Series 2
No. 126

nchs

# Vital and Health Statistics

From the CENTERS FOR DISEASE CONTROL AND PREVENTION / National Center for Health Statistics

# National Health Interview Survey: Research for the 1995–2004 Redesign

July 1999

DEPARTMENT OF HEALTH & HUMAN SERVICES · USA

U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
Centers for Disease Control and Prevention
National Center for Health Statistics

CDC
CENTERS FOR DISEASE CONTROL AND PREVENTION

**Suggested Citation**

National Center for Health Statistics. National Health Interview Survey: Research for the 1995–2004 redesign. Vital Health Stat 2(126). 1999.

**Library of Congress Cataloging-in-Publication Data**

National Health Interview Survey : research for the 1995–2004 redesign.
p. cm. — (Vital and health statistics. Series 2, Data evaluation and methods research ; no. 126) (DHHS publication ; no. (PHS) 99-1326)
Includes bibliographical references.
ISBN 0-8406-0557-9. — ISBN 0-8406-0557-9
1. Health surveys—United States. 2. Medical statistics—United States. 3. Health status indicators—United States. I. Series. II. Series : DHHS publication ; no. (PHS) 99-1326.
[DNLM: 1. National Health Interview Survey (U.S.) 2. Health Surveys—United States. 3. Data Collection—methods—United States. 4. Sampling Studies—United States. 5. Statistics—United States.
W2 AN148vb no. 126 1999]
RA409.U45 no. 126
[RA407.3]
362.1'0723 s—dc21
[362.1'0723]
DNLM/DLC 99-15863
for Library of Congress CIP

For sale by the U.S. Government Printing Office
Superintendent of Documents
Mail Stop: SSOP
Washington, DC 20402-9328
Printed on acid-free paper.

# Vital and Health Statistics

# National Health Interview Survey: Research for the 1995–2004 Redesign

Series 2:
Data Evaluation and Methods Research
No. 126

U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
Centers for Disease Control and Prevention
National Center for Health Statistics

Hyattsville, Maryland
July 1999
DHHS Publication No. (PHS) 99-1326

## National Center for Health Statistics

Edward J. Sondik, Ph.D., *Director*

Jack R. Anderson, *Deputy Director*

Jack R. Anderson, *Acting Associate Director for International Statistics*

Lester R. Curtin, Ph.D., *Acting Associate Director for Research and Methodology*

Jennifer H. Madans, Ph.D., *Acting Associate Director for Analysis, Epidemiology, and Health Promotion*

P. Douglas Williams, *Acting Associate Director for Data Standards, Program Development, and Extramural Programs*

Edward L. Hunter, *Associate Director for Planning, Budget, and Legislation*

Jennifer H. Madans, Ph.D., *Acting Associate Director for Vital and Health Statistics Systems*

Douglas L. Zinn, *Acting Associate Director for Management*

Charles J. Rothwell, *Associate Director for Data Processing and Services*

### Office of Research and Methodology

Lester R. Curtin, Ph.D., *Acting Associate Director*

Lester R. Curtin, Ph.D., *Chief, Statistical Methods Staff*

Trena Ezzati-Rice, *Chief, Survey Design Staff*

Jimmie D. Givens, *Acting Chief, Statistical Technology Staff*

Kenneth W. Harris, *Acting Manager, Questionnaire Design Research Laboratory Staff*

Primary contributors:

David Judkins
David Marker
Joseph Waksberg
Steven Botman
James Massey

Other contributors:

John Edmonds
Mansour Fahimi
Hüseyin Göksel
Doris Northrup (CODA)
Svetlana Ryaboy
Valerija Smith
David Wright

Prepared under Contract number 200–89–7021 by
Westat, Inc.
1650 Research Boulevard
Rockville, Maryland 20850

# Preface

This report presents a detailed description of a major portion of the research undertaken to redesign the National Health Interview Survey (NHIS) for the period 1995–2004.

The NHIS is one of the major data collection programs of the National Center for Health Statistics (NCHS). Through NHIS, information concerning the health of the U.S. civilian noninstitutionalized population is collected in household interviews throughout the United States. NHIS has been in continuous operation since 1957, and its sample design has been reevaluated and modified following each of the last four decennial censuses of the U.S. population.

The 1995–2004 redesign of NHIS was a major undertaking that involved a number of government agencies as well as several private contractors. The Survey Design Staff in the Office of Research and Methodology (ORM), NCHS, in collaboration with the Division of Health Interview Statistics (DHIS), NCHS, had overall responsibility for the development and implementation of the 1995–2004 NHIS redesign. Monroe Sirken, formerly the ORM Associate Director, played a major role in the conceptualization and planning of the research program. The late Jim Massey, former Chief of the Survey Design Staff, and Don Malec, formerly of the Survey Design Staff, had the primary responsibility for directing the redesign research and coordinating the research activities conducted by NCHS, the U.S. Bureau of the Census, and Westat, Inc. The late Steve Botman, formerly of the Survey Design Staff, had the lead responsibility in helping to implement the 1995–2004 NHIS redesign, and also carried out many important steps leading to the publication of this report. Van Parsons of the Statistical Methods Staff, ORM, contributed to the development of subdesigns for the 1995–2004 NHIS redesign. Keith Hoffman and Trena Ezzati-Rice of the Survey Design Staff participated in the research and evaluation of the Social Security Administration files as a potential supplemental sampling frame for NHIS, as described in chapter 6 of this report. Chris Moriarity of the Survey Design Staff carried out the final steps of Steve Botman's work that led to the publication of this report. The late Owen Thornberry, formerly the Director, DHIS, and John Horm, formerly the acting Chief of the Survey Planning and Development Branch, DHIS, were the leading DHIS participants in the 1995–2004 NHIS redesign work.

Under contract with NCHS between 1989 and 1993, Westat, Inc., conducted research for the 1995–2004 NHIS redesign. The primary researchers at Westat, Inc., included David Judkins, David Marker, and Joseph Waksberg. Other contributors included John Edmonds, Mansour Fahimi, Hüseyin Göksel, Doris Northup (CODA), Svetlana Ryaboy, Valerija Smith, and David Wright.

Additional research was conducted by the U.S. Bureau of the Census and coordinated through the Task Force on Household Survey Redesign, assembled and directed by the late Maria Gonzalez of the Office of Management and Budget (OMB). The task force was formed to coordinate and monitor the U.S. Bureau of the Census's simultaneous redesign of all of the household surveys the Bureau conducts for other Federal government agencies. The task force played an important role in coordinating the technical and funding requirements for the redesigns of all of the surveys.

The U.S. Bureau of the Census has been the primary data collector for the NHIS since its inception. The Bureau also has been involved with the research and implementation of the NHIS redesigns. For the 1995–2004 redesign, the Demographic Statistical Methods Division (DSMD) had the primary responsibility for evaluating alternative primary sampling unit definitions for NHIS, and for implementing the redesigned sample. DSMD staff also participated in the research and evaluation of the Social Security Administration files as a potential supplemental sampling frame for NHIS, as described in chapter 6 of this report. Persons at the U.S. Bureau of the Census that deserve special recognition for their contribution to the 1995–2004 NHIS redesign effort include Preston Jay Waite, formerly Chief of DSMD; Thomas Moore, Chief of the Health Surveys and Supplements Branch in DSMD; Robert Mangold, formerly Chief of the Health Surveys Branch in the Demographic Surveys Division; Patricia Wilson of DSMD; and Lloyd Hicks, formerly of DSMD.

The primary focus of the 1995–2004 NHIS redesign research was to explore sample design options to improve the reliability of NHIS statistics for racial, ethnic, economic, and geographic domains. Another objective was to assess the integration of the sample designs of the NCHS surveys of health care providers and NHIS.

The decisions for the 1995–2004 NHIS redesign were based on a number of factors including technical soundness, competing analytical objectives, operational feasibility, costs, and available resources. The research results presented in this report were the primary basis for assessing the technical soundness and costs for a number of design alternatives. In particular, as part of Westat, Inc.'s, research, a sample design referred to as the ''alpha'' design was developed, which assumed a 50-percent data collection budget increase to permit oversampling of racial and ethnic minorities. Westat, Inc., researchers also developed a modification of the ''alpha'' design, the ''beta'' design, which assumed no change in NHIS data collection budget. The ''beta'' design, which became the design implemented in 1995, is described in chapter 17. The research results in this report also led to additional research, primarily in the area of survey integration.

A more detailed description of the 1995–2004 NHIS design will appear in a forthcoming NCHS Series 2 report titled *Design and Estimation for the*

*1995–2004 National Health Interview Survey.*

As required by the contract between NCHS and Westat, Inc., a draft manuscript of this report was submitted to NCHS by Westat, Inc., in early 1994. A revised manuscript was submitted to NCHS by Westat, Inc., in early 1996. This report is very similar to the 1996 manuscript, although personnel from Westat, Inc., and NCHS made additional revisions before publication.

# Contents

## Appendixes



## Text Figure



## Text Tables



## Detailed Tables

Abstract

**Objectives**

This report presents a major portion of the research carried out for the 1995–2004 redesign of the National Health Interview Survey. The primary focus of the 1995–2004 NHIS redesign research was to explore sample design options to improve the reliability of NHIS statistics for racial, ethnic, economic, and geographic domains. Another objective was to assess the integration of the sample designs of the NCHS surveys of health care providers and NHIS.

**Methods**

A number of research tasks were carried out by Westat, Inc., to explore the feasibility and cost of various sample design options for the 1995–2004 National Health Interview Survey redesign. This report provides a detailed description of the research that was carried out.

**Results**

The research results presented in this report were the primary basis for assessing the technical soundness and costs for a number of design alternatives. The first option, called the alpha option, was developed under the assumption of a 50-percent data collection budget increase. The second option, called the beta option, was developed under the assumption of no change in the data collection budget. The beta option was the design implemented in 1995.

Other important research items described in this report include oversampling methods for minority populations and dual-frame sampling methods. The research results in this report also led to additional research, primarily in the area of survey integration.

A more detailed description of the 1995–2004 NHIS design will appear in a forthcoming NCHS Series 2 report titled *Design and Estimation for the 1995–2004 National Health Interview Survey.*

**Keywords:** *integrated survey design • model-based estimation • dual-frame estimation • network sampling*

# National Health Interview Survey: Research for the 1995–2004 Redesign

## Executive Summary

### Introduction

The sample design for the National Health Interview Survey (NHIS) has traditionally been revised following each decennial census. The redesigns serve two purposes. The first is simply to update the sampling material through the use of data from the recently completed census. The second is to redirect the sample toward new goals, making the NHIS responsive to the expected needs of health statistics in the following decade.

The timing of the planning and introduction of the revised sample is coordinated with similar revisions the U.S. Bureau of the Census makes for its other demographic surveys, including the Current Population Survey, the American Housing Survey, the Survey of Income and Program Participation, and the National Crime Survey. The coordination is necessary to ensure that households are not part of the sample in more than one survey; it is also an efficient way of selecting samples for multiple surveys. As a result, however, changing the design is an extremely complex task, even during the traditional postcensus window.

The U.S. Bureau of the Census requires a 4-year lead time to change procedures for selecting blocks, the basis for the area sample employed for NHIS. A 3-year lead time is required to change primary sampling units (PSU's), and an additional year is needed to list the housing in sample blocks and select the households that will be asked to participate in NHIS. Cuts in sample size can be made much more quickly, usually in a matter of months, provided that they follow certain guidelines, but expansions take at least 2 years—longer for expansions that go beyond limits set during the redesign. This problem can be solved by an initial selection of a very large sample, but there are substantial financial costs to be borne for selecting more sample than is actually needed. Because of the coordination requirement, the 1995 redesign is the only opportunity that the National Center for Health Statistics (NCHS) will have to make fundamental changes in the sample design of NHIS at a reasonable cost until the year 2005.

NCHS put considerable effort into debating and consolidating ideas about the most critical objectives for NHIS statistics. A number of objectives were identified—more than it appeared likely could be simultaneously realized, given budget prospects. Westat, Inc., was awarded a contract to assist in the development of a new design that would meet these objectives to the greatest extent feasible, given various funding assumptions and revised priorities. Work to be done by Westat, Inc., consisted of the statistical and cost aspects of sample design options. The contract did not include any provision for research on the instrument content or design. Westat, Inc.'s, full final report synthesizes 4 years of research, meetings, and 119 technical memorandums (a list of their titles is included as appendix 1) on the most efficient sampling methods for achieving the objectives. From this research a proposed design for 1995 was developed. In case budgets did not permit the introduction of the proposed design, a backup design was also developed.

This executive summary briefly reviews the objectives and the methods studied. More discussion is devoted to describing the proposed design and discussing the extent to which it meets each of the objectives. For more detail the reader is referred to the full report.

## Research Objectives

The highest priority requirements for the redesigned NHIS, as specified by NCHS, were:

- Improved precision of health statistics about demographically defined domains, including specifically those defined by the interaction of age and sex with race, ethnicity, and poverty status.
- Improved precision of health statistics for subnational geographic areas—in particular, States.
- Decreased bias by eliminating proxy reporting.
- An ability to quickly and radically change the size of NHIS in response to funding opportunities and cutbacks.
- Improved analytic potential and/or cost reduction through linkage with the provider surveys sponsored by NCHS.
- A continued ability for NHIS to serve as a vehicle for follow-on surveys such as the National Survey of Family Growth (NSFG).

## Research Methodology

### Improved Precision for Demographically Defined Domains

A wide range of techniques was studied that might be used to improve precision for racial and ethnic minorities and other demographically defined domains. These techniques included oversampling geographic areas with strong concentrations of targeted domains, screening a general sample to identify members of the targeted domains (and subsequently subsampling others), sampling from lists such as the Medicare list, network sampling (where members of the targeted domains are asked to nominate their relatives for inclusion in the sample), and using simple techniques such as carrying sample cases of targeted domains over from 1 year to the next or aggregating statistics across years.

Important considerations in these studies were the prevalence of the domain, the extent of geographic clustering of domain members, the contribution of each stage of sampling to the variance for a domain, coverage of lists of domain members, the variances and potential biases of network sampling, and the power of multiyear estimates, given that each year's sample is not independent of other years. Another important component of this work was the projection of costs for alternative designs. An attempt was made to form an extremely general cost model based on interviewer time sheets for NHIS from the U. S. Bureau of the Census, but most cost projections were actually done in a more informal manner, based on information supplied by persons experienced in the management of survey operations.

To study the prevalence and geographic clustering of each domain, data were examined from the NHIS itself, the 1980 and 1990 decennial censuses, and demographic models of population growth from the U. S. Bureau of the Census. To study the contribution of each stage of sampling, an analysis of components of variance was conducted on the 1988 NHIS with new methodology. The main lists considered were the master beneficiary lists maintained by the Social Security Administration (SSA) and Health Care Financing Administration (HCFA) for administration of Social Security and Medicare. Most of the work done on network sampling of rare domains was conceptual in nature with recommendations for further research. The power of multiyear estimates and carryover procedures was based on the work on components of variance and U. S. Bureau of the Census data on the frequency of residential moves by minority persons.

### Improved Precision for Geographically Defined Domains

This research started from an analysis by NCHS staff (1) indicating that State-level statistics from NHIS have very poor precision for most States. Based on U. S. Bureau of the Census projections of State populations in the year 2000 (the midpoint of the period in which the redesigned sample would be used), estimates were made of the size of sample supplements that would be needed to improve the precision of State-level statistics to more acceptable levels. These estimates were prepared under the assumptions that the supplemental samples would be of the traditional area-permit type and the samples would be obtained by means of random digit dialing (RDD). Various groupings of States for which acceptable precision could be more easily obtained were also studied. Finally, there was an intensive study of the potential of various small-area techniques, including model-based statistics.

### Decreased Bias Because of Proxy Reporting

Work by NCHS staff had identified proxy reporting as a major cause of response bias. The size of the bias tends to vary among items but appears to be particularly large for the more subjective items, such as self-perceived health status. Obtaining response rates nearly as good as the current response rates while disallowing proxy response is quite expensive. To offset the increased cost of this requirement, the idea was put forward to interview just one randomly chosen adult per family. Although the merits of this approach were mostly outside the scope of this research, the potential that this system would be imposed conditioned all the other research and required study of design options that assumed all adults would be interviewed in each sample household and that just one adult would be interviewed per family within the household.

## Flexibility for Rapid Sample Expansions and Contractions

Research on rapid sample expansions consisted mostly of discussions about the cost of preparing and maintaining extra samples. The task of a rapid expansion is not as easy as it seems it should be. Given requirements from the Office of Management and Budget for minimization of response burden, the complexity of the designs of other current surveys at the U. S. Bureau of the Census, and the NHIS requirement for no recourse to decennial census address registers, issues of coordination are extremely complex. Also, there are costs for fieldwork done to keep a sample ready to use (even if it is never used), for example, collecting building permit information from selected building permit offices. Finally, it is difficult to select additional samples efficiently for data collection instruments and procedures that have not been designed yet.

Rapid contractions are considerably easier to design. Research in this area focused on alternate panel construction methodologies.

## Integrated Survey Design

Integration of the NHIS design with the design for the National Health Care Survey (NHCS), an umbrella term for a collection of independent surveys of health care providers was an important issue for the NCHS staff. There was interest in the possibility of enhanced analytic strength from an integrated set of surveys and the possibility of operational synergies that might lead to lower overall cost for the integrated system.

Research in this area focused most strongly on the implications of integration at the PSU level, where all the surveys would be located in the same set of PSU's or where the NHCS PSU's were at least nested within the NHIS PSU's. Given the paucity of county-level health and health care data, the research that could be conducted in this area was limited. The research that was done focused on the compatibility of optimal PSU-level measures of size for the various surveys and on the implications for the provider surveys of two-stage versus three-stage design. The former work was based on tabulations of the National Master Facility Inventory and other NCHS sources. Similarity of the set of PSU's to be included with certainty for each survey was closely examined. Also, the correlation in the measure of size for the noncertainty PSU's was examined. The latter set of work was based on National Hospital Discharge Survey (NHDS) and National Ambulatory Medical Care Survey (NAMCS) data. The NHDS work was based on a review of internal NCHS documents. The NAMCS work involved the calculation of components of variance using new methodology.

## Follow-on Surveys

A number of NCHS surveys use respondents from NHIS samples from previous years as screening samples to find domains of special interest for intensive interviews on topics that cannot be included in NHIS because of time or other constraints. The best known example of this sort of sampling approach is NSFG. The flexibility to be able to use retired NHIS samples for this purpose was the reason for the switch in 1985 from an NHIS sample based on a mix of decennial census address registers, manual block listing in areas with poor quality registers, and new construction building permits, to a sample based only on manual block listing and building permits. The cost of this switch was far from trivial because of the requirement for much more listing. This extra listing cost continues to be a factor for the 1995 redesign. To preserve the benefits of this expense for extra listing, a factor in all discussions about the NHIS design was the potential impact of design changes on these follow-on surveys. Particular tension was noted between the large numbers of sample PSU's needed for improved precision for State estimates and the small number of sample PSU's usually needed for follow-on surveys.

## Major Findings and Recommendations

The major outcome of this contract was a new recommended design for NHIS. This recommended design is labeled the ''alpha option.'' The specifications for this design flowed directly from the research conducted by Westat, Inc., with several iterations of refinement in response to feedback from NCHS and the U. S. Bureau of the Census. The alpha option is predicated on the assumption of an ongoing budget for 1995 and beyond that is 50 percent larger than would be required to sustain the current design at a strength of 50,000 completed household interviews per year. Because the current NHIS has been running close to 47,500 completed household interviews per year, this implies a need for an increased budget on the order of 58 percent. If an additional option regarding elderly minority groups is exercised, the total budget increase becomes about 62 percent. There is also the assumption that 1994 monies can be found for the extra listing of blocks and recruiting and training of interviewers required by the alpha option.

Recognizing the uncertainties of the Federal appropriation process, a ''beta option'' was also identified that would have the same long-term costs as the current design with 47,500 annual household interviews. NCHS set the goals for the beta option of maintaining current precision for statistics for black people; and improving precision on statistics for Hispanic persons enough to achieve parity with those for black persons.

## Improved Precision for Demographically Defined Domains

The alpha option responds to all of the goals of the 1995 design to some extent. The primary achievement of this design, however, is to provide substantial improvements in the precision of statistics about the black and Hispanic populations. The improvements for Hispanic persons are particularly strong. The alpha option provides considerable improvement in the precision of statistics on the black and Hispanic populations at the cost of precision for other demographically defined domains. The tradeoff is sharper for the beta option because it improves the precision of statistics on Hispanic persons and holds the precision of statistics on black persons the same, all at current budget levels. Table A quantifies these projected precision changes in terms of changes in effective sample sizes. (The effective sample sizes shown here correct the raw sample sizes for the design effects resulting from differential sampling rates across the minority density strata and across adults in households of different sizes.) The sample sizes do not add up across the domains because of different design effects. The deterioration in precision for totals and for other categories was within a zone with which NCHS staff felt comfortable.

### Statistics on the Black and Hispanic Populations

These improvements in the precision of statistics for the black and Hispanic populations were achieved by a mix of two techniques: Stratification on minority density with nonproportional allocation, and screening. Stratification on minority density with nonproportional allocation means that blocks are stratified by the percentage of the local population that is black and by the percent that is Hispanic, and that a disproportionate share of the initial sample is selected in the strata with high black and/or Hispanic density. Screening means that a brief set of questions is first administered to each household to determine whether any of the residents are black or Hispanic; if the household is black or Hispanic, the household is retained with certainty for a full interview; otherwise, the household is only retained for a full interview with a probability less than one.

The stratification and oversampling by minority density is a technique that was also used for the 1985 design, albeit to a lesser extent. For that design enumeration districts and block groups (higher levels of geographic units than blocks) were stratified by percentage of the local population in 1980 that was black. Also, the degree of oversampling of the strongly black areas was constricted (by plans for the formation of interviewer assignments) in such a manner that there was almost no oversampling in rural southern PSU's with strong black concentrations. The new design employs finer geographic detail and an alternate plan for building interviewer assignments to make the oversampling much more efficient. The new design also focuses more on Hispanic density than on black density, given the fact that the Hispanic population is currently smaller than the black population in the United States.

Screening is a technique that has been used at private survey research firms for many years but has been employed only once at the U. S. Bureau of the Census (for the Survey of Income and Education in the 1960's). Given the long length of time since the last use of screening at the U. S. Bureau of the Census, the Bureau decided to conduct a field test of screening procedures. This field test, conducted by the Bureau in 1993 with very favorable results, focused on a particularly sensitive aspect of the plan for screening (the decision to contact neighbors to keep response rates high and costs low).

The beta option also leads to improvements for statistics on Hispanic persons, but not as much as the alpha option. It provides this improvement with no change in the precision of statistics on the black population and with no increase in cost. As already noted, the beta option provides for even worse precision than the alpha option for the population that is neither black nor Hispanic, although the difference in this area between the alpha and beta options is minor.

An alternative with either the alpha or the beta option is to supplement the area-permit sample with a sample of black and likely Hispanic SSA beneficiaries. (The word ''likely'' is used because the SSA files do not actually have a variable that identifies Hispanic persons. Research has shown, however, that there is a set of surnames that is associated strongly enough with being Hispanic to serve as the basis for sampling.) Table B shows the increases in the effective sample sizes for elderly black and Hispanic persons that could be achieved with supplements from SSA lists of the indicated sizes.

### Other Demographically Defined Domains

Besides the black and Hispanic populations, the other domains of interest that were studied include Asians and Pacific Islander Americans, American Indians, Eskimos, and Aleuts, the poor, the rural population, teenagers, the extreme elderly (85 years of age and over), and Hispanic subgroups such as Puerto Ricans.

A study of 1990 decennial census results indicated that the Asian and Pacific Islander population is too dispersed for oversampling to be very effective and too small for substantial improvement via screening to be practical. The same conclusion applies to the Native American population. It was noted, however, that if interest were focused solely on the Native American population living on reservations, trustlands, tribal jurisdiction statistical areas, and native villages, the relatively few numbers of other racial and ethnic groups living in these areas would make screening highly feasible.

With improved precision for estimates on the black and Hispanic populations, there is no question that the

**Table A. Effective adult sample sizes by redesign option and population domain relative to the 1988 NHIS design**

| Option | Budget[1] | Total | Black | Hispanic | Other |
|---|---|---|---|---|---|
| 1988 NHIS | 100 | 35,600 | 4,500 | 2,200 | 30,900 |
| Alpha option | 158 | +8% | +97% | +299% | –4% |
| Beta option | 100 | –12% | +4% | +113% | –21% |

[1]Budget for 1988, adjusted for inflation, equals 100.

**Table B. Sample sizes for the black and Hispanic populations, aged 65 and older**

| Domain | Desired | Effective sample sizes | | | |
|---|---|---|---|---|---|
| | | Projected under alpha option (one household) | Projected under alpha option (all household) | With extra 2,800 cases to be screened from SSA (one household) | With extra 5,800 cases to be screened from SSA (one household) |
| Black | | | | | |
| Male | 1,000 | 500 | 1,150 | 1,000 | 1,000 |
| Female | 1,000 | 740 | 1,710 | 1,000 | 1,000 |
| Hispanic | | | | | |
| Male | 1,000 | 350 | 910 | 770 | 1,000 |
| Female | 1,000 | 495 | 1,270 | 920 | 1,000 |

alpha option would also improve the precision for statistics about the poor black and poor Hispanic populations, because the oversampling of strongly black and/or Hispanic neighborhoods means oversampling many of the Nation's poorest neighborhoods. Precision for other poor Americans, however, will be worse after the redesign. The effect on precision for statistics on all poor persons has not been quantified but is probably slight.

For statistics on the rural population, it was noted that there are a number of competing definitions of the term. The U.S. Bureau of the Census definition of rural applies at the block level. Rural areas exist within metropolitan areas, and urban areas exist within nonmetropolitan areas. Population counts per block are necessary for the classification of areas as urban or rural under this definition. Because such counts are only collected in the decennial census, statistics about rural areas become outdated fairly soon after each census. The U.S. Department of Agriculture has several definitions of rural counties, but none of these seemed to reflect NCHS needs. On consideration of the various alternatives, the NCHS staff decided that its analytic needs would be met best by compiling data for nonmetropolitan areas rather than for the areas encompassed by one of the definitions of a rural area.

Although precision for NHIS nonmetropolitan statistics is already fairly good, it was decided to further improve the precision by increasing the total number of PSU's for NHIS from 198 in the 1985 full design to 358 in the 1995 full design. Also, it was decided that metropolitan status should be a strict stratifier in the selection of PSU's and that at least one nonmetropolitan PSU would be selected from each of the 50 States (except New Jersey, which is all metropolitan). These improvements would be counterbalanced to some extent; however, because the oversampling of the black and Hispanic populations would tend to shift sample persons from nonmetropolitan areas to metropolitan areas. Also, the reduction in the total number of completed interviews (to pay for the screener interviews) and the design effect due to differential sampling of black, Hispanic, and other persons would tend to degrade the precision of nonmetropolitan statistics. The net effect of these changes is that, although there would be more nonmetropolitan PSU's in the sample in the 1995–2004 decade than during the 1985–1994 decade, the total number of nonmetropolitan interviews would be reduced.

With regard to teenagers, a plan was considered that would have involved screening for age and retaining households with members in targeted age domains at higher rates, but this plan was rejected. At any rate this plan was only meant to provide for oversampling of the black and Hispanic populations in targeted age domains. At the planned budget level, an important increase in the sample size of teenagers could only be achieved by scrapping the oversampling of densely black and Hispanic blocks. This goal (teenage statistics) was essentially put aside.

Oversampling of the extremely elderly population would be quite efficient with Medicare or SSA lists. However, there was no way of accomplishing this goal while simultaneously achieving competing needs for precision. The main reason is that this group is predominantly white, and statistics on white people, in general, will be less precise with the 1995 design than with the 1985 design.

