



# Redesign of the National Crime Survey

## *Background*

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111457

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# Redesign of the National Crime Survey

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**U.S. Department of Justice  
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The Assistant Attorney General, Office of Justice Programs, coordinates the activities of the following program offices and bureaus: the Bureau of Justice Statistics, National Institute of Justice, Bureau of Justice Assistance, Office of Juvenile Justice and Delinquency Prevention, and the Office for Victims of Crime.

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# Contents

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<b>Preface</b>	iv
<b>Introduction</b>	1
<b>NCS background</b>	1
Preparation for the National Crime Survey	2
Resolution of early NCS methodological issues	3
Characteristics of the original NCS	5
Objectives of the NCS program	5
Uses of the NCS	6
<b>Genesis of the NCS redesign</b>	7
<b>Structure and functioning of the consortium</b>	8
Consortium structure	9
Advisory Panel	9
CSRC teleconference	10
Redesign task-oriented committees	10
<b>Objectives of the NCS redesign</b>	11
Conceptual issues	11
Methodological issues	11
Analytical issues	11
Accuracy	11
Enhancement of analysis options	14
Flexibility	17
Improving utilization of NCS data	17
Cost-effectiveness	18
<b>NCS redesign testing</b>	20
<b>Design change decisions</b>	22
Design packages	22
Other design features	25
<b>Implementation of the NCS redesign</b>	28
<b>Conclusion</b>	31
<b>Appendixes</b>	
A Major NCS redesign products	35
B Major NCS redesign decisions	37
C Remaining issues in the NCS redesign	38

# Preface

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The National Crime Survey (NCS) was implemented in 1972 to provide data on the level of criminal victimization in the United States and to collect data on the characteristics of crime incidents and victims.

After several years of experience with the survey and partly in response to an evaluation by the National Academy of Sciences (NAS), a multiyear effort to redesign the NCS was begun in 1979.

This work should result in expanded information on the characteristics and consequences of crime victimization, a more effective strategy for obtaining reports of crime incidents from respondents, adoption of Computer-Assisted Telephone Interviewing (CATI) technology, expanded use of supplements, and an improved ability to measure long-term aspects of victimization, including multiple victimization and extended contacts with the criminal justice system.

Revisions are being carried out in two stages: The first set of changes was implemented in July 1986, and the second set is scheduled to begin phase-in early in 1989.

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An extensive project to redesign the National Crime Survey (NCS) was undertaken in 1979 partly in response to an evaluation by the National Academy of Sciences (NAS) and partly as a result of an internal review by the National Criminal Justice Information and Statistics Service (NCJISS) of the Law Enforcement Assistance Administration (LEAA), the predecessor of the Bureau of Justice Statistics (BJS). The LEAA review included both an examination of the NCS (initially the National Victimization Survey) conducted by a panel of experts and a study of the utilization of the victimization survey. The fruits of the resulting research and development effort are already evident in changes in the survey introduced in 1986; more comprehensive revisions will be implemented over the next several years.

This paper is designed to give an overview of the redesign project. After providing background material on the NCS, the roots and organization of the redesign project will be described. Important objectives and methodological issues will then be discussed, followed by descriptions of the redesign testing program and the major design changes that have been or will be adopted. The final section outlines the manner in which changes are being implemented.

The NCS has been subjected to rigorous scrutiny over the course of the redesign project; the revised survey should provide much improved data on the dynamics and consequences of personal and household crime victimization in the United States. Because the redesign effort is still in its final phases, including an assessment of costs associated with changes, this document cannot provide the final word on changes to the survey; it also cannot do justice to the extensive work undertaken by the redesign project, BJS, and Census Bureau staff in support of this effort over a 5-year period. This document should provide, however, a useful chronicle for those concerned with the issues investigated during the course of the project; the methods used to resolve these questions; and the coverage, organization, methodologies, and technologies of the revised NCS.

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The first victim surveys in the United States were carried out for the President's Commission on Law Enforcement and Administration of Justice in the mid-1960's. The Commission undertook these surveys for two primary reasons:

- Inadequate data existed on the nature and consequences of victimization, and
- Measurement of the total volume of crime and of crime trends over time was extremely difficult with existing data bases.

While the Commission lauded improvements in the FBI's Uniform Crime Reporting (UCR) program, it noted that measurement of crime with this series was confounded with the diligence of citizens in reporting crimes to the police and, in some instances, of police departments in reporting crimes to the UCR. Although a number of problems were identified in the Commission-sponsored surveys, these data collections indicated that the amount of crime being committed in the United States was substantially higher than the amount detected by the UCR. The Commission recommended that a number of different crime indicators be developed to supplement the UCR<sup>1</sup> and that a National Criminal Justice Statistics Center be established, both to gather data on a wide range of crime and criminal justice topics and to assist the States and localities in developing high-quality statistical systems.<sup>2</sup>

Following the Commission's work, the U.S. Bureau of the Census convened a series of conferences in 1967 and 1968 to assess the need for developing data collections dealing with law enforcement, courts, and corrections problems. The need for victimization data was a major concern for conference participants, and the development of a regular data

<sup>1</sup>President's Commission on Law Enforcement and Administration of Justice, "Task Force Report: Crime and Its Impact--An Assessment" (Washington: USGPO, 1967), p. 40.

<sup>2</sup>*Ibid.*, pp. 123-37.

series was recommended for a variety of reasons. The conference report indicated that victimization data could:

- provide an independent calibration for the UCR;
- provide a measure of victim risk;
- enable a shift in concentration in the criminal justice system from the offender to the victim;
- provide an indicator of the crime problem outside those indicators generated by police activity;
- serve as an index of changes in reporting behavior in the population;
- provide an indicator of social "outlook" in the population as well as an indicator of society's definitions of crimes;
- serve as a basis for the study of granting of compensation to victims;
- serve as a statistic to determine the degree of involvement by the victim; and
- serve as a measure of public confidence in police effectiveness.<sup>3</sup>

<sup>3</sup>U.S. Department of Commerce, Bureau of the Census, "Report on National Needs for Criminal Justice Statistics" (Washington: USGPO, 1968), p. 53.

### Preparation for the National Crime Survey

When the Law Enforcement Assistance Administration (LEAA) was established in 1968, statistical functions were incorporated into the enabling legislation (P.L. 90-351, Sec. 515b), and a Statistics Division was organized in 1969 to assume these responsibilities. Building on the experience and recommendations of the Crime Commission and the Census Bureau criminal justice statistics conferences, the LEAA Statistics Division began planning in that year to implement a National Victimization Survey (later renamed the National Crime Survey). The survey was conceived to satisfy two broad goals in providing information on the incidence of crime and its effect on victims. The first goal was to launch a time series tracing changes both in the incidence of crime and in the association of various descriptive attributes with criminal victimization. The second goal was to create a vehicle that would allow study of particular research questions related to criminal victimizations, such as the relationship of victims to offenders, the cost of crime, and the vulnerability of various types of individuals and businesses to victimization. The NCS was intended to complement information available from the FBI's Uniform Crime Reports (UCR) by collecting data on crimes not reported to the police and by providing more detailed information on victims and victimization incidents.

The Statistics Division of LEAA had to resolve a number of methodological issues suggested by previous work. To reach decisions on these issues, the Census Bureau conducted a number of pilot studies in 1970 and 1971. The earliest of these, in Washington, D.C., Baltimore, Maryland, and San Jose, California, comprised "reverse record check studies," in which known victims (identified through police records) were interviewed to test the questionnaire. In addition, general population surveys were conducted in San Jose and Dayton, Ohio, at the

same time as the San Jose reverse record check study. Reverse record check methodology was adopted for the early pilot studies because of the statistical rarity of crime victimization. Data bases developed with this methodology provided a far larger proportion of crime victims than could be obtained from general population surveys of similar size. Therefore, use of record check data enabled the Census Bureau to perform statistically reliable testing of the questionnaire at relatively low cost. This methodology did present problems; it excluded nonreported crimes and included crimes against victims who did not reside in the sample area. Also, the sampling frame could not be implemented directly by the collecting agency and was only as accurate as the police reporting for the jurisdiction in question. To test and improve the adequacy of the questionnaire for measuring actual crime incidents, however, this method did prove quite adequate.

In addition to these local tests, four victimization supplements were added to the Quarterly Household Survey (QHS), which was administered nationally in January and July of 1971 and 1972. These supplements were intended both to give an indication of the sample size that would be needed to provide reliable estimates of criminal victimizations and to help resolve a number of methodological issues. Issues examined included the optimal length of the reference period for persons reporting incidents/victimizations, the need to bound NCS interviews with previously collected data, and the feasibility of using mail questionnaires for initial screening of crime incidents. The QHS interviewed only one respondent per household. However, the San Jose and Dayton field tests showed the superiority of collecting data from each household member, and this design was selected for the final version of the NCS.



## Resolution of early NCS methodological issues

The tests listed above were intended to probe a number of unresolved problems in victim survey methodology. Results from these tests had to be weighed against then-existing policy concerns and budgetary constraints in reaching final design decisions for the NCS as originally implemented. The most salient of these issues are discussed below:

### *The degree to which UCR crime classifications would be emulated*

Discussions on this topic dealt with the feasibility of reproducing UCR crime classifications and definitions and with the desirability of doing so. The NCS was designed to make an approximation of this classification possible because of the expectation that comparisons of the two series were inevitable. An error-free comparison has never been possible, however, due to differences in the scope of crimes covered by the two series, the at-risk populations, and the modes of data collection. The NCS instrument was refined to develop a question sequence that would allow such a classification, without collecting massive amounts of data from the respondent. Victim and crime incident data provided in other sections of the questionnaire allow users to construct alternate classifications, if they desire.

### *How to cope with erroneous recall of the date of a crime incident (telescoping)*

All retrospective surveys in which respondents are asked to report the occurrence of discrete events run the risk that respondents may inaccurately report the dates on which, or time periods over which, these events occurred. Events may be reported to have taken place more recently than they actually occurred (forward telescoping) or at an earlier time than the actual event (backward telescoping). To the extent that time-related analysis or the reporting of annual level and change estimates is important, techniques to reduce telescoping error must be

developed. Because the NCS reverse record check studies provided both dates on which incidents were reported to the police and dates for the same incidents reported retrospectively by respondents to survey interviewers, these data collections enabled researchers to study the extent and direction of the telescoping problem.

Although both forward and backward telescoping were observed, forward telescoping was more typical; however, the extent and nature of telescoping varied depending on the type of crime. The strategy chosen to correct telescoping error was to "bound" each interview by the previous interview. This procedure relies on the rotating panel design of the NCS sample, in which residents of a sampled housing location are interviewed seven times at fixed intervals and then retired from the sample. Bounding is accomplished by checking incidents reported in a particular interview against incidents reported in the previous interview and then deleting incidents that have been erroneously reported for the most recent reference period.

Although this procedure reduces telescoping and deals well with the more common problem of forward telescoping out-of-scope incidents into the time frame or reference period covered by a particular interview, it does not totally eliminate such error. It obviously does not detect (1) telescoping within a reference period or (2) incidents that have been backward telescoped either into or out of the reference period. The first interviews at a household are also necessarily unbounded, and data collected during these interviews are not used in NCS estimation, which increases NCS sampling variances. Also, if a new individual or a new household enters a sampled housing location during its time in sample, then the first interviews obtained are unbounded. Under current procedures, these unbounded interviews are used for estimation and are included in public-use data sets. Thus, while the bounding of interviews reduces the extent of telescoping, it does not completely eliminate it.

### *The optimal length of reference period for recalling victimization incidents*

In addition to telescoping, memory decay may also produce errors in reporting victimization incidents. The more time passes between the occurrence of a crime incident and the date of interview, the less likely the victim is to report the victimization to the NCS. (This effect, however, appears to be less pronounced for more serious crimes.) Although the effects of memory decay and forward telescoping within a reference period cannot be distinguished in regular NCS data, record check data from pilot studies provided evidence of such an effect. The obvious solution is to minimize the length of the reference period to increase the salience of crime incidents in respondents' memories at the time of the interview.

However, shorter reference periods place an added burden on the survey program. Compared to longer reference periods, field costs will be greater over a given time period because more interviews will be collected during this interval. Alternatively, for a constant data collection budget, sample sizes would have to be reduced to accommodate a shorter reference period, thereby increasing standard errors and decreasing the likelihood of obtaining statistically reliable results in data analysis.

Reference periods of 3, 6, and 12 months were considered for the NCS, with telescoping and cost being the major factors in the choice of a 6-month reference period. The 12-month reference period was deemed acceptable only if the temporal placement of a crime incident was not an important consideration. A 12-month period was also judged undesirable because collection of data on incidents occurring over a given time period would take considerably longer than with either of the other two reference period lengths. The 3-month reference period was also rejected because the amount of increased precision it would produce was insufficient, given the substantial increase in

costs it would entail. The alternative of cutting sample size to offset these costs was judged unsatisfactory because it would reduce the reliability of estimates below an acceptable level.

*Strategies for maximizing accurate reporting of crime incidents*

In designing the NCS questionnaire, the need to obtain full reporting and description of crime incidents had to be balanced against the burden on respondents. With these considerations in mind, planners for the NCS decided to elicit reports on the occurrence of all eligible crime incidents at the beginning of the interview, when respondents were likely to be most interested and least fatigued. Detailed information on the characteristics of each incident would be collected subsequently.

This initial "screening" has the advantage of focusing respondents on the task of recalling discrete events without distracting them by probing between victimization reports for the minutiae of each incident. This design also does not alert them to the number of incident-related questions that they will be asked to answer for each incident reported. If these questions had been asked immediately after any mention of a crime incident, respondents would undoubtedly have had an incentive to omit mention of other incidents they had experienced in order to minimize the length of the interview.

In designing the screener section of the questionnaire, care also had to be exercised to develop a set of questions that were thorough and prodded respondents' memories adequately, but that were not overwhelming in detail or in the time required to administer. One of the major goals of the field tests was to refine the screener to accommodate these two considerations.

*Balancing the need for reporting accuracy against difficulties in data collection to determine the optimum use of proxy interviewing*

Development of respondent rules for the survey entailed a number of considerations. Alternatively, the NCS could interview either all members of a household or a single individual who was available at the time of contact. Use of a single respondent would minimize field costs because repeated call-backs to interview absent household members would not be required. However, proxy data collected from such respondents for other household members might not be as accurate as data collected directly from these other individuals; clearly, a single household respondent might not be aware of the occurrence or details of victimizations against other household members. The effort to obtain data for each household member in turn might also prove fatiguing and lead to incomplete reporting.

A controlled test was incorporated into the San Jose and Dayton surveys to evaluate the relative advantages of these two approaches. In all households, screen questions dealing with household crimes were asked only once. However, in half the sampled households all residents were questioned regarding personal victimizations, while in the other half a single respondent was asked to report this information for all household members. Results showed that the method in which all eligible household members were interviewed yielded more reports of victimizations. These differences varied by type of crime, with the more pronounced differences occurring for petty larcenies and assaults. Consequently, the final NCS design required interviews from a single household respondent for household crimes, and interviews from all respondents regarding personal victimizations. The only respondents for whom a complete proxy was required were children ages 12 and 13.

*The most appropriate mode of data collection, that is, personal interviews vs. mail questionnaires*

The use of mail questionnaires to minimize field costs was considered before the NCS was implemented. An initial personal visit would be made to a household, but subsequent screening data would be collected by mail. Interviewers would be sent only to those households that indicated in the mail questionnaire that a crime victimization had occurred, and these interviewers would then collect detailed data on the incident(s). Because a relatively small proportion of respondents report the incidence of a crime victimization in a given month, this strategy had potential for reducing data collection costs substantially. However, testing of this methodology was disappointing. Response rates were substantially lower than those achieved with personal interviewing, and the quality of data gathered by mail was also poor. The complexity of the NCS instrument and the need for high response rates led to the conclusion that data collection by mail was not a viable option.

## Characteristics of the original NCS

The NCS was inaugurated in July 1972 and initially consisted of three distinct data collection programs:

### *National Crime Panel*

This data series provided annual data on personal and household crimes and initially relied on a national sample of 72,000 households. A rotating panel design was adopted, which required seven successive interviews at a housing location at 6-month intervals. To provide an even workload, a sixth of the active sample was interviewed each month, and, when the sample matured, housing locations that had been in sample for seven interviews were replaced by new housing units for subsequent interviews. This is the only surviving data series from the original NCS program and is now known as the National Crime Survey.

### *Commercial Victimization Survey (CVS)*

Launched at the same time as the National Crime Panel, this survey collected data on victimization of commercial establishments from a national sample of 15,000 businesses. The CVS sample was expanded in 1975, but data collection was suspended in September 1977 because of questions about the aims and design of the survey. An evaluation of the NCS by the National Academy of Sciences (NAS)<sup>4</sup> found the sample too small to allow reliable analysis of important questions, found the questionnaire content narrowly focused on information of which police would most likely be aware, and suggested that the goals of the survey would have to be more carefully developed before the CVS could be a useful data series. In addition, and perhaps most important, there were serious problems with the age and accuracy of the lists of establishments from which the sample was drawn. LEAA suspended data collection for this survey in response to the NAS evaluation.

<sup>4</sup> *Surveying Crime*, Bettye K. Eidson Penick, ed. (Washington: National Academy of Sciences, 1976).

