VITAL & HEALTH STATISTICS

An Experimental Comparison of Telephone and Personal Health Interview Surveys

This report describes the results of an evaluation of random-digit-dialed sampling and telephone data collection procedures for the collection of health interview data from households. The study design included the following components: a comparison of data obtained by personal interview using area probability sampling with that obtained by telephone interview using random-digit-dialed sampling; a comparison of computer-assisted telephone interviewing with paper-and-pencil telephone interviewing; an assessment of experimental telephone interviewing techniques; an evaluation of the effects of respondent rules on health survey reports; and assessments of interviewer errors and nonsampling bias and variance in telephone survey data.

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Cooperation of the U.S. Bureau of the Census

Under the legislation establishing the National Health Interview Survey, the Public Health Service is authorized to use, insofar as possible, the services or facilities of other Federal, State, or private agencies.

In accordance with specifications established by the Division of Health Interview Statistics, the U.S. Bureau of the Census, under a contractual arrangement, participated in planning the survey and collecting the data.

Dedication

This report is dedicated to the memory of John C. Scott (1921–81), Director of Field Office, Survey Research Center, University of Michigan.

Acknowledgments

A complex study such as this requires the efforts of many people. At the Survey Research Center, University of Michigan, this survey used some 35 interviewers, 20 coders, and many support staff. They were a particularly dedicated group, and their diligence helped greatly in the successful collection and processing of information. We especially want to acknowledge Joan Peebles, who directed the interviewing staff with the able assistance of Mary Dawson, Carolyn Hohnke, and Jeanne Domanski.

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Symbols

- - -	Data not available
. . .	Category not applicable
-	Quantity zero
0.0	Quantity more than zero but less than 0.05
Z	Quantity more than zero but less than 500 where numbers are rounded to thousands
*	Figure does not meet standard of reliability or precision
#	Figure suppressed to comply with confidentiality requirements

An Experimental Comparison of Telephone and Personal Health Interview Surveys

by Owen T. Thornberry, Jr., Ph.D., Division of Health Interview Statistics

Introduction

The primary mission of the National Center for Health Statistics (NCHS) is the collection and dissemination of data relating to the health of the population of the United States. The probability sample survey based on face-to-face interviews, such as the National Health Interview Survey (NHIS), represents one of the mechanisms for carrying out this mission. In addition, as provided in the National Health Survey Act of 1956, NCHS conducts a continuing program of research on survey methods and techniques aimed at evaluating current procedures and investigating new techniques of data collection.

This report provides a description of one of those research efforts. Specifically, it contains a methodological evaluation of random-digit-dialed (RDD) sampling and telephone data collection procedures as mechanisms for the collection of health interview data from households. Although earlier research suggests that telephone surveys may produce data generally comparable to face-to-face surveys, many questions remain about method effects and error structures in telephone surveys. Analysis of the method appears to offer opportunity for more comprehensive analysis of components of total survey error and for research in the development of procedures to reduce error. The present study was directed toward these goals.

Telephone survey methodology—NCHS interest and concern

The interest of NCHS in telephone surveys began in 1977 when the director of NCHS established a committee to assess and document the potential applicability of this methodology to the data collection needs of NCHS. This interest was motivated by a number of factors. One factor was the potential for reduction in the costs of NCHS surveys. RDD sampling and telephone interview procedures are less costly than area sampling and personal interview procedures. Another factor was the potential for improvements in efficiency and data quality through the flexibility and quality control that a centralized telephone data collection system offered. Also of interest was the opportunity for expansion of NCHS methodological research capability, including question design and pretesting of supplements to NHIS. A fully developed computer-assisted telephone interview (CATI) system would enhance the ability of NCHS to provide rapid collection and reporting of data on topics of immediate interest within the U.S. Public Health Service. In addition, the telephone methodology could facilitate greater responsiveness to data needs of State and local health agencies and other demands for data on small areas, enhancing the NCHS program of technical assistance. Finally, telephone survey methodology offered the opportunity for the development of an in-house data collection system within NCHS.

There was, therefore, consensus within NCHS that telephone survey methodology had the potential for expanding the scope and increasing the timeliness and efficiency of NCHS data systems, without a reduction in quality and at a cost below that of the personal interview. It was recognized that considerable methodological research and developmental work would be required to realize this potential. There had been few studies designed specifically to compare telephone and personal interviews and because of differences in designs and findings of the previous research, few generalizations could be made about the relative merits of the two approaches. Further, although there had been a significant amount of research on the personal interview, very little was known about the interactive process of the telephone interview.

Because telephone interview methodology (especially RDD sampling, CATI, and interviewing techniques) was less developed than personal interview methodology, there were some basic concerns about the applicability of the telephone mode of data collection to the needs of NCHS. For example, there was some concern as to whether it was possible to conduct a complex survey, such as NHIS, using the telephone. NHIS involves a complex area probability sample, offering presumably complete coverage of the civilian noninstitutionalized population, although perhaps 2–3 percent of households are missed. The household interviews are conducted face-to-face by experienced interviewers employed by the U.S. Bureau of the Census. Households are mailed materials explaining the survey and requesting cooperation prior to contact by an interviewer. All adult household members at home at the time of the interview are asked to respond for themselves. The core questionnaire, which takes on the average about one-half hour to administer, is a detailed and lengthy instrument with complex

skip patterns. Visual aids are used with some questions. Response rates of approximately 96 percent are obtained.

Many NCHS concerns about telephone methodology related to the general areas of coverage, response, and quality of data. Telephone coverage of households in the United States is 5–7 percent lower than coverage by area probability methods. In addition, past research suggested that response rates for telephone surveys would be somewhat lower than those for comparable face-to-face surveys. Persons contacted by telephone may find it easier to refuse to participate than those contacted in person. Further, in RDD surveys there is no opportunity to provide a household with introductory materials prior to first contact by telephone. A lengthy interview, such as NHIS, might have an additional negative effect on telephone response rates. There also was the concern that respondent answers obtained by telephone would differ significantly from those obtained in face-to-face interviews. Respondents might not treat the interview in a serious manner and thus would not have a strong commitment to accurate reporting. Differences in reporting were possible on items using visual aids in the personal interview, on items requiring interviewer explanation, and on questions of a sensitive nature. Other issues of concern related to the cost efficiency of the telephone approach and the use of CATI with a complex questionnaire.

Summary of research design

In 1979, NCHS contracted with the Survey Research Center (SRC) of the University of Michigan to assist in (1) development of a research program to assess telephone survey methodology and (2) design and implementation of an initial research project that would address several methodological issues of telephone surveys and include a comparison of data obtained by the telephone interview and the face-to-face NHIS. A structure for these activities focused on various components of total survey error, with attempts to quantify as many of these errors as possible for the telephone data collection.

The overall objective was to assess the strengths, weaknesses, and limitations of the telephone methodology in order for NCHS to appropriately fit the telephone approach into its program of data collection (both as an independent mode of data collection and as a methodology to complement and supplement the personal interview). A primary goal was to determine whether the telephone approach resulted in data on health variables that were comparable to those obtained in the face-to-face NHIS. This question was addressed by a comparison of the NHIS data with data from the telephone approach. In addition, the NHIS data served as a basis of comparison for various experimental manipulations within the telephone mode. Although NHIS did not formally define the standard, it did provide a reference point to compare experimental findings.

The experimental manipulations (respondent rules, interviewing techniques, paper-and-pencil versus computer-assisted interviews, and so forth) were designed to take into account some of the major factors that may produce, or may be related to, differences between the modes. A first objective was to describe any differences between personal (face-to-face) and telephone interviews; a second objective was to describe the differences across particular telephone survey designs. A variety of comparisons between face-to-face and telephone methods in the experimental telephone treatments was specifically chosen to summarize some major sources of potential differences between personal and telephone interviews. Of course, the various experimental approaches provide valuable information on the telephone method itself by identifying the most appropriate techniques.

In summary, the research conducted for NCHS by the SRC had three primary purposes: *First,* to compare and evaluate face-to-face and telephone interview methods for collecting data in NHIS; *second,* to conduct a number of methodological experiments specific to a national RDD telephone survey; and, *third,* to examine selected components of error in telephone surveys.

In the fourth quarter of 1979, SRC conducted a national probability RDD telephone survey of persons 17 years and over, using a modified NHIS questionnaire. At the same time, the U.S. Bureau of the Census was conducting the ongoing NHIS. The SRC Telephone Survey and the face-to-face NHIS yielded data on 8,200 and 19,800 persons 17 years and over, respectively.

The research design for the SRC Telephone Survey included the random assignment of sample telephone numbers to a set of treatments, resulting in three experimental groups:

1. *Experimental interviewing procedures*—Families were assigned to one of two interviewing methods. The *control* version specified a behavior on the part of the interviewer that was similar to that of the U.S. Bureau of the Census interviewers. The *experimental* version used explicit instructions and feedback to the respondent written into the questionnaire and also sought a commitment from the respondent to answer carefully and honestly.
2. *Respondent rules*—Two alternative respondent rules were used. In the *knowledgeable adult* half-sample, an adult judged as capable of answering the health questions responded for all adults in the family. In the *random respondent* sample, one person 17 years and over was randomly selected to respond for all adults in the family.
3. *Computer-assisted telephone interviewing (CATI)*—Random half-samples of telephone numbers were assigned to either a *CATI* or *paper-and-pencil* version of the questionnaire.

Summary of major findings

This section provides a brief overview of the major findings of the research. The relevant chapters of this report may be consulted for a more comprehensive discussion of findings. In particular, an understanding of the strengths and limitations of the research design, as detailed in chapter I, is essential to interpretation of the telephone interview and personal interview comparisons.

Differences between telephone and personal interview data

A major component of this research was a comparison of data obtained by the personal (face-to-face) interview using

area probability sampling in NHIS with data obtained by the telephone interview using RDD sampling in the SRC Telephone Survey.

The overall response rate for the SRC Telephone Survey was 80 percent; the NHIS response rate was approximately 96 percent. This difference is one of the most evident differences between the two modes and deserves comment. The telephone response rate is consistent with that achieved in many personal interview surveys conducted by survey organizations other than the U.S. Bureau of the Census, and is a higher rate than that obtained by most telephone surveys. The higher than usual telephone response may be attributable to a variety of characteristics of the project—the legitimacy of the U.S. Public Health Service as the sponsor, the topic of health events, the lengthy training of the interviewers, continual monitoring, and high morale of the staff.

The telephone survey response rate was not constant over all subgroups of the sample. Relatively lower response rates were obtained for the poorly educated, young adults, and the elderly. The lack of cooperation among the last group should cause some concern for health researchers because of the negative correlation of age with health status.

Much smaller differences in general were found on the responses to specific survey questions in the two modes, and the findings were contrary to what was expected based on previous research. There was consistently higher reporting of health events among the telephone respondents than among the face-to-face respondents. That is, the majority of measures indicated more reporting of health events for the SRC telephone respondents than for the NHIS respondents. Additional analyses were performed to determine if subgroups of the population exhibited variation in the differences between modes and to search for other interactions in mode effects. However, it was found that higher levels of reporting among telephone respondents appeared within all age, sex, and education groups.

As with most studies comparing modes of data collection, this research was not able to measure a pure effect of mode, but confounded differences in interviewing staffs, questionnaire form, and nonresponse errors with differential response errors. However, it is worth noting that although there was generally more reporting of health events in the telephone survey, the magnitude of differences between the two modes was generally small. In any case, the findings suggest that the initial NCHS concerns about major differences in data quality between the ongoing NHIS and a telephone NHIS were largely unfounded.

The experimental interviewing techniques

Sample cases in the SRC Telephone Survey were randomly assigned to one of two interviewing treatments. The control procedure was an attempt to approximate an NHIS interview as conducted by the U.S. Bureau of the Census, that is, to use the same interviewer behavior as in the personal interview. This approach restricted the interviewer to asking questions as worded in the questionnaire and to using specified probes and introductory statements. For comparison to the control procedure, an experimental treatment was administered to the other half of the telephone sample. This procedure, developed by SRC in previous research, incorporated commitment, instruction, and feedback techniques in the questionnaire. The commitment technique involved verbal agreement by the respondent to give accurate and complete information. Instructions were in the form of statements in the questionnaire at various points for the interviewer to read (for example, "This is sometimes hard to remember, so please take your time."). Feedback was both positive ("I see, this is the kind of exact answer we need.") and negative ("You answered that quickly. Are there any days you might have overlooked?").

For almost all health events, there were higher levels of reporting for the experimental group than for the control group. To search for interaction effects in the experimental interviewing methods, further analysis was performed on demographic subgroups differing in age, sex, or education. In general, the effects of the experimental treatment were not eliminated when controls for respondent characteristics were applied. The experimental techniques appeared to facilitate increased reporting on health variables in this study.

The effects of respondent rules on health survey reports

Two respondent rules were used in the SRC Telephone Survey to facilitate comparison of responses based on self-reporting and proxy reporting. Interviews in half the sampled households were conducted with a knowledgeable adult respondent, often an adult who answered the telephone. The other half of the interviews were conducted with a randomly chosen adult. In each case, the respondent reported for all adult family members.

The results of this study were compared with those of a previous NCHS study designed to measure the effect of proxy reporting on health statistics in the NHIS. The hypothesis that maximum self-reporting would yield higher rates of illness and medical utilization than the standard NHIS procedures, which allow proxy reporting, was supported in the earlier NCHS research. Although similar results were expected with the present study, a different pattern of findings emerged. With the random respondent rule, more health reports were obtained for others than for the self-respondent. Additional analyses, between and within the two respondent samples in the telephone survey, did not alter this finding. Furthermore, even after applying multivariate models to adjust for nonresponse differences, significant proxy effect remained.

This overall tendency toward higher proxy reports runs directly counter to previous findings about self-reports versus proxy reports. Although there are several hypotheses that might explain the effects observed in this study, few are testable without validating data.

A comparison of CATI and non-CATI questionnaires

The research design for the SRC Telephone Survey also included the random assignment of half-samples to one of two methods of administering the questionnaire. Half were assigned to typical paper-and-pencil questionnaires and half were assigned to a CATI system. Each interviewer used both of the modes of asking questions, alternating modes each week.

On most statistics examined in this study, only small differences between CATI and paper-and-pencil modes were found. CATI and non-CATI response rates were identical. There were also no major differences between the two interviewing procedures on response distributions for health measures. Finally, an assessment of preferences of the interviewers revealed no major differences between the two modes.

There were, however, some exceptions to this finding of equivalence between methods. The average number of minutes per CATI interview exceeded the average for the non-CATI interview. With paper questionnaires, interviewers sometimes begin reading the next question on the page while they record the answer to the current one; this procedure is not possible on CATI. The resulting delay and the time required to display the next question may account for the longer CATI interview times.

On the other hand, there is evidence that interviewer variability in the responses tended to be lower in CATI than in non-CATI responses. In addition, there is evidence of fewer skip-pattern problems with the CATI responses. These two findings are indicative of some of the potential benefits of CATI systems.

Measurement of interviewer errors in the SRC Telephone Survey

This study used an interpenetrated design for assignments to interviewers to measure certain components of interviewer variance present in the data. In addition, a monitoring procedure was constructed in which a supervisor listened to the interview and coded each interviewer behavior according to whether it conformed to techniques and procedures in which the interviewers had been trained. This approach permitted investigation of whether the rules prescribed for interviewer behavior were related to the magnitude of interviewer contribution to the variance of the survey statistics; specifically, it permitted an assessment of whether guidelines for interviewer procedures were closely related to interviewer variance.

Although the estimates of interviewer effects are subject to some instability, the major finding from this research was that unusually low levels of interviewer effects were measured in the SRC Telephone Survey. This result could be due to the stringent controls on interviewer behavior that were introduced in this study but were absent in past studies. Because there was little observed interviewer variability, the analyses attempting to use the monitoring data to explain interviewer effects were largely unsuccessful.

Nonsampling bias and variance in the SRC Telephone Survey data

The purpose of this analysis was to examine the data for any relationship between the effect of the experimental interviewing procedures on response bias and levels of interviewer variance. That is, did the experimental interviewing procedures reduce response bias at the cost of increasing the magnitude of interviewer variance? That question is investigated by combining the results from the comparison of statistics on the two experimental groups with the changes in values of intraclass correlations for the same statistics. Although the findings that are presented are limited by the small number of statistics examined, they tend to dismiss the possibility that the increases in reporting associated with the experimental interviewing behaviors were coming at the expense of greater interviewer variance.

Overview of monograph

The following chapters describe the research, present the detailed findings, and discuss the implications for telephone survey methods for NCHS surveys. The research design and the sources and limitations of the data are discussed in detail in chapter I. Comparison and evaluation of data from the SRC Telephone Survey and from the NHIS personal interviews are provided in chapter II. The next three chapters (III–V) address the results of the three experimental treatments: The experimental interviewing techniques, the respondent rules, and the CATI and paper-and-pencil comparison. The final two chapters (VI and VII) examine measurable sources of error related to interviewer behavior in the SRC Telephone Survey.

Appendix I examines the effects of postsurvey adjustments on the comparisons between the SRC Telephone Survey estimates and the NHIS estimates. Estimates of sampling errors for alternative estimators are addressed in appendix II. Detailed tables for the telephone interview and personal interview comparison and the control and experimental interviewing treatment comparison are found in appendixes III and IV, respectively. The interviewer instructions for the SRC Telephone Survey are provided as appendix V. The experimental version of the questionnaire used in the SRC Telephone Survey is given in appendix VI. With the deletion of the statements on feedback, instructions, and commitment, the experimental version is identical to the control version. The NHIS questionnaire can be found in *Current Estimates From the National Health Interview Survey: United States, 1979* (Series 10, No. 136).

Chapter I
Study design

by Charles F. Cannell, Ph.D., Robert M. Groves, Ph.D., and Peter V. Miller, Ph.D., Survey Research Center, Institute for Social Research, University of Michigan, and Owen T. Thornberry, Jr., Ph.D., Division of Health Interview Statistics

Introduction

The primary purpose of this study is to investigate the effects on survey results of different modes of data collection. Conducting interviews by telephone and face-to-face presents different communication problems for interviewers and respondents, and the data obtained through the two modes may reflect these differences. Sampling frames and sampling procedures are likely to differ in surveys employing the two modes, as are questionnaire designs and interviewing methods. The increasing use of computer-assisted telephone interviewing (CATI) systems may alter the dynamics of the interview.

In addition to these basic differences between the face-to-face and telephone modes, there are major variations in procedures that are possible *within* each mode. Sampling methods, respondent rules, specification of interviewer behavior, callback procedures, supervisory methods, and questionnaire form can vary greatly within telephone and face-to-face interview surveys. Hence, any mode comparison must carefully specify the various features of the designs.

The purpose of the discussion of study design features in this chapter is to provide information that is necessary for an understanding of the specific analyses that follow. Summary descriptions are provided for the face-to-face National Health Interview Survey (NHIS) and for the separate components of the Survey Research Center (SRC) Telephone Survey. Alterations in NHIS protocol that were required for application in the SRC Telephone Survey are discussed. The chapter concludes with descriptions of the random-digit-dialed sample design and interviewer training and assignments.

The National Health Interview Survey

The population covered by NHIS is the civilian noninstitutionalized population of the United States living at the time of the interview. The sample does not include members of the Armed Forces or U.S. nationals living in foreign countries. The sampling plan of the survey follows a multistage probability design that permits a continuous sampling of the population. The first stage of the sample design consists of drawing a sample of 376 primary sampling units (PSU's) from approximately 1,900 geographically defined PSU's. A PSU consists of a county, a small group of contiguous counties, or a standard metropolitan statistical area. Without loss of general understanding, the remaining stages can be combined and treated in this discussion as an ultimate stage. Within PSU's, then, ultimate stage units called segments are defined in such a manner that each segment contains an expected four households.

The usual NHIS sample consists of approximately 12,000 segments containing about 51,000 assigned households, of which about 9,000 are vacant, demolished, or occupied by persons not in the scope of the survey. The 42,000 eligible occupied housing units yield a probability sample of about 111,000 persons. Therefore, for a single quarter of a year, an expected 27,750 persons would fall in the sample.

Field operations for the survey are performed by the U.S. Bureau of the Census under specifications established by NCHS. The U.S. Bureau of the Census participates in survey planning, selects the sample, and conducts the hiring, training, and supervision of field interviewers. The data are coded, edited, and tabulated by NCHS.

Each person 19 years of age and over present at the time of the interview is asked to participate in a group interview for the NHIS questionnaire. For children and for adults not present in the home at the time of the interview, the information is obtained from a related household member such as a spouse or the mother of the child. For purposes of the project described in this report, information on children is omitted from the analyses.

SRC Telephone Survey components

Because there is no standard way to define a telephone survey, it is desirable to investigate systematically various alternative features of the mode when making comparisons with personal (face-to-face) interview surveys. This mode comparison was designed to assess independently the effects on the data of several components of the SRC Telephone Survey mode, as well as to estimate the overall mode differences. Two interviewing procedures, experimental versus control; two types of questionnaire administration, CATI versus paper-and-pencil; and two respondent selection rules, knowledgeable respondents versus random respondent, were experimentally manipulated.

The study design, which includes several experimental groups to which sample cases were randomly assigned, is summarized in table A. The cells contain the number of persons for whom health data were collected within each treatment.

Interviewing procedure experiment

The absence of visual cues in telephone interviews requires reconsideration of appropriate interviewing techniques.

Table A. Number of persons with interview data, by experimental group, in the Survey Research Center Telephone Survey

Mode	All persons	Random respondent rule			Knowledgeable respondent rule		
		Total	Experimental interviewing behavior	Control interviewing behavior	Total	Experimental interviewing behavior	Control interviewing behavior
All modes	8,001	3,874	1,846	2,028	4,127	2,028	2,099
CATI	3,671	1,770	837	933	1,901	964	937
Non-CATI	4,330	2,104	1,009	1,095	2,226	1,064	1,162

NOTES: 209 individuals in households where the random respondent could not be interviewed, but where some other family member responded, have been removed from this table. CATI = computer-assisted telephone interviewing.

In face-to-face interviews, interviewers often communicate understanding of responses in nonverbal gestures. In addition, visual aids such as calendars and response cards are commonly employed in face-to-face contacts to illustrate response tasks. Methods of communicating response tasks and of acknowledging responses or the need for more information need to be systematically used in telephone interviews. Further, there are indications that telephone respondents may be less motivated to participate in the interview than are those contacted personally. Telephone response rates are typically lower than those achieved by personal contacts. Responses to open questions appear to be truncated by telephone respondents. The speed of interviewer-respondent interaction tends to be faster over the telephone, and telephone respondents often report they would prefer to be interviewed in person.[1] These findings suggest the need to motivate telephone respondents to participate conscientiously in the survey.

For this project, standardized interviewing procedures were developed to address these problems of telephone contacts. The interviewing techniques—instructions, feedback, and commitment—were intended to inform respondents about the response tasks, to communicate that they have performed them adequately, and to motivate them to take the interview seriously and expend conscientious and diligent effort in responding.[2] These interviewing techniques were used in an experimental interviewing procedure assigned to a random half-sample of the SRC Telephone Survey households. The results of this treatment will be compared to a control interviewing procedure designed to mirror the techniques used by U.S. Bureau of the Census interviewers in the face-to-face interview survey.

CATI experiment

The increasing use of the telephone in survey research has been paralleled by research and development of CATI. With a CATI system, interviewers use video display terminals that present questions and permit the interviewer to enter responses. The computer performs checks on whether responses, as entered by the interviewer, are valid codes and moves the interviewer from question to question according to programmed logic. This technology might offer greater flexibility in questionnaire construction, greater control over interviewer behavior, faster production of data files for analysis, and possibly even lower costs because coding, keypunching, and data cleaning are reduced or eliminated altogether. In the SRC and NCHS study, interviewers conducted a random half-sample of the telephone interviews using a CATI system, and the other half using paper questionnaires. Table A shows that fewer CATI than non-CATI interviews were taken. This difference is due to technical difficulties with the CATI system early in the study. During several days of computer difficulties, cases that had been randomly assigned to the CATI group were administered interviews using paper-and-pencil questionnaires.

Respondent selection experiment

In the ongoing NHIS, all members of sampled households who are at home when the interviewer calls are interviewed in person. The questionnaire is administered for the whole family in a group setting with all those present participating in the interview. Parents always respond for children 16 years and under, and some family member at home responds for other absent adult family members. It was obvious that the group interview format was not feasible on the telephone, but it was not clear what alternative procedure was best. To investigate the effects of alternative respondent selection rules, half of the households in the telephone sample were assigned to a random respondent rule and the other half to a knowledgeable respondent rule.

In interviews with the first half-sample of respondents assigned to the random respondent rule, adults in each family of the household were listed. One from each family was randomly selected to answer questions concerning his or her own health and that of other adult family members living in the household. In households assigned to the knowledgeable respondent rule, any person 19 years and over who answered the telephone and was capable of responding for himself or herself and other adult family members was used as family informant.

Neither respondent rule sought to interview each individual in a family separately. Rather the informants first answered questions about themselves, then one by one about other family members. The knowledgeable telephone answerer rule is closer to the NHIS procedure than the random respondent rule, because an available adult serves as a proxy respondent for others in the family. In contrast to NHIS, however, no attempt was made to speak with other members of the family even if they were at home at the time of the interview. Thus, there is a single self-respondent per family. In the random respondent rule, the self-respondents so selected comprise a probability sample of adults in telephone households. Thus, a comparison can be made between statistics based on data from all family members, many of whom did not report for themselves, and statistics based only on randomly selected adults, most of whom were self-respondents. Table A shows that there

were more telephone answerer cases than randomly selected cases. This difference is due to refusals or inability to contact the randomly selected respondent. In the case of 94 families in the random respondent selection rule, the selected respondent could not be interviewed. In situations covering some 209 people, another family member was selected to respond for the family. These individuals are not included in table A.

Alterations made in standard NHIS procedures in the SRC Telephone Survey

It was necessary to make alterations in the NHIS protocol for its application in a telephone survey. The alterations were made to adapt the questionnaire for telephone use and were not manipulated experimentally, so their effects cannot be measured directly. The NHIS is structured to accommodate a group format for the interview. In some sections of the questionnaire, questions are asked of or about each member of the family before a new question or section is begun. This structure is well suited to the situation in which the interviewer is able to gather the family together and involve them in the interview. However, on the telephone it is difficult to maintain this sort of flow in the questionnaire because only one respondent can hear the questions at any one time. It is then necessary to restructure the NHIS questionnaire to ask each section separately about each person, making sure of the focus of the questions. This involves making a decision about how best to stimulate the respondent's memory. Should one focus on the *event*—bed day, doctor visit, hospitalization, and so forth—as in the NHIS, or organize the interview by the *person* and ask about each individual's health events in turn?

The procedure on the telephone consisted of asking the respondent all of the questions concerning his or her own health. Next, all of the questions about the next-listed eligible person were asked, and this procedure was followed for all eligible persons. After this was completed, the questions about conditions, doctor visits, and hospitalizations were asked for the respondent first and then for the next-listed person, and so on.

The rationale is that focusing longer on each person will lead to more careful consideration of that person's health history by the family respondent. The person pages were separated from the condition, doctor visit, and hospitalization sections for the same reason that they are separated in the personal NHIS—so as not to discourage reporting of health experiences by teaching the respondents that each time they report something they will be asked a series of followup questions.

Other changes were forced by the fact that the telephone permits only audio communication. Using the telephone to collect information generally obviates the use of visual aids such as calendars as used in NHIS. Attempts must be made to compensate for their absence. Tests were included in the pretests to see whether respondents had calendars available and were willing to use one of their own calendars for the interview. Because about half of the respondents did use a calendar in the pretests, the procedure was used in the survey. In addition, the reference dates were repeated frequently. These procedures and changes in the flow of the interview may have had unmeasurable effects on the data.

A final questionnaire alteration made for the telephone interview was the omission of certain questions from the standard NHIS protocol. Among the NHIS core items, the chronic condition list (question 32) and some sections of the condition pages were not asked. The 1979 NHIS supplement sections (for example, the home care page, immunization page, residential mobility page, and the medicaid and social security questions) were also eliminated. These alterations were made because of financial constraints that limited the length of the telephone interviews. The data were analyzed in a manner designed to minimize the effects of the condition list omission on estimates of other NHIS core items. The comparisons use only the data from the NHIS interview prior to questions in which the lists were administered.

Finally, the comparison between telephone and face-to-face surveys, again due to financial limitations, was confined to the comparisons of *adult* reports, those 17 years and over. Information on children was not collected over the telephone because of the increased length of the interview that this procedure would have required.

Sample design

The telephone sample used in this project is a two-stage stratified design selecting telephone numbers that are randomly generated using computer algorithms. The design is a variation of that described by Waksberg[3] and evaluated by Groves.[4] The design uses as its sampling frame the list of working area and central office code combinations (AC–CO) in the coterminous United States. Area codes form the first three digits of U.S. telephone numbers, and central office codes or prefixes form the second set of three digits. Several aspects of the design used are as follows: (1) stratification of the frame prior to selection, (2) design of the clustering of the sample into groups of consecutive numbers, and (3) selection and implementation of the sample design.

The SRC telephone samples make use of the AC-CO frame, stratified by sorting the file of AC–CO records in the following manner:

- Separating exchanges with one central office code, which average 10 percent working household numbers, from those with more than one, which average about 30 percent working household numbers.
- Sorting the records by major U.S. Bureau of the Census region, State, and area code.
- Within area codes, grouping together all central office code records that are located in the same exchange and ordering the exchange groupings within area codes by the numbers of the central office codes in the exchange.
- Within groups of exchanges that have the same number of central office codes, ordering the exchange groups by the two geographical coordinates, rotating the order across size groups—northwest to southeast, southeast to northwest, and so forth.

The result of such sorting is a file that groups together telephone numbers that are located geographically proximate and in an area with the same relative population density, as measured by the number of central office codes required to serve the exchange area. After this sorting is implemented, a systematic sample of records is taken, and four-digit random numbers are appended to the chosen AC–CO combinations to form the sample telephone numbers.

The sample is selected in two stages, one that identifies clusters of numbers to be selected, and one that selects numbers from those clusters. The first-stage units are clusters of 100 consecutive numbers within a central office code. For example, the numbers 313–764–4400 to 313–764–4499 form a cluster of 100 consecutive numbers within the 764 central office code in area code 313.

Clustering is usually introduced in area probability samples for personal interview surveys to save travel costs for contacting respondent households. However, clustering is introduced in this telephone sample design to increase the proportion of generated numbers that are working household numbers. If a systematic sample of AC–CO combinations is selected from the sorted file described above, the four-digit numbers are selected with probabilities proportionate to the number of working household numbers within them, then the proportion of working household numbers can be approximately 60 percent within selected clusters.

The two-stage design is implemented in the following way:

- A systematic sample of AC–CO records is taken in each stratum, in single and multiple CO exchanges.
- Four-digit random numbers generated using standard computer routines are appended to each AC–CO selected. The resulting numbers could be called primary numbers.
- The primary numbers are telephoned. If the number is a working household number, the cluster of 100 consecutive numbers of which it is a member is included as a sample cluster. If the number is not a working household number, its cluster is not included in the sample.
- Within sample clusters, a fixed number of working household numbers is selected as secondary numbers. For example, if the cluster size was set at five working household numbers, four more numbers in addition to a primary number would be selected from the 100 series. Each number would be called; if any number proved to be not a working household number, another number in the 100 series would be generated. This process would continue until four working household numbers in addition to the primary number are generated.

The probability of selection of each working household number can be described as the product of three probabilities. The probability that a primary number is generated from a particular 100 series is $a/100A$, where a is the number of primary numbers selected from A total AC–CO records. The second factor is the probability that the generated primary number is a working household number. That probability is the number of household numbers in the 100 series divided by 100. Finally, given that the primary number is a working household number, the probability that a particular number in the 100 series is chosen is b divided by the number of household numbers in the 100 series, where b is the number of working household numbers chosen per cluster.

The overall probability of selection of each number is, therefore, $ab/100A$. This probability is a constant for all telephone numbers within the stratum; thus, the design is a self-weighting sample of telephone numbers within strata.

Implementation of the sample design

Such a two-stage design was implemented in each stratum of single and multiple CO code exchanges. Different cluster sizes were used in the two strata in an attempt to improve the precision in the low-density stratum. The first-stage sampling fraction in the single CO code stratum was twice that in the other stratum, but the second-stage fractions were such that the overall design is a self-weighting design of telephone numbers.

Because the survey was designed to collect data throughout the last quarter of 1979, as defined by the NHIS field procedures, some internal replication could be introduced within the sample. Each of the 3 months within the quarter was assigned separate samples of identical design. All three sets of primary numbers were selected at the same time, each month's sample forming a one-third systematic random sample of the entire set of primary numbers. The three separate samples differed in size somewhat in reaction to potential losses of interviewing productivity during the holiday month of December. The first month's sample consisted of about 1,850 numbers; the second, of about 2,350 numbers; the third, of about 1,350 numbers. Statistics sensitive to monthly differences during the last quarter can be computed using weights to adjust for the unequal probabilities of selection among the 3 months. The three samples were introduced sequentially in the middle of October, the middle of November, and the middle of December. Supervisory efforts were made to finish each sample completely before the next month's sample was introduced. For those numbers that were not contacted by the time the next month's sample was introduced, calling continued. The recorded date of the interview for these cases will permit separate analysis to explore the effects of this rule.

Telephone interviewer training

Thirty-five interviewers were hired for this study. Ten left before the interviewing was completed. Of those who remained, 7 were male and 18 were female. Nearly all had at least some college training. About half were between 20 and 25 years of age. All were new to interviewing, except two with some minor, short-time interviewing experience. Interviewers without previous experience were sought on the assumption that they could more easily be trained in new procedures.

Interviewer training consisted of three segments: (1) training in interviewing techniques and use of the questionnaires and procedures, (2) training in CATI computer terminal operations, and (3) interviewing practice.

The first 2 days of training were devoted primarily to instruction on techniques and questionnaire content. Included

were demonstration and role-playing interviews. Lectures were kept to a minimum, with heavy trainee participation in discussion and role playing. The goal was to inform the trainees about what was to be done and how it was to be done, then to schedule them for practice under supervision with continual feedback.

Sampling procedures were described on the third day. Additional role playing was included. On day 4, interviewers were introduced to the computer terminal operations. The first 3 hours were demonstrations and practice only in terminal techniques. The remainder of the day was spent on role-playing interviews and entering answers into the terminal. The fifth day was spent in practice interviewing. Using CATI, interviewers first called acquaintances and then strangers.

The next 3 days were spent in closely supervised practice interviews with strangers. At the conclusion of this period, most interviewers were judged to be competent to begin production interviewing. A few were given 1 or 2 more days of practice prior to regular interviewing.

In addition to the formal training sessions, several methods were used to update and review information with interviewers during the course of the study.

- Written memoranda on changes, corrections, or problem areas were used.
- Meetings were held with interviewers to review administrative procedures and discuss interviewing techniques. The latter included role-playing introductions and sharing successful refusal conversion techniques.
- The study managers or supervisors consulted with individual interviewers on specific problems, using discussions, monitoring, role playing, additional study, practicing with a tape recorder, or any combination of these.

There are three questions that must be considered in any system that evaluates the effectiveness of an interviewer. First, does the interviewer know what constitutes an adequate performance? Second, is the interviewer sufficiently skilled to behave in the correct manner? Third, is the interviewer motivated to perform correctly and adequately? Knowledge of correct behavior is, of course, a major component of the interviewer's training. The principles and techniques that are specified during interviewer training are by definition the correct behaviors.

For that reason, evaluations of a performance may differ in some respects from one staff to another, depending upon the principles of interviewing that each one teaches or stresses. A monitoring system should focus on the major tasks that are taught during training, identify each one, and evaluate the interviewers' performance of them. For this study, a monitoring system was developed that involved the coding of interviewer behavior. Monitors listened to the interview and coded the interviewer activity as it occurred. The major purpose of monitoring is to identify interviewer errors for supervisors' use in improving interviewing. Monitoring is also used in training to help to identify and correct errors. Table B summarizes findings from monitoring interviewers during the study.

Table B shows that overall, interviewers delivered questions clearly and exactly as worded. Open questions presented the most problems for interviewers. Because of skip patterns, these questions were seldom asked and can also be classified as questions that were burdensome to both the interviewer and respondent. Few questions, less than 9 percent of all observed questions, required the interviewer to define terms or probe for more information. The experimental interviewing techniques, which provide the respondent with information to adequately perform the interviewing task, reduce the interviewer's need to use probes.

Table B. Mean proportion of selected interviewer behaviors, by type of question

Interviewer behavior	*Type of question*[1]		
	Closed N = *6,905*[2]	*Restricted open* N = *2,985*[2]	*Open* N = *330*[2]
Question delivery	Mean proportion		
Correct reading	0.87	0.89	0.60
Minor changes	0.08	0.08	0.08
Major changes	0.05	0.03	0.32
Evaluation of question reading			
Correct pace, clear speech	0.94	0.93	0.94
Fast pace	0.03	0.03	0.00
Unclear speech	0.03	0.04	0.06
Probing and defining activities			
Proportion of questions probed	0.03	0.12	0.09
Correct probing	0.75	0.74	0.77
Incorrect probing	0.25	0.26	0.23
Proportion of questions with definitions	0.02	0.04	0.03
Correct	0.81	0.87	0.91
Incorrect or inappropriate	0.19	0.14	0.09

[1]Of the 153 different questions monitored, 69 were classified as closed, 37 were classified as restricted open, and 57 were classified as open.
[2]The *N*'s report the number of observations of each question type.

Assignment of sample cases to interviewers

Associated with the coding of interviewer behavior is the measurement of interviewer variance. This approach seeks to describe the extent to which respondents' reports of health events tend to vary depending on which interviewer obtained the report. To measure interviewer variance, it is necessary to randomly assign respondents to interviewers. Although this random assignment is usually not financially possible for personal interview surveys, it is quite feasible in a centralized telephone facility. An interpenetrated design required to assess interviewer variance was used in this study.

Interviewers employed for this study conducted interviews using all of the experimental manipulations described earlier in table A. That is, there were no specialists in the control interviewing procedure or in the CATI technique. Interviewers did not select the procedures they were to perform, because households were assigned to interviewing techniques, questionnaire administration procedures, and respondent selection rule by the sample coversheet. Moreover, the allocation of work was accomplished in such a way that interviewers did not perform the techniques in any particular order. As mentioned, interviewers also were monitored throughout the study to be certain that they continued to maintain operational distinctions between the treatments, for example, that they did not use experimental interviewing techniques in control interview households or *vice versa.* For comparison of CATI and paper-and-pencil questionnaires, interviewers worked on the automated system during alternate study weeks—1 week on CATI, 1 week on paper-and-pencil questionnaires. The CATI and non-CATI interviews shared a common component—a family folder—in which interviewers kept track of family members, and their conditions, doctor visits, and hospitalizations.

Summary and qualifications

From this introduction to the design of the project, it should be clear that the telephone version of NHIS is actually several different telephone survey designs conducted concurrently. All of them are based on the same random-digit-dialed sample design, but they differ radically on respondent rules, interviewing behavior, and use of computer assistance.

The experimental design described above was constructed to measure the effects of different components of telephone surveys so as to decompose differences between telephone and face-to-face surveys for better understanding and methodological evaluation. However, there are aspects of telephone and personal interviews with effects that could not be measured or controlled. In addition, some controls that limit the inferences from this study were applied.

Because no experimental variations were made in the NHIS personal interview survey, the effects of individual features of the face-to-face procedure cannot be identified in the same way as the telephone survey. Although some of the factors underlying differences between telephone and face-to-face interviews can be identified, questions about what features of the face-to-face interview procedure might have produced the differences will remain.

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Chapter II
Differences between the telephone and personal interview data

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Introduction

One orientation to the potential differences between telephone and personal interviews arises from the considerable experimental research on mediated versus face-to-face communication.[1] This literature has noted that audio and face-to-face communication differ in their channel capacity and their intimacy. Regarding channel capacity, it has been found that face-to-face communication is more capable of conveying affect and evaluation of others,[2,3] and also serves to regulate the conversation flow.[4,5] Regarding the characteristic of intimacy, Mehrabian[6] noted that the telephone reduced the immediacy of persons in communication; Morley and Stephenson[7] noted more formality in telephone communication.

There has also been speculation that although these factors may influence free-flowing communication, they will not affect the more restricted communication involved in response to survey questions. There may be other factors that have greater influence on response differences between telephone and personal interviews. For example, Cannell and Fowler[8] have suggested that the most relevant variable is the greater anonymity of the phone interview. Colombotos[9] has approached the issue from the perspective of the degree of interviewer presence and, thus, the potential for interviewer-respondent involvement. It can be argued that if the respondent's reaction derives largely from social involvement it can be expected to result in bias. That is, the response will be primarily a function of the social relationship between the respondent and the interviewer instead of a response to the task of the interview.[10] Assuming that the respondent will be more sensitized to the interviewer when the latter is physically present, the argument can be made that the telephone interview will provide more accurate reporting than the personal interview. The converse argument is that the greater interpersonal involvement in the personal interview can result in greater rapport that will facilitate a commitment to the task of the interview and consequently accurate reporting.

The inference from the above discussion is that major differences between telephone and face-to-face interviews will be found. However, these expected differences generally do not appear. The most consistent finding from previous research is one of no difference in response distributions among respondents to the two modes. Three health surveys based on similar research designs[11-13] reached very similar conclusions. There were few personal and telephone interview differences in responses to questions of a factual, nonthreatening nature. However, for some questions defined as threatening or with a potential for social desirability bias, there was generally less reporting in the personal interview than in the telephone interview. For example, Hochstim[11] found that women were less likely to report that they drank wine, beer, or whiskey in personal interviews than in telephone interviews. In a study of physicians' attitudes, Colombotos[9] found no difference in susceptibility to social desirability bias between the modes. Other studies (for example, Wiseman,[14] Rogers,[15] Locander,[16] and Klecka and Tuchfarber[17]) report few, if any, significant differences between the modes. Groves and Kahn,[18] although in general finding few differences, note that telephone respondents tended to express more optimism about the state of the economy, had shorter answers to open questions on important problems facing the country, and tended to report feeling uneasy discussing certain sensitive topics than personal interview respondents did. Both in that study and in another,[19] respondents reported preferring the face-to-face mode to the telephone mode of data collection. Jordan, Marcus, and Reeder[20] compared responses to health attitude and behavior questions in telephone and personal interviews and found that telephone respondents evidenced more acquiescence, evasiveness, and extreme responses on attitude items.

In conclusion, the previous survey literature gives little clear guidance to the presence and nature of differences between modes. This lack may be partly due to the ad hoc nature of many of the comparisons and the corresponding lack of control over interviewing procedures and sample characteristics. In all cases, the comparisons confound response and nonresponse errors. In most cases, differences in the coverage of the population by telephone contribute to the differences between statistics calculated from the two modes. Even if there were a simple consistent mode effect, the previous literature would not reveal it because the other nonsampling errors, with which it is confounded in these designs, vary greatly across the studies.

The purpose of this chapter is to present comparisons in results between the face-to-face National Health Interview Survey (NHIS) and the Survey Research Center (SRC) Telephone Survey. The chapter is divided into two major sections. The first section reports on response rates in the two data collection modes. The second section investigates the magnitude of response differences between the two modes as well as any demographic subgroup interactions in the comparison.

Response rates

The generally accepted definition of a survey response rate is the percent of eligible units sampled that provided the desired survey measurements. The two survey modes present different problems for both the measurement and level of response rates. These differences occur both in the numerator and the denominator of the rate. In this section, the response rates for both surveys are presented and discussed.

The data available from NHIS on cooperation present results on the household level. The response rates are sensitive to the performance of the interviewers and the interviewing procedures in generating cooperation and the identification of sample units that are eligible for the survey. The former characteristic affects the numerator of the response rate; the latter, the denominator.

Interviewers are required to determine whether a unit sampled fulfills the housing unit definition. This means that they are required to identify vacant or demolished units, and, therefore, distinguish units with temporarily absent households from those truly vacant. Vacant units are omitted from the response rate calculation. Units with absent households are included as noninterview cases. Vacant units represent 10–15 percent of all sample units in NHIS.

Table C shows the NHIS response rates for the fourth quarter of 1979 and the full year, 1979. Both in the last quarter and in the full year, the survey achieved a 96-percent response rate. The reasons for noninterviews appear to be about evenly divided between refusals and all other reasons, such as failure to contact and incapacitation.

The calculation of response rates from telephone surveys using samples of randomly generated numbers presents different problems than does an area probability sample personal interview survey. First, in the numerator of the response rate it has been found repeatedly[18] that partial nonresponse occurs to a much greater extent in telephone interview surveys. It is indeed rare that an interviewer is asked to leave the respondent's home in the middle of administering an interview. However, terminating a telephone conversation initiated by a stranger who is asking a battery of unanticipated questions is evidently found to be a more acceptable behavior.

A more fundamental problem concerning the calculation of the telephone survey response rate also exists. This problem concerns the denominator of the response rate—the total number of eligible units in the sample. Randomly generated telephone numbers include residential numbers, nonresidential numbers, and nonworking numbers. The first category *should* be included in the denominator of the response rate; the last two categories should not. Some nonworking numbers, when dialed, provide a ringing tone, exactly like that provided by working residential numbers. In addition, some nonresidential numbers, for example, pay telephones in remote locations, may rarely be answered, regardless of how frequently the number is dialed. Thus, as a telephone survey progresses, sample numbers that ring without an answer over repeated dialings accumulate. It is not clear whether such numbers should be treated as working household numbers and included in the denominator of the response rate or as ineligible numbers and removed from the denominator.

It has been SRC practice to place telephone calls to the local telephone business office responsible for sample numbers that ring repeatedly without answer. The vast majority of such offices will indicate whether the telephone number is a working household number. This information is then used to make decisions concerning the replacement of the number for the sample administration and, thus, whether the case should be included in the denominator of the response rate.

Given this prelude to the problems of calculating a response rate for telephone surveys using samples of randomly generated numbers, two different response rates were calculated and are shown in tables D and E. The total response rate for the telephone interview survey was about 80 percent. This rate is the ratio of the number of families having complete and partial interviews with at least one family member to the total eligible number of sample telephone numbers. Thus, it is a family level response rate. All sample working household numbers that were never answered are included in the base of the response rate. About 2 percent of the sample families provided only partially complete interviews, but with at least one complete person section of the questionnaire.

Table D. Number of families and proportion of all eligible families, by disposition category for the Survey Research Center Telephone Survey

Disposition category	*Number of families*	*Proportion of all eligible families*
Interviewed families	4,389	0.787
Partially completed families	85	0.015
Family refusals	807	0.145
Other noninterviews	297	0.053
Nonsample, nonworking number	2,114	...
Nonsample, other	1,043	...

Table C. Number and proportion of households in the National Health Interview Survey, by response category: Fourth quarter, 1979, and full year, 1979

Response category	*Fourth quarter, 1979*		*Total year, 1979*	
	Number of households	*Proportion of households*	*Number of households*	*Proportion of households*
Interviews	10,122	0.963	40,422	0.965
Refusals	200	0.019	816	0.019
Other noninterviews	184	0.018	656	0.016

Table E. Number of persons and proportion of total estimated eligible persons, by disposition category for the Survey Research Center Telephone Survey

Disposition category	*Number of persons*[1]	*Proportion of total estimated eligible persons*[1]
With interview data	8,210	0.795
Refusals[2]	1,579	0.153
Other noninterview[2]	532	0.052

[1]Households without complete enumerations were estimated to contain, on the average, 1.86 eligible persons.
[2]Estimated.

Table F. Estimated percent response, by respondent rule and selected demographic characteristics for the Survey Research Center Telephone Survey

Demographic characteristic	*Knowledgeable respondent*	*Random respondent*
	Percent	
Total	81	75
Sex		
Male	83	72
Female	81	79
Age		
17–24 years	82	61
25–44 years	88	86
45–64 years	81	81
65–74 years	66	57
Race		
White	81	77
All other	82	69
Education		
0–11 years	74	58
12 years	80	72
13 years or more	91	96
Marital status		
Married	81	79
Widowed	76	58
Divorced	89	97
Separated	80	86
Single	83	65
Usual activity		
Working	83	80
Keeping house	83	85
Other	77	47

As expected, the response rates for various experimental groups within the telephone sample varied somewhat. The respondent rule, which used a knowledgeable adult informant, achieved a higher family response rate than the random respondent rule did (81 versus 75 percent). This difference illustrates one benefit of the knowledgeable adult respondent rule, and stems from the avoidance in following the rule of respondents who are rarely at home or who tend to refuse the interview request. Similar differences in response rates were found between the experimental version of the questionnaire and the standard version of the questionnaire (79 versus 82 percent). These differences are statistically significant at traditional levels ($p = 0.05$) and probably arise from differences in the interviewers' reactions to the different procedures required for each experimental group.

A person-level response rate differs from the family-level rate only because of cases where data were not obtained on all eligible persons in the household. The person-level response rate for the telephone survey is 79.5 percent. The response rate for the telephone survey is higher than that obtained by most telephone surveys conducted by SRC. The higher than usual response rate may be attributable to a variety of characteristics of the project: To the legitimacy of the Public Health Service as a health survey sponsor, to the topic of health events, to lengthy training of the interviewers, to continual monitoring, and to high morale of the staff.

To speculate on the nature of nonresponse bias in the two data sets, it is useful to estimate response rates for various subgroups of the sample. This estimation generally cannot be made because relative sizes of different demographic groups within the telephone household population are generally not known. In this case, however, with the assumption of no *nonresponse* bias in the NHIS data and no *response* bias in the NHIS or SRC data for variables identifying demographic subgroups, a response rate for subgroups within the telephone sample can be estimated from the expression $P_{i,\mathrm{SRC}}R_{\mathrm{SRC}}/P_{i,\mathrm{NHIS}}$ where $P_{i,\mathrm{SRC}}$ is the proportion of respondents in category i of the SRC sample, $P_{i,\mathrm{NHIS}}$ is the proportion of respondents in category i of the NHIS sample, and R_{SRC} is the overall person-level response rate for the SRC sample.

Table F shows the estimated response rates for various demographic subgroups separately for the knowledgeable respondent and the random respondent rule. For the knowledgeable respondent rule, table F demonstrates the low response rates among the elderly (66 versus 81 percent overall) and among the poorly educated (74 versus 81 percent overall). There are no apparent differences by sex or race.

The response rates for the random respondent group were generally lower for all subgroups, reflecting the greater difficulty of gaining cooperation in this rule. There are some subgroup differences between the two respondent rules. For example, both the younger and the older respondent groups had low estimated response rates in the random respondent rule (61 percent for the age group 17–24 years and 57 percent for the age group 65 years and over). Also, single persons and widows appeared to have relatively low response rates in the random respondent rule. Females tended to have a higher response rate than males (79 versus 72 percent) and white respondents tended to have a higher response rate than all other respondents (77 versus 69 percent). That the older respondent group tends to have lower response rates than other groups to telephone surveys has been found previously.[18] This should cause some concern for researchers interested in health variables because of the ubiquitous correlation of age and health status.

Given these estimates of nonresponse among subgroups, one must be careful to factor out any effects of different demographic distributions in comparisons between NHIS and SRC Telephone Survey data.

Response differences between the telephone data and NHIS data

The analysis of these next sections concentrates on several measures that are standard dependent variables in NHIS analysis. Table G shows four categories of statistics. First, because reports of any particular health condition are generally given for only a small proportion of the population, table G presents the percent of respondents who reported at least one event in that category. Two such sets of percents were reported—those for items that asked about the last 2 weeks and those that asked about the last 12 months. The third type of statistic was the percent of persons in the modal category of variables with response distributions that were more dispersed than those in the first two categories. The last class of statistics contains averages (means) for some of the variables that were counts of events.

Each of the statistics are presented for the total telephone sample, the total NHIS sample, and the telephone households in the NHIS sample. Four different experimental groups result from the cross-classification of the two forms of interviewing behavior, control and experimental, and the two respondent rules, random respondent and knowledgeable respondent. Because the choice of one respondent per family leads to unequal selection probabilities for the random respondent, the self-reports of those respondents are weighted by the reciprocal of the sampling probabilities. For the knowledgeable respondent rule, the reports for all eligible persons in the household are presented. The NHIS statistics are based on all adults within the sample families.

The purpose in presenting separate statistics on several of the experimental groups in table G is to check on the hypothesis that the nature of the differences between the NHIS results and the telephone survey depends on the experimental group of the telephone survey that is examined. The nature of interaction effects related to the experimental groups is investigated in detail in chapter III. Here they are merely examined for evidence that the conclusions drawn from the comparison of the telephone and NHIS surveys would be greatly different depending on which experimental telephone survey group was used. The results presented in table F suggested that this was not the case.

Columns 1 and 9 of table G show the comparison between the pooled telephone sample and the NHIS. The vast majority of measures indicate more health reporting for telephone re-

Table G. Percent and number of persons in selected response categories for experimental groups of the Survey Research Center (SRC) Telephone Survey and for the National Health Interview Survey (NHIS)

Characteristic	Total SRC sample	Random respondent self-reports[1]			Knowledgeable respondent families[2]			Total NHIS sample	NHIS telephone households
		Total	Interview form		Total	Interview form			
			Control	Experimental		Control	Experimental		
	Percent with 1 or more in past 2 weeks								
Bed days	[3]8.7	5.8	[3]4.9	6.9	9.3	7.8	10.9	7.8	7.7
Work loss days	[3]7.6	6.1	5.1	[3]7.2	8.0	[3]6.7	[3]9.4	4.5	4.5
Cut-down days	[3]9.8	8.7	[3]7.6	[3]9.8	10.4	8.8	[3]12.0	7.0	7.1
Dentist visits	[3]7.1	6.3	6.1	6.6	7.1	6.7	[3]7.4	5.2	5.3
Doctor visits	[3]17.5	16.1	16.0	[3]16.3	17.8	[3]17.8	[3]17.8	13.5	13.6
Acute conditions	16.3	14.1	13.5	14.9	16.4	15.0	18.1	---	---
	Percent with 1 or more in past 12 months								
Doctor visits	73.5	75.9	74.1	78.1	72.9	72.3	73.4	73.3	73.5
Hospital episodes	13.0	12.7	13.7	[3]11.7	13.1	13.9	12.6	12.5	13.3
	Percent with 1 or more								
Chronic conditions	32.3	34.4	31.1	38.0	31.8	29.2	34.5	---	---
Limitation of activity	[3]23.9	24.9	21.5	[3]28.5	23.3	20.3	[3]26.5	18.9	18.7
	Percent in modal category								
No bed days in past 12 months	[3]46.0	46.4	[3]48.0	[3]44.6	45.3	[3]47.6	[3]42.9	53.7	53.9
2 weeks to 6 months since last doctor visit	[3]39.2	41.1	41.1	41.1	39.4	[3]38.8	[3]39.9	43.5	43.5
2 weeks to 6 months since last dentist visit	[3]33.7	32.2	31.4	33.1	34.6	[3]35.3	[3]33.8	30.3	31.3
Excellent subjective health status	[3]41.5	41.6	42.4	40.8	34.6	[3]35.3	[3]33.8	43.3	44.0
	Number per 100 persons per quarter								
Bed days	189.8	117.7	110.5	126.1	191.8	161.9	223.0	216.5	208.0
Work loss days	192.4	143.0	101.4	187.9	197.6	167.1	228.8	111.2	111.5
Dentist visits	59.2	52.7	51.4	54.6	57.2	55.8	59.2	41.0	40.9
Doctor visits	166.4	149.5	146.9	152.8	170.3	172.3	167.7	126.8	124.8
Acute conditions	119.0	106.0	104.7	107.3	122.9	113.8	132.0	75.4	68.3

[1]1 person per family weighted by (number of eligible persons in the family)/(number of telephone numbers for family).
[2]Total family reports weighted by 1/(number of telephone numbers for family).
[3]Statistically significant difference between SRC and NHIS estimates.

spondents than for NHIS respondents in telephone households. For example, about 14 percent of the NHIS respondents reported at least one doctor visit in the last 2 weeks, but about 18 percent of the telephone survey respondents reported a visit. Only reports of hospitalizations in the past 12 months were lower on the telephone than in the NHIS sample. Almost all of the variables for which means or rates per 100 people per quarter are presented demonstrate higher reporting among telephone respondents than among NHIS respondents. Thus, there is some consistency between the differences on means and those on percents with at least one health event reported. Further, this consistency argues against the possibility that the increase in telephone reporting is merely a change from reporting no eligible health events on a question to reporting one.