With regard to Hispanic subgroups, it was realized that however desirable this goal might be in light of the diversity of the Hispanic population, it would simply require more funds than NCHS foresees being available. Model-based techniques were investigated that might involve special samples in only blocks with very high concentrations of the targeted subgroups, but the investigation was inconclusive.

## Improved Precision for Geographically Defined Domains

The 1995 design will provide improved flexibility for sample supplements that would lead to State-level statistics with acceptable precision, but the design will not by itself provide such statistics for more than three States—none if the one-adult-per-family rule is used for interviewing. This flexibility was achieved by stratifying the PSU sample by State and by metropolitan status. This stratification will make it far easier to integrate the regular NHIS sample with any State-specific sample. Also, at least two PSU's (one metropolitan and one nonmetropolitan) were selected from each State that contains metropolitan and nonmetropolitan areas. Having at least this marginal representation in every State will provide an anchor for State supplements, whether such supplements are selected by RDD or by area-sampling methods. It also enhances the prospects for model-based State-specific estimates; intensive study of techniques for such estimates, however, left us rather dubious about long-term prospects in this area.

## Decreased Bias by Elimination of Proxy Reporting

The alpha option does provide strongly improved precision for the black and Hispanic populations compared with the 1985 design, assuming that in both cases the one-adult-per-household rule is used for interviewing. However, there is still a net decline in the precision of these statistics comparing the old design with all adults interviewed in each sample household to the new design with only one adult interviewed in each sample household. The decision of how many adults to interview in each sample household is independent of the other design features. The one-adult-per-household rule has already been used for a number of NHIS questionnaire supplements in the 1985 design. Conversely, there is nothing in the design proposed for 1995 that is inconsistent with interviewing all adults in every sample household. It is thus apparent that the one-adult-per-household rule should be structured in a way to at least obtain a limited set of core data for all household members.

## Flexibility for Rapid Sample Expansions and Contractions

The new design does not really provide improved flexibility for rapid expansions. The cost of carrying a reserve sample was judged by NCHS to be too great. However, general systems changes being made at the U. S. Bureau of the Census for all of its current surveys will make such expansions easier. Nonetheless, 2–4 years will still be needed for expansions. Rapid contractions are easier. The new design has a more flexible panel structure than was used previously. It allows cuts of whole PSU's by sixths, in addition to cuts by fourths. Of course, there is still the same capability for across-the-board cuts (where there is a small reduction in the sample size in each PSU), although it is well known that such cuts save very little money.

## Integrated Survey Design

The main conclusions in this area were with respect to integration at the PSU level. One product of the research, however, was a fairly detailed plan for how network sampling theory could be used to provide unbiased estimates for provider surveys when the provider sample is based on those providers identified in NHIS interviews. The differential sampling of black, Hispanic, and other people makes estimation with such a network-based approach more complicated, but it was demonstrated to be theoretically possible. More research on this topic is being done under a successor contract (2).

With regard to PSU's, one conclusion was that the PSU's should be defined much as they have always been, consisting of counties, groups of counties, metropolitan statistical areas (MSA's) and New England county metropolitan areas (NECMA's), with the primary consideration for grouping counties consisting of feasible interviewer travel. Plans were considered for defining PSU's in terms of health care commuting patterns, but these plans were rejected as increasing within-PSU travel costs to an unwarranted extent.

Another conclusion was that the measure of size for each PSU should be defined in terms of total population, not some measure of health care provided or consumed. Health care consumption was found to be substantially correlated with population. On the other hand, a type of measure of size based on racial and ethnic composition that would have brought more heavily black and Hispanic PSU's into the sample was rejected as being unnecessarily harmful to the health care provider surveys that do not, as a rule, oversample health care providers according to the racial or ethnic mix of their clientele.

Another conclusion was that the health care provider surveys could most likely be restricted to a special subset of the NHIS PSU's with little effect on their precision or cost, although such a subset would have to be defined differently than the subsets planned for reductions in the NHIS. This area is also a focus of the successor contract for more research.

## Follow-on Surveys

It was believed that the policy of oversampling the black and Hispanic populations for the NHIS would also be beneficial to such follow-on surveys as the NSFG. Even though some of these surveys have traditionally only oversampled the black population, it was thought that oversampling Hispanic persons is becoming more important for most demographic surveys. The larger number of PSU's and the smaller number of interviews with households that are neither black nor Hispanic may reduce efficiency for some of these surveys, but it was believed that such losses were justified, given the importance of improving the precision of statistics on the black and Hispanic populations for NHIS and of creating the needed flexibility for State supplemental samples.

# Part I.
# Introduction

# Chapter 1. Background on the National Health Interview Survey

The National Health Interview Survey (NHIS) is an ongoing survey of the population of the United States on health issues. The survey has been in continuous operation since 1957. NHIS data are obtained through personal interviews of the civilian, noninstitutionalized population conducted by the U.S. Bureau of the Census. The official goal for the sample size traditionally has been around 50,000 interviewed households per year, but the actual number of households is usually smaller, sometimes much smaller, depending on funding.

## Schedule and Content

The interviews are conducted during the year in weekly samples (each weekly sample consists of a probability sample of the population), so that seasonal trends can be followed. The core interview focuses on nonfinancial aspects of health that household respondents usually know, for example, visits to doctors, visits to hospitals, chronic ailments, and recent acute conditions are all covered in the core interview. Financial data, such as annual household income and whether the person is covered by health insurance, are also collected. The collection of data on diet, the cost of medical care, and details of diagnoses and treatments is left to other surveys conducted by the U.S. Department of Health and Human Services. Each year supplements to the core interview cover topics such as smoking behavior, knowledge about acquired immunodeficiency syndrome, and disabilities. Many of these supplements require a self-response rather than rely on a household respondent.

## Sample Design for 1985–94

The first stage of the NHIS sample consists of 198 multicounty PSU's. These sample PSU's were selected, using a stratified probability design, from a much larger set of PSU's that covers the United States. For stratification purposes, the PSU's were grouped before selection to ensure that the chosen PSU's would be broadly representative in terms of several demographic and economic characteristics. Some of these PSU's are so populous that they were included in the sample with certainty. These are called self-representing (SR) PSU's. The full NHIS design contains 52 SR PSU's. The remaining 146 PSU's had a chance of not being selected. These PSU's, which represent themselves and other PSU's that were not selected, are called nonself-representing (NSR) PSU's.

Within each sample PSU, a sample of blocks (or small groups of blocks) was selected as the second stage of the sample. In PSU's in which the black population consisted of 5 to 50 percent of the total in the 1980 census, blocks in enumeration districts (ED's) with high black populations were selected with a higher probability than other blocks. The selection was made in the early 1980's with a planned, slow rotation through blocks as they become exhausted. For the third stage, within each block, a cluster of eight housing units (HU's) is selected each year after fresh listing or updating of potential residential structures. These HU's are spread throughout the block as evenly as possible.

To gain better control over the size of the sample, HU's constructed since the 1980 census are selected through a sample of building permits rather than through area sampling. These units are selected in clusters of four instead of eight.

To provide continuous coverage of the population throughout the year, the sample of households is spread over the 52 weeks of the year; each week's sample is representative of the U.S. population. Each year a totally new sample of households is selected. Those households tend to be neighbors of the households interviewed the previous year, however.

As a result of the NHIS sample redesign of 1985, it became possible for NCHS to transmit data on NHIS sample households to private contractors for use in conducting follow-on surveys. Before 1985 a significant portion of the NHIS sample was drawn from lists created for the previous decennial census. In 1985 all of the NHIS sample was obtained by area and permit sampling, without any recourse to decennial census lists. These follow-on surveys are then said to be linked to NHIS. The confidentiality of the transmitted data is protected under section 308(d) of the Public Health Service Act. This feature of the design will have been well utilized between 1986 and 1994 and is a feature that NCHS wants to maintain. Thus, decisions about NHIS design must be made with the needs of follow-on surveys in mind.

# Chapter 2. New Objectives for 1995 Redesign

For the 1995 survey, it was desired to improve the precision of basic health statistics about various domains defined in terms of race, ethnic origin, poverty status, and geography. Accompanying this desire was the realization that it would be difficult and expensive to improve the precision of statistics for these domains while maintaining the precision of statistics about the prevalence of rare health conditions. Accordingly, the sorts of statistics that were identified as important for domain analysis include self-assessed health status, having visited a doctor in the prior year, having been hospitalized in the past year, knowledge of AIDS, and smoking behavior. Statistics about the prevalence of specific chronic conditions such as cancer and about the occurrence of acute conditions such as automobile accidents were given a lower priority.

## Statistics for Racial, Ethnic, and Economic Domains

The goal of this part of the research was to develop sampling methods for NHIS that would provide data of acceptable precision for the following:

- National estimates for race and/or ethnicity, age, and sex subdomains. More specifically, the subdomains consisted of the complete cross classification of the following:

- Four racial and/or ethnic groups (i.e., black persons, Hispanic persons, Asians and Pacific Islanders, and all others).
- Males and females.
- Six age groups (i.e., under 5 years, 5–17 years, 18–24 years, 25–44 years, 45–64 years, and 65 years and over).
- National estimates for the three major Spanish-origin subgroups of Hispanic persons in the United States (i.e., Mexican-American, Puerto Rican, and Cuban-American). Sex and age subdomains for these subgroups were also desirable, although with more limited age detail than for the broader race/ethnicity groups. These subdomains consisted of under 18 years, 18–44 years, 45–64 years, and 65 years and over. As an alternative to the production of data for the three subgroups, sample design implications were also examined for data on all Hispanic persons in the three geographic areas in which the subgroups are concentrated (i.e., five Southwestern States, the greater New York City area, and Florida).
- Statistics on the poor, where the poor can alternately be defined as having incomes below the poverty level, below 125 percent of the poverty level, below 150 percent of the poverty level, or below some other cutoff.

## Statistics for Geographic Domains

A second goal was to develop methods of producing State data by sex and three age groups: Under 18 years, 18–64 years, and 65 years and over. As an alternative to State data, the feasibility of producing data for State groupings was to be explored. As described subsequently in this report, a conflict exists between achieving this goal and the improvement of race and/or ethnicity statistics: Most members of the minority groups reside in the larger, urbanized States, whereas the smaller States would require sample supplementation to provide adequately precise State data. Another conflict exists between this goal and the requirements of follow-on surveys. Improved precision for State data requires the use of many more sample PSU's than would usually be desirable for a follow-on survey.

## Integration With Surveys of Health Care Providers

The third set of issues to be examined were those involved in integrating NHIS with surveys of health care providers. Some issues focused on analytic and operational benefits of using the same set of sample PSU's. Others focused on integration at the person-event level.

Integration at the PSU level seemed likely to have operational efficiencies because both sets of surveys are conducted by the U.S. Bureau of the Census. Integration might achieve certain synergies that could reduce travel and other expenses. It would also provide some analytic enhancements, because a relationship probably exists between the practices of health care providers within a PSU and the health-related characteristics of the residents of the PSU. Integration at the person-event level is even more interesting because it allows direct linking of individual practices and outcomes. Penalties for integration also exist, however. The optimal designs for different surveys are not necessarily identical. A major goal of this research was to determine the relative advantages and disadvantages of the different linkage possibilities.

## Reducing Response Errors Through Greater Use of Self-Response on the Core Interview

Prior research (3) had shown that proxy response has a significant impact on a number of statistics, including those for health status. That impact is particularly strong for men. Apparently, wives (the typical proxy respondents) tend to assess their husbands' health as better than the men themselves do. Although this bias is not very troubling for tracking health statistics over time (under the reasonable assumption that the bias due to proxy response is fairly stable over time), it is very worrisome for one-time questionnaire supplements that are designed to compare population domains. Unfortunately, obtaining self-response for all sample persons is considerably more expensive than the policy of requesting self-response but accepting proxy response when obtaining self-response presents difficulties. The most common difficulty is failing to find all household members home at the same time. To keep the cost per interview under control where self-response is necessary, plans for rejecting proxy response involve the interview of just one randomly chosen adult per household. If the designated adult cannot be interviewed by self-response, the household is classified as a noninterview. This procedure is currently used for most NHIS questionnaire supplements. It is very likely that the number of supplements requiring self-response will be increased in the future. This increased number will make it necessary to reduce the length of the core interview to restrain costs and to minimize response burden. With this idea in mind, almost all of the research on how to improve the precision for selected domains was repeated under two assumptions: That data are collected on all household members as is currently the case with the core instrument; and that data are collected on only one random adult per household (and possibly on all children in the household).

# Part II.
# Statistics for Racial, Ethnic, and Economic Domains

# Chapter 3. Current Precision

To determine the level of precision in the 1985–94 NHIS and the sample size necessary to achieve satisfactory levels after the redesign, it was necessary to examine the current NHIS variances. Furthermore, estimates of variance components would indicate how any necessary increases in sample sizes should be implemented. An early research step therefore was to calculate variances for the current NHIS design.

## Variances in Current Design

As a guide to the reliability that can be achieved with the multistage sample design and estimation procedure used in NHIS, Westat, Inc., computed estimates of the sampling variances in 1988 for a group of variables collected in NHIS. The methodology is explained in great detail in *National Health Interview Survey: Report on Variance Estimation* (4), which is also listed as Redesign Memorandum #40 in appendix 1. In brief, a variation due to Fay on balanced repeated replications was used to estimate the variances, both total and within PSU.

The variables covered a range of items, and variances were calculated for statistics relating to the total population and to the age categories. The 15 items selected from NHIS for direct variance estimation are listed in table 1. They represent several parameter sets of characteristics in NHIS. The first set of items is based on nonbinary person-level data; that is, the measurement variable at the person level can take on values other than 1 (yes) and 0 (no). It is also important to note that within the first set of items, the reference period for the respondent is the 2 weeks preceding the interview. Because the NHIS sample is spread out over the 52 weeks of the year, only 1/26th of the events experienced by a person within a year can be reported.

The second set of items is based on binary person-level data or recodes of multinomial responses into simple yes and no categories. The reference period for these questions is either irrelevant or it is a year.

The third set of items is composed of counts of chronic conditions reported by targeted respondents. Six different flash cards of chronic conditions are used in NHIS. The sample is divided into (nearly) equal sixths, and each sixth is shown a different prompting flash card. Thus, these statistics are based on only one-sixth of the full sample.

The fourth set of items consists of counts of acute conditions that have afflicted the respondents within the previous 2 weeks. Although a few respondents report multiple attacks of the same acute condition within the 2-week reference period, this question is basically binary, that is, either the person has experienced the condition in the last 2 weeks or has not experienced it. The varying reference periods and sampling fractions have important effects on reliability, as will be seen presently.

Variances of these items were estimated from the 1988 NHIS for several subdomains. The subdomains are the two-way classifications involving age and sex, race, census region, or census division. Age is classified into six categories: Under 5 years, 5–17 years, 18–24 years, 25–44 years, 45–64 years, and 65 years and over. Sex has two classifications: Male and female. Race is categorized twice: First, in three classifications—Hispanic, black, but not Hispanic, and others; and second, in two classifications—Asian or Pacific origin, and others. There are four census regions and nine census divisions.

Appendix 2 shows the estimated variances. For each item, there are three pages of estimates and coefficients of variation for breaks by the various domains. (The *cv*, or coefficient of variation, is the ratio of standard error to mean, expressed as a percentage.) For those subdomains where the estimated mean is 0, the *cv* is given as 0, even though the *cv* is undefined. The *cv*'s vary widely. For example, the *cv* on the percent of the total population reporting at least one doctor visit in the past year is just 0.27 percent, but the *cv* on the number of moving motor vehicle injuries to young adults between 18 and 24 years of age in the West North Central Census Division is 117.04 percent.

Because it was evident that there was considerable instability in the variance estimates, the decision was made to generalize some of them.

**Table 1. Items selected for direct variance estimation**

| |
|---|
| Statistics based on nonbinary person-level data about the prior 2 weeks |
| Average number of doctor's visits |
| Average number of bed days |
| Average number of chronic conditions |
| Statistics based on binary person-level data from the full sample |
| Percent reporting poor health status |
| Percent reporting at least two hospitalizations in the past year |
| Percent reporting at least one doctor's visit in the past year |
| Percent unable to carry on major activity |
| Percent with more than 5 years since last doctor's visit |
| Statistics based on binary person-level data from one-sixth of the sample |
| Number with chronic arthritis |
| Number with color blindness |
| Number with chronic epilepsy |
| Statistics based on (almost) binary person-level data about the prior 2 weeks |
| Number of acute digestive system conditions |
| Number of acute bronchitis conditions |
| Number of acute urinary conditions |
| Number injured in moving motor vehicle accidents |

Variance generalization is a technique for improving the stability of variance estimates that is based on the fact that the variance of the mean of a set of independent and identically distributed (iid) Bernoulli random variables is functionally related to the common mean of the variables.

In complex surveys many statistics of interest are means of Bernoulli random variables that are not quite independent and not quite identically distributed. Experience has shown that the variance of these means can often be well predicted by functions of the overall mean of the variables (5). In fact, if there are few degrees of freedom for direct variance estimates, the mean square error of the generalized variance estimates can be lower than that of the direct variance estimates (6). Various attempts have been made over the years to develop similar models for statistics that are not sums of Bernoulli variables, but these have generally been unsuccessful (and thus, not published). Accordingly, for this report, of the items shown in table 1, generalized variance estimates were only prepared for the second, third, and fourth sets of items. See *National Health Interview Survey: Report on Variance Estimation* (4) for details of how the generalized variance estimates were prepared for this study.

Table 2 contains a variety of estimated *cv*'s for the selected variables in total and by age. The table shows the *cv*'s as the following:

- Calculated from generalized variance functions published by NCHS.
- Calculated from generalized variance functions fitted by Westat, Inc., when the domain to which a statistic is restricted is subject to poststratification (e.g., the percentage of females who report poor health).
- Calculated from generalized variance functions fitted by Westat, Inc., when the domain to which a statistic is restricted is not subject to poststratification (e.g., percentage of those with low income who report poor health).
- Estimated directly with Fay's variant of balanced repeated replication (BRR).

Poststratification is an estimation technique used to bring survey estimates into conformity on certain dimensions with known or more accurately estimated population totals. NHIS estimation routinely involves poststratification on age, sex, and race.

The generalized variances are somewhat lower than the ones reported in the published NHIS reports. After

**Table 2. Coefficients of variation for selected NHIS items: 1988**

| | Estimates | Coefficient of variation (%) | | | | | *cv*'s (%) for limited age ranges direct by BRR | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | From new curves | | | | | | | | |
| | Published | Westat, Inc. tabulated | From published curves | Controlled margin[1] | Interior cell[1] | Direct by BRR | Age 0.5 | Age 6–17 | Age 18–24 | Age 25–44 | Age 45–64 | Age 65 years and over |
| Quantitative items based on 2-week data | | | | | | | | | | | | |
| Doctor visits per person | 5.400 | 3.863 | . . . | . . . | . . . | 0.9 | 1.7 | 1.8 | 2.4 | 1.8 | 2.3 | 2.3 |
| Bed days per person | 6.300 | 5.344 | . . . | . . . | . . . | 2.0 | 3.6 | 2.7 | 3.7 | 2.5 | 3.6 | 4.1 |
| Conditions per person | . . . | 4.298 | . . . | . . . | . . . | 0.7 | 2.1 | 1.6 | 1.8 | 1.1 | 1.2 | 1.3 |
| Rates based on full sample | | | | | | | | | | | | |
| Percent with poor health | 0.027 | 0.027 | 2.4 | 1.7 | 2.0 | 2.0 | 16.1 | 13.9 | 15.6 | 4.6 | 3.4 | 2.4 |
| Percent with 2 or more hospitalizations | 0.016 | 0.016 | 3.2 | 2.3 | 2.6 | 2.7 | 9.6 | 11.5 | 9.4 | 4.7 | 4.5 | 4.1 |
| Percent with no doctor visits | 0.233 | . . . | 1.0 | . . . | . . . | . . . | | | | | | |
| Percent with 1 or more doctor visits | . . . | 0.747 | . . . | 0.2 | 0.2 | 0.3 | 0.4 | 0.6 | 0.7 | 0.4 | 0.5 | 0.4 |
| Percent unable major activity | 0.040 | 0.040 | 2.0 | 1.4 | 1.6 | 1.5 | 13.0 | 11.6 | 6.3 | 3.3 | 2.3 | 2.6 |
| Percent 5+ years last doctor visit | 0.036 | 0.034 | 2.1 | 1.5 | 1.8 | 2.0 | undefined | 6.6 | 6.2 | 2.7 | 3.1 | 4.1 |
| Generic full sample items | | | | | | | | | | | | |
| $p = .01$ | | | 3.5 | 2.9 | 3.3 | | | | | | | |
| $p = .1$ | | | 1.2 | 0.9 | 1.0 | | | | | | | |
| $p = .2$ | | | 0.8 | 0.6 | 0.7 | | | | | | | |
| $p = .5$ | | | 0.4 | 0.3 | 0.3 | | | | | | | |
| Rates based on 1/6 sample | | | | | | | | | | | | |
| Percent with chronic arthritis | 0.130 | 0.126 | 2.2 | 1.9 | 2.0 | 2.2 | 100.4 | 32.3 | 18.7 | 5.1 | 3.2 | 3.3 |
| Percent with chronic color blindness | 0.012 | 0.011 | 6.7 | 6.6 | 7.0 | 6.9 | undefined | 17.0 | 27.2 | 10.4 | 12.6 | 17.1 |
| Percent with chronic epilepsy | 0.004 | 0.004 | 11.6 | 11.5 | 12.1 | 12.0 | 40.5 | 33.1 | 44.0 | 21.0 | 21.1 | 28.9 |
| Rates based on 2-week data | | | | | | | | | | | | |
| Percent with acute digestive condition | 0.063 | 0.063 | 6.9 | 6.0 | 6.0 | 6.9 | 17.4 | 12.7 | 16.9 | 12.1 | 16.2 | 18.0 |
| Percent with acute bronchitis | 0.034 | 0.034 | 9.3 | 8.4 | 8.4 | 7.8 | 16.5 | 17.6 | 35.8 | 15.7 | 26.0 | 27.1 |
| Percent with acute urinary condition | 0.028 | 0.028 | 10.1 | 9.2 | 9.1 | 10.0 | 44.9 | 38.7 | 30.6 | 14.7 | 19.8 | 18.9 |
| Percent injured in motor vehicle accident | 0.017 | 0.017 | 12.5 | 11.9 | 11.7 | 13.7 | 99.9 | 28.2 | 28.0 | 20.6 | 34.6 | 46.1 |

. . . Category not applicable.

[1]Some domains have population estimates with zero variance due to poststratification. Such a domain is referred to as a controlled margin. Other domains are referred to as interior cells.

discussions were held with NCHS staff, the differences were ascribed to the mix of items that formed the basis for the generalization process. It was agreed that the new set of items was more representative of the sets of items in the published reports than the ones used in the NCHS computations of generalized variance functions. It was thus decided to use the new generalized variance functions in the decision-making process for determining required sample sizes.

Normally in a sample survey, the *cv* depends mainly on the statistic (i.e., the value of *p*) and the size of the subdomain for which the statistic is estimated. However, in NHIS the *cv*'s also differ sharply among the three classes of statistics into which the rates have been classified in table 2; rates based on the full sample, those based on the one-sixth sample used for chronic conditions, and rates based on 2-week data. (The 2-week data can be considered as approximately based on a 1/26-sample of NHIS.) For the same value of *p* and the same subdomain, the *cv*'s for the three classes of items reflect the different effective sample sizes resulting from the subsampling for condition items and 2-week data.

## Desired levels of precision for black; Hispanic; Asian and Pacific Islander; American Indian, Eskimo, and Aleut populations; and persons with low income

For this research on the 1995 redesign, the required level of precision was a 30-percent *cv*. Taking into account the observed *cv*'s for the current design from table 2 and known NHIS sample sizes for 1988 (not shown), table 3 was developed; it shows the required sample sizes for many different statistics to have a 30-percent *cv*. This assumes the same extent of clustering as the 1988 NHIS but without the differential sampling by racial composition of neighborhood. The formula used was

$$n_R = n_D \left( \frac{cv_D}{30} \right)^2 \times \frac{1}{1.03}$$

where $n_D$ is the sample size for the domain in 1988, $cv_D$ is an estimated *cv* for the statistic on the domain in 1988, $n_R$ is the required sample size, and division by 1.03 corrects for the design effect because of the slight oversampling that was done of heavily black neighborhoods in the 1985–94 design.

As is evident from inspection of the table, this guideline of a flat 30-percent *cv* did not really provide much guidance on the magnitude of an appropriate sample size for each domain. For a *cv* guideline to determine a sample size, a

**Table 3. Domain-specific required sample sizes for a 30-percent *cv***

| | General population: Size calculated using *cv* from: | | | | Limited age domains (Total sample size required over all domains to be found by summing across domains) | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Published curves | Controlled margin | Interior cell[1] | Direct BRR | Age 0–5 | Age 6–17 | Age 18–24 | Age 25–44 | Age 45–64 | Age 65 and over |
| **Quantitative items based on 2-week data** | | | | | | | | | | |
| Doctor visits per person | . . . | . . . | . . . | 116 | 30 | 86 | 76 | 126 | 138 | 83 |
| Bed days per person | . . . | . . . | . . . | 541 | 137 | 192 | 179 | 263 | 333 | 272 |
| Conditions per person | . . . | . . . | . . . | 59 | 48 | 66 | 44 | 48 | 36 | 26 |
| **Rates based on full sample** | | | | | | | | | | |
| Percent poor health | 760 | 397 | 520 | 522 | 2,742 | 5,063 | 3,162 | 861 | 294 | 94 |
| Percent with 2 or more hospitalizations | 1,351 | 701 | 917 | 967 | 964 | 3,458 | 1,141 | 892 | 514 | 276 |
| Percent with no doctor visits | 132 | . . . | . . . | . . . | | | | | | |
| Percent with 1 or more doctor visits | . . . | 4 | 5 | 9 | 1 | 8 | 6 | 7 | 6 | 3 |
| Percent unable major activity | 528 | 266 | 348 | 307 | 1,783 | 3,515 | 520 | 447 | 137 | 111 |
| Percent 5+ years last doctor visit | 582 | 314 | 411 | 552 | ([1]) | 1,152 | 501 | 293 | 241 | 280 |
| **Generic full sample items** | | | | | | | | | | |
| *p* = .01 | 1,974 | 1,104 | 1,444 | | | | | | | |
| *p* = .1 | 179 | 100 | 131 | | | | | | | |
| *p* = .2 | 80 | 45 | 58 | | | | | | | |
| *p* = .5 | 20 | 11 | 15 | | | | | | | |
| **Rates based on one-sixth sample** | | | | | | | | | | |
| Percent with chronic arthritis | 639 | 464 | 515 | 610 | 106,248 | 27,127 | 4,505 | 1,069 | 261 | 177 |
| Percent with chronic color blindness | 5,923 | 5,792 | 6,437 | 6,227 | ([1]) | 7,529 | 9,540 | 4,446 | 3,965 | 4,743 |
| Percent with chronic epilepsy | 17,754 | 17,417 | 19,357 | 18,898 | 17,316 | 28,541 | 24,986 | 18,109 | 11,170 | 13,618 |
| **Rates based on 2-week data** | | | | | | | | | | |
| Percent with acute digestive condition | 6,282 | 4,825 | 4,712 | 6,236 | 3,190 | 4,207 | 3,709 | 5,985 | 6,621 | 5,281 |
| Percent with acute bronchitis | 11,412 | 9,420 | 9,201 | 7,955 | 2,855 | 8,116 | 16,554 | 10,080 | 16,953 | 12,000 |
| Percent with acute urinary condition | 13,459 | 11,178 | 10,918 | 13,202 | 21,250 | 39,022 | 12,069 | 8,896 | 9,835 | 5,804 |
| Percent injured in motor vehicle accident | 20,616 | 18,608 | 18,174 | 24,627 | 105,130 | 20,771 | 10,154 | 17,427 | 30,041 | 34,630 |

. . . Category not applicable.