### *City surveys*

In 1972 LEAA began using NCS methodology to evaluate the impact of its programs in 26 large central cities. Both household and commercial surveys were conducted, and samples of 12,000 households and 2,000 commercial establishments were drawn for this purpose in each city. The city surveys utilized a 12-month reference period and a cross-sectional design. While these surveys collected entirely unbounded data, producing inaccuracies in the level of crimes reported, the intent was to compare these data to later surveys of similar design to allow pre- and posttreatment measures of the effects of LEAA programs. Surveys were conducted in eight "Impact Cities" (Atlanta, Baltimore, Cleveland, Dallas, Denver, Newark, Portland, Oregon, and St. Louis) in 1972 and then again in 1975. Similar surveys were conducted in very large urban centers (Chicago, Detroit, Los Angeles, New York, and Philadelphia) in 1973 and in 1975, and one-time surveys were also administered in Boston, Buffalo, Cincinnati, Houston, Miami, Milwaukee, Minneapolis, New Orleans, Oakland, Pittsburgh, San Diego, San Francisco, and Washington, D.C., in 1974. These surveys were not continued due to the cost involved in administering them.

## Objectives of the NCS program

The survey administered to the national NCS sample has remained largely unchanged from its inception in 1972 until 1986, although modest revisions were made at several junctures during this period.<sup>5</sup> As the NCS program matured, additional uses for NCS data became apparent, and new BJS policies regarding the NCS developed as a result. Current BJS objectives for the NCS program are as follows:

- To provide trend data that will serve as a set of continuous and comparable national social indicators for the rate of victimization for selected crimes of violence and crimes of theft and for other factors related to crime and victimization in support of national criminal justice policy and decisionmaking and in support of informed public discussion.
- To provide policymakers at the national, State, and local levels as well as the research community with a data base that constitutes the best available empirical information concerning crime victims and victimization.
- To facilitate analytical research on issues of public concern and of consequence to the development of national, State, and local criminal justice policy.
- To provide empirical information relevant to understanding the differences between the rate of crime reported to police and the victimization rate.
- To provide empirical information concerning the characteristics of victims and consequences of the victimization that will be useful in designing, implementing, and maintaining victim assistance programs.

<sup>5</sup> For a detailed examination of these changes, cf. Elizabeth E. Martin, "Procedural History of Changes in NCS Instruments, Interviewing Procedures, and Definitions," manuscript (Washington: Bureau of Social Science Research, Inc., 1982).

- To provide empirical information that assists individuals and households in avoiding victimization.
- To assist State and local governments in evaluating the feasibility and utility of local victimization surveys.
- To provide empirical information on perceived satisfaction with the criminal justice system.
- To gather information on a regular basis concerning attitudes toward crime, criminals, and crime control.

### Uses of the NCS

NCS data have been used for a variety of purposes, including (1) generating BJS publications, (2) providing analytic opportunities for research organizations, and (3) offering information and guidance in applied and operational settings.

#### *BJS publications*

NCS data are used to produce annual publications and a variety of special reports. BJS releases three types of NCS publications annually:

#### Criminal Victimization in the United States

This comprehensive annual publication relies solely on NCS data and provides a basic compendium on the characteristics of NCS-measured crime, its victims, and its consequences. About 100 tables, updated each year, are included. The survey covers the crimes of rape, robbery, simple and aggravated assault, personal and household larceny, burglary, and motor vehicle theft. Data also are presented for sub-categories of each of these crimes.

#### Preliminary and final change reports

These releases detail annual changes in major NCS crime categories and are available before Criminal Victimization in the United States is published. The preliminary data, released in press-release form each March, are developed from data collected through December of the previous year. This release provides the first estimate of changes in the victimization rate for the previous calendar year. The final data, based upon full data collection completed in June, are published in a BJS Bulletin released in September/October of each year. This report covers changes in crime trends and trends in police reporting rates.

#### Households Touched by Crime

This annual bulletin characterizes the impact of crime by determining the proportion of U.S. households in which members have been victimized.

These data are provided for basic household characteristics, such as race, income level, and location; data from previous years also are included to permit examination of trends.

In addition to these annual publications, NCS data have been analyzed for a variety of special reports dealing with particular types of crime (for example, rape, household burglary, and robbery), crimes against special populations (Hispanic victims, the elderly, teenage victims, and domestic violence against women), and particular features of crime (the use of weapons, family violence, the cost of crime, the risk of violent crime, and the characteristics of violent crimes committed by strangers and non-strangers). NCS data have also been utilized for The Report to the Nation on Crime and Justice. Victimization data are included in a number of other publications, including the annual Sourcebook of Criminal Justice Statistics and the yearly Statistical Abstract of the United States.

#### *Research*

Researchers at colleges and universities and at research institutes have analyzed NCS data extensively in preparing scholarly publications and other reports. Machine-readable NCS data are made available through the Inter-university Consortium for Political and Social Research in Ann Arbor, Michigan. Files are updated regularly and include both complete files of NCS data in hierarchical format and extract files for victims and incidents. The former are structured files designed to minimize storage space for the variable-length records that inevitably result from households of different sizes and from households and individuals who experience different numbers of crimes. Computer software designed to analyze files with this structure is required to access these files. The victim and incident extract files are unstructured, rectangular data sets that contain records for individual victims and crime incidents for a particular period. These files are much easier to access and analyze

## Genesis of the NCS redesign

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than the hierarchical files, and a wider variety of software may be used, but the available analytic options are more limited than with the full NCS files.

### *Applied uses*

NCS data have been used to inform a wide range of audiences concerned with crime and crime prevention. NCS findings have been studied by community groups and government agencies seeking to establish neighborhood watch, victim assistance, and victim compensation programs. Data have been sought by victim advocacy groups trying to improve treatment of crime victims. Police academies have used NCS findings in their training of cadets. NCS data have been used to prepare public service announcements, crime prevention information sheets, and documentaries related to crime. Print and broadcast media have made use of survey results to provide insight into a host of crime-related topics.

Officials and legislators at the local, State, and Federal levels have requested data to inform policy and legislation and to deal with problems of special populations, such as teenagers, the elderly, and black victims. NCS data have also been cited in a number of judicial decisions. In addition, these data have been used by the business sector, such as insurance companies, or market research firms seeking to establish the potential market for products, such as burglar alarms, intended to reduce the likelihood of criminal victimization.

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The roots of the NCS redesign extend back a decade to an evaluation of the NCS by the National Academy of Sciences (NAS). The academy's recommendations concerning ways in which the program should be redesigned were published in 1976.<sup>1</sup> Among the NAS recommendations were the following:

- More NCS resources should be devoted to "delineation of product objectives, to managerial coordination, to data analysis and dissemination, and to a continuing program of methodological research and evaluation."
- The NCS should produce "not only nationwide and regional data, but, on the same timetable, estimates for separately identifiable Standard Metropolitan Statistical Areas (SMSA's) and for at least the five largest central cities within them..."
- The NCS screener, that is, that part of the questionnaire that ascertains whether the respondent has been a crime victim, needs to be drastically altered to increase its effectiveness in prodding respondents' memories and to minimize its complexity.
- Additional questions need to be added to allow measurement of independent variables important for understanding the dynamics of crime victimization. These would include questions dealing with ecological factors, victim characteristics, lifestyle, and protective or preventive measures.
- "A major methodological effort on optimum field and survey design for the NCS should be undertaken."

Following the academy's evaluation, an internal review of the NCS program was begun. In conjunction with this review, a conference was held in 1978 to discuss topics and priorities for a 5-year research program on national victimization survey statistics. This conference produced a

<sup>1</sup>*Surveying Crime*, Bettye K. Eidson Peniek, ed. (Washington: National Academy of Sciences, 1976).

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report outlining a number of important methodological research questions that merited investigation.<sup>2</sup> A study of the utility and benefits of the National Crime Survey was also conducted as part of this agency-sponsored review of the NCS.<sup>3</sup> These assessments indicated a need for intensive examination and subsequent redesign of the NCS. A request for proposals (RFP) was published in 1978<sup>4</sup> to solicit bids on a contract to perform this work, and in 1979 the contract was awarded to the Bureau of Social Science Research (BSSR). BSSR headed a consortium of experts in criminology, survey design, and statistics who contributed to various phases of the project. In addition, the project received guidance from an advisory panel drawn from the criminal justice, statistics, and social science communities.

<sup>2</sup>"Toward an Agenda for Research on National Victimization Survey Statistics," Albert D. Biderman, ed., manuscript (Washington: Bureau of Social Science Research, Inc., 1978).

<sup>3</sup>Philip S. McMullan, Jr., James J. Collins, Jr., Robert Gandossy, and Joan Gutmann Lenski, "Analysis of the Utility and Benefits of the National Crime Survey (NCS)" (Research Triangle Park, N.C.: Research Triangle Institute, 1978).

<sup>4</sup>"National Crime Survey (NCS) Redesign, Request for Proposal No. J-002-LEAA-9" (Washington: Law Enforcement Assistance Administration, 1978).

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The NCS redesign project was intended to be a comprehensive reexamination of all aspects of the survey, including questionnaire design, sample, collection strategies, administration of the survey, error properties, analytic capabilities, dissemination of data and findings, and utilization of NCS data.

To undertake this broad investigation of the NCS program, BJS sought a consortium of organizations possessing different skills as the most appropriate organizational model for the conduct of the redesign. The contractor for the project was selected competitively.

### **Consortium structure**

The prime contractor was the Bureau of Social Science Research (BSSR) in Washington, D.C. BSSR was ultimately responsible for the quality and timeliness of all work submitted during the course of the project and provided liaison between BJS and the various performance units and consultants comprising the Crime Survey Research Consortium (CSRC). In addition, BSSR served as the point of contact for consortium members with the Inter-university Consortium for Political and Social Research (ICPSR), which disseminates NCS public-use tapes. BSSR provided bibliographic services to all participants in the consortium as well as to the sponsor agency, maintained a computer teleconference to facilitate interaction among all organizations and individuals working on the project, undertook much of the questionnaire design activities during the latter part of the project, developed computer routines to facilitate analysis, and performed research on survey error and NCS file management problems.

BSSR also coordinated interaction between CSRC members and the Census Bureau. Consortium members communicated regularly with Census Bureau staff in the Demographic Surveys, Statistical Methods, and Field Divisions who were active in on-going NCS work. These staff members provided information on NCS design and activities, provided special data sets to the consortium for analysis, developed statistical models required for decisionmaking at various points in the project, and provided invaluable advice on the effects and feasibility of proposed NCS design changes in the ongoing NCS data collection program. CSRC members were active participants in redesign work carried out by the Census Bureau, particularly in development of a feasibility study for Computer-Assisted Telephone Interviewing (CATI) and in design of the Victim Risk Supplement (VRS) and the revised questionnaire implemented in July 1986.

The composition of CSRC varied over the life of the project, reflecting the changing nature of the work required during different phases of the redesign. The following organizations, listed with their primary responsibilities, participated in CSRC at one time or another:

- 1) Carnegie-Mellon University (statistical modeling and design, series splicing);
- 2) National Opinion Research Center (questionnaire design);
- 3) Research Triangle Institute (error modeling, utilization);
- 4) Survey Research Center, University of Michigan (sampling, data collection modes (CATI), field testing, questionnaire design);
- 5) Westat (adaptation of CSRC work to Census Bureau environment, longitudinal design); and
- 6) Yale University (organization of complex data structures, longitudinal matching).

In addition, a number of consultants were hired for various short-term and intermittent tasks, which included statistical modeling and questionnaire construction.

### **Advisory Panel**

Recommendations were also obtained from an Advisory Panel selected specifically for the NCS redesign project. Membership was drawn from a variety of communities, including statistics, criminology and criminal justice, victim assistance, and survey methodology. Panel meetings were held roughly once a year and were also attended by project staff, Census Bureau NCS personnel, and NCS staff members from BJS. Depending on the phase of the project under way, panel members were informed of project plans, research and testing, results from tests, and unresolved problems. Although CSRC members were also experts in many of these areas, the panel provided a useful source of advice in developing priorities for methodological work and in planning end products of value for users of NCS data.

### CSRC teleconference

Coordinating the efforts of this large, diverse, and geographically dispersed group of project staff and advisers was a complex undertaking. To facilitate interaction among participants, BSSR established a computer-based teleconference, first at the University of Michigan and later at Wayne State University. Computer accounts were provided for project, BJS, and Census Bureau staff and interested Advisory Panel members. The teleconference software enabled users to send private messages and to post public "items" and comments on suggestions for project work. Longer documents were entered in files whose location was reported to intended recipients via the teleconference.

Such communication supplanted much hard-copy communication and provided some advantages over the latter. For instance, when a participant desired information on a particular topic, the teleconference software enabled a search for key words in all teleconference "items," thereby providing a means to assemble all text on the subject quickly. The teleconference also provided an archival record of redesign products and of interactions among project participants, thereby preserving much of the intellectual work that would not be reflected in final memos and papers on various redesign topics.<sup>1</sup>

<sup>1</sup>BJS staff, Census Bureau personnel, and a large portion of project participants were active users of the teleconference. This interaction generated a large number of useful suggestions for NCS changes and project research. However, the facile communication that the teleconference encouraged did create some problems for project management. One difficulty was that participants with limited involvement were not able to keep abreast of the large amount of material generated. Another problem was that because many suggestions did not fall into the formally assigned tasks, some promising suggestions were not pursued. BJS and the contractor became aware of this problem early in the project but never resolved it completely, largely because the man-hours available from project staff did not permit development of all suggestions and required setting priorities for completion of work. Because the archival function of the teleconference preserves these suggestions, they may be pursued at a later date, if BJS finds this useful and feasible.

### Redesign task-oriented committees

In addition to the consortium and the Advisory Panel, other bodies were established during the life of the project to address specific problems:

#### *Longitudinal Task Force*

The NCS utilizes a "rotating panel design" in collecting data about criminal victimization. This design involves selecting a household as part of the sample and then reinterviewing residents at the same location every 6 months for a total of seven visits. Although many of the same respondents remain at a household from interview to interview, the data are processed and analyzed cross-sectionally and do not take advantage of the survey's potential for longitudinal analysis. Some researchers have undertaken the task of linking records across interviews to enable certain longitudinal analyses. However, many respondents move into or out of sample housing units during the life of a panel, which creates many discontinuities in respondent records for a longitudinal data set assembled in this manner. One proposal suggested by consortium members was the adoption of a true longitudinal design for the NCS that would follow initial sample respondents during the life of a panel, even if they move from the original sample location, rather than the current procedure that requires returning to the original sample address for all seven interviews, even if all original respondents have moved.

To study the desirability and feasibility of such a change, the consortium created a Longitudinal Task Force, which consisted of consortium members, BJS staff, and Census Bureau personnel. Convened in December 1982, the task force submitted a report in June 1983.<sup>2</sup>

<sup>2</sup>Thomas B. Jabine, "Longitudinal Design Task Force Findings and Recommendations," manuscript (Washington: Bureau of Social Science Research, Inc., 1983).

that discussed the desirability of a longitudinal component and proposed a test to evaluate its feasibility. Although the test was not implemented, the work of the Longitudinal Task Force provided the basis of later discussions regarding development of a longitudinal design, which will be evaluated in a later section of this report.

#### *Implementation Task Force*

After the NCS redesign project had produced a number of findings related to possible changes in the survey, BJS formed a task force in December 1983 to evaluate the utility and feasibility of proposed changes and their impact on estimates and to schedule revisions to the NCS. The task force was chaired by the Deputy Director of BJS and was composed of staff members from the NCS Redesign Consortium, BJS, and the Census Bureau. As an adjunct to the task force, a work group drawn from staff members in all three organizations evaluated proposed changes for desirability, feasibility, and effects on NCS estimates. These evaluations of the numerous options for change were subsequently presented to the task force, and recommendations resulting from task force discussions were subsequently provided to the Director of BJS.

The task force recommended that proposed NCS revisions be divided into two groups. The first changes to be adopted would be those that provided immediate improvements to the survey with minimal potential impact on rates or programming of the NCS processing system. The second set of changes would comprise more basic changes to the design of the survey and would most likely affect estimates for crime levels and rates. To minimize disruption to the NCS series, the task force discussed plans to implement these rate-affecting changes simultaneously, if feasible, so that only one series break would result. The task force also discussed options for "splicing" the new NCS series to the old, so that differences due to instrumentation and data collection changes could be measured.



# Objectives of the NCS redesign

The NCS redesign was intended to be a comprehensive reevaluation of the means used to collect victimization data. The goals for this project were articulated in an address given by the Director of the LEAA Statistics Division at the American Statistical Association meetings in 1978.<sup>1</sup> These goals were further elaborated in the RFP for the project issued in the same year.<sup>2</sup> The required tasks listed in the RFP included the following:

## Conceptual issues

- alternative methods for measuring criminal victimization;
- usefulness of various external validation sources to determine how to treat events with definitional problems (for example, victim provocation, uncertainty of respondent regarding whether there was evidence of crime, and determining how to treat events involving children);
- scope of crimes to be covered;
- development of concepts and statistical models relating to multiple and time-extended victimization;
- measuring risk and vulnerability to crime; and
- evaluating the utility of other policy-relevant (independent) variables meriting inclusion on instruments or in the sampling design.