Although empirical estimates of differences between NHIS and the telephone survey data are informative, they do not indicate which estimates are closer to the true values of the target population. That is, they do not provide estimates of net bias in the statistics and thus do not indicate which procedure is more accurate. Most studies of response error for measures like those of NHIS find net underreporting of health events to be the most common bias. For example, Madow[21] found net underreporting of embarrassing chronic conditions in a study using health records for validation. There are two psychological influences that support the hypothesis of underreporting. Across a wide range of substantive topics, researchers have found bias in questions with a particular response being socially desirable. Respondents tend to avoid socially undesirable responses.[22,23] To the extent that respondents feel that reporting of no illnesses is socially desirable, there may be a bias to underreporting of health events. In addition to the hypothesis of social desirability, past work has suggested that respondents need some assistance for accurate retrieval of information from their memory. As the task becomes more difficult by extension of the reference period or more complex in terms of the amount of information, underreporting tends to increase. Similarly, events that are important and salient are more easily recalled. The failure of human recall of health events is another possible reason for underreporting.

However, there are arguments in opposition to the hypothesis that more is better. Some of these note that the greater rapport between interviewer and respondent that often accompanies procedures yielding higher reporting may actually produce overreports. In this study, overreports might result from assignment of conditions suffered by one person to others in a family through error of the family informant. Overreports might also result from an informant reporting events that occurred before the reference period of 2 weeks or 1 year, depending on the question. The data from this project do not offer a way to refute these alternative hypotheses.

A reasonable conclusion from past work is that, although overreporting might exist among some subgroups of the population or for some topics, the weight of the evidence is that there is a net underreporting of health events. Following this reasoning, the results shown in table G suggest more accurate reporting of health events in the telephone survey. Without further measures of the validity of reports, however, this conclusion must be viewed to be only an interpretation of the mode differences.

The finding of consistently greater reporting of events among the telephone respondents is unusual among such mode comparisons. Most studies show negligible differences between modes. The few differences that *have* been observed between modes have favored the personal interview mode. Two examples are (1) the finding that telephone respondents seem to shorten their answers to open questions more often than personal interview respondents[24] and (2) the result of the maximum telephone and maximum personal interview experiment of the National Crime Survey that showed lower reporting of victimizations on telephone interviews.[25]

Variation in mode effects across demographic subgroups

Given the unexpected finding of greater reporting of health events by telephone respondents, it is of some interest to attempt to locate subgroups of the population for which the overall result does not apply. This is especially relevant because the telephone data are subject to greater nonresponse error than the personal interview data. Thus, the differences between the two modes may be produced through the influence of nonresponse bias rather than differential response errors. This can be studied by examining those variables that were associated with differential response rates in the telephone survey—age and education. Because larger nonresponse rates were observed among the elderly and the poorly educated, larger differences between the modes for those groups than for their complementary groups might be expected. In addition, the sample was split by gender groups, following the results of past response error studies that have found lower reporting accuracy for health events for males.[26]

Tables H, J, and K show the percent of sample persons falling in specified categories of six health variables separately for different demographic subgroups. Most of the percents measure the relative number of people who report having at least one health event of the given type. Given the past methodological work, one would assume that the mode that has the smallest percent "none" would suffer from relatively less response error. Table H shows the data for males and females separately. For both males and females, the telephone survey produces relatively more reports of health events. Table J shows that the same is true for all age groups in the sample over all variables examined. Table K shows the same result for all educational groups. Thus, there was no success in finding subgroups of the population with reduced reporting of health events on the telephone survey. Therefore, it appears that the tendency for increased reporting in this telephone survey is a result unaffected by differential nonresponse problems in age and educational groups. Many of the differences between modes within demographic groups are not significant statistically. The results of the other experimental groups in the telephone survey for the most part exhibit the same tendencies, as shown in tables H, J, and K.

Searching for interactions in mode effects

Tables H, J, and K also permit an investigation of whether the *magnitude* of the increased reporting on the telephone itself varies by the various demographic subgroups. In fact, to model

Table H. Percent of persons with selected health characteristics, by sex for the Survey Research Center (SRC) Telephone Survey and the National Health Interview Survey (NHIS)

Characteristic	Male		Female	
	NHIS survey	SRC Telephone Survey	NHIS survey	SRC Telephone Survey
	Percent			
At least 1 work loss day in past 2 weeks	5.1	[1]8.1	4.2	[1]7.2
At least 1 cut-down day in past 2 weeks	7.1	[1]8.7	7.9	[1]10.7
At least 1 doctor visit in past 2 weeks	11.3	[1]13.6	15.4	[1]17.9
At least 1 dentist visit in past 2 weeks	5.4	[1]6.9	5.3	[1]7.4
At least 1 bed day in past 12 months	42.3	[1]50.3	49.4	[1]55.6
Excellent subjective health status	52.3	[1]55.8	59.1	59.5
	Number			
Approximate *N*	8,400	3,800	10,000	4,400

[1]Telephone percent different from NHIS percent at 0.05 level of significance.

NOTE: *N* = number of persons.

Table J. Percent of persons with selected health characteristics, by age for the Survey Research Center (SRC) Telephone Survey and the National Health Interview Survey (NHIS)

Characteristic	17–24 years		25–44 years		45–64 years		65 years and over	
	NHIS	SRC	NHIS	SRC	NHIS	SRC	NHIS	SRC
	Percent							
At least 1 work loss day in past 2 weeks	6.0	9.2	5.4	[1]8.5	4.4	[1]7.2	1.0	3.1
At least 1 cut-down day in past 2 weeks	5.8	8.8	6.4	[1]10.3	7.8	9.4	9.0	10.7
At least 1 doctor visit in past 2 weeks	11.8	[1]15.0	12.0	13.3	14.1	[1]17.3	17.3	[1]28.2
At least 1 dentist visit in past 2 weeks	5.5	7.0	5.3	[1]7.3	6.0	[1]8.1	4.0	4.3
At least 1 bed day in past 12 months	50.9	[1]62.4	51.3	[1]59.3	40.4	[1]45.6	36.6	37.1
Excellent subjective health status	51.2	51.8	51.6	[1]47.0	37.6	[1]34.9	30.3	29.1
	Number							
Approximate *N*	3,400	1,500	6,900	3,200	5,200	2,300	2,800	1,000

[1]Telephone percent different from NHIS percent at 0.05 level of significance.

NOTE: *N* = number of persons.

Table K. Percent of persons with selected health characteristics, by education for the Survey Research Center (SRC) Telephone Survey and the National Health Interview Survey (NHIS)

Characteristic	Education					
	0–11 years		12 years		13 years or more	
	NHIS	SRC	NHIS	SRC	NHIS	SRC
	Percent					
At least 1 work loss day in past 2 weeks	3.7	[1]8.1	4.8	[1]7.6	5.1	[1]7.2
At least 1 cut-down day in past 2 weeks	8.1	[1]10.7	6.3	[1]8.7	6.9	[1]10.2
At least 1 doctor visit in past 2 weeks	14.8	[1]20.5	13.4	[1]14.9	14.3	13.3
At least 1 dentist visit in past 2 weeks	3.6	[1]5.4	5.0	[1]7.5	7.4	[1]8.2
At least 1 bed day in past 12 months	41.3	[1]45.9	44.3	[1]53.6	51.3	[1]58.7
Excellent subjective health status	30.1	[1]29.1	45.4	[1]41.3	57.2	[1]53.6
	Number					
Approximate *N*	5,400	2,200	6,900	3,000	5,700	2,900

[1]Telephone percent different from NHIS percent at 0.05 level of significance.

NOTE: *N* = number of persons.

the full response distributions of the health variables, the percent distribution was expanded to include all other response categories on the six dependent variables, and log linear models[27] were fit to the three-way tables, for example, number of work-loss days in the past 2 weeks by mode of data collection by sex of sample person. This method, which models the logarithms of cell frequencies or proportions in the three-way tables, is a way of measuring the impact of several variables simultaneously on responses to each of the six dependent variables. This approach permits an examination of various interaction effects, including a three-way interaction of the health measure, the mode of data collection, and the demographic subgroup variable, to determine whether the effects of mode differ across the subgroups, for example, whether the telephone and personal survey differences are larger for females than for males.

Because of the large sample sizes involved in this analysis, very small differences in mode effects across the demographic subgroups statistically can be shown to be significantly different from zero. Of the 18 different multivariate models estimated, (6 health variables for each of 3 demographic predictors), 15 of the models show statistically significant three-way interaction terms; that is, in almost all cases, mode effects vary by demographic subgroup to a degree beyond that expected by sampling error alone.

However, the substantive importance of interaction effects depends on their interpretability and the overall fit of models that exclude them (that is, assume no such interaction effects exist). Taking that view, a very different picture emerges (see table L). For the six health variables examined, the three-way tables including sex, mode, and the health variable tend to be well described by models that contain no three-way interaction terms. That is, for the most part, men and women tend to exhibit the same differences between modes. For example, the three-way table with work-loss days, sex, and mode of interview has a good fit ($\chi^2 = 2.03$, $0.7 < p < 0.8$) for the model with all two-way interactions (specifying constant relative differences between modes for both sexes). On the basis of these six variables, one would conclude that there are no important differences between the sexes in the tendency to report more health events on the telephone survey. This provides some statistical support for similar observations more informally taken from tables H, J, and K.

The second column in table L presents the fit statistics for models that hypothesize no variation in mode effects by age. In contrast to the first column, it can be seen that using a model that hypothesizes equal mode differences for all age groups leads to rather consistently poor model fits. There is support for the argument that personal and telephone interview differences vary over age groups. For example, the model specifying equal relative mode differences for all age groups on 12-month bed days has a very poor fit ($\chi^2 = 35.00, p = 0.001$). Only the table with dental visits as the dependent variable is well described by a model that specifies no differences across the age groups in their mode effects. When the three-way interaction terms are examined, however, no clear pattern emerges. It is not uniformly the case that elderly persons tend to report more health events and that younger persons tend to report less in the NHIS survey than in the telephone survey. Rather each

Table L. Likelihood ratio chi-square statistics for models of 6 health measures, by 3 demographic variables

Variables[1] in the model	*Goodness-of-fit of model[2]*		
	Chi square	*Probability*	*Degrees of freedom*
Sex			
Work loss days	2.03	$0.7 < p < 0.8$	4
Cut-down days	1.27	$0.8 < p < 0.9$	4
Doctor visits	1.04	$0.95 < p < 0.98$	5
Dentist visits	8.34	0.054	3
12-month bed days	3.26	$0.5 < p < 0.7$	4
Health status	7.83	0.049	3
Age			
Work loss days	11.88	0.455	12
Cut-down days	15.53	0.214	12
Doctor visits	16.31	0.361	15
Dentist visits	3.86	$0.9 < p < 0.95$	9
12-month bed days	35.00	0.011	12
Health status	23.98	0.005	9
Education			
Work loss days	19.47	0.012	8
Cut-down days	5.33	$0.7 < p < 0.8$	8
Doctor visits	36.06	0.000	8
Dentist visits	9.08	0.059	4
12-month bed days	18.34	0.019	8
Health status	7.75	0.257	6

[1]Log linear models fit using ECTA. 4 age categories were used: 17–24, 25–44, 45–64, and 65 years and over. 3 education categories were used: 0–11 years, 12 years, and 13 years or more.
[2]With all 2-way iterations but no 3-way iterations.

variable seems to exhibit different patterns. That is, despite their influence on the goodness of fit of the models there is no parsimonious interpretation of the age differences in mode effects.

The tables with education as the control variable exhibit results similar to those containing the age variable. In general, a three-way interaction term reflecting differences across the education groups in their sensitivity to mode of interview is required. For example, the table containing doctor visits, education, and mode has a very poor fit for the model with all two-way interactions ($\chi^2 = 36.06, p < 0.0001$). Like the results for the age variable, however, the pattern of the three-way interaction terms in the saturated model is not consistent over variables and cannot be easily summarized for any one health variable. Although there are differences across the education groups, they do not appear to be interpretable.

The reader will recall that there appear to be nonresponse differences across age and education groups in the telephone survey; for example, the elderly are disproportionately nonrespondents in the telephone mode. For that reason, it cannot be determined from the model-fitting results whether the need for three-way interaction terms stems from nonresponse differences or response differences. These results demand replication over different measures and data sets, but may have important implications for the use of telephone surveys among elderly and poorly educated groups.

To summarize the multivariate models using mode and a demographic variable as predictors of the health variables, the higher levels of reporting among telephone respondents found

in the earlier tables appear to remain present within different sex, age, and education groups. This result reinforces our conclusions based on simpler analytic techniques.

Speculations on causes of the differences between modes

It has been frequently noted by survey researchers that the attitudes and morale of the interviewers are likely to influence their performance and the quality of the data. In this study, data from two different interviewing staffs in quite different situations are compared. For the U.S. Bureau of the Census interviewers, the NHIS was simply regular assignments of the NHIS household interviews, with nothing special to motivate extra effort. The SRC interviewers were all new, enthusiastic about a new job, and interested in participating in a university research project. The latter characteristics can be expected to result in some increased diligence and effort to perform well.

In addition, the data collection procedures themselves vary to some extent by mode. Specifically, the apparent improved reporting of health events in the telephone survey may be for the following reasons:

1. The rigorous training of interviewers preceding the survey.
2. The close contact between the principal investigators and the interviewing staff.
3. The greater specification of the interviewing task through adjustment of the questionnaire to include specific instructions about interviewer feedback and probing.
4. Continual monitoring of interviews in progress, with feedback given to interviewers weekly on their performance.
5. Supervisory review of all cases after completion.
6. The sole attention of the interviewer given to a single respondent in the household instead of group interviews as in NHIS.
7. Certain differences in the nature of proxy reporting in the two modes. For example, the NHIS interview asks the same question or set of questions about every family member in sequence, one by one, until the whole questionnaire is complete. The telephone interview asks the major sections of the questionnaire for each family member singly.

As with all experimental surveys using complex designs, replication of the findings of this comparison is needed before the extent of their generality can be known. As with most studies comparing modes of data collection, it is unable to measure a pure effect of mode unconfounded by differences in interviewers, questionnaire form, response rates, and so forth. It seems clear, however, from this study and others that the magnitude of differences between the two modes is within the range of effective manipulation by careful survey design.

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Chapter III
Experimental interviewing techniques

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Introduction

This chapter focuses on the effects of experimental questionnaire design and interviewing techniques in the telephone interviews. Each phone number in the sample was assigned randomly to an experimental or control group; approximately 4,000 interviews were obtained for each group.

The experimental techniques were developed over the past several years for use in face-to-face interviews and recently have been adapted to telephone interviews.[1] The techniques are designed to achieve two objectives: first, to reduce response error by using techniques to inform and motivate good response behavior; and, second, to reduce variability among interviewers by standardizing more of the interviewer's behavior.

The comparison of interviewing techniques for the telephone sample was motivated by the idea that interviewing procedures are one likely cause of differences between telephone and face-to-face interviews. Manipulation of interviewing procedures in the telephone study allowed for measurement of the contribution of this factor to overall mode differences.

This analysis examines the effect of these different interviewing techniques on response distributions. One of the difficulties with comparisons between telephone and personal interview surveys is that the style of interviewing may vary across the modes and confound interpretations of differences between them. This study was designed to provide an independent reading on interviewing effects by experimentally manipulating two interviewing treatments in the telephone survey.

Description of experimental treatments

The telephone sample was randomly assigned to one of two interviewing treatments. The first, called the control treatment, featured techniques designed to be similar to U.S. Bureau of the Census procedures. It was based on observations of U.S. Bureau of the Census interviewer training sessions and an analysis of tapes of mock National Health Interview Survey (NHIS) interviews taken by U.S. Bureau of the Census interviewers. The intent in designing this treatment was to standardize interviewer behavior as much as possible across the telephone and personal modes so that differences between the Survey Research Center (SRC) and U.S. Bureau of the Census interviewers' questioning style would not be confounded with the effects of the mode of communication in the comparison between telephone and personal interview surveys.

The control procedure restricted interviewer-to-respondent communication to asking questions exactly as presented in the questionnaire and to using a small set of probes and introductory statements at the interviewer's discretion. For comparison to this procedure, the other half of the sample was interviewed using the experimental techniques: commitment, instructions, and feedback. These techniques were designed into the questionnaire, and the interviewer had only very limited freedom to use additional techniques.

Commitment

It is important that respondents understand that the interview is a serious undertaking, that the information is valuable, and that some effort will be needed to perform response tasks adequately. If respondents are properly motivated, they will be less likely to treat the interview lightly or to rush through it. More careful thought is likely to produce better reporting. One technique used in earlier research in personal interviews to help motivate respondents is commitment.

The concept of commitment has received considerable attention in social psychology and sociology. Within sociology, commitment has been used to account for the fact that people can persist in consistent goal-related activity even in the face of adverse experiences that could be expected to deter them from further effort.[2–4] Within social psychology, commitment has become a key concept in theoretical positions growing out of dissonance theory.[5–7]

In personal interviews, commitment is operationalized by having the respondents sign a statement that said they promised to devote the effort and work needed to give accurate and complete information. In telephone interviews, as in this study, the commitment statement is read to respondents and they are asked to indicate verbal agreement. If the respondent is unwilling to commit himself or herself, the interview is terminated. In practice, over the telephone, almost no respondents refused to agree. The commitment statement used in this study was the following:

> This research is authorized by the Public Health Service Act. It's important for the Public Health Service to get exact details on every question, even on those which may seem unimportant to you. This may take extra effort. Are you willing to think carefully about each question in order to give accurate information?

If the respondent agreed, the following statement was read:

> For our part, we will keep all information you give confidential. Of course, the interview is voluntary. Should we come to any question which you do not want to answer, just let me know and we'll move on to the next one.

Instructions

In addition to commitment as a motivating technique, an attempt was made to orient respondents and generate role expectations by instructions on the purposes and goals of questions, and on how to go about answering them. Respondents typically pick up such cues only incidentally through interaction with the interviewer. Attempting to teach respondents what is expected of them through such casual methods is frequently ineffective. In this study, an attempt to communicate desirable behavior was made by incorporating instructions as a part of the question itself. By using standard instructions that are more detailed and frequent than in most surveys, it was hoped that the distinction between purposeful and incidental learning behavior[8,9] would be achieved.

Researchers concerned with task performance have identified two main functions of instructions: first, to clarify the goal toward which the performance is directed;[10,11] and, second, to clarify specific tasks required to achieve the goal. In the interview, this first type clarifies the goal of the interview by informing the respondent of what is expected of him or her—to give accurate and complete answers to all questions. In this study, these general goals were articulated by including performance instructions preceding the questions as well as in the commitment statement. The second type of instruction details how the respondent should go about producing accurate answers on individual questions and the level of accuracy that is required. Two examples of specific question instructions follow:

> This is sometimes hard to remember, so please take your time.
>
> For this question, we'd like to get as exact a number as possible.

Feedback

The instructions procedure is designed to clarify general and specific goals of the interview and also to motivate better performance. Instructions may not be effective, however, without communication to respondents on how well they are performing the task. Thus, the third experimental technique used is feedback.

The idea of programming feedback in interviews was developed from an analysis of personal interview interactions. This research demonstrated that much of the interaction that takes place in face-to-face and telephone surveys is not limited simply to the asking and answering of questions but includes other activities, the most frequent being interviewer feedback to respondents' behavior.[12,13] The findings led to a focus on the two-way process or on chaining of behaviors between interviewer and respondent, rather than on the separate activity of each.

In this view of the communication, the way that interviewers react to respondents' earlier answers is an important determinant of their behavior in later questions. Interviewers' reactions constitute a feedback to respondents that can influence their behavior in general and the accuracy and completeness of the reported information in particular. Like commitment and instructions, feedback reactions can be both informative and motivational in quality. They tell respondents when they have fulfilled task requirements, and they serve as reinforcers capable of shaping subsequent behavior.

Following previous practice, feedback statements were designed into the questionnaire in the experimental interviewing treatment. In general, feedback statements were made contingent on good performance, and both negative and positive feedback statements were used. For example, interviewers estimated the length of time that the respondents took to think over answers to some of the questions that required respondents to search their memories. Respondents who took less than about 3 seconds before replying negatively to a question that asked if they had cut down on usual activities in the recent past because of illness or injury were read the following:

> You answered that quickly. Are there any days you might have overlooked?

Positive feedbacks, on the other hand, were used to indicate to the respondent that the answer given fulfilled the goals of the question. Examples of positive feedbacks include the following:

> I see.
>
> This is the kind of exact answer we need.
>
> That's useful information.
>
> Thank you. This is helpful.

Commitment, instructions, and feedback, in summary, are three procedures that have been used in several studies in an effort to improve reporting. The techniques become part of a script that interviewers are trained to use in a standardized manner, thus reducing between-interviewer variability in the use of techniques and communicating more productively with respondents.

These techniques, singly and in combination, have been shown to improve reporting in face-to-face and telephone interview surveys on health and mass media use.[1,14,15] It was anticipated that using the procedures in this study would improve reporting on health variables.

Overall effects of experimental interviewing techniques

Table M shows the overall effects of the experimental techniques. As is characteristic of the health variables, only a small proportion of the population reported affirmatively to questions asking for incidents of illness and health care utilization during the past 2 weeks. Larger numbers of respondents reported health events and experiences for the previous year. Table M shows the percent of the sample for which one or more illnesses or health behaviors were reported, or, for variables that are not counts of health events, the percent in the modal category.

Nearly all of the health events—bed days, work-loss days, doctor visits for the past 2 weeks and for 12 months, and so forth—were reported more frequently by the experimental group. The majority of the differences are significant at the 5-percent level. Nonsignificant differences were found for reporting of doctor and dental visits within the past 2 weeks, and for ratings of subjective health status. In addition to more health events and behaviors, the experimental group reported a higher level of limitation of activities, largely nonmajor.

Table M. Percent of persons in selected response categories, by experimental interview treatment

Response category	Experimental interview treatment	
	Control form	Experimental form
	Percent with 1 or more in past 2 weeks	
Bed days	7.3	[1]10.0
Work loss days	6.3	[1]8.8
Cut-down days	8.4	[1]11.5
Dentist visits	6.8	7.4
Doctor visits	17.4	17.5
Acute conditions	14.9	[1]17.7
	Percent with 1 or more in past 12 months	
Doctor visits	72.6	74.5
Hospital episodes	13.4	[1]12.5
	Percent with 1 or more	
Chronic conditions	29.2	[1]35.8
Limitation of activity	20.4	[1]27.6
	Percent in modal category	
No bed days in past 12 months	48.1	[1]43.6
2 weeks to 6 months since last dentist visit	37.3	37.1
2 weeks to 6 months since last doctor visit	39.2	39.2
Excellent subjective health status	42.1	41.3
	Number	
Approximate *N*	4,217	3,993

[1]Difference between control and experimental forms significant at $p < 0.05$.

NOTE: *N* = number of persons.

Acute and chronic conditions were also reported more frequently in the experimental group. These findings suggest that the experimental techniques may have motivated respondents to be more diligent in recalling information or increased their willingness to report conditions. The techniques may also have sensitized respondents to health, making it more salient and enhancing the respondents' tendencies to perceive themselves as having more health problems, or it may be that the techniques make it easier to *admit* to poor health.

During the coding process, each reported condition was rated on two scales: one for *seriousness* and the other for the potential *embarrassment* it might cause to report it. Three-point scales were used. Seriousness was defined as, "Conditions which are disabling/crippling, fatal, especially painful, or a condition which significantly restricts normal activities over a prolonged period." Embarrassment or social threat was defined as, "Any disease or condition of the male or female sex organs (including D and C, abortion, hysterectomy, or prostate); venereal disease; cancer, any site or type; mental or emotional disorders (including retardation and senility); conditions involving loss of limbs, paralysis, or deformities; diseases or conditions involving the brain or skull; conditions or diseases of the urinary tract, bladder, or kidney; hernias; hemorrhoids; visible sores or lesions, rashes." Of the total conditions reported, approximately 33 percent in both the experimental and control groups were classified as serious; and 17 and 18 percent, respectively, were rated as embarrassing. The increased reporting in the experimental group was not accounted for simply by increased reporting of less serious or embarrassing conditions, but appeared to reflect an overall increase in condition reporting.

Demographic differences within experimental treatments

The next question to be raised is whether the increase in reporting health events in the experimental treatment was shared by all segments of the sample or whether some particular groups were more strongly affected than others. For example, men and women might react differently to the techniques, as might respondents of different ages or educational attainment. The following analyses examine reporting for experimental and control procedures for respondents with different demographic characteristics.

The first subgroup analysis (table N) examines the experimental procedures by the sex of the family reporter. Respondents in this study were selected at random from household members in half of the interviews and the other half were adults who answered the telephone and expressed an ability to answer

Table N. Percent of persons in selected response categories, by experimental interview treatment and sex of reporter

Response category	Control form		Experimental form	
	Male reporter	Female reporter	Male reporter	Female reporter
	Percent with 1 or more in past 2 weeks			
Bed days[1]	6.6	8.1	8.8	10.9
Work loss days	6.9	6.1	9.1	8.6
Cut-down days	6.8	9.2	10.3	12.5
Dentist visits	6.8	9.2	10.3	12.5
Doctor visits	14.9	19.0	16.2	18.1
Acute conditions	12.9	15.9	16.6	18.2
	Percent with 1 or more in past 12 months			
Doctor visits	70.8	73.4	74.2	74.8
Hospital episodes	11.0	14.9	10.7	13.0
	Percent with 1 or more			
Chronic conditions	25.7	31.4	31.9	37.6
Limitation of activity	18.3	22.0	27.4	27.4
	Percent in modal category			
No bed days in past 12 months	50.2	48.1	45.5	43.0
2 weeks to 6 months since last dentist visit	36.4	37.8	38.6	36.1
2 weeks to 6 months since last doctor visit	47.6	50.0	48.7	50.2
Excellent subjective health status	42.0	42.1	41.7	41.1
	Number			
Approximate *N*	1,968	2,243	1,864	2,120

[1]Significant interaction between treatment and sex ($p < 0.05$).

NOTE: *N* = number of persons.

health questions for the family. In both halves of the sample, the respondent reported for himself or herself and for all other adult family members.

In both experimental and control groups, 53 percent of the reporters were female. The female respondents fairly consistently reported more health events in both experimental and control groups. This pattern depends on two factors: the characteristics of the persons doing the reporting *and* the characteristics of those being reported *for*. As table N shows, reporters of *both* sexes reported more events in the experimental group.

Respondents of various ages might react differently to the experimental treatments. The data in table O show results similar to those in the previous table. With a couple of exceptions, the experimental techniques produced higher reporting of health events across all age groups. The same general pattern is seen in table P, which examines effects of the experimental techniques by education of the reporter.

To examine more carefully the effects of the interviewing treatments by demographic subgroups, six of the health variables were selected for multivariate log linear model analysis to examine to what extent the interviewing treatments interacted with demographic characteristics of the reporters.

The analysis revealed that only a few of the interactions approached significance. For example, the interaction between interviewing techniques and respondent gender approached significance only for 2-week dental visits. The techniques and respondent education interaction approached significance for cut-down days and dentist visits, with higher educated respondents reporting more such events. No significant interaction was observed with respondent age. In general, the effect of the interviewing techniques is not specified by these demographic characteristics of the reporter.

Interpreting the experimental effects

One other issue remains for consideration. Earlier it was mentioned that for the experimental interviewing techniques to be considered improvements in data collection methods, assumptions have to be made about the direction of the reporting errors for the health variables. The predominant assumption among researchers in the field, as noted, is that health events are underreported.[16–18] If one believes this assumption, the object of data collection techniques should be to increase reporting on health measures. As shown in previous tables, the experimental interviewing procedures do tend to produce higher reports of illness and health care utilization than control procedures do. Therefore, one might suspect that the experimental techniques produce *better* reporting than the control procedure that was modeled on the current NHIS techniques.

There is not, however, unequivocal acceptance of the underreporting hypothesis. Marquis,[19] in particular, has raised some cogent arguments about the evidence on which this hypothesis is based. In analyzing hospitalization record check studies, Marquis points out that the finding of underreporting of hos-

Table O. Percent of persons in selected response categories, by experimental interview treatment and age of reporter

Response category	Control form				Experimental form			
	Age of reporter				Age of reporter			
	17–24 years	25–44 years	45–64 years	65 years and over	17–24 years	25–44 years	45–64 years	65 years and over
	Percent with 1 or more in past 2 weeks							
Bed days	8.4	8.3	6.7	6.3	11.4	10.4	10.3	6.8
Work loss days	7.9	7.3	6.4	1.6	11.2	10.0	8.3	2.4
Cut-down days	7.5	8.9	8.2	8.3	14.0	12.1	10.7	9.9
Dentist visits	7.9	7.9	6.3	5.6	7.2	7.5	8.4	5.6
Doctor visits[1]	17.3	16.1	17.6	22.5	17.7	15.5	18.1	22.2
Acute conditions	20.8	17.3	12.4	5.8	25.2	19.3	14.3	10.9
	Percent with 1 or more in past 12 months							
Doctor visits	74.1	74.8	69.8	69.2	81.5	74.9	71.0	76.2
Hospital episodes	12.2	13.2	12.7	18.3	12.2	11.7	10.5	17.6
	Percent with 1 or more							
Chronic conditions	18.4	22.2	36.3	49.0	28.4	28.4	41.7	54.7
Limitation of activity[1]	16.1	13.9	29.0	49.3	24.9	20.9	32.5	49.0
	Percent in modal category							
No bed days in past 12 months	34.5	42.8	56.7	66.5	28.4	37.7	51.2	67.1
2 weeks to 6 months since last dentist visit	40.2	39.5	37.7	25.1	37.8	40.3	36.0	27.5
2 weeks to 6 months since last doctor visit	48.7	47.8	48.1	57.5	52.7	48.1	48.2	57.3
Excellent subjective health status	44.7	46.4	39.5	31.5	40.7	48.7	35.7	33.2
	Number							
Approximate *N*	781	1,676	1,190	507	743	1,569	1,154	499

[1]Significant interaction between treatment and age ($p < 0.05$).

Note: *N* = number of persons.

Table P. Percent of persons in selected response categories, by experimental interview treatment and education of reporter

Response category	Control form			Experimental form		
	Education of reporter			Education of reporter		
	0–11 years	12 years	13 years or more	0–11 years	12 years	13 years or more
	Percent with 1 or more in past 2 weeks					
Bed days[1]	7.7	7.3	7.9	10.3	10.7	9.7
Work loss days	5.2	6.6	7.0	8.7	9.6	8.1
Cut-down days[1]	9.9	7.5	8.0	10.7	11.5	12.5
Dentist visits	4.1	8.0	7.5	6.4	7.3	8.4
Doctor visits	21.5	15.8	16.4	20.7	16.6	16.1
Acute conditions	12.2	14.7	16.8	15.4	18.4	18.6
	Percent with 1 or more in past 12 months					
Doctor visits	68.3	73.5	74.8	71.5	74.4	76.7
Hospital episodes	16.1	14.6	10.9	16.7	11.3	10.1
	Percent with 1 or more					
Chronic conditions	42.3	27.6	22.5	45.6	31.9	32.6
Limitation of activity[1]	37.4	19.3	17.6	38.1	24.5	25.3
	Percent in modal category					
No bed days in past 12 months[1]	58.4	49.0	42.2	56.5	43.0	37.0
2 weeks to 6 months since last dentist visit	24.3	38.2	44.9	23.1	36.6	45.9
2 weeks to 6 months since last doctor visit	47.8	48.7	50.1	49.6	47.5	51.6
Excellent subjective health status	29.2	41.4	51.2	26.5	40.1	52.9
	Number					
Approximate *N*	1,113	1,514	1,486	1,071	1,454	1,401

[1]Significant interaction between treatment and education ($p < 0.05$).

NOTE: *N* = number of persons.

pitalization episodes is common to *retrospective* record check studies—those that select respondents from hospitalization records and interview them to see if they report the events. Marquis notes that the only error that is discoverable in such studies is underreporting, because people who were known not to be in the hospital are never contacted. He suggests, therefore, that the underreporting uncovered in such record check studies might well be random error. If this argument is correct, techniques designed on the assumption of an underreporting bias in the measures may actually produce overreporting on the health variables.

Another argument that supports the possibility of overreporting involves the notion of forward telescoping. The health events mentioned in the analyses above often require respondents to report things that they experienced during particular time intervals prior to the interview. It is possible that those who received the experimental interviewing treatment tended to recall events as being experienced more recently than they actually were, which placed the events within the reference period in the questionnaire. For example, respondents in the experimental group might have reported more 2-week cut-down days because they were motivated to report *some* health experiences. Also, they might have reported things that had actually happened prior to the 2-week period as having occurred during the reference period. Such telescoping is a common finding.[20,21] It is also possible that backward telescoping is occurring instead of or in addition to forward telescoping. The data merely reflect the net effects of both types of response errors.

The possibility that the experimental interviewing treatments produced overreporting cannot be entirely ruled out. A previous study using the procedures, however, found that they tended to reduce both underreporting and overreporting. Miller and Cannell[15] report that the experimental procedures, administered to a sample of women in a study of mass media use, elicited *more* reports of television watching and X-rated movie attendance, and *fewer* reports of book reading. If one accepts the hypothesis that the former two behaviors are likely to be underreported and that the latter one is likely to be overstated, then there is some evidence that the interviewing procedures can reduce reporting biases in both directions.

There are some data from the present study that indirectly bear on this issue. At the beginning of the telephone interview, the interviewer suggested to respondents that they might find it easier to report health events if they had a calendar handy for reference. Approximately 75 percent of the 4,400 family respondents indicated they had a calendar ready for use. Because these individuals may have been less likely to telescope health events into the reference periods set up in the interview, an analysis was made of the relationship of calendar usage to health reporting, to demographic characteristics of the reporter, and to experimental interviewing treatments. If it is found that those saying they used calendars reported fewer health events, one would suspect that the hypothesis that more is better is not

tenable. Further, if there are substantial differences between experimental interviewing treatments in reported calendar usage, one would be obligated to see whether the experimental effects were explained or specified by this variable. There was no difference between experimental interviewing treatments in reported calendar use. Finally, for several selected health events, there were small or no differences in reporting between those who said they used a calendar and those who did not. The differences, however, tended to favor the more-is-better hypothesis, because those who reported using a calendar reported slightly more health events. Again, these analyses only suggest that the experimental interviewing treatment produced better reporting. A study with external validating records would be required for sorting out the interviewing treatment differences. The most tenable hypothesis is that the experimental techniques facilitated accurate reporting on health variables in this study.

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Chapter IV
The effects of respondent rules on health survey reports

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Introduction

The increasing cost of survey research has prompted a greater concern about the interplay of costs and sampling errors, nonresponse errors, and response errors. One method of decreasing sampling error with negligible increases in costs has been the use of household or family informants to report information both about themselves and all other members in the unit. Sampling variance of statistics based on persons can be decreased by the increase in the number of sample persons for whom data are collected. The clustering of these additional sample persons in the informant's household increases design effects, but the design effect losses generally do not outweigh the sampling variance gains. However, the decrease in sampling variance may be at the expense of an increase in response *errors,* because informants may not report as accurately for others as they do for themselves. The magnitude of response error differences between self-reporters and proxy reporters is probably related to the level of sharing of information among household members, the difficulty of recall of events, and psychological factors, for example, social desirability of certain responses, that may operate differently for self-respondents and proxy respondents.

This chapter examines the relationship between rules for selecting respondents within households on the one hand, and survey error on the other. It begins with a discussion of sampling error properties of different respondent rules. A review of the literature on response error for different respondent rules is also presented. The major portion of the chapter is devoted to presentations of findings from the telephone survey experiment designed to investigate various aspects of self-responses and proxy responses. The discussion that follows these presentations speculates on the nature of self-response and proxy response differences and their sensitivity to respondent rules.

Sampling variance implications of alternate respondent rules

The relative sampling variance of means for designs obtaining data on all persons in a household is approximately the following:

$$\frac{\sigma^2}{n}+\frac{\sigma^2}{n}\delta_L(N-1)[1+\delta_2(K-1)]$$

where σ^2 = element variance

n = total number of persons for whom data were collected

δ_L = intraclass correlation for households within a primary area

N = number of households per primary area

δ_2 = intraclass correlation for persons within a household

K = average number of persons per household

The collection of data on more than one individual in a household increases the design effect of the study by adding the quantity $1+\delta_2(K-1)$ to the design effect expression. The expression allows comparison of the variances of two alternative designs, the first selecting one respondent per household; the second, taking all eligible persons in the household. In many area probability designs, δ_L values of about 0.05 for many variables are not uncommon. In such cases, design effects for the single respondent per household sample would be about 1.450 in a sample where there were 10 households per primary area. If the intraclass correlation within the household were 0.2, the design effect would increase to about 1.540. These calculations assume an average of 2 persons per household and 10 persons per primary area.

If the number of sample persons in each design is kept constant, interviewing one respondent per household would produce a smaller design effect by eliminating the within-household intraclass correlation factor. If, however, the number of *households* is kept the same in both designs, the sample size in the survey with a single sample person per household will be approximately half of that for the design taking all adults per household because the average number of adults in households is about 1.9. In this comparison, the sampling variance for the design that takes all eligible persons in the household would be much lower than that of the design that selects only one adult. For a sample of 1,500 households and of 1,500 persons, the variance of the first design would be $\sigma^2(9.7\times10^{-4})$. The second design with 1,500 households and 3,000 persons would have a sampling variance for the mean of $\sigma^2(5.1\times10^{-4})$.

These comparisons make clear the advantage of the design selecting all persons in the household. The relative advantage of the design is likely to vary across different measures depending on the nature of clustering effects. Despite this variation, however, the costs per unit of sampling variance for the design selecting all in the household would generally be lower than for the design selecting a single respondent per household. For this reason, the *nonsampling* errors for the single-respondent design need to be smaller to make it attractive.

Response error differences in self-reporting and proxy reporting

There are a variety of reasons that self-reporters might be more accurate than reporters for others. First, persons reporting about someone else may not know about the event or characteristic in question. Second, even if an informant knows about an event which happened to another person, the informant may not recall it because it is not personally salient. Third, proxy informants may be subject to more telescoping error in reporting events for others, moving the event forward or backward in time relative to its true time of occurrence. On the other hand, respondents may sometimes report better for others than for themselves because of social desirability. It may be more acceptable to report embarrassing information about someone else than about oneself.

Among survey researchers there is a widely shared expectation that self-reporters will be more accurate. Despite this general belief, the literature does not provide consistent support for the hypothesis that self-reports are more accurate than proxy reports.

Several studies conducted during the 1950's and 1960's utilized physician and hospital records to validate survey responses. The San Jose Health Study found less agreement between medical and survey records for proxy reports than for self-reports.[1] The underreporting rate for proxy reports was found to be twice the rate for self-reports in a retrospective sample of patients discharged from hospitals within the year.[2] In two studies that matched survey responses with clinical diagnosis following the study, the proportion of matched conditions was slightly greater for self-reports than for proxy reports.[3,4] Differences in self-underreports and proxy underreports varied by condition type. Self-reports resulted in more underreporting of diabetes, nervous system injuries, and impairments; proxy reports in more underreporting of infectious diseases, mental and psychoneurotic disorders (underreporting in this case being contrary to the social desirability hypothesis), heart disease, and respiratory problems.

However, other studies have found little or no difference in the underreporting rates of self-reports and proxy reports. A study of doctor visits to a community health association found no difference in the underreporting rates of self-reports and proxy reports.[5] A more recent validation study[6] in which survey data were verified after the interview also showed no difference between self-reports and proxy underreporting for physician visits, hospitalizations, and surgical conditions.

Two general conclusions were drawn from the studies in which responses were verified using hospital and physicians' records:

1. Self-reports tended to be more accurate than proxy reports.
2. Health events were generally underreported.

A number of studies comparing respondent rules, in which responses were not validated, have also been conducted. Subsamples where respondents reported *more* health events were considered more accurate. The first of these was a National Health Interview Survey (NHIS) study conducted in Charlotte, North Carolina.[7] Households were assigned to one of two respondent rules, one which contained all self-reporters (adults) or one in which interviews were conducted using the standard NHIS respondent rule under which all adults home at time of interview report for themselves. The all self-reporter rule resulted in significantly more reports of conditions than the standard rule. However, for all other health measures, there was no difference between the two respondent rules. A similar experimental design was used in a 1972 NHIS experiment.[8] In this study, the all self-reporter rule yielded more reports for 6 of the 10 measures studied, using a one-tailed test.

Although research has typically found self-reports to be more accurate than proxy reports, the results must be viewed cautiously. A number of these studies are plagued by sampling and design features that limit the inferences that can be drawn. The major sampling and design problems can be categorized in the following three groups:

1. Lack of randomization of household members to self-report and proxy report treatments—resulting in confounding true differences between groups with differential response error.
2. Comparison of reinterview data from self-respondents with proxy reports originally obtained—this design is affected both by contamination due to original interview and longer recall periods for the respondent in the reinterview.
3. Samples of populations that are different from the NHIS population, for example, for local areas, and retrospective sampling of physician and hospital records.

It is difficult to interpret the findings from previous research with confidence. Although early validation studies point to more accurate self-reports, these studies suffer from lack of randomized selection of respondents from household members. For that reason, the studies do not measure the pure effect of proxy response but the difference between self-responses for persons at home when the interviewer calls and proxy reports for others. Later studies assume that more reports of health events mean more accurate reporting, but provide no supporting evidence for the assumption.

A comparison of telephone and personal self-proxy experiments

As mentioned earlier, a special study designed to measure the effect of proxy respondents on national statistics from NHIS was conducted by the National Center for Health Statistics during the spring of 1972.[8] In this study, a control sample of households was interviewed using the standard respondent rule, a rule that permits any adult family member to report for all other family members who are absent. Adults who were home at the time of the interview were encouraged to report for themselves. This rule resulted in approximately 67 percent of adults age 19 years and over reporting for themselves. Conversely, for the households assigned to the experimental group, where self-response was maximized, 96 percent of the adults were self-respondents. In both groups, proxy respondents were used for all children and for adults for whom it was impossible to obtain a self-report due to severe disability or absence over the entire interview period. Each rule was assigned to pairs of weeks. During the first 2 weeks of the quarter, all households

scheduled for interviewing were interviewed using the standard procedures, during the next 2 weeks using the self-respondent rule, and so on through the quarter.

Given the findings from the studies that employed record checks, it was hypothesized in the 1972 NHIS study that the self-respondent rule would yield higher rates of illness and medical utilization than the standard interviewing procedure. As noted, because of the large rates of underreporting, higher rates of reporting health events are understood to represent more accurate responses. A trend towards better reporting by self-respondents was evident for 8 of the 10 health measures analyzed in the 1972 study, 6 of which were significant using a one-tailed test.[8]

The two respondent rules in the telephone implementation of NHIS offer a comparison of response differences for the telephone sample similar to the 1972 NHIS study. As described previously, interviews in half of the sampled households were conducted with randomly chosen respondents and the remaining interviews were conducted with a knowledgeable adult respondent. Because the random respondents are all self-reporters by definition, they provide an estimate of rates of illness, disability, and health care utilization similar to the self-respondent rule in the 1972 NHIS study. Of all respondent rules used, the knowledgeable respondent reporting for all family members most nearly replicates the NHIS standard interviewing procedures. Approximately 55 percent of adults were self-reporters in this sample compared with 67 percent self-respondents in the standard NHIS rule. The difference arises because of the restriction of one self-respondent per family in the telephone survey.

The results of both the personal interview and telephone interview experiments are presented in table Q. (In 94 families in the telephone survey, the random respondent was unable to be interviewed; the interview was conducted with a knowledgeable adult family member. These interviews are eliminated from this analysis.)

In the telephone survey, for all but two variables, activity limitations and 12-month doctor visits, the responses from the standard respondent rule result in higher rates than those from the self-respondents. Although most of the differences are not statistically significant, this of itself is an important finding because most of the previous studies concluded that self-reports were better than proxy reports. Although in the 1972 NHIS study, self-reporting resulted in a rather consistent trend toward better reporting, the opposite finding, that better reporting occurs when using the standard procedures, appears to be the case for the telephone interviews.

There are a number of issues that affect the comparison of the NHIS and Survey Research Center (SRC) samples. The first of these relates to the quarter during which the respective experiments were conducted. As seen in table Q, the overall rates for the SRC sample tended to be higher than those of the NHIS sample, especially for 2-week events. In part, this is due to the greater number of illnesses that occurred during the fourth quarter (October–December) than during the second quarter

Table Q. Number and percent of persons with selected health characteristics, by respondent rule for the Survey Research Center (SRC) Telephone Survey and the 1972 National Health Interview Survey (NHIS) self-proxy study

Characteristic	*Self-respondent rule*[1]		*Standard respondent rule*[3]		*Difference*[4]	
	NHIS	*SRC*[2]	*NHIS*	*SRC*[2]	*NHIS*	*SRC*[2]
	Number per 100 persons per quarter				Percent	
Bed days	141.1	110.5	148.9	161.9	−5.2	−31.7
Work loss days	140.7	101.4	117.6	167.1	[5]19.6	[5] −39.3
Cut-down days	---	178.1	---	246.4	---	−27.7
Restricted activity days	404.3	---	377.4	---	[5]7.1	---
Doctor visits	128.9	146.9	114.8	172.3	[5]12.3	−14.7
Dentist visits	36.4	51.4	38.3	55.9	−5.0	−8.1
Acute conditions	47.9	104.7	42.6	113.8	[5]12.4	−8.0
	Number per 100 persons per year					
Hospital episodes	14.7	15.6	13.8	16.9	6.5	−7.7
	Percent with 1 or more					
Activity limitation	13.6	21.5	12.4	20.2	[5]9.7	6.4
Mobility limitation	3.6	---	3.1	---	[5]16.1	---
Doctor visit in past 12 months	73.6	74.1	72.0	72.3	2.2	2.5
Chronic condition	---	31.1	---	29.2	---	6.5
	Number					
Approximate *N*	15,178	1,068	18,145	2,099	---	---

[1]Self-respondents: NHIS self-respondent rule consists of self-reports for all but 4 percent of adults but includes proxy reporting for all children. SRC column presents weighted values for random, 100 percent self-reporting, adult respondents where weight = (number of eligible adults in family)/(number of telephone numbers).
[2]SRC columns based on control questionnaire data only.
[3]Standard respondents: NHIS standard respondent rule has 67 percent self-reporters among adults and proxy reporting for all children. SRC column reports results for knowledgeable adult rule where 55 percent of the adults were self-reporters and children are excluded. SRC data weighted to adjust for the number of telephone numbers in the household, weight = 1/(number of telephone numbers).
[4]Difference = [(self − standard)/standard] × 100.
[5]Significant difference between self-respondent rule and standard respondent rule at 0.05 level, using standard errors reflecting the complexity of the sample designs.

NOTE: *N* = number of persons.

(April–June). However, this seasonal difference should not affect the comparison of self-respondent and standard respondent rules within each study.

Other design differences that potentially affect the comparison of the 1972 NHIS self-respondent and proxy respondent study and the ones reported here include the mode of interviewing (personal versus telephone), the format of the questionnaires, the definition of self-respondents and proxy respondents, and the interviewing procedures. These factors and their potential effects are reviewed in detail at the end of this chapter.

Despite the design differences, one would expect similar results for the self-proxy comparison across the two studies. However, it can be seen in table Q that the SRC study produced a very different pattern of findings. In particular, the telephone survey found higher levels of reporting for *proxy* respondents than for *self*-respondents. The differences observed in table Q may be elucidated by further investigation of the telephone survey respondent groups. The following two sections look at differences between self-reporting and proxy reporting for the knowledgeable respondent rule subsample, which resembles the sort of sample used in previous analyses of this issue in NHIS, and then examines the differences used in the *random* respondent rule, because that procedure provides a clearer test of response error differences.

Self-proxy differences under the knowledgeable respondent rule

The knowledgeable respondent resembles the reporting rule most often used in past self-proxy comparisons, in which only those adults who are at home at the time of the interviewer's call can be family informants. The rule as implemented in the telephone survey differs, however, from that implemented in the NHIS: (1) Only one person among those present provided self-reports—even though other adults might have been present, they did not respond for themselves; (2) the family informant was not designated randomly from among those present, but rather was usually the one who answered the telephone. If those who inform differ from others present in the household at the time on the variables measured, even in the absence of response errors, the expected values of self-reports from this sample in the study may be different from those of self-reports in the NHIS.

Table R shows the difference between self-respondents

Table R. Number and percent of persons with selected health characteristics for self-respondents and persons with proxy reports: Knowledgeable respondent rule

[Estimates adjusted for the existence of multiple telephone numbers in a household, weight = 1/(number of telephone numbers). Estimates based on both the control and experimental questionnaire data]

Characteristic	Self-respondents			Proxy respondents	Difference[1]
	Total	1-person families	2-person-or-more families		
	Number per 100 persons per quarter				
Bed days	165.1	215.8	143.0	225.6	[2]–82.6
Work loss days	172.9	199.6	161.9	228.2	−66.3
Cut-down days	292.5	357.5	263.9	241.2	+22.7
Doctor visits, person section	159.3	195.0	143.0	150.2	−7.2
Doctor visits, supplements	176.2	217.1	158.0	161.9	−3.9
Dentist visits	58.5	68.3	41.0	55.9	−14.9
Acute conditions	132.0	137.2	129.4	111.2	+18.2
	Number per 100 persons per year				
Hospital episodes	17.5	18.2	17.2	14.4	[2]+2.8
	Percent with 1 or more in past 2 weeks				
Bed days	8.9	10.4	8.2	9.9	−1.7
Work loss days	6.9	7.8	6.5	9.5	[2]–3.0
Cut-down days	10.7	11.3	10.5	9.9	+0.6
Doctor visits, person section	16.3	19.1	15.0	15.5	−0.5
Doctor visits, supplements	18.8	21.5	17.6	16.5	+1.1
Dentist visits	7.0	7.4	6.8	7.2	−0.4
Acute conditions	17.3	17.3	17.3	15.5	+1.8
	Percent with 1 or more				
Chronic conditions	33.2	36.3	31.9	30.0	+1.9
Hospitalizations	14.5	14.5	14.5	11.8	[2]+2.7
Doctor visits	76.8	75.1	77.6	67.8	[2]+9.8
	Number				
Approximate *N*	2,295	704	1,591	1,832	...

[1]Difference = number of self-respondents in 2-person-or-more families − number of persons with proxy reports.
[2]Significant difference at 0.05 level, using standard errors reflecting the complexity of sample design.

NOTE: *N* = number of persons.

and proxy respondents for that portion of the telephone sample interviewed under the knowledgeable respondent rule. The first three columns in the table present results for total respondents and different groups of self-respondents, those in single-person families (who must be self-respondents) and those in families of two or more persons.

Most analyses in previous studies of this issue examined differences between all self-respondents and all proxy respondents. If a similar analysis is performed on the telephone survey data by comparing the first and fourth columns, it can be seen that for some measures self-respondents report more health events for themselves, and for other measures they report fewer events for themselves than for proxies. Only 5 out of 18 differences are statistically significant. The results fail to conform to the generally accepted belief that self-respondents report more health events for themselves than for others. There is some indication, however, that measures requiring 12-month recall exhibit more reporting by self-respondents. Three of the five significant differences were found among the 12-month recall variables.

Table R provides another comparison of self-respondents and proxy respondents, one that recognizes the fact that one-person families are, by definition, all self-respondents. Thus, removing the one-person families from the self-respondent group can purify the comparison of self-responding and proxy responding. Self-reports of health events among people in two-person families are generally lower than those for proxy reports. For example, there were about 83 fewer bed days reported per 100 self-respondents per quarter than per 100 proxied persons. Although this difference is statistically significant at the 0.05 level, most of the differences in the table are not significant. There is also some indication of *higher* reporting for self-respondents on measures involving a 12-month recall.

Self-proxy differences under the random respondent rule

Although the removal of reports for one-person families purified the self-proxy comparison, the differences observed in the comparisons using the knowledgeable respondent rule were still confounded with true health differences between the phone answerers and others in their families. This is the problem that has confronted all previous studies of self-reporting versus proxy reporting. The random respondent rule in this study removed this confounding because the self-respondent is a random selection from among all adults in the household, and, thus, the proxied persons are a complementary random sample. Thus the expected values of these two groups for the measured health variables should be identical. Differences that are discovered are attributable to response and nonresponse errors rather than to true differences between the self-respondents and proxy respondents. The random respondent rule, in short, more accurately addresses the question of whether, on the average, respondents can report as well for others in the family as they do for themselves.

Table S presents the self-proxy comparison under the random respondent rule. As in table R, the self-respondents in single-person families are separated from those in multiple-person families. Thus, assuming no response or nonresponse error differences, the self-respondents from those in multiple-person families should have the same expected value as those for *proxy* respondents in multiple-person families.

The trend in the table is clearly one of greater reporting for proxy respondents than for self-respondents. For example, self-respondents have 125 fewer bed days per 100 persons per quarter than proxy respondents. This is the same trend observed in the knowledgeable respondent group, but the magnitude and consistency of the differences are much greater in this group. Again, however, there is evidence of a reversal of this trend for measures involving 12-month recall, where self-reports appear to produce slightly more events than do proxies. The overall tendency toward higher proxy reports runs directly counter to previous beliefs about self-reports versus proxy reports.

Multivariate models to adjust for nonresponse

There are two possible sources of error that may explain the earlier chapter findings. As noted previously, the expected values for self-reports and proxy reports in households with two or more adults assigned to the random respondent rule should be identical. Deviations from equal estimates may be because of differential nonresponse or response error. If nonresponse is consistent across all demographic subgroups, the distribution of demographic characteristics should be identical for the two groups.

Table T presents the sex, age, and race characteristics of self-respondent and proxy-respondent persons in families with at least two adults. The figures indicate that the self-proxy differences shown in table S may be the result of lower response rates for males and individuals at both ends of the age distribution, 17–24 years and 75 years and over.

To adjust for nonresponse differences, logistic response models were fitted for seven of the dependent variables presented in table S. The model used for each variable was

$$\ln \frac{p}{1-p} = \mu + \beta_1 X_{i1} + \beta_2 X_{i2} + \beta_3 X_{i3}$$

where p = proportion with at least one episode of the specific health event

μ = constant

X_1 = age of the person being reported for, or, for self-respondents, the age of the person reporting

X_2 = indicator variable for the sex of the person being reported for

X_3 = indicator for self-proxy reporting

The estimated coefficients and standard errors are presented in table U. Even after attempting controls on nonresponse differences by using age and sex, significant proxy effects remain. Only one variable, chronic conditions, exhibits more reports for self-respondents. The estimates for bed days, work loss days, dental visits, and acute conditions repeat the findings in table S of more reports for proxies.

