The computed value of r_S must be greater than or equal to critical value for a one-sided test to justify rejection of H_0: no association between rate and time in sample in favor of H_1: monotonic increasing relationship between rate and time in sample.
** The computed value of r_S must be less than or equal to the negative of the critical value for a one-sided test to justify rejection of H_0: no association between rate and time in sample in favor of H_1: monotonic decreasing relationship between rate and time in sample.
*** Significant at the 5-percent level (one-sided test).

where $R_t < R_{t+1}$ for at least one value of t rather than the more general hypothesis usually employed

$H_1 : R_t \neq R_{t'}$, for some t and t'

In these hypotheses, R_t is the refusal rate for panel-rotations in sample for the t-th time and the strict inequality $R_t < R_{t+1}$ is required for at least one value of t to distinguish H_2 from H_o.

This test requires all the assumptions used in analysis of variance in addition to the ordering of refusal rates stated in H_2. The test statistic is the ratio of the sum of squared differences between time-in-sample categories to the total sum of squared differences:

$\bar{E} = SS_B/SS_T$

For values of $\bar{E}$ larger than the critical value, H_o is rejected in favor of H_2.

The test statistics for each data set indicate that H_o should be rejected. The critical values of $\bar{E}$ at the 1-percent level are much smaller than 0.2. Since the computed values of $\bar{E}$ for the three data sets are 0.872, 0.857, and 0.714, respectively, the hypotheses of equal refusal rates over time-in-sample can be safely rejected. In fact, the probability of incorrectly rejecting H_o is less than 10^{-8}.

In order to perform this test, it is necessary that the sample values of the refusal rates be ordered as stated in H_2. Since in the first and third data sets this does not occur, some of the time-in-sample categories must be collapsed to agree with H_2. For example, in the first data set we have

$\hat{R}_2 < \hat{R}_3 < \hat{R}_4 < \hat{R}_7 < \hat{R}_6$ (see table 100)

so we have combined the panel-rotations producing $\hat{R}_7$ and $\hat{R}_6$ and we now have

$\hat{R}_2 < \hat{R}_3 < \hat{R}_4 < \hat{R}_{6+7}$

Thus, the hypothesis being tested is

$H_o : R_2 = R_3 = R_4 = R_{6+7}$

against the alternative

$H_2 : R_2 \leq R_3 \leq R_4 \leq R_{6+7}$

Despite the need to combine categories this does indicate a tendency for the refusal rate to increase the longer a panel-rotation is retained in sample. Similarly in the third data set, the panel-rotations in sample for the second and third times were combined to fulfill the assumptions of the test.

It must also be pointed out that a small probability of erroneously rejecting H_o does not provide protection against the possibility that some other ordering of refusal rates may be operative. For example, in the second data set we may actually have

$R_2 < R_3 < R_5 < R_4 < R_7 < R_8$

or some other ordering of the refusal rates by time-in-sample. However, the values of the Spearman rank correlation coefficient and the results of Bartholomew's test taken together provide evidence of the trend for the refusal rate to increase with time-in-sample.

100. Type-A noninterview rates, by reason, by time in sample for data collected in July–December 1975; January–June 1976; and July–December 1976

Data collection period	Times in sample								
	2	3	4	5	6	7	8	9	Overall
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	
July–December 1975									
Total households	10,459	10,354	10,513	x	15,722	15,783	x	x	62,831
Type-A noninterview rate	4.12	4.07	4.00	x	4.52	4.46	x	x	4.28
No one home	0.96	0.93	0.73	x	0.75	0.92	x	x	0.85
Temporarily absent	0.98	1.02	0.86	x	1.03	0.75	x	x	0.92
Refused	1.86	1.90	2.20	x	2.58	2.52	x	x	2.27
Other	0.32	0.212	0.209	x	0.16	0.27	x	x	0.23
January–June 1976									
Total households	10,318	10,417	10,585	10,564	x	10,560	10,691	x	63,135
Type-A noninterview rate	3.91	4.21	4.34	4.47	x	4.60	4.94	x	4.41
No one home	0.79	0.76	0.97	0.81	x	0.98	0.86	x	0.86
Temporarily absent	0.94	0.84	0.78	0.74	x	0.67	0.80	x	0.79
Refused	1.76	2.24	2.31	2.56	x	2.62	2.94	x	2.41
Other	0.41	0.38	0.27	0.36	x	0.33	0.35	x	0.35
July–December 1976									
Total households	10,827	10,368	10,564	10,565	10,677	x	5,293	5,395	63,689
Type-A noninterview rate	4.30	3.83	3.99	4.46	4.48	x	4.36	4.91	4.28
No one home	0.89	0.67	0.64	0.66	0.68	x	0.60	0.96	0.72
Temporarily absent	0.95	0.80	0.84	0.98	1.07	x	0.66	0.82	0.90
Refused	2.24	2.06	2.29	2.546	2.548	x	2.76	2.87	2.42
Other	0.22	0.30	0.21	0.27	0.18	x	0.34	0.26	0.24

Other reasons for type A noninterviews

There is no statistical evidence that length of time in sample has any effect on type A noninterviews due to no one home, the household members being temporarily absent, or "other" reasons.

The overall type A noninterview rate

It appears that the type A noninterview rate also tends to increase with the length of time a panel-rotation is retained in sample. However, the evidence supporting this statement is not as conclusive as that for the refusal rate.

Only in the comparison between two versus six times in sample is the type A noninterview rate for a panel-rotation in sample for the t-th time significantly higher than the rate for a panel in sample for the second time ($t=6$ in table 97).