## Methodological issues

- method of interviewing--telephone vs. personal interviews, computer-assisted telephone interviews;
  - reference period choices--optimal reference period to meet objectives of the survey;
  - bounded vs. unbounded interviews; and
  - alternative sampling designs.
- studies concerning ways in which the data can best be organized and analyzed.

<sup>1</sup>Benjamin H. Renshaw III, "A Managerial Perspective on the Redesign of the National Crime (Victimization) Survey," Annual Meeting of the American Statistical Association, unpublished.

<sup>2</sup>"National Crime Survey (NCS) Redesign, Request for Proposal No. J-002-LEAA-9" (Washington: Law Enforcement Assistance Administration, 1978).

## Analytical issues

It is beyond the scope of this report to detail the voluminous material produced over the course of the NCS redesign that examined these and other issues related to improvements in the NCS program.<sup>3</sup> The discussion below will focus on six key redesign issues. In this context, conceptual and methodological problems addressed in studying these issues will be described, along with the benefits and liabilities of implementing relevant redesign options.

<sup>3</sup>See Appendix A for a listing of major NCS redesign products. BJS intends to publish a volume of NCS redesign-related papers at a future date to circulate this material more widely. In the interim, individual papers cited are available on request from BJS.

## Accuracy

There are several sources of response error in NCS data. Among these are respondent failure to report crime incidents, errors in the temporal placement of reported victimizations, and inaccuracy in reporting incident details. The redesign project examined a number of features of NCS data collection in which changes might improve the quality of reported crime incident data.

### *Screening strategy*

As noted above, the NCS questionnaire is divided into two components, an initial screener administered to all respondents and a crime incident form, which is administered for each crime incident reported and which collects detailed information about the incident. Redesign project staff investigated a number of changes in screening strategy that had potential for increasing the number of eligible incidents reported. A major question was whether the current approach, which involves asking respondents directly about several types of violent or theft-related events, adequately stimulated respondents' memories of crimes that had occurred to them during the previous 6 months. A related concern was that certain classes of respondents, differentiated by education, income, and race, might react differently to the screening task, thereby producing biases related to these attributes.

The NCS Redesign Consortium sponsored a conference on "Applying Cognitive Psychology to Recall Problems of the Survey" in September 1980 to explore these and related issues, and subsequent work on screening strategies was in part guided by the work of this conference.<sup>4</sup> The major proposed changes that were ultimately tested included asking respondents about victimizations that occurred in various life

<sup>4</sup>Cf. Jeffrey C. Moore, "Report of the Workshop on Applying Cognitive Psychology to Recall Problems of the National Crime Survey," manuscript, 1980.

"domains" such as work and leisure, providing many short cues to help trigger memory of incidents in these life contexts, and attempting to evoke the sort of emotional states that might result from a crime incident (such as anger or fear) before administering the screen questions. The aim of these innovations was both to elicit increased reporting of crime incidents and to structure the recall task to a greater degree, so that cognitive and subcultural differences among respondents would have a smaller impact on the reporting of crime incidents.

NCS respondent rules for eliciting reports of personal and household crimes were also studied. As originally implemented, all household members age 14 and older were interviewed directly to determine whether they had experienced any violent or personal property crimes. Proxy interviews were obtained for respondents ages 12 and 13. For household crimes (household larceny, burglary, and motor vehicle theft), one knowledgeable household member was selected as a household respondent to provide reports on any eligible incidents that had occurred.

A number of problems with these procedures were identified. First, proxy interviews have been shown to be less reliable than personal interviews.<sup>5</sup> The redesign project advocated interviewing all respondents age 12 and older to minimize the effect of proxy interviewing on data quality. Second, research conducted as part of the redesign indicates that household respondents are more likely to report personal incidents than are nonhousehold respondents. For those individuals whose household respondent status changed from one interview to the

next, their rate of reporting of personal crimes showed a corresponding change.<sup>6</sup>

There are several explanations for this effect. For reports of personal larcenies without contact, NCS interviewing procedures can contribute to household/nonhousehold reporting differences. When several respondents in a household report the same no-contact larceny away from the home, this incident is assigned to the first respondent to report it. Because the first respondent to be interviewed is almost invariably the household respondent, this procedure artifactually inflates personal larceny reports of household respondents.

Another artifact, contributed by the NCS questionnaire used for many years, results from the way in which some victimizations involving motor vehicles were reported. Theft of motor vehicle parts is classified as personal larceny if the theft occurs away from the home, and such a theft is assigned to the respondent who reports the incident. However, only the household respondent is explicitly asked about this type of theft. The fact that household respondents are more likely to report theft of motor vehicle parts can be attributed largely to this difference in screening procedures for household and other respondents.

Finally, it is possible that administration of household questions serves to "warm up" respondents to the task of recalling crime incidents, making household respondents more effective at answering the personal screen questions that follow the household questions. Nonhousehold respondents, who do not answer household screen questions, do not have the benefit of this additional stimulus to their memories.

The NCS redesign staff recommended adopting a uniform screening procedure that would administer household screen questions to all respondents.

<sup>5</sup>Cf. Albert J. Reiss, Jr., "Victimization Productivity in Proxy Interviews," manuscript, (New Haven: Institution for Social and Policy Studies, Yale University, 1982).

<sup>6</sup>Albert D. Biderman, David Cantor, and Albert J. Reiss, Jr., "Household and Secondary Respondents: A Quasi-Experimental Analysis of Interviewing and Classification Problems in the National Crime Survey," manuscript (Washington: Bureau of Social Science Research, Inc., 1982, revised 1985).

This change would provide the benefit of warming up all respondents with the household screen items. In addition, it is likely that such a change would produce more reports of household victimizations because a single household respondent may not recall or be aware of household victimizations that other household members may report. Such a change would also enable reduction of the conceptual confusion caused by the current definitions of household larceny and personal larceny without contact, which are differentiated not by the nature or value of property taken, but by the location from which property was stolen.<sup>7</sup> A difficulty created by this proposed change in respondent rules is that a single household incident may be reported several times during the interview by different respondents. A number of procedures for recognizing and adjusting for such multiple reports of the same incident have been suggested, such as computer-based unduplication procedures and the development of a multiplicity estimator, which would reduce the weight of a household incident in estimation, based on the number of household members eligible to report.

#### *Bounding*

Another source of NCS error is the reporting of incidents that occurred outside the reference period for the interview. NCS data collection procedures are designed to minimize such error by "bounding" interviews with data collected during previous interviews. Incidents reported during the second through seventh interviews at a housing location are checked against data reported during the previous interview to ensure that the incident is indeed a new one and did not occur during the previous reference period. Because the first interview is unbounded, these data are not currently used for estimation and are used only to bound the subsequent interview. However, if a respondent is unavailable for one or more interviews while he or she is in

<sup>7</sup>For instance, a bicycle stolen from a teenager would be classified as a personal larceny if it were stolen from a schoolyard. However, theft of the same bicycle would be classified as a household larceny if it disappeared from the driveway of the home.

sample and is then subsequently interviewed, data collected during this later interview are necessarily unbounded. Also, data will be unbounded for individuals who become household members after the initial interview of a household and for replacement households that enter the sample because the original household has moved. Unlike data collected during initial interviews, unbounded data collected in these cases are in fact used in estimation. The proposed strategy to reduce the amount of such unbounded data is the adoption of a longitudinal design for the NCS, in which original sample persons would be retained in sample and followed if they moved from the original sample location.

#### *Reference period*

The NCS redesign project investigated the effect of reference period length on the volume of crimes reported and found that reported incidents are not distributed evenly throughout the reference period, as one might expect. In fact, a disproportionate number of events are reported in the months closest to the interview.<sup>8</sup> This finding could result either from forward telescoping within the reference period or from an increased likelihood of forgetting incidents that occurred further from the date of interview. The conclusion reached was that shortening the reference period would increase the accuracy of NCS reporting.<sup>9</sup>

<sup>8</sup>Cf. Albert D. Biderman and James P. Lynch, "Recency Bias in Data in Self-Reported Victimization," Annual Meeting of the American Statistical Association, Detroit, 1981, unpublished.

<sup>9</sup>Cf. Edward L. Kobilarcik, Charles H. Alexander, Rajendra P. Singh, and Gary M. Shapiro, "Alternative Reference Periods for the National Crime Survey," Proceedings of the Section on Survey Research Methods (Washington: American Statistical Association, 1983), pp. 197-202; David Cantor, "Operational and Substantive Implications of Changing the NCS Reference Period," Annual Meeting of the American Statistical Association, Las Vegas, 1985, unpublished.

However, such a change would not come without cost. Assuming a constant budget for field costs, shortening the reference period would require a reduction in the number of respondents interviewed to accommodate the increased field costs required for more frequent interviews. This would increase standard errors, and statistical tests would be less likely to generate significant findings as a result. The alternative would be to maintain sample size and increase field costs for the survey. The choices available then allow the survey sponsor to optimize on only 2 of the 3 parameters of accuracy of reporting, statistical reliability, and cost. Implementation of such a change in the reference period requires examination of the importance of all these parameters for the integrity and quality of the data series.

#### *Interview-to-interview recounting*

Another issue related to the length of the reference period is the question of when to terminate the reference period in recording incidents during an interview. The current practice is to accept incidents that occurred during the 6-month period ending on the last day of the month preceding the interview. This procedure provides a clean, temporally defined breakoff, which permits accurate computation of crime estimates for a specific period and which facilitates trend analysis. However, because interviews may occur weeks after the end of this period, this practice may exclude incidents occurring immediately prior to the interview, when details of the event are fresh in respondents' memories. Postponing collection of data on these incidents to the subsequent interview may lead to less accurate recall of incident characteristics and also result in respondents completely forgetting to report the incident. The NCS Redesign Consortium advocated an interview-to-interview recounting period, which would result in reports of all incidents occurring after the previous interview being reported to NCS interviewers. Not only would

this change in reporting procedures result in enhanced accuracy of victimization reporting, but it would also provide some flexibility in scheduling interviews beyond the current monthly closeout dates. This latter advantage could be particularly useful in scheduling interviews out of a centralized telephone facility. Some changes in the NCS processing system would be required if such a change in recounting procedures were implemented to facilitate estimation for given time periods. Specifically, reports of incidents that occurred during the month of interview would have to be deleted from the record of this interview and attached to that for the subsequent interview.

#### *Calendrical anchoring*

The consortium also investigated ways to enhance the reliability of dating incidents within the reference period. Respondents are often unable to report precisely when an incident occurred, and the development of "calendrical anchoring" devices was explored to help respondents report the dates of crime incidents more precisely. Salient benchmarks (national or religious holidays) and personally significant dates (birthdays and anniversaries) would be recorded, and respondents would be asked whether an incident occurred before or after these events.

#### *Series crimes*

One class of crime that presents particular difficulties for estimation and for collecting detailed crime data is the series crime—that in which the respondent has been repeatedly victimized to the point that details blur in memory and cannot be disentangled into separate crime incident reports. (Some examples are repeated spouse abuse, frequent incidents of vandalism, and regular threats or actual assault at school.) A series incident is currently defined as a crime in which at least three similar incidents have occurred and for which the respondent cannot recall dates and other details well enough to report them separately.

At this time, crime incident data are collected only for the most recent series event.

Series incidents have been excluded from annual victimization estimates published by BJS and in recent years have been counted as only one victimization in special NCS reports. In both cases, crime incidents are obviously undercounted, but BJS has been reluctant to weight these incidents up to the estimated number of recurrences. One reason for this practice is that several different types of crime may be bound together in a series. In such a case, the type of crime committed in the last incident in the series would be inflated above its actual level. Similar distortions may occur when respondents indicate that they have experienced a very large number of victimizations (for example, "This has happened to me hundreds of times."). Another problem is that multiple victimizations, such as continual spouse abuse, are not easily recalled as a discrete number of victimization incidents, and attempting to record this type of series victimization as an enumeration of incidents may not accurately reflect the nature of this type of victimization.

The NCS Redesign Consortium has recommended that the manner in which series victimization data are collected and estimated be reevaluated in order to reduce the error generated by this type of victimization reporting in NCS data. In particular, the consortium recommended that the current threshold of three incidents for recording a series victimization be raised and that additional questions on series be included.

### Enhancement of analysis options

Although the NCS offers a rich source of data on the incidence and characteristics of personal and household victimization, a number of shortcomings have been identified as BJS and other users have gained experience with NCS data. The consortium studied and recommended improvements in a number of data characteristics, which include:

- independent variables useful for measuring the risks of crime for various locations, population subclasses, and life activities;
- variables dealing with the outcomes of crime;
- the scope of crimes measured by the NCS;
- different schemes for classifying crimes; and
- alternate strategies for organizing NCS data.

#### *Independent variables*

**Victim behavior.** One of the most obvious problems with the original questionnaire was its failure to provide much useful information on the interaction between victims and offenders during a violent crime incident. The NCS collected data on the number and characteristics of offenders and on their actions during an incident, but information on victim behavior was restricted to descriptive data on any actions taken by the victim during the incident. The structure of this questionnaire precluded determining whether a victim's self-protective actions affected the outcome of the incident or even in some cases whether a reported action was taken in response to an attack or threat.

A number of approaches for correcting this difficulty were investigated, including asking victims to describe the sequence in which various victim and offender actions and outcomes occurred. (For example, victims could be asked whether they took a particular action before or after they were threatened, attacked, or injured.) The potential complexity of such a detailed chronology for a situation in which actions may have been taken

very quickly and under stress led to the conclusion that such data could be unreliable. In addition, the difficulties of designing and administering a question sequence appropriate for a wide variety of violent crime incidents would probably be insurmountable.

The final proposed solution was to ask respondents what they did about the incident while it was in progress (using a more detailed coding scheme than the original design), whether they believe their actions helped or hurt their situation, and, if so, what the nature of the impact was. In addition, new questions were proposed to help determine the effect of actions by other individuals who may have been present and to examine whether the respondent, the offender, or someone else was the first to use, or threaten to use, physical force. As a group, these revisions should provide a more accurate picture of the effects of various types of interaction among victims, offenders, and bystanders on the likelihood of crime completion and victim injury; they should also permit more reliable detection of those instances in which respondents may have helped precipitate incidents or in which they may have been offenders themselves.

**Lifestyle variables.** Information collected on respondents in the original NCS dealt with age, race, ethnicity, sex, marital status, relationship to other household members, occupation, membership in the Armed Forces, education, frequency of residential mobility, and family income. While these data are useful for distinguishing the victimization experiences of special populations, they do little to illuminate the circumstances under which victimization is more or less likely to occur. One major component of NCS redesign work was to develop and test new questions to provide data on the lifestyles of respondents related to their likelihood of experiencing victimization. These questions dealt with:

- occupational responsibilities, hours, locations, and contacts;
- commuting patterns to work and school;

- for students, perceptions of safety in and around schools;
- evening, shopping, and leisure activities;
- neighborhood characteristics;
- perceptions of safety at home, in respondents' neighborhoods, and in other places where respondents spend time;
- precautions taken by respondents to protect themselves, their home, and their property;
- neighborhood surveillance activities, including existence of and participation in neighborhood watch programs; and
- for respondents who have experienced personal victimization, what they were doing at the time of the incident and, if they were commuting, what means of transportation they were using.

The NCS redesign program also developed observational items for interviewers to fill out, dealing with characteristics of the housing unit and surrounding neighborhood that are relevant to the safety of respondents and their dwellings. This entire set of questions was administered as a Victim Risk Supplement to the regular NCS in 1984 and demonstrated strong potential for enriching the NCS, either as regular items or as part of periodic supplements. Such items not only illuminate the risk of various types of crime in different life settings, but they also can provide new data useful for testing different theoretical models of susceptibility to victimization.

Other independent variables. In addition to the sets of items described above, other new or revised questionnaire items useful for illuminating the dynamics of criminal victimization have been proposed as part of the redesign. These include new questions dealing with perceived substance abuse by offenders and with multiple victimizations by the same offender. Expanded coding has also been recommended for the place of crime occurrence and for weapon use by offenders. This latter change has been dictated by deficiencies in the original questions and skip patterns that often made it difficult to

determine whether or how an offender used a weapon during the course of an incident.

#### *Outcome variables*

Although the NCS has provided a large quantity of data on the consequences of victimization since its inception, a number of improvements have been suggested to improve its analytic utility. These changes include clarification of the means used by offenders to cause injury, expanded coding for property loss, and new items detailing contacts with the criminal justice system. Although the new criminal justice contact questions offer an improvement over the original questionnaire, the utility of such items is limited by the rotating panel design and 6-month reference period of the current NCS because the duration of such contacts may often extend beyond the length of the reference period. The possible implementation of a longitudinal design, which would enable interview-to-interview record linkage and permit followup items to be administered in a subsequent interview, offers the possibility of enhanced utility for such items.