Table S. Number and percent of persons with selected health characteristics for self-respondents and persons with proxy reports: Random respondent rule

[Estimates adjusted for the existence of multiple telephone numbers in a household and the unequal chance of selection as the random respondent. For random respondents, weight = (number of eligible adults in family)/(number of telephone numbers). For persons with proxy reports, weight = [(number of eligible adults in family − 1)/(number of eligible adults in family)][1/(number of telephone numbers)]. Estimates based on both the control and experimental questionnaire data]

Characteristic	Self-respondents		Proxy respondents	Difference[1]
	1-person families	2 persons or more		
	Number per 100 persons per quarter			
Bed days	182.7	102.1	226.9	[2]124.8
Work loss days	162.5	137.8	199.6	−61.8
Cut-down days	256.1	219.7	289.3	−69.6
Doctor visits, person section	155.4	124.8	168.4	[2]−43.6
Doctor visits, supplements	175.5	143.7	168.4	−24.7
Dentist visits	64.4	50.1	67.6	[2]−17.5
Acute conditions	137.8	98.2	119.0	[2]−20.8
	Number per 100 persons per year			
Hospital episodes	16.4	15.6	15.2	+0.4
	Percent with 1 or more in past 2 weeks			
Bed days	8.0	5.3	9.9	[2]−4.6
Work loss days	7.8	5.7	7.5	[2]−1.8
Cut-down days	9.3	8.5	9.5	[2]−1.0
Doctor visits, person section	17.4	14.4	16.1	−1.7
Doctor visits, supplements	19.8	15.2	17.2	−2.0
Dentist visits	6.9	6.2	8.1	[2]−1.9
Acute conditions	17.8	13.3	17.0	[2]−3.7
	Percent with 1 or more			
Chronic conditions	37.4	33.7	29.6	[2]+4.1
Hospitalizations	11.5	13.0	11.8	+1.2
Doctor visits	76.9	75.7	73.2	+2.5
	Number			
Approximate *N*	734	1,345	1,795	...

[1]Difference = number of self-respondents in 2-person-or-more families − number of persons with proxy reports.
[2]Significant difference at 0.05 level, using standard errors reflecting the complexity of sample design.

NOTE: *N* = number of persons.

Table T. Percent distribution of persons with self-reports and proxy reports by sex, age, and race: Random respondent rule

Characteristic	Self-respondents[1]	Proxy respondents[2]
Sex	Percent distribution	
Male	43.5	53.6
Female	56.1	46.2
Age		
17–24 years	13.3	20.4
25–34 years	23.2	21.3
35–44 years	20.2	17.2
45–54 years	18.6	15.9
55–64 years	14.4	13.1
65–74 years	7.9	7.9
75 years and over	1.8	3.0
Race		
White	89.1	89.8
All other	10.9	10.2
	Number	
Approximate *N*	1,345	1,635

[1]Weight = (number of family members)/(number of different phone numbers).
[2]Weight = (number of family members)/(number of family members − 1)(number of different phone numbers).

NOTE: *N* = number of persons.

Table U. Results from logit models for selected health variables adjusting for nonresponse

[Table is based on self-reports and proxy reports from families with 2 adults or more in households assigned to random respondent rule, $N = 2,711$]

Coefficient and standard error	Health variables						
	Bed days	Work loss days	Dental visits	Doctor visits[1]	Hospital visits[1]	Acute conditions	Chronic conditions
Age	−0.004	[3]−0.012	0.000	[3]0.009	[3]0.011	[3]−0.016	[3]0.035
SE	0.004	0.005	0.005	0.003	0.004	0.004	0.003
Sex	0.047	−0.139	0.290	[3]0.343	[3]0.478	0.188	−0.063
SE	0.143	0.154	0.151	0.106	0.121	0.109	0.087
Self-proxy[2]	[3]0.736	[3]0.337	[3]0.422	0.207	−0.204	0.202	−0.141
SE	0.158	0.161	0.156	0.107	0.118	0.111	0.087

[1]No significant self-report or proxy report difference in table S.
[2]Indicator variable for self-report or proxy report: 0 = self-report; 1 = proxy report.
[3]Coefficient/SE > 2.0.

Summary, speculation, and conclusions

Although the conclusions from previous self-proxy comparisons do not consistently support the hypothesis that self-reports are more accurate, or that self-reporters report more health events, the findings from this study are sufficiently contradictory to warrant speculation as to the causes of the differences.

1. The data were collected by telephone in this study, and personal interviews were used in past studies. Others have speculated that response errors may be greater in telephone surveys than in personal interview surveys.[9] This might support a hypothesis of diminished differences between self- and proxy reporting, but not a hypothesis of better proxy than self-response.
2. The format of this instrument was different from that in most previous work. As noted in the chapter on research design, the NHIS personal interview asks some questions about all members of the household at once, "Did anyone in the family go to a dentist?" Others are asked about each family member in sequence before going on to the next question, "Did _____ stay in bed because of any illness or injury?" In such a design, the referent person changes very quickly and there may be some tendency to fail to report events for individuals not present during the interview. In the telephone design, all core questions were asked of each family member individually. This may affect the ability to recall events experienced by other family members. Repetition of questions for each family member may improve the recall by the respondent.
3. As noted earlier, studies differ in the nature of the self-respondent and proxy respondent groups. In the telephone survey only one respondent reported for the entire family. In other studies, each person who was home at the time of the interview responded for himself or herself.

Therefore, there are a variety of hypotheses that might explain the effects observed in this study, but few are testable without validating data. Of the speculations listed above, it is suspected that the alteration of the questionnaire, in which questions were asked about individuals separately, may have had the strongest influence on the nature of proxy responses. With results that are so contrary to dominant beliefs, replication and further experimentation are badly needed.

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Chapter V
A comparison of CATI and non-CATI questionnaires[a]

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Introduction

Part of the research design for this telephone survey included assigning half of the interviews to typical paper and pencil questionnaires and half to a questionnaire programmed for computer-assisted telephone interviewing (CATI). The random assignment was made on a sample number basis; thus, all of the families and persons in the same household were given the same treatment. Each interviewer conducted CATI and non-CATI interviews on alternate weeks.

This part of the research design was only partially fulfilled because of CATI hardware problems that developed in the first month of interviewing. During that time, instead of using CATI on a random half-sample, only 36 percent of the interviews were taken using the computer. Because the problems occurred during a relatively short period of time, the balance of CATI and non-CATI interviews was more affected for some interviewers than for others.

The sample was divided into three replicates, one introduced at the beginning of each of the 3 months of the data collection period. Because of this procedure, the sample cases affected by hardware problems can be separated from those in the other two replicate groups without risking compounding of differences between modes with other differences between the CATI and non-CATI groups.

CATI systems use video display terminals operated by interviewers to present questions and accept respondent answers in a telephone survey. At this writing, there are many different CATI systems functioning in large and small survey organizations throughout the United States.[1-3] They are an alternative to the use of a hard-copy questionnaire for telephone surveys conducted from a centralized facility.

This chapter is designed to give the reader a description of the adaptation of the questionnaire used in the National Health Interview Survey face-to-face interview to a CATI system, to compare data from non-CATI and CATI interviews, to compare interviewer performance on both systems, and to examine interviewer reactions to using CATI.

Some survey errors may be affected by CATI because it changes the interviewer's task in several ways; most basically, the computer, through the exercise of preprogrammed logic, automatically displays the appropriate next question. This change presumably eliminates errors arising from the interviewer's making a mistake at a decision point, asking an inappropriate set of questions, and skipping the appropriate ones. Another basic change in the interviewer's job on CATI is the use of a computer keyboard to enter the respondent's answers, both response choices for closed questions and verbatim answers to open questions. Here one might hypothesize harmful effects of giving the interviewer the added task of manipulating a keyboard. Potential results might be erroneous entries to closed questions or inadequate recordings for open question responses. The first possibility, however, is reduced through the enforcement by the machine of legal ranges of answers to closed questions (for example, if the legal codes for gender are "1. Male" and "2. Female," and the interviewer enters "3," an error message will signal the mistake and force a reentry). The loss of information on open-question responses is, however, not reduced by any features of CATI and must be attacked, as with paper questionnaires, through interviewer training and supervision.

Other potential effects of CATI on data quality arise through the need of the interviewer to review questions previously answered; for example, because a respondent decides to change the answer. CATI systems usually provide the interviewer with special keys or commands to request the movement back to a previous question. In a complex questionnaire, with several possible routes through questions, it is easier for an interviewer to get disoriented and lose his or her place in the instrument than appears to be true with paper questionnaires. The notion of segmentation, the fact that the interviewer sees the entire questionnaire only in discrete displays of single questions, was observed early in CATI work as potentially productive of interviewer errors.[4] In the CATI system used in this experiment, an interviewer sometimes could back up only to the last branch question, not necessarily the immediately previous question.

Because some features of CATI change the task of the interviewer, differences might be expected in response rates, response errors, and interviewer reactions to the survey. Response rates might be affected if, despite extensive training, the interviewers are not comfortable with the use of the terminal to interview respondents, alter their behavior in response to such anxiety, and are not as effective in persuading persons to cooperate. Response errors could arise either through changes in the interviewer's style of delivery of the questions or through changes in the interviewer's behavior in recording answers. For these error sources, measurements of interviewer reactions to the experience might illuminate certain problems in the use of CATI.

[a]This chapter is adapted from Computer assisted telephone interviewing: Effects on interviewers and respondents, *Public Opinion Quarterly*, Fall 1983.

The experiment described in the next section is based on data collection from a particular CATI system used by the Survey Research Center in 1979. Although for some features inferences may be usefully drawn about similar effects arising from other CATI systems, it would be unwise to generalize these results to the potential effects of all CATI systems. The CATI system used in this study was new and relatively untested with some deficiencies and operating problems that should be kept in mind in interpreting the findings in this chapter.

Differences in CATI and non-CATI questionnaires

The CATI questionnaire was designed to replicate as closely as possible the paper and pencil questionnaire. The inherent differences in the two procedures, however, required some adjustment of the paper-and-pencil version to maximize the comparability with the CATI version. The complexity of the questionnaire coupled with the limitations of the CATI system at the time of implementation also resulted in some adaptations of the questionnaire unique to the CATI instrument. This section will describe the major differences in the two instruments.

To fully understand the nature of the differences between the CATI and non-CATI questionnaires requires a review of the flow of the interview and the associated tasks of the interviewer. The questionnaire collected information on all adult members of a family; data were collected both through self-reports and through proxy reports from a family member reporting for someone else in the same family. A set of core health and demographic questions in the person section is asked for each family member. Depending on what information is obtained, further supplements are completed. These supplements are used to collect more detailed information on conditions, doctor visits, and hospitalizations. An example will illustrate this process: Suppose an interview is being conducted with a family that has only two members, a husband, John, and his wife, Jane. John is the informant. First, the core questions in the person section for John would be completed. During these questions, the interviewer may ascertain that John both suffered from a condition and visited the doctor in the 2 weeks prior to the interview. The person section would then be completed for Jane. During these questions, John reports that Jane also had a condition in the past 2 weeks. Following this, the interviewer would then complete supplements on John's condition, on John's doctor visit, and then on Jane's condition. The example illustrates a number of complexities that exist in the administration of the questionnaire. To incorporate these complexities into the CATI system required the following capabilities:

1. Collection of core information for each member of a family in the person sections.
2. Collection of supplemental information for only those family members with health events requiring further questioning.
3. Ability to collect a varying number of these supplement sections per person.
4. Assisting the interviewer in identifying the referent person (person to whom the questions referred) and the current questionnaire segment.

These requirements in the non-CATI format were accommodated through the use of multiple-booklet questionnaires. A separate booklet was used for the person section and for each of the three types of supplements. Booklets were added to the case as needed to complete the questioning. Identifying information consisting of case number, referent person, and interviewer number was recorded on the cover of each booklet used during an interview. Thus, after completing the person section for the respondent, the interviewer could select the next appropriate booklet from stacks in the interviewing station, record necessary identifying information, and proceed with the interview.

The CATI instrument design closely paralleled the flow of the non-CATI questionnaire in its movement between the person and supplement sections. At the end of the first person, a screen presented the available options for continuing the interview. The interviewer entered the desired section to complete next and the information needed to identify the person being referred to in the questions. At the end of each section, which was the equivalent of a booklet in the non-CATI version, the interviewer was returned to this same screen.

As an aid to the interviewer, information concerning the referent person and the relevant section of the interview was displayed at the top of each CATI screen. For example, if the interviewer was collecting information on the third doctor visit for the second person in the family the display would show

PERSON # = 2 DOC VISIT = 3

The Survey Research Center CATI system described here provided the researcher with a number of controls designed to reduce interviewer error. Each CATI screen, which usually was equivalent to one question, had both a text field and a numeric field in which responses could be recorded. A text field was always available to the interviewer to record probes and comments. The researcher determined whether to include a numeric field, and, if so, whether to initialize the cursor on the screen to the numeric or text field. Interviewers could move between the two fields with one keystroke. When a numeric response was required, a list of valid responses was also programmed by the researcher. If an interviewer failed to enter a valid response, an invalid response message would appear at the bottom of the screen with a blinking cursor indicating to the interviewer that a new response had to be entered. In addition to checking for valid responses, the system enforced proper branching to the next question contingent on previous answers.

Two programmed control keys were designed to facilitate backward movement in the questionnaire. The first key, which printed a "Where next?" message, allowed the interviewer to return to any previously asked question. This key was also used to skip out of a terminated interview. The second key was designed to return the interviewer to the question immediately preceding the current question. However, due to the complex branching patterns in the interview, often the program returned

the interviewer to the last question prior to branching. This awkward backward movement was a major disadvantage of the CATI system. Although the non-CATI interviewer could simply turn the page to reexamine a previous question, there was no similar method with the CATI system.

Performance characteristics for CATI and non-CATI interviews

The overall family level response rate for sample cases assigned to CATI treatment compared with non-CATI cases was 78.7 versus 81.5 percent, a statistically significant difference. The family level response rate was defined as the ratio of all complete and partial families interviewed divided by the estimated total number of families sampled; that is, total number of families interviewed plus total number of families refused or not interviewed after contact plus total number of other working household numbers where number of families is unknown. As described earlier, the sample consisted of three replicate groups. The response rates for these groups are shown in table W. The only difference is found in the first replicate group. The large discrepancy between the CATI and non-CATI response rates in replicate 1 is due, in part, to the assignment of CATI cases to non-CATI during the second and third week of interviewing because of CATI hardware difficulties. Successful interviews for these conversions were counted as non-CATI. Refusals were returned to CATI during the final week for replicate 1, thereby increasing the refusal rate for CATI. Some of the response rate differences may reflect the longer time required to become comfortable conducting an interview on CATI. There are no differences for replicates 2 and 3, and overall there was no large effect on response rates in using CATI.

The number of interviewer hours required to obtain one interview differed between CATI and non-CATI cases. An average case required 52 minutes of interviewer time using CATI and 46 minutes for each case using paper and pencil questionnaires. There are three possible explanations for the longer interview length on CATI. The additional time required for the CATI cases may reflect a difficulty interviewers had with using paper coversheets to record calls and household composition but a computer terminal for display of questions and recording of responses. Response time lags between displaying of screens may account for some of the additional 6 minutes. Finally, CATI forces the interviewer to complete the recording of a response before asking the next questions; non-CATI interviewers have been observed to begin asking the next question while recording the previous response. It should be noted that there was considerable variability across interviewers in the length of time required for each case.

Table W. Percent response for CATI and non-CATI interviews by sample replicate

Sample replicate	*Response*	
	CATI	*Non-CATI*
	Percent	
Total	78.7	81.5
Replicate 1	73.9	[1]83.2
Replicate 2	81.5	81.6
Replicate 3	78.8	78.7

[1]Includes 208 interviews originally assigned to CATI. Response rate using only cases assigned to non-CATI is 79.5 percent.

NOTE: CATI = computer-assisted telephone interviewing.

Interviewer and respondent reactions to CATI and non-CATI interviewing

After study, 31 interviewers completed a self-administered questionnaire that sought their attitudes about the survey procedures (table Y). In this study, all had conducted interviews in both CATI and non-CATI modes.

One set of questions asked the interviewers to enumerate the advantages and disadvantages of the two modes. The perceived advantages of CATI reflected the assistance that the computer offered the interviewer; the interviewers appreciated the automatic skipping from one question to another, the freedom from page turning, and ease of typing numeric answers into the terminal. On the other hand, they found the paper version easier to use when corrections to previous answers were required; they also believed it offered them better control over the pace of the interview. This last comment probably reflects a problem of the particular CATI system employed in this study: The length of time between entering a response and the display of the next question was sometimes longer than was desirable. This response lag annoyed the interviewers, and their comments reflected that. Finally, the interviewers claimed that using the paper version of the questionnaire gave them greater confidence, that they knew exactly where in the questionnaire they were at any moment. This last comment reflects the problem of segmentation. When at the end of the survey, however, the inter-

Table Y. Number of responses of interviewers to postsurvey questions on CATI and non-CATI interview modes, by question topic and response category

Question topic and response category	*CATI*	*Non-CATI*
	Number of interviewers	
Total	31	31
Difficulty of learning to interview in mode		
Very difficult	1	0
Somewhat difficult	7	5
Somewhat easy	14	14
Very easy	9	12
Fatigue due to interviewing		
Very tiring	3	1
Somewhat tiring	7	9
Not very tiring	18	19
Not tiring at all	2	2
Not ascertained	1	0
Tension due to interviewing		
Very tense	2	0
Somewhat tense	9	6
Not very tense	13	14
Not tense at all	6	10
Not ascertained	1	1

NOTE: CATI = computer-assisted telephone interviewing.

viewers were asked which mode, CATI or non-CATI, they preferred, there was no clear preference between them; about the same numbers favored one or the other mode and about a quarter had no preference.

Interviewers reported that it was not particularly difficult to learn either CATI or non-CATI procedures. As expected, learning CATI was somewhat more difficult. About one-third of the interviewers found the interviewing task somewhat or very tiring; however, there was little difference between the two modes. A question on tension level showed the CATI procedure to be somewhat more tension producing.

A potential major source of fatigue is the close attention to the screen required in the CATI system; about one-third of the interviewers reported this to be somewhat or very tiring. In response to open questions asking about problems with the screen or the terminal, the most frequently mentioned problems were the glare on the screen and the difficulty of moving from numeric to text entry on the same screen. Both of these problems are parts of human engineering problems of CATI and computer terminal use in general that have received recent attention. Response time as a problem was reported by over one-half of the interviewers (table Z).

The remaining questions concerned differences between modes for various interviewing tasks. The responses generally confirm responses to the open question shown earlier. The CATI procedures were rated as better for following question sequences. Non-CATI was seen as better for recording probes, correcting errors, and editing interviews.

Although a strong hypothesis of no difference between *respondent* reactions to CATI and non-CATI interviewing could be asserted because not all the respondents are aware of the mode being used to record their answers, it is useful to examine the available evidence to check for refutations of that hypothesis. After the completion of the questionnaire, the interviewer completed a form called "interviewer observations." This form includes questions concerning the number of times the respondent asked how much longer the interview would take, the interviewer's perception of the respondent's level of interest in the interview, the number of times that the respondent asked for clarification of a question, and the frequency of questions repeated for the respondent.

The differences that do exist between the modes are typically small, are statistically insignificant, and move in different directions across variables; for example, a higher percent of respondents ask for clarification in the CATI cases, but a higher percent of respondents require repeating of questions in the non-CATI cases. These items are subject to rather large interviewer variability and thus any inferences must be treated carefully.

Table Z. Number of interviewers by response to the question, "Does the response time (the length of time between pushing the 'enter' key and the time the next screen appears) cause problems or not?"

Response	*Number of interviewers*
Total	31
Many problems	1
Some problems	17
Few or no problems	12
Not ascertained	1

Response differences between CATI and non-CATI interviews

Two factors can potentially affect comparisons of CATI and non-CATI response distributions: (1) adaptations in the questionnaire to fit limitations or requirements of the CATI system and (2) changes in the interviewer's behavior as a result of changes in requirements of the task. Despite these possibilities, a strong null hypothesis of no difference between CATI and non-CATI response distributions is reasonable in the analysis of response effects associated with the use of CATI. Different variable types and interviewer characteristics were examined to be relatively exhaustive in the search for evidence against the null hypothesis.

Tables AA and BB present a comparison of several key statistics for CATI and non-CATI interviews only for randomly selected respondents reporting for themselves. The randomly selected persons are used to assure that the two groups being compared come from the same population. This eliminates any complications arising from differential proxy reporting error between CATI and non-CATI cases or from the possibility that knowledgeable adults used as informants might have different characteristics in the CATI and non-CATI samples.

None of the differences between the two modes exceeds those expected because of sampling error. This includes demographic distributions of respondents in the two modes, which provide a check on nonresponse differences, and a variety of health statistics. There appears to be little evidence that statistics based on measures altered for CATI are more likely to show effects. A check on CATI and non-CATI differences in the three replicate groups was performed to examine the hypothesis that difficulties with the CATI system in the first month of interviewing may have affected the statistics. There are no discernable differences across the three replicate groups. The only suggestion of this result is the smaller difference on CATI between reports of doctor visits in the person section versus those in the supplemental questions. The non-CATI version obtained more reports of doctor visits in the supplements than the person section. This may reflect difficulties the interviewers experienced moving to the supplement on CATI.

Experience with training interviewers to use the CATI system indicates that the reactions of older interviewers to the manipulation of the keyboard and the handling of other aspects of the system were different from those of younger interviewers. The level of anxiety evident among some interviewers when first faced with a terminal was sufficiently high to impede the speed of learning the system. Several interviewers required extended training sessions before their proficiency with the system was adequate for production interviewing and their anxieties were reduced to a manageable level. It was hypothesized that, despite effort to assure a desirable minimum level of skill with the terminal, some of these interviewers may have produced different results on CATI interviews than non-CATI cases.

Table AA. Percent distribution of CATI and non-CATI interviews and standard errors of difference, by selected demographic characteristics: Random respondent self-reports

[Each case weighted by the reciprocal of the probability of selection]

Characteristic	CATI	Non-CATI	Standard error of difference
	Percent distribution		
Sex			
Male	42.9	42.7	2.38
Female	56.5	57.1	2.38
Unknown	0.6	0.1	0.29
Age			
17–24 years	14.5	15.3	1.71
25–34 years	25.1	22.9	2.06
35–44 years	17.1	19.3	1.85
45–54 years	17.2	16.1	1.79
55–64 years	13.7	13.7	1.65
65–74 years	8.4	8.2	1.33
75 years and over	2.9	3.1	0.82
Unknown	1.1	1.2	0.51
Race			
White	89.0	86.9	1.56
All other	11.0	13.1	1.56
Education			
0–8 years	9.4	10.3	1.43
9–11 years	14.5	12.3	1.64
12 years	34.8	37.4	2.31
13–16 years	34.6	30.3	2.25
17 years or more	6.2	8.3	1.24
Unknown	0.5	1.4	[1]0.46
Income			
Less than $5,000	8.7	8.6	1.35
$5,000–$9,999	12.3	14.0	1.62
$10,000–$14,999	16.7	13.4	1.72
$15,000–$24,999	24.8	29.0	[1]2.13
$25,000 or more	21.6	20.1	1.95
Unknown	15.8	14.8	1.73
Marital status			
Married	68.8	67.1	2.24
Widowed	6.1	5.9	1.14
Divorced	5.7	7.7	1.20
Separated	2.3	2.1	0.71
Single	17.1	17.2	1.81
	Number		
Approximate N	942	1,137	...

[1]Difference between proportions significant at $p < 0.05$, two-tailed test, using standard errors reflecting the complexity of the telephone design.

NOTES: CATI = computer-assisted telephone interviewing, N = number of persons.

Table BB. Percent and number of CATI and non-CATI interviews and standard errors of difference, by selected health characteristics: Random respondent self-reports

[Each case weighted by the reciprocal of the probability of selection]

Selected characteristic	CATI	Non-CATI	Standard error of difference
	Percent with at least 1		
2-week bed days	6.4	5.3	1.07
2-week work loss days	6.1	6.1	1.08
2-week cut-down days	8.7	8.7	1.28
2-week doctor visits, person section	14.4	15.4	1.61
2-week doctor visits, supplements	14.7	17.3	1.66
2-week dentist visits	3.6	7.8	[1]1.03
Phone call to doctor	3.8	1.6	[1]0.75
12-month hospital episodes, supplements	12.7	12.7	1.51
12-month doctor visits	74.8	76.9	1.94
12-month bed days	51.5	55.4	2.26
Limitations of activity	25.0	24.8	1.96
Excellent or good health status	86.4	84.6	1.59
	Mean number		
Acute conditions	0.152	0.172	0.0193
Chronic conditions	0.474	0.523	[1]0.0391

[1]Difference between proportions significant at $p < 0.05$, two-tailed test, using standard errors reflecting the complexity of the telephone design.

NOTE: CATI = computer-assisted telephone interviewing.

Age was used as an imperfect indicator of proficiency and potential feeling at ease with the system. However, across CATI and non-CATI responses there was no effect of interviewer's age.

Interviewer differences in CATI and non-CATI interviewing

CATI systems should reduce errors caused by interviewers following inappropriate skip patterns. Because errors of this type are usually rare, complex contingency patterns provide the most useful data for comparing CATI and non-CATI instruments. One question sequence in this study, consisting of 28 questions and 42 possible paths, illustrates the difference in the two modes. Theoretically, the CATI system should not have any consistency errors; however, 1.8 percent ($N = 3,759$) of the data entries for this sequence were inappropriate, because of the lack of a wipeout option when backing up. If a respondent changed his or her mind, the interviewer would need to return to the base question and follow a new branching sequence. The original response is then invalid and the conflict would have to be resolved in postsurvey processing. Most current CATI systems automatically erase responses when the interviewer backs up. In contrast, 8.8 percent ($N = 4,451$) of the non-CATI entries for this sequence were inappropriate. These errors are due not only to interviewer errors, but also premature termination of the interview by the respondent and coding errors. There are two problems with the calculation of non-CATI errors. First, it may overestimate errors, because following an inappropriate skip pattern results in missing data for the correct routing and inappropriate data for the incorrect routing. Second, it may underestimate interviewer errors because the introduction of coders may resolve some of the inconsistencies.

Table CC presents estimates of means, proportions, and interviewer effect measures ρ^*_{int} for 14 survey variables by mode of data collection using the randomly selected respondents' reports for themselves. The ρ^*_{int} measures are approximate intraclass correlations associated with differences among the interviewers. High, positive ρ^*_{int} measures indicate that different

Table CC. Measures of interviewer variability by interviewing mode: Random respondent self-reports

Characteristic	CATI (n = 874)		Non-CATI (n = 1,026)	
	Mean or percent	ρ^*_{int}	Mean or percent	ρ^*_{int}
2-week bed days	6.7	−0.0059	5.7	−0.0029
2-week work loss days	6.4	−0.0110	6.4	0.0103
2-week cut-down days	8.6	−0.0028	9.2	−0.0049
2-week doctor visits, person section	15.3	−0.0138	15.1	[1]0.0171
2-week doctor visits, supplements	15.9	−0.0144	17.5	0.0088
2-week phone calls to doctor	3.8	−0.0040	1.9	−0.0035
2-week dentist visits	5.2	−0.0005	7.7	0.0071
12-month hospital episodes, supplements	12.9	−0.0024	12.8	0.0112
12-month doctor visits (2–4 visits)	36.7	−0.0047	37.6	0.0049
Limitation of activity	24.7	−0.0052	25.8	−0.0031
12-month bed days	52.9	0.0077	56.4	−0.0041
Excellent health status	40.8	[1]0.0187	42.7	0.0059
Mean number of acute conditions	0.162	0.0046	0.182	−0.0021
Mean number of chronic conditions	0.475	−0.0063	0.517	0.0061

[1]Values of ρ^*_{int} different from zero at the 0.95 confidence level under the assumption of equal within-interviewer variances, random assignments of cases to interviewers from the same population, simple random sampling. There are violations of these assumptions in this project, but the results of the hypothesis test are a useful guide to the statistical importance of various ρ^*_{int} values.

NOTE: CATI = computer-assisted telephone interviewing. *n* = number of respondents in sample.

interviewers obtain very different answers from their respondent groups. Because assignment of sample cases to interviewers was randomized, differences between interviewers that are larger than expected from sampling error alone are attributed to different behaviors on the part of interviewers. For a more detailed discussion of the response error model underlying ρ^*_{int}, see reference 5.

That the CATI system might decrease between-interviewer variation because of the increased control over interviewer behavior had been hypothesized. With CATI, an interviewer is much less likely to skip questions, change question order, or fail to follow skip patterns correctly. Table CC offers some support to this hypothesis, although statistical significance is not achieved. For interviews taken on CATI, ρ^*_{int} values range from −0.0144 to 0.0187 with an average value of −0.0040 (using the median). Non-CATI interviews have an average ρ^*_{int} value equal to 0.0054 and a range from −0.0049 to 0.0171.

Although there are essentially no interviewer effects on the CATI system, the non-CATI responses show somewhat greater interviewer variability. An average value of ρ^*_{int} of 0.0054 for non-CATI responses may seem relatively small, but it is useful to note that such a value must be multiplied by a function of the average interviewer workload to estimate the inflation in total variance due to interviewer effects. In this study, 33 interviewers took the 1,026 non-CATI random respondent self-reports. Thus, the average interviewer workload was 31.1 interviews. In estimating the inflation in the variance due to the use of these 33 interviewers instead of using a different interviewer for each respondent, the average design effect for the given interviewer assignment is as follows:

$$\text{Deff}_{int} = [1 + 0.0054(31.1 - 1)] = 1.16$$

This result can be interpreted as an approximate 16 percent increase in the total variance of percents due to correlated response deviations on the average over these health variables. The same method shows that the non-CATI interviewer variability causes a 36-percent increase in this variance component over what would be expected from CATI interviews.

The interviewer variability observed in this study appears to be attributable to non-CATI responses obtained by a group of interviewers who spent more of their time on non-CATI interviewing than was typical. These results could be explained by the fact that the interviewers who spend more time on CATI find their behavior more controlled and carry these good habits into their non-CATI interviewing. Conversely, the interviewers who spend more time on non-CATI interviewing are not able to transfer their bad habits to their CATI interviews. If this post hoc hypothesis is correct, the non-CATI effects of these interviewers might be expected to increase over time; that is, as their bad habits develop. However, the data did not show this behavior. Indeed, the values of ρ^*_{int} for these interviewers decreased over the three replicates of the sample. Finally, despite randomized assignment of the sample to interviewers, the ρ^*_{int} values reflect nonresponse differences among interviewers that complicate their interpretation.

Summary and conclusions

It appears likely that a greater proportion of surveys will use the telephone as a medium of sampling and data collection in the future. As software developments proceed and computer hardware costs shrink, many of these will use CATI systems. This experiment can be used as a benchmark for the transition to CATI because it provides documentation on potential problems with changing modes.

For most criteria, there are only small differences between CATI and non-CATI interviewing in this project. The criteria include response rates, reactions of the interviewer and respondent, and most health statistics of interest. There are,

however, some exceptions to this finding of equivalence between methods. The first exception is the result that the average number of minutes per CATI interview exceeded that for non-CATI interviews. There also is some evidence that the interviewer variability estimates tend to be lower in CATI than non-CATI. Finally, there is evidence of lower skip error problems in the CATI interviews. The first of these results affects survey costs; the second and third, survey error. The first may be a function of software or hardware choice and thus can be addressed in new CATI designs. The second and third will be of benefit to all CATI systems in the future.

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Chapter VI
Measurement of interviewer errors in the SRC Telephone Survey

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Introduction

The past literature on the measurement of nonsampling errors has two main focuses. The first is the estimation of components of total variance, sometimes total mean square error, on survey measures. From this literature has come a variety of models and estimation procedures, many of them concentrating on the measurement of effects of interviewers on the data.[1-4] The second literature investigates ways to improve the quality of survey data through questionnaire design, training of interviewers, and supervisory techniques used during the survey period. This work uses experimental variations embedded in the survey design to test alternative procedures.[5] This other literature has aggressively pursued the *reduction* of nonsampling errors but has generally not utilized methods that could facilitate the routine *measurement* of them.

This chapter reports an attempt to link these two approaches to the study of nonsampling errors, specifically those associated with effects that interviewers have on survey data. These effects arise, it is believed, through idiosyncratic behaviors on the interviewer's part that create similar response errors among their respondents. This study used an interpenetrated design for assignment to interviewers to measure certain components of interviewer variance present in the data. Concurrent with the data collection, a monitoring procedure was conducted in which a supervisor listened to the interview and coded each interviewer behavior according to whether it conformed to techniques and procedures in which the interviewers had been trained.

The training for the interviewers specified that they were to read the questions in the instrument exactly as they appeared, with no changes in wording. Interviewers were coached to read the questions slowly, at a pace of about two words per second. In addition, they were given explicit instructions regarding the use of probing for incomplete answers by respondents. Finally, the interviewers were trained in voice techniques that were thought to convey a desirable professionalism. All of these rules grew out of conclusions from past experimental tests of alternative interviewer procedures. Supervisory reviews of interviewers, concerning the quality of individual interviewer's work, were based on the conformity to these procedures. There is a strong belief among survey researchers that one source of interviewer variability can be controlled through standardized reading of questions, consistent probing procedures, and controlled feedback by the interviewers to the respondents. This belief is manifested by nearly all the interviewing guidelines used in training interviewers.

The monitoring procedures in this study were an integral part of the interviewer evaluation used to measure how closely the interviewer followed the training guidelines. The design of the research project, containing both an interpenetrated design for interviewer assignment and monitoring procedures, allows investigating whether the rules prescribed for interviewer behavior are related to the magnitude of interviewer variability about the overall survey statistics. The intent, then, is to integrate the two different literatures to judge whether guidelines for assessing interviewer procedures are closely related to measures of interviewer variance. If it is found that the amount of interviewer variability is related to specific violations of training guidelines, then new training procedures can be developed to reduce that source of error. If, on the other hand, violations of prescribed interviewer behavior result in no unusual interviewer effects, training procedures should be reevaluated. This study is one of the first attempts to correlate a measurable source of response variance to the definition of correct interviewer behavior as judged by training instructions.

Introduction to monitoring

There is abundant evidence[6,7] that both personal and telephone interviews are often distorted as a result of an interviewer's behavior. Failure to read a question exactly as printed, inability to follow skip patterns correctly, and reading a question too fast all contribute to errors in responses. Most persons working with interviewers are aware of the need for supervision to maintain the quality of an interviewer's performance. Although time restrictions and lack of appropriate techniques severely limit the amount of supervision of personal interviews, the increased use of centralized telephone operations has given researchers greater ability to monitor an interviewer's performance. Many survey organizations monitor telephone interviews, listening to interviewers and noting problems as they occur. However, monitoring in the past often has followed ad hoc procedures, lacking both a structure for sampling interviews and a procedure for recording errors. Systematic evaluation of interviewers can be accomplished by identifying the major categories of interviewer behavior and classifying each behavior as correct or incorrect, according to the concepts and training guidelines for that particular study.

Behavior codes

The coding system used in this project is a revision of a more extensive system used for coding personal interviews.[8] The system is quite flexible and can be adapted to the purposes of a particular study. The codes shown here (see the figure) were developed specifically for this methodological study. The study was different from most surveys in that feedback was programmed into the questionnaire and interviewers were restricted in the manner and amount of probing they were allowed to do. The codes reflect these restricted interviewing behaviors. Objective and subjective measures of the interviewers' behavior were taken. The 10's, 20's, 30's, 40's, and 50's codes were used to identify concrete behavior or lack of behavior and to determine whether its occurrence was correct and appropriate. The 60's and 70's codes require that the monitor evaluate the quality of the delivery. These evaluations in terms of pace and clarity can be associated with the reading of a question, defining, clarifying, probing, or delivering feedback.

Reliability among the monitors prior to production monitoring reached an 85-percent level for each of the codes in the 10–50's categories and a 75-percent level for each of the codes in the 60's and 70's levels. These percents reflect the overall agreement between the four monitors and the instructor for each of the codes.

Sample selection

The sampling scheme used to evaluate interviewers is a compromise between a system based completely on a random sample of *interviews* and one that is based on sampling the *interviewers*. The goals were to (1) have the sample of monitored interviews be close to an equal probability design, and (2) to monitor and give feedback to each interviewer twice a week. Selection of interviews to be monitored was based on an equal probability sample. A chart with each interviewer's name and dates of feedback was kept. If, toward the end of a week, an interviewer had not been monitored at least twice, extra effort was taken to evaluate him or her. Due to the unequal and incalculable probability of selection, these evaluations are not included in the analysis presented here.

Analysis of monitoring data

The focus of the analysis is to determine whether interviewers differ in correct and incorrect use of techniques among themselves and whether this variance differs across questions. Although variance measures could be presented for each major category of behavior (question asking, probing, feedback, and so forth), the discussion is limited to two behaviors: (1) question reading and (2) clarity and pace of question delivery. This is done for two reasons. First, interviewers spend the majority of their time asking questions. Only 8 percent of all questions asked required that the interviewer clarify the question or probe for a more complete response. Feedback was given for only 23 percent of the responses. Second, because in this study specific feedback statements were programmed into the questionnaire, there is little or no variation among interviewers in their use of feedback.

Table DD presents a summary comparison of interviewer variation in question delivery for 15 dependent variables. Even with the special emphasis given to training interviewers for this study and constant feedback given to them throughout the study, interviewers showed significant variation in their reading of a number of questions. Although some questions, for example, dentist visits and time since last dentist visit, show significant variation among interviewers in the proportion of times they read the question correctly based on a one-way analysis of variance F-statistic ($p < 0.05$), it should be noted that the overall mean proportion correct is very high (0.979 and 0.917, respectively). The low mean proportion of correct readings for phone calls to doctors, 12-month doctor visits, and chronic con-

Type of interviewer behavior	Code	Explanation of code
Question asking or repeating question	11	Reads question exactly as printed
	12	Reads question incorrectly—minor changes
	15	Reads question incorrectly—major changes
	17	Fails to read a question
	18	Reads inappropriate question (due to prior miscode)
Probes	21	Probe used correctly
	25	Probe used incorrectly (unnecessarily or incorrectly)
	27	Fails to use probe
Defining or clarifying	31	Clarifies or defines correctly
	35	Defined incorrectly
	37	Fails to define
Short feedback	41	Delivers short feedback—correctly
	45	Delivers short feedback—incorrectly or inappropriately
	47	Fails to deliver short feedback
Long feedback	51	Delivers long feedback—correctly
	55	Delivers long feedback—incorrectly or inappropriately
	57	Fails to deliver long feedback
Pace or timing	65	Reads item too fast or too slow
	66	Timing between items improper (too slow or too fast)
Overall clarity	75	Unnatural manner of reading (poor inflection, exaggerated or inadequate emphasis, "wooden" or monotone expression)

Codes for monitoring interviewer behavior by type of behavior

Table DD. Proportion of questions read correctly and read well and values of ρ^*_{int} by type of question

Type of question	Number of observations	Read correctly[1]		Read well[1,2]	
		Proportion	ρ^*_{int}	Proportion	ρ^*_{int}
2-week bed days	211	0.963	−0.0102	0.929	[3]0.0834
2-week work loss days	214	0.897	[3]0.2345	0.883	[3]0.1516
2-week cut-down days	315	0.838	0.0620	0.895	[3]0.0858
2-week doctor visits, person section	236	0.881	[3]0.0760	0.869	[3]0.2291
Doctor visits, supplements	742	0.849	0.0380	0.889	[3]0.1526
12-month hospital episodes	288	0.903	0.0293	0.910	[3]0.1297
2-week phone calls to doctor	246	0.781	[3]0.0700	0.902	[3]0.1063
2-week dentist visits	241	0.979	[3]0.0822	0.938	[3]0.0676
12-month doctor visits	260	0.719	0.0448	0.912	[3]0.1635
12-month bed days	255	0.878	−0.0096	0.902	[3]0.2061
Time since last doctor visit	221	0.928	0.0131	0.919	0.1046
Time since last dentist visit	217	0.917	[3]0.0820	0.885	0.0613
Mean number of acute conditions	172	0.820	−0.0554	0.930	[3]0.1863
Mean number of chronic conditions	236	0.674	0.0136	0.970	[3]0.0878
Health status	253	0.957	−0.0199	0.968	−0.0315

[1]Where ρ^*_{int} (see chapter III) is the intraclass correlation for monitored behavior.
[2]Correct pace, clear speech.
[3]Significant at $p < 0.05$.

ditions indicates that all interviewers had difficulty reading the questions. This suggests that the questions are poorly worded.

Response error model and estimators used for parameters of the model

The response error model used in this analysis is one that views the answer obtained from the respondent as subject to error, a deviation from the actual value for the respondent. For example, for the number of doctor visits for the jth respondent, the answer given by the jth respondent to the ith interviewer, may be expressed as follows:

$$X_{ij} = X_j + d_{ij}$$

where X_{ij} = the number of doctor visits reported to the ith interviewer by the jth respondent

X_j = the expected number of visits for the jth respondent

d_{ij} = the response deviation committed by the jth respondent in answer to the ith interviewer

The expected value of the respondent reply is that average value obtained over all possible repetitions of the questions by all interviewers. This calculation eliminates any biases that may result from procedures used by all interviewers.

Of particular interest is the pattern of deviations d_{ij} that occur among respondents who were interviewed by the same person, that is, the correlation of the d_{ij} within interviewers. Correlation among the respondent deviations will be viewed as interviewer effects.

This model was first examined by Hansen, Hurwitz, and Bershad[2] and then elaborated by Fellegi.[1] It facilitates a comparison of response errors with sampling errors. Sampling *deviation* is defined as $S_j = X_j - X..$, the difference between the respondent's expected number of doctor visits and the mean number of doctor visits in the population. With this formulation, the total variance of the mean can be expressed as

$$\text{Var}(X..) = \frac{N-nk}{N-1}\frac{\sigma_s^2}{nk} + \frac{\sigma_r^2}{nk} \times [1 + (n-1)\rho_{\text{int}} + n(k-1)\rho'_{\text{int}}] + \frac{2(n-1)(N-nk)}{nk(N-n)}\alpha\sigma_s\sigma_r$$

where α = correlation of sampling deviations and response deviations within interviewers

n = sample size for an interviewer from a population of size N

k = number of interviewers

σ_s^2 = variance of sampling deviations

σ_r^2 = variance of response deviations

ρ_{int} = correlation of response deviations by the same interviewer

ρ'_{int} = correlation of response deviations of different interviewers

Ideally, an estimator of interviewer effects that can be compared across variables with different units of measurement should be used.[9,10] For this reason, use of ratios of correlated response variance to total variance[3] or F-statistics[11,12] was not attractive. Instead, the following estimator was used:

$$\left[\frac{1}{n.(k-1)}\sum n(X_{i\cdot} - X..)^2 - \frac{1}{n.(n.-1)k}\sum\sum(X_{ij} - X_{i\cdot})^2\right] \div \left\{\left[\frac{1}{n.(k-1)}\sum n_i(X_{i\cdot} - X..)^2\right.\right.$$

$$- \frac{1}{n.(n.-1)k} \sum\sum (X_{ij} - X_{i\cdot})^2 \Big]$$

$$+ \sum\sum \frac{1}{(n.-1)k} (X_{ij} - X_{i\cdot})^2 \Big\}$$

which is desirable because it is unit free. The expected value of this is approximately $\rho_{int}\sigma_r^2/(\sigma_r^2 + \sigma_s^2)$ as observed by Fellegi.[1]

An essential condition for estimation of the parameters in the interviewer variance models is the random assignment of telephone numbers to interviewers. This interpenetration provides each interviewer with a small national sample, thus removing the possibility of certain interviewers being consistently assigned to a particular type of respondent. In a telephone facility, the close physical proximity of the interviewers makes this randomization a relatively easy and inexpensive procedure.

On the first day of interviewing, all available cover sheets were divided into 40 groups (packs) of approximately equal size and placed in files numbered 1–40. Randomization was handled separately for the online and offline coversheets; that is, there were 40 online packs and 40 offline packs. As interviewers reported to work, supervisors assigned a pack of coversheets to each interviewer using a random number table. Using this system, each interviewer was initially assigned one pack. The remaining packs were to be used as a reserve. When an interviewer's pack no longer provided sufficient work during the interviewer's shift, he or she returned to the supervisor for assignment of a new pack from the reserve. When the reserve became sufficiently small, coversheets were rerandomized.

To rerandomize coversheets, supervisors were instructed as follows:

1. To collect all coversheets for one mode, online or offline, from the interviewers' files, except future appointments and refusal conversions that the interviewer will keep, and form a stack.
2. To take all replacement coversheets currently available.
3. To insert these replacement numbers at various points in the stack; that is, mix up the two types of coversheets.
4. To count the number of empty pack files, which will be less than 40.
5. To use a ruler to divide the stack of coversheets into packs of approximately equal size, the number of packs formed being equal to the number of empty pack files.
6. To place the packs in the pack files, in any order.

Following this, supervisors again began to assign packs randomly to interviewers as they reported to work. During the early part of each month, rerandomization took place at most once a day. As the study progressed and the size of the packs became smaller and smaller, it became necessary to rerandomize several times a day.

Interviewers were encouraged to make appointments with respondents at times when they themselves would be working and able to keep the appointment. However, when an appointment could not be kept by the original interviewer, it was randomly assigned to another interviewer. In a few cases, appointments were not randomly assigned. In addition, the study manager reviewed all the initial refusals and assigned them to different interviewers for the conversion attempt. Because of this nonrandom assignment, all interviews obtained as refusal conversions and some from appointments are deleted from the analysis presented here. Of the 8,210 interviews obtained in the study, 7,174 (about 87 percent) were from randomly assigned phone numbers and were used to obtain the estimates.

In this project, the same questions were asked for each adult member of each family within all sample households. Generally, one adult, selected in accordance with the respondent rules described in chapter I, served as the respondent for all members of the family. Consequently, in addition to the correlation of response deviations within interviewers, the ρ^*_{int} values calculated for the entire sample are affected by two sources of homogeneity: (1) the fact that persons in the same family tend to share some characteristics and (2) the fact that any response errors consistently committed by the family respondent will affect reporting for each family member. To eliminate this component of within-family homogeneity, the values of ρ^*_{int} presented in table N were calculated using only the random respondents.

Estimates of interviewer effects

Table EE presents the mean or proportion for 15 survey estimates of health status and the corresponding value of ρ^*_{int}. These statistics were calculated using only randomly chosen adults ($n = 1{,}918$) from the *random respondent* half-sample. This half-sample included control and experimental interviewing techniques used with both computer-assisted interviews and paper questionnaires.

The sampling distribution of the ρ^*_{int} values is known only under rather rigid conditions. The small number of degrees of freedom due to few interviewers affects the stability of ρ^*_{int}. This instability is reflected in the several negative ρ^*_{int} values that appear in the table. There is some evidence that the magnitudes of within-interviewer variance are not constant across interviewers, thereby violating the assumption of the underlying

Table EE. Means or proportions and corresponding values of ρ^*_{int} for selected health measures: Random respondents

Description of statistic	*Mean or proportion*[1]	ρ^*_{int}
No 2-week bed days	0.937	−0.0057
No 2-week work loss days	0.935	0.0008
No 2-week cut-down days	0.909	−0.0006
No 2-week doctor visits, person section	0.849	[2]0.0092
No doctor visits, supplements	0.831	[2]0.0081
No 12-month hospital episodes	0.869	−0.0000
No 2-week phone calls to doctor	0.972	−0.0020
No 2-week dentist visits	0.936	0.0040
12-month doctor visits (2–4 visits)	0.371	0.0002
No 12-month bed days	0.452	−0.0070
Time since last doctor visit between 2 weeks and 6 months	0.422	0.0004
Time since last dentist visit between 2 weeks and 6 months	0.331	0.0018
Mean number of acute conditions	0.173	0.0004
Mean number of chronic conditions	0.516	[2]0.0097
Health status excellent	0.418	[2]0.0085

[1]1,918 respondents.
[2]Significant at $p < 0.05$.

linear mode. For heuristic guidance, the subscripted values of the ρ^*_{int} values are those that are significantly different from zero, given the assumption of equal variances.

Table EE shows that the values of ρ^*_{int} for these estimates range from −0.0070 to 0.0097 with a mean value of 0.0021 and a median of 0.0008. Although these values seem quite small, it is useful to note that such a value must be multiplied by a function of the average interviewer workload to estimate the inflation in total variance due to interviewer effect. In this study, 33 were used to take the 1,918 random respondent interviews; thus, the average interviewer workload was 58.12 interviews. The approximate design effect, due to using 33 interviewers rather than a different interviewer for each respondent, is

$$\text{Deff}_{int} = 1 + \rho^*_{int}(58.12 - 1)$$

For a value of $\rho^*_{int} = 0.0097$ corresponding to a mean number of chronic conditions, $\text{Deff}_{int} = 1.55$. In other words, one might expect a 55-percent increase in the variance of this estimate due to interviewer effects alone. Using the average value of $\rho^*_{int} = 0.0021$, $\text{Deff}_{int} = 1.12$, a relatively small increase in variance on the average for these health variables. These small interviewer effects are rather startling given previous work.[9,10] However, a study of medical care utilization[13] in Saskatchewan, Canada, reports results similar to the present findings. Although the study found significant interviewer effects for many variables (for example, chronic conditions, and most nonfactual items related to the respondent's perception of his or her state of health), individual variables measuring utilization experiences (for example, 2-week doctor visits, 2-week bed days, and so forth) are generally free of significant interview variance.

Three differences are obvious when these results are compared to those of previous work:

1. The magnitude of interviewer variation is smaller in this study than in previous studies. Past studies have shown variation in interviewer effects based on question format, with open-ended questions sometimes suffering from larger interviewer effects. There is also some evidence that factual questions are subject to less interviewer effect than most attitudinal items. This study contained mostly factual questions about health-related events that would be well remembered by many respondents, for example, "Number of hospitalizations in the last year," and only a few that required complex recall tasks, for example, "How many times have you called a doctor in the last 2 weeks?" Therefore, the substantive topic and format of the questions may contribute to the overall low susceptibility of the measures to interviewer effects.
2. This study introduced an experimental interviewing procedure designed to increase response accuracy of survey results both by decreasing bias and correlated response variance. That is, it was expected that the procedures would reduce the overall tendency to underreport health events across all interviewers and standardize the interviewer behavior to reduce interinterviewer disagreement. The half-sample receiving the experimental interviewer treatment was compared to the complement half-sample in which interviewers were somewhat freer to probe incomplete responses. Even in this procedure, however, interviewers were more restricted in their behavior than, for example, the U.S. Bureau of the Census interviewers who administer the personal National Health Interview Survey questionnaire. For this reason, the lower interviewer effects are not unexpected.
3. The form of the distribution of mean values obtained by each interviewer differs from those of past studies. The previous telephone data yielded distributions of interviewer means that contain few outliers and had relatively smooth patterns about the overall survey mean. These data, however, have many measures in which one or two interviewers deviate markedly from the others. Over different statistics, the identity of the outliers varied. To evaluate the impact of these extreme deviations, the interviewer variability analysis was again performed for five statistics with high values of ρ^*_{int} eliminating these outliers. For the proportion reporting no 2-week doctor visits, one interviewer was eliminated. Two interviewers were dropped for each of the other four variables. The results of the reanalysis appear in table FF. For each variable, the measure of interviewer variability ρ^*_{int} is smaller, and for all variables these new values were not significantly greater than zero. In other words, one or two interviewers are responsible for most of the measured variability.

There are three possibilities that explain this phenomenon:

1. On these measures, most interviewer behavior will produce similar means for their respondent groups, but a small num-

Table FF. Means or proportions and values of ρ^*_{int} before and after elimination of outlying interviewers for selected health measures: Random respondents

Description of statistic	All interviewers			After elimination of outlying interviewers		
	Number of interviews	Mean or proportion	ρ^*_{int}	Number of interviews	Mean or proportion	ρ^*_{int}
No 2-week doctor visits, person section	1,918	0.849	[1]0.0092	1,882	0.853	0.0033
No doctor visits, supplements	1,918	0.831	[1]0.0081	1,798	0.842	−0.0054
Mean number of chronic conditions	1,918	0.516	[1]0.0097	1,800	0.518	0.0034
Health status excellent	1,918	0.418	[1]0.0085	1,818	0.419	0.0001
Time since last dentist visit between 2 weeks and 6 months	1,918	0.331	0.0018	1,824	0.328	−0.0036

[1]Significant at $p < 0.05$.

ber of interviewers will depart from the survey mean greatly. Thus, a similar finding in replications of this design is expected.

2. The one or two outliers are not expected in repetitions of the survey. They represent cases in the tails of the distribution for interviewer means and are unlikely to be found in another survey. Thus, a better estimate of the intraclass correlation due to interviewers is obtained by deleting the outlying cases.
3. The outliers are interviewers with low response rates or very different response rates from most and, thus, they are attributable to a confounding of nonresponse bias and response error.

These various hypotheses cannot be tested without a replication of the survey, but the identities of the outlying interviewers vary over measures. That is, the same interviewers are not consistently outliers on all measures. For that reason, the outliers are not uniformly those with higher or lower response rates. This variation over measures in the identity of outlying interviewers appears to dismiss effectively the hypothesis of nonresponse bias as explanation for the outliers. It also threatens the speculation that this pattern would not occur in replications of the survey, because the variability is not a function of only one or two interviewers.

Correlates of interviewer variation

One method of examining the nature of response differences across interviewers uses the interviewers as the unit of analysis. At that level of aggregation, correlates of variability in interviewer means or in the deviation of individual interviewer means from the overall study value can be examined. The hypothesis is that this deviation is related to interviewer behavior and that this behavior can be measured through a monitoring process.

To examine the relationship between monitored behavior and variability, five statistics in table FF with large values of ρ^*_{int} were considered: (1) proportion reporting no 2-week doctor visits (from person section of the questionnaire), (2) proportion reporting no physician visits (from supplements), (3) mean number of chronic conditions, (4) proportion reporting health status as excellent, and (5) proportion reporting 2 weeks to 6 months since their last dental visit. For each of these variables, scatterplots were created of the squared deviation of the individual interviewer's means from the study mean by two monitoring variables, proportion of time the question was read correctly and proportion of time the question was read well. Of the 33 interviewers available, 30 were monitored over several occurrences of each question. Examination of these scatterplots showed no significant trend.

Because monitored behavior did not prove to be a good predictor of interviewer variability, other interviewer characteristics thought to measure performance were considered. Response rate, size of workload, hours per interview, and number of hours worked on the study were plotted against the interviewer's squared deviations on the five dependent variables described. Again, no apparent relationships between any one of these variables and interviewer deviations were found. The so-called "better" interviewers did not deviate any more or any less from the overall mean than the other interviewers did.

In one last attempt to explain the variability found among interviewer means, mean values of interviewers' squared deviations for categories of several variables used to evaluate each interviewer's performance were examined. These included such things as cooperation, efficiency, commitment to quality and standards, question-asking ability, speech and pace, and eliciting respondent participation. For each variable, interviewers were rated by their supervisors on a five-point scale ranging from poor to excellent. In all, 13 variables were examined, again under the hypothesis that interviewers with poor performance ratings would have larger deviations. This hypothesis was not supported; no pattern of interviewer rating and size of deviation emerged.