[1]Because of an observed $p = 0$, the estimated required sample sizes to achieve the desired precision are infinite.

single statistic for which the design must provide the required *cv* must be specified. Specifying a group of statistics allows the rarest characteristic to drive the sample size. Of course, across the age domains, the rarest health-related characteristic changes. It did not seem to make sense to determine a different required sample size for each age domain. The round figures of 1,000 as a required effective sample size were the result of a reasonable compromise. A sample of 1,000 persons selected with clustering but equal probabilities provides the desired *cv* of 30 percent for an estimated proportion of 1.438 percent.

This standard of 1,000 was used for most of the research. It is referred to loosely as an effective sample size of 1,000, although this is a slight abuse of standard terminology. A sample clustered in the same manner as the current NHIS but selected with equal probabilities will not provide as good precision as a simple random sample of 1,000 cases.

Current sample sizes are quite different for estimates based on data from the core interview than for those based on items from most questionnaire supplements. Most supplements are administered to only one adult per household to reduce the burden on the household. Crossing the desired age breaks by sex and by race and ethnicity leads to the finding that sample sizes are inadequate (i.e., fewer than 1,000) on supplements for almost every minority age-by-sex domain.

For the core interview, most black domains have adequate sample sizes. The exceptions are the domains of black males and females under age 5 and black males ages 65 and over. A few Hispanic domains have adequate sample sizes for the core, but most are inadequate. For the other groups, no domains have adequate sample sizes, even for the core interview.

## Components of Variance

On average, the study found that about 6 percent of the total variance in 1988 resulted from differences among PSU's, with the rest resulting from differences between and within segments within PSU's. Considerable variation was found in the estimated proportion due to between-PSU variance, however. Some of this variation was attributable to sampling error on the variance estimates themselves, and some was attributable to real differences. In particular, because estimates pertaining to large domains tend to have higher average cluster size and thus greater between-PSU variance, the size of the estimate has some relationship to the proportion of the total variance contributed by the between-PSU component.

An average between-PSU variance of 6 percent is perhaps a little smaller than common wisdom among sampling statisticians would have predicted for a survey with only 198 sample PSU's. This indicates that the stratification performed by the U.S. Bureau of the Census in the early 1980's was quite effective.

Between-PSU variances are even smaller for minority groups. The study found that between-PSU variance accounts for between 2 and 3 percent of total variance for black persons and essentially 0 percent for Hispanic persons. Components of variance were not calculated for minorities other than the black and Hispanic populations, but it seems reasonable to believe that between-PSU variance is also not important for Asians and Pacific Islanders. On the other hand, there is good reason to suspect that between-PSU variance is larger for American Indians, Eskimos, and Aleuts because of their large nonmetropolitan populations. Between-PSU variance for nonmetropolitan estimates was a notably large 15 percent of total variance, more than twice as large as for statistics for the United States as a whole.

# Chapter 4. Potential Techniques for Increasing Sample Sizes for Selected Domains

This chapter reviews the basic techniques that can be used for oversampling racial, ethnic, and economic domains. Some of these techniques are not discussed further in subsequent chapters because it is clear they do not meet NCHS requirements for NHIS.

## Sample Sizes With Simple Expansion

The least complicated approach to meeting the new precision requirements for NHIS would be to simply increase the sample size. This option would require no changes in methodology. To evaluate this option, it is useful to first project sample sizes that would be obtained for each domain given an equiprobability design of 50,000 interviewed households, the goal that the 1985 design was built around. There are several steps to making this projection. The first step is to project the population into the future. Given that the 1995 design will be used from 1995 through 2004, it seemed reasonable to evaluate sizes as of the year 2000. Table 4 shows such projections.

**Table 4. Projections of the population by age, sex, race, and ethnicity to 2000**

| Race, Hispanic origin, and age | Male | Female | Total |
|---|---|---|---|
| Black | | | |
| Under 5 | 1,358 | 1,301 | 2,659 |
| 5–17 | 3,910 | 3,732 | 7,642 |
| 18–24 | 1,888 | 1,910 | 3,798 |
| 25–44 | 4,920 | 5,479 | 10,399 |
| 45–64 | 2,933 | 3,538 | 6,470 |
| 65 years and over | 1,238 | 1,793 | 3,031 |
| Total | 16,247 | 17,753 | 33,999 |
| Hispanic | | | |
| Under 5 | 1,685 | 1,612 | 3,297 |
| 5–17 | 4,043 | 3,861 | 7,903 |
| 18–24 | 1,943 | 1,805 | 3,748 |
| 25–44 | 5,423 | 4,841 | 10,263 |
| 45–64 | 2,405 | 2,524 | 4,929 |
| 65 years and over | 774 | 1,086 | 1,860 |
| Total | 16,271 | 15,729 | 32,000 |
| Other | | | |
| Under 5 | 5,707 | 5,409 | 11,115 |
| 5–17 | 17,238 | 16,350 | 33,588 |
| 18–24 | 9,012 | 8,806 | 17,818 |
| 25–44 | 30,445 | 30,236 | 60,681 |
| 45–64 | 24,493 | 25,429 | 49,922 |
| 65 years and over | 12,214 | 17,656 | 29,870 |
| Total | 99,109 | 103,886 | 202,995 |
| Total | | | |
| Under 5 | 8,749 | 8,322 | 17,071 |
| 5–17 | 25,191 | 23,942 | 49,133 |
| 18–24 | 12,844 | 12,520 | 25,364 |
| 25–44 | 40,787 | 40,556 | 81,344 |
| 45–64 | 29,830 | 31,491 | 61,321 |
| 65 years and over | 14,225 | 20,536 | 34,761 |
| Total | 131,627 | 137,367 | 268,994 |

NOTE: Figures may not add to totals because of rounding.

The projections for the total and black populations were based on table 4 of *Projections of the Population of the U.S. by Age, Sex, and Race: 1988 to 2080* (7). The projections for the Hispanic population were based on Series 17 from tables 2 and 3 of *Projections of the Hispanic Population: 1983 to 2080* (8). The projections of ‘‘other’’ populations were computed by subtraction, ignoring the existence of black Hispanic persons. Because all of the U.S. Bureau of the Census projections were made before the 1990 decennial census, those projections were multiplied by correction factors to reflect the error in the projections for 1990. Thus, the numbers in table 4 (above) reflect an adjustment of −3.2 percent for black populations, +11.5 percent for Hispanic populations, and −0.7 percent for ‘‘other’’ populations. Clearly, the potential for error exists in all these projections. The Hispanic projections are particularly sensitive to trends in immigration. Most critically, the projections are sensitive to changes in ethnic identification. Studies by the U.S. Bureau of the Census have shown repeatedly that question wording can have a major impact on estimates of the Hispanic population.

The second step in projecting sample sizes is to account for undercoverage. Historically, population estimates from NHIS have been lower than those from the decennial census, particularly for the black and Hispanic populations. (This is true for practically all surveys.) Table 5 shows projections of undercoverage for typical demographic surveys. The numbers are from *Redesign Memorandum #9* (9) and are averaged over years. From year to year, coverage rates can vary substantially, depending on the size of the survey and survey procedures.

The third step in projecting sample sizes is to project household sizes, that is, the number of persons per household. Given historical trends it was decided to predict household sizes of 2.71, 3.40, and 2.36 for black, Hispanic, and ‘‘other’’ households, respectively. These numbers are sensitive to assumptions about the causes of undercoverage as well as changes in trends for young adults to set up separate households from their parents.

Given these steps the sample sizes by race, ethnicity, age, and sex can be obtained by reducing the eligible population by predicted undercoverage,

**Table 5. Projections of undercoverage by age, sex, race, and ethnicity**

| Race, Hispanic origin, and age | Percent missed | |
|---|---|---|
| | Male | Female |
| Black | | |
| Under 5 | 0 | 0 |
| 5–17 | 1 | 1 |
| 18–24 | 24 | 12 |
| 25–44 | 21 | 10 |
| 45–64 | 15 | 9 |
| 65 years and over | 7 | 5 |
| Hispanic | | |
| Under 5 | 5 | 5 |
| 5–17 | 5 | 5 |
| 18–24 | 25 | 22 |
| 25–44 | 25 | 18 |
| 45–64 | 24 | 18 |
| 65 years and over | 15 | 15 |
| Other | | |
| Under 5 | 0 | 0 |
| 5–17 | 0 | 0 |
| 18–24 | 7 | 5 |
| 25–44 | 6 | 4 |
| 45–64 | 4 | 4 |
| 65 years and over | 5 | 5 |

summing across age and sex, and then dividing by household size (specific to race and ethnicity), thereby yielding estimates of the counts of households that can be subject to discovery by a typical demographic survey in the year 2000. Yields in an equiprobability survey of 50,000 interviewed households are then obtained by proportionality. Table 6 shows the results of these calculations.

Different projections could have been obtained by assuming interactions between household relationships and coverage. For example, there is some evidence to support the proposition that undercoverage of occupied housing is less severe than the undercoverage of persons within listed households because of less severe undercoverage of household heads than of other household members, even after controlling for age and sex. However, definitive studies on the relative importance of within-household undercoverage versus undercoverage of whole households (as a result of mistakes in the listing/screening process such as thinking homes unfit for habitation or vacant) are not available. It was decided that it would be adequate for planning purposes to simply divide estimates of covered persons by projected household sizes to get estimates of covered persons.

Not shown in the table are projections for Asian and Pacific Islanders nor for American Indians, Eskimos, and Aleuts. Projections for these racial groups are just 3,600 and 1,000, respectively, in a survey of 50,000 interviewed households. Those numbers are for population of any age and sex.

Given NCHS interest in subsampling just one adult per household, as discussed in chapter 2, projections were also made for the expected yield for such a procedure given an equiprobability sample of 50,000. Such a procedure reduces the sample size directly and also indirectly. The sample size is directly reduced because fewer people are interviewed. It is indirectly reduced in the sense that the sample is no longer an equiprobability sample. For example, adults in three-adult households end up with weights three times as large as adults in single-adult households. The direct reductions can be obtained by simply dividing by the projected numbers of adults per household. For this research it was assumed that there would be 1.81 adults per black household, 2.06 per Hispanic household, and 1.82 per ‘‘other’’ household. A study of data from the *Current Population Survey* yielded projections that the departure from equiprobability would induce additional design effects of 1.28, 1.25, and 1.19 for black, Hispanic, and ‘‘other’’ adults, respectively. Table 7 shows projections of effective sample sizes that have been adjusted for these design effects but not the design effects resulting from clustering. Because sampling only one adult per household would reduce the component of the design effect due to within-household intraclass correlation, these adjustments might be a little too severe, but they do give a good idea of the precision that could be expected with such a design.

As can be readily inferred from tables 6 and 7, simple expansion of the sample is an extremely expensive method for satisfying the precision requirements discussed in chapter 3. Because the projected sample sizes are always larger for black domains than for Hispanic domains, the expansion ratio required for Hispanic goals would also meet black goals. Thus, it is enough to look at the expansion ratios for Hispanic persons. To meet the precision requirement for elderly Hispanic males by this technique alone would require between a threefold and fourfold increase in the NHIS sample size, even if NCHS were willing to take all adults in minority households. (The required precision is achieved with 1,000 interviews, but the equiprobability design yields just 323.) In other words, instead of interviewing 50,000 households per year, it would be necessary to interview 150,000 to 200,000 households per year. A vital category such as Hispanic males 18–24 years of age would require a full fourfold increase if only one adult is interviewed per household. The numbers are even larger for elderly Asian Americans and Native Americans. Clearly, this is not a feasible technique.

**Table 6. Sample sizes for equiprobability sample of 50,000 interviewed households**

| Race, Hispanic origin, and age | Male | Female | Total |
|---|---|---|---|
| Black | | | |
| Under 5 years | 667 | 639 | 1,306 |
| 5–17 | 1,900 | 1,813 | 3,713 |
| 18–24 | 705 | 825 | 1,529 |
| 25–44 | 1,908 | 2,420 | 4,372 |
| 45–64 | 1,223 | 1,580 | 2,803 |
| 65 years and over | 565 | 836 | 1,401 |
| Total | 6,968 | 8,112 | 15,079 |
| Total 18 years and over | 4,401 | 5,660 | 10,061 |
| Households | . . . | . . . | 5,564 |
| Hispanic | | | |
| Under 5 years | 785 | 751 | 1,536 |
| 5–17 | 1,885 | 1,801 | 3,686 |
| 18–24 | 715 | 691 | 1,406 |
| 25–44 | 1,996 | 1,948 | 3,943 |
| 45–64 | 896 | 1,016 | 1,912 |
| 65 years and over | 323 | 453 | 776 |
| Total | 6,600 | 6,659 | 13,259 |
| Total 18 years and over | 3,930 | 4,107 | 8,037 |
| Households | . . . | . . . | 3,900 |
| Other | | | |
| Under 5 years | 2,800 | 2,654 | 5,454 |
| 5–17 | 8,460 | 8,023 | 16,483 |
| 18–24 | 4,069 | 4,106 | 8,175 |
| 25–44 | 14,045 | 14,245 | 28,290 |
| 45–64 | 11,539 | 11,980 | 23,519 |
| 65 years and over | 5,695 | 8,232 | 13,927 |
| Total | 46,608 | 49,240 | 95,848 |
| Total 18 years and over | 35,348 | 38,563 | 73,911 |
| Households | . . . | . . . | 40,614 |
| Total | | | |
| Under 5 years | 4,252 | 4,044 | 8,296 |
| 5–17 | 12,245 | 11,636 | 23,881 |
| 18–24 | 5,488 | 4,622 | 11,110 |
| 25–44 | 17,948 | 18,612 | 36,561 |
| 45–64 | 13,659 | 14,575 | 28,234 |
| 65 years and over | 6,584 | 9,521 | 16,104 |
| Total | 60,176 | 64,011 | 124,186 |
| Total 18 years and over | 43,679 | 48,330 | 92,009 |
| Households | . . . | . . . | 50,078 |

. . . Category not applicable.

NOTE: Figures may not add to totals because of rounding.

## Oversampling Classes of Primary Sampling Units

It is well known that some nonself-representing PSU's have disproportionate numbers of Hispanic persons and others have disproportionate numbers of black persons. This led to the idea that it might be advantageous to deliberately assign a larger measure of size to such PSU's so that more of them would appear in the sample. This would reduce between-PSU variance for targeted minority statistics and make it easier to oversample targeted minorities at the block level while maintaining reasonable PSU workloads. (The PSU workload is the total number of households interviewed in a PSU over the course of a year.) With this technique, however, the between-PSU variance is increased for the majority population and for untargeted other minorities as well as for statistics for the total population. Also, the set of sample PSU's becomes less suitable for the health care provider surveys and for follow-on surveys, such as the National Survey of Family Growth, that reinterview former NHIS sample persons or housing units.

Because large majorities of the black and Hispanic populations live in large metropolitan areas that are not subject to between-PSU variance (a special tabulation of the 1988 NHIS found 62 percent of black persons and 69 percent of Hispanic persons living in SR PSU's), and between-PSU variance is already small for these domains, and because equal workloads are not an important consideration for NHIS with its traveling interviewers, it was decided not to oversample PSU's with large minority populations. Equal monthly workloads are important, but they can be controlled by the number of times the PSU is visited in the course of the year. More details can be found in *PSU Probabilities Given Differential Sampling at Second Stage* (10).

## Oversampling Classes of Blocks or Block Groups

Using this technique, small areas that are known from the prior decennial census to be rich in targeted minorities are deliberately oversampled. Unless the targeted minority is completely isolated residentially, this technique naturally gives rise to an oversample of those nonminority persons who happen to reside in blocks with heavy concentrations of minorities. This oversampling is usually undesirable as there are no fixed precision requirements for such nonminority persons. Furthermore, the resulting variation in probabilities of selection for the untargeted demographic domains leads to larger-than-necessary design effects for the total estimates and for the majority population. For these reasons, oversampling of blocks and block groups is most effective when combined with screening (see the next section). Nonetheless, this technique can and has been used by itself and so was considered as a possible approach in this research.

The demographic domain is often defined in terms of age, sex, race, or ethnic origin. The underlying theory for this approach is the same regardless of the definition. Stratify the blocks in each PSU into $M$ strata according to the percentage of the population within the

**Table 7. Sample sizes for equiprobability sample of 50,000 interviewed households with one adult per household (corrected for within-household design effect)**

| Race, Hispanic origin, and age | One adult per household Male | Female | Total |
|---|---|---|---|
| Black | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 304 | 356 | 660 |
| 25–44 | 823 | 1,044 | 1,867 |
| 45–64 | 528 | 682 | 1,210 |
| 65 years and over | 244 | 361 | 605 |
| Total | . . . | . . . | . . . |
| Total 18 years and over | 1,900 | 2,443 | 4,343 |
| Households | . . . | . . . | 5.559 |
| Hispanic | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 278 | 268 | 546 |
| 25–44 | 775 | 756 | 1,531 |
| 45–64 | 348 | 394 | 742 |
| 65 years and over | 126 | 176 | 302 |
| Total | . . . | . . . | . . . |
| Total 18 years and over | 1,526 | 1,595 | 3,121 |
| Households | . . . | . . . | 3,901 |
| Other | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 1,879 | 1,896 | 3,775 |
| 25–44 | 6,485 | 6,577 | 13,062 |
| 45–64 | 5,328 | 5,531 | 10,859 |
| 65 years and over | 2,630 | 3,801 | 6,431 |
| Total | . . . | . . . | . . . |
| Total 18 years and over | 16,321 | 17,805 | 34,127 |
| Households | . . . | . . . | 40,610 |
| Total | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 2,460 | 2,520 | 4,980 |
| 25–44 | 8,083 | 8,378 | 16,461 |
| 45–64 | 6,204 | 6,608 | 12,812 |
| 65 years and over | 2,999 | 4,338 | 7,337 |
| Total | . . . | . . . | . . . |
| Total 18 years and over | 19,746 | 21,844 | 41,590 |
| Households | . . . | . . . | 50,070 |

. . . Category not applicable.

NOTE: Figures may not add to totals because of rounding.

block belonging to the targeted domain. There does not appear to be any theory on the optimal number of such strata or on how to choose the cut-points that define them. As a general rule, it would seem that more strata are better than fewer. The limiting constraints are the cost of the computer runs to classify blocks into the strata and the burden of verifying that sampling is done for each stratum correctly.

Let $P_1$–$P_M$ indicate the proportion of the total population living in each of these strata. Let $D_1$–$D_M$ indicate the corresponding proportions of the targeted domain population. Let $SI_1$–$SI_M$ indicate corresponding sampling intervals for the $M$ strata. The problem is to determine the optimal set of sampling intervals for producing statistics about the targeted domain population.

Assuming equal unit variances in all strata, the design effects for total statistics and for statistics about the targeted domain will be, respectively,

$$DE = \left[\sum_{i=1}^{M} P_i SI_i\right] \bullet \left[\sum_{i=1}^{M} \frac{P_i}{SI_i}\right] \text{ and} \qquad (1)$$

$$DE = \left[\sum_{i=1}^{M} D_i SI_i\right] \bullet \left[\sum_{i=1}^{M} \frac{D_i}{SI_i}\right] \qquad (2)$$

The average sampling fractions for the overall sample and for the targeted domain are, respectively,

$$\sum \frac{P_i}{SI_i} \quad \text{and} \quad \sum \frac{D_i}{SI_i}$$

Thus, the ratio of the sampling fraction for the targeted domain to the sampling fraction for the general population (henceforth referred to as the ‘‘nominal oversampling rate’’) is

$$\frac{\sum \frac{D_i}{SI_i}}{\sum \frac{P_i}{SI_i}} \qquad (3)$$

Division of this nominal oversampling rate by the design effect for the targeted domain gives the effective oversampling ratio:

$$\frac{1}{[\sum (P_i / SI_i)]\ [\sum D_i SI_i]} \qquad (4)$$

The problem is to maximize equation 4 subject to the constraint of a constant sample size. Let $SI$ indicate the sampling interval that would yield the allowable fixed sample size with a simple random sample. Then the optimal sampling interval for the $i$-th stratum is

$$SI_i = SI \sqrt{\frac{P_i}{D_i}} \left[\sum_{j=1}^{M} \sqrt{D_j P_j}\right] \qquad (5)$$

The optimal sampling intervals from equation 5 can be substituted back into the earlier equations to obtain the nominal and effective oversampling ratios and the design effects. These basic formulas are used to discuss the suitability of this approach compared

**Table 8. Distributions in 1990 of minority populations across density strata**

| Stratum (designated minority as percent of block) | Percent of designated minority in each stratum | | | |
|---|---|---|---|---|
| | Black | Hispanic | Asian and Pacific Islander | American Indian, Eskimo, and Aleut |
| < 10 | 8.5 | 14.8 | 37.0 | 46.8 |
| < 5 | . . . | 6.6 | 19.4 | 34.6 |
| 5–10 | . . . | 8.1 | 17.7 | 12.1 |
| 10–30 | 13.9 | 22.1 | 32.1 | 15.9 |
| 30–60 | 16.2 | 23.3 | 18.0 | 7.7 |
| 60 and over | 61.4 | 39.8 | 13.0 | 29.6 |

. . . Category not applicable.

SOURCE: 1990 STF1B file

with others for each of the targeted domains in chapters 5 through 9. Some foreshadowing of those results can be obtained from table 8, however. Oversampling integrated and predominantly minority blocks can be seen to have much more potential for black persons than for the other groups because the black population is much more highly concentrated. It is immediately apparent that Asian and Pacific Islanders are not concentrated enough for the technique to be effective. This is particularly obvious when one considers the large boost in the effective sample size that is required for the domain. Among American Indians, Eskimos, and Aleuts, there are two distinct patterns. Those who live on reservations are extremely segregated and thus easy to oversample; those who live off reservations tend to be only slightly segregated.

Table 9 shows the percentage of each density stratum that is not black, Hispanic, Asian, or Native American. The table basically shows the pervasiveness of non-Hispanic white persons (the only racial and/or ethnic domain with excellent precision) in all strata. The table shows that blocks with strong concentrations of black persons or Native Americans are the least likely blocks to contain non-Hispanic white persons. Even in these blocks, however, the numbers of non-Hispanic white persons imply that a sample concentrated in the densest minority blocks will pick up substantial numbers of non-Hispanic white persons. Because there is no desire to improve the precision of statistics about this group of non-Hispanic white persons, oversampling classes of blocks is a wasteful technique by itself. It is far more efficient when combined with screening, as is discussed in the next section.

## Screening With Subsampling

With screening, a larger sample than actually desired is selected. A very brief, inexpensive questionnaire is then administered to the sample to determine the members of the domain to be oversampled. Those units (persons or housing units) that are not part of that domain are subsampled. It is possible to set up multiple cells with different subsampling rates. The subsampling can be done centrally after the entire screening operation is completed, or it can be done by the interviewer on the fly. Techniques have been developed that make the subsampling process very easy for the interviewer (11). Interviewers do not need random numbers. Instead, they are given house-by-house preinterview instructions about which domains can be interviewed at which households. These instructions are randomized centrally before screening to yield the desired sampling rates. Alternatively, with computer-assisted personal interviewing (CAPI), the sampling can be programmed and performed automatically in the laptop computer; the computer notifies the interviewer which households to retain for the full interview and which ones to reject as a result of subsampling.

Experience at Westat, Inc., has shown that it is possible to obtain basic information on sex, race, and ethnicity for the occupants of more than 99 percent of housing units. A success rate this high requires persistent, well-trained, door-to-door interviewers and some recourse to proxy information from neighbors.

The U.S. Bureau of the Census estimates that each screening interview that does not result in a household of the desired type costs as much as one-third of the cost of interviewing the household with standard NHIS instruments. This estimate is parameterized in this report as $R = 3$, meaning that a full standard interview costs three times as much as a screening interview. For example, 51,000 extra attempted screeners should cost as much as 17,000 completed interviews. In fact, the Bureau made an estimate this low only with great hesitancy. Some at the Bureau believed that $R = 2$ or even $R = 1.5$ would be better projections. (The cost estimates are staff member judgments because the Bureau has not had any recent direct experience with screening of this type.) The actual cost will be sensitive to the required response rate, the rules allowing the use of information from neighbors, and the

**Table 9. Distributions in 1990 of each density stratum that is other**

| Percent of block population constituted by a specific racial and/or ethnic group | Black | Hispanic | Asian and Pacific Islander | American Indian, Eskimo, and Aleut |
|---|---|---|---|---|
| < 10 | 86.6 | 83.5 | 77.6 | 76.0 |
| < 5 | . . . | 84.6 | 78.2 | 76.0 |
| 5–10 | . . . | 75.9 | 71.5 | 73.3 |
| 10–30 | 63.6 | 62.9 | 59.3 | 70.3 |
| 30–60 | 40.7 | 37.8 | 35.0 | 50.0 |
| 60 years and over | 8.1 | 11.8 | 15.4 | 7.7 |

NOTE: Other is defined as not black, Hispanic, Asian, Pacific Islander, American Indian, Eskimo, or Aleut. In most tables in this report, other is defined as not black or Hispanic.

complexity of the screening rules. Because the U.S. Bureau of the Census has not attempted screening in household interviews since the 1960's, a decision was made to field test procedures in 1993. A field test was carried out by the Bureau with good results (12).

Although screening can be done on an equiprobability sample, it is often more effective when combined with oversampling at the block level, as is further explored in chapter 5. Screening can be used either at the person level or at the household level. Given the very low marginal cost in NHIS of collecting data on extra members of a household, most of the attention for this study was on screening whole households either in or out of the sample for full interviews.

The formulas in the previous section for optimal sampling intervals need to be changed when screening is conducted in tandem with oversampling. Those formulas assumed constant costs per unit across the strata. When screening is used, cost is no longer constant across the strata. Each minority person found in one of the less dense stratum costs more than a similar minority person in the most dense stratum because more screening interviews with untargeted domains had to be conducted to find him or her.

Let $N$ be the size of the total population and $N_D$ be the size of the targeted domain. Let $c$ be the cost of one screening interview. An interview with a member of the targeted domain in stratum $i$ costs $(R\text{-}1)c$ *plus*

$\frac{NP_i}{N_D D_i}c$ for all the screening interviews

that did not yield households from the targeted domain. The cost per interview

is thus proportional to $\frac{N_D}{N}(R-1) + \frac{P_i}{D_i}$

Under optimal allocation, assuming equal unit variances in all strata, the sampling interval in stratum $i$ should be determined by the equation

$$SI_i \propto \sqrt{\frac{P_i}{D_i} + \frac{N_D}{N}(R-1)} \qquad (6)$$

where the constant of proportionality $k$ is determined by either a cost constraint or a sample size demand. This formula assumes that no interviews are conducted with anyone outside the targeted domain. However, a subsample of the "other" population can be kept from the screened households without affecting the optimal allocation very much. If there are multiple targeted domains, it appears difficult to obtain an optimum allocation formula.

If $R = 1$, (i.e., screening is as expensive as interviewing), this proportionality gives rise to the same relative allocation of sample as equation 5. If the cost of screening is far less than the cost of interviewing (i.e., $R$ is approaching infinity) and the domain is not extremely rare (i.e., $N_D/N$ is not close to 0), this relationship results in close to a flat set of sampling intervals, which is equivalent to allocation in proportion to total population. Table 10 illustrates this relationship well. As the cost of interviewing increases in proportion to the cost of screening, the benefits of oversampling decrease. For example, with a value of $R = 40$, the percent reduction in total cost for statistics on the black population in the year 2000 for screening with oversampling, compared with screening with uniform sampling intervals, is just 1.5 percent. On the other hand, when the population is extremely rare, as is the case with Native Americans, the benefits of oversampling are still substantial (19-percent cost reduction for same precision) even when a full interview costs 60 times as much as a screener interview.