Further, for only the second and third data sets is the Spearman rank correlation coefficient large enough to provide evidence of a positive correlation between the type A rate and time-in-sample (see table 99).

Finally, use of Bartholomew's test supports rejection of the hypothesis

$H_o : A_2 = A_3 = \ldots = A_{max}$

in favor of

$H_2 : A_2 \leq A_3 \leq \ldots \leq A_{max}$

for all three data sets, but for the first and third data sets, several time-in-sample categories had to be collapsed to satisfy the assumptions of the test.

Specifically, in the first data set, we have

$H_2 : A_{2+3} \leq A_{4+6} \leq A_7$

and in the third data set we have

$H_2 : A_{2+3} \leq A_4 \leq A_5 \leq A_6 \leq A_{8+9}$

Nevertheless, the evidence that the type A noninterview rate tends to increase with time-in-sample is fairly conclusive.

It seems likely that the increase in the overall type A noninterview rate is a function of the increase in the refusal rate, the largest single component of the type A rate.

Because the evidence that the overall type A noninterview rate increases with time-in-sample is not as strong as that supporting the positive relationship between time-in-sample and the refusal rate, it might be hypothesized that the portion of the type A rate due to reasons other than refusal decreases as time-in-sample increases.

To test this possibility we have computed, for each data set, the Spearman rank correlation coefficient for the noninterview rates due to the following reasons:

(1) no one home

(2) temporarily absent

(3) other

(4) total type A noninterviews not due to refusal

The computed values of the Spearman rank correlation coefficient are given in table 99. As can be seen, there is no evidence of a decreasing relationship (i.e., a negative correlation) between any of these rates and time-in-sample.

Conclusions

It appears that the refusal rate, and hence the overall type A noninterview rate, tend to increase with the length of time a panel-rotation is retained in sample. There is no evidence, however, that the rates for type A noninterviews due to reasons other than refusal have any relationship to the length of time a panel-rotation is retained in sample.

Analysis of coverage ratios for the National Crime Survey and the Current Population Survey (July–December 1976)*

by CYNTHIA WILDER and MASATO ASANUMA

The purpose of this memorandum is to document the results of the comparative study of coverage ratios in 68 age-sex-color categories. In order to make effective use of available information and computer programs, inverses of the coverage ratios, second stage ratio estimate factors, were employed as the test statistic. The second stage ratio estimate factors for the NCS and the Current Population Survey (CPS) were extracted from the weighting diaries of July–December 1976 for purposes of this study. The comparisons of these factors indicate that there are no serious coverage problems in the NCS in relation to the CPS coverage.

This conclusion was based upon the SIGMA tests applied to the averages of these ratios. Please note that there is a basic difference between NCS and CPS rotation schemes, and this difference makes the results of our statistical tests serve more to identify the coverage problems in NCS as a whole than to define them in the particular age-sex-color cells. Table 101 summarizes the results of this examination as well as the results from a similar study on the 1974 data.

The conclusions formed from this study may be summarized as follows:

(l) In the 1976 data (see table 101), of the 68 comparisons made between average ratios, 5 comparisons showed statistically significant differences in coverage at the 95-percent confidence level, and 9 at the 90-percent confidence level (the five significant at the 95-percent level are, of course, significant at the 90-percent level). However, these five and nine statistically significant comparisons at the respective levels are approximately within the range of what one would expect. A similar conclusion was drawn from the results of comparisons made between monthly ratios: of the 408 comparisons, 15 showed statistically significant differences at the 95-percent confidence level, and 30 at the 90-percent confidence level.

101. Results of comparisons for the 6 individual months and for the averages of these months

Sigma test	1974 data*		1976 data	
	Individual month	6-month average	Individual month	6-month average
Statistically significant at 95-percent confidence level	5	4	15	5
Statistically significant at 90-percent confidence level	16	4	15	4
Total comparisons	408	68	408	68

* Although these comparisons were based on factors biased by an uncorrected type-Z noninterview error, it seems to have a minimum effect on this study. Note that it might have some impact on the number of statistically significant comparisons for the monthly ratios.

(2) It would appear that the second stage ratio estimate factors for CPS are relatively stable on a month-to-month basis as compared with those for NCS. The reason for this stability is probably due to the larger monthly sample size used in CPS. In addition, because of the nature of the CPS rotation scheme some individuals were selected for interviews up to four consecutive times during the 6-month period, whereas every individual in NCS was selected only once during the same period.

(3) There is no evidence to indicate that there are differences between the coverage ratios for whites and nonwhites. Similarly, no significant difference between the coverage ratios for males and females was detected. However, if we look at much smaller subcategories such as white males 20–21, white females 18–19, nonwhite females 14–15, nonwhite females 18–19, and nonwhite males 45–49, tests indicate evidence of discrepancies in these subcategories.

It is recommended that if these subcategories mentioned in conclusion (3) are of prime interest, then a more detailed examination of strictly controlled comparisons of the two surveys' data, using only the incoming rotation groups from both surveys, should be undertaken. Thus, only persons interviewed for the first time should be used so that a bias introduced into the coverage ratios due to the rotation difference—i.e., the number of times a person is interviewed during the reference period —will be eliminated. It is obvious that this type of a study requires cumulative sample data from NCS and CPS to provide estimates of any real coverage differences with any degree of precision and necessitates producing special tabulations of NCS data.

*U.S. Bureau of the Census memorandum, October 4, 1977.