Summary

The most important finding of this section is that unusually low levels of interviewer effects were measured in the telephone survey. This result may be due to the stringent controls on interviewer behavior that were introduced in this study but were absent in past studies. These low interviewer variances inhibited attempts to explain interviewer variability on the health variables. Because there was little interviewer variability, the correlates among the monitoring data were weak, and the findings did not exhibit consistency over variables.

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Chapter VII
Nonsampling bias and variance in the SRC Telephone Survey data

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Introduction

This chapter presents data that interrelate two components of survey error: response bias and interviewer variance. As seen in separate analyses of these factors in chapter III, different interviewing procedures are associated with differences in the level of reporting of health events, and the variance in health measures is also affected by interviewer behavior. The discussion to follow combines these two concerns in asking the question, "Do interviewing techniques that appear to affect response bias also affect interviewer variance?"

Background

The study was designed to measure some components of the total error to which telephone survey data are subject. The measurement of these components was accomplished by the creation of experimental variation in treatments assigned to sample households, for example, the different interviewing treatments and respondent rules, as well as by the interpenetration of assignment of sample cases to the interviewers. Throughout the analysis of the data, the tendency to net underreporting of health events has been assumed to justify preference for methods that produce higher reports of health events. This assumption was defended through reference to record check studies that demonstrate such net underreporting and qualified through reference to studies that question the inference from record checks. (See chapter IV on respondent selection.)

The measurement of error focused in one important example on a component of response bias that may be affected by the behavior of the interviewer during the questioning of the respondent (chapter III). The design assigned two types of interviewer behavior to random half-samples, one type that attempted to simulate the behavior of U.S. Bureau of the Census interviewers and another type that used three experimental interviewing techniques: commitment, instructions, and feedback. Comparisons of statistics estimated on the control half-sample with those on the experimental half-sample were used to estimate the reduction in bias associated with the experimental techniques. The comparisons of the two half-samples thus provide estimates of relative bias associated with interviewer behavior, as judged from the two alternative techniques.

In addition to the experimental variation in interviewer behavior, the design included a random assignment of cases to interviewers. Given this random assignment, intraclass correlations associated with interviewer differences on statistics of interest have been estimated. Estimates of these interviewer effects can be obtained both for the entire sample and for the experimental half-samples. One use of these parameters is in a model of the design effect for a given interviewer workload size:

$$\text{Deff}_{\text{int}} = 1 + \rho^*_{\text{int}}(b - 1)$$

where ρ^*_{int} = intraclass correlation associated with interviewers

b = average workload of interviewers

In the expression for the total mean square error associated with survey statistics, these two components, one measured from the experimental variation of interviewer behavior and the other from the interpenetration of interviewer assignments, appear as follows:

$$\text{MSE}(\bar{y}_{..t}) = \frac{N - nk}{N - 1}\frac{\sigma_s^2}{nk} + \frac{\sigma_r^2}{nk} \times [1 + (n - 1)\rho_{\text{int}} + n(k - 1)\rho'_{\text{int}}] + \frac{2(n - 1)(N - nk)}{nk(N - n)}\alpha\sigma_s\sigma_r + (\bar{y}_{..} + B_t - \bar{Y}_{..t})^2$$

where B_t = effect of the tth interviewer treatment

$\bar{Y}_{..t}$ = mean over interviewers and respondents for the tth interviewer treatment

$\bar{y}_{..}$ = mean over treatments, interviewers, and respondents

(The remainder of the terms have been defined in chapter VI.)

Looking at this view of the relationship between the total survey error and the design aspects of this project, one interest was to examine the results to search for any relationship between the ability of the experimental interviewing procedures to affect response bias and the values of the interviewer variance for the two half-samples. Did the experimental interviewing procedures reduce response bias at the cost of increasing the magnitudes of interviewer variance? This chapter investigates that question by combining the results from the comparison of statistics on the two experimental groups with the changes in values of intraclass correlations for the same statistics. If it is found that statistics that are greatly affected by the experimental interviewing procedures also demonstrate a relative

increase in the value of the intraclass correlation for the experimental group relative to the control group, then the experimental techniques may be reducing bias but increasing interviewer variance. An increase in the mean square error might actually occur despite the decrease in bias.

Ideally, this analysis would include actual estimates of response bias for both of the interviewing behaviors and values of total variance for both of the modes. In this case, the value of the mean square error could be compared for the two groups to decide which group is preferable for the various statistics of interest. Here, however, an estimate of relative change in bias due to the change in interviewer behavior is used and related to some comparison of the intraclass correlations for the two groups.

Values of interviewer variance and relative bias associated with control and experimental interviewing behaviors

Table GG presents a comparison of the control and experimental version of the questionnaire on several different statistics. The third column in the table presents the magnitude of the difference between the two versions. The last two columns present the two different values of the intraclass correlation associated with interviewers corresponding to the two questionnaire versions. Table GG demonstrates, as seen in other chapters, that across the different variables, the most frequent result is that the level of reporting of health-related events is increased in the experimental version of the questionnaire. For example, there is a 4-percent increase in the percent reporting at least one bed day in the last 12 months in the experimental version of the questionnaire. Given the assumption of tendencies to underreport such health events, this result implies a reduction in reporting bias for the experimental interviewing procedures. There is also some tendency for the experimental version of the questionnaire to exhibit larger magnitudes of interviewer effects, as 10 of 15 proportions show increases in the experimental version. Such increases would indicate greater variance for the statistics due to interviewer differences.

Relationships between relative bias and interviewer variance associated with interviewer behavior

The results presented in table GG can be transformed by simple functions to permit the examination of a possible rela-

Table GG. Estimates of means or proportions and interviewer variability for the control and experimental interview groups, by selected health characteristics

Characteristic	Mean or proportion			Interviewer variability	
	Control[1]	Experimental[2]	Difference	Control	Experimental
Reporting 1 or more					
2-week bed days	0.055	0.068	0.013	0.0066	−0.0085
2-week work loss days	0.055	0.073	0.018	0.0031	0.0013
2-week cut-down days	0.076	0.103	0.027	−0.0048	−0.0094
2-week doctor visits, person section	0.138	0.167	0.029	−0.0095	0.0096
2-week doctor visits, supplements	0.169	0.168	−0.001	0.0026	0.0064
12-month hospital episodes, person section	0.137	0.159	0.022	−0.0033	0.0015
12-month hospital episodes, supplements	0.134	0.122	−0.012	−0.0092	−0.0027
2-week phone calls to doctor	0.027	0.027	0.000	−0.0066	−0.0057
2-week dentist visits	0.066	0.066	0.000	0.0068	−0.0021
Limitation of activity					
Some limitation	0.215	0.293	0.078	−0.0104	0.0063
Conditions					
Mean number of acute	0.168	0.179	0.011	0.0097	0.0129
Mean number of chronic	0.435	0.564	0.129	0.0204	−0.0021
Mean number of serious	0.206	0.240	0.034	0.0100	−0.0138
Mean number of threatening	0.117	0.129	0.012	−0.0011	−0.0098
Health status					
Not excellent	0.563	0.601	0.038	0.0030	0.0228
Mean number of—					
2-week bed days	0.205	0.216	0.011	−0.0071	−0.0064
2-week work loss days	0.163	0.332	0.169	−0.0051	−0.0000
2-week cut-down days	0.280	0.452	0.172	0.0014	−0.0095
2-week doctor visits, person section	0.190	0.231	0.041	−0.0140	0.0086
2-week doctor visits, supplements	0.233	0.251	0.018	−0.0086	−0.0070
2-week dentist visits	0.085	0.088	0.003	0.0057	−0.0020
12-month hospital episodes, supplements	0.159	0.161	0.002	−0.0085	−0.0065

[1]961 respondents.
[2]912 respondents.

tionship between the bias reduction due to the experimental interviewing behaviors and the change in components of variance due to the interviewer. The difference between the control and experimental statistics is used as a measure of relative bias reduction due to the experimental treatment. This quantity is squared to represent the appropriate change in the bias component to the total mean square error. For binomial variables, the squared difference is used; for continuous variables the squared difference is divided by the element variance to adjust for different measurement units across variables. The two intraclass correlations presented in table GG are compared by transforming them into design effects due to the interviewer:

$$\text{Deff}_{\text{int}} = 1 + \rho^*_{\text{int}}(28.38 - 1)$$

Then the design effect associated with the experimental group is compared with that of the control group by evaluating the fraction ($\text{Deff}_{\text{int}}$for control)/($\text{Deff}_{\text{int}}$for experimental).

Tables HH and JJ present estimates of squared relative bias associated with different interviewing techniques and design effects. Table HH contains information on the statistics that are proportions, for example, proportion reporting 1 or more work loss days. Comparing the last two columns of the table permits measurement of the relationship between the relative bias terms and the relative interviewer variance values. For example, the squared difference between the experimental and control estimate of the proportion of persons with 1 or more work loss days is 0.32; the ratio of the design effects due to interviewer variability is 1.05. In examining the column, one

Table HH. Relative design effects due to interviewer variance for the control and experimental interview groups, and squared relative bias, by selected health characteristics

Characteristic	Interviewer design effects			Squared relative bias[1]
	Control	Experimental	Control to experimental ratio	
Reporting 1 or more				
2-week bed days	1.18	0.77	1.54	0.17
2-week work loss days	1.08	1.04	1.05	0.32
2-week cut-down days	0.87	0.74	1.17	0.73
2-week doctor visits, person section	0.74	1.26	0.59	0.84
2-week doctor visits, supplements	1.07	1.18	0.91	0.00
12-month hospital episodes, person section	0.91	1.04	0.87	0.48
12-month hospital episodes, supplements	0.75	0.93	0.81	0.14
2-week phone calls to doctor	0.82	0.84	0.97	0.00
2-week dentist visits	1.19	0.94	1.26	0.00
Limitation of activity				
Some limitation	0.72	1.17	0.61	6.08
Health status				
Not excellent	1.08	1.62	0.67	1.44

[1]Squared difference between estimated proportions from experimental and control groups.

Table JJ. Relative design effects due to interviewer variance for the control and experimental interview groups, and ratio of squared relative bias to element variance, by selected health characteristics

Characteristic	Interviewer design effects			Relative bias to element variance ratio
	Control	Experimental	Control to experimental ratio	
Conditions				
Mean number of acute	1.27	1.35	0.94	0.63
Mean number of chronic	1.56	0.94	1.65	22.90
Mean number of serious	1.27	0.62	2.05	3.55
Mean number of threatening	0.97	0.73	1.33	1.06
Other measures				
2-week bed days	0.81	0.82	0.98	0.08
2-week work loss days	0.86	1.00	0.86	14.04
2-week cut-down days	1.04	0.74	1.40	9.99
2-week doctor visits, person section	0.62	1.24	0.50	4.38
2-week doctor visits, supplements	0.76	0.81	0.95	0.72
2-week dentist visits	1.16	0.95	1.22	0.06
12-month hospital episodes, supplement	0.77	0.82	0.93	0.02

sees that there is no simple relationship between the relative bias terms and the interviewer variance ratios. A very similar picture is provided when data are presented on continuous variables in table JJ. In short, there is no evidence that differences in sample estimates obtained by the experimental version of the question are associated with greater or less interviewer variability. There are cases where the experimental behavior leads to no different point estimates but increases interviewer variance. There are other cases where there are large differences in point estimates and lower interviewer variance in the experimental group. The only result in this comparison that would have provided unambiguous information would have been large increases in reporting using the experimental version found together with decreases in interviewer variance. This result would have indicated joint reduction of response bias and interviewer variance with the experimental techniques. This is not the case. Without further data collection, it cannot be determined whether the *net* effect, considering both interviewer variance and response bias, of the experimental treatments is the increase or decrease of mean square error of the estimates.

Summary and conclusions

This small exercise was a check on the nature of the effects of the experimental interviewing behaviors, one that attempted to dismiss the possibility that the increases in reporting were coming at the expense of greater interviewer variance. The findings presented are limited by the small number of statistics examined, but they suggest that the possibility that the experimental behaviors merely move errors from bias terms to variance terms is unlikely to be experienced. It is also unlikely that the experimental effects do not uniformly decrease interviewer variance and they appear to be reducing response bias. The measurement of the net effect of the treatments on mean square error requires estimates of the relative size of total response variance composed both of interviewer and respondent variability and response bias associated with the experimental procedures.

Appendixes

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Appendix I
Effect of postsurvey adjustment[b]

Published statistics from the National Health Interview Survey (NHIS) are not based on simple aggregates of the answers of individual respondents. Rather, the statistics are complex arithmetic combinations of respondents' values on individual measurements. Many of the complexities in the estimators arise through various postsurvey adjustments that are applied to the data. This appendix first outlines the postsurvey adjustments typically performed for the NHIS data, then reviews the procedures used for adjustment of the Survey Research Center (SRC) Telephone Survey data—adjustments for unequal probabilities of selection, for nonresponse, for noncoverage, and finally, for poststratification to improve the precision of the SRC Telephone Survey estimates. For each of these adjustments, this appendix examines the effect on the estimates and on the differences between the NHIS results and the SRC Telephone Survey results.

There are several reasons to suspect that the postsurvey adjustments might affect the comparisons between the SRC Telephone Survey estimates and the NHIS estimates. The area probability sample used by NHIS differs in the following ways from the telephone sample used in this project: (1) The coverage of the household population is greater for the area probability sample, about 97 percent, than for the telephone sample, about 93 percent; (2) the sample clusters consist of counties or county groups in NHIS, but clusters of 100 consecutive numbers in the same prefix in the telephone sample; (3) the stratification introduced before the selection of the NHIS sample is more complex than that possible in the telephone sample; and (4) the size of the sample for NHIS is over twice that obtained in the telephone survey. Further, the response rate obtained in NHIS, about 96 percent, greatly exceeds that obtained in the telephone survey, about 80 percent.

All of these characteristics affect the accuracy of statistics computed on the samples. Some of the effects of differences can be measured, for example, altered sampling variance because of change in cluster definition, but others cannot, for example, bias due to noncoverage and nonresponse in the telephone sample. Various postsurvey adjustments of the data are typically used in hopes of reducing these errors.

Postsurvey adjustments in the NHIS

The processing of NHIS survey data involves several different steps for editing, imputation, construction of new variables, and postsurvey adjustments. This section reviews effects on the estimates of four different adjustments: (1) inflation by the reciprocal of the probability of selection, (2) nonresponse adjustment, (3) first-stage ratio adjustment, and (4) poststratification. The NHIS sample is a self-weighting sample of housing units, thus, a constant inflation factor for all persons is used for estimation of population totals. The nonresponse adjustment is applied on a sample segment level and simply inflates the achieved segment total for a particular variable by the reciprocal of the response rate in the segment. The first-stage ratio adjustment attempts to reduce the effects of variability among primary sampling units (PSU's) in a region by using the later census population counts for region-residence-race groups to weight the survey results. The poststratification uses age-sex-race group population totals from the most current U.S. Bureau of the Census estimate.

Following the format of *Estimation and Sampling Variance,* NCHS Series 2, Number 38, the overall estimator used for the NHIS can be constructed in three steps:

1. Nonresponse adjusted estimate:

$$\hat{x} = \sum_a \sum_c \sum_i \sum_h \sum_k w_{ih} x_{ihkca} \frac{n_{ik}}{n'_{ik}}$$

$$= \sum_a \sum_c \sum_i \sum_h \sum_k w'_{ih} x_{ihkca}$$

where $\hat{x}$ = nonresponse adjusted estimate of health characteristic x

w_{ih} = weight of the hth person in the ith PSU; reciprocal of the product of the probabilities of selection for PSU, segment, and household

x_{ihkca} = measure of health characteristic x of the hth person in the kth segment of the ith PSU belonging to the cth region-residence-race class and the ath age-sex-race class.

n_{ik}/n'_{ik} = nonresponse adjustment

n_{ik} = the number of sample households in the kth segment of the ith PSU

n'_{ik} = the number of interviewed households in the kth segment of the ith PSU

$w'_{ih} = w_{ih} n_{ik}/n'_{ik}$

[b]Prepared by Robert M. Groves, Ph.D., and Lou J. Magilavy, M.P.H., Survey Research Center, Institute for Social Research, University of Michigan.

2. Nonresponse first-stage ratio adjusted estimate:

$$x' = \sum_a \sum_c \frac{\sum_i \sum_h \sum_k w'_{ih} x_{ihkca}}{\sum_i P_i Z_{ci}} Z_c$$

$$= \sum_a \sum_c \frac{x'_{ac}}{Z'_c} Z_c$$

where x' = nonresponse first-stage ratio adjusted estimate of health characteristic x

$$x'_{ac} = \sum_i \sum_h \sum_k w'_{ih} x_{ihkca}$$

Z_c/Z'_c = first-stage ratio adjustment

Z_c = 1970 census population in the cth region-residence-race class

$$Z'_c = \sum_i P_i Z_{ci}$$

Z_{ci} = 1970 census figure for the cth region-residence-race class of the ith PSU

P_i = reciprocal of the probability of selecting the ith PSU

3. Final poststratified estimate:

$$x'' = \sum_a \frac{\sum_c (x'_{ac}/Z'_c) Z_c}{\sum_c (Y'_{ac}/Z'_c) Z} Y_c$$

where x'' = nonresponse two-stage ratio adjusted estimate of health characteristic x

$$\frac{Y_a}{\sum_c (Y'_{ac}/Z'_c) Z}$$ = poststratified adjustment

Y_a = independent control of population count in the ath age-sex-race class

$$Y'_{ac} = \sum_i \sum_h \sum_k w'_{ith} Y_{ihkca}$$

= nonresponse adjusted estimate of the population in the acth class

Y_{ihkca} = 1 if the hth person in the kth segment of the ith PSU falls in the acth class; 0 otherwise

Table I presents unadjusted and adjusted estimates of proportions of the population in various categories of 17 major variables from NHIS. The estimates are presented for both the total population and the population residing in telephone households. The unadjusted results are those that are estimated from the version of the data prior to final runs of the NHIS data; the adjusted figures include all of the weighting, probabilities of selection, nonresponse, first-stage ratio adjustment, and poststratification. Without any postsurvey adjustment, 92.31 percent of the adult respondents in telephone households had no 2-week bed days; with all adjustments made, 92.26 percent of the persons had no 2-week bed days. This is a very small difference, one with little practical significance for most purposes. A similar result applies for the percent of the total population, 92.24 percent unadjusted versus 92.19 percent adjusted. Indeed, such minor differences apply to all of the estimates presented in table I. No percent is changed by 1 percentage point or more through the adjustment procedures; most differ by less than 0.5 percentage point.

Some comment is warranted on the small differences associated with the complex postsurvey adjustments used in NHIS. Only the nonresponse adjustments, first-stage ratio adjustments, and the poststratification could possibly change the value of estimated percents like those in table I. The inflation for probabilities of selection, because it uses a constant factor, cannot alter the value of those statistics. Nonresponse adjustments will have effects that are proportionate to the variation in nonresponse rates over segments and the intersegment variability on survey variables. The overall nonresponse rate for NHIS is near 3 percent; therefore, large effects are unlikely on estimates of percents based on the total population. The effects of the first-stage ratio adjustments and the poststratification are reduced by the large number of primary areas in the sample. Thus, it is not unexpected that for statistics on the total population, NHIS postsurvey adjustments have small effects. It is to be expected that larger effects might be present on subclass statistics.

SRC Telephone Survey adjustments

Adjustments for unequal probabilities of selection

Because families in the SRC Telephone Survey were sampled through their household telephone numbers, each family had a probability of selection proportional to the number of telephone numbers in the household. If a household had two different telephone numbers, it had twice the probability of selection as a household with a single number. Table II illustrates that the problem of unequal probabilities is a small one. Only 4.3 percent of the respondents had more than one telephone number. To correct for these unequal probabilities in estimates of means and proportions, the reciprocals of the number of phone numbers were used as weights, that is, 95.7 percent of the sample records received a weight of 1, 3.8 percent received a weight of ½, and so forth. It is unlikely that such a distribution of weights by themselves could have important effects on survey estimates for the total population.

Adjustments for nonresponse

The response rate for the SRC Telephone Sample was much lower than that for the NHIS sample. The policy of making no adjustment for nonresponse when estimating population means and proportions is based on the implicit assumption that nonrespondents have the same values on the health variables as respondents. There is no information available on the health characteristics of the nonrespondents, but using the assumption that the nonrespondents' characteristics are closer to the char-

Table I. Unadjusted and adjusted estimates of proportions or mean numbers of persons with selected health characteristics, for persons in telephone households and all households: National Health Interview Survey

Characteristic	Unadjusted estimate		Adjusted estimate	
	Total[1]	Telephone households[2]	Total[1]	Telephone households[2]
2-week bed days				
At least 1	7.8	7.7	7.8	7.7
2-week work loss days				
At least 1	4.5	4.6	4.6	4.6
2-week cut-down days				
At least 1	7.0	7.1	7.0	7.1
2-week doctor visits, person section				
At least 1	13.5	13.6	13.4	13.5
2-week doctor visits, supplements				
At least 1	13.9	14.0	13.8	14.0
12-month hospital episodes, supplements				
At least 1	12.5	12.3	12.4	12.3
2-week phone calls to doctor				
At least 1	2.0	2.0	2.0	2.0
2-week dentist visits				
At least 1	5.3	5.3	5.2	5.4
12-month doctor visits				
2–4	29.9	30.2	29.8	30.1
12-month bed days				
At least 1	46.3	46.1	46.4	46.3
Time since last doctor visit				
2 weeks–6 months	43.5	43.5	43.5	43.5
Time since last dental visit				
2 weeks–6 months	30.3	31.2	30.2	31.2
Limitation of activity				
Limited	18.9	18.7	18.8	18.6
Acute conditions				
Mean number	0.116	0.116	0.117	0.116
Chronic conditions				
Mean number	0.417	0.415	0.415	0.412
Number of operations				
At least 1	7.8	7.8	7.8	7.8
Health status				
Excellent	43.3	44.0	43.3	44.0

[1]19,800 respondents.
[2]18,388 respondents.

acteristics of respondents in the same sample cluster than they are to the total sample, an adjustment for nonresponse can be made by weighting cluster values by the reciprocal of the response rate in the cluster. This is essentially the same procedure used in NHIS on area segments. The clusters in the telephone sample are groups of 100 consecutive numbers in the same prefix. Generally, numbers in different prefixes of an exchange are distributed throughout the whole exchange area. Thus, the geographical clustering of 100 consecutive numbers is the same as that of any numbers in the same exchange. Telephone ex-

Table II. Distribution of weights for unequal probabilities of selection in the Survey Research Center Telephone Survey

Weight value	Proportion of sample receiving weight value
1/3	0.005
1/2	0.038
1.0	0.957

change areas cover spaces relatively homogeneous on population density, but the clustering effect and the intraclass correlation within clusters are expected to be smaller than with compact segments in an area probability design.

In all, 415 clusters were selected into the telephone sample. The overall response rate was about 80 percent, and the response rates in the individual clusters varied from 33 to 100 percent. Every response in each cluster was multiplied by the reciprocal of the response rate for the cluster, thus weighting the cluster mean to represent both respondents and nonrespondents. The distribution of these weights is presented in table III.

Adjustment for telephone noncoverage

Adjustment for noncoverage in the area probability sample of NHIS is largely a property of the final poststratification adjustment. Because the area probability method offers theoretically complete coverage of the household population, noncoverage arises from errors in field listing procedures. In contrast, the telephone number frame systematically excludes households without telephones, and noncoverage bias is a function of the magnitude of the nontelephone household population and of its distinctive features relative to the rest of the population. Differences, as observed by NHIS, between health characteristics for the total population and the telephone household population are shown in table I. As expected, these differences are quite small, primarily due to the fact that the telephone population forms over 93 percent of the total population. However, previous work[1,2] has noted that this high rate of telephone coverage is not constant over sociodemographic groups. In addition, health status varies over many of the same groups. Therefore, estimates of means and proportions should take into account these varying rates of noncoverage for different population groups.

Table III. Distribution of weights for nonresponse adjustment in the Survey Research Center Telephone Survey

Weight value	Proportion of sample receiving weight value
1.0	0.112
1.04–1.10	0.121
1.11–1.17	0.192
1.18–1.24	0.211
1.25–1.31	0.056
1.32–1.38	0.110
1.39–1.45	0.048
1.46–1.52	0.062
1.53–1.59	0.011
1.60–1.66	0.039
1.67–3.04	0.038

To make this adjustment, the population was divided into 48 cells based on age, education, and region of the country. Using NHIS estimates of telephone coverage, the weight for each cell was computed as the number of persons in the stratum population divided by the number of persons with telephones in the stratum population. This weight was then attached to each response. In effect, this adjustment weights up each cell mean proportional to the total population. This adjustment assumes that *within a particular cell* those persons without a telephone have the same health characteristics as those with telephones. Table IV presents the distribution of the weights.

Poststratification

Proportionate stratified sampling yields sample sizes that correspond to the actual population proportion in each stratum. However, often the stratification possible before selection cannot utilize the entire set of desirable stratifying variables either because they are not available for the sampling units used or because there is no cost efficient way to draw separate samples from different strata. This is especially true for samples of randomly generated telephone numbers because few characteristics of the persons assigned different telephone numbers are known before selection. Poststratification is a technique that can obtain some of the increases in precision that stratification offers. If the population can be classified into poststrata whose means on the variables of interest differ, the overall estimates can be improved by applying the population weight for each stratum to the mean of the sample stratum.

For the poststratification used in the SRC Telephone Survey, the sample was divided into 28 poststrata based on age, sex, and race classifications. The poststratum weight for each stratum was obtained from the Current Population Survey estimates. Each stratum was weighted by the number of persons in the stratum population divided by the total population. These weights were then incorporated into the calculation of the overall means and proportions. The distribution of these weights is shown in table V.

Results of postsurvey adjustments on SRC Telephone Survey data

Table VI presents the results of postsurvey adjustments on the SRC Telephone Survey data. Five columns of estimates

Table IV. Distribution of weights for telephone noncoverage adjustment in the Survey Research Center Telephone Survey

Weight value	Proportion of respondents receiving weight value
1.000–1.014	0.013
1.015–1.027	0.176
1.028–1.041	0.141
1.042–1.054	0.129
1.055–1.068	0.190
1.069–1.082	0.024
1.083–1.095	0.056
1.096–1.109	0.018
1.110–1.122	0.029
1.123–1.136	0.029
1.137–1.150	0.077
1.151–2.475	0.118

Table V. Distribution of poststratification weights for the Survey Research Center Telephone Survey

Weight value ($\times 10^{-2}$)	*Proportion of respondents receiving weight value*
0.036–1.652	0.116
1.653–3.268	0.038
2.269–4.884	0.074
4.885–6.500	0.256
6.501–8.116	0.152
8.117–9.732	0.364

are presented for 19 variables. The estimates include percents in the modal category of the variable and means calculated on the same measures. The first column presents the unadjusted estimate. The second column adjusts the first by weighting each household by the reciprocal of the number of telephone numbers attached to the housing unit. The third column adds the adjustment for nonresponse by weighting each data record by the reciprocal of the response rate in the cluster from which it was chosen. The fourth column then poststratifies the previous calculations by age, sex, and race proportions based on data from the Current Population Survey. The last column adds a further adjustment step before poststratification that weights race, education, and region groups by the reciprocal of their coverage by telephones as estimated from the NHIS data.

The adjustment for unequal probabilities of selection has little effect on the magnitude of the percents; most of the differences are below 0.1 percentage points. This is not surprising, because the percent of households with more than one telephone number is so small and, thus, that group would have to have large differences in its health characteristics to produce changes due to weighting for unequal probabilities.

The adjustments for nonresponse and unequal probabilities of selection have similarly small effects. Although the response rate was about 80 percent for the survey, the variability in response rates across clusters does not produce effects when the adjustment merely inflates cluster totals by the reciprocal of the response rate in the cluster.

The largest differences in the table are connected with the additional adjustment for age-sex-race groups in column 4. Here some of the percents change by as much as 3 percent. For example, the percent of persons with some physical limitation in day-to-day activities is 23.8 percent for the calculations adjusted for unequal probabilities of selection and nonresponse. When the poststratification step is added, the percent of persons with physical limitations decreases to 20.7 percent. Using the standard errors calculated on the telephone sample, such a difference would exceed two standard errors. Most of the percents that show important changes exhibit smaller proportions of persons with reported health events for the adjusted statistics. The poststratified estimate thus tends to estimate higher proportions of healthy persons as judged by these measures. However, the increase is not uniform across those variables exhibiting more than 1 percentage point change, and the majority of the percents do not show this large a difference.

The adjustment for noncoverage of telephones before the poststratification step does not seem to change the results obtained by poststratification alone. The percent estimates in the fourth and fifth columns of the table are typically within 1 percentage point of one another.

When NHIS and SRC Telephone Survey data before and after adjustment are compared by examining tables H and J of chapter II, it becomes clear that the differences between the two surveys are not altered systematically by the adjustment procedures. The NHIS adjustments do not move the percents estimated, and the SRC Telephone Survey adjustments do not change the majority of estimates examined. The few items that do show changes due to postsurvey adjustments in the SRC Telephone Survey data tend to reduce the difference between the NHIS results and the SRC Telephone Survey results.

References

[1]O. T. Thornberry and J. Massey: Correcting for undercoverage bias in random digit dialed national health surveys. *Proceedings of the American Statistical Association Section on Survey Research Methods,* 224–229, 1978.

[2]R. M. Groves and R. L. Kahn: *Surveys by Telephone: A National Comparison with Personal Interviews.* New York. Academic Press, 1979.

Table VI. Unadjusted and postsurvey adjusted estimates of proportions or mean numbers of persons with selected health characteristics: Survey Research Center Telephone Survey

Characteristic	*Unadjusted estimate*[1]	*Estimate adjusted for probability of selection*	*Estimate adjusted for probability of selection and nonresponse*	*Estimate adjusted for probability of selection, nonresponse, and age, sex, and race*	*Estimate adjusted for probability of selection, nonresponse, telephone coverage, and age, sex, and race*
2-week bed days					
At least 1	8.7	8.7	8.6	8.4	8.5
2-week work loss days					
At least 1	7.6	7.6	7.6	7.8	7.9
2-week cut-down days					
At least 1	9.8	9.8	9.8	9.9	9.9
2-week doctor visits, person section					
At least 1	15.9	15.9	15.9	14.8	14.9
2-week doctor visits, supplements					
At least 1	17.5	17.5	17.5	16.5	16.5
12-month hospital episodes, person section					
At least 1	14.4	14.5	15.5	14.1	14.2
12-month hospital episodes, supplements					
At least 1	12.9	13.0	12.9	12.7	12.7
2-week phone calls to doctor					
At least 1	3.5	3.4	3.4	3.5	3.5
2-week dentist visits					
At least 1	7.1	7.1	7.0	7.2	7.1
12-month doctor visits					
2–4	0.343	0.343	0.343	0.341	0.341
12-month bed days					
At least 1	54.0	54.0	53.8	56.7	56.7
Time since last doctor visit					
2 weeks–6 months	0.392	0.392	0.392	0.396	0.396
Time since last dentist visit					
2 weeks–6 months	0.337	0.337	0.337	0.360	0.359
Limitation of activity					
Limited	23.8	23.9	23.9	20.7	20.7
Acute conditions					
Mean number	0.183	0.183	0.182	0.194	0.194
Chronic conditions					
Mean number	0.473	0.475	0.472	0.412	0.412
Number of operations					
At least 1	5.3	5.3	5.3	5.3	5.3
Health status					
Excellent	0.418	0.416	0.415	0.438	0.436

[1]8,210 respondents.

Table VI. Unadjusted and postsurvey adjusted estimates of proportions or mean numbers of persons with selected health characteristics: Survey Research Center Telephone Survey—Con.

Characteristic	*Unadjusted estimate*[1]	*Estimate adjusted for probability of selection*	*Estimate adjusted for probability of selection and nonresponse*	*Estimate adjusted for probability of selection, nonresponse, and age, sex, and race*	*Estimate adjusted for probability of selection, nonresponse, telephone coverage, and age, sex, and race*
Mean number of—					
2-week bed days	0.290	0.292	0.291	0.247	0.250
2-week work loss days	0.300	0.296	0.296	0.278	0.282
2-week cut-down days	0.405	0.407	0.404	0.381	0.383
2-week doctor visits, person section	0.235	0.235	0.234	0.212	0.213
2-week doctor visits, supplements	0.257	0.256	0.256	0.238	0.239
2-week dentist visits	0.091	0.091	0.090	0.092	0.091
12-month hospital episodes, supplements	0.161	0.161	0.161	0.154	0.155

[1]8,210 respondents.

Appendix II
Estimates of sampling errors for alternative estimators[c]

Sampling errors on the SRC Telephone Survey estimates

Statistics from the SRC Telephone Survey are based on a two-stage stratified sample of telephone numbers, following the techniques of Waksberg.[1] Separate systematic samples of primary numbers are drawn from two strata, one which contains numbers that lie in exchanges containing only one central office code, the other which contains numbers that lie in exchanges of more than one central office code. The cluster size within the first exchange is one-half that in the second exchange, but the overall design is a self-weighting one, except for the 4 percent of sample households containing more than one telephone number, a factor which is discussed in chapter II but ignored in the unadjusted estimates presented in table VII. Sampling error computations for estimates of population means and proportions need to take into account the complex sample design and the form of the estimator employed.

For the unadjusted estimates of means and proportions, a Taylor series approximation for the variance of the ratio mean is used to estimate values of sampling errors:

$$\text{var}\left(\frac{\sum\sum y_{hi}}{\sum\sum x_{hi}}\right) = \frac{1}{\left(\sum\sum x_{hi}\right)^2}\left[\text{var}\left(\sum\sum y_{hi}\right) + \left(\frac{\sum\sum y_{hi}}{\sum\sum x_{hi}}\right)^2 \text{var}\left(\sum\sum x_{hi}\right) - 2\left(\frac{\sum\sum y_{hi}}{\sum\sum x_{hi}}\right) \times \text{cov}\left(\sum\sum y_{hi}, \sum\sum x_{hi}\right)\right]$$

where h = index for the stratum ($h = 1,2$)

i = index for a person

The value of the variance and the covariance is estimated using a successive differences computation:

$$\text{var}\left(\sum\sum y_{hi}\right) = \sum_{h=1}^{2} \frac{a_h}{2(a_h - 1)} \sum_{g=1}^{a_h - 1} (y_{hg} - y_{h,g+1})^2$$

These standard errors, presented in table VII, reflect the effects, that is, a loss in precision, of introducing clustering into the design.

For a select group of variables, table VII also presents estimates of standard errors for the adjusted estimates of means and proportions. Because the adjusted estimates are products of two random variables, the balanced repeated replication (BRR) method for calculation of the variance was judged to be simpler to use.

To implement the BRR estimation, a collapsed stratum technique that formed pairs from adjacent first-stage selections in the systematic sample was used. Then using an orthogonal coefficient matrix, the appropriate number of pseudoreplicate half-samples was formed. Then the variance was estimated as

$$\text{var}(\bar{z}) = \sum_{t=1}^{T} \frac{(\bar{z}_t - \bar{z})^2}{T}$$

where T = number of pseudoreplications,

$$\bar{z}_t = \frac{\sum\sum W_{hit} y_{hit}}{\sum\sum W_{hit}}$$

the estimate of the mean from the tth half-sample, and

$$z = \frac{\sum\sum W_{hi} y_{hi}}{\sum\sum W_{hi}}$$

Because of the unmanageable size of the coefficient matrix due to approximately 207 collapsed strata, these BRR variance estimates are calculated separately for the three independent samples (waves) used in this study. A weighted variance estimate is then used to combine these estimates. The overall estimate of the variance is calculated as follows:

$$\text{var}(\bar{y}) = \sum_{k=1}^{3} W_k^2 \text{var}(\bar{y}_k)$$

[c]Prepared by Lou J. Magilavy, M.P.H., and Robert M. Groves, Ph.D., Survey Research Center (SRC), Institute for Social Research, University of Michigan.

where $W_k = 1/p_h$, the probability of selection of the kth replicate

$\text{var}(\bar{y}_k) = \text{var}(\bar{z}_k)$ as obtained from BRR

h = a subscript referring to the three waves of the survey

Table VII shows that the adjustments have virtually no effect on the precision of the estimates. This is understandable in that the adjustments had no effect on the estimates of the means and proportions.

Estimating sampling errors for National Health Interview Survey (NHIS) personal interviews

To assess the importance of the difference between the estimates obtained in the telephone sample and those obtained by personal interview, an approximate average design effect for the NHIS sample was calculated using figure VII, Relative standard errors of percentages of population characteristics, presented in *Current Estimates From the Health Interview Survey: United States, 1977* (p. 50). Assuming a four-quarter sample of 120,000 persons (*Current Estimates,* p. 39) and 376 primary sampling units, an average cluster size, $b = 319.15$, was obtained. For proportions based on the total population (200,000,000), figure VII implies that a proportion equal to 0.5 has a standard error of 0.0020. For a simple random sample of the same size, the standard error is 0.0014 ($p = 0.5$). Given these estimates,

$$\text{Deff} = \frac{\text{Actual NHIS variance}}{\text{SRS variance}}$$

$$= 1.920$$

From Deff,

$$\rho = \frac{\text{Deff} - 1}{b - 1}$$

$$= 0.00289$$

Then, assuming ρ remains constant as sample size decreases, the design effect for the NHIS personal interview sample was 18,388. Here,

$$\text{Deff} = 1 + \rho(b - 1) = 1.1384$$

with $b = 18{,}388/376 = 48.90$. Estimates of standard errors using figure VII of *Current Estimates* and an NCHS estimate that 80 percent of the sampling variance is within sample clusters were also calculated. This method provided estimates of a comparable order of magnitude.

In table VIII, values of Deff for NHIS subclasses were again obtained by assuming $\rho = 0.00289$ and using various values of $b_{\text{sub}} = n_{\text{sub}}/376$, where n_{sub} is the number of respondent cases in the subclass.

Estimated standard errors for comparison of telephone and personal interviews

The calculation and presentation of sampling errors for each category of each variable by each subclass is not feasible. For this reason, table IX presents estimated standard errors based on average design effects for the two samples. The relative uniformity of Deff values for the dependent variables suggests that this averaging may be more appropriate for these variables.

Table IX may be used to judge the importance of the difference in estimates of proportions obtained by the telephone and personal interview samples. For a significant difference between an SRC estimate and an NHIS estimate at $\alpha = 0.01$, the following must hold:

$$\frac{p_{\text{src}} - p_{\text{his}}}{(\text{var}\, p_{\text{src}} + \text{var}\, p_{\text{his}})^{1/2}} \geq 2.58$$

For estimates around 0.9 on a dependent variable,

$$\frac{p_{\text{src}} - p_{\text{his}}}{[(0.0070)^2 + (0.0024)^2]^{1/2}} \geq 2.58$$

whenever the observed differences are greater than 0.019.

Reference

[1]J. Waksberg: Sampling methods for random digit dialing. *J. of the American Statistical Association* 73:40–46, 1978.

Table VII. Estimates of sampling errors, design effects, and intraclass correlations for unadjusted telephone estimates and sampling errors for adjusted estimates: Survey Research Center Telephone Survey

Characteristic	Unadjusted estimates				*Standard error estimates adjusted for probability of selection, nonresponse, and age, sex, and race*	*Standard error estimates adjusted for probability of selection, nonresponse, telephone coverage, and age, sex, and race*
	Value	*Standard error*	*Design effect*	*p*		
2-week bed days						
At least 1	8.7	0.31	0.969	−0.002	0.33	0.33
2-week work loss days						
At least 1	7.6	0.31	1.111	0.006	0.33	0.33
2-week cut-down days						
At least 1	9.8	0.35	1.167	0.009		
2-week doctor visits, person section						
At least 1	15.9	0.45	1.250	0.013	0.45	0.46
2-week doctor visits, supplements						
At least 1	17.5	0.50	1.416	0.022		
12-month hospital episodes, person section						
At least 1	14.4	0.39	1.037	0.002		
12-month hospital episodes, supplements						
At least 1	13.9	0.37	0.983	−0.001		
2-week phone calls to doctor						
At least 1	3.5	0.22	1.200	0.014		
2-week dentist visits						
At least 1	7.2	0.32	1.259	0.014	0.33	0.33
12-month doctor visits						
2–4	34.3	0.59	1.274	0.015	0.63	0.63
12-month bed days						
At least 1	54.0	0.70	1.608	0.032		
Time since last doctor visit						
2 weeks–6 months	39.2	0.57	1.122	0.006	0.56	0.57
Time since last dentist visit						
2 weeks–6 months	33.7	0.64	1.504	0.027		
Limitation of activity						
Limited	23.8	0.55	1.351	0.019		
Acute conditions						
Mean number	0.183	0.0059	1.473	0.025	0.0061	0.0061
Chronic conditions						
Mean number	0.473	0.0108	1.311	0.017	0.0109	0.0109
Number of operations						
At least 1	5.3	0.25	0.989	−0.001		
Health status						
Excellent	41.8	0.68	1.560	0.030		

Table VII. Estimates of sampling errors, design effects, and intraclass correlations for unadjusted telephone estimates and sampling errors for adjusted estimates: Survey Research Center Telephone Survey—Con.

Characteristic	Unadjusted estimates				Standard error estimates adjusted for probability of selection, nonresponse, and age, sex, and race	Standard error estimates adjusted for probability of selection, nonresponse, telephone coverage, and age, sex, and race
	Value	Standard error	Design effect	ρ		
Mean number of—						
2-week bed days	0.290	0.0148	0.896	−0.006	0.0140	0.0141
2-week work loss days	0.300	0.0192	1.159	0.008		
2-week cut-down days	0.405	0.0224	1.279	0.015		
2-week doctor visits, person section	0.235	0.0088	1.167	0.009	0.0081	0.0082
2-week doctor visits, supplements	0.257	0.0090	1.251	0.013		
2-week dentist visits	0.091	0.0045	1.158	0.008		
12-month hospital episodes, supplements	0.161	0.0053	0.963	−0.002	0.0056	0.0057

Table VIII. Estimated standard errors for proportions from National Health Interview Survey subclasses by magnitude of proportion and size of subclass

Value of proportion	Estimated standard error				
	N = 500	N = 1,000	N = 2,500	N = 5,000	N = 10,000
0.1, 0.9	0.010	0.0095	0.0061	0.0043	0.0031
0.3, 0.7	0.021	0.0146	0.0093	0.0066	0.0048
0.4, 0.6	0.022	0.0156	0.0099	0.0071	0.0051
0.5, 0.5	0.022	0.0159	0.0101	0.0072	0.0052

NOTE: *N* = number of respondents in subclass.

Table IX. Estimates of standard errors for proportions based on design effects for the Survey Research Center (SRC) Telephone Survey and the National Health Interview Survey (NHIS)

[Based on the average design effects of 1.523 for demographic variables, 1.158 for dependent variances from the SRC Telephone Survey, and 1.38 for NHIS]

Value of proportion	Estimated standard error	
	SRC Telephone Survey[1]	NHIS personal survey[2]
0.9, 0.1	0.0070	0.0024
0.8, 0.2	0.0094	0.0032
0.7, 0.3	0.0108	0.0036
0.6, 0.4	0.0115	0.0039
0.5, 0.5	0.0117	0.0039

[1] 2,099 respondents.
[2] 18,388 respondents.

Appendix III
Comparison of SRC telephone interviews (total sample) and NHIS face-to-face interviews

Tables X–XXVIII present weighted data from the Survey Research Center Telephone Survey and unweighted data from the National Health Interview Survey personal interviews for the adult civilian noninstitutionalized population. Sample weights are used to adjust for the unequal probabilities of selection. The SRC weight used to adjust for the existence of multiple telephone numbers in some households is equal to 1/(number of telephones).

Table X. Percent distribution of persons in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected demographic characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
Sex			
Male	46.7	46.3	45.8
Female	53.1	53.7	54.2
Unknown	0.2	0.0	0.0
Age			
17–24 years	18.3	19.4	18.5
25–34 years	22.9	22.1	21.7
35–44 years	16.8	15.7	15.9
45–54 years	14.9	14.2	14.6
55–64 years	13.4	13.6	14.0
65–74 years	8.3	9.6	9.8
75 years and over	4.1	5.3	5.4
Unknown	1.3	0.0	0.0
Race			
White	87.5	85.6	86.7
All other	12.5	14.4	13.3
Education			
0–8 years	11.0	14.3	13.5
9–11 years	15.7	16.9	16.2
12 years	36.0	37.0	37.4
13–16 years	28.2	23.7	24.6
17 years or more	7.0	6.1	6.4
Unknown	2.1	2.0	1.9
Income			
Less than $5,000	8.6	10.6	9.1
$5,000–$9,999	11.8	15.5	14.6
$10,000–$14,999	15.0	14.9	14.9
$15,000–$24,999	26.1	24.2	25.1
$25,000 or more	20.8	26.0	27.6
Unknown	17.8	8.9	8.8
Marital status			
Married	65.4	64.4	65.2
Widowed	6.7	7.7	7.8
Divorced	6.1	5.5	5.3
Separated	1.8	2.2	1.9
Single	20.1	20.3	19.8
Usual activity			
Working	59.5	57.7	58.0
Keeping house	24.0	23.9	23.8
Retired, health	2.0	2.5	2.4
Retired, other	3.8	4.9	5.0
Going to school	7.3	7.4	7.5
Something else	2.9	3.3	3.0
Unknown	0.5	0.3	0.3

[1] 8,210 respondents.
[2] 19,800 respondents.
[3] 18,388 respondents.

Table XI. Percent distribution of persons in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	91.3	92.2	92.3
1–3 days	6.4	5.0	5.0
4–7 days	1.3	1.4	1.4
8–10 days	0.2	0.4	0.4
11–14 days	0.7	1.0	1.0
2-week work loss days			
None	92.4	95.5	95.5
1–3 days	5.3	3.1	3.1
4–7 days	1.0	0.6	0.6
8–10 days	0.2	0.6	0.6
11–14 days	1.1	0.2	0.2
2-week cut-down days			
None	90.2	93.0	92.9
1–3 days	6.8	3.3	3.4
4–7 days	1.5	1.8	1.9
8–10 days	0.2	0.4	0.4
11–14 days	1.3	1.5	1.5
2-week doctor visits, person section			
None	84.1	86.5	86.5
1–3 visits	15.1	12.7	12.8
4–7 visits	0.6	0.3	0.3
8–10 visits	0.1	0.1	0.1
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.2	0.2
Physician visits, supplements			
None	82.5	86.1	86.0
1–3 visits	16.7	13.4	13.6
4–7 visits	0.7	0.4	0.4
8–10 visits	0.1	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	87.0	87.5	87.7
1 episode	10.9	10.0	9.9
2 episodes	1.5	1.8	1.8
3 episodes	0.4	0.5	0.5
4 episodes	0.1	0.1	0.1
5 episodes	0.1	0.1	0.1
6 episodes or more	0.0	0.1	0.1
Days per hospital episode			
1–3 days	35.8	35.9	35.2
4–7 days	31.8	35.3	35.9
8–10 days	8.7	11.0	11.1
11–14 days	9.6	8.5	8.3
15 days or more	7.3	9.4	9.4
2-week phone call to doctor			
None	96.6	98.1	98.0
1–3 phone calls	3.3	1.6	1.7
4–7 phone calls	0.1	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.1	0.1

See footnotes at end of table.

Table XI. Percent distribution of persons in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	92.9	94.8	94.7
1–3 visits	7.0	5.1	5.2
4–7 visits	0.1	0.1	0.1
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	26.5	26.8	26.5
1 visit	17.9	21.1	21.3
2–4 visits	34.3	29.9	30.2
5–12 visits	17.0	16.7	16.7
13–24 visits	3.0	3.7	3.7
25–52 visits	1.1	1.5	1.4
53 visits or more	0.2	0.3	0.3
12-month bed days			
None	46.0	53.7	53.9
1–7 days	38.0	32.6	32.7
8–30 days	10.7	9.6	9.5
31–180 days	2.7	2.9	2.8
181 days or more	0.5	0.5	0.4
Unknown	2.1	0.8	0.7
Time since last doctor visit			
2 weeks	19.6	15.2	15.3
2 weeks–6 months	39.2	43.5	43.5
6–12 months	18.0	15.5	15.7
1 year	11.3	10.4	10.4
2–4 years	6.2	9.7	9.7
5 years or more	2.5	4.2	4.1
Never	0.1	0.2	0.1
Unknown	3.2	1.2	1.2
Time since last dental visit			
2 weeks	7.3	5.1	5.3
2 weeks–6 months	33.7	30.3	31.3
6–12 months	19.0	14.1	14.3
1 year	13.8	14.3	14.2
2–4 years	10.1	15.1	14.7
5 years or more	11.7	17.8	17.3
Never	0.7	1.5	1.1
Unknown	3.8	1.7	1.7
Limitations of activity			
Unable to perform major activity	4.2	5.3	5.2
Limited in kind or amount of major activity	11.3	9.7	9.7
Limited in other activity	8.4	3.9	3.9
Not limited	76.1	81.1	81.3
Conditions			
Mean number of acute	0.183	0.116	0.116
Mean number of chronic	0.475	0.417	0.415
Health status			
Excellent	41.5	43.3	44.0
Good	41.7	40.1	40.0
Fair	11.9	12.2	11.7
Poor	3.8	3.8	3.5
Unknown	1.1	0.7	0.7

[1] 8,210 respondents.
[2] 19,800 respondents.
[3] 18,388 respondents.

Table XII. Percent distribution of males in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	91.9	93.8	93.8
1–3 days	6.0	4.1	4.1
4–7 days	1.1	1.0	1.0
8–10 days	0.2	0.3	0.3
11–14 days	0.8	0.9	0.8
2-week work loss days			
None	92.0	95.0	95.0
1–3 days	5.4	3.2	3.2
4–7 days	1.1	0.6	0.7
8–10 days	0.2	0.9	0.9
11–14 days	1.4	0.3	0.3
2-week cut-down days			
None	91.3	93.9	93.8
1–3 days	5.7	2.8	2.8
4–7 days	1.6	1.7	1.8
8–10 days	0.2	0.3	0.3
11–14 days	1.3	1.3	1.3
2-week doctor visits, person section			
None	86.4	88.7	88.7
1–3 visits	12.6	10.5	10.6
4–7 visits	0.8	0.3	0.3
8–10 visits	0.1	0.0	0.1
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.1	0.1
Physician visits, supplements			
None	85.1	86.1	86.0
1–3 visits	14.2	13.4	13.6
4–7 visits	0.6	0.4	0.4
8–10 visits	0.0	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	90.1	90.2	90.2
1 episode	8.0	7.9	7.8
2 episodes	1.3	1.4	1.5
3 episodes	0.3	0.4	0.3
4 episodes	0.1	0.1	0.1
5 episodes	0.1	0.0	0.0
6 episodes or more	0.0	0.1	0.1
Days per hospital episode			
1–3 days	32.8	33.1	32.5
4–7 days	27.5	32.9	33.2
8–10 days	12.2	11.7	12.0
11–14 days	16.0	10.1	9.9
15 days or more	8.4	12.2	12.4
2-week phone calls to doctor			
None	97.5	98.6	98.6
1–3 phone calls	2.5	1.1	1.1
4–7 phone calls	0.0	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.1	0.0	0.0
15 phone calls or more	0.0	0.1	0.1

See footnotes at end of table.

Table XII. Percent distribution of males in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	93.2	94.9	94.7
1–3 visits	6.6	5.1	5.3
4–7 visits	0.2	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	33.0	33.5	33.1
1 visit	19.0	21.5	21.7
2–4 visits	32.4	28.3	28.6
5–12 visits	12.8	12.8	12.9
13–24 visits	1.9	2.5	2.4
25–52 visits	0.8	1.1	1.1
53 visits or more	0.2	0.2	0.2
12-month bed days			
None	49.0	57.5	57.7
1–7 days	37.5	30.9	31.1
8–30 days	8.8	7.7	7.6
31–180 days	2.4	2.6	2.4
181 days or more	0.6	0.4	0.4
Unknown	1.7	0.8	0.8
Time since last doctor visit			
2 weeks	16.6	12.6	12.8
2 weeks–6 months	34.5	38.6	38.7
6–12 months	18.5	16.2	16.3
1 year	14.0	12.1	12.1
2–4 years	8.8	13.0	13.1
5 years or more	3.9	5.8	5.5
Never	0.2	0.2	0.1
Unknown	3.5	3.5	1.4
Time since last dental visit			
2 weeks	7.1	5.1	5.3
2 weeks–6 months	31.1	28.4	29.5
6–12 months	19.1	14.0	14.2
1 year	15.2	14.5	14.5
2–4 years	11.3	15.6	15.4
5 years or more	12.1	18.5	17.7
Never	0.8	1.9	1.4
Unknown	3.3	1.8	1.8
Limitations of activity			
Unable to perform major activity	6.4	8.3	7.9
Limited in kind or amount of major activity	9.1	7.4	7.4
Limited in other activity	7.7	3.7	3.7
Not limited	76.7	80.6	81.0
Conditions			
Mean number of acute	0.166	0.100	0.099
Mean number of chronic	0.439	0.394	0.392
Health status			
Excellent	43.6	46.9	47.7
Good	40.7	37.8	37.6
Fair	10.6	10.8	10.5
Poor	4.2	3.8	3.5
Unknown	0.9	0.7	0.7

[1]3,832 respondents
[2]9,175 respondents
[3]8,423 respondents

Table XIII. Percent distribution of females in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	90.8	90.9	91.0
1–3 days	6.9	5.8	5.7
4–7 days	1.5	1.7	1.7
8–10 days	0.2	0.4	0.4
11–14 days	0.7	1.2	1.2
2-week work loss days			
None	92.8	95.9	95.9
1–3 days	5.3	3.0	3.0
4–7 days	0.9	0.5	0.5
8–10 days	0.2	0.4	0.4
11–14 days	0.8	0.2	0.2
2-week cut-down days			
None	89.2	92.2	92.2
1–3 days	7.8	3.8	3.8
4–7 days	1.4	1.9	1.9
8–10 days	0.3	0.4	0.4
11–14 days	1.3	1.7	1.7
2-week doctor visits, person section			
None	82.1	84.6	84.6
1–3 visits	17.3	14.6	14.6
4–7 visits	0.5	0.3	0.3
8–10 visits	0.1	0.1	0.1
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.2	0.2
Physician visits, supplements			
None	80.2	84.0	83.9
1–3 visits	18.9	15.4	15.5
4–7 visits	0.8	0.6	0.6
8–10 visits	0.1	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	84.3	85.2	85.6
1 episode	13.4	11.9	11.6
2 episodes	1.7	2.0	2.0
3 episodes	0.4	0.6	0.6
4 episodes	0.1	0.1	0.1
5 episodes	0.1	0.1	0.1
6 episodes or more	0.1	0.0	0.0
Days per hospital episode			
1–3 days	37.5	37.5	36.8
4–7 days	34.4	36.7	37.5
8–10 days	6.7	10.6	10.6
11–14 days	5.8	7.5	7.4
15 days or more	6.7	7.7	7.7
2-week phone calls to doctor			
None	95.8	97.6	97.5
1–3 phone calls	4.1	2.1	2.2
4–7 phone calls	0.1	0.1	0.1
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.1	0.1
15 phone calls or more	0.0	0.0	0.0

See footnotes at end of table.