#### *Scope of crimes covered*

Since its inception, the NCS has collected data on rape, personal robbery, assault, personal and household larceny, burglary, and motor vehicle theft. The NCS Redesign Consortium devoted part of its efforts to an investigation of ways in which the scope of crimes measured by the NCS could be expanded. Among the possibilities investigated were bombings, parental kidnaping, arson, fraud, and vandalism. A number of the crimes studied did not appear to be promising for measurement, using victim survey methods, because of the rarity of the crime or concerns about the potential unreliability of victim reports. Vandalism appeared to be the most promising addition to the survey, but several measurement difficulties had to be overcome before it could be included as a regular NCS crime type. One problem is that personal and household vandalism must be distinguished from other types of

vandalism, such as damage to common areas in apartment buildings or damage to objects in neighborhoods, such as street signs. Another difficulty is that the NCS crime incident form has not been appropriate in many ways for the measurement of this crime, and alternate ways to collect incident data had to be developed. In addition, we would not be able to obtain information on frequent targets of vandalism, such as schools or businesses, which would limit the analytic utility of these data. Finally, many vandalism incidents, such as damage to screens or windows, may be confused with attempted burglaries. We expect that many incidents that would be recorded as the latter in the current instrument will be reported as vandalism, if this type of crime is included in the regular NCS. As a result, we would experience a reduction in the number of attempted burglaries, solely as a result of such a questionnaire change.

#### *Alternate classification schemes*

As discussed in the section providing background on the NCS, designers of the survey decided to classify crime victimizations in a way that would facilitate comparison with UCR crimes, while also allowing alternate classifications to be developed. A number of such typologies were developed during the course of the NCS redesign by staff members of the redesign project, BJS, and the Census Bureau. These new classification schemes facilitate study of households touched by crime (and alternatively households free from crime), home intrusion, crimes in which motor vehicles either were the objects of crime or were used in the commission of crime, domestic violence, and crimes committed by strangers and nonstrangers.<sup>10</sup> Such

<sup>10</sup> Cf. "Households Touched by Crime, 1986," BJS Bulletin, NCJ-105289, June 1987; James P. Lynch and Albert D. Biderman, "Cars, Crime, and Crime Classification: What the UCR Index Doesn't Tell Us That We Should Know," Annual Meeting of the American Society of Criminology, Cincinnati, 1985, unpublished; "Family Violence," BJS Special Report, NCJ-93449, April 1984; "Violent Crime by Strangers and Nonstrangers," BJS Special Report, NCJ-103702, January 1987.

typologies have illustrated the ways in which our understanding of the dynamics of crime can be expanded beyond the information available from legally based classifications and demonstrate the utility of developing additional attribute-based classifications for criminal events.

#### *Longitudinal design*

In addition to changes in the NCS questionnaire, changes in NCS data collection and processing were discussed that had potential for increasing analytic options. Specifically, substitution of a true longitudinal design for the current cross-sectional design was evaluated in terms of its potential for enhanced accuracy, analytic options, feasibility, and cost. The NCS has had some features of a longitudinal survey since its inception, in that rotating panels of households are interviewed for seven successive interviews at 6-month intervals. However, no attempt has been made to retain in sample those respondents who move, though attempts to link NCS records have been performed post hoc by independent researchers for special purposes. Retaining in sample those respondents who move and introducing a longitudinal processing system that facilitates the linkage of records will allow use of more powerful statistical techniques for calculating annual change estimates and will also enhance the long-term representativeness of a population-based NCS sample, thereby reducing error in these estimates. In addition, introducing such a design will allow us to address a number of important analytic issues for the first time, including the relation of criminal victimization to the decision of respondents to move.

Long-term consequences of criminal victimization. Often, a criminal victimization has a major impact on the victim's life that extends beyond the length of the NCS reference period of 6 months. The current cross-sectional design does not allow us to measure adequately the prevalence and duration of these consequences, such as long-term disability or the filing and

settlement of insurance claims. A longitudinal design will permit us to collect followup data in subsequent interviews and to merge this information with original incident records to gain a more adequate picture of these enduring consequences of victimization. While the number of interviews in which such followup information may be collected is limited by the rotating panel design of the NCS, data thus obtained will be far more comprehensive than current measures.

Victim contacts with the criminal justice system. In addition to a lack of information on the long-term consequences of crime for victims, the NCS has so far been unable to provide a systematic picture of the interaction of victims with the criminal justice system, which is largely a function of the length of time required to bring a case to trial. A longitudinal design will enable us to collect followup information on such contact, thereby allowing us to measure the proportion of victims having contact with police, prosecutors, and court personnel in case processing and also permitting analysis of differences in the extent of contact for different types of crime and victim characteristics. This design will also allow us to determine the proportion of victims having contact with ancillary organizations, such as victim-witness assistance groups, and to study whether there is a relationship between such contact and the processing or outcome of cases.

Multiple victimization. Another area for which a longitudinal NCS promises enhanced analytic utility is the study of repeated victimization. Our ability to link records over time will allow study of multiple victimizations whose incidence extends beyond the length of the reference period. We will be able to determine both the proportion of victims who experience one-time, periodic, or relatively continuous victimizations and also the factors--such as the type of crime and victim or offender characteristics--that vary across these different temporal patterns.

We will be able to assemble and analyze attributes of victimization histories such as periodicity, duration, and temporal causal factors in ways that are not possible with the current design. In addition, the longitudinal design will have benefits for related macro-level analysis because we will be able to measure gross change in victimization in addition to the net change measures that are currently available. Specifically, we will be able to determine the degree to which respondents victimized in one year also account for victimizations in other years; this information will have important implications for our ideas about the randomness of crime and the probabilities of victimization over extended periods.

Causal factors in victimization. A longitudinal design will allow more extensive analysis of variables contributing to criminal victimization in that change in these variables and their consequent effect on the likelihood of victimization may be determined. Differences in the likelihood of victimization have been noted for such factors as occupation and marital status. However, a longitudinal design will allow more precise isolation of the impact of these factors by providing measures of victimization levels before and after changes in these variables. In addition, a longitudinal design that follows movers will allow adequate testing of the hypothesis that geographic mobility is, to a certain extent, a function of crime vulnerability.

A number of questions important for the ultimate implementation of a longitudinal NCS have yet to be resolved. Among these are the degree of effort that it is reasonable to undertake in following movers, the field procedures required to produce reliable linkage of records across interviews, the decision rules to follow in reporting household data for households that have dissolved since a prior interview, and the nature of the processing systems required for producing annual estimates and longitudinal files in the Census Bureau environment.

## Flexibility

One complaint that researchers who utilize the NCS have expressed is that the focus of the survey is relatively narrow for their needs. While the social indicator function of the NCS as a barometer of the level and prevalence of crime in the United States is a central feature of the survey, as well as the delineation of the victim proneness of special populations, the NCS has a great deal of unused potential as a research tool. To respond to this need for analytic flexibility, BJS plans to attach supplements to the survey more frequently. These supplements would include regularly scheduled sets of items to collect data periodically, which are not deemed absolutely essential for regular administration. The remainder would be one-time supplements on crime-related topics of interest to policymaking officials for which the NCS would be an appropriate vehicle.

Planning for these changes is well under way. BJS and the National Institute of Justice (NIJ) have developed a jointly sponsored research program to utilize the NCS for collection of data on special topics having relevance for both policy and basic knowledge of criminal victimization. BJS and NIJ will select, through a competitive process, a research firm to coordinate development of the questionnaire for each one-time topical supplement. This organization will solicit the assistance of experts on the topic to be studied in developing questions and issues to be covered, and these experts will also be invited to suggest analysis strategies for the resulting data set. Funding from BJS and NIJ will be available for analyses of supplement data. Thus, in addition to providing higher quality data for estimation of crime levels and analysis of various attributes of criminal victimization, the NCS will soon be able to serve as an omnibus survey for crime-related topics, thereby enhancing its utility as a major data source for criminal justice policy formulation and research.

The NCS program has already fielded a number of supplements over the years. Attitude supplements were administered as part of the city surveys, and supplements dealing with crime severity and victim risk (described above) have been attached to the regular NCS. Administration of supplements must be guided by concern for their impact on response to regular NCS items and their potential for affecting NCS rates. Among the possible solutions for this problem is the administration of some supplements for outgoing rotation groups only, so that respondent experience with these supplements cannot affect data collected in subsequent interviews.

## Improving utilization of NCS data

One of the major goals of the redesign was to enhance the value of NCS data to a wide range of users. In addition to efforts to improve the content of the NCS questionnaire and to facilitate new types of analysis, the redesign has taken a number of steps to broaden the scope of applications for the series. This work was facilitated by input from the Advisory Panel and from a panel of practitioners assembled to provide advice on these concerns.

A major criticism of the NCS program has been its failure to provide data for specific States and localities. Because of the stratified probability sample employed, the survey collects data in only a limited number of locations. However, the major problem in releasing State and local data has been the Title XIII restrictions under which the Census Bureau operates. This statute is designed to protect the confidentiality of data collected by the decennial census, and any sampling frame, such as that for the NCS, that relies on decennial data is also covered by these restrictions. Problems arise in the release of subnational data because combining this information with demographic information about respondents on the public-use files could result in the identification of particular respondents. The Census Bureau has released NCS tables annually for the largest States and has performed special analyses for subnational areas on request, but this procedure is costly and time consuming and does not allow the user direct access to data files to investigate different issues that may arise as the user becomes better acquainted with the data.

BJS plans to address some of these difficulties by releasing NCS files aggregated at the State and county level and for major cities, beginning with the 1987 data year. These files will contain key NCS variables and important economic and demographic data for the appropriate geographic unit. We also hope to include Uniform Crime Reporting (UCR) data for corresponding jurisdictions.

Release of such files will allow BJS to deal swiftly with requests for data on particular subnational units and will allow users some analytic flexibility in investigating victimization patterns for the geographic areas of interest.

To facilitate use of these files, BJS plans to investigate their release in a form compatible with microcomputers. In addition to the data files, the release could include menu-driven software dedicated to analysis of the data with routines for "generic area" modeling. Many subnational units are not represented in NCS files, and this capability would allow users to estimate victimization levels for areas not covered in the NCS by using information from areas with similar characteristics. The utilization panel assembled for the redesign project was particularly helpful in developing a typology for this purpose that includes 14 subnational area types and an additional residual category. Classification was based on population, land use (urban, rural nonfarm, and rural farm), MSA status, and incorporation of the geographic unit.

### Cost-effectiveness

The NCS in its original form already consumed a large portion of the BJS budget, and a number of the changes being considered, such as the longitudinal design and the addition of new or revised questionnaire items, would increase the costs of administering the NCS program. Consequently, a number of possibilities for reducing NCS costs were considered, primarily to minimize the potential impact of revisions on the NCS budget.

#### *Deletion of NCS items*

Taken together, many of the questionnaire changes described in previous sections would result in a substantially longer NCS interview, with negative consequences for data collection costs and respondent burden. To compensate, a number of items currently in the questionnaire are being eliminated from the regular NCS instrument. Some questions, such as the long battery dealing with unemployment and attempts to find work, will be deleted permanently. Other items would be included only in periodic supplements. Questions being considered for this treatment deal with medical and property insurance coverage, recovery and/or repair of stolen property, and time lost from work as a result of an incident. Decisions on questionnaire cuts are being guided by a desire to maximize the analytic utility of the data collected at every interview, to maintain useful time series, and to collect enough data on rare events to make reliable analysis possible. Questions proposed for supplement administration were judged either to be relatively stable over time, so that detection of trends would not be compromised, or frequent enough responses, so that periodic administration would still produce an adequate number of cases for analysis.

#### *Telephone interviewing*

One strategy for minimizing field costs is to rely more heavily on telephone, rather than face-to-face, interviewing. The NCS Redesign

Consortium evaluated previous Census Bureau research and other relevant work on the effect of telephone interviewing and strongly recommended that the amount of telephone interviewing be increased.<sup>11</sup> The plans approved by BJS, and now in effect, involve conducting the first interview at a household in person and then all but one subsequent interview by telephone, to the extent possible (that is, if the respondent has access to a phone and is willing to accept a telephone interview and can be reached by phone for the scheduled interview). In addition to investigating increased telephone interviewing as a general, desirable change, two design options were considered to enhance telephone interviewing--Computer-Assisted Telephone Interviewing (CATI) and a dual-frame, mixed-mode sample design.

Computer-Assisted Telephone Interviewing. CATI technology played a major role in testing revisions to the NCS because all tests conducted by the Survey Research Center (SRC) at the University of Michigan utilized CATI. This technology involves programming the questionnaire into a computer and flashing screens containing questionnaire items onto a monitor for interviewers to read during the interview. Responses are entered at the interviewer's keyboard and become part of the record for that interview. This procedure offers a number of advantages in the collection and processing of questionnaire data:

- Interviewers work out of a centralized facility. Supervisors can unobtrusively monitor interviews in progress and detect problems in interview practices, which enhances quality control.

<sup>11</sup>For a review of the relevant telephone interviewing literature and discussion of the methodological issues facing the NCS program in implementing telephone interviewing, see Robert M. Groves, Velma J. Handlin, and Peter V. Miller, "Telephone Survey Methodology: A Review," manuscript (Ann Arbor: Survey Research Center, Institute for Social Research, University of Michigan, 1982).



● CATI software can be programmed to reject obviously erroneous codes, thereby reducing the likelihood of interviewer error in keying data.

● Because skip patterns are programmed, the possibility of interviewers skipping over required questions is greatly reduced. This CATI feature makes possible the development of more complex instruments than would be possible with a paper questionnaire.

● Because no hard copy is involved, the keying of paper instruments to a computer record is eliminated in data processing, thereby saving time, reducing costs, and eliminating a processing step in which errors may be introduced.

● Because CATI interviewing need not be conducted with interviewing personnel who reside in the same area as the respondent, CATI facilities may be located in areas where the wages available for interviewers are attractive. This makes possible the recruitment of a higher quality interviewing staff, reduces turnover, and thereby minimizes training costs.

While CATI allows some economies over face-to-face interviewing in its reduction of field and some data processing costs, it does require additional expenditures in other areas. A site for an interviewing facility must be acquired and developed, computing hardware must be requisitioned, and software must be written. In addition, some interviews scheduled for CATI administration may not be completed due to failure to reach respondents over the telephone. Such cases must be recycled back to regional offices for administration in person by regular field interviewers. Finally, two processing systems must be developed, one for CATI interviews and one for non-CATI interviews. Data collected by both collection modes must ultimately be merged to create a single data set.

Dual-frame, mixed-mode sample design. The NCS Redesign Consortium devoted serious attention to developing a sample design combining two frames--one resembling the current

stratified sample in which personal and telephone interviews would be conducted and the other comprising a telephone frame in which telephone numbers would be sampled and interviews conducted by phone.<sup>12</sup> Such a design would offer economies in both sample selection and in field costs and also provide coverage of households without telephones, which a single-frame telephone sample could not; it would also provide some methodological advantages by enabling analysis of collection mode effects. Cost and error models were developed to evaluate the best mix of personal and telephone frames to minimize data collection costs; these analyses showed that a dual-frame design had potential for reducing costs. However, no data were available on the effect of repeated contacts by telephone on cumulative response rates or data quality. Consequently, implementing such a design would entail risk of increased nonsampling error in NCS data.

#### *Cost implications of redesign options*

In addition to evaluating ways in which costs of the current NCS could be reduced, BJS also scrutinized new options for the redesigned survey to evaluate whether their implementation would be cost-beneficial. Adoption of a 4-month reference period would increase field costs unless the sample size is reduced. Uniform screening was also found to have serious cost implications in addition to other methodological problems.<sup>13</sup> As mentioned above, adoption of CATI was evaluated at least partially in terms of its potential for cost reduction. Implementation of a longitudinal design will also receive scrutiny in this light. While some field and processing costs would be increased by adoption of a longitudinal design,

<sup>12</sup>Cf. Robert M. Groves and James M. Lepkowski, "Dual-Frame, Mixed-Mode Survey Designs for the National Crime Survey," manuscript (Ann Arbor: Survey Research Center, Institute for Social Research, University of Michigan, 1982).

<sup>13</sup>These methodological difficulties will be discussed in "Design change decisions."

the ability to use more powerful statistical techniques in making NCS year-to-year comparisons would also allow BJS to reduce sample size and still achieve the same level of reliability currently obtained. This design would consequently provide some offsetting economies.

Other design changes suggested by NCS redesign work have shown potential for reducing costs in addition to providing methodological improvements. An important feature of the proposed new screening strategy is that it yielded approximately 28% more reports of victimizations than the current NCS screener when the two were administered in a split ballot test by the Survey Research Center. If the new screening strategy yields corresponding improvements in an operational NCS, BJS will be able to consider either retaining the current sample size with an increase in the reliability of NCS data or implementing a sample cut to save funds while retaining current variance levels.

Another strategy for reducing costs is to begin using data collected during the initial, bounding interview in calculating NCS estimates. These data would have to be weighted differently from other data to correct for their unbounded character, but being able to utilize this information would offer choices regarding variances and costs that are similar to those created by a more effective screener. In addition, adopting the recommendation to rely more heavily on telephone interviewing will help minimize data collection costs.

## NCS redesign testing

Five major data collection efforts were carried out in support of NCS redesign and development work. Four of these were intended to provide data useful for improving the NCS screener, and the fifth (a Victim Risk Supplement) was designed to provide data on variables useful for analyzing the lifestyle characteristics that distinguish victims from nonvictims.<sup>1</sup> The majority of testing was devoted to screening strategies because this feature of the NCS interview was identified as the most seriously flawed and most difficult to remedy. Criticism of the current NCS screening questions extends back to the 1976 report of the National Academy of Sciences, which found these items too long and complex, too closely tied to UCR definitions, and not intended to help respondents search their memories for in-scope crime incidents.