Research on within-household coverage gains from control-card coverage (Current Population Survey and National Crime Survey)*

by IRENE MONTIE, DAVID BATEMAN, MICHAEL TENEBAUM, and JOHN BUSHERY

Background and purpose of research

In the CPS and the NCS, probe questions are asked to ensure complete coverage of all household members. These questions appear in item 15a on the CPS control card and item 14 on the NCS control card. The purpose of this research is to measure the potential and actual coverage improvement derived from these control card coverage questions.

The research was conducted in two phases. In phase I, a records check was done on retired CPS and NCS control cards. The purpose was to measure the frequency and rate of omission for each coverage question in the first interview visit. Since there is no ongoing provision for identifying household members added to the roster through these questions, actual gains could not be obtained in phase I. However, the phase-I results could indicate potential gains from the coverage questions. These data could also serve a control function for phase-II results.

The second phase involved field collection of data from new incoming households in CPS and NCS. Actual coverage gains could be obtained from the phase II data because provision had been made to identify persons added to the household roster through the coverage questions.

Overview of final results

A comparison of the CPS and NCS control cards completed in phase II of the research shows the following differences (significant at the 5-percent level):

- Assuming that an entry for the coverage questions on the control card indicates that the questions were asked, interviewers asked the coverage questions most of the time. However, they failed to ask the questions more frequently (1.9 percent of the questions) for CPS than for NCS (0.4 percent of the questions).
- A positive response to the coverage questions was more frequently obtained in CPS (1.6 percent of the households) than in NCS (0.6 percent of the households).
- Persons were added from the coverage questions in CPS more frequently than in NCS;
  - —for CPS, persons were added to 0.9 percent of the sample households.
  - —for NCS, persons were added to 0.3 percent of the sample households.

There is marginally significant evidence that persons added to the roster were more apt to be household members in CPS (70 percent of those added) than in NCS (41 percent of those added).

- Overall about 822,000 persons were added to the CPS universe by the control card coverage checks (with a 95-percent confidence interval extending from 483,000 to 1,160,000 persons). In the NCS only about 179,000 persons were added by the coverage checks (with a 95-percent confidence interval from 62,000 to 296,000 persons).

As can be seen, the coverage checks were much more effective in the CPS than in the NCS. The number of persons added to the NCS by these checks was only about 22 percent as large as the number added to the CPS. In both surveys the above discussions refer to coverage checks on households in sample for the first time.

In summary, the yield from the coverage questions was significantly better for CPS than for NCS despite the fact that entries for the coverage questions were made more frequently in NCS. Detailed results from the phase I and II operations follow.

Phase I operation

This phase of the study involved a records check to compare the completion of control card coverage questions in CPS and in NCS, and the potential improvement for each survey.

Sample source

The sample source for Phase I was control cards completed on the first interview visit for the following samples and rotations.

- For CPS, all control cards for interviewed households in sample A33, rotation 7. This rotation was retired after September enumeration, 1975.
- For NCS, all control cards for interviewed households in samples J01 and J02, panel 5, rotation 3 and panel 6, rotation 3. These rotations were retired after May and June enumeration, 1975. The two NCS panel-rotations are equivalent in size to one CPS rotation.

The rotation groups in the CPS and the panel-rotations in the NCS are systematic subsamples of the full samples for each survey. Sample A-33, rotation 7 is a systematic one-twelfth of the entire CPS sample. Since the sampling factor for this sample and rotation group is 1456.1758, the sampling factor for the CPS in phase I of this study is 17,474.110. The two panel-rotations of J01 and J02 (panels 5 and 6 of rotation group 3) are a systematic one-eighteenth of the entire NCS sample. Since the sampling factor for the NCS is 1104.267, the sampling factor for the NCS in phase I is 19,876.806.

The sampling factors for phase I can be applied to the unweighted data obtained in phase I to produce weighted counts of persons added to the universe by the coverage checks.[1]

Results

The results of the records check are summarized in table 102.

CPS: A total of 4,287 control cards from interviewed households were included in the records check. From these, 467 (2.7 percent) omissions on the coverage questions were identified. (Since each control card contains 4 coverage questions, there were 17,148 coverage questions in all.)

- For 91 (2.1 percent) of the households, no entries were circled for the first month visit, indicating that the coverage questions were not asked that month.
- For 48 (1.1 percent) of the households, "yes" was circled for one or more coverage questions in the first month's visit, indicating that persons were missed in the original listing. The missed persons may or may not have been household members.

NCS: Comparable data for NCS shows a total of 3,686 control cards reviewed (14,744 coverage questions) with only 18 omissions (0.1 percent) on the coverage questions, including 1 household for which no probe questions were asked.

- For 34 (0.9 percent) households, "yes" was circled for one or more coverage questions in the first quarter's visit.

Comparison of assumed adds: If we assume that in the "yes" response households cited above, the last person listed

*U.S. Bureau of the Census memorandum, May 30, 1978.

[1]In phase I we have assumed that the last person listed on the control card was detected by the coverage check items if there was a "yes" response to any coverage item for the first visit.

was added through the coverage questions, the following distribution would be found:[2]

	CPS	NCS
• Persons added with usual residence elsewhere	29	12
• Household members added	19	22
—Lodgers or boarders	4	4
—Persons traveling, at school, or in a hospital	4	5
—Newborn infant	1	2
—Other household members	10	11

In terms of inflated estimates this means that the potential gains from the coverage questions, based on the distribution of the "yes" response cases cited above, are:

- 332,000 persons added for CPS (95-percent confidence interval from 183,000 to 481,000)
- 437,000 persons added for NCS (95-percent confidence interval: 255,000 to 619,000)

These estimates may be understated since some persons with usual residence elsewhere become household members in later months. Note that the potential undercoverage for a full sample is greater for CPS than for NCS because the full battery of coverage questions are asked for each interview in NCS but are asked only in the first and fifth interview period for CPS.