Table XIII. Percent distribution of females in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	92.6	94.8	94.7
1–3 visits	7.3	5.1	5.2
4–7 visits	0.1	0.1	0.1
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	20.6	20.9	20.9
1 visit	17.0	20.8	20.9
2–4 visits	36.0	31.2	31.5
5–12 visits	20.7	20.1	19.9
13–24 visits	3.9	4.8	4.7
25–52 visits	1.3	1.8	1.7
53 visits or more	0.3	0.4	0.4
12-month bed days			
None	43.4	50.4	50.7
1–7 days	38.5	34.0	34.1
8–30 days	12.4	11.3	11.0
31–180 days	2.9	3.2	3.1
181 days or more	0.4	0.5	0.4
Unknown	2.4	0.7	0.7
Time since last doctor visit			
2 weeks	22.3	17.4	17.5
2 weeks–6 months	43.3	47.7	47.5
6–12 months	17.6	15.0	15.1
1 year	8.8	8.9	9.0
2–4 years	3.8	6.9	6.9
5 years or more	1.3	2.9	2.8
Never	0.0	0.1	0.1
Unknown	2.9	1.1	1.1
Time since last dental visit			
2 weeks	7.4	5.2	5.3
2 weeks–6 months	36.0	31.8	32.8
6–12 months	18.9	14.2	14.4
1 year	12.5	14.1	13.9
2–4 years	9.2	14.6	14.2
5 years or more	11.4	17.2	17.0
Never	0.7	1.2	0.8
Unknown	4.0	1.6	1.6
Limitations of activity			
Unable to perform major activity	2.3	2.8	2.8
Limited in kind or amount of major activity	13.2	11.6	11.5
Limited in other activity	9.0	4.1	4.0
Not limited	75.5	81.5	81.6
Conditions			
Mean number of acute	0.198	0.131	0.130
Mean number of chronic	0.505	0.437	0.434
Health status			
Excellent	39.7	40.1	40.9
Good	42.6	42.2	42.1
Fair	13.1	13.3	12.8
Poor	3.4	3.8	3.5
Unknown	1.2	0.7	0.7

[1] 4,363 respondents.
[2] 10,625 respondents.
[3] 9,965 respondents.

Table XIV. Percent distribution of persons 17–24 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	90.8	91.5	91.4
1–3 days	8.0	6.7	6.8
4–7 days	0.9	1.2	1.2
8–10 days	0.0	0.2	0.2
11–14 days	0.3	0.4	0.4
2-week work loss days			
None	90.8	94.1	94.0
1–3 days	7.7	4.6	4.8
4–7 days	0.9	0.7	0.7
8–10 days	0.1	0.6	0.5
11–14 days	0.4	0.0	0.0
2-week cut-down days			
None	91.0	94.4	94.2
1–3 days	7.4	3.6	3.7
4–7 days	1.1	1.0	1.1
8–10 days	0.2	0.1	0.1
11–14 days	0.3	0.9	0.8
2-week doctor visits, person section			
None	85.0	88.0	87.9
1–3 visits	14.7	11.3	11.4
4–7 visits	0.2	0.1	0.1
8–10 visits	0.1	0.0	0.0
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.3	0.2
Physician visits, supplements			
None	83.4	87.7	87.6
1–3 visits	16.1	12.0	12.1
4–7 visits	0.5	0.2	0.3
8–10 visits	0.1	0.1	0.1
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	89.5	88.3	88.8
1 episode	9.2	10.4	9.8
2 episodes	0.9	0.9	1.0
3 episodes	0.3	0.4	0.4
4 episodes	0.1	0.0	0.4
5 episodes	0.1	0.0	0.0
6 episodes or more	0.0	0.0	0.0
Days per hospital episode			
1–3 days	44.0	55.9	55.9
4–7 days	44.0	34.1	35.0
8–10 days	4.0	4.0	3.4
11–14 days	4.0	2.4	2.1
15 days or more	2.0	3.5	3.7
2-week phone calls to doctor			
None	97.0	98.4	98.3
1–3 phone calls	3.0	1.3	1.4
4–7 phone calls	0.0	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.2	0.2

See footnotes at end of table.

Table XIV. Percent distribution of persons 17–24 years of age in the Survey Research Center (SRC) Telephone Survey and in telephone households and all households in the National Health Interview Survey (NHIS) by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	93.0	94.9	94.5
1–3 visits	6.8	5.1	5.4
4–7 visits	0.1	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	24.8	27.5	27.6
1 visit	17.9	24.5	24.7
2–4 visits	36.7	28.6	28.8
5–12 visits	17.0	15.3	15.0
13–24 visits	2.4	3.3	3.1
25–52 visits	1.1	0.7	0.7
53 visits or more	0.1	0.1	0.1
12-month bed days			
None	36.9	48.7	48.7
1–7 days	47.4	40.4	41.0
8–30 days	11.9	8.5	8.1
31–180 days	1.7	1.6	1.4
181 days or more	0.1	0.1	0.1
Unknown	2.0	0.7	0.8
Time since last doctor visit			
2 weeks	18.2	13.2	13.4
2 weeks–6 months	40.0	43.9	43.3
6–12 months	18.6	16.8	17.1
1 year	13.3	12.4	12.6
2–4 years	5.2	9.9	9.9
5 years or more	1.4	2.1	2.0
Never	0.1	0.2	0.2
Unknown	3.3	1.4	1.4
Time since last dental visit			
2 weeks	7.0	5.1	5.5
2 weeks–6 months	36.8	31.9	33.3
6–12 months	23.9	17.0	17.3
1 year	15.7	17.9	17.8
2–4 years	8.8	16.6	16.0
5 years or more	3.5	6.6	5.8
Never	1.2	3.0	2.3
Unknown	3.0	1.9	1.9
Limitations of activity			
Unable to perform major activity	0.8	1.1	1.2
Limited in kind or amount of major activity	4.1	2.9	2.7
Limited in other activity	7.2	3.0	2.9
Not limited	87.8	92.9	93.2
Conditions			
Mean number of acute	0.252	0.145	0.145
Mean number of chronic	0.217	0.139	0.136
Health status			
Excellent	50.7	49.4	50.8
Good	40.2	42.9	42.2
Fair	7.7	6.1	5.5
Poor	1.1	0.8	0.7
Unknown	0.3	0.7	0.7

[1] 1,524 respondents.
[2] 3,846 respondents.
[3] 3,409 respondents.

Table XV. Percent distribution of persons 25–34 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	90.5	91.6	91.8
1–3 days	7.9	6.1	6.1
4–7 days	1.3	1.4	1.3
8–10 days	0.2	0.4	0.4
11–14 days	0.2	0.5	0.5
2-week work loss days			
None	91.2	94.1	94.1
1–3 days	6.8	4.4	4.5
4–7 days	1.0	0.6	0.5
8–10 days	0.3	0.7	0.7
11–14 days	0.7	0.2	0.2
2-week cut-down days			
None	89.6	93.3	93.3
1–3 days	7.9	4.1	4.2
4–7 days	1.3	1.8	1.7
8–10 days	0.2	0.2	0.2
11–14 days	1.0	0.6	0.5
2-week doctor visits, person section			
None	86.5	86.8	87.0
1–3 visits	13.2	12.5	12.3
4–7 visits	0.2	0.3	0.3
8–10 visits	0.1	0.1	0.1
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.1	0.1
Physician visits, supplements			
None	84.4	86.4	86.4
1–3 visits	15.1	13.0	13.0
4–7 visits	0.4	0.6	0.6
8–10 visits	0.1	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	85.5	87.7	88.0
1 episode	13.0	10.4	10.2
2 episodes	1.2	1.4	1.4
3 episodes	0.3	0.4	0.4
4 episodes	0.0	0.0	0.0
5 episodes	0.0	0.0	0.0
6 episodes or more	0.1	0.0	0.0
Days per hospital episode			
1–3 days	50.5	46.8	46.6
4–7 days	31.2	37.3	38.7
8–10 days	7.5	5.2	5.0
11–14 days	5.4	6.3	6.0
15 days or more	1.1	4.4	3.7
2-week phone calls to doctor			
None	96.5	97.7	97.6
1–3 phone calls	3.4	1.9	2.0
4–7 phone calls	0.1	0.1	0.1
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.1	0.0	0.0
15 phone calls or more	0.0	0.1	0.0

See footnotes at end of table.

Table XV. Percent distribution of persons 25–34 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	93.2	94.7	94.5
1–3 visits	6.8	5.3	5.4
4–7 visits	0.1	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	24.9	26.2	25.9
1 visit	18.8	22.0	22.2
2–4 visits	33.7	29.7	30.2
5–12 visits	18.0	16.1	15.8
13–24 visits	3.4	4.2	4.2
25–52 visits	0.9	1.5	1.5
53 visits or more	0.3	0.3	0.3
12-month bed days			
None	35.9	45.2	44.9
1–7 days	49.6	41.9	42.6
8–30 days	11.3	9.5	9.4
31–180 days	2.3	2.4	2.1
181 days or more	0.1	0.2	0.2
Unknown	0.8	0.8	0.8
Time since last doctor visit			
2 weeks	16.9	14.7	14.7
2 weeks–6 months	40.8	42.1	42.2
6–12 months	18.7	17.6	18.0
1 year	13.1	11.0	11.0
2–4 years	6.0	9.7	9.8
5 years or more	1.9	3.2	2.9
Never	0.0	0.2	0.1
Unknown	2.7	1.5	1.4
Time since last dental visit			
2 weeks	6.7	5.3	5.5
2 weeks–6 months	35.8	33.3	34.6
6–12 months	21.8	16.2	16.7
1 year	17.0	17.3	17.0
2–4 years	10.5	15.2	14.9
5 years or more	5.2	9.3	8.4
Never	0.5	1.9	1.3
Unknown	2.4	1.5	1.5
Limitations of activity			
Unable to perform major activity	0.7	1.5	1.3
Limited in kind or amount of major activity	4.7	4.9	4.7
Limited in other activity	8.3	3.1	3.0
Not limited	86.3	90.5	91.0
Conditions			
Mean number of acute	0.228	0.140	0.141
Mean number of chronic	0.271	0.189	0.180
Health status			
Excellent	47.0	52.0	53.1
Good	43.2	38.6	38.4
Fair	7.5	7.3	6.8
Poor	1.8	1.5	1.2
Unknown	0.6	0.5	0.5

[1] 1,852 respondents.
[2] 4,381 respondents.
[3] 3,998 respondents.

Table XVI. Percent distribution of persons 35–44 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	92.0	92.6	92.6
1–3 days	6.1	5.3	5.3
4–7 days	1.4	1.2	1.1
8–10 days	0.1	0.3	0.4
11–14 days	0.4	0.6	0.6
2-week work loss days			
None	92.0	95.3	95.3
1–3 days	5.1	3.3	3.1
4–7 days	1.4	0.6	0.6
8–10 days	0.3	0.6	0.7
11–14 days	1.1	0.3	0.3
2-week cut-down days			
None	89.8	94.1	94.1
1–3 days	7.5	3.3	3.3
4–7 days	1.6	1.5	1.5
8–10 days	0.2	0.1	0.1
11–14 days	0.9	1.0	1.0
2-week doctor visits, person section			
None	87.1	89.1	89.0
1–3 visits	12.2	9.8	10.2
4–7 visits	0.6	0.3	0.3
8–10 visits	0.0	0.1	0.1
11–14 visits	0.1	0.2	0.0
15 visits or more	0.0	0.0	0.2
Physician visits, supplements			
None	85.0	86.1	86.0
1–3 visits	14.4	13.4	13.6
4–7 visits	0.6	0.4	0.4
8–10 visits	0.0	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	89.5	90.1	90.1
1 episode	8.8	8.1	8.2
2 episodes	1.2	1.3	1.3
3 episodes	0.2	0.3	0.3
4 episodes	0.1	0.1	0.1
5 episodes	0.1	0.0	0.0
6 episodes or more	0.1	0.1	0.1
Days per hospital episode			
1–3 days	36.6	33.9	34.1
4–7 days	39.0	38.4	38.6
8–10 days	7.3	10.1	9.7
11–14 days	12.2	7.8	8.3
15 days or more	2.4	9.8	9.3
2-week phone calls to doctor			
None	96.1	98.0	98.0
1–3 phone calls	3.9	1.5	1.5
4–7 phone calls	0.0	0.0	0.0
8–10 phone calls	0.0	0.1	0.1
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.1	0.1

See footnotes at end of table.

Table XVI. Percent distribution of persons 35–44 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	92.1	94.1	93.9
1–3 visits	7.7	5.9	6.1
4–7 visits	0.2	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	28.4	29.5	29.1
1 visit	21.9	24.2	24.4
2–4 visits	32.5	29.5	29.6
5–12 visits	13.5	12.1	12.2
13–24 visits	2.6	2.5	2.5
25–52 visits	0.9	1.7	1.6
53 visits or more	0.3	0.5	0.5
12-month bed days			
None	46.3	53.0	53.1
1–7 days	40.0	35.5	35.7
8–30 days	9.8	8.0	7.8
31–180 days	2.7	2.6	2.6
181 days or more	0.2	0.3	0.2
Unknown	1.0	0.6	0.5
Time since last doctor visit			
2 weeks	16.7	12.9	13.6
2 weeks–6 months	37.0	40.7	41.9
6–12 months	20.8	18.0	15.8
1 year	11.9	12.3	11.7
2–4 years	7.6	10.8	10.2
5 years or more	3.1	4.2	5.6
Never	0.0	0.2	0.1
Unknown	3.0	1.0	1.0
Time since last dental visit			
2 weeks	8.1	4.9	5.2
2 weeks–6 months	36.7	32.5	33.3
6–12 months	18.8	16.3	16.4
1 year	12.9	15.5	15.6
2–4 years	11.1	15.7	15.5
5 years or more	8.1	12.2	11.4
Never	0.6	1.1	0.7
Unknown	3.8	1.8	1.9
Limitations of activity			
Unable to perform major activity	1.5	2.2	2.1
Limited in kind or amount of major activity	9.2	6.6	6.2
Limited in other activity	8.8	3.2	3.1
Not limited	80.5	88.0	88.6
Conditions			
Mean number of acute	0.179	0.113	0.112
Mean number of chronic	0.382	0.272	0.263
Health status			
Excellent	46.2	48.0	49.0
Good	41.1	39.8	39.6
Fair	9.4	9.5	8.8
Poor	2.5	2.2	2.1
Unknown	0.8	0.5	0.5

[1] 1,394 respondents.
[2] 3,115 respondents.
[3] 2,927 respondents.

Table XVII. Percent distribution of persons 45–54 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	91.9	93.1	93.2
1–3 days	6.5	4.3	4.3
4–7 days	1.0	1.4	1.3
8–10 days	0.1	0.3	0.4
11–14 days	0.5	0.9	0.8
2-week work loss days			
None	92.4	95.4	95.3
1–3 days	5.1	2.6	2.6
4–7 days	1.0	0.9	0.9
8–10 days	0.2	0.9	0.9
11–14 days	1.3	0.3	0.3
2-week cut-down days			
None	91.3	93.5	93.5
1–3 days	6.1	2.6	2.6
4–7 days	1.6	2.0	2.0
8–10 days	0.2	0.4	0.4
11–14 days	0.9	1.6	1.4
2-week doctor visits, person section			
None	85.5	88.3	88.2
1–3 visits	13.4	10.9	11.0
4–7 visits	1.1	0.5	0.5
8–10 visits	0.1	0.1	0.1
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
Physician visits, supplements			
None	84.5	87.8	87.7
1–3 visits	14.2	11.7	11.7
4–7 visits	1.1	0.5	0.4
8–10 visits	0.1	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	89.5	89.7	89.9
1 episode	8.5	8.2	8.1
2 episodes	1.5	1.7	1.6
3 episodes	0.2	0.3	0.3
4 episodes	0.2	0.2	0.2
5 episodes	0.0	0.0	0.0
6 episodes or more	0.0	0.0	0.0
Days per hospital episode			
1–3 days	31.0	29.2	29.3
4–7 days	31.0	37.1	37.4
8–10 days	12.1	15.5	15.4
11–14 days	6.9	7.2	6.6
15 days or more	12.1	11.0	11.4
2-week phone calls to doctor			
None	96.9	98.2	98.1
1–3 phone calls	2.8	1.5	1.5
4–7 phone calls	0.3	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.1	0.1

See footnotes at end of table.

Table XVII. Percent distribution of persons 45–54 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	92.6	94.1	93.9
1–3 visits	7.2	5.9	6.1
4–7 visits	0.2	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	30.0	29.8	29.6
1 visit	17.9	21.8	22.3
2–4 visits	33.0	28.1	28.3
5–12 visits	14.5	15.1	15.1
13–24 visits	3.2	3.1	2.9
25–52 visits	1.2	1.6	1.5
53 visits or more	0.3	0.4	0.4
12-month bed days			
None	51.1	58.0	58.3
1–7 days	35.5	28.3	28.6
8–30 days	9.0	9.0	8.8
31–180 days	2.1	3.1	2.9
181 days or more	0.4	0.7	0.6
Unknown	1.9	0.9	0.9
Time since last doctor visit			
2 weeks	17.6	13.5	13.6
2 weeks–6 months	37.1	42.1	41.9
6–12 months	19.1	15.5	15.8
1 year	11.7	11.7	11.7
2–4 years	7.5	10.3	10.2
5 years or more	3.5	5.7	5.6
Never	0.1	0.1	0.1
Unknown	3.3	1.0	1.0
Time since last dental visit			
2 weeks	7.6	5.9	6.1
2 weeks–6 months	35.6	32.4	33.1
6–12 months	17.9	13.0	13.3
1 year	13.2	14.0	14.3
2–4 years	10.4	14.6	14.1
5 years or more	11.4	17.8	17.1
Never	0.5	0.8	0.6
Unknown	3.4	1.5	1.5
Limitations of activity			
Unable to perform major activity	2.8	4.0	3.5
Limited in kind or amount of major activity	12.3	10.7	10.4
Limited in other activity	8.7	3.8	3.6
Not limited	76.3	81.5	82.5
Conditions			
Mean number of acute	0.153	0.096	0.097
Mean number of chronic	0.484	0.422	0.399
Health status			
Excellent	36.2	41.0	42.1
Good	46.2	40.2	40.2
Fair	13.1	13.7	13.0
Poor	3.5	5.0	4.0
Unknown	1.0	0.7	0.7

[1] 1,239 respondents.
[2] 2,822 respondents.
[3] 2,691 respondents.

Table XVIII. Percent distribution of persons 55–64 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	92.3	93.1	93.2
1–3 days	4.1	3.8	3.7
4–7 days	1.8	1.6	1.5
8–10 days	0.7	0.4	0.3
11–14 days	1.1	1.2	1.2
2-week work loss days			
None	93.2	96.1	96.0
1–3 days	3.4	2.2	2.3
4–7 days	1.1	0.5	0.5
8–10 days	0.3	0.9	0.9
11–14 days	2.0	0.4	0.4
2-week cut-down days			
None	89.9	91.1	90.9
1–3 days	5.6	3.2	3.2
4–7 days	2.1	2.8	2.9
8–10 days	0.3	0.7	0.7
11–14 days	2.0	2.2	2.2
2-week doctor visits, person section			
None	79.9	83.2	83.2
1–3 visits	18.3	15.9	16.0
4–7 visits	1.2	0.3	0.3
8–10 visits	0.2	0.1	0.1
11–14 visits	0.4	0.0	0.0
15 visits or more	0.0	0.2	0.2
Physician visits, supplements			
None	79.0	83.0	82.8
1–3 visits	20.2	16.5	16.7
4–7 visits	0.6	0.6	0.5
8–10 visits	0.2	0.0	0.0
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	86.7	87.3	87.3
1 episode	10.3	8.9	8.9
2 episodes	2.1	2.8	2.8
3 episodes	0.5	0.5	0.5
4 episodes	0.3	0.1	0.2
5 episodes	0.0	0.2	0.2
6 episodes or more	0.1	0.1	0.1
Days per hospital episode			
1–3 days	21.7	27.4	26.7
4–7 days	19.6	32.1	32.2
8–10 days	15.2	17.1	17.5
11–14 days	23.9	10.6	10.4
15 days or more	2.2	13.0	13.2
2-week phone calls to doctor			
None	96.2	98.1	98.1
1–3 phone calls	3.5	1.6	1.6
4–7 phone calls	0.2	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.1	0.0	0.0
15 phone calls or more	0.0	0.1	0.1

See footnotes at end of table.

Table XVIII. Percent distribution of persons 55–64 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	91.4	94.3	94.2
1–3 visits	8.5	5.5	5.6
4–7 visits	0.2	0.2	0.2
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	26.9	25.7	25.4
1 visit	15.8	17.9	18.2
2–4 visits	34.9	30.0	30.3
5–12 visits	17.6	19.6	19.5
13–24 visits	3.3	4.6	4.5
25–52 visits	1.2	1.8	1.8
53 visits or more	0.3	0.3	0.3
12-month bed days			
None	55.7	61.0	61.1
1–7 days	26.1	23.3	23.5
8–30 days	9.8	10.6	10.4
31–180 days	3.8	4.0	3.9
181 days or more	1.4	0.7	0.7
Unknown	3.2	0.5	0.4
Time since last doctor visit			
2 weeks	23.7	18.1	18.3
2 weeks–6 months	39.0	43.7	43.9
6–12 months	16.7	13.4	13.3
1 year	9.2	8.2	8.2
2–4 years	5.5	10.1	10.2
5 years or more	2.7	5.1	4.7
Never	0.3	0.1	0.1
Unknown	2.9	1.2	1.2
Time since last dental visit			
2 weeks	9.2	5.7	5.8
2 weeks–6 months	30.6	29.6	30.6
6–12 months	15.2	11.7	11.9
1 year	12.2	10.9	10.8
2–4 years	9.9	13.6	13.3
5 years or more	18.3	25.8	25.1
Never	0.4	0.8	0.7
Unknown	4.2	1.8	1.8
Limitations of activity			
Unable to perform major activity	10.(	9.8	9.3
Limited in kind or amount of major activity	19.ᵢ	14.9	15.1
Limited in other activity	9.3	5.2	5.1
Not limited	61.2	70.0	70.5
Conditions			
Mean number of acute	0.126	0.097	0.098
Mean number of chronic	0.839	0.684	0.677
Health status			
Excellent	31.9	32.1	32.3
Good	40.5	40.9	41.8
Fair	17.4	17.7	17.1
Poor	7.9	8.5	8.0
Unknown	2.3	0.8	0.8

[1] 1,094 respondents.
[2] 2,686 respondents.
[3] 2,569 respondents.

Table XIX. Percent distribution of persons 65–74 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	92.3	92.9	92.8
1–3 days	3.8	3.1	3.1
4–7 days	1.4	1.6	1.7
8–10 days	0.5	0.3	0.3
11–14 days	2.1	2.1	2.2
2-week work loss days			
None	97.4	98.9	98.9
1–3 days	1.2	0.4	0.4
4–7 days	0.5	0.2	0.2
8–10 days	0.0	0.3	0.3
11–14 days	0.9	0.2	0.2
2-week cut-down days			
None	89.2	91.2	91.3
1–3 days	4.7	2.7	2.7
4–7 days	1.8	2.0	2.0
8–10 days	0.5	0.6	0.6
11–14 days	3.9	3.4	3.4
2-week doctor visits, person section			
None	76.8	83.4	83.1
1–3 visits	22.1	15.8	16.1
4–7 visits	1.1	0.2	0.2
8–10 visits	0.0	0.0	0.1
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.3	0.3
Physician visits, supplements			
None	75.8	82.8	82.3
1–3 visits	23.0	16.7	17.2
4–7 visits	1.2	0.4	0.4
8–10 visits	0.0	0.0	0.1
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	81.5	83.2	82.3
1 episode	15.1	12.8	12.7
2 episodes	2.1	2.7	2.7
3 episodes	1.2	1.1	1.1
4 episodes	0.2	0.2	0.2
5 episodes	0.0	0.0	0.0
6 episodes or more	0.0	0.0	0.1
Days per hospital episode			
1–3 days	23.3	19.2	19.5
4–7 days	32.6	35.2	35.1
8–10 days	11.6	17.0	16.9
11–14 days	4.7	16.0	15.6
15 days or more	23.3	12.6	12.9
2-week phone calls to doctor			
None	96.8	97.8	97.7
1–3 phone calls	3.2	1.9	2.1
4–7 phone calls	0.0	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.0	0.0

See footnotes at end of table.

Table XIX. Percent distribution of persons 65–74 years of age in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	95.9	95.7	95.7
1–3 visits	4.0	4.2	4.2
4–7 visits	0.2	0.0	0.1
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	22.6	23.0	22.2
1 visit	15.2	14.6	14.8
2–4 visits	36.8	32.9	33.1
5–12 visits	22.1	22.4	22.7
13–24 visits	2.3	4.9	5.0
25–52 visits	1.1	2.0	1.9
53 visits or more	0.0	0.3	0.3
12-month bed days			
None	62.1	63.7	64.0
1–7 days	19.1	18.5	18.3
8–30 days	11.1	11.8	11.8
31–180 days	3.8	4.3	4.3
181 days or more	0.9	0.6	0.6
Unknown	3.1	1.0	1.0
Time since last doctor visit			
2 weeks	28.1	19.0	19.6
2 weeks–6 months	40.4	47.7	48.0
6–12 months	13.0	10.7	10.6
1 year	5.8	6.4	6.4
2–4 years	5.6	8.3	8.2
5 years or more	2.9	6.4	6.0
Never	0.2	0.2	0.1
Unknown	4.1	1.2	1.1
Time since last dental visit			
2 weeks	4.3	4.3	4.3
2 weeks–6 months	24.6	22.4	23.2
6–12 months	13.3	9.3	9.4
1 year	10.9	7.6	7.7
2–4 years	10.4	15.1	15.2
5 years or more	30.5	38.4	37.9
Never	0.9	1.0	0.7
Unknown	5.2	1.8	1.7
Limitations of activity			
Unable to perform major activity	12.9	14.1	13.8
Limited in kind or amount of major activity	24.6	21.9	22.0
Limited in other activity	8.3	5.6	5.6
Not limited	54.2	58.4	58.5
Conditions			
Mean number of acute	0.116	0.092	0.094
Mean number of chronic	0.934	0.945	0.948
Health status			
Excellent	28.2	29.4	29.9
Good	39.5	38.3	38.6
Fair	22.2	24.5	24.3
Poor	8.4	6.9	6.5
Unknown	1.7	0.9	0.8

[1] 670 respondents.
[2] 1,896 respondents.
[3] 1,806 respondents.

Table XX. Percent distribution of persons 75 years and over in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	89.0	90.8	91.1
1–3 days	5.1	2.1	1.9
4–7 days	2.3	1.8	1.8
8–10 days	0.0	0.9	0.9
11–14 days	3.6	4.5	4.2
2-week work loss days			
None	95.8	99.3	99.3
1–3 days	1.8	0.1	0.1
4–7 days	0.6	0.3	0.3
8–10 days	0.0	0.1	0.1
11–14 days	1.8	0.2	0.2
2-week cut-down days			
None	89.2	90.2	90.4
1–3 days	6.9	2.4	2.3
4–7 days	0.9	2.4	2.4
8–10 days	0.0	1.0	1.0
11–14 days	3.0	4.0	3.9
2-week doctor visits, person section			
None	77.0	81.4	81.5
1–3 visits	22.2	17.7	17.8
4–7 visits	0.5	0.2	0.1
8–10 visits	0.0	0.1	0.1
11–14 visits	0.3	0.2	0.1
15 visits or more	0.0	0.2	0.2
Physician visits, supplements			
None	74.8	81.5	81.5
1–3 visits	23.6	18.2	18.2
4–7 visits	1.4	0.1	0.1
8–10 visits	0.0	0.1	0.1
11–14 visits	0.3	0.1	0.1
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	75.5	79.0	79.0
1 episode	18.8	16.0	15.9
2 episodes	3.9	3.3	3.4
3 episodes	0.9	1.4	1.4
4 episodes	0.3	0.1	0.1
5 episodes	0.3	0.1	0.1
6 episodes or more	0.3	0.0	0.0
Days per hospital episode			
1–3 days	21.7	17.2	16.4
4–7 days	21.7	31.2	32.8
8–10 days	0.0	16.7	17.4
11–14 days	17.4	14.5	13.5
15 days or more	21.7	20.4	19.8
2-week phone calls to doctor			
None	95.6	98.1	98.0
1–3 phone calls	4.4	1.6	1.7
4–7 phone calls	0.0	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.2	0.2

See footnotes at end of table.

Table XX. Percent distribution of persons 75 years and over in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	95.5	96.7	96.5
1–3 visits	4.5	3.3	3.5
4–7 visits	0.0	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	24.2	19.7	19.4
1 visit	11.1	14.4	14.4
2–4 visits	33.7	35.6	36.1
5–12 visits	24.4	25.1	25.1
13–24 visits	4.5	4.2	4.0
25–52 visits	1.5	1.0	1.0
53 visits or more	0.6	0.0	0.0
12-month bed days			
None	56.2	61.0	60.7
1–7 days	13.8	16.8	17.3
8–30 days	15.4	14.4	14.6
31–180 days	4.5	4.8	4.7
181 days or more	2.2	2.2	2.0
Unknown	7.8	0.8	0.7
Time since last doctor visit			
2 weeks	30.5	20.6	20.7
2 weeks–6 months	42.3	51.8	51.7
6–12 months	11.1	9.2	9.2
1 year	3.9	5.1	5.3
2–4 years	4.9	5.1	5.9
5 years or more	4.2	6.1	6.0
Never	0.0	0.0	0.0
Unknown	3.0	1.1	1.2
Time since last dental visit			
2 weeks	4.5	3.3	3.5
2 weeks–6 months	18.1	15.6	16.0
6–12 months	10.1	6.3	6.5
1 year	6.9	6.7	6.6
2–4 years	10.5	12.2	12.0
5 years or more	39.7	53.7	53.2
Never	1.0	0.7	0.5
Unknown	9.2	1.5	1.6
Limitations of activity			
Unable to perform major activity	18.8	22.3	21.6
Limited in kind or amount of major activity	29.8	24.8	25.0
Limited in other activity	9.9	7.1	6.7
Not limited	41.5	45.8	46.6
Conditions			
Mean number of acute	0.095	0.064	0.070
Mean number of chronic	1.03	1.17	1.16
Health status			
Excellent	29.2	30.1	30.4
Good	37.5	38.2	38.4
Fair	20.0	21.5	21.2
Poor	10.6	8.7	8.7
Unknown	2.7	1.4	1.4

[1]336 respondents.
[2]1,054 respondents.
[3]988 respondents.

Table XXI. Percent distribution of white persons in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	91.7	92.5	92.5
1–3 days	6.3	5.0	5.0
4–7 days	1.1	1.2	1.2
8–10 days	0.2	0.3	0.3
11–14 days	0.7	1.0	1.0
2-week work loss days			
None	92.8	95.6	95.6
1–3 days	5.2	3.1	3.1
4–7 days	0.9	0.5	0.5
8–10 days	0.2	0.6	0.6
11–14 days	1.0	0.2	0.2
2-week cut-down days			
None	90.1	93.0	93.0
1–3 days	6.8	3.4	3.4
4–7 days	1.5	1.7	1.8
8–10 days	0.2	0.3	0.4
11–14 days	1.4	1.5	1.4
2-week doctor visits, person section			
None	84.7	86.8	86.7
1–3 visits	14.7	12.6	12.8
4–7 visits	0.5	0.3	0.2
8–10 visits	0.0	0.1	0.1
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.1	0.1
Physician visits, supplements			
None	83.0	86.1	86.0
1–3 visits	16.3	13.4	13.5
4–7 visits	0.7	0.4	0.4
8–10 visits	0.0	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	87.1	87.6	87.7
1 episode	10.8	10.0	9.8
2 episodes	1.5	1.7	1.7
3 episodes	0.4	0.5	0.5
4 episodes	0.1	0.1	0.1
5 episodes	0.1	0.1	0.0
6 episodes or more	0.0	0.1	0.1
Days per hospital episode			
1–3 days	34.6	36.7	36.1
4–7 days	32.4	35.3	35.9
8–10 days	9.1	10.9	10.9
11–14 days	10.4	8.1	8.2
15 days or more	6.6	9.0	9.0
2-week phone calls to doctor			
None	96.5	98.2	98.2
1–3 phone calls	3.4	1.7	1.7
4–7 phone calls	0.1	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.0	0.0

See footnotes at end of table.

Table XXI. Percent distribution of white persons in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	92.8	94.5	94.4
1–3 visits	7.0	5.4	5.5
4–7 visits	0.1	0.1	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	26.3	26.5	26.1
1 visit	18.4	21.6	21.8
2–4 visits	34.0	30.1	30.4
5–12 visits	17.2	16.7	16.7
13–24 visits	2.9	3.5	3.4
25–52 visits	1.0	1.4	1.4
53 visits or more	0.2	0.2	0.2
12-month bed days			
None	45.6	53.7	53.9
1–7 days	38.8	33.2	33.3
8–30 days	10.7	9.4	9.3
31–180 days	2.6	2.6	2.5
181 days or more	0.5	0.4	0.4
Unknown	1.9	0.7	0.6
Time since last doctor visit			
2 weeks	19.0	14.9	15.1
2 weeks–6 months	39.5	43.5	43.5
6–12 months	18.1	15.9	16.0
1 year	11.5	10.2	10.3
2–4 years	6.5	10.1	10.0
5 years or more	2.5	4.2	4.0
Never	0.1	0.1	0.1
Unknown	2.8	1.1	1.1
Time since last dental visit			
2 weeks	7.3	5.5	5.6
2 weeks–6 months	35.1	31.7	32.6
6–12 months	18.8	14.4	14.6
1 year	13.6	13.9	13.8
2–4 years	9.5	14.4	14.1
5 years or more	11.8	17.7	17.2
Never	0.5	1.1	0.8
Unknown	3.5	1.4	1.4
Limitations of activity			
Unable to perform major activity	4.1	5.0	4.8
Limited in kind or amount of major activity	11.1	9.3	9.3
Limited in other activity	8.7	4.0	3.9
Not limited	76.0	81.7	81.9
Conditions			
Mean number of acute	0.182	0.115	0.115
Mean number of chronic	0.476	0.406	0.405
Health status			
Excellent	42.2	45.1	45.7
Good	41.7	39.8	39.7
Fair	11.4	11.2	10.8
Poor	3.7	3.5	3.3
Unknown	1.0	0.4	0.4

[1] 7,182 respondents.
[2] 16,955 respondents.
[3] 15,944 respondents.

Table XXII. Percent distribution of all other persons in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	88.8	90.5	90.8
1–3 days	7.2	5.3	5.1
4–7 days	2.6	2.3	2.3
8–10 days	0.3	0.6	0.7
11–14 days	1.1	1.3	1.2
2-week work loss days			
None	89.5	94.5	94.7
1–3 days	6.6	3.4	3.1
4–7 days	1.6	0.8	0.8
8–10 days	0.6	1.1	1.0
11–14 days	1.7	0.3	0.3
2-week cut-down days			
None	90.6	92.8	92.7
1–3 days	6.7	3.0	3.1
4–7 days	1.5	2.1	2.2
8–10 days	0.4	0.3	0.3
11–14 days	0.8	1.8	1.8
2-week doctor visits, person section			
None	80.1	84.9	84.9
1–3 visits	18.1	13.0	13.0
4–7 visits	1.4	0.6	0.6
8–10 visits	0.2	0.2	0.2
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.5	0.5
Physician visits, supplements			
None	79.4	85.8	85.8
1–3 visits	19.4	13.6	13.7
4–7 visits	0.9	0.5	0.5
8–10 visits	0.3	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	86.4	87.3	87.4
1 episode	11.3	10.3	10.2
2 episodes	1.4	1.9	2.0
3 episodes	0.5	0.4	0.2
4 episodes	0.2	0.1	0.0
5 episodes	0.0	0.1	0.0
6 episodes or more	0.2	0.0	0.0
Days per hospital episode			
1–3 days	45.9	30.9	30.0
4–7 days	27.0	35.4	36.5
8–10 days	5.4	11.6	12.4
11–14 days	2.7	10.8	9.1
15 days or more	13.5	11.3	12.0
2-week phone calls to doctor			
None	97.1	97.0	99.8
1–3 phone calls	2.7	1.3	1.3
4–7 phone calls	0.1	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.1	0.0	0.0
15 phone calls or more	0.0	0.6	0.7

See footnotes at end of table.

Table XXII. Percent distribution of all other persons in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	93.3	96.7	96.2
1–3 visits	6.5	3.3	3.8
4–7 visits	0.2	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	27.8	28.5	28.6
1 visit	15.0	18.5	18.3
2–4 visits	36.2	28.7	28.9
5–12 visits	15.8	16.8	16.5
13–24 visits	3.3	5.0	5.2
25–52 visits	1.6	1.9	1.8
53 visits or more	0.3	0.6	0.7
12-month bed days			
None	48.7	53.4	53.6
1–7 days	32.7	28.8	29.0
8–30 days	11.3	11.1	10.7
31–180 days	3.1	4.8	4.8
181 days or more	0.8	0.6	0.5
Unknown	3.4	1.3	1.3
Time since last doctor visit			
2 weeks	23.5	16.8	17.0
2 weeks–6 months	37.2	43.7	43.3
6–12 months	17.5	13.4	13.7
1 year	9.5	11.5	11.5
2–4 years	4.0	7.7	7.7
5 years or more	3.1	4.6	4.5
Never	0.2	0.4	0.3
Unknown	5.0	2.0	2.0
Time since last dental visit			
2 weeks	6.5	3.3	3.8
2 weeks–6 months	23.9	21.9	22.6
6–12 months	20.8	12.4	12.5
1 year	15.5	16.9	16.9
2–4 years	14.4	19.0	19.1
5 years or more	11.2	18.8	18.2
Never	2.4	4.0	3.0
Unknown	5.3	3.7	3.8
Limitations of activity			
Unable to perform major activity	4.9	7.5	7.3
Limited in kind or amount of major activity	13.0	11.7	11.7
Limited in other activity	6.0	3.5	3.5
Not limited	76.1	77.3	77.5
Conditions			
Mean number of acute	0.191	0.125	0.122
Mean number of chronic	0.465	0.488	0.477
Health status			
Excellent	37.0	32.3	32.8
Good	42.0	42.1	42.3
Fair	15.2	17.8	17.7
Poor	4.4	5.6	4.9
Unknown	1.4	2.2	2.3

[1] 1,028 respondents.
[2] 2,845 respondents.
[3] 2,444 respondents.

Table XXIII. Percent distribution of persons with less than $5,000 family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	88.5	89.3	89.4
1–3 days	6.8	5.1	4.7
4–7 days	2.0	2.5	2.6
8–10 days	0.3	0.6	0.6
11–14 days	2.5	2.5	2.6
2-week work loss days			
None	95.0	97.1	97.2
1–3 days	3.0	1.8	1.7
4–7 days	0.7	0.5	0.5
8–10 days	0.0	0.2	0.2
11–14 days	1.2	0.4	0.4
2-week cut-down days			
None	87.4	88.6	87.8
1–3 days	7.1	4.0	4.3
4–7 days	2.8	2.6	2.9
8–10 days	0.3	0.9	1.0
11–14 days	2.5	4.0	4.1
2-week doctor visits, person section			
None	78.8	83.2	82.7
1–3 visits	20.4	14.8	15.2
4–7 visits	0.4	0.7	0.8
8–10 visits	0.3	0.2	0.2
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.6	0.7
Physician visits, supplements			
None	76.6	83.3	82.9
1–3 visits	22.7	15.9	16.3
4–7 visits	0.6	0.8	0.8
8–10 visits	0.1	0.1	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	84.8	82.8	82.9
1 episode	11.3	13.4	13.2
2 episodes	2.5	2.8	3.0
3 episodes	0.6	0.8	0.7
4 episodes	0.6	0.1	0.1
5 episodes	0.1	0.1	0.2
6 episodes or more	0.0	0.0	0.0
Days per hospital episode			
1–3 days	22.7	24.2	20.7
4–7 days	31.8	34.8	36.8
8–10 days	13.6	13.4	14.7
11–14 days	4.5	12.3	11.2
15 days or more	15.9	15.3	16.5
2-week phone calls to doctor			
None	96.4	97.5	97.1
1–3 phone calls	3.5	2.0	2.2
4–7 phone calls	0.1	0.1	0.1
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.1	0.2

See footnotes at end of table.

Table XXIII. Percent distribution of persons with less than $5,000 family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	94.6	96.3	96.1
1–3 visits	5.2	3.6	3.8
4–7 visits	0.1	0.1	0.1
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	26.3	23.4	22.5
1 visit	14.2	16.2	15.8
2–4 visits	30.8	28.8	29.5
5–12 visits	20.3	21.5	22.4
13–24 visits	5.8	6.6	6.4
25–52 visits	1.8	2.8	2.6
53 visits or more	0.7	0.7	0.7
12-month bed days			
None	48.9	49.3	49.0
1–7 days	27.8	27.4	27.7
8–30 days	12.1	15.4	15.5
31–180 days	5.2	6.2	6.1
181 days or more	1.6	1.1	1.2
Unknown	4.3	0.6	0.5
Time since last doctor visit			
2 weeks	27.2	18.5	19.3
2 weeks–6 months	37.7	48.1	48.2
6–12 months	12.9	11.6	11.2
1 year	10.4	7.8	7.8
2–4 years	4.8	8.9	8.6
5 years or more	3.7	4.2	3.9
Never	0.1	0.1	0.1
Unknown	3.2	1.0	0.8
Time since last dental visit			
2 weeks	6.3	3.7	3.9
2 weeks–6 months	23.8	18.8	19.1
6–12 months	14.9	10.9	11.0
1 year	13.1	12.9	12.1
2–4 years	12.4	17.6	17.2
5 years or more	23.4	31.7	32.8
Never	1.6	2.6	1.8
Unknown	4.6	1.9	2.1
Limitations of activity			
Unable to perform major activity	12.0	13.0	12.6
Limited in kind or amount of major activity	18.3	20.1	21.5
Limited in other activity	7.3	5.7	5.9
Not limited	62.4	61.3	59.9
Conditions			
Mean number of acute	0.203	0.139	0.139
Mean number of chronic	0.845	0.885	0.925
Health status			
Excellent	29.1	29.3	29.7
Good	36.8	37.2	37.1
Fair	21.2	22.2	22.0
Poor	10.5	10.5	10.3
Unknown	2.4	0.9	1.0

[1] 693 respondents.
[2] 2,089 respondents.
[3] 1,667 respondents.

Table XXIV. Percent distribution of persons with $5,000–$9,999 family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	91.2	91.5	91.8
1–3 days	5.9	4.6	4.3
4–7 days	2.1	1.7	1.7
8–10 days	0.1	0.5	0.5
11–14 days	0.7	1.7	1.6
2-week work loss days			
None	90.5	95.7	96.1
1–3 days	6.0	2.4	2.1
4–7 days	1.1	0.5	0.5
8–10 days	0.2	0.8	0.8
11–14 days	2.2	0.5	0.5
2-week cut-down days			
None	89.1	92.0	91.8
1–3 days	6.1	3.2	3.1
4–7 days	2.3	2.0	2.2
8–10 days	0.5	0.5	0.5
11–14 days	1.9	2.3	2.4
2-week doctor visits, person section			
None	79.7	84.8	84.3
1–3 visits	18.5	14.3	14.9
4–7 visits	1.5	0.4	0.4
8–10 visits	0.1	0.1	0.1
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.2	0.2
Physician visits, supplements			
None	78.1	84.6	84.0
1–3 visits	20.4	14.7	15.2
4–7 visits	1.1	0.7	0.7
8–10 visits	0.3	0.0	0.0
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	85.2	85.4	85.8
1 episode	11.9	10.8	10.5
2 episodes	2.2	2.6	2.6
3 episodes	0.5	0.9	0.9
4 episodes	0.0	0.1	0.2
5 episodes	0.1	0.0	0.0
6 episodes or more	0.1	0.1	0.1
Days per hospital episode			
1–3 days	26.5	30.4	27.4
4–7 days	32.4	35.1	36.8
8–10 days	11.8	10.7	11.0
11–14 days	17.6	10.5	10.7
15 days or more	11.8	13.2	14.1
2-week phone calls to doctor			
None	95.7	98.4	98.3
1–3 phone calls	4.1	1.4	1.6
4–7 phone calls	0.2	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.0	0.0

See footnotes at end of table.

Table XXIV. Percent distribution of persons with $5,000–$9,999 family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	93.8	96.0	95.8
1–3 visits	5.9	4.0	4.2
4–7 visits	0.3	0.1	0.1
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	24.2	26.6	26.4
1 visit	16.0	18.0	18.2
2–4 visits	33.6	29.1	29.4
5–12 visits	21.0	20.0	19.7
13–24 visits	3.5	4.2	4.3
25–52 visits	1.4	1.8	1.8
53 visits or more	0.3	0.3	0.3
12-month bed days			
None	45.2	56.3	57.4
1–7 days	35.3	26.9	26.1
8–30 days	12.8	11.2	11.0
31–180 days	3.5	4.0	4.0
181 days or more	0.6	0.9	0.8
Unknown	2.7	0.7	0.7
Time since last doctor visit			
2 weeks	24.1	16.5	17.3
2 weeks–6 months	41.1	44.7	44.2
6–12 months	14.1	12.9	13.0
1 year	9.2	9.9	10.1
2–4 years	4.8	9.1	9.2
5 years or more	3.6	5.2	4.9
Never	0.0	0.4	0.3
Unknown	3.2	1.1	1.1
Time since last dental visit			
2 weeks	6.1	4.0	4.2
2 weeks–6 months	28.5	23.1	24.1
6–12 months	16.9	10.5	10.4
1 year	13.5	12.3	12.1
2–4 years	12.5	18.5	18.0
5 years or more	17.1	27.9	28.3
Never	1.5	2.7	1.9
Unknown	3.9	0.9	1.0
Limitations of activity			
Unable to perform major activity	9.5	10.3	10.7
Limited in kind or amount of major activity	15.2	14.7	15.3
Limited in other activity	8.6	4.7	4.6
Not limited	66.7	70.3	69.4
Conditions			
Mean number of acute	0.204	0.114	0.113
Mean number of chronic	0.649	0.641	0.669
Health status			
Excellent	34.6	31.7	32.2
Good	40.8	43.3	42.3
Fair	17.7	17.6	17.8
Poor	6.2	6.8	7.1
Unknown	0.6	0.6	0.6

[1]950 respondents.
[2]3,073 respondents.
[3]2,690 respondents.

Table XXV. Percent distribution of persons with $10,000–$14,999 family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	91.2	91.9	91.6
1–3 days	7.1	6.0	6.2
4–7 days	1.0	1.1	1.0
8–10 days	0.1	0.3	0.4
11–14 days	0.7	0.8	0.9
2-week work loss days			
None	91.2	95.0	95.0
1–3 days	6.3	3.6	3.6
4–7 days	0.7	0.4	0.4
8–10 days	0.3	0.9	0.9
11–14 days	1.5	0.1	0.1
2-week cut-down days			
None	89.3	93.1	93.0
1–3 days	7.8	3.1	3.2
4–7 days	1.2	1.8	1.9
8–10 days	0.2	0.3	0.3
11–14 days	1.5	1.7	1.7
2-week doctor visits, person section			
None	84.4	87.4	87.3
1–3 visits	14.8	12.2	12.3
4–7 visits	0.7	0.2	0.2
8–10 visits	0.0	0.0	0.0
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.1	0.1
Physician visits, supplements			
None	82.6	86.7	86.5
1–3 visits	16.2	12.9	13.1
4–7 visits	1.1	0.3	0.4
8–10 visits	0.0	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	83.8	86.1	86.0
1 episode	13.2	11.5	11.5
2 episodes	2.0	1.8	1.9
3 episodes	0.6	1.8	1.9
4 episodes	0.2	0.5	0.5
5 episodes	0.1	0.0	0.0
6 episodes or more	0.1	0.1	0.1
Days per hospital episode			
1–3 days	43.8	37.8	37.6
4–7 days	26.6	34.6	34.5
8–10 days	10.9	9.8	9.9
11–14 days	9.4	6.3	6.3
15 days or more	9.4	11.5	11.8
2-week phone calls to doctor			
None	96.7	98.2	98.2
1–3 phone calls	3.1	1.6	1.7
4–7 phone calls	0.2	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.1	0.0	0.0
15 phone calls or more	0.0	0.1	0 1

See footnotes at end of table.

Table XXV. Percent distribution of persons with $10,000–$14,999 family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	93.8	95.7	95.6
1–3 visits	6.2	4.2	4.3
4–7 visits	0.1	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	24.2	26.7	26.5
1 visit	17.4	20.3	20.1
2–4 visits	34.9	29.4	29.7
5–12 visits	19.6	18.0	18.2
13–24 visits	2.9	3.9	3.9
25–52 visits	0.8	1.4	1.3
53 visits or more	0.2	0.3	0.3
12-month bed days			
None	42.7	49.9	49.6
1–7 days	40.1	35.7	36.2
8–30 days	12.8	10.3	10.1
31–180 days	2.6	2.7	2.7
181 days or more	0.5	0.4	0.4
Unknown	1.3	1.0	1.0
Time since last doctor visit			
2 weeks	19.6	14.2	14.3
2 weeks–6 months	40.1	44.2	44.6
6–12 months	18.4	15.4	15.3
1 year	11.4	10.4	10.2
2–4 years	5.7	10.3	10.5
5 years or more	2.2	4.2	4.1
Never	0.1	0.2	0.2
Unknown	2.5	1.0	0.9
Time since last dental visit			
2 weeks	6.6	4.3	4.4
2 weeks–6 months	27.7	25.7	26.4
6–12 months	20.0	13.0	13.1
1 year	16.8	16.7	16.7
2–4 years	12.9	17.5	17.0
5 years or more	12.1	20.1	20.0
Never	0.7	1.5	1.2
Unknown	3.2	1.1	1.1
Limitations of activity			
Unable to perform major activity	3.9	5.4	5.7
Limited in kind or amount of major activity	10.8	8.5	8.7
Limited in other activity	8.6	4.3	4.4
Not limited	76.8	81.9	81.2
Conditions			
Mean number of acute	0.213	0.127	0.128
Mean number of chronic	0.474	0.387	0.396
Health status			
Excellent	38.3	38.3	38.6
Good	45.7	44.9	44.4
Fair	12.0	12.7	12.9
Poor	3.6	3.5	3.4
Unknown	0.4	0.6	0.6

[1] 1,218 respondents.
[2] 2,959 respondents.
[3] 2,736 respondents.

Table XXVI. Percent distribution of persons with $15,000–$24,999 family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	90.8	92.5	92.4
1–3 days	7.1	5.3	5.3
4–7 days	1.4	1.2	1.2
8–10 days	0.3	0.3	0.3
11–14 days	0.4	0.8	0.8
2-week work loss days			
None	92.0	94.5	94.4
1–3 days	5.8	4.0	4.0
4–7 days	1.3	0.7	0.7
8–10 days	0.2	0.7	0.8
11–14 days	0.7	0.1	0.1
2-week cut-down days			
None	89.6	93.4	93.4
1–3 days	7.8	3.9	3.9
4–7 days	1.4	1.7	1.7
8–10 days	0.1	0.1	0.1
11–14 days	1.1	0.9	0.9
2-week doctor visits, person section			
None	86.3	86.9	86.8
1–3 visits	13.4	12.3	12.4
4–7 visits	0.3	0.2	0.2
8–10 visits	0.0	0.1	0.1
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.2	0.2
Physician visits, supplements			
None	84.9	85.9	85.8
1–3 visits	14.6	13.7	13.8
4–7 visits	0.4	0.3	0.3
8–10 visits	0.0	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	87.9	86.1	86.0
1 episode	10.6	11.5	11.5
2 episodes	1.2	1.8	1.9
3 episodes	0.3	0.6	0.6
4 episodes	0.0	0.5	0.5
5 episodes	0.0	0.0	0.0
6 episodes or more	0.0	0.1	0.1
Days per hospital episode			
1–3 days	47.8	41.9	41.1
4–7 days	37.8	36.6	37.2
8–10 days	6.7	10.9	10.9
11–14 days	3.3	5.6	5.8
15 days or more	1.1	4.9	5.0
2-week phone calls to doctor			
None	96.8	97.9	97.9
1–3 phone calls	3.2	1.8	1.9
4–7 phone calls	0.0	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.0	0.0

See footnotes at end of table.

Table XXVI. Percent distribution of persons with $15,000–$24,999 family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	93.2	94.6	94.5
1–3 visits	6.7	5.4	5.5
4–7 visits	0.1	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	25.7	25.8	25.7
1 visit	19.3	22.1	21.9
2–4 visits	36.4	31.1	31.2
5–12 visits	14.8	16.2	16.4
13–24 visits	2.6	3.3	3.3
25–52 visits	1.0	1.2	1.2
53 visits or more	0.2	0.3	0.3
12-month bed days			
None	45.0	52.9	53.0
1–7 days	41.2	35.3	35.2
8–30 days	9.8	8.9	9.0
31–180 days	2.8	2.1	2.1
181 days or more	0.2	0.2	0.2
Unknown	1.0	0.5	0.5
Time since last doctor visit			
2 weeks	17.0	14.9	14.4
2 weeks–6 months	39.0	43.1	42.1
6–12 months	20.0	16.8	18.4
1 year	12.8	10.2	11.4
2–4 years	6.3	9.9	9.6
5 years or more	2.4	4.1	3.0
Never	0.0	0.1	0.1
Unknown	2.5	1.0	1.0
Time since last dental visit			
2 weeks	6.7	5.4	5.5
2 weeks–6 months	37.7	32.9	33.3
6–12 months	19.8	15.5	15.6
1 year	14.6	15.2	15.1
2–4 years	10.1	15.4	15.3
5 years or more	8.3	13.2	13.2
Never	0.5	1.1	0.8
Unknown	2.5	1.3	1.2
Limitations of activity			
Unable to perform major activity	1.5	2.6	2.6
Limited in kind or amount of major activity	9.5	7.2	7.2
Limited in other activity	8.5	3.4	3.4
Not limited	80.5	86.9	86.8
Conditions			
Mean number of acute	0.194	0.122	0.123
Mean number of chronic	0.378	0.290	0.294
Health status			
Excellent	43.4	48.2	47.9
Good	45.4	40.3	40.7
Fair	8.7	9.5	9.4
Poor	1.9	1.7	1.6
Unknown	0.5	0.4	0.4

[1] 2,138 respondents.
[2] 4,785 respondents.
[3] 4,616 respondents.