## Network Sampling by Nomination of Relatives

Prior applications of network sampling involved having an initial probability sample of persons (obtained by area or list sampling) report on the number of relatives they have outside the household and whether any of them have certain characteristics of interest, such as diabetes (13). A variant of this method was studied as part of the NHIS redesign research. It was not believed that most persons would be able to answer most of the questions in NHIS accurately for relatives not living in the household. In fact, for many NHIS supplements, even a household respondent is considered unsatisfactory; interviewers are instructed to accept nonresponse instead of proxy response if self-response is impossible. Thus, the scheme proposed and studied for NHIS was to have interviewers track the nominated relatives and administer the NHIS instruments directly to them. Use of the telephone for the tracking and follow-on interviews was a possible refinement considered.

This scheme is still under consideration for future redesigns but was dropped from active consideration for 1995 for several reasons. First, the scheme is very new. No published descriptions came to light in which network sampling had been used on a large-scale survey in which the nominees had to be tracked and interviewed within a short time. Also, available data on the sizes of familial networks by race and ethnicity are limited. The risks of implementation without extensive pretesting would be considerable. Because there was insufficient time to pretest the method, and reasonable alternatives were identified at least for the black and Hispanic populations, the idea was put on the shelf until future redesigns.

### Detailed Considerations

The areas carefully considered include the following:

- The racial and/or ethnic groups for which network sampling might be most advantageous.
- Whether open or closed networks would be more appropriate.
- The adequacy of existing information on multiplicities associated with different network definitions.
- The biases resulting from misreporting of multiplicities and how to reduce those biases.
- The use of the telephone to reduce costs.
- Unduplication with other current surveys run by the U.S. Bureau of the Census.
- Control over sample sizes.

**Table 10. Aspects of the optimal mix of screening and oversampling for various cost assumptions and racial and/or ethnic subdomains**

| Cost assumption | Ratio of interview cost to screening cost | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1:1 | 1.5:1 | 2:1 | 3:1 | 4:1 | 5:1 | 10:1 | 20:1 | 40:1 | 60:1 |
| Based on 1990 distributions | Optimal ratios of screening-to-interview sample | | | | | | | | | |
| Black | 2.6 | 2.8 | 3.0 | 3.3 | 3.5 | 3.7 | 4.3 | 5.1 | 5.9 | 6.4 |
| Hispanic | 3.7 | 4.0 | 4.2 | 4.5 | 4.8 | 5.0 | 5.7 | 6.6 | 7.6 | 8.2 |
| Asian and Pacific Islander | 10.1 | 10.8 | 11.3 | 12.1 | 12.8 | 13.3 | 15.4 | 18.1 | 21.2 | 23.2 |
| American Indian, Eskimo, Aleut | 15.8 | 18.5 | 20.5 | 23.7 | 26.1 | 28.2 | 35.8 | 45.4 | 57.2 | 65.2 |
| Based on projections to 2000 | | | | | | | | | | |
| Black | 3.1 | 3.4 | 3.6 | 4.0 | 4.2 | 4.5 | 5.2 | 5.9 | 6.6 | 6.9 |
| Hispanic | 3.9 | 4.2 | 4.4 | 4.7 | 4.9 | 5.1 | 5.8 | 6.5 | 7.1 | 7.4 |
| Based on 1990 distributions | Optimal oversampling rates for densest stratum | | | | | | | | | |
| Black | 8.2 | 6.8 | 6.0 | 5.0 | 4.4 | 3.9 | 2.9 | 2.2 | 1.7 | 1.5 |
| Hispanic | 9.7 | 8.2 | 7.3 | 6.1 | 5.3 | 4.8 | 3.5 | 2.6 | 2.0 | 1.7 |
| Asian and Pacific Islander | 10.7 | 9.1 | 8.1 | 6.8 | 6.0 | 5.4 | 3.9 | 2.9 | 2.2 | 1.9 |
| American Indian, Eskimo, Aleut | 20.4 | 16.2 | 13.9 | 11.2 | 9.7 | 8.6 | 6.1 | 4.4 | 3.2 | 2.6 |
| Based on projections to 2000 | | | | | | | | | | |
| Black | 5.5 | 4.6 | 4.0 | 3.4 | 3.0 | 2.7 | 2.0 | 1.6 | 1.3 | 1.2 |
| Hispanic | 5.6 | 4.7 | 4.1 | 3.4 | 3.0 | 2.8 | 2.1 | 1.6 | 1.4 | 1.3 |
| Based on 1990 distributions | Screener sample sizes for precision equivalent to simple random sample of 1,000 | | | | | | | | | |
| Black | 3,900 | 3,900 | 4,000 | 4,100 | 4,200 | 4,300 | 4,700 | 5,300 | 6,000 | 6,400 |
| Hispanic | 5,500 | 5,500 | 5,600 | 5,700 | 5,800 | 5,900 | 6,300 | 7,000 | 7,800 | 8,300 |
| Asian and Pacific Islander | 18,000 | 18,000 | 18,000 | 18,000 | 19,000 | 19,000 | 20,000 | 21,000 | 23,000 | 25,000 |
| American Indian, Eskimo, Aleut | 61,000 | 61,000 | 61,000 | 61,000 | 62,000 | 63,000 | 65,000 | 69,000 | 75,000 | 79,000 |
| Based on projections to 2000 | | | | | | | | | | |
| Black | 4,700 | 4,700 | 4,800 | 4,900 | 5,000 | 5,100 | 5,600 | 6,100 | 6,700 | 7,000 |
| Hispanic | 5,500 | 5,500 | 5,500 | 5,600 | 5,700 | 5,800 | 6,200 | 6,700 | 7,200 | 7,500 |
| Based on 1990 distributions | Interviewed sample sizes for precision equivalent to simple random sample of 1,000 | | | | | | | | | |
| Black | 1,500 | 1,400 | 1,300 | 1,200 | 1,200 | 1,200 | 1,100 | 1,000 | 1,000 | 1,000 |
| Hispanic | 1,500 | 1,400 | 1,300 | 1,300 | 1,200 | 1,200 | 1,100 | 1,000 | 1,000 | 1,000 |
| Asian and Pacific Islander | 1,800 | 1,700 | 1,600 | 1,500 | 1,500 | 1,400 | 1,300 | 1,200 | 1,100 | 1,100 |
| American Indian, Eskimo, Aleut | 3,800 | 3,300 | 3,000 | 2,600 | 2,400 | 2,200 | 1,800 | 1,500 | 1,300 | 1,200 |
| Based on projections to 2000 | | | | | | | | | | |
| Black | 1,500 | 1,400 | 1,300 | 1,200 | 1,200 | 1,200 | 1,100 | 1,000 | 1,000 | 1,000 |
| Hispanic | 1,400 | 1,300 | 1,300 | 1,200 | 1,200 | 1,100 | 1,100 | 1,000 | 1,000 | 1,000 |
| Based on 1990 distributions | Percent reduction in cost compared to screening without oversampling | | | | | | | | | |
| Black | 53 | 48 | 43 | 36 | 31 | 27 | 16 | 7.6 | 3.1 | 1.7 |
| Hispanic | 51 | 47 | 43 | 38 | 33 | 30 | 19 | 10.2 | 4.6 | 2.7 |
| Asian and Pacific Islander | 47 | 45 | 43 | 41 | 38 | 36 | 28 | 18.8 | 10.4 | 6.6 |
| American Indian, Eskimo, Aleut | 52 | 51 | 50 | 48 | 47 | 46 | 40 | 33.1 | 24.3 | 19.0 |
| Based on projections to 2000 | | | | | | | | | | |
| Black | 41 | 35 | 32 | 26 | 21 | 18 | 10 | 4.1 | 1.5 | 0.8 |
| Hispanic | 35 | 31 | 27 | 22 | 19 | 16 | 9 | 3.7 | 1.2 | 0.5 |

*Domains of Application*—Satisfactory techniques had already been identified for the black and Hispanic populations (see chapter 5). American Indians, Eskimos, and Aleuts seemed totally out of reach with any design-unbiased sampling technique (chapter 8). The techniques identified for the black and Hispanic populations would be too expensive to use for Asians and Pacific Islanders (also chapter 8). Network sampling, when coupled with multiyear aggregation (discussed later in this chapter), however, seems to offer the possibility of achieving the desired precision for statistics on Asians and Pacific Islanders at a reasonable cost. Thus, there is reason to continue to study the potential uses of network sampling for oversampling demographic domains for NHIS.

*Open Versus Closed Networks*—A closed network is defined as one in which anyone in the network can nominate anyone else in the network. An open network is defined as any other type of network. An example of a closed network is induced by the sibling rule, and an example of an open network is induced by the parent rule. (See *National Network Surveys of Diabetes* (13) for descriptions of both rules.) The multiplicity of a rule is the average number of persons that someone may nominate, including himself or herself.

*Existing data on multiplicities*—Unpublished tabulations of a special experiment on the 1976 NHIS reveal that the standard open network, allowing nomination of parents and siblings, yields a multiplicity of 5.1 and that the standard closed network, allowing nomination only of siblings, yields a multiplicity of 3.6. Because of increased variation in weights, design effects increase by 30 to 260 percent for the sibling rule. The networks also lead to an increase in design effects as a result of clustering; this effect has not been estimated. (The ‘‘design effects’’ in *National Network Surveys of Diabetes* (13) need to be inverted and multiplied by 3.6 to obtain the more common design effects that indicate an increase in variance over a simple random sample of the same size.) Obviously, if the multiplicity is 3.6 but the design effect increases by 260 percent, there is no net gain in precision. Several other caveats must be noted here. First, it is difficult to predict the number of households over which nominated persons will be spread. Many of the nominated persons will be in the original sample household. On the other hand, if each spouse nominates blood relations, the multiplicity may increase strongly. Second, these numbers apply to the total population; results might be very different for the Asian American population. Third, these networks were not geographically restricted to be in the same PSU or in any sample PSU.

*Biases*—Misreporting of multiplicity results in biased and inconsistent estimators. Of course, all sample survey estimators are biased and inconsistent to some degree because of listing errors, misclassification of the occupancy of HU’s, incomplete household rosters, nonresponse, and other nonsampling errors. However, there is an additional potential for bias in multiplicity estimators based on network samples. The actual level of bias depends strongly on how the networks are defined. If the network is defined in terms of ambiguous relationships, the bias will be worse. For example, if the concept ‘‘friend’’ is used to define the network, it is easy to see that many people will have no real idea of how many other people would regard them as friends. Without that knowledge, an individual cannot inform the survey taker of his or her personal multiplicity. Without knowledge of that multiplicity, it is not possible to form an unbiased weight for the respondent.

Even when the network is defined in terms of less ambiguous relationships such as those of siblings or parents, uncertainties will remain because of step relationships, adoptions, foster relationships, and other special relationships. Furthermore, it is not sufficient to know how many persons could have nominated the sample person. Rather, it is necessary to know how many persons would have nominated the sample person had they been selected in the basic NHIS sample. For example, if brother Ken would never provide to interviewers information on brother Harry, Ken does not count in computing the multiplicity of Harry. Would Harry know whether or not Ken would nominate him?

Biases attributable to poor respondent estimation of personal multiplicity that are common in many network sampling applications can be sharply reduced for a closed network if nominated persons are also interviewed and asked to make nominations of their own. Depending upon the effectiveness of procedures for resolving discrepancies between nominations by various members of the network, biases could be largely eliminated. Of course, this circular nomination and resolution would be time consuming, in terms of person hours and calendar time. Current schedules for completing assignments would have to be relaxed.

The only closed network to have been tried in the past is the sibling rule. Great care must be taken with half-siblings to make even this network closed. It might be possible to devise a larger closed network by following only male or only female bloodlines. For example, if an eldest son could nominate only his father, father’s father, father’s father’s father, eldest son, eldest son’s eldest son, eldest son’s eldest son’s eldest son, the network appears to be closed; everyone in the network can nominate everyone else in the network. In general, this appears to be an extremely difficult task of dubious utility.

Biases attributable to poor respondent estimation of personal multiplicity in open networks are much more difficult to avoid. One possible way of avoiding them is to check with all potential nominators to see whether they would indeed have nominated the sample person. This would be a costly operation because unless the potential nominators can also be nominated themselves, there is no other reason to trace and contact them. Furthermore, there is no way to determine whether there are other people (in addition to those mentioned by the sample person) who might have nominated the sample person.

*Use of The Telephone*—Some of the difficulties of timing could be reduced by using telephone interviews for nominated households (or persons). Also, use of the telephone would make it possible to allow nationwide nominations instead of only local nominations. On the negative side, telephoning could introduce another bias because some households do not have telephones and others might refuse to provide telephone numbers.

*Unduplication*—It would be extremely difficult, perhaps impossible, to prevent duplication of network sample cases by other census surveys. Duplication would, of course, be very rare, but a few cases would be nearly certain.

*Control Over Sample Sizes*—Network sampling would result in poor control over sample sizes. Controlling the mix of ages would also be difficult. It is likely that a network sample that yielded enough elderly cases for one age domain would yield far more cases from other age domains than would optimally be desired. Subsampling within households is possible, but this introduces a whole new dimension of operational complexities. If a sibling rule were used, it might be possible—by restricting the ages of persons who could make nominations—to target the nominated sample more closely by age. Of course, such a rule would change a closed network to an open one.

## Further Research

The first step in extending research forward on this idea would be to add a supplement to some major national survey that would fill in information gaps about multiplicities for racial and ethnic domains. NHIS is a possible vehicle for such a supplement, but the *Current Population Survey* would be preferable given its larger size. The supplement should be developed to determine the multiplicity for various household rules—with and without local restrictions, and with and without restriction to persons with listed telephone service. The next step would be to design an actual pretest. No such design work was performed as part of this project. Clearly, planning will need to closely involve NCHS and the U.S. Bureau of the Census.

## Dual-Frame Sampling With Administrative Lists

The only lists of persons that are maintained by the Federal Government and made available for sampling for health-related research are the list of Social Security and Railroad Retirement Act beneficiaries maintained by SSA and the list of Medicare beneficiaries maintained by HCFA. Use of these lists falls under provisions of the Privacy Act of 1974, which permits transfer of the data (including names and addresses) within the Public Health Service for health-related research. Westat, Inc., has used the Medicare list for a number of surveys (including the Medicare Current Beneficiary Survey for HCFA); the list has been easy to work with and Westat, Inc., staff members have been able to locate almost all of the sample persons on it.

Commercial lists of other segments of the population do exist, but these were not thoroughly investigated as part of this research.

Integration of samples from multiple frames into a single microdata file with a single weight requires, at a minimum, the ability to tell which units had more than one chance of selection. This is enough to create unbiased weights. Furthermore, if it is possible to determine the sampling stratum for each unit in each of the frames, the precision of the estimators can be improved. If the full joint probabilities of selection for all units within and across all frames can be determined, even more precise estimators are possible. Unbiased multiplicity-based weights can be developed without knowing the overall probability of selection for a unit. If this overall probability of selection is known, however, the Horvitz-Thompson estimator will usually be more precise.

## Multiyear Aggregation

The least costly way to improve the precision of core statistics for rare domains is to aggregate statistics over multiple years. The precision thus achieved is not quite as great as would result from independent samples, because each year's sample is in the same set of PSU's and segments, but the precision loss is only moderate. Combining statistics over 2 years adds about 10 to 15 percent to design effects. A 3-year combination adds about 19 to 26 percent. For rare domains quite a few years of aggregation might be necessary. This method is clearly not suitable for questionnaire supplements that are carried for only a single year, as is often the case. Still, some data on rare domains can be collected this way. In the absence of any other data for such domains, NCHS should have a tabulation program to produce these aggregations.

## Carrying Samples Over From Prior Years

For questionnaire supplements, a method that is closely related to multiyear aggregation is to carry samples over from previous years. Clearly, the least costly source of samples for rare domains consists of those samples that have already been used. The drawbacks to this method are higher nonresponse and higher respondent burden. High response is very difficult to maintain in the face of physical moves and declining interest in participation.

This method can be made even less expensive by not following movers, but this reduces the available sample size and increases design effects. Internal U.S. Bureau of the Census' data indicate that when movers are not followed, a total response rate of 70 percent can be obtained, where all the movers are counted as nonrespondents. If $d$ years of previous sample are used, this means that the nominal sample size increases by a factor of $1 + (.7)d$. The recent movers that are identified in the new sample must be weighted up by a factor of $d + 1$ to obtain unbiased weights. This means that a design effect will be incurred of

$$DE = \frac{d+1}{(1+.7d)^2}\left[.7(d+1) + \frac{.3}{d+1}\right] \tag{7}$$

Thus, the effective sample size only increases by a factor of

$$\frac{\text{Effective multiyear sample size}}{\text{One year sample size}} = \frac{(1+.7d)^3}{.7\,(d+1)^2 + .3} \tag{8}$$

Adding all stationary persons of a targeted domain from 1 prior year thus results in an increase in the effective sample size for the targeted domain of about 60 percent. Two prior years yield an increase of 110 percent. Three prior years yield an increase of 160 percent. Of course, if members of the targeted domain tend to move at higher or lower rates than the general population, these projections will either be too optimistic or too pessimistic. Also, there are recall issues that could affect how unbiased the weights are. To the extent that recent movers make errors in reporting the fact or timing of their moves, the weights will suffer bias. It seems to make sense that longer periods of recall will increase the potential for that sort of bias.

A solution that avoids the design effects and possible biases of not following movers is to retain addresses of targeted domains rather than the occupants themselves. Further study of this option would require data on the frequency with which successor occupants of minority households are also members of the same minority. Such data were not readily available, and so this option was not seriously studied as part of this research. However, it is an option that may be worthy of further study. Data could be assembled on change in the minority status of household members through longitudinal address surveys such as the American Housing Survey.

## Cost Models

Making rational choices between alternative techniques clearly requires some understanding of the costs of the alternatives. An effort was made to develop a comprehensive cost model for NHIS (14); unfortunately, the robustness of the model was uncertain. Existing data are thin and unlikely to be improved upon. Instead of relying on comprehensive cost models, the research described in this report proceeded by drafting fairly specific plans for intuitive costing by survey administration experts.

A number of steps that could be taken to improve the quality of cost data at the U.S. Bureau of the Census were outlined, but these steps would be rather expensive. It is not enough to know how much each interviewer charged. A careful accounting of all interviewers' actions and their associated costs would be a useful start, but even that would not be enough to allow major extrapolations from current practice. NCHS would need either to raise budgets or reduce the collection of substantive data enough to allow the collection of cost data that are good enough to support reliable cost modeling. It is doubtful that the gains in sample design resulting from the improved cost models would be great enough to compensate for the high cost of the data collection required to create such models.

# Chapter 5. Best Techniques for Sampling the Nonelderly Black and Hispanic Populations

## Oversampling Small Decennial Census Geographic Units Without Screening

In the 1985–94 design, enumeration districts (ED's) and block groups (BG's) with strong concentrations of black persons were oversampled, a random sample of households within these ED's and BG's was selected, and all residents of selected households were retained for full interviews regardless of race. This procedure is not very helpful in increasing the effective sample size for black persons because it is coupled with a policy of using total population as the measure of size for the selection of PSU's and keeping tight constraints on total PSU workloads. This yields a situation in which strongly black ED's and BG's in some rural southern PSU's are not oversampled, because to do so would result in unacceptably large PSU-level workloads. This concern about total workloads is probably mistaken; the U.S. Bureau of the Census has the latitude to handle the large workloads by scheduling more visits by its traveling interviewers to such PSU's. Even if the workload constraints were viewed as absolutely necessary, the large workloads in some PSU's could have been prevented if the PSU size measure had emphasized the presence of black persons.

This strategy has another inherent problem: It results in an oversample of white persons, Asians, Native Americans, and others who live in predominantly black neighborhoods. Although a relatively small proportion of the population that is not black lives in such neighborhoods, that proportion is a substantial part of the population in these neighborhoods. People who are not black in these neighborhoods are not of greater policy interest than other people who are not black; this oversampling, therefore, is largely a waste of effort.

To quantify the limitations of this approach (and to serve as the basis for other research), new tabulations of the 1990 decennial census were made showing how the various domains are distributed across strata defined by the density of minorities within blocks. Tables 11–14 summarize key results. Table 13 is of particular interest; it shows that a little less than 1 percent of the population that is neither black nor Hispanic lives in blocks that are more than 60-percent Hispanic and that a comparable proportion of this population lives in blocks that are more than 60-percent black. Although these proportions may seem small, the ''other'' population is so large (about 80 percent of total population) that about 14 percent of the total population is ''other'' in the blocks where Hispanic persons exceed 60 percent. Similarly, about 9 percent of the total population is ''other'' in the blocks that are more than 60-percent black.

By the time the new design is implemented, the results from the 1990 decennial census will no longer be current. The prevalence of each group in each stratum has been projected forward to 2000, however, the midpoint of the life of the new design, based upon study of the deterioration in strata observed between 1980 and 1988 (15).

Despite the prevalence of the ''other'' population in the blocks with

**Table 11. Distribution of the black population across density strata in 1990**

| Percent of block that is black | Percent of block that is Hispanic: < 5 | 5–10 | 10–30 | 30–60 | 60+ | Total |
|---|---|---|---|---|---|---|
| < 10 | 4.6 | 1.2 | 1.5 | 0.7 | 0.5 | 8.5 |
| 10–30 | 6.6 | 2.1 | 2.8 | 1.3 | 1.0 | 13.9 |
| 30–60 | 8.4 | 1.7 | 2.8 | 2.5 | 0.7 | 16.2 |
| 60+ | 50.0 | 4.5 | 5.6 | 1.3 | 0.1 | 61.4 |
| Total | 69.6 | 9.6 | 12.7 | 5.8 | 2.2 | 100.0 |

SOURCE: 1990 decennial census, tabulated by Westat, Inc.

NOTE: Figures may not add to totals because of rounding.

**Table 12. Distribution of the Hispanic population across density strata in 1990**

| Percent of block that is black | Percent of block that is Hispanic: < 5 | 5–10 | 10–30 | 30–60 | 60+ | Total |
|---|---|---|---|---|---|---|
| < 10 | 5.2 | 6.1 | 15.1 | 14.6 | 32.5 | 73.4 |
| 10–30 | 0.7 | 1.2 | 3.8 | 4.4 | 5.5 | 15.5 |
| 30–60 | 0.3 | 0.4 | 1.6 | 3.4 | 1.7 | 7.4 |
| 60+ | 0.5 | 0.5 | 1.7 | 0.9 | 0.1 | 3.6 |
| Total | 6.6 | 8.1 | 22.1 | 23.3 | 39.8 | 100.0 |

SOURCE: 1990 decennial census, tabulated by Westat, Inc.

NOTE: Figures may not add to totals because of rounding.

**Table 13. Distribution of the other population across density strata in 1990**

| Percent of block that is black | Percent of block that is Hispanic: < 5 | 5–10 | 10–30 | 30–60 | 60+ | Total |
|---|---|---|---|---|---|---|
| < 10 | 68.5 | 8.8 | 8.1 | 2.1 | 0.7 | 88.3 |
| 10–30 | 4.7 | 1.4 | 1.6 | 0.5 | 0.1 | 8.3 |
| 30–60 | 1.6 | 0.3 | 0.4 | 0.1 | 0.0 | 2.5 |
| 60+ | 0.8 | 0.1 | 0.1 | 0.0 | 0.0 | 0.9 |
| Total | 75.6 | 10.6 | 10.2 | 2.8 | 0.8 | 100.0 |

0.0 Quantity more than zero but less than 0.05

SOURCE: 1990 decennial census, tabulated by Westat, Inc.

NOTE: Figures may not add to totals because of rounding.

high concentrations of black or Hispanic persons, oversampling without screening can be used to achieve modest increases in effective sample sizes for the black and Hispanic populations, particularly if only one of the groups is targeted. (Table 19 illustrates this.) The table displays the same strata and population distributions that are displayed in tables 15–18, but its strata are arrayed vertically instead of in matrix form. The oversampling rates are relative to the sampling fraction implied by an equiprobability sample of 50,000 households. (An oversampling rate of less than 1 means that the sampling fraction in that stratum is less than that for an equiprobability sample.) The oversampling rates were derived with the formulas provided in chapter 4, in the section on oversampling classes of blocks, under the assumption of constant cost.

Examination of table 19 reveals that oversampling without screening can boost the nominal sample for black persons by 157 percent. Unfortunately, the design effect jumps up to 1.51; thus, the increase in the effective sample size is only 71 percent. The cost of such a strategy is a 28-percent decline in the effective sample size for the total population, a 19-percent decline for the Hispanic population, and a 35-percent decline for the other population. The best that can be done for Hispanic persons is a 54-percent increase in the effective sample size, accompanied by declines of 28 percent, 48 percent, and 29 percent for the total population, black persons, and others, respectively. The last four columns of the table show various compromise allocations of the sample across the strata. Compromises A and B reflect optimal allocations for an artificial population that is part black and part Hispanic. Compromise C takes an oversampling rate for each minority stratum that is the maximum of those required for either black or Hispanic persons and then dramatically undersamples the first stratum that has the lowest concentrations of the black and Hispanic populations. Compromise D sets oversampling rates that are proportional to the maximums required by the black and Hispanic populations. None of these compromises offers the desired level of improvement in

**Table 14. Distribution of the total population across density strata in 1990**

| Percent of block that is black | Percent of block that is Hispanic < 5 | 5–10 | 10–30 | 30–60 | 60+ | Total |
|---|---|---|---|---|---|---|
| < 10 | 55.3 | 7.7 | 8.0 | 3.1 | 3.5 | 77.5 |
| 10–30 | 4.6 | 1.4 | 1.9 | 0.9 | 0.7 | 9.6 |
| 30–60 | 2.3 | 0.5 | 0.8 | 0.7 | 0.2 | 4.5 |
| 60+ | 6.6 | 0.6 | 0.9 | 0.2 | 0.0 | 8.4 |
| Total | 68.9 | 10.3 | 11.5 | 4.9 | 4.4 | 100.0 |

SOURCE: 1990 decennial census, tabulated by Westat, Inc.

NOTE: Figures may not add to totals because of rounding.

**Table 15. Distribution of the black population across density strata in 2000**

| Percent of block that is black | Percent of block that is Hispanic < 5 | 5–10 | 10–30 | 30–60 | 60+ | Total |
|---|---|---|---|---|---|---|
| < 10 | 11.5 | 3.3 | 2.8 | 0.9 | 0.8 | 19.3 |
| 10–30 | 6.3 | 1.6 | 3.0 | 1.4 | 1.0 | 13.3 |
| 30–60 | 8.1 | 1.9 | 3.1 | 2.8 | 0.4 | 16.2 |
| 60+ | 44.9 | 2.6 | 3.1 | 0.6 | 0.0 | 51.2 |
| Total | 70.8 | 9.3 | 12.0 | 5.7 | 2.2 | 100.0 |

SOURCE: Westat, Inc., projection.

NOTE: Figures may not add to totals because of rounding.

**Table 16. Distribution of the Hispanic population across density strata in 2000**

| Percent of block that is black | Percent of block that is Hispanic < 5 | 5–10 | 10–30 | 30–60 | 60+ | Total |
|---|---|---|---|---|---|---|
| < 10 | 15.3 | 10.0 | 15.5 | 13.0 | 23.5 | 77.3 |
| 10–30 | 1.0 | 1.4 | 3.0 | 3.5 | 5.0 | 13.9 |
| 30–60 | 0.2 | 0.4 | 2.5 | 3.0 | 0.6 | 6.7 |
| 60+ | 0.5 | 0.3 | 0.9 | 0.3 | 0.1 | 2.0 |
| Total | 17.0 | 12.1 | 22.0 | 19.8 | 29.2 | 100.0 |

SOURCE: Westat, Inc., projection.

NOTE: Figures may not add to totals because of rounding.

**Table 17. Distribution of the other population across density strata in 2000**

| Percent of block that is black | Percent of block that is Hispanic < 5 | 5–10 | 10–30 | 30–60 | 60+ | Total |
|---|---|---|---|---|---|---|
| < 10 | 73.9 | 8.1 | 7.3 | 1.7 | 0.3 | 91.2 |
| 10–30 | 4.3 | 0.7 | 0.8 | 0.2 | 0.1 | 6.1 |
| 30–60 | 1.7 | 0.3 | 0.3 | 0.0 | 0.0 | 2.3 |
| 60+ | 0.4 | 0.1 | 0.0 | 0.0 | 0.0 | 0.5 |
| Total | 80.2 | 9.1 | 8.4 | 1.8 | 0.4 | 100.0 |

0.0 Quantity more than zero but less than 0.05.

SOURCE: Westat, Inc., projection.