In terms of inflated estimates the potential losses from not asking coverage questions, based on the cases cited above (CPS–2.7 percent; NCS–0.1 percent), are:

- 4,583 persons for CPS (upper 95-percent confidence interval is 29,418)
- 535 persons for NCS (upper 95-percent confidence interval is 9,550)

102. Results of within-household coverage study: Phase I—Jefferson records check of retired control cards

	Total households	Total households interviewed	Total omissions on any probe item	Households with omissions on all probe items		Households with YES to any probe item	
				No.	Percent	No.	Percent
CPS*	4,991	4,287	467	91	2.1	48	1.1
NCS**	4,143	3,686	18	1	—	34	0.9

* Sample source: CPS sample A33, rotation 7
** Sample source: NCS samples J01 and J02, panel 5, rotation 3 and panel 6, rotation 3.

103. Results of within-household coverage study: Phase II—field collection of data from new incoming rotations

	Total households	Total households interviewed	Total omissions on any probe item		Households with omissions on all probe items		Households with YES to any probe item		Households with persons added to roster 3***	
			No.	Percent	No.	Percent	No.	Percent	No.	Percent
CPS*	4,567	3,464	268	1.9	62	1.8	56	1.6	32	0.9
NCS**	4,089	3,249	46	0.4	9	0.3	21	0.6	9	0.3

*Sample source: CPS sample A36, rotation 3 for February 1976.
**Sample source: NCS samples J03 and J04, panel 2, rotation 5 in February 1976 and panel 3, rotation 5 in March 1976.
*** In CPS, 24 households had one person added, five households had two persons added, one household had three persons added, one household had four persons and one household had five persons added. In NCS all nine households had only one person added.

Phase II operation

As stated earlier, phase I data provided a comparison of the frequency of "yes" responses to the coverage questions but the number of persons added was estimated, based on certain assumptions. In phase II, field controls were introduced so that the actual number of persons added by the coverage questions could be counted. The results are shown below.

Sample source

The sample source for phase II was control cards completed on the first interview visit for the following samples and rotations.

- For CPS, all control cards for households in sample A36, rotation 3, which were introduced into CPS in February 1976.
- For NCS, all control cards for households in sample J03 and J04, panel 2, rotation 5, which were introduced into NCS in February 1976, and panel 3, rotation 5, which were introduced into NCS in March 1976.

These households represent systematic subsamples of the CPS and NCS samples, as did the subsamples used in phase I. In order to arrive at estimates on the national level, weighting factors of $(1488.8989) \times (12) = 17{,}866.787$ and $(1104.267) \times (18) = 19{,}876.806$ need to be applied to the unweighted CPS and NCS counts, respectively.

Results

The results of the field research are summarized in table 103.

CPS: A total of 3,464 control cards for interviewed households were completed. From these, 268 (1.9 percent) omissions of coverage items were found.

- For 62 (1.8 percent) households, no entries were circled for the coverage questions and no persons were added to the household roster, indicating that the coverage questions were not asked.
- For 56 (1.6 percent) households, "yes" was circled for one or more probe questions, which yielded 46 persons added to the household roster.

NCS: Comparable data for NCS shows that a total of 3,249 control cards for interviewed households were completed. From these 46 (0.4 percent) omissions in coverage items were found, including 9 households for which no coverage questions were asked.

- For 21 (0.6 percent) households, "yes" was circled for one or more coverage questions, which yielded 9 persons added to the household roster.

Comparison of persons added: A count was made of persons added to the control cards in response to the four coverage questions asked in the first interview visit. The following distribution shows these persons, by household membership status, for CPS and NCS. (Note that this is a finer breakdown than the data permitted for comparable tables in phase I.)

	Household members		Usual residence elsewhere	
	CPS	NCS	CPS	NCS
• Lodgers or boarders	3	0	0	0
• Traveling, at school, in hospital	9	4	4	2
• Newborn infant	19	3	0	0
• Other	15	2	16	11
	46	9	20	13

In terms of inflated estimates this means that the gains from the coverage questions, based on the distribution of the cases for which household members were added to the roster, are:

- 822,000 persons for CPS (95-percent confidence interval: 483,000 to 1,160,000)
- 179,000 persons for NCS (95-percent confidence interval: 62,000 to 296,000)

As indicated for phase I, these results are probably a potential understatement since some of the persons with usual residence elsewhere may become household members in later months. This would not be reflected in the coverage questions for CPS, although it might be for NCS since the full battery of questions are asked for each period of NCS.

In terms of inflated estimates the potential losses from not asking the coverage questions, based on the cases cited above (CPS—1.9 percent; NCS—0.4 percent), are:

- 16,210 persons for CPS (upper 95-percent confidence interval is 83,754)
- 786 persons for NCS (upper 95-percent confidence interval is 11,739)

The main consideration is that this potential undercoverage represents persons who have the unique characteristic of not being reported and, therefore, may have a particular impact on labor force and crime data.