Table XXVII. Percent distribution of persons with $25,000 or more family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	93.0	93.7	93.7
1–3 days	5.6	4.7	4.7
4–7 days	0.9	1.0	1.0
8–10 days	0.2	0.2	0.3
11–14 days	0.3	0.4	0.4
2-week work loss days			
None	93.1	95.6	95.6
1–3 days	5.4	3.2	3.2
4–7 days	0.7	0.6	0.6
8–10 days	0.2	0.4	0.4
11–14 days	0.6	0.2	0.2
2-week cut-down days			
None	91.7	94.6	94.6
1–3 days	6.5	3.2	3.2
4–7 days	1.2	1.3	1.3
8–10 days	0.0	0.3	0.3
11–14 days	0.6	0.6	0.6
2-week doctor visits, person section			
None	86.5	87.4	87.4
1–3 visits	13.1	12.1	12.1
4–7 visits	0.3	0.2	0.1
8–10 visits	0.0	0.1	0.1
11–14 visits	0.1	0.0	0.0
15 visits or more	0.0	0.0	0.0
Physician visits, supplements			
None	84.8	87.0	87.1
1–3 visits	14.8	12.7	12.7
4–7 visits	0.4	0.2	0.3
8–10 visits	0.0	0.0	0.0
11–14 visits	0.0	0.0	0.0
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	90.8	90.0	90.1
1 episode	8.2	8.4	8.4
2 episodes	0.6	1.2	1.2
3 episodes	0.3	0.2	0.2
4 episodes	0.1	0.1	0.1
5 episodes	0.0	0.1	0.1
6 episodes or more	0.1	0.0	0.0
Days per hospital episode			
1–3 days	38.7	41.9	41.2
4–7 days	29.0	36.7	37.2
8–10 days	6.5	10.9	10.9
11–14 days	16.1	5.7	5.8
15 days or more	8.1	4.9	5.0
2-week phone calls to doctor			
None	96.6	98.2	98.2
1–3 phone calls	3.3	1.5	1.5
4–7 phone calls	0.1	0.0	0.0
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.1	0.1

See footnotes at end of table.

Table XXVII. Percent distribution of persons with $25,000 or more family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	90.7	93.1	93.0
1–3 visits	9.3	6.8	6.9
4–7 visits	0.1	0.1	0.1
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	26.0	26.0	25.9
1 visit	19.8	25.7	25.7
2–4 visits	35.4	30.6	30.6
5–12 visits	15.2	13.7	13.7
13–24 visits	2.7	2.6	2.7
25–52 visits	0.7	1.1	1.1
53 visits or more	0.2	0.3	0.3
12-month bed days			
None	44.4	54.8	54.9
1–7 days	44.0	35.6	35.5
8–30 days	8.9	7.0	7.1
31–180 days	1.6	1.9	1.9
181 days or more	0.3	0.1	0.1
Unknown	0.6	0.6	0.5
Time since last doctor visit			
2 weeks	16.4	14.4	14.4
2 weeks–6 months	40.1	42.0	42.1
6–12 months	20.0	18.4	18.4
1 year	11.8	11.4	11.4
2–4 years	7.3	9.6	9.5
5 years or more	2.2	3.0	3.0
Never	0.0	0.1	0.1
Unknown	2.3	1.1	1.1
Time since last dental visit			
2 weeks	9.4	6.9	7.0
2 weeks–6 months	43.0	40.6	40.7
6–12 months	22.3	17.7	17.8
1 year	12.8	14.3	14.3
2–4 years	6.1	10.3	10.2
5 years or more	3.6	7.9	7.9
Never	0.2	0.5	0.4
Unknown	2.6	1.8	1.8
Limitations of activity			
Unable to perform major activity	1.3	1.6	1.7
Limited in kind or amount of major activity	7.0	5.3	5.3
Limited in other activity	8.0	3.0	3.0
Not limited	83.8	90.0	90.0
Conditions			
Mean number of acute	0.161	0.103	0.102
Mean number of chronic	0.313	0.237	0.238
Health status			
Excellent	55.2	48.1	47.9
Good	36.5	40.3	40.7
Fair	6.4	9.5	9.4
Poor	1.7	1.7	1.7
Unknown	0.2	0.4	0.4

[1] 1,752 respondents.
[2] 5,139 respondents.
[3] 5,067 respondents.

Table XXVIII. Percent distribution of persons with unknown family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week bed days			
None	91.7	92.8	92.8
1–3 days	6.1	4.3	4.2
4–7 days	1.2	1.5	1.4
8–10 days	0.2	0.3	0.3
11–14 days	0.8	1.1	1.2
2-week work loss days			
None	93.3	96.1	96.0
1–3 days	4.5	2.3	2.5
4–7 days	1.0	0.5	0.4
8–10 days	0.2	0.8	0.8
11–14 days	1.0	0.3	0.3
2-week cut-down days			
None	92.0	93.8	93.7
1–3 days	5.1	2.0	2.1
4–7 days	1.2	2.2	2.3
8–10 days	0.4	0.3	0.4
11–14 days	1.3	1.5	0.6
2-week doctor visits, person section			
None	83.5	88.2	88.3
1–3 visits	15.3	10.7	10.7
4–7 visits	0.9	0.3	0.2
8–10 visits	0.1	0.1	0.1
11–14 visits	0.1	0.1	0.1
15 visits or more	0.0	0.2	0.1
Physician visits, supplements			
None	82.1	88.7	88.5
1–3 visits	16.9	10.6	10.9
4–7 visits	0.9	0.6	0.6
8–10 visits	0.1	0.1	0.1
11–14 visits	0.0	0.1	0.1
15 visits or more	0.0	0.0	0.0
12-month hospital episodes, supplements			
None	86.4	89.6	89.6
1 episode	11.5	8.4	8.2
2 episodes	1.6	1.2	1.4
3 episodes	0.3	0.3	0.4
4 episodes	0.1	0.2	0.2
5 episodes	0.0	0.1	0.1
6 episodes or more	0.1	0.2	0.2
Days per hospital episode			
1–3 days	21.3	36.3	36.9
4–7 days	31.1	35.2	33.9
8–10 days	6.6	13.7	13.7
11–14 days	11.5	7.7	8.3
15 days or more	4.9	7.1	7.1
2-week phone calls to doctor			
None	96.9	97.7	97.6
1–3 phone calls	3.1	1.2	1.2
4–7 phone calls	0.0	0.1	0.1
8–10 phone calls	0.0	0.0	0.0
11–14 phone calls	0.0	0.0	0.0
15 phone calls or more	0.0	0.5	0.6

See footnotes at end of table.

Table XXVIII. Percent distribution of persons with unknown family income in the Survey Research Center (SRC) Telephone Survey, in telephone households, and all households in the National Health Interview Survey (NHIS), by selected health characteristics—Con.

Characteristic	SRC Telephone Survey[1]	NHIS personal survey	
		Total[2]	Telephone households[3]
2-week dental visits			
None	93.0	95.4	95.3
1–3 visits	6.8	4.6	4.7
4–7 visits	0.1	0.0	0.0
8–10 visits	0.0	0.0	0.0
12-month doctor visits			
None	31.4	36.0	34.8
1 visit	17.3	18.0	18.6
2–4 visits	31.5	27.9	28.7
5–12 visits	16.1	13.3	13.2
13–24 visits	2.3	3.3	3.5
25–52 visits	1.3	1.2	1.1
53 visits or more	0.1	0.2	0.2
12-month bed days			
None	51.1	59.4	59.4
1–7 days	31.4	26.9	27.4
8–30 days	10.6	8.4	8.6
31–180 days	1.9	2.7	2.4
181 days or more	0.5	0.8	0.7
Unknown	4.5	1.8	1.6
Time since last doctor visit			
2 weeks	20.3	13.3	13.8
2 weeks–6 months	37.1	40.3	40.5
6–12 months	17.6	13.2	13.7
1 year	10.1	12.1	11.4
2–4 years	6.6	10.7	10.7
5 years or more	2.2	6.5	6.1
Never	0.4	0.1	0.1
Unknown	5.7	3.8	3.8
Time since last dental visit			
2 weeks	7.2	4.6	4.7
2 weeks–6 months	30.0	26.9	28.4
6–12 months	16.9	11.9	12.3
1 year	11.8	13.1	12.8
2–4 years	10.0	15.1	15.4
5 years or more	16.7	21.2	20.0
Never	0.8	2.6	2.0
Unknown	6.7	4.7	4.5
Limitations of activity			
Unable to perform major activity	4.6	5.9	5.7
Limited in kind or amount of major activity	13.4	10.1	10.1
Limited in other activity	9.2	4.0	3.3
Not limited	72.8	79.9	80.9
Conditions			
Mean number of acute	0.146	0.101	0.101
Mean number of chronic	0.512	0.399	0.391
Health status			
Excellent	36.3	38.2	39.4
Good	42.0	40.6	41.1
Fair	14.5	15.0	14.0
Poor	4.2	4.0	3.4
Unknown	3.1	2.2	2.1

[1] 1,459 respondents.
[2] 1,628 respondents.
[3] 1,496 respondents.

Appendix IV
Distributions for telephone interviews by experimental treatment

Tables XXIX–XXXIII present weighted data from the Survey Research Center telephone interviews of the civilian noninstitutionalized population. Sample weights are used to adjust for unequal probabilities of selection in the tables. These weights arise from two design features:

1. The existence of multiple telephone numbers in some households requires the weight 1/(number of telephone numbers).
2. The unequal probability of selection as the random respondent, depending upon the size of the family. For the chosen respondent, this weight is equal to the number of eligible adults in the sample household.

For estimates based on the random respondent cases, the overall weight is the product of (1) and (2). For all other groups, only weight (1) is required.

Table XXIX. Percent distribution of persons in the control questionnaire and experimental questionnaire samples, by selected demographic and health characteristics

Characteristic[1]	*Control questionnaire*[2]	*Experimental questionnaire*[3]
Sex		
Male	46.7	46.7
Female	53.2	53.1
Age		
17–24 years	18.3	18.4
25–34 years	23.1	22.6
35–44 years	16.8	16.8
45–54 years	15.4	14.3
55–64 years	12.6	14.2
65–74 years	8.3	8.3
75 years and over	4.0	4.3
Unknown	1.5	1.0
Race		
White	87.2	87.8
All other	12.8	12.2
Education		
0–8 years	11.1	11.0
9–11 years	15.4	16.0
12 years	35.6	36.5
13–16 years	28.7	27.6
17 years or more	6.7	7.3
Unknown	2.5	1.7
Income		
Less than $5,000	8.3	8.8
$5,000–$9,999	11.2	12.4
$10,000–$14,999	14.3	15.7
$15,000–$24,999	26.7	25.4
$25,000 or more	20.7	20.9
Unknown	18.9	16.7
Marital status		
Married	64.9	65.9
Widowed	7.0	6.4
Divorced	6.2	5.9
Separated	1.9	1.7
Single	20.1	20.1
Usual activity		
Working	59.3	59.6
Keeping house	24.4	23.5
Retired, health	1.8	2.3
Retired, other	3.7	4.0
Going to school	7.4	7.1
Something else	2.7	3.1
Unknown	0.6	0.4
2-week bed days		
None	92.6	90.0
1–3 days	5.5	7.4
4–7 days	1.0	1.6
8–10 days	0.1	0.3
11–14 days	0.7	0.7
2-week work loss days		
None	93.6	91.2
1–3 days	4.5	6.2
4–7 days	0.9	1.0
8–10 days	0.1	0.3
11–14 days	0.8	1.3

See footnotes at end of table.

Table XXIX. Percent distribution of persons in the control questionnaire and experimental questionnaire samples, by selected demographic and health characteristics—Con.

Characteristic[1]	*Control questionnaire*[2]	*Experimental questionnaire*[3]
2-week cut-down days		
None	91.7	88.6
1–3 days	5.6	8.1
4–7 days	1.4	1.6
8–10 days	0.2	0.3
11–14 days	1.2	1.5
2-week doctor visits, person section		
None	84.8	83.4
1–3 visits	14.4	15.8
4–7 visits	0.6	0.6
8–10 visits	0.1	0.1
11–14 visits	0.1	0.1
Physician visits, supplements		
None	82.6	82.5
1–3 visits	16.6	16.7
4–7 visits	0.7	0.7
8–10 visits	0.1	0.1
11–14 visits	0.0	0.0
12-month hospital episodes, supplements		
None	86.5	87.5
1 episode	11.4	10.3
2 episodes	1.4	1.6
3 episodes	0.5	0.3
4 episodes	0.1	0.1
5 episodes	0.0	0.1
6 episodes or more	0.0	0.1
2-week phone calls to doctor		
None	96.7	96.4
1–3 phone calls	3.2	3.5
4–7 phone calls	0.1	0.1
8–10 phone calls	0.0	0.0
11–14 phone calls	0.0	0.0
2-week dental visits		
None	93.2	92.6
1–3 visits	6.7	7.3
4–7 visits	0.1	0.1
8–10 visits	0.0	0.0
12-month doctor visits		
None	27.3	25.6
1 visit	18.4	17.5
2–4 visits	34.2	34.4
5–12 visits	15.9	18.2
13–24 visits	2.9	3.1
25–52 visits	1.0	1.1
53 visits or more	0.2	0.2
12-month bed days		
None	48.2	43.7
1–7 days	37.1	39.1
8–30 days	9.7	11.9
31–180 days	2.5	2.8
181 days or more	0.5	0.5
Unknown	2.1	2.1

See footnotes at end of table.

Table XXIX. Percent distribution of persons in the control questionnaire and experimental questionnaire samples, by selected demographic and health characteristics—Con.

Characteristic[1]	*Control questionnaire*[2]	*Experimental questionnaire*[3]
Time since last doctor visit		
2 weeks	19.0	20.2
2 weeks–6 months	39.2	39.2
6–12 months	18.4	17.6
1 year	11.3	11.2
2–4 years	5.9	6.4
5 years or more	2.6	2.5
Never	0.1	0.1
Unknown	3.5	2.8
Time since last dental visit		
2 weeks	6.8	7.6
2 weeks–6 months	33.9	33.4
6–12 months	19.5	18.5
1 year	13.5	14.1
2–4 years	10.1	10.2
5 years or more	11.2	12.2
Never	0.8	0.7
Unknown	4.1	3.4
Limitation of activity		
Unable to perform major activity	3.7	4.8
Limited in major activity	10.2	12.4
Limited in other activity	6.5	10.4
Not limited	79.6	72.4
Conditions		
Acute (mean per person)	0.170	0.196
Chronic (mean per person)	0.421	0.529
Health status		
Excellent	42.1	41.0
Good	42.0	41.4
Fair	11.4	12.4
Poor	3.5	4.1
Unknown	1.0	1.0

[1]The *N*'s reported for the 2 experimental treatments include 55 cases in which the actual treatment differed from the assigned treatments. The actual treatment received is reported in these tables.
[2]4,217 respondents.
[3]3,993 respondents.

Table XXX. Percent distribution of persons in the CATI and non-CATI samples, by selected demographic and health characteristics

Characteristic[1]	*CATI*[2]	*Non-CATI*[3]
Sex		
Male	46.8	46.6
Female	53.0	53.2
Age		
17–24 years	18.1	18.5
25–34 years	23.2	22.6
35–44 years	16.7	16.9
45–54 years	14.6	15.2
55–64 years	14.0	12.9
65–74 years	8.4	8.2
75 years and over	3.8	4.4
Unknown	1.3	1.3
Race		
White	88.5	86.6
All other	11.5	13.4
Education		
0–8 years	11.2	10.9
9–11 years	16.3	15.2
12 years	36.3	35.8
13–16 years	28.3	28.1
17 years or more	6.6	7.3
Unknown	1.4	2.7
Income		
Less than $5,000	8.1	9.0
$5,000–$9,999	11.0	12.4
$10,000–$14,999	14.8	15.1
$15,000–$24,999	26.0	26.1
$25,000 or more	21.1	20.6
Unknown	19.1	16.7
Marital status		
Married	67.0	64.0
Widowed	6.4	7.0
Divorced	5.5	6.5
Separated	1.7	1.9
Single	19.4	20.7
Usual activity		
Working	58.9	60.0
Keeping house	24.2	23.8
Retired, health	2.0	2.1
Retired, other	3.9	3.8
Going to school	7.6	7.0
Something else	3.2	2.7
Unknown	0.4	0.6
2-week bed days		
None	91.2	91.4
1–3 days	6.4	6.4
4–7 days	1.4	1.3
8–10 days	0.3	0.1
11–14 days	0.7	0.7
2-week work loss days		
None	92.7	92.2
1–3 days	4.9	5.7
4–7 days	1.0	0.9
8–10 days	0.3	0.2
11–14 days	1.1	1.0

See footnotes at end of table.

Table XXX. Percent distribution of persons in the CATI and non-CATI samples, by selected demographic and health characteristics—Con.

Characteristic[1]	*CATI*[2]	*Non-CATI*[3]
2-week cut-down days		
None	90.2	90.2
1–3 days	6.9	6.7
4–7 days	1.3	1.6
8–10 days	0.2	0.3
11–14 days	1.4	1.3
2-week doctor visits, person section		
None	84.0	84.3
1–3 visits	15.3	14.9
4–7 visits	0.7	0.6
8–10 visits	0.1	0.1
11–14 visits	0.0	0.1
Physician visits, supplements		
None	82.6	82.5
1–3 visits	16.8	16.6
4–7 visits	0.5	0.9
8–10 visits	0.0	0.1
11–14 visits	0.0	0.0
12-month hospital episodes, supplements		
None	86.5	87.4
1 episode	11.1	10.6
2 episodes	1.6	1.4
3 episodes	0.6	0.3
4 episodes	0.2	0.1
5 episodes	0.0	0.1
6 episodes or more	0.0	0.1
2-week phone calls to doctor		
None	96.0	97.0
1–3 phone calls	3.9	2.8
4–7 phone calls	0.1	0.1
8–10 phone calls	0.0	0.0
11–14 phone calls	0.0	0.0
2-week dental visits		
None	93.3	92.6
1–3 visits	6.6	7.2
4–7 visits	0.1	0.2
8–10 visits	0.0	0.0
12-month doctor visits		
None	27.5	25.5
1 visit	16.3	19.4
2–4 visits	34.5	34.1
5–12 visits	17.7	16.4
13–24 visits	2.6	3.3
25–52 visits	0.9	1.2
53 visits or more	0.5	0.0
12-month bed days		
None	47.5	44.7
1–7 days	36.2	39.5
8–30 days	11.0	10.6
31–180 days	2.9	2.5
181 days or more	0.7	0.4
Unknown	1.8	2.3

See footnotes at end of table.

Table XXX. Percent distribution of persons in the CATI and non-CATI samples, by selected demographic and health characteristics—Con.

Characteristic[1]	*CATI*[2]	*Non-CATI*[3]
Time since last doctor visit		
2 weeks	20.1	19.2
2 weeks–6 months	40.8	37.8
6–12 months	17.4	18.6
1 year	10.5	11.9
2–4 years	6.7	5.7
5 years or more	2.4	2.7
Never	0.1	0.1
Unknown	2.1	4.1
Time since last dental visit		
2 weeks	7.2	7.3
2 weeks–6 months	34.1	33.4
6–12 months	19.3	19.0
1 year	13.6	13.8
2–4 years	9.5	10.7
5 years or more	12.2	11.2
Never	0.7	0.8
Unknown	3.4	3.9
Limitation of activity		
Unable to perform major activity	4.2	4.2
Limited in major activity	11.4	11.2
Limited in other activity	8.4	8.4
Not limited	75.9	76.2
Conditions		
Acute (mean per person)	0.171	0.194
Chronic (mean per person)	0.460	0.487
Health status		
Excellent	41.8	41.3
Good	41.2	42.2
Fair	12.4	11.5
Poor	3.6	3.9
Unknown	1.0	1.0

[1]The *N*'s reported for the 2 modes of data collection represent the actual mode used rather than the assigned mode. The assigned and actual treatments differ in that 396 cases assigned CATI were completed non-CATI and 20 cases assigned to non-CATI were completed CATI.
[2]3,759 respondents.
[3]4,451 respondents.

Table XXXI. Percent distribution of random respondents and persons in knowledgeable respondent families, by selected demographic and health characteristics: Total Survey Research Center sample

Characteristic	*Random respondent, respondent only*[1,2]	*Knowledgeable respondent, entire family*[3]
Sex		
Male	43.8	46.4
Female	56.8	53.5
Age		
17–24 years	14.9	18.2
25–34 years	23.9	23.1
35–44 years	18.3	17.0
45–54 years	16.7	14.7
55–64 years	13.7	13.3
65–74 years	8.3	8.0
75 years and over	3.0	4.3
Unknown	1.2	1.3
Race		
White	87.9	86.8
All other	12.1	13.2
Education		
0–8 years	9.9	11.1
9–11 years	13.3	15.9
12 years	36.2	36.2
13–16 years	32.3	27.4
17 years or more	7.3	7.0
Unknown	1.0	2.3
Income		
Less than $5,000	8.6	8.8
$5,000–$9,999	13.2	10.2
$10,000–$14,999	14.9	15.3
$15,000–$24,999	27.1	25.2
$25,000 or more	20.8	21.0
Unknown	15.3	19.4
Marital status		
Married	67.9	65.3
Widowed	6.0	7.2
Divorced	6.8	5.7
Separated	2.2	1.9
Single	17.1	19.9
Usual activity		
Working	61.7	58.5
Keeping house	26.7	24.3
Retired, health	1.3	2.2
Retired, other	3.6	3.9
Going to school	4.2	7.6
Something else	2.1	2.9
Unknown	0.3	0.5
2-week bed days		
None	94.2	90.7
1–3 days	4.5	7.1
4–7 days	0.7	1.4
8–10 days	0.2	0.2
11–14 days	0.4	0.6
2-week work loss days		
None	93.9	92.0
1–3 days	4.3	5.9
4–7 days	0.9	0.8
8–10 days	0.3	0.3
11–14 days	0.6	1.1

See footnotes at end of table.

Table XXXI. Percent distribution of random respondents and persons in knowledgeable respondent families, by selected demographic and health characteristics: Total Survey Research Center sample—Con.

Characteristic	*Random respondent, respondent only*[1,2]	*Knowledgeable respondent, entire family*[3]
2-week cut-down days		
None	91.3	89.6
1–3 days	6.3	7.3
4–7 days	1.1	1.6
8–10 days	0.2	0.2
11–14 days	1.2	1.2
2-week doctor visits, person section		
None	85.0	84.1
1–3 visits	14.5	15.2
4–7 visits	0.4	0.6
8–10 visits	0.1	0.1
11–14 visits	0.0	0.1
Physician visits, supplements		
None	83.9	82.2
1–3 visits	15.4	17.1
4–7 visits	0.7	0.6
8–10 visits	0.1	0.1
11–14 visits	0.0	0.0
12-month hospital episodes, supplements		
None	87.3	86.7
1 episode	10.9	11.4
2 episodes	1.2	1.4
3 episodes	0.4	0.4
4 episodes	0.1	0.1
5 episodes	0.1	0.0
6 episodes or more	0.1	0.0
2-week phone calls to doctor		
None	97.4	96.3
1–3 phone calls	2.5	3.5
4–7 phone calls	0.1	0.1
8–10 phone calls	0.0	0.0
11–14 phone calls	0.0	0.0
2-week dental visits		
None	93.7	92.9
1–3 visits	6.2	7.0
4–7 visits	0.1	0.1
8–10 visits	0.0	0.0
12-month doctor visits		
None	24.1	27.1
1 visit	18.0	17.6
2–4 visits	36.6	33.3
5–12 visits	17.6	17.2
13–24 visits	2.6	3.2
25–52 visits	1.0	1.2
53 visits or more	0.1	0.3
12-month bed days		
None	46.4	45.3
1–7 days	39.4	38.4
8–30 days	10.8	10.9
31–180 days	1.9	2.8
181 days or more	0.1	0.5
Unknown	1.4	2.2

See footnotes at end of table.

Table XXXI. Percent distribution of random respondents and persons in knowledgeable respondent families, by selected demographic and health characteristics: Total Survey Research Center sample—Con.

Characteristic	*Random respondent, respondent only*[1,2]	*Knowledgeable respondent, entire family*[3]
Time since last doctor visit		
2 weeks	18.5	19.5
2 weeks–6 months	41.1	39.4
6–12 months	18.7	17.8
1 year	10.6	11.3
2–4 years	7.0	5.9
5 years or more	2.4	2.8
Never	0.0	0.1
Unknown	1.8	3.2
Time since last dental visit		
2 weeks	6.6	7.0
2 weeks–6 months	32.1	34.6
6–12 months	20.8	18.5
1 year	14.7	13.4
2–4 years	10.7	10.0
5 years or more	11.8	12.3
Never	0.5	0.8
Unknown	2.8	3.4
Limitation of activity		
Unable to perform major activity	2.7	4.3
Limited in major activity	11.5	11.1
Limited in other activity	10.7	8.0
Not limited	75.1	76.7
Conditions		
Acute (mean per person)	0.163	0.189
Chronic (mean per person)	0.500	0.464
Health status		
Excellent	41.6	41.8
Good	43.9	40.8
Fair	11.4	12.2
Poor	2.3	4.1
Unknown	1.2	1.2

[1]The data for 219 people (95 families) assigned to the random respondent rule, but for whom information was obtained from the knowledgeable respondent rule, are deleted from these estimates.
[2]2,079 respondents.
[3]4,127 respondents.

Table XXXII. Percent distribution of random respondents and persons in knowledgeable respondent families, by selected demographic and health characteristics: Control questionnaire sample

Characteristic[1]	*Random respondent, respondent only*[2,3]	*Knowledgeable respondent, entire family*[4]
Sex		
Male	43.8	46.6
Female	56.0	53.3
Age		
17–24 years	15.9	17.9
25–34 years	24.1	22.9
35–44 years	17.8	17.2
45–54 years	16.6	15.3
55–64 years	13.0	12.5
65–74 years	8.4	8.3
75 years and over	3.0	4.2
Unknown	1.2	1.8
Race		
White	87.1	87.6
All other	12.9	12.4
Education		
0–8 years	10.3	10.9
9–11 years	13.5	15.4
12 years	35.3	35.8
13–16 years	33.4	28.1
17 years or more	6.5	6.8
Unknown	1.0	2.8
Income		
Less than $5,000	8.1	8.8
$5,000–$9,999	12.8	9.6
$10,000–$14,999	14.4	14.4
$15,000–$24,999	26.7	26.6
$25,000 or more	21.8	19.7
Unknown	16.2	20.9
Marital status		
Married	68.5	63.6
Widowed	6.2	7.6
Divorced	6.4	6.4
Separated	1.9	2.0
Single	17.0	20.4
Usual activity		
Working	62.4	57.9
Keeping house	26.3	25.1
Retired, health	1.1	2.1
Retired, other	3.8	3.7
Going to school	3.9	7.7
Something else	2.0	2.9
Unknown	0.4	0.7
2-week bed days		
None	95.1	92.2
1–3 days	3.5	6.1
4–7 days	0.7	0.9
8–10 days	0.2	0.1
11–14 days	0.5	0.6
2-week work loss days		
None	94.9	93.3
1–3 days	3.7	4.8
4–7 days	1.1	0.9
8–10 days	0.1	0.2
11–14 days	0.3	0.9

See footnotes at end of table.

Table XXXII. Percent distribution of random respondents and persons in knowledgeable respondent families, by selected demographic and health characteristics: Control questionnaire sample—Con.

Characteristic[1]	*Random respondent, respondent only*[2,3]	*Knowledgeable respondent, entire family*[4]
2-week cut-down days		
None	92.4	91.2
1–3 days	5.8	6.0
4–7 days	0.9	1.5
8–10 days	0.2	0.1
11–14 days	0.7	1.2
2-week doctor visits, person section		
None	86.0	84.6
1–3 visits	13.6	14.7
4–7 visits	0.3	0.6
8–10 visits	0.1	0.1
11–14 visits	0.0	0.0
Physician visits, supplements		
None	84.0	82.2
1–3 visits	15.5	16.9
4–7 visits	0.4	0.8
8–10 visits	0.1	0.1
11–14 visits	0.0	0.0
12-month hospital episodes, supplements		
None	86.3	86.1
1 episode	12.3	11.9
2 episodes	1.0	1.3
3 episodes	0.3	0.6
4 episodes	0.1	0.0
5 episodes	0.0	0.0
6 episodes or more	0.0	0.0
2-week phone calls to doctor		
None	97.6	96.7
1–3 phone calls	2.2	3.2
4–7 phone calls	0.2	0.0
8–10 phone calls	0.0	0.0
11–14 phone calls	0.0	0.0
2-week dental visits		
None	93.9	93.3
1–3 visits	5.9	6.6
4–7 visits	0.2	0.1
8–10 visits	0.0	0.0
12-month doctor visits		
None	25.9	27.6
1 visit	18.6	18.1
2–4 visits	36.1	33.3
5–12 visits	15.6	16.5
13–24 visits	3.1	3.0
25–52 visits	0.6	1.3
53 visits or more	0.2	0.2
12-month bed days		
None	48.0	47.6
1–7 days	39.0	37.8
8–30 days	9.8	9.4
31–180 days	1.4	2.6
181 days or more	0.4	0.6
Unknown	1.5	2.1

See footnotes at end of table.

Table XXXII. Percent distribution of random respondents and persons in knowledgeable respondent families, by selected demographic and health characteristics: Control questionnaire sample—Con.

Characteristic[1]	*Random respondent, respondent only*[2,3]	*Knowledgeable respondent, entire family*[4]
Time since last doctor visit		
2 weeks	17.6	19.2
2 weeks–6 months	41.1	38.8
6–12 months	19.4	18.5
1 year	10.1	11.4
2–4 years	7.0	5.5
5 years or more	3.1	2.6
Never	0.1	0.1
Unknown	1.5	3.7
Time since last dental visit		
2 weeks	5.9	6.8
2 weeks–6 months	31.3	35.3
6–12 months	21.4	18.7
1 year	14.3	13.4
2–4 years	11.7	9.6
5 years or more	11.8	11.9
Never	0.5	0.8
Unknown	3.1	3.4
Limitation of activity		
Unable to perform major activity	2.1	4.1
Limited in major activity	11.6	9.8
Limited in other activity	7.8	6.3
Not limited	78.5	79.8
Conditions		
Acute (mean per person)	0.161	0.175
Chronic (mean per person)	0.437	0.421
Health status		
Excellent	42.4	42.1
Good	43.7	40.7
Fair	11.8	11.8
Poor	1.5	3.9
Unknown	0.6	1.5

[1]The *N's* reported for 2 experimental treatments include 55 cases in which the actual treatment differed from assigned treatments. The actual treatment received is reported in these tables.
[2]Data for 219 people (95 families) assigned to the random respondent rule, but for whom information was obtained from the knowledgeable respondent rule, are deleted from these estimates.
[3]1,068 respondents.
[4]2,099 respondents.

Table XXXIII. Percent distribution of random respondents and persons in knowledgeable respondent families, by selected demographic and health characteristics: Experimental questionnaire sample

Characteristic[1]	*Random respondent, respondent only*[2,3]	*Knowledgeable respondent, entire family*[4]
Sex		
Male	41.7	46.1
Female	57.7	53.7
Age		
17–24 years	13.9	18.6
25–34 years	23.7	23.4
35–44 years	18.8	16.7
45–54 years	16.8	14.1
55–64 years	14.4	14.1
65–74 years	8.2	7.8
75 years and over	3.0	4.5
Unknown	1.1	0.8
Race		
White	88.7	85.9
All other	11.3	14.1
Education		
0–8 years	9.3	11.3
9–11 years	13.1	16.5
12 years	37.2	36.7
13–16 years	31.1	26.6
17 years or more	8.3	7.2
Unknown	1.0	1.7
Income		
Less than $5,000	9.2	8.8
$5,000–$9,999	13.7	10.9
$10,000–$14,999	15.5	16.3
$15,000–$24,999	27.6	23.8
$25,000 or more	19.8	22.4
Unknown	14.3	17.8
Marital status		
Married	67.3	67.0
Widowed	5.7	6.9
Divorced	7.3	5.0
Separated	2.5	1.8
Single	17.3	19.4
Usual activity		
Working	61.0	59.2
Keeping house	27.1	23.5
Retired, health	1.5	2.3
Retired, other	3.4	4.2
Going to school	4.4	7.6
Something else	2.3	2.8
Unknown	0.2	0.4
2-week bed days		
None	93.1	89.1
1–3 days	5.6	8.1
4–7 days	0.7	1.9
8–10 days	0.3	0.3
11–14 days	0.3	0.6
2-week work loss days		
None	92.8	90.6
1–3 days	4.9	7.1
4–7 days	0.8	0.7
8–10 days	0.4	0.3
11–14 days	1.0	1.3

See footnotes at end of table.

Table XXXIII. Percent distribution of random respondents and persons in knowledgeable respondent families, by selected demographic and health characteristics: Experimental questionnaire sample—Con.

Characteristic[1]	*Random respondent, respondent only*[2,3]	*Knowledgeable respondent, entire family*[4]
2-week cut-down days		
None	90.2	88.0
1–3 days	6.7	8.7
4–7 days	1.3	1.7
8–10 days	0.2	0.4
11–14 days	1.6	1.2
2-week doctor visits, person section		
None	83.9	83.5
1–3 visits	15.4	15.8
4–7 visits	0.6	0.5
8–10 visits	0.0	0.1
11–14 visits	0.0	0.1
Physician visits, supplements		
None	83.7	82.2
1–3 visits	15.2	17.3
4–7 visits	1.1	0.4
8–10 visits	0.0	0.1
11–14 visits	0.0	0.1
12-month hospital episodes, supplements		
None	88.3	87.3
1 episode	9.4	10.8
2 episodes	1.5	1.5
3 episodes	0.5	0.2
4 episodes	0.1	0.2
5 episodes	0.1	0.0
6 episodes or more	0.2	0.1
2-week phone calls to doctor		
None	97.2	96.0
1–3 phone calls	2.8	3.8
4–7 phone calls	0.0	0.2
8–10 phone calls	0.0	0.0
11–14 phone calls	0.0	0.1
2-week dental visits		
None	93.4	92.6
1–3 visits	6.5	7.3
4–7 visits	0.1	0.1
8–10 visits	0.0	0.0
12-month doctor visits		
None	22.0	26.6
1 visit	17.3	17.3
2–4 visits	37.2	33.2
5–12 visits	19.8	18.1
13–24 visits	2.2	3.5
25–52 visits	1.4	1.0
53 visits or more	0.1	0.4
12-month bed days		
None	44.6	42.9
1–7 days	39.7	39.1
8–30 days	12.0	12.3
31–180 days	2.4	3.1
181 days or more	0.2	0.4
Unknown	1.1	2.3

See footnotes at end of table.

Table XXXIII. Percent distribution of random respondents and persons in knowledgeable respondent families, by selected demographic and health characteristics: Experimental questionnaire sample—Con.

Characteristic[1]	*Random respondent, respondent only*[2,3]	*Knowledgeable respondent, entire family*[4]
Time since last doctor visit		
2 weeks	19.3	2.0
2 weeks–6 months	41.1	39.9
6–12 months	17.9	17.1
1 year	11.1	11.2
2–4 years	7.0	6.3
5 years or more	1.6	3.0
Never	0.0	0.1
Unknown	2.1	2.6
Time since last dental visit		
2 weeks	7.2	7.3
2 weeks–6 months	33.1	33.8
6–12 months	20.2	18.5
1 year	15.2	13.4
2–4 years	9.6	10.4
5 years or more	11.8	12.7
Never	0.4	0.9
Unknown	2.5	3.4
Limitation of activity		
Unable to perform major activity	3.4	4.4
Limited in major activity	11.4	12.4
Limited in other activity	13.8	9.7
Not limited	71.5	73.5
Conditions		
Acute (mean per person)	0.165	0.203
Chronic (mean per person)	0.569	0.505
Health status		
Excellent	40.8	41.5
Good	43.9	40.9
Fair	11.0	12.6
Poor	3.1	4.2
Unknown	1.2	0.9

[1]The *N*'s reported for 2 experimental treatments include 55 cases in which the actual treatment differed from assigned treatments. The actual treatment received is reported in these tables.
[2]Data for 219 people (95 families) assigned to the random respondent rule, but for whom information was obtained from the knowledgeable respondent rule, are deleted from these estimates.
[3]1,011 respondents.
[4]2,028 respondents.

Appendix V
Interviewer instructions for the Survey Research Center Telephone Survey

Selected interviewer instructions related to the experimental interviewing techniques are provided in this appendix.

C. Background of this study

This research is done under a contract between the National Center for Health Statistics of the U.S. Public Health Service (USPHS) and the University of Michigan. This is an experimental study designed to investigate similarities and differences between interviews taken in person and on the telephone.

The Health Interview Survey is a large national survey, with interviews taken every week of the year by the Bureau of the Census for the U.S. PHS. We have adapted the questionnaire used in that survey to telephone interview format. The information from the two surveys (ours and Census) will be compared to see how comparable the data are, to examine how and on what items results appear to differ (if they do) and to attempt to understand why they are different.

A second major objective of this study is a comparison of interviews taken using a traditional "hard-copy" questionnaire with a computer-based questionnaire in which questions are read from a computer terminal screen and answers entered on a console directly into the computer. You will be using both the hard copy and computer-based questionnaire in your work.

A third major objective is a comparison of information using various interviewing techniques. You will use two forms of the questions; one using "standard" procedures and the other a set of experimental techniques.

A fourth major objective of the study is a comparison of results that different interviewers obtain asking the same questions.

D. The experimental techniques

Scientific knowledge is generated through testing of hypotheses. Our experimental interviewing techniques are designed to provide data for testing hypotheses concerning the effects of various techniques on the quality of data in responses in a telephone survey interview.

In general, hypotheses are stated as basic questions which the dictionary calls provisional "conjecture to guide investigation." It is also the case in this research, as in much of social science investigation, that there are *competing* hypotheses. One hypothesis predicts that the findings will show the effects of techniques as being beneficial based on well founded reasons, and the opposite hypothesis predicting that the technique will make the data less good, due to another set of equally well founded reasons.

We can state the hypotheses guiding this research. We will experiment with three interviewing techniques; commitment, instructions to respondents, and feedback.

Commitment asks the respondent to agree to think hard and work diligently. One hypothesis is that such commitment results in harder work by the respondent and better data. The opposite hypothesis is that the respondent has already accepted the task and that asking for further commitment is demeaning and sets up negative reactions, resulting in poorer data.

Instructions. A reasonable hypothesis is that respondents do not know what is expected of them or how to process information and that specific instructions will thus improve data quality. The opposing hypothesis is that respondents obviously know how to answer questions and it is perceived as insulting to their intelligence to tell them how to respond. The procedures without instructions may thus produce higher quality data.

Feedback. Interviewers frequently give feedback to respondents. By "feedback" we refer to the comments made by interviewers reacting to responses, "I see," "Um hmm," etc. In this study the feedback we are using is frequently given, varied in form, and standardized in content. The hypothesis is that the "programmed" feedback is conducive to hard work and better data because it both provides information by which respondents evaluate their performance and because it is rewarding and motivates respondents to be more active in their role. The counter hypothesis is that the feedbacks may be perceived as inappropriate and patronizing. If this is so, the effects are negative and will yield poorer data.

This survey is designed to measure the effects of these experimental interviewing and question techniques on the quality of information obtained from respondents. Because of this, some of the rules and procedures differ from those of the usual survey. Also, because we are primarily interested in ascertaining the effects of the experimental techniques, we will be *very* rigid in the use of the techniques. We need to be sure that if differences are found, they are due to the experimental techniques and not the variability in how the techniques were used or to other interviewer differences in behavior. If we appear overly strict about the interviewing, this is the reason.

Section II. Interviewing techniques

A. General interviewer instructions

One goal of survey research is to gather information from a small group of people so that we know more about a larger group of people. It is very important that the small group be

selected so that the larger group is well represented. In survey research respondents are chosen through some system creating a random or probability sample of the larger group.

In this study, the sample consists of telephone numbers generated by a computer such that they are representative of all telephone numbers in the continental United States excluding Alaska. This means that responses from this "random sample" of telephone numbers will truly reflect the responses which would be obtained if *all* telephone households were interviewed and report accurately. That is, if a quarter of the sample say that they were sick last week, or 5% report that they consulted a doctor, these figures reflect the behavior of the United States population. This will be true *only* if all of the research procedures are carried out properly.

Elaborate rules and procedures are required for accurate measurement to insure that our small group truly reflects the entire population. It is for this reason that you will find we are very strict in the application of procedures.

We must be careful to measure each respondent's feelings and behaviors in the same way if we want the results to generalize accurately to the population. If we ask some questions of part of the sample (or small group) and other questions of the rest of the sample, we ruin the scientific procedures which guarantee that the sample truly represents the larger group. For example, we cannot report how people visit doctors if we don't ask everyone in the sample a question about visits to doctors. Similarly, we must ask the question in the same way for each person. When the same question is asked in two different ways, it actually becomes two different questions. If we want to talk about how the large group would answer a question, then we must make sure *each* person in the sample is asked the *same question* in the *same way.*

This brings us to interviewing—the procedures by which the questions are posed and the techniques by which interaction with the respondent is guided and directed. A major issue in interviewing is that each interviewer is different and creates a different interactive pattern with the respondent. Some people have suggested that interviewing by machine might avoid these individual differences. A machine could be programmed to say the questions and then wait for the respondent to answer into a recording device. The trouble with this is that the human touch is often needed to determine whether the respondent understood the question (it might need to be repeated) or whether the respondent has said enough to fully answer the question. Instead of reporting visits to physicians, perhaps the respondent begins to talk about dentists. It would be impossible to program our machine to deal fully with that situation. (Additionally, most respondents would probably much rather interact with a person rather than with a machine!)

So, where are we? Our goal is to have standard questions; yet, interviewers are human. Therefore, we try to direct and control the interviewers in several ways so that their behavior will be as much alike as possible. All these constraints are described in the general instructions, and you should understand that they are designed to insure that the questions are asked the *same* way for each respondent.

The questionnaire in a survey is the measuring instrument. Think of this example: a doctor takes Fred's blood pressure at his office. Fred then walks across town to another doctor and has his blood pressure taken once again. If Fred's blood pressure is higher the second time, under what circumstances can we say Fred's pressure actually went up? Only if both instruments are used properly. Both instruments must be adjusted in the same manner. Both gauges must be read correctly. To get the same quality of reading from each respondent, interviewers (like the doctor) must measure the respondent using proper procedures—the same questions, the same probing for clearer answers, and the same professional manner.

Sometimes the researchers have worded a question awkwardly. But it is still important that interviewers adhere strictly to the question as it is written so that all respondents answer the same awkward question rather than several other versions of it. Most procedures are straightforward as well as important for standardization, such as speaking slowly and clearly so that the respondent will hear the question. Because we feel this standardization is so important to assure that we are measuring each response in the same manner, we have tried to standardize much of the interviewer's speech and actions. So when we insist that you use exact words in interacting with the respondent, you will realize why.

B. Interviewing techniques

The goal in interviewing is for each interviewer to use techniques in exactly the same way. This is the essence of good measurement in any science, that the processes of measurement are so controlled and standardized that the results obtained do not vary depending on which interviewer took the interview. The principles and rules which follow are to help insure the comparability between interviewers.

1. Ask the questions exactly as they are worded in the questionnaire. Since exactly the same questions must be asked of each respondent, you should not make changes in their phrasing. Avoid not only deliberate word changes, but also inadvertent ones. In an effort to be conversational you may unwittingly leave out part ot a question or change some of the words; or you may ask the question just as it is worded, adding just a few words at the end of a question. The respondent's answer is prompted by the words in the question, and a change in wording can very easily produce a change in response. So, read the questions *exactly* as they are written and if the respondent starts to respond *while* you are reading a question, continue reading until you have read the entire question.
2. If you are using "hard copy" (pencil and paper copy) ask the questions in the order in which they appear in the questionnaire. The question sequence is designed to create a sense of continuity and to ensure that early questions will not have a harmful effect on the respondent's answers to later questions. Furthermore, question order needs to be standardized from respondent to respondent if the interviews are to be comparable.
3. Ask every question specified in the questionnaire. In answering one question, a respondent will sometimes also answer another question which appears later in the interview. Or, from time to time, when an interviewer needs to

ask a series of apparently similar questions, the respondent may say, "Just put me down as 'Yes' to all of them." In these cases, you may wonder whether you should skip the questions which are apparently answered. YOU SHOULD NOT. It is your responsibility to make certain, wherever possible, that the respondent is fully exposed to each question specified in the questionnaire.

4. There is one exception to the rule of asking every question. You may skip a question when the respondent has given a *specific and complete* response to a simple *factual* item before the question is asked. For example: In the questions on education in response to the first question the respondent says, "I graduated from college, got my B.A. degree, and then went on and got a Ph.D." You should code all the education questions without asking other questions. Similarly if R says that she saw a doctor yesterday, you do not need to ask the question "Did you see a doctor this week or last week." Simply code the response.

Occasionally a respondent will give a complete answer to two or three questions at one time. On questions of fact where a complete and exact response has been given, the answer can be recorded without asking. In cases where a partial response was given or the question asks for options, not facts, the question must be asked.

Principle: Skip a question *only* when it asks for facts (not opinions) and only when it is fully answered in preceding questions.

C. Pace

Studies in interviewing methodology indicate that the ideal reading pace is about *two words per second.* Even if you read a question correctly, it does not do much good if the words are all pushed together in a rush or lost in a mumble. A slow and deliberate pace gives the respondent time to understand the full scope of the question and to formulate a careful reply.

It is also important to read slowly for other reasons. A slow pace communicates the importance of considering the questions carefully. The respondent will take a more serious attitude when the pace is slow and deliberate.

The slow pace communicates that the interviewer is interested in hearing the respondent's answers. A respondent will try harder if he/she believes that answers are truly interesting to the interviewer, and the slow pace is a useful way in which to communicate this interest.

You may feel at the beginning that a slow pace sounds unnatural. But familiarity with the questions and several practice sessions on inflection should give your speaking voice the naturalness it needs for the slower pace. *Do* spend some time with the tape recorder, practicing portions of the questionnaire and listening to the way your voice sounds, until you are satisfied with it.

A common reason for a pace that is too quick is a respondent saying, "I only have a few minutes so you'll have to hurry up." Do not let the respondent hurry you in this manner. If the interviewer hurries through the questions, the respondent tends to hurry also to the point of answering a question before the interviewer finishes reading it.

Although you will become very familiar with the questionnaire during the course of a study, you must remember that it is all new to each respondent, and each should be given an equal chance to understand and respond to all of the questions.

Proper pace also requires proper timing between the end of a response and the next interviewer behavior. Feedback may either encourage or close off further response depending on the timing. Some pause should always be allowed prior to a feedback or the asking of the next question.

D. Inflection in reading questions

Especially important, together with a slow pace, is inflection. Watch the rising and declining tones in your voice so that the questions sound important, but natural. Questions in everyday speaking often have a rising tone in the last phrase or word. You can encourage answers by letting your voice rise on the last word of a question.

Many of the questions have underlined words. The purpose is that not only the words but the emphases are similar for all interviews. Practice so that you can read the questions, emphasizing the underlined words in a natural manner.

E. Naturalness in reading questions and feedback statements

Perhaps the most difficult of the interviewing tasks is to ask the questions and give feedback statements so that they sound natural. Especially, the feedback statement *must* sound spontaneous, as though you just thought of it. If these statements sound artificial, as though they are read from the questionnaire, they are ineffective. Moreover, you will feel embarrassed or uneasy using them because they sound unnatural. Most of us find some of the feedbacks awkward at first. "I would never say that!" or "I feel peculiar saying that," or "I'll just never be able to say that properly."

To overcome this, you must approach the task as an actor does a play. *Learn* the statements and *practice* them. Use a tape recorder, read questions and feedbacks and listen. Do they sound spontaneous and natural? If not, why? Practice again. Soon you will find that they are part of your "interviewer role" and are comfortable for you to use.

These are general interviewing principles which apply to all forms of the questionnaire. Other techniques vary depending on which questionnaire form is being used. These differences are described in the following pages.

F. Special techniques for questionnaire forms EP and EO

These forms use specified commitment, instructions, and feedback procedures.

1. *Commitment.* At the beginning of the questionnaire you will find a statement to be read to the respondent asking whether he or she is willing to work hard and be diligent in the interview. If such assurance is not given we will conclude the interview.
2. *Instructions.* In many places in the questionnaire you will find that questions contain special instructions on what is required for an adequate response. It may be instructions

on how to process information, a warning that the task is difficult, etc. These are part of the question and are to be read as worded.

3. *Feedback.* The most obvious difference between this questionnaire and others you may have used is that practically every response is followed by a comment to the respondent about that response. There are two types of feedback statements. One we call "short feedback" (I see, Um hmm). These are to be used as indicated by an **F** on the questionnaire. When **F** appears, you select an appropriate brief comment from a list that will be supplied. "Long feedback" statements ("Thanks, that is useful information") are included in the questionnaire and are to be used as written.
4. *When to give feedback.* For feedback statements to be effective, they must meet three criteria:

   (a) Sound spontaneous, not read.
   (b) Be given at the appropriate time.
   (c) Be given so as not to close off responses.

   Spontaneity in giving feedback is achieved simply by "learning" the statement and saying it as though you meant it. This comes only with practice and familiarity. Timing is important. If feedback is given before R has completed the response, it is likely to cut off added response. Therefore, allow some pause after the response to be sure R has finished. (The timing of feedbacks is similar to that of a comedian delivering the punch line. Its effectiveness all depends on timing.)

   Principle: Both timing and naturalness of delivery of feedback statements are crucial to their effectiveness.

5. *Use of additional short feedback statements.* When a response has a feedback (short or long) connected with it, an additional short feedback may be given when the respondent makes a lengthy statement following the original feedback. For example, R reports conditions and is given a long feedback. Following this, R continues describing the condition in more detail, giving relevant information. A short feedback should be given at the conclusion of these added comments.

   If, however, the added information is *not* relevant (talks about someone else, about the medical system in general, etc.) no feedback should be given.

   Principle: R is given an initial positive feedback for good reporting behavior. If good behavior continues it should be recognized. But, we do not want to provide positive feedback for poor performance. Therefore, irrelevant material should not receive positive feedback.

6. *Special note.* There may be certain times in your interviewing experience when the feedback you are to give is so inappropriate that it is embarrassing or insulting. Perhaps a cancer patient tells you he or she is about to die. Certainly the feedback, "Thanks, this is useful information" would seem very inappropriate. In such cases, *do not* use the specified feedback, but select an appropriate one from the short feedback list.

G. Special techniques for questionnaire form ST

This form contains exactly the same questions as do the others but they do not include instructions or feedbacks. Neither is a commitment statement used.

In using this form you must stay strictly to the questions as worded and *not* include any instructions and you *must not* use any feedback statements.

With the exception of these techniques, the procedures used in interviewing are the same regardless of the particular form you are using.

H. Clarification and definitions for respondent

There may be times when a response doesn't quite fit the pattern we have set up in the instrument. These situations are difficult to anticipate and so we cannot standardize procedures to straighten things out. But we do want to formulate rules for you to follow so that we can insure some comparability between interviewers. This goes back to the notion of *standardizing* the measurement process so that we can be sure that each respondent gets the same (or very nearly the same) interview experience. Here, then, are some difficult situations and what you should use in each of them.

1. *Respondent questions.* One of the responses that could give you trouble during the course of the interview is a respondent inquiry about the meaning or intent of the question you have just asked. For example, after you have asked whether R stayed in bed because of illness or injury he/she says, "Well, I wasn't feeling very well, do you call that an illness?" or "What do you mean by medical care?" There are three "rules" to guide your response:

a. If R asks you the meaning of the questions, you cannot provide a definition but must leave it to R to define for him/herself.

   Example:

   Interviewer: "Are you limited in the kind or amount of other activities because of your health?"
   A: "What do you mean by kind of activities?"
   Interviewer: "Whatever it means to you," or "Whatever you think should be included here."

b. For some questions the Q-by-Q instructions provide specific definitions for terms. For these questions if R asks whether something is or is not to be included, you can provide the information.

   Example:

   Q: "Is a chiropractor included here?"
   A: "No" (See instructions for questions 20–29)

   Example:
   Q: "Well I was bitten by a dog. Is that considered an injury?"
   A: "Yes" (See instructions for questions 16–17)

c. For questions without specific definitions in the Q-by-Q's do not provide any definition for the respondent, but say

"Whatever it means to you." or "Whatever you think should be included."

2. *Other respondent questions.* R may make a general statement like "I'm not sure what you mean," or "How do you mean that?" or "I don't understand the question." Your first, and probably most effective response is to *repeat the original question.* It may be that R didn't hear all of the words, or wasn't paying complete attention. Repeating the question may clear up this kind of confusion quickly and easily. Some situations will only require that you repeat part of the question in order to have R understand it. You must, however, *not change the original wording.*

3. *Irrelevant responses.* A second type of response problem is the irrelevant response. This answer simply misses the point of the question, as when R tells you about his operation when you ask about his visits to the doctor. The response may be the result of the respondent's not hearing the question correctly, and so your remedy for this situation is to *repeat the question.* This technique will also work in the cases where R has heard the question but has misconstrued it.

Principle: The respondent should use his/her own definition, but we want the question understood.

4. *The "I don't know" response.* The "I don't know" answer can mean any number of things. For instance:

- The respondent doesn't understand the question and answers "don't know" to avoid saying he doesn't understand.
- The respondent is thinking the question over and says "don't know" to fill the silence and to give time for thought.
- The respondent may be trying to evade the issue or may feel that a question is too personal and doesn't want to hurt the interviewer's feelings by saying so in a direct manner.
- The respondent really does not know, or does not have an opinion or attitude on the subject.

If the respondent actually doesn't have the information requested, this is in itself significant to the survey result. It is the interviewer's responsibility to be sure that this is, in fact, the case and not mistake "I have no opinion on that" for "Wait a minute, I'm thinking."

When a respondent gives a "don't know" answer: (a) Wait a few seconds to give R time to think. If R still does not give an answer, (b) check "don't know" answer (code 8), and (c) *repeat the question. Always* repeat the question unless R has elaborated on the "don't know" in such a way that it is clear that he really means it.

Principle: "Don't know" is a valid response, but we want to be sure R is a true "don't know" and is not giving the response for some other reason.

I. Use of probes

Occasionally an answer is not sufficiently complete or clear. At times R gives a general answer instead of the specificity you need. Sometimes a range is given (3–6 days) when you want an exact number of days, etc. When these kinds of things happen some techniques are needed to make the response fit the objective.

Such responses require you to use some sort of probe. A probe is a sub-question used where necessary to clarify or to increase the specificity or the precision of the response. EP and EO forms of the questionnaire have specific probes for many of the questions. For the ST forms *standard* probes are provided for your use. These are in addition to the preferable technique of repeating the question. These probes can be used with any of the question forms.

These are illustrated below. (The question calls for the number of days.)

Example:

Q: How many days did you stay in bed?
A: Oh, three–four days.
Probe: Which would be closer?

Example:

Q: How many days during the past two weeks did you cut down on things you usually do?
A: Oh, about a week I guess.
Probe: How many *days* would you say?

Example:

Q: Compared to other people your age, would you say your health is excellent, good, fair, or poor?
A: I'm in great health.
Probe: Would you say your health is excellent, good, fair, or poor? (This probe simply repeats relevant parts of the question. Notice that it repeats all of the choices.)

Example:

Q: About how long has it been since you *last* went to a dentist?
A: I haven't been for a long time.
Probe: (Repeat question.)
A: It's been five or six years.
Probe: Would it be between two and five years or over five years? (The categories for this question include 2–5 years and over 5 years. This probe asks which category he thinks is correct.)

1. *Probe for incomplete or unclear responses.* Sometimes R gives an answer which does not meet the objectives of the question or the response is unclear. The best technique is, as always, to repeat the question. Sometimes this seems inappropriate and you need some other probe.

Example:

Q: What condition caused you to cut down?
A: It was the heat.
Probe: How do you mean that?
A: Well, it was so hot on the job that I got sick to my stomach.