NOTE: Figures may not add to totals because of rounding.

effective sample sizes for minorities. Compromises A and B seem reasonable but still fall short of goals. Compromise D is unacceptable, and compromise C is counterproductive even for the black and Hispanic populations. The problem comes from the objective of oversampling both minorities. These groups tend not to share the same areas of high concentration. The blocks with strong concentrations of black persons have only a small proportion of the Hispanic population, and vice versa. Because oversampling with screening offers much larger improvements (see the discussion later in this chapter), the technique of oversampling without screening was dropped from further

**Table 18. Distribution of the total population across density strata in 2000**

| Percent of block that is black | Percent of block that is Hispanic | | | | | |
|---|---|---|---|---|---|---|
| | < 5 | 5–10 | 10–30 | 30–60 | 60+ | Total |
| < 10 | 59.2 | 7.8 | 7.7 | 2.9 | 3.1 | 80.7 |
| 10–30 | 4.2 | 0.9 | 1.3 | 0.7 | 0.8 | 7.8 |
| 30–60 | 2.3 | 0.5 | 0.9 | 0.7 | 0.1 | 4.5 |
| 60+ | 6.0 | 0.4 | 0.5 | 0.1 | 0.0 | 7.0 |
| Total | 71.7 | 9.5 | 10.4 | 4.4 | 4.0 | 100.0 |

0.0 Quantity more than zero but less than 0.05.

SOURCE: Westat, Inc., projection.

NOTE: Figures may not add to totals because of rounding.

consideration for the black and Hispanic populations.

## Screening a National Equiprobability Sample

Screening without oversampling blocks is probably the only technique that makes sense for oversampling domains that are fairly evenly spread throughout the land, such as teenagers. For black and Hispanic persons, however, screening by itself is much more costly than screening in conjunction with oversampling blocks; it was thus dropped from further consideration.

## Oversampling at the Block Level With Screening for Race, Ethnicity, Age, and Sex

Given that black and Hispanic populations tend to be separately segregated, that there is no particular interest in those members of the majority population who happen to live in blocks where they are the minority, and that screening interviews are expensive, the most clearly efficient procedure is to combine oversampling of blocks with screening. The term ‘‘screening’’ is used here to include the standard subsampling of untargeted domains that immediately follows the short interview that reveals the domain of a household or person.

By screening households in oversampled blocks, it is possible to eliminate the undesired oversample of others living there. If screening is conducted even in the stratum with the lowest concentration of black and Hispanic persons (and thus the smallest sampling fraction), it is possible to increase the effective sample sizes further for black and Hispanic persons. From table 10, it is apparent that minorities in the only-slightly-integrated blocks should not be sampled at rates less than 4 to 5 times those used in highly segregated blocks. Finally, with screening, it is possible to oversample each age group at a separate rate. This is an important capability, because the age bracket of 18–24 years is critically important for some current public health issues (such as acquired immunodeficiency syndrome) yet is quite narrow; thus it is difficult to achieve the required precision for young black adults and Hispanic persons without simultaneously getting larger-than-necessary samples of the more broadly defined age brackets such as 25–44 years.

Accordingly, a detailed plan was developed to oversample blocks and screen sample households by race, ethnicity, sex, and age. To do this, the plan included screening whole households in or out of the sample that will be asked to go through the full interview depending on their compositions. An alternative would have been to screen some household members in and others out; but this did not make sense, given the NHIS core instrument’s structure, which makes the interview of additional household members very inexpensive.

The plan was ultimately rejected for two reasons. The first was a concern about the complexity of the screening process. In those cases where the occupants of a sample housing unit cannot be directly contacted during the screening phase, it is often fairly easy to get neighbors to provide occupants’ race and sex. Ethnicity (Hispanic or not) is a little more difficult to obtain, and age is the most difficult.

The second reason for rejecting the plan was a feeling that, even though statistics about the age bracket of 18–24 years are very important, it was sufficient just to increase the total minority sample size. There was great reluctance to spend money on screening to find minority households only to discard some of those households because they did not contain an occupant of the desired age and sex.

## Oversampling at the Block Level With Screening for Race and Ethnicity Only

Once the decision had been made to give up on the required effective sample size of 1,000 for the age bracket of 18–24 years by sex for each minority, the proposed design had to be altered significantly. This relaxation of requirements led to substantial simplifications and cost savings. The basic combination of oversampling blocks with high concentrations of black and Hispanic persons and then following with screening is still by far the most efficient method of increasing total minority effective sample sizes, however. The reasons for this are the same as discussed in the previous section.

To set the details of the design, NCHS delineated the following new requirements and assumptions:

- An assumption of a budget 50-percent larger than is needed for a sample of 50,000 households in the current design.
- Tolerance of a reduction in the sample size for the population that is neither black nor Hispanic, if necessary, to obtain the required effective sample sizes for black and Hispanic populations.
- A requirement of roughly equal effective sample sizes for black and Hispanic persons of 8,000 adults each, given the random selection of one adult per household.
- The assumption that $R = 3$, meaning that a full NHIS interview costs 3 times as much as a screening

**Table 19. The limits of oversampling without screening**

| Density stratum | Projections to the year 2000 | | | | | | Oversampling rates by stratum subject to fixed total sample size | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Percent black in 1990 | Percent Hispanic in 1990 | Percent of total population | Percent of black population | Percent of Hispanic population | Percent of other population | To minimize variance for blacks | To minimize variance for Hispanic | Compromise A | Compromise B | Compromise C | Compromise D |
| 1 . . . . . . . . | < 10 | < 5 | 59.2 | 11.5 | 15.3 | 73.9 | 0.58 | 0.63 | 0.58 | 0.56 | 0.10 | 0.48 |
| 2 . . . . . . . . | < 10 | 5–9 | 7.8 | 3.3 | 10.0 | 8.1 | 0.86 | 1.41 | 1.23 | 1.09 | 1.41 | 1.08 |
| 3 . . . . . . . . | < 10 | 10–29 | 7.7 | 2.8 | 15.5 | 7.3 | 0.79 | 1.76 | 1.50 | 1.28 | 1.76 | 1.34 |
| 4 . . . . . . . . | < 10 | 30–59 | 2.9 | 0.9 | 13.0 | 1.7 | 0.73 | 2.63 | 2.20 | 1.82 | 2.63 | 2.00 |
| 5 . . . . . . . . | < 10 | 60+ | 3.1 | 0.8 | 23.5 | 0.3 | 0.66 | 3.40 | 2.83 | 2.32 | 3.40 | 2.59 |
| 6 . . . . . . . . | 10–29 | < 5 | 4.2 | 6.3 | 1.0 | 4.3 | 1.60 | 0.61 | 0.86 | 1.10 | 1.60 | 1.22 |
| 7 . . . . . . . . | 10–29 | 5–9 | 0.9 | 1.6 | 1.4 | 0.7 | 1.74 | 1.55 | 1.49 | 1.52 | 1.74 | 1.33 |
| 8 . . . . . . . . | 10–29 | 10–29 | 1.3 | 3.0 | 3.0 | 0.8 | 1.98 | 1.89 | 1.78 | 1.79 | 1.98 | 1.51 |
| 9 . . . . . . . . | 10–29 | 30–59 | 0.7 | 1.4 | 3.5 | 0.2 | 1.85 | 2.78 | 2.44 | 2.20 | 2.78 | 2.12 |
| 10 . . . . . . . | 10–29 | 60+ | 0.8 | 1.0 | 5.0 | 0.1 | 1.51 | 3.21 | 2.74 | 2.35 | 3.21 | 2.44 |
| 11 . . . . . . . . | 30–59 | < 5 | 2.3 | 8.1 | 0.2 | 1.7 | 2.45 | 0.37 | 1.11 | 1.58 | 2.45 | 1.87 |
| 12 . . . . . . . | 30–59 | 5–9 | 0.5 | 1.9 | 0.4 | 0.3 | 2.60 | 1.17 | 1.49 | 1.83 | 2.60 | 1.98 |
| 13 . . . . . . . | 30–59 | 10–29 | 0.9 | 3.1 | 2.5 | 0.3 | 2.40 | 2.07 | 2.01 | 2.07 | 2.40 | 1.83 |
| 14 . . . . . . . | 30–59 | 30–59 | 0.7 | 2.8 | 3.0 | 0.0 | 2.61 | 2.55 | 2.40 | 2.39 | 2.61 | 1.99 |
| 15 . . . . . . . | 30–59 | 60+ | 0.1 | 0.4 | 0.6 | 0.0 | 2.58 | 3.04 | 2.76 | 2.62 | 3.04 | 2.32 |
| 16 . . . . . . . | 60+ | < 5 | 6.0 | 44.9 | 0.5 | 0.4 | 3.57 | 0.36 | 1.58 | 2.29 | 3.57 | 2.72 |
| 17 . . . . . . . | 60+ | 5–9 | 0.4 | 2.6 | 0.3 | 0.1 | 3.40 | 1.10 | 1.74 | 2.29 | 3.40 | 2.59 |
| 18 . . . . . . . | 60+ | 10–29 | 0.5 | 3.1 | 0.9 | 0.0 | 3.25 | 1.70 | 1.99 | 2.37 | 3.25 | 2.48 |
| 19 . . . . . . . | 60+ | 30–59 | 0.1 | 0.6 | 0.3 | 0.0 | 3.20 | 2.26 | 2.33 | 2.54 | 3.20 | 2.44 |
| 20 . . . . . . . | 60+ | 60+ | 0.0 | 0.0 | 0.1 | 0.0 | 2.92 | 3.11 | 2.87 | 2.70 | 3.11 | 2.37 |
| | Totals for 2000 | | 268,000,000 | 34,000,000 | 32,000,000 | 203,000,000 | | | | | | |
| | Resulting nominal oversampling rates across strata | | | | | | | | | | | |
| Total | 1.00 | 1.00 | 1.00 | 1.00 | 1.00 | 1.00 | | | | | | |
| Black | 2.57 | 0.87 | 1.47 | 1.85 | 2.64 | 2.05 | | | | | | |
| Hispanic | 1.02 | 2.18 | 1.89 | 1.65 | 2.17 | 1.72 | | | | | | |
| Other | 0.74 | 0.84 | 0.79 | 0.76 | 0.55 | 0.72 | | | | | | |
| | Design effects from unequal weights | | | | | | | | | | | |
| Total | 1.30 | 1.39 | 1.30 | 1.33 | 5.93 | 1.49 | | | | | | |
| Black | 1.51 | 1.67 | 1.17 | 1.25 | 3.79 | 1.37 | | | | | | |
| Hispanic | 1.26 | 1.41 | 1.33 | 1.27 | 4.01 | 1.37 | | | | | | |
| Other | 1.14 | 1.19 | 1.16 | 1.16 | 4.01 | 1.24 | | | | | | |
| | Effective oversampling rates | | | | | | | | | | | |
| Total | 0.72 | 0.72 | 0.77 | 0.75 | 0.17 | 0.67 | | | | | | |
| Black | 1.71 | 0.52 | 1.26 | 1.49 | 0.69 | 1.50 | | | | | | |
| Hispanic | 0.81 | 1.54 | 1.42 | 1.30 | 0.54 | 1.25 | | | | | | |
| Other | 0.65 | 0.71 | 0.68 | 0.65 | 0.14 | 0.58 | | | | | | |

0.0 Quantity more than zero but less than 0.05.

interview. See the fourth section of chapter 2 for the rationale on requiring only one sample adult per household.

Designing a sample to meet these requirements was a complex undertaking. The theory in chapter 4 guides the allocation of sample across minority density strata when there is only one targeted domain, but it does not determine an optimal allocation when there are two targeted domains. Also, that theory assumes that all persons found in the screening who are not members of the targeted domain will be dropped from the sample. For this design it was necessary to balance the cost of screening and the design effects associated with oversampling against the gains in precision for the black and Hispanic populations.

The basic procedure used during the design process was to experiment with different allocations of the sample across the minority density strata defined earlier in this chapter. For every trial allocation, the nominal oversampling rates, design effects, and effective oversampling rates were calculated for all three domains using equations 3, 2, and 4, respectively. The nominal oversampling rates were then applied to the sample sizes that could be expected from an equiprobability sample of 50,000 interviewed households shown in table 6. The costs of the screening interviews and of the full interviews for the black and Hispanic households were then calculated. This amount of money was then subtracted from the total assumed available given a 50-percent increase in funding over the level required for an equiprobability sample of 50,000 interviewed households. This then determined the number of full interviews that could be conducted with "other" households. The allocation of the sample was then chosen that yielded the required effective sample sizes of 8,000 interviewed black adults and 8,000 interviewed Hispanic adults (given the random selection of one adult per household), while maximizing the effective sample size for other adults.

The resulting oversampling rates are shown in table 20. Recall that these

**Table 20. Oversampling rates for the alpha design**

[Relative to a design that would yield 50,000 interviews]

| Percent of block that is black | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 1.6 | 2.00 | 3.20 | 4.00 | 4.00 |
| 10–30 | 1.6 | 2.00 | 3.50 | 4.00 | 4.00 |
| 30–60 | 1.7 | 2.00 | 3.50 | 4.00 | 4.00 |
| 60+ | 2.0 | 2.00 | 3.50 | 4.00 | 4.00 |

**Table 21. Persons in screened households for the alpha option**

| Race, Hispanic origin, and age | All adults in each household | | |
|---|---|---|---|
| | Male | Female | Total |
| Black | | | |
| Under 5 | 1,475 | 1,413 | 2,888 |
| 5–17 | 4,203 | 4,010 | 8,213 |
| 18–24 | 1,559 | 1,824 | 3,383 |
| 25–44 | 4,220 | 5,352 | 9,572 |
| 45–64 | 2,706 | 3,494 | 6,200 |
| 65 and over | 1,250 | 1,850 | 3,100 |
| Total | 15,412 | 17,943 | 33,355 |
| Total 18 and over | 9,735 | 12,520 | 22,255 |
| Households | . . . | . . . | 12,295 |
| Hispanic | | | |
| Under 5 | 2,508 | 2,401 | 4,908 |
| 5–17 | 6,023 | 5,753 | 11,776 |
| 18–24 | 2,283 | 2,208 | 4,492 |
| 25–44 | 6,376 | 6,223 | 12,599 |
| 45–64 | 2,864 | 3,245 | 6,109 |
| 65 and over | 1,033 | 1,446 | 2,479 |
| Total | 21,087 | 21,276 | 42,363 |
| Total 18 and over | 12,556 | 13,122 | 25,678 |
| Households | . . . | . . . | 12,465 |
| Other | | | |
| Under 5 | 5,127 | 4,859 | 9,986 |
| 5–17 | 15,490 | 14,690 | 30,180 |
| 18–24 | 7,450 | 7,518 | 14,968 |
| 25–44 | 25,716 | 26,083 | 51,799 |
| 45–64 | 21,128 | 21,935 | 43,063 |
| 65 and over | 10,428 | 15,073 | 25,500 |
| Total | 85,339 | 90,158 | 175,498 |
| Total 18 and over | 64,722 | 70,609 | 135,331 |
| Households | . . . | . . . | 74,358 |
| Total | | | |
| Under 5 | 9,110 | 8,673 | 17,783 |
| 5–17 | 25,716 | 24,453 | 50,169 |
| 18–24 | 11,292 | 11,551 | 22,843 |
| 25–44 | 36,312 | 37,658 | 73,970 |
| 45–64 | 26,698 | 28,674 | 55,372 |
| 65 and over | 12,711 | 18,369 | 31,080 |
| Total | 121,839 | 129,377 | 251,216 |
| Total 18 and over | 87,013 | 96,251 | 183,264 |
| Households | . . . | . . . | 99,118 |

. . . Category not applicable.

NOTE: Figures may not add to totals because of rounding.

ratesare relative to an equiprobability sample that yields 50,000 completed household interviews.

These oversampling rates result in nominal oversampling rates for the black, Hispanic, and ‘‘other’’ populations of 2.212, 3.195, and 1.831, respectively (equation 3). Applying these nominal oversampling rates to table 6 yields table 21, which shows the total number of persons in screened households.

Clearly, the number of ‘‘other’’ households discovered during screening is much larger than can be afforded with a 50-percent budget increase. At the conclusion of the screening interview, it is necessary to tell some of them, ‘‘Thanks for your time and have a nice day.’’ Of course, there is no reason to subsample at a uniform rate. Given the differential oversampling rates shown in table 20, it is much better to design the retention rates in such a manner that an ‘‘other’’ household (containing only non-Hispanic white persons, Asians, or Native Americans) will have the same probability of selection whether it is in a predominantly black, Hispanic, or other neighborhood.

The computation of retention rates for ‘‘other’’ households depends strongly on the relative costs of screening and full interviews. As mentioned already, a fundamental assumption underlying the design was that a full interview costs 3 times as much as a screening interview ($R$ = 3). The differential oversampling rates shown in table 20 are projected to yield 123,900 addresses for screening (before identification of vacants, other ineligibles, and nonrespondents). With a sample of 50,000 interviewed households, the number of addresses to screen is just 62,500 (allowing for a combined loss to ineligibility and nonresponse of 20 percent). If the hypothetical survey of 50,000 completes were expanded by 50 percent, that would mean 93,750 screened addresses. The difference of 30,150 screened households means that the number of full interviews must be cut by at least 10,050 households from the 75,000 that could be done with a straight 50-percent increase over 50,000 interviewed households. Cutting 10,050 households from the hypothetical 75,000 does not save all the cost associated with those contacts, however. There are still the screening costs. The actual number of total households that can be retained for full interviews is determined by the equation

$$\left[50{,}000 + \frac{62{,}500 - 50{,}000}{3}\right](1.5) = \left[x + \frac{(123{,}900 - x)}{3}\right]$$

Solving this equation yields the answer that 59,900 households can be retained for full interviews. Subtracting out the 12,300 black households and the 12,500 Hispanic households that are expected to receive full interviews, this leaves room for 35,100 ''other'' households to receive full interviews. Given that it is expected that the screening will yield 74,300 ''other'' households, this implies that the overall retention rate of other households discovered during screening should be 47.3 percent.

Let $r_i$ be the retention rate for screened ''other'' households in $i$-th minority density stratum. Also, let $k_i$ be the oversampling rate for the stratum from table 20, and let $Q_i$ be the proportion of the national ''other'' population that is in the $i$-th stratum (in table 17). It is optimal to have the two following relationships both be true:

$$\sum Q_i r_i = 0.473 \text{ and } r_i \sim \frac{1}{k_i}$$

The first equation simply requires that the average weighted retention rate for other households across all the strata be correct. The second relationship will ensure an equiprobability sample of ''other'' households. Both relationships can be satisfied by setting

$$r_i = \frac{0.473}{k_i \sum_j (Q_j / k_j)}$$

The results of this calculation are shown in table 22.

The differential weights associated with the oversampling shown in table 20 leads to design effects of 1.084 and 1.135 for the black and Hispanic populations, respectively. After subsampling, the design effect for the ''other'' population will be 1.00 because there will be an equiprobability sample of the ''other'' population. Thus, the effective sample sizes for the three domains can be achieved by adjusting the numbers of screened persons shown in table 21 for the design effects and subsampling. This means dividing by 1.084 for the black population, dividing by 1.135 for the Hispanic population,

**Table 22. Percent of other households discovered during screening to be kept for full interviews under the alpha design**

| | Percent of black that is Hispanic | | | | |
|---|---|---|---|---|---|
| Percent of block that is black | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 54.1 | 43.3 | 27.0 | 21.6 | 2 |
| 10–30 | 54.1 | 43.3 | 24.7 | 21.6 | 2 |
| 30–60 | 50.9 | 43.3 | 24.7 | 21.6 | 2 |
| 60+ | 44.4 | 43.3 | 24.7 | 21.6 | 2 |

**Table 23. Effective sample sizes for alpha design when all household members are sampled**

| | All adults in each household | | |
|---|---|---|---|
| Race, Hispanic origin, and age | Male | Female | Total |
| Black | | | |
| Under 5 | 1,361 | 1,304 | 2,664 |
| 5–17 | 3,877 | 3,599 | 7,576 |
| 18–24 | 1,438 | 1,683 | 3,121 |
| 25–44 | 3,893 | 4,938 | 8,830 |
| 45–64 | 2,496 | 3,223 | 5,720 |
| 65 and over | 1,153 | 1,706 | 2,860 |
| Total | 14,218 | 16,553 | 30,771 |
| Total 18 and over | 8,980 | 11,550 | 20,530 |
| Households | . . . | . . . | 11,343 |
| Hispanic | | | |
| Under 5 | 2,209 | 2,115 | 4,325 |
| 5–17 | 5,307 | 5,068 | 10,375 |
| 18–24 | 2,012 | 1,946 | 3,957 |
| 25–44 | 5,618 | 5,483 | 11,100 |
| 45–64 | 2,523 | 2,859 | 5,382 |
| 65 and over | 910 | 1,274 | 2,184 |
| Total | 18,579 | 18,745 | 37,324 |
| Total 18 and over | 11,063 | 11,562 | 22,624 |
| Households | . . . | . . . | 10,983 |
| Other | | | |
| Under 5 | 2,425 | 2,299 | 4,724 |
| 5–17 | 7,327 | 6,948 | 14,275 |
| 18–24 | 3,524 | 3,556 | 7,080 |
| 25–44 | 12,164 | 12,337 | 24,501 |
| 45–64 | 9,994 | 10,375 | 20,369 |
| 65 and over | 4,932 | 7,129 | 12,062 |
| Total | 40,365 | 42,645 | 83,010 |
| Total 18 and over | 30,614 | 33,898 | 64,012 |
| Households | . . . | . . . | 35,171 |
| Total | | | |
| Under 5 | 4,242 | 4,035 | 8,277 |
| 5–17 | 12,216 | 11,609 | 23,826 |
| 18–24 | 5,475 | 5,609 | 11,084 |
| 25–44 | 17,906 | 18,569 | 36,475 |
| 45–64 | 13,627 | 14,541 | 28,168 |
| 65 and over | 6,568 | 9,499 | 16,067 |
| Total | 60,035 | 63,861 | 123,896 |
| Total 18 and over | 43,576 | 48,217 | 91,794 |
| Households | . . . | . . . | 46,794 |

. . . Category not applicable.

NOTE: Figures may not add to totals because of rounding.

and multiplying by 0.473 for the "other" population. The effective sample size for the total population is slightly more complicated to calculate and present because the oversampling of black and Hispanic households means that the average household size will increase for the sample. After subsampling, the nominal oversampling rate for the total population will be 1.278 with a design effect of 1.281. Also, the nominal oversampling rate for households will be 1.197 with the same design effect of 1.281. The effective sample size for the total population is obtained by multiplying the sample sizes shown in table 6 by either 1.278 or 1.197 and then dividing by 1.28. The results of these adjustments are shown in table 23.

To get effective sample sizes when just one adult is sampled per household, it is necessary to adjust the numbers in table 23 in the same manner as the numbers in table 6 were adjusted to obtain table 7. The results are shown in table 24.

**Table 24. Effective sample sizes for alpha design when one adult is sampled per household**

| Race, Hispanic origin, and age | One adult per household | | |
|---|---|---|---|
| | Male | Female | Total |
| Black | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 621 | 726 | 1,347 |
| 25–44 | 1,680 | 2,131 | 3,811 |
| 45–64 | 1,078 | 1,391 | 2,469 |
| 65 and over | 498 | 737 | 1,234 |
| Total | . . . | . . . | . . . |
| Total 18 and over | 3,876 | 4,985 | 8,861 |
| Households | . . . | . . . | 11,343 |
| Hispanic | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 781 | 756 | 1,537 |
| 25–44 | 2,182 | 2,129 | 4,311 |
| 45–64 | 980 | 1,110 | 2,090 |
| 65 and over | 353 | 495 | 848 |
| Total | . . . | . . . | . . . |
| Total 18 and over | 4,296 | 4,490 | 8,786 |
| Households | . . . | . . . | 10,983 |
| Other | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 1,627 | 1,642 | 3,269 |
| 25–44 | 5,616 | 5,696 | 11,313 |
| 45–64 | 4,614 | 4,791 | 9,405 |
| 65 and over | 2,277 | 3,292 | 5,569 |
| Total | . . . | . . . | . . . |
| Total 18 and over | 14,135 | 15,421 | 29,556 |
| Households | . . . | . . . | 35,171 |
| Total | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 2,488 | 2,566 | 5,053 |
| 25–44 | 7,739 | 8,137 | 15,876 |
| 45–64 | 5,382 | 5,901 | 11,283 |
| 65 and over | 2,512 | 3,631 | 6,144 |
| Total | . . . | . . . | . . . |
| Total 18 and over | 18,121 | 20,235 | 38,356 |
| Households | . . . | . . . | 46,794 |

. . . Category not applicable.

# Chapter 6. Choosing a Sampling Technique for the Elderly Black and Hispanic Populations

## Area Sampling With Oversampling at the Block Level With Screening for Race, Ethnicity, Age, and Sex

As shown in table 20, even with a 50-percent budget increase, the oversampling of blocks with high concentrations of black or Hispanic populations and screening (as a result of mortality and immigration patterns), black and Hispanic persons constitute a much smaller proportion of the elderly population in this country than is the case in other age groups. There are not enough elderly black and Hispanic persons to yield sample sizes from the proposed design that meet NCHS requirements. Although the sample sizes for both sexes have projected deficiencies, the problems are especially acute for males; fewer than 500 are projected for black males (on an effective basis) and about 350 for Hispanic males (also on an effective basis). A budget increase much larger than 50 percent would be necessary to meet the goals by oversampling blocks and screening alone. Other methods of increasing the effective sample sizes for these domains were therefore researched.

## Dual-Frame Sampling With Social Security Administration Lists

Because the Social Security Administration (SSA) maintains files with excellent coverage of the elderly population (at least for those age 66 years and over), research was conducted on dual-frame sampling that would combine the traditional area-permit sample with a supplemental list sample.

While this research was being conducted, interest arose in supplementing the NHIS sample for 1994 (1 year before the planned redesign) with a list sample of disabled SSA beneficiaries. This work is reported separately in *A Dual Frame Design for Sampling Elderly Minorities and Persons With Disabilities* (16). The idea of oversampling SSA disabled beneficiaries for NHIS has been dropped for now. A decision to use SSA lists for oversampling either disabled beneficiaries or elderly minority persons, however, makes it easier, of course, to use the list for the other type of oversampling as well.

The task of oversampling the elderly black population from SSA files is fairly straightforward because race is indicated for about 97 percent of the file. Unfortunately, SSA files do not have an indicator of Hispanic origin for persons who are currently elderly. (SSA started to collect ethnicity data in the 1980's, too late to be of use in classifying the current elderly population.) Thus, an important issue is the identification of those who are likely to be Hispanic. This task is possible through the use of surname classification. The following sections discuss this use of surnames, effective sample sizes that can be achieved with dual-frame sampling, and whether to use SSA lists to oversample either addresses or persons. Assuming that persons are oversampled, the issue of other household members and estimation issues for dual-frame sampling are discussed in the last two sections of this chapter.

### Using Surnames to Identify Hispanic Persons

To use the SSA files for oversampling elderly Hispanic persons despite the lack of an indicator for Hispanic origin, a strategy that identifies persons likely to be Hispanic on the basis of surname was considered. The Hispanic surname file developed by J. Passel and D. Word at the U. S. Bureau of the Census (17) was used to classify surnames by ethnic origin. The Passel-Word file contains 12,497 surnames that tend to belong to Hispanic persons.