Summary and conclusion

The yield from the coverage questions was significantly higher in CPS than in NCS. An unusual point is that the coverage questions were circled more frequently for NCS than for CPS. This might indicate that the questions were asked more frequently for NCS, but were not asked as effectively, or that the control cards were edited better for NCS.

There is indication that the coverage questions are not being handled correctly for NCS. The importance of these questions should be stressed in training, observation, reinterview, and other supervisory contact with the NCS interviewers.

This study deals with undercoverage resulting from improper handling of the coverage question during the first visit to a CPS or NCS household. Now that all visits have been completed at the sample units in this study, followup research is planned to evaluate the impact of underutilization of the coverage questions for a full sample. This should also provide some insight into the impact of asking the full battery of coverage questions each visit for NCS, as compared to a single coverage question asked in six of the eight visits for CPS.

[2]In comparing these household membership data with the phase II data presented later, it appears that the assumed relationship between a "yes" response and the last person listed in phase I provides an underestimate of household members added for CPS but an overestimate for NCS.

Chapter 6

Sample design and estimation issues

Introduction

The papers in this chapter focus on how changes in the sample and panel design might affect the accuracy and costs of victimization estimates.

The first paper by Henry Woltman and John Bushery considers the tradeoffs in cost versus efficiency for the number of interviews conducted with a household (now seven). They conclude that restricting the panels to five interviews will improve the efficiency of the NCS by reducing error in the estimates of victimization rates without substantially increasing costs.

The research reported in the second paper by Woltman and Bushery examines the tradeoffs in using a 12- versus 6-month recall period and in using differentially weighted versus equally weighted monthly data. Taking a different approach to controlling costs, Masato Asanuma examines the advantages of stratifying the National Crime Survey (NCS) national sample by crime characteristics and size of place. He finds that the gains in efficiency will probably be offset by the increased costs. The final paper by Anthony G. Turner proposes dual-frame sampling using police records as a means of reducing NCS costs.

Results of a study to determine the "optimum times to retain a panel in sample"*

by HENRY WOLTMAN and JOHN BUSHERY

Table 104 indicates that the minimum Mean Square Error (MSE), when the combined field and processing costs are held fixed using the point estimates of the panel bias parameters, is obtained when $k = 2$ for the major crime categories. The adoption of a rotation scheme in which panels were interviewed two times would certainly be ill-conceived however, since the increase in variance on estimates of annual change would be substantial—something like 2.7 times as great as the current variance (about 1.6 times on the standard errors). In fact one could also expect a substantial increase in variance on estimates of annual change for rotation schemes where panels are interviewed three or four times (see table 105).

In our view an acceptable compromise solution would be to interview panels five times. With this rotation scheme substantial gains in the accuracy on estimates of level are obtained; as much as a 20- to 35-percent reduction in the root MSE (see table 104). At the same time the standard errors on annual change estimates would increase slightly (by about 8 percent) but with relatively little effect on the inferences which can be drawn from the change data considering the estimated changes that have taken place between 1973, 1974, and 1975. An even better solution would be to obtain additional funds such that the current interviewed sample size could be maintained while at the same time reducing the number of times a panel is interviewed to that number which is commensurate with the additional funds received. For example, roughly a 17-percent increase in the field in processing costs would produce an interviewed sample of 60,000 where panels were interviewed five times. Similarly a 35-percent increase would produce an interviewed sample size of 60,000 where panels were interviewed four times. The increase in cost required to maintain the sample size at 60,000 interviewed households where panels are interviewed k times is roughly equal to the increase in variance (f_k factor) shown in table 105.

*Excerpted from U.S. Bureau of the Census memorandum, July 11, 1977.

104. Estimated mean square error of annual victimization rates for various rotation schemes using point estimates of the panel bias parameters

(All rates and mean square errors expressed per 1,000 persons age 12 and over or households)

Type of crime		Number of times interviewed (k)					
		7	6	5	4	3	2
Total personal crimes	$\bar{v}_k$*	127.7	129.5	130.9	133.0	137.3	142.4
	MSE**	218.4	169.9	135.2	89.9	28.2	3.2
Crimes of violence	$\bar{v}_k$	32.8	33.1	34.3	34.6	36.1	37.7
	MSE	24.3	21.9	12.1	9.9	3.0	0.9
Assault	$\bar{v}_k$	24.7	25.2	26.3	26.8	28.1	29.2
	MSE	20.9	16.8	9.3	6.3	1.7	0.7
Personal theft	$\bar{v}_k$	94.9	96.3	96.5	98.3	101.1	104.7
	MSE	96.7	70.3	67.7	41.5	14.2	2.5
Total household crimes	$\bar{v}_k$	234.7	237.5	239.4	242.2	246.6	253.1
	MSE	342.9	248.0	192.7	124.6	57.7	11.7
Burglary	$\bar{v}_k$	92.6	93.3	94.6	96.1	96.4	97.9
	MSE	30.1	23.3	13.2	6.0	5.7	5.5
Household larceny	$\bar{v}_k$	123.4	125.9	126.4	126.8	130.2	135.2
	MSE	141.8	89.2	80.5	74.1	29.4	7.1
Motor vehicle theft	$\bar{v}_k$	18.7	18.6	18.6	19.5	19.6	20.3
	MSE	3.0	3.4	3.4	1.2	1.2	1.2

Note: All rates and mean square errors expressed per 1,000 persons age 12 and over or per 1,000 households.

victimization rates shown for the column header "7" times interviewed. The victimization rates shown in this column are 1974 annual estimates and were used to approximate $E(\bar{v}_7)$ in the bias computation. However, the monthly samples on which these estimates were based—February 1975 through June 1976—were not completely balanced by time in sample.