A report prepared by redesign staff<sup>2</sup> classified NCS-related response error into three categories:

- **Failures of concept**--Respondents may not completely understand the task that they are expected to undertake. They may fail to comprehend the purpose of the survey, the crime scope, and the definitions of victimization incidents.
- **Failures of memory**--Respondents may terminate their search of memory prematurely, adequate cues to memory may not be provided by the survey instrument, their memory of salient cues may be blocked by mention of other possible cues, or their memory of incident dates may not be adequately stimulated by the questionnaire.

<sup>1</sup>The Victim Risk Supplement was described in "Objectives of the NCS redesign" and will not be dealt with further in this section.

<sup>2</sup>Elizabeth E. Martin (with contributions by Robert M. Groves, Jay Matlin, and Carolyn Miller), "Report on the Development of Alternative Screening Procedures for the National Crime Survey," manuscript (Washington: Bureau of Social Science Research, Inc., 1986).

- **Response inhibition and distortion**--Victims may fail to report crime incidents that they find emotionally difficult. Such suppression may be either conscious or unconscious and may result from fear, pain, shame, or embarrassment.

New screening strategies were designed to provide more structured, varied, and detailed probing for crime incidents and were intended to help surmount these barriers to remembering and reporting crime incidents. An initial pilot test performed with a reverse record check sample from Peoria, Illinois, was conducted by the Survey Research Center (SRC) at the University of Michigan in 1981 and demonstrated that substantially higher rates of victimization reporting could be achieved with a revised screening strategy providing additional cues to aid the respondent in reporting victimization incidents. A second instrument was designed for the District of Columbia Victimization Study, a victimization survey mandated by Congress, administered by BJS, and fielded by the Research Triangle Institute in 1983. This survey adopted a "short cues" approach to victimization screening, designed by the Bureau of Social Science Research (BSSR). After an initial description of the kinds of crimes being measured, respondents were provided lists of specific cues and phrases related to crime experiences and potential features of victimization incidents. Roughly half the crime incidents mentioned were in response to these cues.

In 1984 a third test was conducted by SRC in which 1,016 Peoria respondents were contacted. A sample of victims was drawn from Peoria police records and was supplemented by a random-digit-dial (RDD) sample of the Peoria area. Three screener designs were tested:

- 1) **Integrated screener.** This design alternated victimization screening questions with blocks of questions about the respondents' home, other places where they spend time, and their activities.

- 2) **Segmented screener.** This screener was a modified version of the original Peoria design. Respondents were first asked questions about their activities and living situation and then asked questions regarding situations that had made them angry or scared to stimulate recall of crime incidents. These items were followed by crime screening questions.

- 3) **Short-cues screener.** This design was very similar to that used in the D.C. Crime Victimization Survey and provided an extended list of cues regarding crime characteristics and situations in which crimes might have occurred.

All screeners utilized the same incident form, and all respondents received the same debriefing interview to determine their understanding of the survey and the tasks that were required of them. Analysis of test data showed that all three instruments were more productive than the current NCS, but that no single test instrument was uniformly superior to the other two proposed screeners. The short-cues screener elicited more reports of street crimes, took less time to administer, and showed evidence of being more easily understood by respondents; however, this instrument showed higher interviewer variance than the other two designs, most likely because interviewer pace in reading cues played a more important role than it did in the designs that relied on a more traditional question-and-answer approach. The short-cues screener also did not produce as many reports of incidents involving offenders who were family members as did the segmented screener.

The final test involved a split-ballot comparison of a short-cues questionnaire with an adapted version of the NCS questionnaire currently in use. Data were collected in January through March 1985 from a national random-digit-dial sample. The short-cues instrument was adopted for this test because it demonstrated the highest rate of reporting in the previous test while at the same time minimized respondent burden. The instrument was revised to include more cues regarding crimes commit-

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ted by offenders known to the victim, thereby correcting the weakness that was revealed in the earlier test. A split-ballot test was also conducted with the short-cues subsample to evaluate whether questions about events that caused anger or fear improved reporting of incidents in which the offender was known to the victim. The results of this test showed no significant differences between conditions.

Comparison of the two questionnaires demonstrated a marked superiority of the short-cues instrument over the NCS version in eliciting reports of criminal victimization. The short-cues version achieved a reporting rate of .99 incidents per respondent, while the NCS version produced a rate of .60. Further, 23% of short-cues respondents reported more than one incident, while 14% of NCS respondents reported multiple victimizations. Increased reporting with the short-cues questionnaire was evident across all types of victimization, which refuted the hypothesis that improvements in screening would be evident largely for less serious crimes, where forgetting would be most likely to occur. When a number of discontinuities in screening strategies were controlled for, the short-cues approach showed a 39% increase in victimization reports. When vandalism, a crime not currently measured by the NCS, was excluded from the analysis, the increase in reporting was 28%.

There are differences between this test and NCS production interviewing such as the use of Michigan Survey Research Center interviewers instead of Census Bureau field staff and the use of unbounded data instead of the usual NCS bounded data. However, the improvements in reporting resulting from the short-cues approach are sufficiently compelling that BJS and the Census Bureau are now testing an adapted version of this NCS screener for use in NCS production interviewing. The improvements in this revised screener are not restricted to more complete reporting of crime victimization by respondents; for instance, new lifestyle items are included to

facilitate comparisons of the life activities of victims and nonvictims. The redesign staff have also provided a number of recommendations regarding Census Bureau interview procedures, the scope of crimes to be covered, and the procedures for determining incident counts.

# Design change decisions

## Design packages

In addition to the Implementation Task Force, described above, which evaluated the utility and feasibility of proposed changes to the survey, a group composed of NCS redesign, BJS, and Census Bureau staff began meeting in 1985 to evaluate the cost and error characteristics of alternative combinations of potential NCS design features. The aim of these meetings was to develop design package options to facilitate consideration and final decisions on the redesigned NCS. Discussions focused on the potential adoption of six significant design features: a longitudinal design, 4-month reference period, collection of NCS data through a centralized telephone facility, a dual-frame sampling design, maximum possible utilization of telephone interviewing, and use of the initial bounding interview for estimation. Deliberations focused to a large extent on the following questions:

- What design changes could produce quality gains that could lead to cost savings in the NCS?
- What design changes could produce cost savings, and what quality impacts would they have?
- What are the minimum acceptable standards for quality or quantity of estimates from the NCS?
- What NCS objectives does BJS think should be given high priority? How much money needs to be reallocated for those objectives?
- What combinations of NCS features offer such cost savings?
- What transition costs will be experienced if such changes to the NCS are made?

A number of different design packages, which combined various major design options, were developed in initial discussions, and, subsequently, cost models were developed to estimate the impact of each proposed component on the NCS budget. Design options were also evaluated in terms of their potential sampling and nonsampling error and analytic utility. Some design packages were eliminated early in discussions, but six packages remained under active consideration throughout the discussions. These are listed as follows:

## Six design packages under consideration

	Longitudinal design	4-month reference period	Central telephone	Dual frame	Maximum telephone	Use of bounding interview
1)	X					
2)	X				X	X
3)	X		X		X	X
4)	X	X	X		X	X
5)		X	X		X	X
6)			X		X	X

"X" indicates that feature was chosen for proposed design.

The third package listed above, which includes a full, person-based longitudinal design, retention of the current 6-month reference period, centralized telephone interviewing, maximum use of phone interviewing, and utilization of bounding interviews for estimation, was the model most favorably received by BJS. Individual design components are described below, along with their evaluations by the design group and by BJS.

### Longitudinal personal option

This option would follow persons interviewed in the first wave ("principal persons") for a total of seven waves, regardless of where they moved within the United States. Those who moved to a residence more than 100 miles from an NCS primary area would be interviewed by telephone. If a phone interview could not be completed for such a mover, a nonresponse would be recorded. All age-eligible members of households in which any principal person resides would also be interviewed at each wave.

Implementation of this option would make possible analysis of the cross-wave experiences of individuals and would greatly facilitate study of long-term consequences of crime, including experiences with the criminal justice system. It would likely cause a small increase in NCS variances and nonsampling error and a large increase in the costs of administering the survey. These additional funds would be required for field activities related to following movers, additional Census Bureau professional and clerical

staff, and data processing necessary for record linkage and longitudinal file production. However, the incremental costs of implementing a longitudinal design would be reduced if other design features were also adopted. Maximal use of telephone interviews would reduce field costs. Also, adoption of an interview-to-interview recounting strategy would reduce the marginal costs of a longitudinal design because some cross-wave record linkage would be required for this reporting strategy to work properly, regardless of whether a longitudinal design were adopted.

Several possible longitudinal designs were considered, and BJS endorsed the full, person-based design for two primary reasons. First, the marginal cost increase of this design over the other longitudinal options is more than offset by the analytic utility of the data that would be produced by a full longitudinal design. Second, longitudinal data could be amassed much more quickly for small-sample analytic problems if the full longitudinal model were implemented.

BJS staff were not sanguine, however, about the prospects of implementing such a design in the immediate future. They expressed a strong preference to defer designing a longitudinal processing system for the NCS until the major programing and design problems for such a system in the Census Bureau environment were solved for the Survey of Income and Program Participation (SIPP), which is using a similar design. While the NCS would not be able to utilize the SIPP procedures and programs as written, adaptation of the practices developed for SIPP

should prove more efficient than creation of such procedures for the NCS from scratch.

As an interim measure, the design group endorsed a version of a less costly alternative longitudinal design. This option would identify a subpopulation of interest, for example, victims of violent crime, and would administer a set of followup questions at the next interview regarding consequences and subsequent contact with the criminal justice system. This set of questions would be administered as a supplement and would not be integrated with the regular NCS processing system. Victims who had moved from the sample address would be followed to obtain these data. If a victimization were reported by a member of an outgoing rotation group, an additional interview could be administered to collect this information. Although this scheme satisfies only a limited portion of the objectives for which a longitudinal design is needed, it would produce some longitudinal data in the short term, allowing BJS to exploit NCS data more fully; it would also provide some experience with longitudinal field and processing requirements before implementation of a full longitudinal design.

#### *Six-month reference period*

This option continues the current practice of interviewing NCS respondents every 6 months for a total of seven interviews over the 3-year life of a panel. Other options considered were 3-, 4-, and 12-month reference periods, but the 4-month reference period received the most serious consideration and was in fact endorsed by a number of the design group members.

A 4-month reference period would require 10 interviews over the same time period and would result in an increase in the precision of NCS estimates (that is, a reduction in nonsampling error) because it would reduce response errors due to forgetting and to internal telescoping; it would also facilitate following movers in a longitudinal design because

movers, on average, would have taken place more recently. In addition, implementation of a 4-month reference period would improve the speed with which annual estimates could be produced by shortening the interviewing period needed to collect all data for a calendar year by 2 months.

However, if we assume a constant NCS data collection budget, adoption of a 4-month reference period would require a sample cut to prevent an increase in the number of interviews administered in a fiscal year. This would inevitably raise NCS variances. Adoption of such a reference period then would result in more accurate measurement of crime levels, but would make more difficult the detection of year-to-year change. Available data suggest that nonsampling errors resulting from the administration of three additional interviews during the 3-year life of a panel would be inconsequential, compared to the effects now experienced in interviews one through seven.

When comparing the tradeoffs in precision and variance implied by the two designs, it is also important to consider the impact of precision on year-to-year change estimates. Generally, the greater response error associated with the 6-month reference period can be assumed to be similar from year to year, thereby producing no net effect on the calculation of change.<sup>1</sup> Weighing the importance BJS attaches to calculating annual changes in NCS rates against the potential errors resulting from a longer reference period, retaining the 6-month reference period appears to be the desirable course at this time.

<sup>1</sup>We assume a comparable distribution of various crime types from one year to the next. Different types of crime are likely to vary in their susceptibility to response errors related to reference period length; consequently, changes in aggregate crime rates may be at least partially artifactual if such changes are due to a disproportionate change in the distribution of the types of crime measured by the NCS.

#### *Centralized telephone interviewing*

In principle this feature does not necessarily include Computer-Assisted Telephone Interviewing (CATI) technology, but all discussions on adopting this feature for the NCS have involved CATI. Implementation of CATI for the NCS should result in some reduction in interviewer error, will permit measures of administrative errors, and may provide a small decrease in field costs. The design group endorsed the development of a centralized CATI capability for the NCS for the reasons discussed in the previous discussion of this technology.

Yet, not all NCS interviews can be conducted with CATI. All initial interviews at a sample location would be conducted in person to establish the legitimacy of the survey and to create rapport with respondents. The fifth interview would also be a personal visit. In addition, some respondents may prefer a personal interview, or may not be reachable by phone, and these would continue to be interviewed by field interviewers assigned to Census Bureau regional offices. Finally, CATI would not be implemented in single-interviewer Primary Sampling Units (PSU's).<sup>2</sup> Taking all these factors into account, testing and experience to date indicate that approximately 40% of NCS interviews would be eligible for CATI and that at least 30% could actually be completed with CATI.

A final decision on CATI implementation has not yet been reached. After several years of development work, the Census Bureau began rigorous testing of CATI on live NCS cases in January 1987 to determine its impact on data quality, reporting rates, and costs. This testing is also designed to determine the feasibility of recycling uncompleted CATI cases back to regional offices for personal interviews. Data on CATI perform-

<sup>2</sup>To implement CATI in these PSU's, the Census Bureau would have to consolidate interviews in a smaller number of PSU's of this type in order to maintain a reasonable amount of work for field interviewers. The necessary reduction of 60 to 70 PSU's would result in an increase in NCS variances, which was viewed as an undesirable consequence.

ance is currently being studied, and additional analyses will be performed as more data are collected. A final decision on implementation will be made in time to begin CATI for all eligible respondents who are administered the new NCS questionnaire in January 1990.

#### *Sample design*

A number of options for revising the NCS sample design were evaluated during the course of the redesign project. One alternative was a dual-frame, mixed-mode design that would use simultaneous list and telephone frame sampling. Telephone interviews would be taken with those in the telephone number sample, and a combination of telephone and personal visit interviews (with the current rotation) would be used for the area frame cases. Although development costs would be larger than for a single-frame design, this option would result in reduced field costs and would provide better measurement of collection mode effects than the current design.

This option was rejected for two major reasons. First, cold-contact (no prior personal contact) phone interviews produced disappointing response rates in NCS CATI tests. Second, there is little information available on cumulative response rates for RDD-based panel surveys, and adequate time and funds were not available to conduct a multiwave test of RDD data collection for the NCS. Given the effort and expense that would be required to develop an RDD sampling frame for the NCS, and the risk that use of this frame would have to be discontinued, BJS concluded that it was not advisable to implement an RDD phone sample at this time.

Several other sample design alterations were considered in the course of NCS redesign work. One option was to terminate use of the present sampling frame based on decennial census address records and to replace it with a frame listed by field personnel before selection of sample housing units. This option would increase costs at the sampling stage but free BJS to use the sample

units for followup surveys that could be conducted by non-Census Bureau survey organizations. Title XIII statutory restrictions covering decennial census records prohibit such use with the current sample.

This option was rejected largely because its potential utility did not justify the substantial costs required to implement and maintain the sampling frame. The Health Interview Survey (HIS) has adopted such a sample, and extensive cooperation with HIS could reduce development costs, but startup costs would still run well over \$1 million. Annual maintenance costs would also amount to several hundred thousand dollars. When evaluated in conjunction with the new BJS/NIJ effort to attach regular supplements to the NCS, the additional flexibility provided by this option did not appear to justify the expense.

Finally, alternate ways to stratify the NCS sample were studied in order to make it more accurate. One major change was to stratify by a composite crime index developed from jurisdiction-level UCR data. This change was implemented in 1986 as part of a sample redesign based on the 1980 decennial census and should enable production of NCS estimates that are closer to the "true" population values.

#### *Maximum use of telephone interviews*

This option would require interviewing as many sample persons as possible by telephone after the first interview. The NCS has been moving in this direction gradually over recent years in order to reduce data collection costs and now uses such a procedure for the second through the fourth and the sixth and seventh interviews. In essence this collection strategy has already been largely implemented, but approval of this design feature will make this mode of data collection a permanent feature of the NCS. Reliance on maximal phone interviewing risks a small decline in response rates but provides substantial cost savings.

The design group and BJS reacted very favorably to this option. All initial interviews at a sample location would be conducted in person to avoid problems discussed in relation to a pure telephone sample. Implementation of maximal phone interviewing would be implicit in the decision to adopt CATI, but only to a degree. CATI would not be adopted for single-interviewer PSU's, and the decision to adopt maximal telephone interviewing for this group of PSU's would have an impact on field costs over and above those involved in CATI implementation.

#### *Use of bounding interviews for estimation*

This option would use information collected in the first interview at a sample location in calculating victimization incidence and change estimates. These data currently are not used for this purpose due to their unbounded character and are used only for bounding subsequent interviews. Data from these interviews would be adjusted for inflated victimization reporting due to lack of bounding, time-in-sample effects, and other design features.

BJS and the design group have recognized the desirability of this option, and a decision on implementation awaits further work at the Census Bureau to develop options for weighting these data. This option would allow either an increase in precision by effectively increasing the usable NCS sample size by a sixth or a major cost savings through a sample cut. However, these advantages could not be realized immediately because enough data would have to be collected with the new questionnaire to allow comparisons of bounding interviews with data from later waves. Bounding interviews from early rotation groups could then be weighted retrospectively. The possibility exists that such weights could also be applied to bounding interviews collected at later dates, thereby making it possible to utilize these later interviews immediately for rate and change estimates.