The precision and cost of a dual-frame sample based on surnames depends strongly on the sensitivity and specificity of the Passel-Word file. Every false positive costs money to interview or screen out; every false negative increases the portion of the area-permit sample that is not covered by the list and that will therefore have high sampling weights, thereby increasing design effects because of unequal weights. (Note, however, that neither false positives nor false negatives make unbiased estimation impossible. This is discussed later in this chapter.) Passel and Word indicated that false positives (also called errors of commission) run at around 15 percent and that false negatives (also called errors of omission) run at around 20 percent. Given the time lapse since that original study, it was judged prudent to repeat the study. To that end, the surname of every member of the 1988 NHIS was matched against the Passel-Word file. This allowed the comparison of self-reported ethnicity with the ethnicity that would be imputed on the basis of surname using this file. At the same time, some of the characteristics of persons with nonconforming names were analyzed; it was thought that this information should be useful in making decisions about sample allocation.

The overall false-positive rate was 12.6 percent, lower than had been expected on the basis of previously published research; the overall false-negative rate was 31.6 percent, higher than expected. Passel and Word originally reported error rates with the March 1976 *Current Population Survey* of 15.0 percent false-positives and 20.7 percent false-negatives. Part of the discrepancy involves persons who did not classify their ethnicity, who had hyphenated names, or who refused to provide their names; Passel and Word treated all of these differently than was done in this research. An inspection of nonconforming surnames of Hispanic persons indicates, however, that even allowing for these variations in matching procedure, a substantial and unexplained difference exists in the false-negative rate. The reason for the poorer performance could be either lower quality recording, transcription, and keying of names on the 1988 NHIS than on the March 1976 CPS (interviewers, not respondents, record name spellings) or real change in the population in the relationship between surname and self-reported ethnicity. Both rates are slightly higher for the elderly than for the general population: 16.6 percent false-positives and 33.6 percent false-negatives.

Both types of misclassification based on surname are far more common among women than among men. Intermarriage evidently plays a strong role in both misclassification rates. Socioeconomic status also plays a strong role in both rates. The general trend seems to be that higher socioeconomic status means a weaker association between surname and self-reported ethnic origin. Differences also exist by metropolitan status, part of the country, and country of origin (for self or ancestors). See *Using Surnames to Oversample Hispanics From a List Frame* (18) for more detail.

**Table 25. Required sample sizes by sampling method and precision target**

| Method | Sex | Effective Hispanic sample size[1] | Screener interviews[2] |
|---|---|---|---|
| SSA list with match to Passel-Word file under observed error rates | M | 770 | 1,000 |
| | F | 920 | 1,000 |
| Total screener interviews required | | | 2,000 |
| SSA list without match to Passel-Word file | M | 770 | 9,750 |
| | F | 920 | 10,350 |
| Total screener interviews required | | | 20,100 |
| Area sample | M | 770 | |
| | F | 920 | |
| Total screener interviews required | | | 100,000 |
| SSA list with match to Passel-Word file under observed error rates | M | 1,000 | 3,050 |
| | F | 1,000 | 1,950 |
| Total screener interviews required | | | 5,000 |
| SSA list with match to Passel-Word file under 1976 error rates | M | 1,000 | 1,280 |
| | F | 1,000 | 1,120 |
| Total screener interviews required | | | 2,400 |
| SSA list with match to Passel-Word file under 0 error rates | M | 1,000 | 760 |
| | F | 1,000 | 590 |
| Total screener interviews required | | | 1,350 |
| SSA list without match to Passel-Word file | M | 1,000 | 15,100 |
| | F | 1,000 | 12,500 |
| Total screener interviews required | | | 27,600 |
| Area sample | M | 1,000 | |
| | F | 1,000 | |
| Total screener interviews required | | | 180,000 |

[1]Nominal Hispanic sample sizes are larger than the effective sizes shown.

[2]List either persons to be tracked and screened or households from area sample to be screened in order to achieve the effective sample sizes indicated for Hispanics. All of these screener interviews would be in addition to the 99,000 annual household screener interviews proposed for the 1995 design.

## Effective Sample Sizes With Dual-Frame Sampling

Although the estimated sensitivity of the Passel-Word Hispanic surname list was not as great as had been hoped, using it to create a list sample of elderly Hispanic persons still has good potential. The technique is far cheaper than area sampling with screening for the same precision. These implications are explained more fully in this section. Table 25 contrasts some of the numbers that appear in the following text.

Under current plans for 1995 and beyond, the area-permit NHIS sample will yield nominal elderly Hispanic sample sizes of about 500 males and 700 females. After accounting for the design effect due to disproportionate sampling of heavily Hispanic blocks, the effective sample sizes (compared with a similarly clustered sample with equal probabilities) will be only about 350 and 500. By adding 1,000 males with Hispanic surnames from SSA lists and another 1,000 females in the same manner, the nominal sample sizes can be boosted to around 1,240 and 1,400, and the effective sample sizes can be boosted to around 770 and 920. (The effective sample sizes do not increase as much as the nominal sample sizes because of the design effect due to the large weights that Hispanic persons without Hispanic surnames will bear.) To get a comparable boost from the area-permit sample alone would require the screening of an additional 100,000 households.

For another contrast, suppose that one used SSA lists as a supplemental list sampling frame without paying attention to surname. In that case, more than 20,000 persons would have to be located and screened on ethnicity to obtain comparable boosts for elderly Hispanic persons. Thus, although screening a list of elderly persons is far more efficient than screening a sample of households (even if those households were skewed toward heavily Hispanic blocks), it is not as efficient as using a list in combination with the surname list.

Nonetheless, boosting the effective sample sizes even more sharply for elderly Hispanic persons, to as high as 1,000 males and 1,000 females, was desired. Here it turns out that the false-negative rate is too high to make that economical. About 5,000 list persons with Hispanic surnames would have to be added to the sample.

Interest in determining ways to decrease the false-negative rate is significant, even if it means some increase in the false-positive rate as it likely would. If, for example, the 1976 findings of Passel and Word still held, the supplemental sample size required for the desired effective sample sizes by sex would be 2,400 instead of 5,000. (Even with a perfect indicator of Hispanic origin on the list, a supplemental sample of 1,350 persons would be required to achieve the desired effective sample sizes of 1,000 by sex.) It is suspected that adjustments in interviewer training and in keypunching could reduce the frequency of false negatives. Even though the U. S. Bureau of the Census would be sampling from an SSA list that would probably have more accurate name spelling, it would still be necessary to classify everyone in the area-permit sample to determine appropriate sampling weights for the dual-frame estimator.

## Addresses or Persons?

One of the issues that was discussed with respect to dual-frame sampling was whether SSA beneficiaries or their addresses should be the sampling units. The field costs would be lower for the address option because tracking of movers and persons with bad addresses (e.g., addresses of custodians) would not be necessary. This idea was rejected, however, for two reasons. First, the efficiency of the oversample would be degraded. No data were available on how often beneficiaries move or on the characteristics of subsequent occupants of their housing units. Second (and more important), weighting would be more difficult with the address-based approach. Using this approach, it would be necessary to match every address in the regular NHIS sample against SSA files to determine whether SSA had a record of an elderly black person or an elderly person with a Hispanic surname living at the address. Given the fact that SSA addresses have neither isolated components nor standardized abbreviations, the false-nonmatch rate would be substantial.

A related approach would be to make the person the sampling unit but to conduct the interview only if the SSA address was current and correct. This would have reduced the cost of the sample without increasing matching difficulties. This idea was rejected because it would result in an oversample of a narrower domain than the domain of interest.

## Other Household Members

If persons sampled from SSA lists are to be followed, the question of whether to collect data about other household members naturally arises. A flexible approach was developed that involves collecting data about other household members for core items and for some (but not all) questionnaire supplements. When other household members are interviewed, a multiplicity estimator would be used to weight them.

The recommendation to administer the core questionnaire to all household members was based on several considerations. First, plans have been made to trim the core to a very short instrument. Furthermore, the core questionnaire structure makes it unlikely that substantial cost savings could be gained by restricting the core to the SSA-designated sample person. Of course, if those plans to trim the core are never realized, a review of this recommendation would be appropriate.

The decision about any particular supplement to the core would be influenced by several factors. First, are other household members likely to be in domains of interest? Second, is the burden on the household tolerable? Third, is the marginal cost of collecting the data high?

Some core supplements are administered to random samples of household members. (The most common rule is one random adult.) If it is decided to administer such a supplement in households that are in the SSA list sample, the within-household subsampling must be done independent of which person or persons in the household were selected directly from the SSA list. The alternatives involve sharp variations in weights.

## Dual-Frame Estimation

Dual-frame sampling with SSA lists requires somewhat more complex weighting than surveys using a single frame. Extra data are needed for unbiased weighting. Specifically, it must be known whether every person in the regular NHIS area-permit sample has a chance of being selected through the list. For list sampling of elderly black and Hispanic persons, ideally the following information would be known about members of the area-permit sample:

- Whether he or she is a Social Security beneficiary.
- If so, how race is recorded on SSA files for the person (black, other, or unspecified).
- The exact spelling of surname on SSA files.

One possible way to obtain these data would be to do an exact match of all elderly persons in the area-permit sample against SSA files. However, an exact match would be expensive and difficult. Several considerations indicate that such a match is not really necessary. First, almost all elderly persons are SSA beneficiaries. (Some problems might exist with persons 65 years of age in terms of the lag time between individuals becoming beneficiaries and files from SSA being processed through sampling operations. Thus, it might be best to use the list to oversample only persons aged 66 years and over rather than those aged 65 years and over.) With respect to race and surname spelling, it is reasonable to assume that both are reported consistently enough. (Black versus not black is the only important racial classification for weighting. Also, exact consistency on surname spelling is not required; it is enough if the misspelling has the same status with respect to presence on the Hispanic surname file.) If a match were to be planned anyway for a disabled list sample, however, it would make sense to match the elderly area sample at the same time to pick up race and surname spelling from SSA files. If no match is done, it might be possible to harmonize spelling between NHIS and SSA by asking to see a Social Security card.

## Details of Weighting the Core

If dual-frame sampling with SSA lists were done, the first step each year would be to count the number of persons in each NHIS area-permit household who are age 65 years or over and either are black or have a Hispanic surname. Then household base weights could be developed. These base weights would not be the traditional inverse probabilities of selection because it would be impossible to compute the probability of selection for a household across all possible area-permit and list samples from the given joint design. (This is because of the systematic multistage approach being used in the list sample.) Instead, a multiplicity-based base weight would be determined for each household in the list sample;

the standard base weight would be assigned to each household in the area-permit sample; then the two weights would be averaged for every sample household. The average would most likely be a weighted average. For a household that is only in one sample, the base weight for the other component would be zero. Base weights for area-permit sample households that do not contain any possible list sample persons are, of course, not changed as a result of dual-frame estimation.

As a variant, it might be preferable to work out adjustments for nonresponse separately for each component and then average together the nonresponse-adjusted weights.

Poststratification could be done just as it is now, using demographic models by age, race, and sex. Once the weights have been calculated, the more complicated estimation for the dual sample would be irrelevant to users. They would be able to make ordinary weighted tabulations of the core in the same way they would have for the area-permit sample by itself.

Weighting procedures for questionnaire supplements would depend on the within-household sampling rules. It is anticipated that different weights would be needed for analysis of the core interview than for analysis of supplements.

# Chapter 7. Prospects for Statistics on Detailed Hispanic Subdomains

Despite the progress that the proposed design (see Part V) will make in improving the precision of NHIS statistics about Hispanic persons, the diversity of the Hispanic experience in this country is such that the analytic potential of NHIS would be improved by additional breakdowns in tabulations. Accordingly, separate statistics on Cuban Americans, Puerto Rican Americans, Mexican Americans (separately by whether their ancestors lived in the United States before 1900 or not), Central American Americans, and other Hispanic Americans appear to be desirable.

## Design-Based Techniques

Although precise statistics on such detailed domains would undoubtedly be extremely interesting, limits are imposed by a fixed budget. Given the overwhelming importance of age and sex for questions related to health, valid contrasts between detailed Hispanic subgroups would have to be supported by adequate sample sizes for each age and sex subgroup within each Hispanic subgroup. Given the required precision levels discussed in chapter 3, eight age-by-sex subdomains for each detailed Hispanic subgroup would require 8,000 persons. Multiplying by 6, detailed Hispanic subgroups give a total required sample size of 48,000 persons, about 40 percent of the total sample. Even if some way to obtain addresses cheaply for so many Hispanic persons existed (it does not), devoting such a large proportion of the total sample to Hispanic persons would seriously erode the utility of NHIS for addressing other questions. The research conducted under this contract yields the conclusion that it is not possible to obtain design-based statistics of the desired reliability about detailed Hispanic subgroups with the current budget. Even a doubling or tripling of the budget would allow only modest precision for each Hispanic subgroup.

The possibility of breaking Hispanic statistics geographically instead of ethnically was also investigated. This approach is related to that used in the Hispanic Health and Nutrition Examination Survey. The regions investigated were the following: The New York Consolidated Metropolitan Statistical Area; Florida; and the five southwestern States of California, Arizona, New Mexico, Texas, and Colorado. Unfortunately, this investigation also showed the need for budget increases far larger than NCHS staff thought were possible. More details can be found in chapter 11.

## Model-Based Techniques

Given the impracticality of design-based techniques, research was conducted on model-based techniques. In this research, the possibility was considered of oversampling only those Hispanic persons who live in heavily Hispanic blocks and then extrapolating the results to all Hispanic persons using models. A key factor here is the relationship between health and segregation. If health is not related to segregation or if the relationship can be explained in terms of socioeconomic variables that are available for the whole sample, the model-based approach would be much more precise than design-based estimators that could be produced at the same cost. Even if neither of these conditions applies (and it seems natural to believe that there is some relationship that cannot be entirely explained by other variables), and thus the model-based estimates would be biased, it is possible that model-based estimators would still have an acceptable mean square error.

To study the question, the relationship between segregation and health was studied for Hispanic persons in the 1988 NHIS. That sample contained too few Hispanic people to study the detailed groups separately, so they were simply studied as a group. The health characteristics of Hispanic persons were contrasted across the five minority density strata that had been created for the Hispanic population. (The strata were defined as of 1980.) Simple comparisons showed significant differences. Logistic models were then created for three binary health measures. The inclusion of socioeconomic variables reduced the nominal relationship between segregation and health but did not rule out the existence of important residual relationships. (For more detail on this study, see *Residential Segregation and Health Characteristics of Minority Populations* (19).) The sample sizes were not large enough to allow very firm projections of the mean square error for model-based estimators. More years of NHIS samples need to be studied to arrive at more definite conclusions.

# Chapter 8. Prospects for Statistics on Other Minority Groups

Prospects are not good for inexpensive, large samples of American Indians, Eskimos, and Aleuts, or of Asians and Pacific Islanders.

## Asians and Pacific Islanders

The main obstacle to providing precise health statistics for Asians and Pacific Islanders is that, despite the concentrations of population in the Chinatowns of New York and San Francisco, most Asians and Pacific Islanders live in more integrated neighborhoods. Considerable screening at substantial cost would be necessary to locate them.

Based on statistics from the 1990 census on the concentration within blocks of Asians and Pacific Islanders (tables 5 and 6), it is estimated that a modest oversample of Asians and Pacific Islanders could be achieved by screening an additional 15,000 households (beyond the 99,000 already planned) and by interviewing an additional 2,580 households. The resulting oversample would yield an effective sample size of around 8,000 persons. (Effective sample size here means the nominal sample size divided by the design effect due to unequal probabilities but not due to clustering.) This sample size of 8,000 persons could be achieved only by interviewing all members of Asian households. (Asian households are simply too rare to allow subsampling within households to reduce household burden.) Also, the distribution across ages would not be optimal. Narrow age bands of particular analytic interest such as persons ages 18–24 years and children under age 6 would have smaller sample sizes than desired. It was estimated that this additional screening and interviewing would cost an additional $1.4 million per year, more than the budget NCHS indicated would be available.

Some of the other potential techniques discussed in chapter 4 may have some promise for sampling Asians. Network sampling is particularly interesting given large family sizes for this domain. (Among Asians and Pacific Islanders, the number of persons per occupied housing unit was 3.59 in 1991 compared with 2.64 for all races.) To develop the idea further would require the sort of research described earlier in chapter 4. Multiyear aggregation is also a technique to be considered.

Also, at least one or two companies are experimenting with lists of typical Asian surnames as a means of identifying Asians and Pacific Islanders. If such an approach yields a false-negative rate below 40 percent, at least modest oversampling would be possible. Perhaps it could be combined with oversampling by block and screening and with network sampling. Although this option has not been pursued very vigorously, it probably is worth further effort.

A further problem is the diversity of the populations covered by the Office of Management and Budget classification of ‘‘Asian and Pacific Islander.’’ Just as interest exists in detailed statistics on Mexican Americans, Puerto Rican Americans, Cuban Americans, and so on, some of the analysts interested in Asian statistics stress the need to distinguish among Chinese, Japanese, Filipinos, Polynesians, Indians, Malays, Vietnamese, Hmong, Mongolians, and so on. Given the relative prosperity of some of these subgroups, a fundamental question remains unanswered: If it is not feasible to create precise statistics for each subgroup of Asians and Pacific Islanders separately (and it is surely not feasible with any procedure short of a special census), are statistics on the amalgam group truly useful? It is assumed in this report that such statistics are indeed important, but this question should receive more careful attention by subject matter specialists.

In conclusion, it was believed that the budget for NHIS could not be increased enough to pay for obtaining truly useful statistics about this domain broken down by age and sex.

## American Indians, Eskimos, and Aleuts

The main obstacle to providing precise health statistics about Native Americans is their small numbers. About one-half of Native Americans live on or near reservations where they are easy to find (although transportation can be difficult and expensive in those areas), but the other one-half live in mostly well-integrated neighborhoods where finding them is very expensive. If the goal were only to provide reliable statistics about Native Americans living on reservations, it could probably be achieved with a modest budget increase (perhaps less than 10 percent).

Otherwise, it is estimated that a screening sample of an additional 128,000 households (on top of the planned 99,000) and an additional 5,860 household interviews would be required just to produce an effective sample size of 8,000 American Indians, Eskimos, and Aleuts—even if all household members were interviewed. That would cost about $9 million per year. With the budget that NCHS has indicated would be available, this would require substantial cutbacks in other domains covered by NHIS.

# Chapter 9. The Poor

The 1985–94 NHIS has a reasonable sample size for the poor (roughly 16,000 persons per year); it is sufficient for moderately detailed analyses. Because of the oversampling of black and Hispanic populations, the sample of the poor will be even larger after the redesign is completed. The research performed did not reveal any inexpensive method of increasing the sample size of the poor any further.

The main obstacle to inexpensive, larger samples of the poor is that persons with low income are far less segregated than many people believe (see table 26). (Poverty data are available only from the decennial census at the block-group level rather than at the block level. The poor may be more dramatically segregated at the block level; but no data exist on the subject, nor can an oversampling strategy be built based upon data that do not exist.) Poverty is not sufficiently concentrated at the block-group level to make stratification and oversampling a feasible strategy for major increases in effective sample sizes.

As mentioned earlier, oversampling black and Hispanic populations will expand the sample size of the poor a little. Larger increases require expensive and problematic screening. (Persuading neighbors to speculate on the financial condition of sample households is far more difficult than obtaining proxy reports on race and ethnic origin. Ethical considerations about whether neighbors should be asked such questions also emerge. Even when a member of the sample household is willing to respond, total income tends to be inadequately reported unless an extensive set of screens and probes and encouragement of record checks are done; and all of these are incompatible with the idea that a screening interview should be cheap and quick to administer.)

Dual-frame samples were not seriously considered, given the knowledge that no national database of beneficiaries of any of the welfare programs has been constructed and the fact that other surveys (such as the *Current Population Survey* and the Survey of Income and Program Participation) have shown repeatedly that a very large segment of the poor do not participate in welfare programs.

Network estimators were also not seriously considered. It thus appears that the only feasible techniques for oversampling the poor for NHIS are either to retain those found in the regular area-permit sample for extra years of interviewing or to aggregate statistics across years.

**Table 26. Geographic concentration of the poor in 1990**

| Percent of block group that is living below poverty | Percent of the poor in such block groups | Percent of total population in such block groups | Percent of population in such block groups that is not poor |
|---|---|---|---|
| < 5 | 5.8 | 33.3 | 97.7 |
| 5 to 10 | 12.3 | 22.1 | 92.7 |
| 10 to 20 | 24.8 | 22.8 | 85.7 |
| 20 to 30 | 19.8 | 10.7 | 75.6 |
| 30 to 40 | 14.3 | 5.4 | 65.5 |
| 40 to 50 | 10.0 | 2.9 | 55.5 |
| 50+ | 13.0 | 2.8 | 38.2 |

# Part III.
# Statistics for Geographic Domains

# Chapter 10. Subnational Estimates for State Groupings and Hispanic Persons

## Subnational Domains With Current Design

The ability of the current NHIS design to produce accurate estimates for States was examined. For estimates to have the precision specified by the NCHS technical staff, an effective sample size of at least 1,000 individuals is necessary (see the first section of chapter 3). If the NHIS sample of 124,000 individuals (50,000 households) were distributed according to a self-weighting sample, only 33 of the States would have the necessary 1,000 completed interviews to produce Statewide estimates. The same number of States would satisfy the minimum sample size requirement if the sample were to oversample the black and Hispanic populations at twice the rate applied to persons who are not black or Hispanic.

Reasonable analyses of health characteristics, however, require State-level estimates by age and sex. The four standard age breaks for subnational statistics are 0–17 years, 18–44 years, 45–64 years, and 65 years and over. The rarest of the eight age-by-sex categories is males 65 years of age and over; that category is estimated to consist of 5.3 percent of the U.S. population in the year 2000. Using a self-weighting design (or one that oversamples at a 2-to-1 rate), no State will contain a sample large enough to include 1,000 members of this age-by-sex category. A fallback position is simply to divide States by the four age categories. (Given the strong age patterns in most health statistics, it appears that control by age is essential.)

The smallest category then is the age group 65 years and over, constituting 13 percent of the population. Only three States—California, Texas, and New York—are sufficiently large to anticipate including 1,000 elderly. (Florida is not quite large enough. Given that the elderly are a significantly larger percentage of the population in Florida than in the average State, it is likely that, by the year 2000, Florida will also include 1,000 sampled elderly.) To select a sample of 1,000 completed interviews of the elderly from each of the 51 States (including the District of Columbia), however, requires a total sample of approximately 390,000 [(1,000/0.13)*51]. This is 3 times the current sample size.

The possibility of producing State statistics from a combination of 3 years of NHIS data was also examined. Many NHIS variables (e.g., chronic condition rates) are relatively stable from year to year. Items covered by supplements to the core questionnaire, however, would have to be retained for 3 years. Because there are correlations between years induced by remaining in the same PSU's and many of the same blocks, the annual State sample size must be larger than the 333 appropriate for combining independent samples. Based on analysis of intra-PSU and block correlations, and on the current level of oversampling, it was estimated that 397 completed interviews would be required each year to yield an effective sample of 1,000 after 3 years (NHIS Redesign Memorandum #40 (4)). With a self-weighting sample design, only 12 of the States would receive the 397 completed interviews per year to produce State estimates every 3 years.

NCHS suggested examining a set of 28 State groupings to ascertain whether a sufficient sample would be available to produce estimates for each grouping every 3 years. Only 21 of the 28 groupings were of the necessary size. An alternative set of 22 groupings in which each group met this criterion was established (see table 27). In addition to producing estimates every 3 years, two-thirds of these groups could produce estimates every 2 years and three of them every year.

**Table 27. State groupings to produce State estimates every 3 years**

[Roughly 6,600,000 needed per group]

| State | Population (000's) | State | Population (000's) | State | Population (000's) |
|---|---|---|---|---|---|
| Washington | 4,991 | North Dakota | 629 | Florida | 15,415 |
| Alaska | 687 | South Dakota | 714 | | |
| Oregon | 2,877 | Minnesota | 4,490 | South Carolina | 3,906 |
| | 8,555 | Nebraska | 1,556 | Georgia | 7,957 |
| | | Kansas | 2,529 | | 11,863 |
| California | 33,500 | | 9,918 | | |
| Hawaii | 1,345 | | | North Carolina | 7,483 |
| | 34,845 | Iowa | 2,549 | | |
| | | Missouri | 5,383 | Virginia | 6,877 |
| Idaho | 1,047 | | 7,932 | | |
| Neveda | 1,303 | | | West Virginia | 1,722 |
| Utah | 1,991 | Wisconsin | 4,784 | Maryland | 5,274 |
| Colorado | 3,813 | Illinois | 11,580 | Delaware | 734 |
| Montana | 794 | | 16,364 | District of Columbia | 610 |
| Wyoming | 489 | | | | 8,349 |
| | 9,437 | Michigan | 9,250 | | |
| | | | | New Jersey | 8,546 |
| Arizona | 4,618 | Indiana | 5,502 | | |
| New Mexico | 1,968 | Ohio | 10,629 | Pennsylvania | 11,503 |
| | 6,586 | | 16,131 | | |
| | | | | New York | 17,986 |
| Texas | 20,211 | Kentucky | 3,733 | | |
| | | Tennessee | 5,266 | Maine | 1,271 |
| Oklahoma | 3,376 | | 8,999 | New Hampshire | 1,333 |
| Arkansas | 2,529 | | | Vermont | 591 |
| Louisiana | 4,516 | Mississippi | 2,877 | Massachusetts | 6,087 |
| | 10,421 | Alabama | 4,410 | Rhode Island | 1,049 |
| | | | 7,287 | Connecticut | 3,445 |
| | | | | | 13,776 |

NOTE: Figures may not add to totals because of rounding.

## Subnational Domains for Hispanic Persons

The possibility of producing estimates for Hispanic persons living in three different areas of the country was also examined. The three areas are the Southwest, Florida, and New York. Hispanic persons in these areas are predominantly of Mexican, Cuban, and Puerto Rican ancestry, respectively. The area probability sample would have to produce an effective sample size of 1,000 completed interviews in each of 24 cells: Four ages (0–17 years, 18–44 years, 45–64 years, and 65 years and over) by two sexes for each of the three areas.

Hispanic growth rates in the three areas were assumed to be the same as for the dominant subgroup in each area. Also, because no information is available on undercoverage losses for Hispanic persons by individual geographic areas, it was assumed that overall Hispanic-coverage rates would apply in all areas except Florida. Hispanic persons in Florida are predominantly Cuban Americans, who are older than other U.S. Hispanic persons and have higher incomes and educational attainment. We have assumed the Hispanic-undercoverage rate in Florida is one-half that of Hispanic persons in general.

Although the results presented here are plausible, they are based on a fair amount of speculation. The research was carried out before the 1990 census update became available. These assumptions should be updated with 1990 census data if oversampling these subdomains is still being seriously considered. The sampling rates needed and screening workloads are quite sensitive to the geographic distribution of Hispanic persons.

The definitions of the geographic areas were initially assumed to be similar to those used in the Hispanic Health and Nutrition Examination Survey (HHANES). In HHANES the Southwest was defined as the following five States: California, Arizona, New Mexico, Texas, and Colorado; Florida was only Dade County; and the New York area included Hartford and Fairfield Counties, Connecticut; Essex, Passaic, Hudson, Middlesex, and Union Counties, New Jersey; and Nassau, Westchester, and Suffolk Counties, and New York City, New York. With the rapid growth of the Hispanic population predicted from 1980 to 2000, it is likely that the New York and Florida areas with high Hispanic densities will expand beyond those used in HHANES. State-level U. S. Bureau of the the Census' projections to the year 2000, made on the basis of the 1980 census, have been used for all three areas (assumptions about Hispanic persons are detailed in NHIS Redesign Memorandums #9 and #26). For purposes of this research, the same five States were used for the Southwest, all of Florida for Florida, and New York and New Jersey (which includes most of the suburban Hispanic population) for New York.