* The estimated bias due to interviewing a panel k times is given by the difference between $\bar{v}_2$ and $\bar{v}_k$.

** The bias component of the MSE is derived using point estimates of the panel bias parameters and the

105. Monthly sample sizes and total sample size for rotation schemes involving seven or fewer interviews

	Number of households by total number of times a panel is to be interviewed (assumes total of field and processing costs remains constant)					
	7	6	5	4	3	2
Monthly sample size for each time-in-sample category ($\bar{n}_k$)	2,000	2,250	2,560	2,975	3,550	4,400
Number of interviews completed each month 0.83 (k) ($\bar{n}_k$)	11,600	11,200	10,600	9,900	8,800	7,300
Interviewed sample size for quarterly estimate* (n_k)	60,000	56,300	51,200	44,600	35,500	22,000
$f_k = n_7$** / n_k	1.00	1.07	1.17	1.35	1.69	2.73

* The annual affective interviewed sample size is also about the same since the quarter-to-quarter correlation of the rates is quite small.

**The relationship between variances for different rotation schemes uses f_k. That is, Var $(\bar{v}_k) = f_k$ Var $(\bar{v}_7)$.

Summary of a study examining "differentially weighted estimates of annual victimization rates using a 12-month bounded reference period"*

by HENRY WOLTMAN and JOHN BUSHERY

Introduction

In an earlier report we developed an alternative to the current NCS estimator for annual victimization rates.[1] That estimator made use of differential weighting of victimizations reported in the 6-month bounded reference period. We showed that much of the bias resulting from recall loss (relative to that obtained if a 3-month bounded reference period were used) could be eliminated if the victimizations reported as occurring in the more recent half of the 6-month reference period were given more weight than those reported as occurring in the more distant half of the reference period. This type of estimator had a considerably smaller Mean Square Error (MSE) than the current estimator even when the recall loss factor was not known with a great deal of precision.

Another point of interest is whether this type of estimator in combination with a longer reference period, say 12 months, could be used to produce estimates with accuracy greater than or equal to that achieved with differentially weighted estimates obtained from a 6-month reference period.

To assure that these estimates are comparable we have assumed that the number of interviews to be conducted each month is to be held constant, regardless of the length of the reference period used. This means that if the 6-month reference period is employed with a total sample size of n households then the monthly interview workload is $n/6$. Thus a 3-month reference period would also involve a monthly interview workload of $n/6$ but the total sample size would be $3(n/6) = n/2$ since only 3 months of interview would pass before households were interviewed in the next interview cycle. A 12-month reference period with a monthly workload of $n/6$ households would involve a total sample size of $12(n/6) = 2n$ households. The variances of the estimates thus obtained are, of course, inversely proportional to these total sample sizes.

106. Ratio of the mean square error of a differentially weighted estimate using a 12-month, bounded reference period to the mean square error of the current estimator for an annual estimate of the victimization rate for total personal crimes

Weighting factor	δ = .90*			δ = .85			δ = .80			δ = .75			
a_{12}	ρ** = .85	ρ = .75	ρ = .70	ρ = .85	ρ = .75	ρ = .70	ρ = .85	ρ = .75	ρ = .70	ρ = .85	ρ = .75	ρ = .70	a_{12}
.50	5.8	11.1	14.5	3.7	6.4	8.0	2.8	4.5	5.5	2.3	3.5	4.2	.50
.52	4.4	9.0	12.0	3.0	5.4	6.8	2.3	3.8	4.7	2.0	3.1	3.7	.52
.54	3.3	7.2	9.7	2.4	4.4	5.7	1.9	3.3	4.0	1.7	2.6	3.2	.54
.56	2.3	5.5	7.6	1.8	3.6	4.6	1.6	2.7	3.4	1.4	2.3	2.8	.56
.58	1.5	4.1	5.8	1.3	2.8	3.7	1.2	2.2	2.8	1.2	1.9	2.3	.58
.60	0.9	2.9	4.3	0.9	2.1	2.9	0.9	1.8	2.3	1.0	1.6	2.0	.60
.62	0.4	1.7	3.0	0.6	1.5	2.2	0.7	1.4	1.8	0.8	1.3	1.6	.62
.64	0.1	1.1	1.9	0.3	1.1	1.6	0.5	1.1	1.4	0.6	1.0	1.3	.64
.66	***	0.5	1.1	0.2	0.7	1.0	0.3	0.8	1.1	0.4	0.8	1.0	.66
.68	***	0.2	0.5	***	0.4	0.6	0.2	0.5	0.7	0.3	0.6	0.8	.68
.70	0.3	***	0.1	***	0.2	0.3	***	0.3	0.5	0.2	0.4	0.6	.70
.72	0.7	***	***	***	***	0.1	***	0.2	0.3	0.1	0.3	0.4	.72
.74	1.2	0.4	0.2	0.2	***	***	***	***	0.1	***	0.2	0.2	.74
.76	2.0	0.9	0.9	0.3	***	***	***	***	***	***	***	0.1	.76
.78	2.9	1.6	1.2	0.6	0.2	0.1	***	***	***	***	***	***	.78
.80	3.9	2.6	2.0	0.9	0.5	0.3	0.2	***	***	***	***	***	.80
.82	5.2	3.8	3.1	1.3	0.8	0.6	0.3	0.2	0.1	***	***	***	.82
.84	6.6	5.1	4.5	1.8	1.3	1.1	0.5	0.3	0.2	0.1	***	***	.84
.86	8.2	6.7	6.1	2.3	1.8	1.6	0.7	0.5	0.4	0.2	0.1	***	.86
.88	9.9	8.5	7.9	2.9	2.4	2.2	0.9	0.8	0.7	0.3	0.2	0.2	.88
.90	11.8	10.6	10.0	3.6	3.2	2.9	1.2	1.1	0.9	0.4	0.3	0.3	.90
opt	.66	.70	.72	.70	.74	.75	.74	.77	.78	.78	0.81	0.82	