The "What do you mean?" or "How do you mean that?" or "How is that?" will usually stimulate a more adequate response.

Example:

Q: How many days did illness or injury keep you from work?
A: Well I goofed off one day last week.
Probe: How do you mean that?
A: Well I just felt all worn out and took a day off.

If one probe does not clarify the information, simply record what you have. Do not probe more than once. Again we want to stress that probes are to be used sparingly. Repetition of the question is the most desirable technique.

2. *Introducing a question for which you have partial information.* Because this questionnaire asks many questions about health it is likely that R will have given a partial answer to a question in an earlier question. For example: R may say that he/she had gone to a dentist the day before the interview. When you come to the question asking the number of dental visits you can acknowledge that dental visits have been mentioned before. "You said earlier that you had been to the dentist yesterday," prior to asking how many times R went to a dentist during the past two weeks. You need to be careful in using this introduction to be certain it reflects what R said. Use the technique only when needed to avoid awkwardness.

3. *Comments associated with recording responses or computer delay.* At times when a lengthy response has been given, typing will create a lengthy delay. To ease the break in the interview, you may say, "Let me get this down," or "I want to get this down." These comments should be used sparingly but a statement or two in early responses will make the pause less annoying for the R. An allied problem exists when machine difficulties stop the interview. You may say, "Please excuse the delay. I'm having a problem with my computer."

J. How to record answers

1. *Recording conditions.* The first rule in recording is not to interpret what R says but record it as stated. That is, if the question calls for a specific number of days and R gives a range even after a probe, record the range as he/she gives it.

Especially in recording names of conditions do not interpret or diagnose—record the condition as it is stated. A response of "high blood" or "feeling poorly" is recorded as such.

Sometimes, however, R will give a sentence or two description of a condition or set of symptoms. There is not sufficient space to record this. You will also need to refer to this event later in the "condition page." Thus a summary is needed. It should be a *summary* not an interpretation or diagnosis.

Example: "I felt awful, had a bad stomach ache that lasted all night and part of the next day. I was just doubled up with the pain."
Record only: Stomach ache.

Example: "I guess I had the flu, a headache and a fever."
Record three conditions: Flu, headache, fever. It's likely these latter two are simply symptoms of the flu but don't make this evaluation.

If R *himself/herself* were to say that headache and fever were only symptoms then you record only one condition.

Example: "I had a headache and fever. These were due to the flu I had."
Record only: Flu.

At times it will be difficult to decide what should be recorded. Always err on the side of recording too many conditions rather than too few. These will usually be clarified when the condition pages are asked.

Example: You have recorded flu, headache, fever as separate conditions. When you ask the condition question R will report about the flu. Then you ask about the headache and the response is likely to be "Oh, the headache and fever were because of my flu."

2. *Recording R's or interviewer's comments.* Occasionally the respondent will make a comment which is relevant to clarifying or modifying his response, particularly to an open question. You may also note something about the response which is important. These comments should be recorded. If on-line they are put in the text field and off-line in the margin or bottom of the page.

3. *Recording repeated questions and probes.* Whenever a question is repeated or a probe not in the question issued, these should be recorded. For open responses the entries are made where they occur in the text. For closed questions they are recorded in the margin for hard copy and in the text field on-line. The repeat of a question simply requires the notation RQ. For a probe the notation is P followed by the nature of the probe. "What mean" or "Which closer" etc. The purpose is that we can consider the probes in analysis of the data.

4. *Correcting responses.* If R changes his response or you have incorrectly recorded the original answer, you should record the corrected response. On hard copy—do not erase the original answer. To change an answer:

a. line-out incorrect written entries;
b. for checked boxes, circle the incorrect box and check the correct one;
c. for incorrect circled items, line-out the incorrect response and circle the correct one.

On-line—you will record the corrected response by recording *over* the original entry.

5. *Editing the interview.* When you have completed the interview you may need to go back to complete or clarify some things in the interview. Don't take time to improve your recording except where you think the coder will not be able to read or understand the information.

You should, however, check to see that all forms are properly identified with correct numbers and other information. You should also be sure you have all relevant forms in the family folder. If three conditions were reported you should have three forms.

Appendix VI
Questionnaires and reporting forms

The following questionnaires and reporting forms were used in the Survey Research Center Telephone Survey.

Cover sheet

The interview began with the cover sheet, which specified procedures for selecting the person to be interviewed, either the knowledgeable respondent or a randomly selected adult, depending on which respondent rule was specified for the particular interview.

Family folder

The family folder was the form for summarizing information collected during the interview and was used to guide the interviewer. This form was used for both computer-assisted telephone interviewing (CATI) and non-CATI interviews.

Eligible household members were listed at the time the cover sheet was completed. As the interview progressed, information was recorded pertaining to health conditions, doctor visits, and hospitalizations; and other relevant notes were made.

Questionnaires

There were three person questionnaire forms, labeled ST, EP, and EO. ST was the standard form without use of experimental techniques and was used both when interviewing the respondent or when obtaining information about another household member if a standard form was designated.

EP and EO cover exactly the same variables but contain the experimental interviewing techniques. EP was used for the respondent and EO was used when a respondent reported for someone else. Designation of the form to be used appeared on the cover sheet for each phone number, predetermined by the sampling design.

Each form had a separate demographic section.

The non-CATI telephone interviews used exactly the same procedures and questions as projected on the computer screen for CATI interviews.

Questionnaires for reporting conditions, doctor visits, and hospitalizations

These questionnaires were completed, as needed, following the person questionnaires.

The experimental versions of the questionnaires are provided in this appendix. With the deletion of the statements on commitment, instructions, and feedback, these experimental forms are identical to the standard or control forms.

OMB No. 68-578024
Expires: March 31, 1980

P. 468161

HOUSEHOLD ID NO. [ ][ ][ ][ ]

FAMILY AND PERSON ID NO. [ ][ ]

SURVEY RESEARCH CENTER
INSTITUTE FOR SOCIAL RESEARCH
THE UNIVERSITY OF MICHIGAN
ANN ARBOR, MICHIGAN 48106

INTERVIEWER: _____

DATE OF INTERVIEW: _____

EXACT TIME NOW: _____

HEALTH IN AMERICA

HIS/SRC **EP** QUESTIONNAIRE

1. INTERVIEWER CHECKPOINT

[ ] 1. FIRST PERSON SECTION TO BE COMPLETED FOR THIS FAMILY, OR NEW R, THIS FAMILY

[ ] 5. FIRST PERSON SECTION FOR THIS FAMILY ALREADY COMPLETED → GO TO EO QUESTIONNAIRE

1a. This research is authorized by the Public Health Service Act. It's important for the Public Health Service to get exact details on every question, even on those which may seem unimportant to you. This may take extra effort. Are you willing to think carefully about each question in order to give accurate information?

[ ] 1. YES

We appreciate your willingness to make the extra effort.

[ ] 5. NO

Since getting accurate information is important, it's necessary to get your agreement to think carefully if we are to continue the interview. → TERMINATE

For our part, we will keep all information you give confidential. Of course, the interview is voluntary. Should we come to any question you do not want to answer, just let me know and we'll move on to the next one.

1b. Many people feel it helps them to look at a calendar to recall dates of visits to doctors, illnesses, and other things asked for in these questions. Do you have a calendar handy? I'll be happy to wait while you get one.

[ ] 1. YES

[ ] 5. NO

The next few questions refer to the period beginning Monday _____ and ending this past Sunday evening, _____. This does not include any of the days since Sunday.

2. During those two weeks, did you stay in bed because of any illness or injury?

1. YES

5. NO
TURN TO P. 7 , Q7.

2a. For this question we'd like to get an exact a number as possible. During that two-week period, how many days did you stay in bed all or most of the day?

_____ DAYS

00. NONE
TURN TO P. 7 , Q7.

2b.

1. EXACT NUMBER → ** I see, this is the kind of exact answer we need.** TURN TO P. 3 , Q3.

3. RANGE; "ALL WEEK" → 2c. Could you be more exact about the number of days?

_____ DAYS

(IF EXACT): **I see, this is kind of exact answer we need.** TURN TO P. 3 , Q3.

8. DON'T KNOW → 2d. Would you think for a minute and give me your best estimate?

_____ DAYS

(IF EXACT): **I see, this is the kind of exact answer we need.** TURN TO P. 3, Q3.

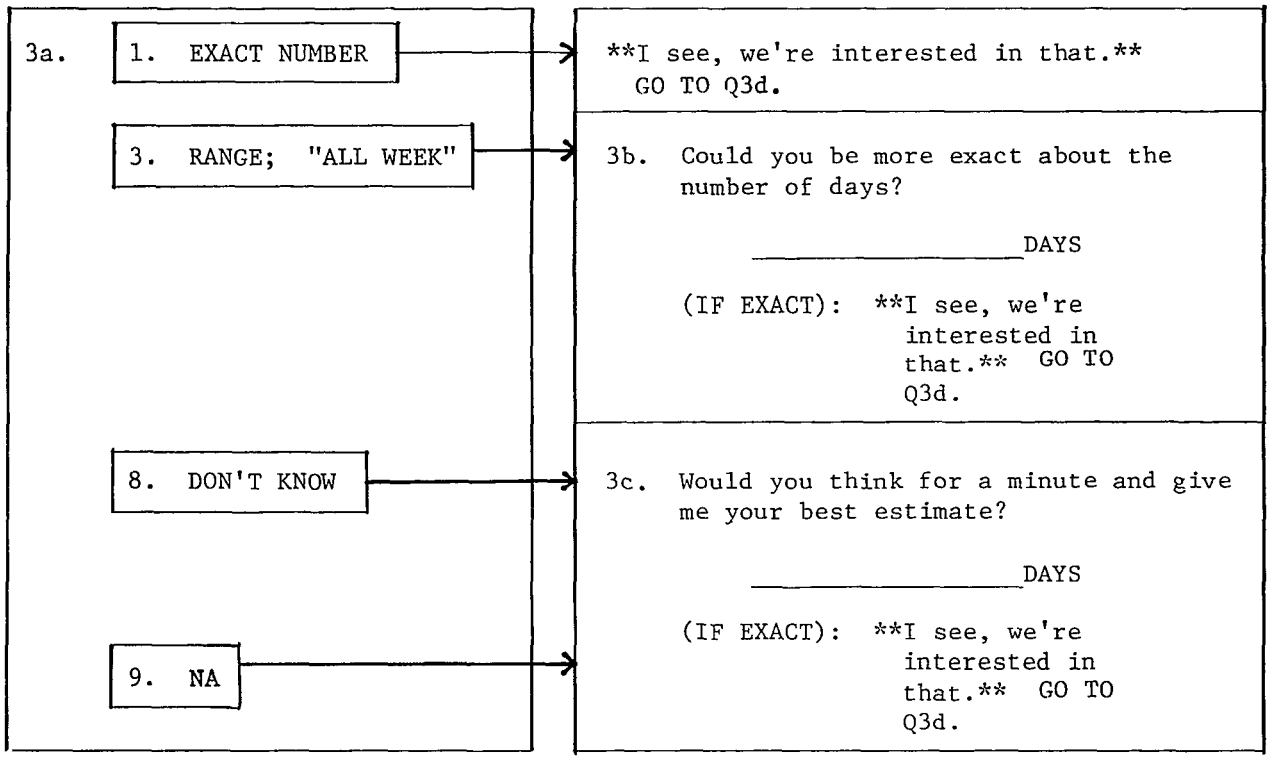
9. NA →

3. For this question, we'd like to get the number as exact as you can report it. During that two-week period, that is from _____ to _____, how many days did illness or injury keep you from work?
(FOR FEMALE): Not counting work around the house?

_____DAYS

00. NONE

TURN TO P. 4, Q4.

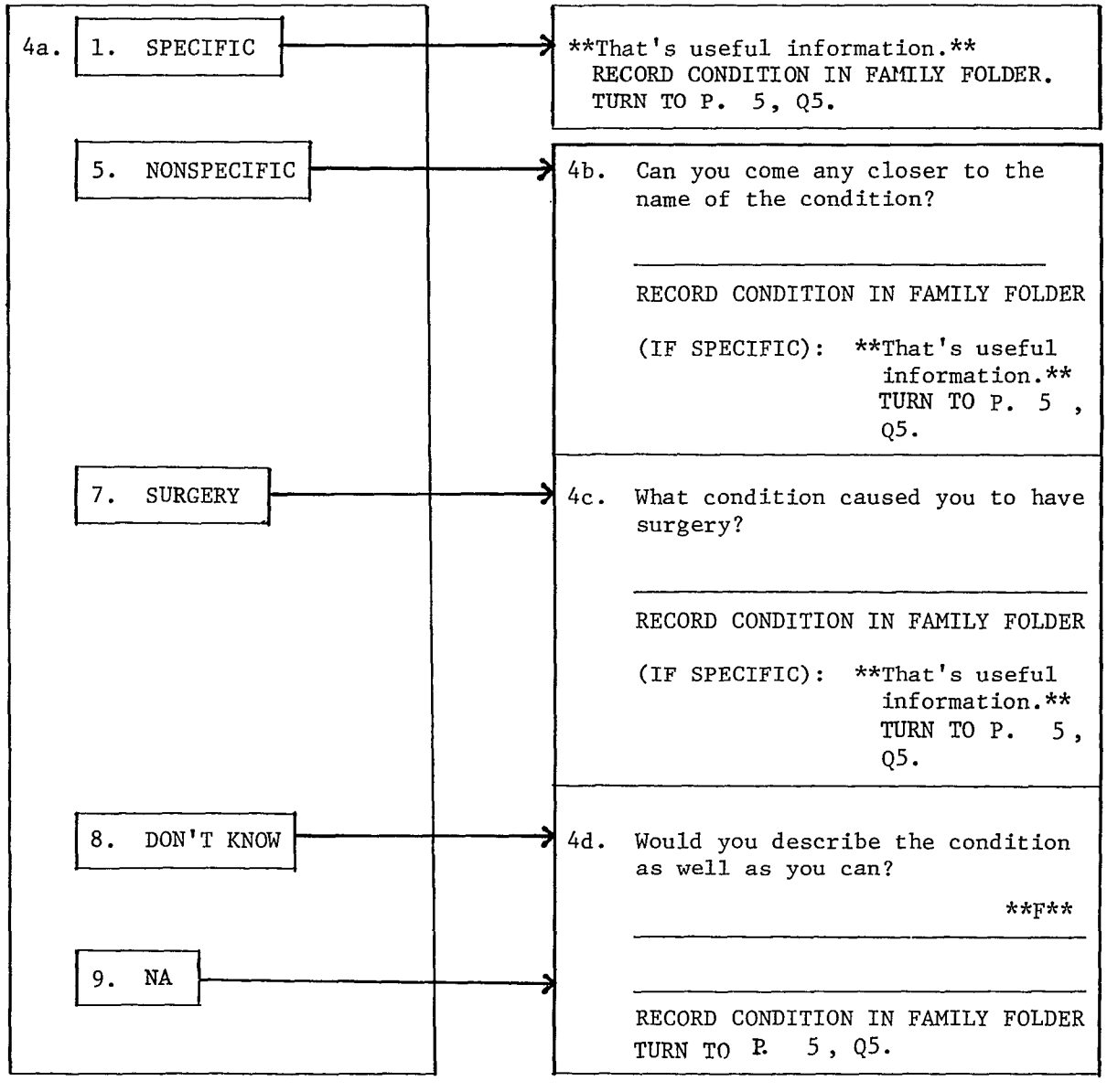


3d. On how many of these days lost from work did you stay in bed all or most of the day?

_____DAYS (IF EXACT) **F**

4. What condition caused you to (stay in bed/miss work) during those two weeks? We'd like to get the name of the condition as well as you can report it?

_____



5. Did any other condition cause you to (stay in bed/miss work) during that period?

NO → TURN TO P. 11, Q11.

5a.

1. SPECIFIC → **F** RECORD CONDITION IN FAMILY FOLDER. TURN TO P. 6, Q6.

5. NONSPECIFIC → 5b. Can you come any closer to the name of the condition?

_____

RECORD CONDITION IN FAMILY FOLDER

(IF SPECIFIC): **F** TURN TO P. 6, Q6.

7. SURGERY → 5c. What condition caused you to have surgery?

_____

RECORD CONDITION IN FAMILY FOLDER

(IF SPECIFIC): **F** TURN TO P. 6, Q6.

8. DON'T KNOW → 5d. Would you describe the condition as well as you can?

F

_____

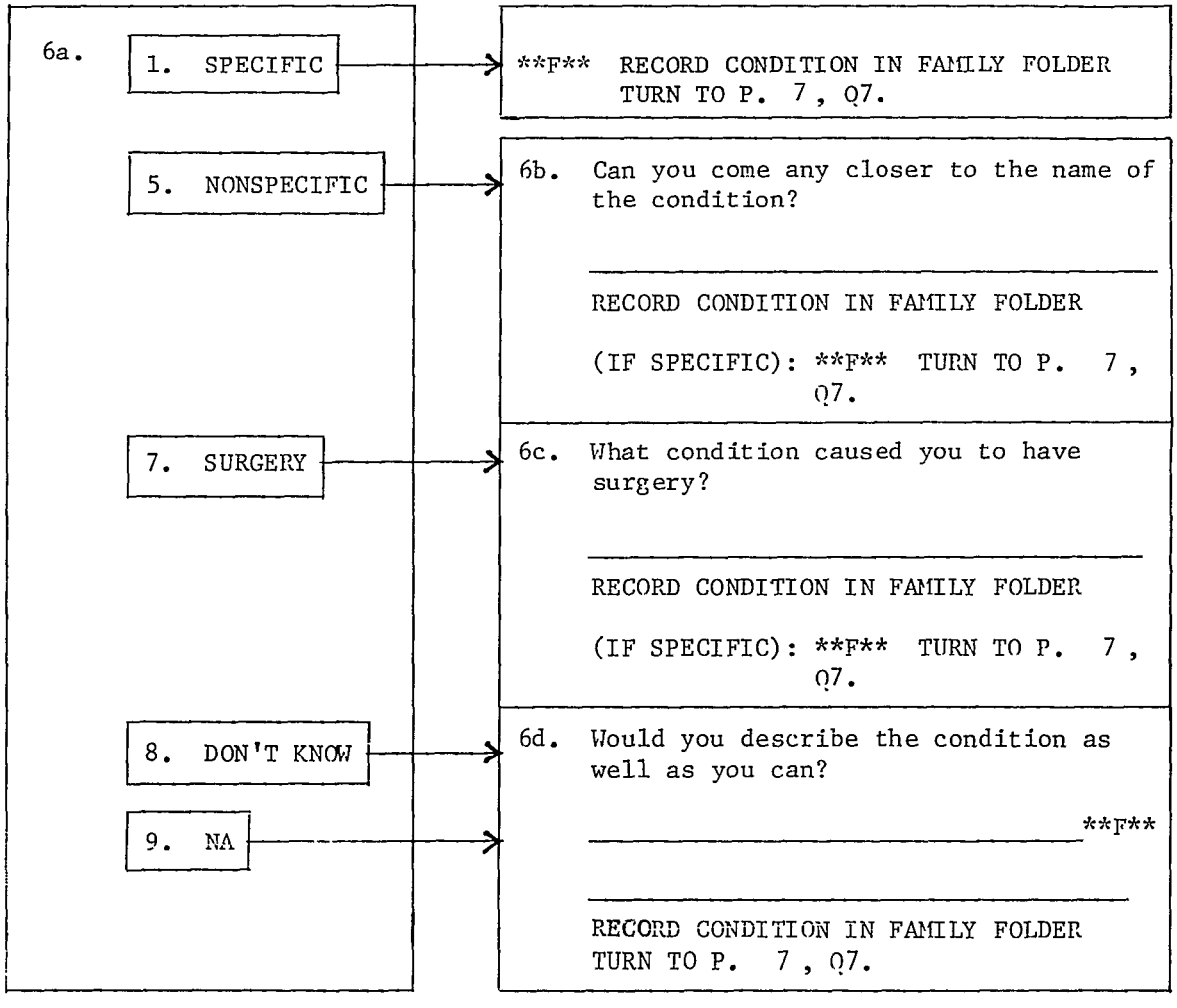
_____

RECORD CONDITION IN FAMILY FOLDER
TURN TO P. 6, Q6.

9. NA →

6. Did any <u>other</u> condition cause you to (stay in bed/miss work) during that period?

NO → TURN TO P. 11, Q11.



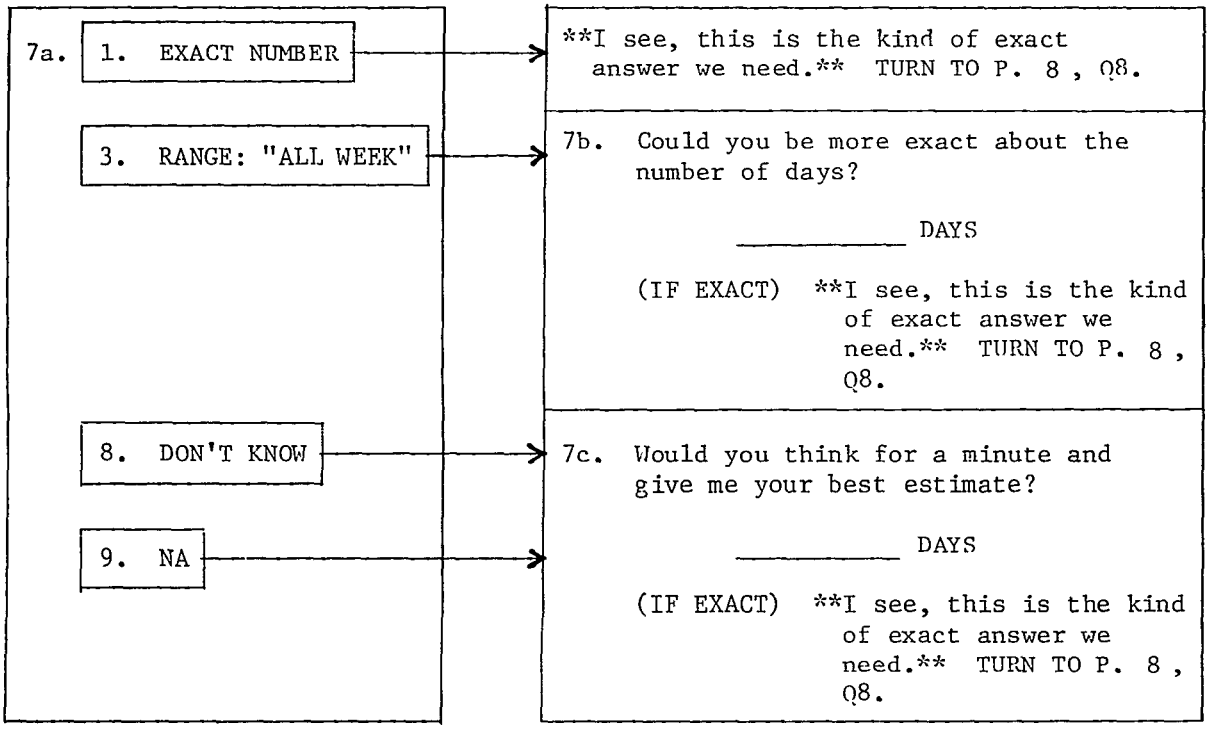
7. We'd like to get as exact a number as possible on this question.

During those two weeks, that is, from _____ to _____, how many days did illness or injury keep you from work? (FOR FEMALES):...not counting work around the house?

_____ DAYS

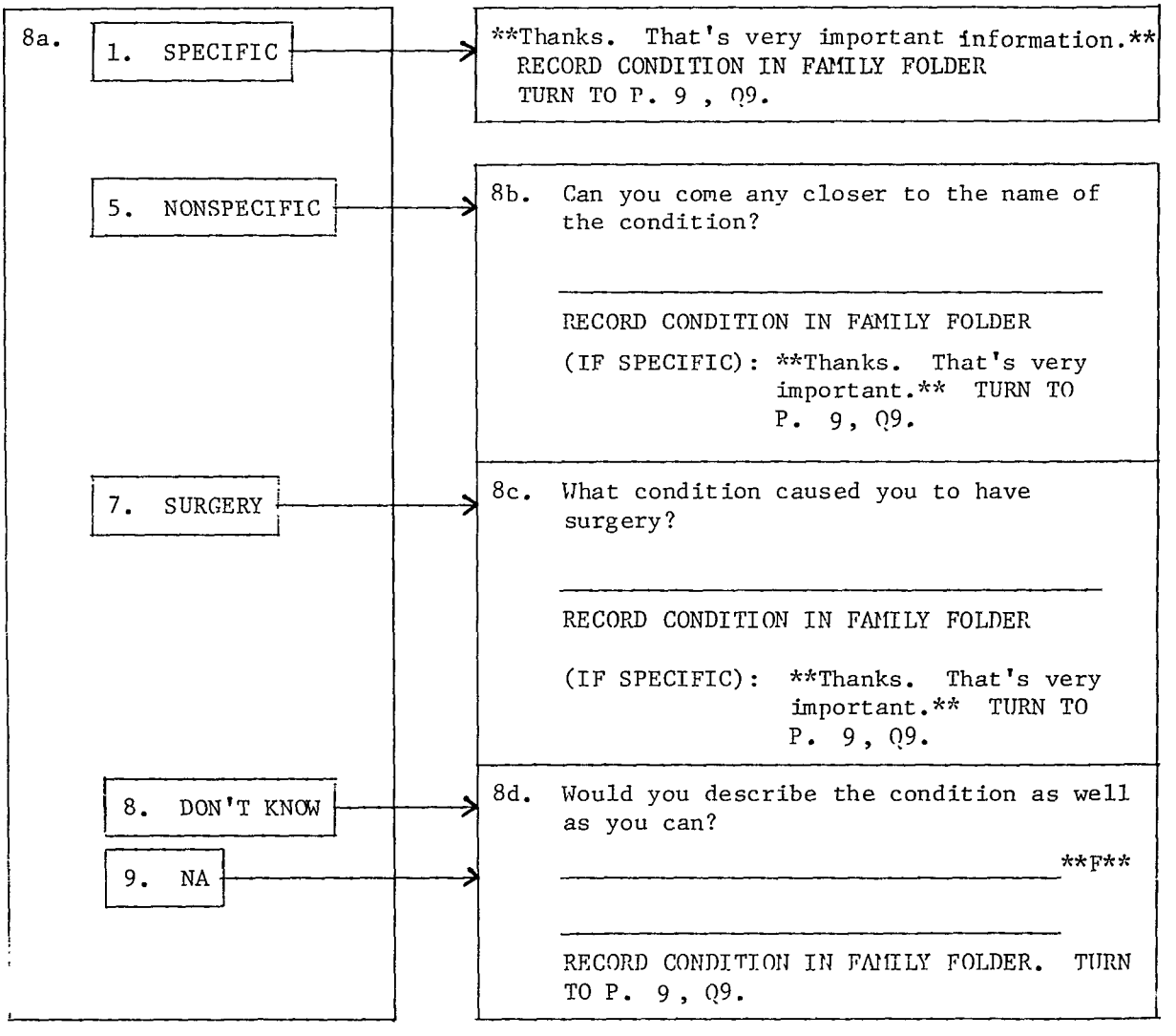
00. NONE

TURN TO P. 11, Q11.



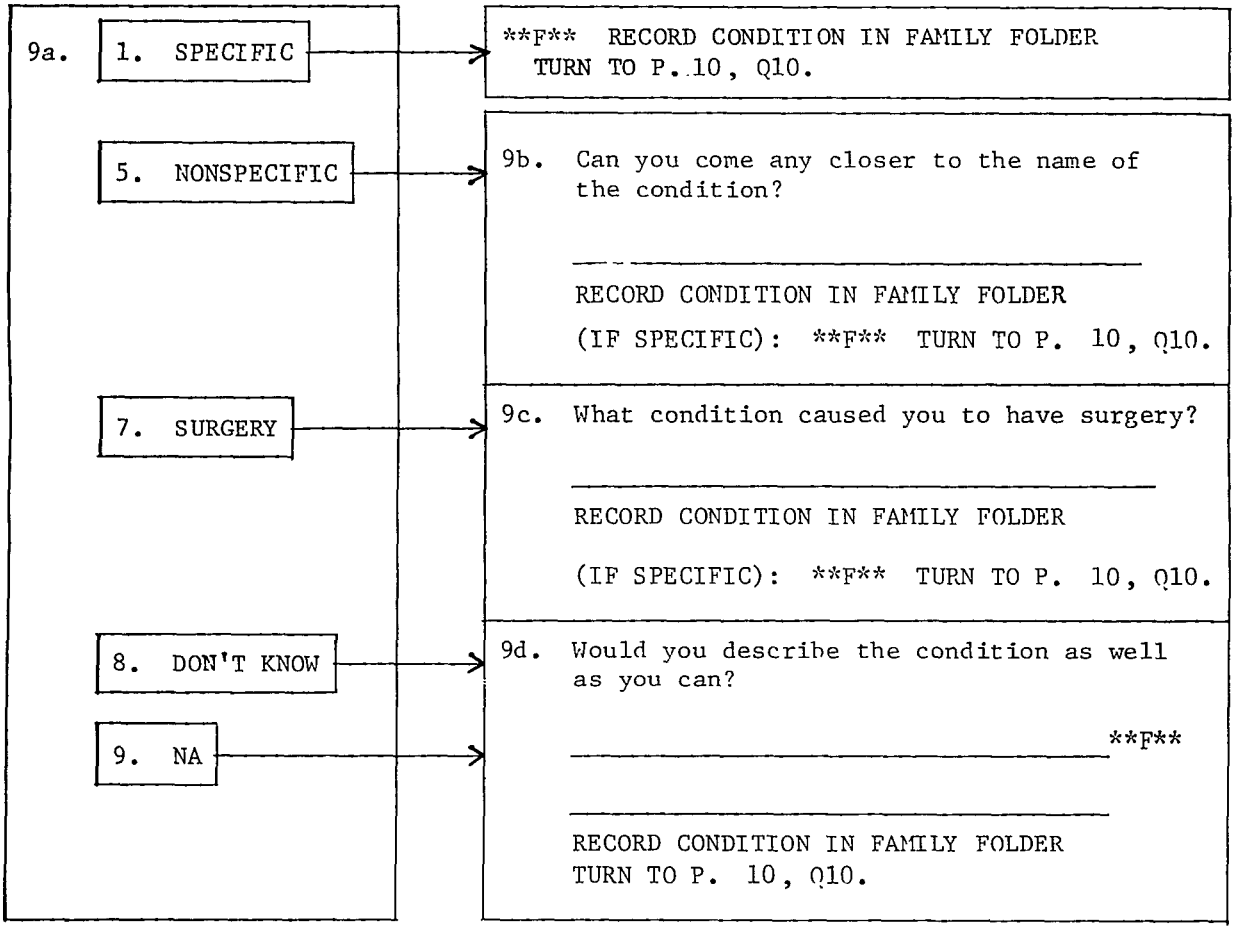
8. What condition caused you to miss work during those two weeks? We'd like to know the name of the condition as well as you can report it.

_____



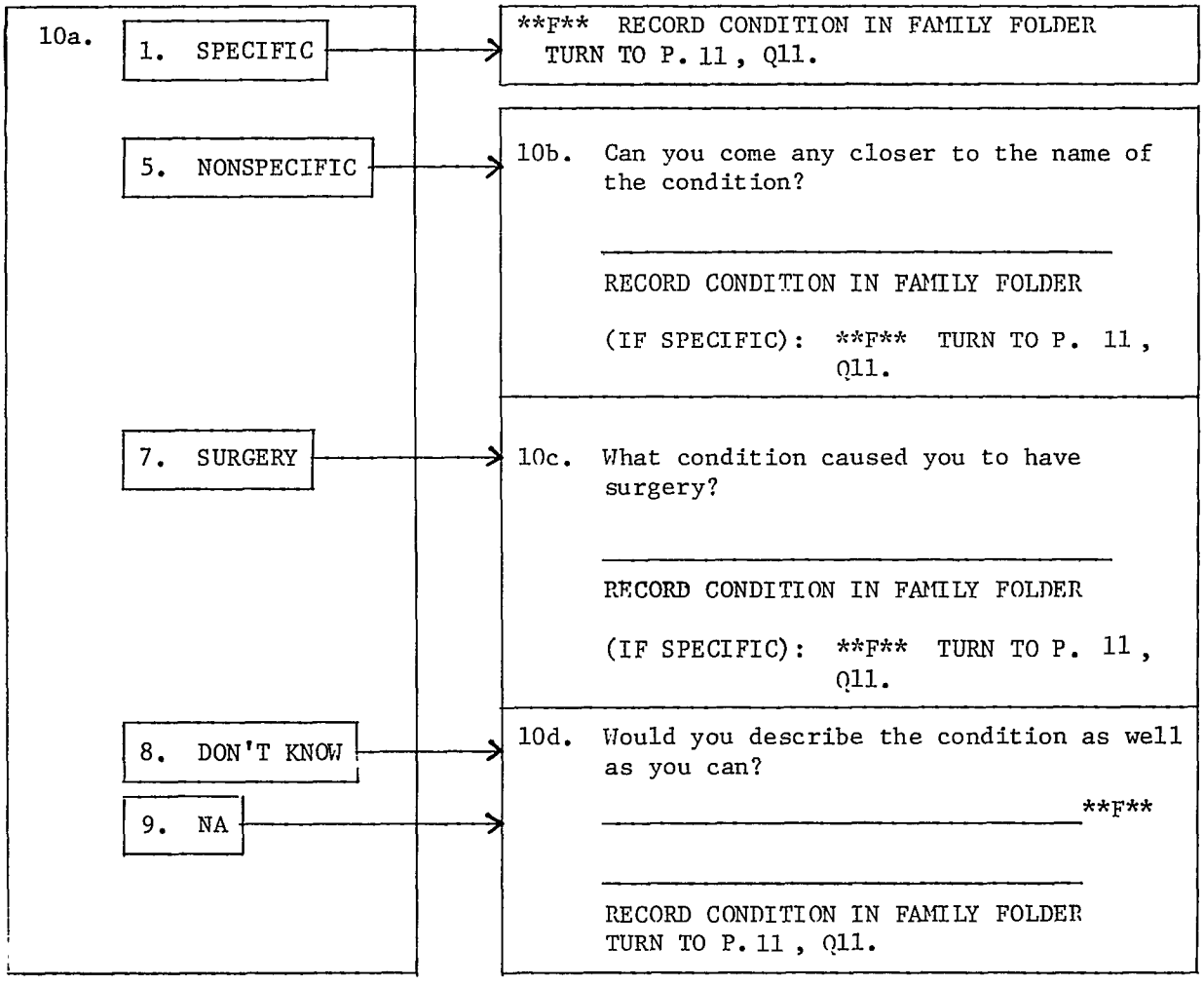
9. Did any other condition cause you to miss work during that period?

NO → TURN TO P. 11, Q11.



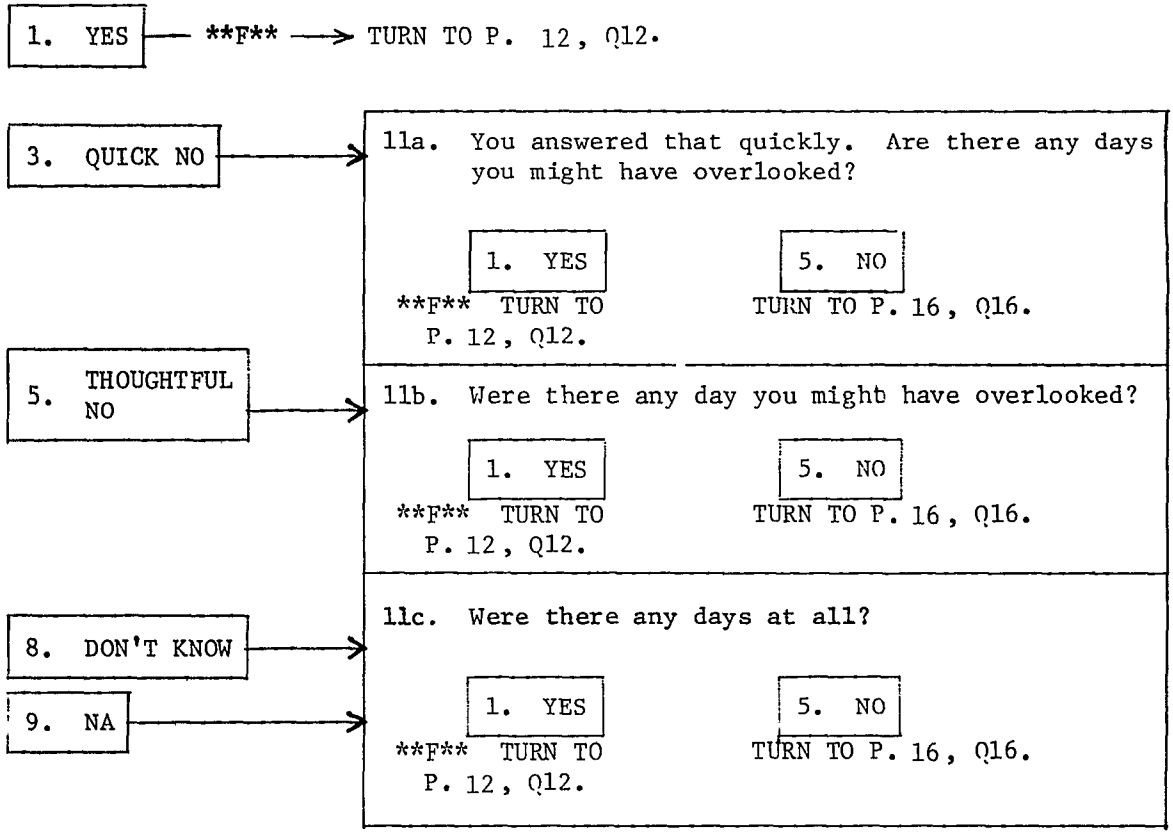
10. Did any other condition cause you to miss work during that period?

NO → TURN TO P. 11, Q11.



11. (Not counting the days in bed and/or lost from work)

Were there any (other) days during that two-week period that you cut down on the things you usually do because of any illness or injury? This is sometimes hard to remember, so please take your time.



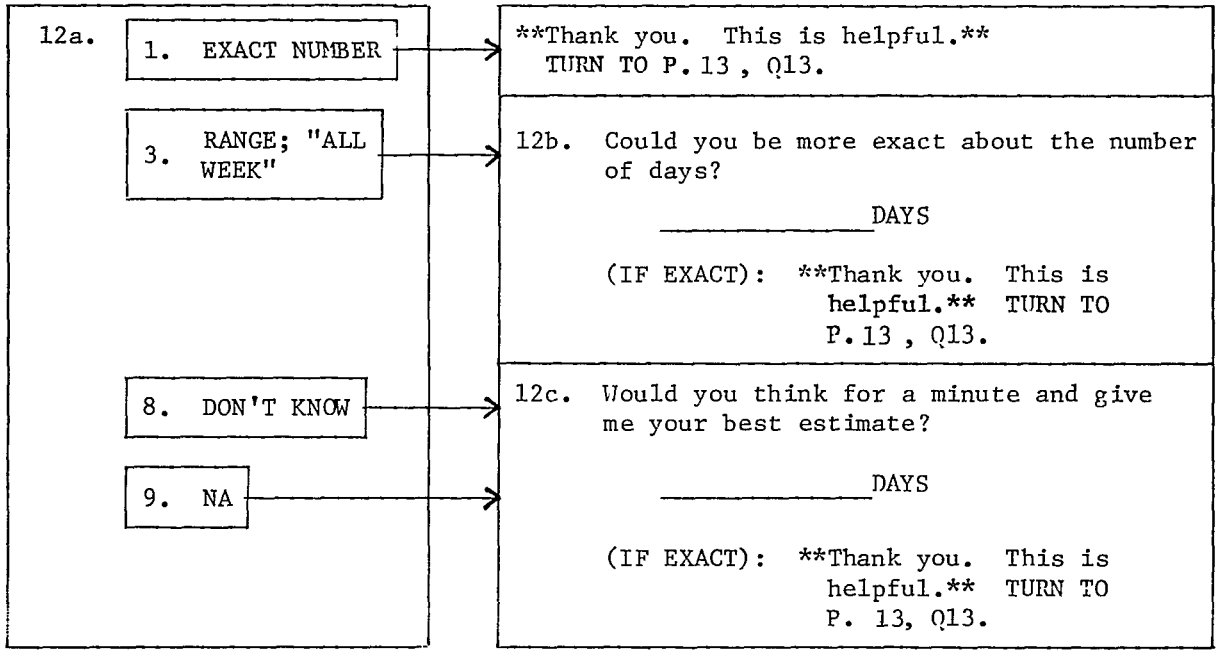
12. (Again not counting the day(s) in bed and/or lost from work)

During that period, how many (other) days did you cut down for as much as a day?

_____ DAYS

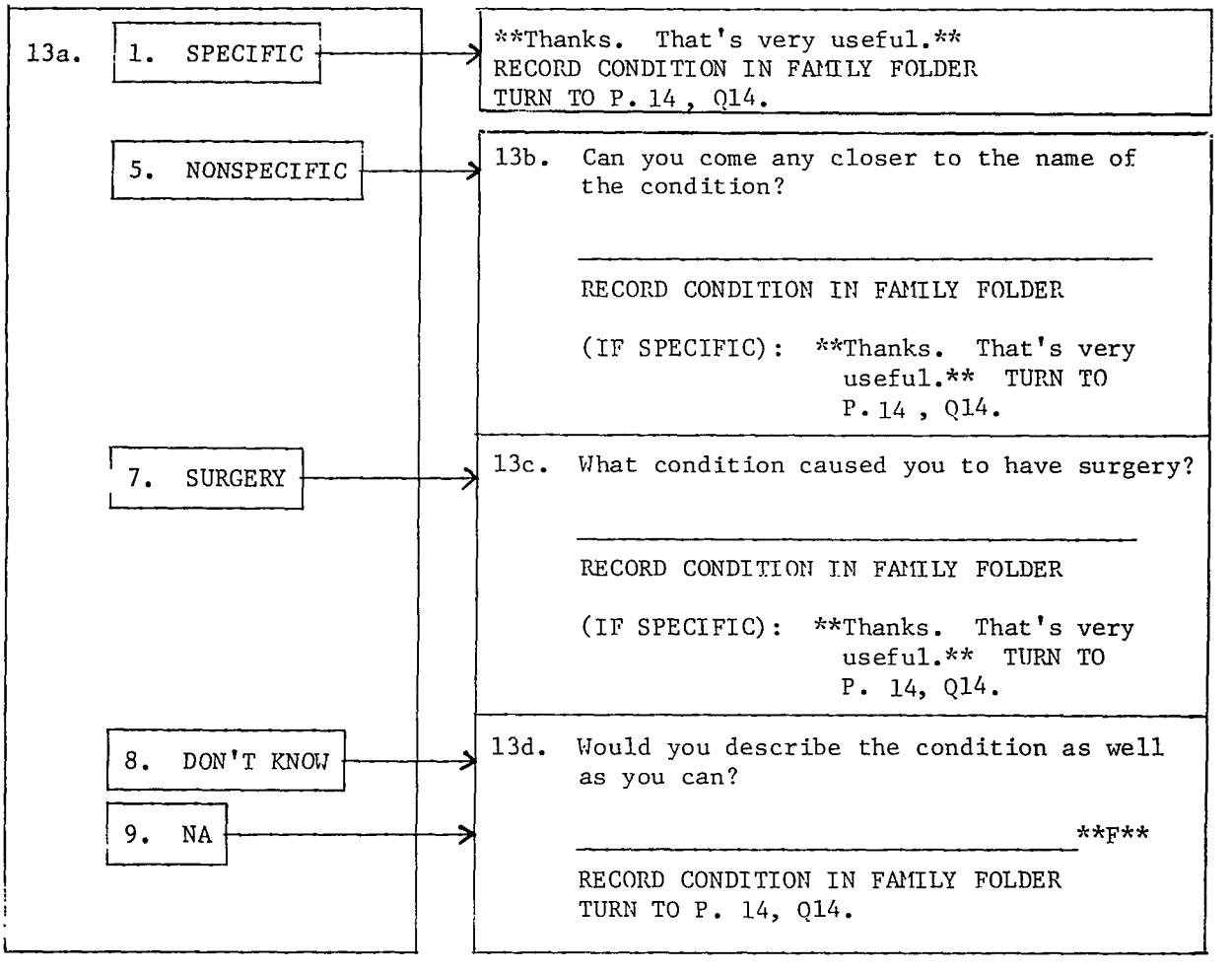
00. NONE

TURN TO P. 16 , Q16.



13. What condition caused you to cut down during that period? We'd like to know the name of the condition as well as you can report it.

_____



14. Did any other condition cause you to cut down during that period?

NO → TURN TO P. 16, Q16.

14a.

1. SPECIFIC → **F** RECORD CONDITION IN FAMILY FOLDER
TURN TO P. 15, Q15.

5. NONSPECIFIC → 14b. Can you come any closer to the name of the condition?

_____

RECORD CONDITION IN FAMILY FOLDER

(IF SPECIFIC): **F** TURN TO P. 15, Q15.

7. SURGERY → 14c. What condition caused you to have surgery?

_____

RECORD CONDITION IN FAMILY FOLDER

(IF SPECIFIC): **F** TURN TO P. 15, Q15.

8. DON'T KNOW → 14d. Would you describe the condition as well as you can?

_____ **F**

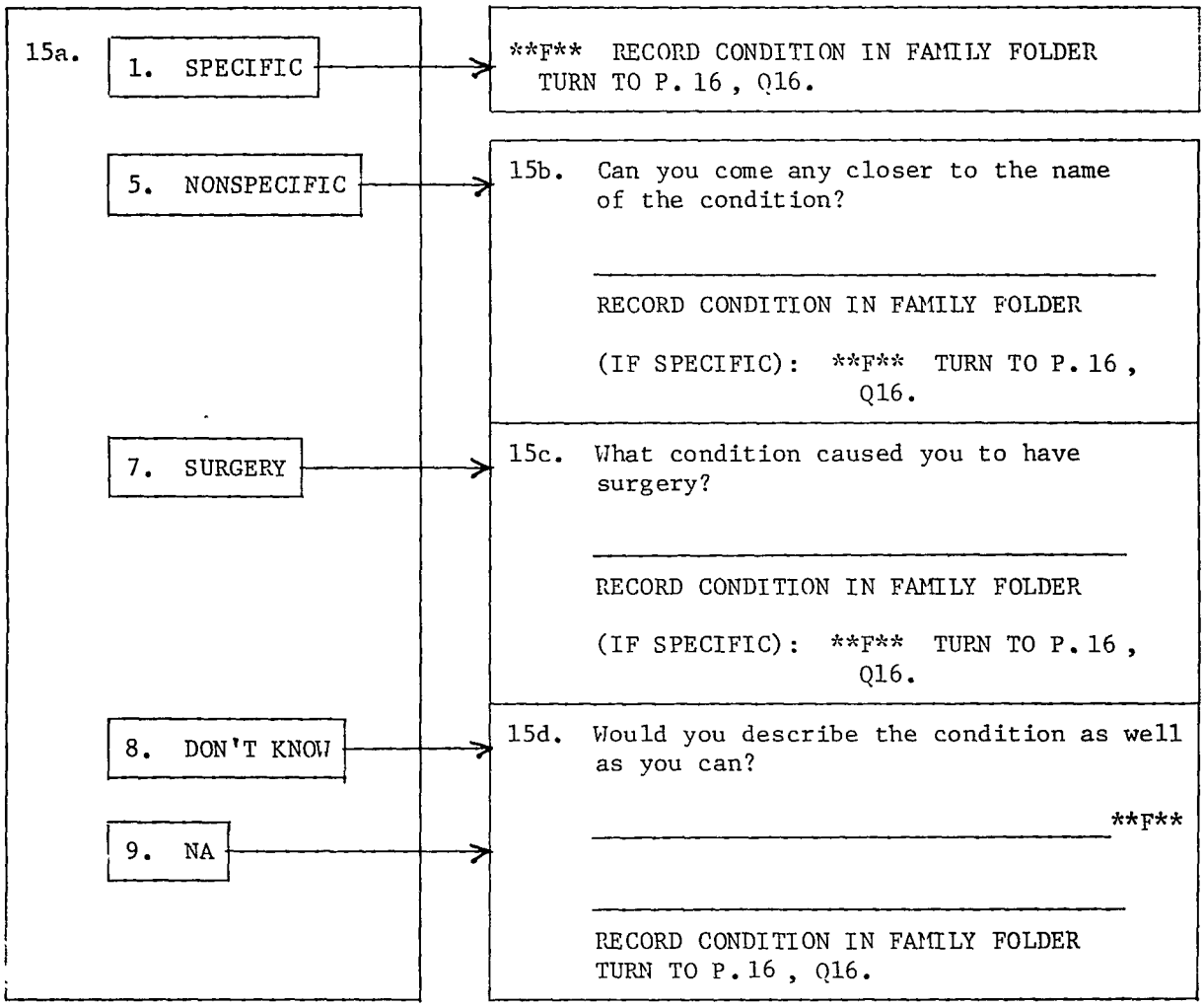
_____

RECORD CONDITION IN FAMILY FOLDER
TURN TO P. 15, Q15.

9. NA →

15. Did any <u>other</u> condition cause you to cut down during that period?

NO → TURN TO P. 16 , Q16.



16. Once again we are talking about the period from _____ to _____. During those two weeks, did you have any (other) accidents or injuries? We are interested in both serious and less serious things.

[1. YES] **F**

[5. NO] TURN TO P. 17, Q18.

16a. What was the injury? _____

_____

I see, this is the kind of thing we need to find out.

16b. As a result of the (accident/injury) did you see a doctor or did you cut down on the things you usually do?

[1. YES] **F**

RECORD INJURY
IN FAMILY FOLDER

[5. NO]

17. Did you have any other accidents or injuries during this period?

[1. YES] **F**

[5. NO] TURN TO P. 17, Q18.

17a. What was the injury? _____

_____ **F**

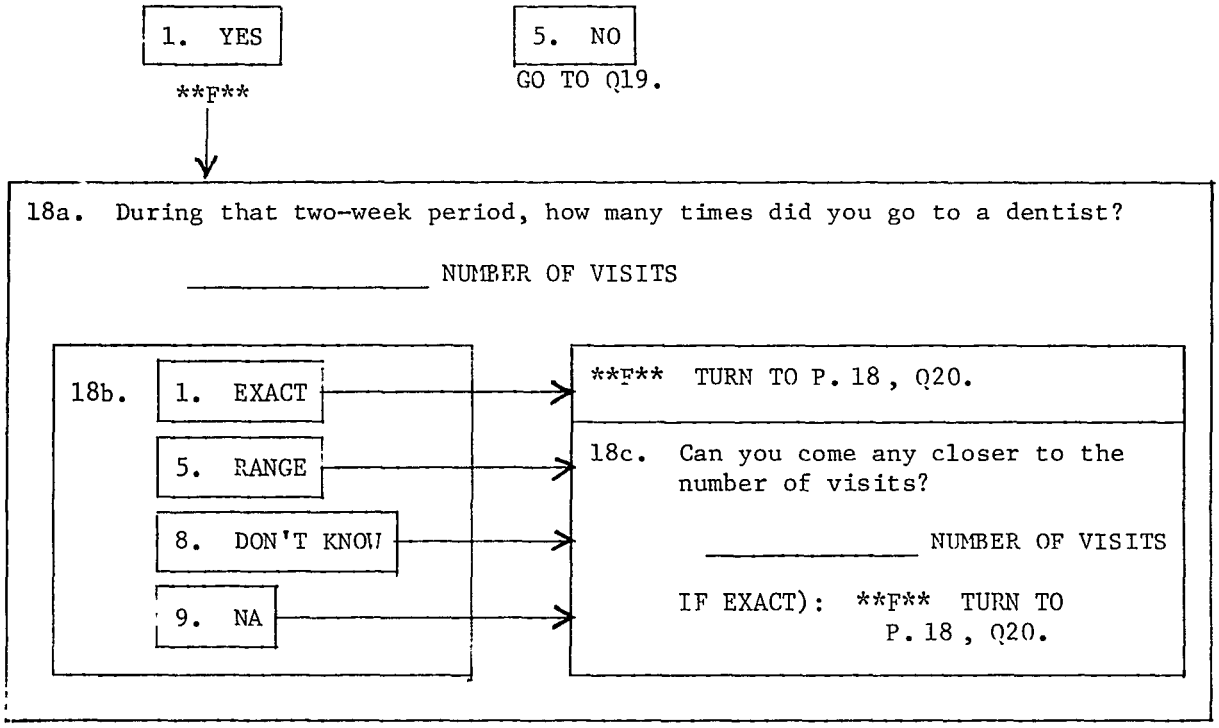
17b. As a result of the (accident/injury) did you see a doctor or did you cut down on the things you usually do?

[1. YES] **F**

RECORD INJURY
IN FAMILY FOLDER

[5. NO]

18. During those two weeks, did you go to the dentist?



19. We need you best estimate about when your last dentist appointment was. About how long has it been since you last went to a dentist?

0. INTERVIEW WEEK - (RE-ASK Q): "Other than that visit...?"

1. TWO-WEEK PERIOD (NOT REPORTED)

2. TWO WEEKS - 6 MONTHS **F**

3. OVER 6 MONTHS - 1 YEAR **F**

4. OVER 1 YEAR - 2 YEARS **F**

5. OVER 2 YEARS - 5 YEARS **F**

6. OVER 5 YEARS **F**

7. NEVER **F**

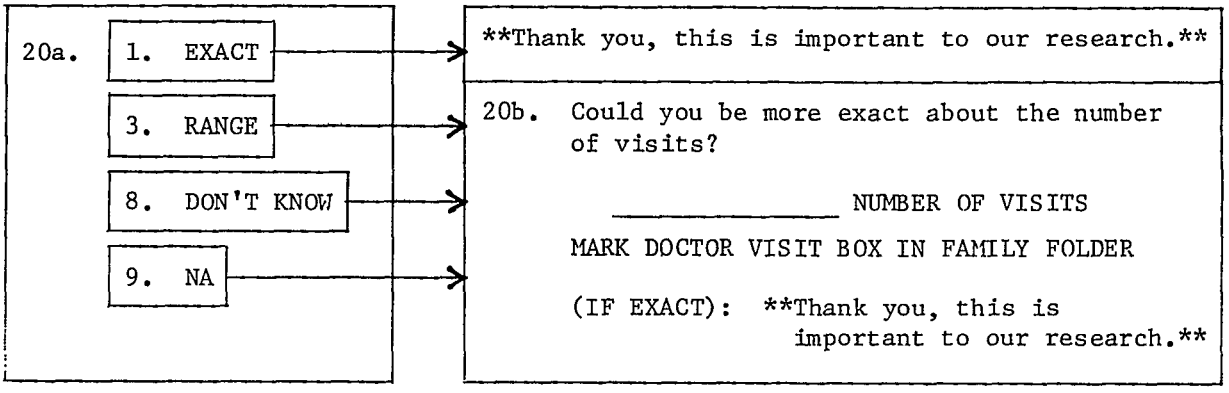
20. The next few questions are about contacts with doctors and their assistants.

During the two-week period from _____ to _____, how many times did you see a medical doctor? (DO NOT COUNT DOCTORS SEEN WHILE A PATIENT IN A HOSPITAL.)