## Oversampling Hispanic Persons

If 24,000 completed interviews are allocated to these 24 cells and the total number of interviewed households is kept to 50,000, the base sampling rate for persons who are not Hispanic (and Hispanic persons in other areas of the country) is reduced from 0.000497 to 0.000414 (see NHIS Redesign Memorandums #16 and #31 for derivation of base sampling rate). Without any oversampling, only 4 of the 24 cells will attain the 1,000 interviews. These are males and females 0–17 years of age and 18–44 years of age in the Southwest. To reach 1,000 in each of the remaining 20 cells requires oversampling Hispanic persons at a rate of 9.3 in the Southwest, 43.5 in New York, and 19.6 in Florida. (Although the number of Hispanic persons will be similar in New York and Florida, Florida has many more elderly Hispanic persons.) Because 21 percent of the Hispanic population lives outside dense Hispanic blocks, it is not possible to adjust for such large oversampling rates without increasing the base sampling rate (see NHIS Redesign Memorandum #26). It is necessary, therefore, to increase the base sampling rate in all three areas of the country. All oversampling, especially increasing the base sampling rate, will require screening significant numbers of households that will not be interviewed.

Even after tripling the base sampling rate in the Southwest, there are still three cells for which high-density Hispanic strata must be further oversampled: Males 65 years and over, females 65 years and over, and males 45–64 years of age. After a sixfold increase in Florida's base rate, six cells require additional oversampling. In New York a thirteenfold increase results in only three cells needing additional oversampling. These 12 cells still require oversampling of the most dense Hispanic blocks after increasing the base sampling rate in the area. The differential sampling rates would range from 7.0 to 1 in the Southwest, from 8.5 to 1 in Florida, and from 9.0 to 1 in New York. The oversampling rates were determined for each cell to avoid oversampling any stratum at 10 times the rate used for other strata in the same geographic area. It was then necessary for the oversampling to be sufficient for the expected cell size to exceed 1,000 multiplied by the design effect arising from oversampling high-density Hispanic blocks. Table 28 summarizes these findings.

These 24 cells will require data collected from 30,402 persons, corresponding to all persons in 10,134 households (Hispanic households are assumed to average 3.0 persons, compared with non-Hispanic households, which average 2.5 persons).

**Table 28. Oversampling rates to produce subnational estimates for Hispanics**

| Area | Increased base rate | Oversampled cells | Oversampling some dense stratum |
|---|---|---|---|
| Southwest | 3 | 3 | 7.0:1 |
| Florida | 6 | 6 | 8.5:1 |
| New York | 13 | 3 | 9.0:1 |

The screening from oversampling the 12 cells, however, will identify 90,339 Hispanic households; 80,205 of these will not need to be interviewed.

To retain a person sample of the size provided by a 50,000-household sample requires 39,866 households (50,000−10,134) from the remainder of the country, including the population that is not Hispanic of these three areas. This population of the three areas accounts for 32.89 percent of the remaining population of the United States and should therefore include 13,112.0 (0.3289 × 39,866) sampled households. This procedure will screen 187,452 households that are not Hispanic in these areas, so an extra 174,341 households would be screened. The U.S. Bureau of the Census estimates that screening costs equal between one-third and one-half of interviewing costs. Screening the 254,546 extra households (80,205 + 174,341) in these three areas will therefore use considerably more money than the current NHIS data collection budget—without conducting a single interview. Thus, at the current budget, it is impossible to produce design-based estimates for Hispanic persons living in these three areas, using a strictly area-sample approach.

## Oversampling Hispanic Persons When Using the Health Care Financing Administration Frame

Subnational estimates for Hispanic persons, therefore, can only be produced if an alternative more efficient methodology is available. A possible alternative is to select the Hispanic elderly from a frame of Social Security recipients maintained by the Health Care Financing Administration (HCFA). The selection could be carried out in the sampled PSU's. This would reduce the extensive screening needed for the rarest age-sex domains, but it would still require the use of the area sample to conduct 1,000 interviews in 18 cells (sex (male or female) by age (0–17 years, 18–44 years, 45–64 years) in three areas).

Producing a sufficient number of interviews in each of these cells would require increasing the sample in the Southwest by a factor of 3.4, in New York by 13.5, and in Florida by 12.2. Two cells in the Southwest require this oversampling of high-density Hispanic strata: Males 45–64 years of age and females 45–64 years of age. After quadrupling the base rate in Florida, all six cells require further oversampling. After quadrupling the base rate in New York, three cells require oversampling: Males 45–64 years, females 45–64 years, and males 18–44 years. It is interesting to note that in Florida the most uncommon cell is females 0–17 years, not the older people.

The lower oversampling rates shown in table 29 compared with table 28 significantly reduce the number of households that need to be screened. The number of Hispanic households to be screened dropped from 90,339 to 36,089 and non-Hispanic households from 187,452 to 70,029.

The size of the Hispanic sample in the three areas has hardly changed, to 30,315 (including the 6,000 coming from the HCFA frame). This is equivalent to 10,105 households (of which 8,105 come from the area probability frame). Because 36,089 Hispanic households will be screened in the three areas, an unused 27,984 Hispanic households will be screened.

To retain a 50,000-household sample, 39,895 households (50,000−10,105) must be sampled from the remainder of the country and the population that is not Hispanic of these three areas. The non-Hispanic sample from the three areas should therefore include 13,121 sampled households (0.3289 × 39,895). Given that 70,029 non-Hispanic households will be screened in these areas, an extra 56,909 households will be screened. Again, using the U.S. Bureau of the Census' estimate for screening costs, the 84,893 extra households (27,984 + 56,909) in these three areas will reduce the number of completed interviews, for a constant-cost NHIS, by approximately 28,000.

It is therefore necessary to redo the calculations, beginning with an overall sample size of fewer than 25,000 households. This reduces the desired number of interviews of people who are not Hispanic, thereby increasing the number of extra households being screened and even further reducing the money available. This iterative process converges on a total sample (excluding Hispanic persons in these three areas) of 16,771 households, 5,516 of whom are not Hispanic persons in the Southwest, New York, or Florida.

If retaining a cost equivalent to the existing budget is desired, it is possible to produce separate age and sex results for Hispanic persons in each of the three geographic areas from NHIS. There are, however, two important caveats to this statement. First, use of the HCFA frame to sample Hispanic elderly is required. The preceding calculations assumed that costs are equivalent for the HCFA and area probability frames. If, for example, it costs 50 percent more to interview a sample from HCFA records than from an area sample, the number of non-Hispanic households that can be sampled will drop by more than 1,000. Second, the non-Hispanic household sample size has been reduced from the 45,985 households (50,000 − (0.000497 × 24,236,000/3)) of a non-oversampled design (such as is used for the existing NHIS) to 16,771. This will increase the standard error on non-Hispanic estimates by 66 percent. It will also make it impossible to provide national estimates for black persons and for persons who are not black or Hispanic for all 24 age-by-sex-by-race cells.

**Table 29. Oversampling rates when the HCFA frame is used as a sampling frame for the elderly**

| Area | Increase base rate | Oversampled cells | Oversampling most dense stratum |
|---|---|---|---|
| Southwest | 4 | 2 | 9.5:1 |
| Florida | 4 | 6 | 7.0:1 |
| New York | 4 | 3 | 9.5:1 |

## Model-Dependent Subnational Hispanic Estimates

Given the significant reduction in accuracy for estimates of people who are not Hispanic resulting from the preceding design, the possibility of producing model-based estimates was explored. The levels of oversampling that would be required were examined for all three areas, as well as only for all Hispanic persons in the Southwest. The latter estimates would closely approximate national estimates for Hispanic persons of Mexican descent. The category of persons 65 years of age and over is again assumed to be sampled from a frame maintained by HCFA.

For this part of the research, it was assumed that a composite estimator would be used, combining a direct sample estimator and an estimator drawn from a model. The sample would be drawn only from blocks with high densities of Hispanic residents (at least 5 percent). Such an estimate will only provide unbiased estimates of all Hispanic persons in the geographic area if the health characteristic has the same expectation for the 21 percent of Hispanic persons living outside high-density blocks as for the 79 percent living inside those blocks, within the variables used in the model. (See the second section of chapter 7 for more detail.)

Table 30 summarizes the sample sizes and sampling rates when screening and subsampling are used. Data are shown for two different ratios of the cost of a completed household interview to the cost of screening a household. The findings for a 3:1 cost ratio are described first.

Because of the design effect of differential sampling rates in some cells, the 24 cells (including 65 years and over from HCFA) will require interviews with 25,532 individuals; of these, 19,533 must come from the area sample (corresponding to 6,511 Hispanic households).

To retain a 50,000-household sample, 41,489 households (50,000−6,511−2,000 from HCFA) must be sampled from the remainder of the country, as well as from the population that is not Hispanic of these three areas

**Table 30. Summary of number screened and interviewed, assuming two different screening costs**

| | Cost ratio 3 to 1 | Cost ratio 2 to 1 |
|---|---|---|
| Hispanic person sample in three areas | 25,532 | 25,580 |
| Hispanic non-HCFA person sample in three areas | 19,532 | 19,580 |
| Hispanic non-HCFA household sample in three areas | 6,511 | 6,527 |
| Remaining desired sample households | 41,489 | 41,473 |
| Desired nondense strata sample in three areas | 7,926 | 7,923 |
| Final remaining sample households | 26,411 | 18,152 |
| Final non-Hispanic household sample in three areas | 5,046 | 3,468 |
| Total households screened in dense strata[1] | 56,790 | 56,637 |
| Of these: | | |
| Not retained for interviewing | 45,234 | 46,643 |
| Hispanic households interviewed | 6,511 | 6,527 |
| Non-Hispanic households interviewed | 5,046 | 3,468 |
| Total households interviewed in nondense strata | 21,365 | 14,684 |
| Total HCFA households interviewed | 2,000 | 2,000 |
| Total households interviewed in entire United States | 34,922 | 26,679 |
| Maximum oversampling rate, blocks at least 50-percent Hispanic: | | |
| Southwest — Male 45–64 years | 4.93 | 5.03 |
| Florida — Female 0–17 years | 17.65 | 18.11 |
| New York — Male 45–64 years | 24.08 | 24.71 |

[1]Dense strata are defined as census blocks with more than 5 percent Hispanics in the most recent census.

and the Hispanic population in the nondense stratum of these three areas. The population in nondense Hispanic blocks in the three areas comprises 19.10 percent of the remaining population in the United States; thus the remaining sample from the three areas should include 7,924 sampled households (0.1910 × 41,489). Identifying these sampled households in the dense strata will require screening an additional 12,516 Hispanic (from nonrare cells) and 29,837 non-Hispanic households in these three areas. Using the U.S. Bureau of the Census' 3:1 cost estimate for screening the 42,353 extra households (12,516 + 29,837) will reduce the number of remaining interviews that can be funded, for a constant-cost NHIS, to 27,372.

It is therefore necessary to redo the calculations beginning with a smaller affordable sample size (27,372 instead of 41,489). This reduces the desired number of interviews of persons who are not Hispanic, thereby increasing the number of extra households being screened and reducing even further the money available. This iterative process converges on a total sample (excluding Hispanic persons in dense strata in these three areas) of 26,411 households, 5,046 of whom are in the Southwest, New York, or Florida.

With a 3:1 cost ratio, developing design-unbiased estimates for the dense blocks of each of the 24 cells will require screening 56,790 households. Of these, 45,234 are not retained for the full interview, and 11,557 are interviewed. Another 21,365 households are interviewed from other than the dense strata of the three areas, and 2,000 households are interviewed from HCFA files. This results in a total of 34,922 households being interviewed in the entire United States.

Table 30 also shows that, as the cost of screening grows even larger, the number of Hispanic interviews increases only slightly. The reason it increases at all is that as the sample becomes more concentrated in the most densely Hispanic blocks, larger design effects are created, requiring more interviews for the same degree of accuracy. The maximum oversampling rates are shown at the bottom of table 30. The number of families that are and are not Hispanic that are screened declines only slightly as the cost of screening increases. Unfortunately, the cost associated with such screening increases much faster than the number of screened households decreases. As a result, the total number of households included in NHIS drops from 50,000 without screening to 34,922 with a 3:1 cost ratio, and to only 26,679 with a 2:1 cost ratio. This dropoff in sample size is even more pronounced when the Hispanic households in the high-density strata are excluded. The number then drops from 26,411 with a 3:1 cost ratio to 18,152 with a 2:1 cost ratio. From these households (i.e., approximately 55,000 individual respondents) must come estimates for the 12 age-by-sex cells for both black persons and persons who are neither black nor Hispanic.

The potential of model-dependent estimates only for Hispanic persons of Mexican ancestry was also examined. The majority of such Hispanic persons reside in the Southwest and include approximately 90 percent of all Hispanic persons in that area. Thus, it is possible to approximate this target population by using model-dependent estimates for Hispanic persons in the Southwest. Table 31 is of the same format as table 30, but indicates the effect of only oversampling high-density Hispanic strata in the Southwest.

Table 31 shows that this oversampling would have only a minor effect on the quality of statistics for black persons or persons who are not black or Hispanic. The overall sample size would be reduced only about 10 percent (from 50,000 households to 46,000 or 44,000). The corresponding reduction in the non-Hispanic sample would be from 45,985 to 40,478 or 38,513.

The same caveats apply as in the previous section, if it is desired to retain a cost equivalent to the existing budget. The non-Hispanic household sample size has been reduced to between 26,411 and 18,152, depending on the cost of screening. This will increase the standard error on non-Hispanic estimates by 32 to 59 percent, also depending on the cost of screening.

Oversampling only in the Southwest would have a much smaller effect on the quality of non-Hispanic estimates. The reduction in the non-Hispanic sample would be from 45,985 to somewhere between 40,478 and 38,513—a reduction in accuracy of only 7 or 9 percent. The design-unbiased Hispanic estimates would, however, be only for those in dense strata of the Southwest.

**Table 31. Summary of number screened (in the Southwest) and interviewed, assuming two different screening costs**

| | Cost ratio 3 to 1 | Cost ratio 2 to 1 |
|---|---|---|
| Hispanic person sample in the Southwest | 17,395 | 17,409 |
| Hispanic non-HCFA person sample in the Southwest | 11,395 | 11,409 |
| Hispanic non-HCFA household sample in the Southwest | 3,798 | 3,803 |
| Remaining desired sample households | 44,202 | 44,197 |
| Desired nondense strata household sample in the Southwest | 5,591 | 5,590 |
| Final remaining sample households | 40,478 | 38,513 |
| Final non-Hispanic household sample in the Southwest | 5,120 | 4,871 |
| Total households screened in dense strata | 20,089 | 20,043 |
| Of these: | | |
| Not retained for interviewing | 11,171 | 11,369 |
| Hispanic households interviewed | 3,798 | 3,803 |
| Non-Hispanic households interviewed | 5,120 | 4,871 |
| Total households interviewed in nondense strata | 35,358 | 33,642 |
| Total HCFA households interviewed | 2,000 | 2,000 |
| Total households interviewed in entire United States | 46,276 | 44,316 |
| Maximum oversampling rate, blocks at least 50-percent Hispanic: | | |
| Southwest — Male 45–64 years | 4.93 | 5.03 |
| Florida — Female 0–17 years | 1.00 | 1.00 |
| New York — Male 45–64 years | 1.00 | 1.00 |

Determining whether to implement the foregoing procedure for oversampling high-density Hispanic blocks will obviously depend on a number of factors, including the quality of the model-dependent estimators and the cost of screening. A major factor in the determination is examining the potential bias in estimating Hispanic health characteristics from a sample in high-density blocks. This issue was examined and is reported in chapter 7 in the section on model-based techniques.

# Chapter 11. Design-Based Approaches for State Estimation

## Estimation Based Solely on the National Health Interview Survey

Table 32 shows the expected sample sizes by State for a self-weighting sample of 50,000 households when black and Hispanic persons are oversampled at a rate of 2 to 1. (These are similar to the rates in the proposed beta option; see chapter 17 for more details.) These sizes are shown for the entire State and for the rarest age-by-sex cell, males age 65 years or over. This table assumes that only an area sample is used, that is, there is no supplementation of persons age 65 and over from HCFA or SSA lists. For the oversampled design, the effective sample sizes are provided, adjusting for the design effect resulting from the differential survey weights.

In the first section of chapter 3, the rationale was provided for the determination that an effective sample size of 1,000 persons or more was required for an individual analytic domain. It is clear from the table that no State will have that size for all age and sex domains; and for about one-third of the States, it would be impossible to produce estimates for the entire State population. Table 33 summarizes these findings. Even if the sample size were doubled or quadrupled, some States would not have the accuracy to be able to have State totals published; and fewer than 10 would be able to have all age and sex domains published.

Examination of table 32 indicates that, after the redesign, the number of States for which the sample size will be sufficient to produce design-based estimates from NHIS will not change significantly from that reported in table 33.

Given the preceding results, all further research into design-based estimates assumed that HCFA lists would be used for persons 65 years of age or over. The rarest cell in the area sample becomes males 45–64 years of age. Table 34 follows a format similar to that of table 32 but now provides estimates for total adults and males age 45–64 years, both when using a self-weighting sample and when using a design with oversampling rates similar to those proposed in the alpha option (see chapter 5) for the redesign. (The alpha option assumes a budget 50-percent greater than that of the current design.) To improve the estimates, the final columns examine the impact of combining 3 years of data (the effective sample sizes shown in table 34 do not take into account the slight reduction in accuracy resulting from using the same PSU's and neighboring blocks each year). The final column shows the coefficient of variation (*cv*) for the rarest cell for a variable that achieves a *cv* of 30 percent from a sample of 1,000 persons (see the first section of chapter 3). With 3 years of data, only three States (California, Texas, and New York) provide the desired accuracy for all eight age and sex cells. Florida will provide close to the desired level of accuracy (a *cv* of 32 percent for males 45–64 years of age). For all remaining States, it will not be possible to make State-level age and sex estimates for all of the eight cells.

## Dual-Frame Estimation Using Random Digit Dialing Supplementation

States that are interested in producing age-by-sex estimates for their own population will have to supplement the NHIS area sample. Given the results of the previous section and the cost of area sampling, it appears that random digit dialing (RDD) supplementation is the method most States would choose. In this section the size of the RDD supplement that would be needed to provide the desired level of accuracy ($cv \le 30$ percent) is determined. Three composite estimators are considered: Unbiased with a $cv = 30$ percent; unbiased with an equal-sized RDD supplement in each State; and a minimum variance, possibly biased, version of the second estimator.

The following assumptions were made for this section:

- Close geographic clustering of the supplemental sample by PSU or segment is not necessary because of the use of telephone data collection. The only clustering would be within sets of 100 telephone numbers having the same area code, prefix, and first two extension digits. The Mitofsky-Waksberg method of sample selection would be used. (If other RDD methods were to be used, a different design effect would be needed in the next subsection.)
- The NHIS redesign used in table 34 (see NHIS Redesign Memorandum #50R for more details) would be the alpha option plus subsampling of one person per household, the procedure generally used in NHIS supplements.
- The estimated RDD supplement sizes assume that States are interested only in age and sex cells. For those States interested in other decompositions by race or Hispanic origin, it may be necessary to enlarge the supplement further. This will depend on whether the size of the smallest desired cell is larger or smaller than the smallest age and sex cell.
- Telephone coverage is 93 percent in each State. (Variation among States is small, ranging from 83 percent in Alaska to 97 percent in Wisconsin. Using 93 percent for each State simplifies the calculation and the results are subject to only minor error.)

### Design Effects

With an RDD sample, it is not necessary to use PSU's, and the only clustering is the clusters of 100 telephone numbers used in the Mitofsky-Waksberg sample selection method. Optimizing the cluster size generally results in a fairly small cluster for interviewing, usually three to five or

**Table 32. State sample sizes with and without oversampling of blacks and Hispanics; States ranked by population size in 2000**

| | | | | Sample size with self-weighting sample of 50,000 households[2] | | Sample size with 50,000 household sample and blacks and Hispanics oversampled at 2 to 1 | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | | Actual sample[3] | | Effective sample[4] | |
| Rank | State | Population year 2000 (000's) | Percent black or Hispanic[1] | Total adults | Males 65 years of age and older | Total adults | Males 65 years of age and older | Total adults | Males 65 years of age and older |
| 1 | California | 33,500 | 38.7 | 15,444 | 819 | 17,564 | 931 | 15,701 | 832 |
| 2 | Texas | 20,211 | 39.5 | 9,317 | 494 | 10,657 | 565 | 9,520 | 505 |
| 3 | New York | 17,986 | 33.7 | 8,292 | 439 | 9,090 | 482 | 8,176 | 433 |
| 4 | Florida | 15,415 | 33.2 | 7,106 | 377 | 7,761 | 411 | 6,987 | 370 |
| 5 | Illinois | 11,580 | 27.1 | 5,338 | 283 | 5,563 | 295 | 5,063 | 268 |
| 6 | Pennsylvania | 11,502 | 12.3 | 5,303 | 281 | 4,883 | 259 | 4,633 | 246 |
| 7 | Ohio | 10,629 | 14.0 | 4,900 | 260 | 4,580 | 243 | 4,320 | 229 |
| 8 | Michigan | 9,250 | 17.0 | 4,264 | 226 | 4,091 | 217 | 3,821 | 203 |
| 9 | New Jersey | 8,546 | 27.4 | 3,940 | 209 | 4,116 | 218 | 2,743 | 198 |
| 10 | Georgia | 7,957 | 28.0 | 3,668 | 194 | 3,850 | 204 | 3,497 | 185 |
| 11 | North Carolina | 7,483 | 22.0 | 3,450 | 183 | 3,451 | 183 | 3,178 | 168 |
| 12 | Virginia | 6,877 | 20.0 | 3,170 | 168 | 3,119 | 165 | 2,888 | 153 |
| 13 | Massachusetts | 6,087 | 7.5 | 2,806 | 149 | 2,473 | 131 | 2,391 | 127 |
| 14 | Indiana | 5,502 | 10.0 | 2,536 | 134 | 2,288 | 121 | 2,189 | 116 |
| 15 | Missouri | 5,383 | 12.0 | 2,482 | 132 | 2,279 | 121 | 2,165 | 115 |
| 16 | Maryland | 5,274 | 30.0 | 2,431 | 129 | 2,592 | 137 | 2,345 | 124 |
| 17 | Tennessee | 5,266 | 17.0 | 2,428 | 129 | 2,329 | 123 | 2,175 | 115 |
| 18 | Washington | 4,991 | 3.0 | 2,301 | 122 | 1,943 | 103 | 1,915 | 102 |
| 19 | Wisconsin | 4,784 | 6.5 | 2,205 | 117 | 1,926 | 102 | 1,869 | 99 |
| 20 | Arizona | 4,618 | 24.6 | 2,129 | 113 | 2,175 | 115 | 1,990 | 105 |
| 21 | Louisiana | 4,516 | 33.0 | 2,082 | 110 | 2,270 | 120 | 2,044 | 108 |
| 22 | Minnesota | 4,490 | 2.0 | 2,070 | 110 | 1,731 | 92 | 1,714 | 91 |
| 23 | Alabama | 4,410 | 26.0 | 2,033 | 108 | 2,100 | 111 | 1,916 | 102 |
| 24 | South Carolina | 3,906 | 30.5 | 1,801 | 95 | 1,927 | 102 | 1,742 | 92 |
| 25 | Colorado | 3,813 | 15.2 | 1,758 | 93 | 1,660 | 88 | 1,560 | 83 |
| 26 | Kentucky | 3,733 | 8.0 | 1,721 | 91 | 1,524 | 81 | 1,470 | 78 |
| 27 | Connecticut | 3,445 | 11.5 | 1,588 | 84 | 1,452 | 77 | 1,382 | 73 |
| 28 | Oklahoma | 3,376 | 7.5 | 1,556 | 82 | 1,372 | 73 | 1,326 | 70 |
| 29 | Oregon | 2,877 | 2.0 | 1,326 | 70 | 1,109 | 59 | 1,098 | 58 |
| 30 | Mississippi | 2,877 | 36.5 | 1,326 | 70 | 1,484 | 79 | 1,330 | 71 |
| 31 | Iowa | 2,549 | 2.5 | 1,175 | 62 | 988 | 52 | 976 | 52 |
| 32 | Arkansas | 2,529 | 16.0 | 1,166 | 62 | 1,109 | 59 | 1,039 | 55 |
| 33 | Kansas | 2,529 | 6.5 | 1,166 | 62 | 1,018 | 54 | 988 | 52 |
| 34 | Utah | 1,991 | 1.0 | 918 | 49 | 760 | 40 | 756 | 40 |
| 35 | New Mexico | 1,968 | 37.7 | 907 | 48 | 1,024 | 54 | 917 | 49 |
| 36 | West Virginia | 1,722 | 3.0 | 794 | 42 | 670 | 36 | 661 | 35 |
| 37 | Nebraska | 1,556 | 4.0 | 717 | 38 | 612 | 32 | 600 | 32 |
| 38 | Hawaii | 1,345 | 2.5 | 620 | 33 | 521 | 28 | 515 | 27 |
| 39 | New Hampshire | 1,333 | 2.0 | 615 | 33 | 514 | 27 | 509 | 27 |
| 40 | Nevada | 1,303 | 8.0 | 601 | 32 | 532 | 28 | 513 | 27 |
| 41 | Maine | 1,271 | 1.0 | 586 | 31 | 485 | 26 | 483 | 26 |
| 42 | Rhode Island | 1,049 | 6.0 | 484 | 26 | 420 | 22 | 409 | 22 |
| 43 | Idaho | 1,047 | 0.0 | 483 | 26 | 396 | 21 | 396 | 21 |
| 44 | Montana | 794 | 0.0 | 366 | 19 | 300 | 16 | 300 | 16 |
| 45 | Delaware | 734 | 22.0 | 338 | 18 | 338 | 18 | 312 | 17 |
| 46 | South Dakota | 714 | 0.5 | 329 | 17 | 271 | 14 | 271 | 14 |
| 47 | Alaska | 687 | 3.5 | 317 | 17 | 269 | 14 | 264 | 14 |
| 48 | North Dakota | 629 | 0.5 | 290 | 15 | 239 | 13 | 238 | 13 |
| 49 | Vermont | 591 | 1.0 | 272 | 14 | 226 | 12 | 225 | 12 |
| 50 | Wyoming | 489 | 0.0 | 225 | 12 | 185 | 10 | 185 | 10 |

0.0 Quantity more than zero but less than 0.05.

[1]For total population and blacks, Series P-25 No. 1017; Hispanics from table 5.

[2]Total sample size = population (State)/U.S. population × 124,000; males 65+ = .053 of total sample. The overall sampling rate is (.94) × 124,000/252,668,000 = .000461. The equal-weighted sample overall sampling rate is (.94) × 124,000/252,668,000 = .000461.

[3]Blacks and Hispanics will be .222 of the U.S. population in 2000. If they are oversampled at a rate of 2 to 1, the sample rate for nonblack non-Hispanics will be .00378 and the rate for blacks and Hispanics will be .00804.

[4]The design effect for oversampling at a rate of 2 to 1 is (1 + *p*) (1 − *p*/2) where *p* is the proportion of blacks and Hispanics in the State. The effective sample size (for statistics of total population) in a State is the actual size divided by the design effect for that State.

**Table 33. Number of States for which estimates would have the desired accuracy for three different sample sizes**

| Sample size | Self-weighting sample: Total States | Self-weighting sample: Male 65 and over | Minorities oversampled 2 to 1: Total States | Minorities oversampled 2 to 1: Male 65 and over |
|---|---|---|---|---|
| 50,000 | 33 | 0 | 32 | 0 |
| 100,000 | 41 | 1 | 40 | 2 |
| 200,000 | 50 | 7 | 48 | 5 |

six households. Intraclass correlations for these clusters have not been calculated for health-related variables, but other social and demographic surveys report very low intraclass correlations. RDD design effects also have a component arising from variability in weights from nonresponse adjustments and from within-household clustering. It was assumed that a design effect of 1.20 was a reasonable average for RDD samples. The current (area sample) design of the NHIS has a design effect of approximately 1.25; thus the RDD design effect relative to the current design effect is 1.20/1.25 = 0.96. (If, for example, list-assisted RDD methods were used, the relative design effect would be smaller because of a reduction in clustering.)