Note: Values in the boxes identify a pattern in the data—entries less than 1. Ratios less than 1 indicate that under the conditions specified in the table, the differentially weighted approach produces a smaller standard error than the current method of estimation.

* δ is the 3-6 month recall loss factor.

** ρ is the 6-12 month recall loss factor.

*** indicates ratio is less than 0.1.

Analysis and conclusions

The values of $\alpha_{(12)}$ for which MSE $(\hat{V}_{\alpha(12)})$ < MSE $(\hat{V}_{(6)})$ are shown in the blocked areas of table 106. Similarly the values of $\alpha_{(6)}$ for which MSE $(\hat{V}_{\alpha(6)})$ < MSE $(\hat{V}_{(6)})$ are shown in the blocked areas of table 107.

In each case there is a fairly wide range of values of α for which improved accuracy (i.e., a smaller MSE) can be achieved. The next question to be addressed is how does the accuracy of a differentially weighted estimator obtained using a 6-month reference period compare with that of a similar estimator obtained using a 12-month reference period when the recall loss parameters δ and ρ are not precisely known.

In our proposed recall bias study[2] we will obtain point estimates and interval estimates of the recall loss parameters. The interval estimates can be used to construct tables similar to tables 106 and 107. For example, if we find that $.75 \leq \delta \leq .90$ and $.70 \leq \rho \leq .85$ then tables 106 and 107 could be used to determine for each reference period a range of weights which will

*Excerpted from U.S. Bureau of the Census memorandum, July 11, 1977.

[1]Memorandum for Bateman and Shapiro from Woltman and Bushery; December 20, 1976; "An alternative estimator for NCS victimization rates."

[2]Memorandum for Thompson from Jones; June 11, 1976; "Alternative sample designs for evaluating reference periods of three months and twelve months relative to the current 6-month reference period in the NCS."

produce an improvement in accuracy over the estimated ranges of δ and ρ. Within each range a value of ∝ can then be selected which would be expected to produce substantial gains in accuracy over the entire estimated ranges of δ and ρ. As can be seen in table 106, any value of $\propto_{12}$ between .68 and .72 would result in an improvement in accuracy in comparison with the current estimator. Similarly, table 107 indicates that any value of $\propto_{16}$ between .52 and .70 would also produce an improvement in accuracy. Use of $\propto_{12} = .70$ and $\propto_6 = .64$ would probably result in estimates of roughly comparable accuracy if δ and ρ fell within the ranges given above. However, which of these two estimates is actually more accurate would depend on the true values of δ and ρ, which are unknown. For this reason we would be unable to say that if $.75 \leq \delta \leq .90$ and $.70 \leq \rho \leq .85$ then a differentially weighted estimate with $\propto_{12} = .70$, obtained using a 12-month reference period, is more (or less) accurate than a differentially weighted estimate with $\propto_6 = .64$, obtained using a 6-month reference period.

It should be kept in mind that factors other than the accuracy of annual estimates of level for victimization rates must also be considered. Estimates of year-to-year change in the victimization rate may be more important than annual estimates of level. This statistic is essentially unaffected by recall loss (if recall loss is constant over time) so variance is of primary concern. The differentially weighted estimator obtained using a 6-month reference period has a slightly larger variance on year-to-year change than the current estimator, while the differentially weighted estimator obtained using a 12-month reference period will have a variance smaller than that of the current estimator (about 40-percent smaller when ∝ 12 = .70 is used).

107. Ratio of the mean square error of a differentially weighted estimate using a 6-month bounded reference period to the mean square error of the current estimator, for an annual estimate of the victimization rate for total personal crimes

a_6	d = .90*	d = .85	d = .80	d = .75
.50	1.0	1.0	1.0	1.0
.52	0.7	0.8	0.8	0.9
.54	0.4	0.6	0.7	0.7
.56	0.2	0.4	0.5	0.6
.58	0.1	0.3	0.4	0.5
.60	**	0.2	0.3	0.4
.62	**	0.1	0.2	0.3
.64	0.1	**	0.1	0.3
.66	0.3	**	0.1	0.2
.68	0.5	**	**	0.1
.70	0.8	0.1	**	**
.72	1.2	0.1	**	**
.74	1.7	0.3	**	**
.76	2.2	0.4	**	**
.78	2.8	0.6	0.1	**
.80	3.4	0.8	0.1	**
.82	4.2	1.0	0.2	**
.84	5.0	1.2	0.3	**
.86	5.9	1.5	0.4	**
.88	6.8	1.9	0.5	0.1
.90	7.8	2.2	0.7	0.2
opt	.60	.66	.72	.78

Note: Values in the boxes identify a pattern in the data—entries less than 1. Ratios less than 1 indicate that under the conditions specified in the table, the differentially weighted approach produces a smaller standard error than the current method of estimation.
* d is the 3-6 month recall loss factor.
** Indicates ratio is less than 0.1.