## Other design features

In addition to the design package features described above, the design group discussions also dealt with several other major design features that were viewed as important candidates for implementation in a redesigned NCS. The most important of these were adoption of a core and supplement format, transition from current to revised versions, and implementation of a uniform screening strategy.

### *Core and supplement format*

As described above, this NCS feature would allow periodic administration of crime-related supplements in conjunction with collection of regular NCS data. Some of these would be periodic and would include items for which regular NCS data collection is deemed unnecessary. Other supplements would be one-time studies relevant to criminal justice issues, for which the NCS would be an appropriate vehicle.

The ability of the NCS program to include supplements will be constrained by a number of factors, mainly cost and the availability of Census Bureau staff. Recurring supplements will be less expensive to administer in the long run because major development and programming work should be required only for the first administration of such a supplement. The nature of the sample required for a supplement will also have a major bearing on cost. A supplement relying on the full sample or on a randomly selected partial sample will be less expensive to program than will a "screened" or "contingent" sample, which draws respondents with a particular attribute (race or sex) or who respond in a particular manner to one or more regular NCS items (for example, respondents who have experienced a violent crime in the previous 6 months). The design group and BJS have both endorsed the development of such a capability for the NCS, and BJS, in conjunction with the National Institute of Justice, will sponsor one-time supplements dealing with topical, crime-related issues. Two supplements have already been scheduled.

One dealing with school crime will be developed and pretested during fiscal 1988, and actual data collection is currently planned during fiscal 1989. BJS has also decided to repeat a Victim Risk Supplement (VRS), possibly during fiscal 1989, that will be similar to a VRS carried out in February 1984.

### *Transition to a revised NCS*

BJS and the NCS Redesign Consortium have recognized from the outset of the redesign project that revisions may have an effect on NCS rates. For instance, a revised NCS screener is likely to elevate rates solely as a result of its greater effectiveness in soliciting reports of crime incidents. Various models have been discussed to "calibrate" data collected with new instruments against data collected with older instruments. An optimal strategy would be to administer each instrument at full sample until the new design matures (roughly 3.5 years).<sup>3</sup> Budgetary constraints clearly preclude such a design for a survey whose routine costs are \$7 million annually. Current plans call instead for the survey to administer the old and new instruments concurrently at half-sample each for 18 months during phase-in of the new instrument. This plan should provide adequate data for testing and selecting an appropriate statistical splice for the two series to provide data on questionnaire effects for at least the major crime types.

### *Uniform screening*

This has been perhaps the most difficult methodological problem that the NCS redesign has confronted. As discussed above, the current screening strategy involves administration of screen questions for household crimes (household larceny, burglary, and motor vehicle theft) to only one respondent in a household. Both household and nonhousehold respondents

<sup>3</sup>Cf. Steven E. Fienberg, "The Splicing of Revisions in the NCS Questionnaire and Design: Preserving Longitudinal Continuity," manuscript (Pittsburgh: Carnegie-Mellon University, 1982). This design does risk introducing some measurement error, however, because a large number of new interviewers, inexperienced with the survey, would be required to collect NCS data during the phase-in period.

are asked the screen questions for personal crimes (rape, robbery, assault, and personal larceny) that follow the household questions in the questionnaire. The underlying logic for this procedure is that crimes whose target may be identified as the aggregate household, and of which all household members may be expected to be aware, should be reported by one knowledgeable respondent to minimize the likelihood of duplicate reports with possible conflicting details from other respondents.

This procedure has been criticized for a number of reasons. Most important is the distinct likelihood that any one respondent may not be aware of all household crimes that have occurred during the reference period. As such, a number of household crimes may go undetected, resulting in underestimates of the number of these crimes. A second criticism is that respondents who report household crimes also report higher levels of personal crimes. Some of this difference in reporting is undoubtedly caused by NCS questionnaire design features and interviewing procedures and can be corrected by changes in NCS data collection methodology. However, the initial questioning regarding household crimes may also serve to concentrate the household respondent's attention on the task and thereby produce fuller reporting of personal crimes in subsequent screen questions. A solution for this difficulty is to interview all respondents regarding the incidence of personal and household crimes. However, this modification would also create a new set of problems in determining accurate estimates of crime incidence levels.

Specifically, this alternate procedure may inflate estimates of household crimes by eliciting duplicate reports of the same incident from several household members. Several options have been proposed to correct this problem, but none has yet been demonstrated to be without its own flaws or potentially biasing effects. One possible solution is to use details of reported household incidents to unduplicate multiple reports of the

same incident and to discard duplicate reports. Another is to downweight the number of household incidents reported by household members by the number of respondents in a household eligible to report.

Unduplication poses a number of procedural and logistical difficulties. One possibility is to assign the task to interviewers. On the basis of information supplied by respondents, interviewers would flag duplicate records for household incidents during postinterview editing, so that the processing system would be able to recognize these records and process them accordingly. This procedure relies heavily on the judgment of interviewers, particularly if there is inconsistency in details between potential duplicate records, thereby creating the possibility of biases due to interviewer effects. Given the unique character of many crime events, the Census Bureau may not be able to provide adequate specific guidance in training and manuals that would enable interviewers to perform this task reliably.

More supervision would be available in a centralized phone-interviewing facility to be used if BJS implements a CATI capability for the NCS. But, it may be difficult for a phone interviewer to detect duplicate reports because households where occupants are not all available at the same time may be interviewed by more than one interviewer. Summary records of these incidents may provide inadequate detail for such matching, and the ability to call such records up onto the screen may prove to be an impossible programming task. Tests of interviewers' ability to perform such a task have so far produced mixed results. In the final split-ballot SRC test comparing a proposed revised instrument to a form resembling the current NCS questionnaire, interviewers were able to detect duplicate reports quite well when uniform screening was used with the current questionnaire, but they fared worse with the proposed new form. (Duplicate reports identified by post hoc examination of records provide the criterion for comparison in both cases.) Given the host of problems

with this procedure, it does not show much promise.

Another option proposed would be to accept all reports of household incidents from any respondent and then to unduplicate incidents during the processing stage. This procedure could conceivably be accomplished either by examination of individual records by hand or by some machine-based matching algorithm. The former promises to be extremely labor-intensive and thus prohibitively expensive and also is likely to delay release of data substantially; the latter is likely to be highly error-prone if respondents differ in reporting such basic incident details as the date or the nature of the article taken. Again, this strategy is not very promising.

A third solution is not to attempt unduplication but to downweight household incidents by the number of respondents in a household eligible to report. In its ideal form, this proposal assumes that all members of a household should be aware of any crime affecting the household and are all likely to report it. The SRC test cited above does not provide strong evidence for the validity of this assumption. If a burglary incident was reported by a member of a two-person household, in only 32% of such households did both respondents report the incident. In larger households, only 12% of eligible respondents verified an initial burglary report.

Respondents also frequently do not conceptualize household crime in the communal sense that underlies the logic of household crime classification. This is a particular problem for the classification of larceny, which is classified as "household" or "personal without contact" solely as a function of the location of the incident. Such variation in the location of a theft may not alter other household members' perception of ownership and consequently their likelihood of reporting such a theft. Analysis of the final SRC test, while based on a small number of cases, indicates that location plays a small part in respondents' evaluation of

communal loss. Controlling for ownership of stolen property, there was no significant association between location of a theft incident and the number of household victims reported by respondents. This finding holds for all larcenies and for larcenies reported in households with two or more respondents.

Given the failure of most eligible respondents in multiple-person households to verify initial reports of household burglaries and also given the failure of many respondents in such households to conceptualize household larcenies in a fashion that would be amenable to full reporting of such incidents by all respondents, a downweighting scheme based simply on the number of household members appears to be inadequate; such a scheme would likely result in underestimates of household crimes because the number of duplicate reports being adjusted for would be overestimated. While the number of discrete household incidents measured with uniform screening would probably be greater than with the current method, it is unclear whether this increase would offset the underestimates produced by downweighting. A more adequate downweighting scheme would take account of the likelihood of incomplete reporting of household crimes in multiple-person households, but the appropriate means to adjust for such nonreporting remain to be identified. The likelihood of nonreporting could be determined by examining records to detect the actual number of duplicate reports, compared to the expected number for some portion of the NCS sample. The difference divided by the expected number could then be used as a multiplier for the downweighting factor for different classes of crime and different household sizes. However, such a procedure relies on actual unduplication of records, a procedure whose flaws were discussed above, and thus confounds the error properties of downweighting with another potential set of biases. While downweighting remains an attractive option conceptually, the procedures available to operationalize this adjustment are compromised by empirical difficulties



rooted in discontinuities between survey definitions and respondents' perceptions of household crimes and by the potential flaws in verifying the downweighting algorithms selected.

Another important factor to consider in evaluating the effectiveness of uniform screening is the inability of NCS interviewers to control whether other individuals are present during NCS interviews. This feature of the interview may affect the victim's likelihood of reporting crime incidents in a host of ways. The most frequently discussed possibility is that the presence of other family members may decrease the likelihood of reporting incidents of domestic violence. However, the presence of other individuals may also affect the reporting of household incidents in conflicting ways and may therefore affect the efficacy of both the current household screening procedure and the suggested alternatives. In addition to its effects on the level and type of crime reports obtained, the presence of others during an interview also compromises the confidentiality of respondents' answers that ideally should be maintained in collecting potentially sensitive information. Given the unpredictable effects of the presence of others and our inability to control traffic in respondents' homes, the only conclusion we can draw is that there is no way to design a household screening procedure that is entirely free of error.

The presence of other individuals during an interview can have various effects. One benefit is that incidents that would be classed as household crimes, but of which the household respondent was unaware or had forgotten, may be brought to the attention of this respondent if other knowledgeable household members are present. On the negative side, a household respondent may be less likely to report disturbing incidents, such as break-ins, if the occurrence of the incident had somehow been hidden from children or elderly relatives and if any of these individuals is present.

These effects may also influence reporting if uniform screening procedures are implemented. However, an additional problem resulting from the presence of more than one eligible household member is that rigid administration of the household screener to one respondent after another may appear nonsensical, particularly if later respondents have already volunteered information regarding a household incident during an interview with another respondent. This problem may lead to a perception that the interviewer is wasting respondents' time, possibly resulting in respondents answering screen questions carelessly, terminating the interview, or refusing to participate in future interviews. Alternatively, nontelephone interviewers may choose to administer household screen questions in abbreviated fashion if they detect such problems, which may produce a negative bias in household incident reporting for multiple-person households.

Given our inability to insist on private interviews in respondents' homes and the pervasive influence of this uncontrollable variable across all types of household screening, it appears that we must live with some error in this aspect of the survey. However, two strategies may be proposed to reduce the impact of this factor on NCS reporting. A basic feature of these proposals is to continue the current procedure of collecting household incident data from only one respondent in the household.

First, interviewers can collect these data only from a knowledgeable respondent (for example, a parent or the individual who pays the rent or mortgage). If this individual is unavailable, the household screener may be deferred until this respondent can be interviewed. Second, when other respondents are present, they can be encouraged to contribute incidents they recall and that the household respondent fails to report during household screening questions. These strategies should do much to increase household incident reporting, but without the potential negative

impact of uniform screening on respondents' diligence or the problems which result from unduplication and downweighting.

Continuing to collect household data from only one respondent will not provide nonhousehold respondents with a "warmup" from these questions. However, the personal screener items planned for final revisions should be more effective in prodding respondents' memories than the current screener and therefore less prone to the warmup effect of the household screen items. Consequently, we expect the "warmup" effect to be reduced in data collected with the final version of the NCS questionnaire.<sup>4</sup>

Another problem in attenuating household respondent effects is the artifactual distinction between personal and household larceny. BJS and Census Bureau staff believe that the most promising solution to this problem is to eliminate "personal larceny without contact" as a crime classification. The artifactual distinction between personal and household larceny is also responsible for some household respondent effects. All larcenies, except for personal larcenies with contact (for example, pocket picking and purse snatching) and those related to motor vehicles, will be recorded as household larcenies, regardless of which household member first reported the incident. This procedure may be vulnerable to error caused by interviewers unduplicating multiple reports of the same incident. However, it will eliminate the analytic and conceptual difficulties caused by the current location-based definitions of larceny. Also, it will not rely on distinctions regarding which household member(s) actually owns stolen property, thereby avoiding questionable decisions on larceny weights and assignment of incidents to person records in NCS data files.

<sup>4</sup>SRC tests of the proposed final version of the screener adopted a uniform screening procedure, which makes it impossible to compare the effect of household and nonhousehold respondent status on personal screening. Assessment of this effect must await availability of census-produced pretest and/or production data that contain information on assignment of household respondents.

## Implementation of the NCS redesign

The Implementation Task Force, described in the section dealing with the structure and functioning of the NCS Redesign Consortium, began its work in December 1983. The purpose of this body was to develop a strategy for evaluating and executing the large number of proposed changes resulting from NCS redesign research and development work. The task force functioned concurrently with the design group described in the previous section of this report, but it focused on the more detailed aspects of proposed changes to the survey, rather than on the larger packages that were the concern of the design group.

Early in the task force meetings the group decided that two primary goals should guide implementation strategy. First, changes to the survey should be implemented as quickly as possible, so that analysis of important new questions could begin. Second, changes should be introduced in a way that minimized potential disruption to the NCS series. A number of proposed changes were judged ready for implementation early in the work of the task force, while others required more testing and evaluation. Among the latter group were some changes that had potential for affecting NCS rates, such as the introduction of revised screening questions and a uniform screening strategy.

The task force concluded that changes to the survey should be divided into two groups--those that had potential for affecting NCS rates and those that had little potential for change due solely to alterations in instrumentation. Alterations judged to be non-rate affecting would be implemented as soon as pretesting by the Census Bureau allowed, and the remaining revisions would be implemented at a later date, simultaneously if possible. Concurrent implementation of rate-affecting changes would not allow measurement of the individual impact of each change, but it would minimize series disruption by providing a clean series break.<sup>1</sup>

<sup>1</sup>A summary of the major decisions on survey modification is provided in Appendix B. Issues that have yet to be resolved are listed in Appendix C.

Once near-term and long-term packages of changes had been developed, the task force began to concentrate on near-term implementation decisions. These consisted mainly of revisions to the NCS incident form, which collects data on the characteristics and consequences of crime victimization. Because these changes would have no foreseeable impact on the screening strategy used to elicit reports of crime incidents, their impact on NCS rates was expected to be negligible.

The task force also proposed two changes to the screener that were expected to have negligible impact on rates. First, the survey would begin to interview 12- and 13-year-old respondents directly, rather than collect proxy interviews; proxies would be collected for these respondents only if adult household members objected. The intent of this change is to enhance the accuracy of victimization data collected from these respondents. Second, a long battery of questions was dropped that dealt with the nature of employment and with attempts by unemployed respondents to seek work. These items had been imported from the Current Population Survey (CPS) when the NCS was initially designed. While some useful analyses could be performed with these data, such as determining the comparative risk of different occupations particularly if several years of NCS data were merged to provide a large data set, many of these items caused difficulties in establishing respondent rapport. The questions regarding unemployment and searching for work were particularly irrelevant to the content of a crime victimization survey. The items that enabled classification of respondents' occupations and the industries in which they work were also extremely expensive to process and were regarded as not having enough analytic utility to justify the costs of providing this information.

Collection of occupational data for respondents who were victimized will be continued in the near-term version of the NCS, but comparisons with the occupational characteristics of nonvictims will not be possible for data collected with this instrument.

An entirely new strategy for collecting occupational data is being studied for the long-term questionnaire. A new set of items has been developed for use with both the screener and incident form that will measure (1) whether the respondent is employed in any of several occupations which are high-risk or have other substantive interest, (2) whether he or she works for government or a private firm or is self-employed, and (3) the degree of urbanization of the respondent's work site. These items are being prepared for testing and should be useful for comparing these targeted occupations to the general population, without requiring the extensive manual coding needed for the previous industry and occupation items. The gaps in our knowledge of victimization risk for other occupations can be filled by periodic special supplements dealing with the victimization experiences of all respondents in the labor force or of those in particular occupations. Using supplements to collect such data promises to provide more exhaustive and analytically useful information on occupational risk than can ever be gathered by a necessarily small set of items in the regular NCS instrument.

Once BJS approved the package of proposed near-term changes, the Census Bureau began field testing the new questionnaire. Two tests were conducted utilizing regular NCS interviewers in 1985, and the final near-term version was introduced in July 1986. Except for the employment status data described above, the near-term revised NCS should extend the time series for all previous NCS data and will provide enhanced incident data on victim and bystander behavior, offender substance abuse, place of crime occurrence, weapon use by offenders, property loss, and contacts with the criminal justice system.

In addition to questionnaire changes, the task force also endorsed the development of a county-level NCS file for near-term implementation that would enable subnational analysis of NCS data. This file will be produced annually and will include data for those counties in which a

minimum number of NCS interviews have been conducted in a given year. To protect respondents' privacy, Primary Sampling Unit (PSU) data on public-use tapes will be scrambled. This encoding will prevent the identification of PSU's on these tapes by making impossible a match with corresponding, geographically identifiable data on the county-level file. The first full calendar year for which this file will be prepared is 1987. BJS will evaluate releasing this file in a format compatible with microcomputers, but the necessary planning for such a release has not yet been completed.