_____ NUMBER OF VISITS

MARK DOCTOR VISIT BOX IN FAMILY FOLDER

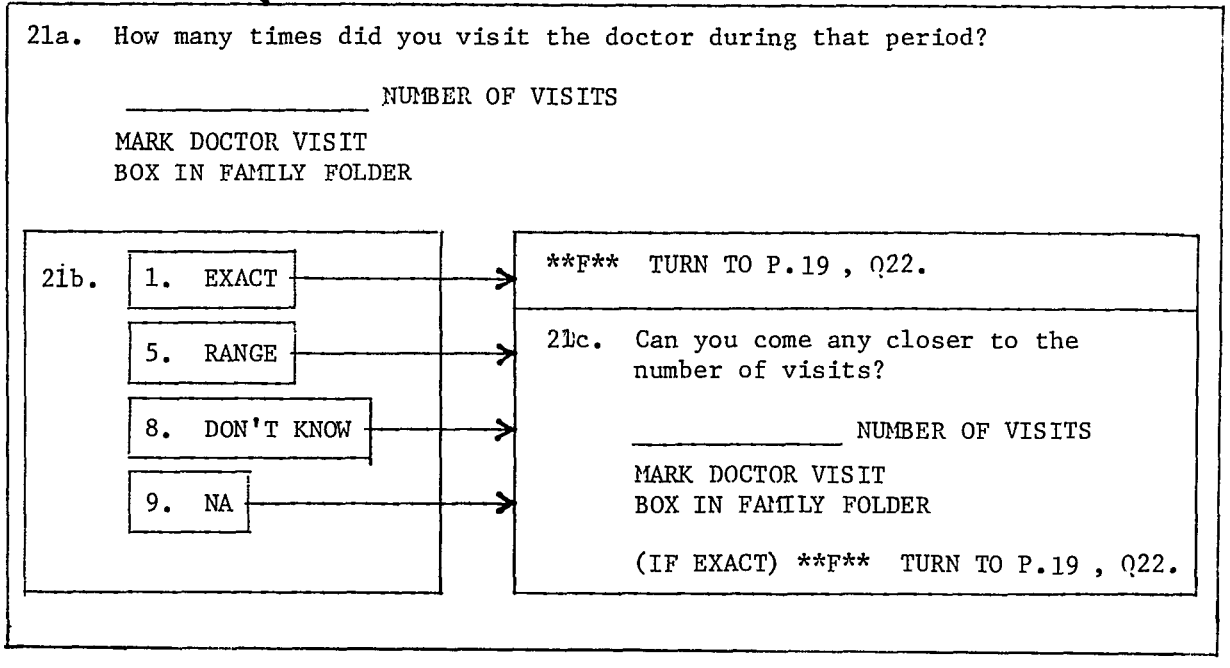
5. NONE
GO TO Q21.



21. [Besides (that/those) visit(s)] During that two-week period, did you go to a doctor's office or clinic for shots, x-rays, tests or examinations?

1. YES
F

5. NO
TURN TO P.19 , Q22.



22. In the next question, we're interested in medical advice obtained over the telephone (either through calls you made yourself, or through calls someone else made about you).

During that period, did you get any medical advice from a doctor over the telephone?

1. YES

F

5. NO

GO TO Q23.

22a. It's important for us to get an exact number on this question. How many telephone calls were made to get medical advice about you?

_____ NUMBER OF CALLS

MARK DOCTOR VISIT BOX IN FAMILY FOLDER

22b.

1. EXACT → **Thanks. This is the kind of exact information we want.

5. RANGE → 22c. Can you be any more exact about the number of calls?

_____ NUMBER OF CALLS

MARK DOCTOR VISIT BOX IN FAMILY FOLDER

(IF EXACT): **Thanks, this is the kind of exact information we want.**

8. DON'T KNOW →

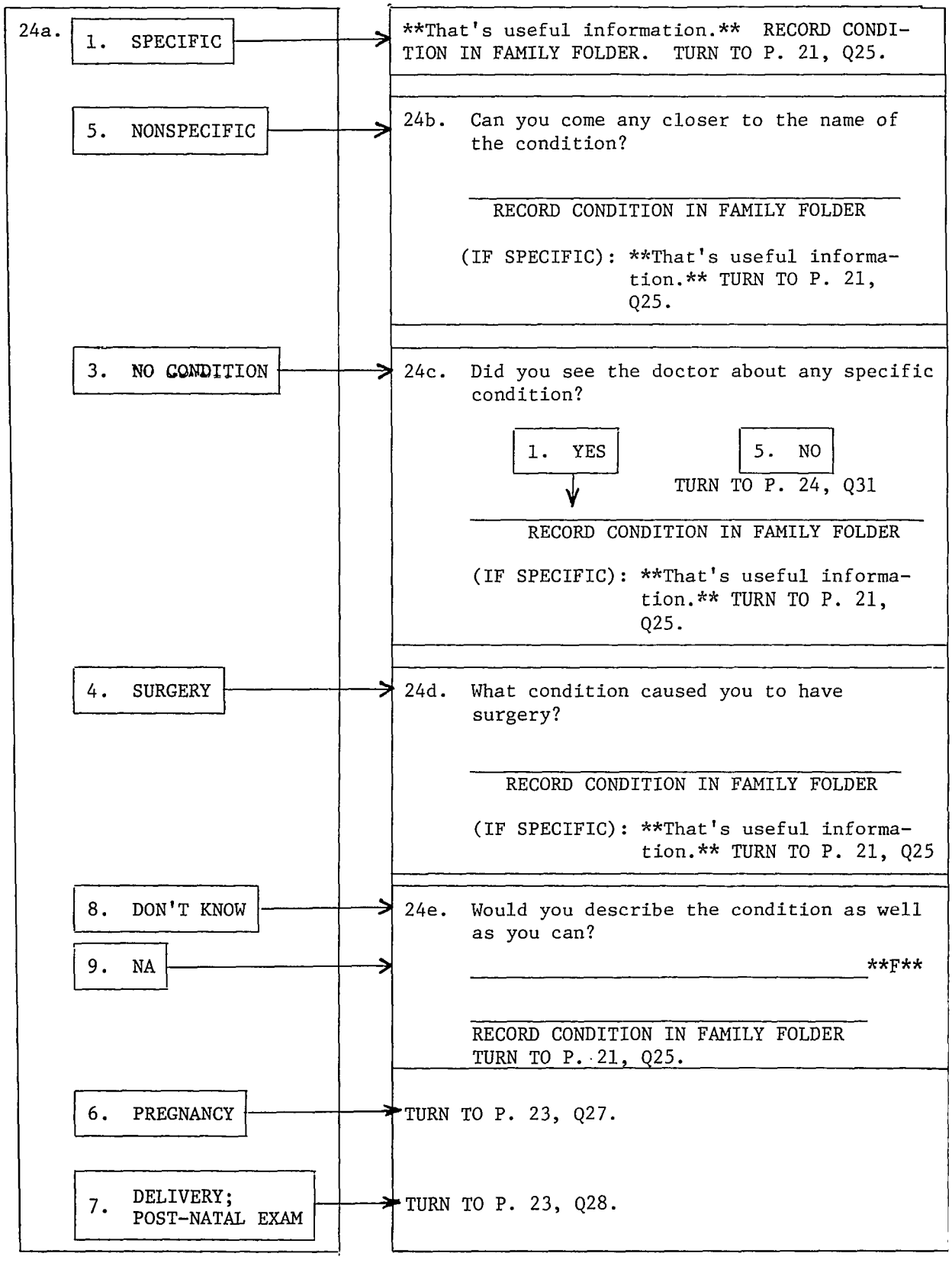
9. NA →

23. INTERVIEWER CHECKPOINT

☐ 1. IF 1 OR MORE DOCTOR'S VISITS FROM Q20 - 22c. → TURN TO P. 20 , Q24.

☐ 2. IF 0 DOCTOR'S VISITS FROM Q20 - 22c. → TURN TO P. 24 , Q30.

24. For what condition did you see or talk to a doctor during those two weeks? We'd like the name of condition as well as you can report it.



25. Did you see or talk to a doctor about any other condition during that period?

NO → TURN TO P. 24, Q31.

25a.

1. SPECIFIC → **F** RECORD CONDITION IN FAMILY FOLDER
TURN TO P. 22, Q26.

5. NONSPECIFIC → 25b. Can you come any closer to the name of the condition?

_____

RECORD CONDITION IN FAMILY FOLDER

(IF SPECIFIC): **F** TURN TO P. 22, Q26.

7. SURGERY → 25c. What condition caused you to have surgery?

_____

RECORD CONDITION IN FAMILY FOLDER

(IF SPECIFIC): **F** TURN TO P. 22, Q26.

8. DON'T KNOW → 25d. Would you describe the condition as well as you can?

9. NA →

_____ **F**

_____

RECORD CONDITION IN FAMILY FOLDER
TURN TO P. 22, Q26.

6. PREGNANCY → TURN TO P. 23, Q27.

7. DELIVERY; POST-NATAL EXAM → TURN TO P. 23, Q28.

26. Did you see or talk to a doctor about any other condition during that period?

NO → TURN TO P. 24 , Q31.

26a.

1. SPECIFIC → **F** RECORD CONDITION IN FAMILY FOLDER
TURN TO P. 24 , Q31.

5. NONSPECIFIC → 26b. Can you come any closer to the name of the condition?

_____

RECORD CONDITION IN FAMILY FOLDER

(IF SPECIFIC): **F** TURN TO P. 24 , Q31.

7. SURGERY → 26c. What condition caused you to have surgery?

_____

RECORD CONDITION IN FAMILY FOLDER

(IF SPECIFIC): **F** TURN TO P. 24 , Q31.

8. DON'T KNOW → 26d. Would you describe the condition as well as you can?

9. NA → _____ **F**

_____

RECORD CONDITION IN FAMILY FOLDER
TURN TO P. 24, Q31.

6. PREGNANCY → TURN TO P. 23, Q27.

7. DELIVERY; POST-NATAL EXAM → TURN TO P. 23, Q28.

27. During those two weeks were you sick because of your pregnancy?

1. YES

5. NO
GO TO Q27b.

27a. What was the matter?

_____

RECORD CONDITION IN FAMILY FOLDER

We appreciate your giving us those details.

27b. During that period, did you see or talk to a doctor about any other condition?

_____ **F** NO → TURN TO P. 24 , Q31.

RECORD CONDITION IN FAMILY FOLDER.
GO TO Q29.

28. Were there any problems with the delivery?

1. YES
F

5. NO
GO TO Q28b.

28a. What was the matter?

_____

RECORD CONDITION IN FAMILY FOLDER

We appreciate your giving us thos details.

28b. During that period, did you see or talk to a doctor about any other condition?

_____ **F** NO → TURN TO P. 24 , Q31.

RECORD CONDITION IN FAMILY FOLDER.
GO TO Q29.

29. Did you see or talk to a doctor about any <u>other</u> condition during that period?

_____ **F**

RECORD CONDITION IN FAMILY FOLDER
TURN TO P. 24 , Q31.

30. About how long has it been since you last saw or talked to a medical doctor? (INCLUDE DOCTORS SEEN WHILE A PATIENT IN THE HOSPITAL.)

0. INTERVIEW WEEK - (RE-ASK Q): "Other than that visit...?"
1. TWO-WEEK PERIOD (NOT REPORTED)—— REPEAT Q20 - Q29.
2. TWO WEEKS - 6 MONTHS
3. OVER 3 MONTHS - 1 YEAR
4. OVER 1 YEAR - 2 YEARS
5. OVER 2 YEARS - 5 YEARS
6 OVER 5 YEARS
7. NEVER

31. In the next question, we want to talk about the last twelve months, that is, since (DATE), a year ago. About how many times did you see or talk to a medical doctor during the past 12 months.

(Do not count doctors seen while a patient in a hospital.) (Include the ( ) visits you already told me about.)

_____ NUMBER OF VISITS

00. NONE OR ONLY WHILE IN HOSPITAL

Thanks. It isn't always easy to remember that.

32. The next question is a little different from those we've been asking you. What were you doing <u>most of the past 12 months</u>—

(FOR MALES): working or doing something else?

(FOR FEMALES): keeping house, working, or doing something else?

1. WORKING **F**

TURN TO P. 27, Q39.

2. KEEPING HOUSE **F**

TURN TO P. 28, Q46.

3. DOING SOMETHING ELSE

32a. What were you doing?

1. RETIRED **F**

2. GOING TO SCHOOL **F**

TURN TO P. 29, Q52.

3. SOMETHING ELSE **F**

GO TO Q33.

32b. Did you retire because of your health?

1. YES **F**

5. NO

33. Does your health <u>now</u> keep you from working?

1. YES **F**

TURN TO P. 26, Q36.

5. NO

34. In these questions, we want to find out about anything you can't or don't do because of your health or disability. Are you limited in the <u>kind</u> of work you <u>could</u> do because of your health?

1. YES **F**

TURN TO P. 26 , Q36

5. NO

34a. Are you limited in the <u>amount</u> of work you <u>could</u> do because of your health?

1. YES **F**

TURN TO P. 26 , Q36.

5. NO

TURN TO P. 26, Q35.

35. Are you limited in the kind or amount of other activities because of your health?

1. YES	5. NO
F	TURN TO P. 31, Q59.

36. About how long have you been limited in--(the kind of work), (the amount of work), (other activities) you could do?

_____ YEARS _____ MONTHS

F

37. What condition causes this limitation?

_____

_____

F

RECORD CONDITION IN FAMILY FOLDER
GO TO Q38.

997. "OLD AGE"	999. VAGUE OR UNSPECIFIC

37a. Is this limitation caused by any specific condition?

1. YES	5. NO
	TURN TO P.32 , Q65.

37b. What condition?

_____ **F**

RECORD CONDITION IN FAMILY FOLDER

38. INTERVIEWER CHECKPOINT

☐ 1.	LIMITATION LESS THAN 3 MONTHS, OTHER THAN PERMANENT DISABILITIES →	RE-ASK Q.34 - 35 WITH "Except for (CONDITION NAME)...."
☐ 2.	PREGNANCY AS CONDITION →	RE-ASK Q34 - 35. WITH "Except for (CONDITION NAME)...."
☐ 3.	OTHER CONDITION OR LONGER DURATION →	TURN TO P. 32, Q62.

39. In these questions, we want to find out about anything you can't or don't do because of your health or a disability.

Do you <u>now</u> have a job?

1. YES	5. NO
GO TO Q41.	↓

40. In terms of health, are you <u>now</u> able to work at all?

1. YES	5. NO
↓	**F**
	GO TO Q43.

41. Are you limited in <u>kind</u> of work you can do because of your health?

1. YES	5. NO
F	↓
GO TO Q43.	

41a. Are you limited in the <u>amount</u> of work you can do because of your health?

1. YES	5. NO
F	↓
GO TO Q43.	

42. Are you limited in the <u>kind</u> or <u>amount</u> of <u>other activities</u> because of your health?

1. YES	5. NO
F	TURN TO P. 31, Q59.
↓	

43. About how long have you been limited in--(the kind of work), (the amount of work), (other activities) you can do?

_____ YEARS _____ MONTHS

F

44. What condition causes this limitation?

_____

_____

F

RECORD CONDITION IN FAMILY FOLDER
TURN TO P. 28, Q45.

997. "OLD AGE"	999. VAGUE OR UNSPECIFIC

TURN TO P.28 , Q44a

44a. Is this limitation caused by any specific condition?

1. YES

5. NO

TURN TO P. 32 , Q65.

44b. What condition?_____ **F**

RECORD CONDITION IN FAMILY FOLDER

45. INTERVIEWER CHECKPOINT

☐ 1. LIMITATION LESS THAN 3 MONTHS, OTHER THAN PERMANENT DISABILITIES → RE-ASK Q41 - 42 WITH "Except for (CONDITION Name)...."

☐ 2. PREGNANCY AS CONDITION → RE-ASK Q41 - 42 WITH "Except for (CONDITION NAME)...."

☐ 3. OTHER CONDITION OR LONGER DURATION → TURN TO P. 32 , Q62

46. In these questions we want to find out about anything you can't or don't do because of your health or disability.

In terms of health, are you now able to keep house at all?

1. YES

5. NO

F

TURN TO P. 29 , Q49.

47. Are you limited in the kind of housework you can do because of your health?

1. YES

F

TURN TO P. 29 , Q49.

5. NO

47a. Are you limited in the amount of housework you can do because of your health?

1. YES

F

TURN TO P. 29, Q49.

5. NO

48. Are you limited in the kind or amount of other activities because of your health?

1. YES

F

5. NO

TURN TO P. 31, Q59.

49. About how long have you been limited in--(the kind of housework), (the amount of housework), (other activities) you can do?

_____ YEARS _____ MONTHS

F

50. What condition causes this limitation?

_____

_____

F

RECORD CONDITION IN FAMILY FOLDER
GO TO Q51.

997. "OLD AGE"

999. VAGUE OR UNSPECIFIC

50a. Is this linitation caused by any specific condition?

1. YES

5. NO
TURN TO P. 32, Q65.

50b. What condition?

_____ **F**

RECORD CONDITION IN FAMILY FOLDER

51. INTERVIEWER CHECKPOINT

☐ 1. LIMITATIONS LESS THAN 3 MONTHS, OTHER THAN PERMANENT DISABILITIES → RE-ASK Q47 - 48. WITH "Except for (CONDITION NAME)...."

☐ 2. PREGNANCY AS CONDITION → RE-ASK Q47 - 48. WITH "Except for (CONDITION NAME)..."

☐ 3. OTHER CONDITION OR LONGER DURATION → TURN TO P. 32, Q62.

52. In these questions we want to find out about anything you can't or don't do because of your health or disability. Do you have to go to a certain type of school because of your health?

1. YES
F
TURN TO P. 30, Q55.

5. NO

53. Are you limited in school attendance because of your health?

1. YES
F
TURN TO P. 30, Q55.

5. NO
TURN TO P. 30, Q54.

54. Are you limited in the kind or amount of other activities because of your health?

1. YES	5. NO
F	TURN TO P. 31 , Q59

55. About how long have you--(had to go to a certain type of school), (been limited in school attendance), (been limited in other activities) you can do?

_____ YEARS _____ MONTHS

F

56. What condition causes this limitation?

_____

_____

F

RECORD INFORMATION IN FAMILY FOLDER.
GO TO Q58.

997. "OLD AGE"	999. VAGUE OR UNSPECIFIC

57. Is this limitation caused by any specific condition?

1. YES	5. NO
	TURN TO P. 32 , Q65.

57a. What condition? _____ **F**

RECORD CONDITION IN FAMILY FOLDER

58. INTERVIEWER CHECKPOINT

☐ 1. LIMITATION LESS THAN 3 MONTHS, OTHER THAN PERMANENT DISABILITY → RE-ASK Q52 - 54. WITH "Except for (CONDITION NAME)..."

☐ 2. PREGNANCY AS CONDITION → RE-ASK Q52 - 54. WITH "Except for CONDITION NAME)...."

☐ 3. OTHER CONDITION OR LONGER DURATION → TURN TO P. 32 , Q62.

59. Please take your time and think carefully on this question. Are you limited in any way because of a disability or health?

1. YES
F

5. NO
TURN TO P. 32, Q65.

59a. In what way are you limited? (NOTE: WANT LIMITATION, NOT CONDITION)

_____

** Thank you. We're interested in getting details like that.**
RECORD LIMITATION IN FAMILY FOLDER.

59b. About how long have you been limited in (LIMITATION NAME)?

_____ YEARS _____ MONTHS

F

60. Please be as specific as you can on this question. What condition causes this limitation?

_____

997. "OLD AGE"

999. VAGUE OR UNSPECIFIC

_____

I see. That's important to us.
RECORD CONDITION IN FAMILY FOLDER.

60a. Is this limitation caused by any specific condition?

1. YES

5. NO
TURN TO P. 32, Q65.

60b. What condition? _____ **F**
RECORD CONDITION IN FAMILY FOLDER.

61. INTERVIEWER CHECKPOINT

☐ 1. LIMITATION LESS THAN 3 MONTHS, OTHER THAN PERMANENT DISABILITY → RE-ASK Q59. WITH "Except for (CONDITION NAME)..."

☐ 2. PREGANANCY AS CONDITION → RE-ASK Q59. WITH "Except for (CONDITION NAME)..."

☐ 3. OTHER CONDITION OR LONGER DURATION → TURN TO P. 32, Q62.

62. Is this limitation caused by any other condition?

_____ NO → GO TO Q65.
RECORD CONDITION IN FAMILY FOLDER

63. Is this limitation caused by any other condition?

_____ NO → GO TO Q65.
RECORD CONDITION IN FAMILY FOLDER

64. Which of these conditions would you say is the main cause of your limitation?

_____

65. Now we want to talk about hospitalizations during the last year, that is, since (MONTH), 1978. Please think back over that period. Were you a patient in a hospital at any time since (MONTH), a year ago?

1. YES	5. NO
F	GO TO Q66.

65a. How many times were you in a hospital since (MONTH) a year ago?

_____ NUMBER OF HOSPITALIZATIONS

66. Were you in a nursing home, convalescent home, or similar place since (MONTH) a year ago?

1. YES	5. NO
F	GO TO Q66.

66a. During that period, how many times were you in a nursing home or similar place?

_____ NUMBER OF TIMES

67. INTERVIEWER CHECKPOINT

RECORD NUMBER OF HOSPITALIZATIONS FROM Q65a. and 66a.
HERE AND IN FAMILY FOLDER _____

68. During the past 12 months, that is since (DATE) a year ago, about how many days did illness or injury keep you in bed all or most of the day? (Include the days during the 2-week period.) (Include the days while a patient in a hospital.)

0. NONE	1. 1 - 7 DAYS	2. 8 - 30 DAYS	3. 31 - 180 DAYS (1 - 6 MONTHS)	4. 181+ DAYS (6 MONTHS +)

Thanks. It isn't always easy to remember that.

69. Compared to other persons you age, would you say that your health is excellent, good, fair, or poor?

1. EXCELLENT	2. GOOD	3. FAIR	4. POOR
F	**F**	**F**	**F**

70. About how tall are you without shoes? _____ FEET _____ INCHES **F**

71. About how much do you weigh without shoes? _____ POUNDS **F**

72. What is your date of birth? _____ **F**
RECORD MONTH, DAY, YEAR

73. Are you now married, widowed, divorced, separated, or have you never been married?

1. MARRIED	2. WIDOWED	3. DIVORCED	4. SEPARATED	5. NEVER MARRIED
F	**F**	**F**	**F**	**F**

74. Are you now on full-time active duty with the Armed Forces of the United States?

1. YES	5. NO
F	**F**

75. It's important for us to find out about the health of different groups of people in the country.

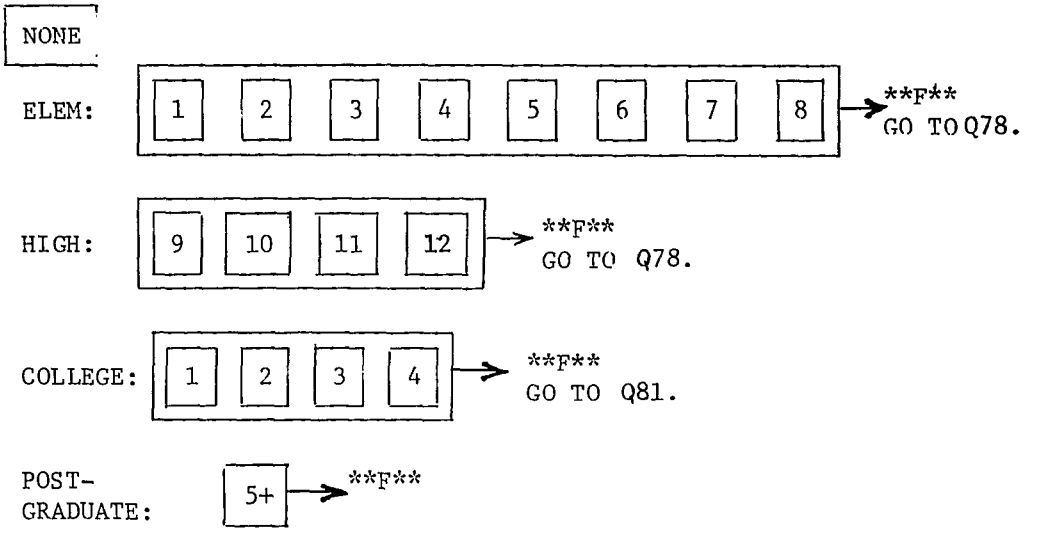
Would you mind telling me which of these groups describes your racial background? Are you white, black, Aleut, Eskimo or American Indian, Asian or Pacific Islander, or other group I haven't mentioned?

1. WHITE	**F**
2. BLACK	**F**
3. ALEUT, ESKIMO, OR AMERICAN INDIAN	**F**
4. ASIAN OR PACIFIC ISLANDER	**F**
7. OTHER: _____	**F**

76. Do any of these groups represent your national origin or ancestry? Are you of Puerto Rican, Cuban, Mexican, Mexicano, Mexican-American, Chicano, other Latin American, or other Spanish descent?

1. PUERTO RICAN	**F**
2. CUBAN	**F**
3. MEXICAN	**F**
4. MEXICANO	**F**
5. MEXICAN-AMERICAN	**F**
6. CHICANO	**F**
7. OTHER LATIN AMERICAN	**F**
8. OTHER SPANISH	**F**
9. NONE OF THESE	**F**

77. What is the highest grade or year you attended in school?



78. Did you finish the _____ grade?

1. YES
F

5. NO
F

79. Did you get a high school diploma or pass a high school equivalency test?

1. YES
F

5. NO
F

80. Have you had any other schooling?

1. YES

5. NO
GO TO Q82.

80a. What kind? _____

_____

F
GO TO Q82.

81. Did you finish the _____ year of college?

1. YES
F

5. NO
F

82. Do you have a college degree?

1. YES

5. NO
TURN TO P. 36, Q83

82a. What degree is that? _____ **F**

83. Did you work at any time last week or the week before--not counting work around the house?

1. YES
F

5. NO
F

83a. Even though you did not work during these two weeks, do you have a job or business?

1. YES
F

5. NO
F

83b. Were you looking for work, or on lay-off from a job?

1. LOOKING FOR WORK
F

2. ON LAY-OFF
F

3. NEITHER
F

84. INTERVIEWER CHECKPOINT

☐ 1. FIRST R, THIS FAMILY → GO TO FAMILY FOLDER, LIST OTHER FAMILY MEMBERS

☐ 2. NEW R, THIS FAMILY → COMPLETE QUESTIONNAIRES FOR OTHER FAMILY MEMBERS

OMB No. 68-578024
Expires: March 31, 1980
Project 468161

HOUSEHOLD ID NO. | | | | |

FAMILY AND PERSON ID NO. | | |

SURVEY RESEARCH CENTER
INSTITUTE FOR SOCIAL RESEARCH
THE UNIVERSITY OF MICHIGAN
ANN ARBOR, MICHIGAN 48106

INTERVIEWER: _____

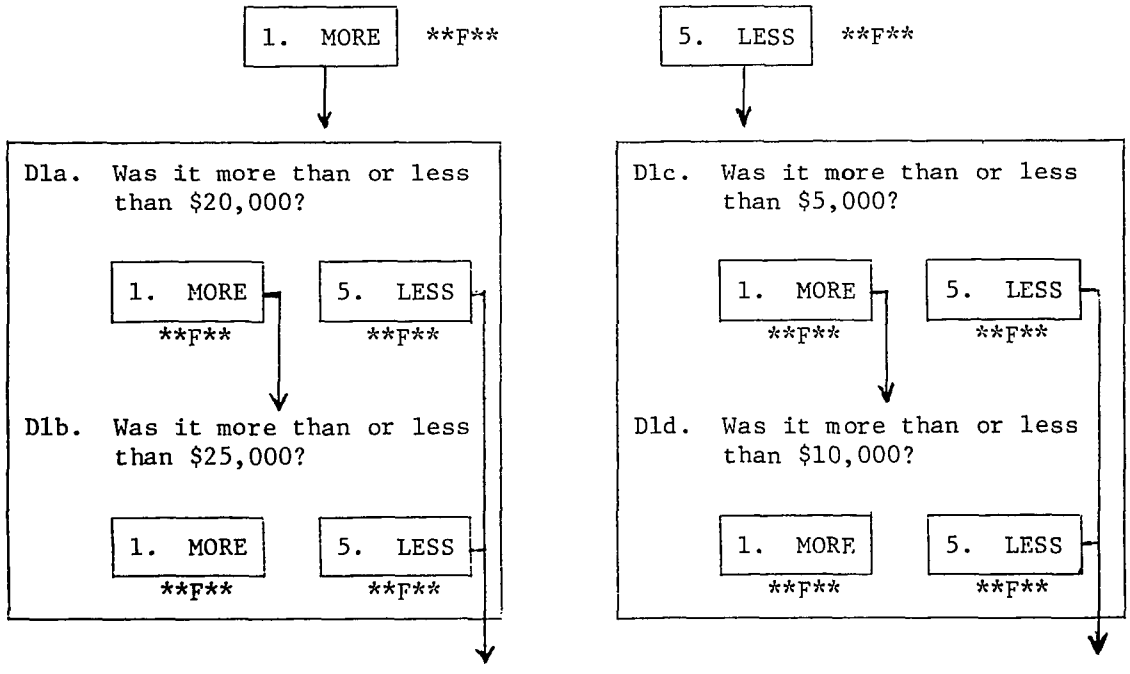
DATE OF INTERVIEW: _____

HEALTH IN AMERICA

EP - EO

DEMOGRAPHIC SECTION

D1. Now, thinking about your (family's) total income from all sources, did (you/your family) receive more than or less than $15,000 in 1978?



D2. I am supposed to ask these questions for our records. Other than the telephone number we're now using, could I reach you at home by dialing any other number?

1. YES **F**

5. NO **F** → TURN TO P. 2 , D6

D3. In total , how many telephone numbers do you have in your home?

_____NUMBER

D4. Are any of these numbers for business only?

1. YES	5. NO → GO TO D6
F	**F**

D5. How many are used only for business?

_____**F**
NUMBER

D6. INTERVIEWER CHECKPOINT

☐ 1. ODD NUMBERED HOUSEHOLD →	GO TO Q.D7
☐ 2. EVEN NUMBERED HOUSEHOLD →	GO TO Q.D13

D7. These last questions ask for your personal feelings about your health and your life in general. In answering them, please think carefully about your experience in the past and what you expect in the near future. Of course, if you don't have any feelings on a question or if you've never thought about it, just tell me.

Some people think about their health a great deal, while others take it for granted and don't think much about it. Would you say you think about your health very often, often, now and then, rarely, or never?

1. VERY OFTEN	2. OFTEN	3. NOW AND THEN	4. RARELY	5. NEVER

Now, I'll ask you to give me a number between one and seven that describes how you feel about your health -- "One" stands for "completely dissatisfied" and "Seven" for "completely satisfied". If you are right in the middle, answer "four". So, the low numbers indicate that you are dissatisfied, the high numbers that you are satisfied.

D8. We'd like to get your ideas very accurately on these questions so please take time and give me the number which best describes your feelings.

First, what number comes closest to how satisfied or dissatisfied you are with your health and physical condition in general?

_____**F**
NUMBER

8. NEVER THOUGHT: NO FEELINGS

D9. And, what number best describes how you feel about your physical ability to do the things you want to do?

_____ **F**
NUMBER

8. NEVER THOUGHT: NO FEELINGS

D10. What number comes closest to your feelings about the amount of energy or pep you have?

_____ **F**
NUMBER

8. NEVER THOUGHT: NO FEELINGS

D11. And what number comes closest to how satisfied or dissatisfied you are with your resistance to illness?

_____ **F**
NUMBER

8. NEVER THOUGHT: NO FEELINGS

D12. We have talked about various aspects of your health. Now I want to ask you about your life as a whole, and I want to get your ideas very accurately. Thinking about all the parts of your life, which number comes closest to how satisfied or dissatisfied you are?

_____ **F**
TURN TO P. 9 , D19

8. NEVER THOUGHT: NO FEELINGS

D13. These last questions ask for your personal feelings about your health and your life in general. In answering them, please think carefully about your experience in the past and what you expect in the near future. Of course, if you don't have any feelings on a question or if you've never thought about it, just tell me.

Some people think about their health a great deal, while others take it for granted and don't think much about it. Would you say you think about your health very often, often, now and then, rarely, or never?

1. VERY OFTEN	2. OFTEN	3. NOW AND THEN	4. RARELY	5. NEVER

D14. Now, thinking about your health and physical condition in general, would you say you are satisfied, dissatisfied or somewhere in the middle?

7. SATISFIED **F** 1. DISSATISFIED **F** 8. NEVER THOUGHT: NO FEELINGS
GO TO D15

D14a. We'd like to get your ideas very accurately; how satisfied are you with your health and physical condition--completely satisfied, mostly, or somewhat?

7. COMPLETELY
6. MOSTLY
5. SOMEWHAT
F TURN TO P. 5, D15

D14b. We'd like to get your ideas very accurately; how dissatisfied are you with your health and physical condition--completely dissatisfied, mostly, or somewhat?

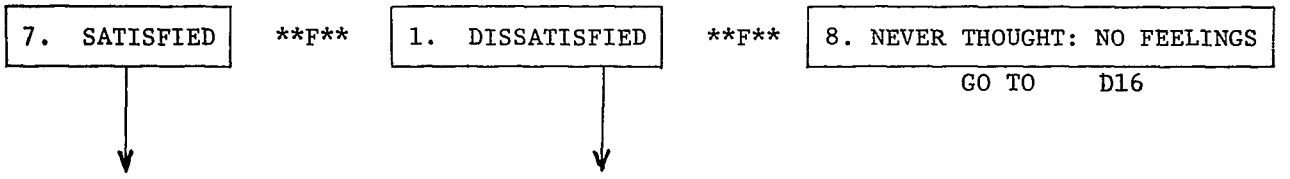
7. COMPLETELY
6. MOSTLY
5. SOMEWHAT
F TURN TO P. 5, D15

4. IN THE MIDDLE
F

D14c. We'd like to get your ideas very accurately. If you had to choose, would you say you are closer to being satisfied or dissatisfied with your health and physical condition, or are you right in the middle?

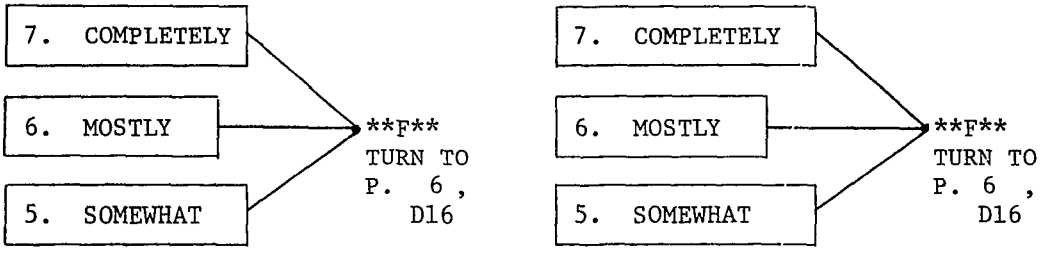
5. SATISFIED
3. DISSATISFIED
4. IN THE MIDDLE
F

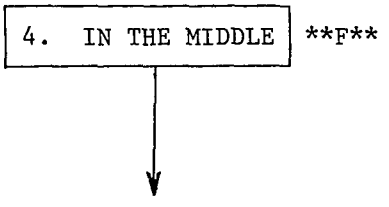
D15. And how do you feel about your physical ability to do the things you want to do--satisfied, dissatisfied, or somewhere in the middle?



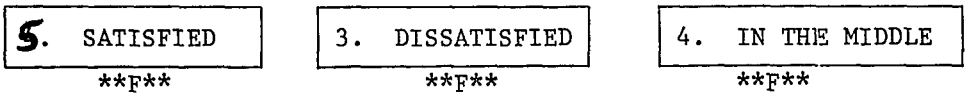
D15a. Are you completely satisfied, mostly satisfied, or somewhat satisfied with your physical ability to do the things you want to do?

D15b. Are you completely dissatisfied, mostly dissatisfied, or somewhat dissatisfied with your physical ability to do the things you want to do?

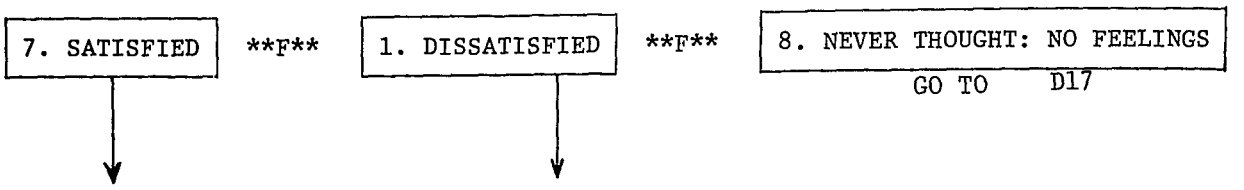




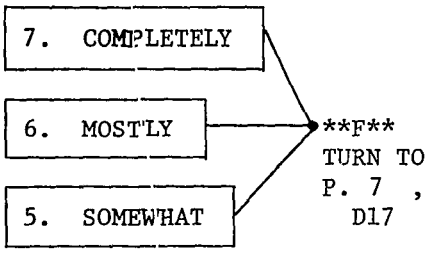
D15c. If you had to choose, would you say you are closer to being satisfied or dissatisfied with your physical ability to do the things you want to do, or are you right in the middle?



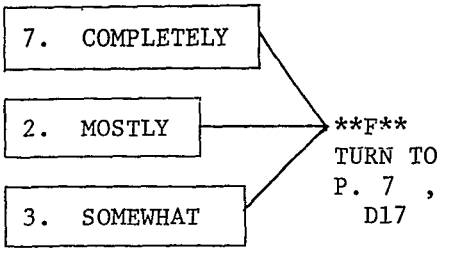
D16. How do you feel about the amount of energy or pep you have -- satisfied, dissatisfied, or somewhere in the middle?

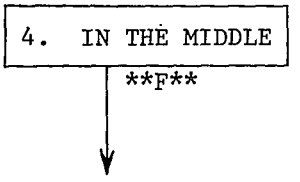


D16a. Are you completely satisfied, mostly satisfied, or somewhat satisfied with the energy or pep you have?

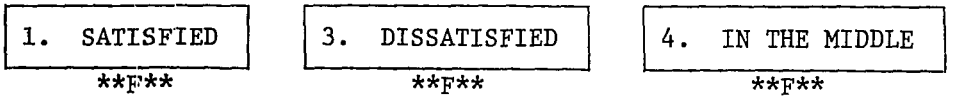


D16b. Are you completely dissatisfied, mostly dissatisfied, or somewhat dissatisfied with the energy or pep you have?

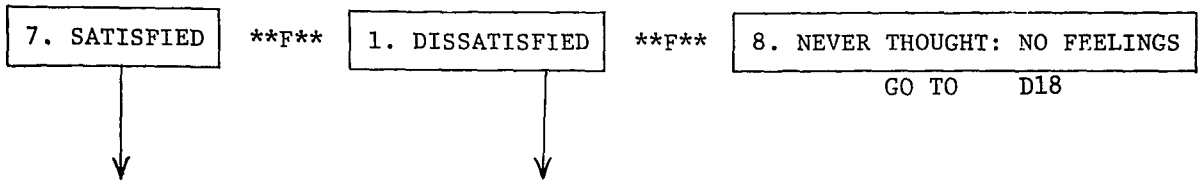




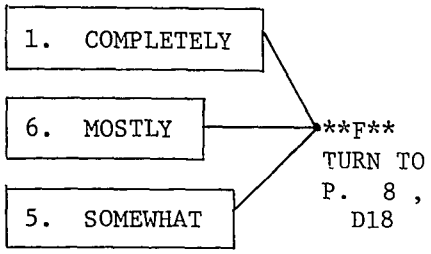
D16c. If you had to choose, would you say you are closer to being satisfied or dissatisfied with the amount of energy or pep you have, or are you right in the middle?



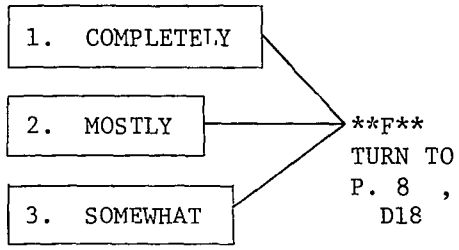
D17. And how do you feel about your resistance to illness -- satisfied, dissatisfied, or somewhere in the middle?

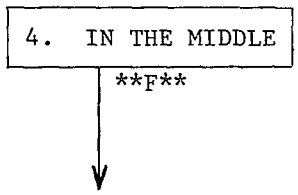


D17a. Are you completely satisfied, mostly satisfied, or somewhat satisfied with your resistance to illness?

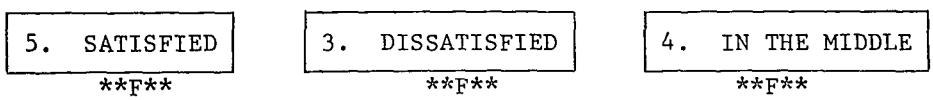


D17b. Are you completely dissatisfied, mostly dissatisfied, or somewhat dissatisfied with your resistance to illness?





D17c. If you had to choose, would you say you are closer to being satisfied or dissatisfied with your resistance to illness, or are you right in the middle?



D18. We have talked about various aspects of your health. Finally, I want to ask you about your life as a whole. Thinking about all the parts of your life, would you say you are satisfied, dissatisfied, or somewhere in-between?

7. SATISFIED **F** | 1. DISSATISFIED **F** | 8. NEVER THOUGHT: NO FEELINGS GO TO D19

D18a. Again, we'd like to get your ideas very accurately. Are you completely satisfied, mostly satisfied, or somewhat satisfied with your life as a whole?

7. COMPLETELY
6. MOSTLY
5. SOMEWHAT
→ **F** TURN TO P. 9 , D19

D18b. Again, we'd like to get your ideas very accurately. Are you completely dissatisfied, mostly dissatisfied, or somewhat dissatisfied with with your life as a whole?

7. COMPLETELY
2. MOSTLY
3. SOMEWHAT
→ **F** TURN TO P. 9 , D19

4. IN-BETWEEN **F**

D18c. Again, we'd like to get your ideas very accurately. If you had to choose, would you say you are closer to being satisfied or dissatisfied with your life as a whole, or are you right in the middle?

5. SATISFIED **F** | 3. DISSATISFIED **F** | 4. IN THE MIDDLE **F**

D19. INTERVIEWER CHECKPOINT

☐	1. ANOTHER FAMILY TO BE INTERVIEWED →	GO TO D20
☐	2. UNRELATED INDIVIDUAL(S) TO BE INTERVIEWED →	GO TO D21
☐	3. NO OTHERS TO BE INTERVIEWED →	GO TO D24

D20. I will be calling back to talk to a nember of the other family who lives at this number. Before I can speak with them, I need to see my supervisor. I'd like to thank you for your time and answers to our questions. Do you have any questicns you'd like to ask about our research?

[1. YES] [5. NO]

D21. Thank you for your time and naswers to our questions. Do you have any questions you'd like to ask about our research?

Now I'd like to talk to (NAME). Would you ask (NAME) to come to the phone?

[NEW R AVAILABLE] [NEW R UNAVAILABLE]
GO TO D24

D22. Thank you for coming to the phone. As part of a research project we're interviewing people throughout the country for the U.S. Public Health Service. The research concerns your health and the health care you receive.

Compared to this time last year, would you say your health is better, worse, or about the same?

[1. BETTER] [2. WORSE] [3. ABOUT THE SAME]

D23. How do you feel about the health care you receive?

_____

_____

GO TO PERSON QUESTIONNAIRE

D24. I'd like to call back to talk to (NAME) in the next few days. Can you suggest a time when you think (he/she) would be available?

(RECORD ON APPOINTMENT SHEET)

D25. Thank you for you time and answers to our questions. Do you have any questions you'd like to ask about our research?

D26. INTERVIEWER OBSERVATION: In this family, who reported the information for:

	SELF, COMPLETE	PARTSELF-PART OTHER	OTHER, COMPLETE
PERSON 1			
PERSON 2			
PERSON 3			
PERSON 4			
PERSON 5			
PERSON 6			

D27. INTERVIEWER CHECKPOINT

☐ 1. SINGLE R, THIS FAMILY ⟶ COMPLETE OTHER OBSERVATION ITEMS

☐ 2. MULTIPLE Rs, THIS FAMILY ⟶ DO NOT COMPLETE OTHER OBSERVATION ITEMS

D28. How many times did R ask how much longer the interview would last?

1. R NEVER ASKED | 2. R ASKED ONCE | 3. R ASKED TWO OR MORE TIMES

D29. In general, how interested in the interview do you think R was?

1. VERY INTERESTED | 2. SOMEWHAT INTERESTED | 3. NOT VERY INTERESTED

D30. How often did R ask for clarification of a question in the interview?

1. R NEVER ASKED FOR CLARIFICATION | 2. R ASKED ONCE | 3. R ASKED TWO OR MORE TIMES

D31. How many questions did you have to repeat so that R could answer them?

1. NO QUESTIONS REPEATED | 2. A FEW QUESTIONS REPEATED | 3. MANY QUESTIONS REPEATED

THUMBNAIL SKETCH

_____

_____

HOUSEHOLD I.D. ☐☐☐☐ **EP-EO**

FAMILY & PERSON I.D. NUMBER ☐☐

OMB NO. 68-578024
Expires March 31, 1980

P468161

CONDITIONS

ENTER CONDITION NAME _____

1. I'd like to get some more information about (your/NAME'S) (CONDITION NAME) .

INTERVIEWER CHECKPOINT

☐ 1. PREGNANCY → GO TO Q2

☐ 2. DELIVERY → GO TO Q3

☐ 3. ANY OTHER CONDITIONS → GO TO Q4

2. (Were you/Was NAME) sick because of (your/her) pregnancy?

[1. YES] **F** [5. NO] → GO TO Q14

2a. What was the matter?

USE THIS AS CONDITION NAME; GO TO Q4

3. Were there any problems with the delivery?

[1. YES] **F** [5. NO] → GO TO Q14

3a. What was the matter?

USE THIS AS CONDITION NAME

4. When did (you/NAME) last see or talk to a doctor about (your/NAME'S) (CONDITION NAME)? We'd like to get an exact date.

[0. INTERVIEW WEEK] **F**

[1. TWO-WEEK PERIOD (CHECK IN DOCTOR VISIT COLUMN)] **F**

[2. 2 WEEKS - 6 MONTHS] **F**

[3. 7-12 MONTHS] **F**

[4. OVER 1 YEAR - 2 YEARS] **F**

[5. OVER 2 YEARS - 5 YEARS] **F**

[6. OVER 5 YEARS] **F**

[7. NEVER] **F** GO TO Q6

[8. DK IF DOCTOR SEEN] **F** GO TO Q6

[9. DK WHEN DOCTOR SEEN] **F**

5. What did the doctor say it was? Did he or she give it a medical name?

_____ **F** (IF CANCER OR PREGNANCY, GO TO Q7)

6. What was the cause of (CONDITION NAME)?

_____ **F**

7. Please take your time and think carefully on the following questions. Once again we are interested in the time from _____ to _____. during those two weeks, did the (CONDITION NAME) cause (you/NAME) to cut down on the things (you/NAME) usually (do/does)?

1. YES **F** → 8.

5. NO → GO TO Q11

8. During that period, how many days did (you/NAME) cut down for as much as a day?

_____ DAYS **F** → 9.

00. NONE → GO TO Q11

9. During that period, how many days did (your/NAME'S) (CONDITION NAME) keep (you/NAME) in bed all or most of the day?

_____ DAYS **F** → 10.

00. NONE → 10.

10. How many days did (your/NAME'S) (CONDITION NAME) keep (you/NAME) from work during that two-week period, not counting work around the house?

_____ DAYS **F** → 11.

00. NONE → 11.

11. On this question, we'd like to get as exact a date as possible. When did (you/NAME) first notice (your/NAME'S) (CONDITION NAME)?

1. LAST WEEK **F**

2. WEEK BEFORE **F**

3. PAST TWO WEEKS-- DK WHICH **F**

4. OVER 2 WEEKS - 3 MONTHS **F**

5. 3-12 MONTHS **F**

6. MORE THAN 12 MONTHS AGO **F**

8. DK → 11a.

9. NA → 11a.

11a. Was it more or less than 3 months ago?

1. MORE **F**

5. LESS **F**

8. DK

9.NA

12. Was the (CONDITION NAME) due to an accident or injury?

1. YES **F** → 13

5. NO → GO TO Q.14

13. When did the accident/injury happen?

0. INTERVIEW WEEK **F**

1. TWO-WEEK PERIOD **F**

2. OVER 2 WEEKS - 3 MONTHS **F**

3. OVER 3 MONTHS - 1 YEAR **F**

4. OVER 1 YEAR - 2 YEARS **F**

5. OVER 2 YEARS **F**

8. DK → 13a

9. NA → 13a

13a. Was it more or less than 3 months ago?

1. MORE **F**

5. LESS **F**

14. INTERVIEWER CHECKPOINT

☐ 1. COMPLETE ANY OTHER CONDITION, DOCTOR VISIT, OR HOSPITALIZATION SECTIONS, THIS PERSON.

☐ 2. COMPLETE ANY CONDITION, DOCTOR VISIT, OR HOSPITALIZATION SECTIONS, NEXT PERSON THIS FAMILY.

☐ 3. COMPLETE THE DEMOGRAPHIC SECTION FOR THIS FAMILY.

HOUSEHOLD I.D. [ ][ ][ ][ ] **EP - EO**

FAMILY & PERSON I.D. NUMBER [ ][ ]

VISIT NUMBER _____

OMB NO. 68-5/8024

Expires March 31, 1980

P468161

DOCTOR VISITS

2+ VISITS THIS PERSON

Earlier you told me that (you/NAME) had seen or talked to a doctor during the past two weeks. Remember, this is the period from _____ to _____.

1. On what dates during that two-week period did (you/NAME) visit or talk to a doctor? We'd like to get an exact date.

_____(EXACT DATE) **F**

9994. OUTSIDE 2-WEEK PERIOD → GO TO Q7

9995. LAST WEEK | 9996. WEEK BEFORE | 9998. DK | 9999. NA

2. Were there any other doctor visits for (you/him/her) during that period?

1. YES **F** → 2a. On what dates were these other visits?

RECORD DATES FOR OTHER VISITS IN DOCTOR VISIT COLUMN

5. NO → GO TO Q3

→ [You told me (you/NAME) saw or talked to a doctor another time during those 2 weeks.]

3. Where did (you/he/she) see the doctor on the (DATE), at a doctor's office, hospital, clinic, or some other place?

1. WHILE INPATIENT (VOLUNTEERED) → GO TO Q7 INTERVIEWER CHECKPOINT

2. DOCTOR'S OFFICE **F**

3. TELEPHONE **F**

4. HOSPITAL

3a. Was it an outpatient clinic or hospital emergency room?

1. OUTPATIENT CLINIC **F**

2. HOSPITAL EMERGENCY **F**

3. INPATIENT (VOLUNTEERED) → GO TO Q7, INTERVIEWER CHECKPOINT

5. CLINIC

3b. Was it a company or industry clinic, or what?

1. COMPANY OR INDUSTRY **F**

2. OTHER (SPECIFY) _____ **F**

4. During the (visit/call) did (you/NAME) actually (see/talk to) the doctor?

1. YES **F**

5. NO **F**

5. Was the doctor a general practitioner or a specialist?

5. SPECIALIST → **F**

6. GENERAL PRACTITIONER → GO TO Q6 **F**

5a. What kind of specialist was (he or she)?

_____

6. Please be as specific as you can on this question. Why did (you/NAME) (visit/call) the doctor on (DATE)? (RECORD VERBATIM)

_____

_____ **F**

_____

6a. CHECK APPROPRIATE BOX:

1. DIAGNOSIS OR TREATMENT
2. GENERAL CHECKUP
3. PRE- OR POST-NATAL EXAM
4. EYE EXAM
5. IMMUNIZATION
6. OTHER (SPECIFY)_____

→ GO TO Q7 INTERVIEWER CHECK POINT

6b. Was this for any specific condition?

1. YES → **F**

5. NO → GO TO Q7, INTERVIEWER CHECKPOINT **F**

6c. What condition?

(INTERVIEWER): CHECK IF CONDITION LISTED IN CONDITION COLUMN. IF NOT, ENTER NAME

7. INTERVIEWER CHECKPOINT

☐ 1. COMPLETE ANY OTHER DOCTOR VISIT, CONDITION, OR HOSPITALIZATION SECTIONS FOR THIS PERSON.

☐ 2. COMPLETE ANY CONDITION, DOCTOR VISIT, OR HOSPITALIZATION SECTIONS FOR THE NEXT PERSON.

☐ 3. COMPLETE THE DEMOGRAPHIC SECTION FOR THIS FAMILY.

HOUSEHOLD I.D. [| | | |] **EP-EO**

OMB No. 68-578024
Expires: March 31, 1980

P468161

FAMILY & PERSON I.D. NUMBER [| |]

VISIT NUMBER_____

HOSPITALIZATIONS

1. You said that (you were/ NAME was) in the hospital (nursing home) (another time) during the past year. On these questions, we'd like to get exact details. On what date did (you/NAME) enter the hospital (nursing home) (the last time/ that time)?

_____MONTH _____DATE _____YEAR (IF EXACT): **F**

999999. NOT EXACT DATE —— 1a. Can you be any more exact about the date?

_____MONTH _____DATE _____YEAR (IF EXACT) **F**

989898. DK —— 1b. Would you think for a moment and give me your best estimate?

_____MONTH _____DATE _____YEAR (IF EXACT) **F**

2. How many nights (were you/was NAME) in the hospital (nursing home)?

_____NIGHTS (IF EXACT NUMBER): **F**

999. NOT EXACT NUMBER —— 2a. Can you be any more exact about the number of nights?

_____NIGHTS (IF EXACT): **F**

998. DK —— 2b. Would you think for a moment and give me your best estimate?

_____NIGHTS (IF EXACT): **F**

3. What is the name and address of this hospital (nursing home)?

_____
NAME

_____
ADDRESS

4. For what condition did (you/NAME) enter the hospital (nursing home) -- do you know medical name?

_____
CHECK CONDITION COLUMN; IF CONDITION NOT LISTED, ENTER NAME
GO TO Q5

998. DK → 4a. Could you give me a description of the condition?

_____
CHECK CONDITION COLUMN; IF CONDITION NOT LISTED, ENTER NAME → GO TO Q5

5. Were any operations performed on (you/NAME) during this stay at the hospital (nursing home)?

1. YES **F** → 5a

5. NO → GO TO Q6, INTERVIEWER CHECKPOINT

5a. What was the name of the operation? Please give me as much detail as possible.

_____

_____

998. DK → 5b. Could you give me a description of the operation?

_____

_____

5c. Any other operations during this stay?

1. YES → 5d

5. NO → GO TO Q6, INTERVIEWER CHECKPOINT

5d. What was the name of the operation? Please give me as much detail as possible.

_____

_____

6. INTERVIEWER CHECKPOINT:

☐ 1. COMPLETE ANY OTHER HOSPITALIZATION OR CONDITION SECTIONS, THIS PERSON.

☐ 2. COMPLETE ANY CONDITION, DOCTOR VISIT, OR HOSPITALIZATION SECTIONS, NEXT PERSON THIS FAMILY.

☐ 3. COMPLETE THE DEMOGRAPHIC SECTION FOR THIS FAMILY.

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