Unfortunately, a reference period of 12 months may cause a substantial loss in the accuracy of the details of the crime incidents. Further, a longer reference period would increase respondent burden for each interview and may result in increased respondent fatigue. A reverse record check study has been proposed which should provide some idea of the loss in accuracy for details of the incident by length of recall.

A further disadvantage of a 12-month reference period is that annual estimates require 23 months of interview rather than the 17 months required by a 6-month reference period. This means that annual estimates would be available 6 months later than under the current system.

Obviously, before any recommendations can be made about reference period length, the effects of all these factors must be considered. However, we believe the theoretical framework for the differentially weighted type of estimator has been set in place. We must await the estimates of the recall loss factors in order to evaluate more fully the potential gains in accuracy by length of reference period.

A study on restratification of the National Crime Survey by crime characteristics and the size of city*

by MASATO ASANUMA

The purpose of this study is to provide a basis for deciding whether to try to restratify the NCS universe using past sample data for crime characteristics for various geographic strata that show differential crime rates and then to optimally allocate the sample to these strata. For this study, strata were defined by the size of the central city of a Standard Metropolitan Statistical Area (SMSA), by inside central city and the balance of SMSA, and by non-SMSA.

Tables 108 and 109 give both the proportional allocation and the optimum allocation using the 1974 robbery rates in the nine strata, and the relative precision of proportional to optimum allocation.

The relatively small gain (9 percent at the most) shown in table 109 provides ground for an immediate decision: the gain of optimum stratification is not enough to be worthwhile, considering the costs involved in restratification. Since the differences in rates were less for other types of crime, strata formed on the basis of assault, personal larceny, or burglary would give even less gain than robbery.

108. Stratification by the 1974 robbery rates

(Stratum) SMSA with the size of central city	Robbery rate (per 1,000)	Stratum weight (W_h)	Allocation of sample: Proportional (presently used)	Allocation of sample: Optimum with robbery rate
1,000,000 and over				
Inside central city	19.3	.09	13,500	23,100
Outside central city	9.2	.08	12,000	15,000
500,000-999,999				
Inside central city	10.9	.06	9,000	11,900
Outside central city	6.1	.09	13,500	13,100
250,000-499,999				
Inside central city	8.7	.06	9,000	9,900
Outside central city	4.8	.09	13,500	11,500
Less than 250,000				
Inside central city	9.4	.09	13,500	16,600
Outside central city	4.9	.12	18,000	15,800
Nonmetropolitan	3.2	.32	48,000	33,100

109. Relative precision of proportional to optimum

P_h	Proportional $\Sigma W_h P_h Q_h$	Optimum $(\Sigma W_h \sqrt{P_h Q_h})^2$	Relative precision (percent)
.0193, .0092, .0109, .0061, .0087, .0048, .0094, .0049, .0032	.00702	.00643	91

*U.S. Bureau of the Census memorandum, September 23, 1976.

Research on dual-frame sampling*

by ANTHONY G. TURNER

Purposes

1. To assess the theoretical improvements in estimates of (selected) victimization rates through the use of two-frame sampling.

2. To study the practical problems associated with sample selection from police records.

Discussion

For the majority of crimes which are studied in the NCS, their incidence is statistically rare. This makes for a relatively inefficient use of sample units, in that the great number of the cases canvassed yield no victimizations. Large numbers of persons must be interviewed in order to locate a single victim of armed robbery or rape.

It is theoretically possible to increase the sample size (of victims) substantially by selecting cases of known victims from police records, and to utilize regular interviews from this group of individuals as supplementary input in the estimation procedure. The results, presumably, would be an estimate with lower variance. An alternative design change would be to maintain the current variance level but to adjust the sample sizes between the two strata (household sample stratum and police record sample stratum). The sample size for the household sample would presumably be considerably less than is currently needed.

One phase of the research plan, then, is to conceptualize and develop the theoretical model for sampling and estimation. Advantage can be taken of development of such models for other programs, notably the current Registration and Voting Survey and the Area Labor Survey conducted during the mid-1960's. Estimates for most of the parameters that are likely to be needed in the theoretical modeling should be available from existing information—NCS, UCR, and possibly some of our old reverse record checks. From the model(s) developed, hypothetical displays would be produced showing (1) how much the reliability of the estimates of the survey victimization rates could be improved over the existing method, and (2) how the sample should be allocated between the two strata for alternative degrees of reliability. The investigation would be explored for each of several key crime rates.

The second phase of the project would address the second purpose mentioned at the outset, namely to study the operational feasibility of implementing a two-frame sample procedure. In essence, the problem boils down to one of how to design or create a national sample frame of police-known crime victimizations. If it can be created nationally, one needs to determine how it can be maintained and controlled. Some of the ideas to be explored include:

1. The FBI as an initial source of either records or information about record systems.

2. The possibility of interviewer-assisted frame development, given that a sample of first-stage units (police departments) will have been chosen.

3. A survey of police departments to assess record-keeping practices and the potential for police-generated sampling frames.

Finally, there is the very strong possibility that a dual-frame sample design may prove to be impractical to implement nationwide due to the wide diversity of police department record systems. It may be, however, that a dual-frame approach could be efficiently utilized for national estimates even if the police sample were restricted to a portion of the universe—say, large SMSAs—or it may turn out that a dual-frame approach may be very efficient for local area surveys.

*Excerpted from U.S. Bureau of the Census memorandum, March 8, 1977.

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