After planning for near-term changes was concluded, the task force devoted its attention to the major changes to the survey that would result from the proposed long-term revisions. Final recommendations to the Director of BJS were prepared, and in 1986 the Director's decisions on long-term revisions were communicated to the Director of the Census Bureau. These alterations include a revised questionnaire, incorporating new screening procedures, and components of the approved design package described in the previous section on design changes. The implementation status of each of these changes is described below:

### Revised questionnaire

Near-term instrument changes have already been implemented, and BJS and NIJ will cosponsor one-time, topical supplements. The most important changes planned for long-term implementation include:

- major redrafting of the screener form to incorporate a "short-cues" screening strategy;
- changes in the treatment of series crimes, so that a series would comprise a minimum of six incidents. In addition to data currently gathered on the last incident in a series, new information will be collected on the aggregate series; and
- utilization of supplements to collect some data that have traditionally been gathered by the ongoing NCS.

Work to implement the long-term revised questionnaire is well under way. Questionnaire drafts were prepared by the Census Bureau, and an initial, two-wave pretest using Census Bureau interviewers was fielded in June and September 1987 in the Washington, D.C., area. A comprehensive plan for the remaining implementation work has been agreed to by BJS and the Census Bureau and includes four components:

**Testing.** A three-wave national test of 1,000 cases will be used for final revisions to the long-term questionnaire. Data for the first wave were collected in February/March 1988, and two subsequent waves will be administered at 6-month intervals. Respondents are drawn from unused NCS sample cases. A control group of 1,000 from the regular NCS sample with the same maturity in sample has been selected to facilitate comparisons of current and proposed screening strategies. Experience with the revised questionnaire should allow final decisions on instrument content to be reached by November/December 1988. The third wave of this test will be conducted primarily to provide data on cumulative response rates and multiwave trends in victimization reporting.

**Phase-in.** A three-step, phased implementation of the new questionnaire will begin in January 1989. At this time an instrument reflecting experience from the first wave of the national test will be implemented for a random 5% of active NCS cases. This first step of the phase-in will give Census Bureau regional office staff and interviewers experience in using the new questionnaire and will allow us to identify and correct problems. Data collected with the new instrument during this step of the phase-in will not be used for estimation. Consequently, any problems arising from use of the new questionnaire will not be reflected in published NCS data.

Final revisions to the questionnaire will be incorporated after all three pretests are completed. This revised instrument will be implemented for a random 50% of the NCS sample in January 1990. The final step of the

phase-in calls for the new questionnaire to be implemented for the full sample in July 1991.

There are a number of desirable features to this plan. First, it allows BJS to continue to publish annual and yearly change estimates throughout the phase-in period. These estimates will be developed from annual files that are close to full sample size, except for 1990, which will rely on files that are 50% the usual size. Splitting the 1990 data between near- and long-term instruments allows 1989-90 change estimates to be calculated solely with data collected with the near-term instrument, and 1990-91 estimates to be computed using long-term data. By allowing 18 months for the 50% step of the phase-in, the plan also allows for up to 6 months of long-term data to be discarded if startup problems are identified with the new instrument, without major damage to 1990 long-term data. Finally, by providing 18 months for this second step, the plan facilitates development of a statistical splice for data collected with near- and long-term instruments by providing a substantial period of concurrent data collection with the two questionnaires.

**Statistical splice.** Work on this activity--designed to facilitate comparisons of NCS near- and long-term data--should begin in 1991. Statistical models will be developed to adjust for the effects of long-term changes (for example, differences in screener efficiency and data collection modes) on victimization reporting. Adjustment factors will be developed at least for major crime types and possibly for other important variables if reliable differences are found. Second-step phase-in data will be used for these analyses. This activity should be completed by July 1992.

**Processing system.** A new processing system must be written to prepare NCS data files and produce annual tabulations. This work may begin as soon as final questionnaire decisions are made and should be completed before the new questionnaire is implemented for the full sample in July 1991.

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### **Longitudinal design**

While BJS recognizes the desirability of implementing such a design, a final decision on this option awaits review of the Survey of Income and Program Participation (SIPP) longitudinal processing system and file production procedures. A less costly longitudinal option discussed by the design group has been endorsed as an interim measure. This option would identify a subpopulation of crime victims and would administer a set of followup questions at the subsequent interview regarding (1) victimization consequences and (2) subsequent contact with the criminal justice system. This set of questions would be administered as a supplement and would not be integrated with the regular NCS processing system. Victims who had moved from the sample address would be followed to obtain these data.

### **Centralized telephone interviewing**

This design feature involves the implementation of Computer-Assisted Telephone Interviewing (CATI). The final test of this capability began in January 1987 and relies on live cases from the ongoing NCS sample to evaluate the feasibility, cost, and data quality questions related to this technology. Current plans call for the Census Bureau to implement CATI for interviews utilizing the long-term questionnaire in all multiple-interviewer PSU's, beginning with the second step (50%) of phase-in. CATI will not be utilized for interviews in single-interviewer PSU's.

### **Maximum use of telephone interviewing**

BJS has requested that the Census Bureau utilize telephone interviewing for non-CATI interviews to the maximum extent possible. This change would help minimize data collection costs and would be implemented in all but one interview following the initial bounding interview for those respondents who can be reached by phone and who have consented to be interviewed by phone.

### **Use of bounding interview in estimation**

At the request of BJS, the Census Bureau has begun to study strategies for including bounding interview data in the data base used to produce NCS estimates. Final plans for this change have not been completed, but BJS expects that bounding interview data collected with the final revised questionnaire can be used for this purpose. Discussions have focused on downweighting incidents collected during the first interview to levels achieved during subsequent interviews with the long-term, revised questionnaire.

### **Interview-to-interview recounting**

BJS has directed the Census Bureau to change the current practice of accepting reports of crime incidents only for the 6-month period ending with the month prior to the interview. This revision will enable interviewers to collect data on more recent incidents while details are fresh and will provide for linking data for such incidents with data collected in the subsequent interview. Implementation of this feature will allow retention of the same 6-month reference period now used for estimation and may also allow some flexibility in CATI scheduling by allowing an extension of the monthly closeout period for NCS interviewing.

## Conclusion

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The Bureau of Justice Statistics has devoted more than a decade to evaluating all aspects of the National Crime Survey (NCS), beginning with the National Academy of Sciences report and extending through the NCS redesign project. The NCS is one of the largest surveys conducted by the Federal Government and, given its central role in providing data and statistics on the incidence of criminal victimization in the United States, clearly merits the years of assessment and redesign that this report describes.

The redesign project will not be complete until BJS has both granted final approval for several remaining revisions and implemented approved changes successfully. The various decision points for questionnaire implementation are presented in the flowchart on page 33. In addition to questionnaire changes, several other issues remain to be resolved before the project is completed. Most important of these are implementation of Computer-Assisted Telephone Interviewing and a person-based longitudinal design.

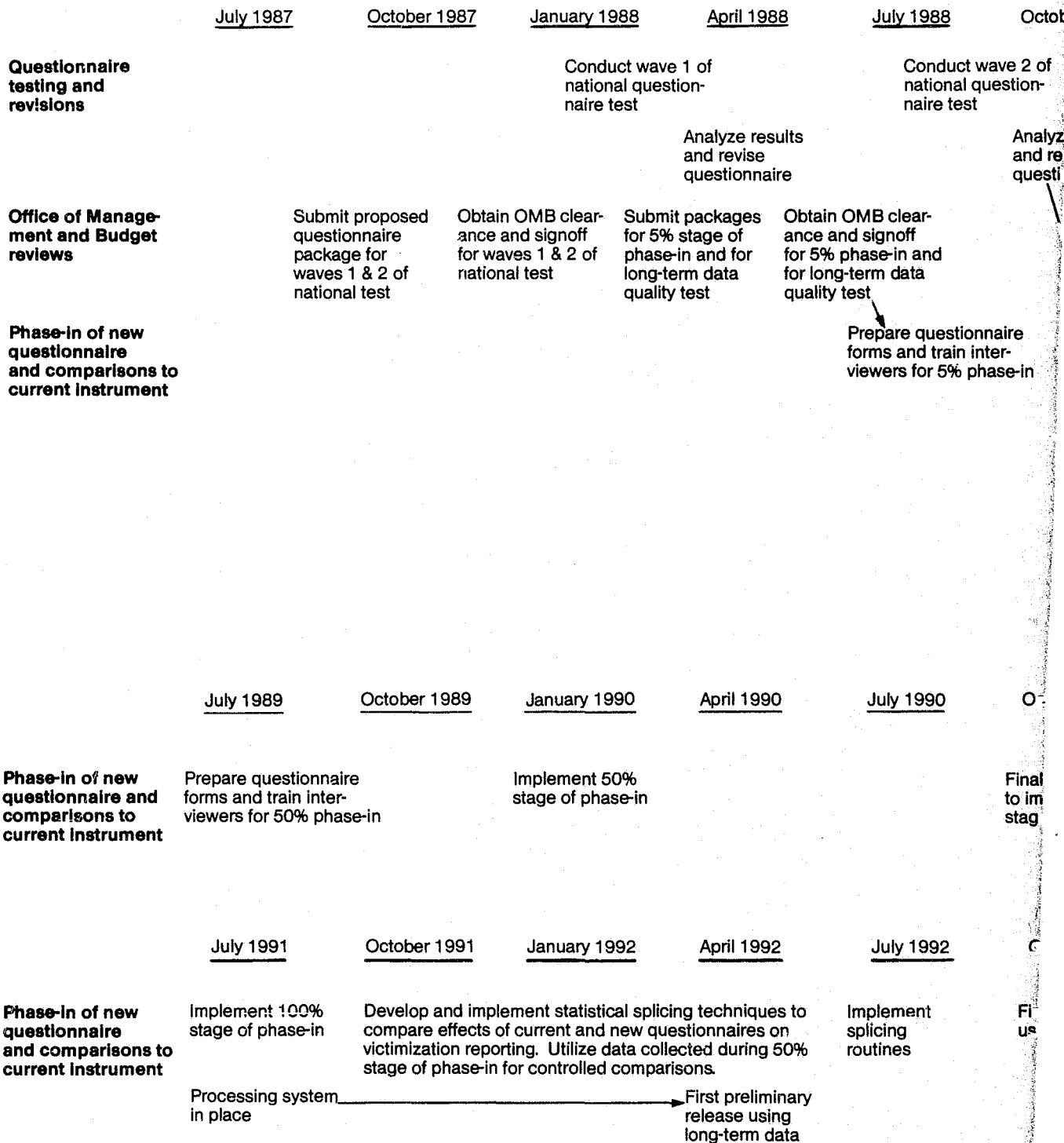
BJS is satisfied that the research necessary to proceed with the implementation of the NCS redesign phase has been completed. A structure has been developed that will allow rational decisions to be made at each stage of the implementation process based upon data from the previous stages.

The NCS Redesign Consortium was charged with recommending the best possible design for a survey designed to collect data on criminal victimization. BJS is satisfied that the high-quality research and development performed for this project have in fact provided the best available methodological information for improving the NCS. In making final design decisions, these recommendations have had to be balanced against the organizational and budgetary constraints inherent in an ongoing survey program. Although BJS was not able to adopt all revisions suggested by the consortium, we believe that the redesigned survey is the best that can be achieved in an operational NCS program.

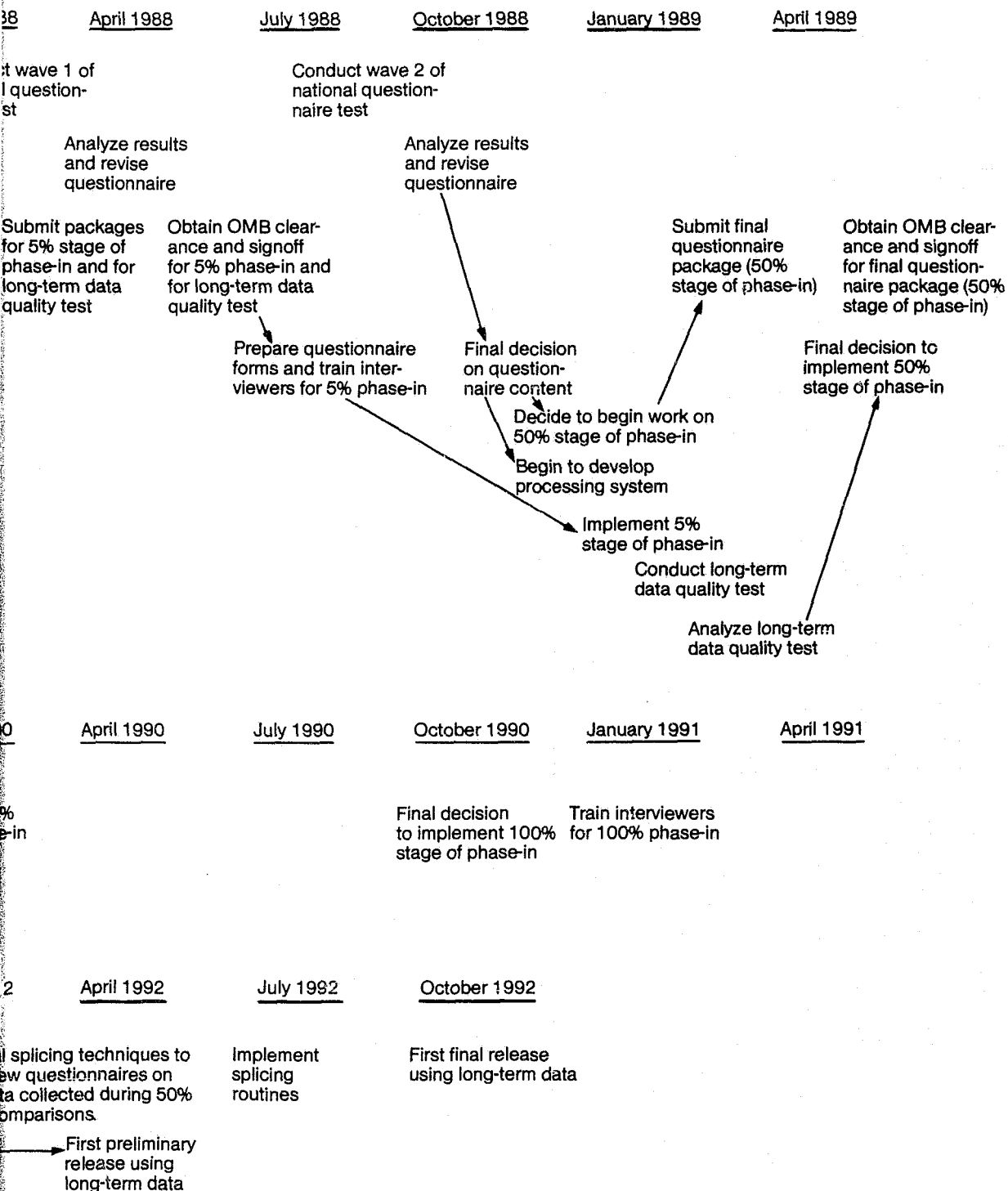
We expect that this revised version of the National Crime Survey will provide a more reliable, flexible, and useful data series on household and personal victimization well into the next century.

BJS will continue to present results from the redesign project in a variety of formats, including Special Reports and Technical Reports that present data from the new questionnaire items and that outline technical issues raised through the analysis of new data. BJS believes that the redesign project has led, and will continue to lead, to improved victimization measurement both in terms of increased reporting of victimizations and more comprehensive information concerning the nature of these victimizations.

# National Crime Survey redesign long-term implementation schedule: Questionnaire testing, processing system development, and instrument comparisons



**Implementation schedule:  
Data collection and instrument comparisons**



## Appendix A

# Major National Crime Survey redesign products

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### Papers

- 1) Biderman, Albert D., "The Effects of Non-Sampling Error on the Relationship Between Age and Victimization in the NCS," 35th Annual Meeting of the American Society of Criminology, Denver, 1983.
- 2) Biderman, Albert D., and David Cantor, "A Longitudinal Analysis of Bounding, Respondent Conditioning, and Mobility as Sources of Panel Bias in the National Crime Survey," Annual Meeting of the American Statistical Association, 1984.
- 3) Biderman, Albert D., David Cantor, James P. Lynch, and Elizabeth E. Martin, "Final Report of Research and Development for the Redesign of the National Crime Survey," manuscript (Washington: Bureau of Social Science Research, Inc., 1986).
- 4) Biderman, Albert D., David Cantor, and Albert J. Reiss, Jr., "Household and Secondary Respondents: A Quasi-Experimental Analysis of Interviewing and Classification Problems in the National Crime Survey," manuscript (Washington: Bureau of Social Science Research, Inc., 1982, revised 1985).
- 5) Biderman, Albert D., and James P. Lynch, "Recency Bias in Data in Self-Reported Victimization," Annual Meeting of the American Statistical Association, Detroit, 1981.
- 6) Biderman, Albert D., and James P. Lynch, "Why the NCS Diverges from the UCR: The Importance of Non-Uniformity in Measurement," 36th Annual Meeting of the American Society of Criminology, Cincinnati, 1984.
- 7) Biderman, Albert D., James P. Lynch, and James L. Peterson, "Why NCS Diverges from the UCR Index Trends," 35th Annual Meeting of the American Society of Criminology, Denver, 1983.
- 8) Cantor, David, "Operational and Substantive Implications of Changing the NCS Reference Period," Annual Meeting of the American Statistical Association, Las Vegas, 1985.
- 9) Collins, James J., Jr., "The Subnational Use of National Crime Survey (NCS) Data: A Subnational Area Typology" (Research Triangle Park, N.C.: Research Triangle Institute, 1984).
- 10) Fienberg, Steven E., "The Measurement of Crime Victimization: Prospects for Panel Analysis of a Panel Survey," *The Statistician*, 29 (4), 1980.
- 11) Fienberg, Steven E., "The Splicing of Revisions in the NCS Questionnaire and Design: Preserving Longitudinal Continuity," manuscript (Pittsburgh: Carnegie-Mellon University, 1982).
- 12) Groves, Robert M., Peter V. Miller, and Velma J. Handlin, "Telephone Survey Methodology: A Review," manuscript (Ann Arbor: Survey Research Center, Institute for Social Research, University of Michigan, 1982).
- 13) Groves, Robert M., and James M. Lepkowski, "Dual-Frame, Mixed-Mode Survey Designs for the National Crime Survey," manuscript (Ann Arbor: Survey Research Center, Institute for Social Research, University of Michigan, 1982).
- 14) Jabine, Thomas B., "Longitudinal Design Task Force Findings and Recommendations," manuscript (Washington: Bureau of Social Science Research, Inc., 1983).
- 15) LaVange, Lisa M., and Ralph E. Folsom, "Development of NCS Error Adjustment Models," manuscript (Research Triangle Park, N.C.: Research Triangle Institute, 1985).



Appendix A  
Major National Crime Survey redesign products

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|--|---|
| 16) Lynch, James P., "Changes in Police Organization and Their Effects on the Divergence of the UCR and NCS Trends," 35th Annual Meeting of the American Society of Criminology, Denver, 1983.   | Data files  |
| 17) Lynch, James P., and Albert D. Biderman, "Cars, Crime, and Crime Classification: What the UCR Index Doesn't Tell Us That We Should Know," 36th Annual Meeting of the American Society of Criminology, Cincinnati, 1984.  | 1) Baltimore and San Jose Reverse Record Check Studies (1970)   |
| 18) Martin, Elizabeth E., "Procedural History of Changes in NCS Instruments, Interviewing Procedures, and Definitions," manuscript (Washington: Bureau of Social Science Research, Inc., 1982).  | 2) Flattened Versions of Longitudinal Files for All Incidents Reported During 1975-79 and for Selected Person Information |
| 19) Martin, Elizabeth E., "Some Effects of Procedural Change on Estimates of Victimization from NCS," 35th Annual Meeting of the American Society of Criminology, Denver, 1983.  | 3) Longitudinal Version of Census Reference Period Research Experiment  |
| 20) Martin, Elizabeth E., "Some Conceptual Ambiguities in the National Crime Survey," Annual Meeting of the American Statistical Association, 1984.  | 4) Peoria Screener Test 1 (1981)  |
| 21) Martin, Elizabeth E., with contributions by Robert M. Groves, Jay Matlin, and Carolyn Miller, "Report on the Development of Alternative Screening Procedures for the National Crime Survey," manuscript (Washington: Bureau of Social Science Research, Inc., 1986). | 5) D.C. Crime Victimization Survey (1983)   |
| 22) Reiss, Albert J., Jr., "Designing Explanatory Variables for the NCS," manuscript (New Haven: Institution for Social and Policy Studies, Yale University, 1981).  | 6) Victim Risk Supplement (1984)  |
| 23) Reiss, Albert J., Jr., "Victimization Productivity in Proxy Interviews," manuscript (New Haven: Institution for Social and Policy Studies, Yale University, 1982).   | 7) Peoria Screener Test 2 (1984)  |
|  | 8) National Test of Experimental Instruments (1985)   |
|  | 9) NCS-UCR Aggregate Annual Files   |

## Appendix B

# Major NCS redesign decisions

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### I. Questionnaire

#### A. **Screeners**

1. Adopt short-cues design for screener (long-term).
2. Revise Industry and Occupation items to target victimization-related occupational characteristics (long-term).
3. Begin to collect data on vandalism (long-term).
4. Adopt victimization-related lifestyle items (long-term).
5. Continue to collect household incident data from one household respondent.
6. Retain 6-month reference period.

#### B. **Incident form**

1. Adopt new self-protection items detailing consequences and bystander behavior (near-term).
2. Adopt items on substance abuse by offenders (near-term).
3. Adopt items on long-term contacts with the criminal justice system (near-term).
4. Correct ambiguities in offender weapon-use items (near-term).
5. Provide greater detail in "place of occurrence" item (near-term).
6. Adopt new item measuring threats before actual attacks (near-term).
7. Provide greater detail in "type of property taken" item (near-term).
8. Revise "type of property recovered" variable (near-term).
9. Expand codes for items measuring reasons for reporting or not reporting crimes to the police (near-term).

#### C. **General**

1. Adopt "core and supplement" format (near-term).
2. Raise threshold for defining series crime to six incidents (long-term).
3. Adopt machine-readable control card (long-term).

### II. Data collection procedures

- A. Eliminate proxy interviews for 12- and 13-year-old respondents when possible (near-term).
- B. Implement "maximum telephone" data collection (near- and long-term).
- C. Adopt interview-to-interview recounting of crime incidents (long-term).
- D. Implement final questionnaire in three-step phase-in, to be completed by July 1991 (long-term).

### III. Sample design

- A. Reject use of dual-frame (telephone and area) design.
- B. Retain current frame, based on decennial census.
- C. Stratify sample by areal UCR data to improve sample accuracy (near-term).

### IV. Estimation

- A. Develop models to incorporate bounding interview data in estimates (long-term).
- B. Develop statistical splice to allow comparisons of data collected with long-term instrument to earlier NCS data.

## Appendix C

# Remaining issues in the NCS redesign

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I. Release of county-level NCS data in microcomputer format.

II. Adoption of full, person-based longitudinal design.

III. Design and scheduling for longitudinal supplement.

IV. Computer-Assisted Telephone Interviewing (CATI) implementation in multiple-interviewer PSU's for 50% of sample receiving long-term questionnaire during second step of phase-in.

## Bureau of Justice Statistics reports

(revised January 1989)

Call toll-free 800-732-3277 (local 301-251-5500) to order BJS reports, to be added to one of the BJS mailing lists, or to speak to a reference specialist in statistics at the Justice Statistics Clearinghouse, National Criminal Justice Reference Service, Box 6000, Rockville, MD 20850.

### BJS maintains the following mailing lists:

- Drugs and crime data (new)
- White-collar crime (new)
- National Crime Survey (annual)
- Corrections (annual)
- Juvenile corrections (annual)
- Courts (annual)
- Privacy and security of criminal history information and information policy
- Federal statistics (annual)
- BJS bulletins and special reports (approximately twice a month)
- Sourcebook of Criminal Justice Statistics (annual)

Single copies of reports are free; use NCJ number to order. Postage and handling are charged for bulk orders of single reports. For single copies of multiple titles, up to 10 titles are free; 11-40 titles \$10; more than 40, \$20; libraries call for special rates.

Public-use tapes of BJS data sets and other criminal justice data are available from the National Archive of Criminal Justice Data (formerly CJAIN), P.O. Box 1248, Ann Arbor, MI 48106 (toll-free 1-800-999-0960).

## National Crime Survey

### Criminal victimization in the U.S.:

- 1986 (final report), NCJ-111456, 9/88
- 1985 (final report), NCJ-104273, 5/87
- 1984 (final report), NCJ-100435, 5/86
- 1983 (final report), NCJ-96459, 10/85

### BJS special reports:

- The redesigned National Crime Survey: Selected new data, NCJ-114746, 1/89
- Motor vehicle theft, NCJ-109978, 3/88
- Elderly victims, NCJ-107676, 11/87
- Violent crime trends, NCJ-107217, 11/87
- Robbery victims, NCJ-104638, 4/87
- Violent crime by strangers and nonstrangers, NCJ-103702, 1/87
- Preventing domestic violence against women, NCJ-102037, 8/86
- Crime prevention measures, NCJ-100438, 3/86
- The use of weapons in committing crimes, NCJ-99643, 1/86
- Reporting crimes to the police, NCJ-99432, 12/85
- Locating city, suburban, and rural crime, NCJ-99535, 12/85
- The risk of violent crime, NCJ-97119, 5/85
- The economic cost of crime to victims, NCJ-93450, 4/84
- Family violence, NCJ-93449, 4/84

### BJS bulletins:

- Criminal victimization 1987, NCJ-113587, 10/88
- Households touched by crime, 1987, NCJ-111240, 5/88
- The crime of rape, NCJ-96777, 3/85
- Household burglary, NCJ-96021, 1/85
- Violent crime by strangers, NCJ-80829, 4/82
- Crime and the elderly, NCJ-79614, 1/82
- Measuring crime, NCJ-75710, 2/81

### Redesign of the National Crime Survey, NCJ-111457, 1/89

- The seasonality of crime victimization, NCJ-111033, 6/88
- Series crimes: Report of a field test (BJS technical report), NCJ-104615, 4/87
- Crime and older Americans information package, NCJ-104569, \$10, 5/87
- Lifetime likelihood of victimization, (BJS technical report), NCJ-104274, 3/87
- Teenage victims, NCJ-103138, 12/86

Response to screening questions in the National Crime Survey (BJS technical report), NCJ-97624, 7/85

Victimization and fear of crime: World perspectives, NCJ-93872, 1/85, \$9.15

The National Crime Survey: Working papers, vol. I: Current and historical perspectives, NCJ-75374, 8/82

vol. II: Methodological studies, NCJ-90307, 12/84

## Corrections

### BJS bulletins and special reports:

- Capital punishment 1987, NCJ-111939, 7/88
- Drug use and crime: State prison inmate survey, 1986, NCJ-111940, 7/88
- Prisoners in 1987, NCJ-110331, 4/88
- Time served in prison and on parole 1984, NCJ-108544, 1/88
- Profile of State prison inmates, 1986, NCJ-109926, 1/88
- Imprisonment in four countries, NCJ-103967, 2/87
- Population density in State prisons, NCJ-103204, 12/86
- State and Federal prisoners, 1925-85, 102494, 11/86
- Prison admissions and releases, 1983, NCJ-100582, 3/86
- Examining recidivism, NCJ-96501, 2/85
- Returning to prison, NCJ-95700, 11/84
- Time served in prison, NCJ-93924, 6/84

### Correctional populations in the U.S.:

- 1986, NCJ-111611, 1/89
- 1985, NCJ-103957, 2/88

### Historical statistics on prisoners in State and Federal institutions, yearend 1925-86, NCJ-111098, 6/88

1984 census of State adult correctional facilities, NCJ-105585, 7/87

### Historical corrections statistics in the U.S., 1850-1984, NCJ-102529, 4/87

1979 survey of inmates of State correctional facilities and 1979 census of State correctional facilities:

### BJS special reports:

- The prevalence of imprisonment, NCJ-93657, 7/85
- Career patterns in crime, NCJ-88672, 6/83

### BJS bulletins:

- Prisoners and drugs, NCJ-87575, 3/83
- Prisoners and alcohol, NCJ-86223, 1/83
- Prisons and prisoners, NCJ-80697, 2/82
- Veterans in prison, NCJ-79232, 11/81

### Census of jails and survey of jail inmates:

- BJS bulletins and special reports: Jail inmates, 1987, NCJ-114319, 12/88
- Drunk driving, NCJ-109945, 2/88
- Jail inmates, 1986, NCJ-107123, 10/87
- The 1983 jail census, NCJ-95536, 11/84

Census of local jails, 1983: Data for individual jails, vols. I-IV, Northeast, Midwest, South, West, NCJ-112796-9; vol. V, Selected findings, methodology, summary tables, NCJ-112795, 11/88

Our crowded jails: A national plight, NCJ-111846, 8/88

## Parole and probation

### BJS bulletins:

- Probation and parole: 1987, NCJ-113948, 11/88
- 1986, NCJ-108012, 12/87
- 1985, NCJ-103683, 1/87
- Setting prison terms, NCJ-76218, 8/83

### BJS special reports:

- Time served in prison and on parole, 1984, NCJ-108544, 1/88
- Recidivism of young parolees, NCJ-104916, 5/87

Parole in the U.S., 1980 and 1981, NCJ-87387, 3/86

Characteristics of persons entering parole during 1978 and 1979, NCJ-87243, 5/83

## Children in custody

- Census of public and private juvenile detention, correctional, and shelter facilities, 1975-85, NCJ-114065, 12/88
- Survey of youth in custody, 1987 (special report), NCJ-113365, 9/88
- Public juvenile facilities, 1985 (bulletin), NCJ-102457, 10/86
- 1982-83 census of juvenile detention and correctional facilities, NCJ-101686, 9/86

## Expenditure and employment

### BJS bulletins:

- Justice expenditure and employment: 1985, NCJ-104460, 3/87
- 1983, NCJ-101776, 7/86
- 1982, NCJ-98327, 8/85

### Justice expenditure and employment:

- Extracts, 1982 and 1983, NCJ-106629, 8/88
- Extracts, 1980 and 1981, NCJ-96007, 6/85
- 1971-79, NCJ-92596, 11/84

## Courts

### BJS bulletins:

- Criminal defense for the poor, 1986, NCJ-112919, 9/88
- State felony courts and felony laws, NCJ-106273, 8/87
- The growth of appeals: 1973-83 trends, NCJ-96381, 2/85
- Case filings in State courts 1983, NCJ-95111, 10/84

### BJS special reports:

- Felony case-processing time, NCJ-101985, 8/86
- Felony sentencing in 18 local jurisdictions, NCJ-97681, 6/85
- The prevalence of guilty pleas, NCJ-96018, 12/84
- Sentencing practices in 13 States, NCJ-95399, 10/84

### Sentencing outcomes in 28 felony courts, NCJ-105743, 8/87

National criminal defense systems study, NCJ-94702, 10/86

### The prosecution of felony arrests:

- 1982, NCJ-106990, 5/88
- 1981, NCJ-101380, 9/86, \$7.60

Felony laws of the 50 States and the District of Columbia, 1986, NCJ-105066, 2/88, \$14.70

State court model statistical dictionary, Supplement, NCJ-98326, 9/85

1st edition, NCJ-62320, 9/80

## Privacy and security

### Compendium of State privacy and security legislation:

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# Drugs & Crime Data

Data Center &  
Clearinghouse for  
Drugs & Crime



## Illicit drugs— Cultivation to consequences

### The worldwide drug business

Cultivation & production  
Foreign  
Domestic

Distribution  
Export  
Transshipment  
Import into U.S.

Finance  
Money laundering  
Profits

### The fight against drugs

Enforcement  
Border interdiction  
Investigation  
Seizure & forfeiture  
Prosecution

Consumption reduction  
Prevention  
Education  
Treatment

### Consequences of drug use

Abuse  
Addiction  
Overdose  
Death

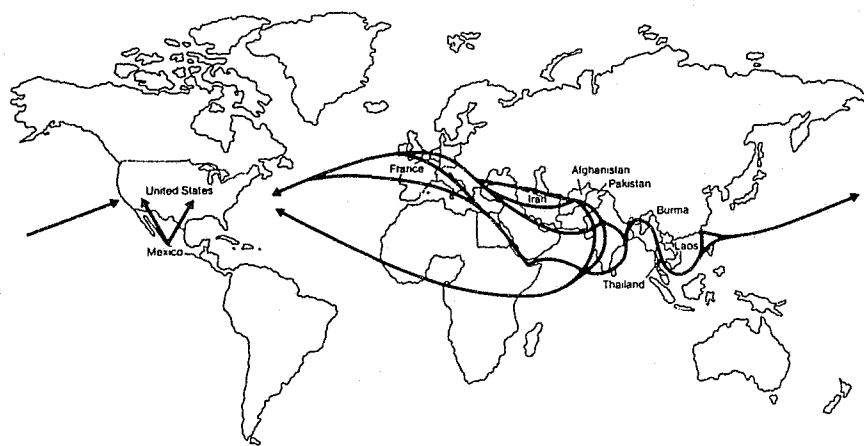
Crime  
While on drugs  
For drug money  
Trafficking

Impact on justice system

Social disruption

The Data Center & Clearinghouse for Drugs & Crime is funded by the Bureau of Justice Assistance and directed by the Bureau of Justice Statistics of the U.S. Department of Justice.

## Major heroin smuggling routes into the United States



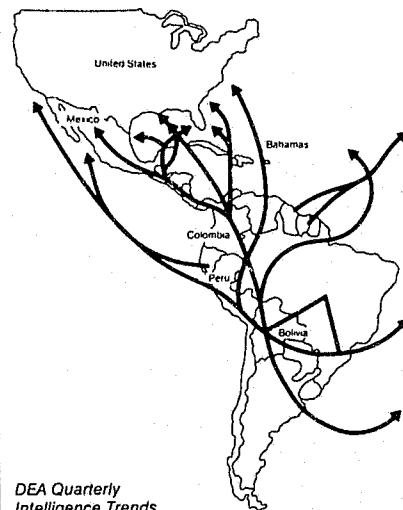
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