In addition to the current NHIS design effect, the proposed area sample has a design effect that is computed in two parts. The first component includes the subsampling of one per household and differential oversampling rates by density strata computed separately for black persons, Hispanic persons, and ‘‘others.’’ The second component accounts for the combination of the three groups with different average sampling rates, computed separately by State. The effective sample size for the area sample is then computed by dividing the actual sample size by the two design effect components for that State. The value for the first component (see NHIS Redesign Memorandum #56R) is close to 1.33 for all three groups; therefore this value is used for all three. The value of the second component was computed separately for each State; values vary from 1.35 to 1.93.

## Variance of an Unbiased Dual-Frame Estimator

A combined RDD and area sample will thus have a smaller design effect than an area sample alone and will need a smaller actual sample size to achieve the accuracy of an effective 1,000 cases needed for the reliability specified by NCHS. To derive the variance of the dual-frame sample design, one can proceed as follows. Let $\bar{x}_{a,t}$ and $\bar{x}_r$ be the mean for the telephone households in the area sample and the mean for the RDD sample, respectively, in a particular State. Let $\bar{x}_{a,n}$ be the mean for nontelephone households in the area sample. The sample estimate of a dual-frame sample can be expressed as

$$\bar{x} = (0.93)\left[f\,\bar{x}_{a,t} + (1-f)\bar{x}_r\right] + 0.07\bar{x}_{a,n}$$

where 0.93 is the expected proportion of households with telephones and $f$ is a weight. Any value for the weight between 0 and 1 will provide unbiased estimates if both $\bar{x}_{a,t}$ and $\bar{x}_r$ are unbiased, but a value should be selected that comes close to minimizing the total variance.

First derive the value of $f$ that minimizes the variance of the term in brackets. Use the following notation:

- $n_a$ and $n_r$ are the sample sizes in the area sample and RDD sample for the domain of interest
- $d_a$ and $d_r$ are the design effects in the area and RDD samples, relative to the current design
- $\sigma^2$ is the population variance of the statistic being considered. The same $\sigma^2$ applies to $\bar{x}_{a,t}$ and $\bar{x}_r$ because the populations are identical and no mode-of-interview effects are considered.

The variance of the term in the brackets is

$$\sigma^2_{x_t} = f^2\,\sigma^2_{x_{a,t}} + (1-f)^2\,\sigma^2_{x_r}$$

The expression is minimized by taking weights that are proportional to the reciprocal of the variances of each term; that is,

$$f = \frac{\sigma^2_{x_r}}{\sigma^2_{x_{a,t}} + \sigma^2_{x_r}}$$

where

$$\sigma^2_{x_r} = d_r\,\frac{\sigma^2}{n_r}$$

$$\sigma^2_{x_{a,t}} = \frac{d_a\sigma^2}{.93n_a}$$

The variance of the full estimator is then

$$V(\bar{x}) = \left(\frac{d_a}{n_a}\right)\sigma^2\,\frac{.93d_rn_a + .07d_an_r}{.93d_rn_a + d_an_r} \qquad (9)$$

This formula understates the variance slightly because it assumes no covariance between the sample of telephone households and nontelephone households. Such a covariance will exist for the area sample, but its effect should be quite small.

Given the size of the area sample, $n_a$, the goal is to determine the size of the RDD supplemental sample, $n_r$, which will provide the same level of accuracy for the composite estimator as provided by an area sample with an effective sample size of 1,000 persons for the same domain. This implies that

$$V(\bar{x}) = \frac{\sigma^2}{1{,}000}$$

Substituting this into equation 9 produces

$$1 = \left(\frac{d_a}{n_a}\right)1{,}000\left(\frac{.93d_rn_a + .07d_an_r}{.93d_rn_a + d_an_r}\right)$$

This equation can be simplified to

$$.93d_rn_a + d_an_r = 1{,}000\left(\frac{d_a}{n_a}\right)(.93d_rn_a + .07d_an_r)$$

**Table 34. State sample sizes (ranked by population size in 2000) for collapsed ages with and without oversampling of blacks and Hispanics**

| | | | | | Sample size with self-weighting sample of 50,000 households[2] | | Sample size with 50 percent budget increase, one per household and black and Hispanics oversampled at 2.59 and 3.59 to 1 | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | Actual sample[3] | | Effective sample[4] | | Effective sample[4] using 3 years of data | | |
| Rank | State | Population year 2000 (000's) | Percent black[1] | Percent Hispanic[1] | Total adults | Males 45–64 | Total adults | Males 45–64 | Total adults | Males 45–64 | Total adults | Males 45–64 | *cv* (%) |
| 1 | California | 33,500 | 8.7 | 32.0 | 11,660 | 1,714 | 9,394 | 1,381 | 5,016 | 737 | 15,049 | 2,212 | 20 |
| 2 | Texas | 20,211 | 12.1 | 30.0 | 7,731 | 1,136 | 6,234 | 916 | 3,356 | 493 | 10,067 | 1,480 | 26 |
| 3 | New York | 17,986 | 17.7 | 17.2 | 6,260 | 920 | 4,427 | 651 | 2,512 | 369 | 7,537 | 1,108 | 29 |
| 4 | Florida | 15,415 | 14.8 | 14.3 | 5,365 | 789 | 3,529 | 519 | 2,050 | 301 | 6,150 | 904 | 32 |
| 5 | Illinois | 11,580 | 17.5 | 10.9 | 4,030 | 592 | 2,577 | 379 | 1,525 | 224 | 4,576 | 673 | 37 |
| 6 | Pennsylvania | 11,502 | 9.8 | 2.8 | 4,004 | 589 | 2,016 | 296 | 1,342 | 197 | 4,025 | 592 | 39 |
| 7 | Ohio | 10,629 | 12.0 | 1.8 | 3,699 | 544 | 1,876 | 276 | 1,248 | 183 | 3,744 | 550 | 40 |
| 8 | Michigan | 9,250 | 16.2 | 3.0 | 3,220 | 473 | 1,761 | 259 | 1,128 | 166 | 3,385 | 498 | 43 |
| 9 | New Jersey | 8,546 | 15.8 | 12.1 | 2,974 | 437 | 1,906 | 280 | 1,123 | 165 | 3,369 | 495 | 43 |
| 10 | Georgia | 7,957 | 27.0 | 1.9 | 2,769 | 407 | 1,678 | 247 | 1,040 | 153 | 3,119 | 459 | 44 |
| 11 | North Carolina | 7,483 | 21.9 | 1.4 | 2,604 | 383 | 1,478 | 217 | 938 | 138 | 2,814 | 414 | 47 |
| 12 | Virginia | 6,877 | 19.4 | 3.3 | 2,394 | 352 | 1,366 | 201 | 860 | 126 | 2,580 | 379 | 49 |
| 13 | Massachusetts | 6,087 | 5.4 | 6.6 | 2,119 | 311 | 1,091 | 160 | 710 | 104 | 2,130 | 313 | 54 |
| 14 | Indiana | 5,502 | 9.3 | 2.5 | 1,915 | 282 | 952 | 140 | 638 | 94 | 1,914 | 281 | 57 |
| 15 | Missouri | 5,383 | 11.1 | 1.6 | 1,874 | 275 | 936 | 138 | 627 | 92 | 1,882 | 277 | 57 |
| 16 | Maryland | 5,274 | 27.9 | 3.3 | 1,836 | 270 | 1,149 | 169 | 702 | 103 | 2,106 | 310 | 54 |
| 17 | Tennessee | 5,266 | 16.8 | .9 | 1,833 | 269 | 970 | 143 | 634 | 93 | 1,903 | 280 | 57 |
| 18 | Washington | 4,991 | 2.3 | 6.0 | 1,737 | 255 | 849 | 125 | 568 | 84 | 1,704 | 251 | 60 |
| 19 | Wisconsin | 4,784 | 5.7 | 2.7 | 1,665 | 245 | 792 | 116 | 543 | 80 | 1,628 | 239 | 61 |
| 20 | Arizona | 4,618 | 2.7 | 20.8 | 1,607 | 236 | 1,041 | 153 | 594 | 87 | 1,782 | 262 | 59 |
| 21 | Louisiana | 4,516 | 32.2 | 2.9 | 1,572 | 231 | 1,021 | 150 | 619 | 91 | 1,858 | 273 | 57 |
| 22 | Minnesota | 4,490 | 1.7 | 1.7 | 1,563 | 230 | 686 | 101 | 493 | 72 | 1,479 | 217 | 64 |
| 23 | Alabama | 4,410 | 25.8 | .8 | 1,535 | 226 | 899 | 132 | 565 | 83 | 1,696 | 249 | 60 |
| 24 | South Carolina | 3,906 | 30.0 | 1.1 | 1,360 | 200 | 838 | 123 | 518 | 76 | 1,554 | 228 | 63 |
| 25 | Colorado | 3,813 | 4.1 | 15.5 | 1,327 | 195 | 797 | 117 | 474 | 70 | 1,421 | 209 | 66 |
| 26 | Kentucky | 3,733 | 7.9 | .8 | 1,299 | 191 | 610 | 90 | 423 | 62 | 1,270 | 187 | 69 |
| 27 | Connecticut | 3,445 | 9.1 | 8.6 | 1,199 | 176 | 672 | 99 | 419 | 62 | 1,256 | 185 | 70 |
| 28 | Oklahoma | 3,376 | 6.8 | 3.6 | 1,175 | 173 | 579 | 85 | 388 | 57 | 1,165 | 171 | 72 |
| 29 | Oregon | 2,877 | 1.7 | 5.5 | 1,001 | 147 | 480 | 71 | 325 | 48 | 975 | 143 | 79 |
| 30 | Mississippi | 2,877 | 36.0 | .8 | 1,001 | 147 | 654 | 96 | 399 | 59 | 1,197 | 176 | 72 |
| 31 | Iowa | 2,549 | 2.3 | 1.8 | 887 | 130 | 394 | 58 | 281 | 41 | 843 | 124 | 85 |
| 32 | Arkansas | 2,529 | 15.7 | 1.1 | 880 | 129 | 461 | 68 | 303 | 45 | 909 | 134 | 82 |
| 33 | Kansas | 2,529 | 6.2 | 5.2 | 880 | 129 | 445 | 65 | 294 | 43 | 881 | 129 | 83 |
| 34 | Utah | 1,991 | .7 | 5.9 | 693 | 102 | 330 | 49 | 224 | 33 | 672 | 99 | 95 |
| 35 | New Mexico | 1,968 | 1.7 | 41.1 | 685 | 101 | 587 | 86 | 304 | 45 | 913 | 134 | 82 |
| 36 | West Virginia | 1,722 | 2.6 | .7 | 599 | 88 | 260 | 38 | 189 | 28 | 566 | 83 | 104 |
| 37 | Nebraska | 1,556 | 3.7 | 3.3 | 542 | 80 | 254 | 37 | 175 | 26 | 525 | 77 | 108 |
| 38 | Hawaii | 1,345 | 1.8 | 8.4 | 468 | 69 | 239 | 35 | 155 | 23 | 466 | 69 | 115 |
| 39 | New Hampshire | 1,333 | .8 | 1.2 | 464 | 68 | 198 | 29 | 145 | 21 | 435 | 64 | 119 |
| 40 | Nevada | 1,303 | 7.2 | 13.3 | 454 | 67 | 271 | 40 | 163 | 24 | 488 | 72 | 112 |
| 41 | Maine | 1,271 | .3 | .8 | 442 | 65 | 186 | 27 | 137 | 20 | 412 | 61 | 122 |
| 42 | Rhode Island | 1,049 | 4.3 | 6.1 | 365 | 54 | 183 | 27 | 121 | 18 | 363 | 53 | 130 |
| 43 | Idaho | 1,047 | .6 | 7.1 | 364 | 54 | 178 | 26 | 119 | 17 | 356 | 52 | 131 |
| 44 | Montana | 794 | .3 | 2.1 | 276 | 41 | 120 | 18 | 87 | 13 | 260 | 38 | 153 |
| 45 | Delaware | 734 | 21.1 | 3.0 | 255 | 38 | 148 | 22 | 93 | 14 | 278 | 41 | 148 |
| 46 | South Dakota | 714 | .3 | 1.0 | 249 | 37 | 105 | 15 | 77 | 11 | 232 | 34 | 162 |
| 47 | Alaska | 687 | 3.3 | 3.6 | 293 | 35 | 112 | 17 | 77 | 11 | 232 | 34 | 162 |
| 48 | District of Columbia | 634 | 67.8 | 7.2 | 221 | 32 | 205 | 30 | 128 | 19 | 383 | 56 | 126 |
| 49 | North Dakota | 629 | .6 | 1.0 | 219 | 32 | 93 | 14 | 68 | 10 | 205 | 30 | 173 |
| 50 | Vermont | 591 | .5 | .9 | 206 | 30 | 87 | 13 | 64 | 9 | 192 | 28 | 179 |
| 51 | Wyoming | 489 | .8 | 7.4 | 170 | 25 | 84 | 12 | 56 | 8 | 167 | 25 | 191 |

[1]For total population and blacks, Series P-25 No. 1017. For Hispanics, 1990 census figures are projected to grow 39.6 percent in each State by 2000. This percentage represents the ratio of the high 2000 projection to the 1990 figure, since the 1990 figure (P-25 No. 995) exceeded the high projection for 1990.

[2]Total adult sample size = population (State)/U.S. population × 124,000 × 0.755; males 45–64 are 0.147 of the total adult sample. The equal-weighted sample overall sampling rate is (0.94) × 124,000/252,668,000 = 0.000461.

[3]Blacks and Hispanics will be oversampled and screened as described in Memorandum #50R, using the design resulting in 25,000 sampled ''others.'' The average oversampling rates relative to the rate for ''others'' are 2.59 for blacks and 3.59 for Hispanics.

[4]There are two separate components of the design effect. Each is calculated using the standard formula found in *Survey Sampling* (20). The first component includes the subsampling of one per household and differential oversampling rates by density strata, computed separately for blacks, Hispanics, and ''others.'' The second component accounts for the combination of the three groups with different average sampling rates.This design effect will vary from State to State. The effective sample size in a State is the actual sample size divided by the two design effect components for that State. The effective sample size using 3 years of data could be reduced by an additional 19 to 26 percent to account for the clustering of the sample in the same PSU's each year. This would increase the *cv*'s by a factor of 1:1.

Solving this equation for the size of the RDD supplement yields

$$n_r = \frac{930 d_a d_r - (.93)\, d_r n_a}{d_a - 70\,(d_a^2/n_a)}$$

$$= \frac{930 d_r - (.93)\, d_r n_a^*}{1 - (70/n_a^*)} \qquad (10)$$

where $n_a^* = n_a/d_a$ is the State-specific effective size of the area sample.

Substituting the RDD design effect $d_r = 0.96$ and the area effective sample size into equation 10 only produces a positive value of $n_r$ when $n_a^* > 70$. When the area sample for a cell is less than 71, the variability in the area sample component already exceeds the total variability allowed in the composite estimator.

Thus, it is impossible for RDD supplementation to provide an unbiased composite estimator with the desired accuracy if the cell has an effective sample size from the area sample of less than 71. The reason for this is that when the sample sizes are very small, the component of variance arising from nontelephone households is so large that it exceeds the permissible variance from the total sample.

An examination of table 35 shows that, using a single year of data, RDD supplementation can help for every cell in only 24 States, but 10 of these require more than 20,000 cases. Even in California it would require 1,762 interviews. It can also be noted that when interested in estimates for all adults, it is impossible to provide an unbiased estimator using RDD supplementation in three of the States.

When 3 years' data are combined, RDD supplements can provide the desired precision for all cells in 38 States, although 3 of them require more than 20,000 interviews. In the 22 States that do not have 1,000 area sample adult cases, moderate-size RDD sample supplementation will provide sufficient precision.

To summarize, unbiased dual-frame estimators with RDD supplementation of 1 year's NHIS data are of very limited utility. With 3 years' data this approach does not help in a number of States, but it can be used in a majority of States. Table 36 presents these findings.

## Precision of Unbiased Dual-Frame Estimator With a Fixed-Size Random Digit Dialing Supplement

The next question examined was, if the same RDD resources were available in every State, in how many (and which) States would different conditions provide estimates for all adults, and for all of the eight age-by-sex cells, with coefficients of variation below 30 percent? Using the proposed design, the 16 sets for analysis were formed by the complete crossing of the following four dichotomies:

- 1,000 versus 2,000 RDD households interviewed per State.
- One adult versus all adults interviewed per RDD sample household.
- Characteristics with $p = 0.5$ versus $p = 0.1$.
- 1 year versus 3 years of data.

First, the sampling errors provided by use of the RDD sample without the area probability sample are examined. Then the accuracy of the best unbiased composite estimator is examined—where the composite combines the RDD sample with the NHIS area sample in the State. Finally, the impact of limiting the variation in the weights in the composite estimator is examined, along with the extent of bias that can be expected for such biased estimators.

The accuracy of estimates made solely from the RDD sample will be the same for every State. Thus only the accuracy of the estimator for 16 conditions need be examined: 1,000 or 2,000 households, one adult or all adults in a household, $p = 0.5$ or 0.1, and 1 year's or 3 years' data.

The variance, standard error, and coefficient of variation for each of these 16 sets, for all adults and for males 45–64 years of age, are provided in table 37. The average number of adults per household is assumed to be 1.85 (see NHIS Redesign Memorandum #66), and because the average number of adult males between ages 45–64 years per household is assumed to be one, it is assumed that there is only a trivial proportion of households with more than one male in this age range. It is therefore necessary only to include an intrahousehold correlation for the ''all adult'' cell. Based on the calculations in NHIS Redesign Memorandum #40 (4), a conservative estimate for this correlation, $\rho = 0.1$, is used.

It was also assumed that the effect of clustering is negligible for RDD samples (the number of completes per cluster and the intracluster correlation are likely to be small). The table assumes further that an equal probability sample of households is used for the RDD supplement; if the modified Waksberg method (which eliminates the need for sequential sampling, but introduces some differential weighting, see *Avoiding Sequential Sampling With Random Digit Dialing* (21)) is used, then the variation in weights will yield some increase in *cv*'s.

Table 37 shows that a telephone sample of 1,000 households will provide a *cv* less than 30 percent for all States for each of the age and sex categories. This is true for a characteristic with an occurrence of 0.10 or 0.50, with one adult interviewed per household or all adults interviewed. It is not necessary to use more than 1 year's sample or to double the number of households.

Unfortunately, these estimates of *cv* do not take into account the bias from sampling only telephone households. This bias will vary from State to State (the percentage of the 1980 population in a State that was without telephones varied from 3.3 percent to 16.7 percent) and can be considerable for some health characteristics. These biases are also likely to be greater for black persons and Hispanic persons than for the general population because a higher percentage of minorities than of white persons lack telephones.

The minimum variance for an unbiased dual-frame estimator was presented in equation 9. This variance was computed for every State for each of the 16 models. When $p = 0.5$ all of the proposed models provide unbiased composite estimators with the desired $cv < 30$ percent. In fact, with $p = 0.5$, all of the models provide estimates with $cv < 15$ percent. This is not the case for the eight conditions with $p = 0.1$; these

**Table 35. RDD supplementation sample sizes for age groups with one adult sampled per household**

| Rank | State | Population year 2000 (000's)[1] | Area effective sample[3] using 1 year of data | | RDD actual sample using 1 year of data | | Area effective sample[3] using 3 years of data | | RDD actual sample using 3 years of data | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | Total adults[2] | Males 45–64 | Total adults[2] | Males 45–64 | Total adults[2] | Males 45–64 | Total adults[2] | Males 45–64 |
| 1 | California | 33,500 | 5,016 | 737 | + | 1,762 | 15,049 | 2,212 | + | + |
| 2 | Texas | 20,211 | 3,053 | 449 | + | 3,966 | 9,160 | 1,347 | + | + |
| 3 | New York | 17,986 | 2,512 | 369 | + | 4,726 | 7,537 | 1,108 | + | + |
| 4 | Florida | 15,415 | 2,050 | 301 | + | 5,527 | 6,150 | 904 | + | 631 |
| 5 | Illinois | 11,580 | 1,525 | 224 | + | 6,850 | 4,576 | 673 | + | 2,218 |
| 6 | Pennsylvania | 11,502 | 1,342 | 197 | + | 7,558 | 4,025 | 592 | + | 2,813 |
| 7 | Ohio | 10,629 | 1,248 | 183 | + | 8,020 | 3,744 | 550 | + | 3,129 |
| 8 | Michigan | 9,250 | 1,128 | 166 | + | 8,765 | 3,385 | 498 | + | 3,551 |
| 9 | New Jersey | 8,546 | 1,123 | 165 | + | 8,804 | 3,369 | 495 | + | 3,570 |
| 10 | Georgia | 7,957 | 1,040 | 153 | + | 9,492 | 3,119 | 459 | + | 3,881 |
| 11 | North Carolina | 7,483 | 938 | 138 | 60 | 10,635 | 2,814 | 414 | + | 4,286 |
| 12 | Virginia | 6,877 | 860 | 126 | 136 | 11,887 | 2,580 | 379 | + | 4,623 |
| 13 | Massachusetts | 6,087 | 710 | 104 | 287 | 16,516 | 2,130 | 313 | + | 5,373 |
| 14 | Indiana | 5,502 | 638 | 94 | 363 | 21,690 | 1,914 | 281 | + | 5,809 |
| 15 | Missouri | 5,383 | 627 | 92 | 374 | 22,868 | 1,882 | 277 | + | 5,880 |
| 16 | Maryland | 5,274 | 702 | 103 | 295 | 16,927 | 2,106 | 310 | + | 5,418 |
| 17 | Tennessee | 5,266 | 634 | 93 | 367 | 22,086 | 1,903 | 280 | + | 5,834 |
| 18 | Washington | 4,991 | 568 | 84 | 440 | 34,428 | 1,704 | 251 | + | 6,317 |
| 19 | Wisconsin | 4,784 | 543 | 80 | 469 | 45,597 | 1,628 | 239 | + | 6,530 |
| 20 | Arizona | 4,618 | 594 | 87 | 411 | 27,949 | 1,782 | 262 | + | 6,117 |
| 21 | Louisiana | 4,516 | 619 | 91 | 383 | 23,899 | 1,858 | 273 | + | 5,937 |
| 22 | Minnesota | 4,490 | 493 | 72 | 528 | 165,993 | 1,479 | 217 | + | 7,011 |
| 23 | Alabama | 4,410 | 565 | 83 | 443 | 35,278 | 1,696 | 249 | + | 6,338 |
| 24 | South Carolina | 3,906 | 518 | 76 | 497 | 69,407 | 1,554 | 228 | + | 6,756 |
| 25 | Colorado | 3,813 | 474 | 70 | 551 | # | 1,421 | 209 | + | 7,226 |
| 26 | Kentucky | 3,733 | 423 | 62 | 617 | # | 1,270 | 187 | + | 7,903 |
| 27 | Connecticut | 3,445 | 419 | 62 | 623 | # | 1,256 | 185 | + | 7,975 |
| 28 | Oklahoma | 3,376 | 388 | 57 | 666 | # | 1,165 | 171 | + | 8,510 |
| 29 | Oregon | 2,877 | 325 | 48 | 768 | # | 975 | 143 | 24 | 10,171 |
| 30 | Mississippi | 2,877 | 399 | 59 | 651 | # | 1,197 | 176 | + | 8,312 |
| 31 | Iowa | 2,549 | 281 | 41 | 855 | # | 843 | 124 | 153 | 12,227 |
| 32 | Arkansas | 2,529 | 303 | 45 | 810 | # | 909 | 134 | 88 | 11,058 |
| 33 | Kansas | 2,529 | 294 | 43 | 828 | # | 881 | 129 | 116 | 11,515 |
| 34 | Utah | 1,991 | 224 | 33 | 1,008 | # | 672 | 99 | 327 | 18,784 |
| 35 | New Mexico | 1,968 | 304 | 45 | 806 | # | 913 | 134 | 84 | 10,985 |
| 36 | West Virginia | 1,722 | 189 | 28 | 1,151 | # | 566 | 83 | 442 | 35,042 |
| 37 | Nebraska | 1,556 | 175 | 26 | 1,227 | # | 525 | 77 | 489 | 60,021 |
| 38 | Hawaii | 1,345 | 155 | 23 | 1,372 | # | 466 | 69 | 561 | # |
| 39 | New Hampshire | 1,333 | 145 | 21 | 1,477 | # | 435 | 64 | 601 | # |
| 40 | Nevada | 1,303 | 163 | 24 | 1,314 | # | 488 | 72 | 534 | 242,780 |
| 41 | Maine | 1,271 | 137 | 20 | 1,570 | # | 412 | 61 | 632 | # |
| 42 | Rhode Island | 1,049 | 121 | 18 | 1,861 | # | 363 | 53 | 704 | # |
| 43 | Idaho | 1,047 | 119 | 17 | 1,915 | # | 356 | 52 | 715 | # |
| 44 | Montana | 794 | 87 | 13 | 4,248 | # | 260 | 38 | 904 | # |
| 45 | Delaware | 734 | 93 | 14 | 3,306 | # | 278 | 41 | 861 | # |
| 46 | South Dakota | 714 | 77 | 11 | 8,729 | # | 232 | 34 | 982 | # |
| 47 | Alaska | 687 | 77 | 11 | 8,738 | # | 232 | 34 | 982 | # |
| 48 | District of Columbia | 634 | 128 | 19 | 1,723 | # | 383 | 56 | 674 | # |
| 49 | North Dakota | 629 | 68 | 10 | # | # | 205 | 30 | 1,079 | # |
| 50 | Vermont | 591 | 64 | 9 | # | # | 192 | 28 | 1,135 | # |
| 51 | Wyoming | 489 | 56 | 8 | # | # | 167 | 25 | 1,280 | # |

+ Cells for which the area effective sample size already exceeds 1,000 and do not require RDD supplementation.

# Cells for which the area effective sample size is less than 71 and RDD cannot help achieve the desired level of accuracy.

[1]For total population, Series P-25 No. 1017.

[2]Total adult sample size = population (State)/U.S. population x 124,000 × 0.755; males 18–24 are 0.147 of the total adult sample.

[3]There are two separate components of the design effect. Each is calculated using the standard formula found in *Survey Sampling* (20). The first component includes the subsampling of one per household and differential oversampling rates by density strata, computed separately for blacks, Hispanics, and ''others.'' The second component accounts for the combination of the three groups with different average sampling rates. This design effect will vary from State to State. The effective sample size in a State is the actual sample size divided by the two design effect components for that State.

**Table 36. Number of States for which all cells meet the desired level of accuracy**

| Estimator | All 8 age and sex cells | All adults |
|---|---|---|
| 1 year area sample only | 0 | 10 |
| 1 year RDD, less than 20,000 supplement per State | 14 | 48 |
| 1 year RDD, unlimited supplement | 24 | 48 |
| 3 year area sample only | 3 | 29 |
| 3 year RDD, less than 20,000 supplement per State | 35 | 51 |
| 3 year RDD, unlimited supplement | 38 | 51 |

results can be seen in table 38. This table indicates many fewer States than in earlier tables with the desired level of accuracy. This is because the 1,000-interview requirement was based on a *cv* of 30 percent for a characteristic with $p = 0.014$, not 0.1.

Table 38 shows that the minimum condition of a $cv < 30$ percent can be achieved for most States under any of the eight conditions. It is achieved for all States when 3 years' of NHIS data are used.

The differences are much more pronounced when the number of States for which some cells will have estimates with *cv*'s exceeding 15 percent is examined. In general the results show the impact of increasing the size of the two samples. When interviewing all adults in 1,000 households, the sample size is quite similar to that when interviewing 1 adult from 2,000 households, and so are the number of States exceeding the 15-percent cutoff. Unless all adults in 2,000 households are interviewed by telephone and combined with 3 years' of NHIS data, one can expect at least 20 of the States to have some of the age-by-sex cells for which the *cv* will exceed 15 percent.

## Comparisons of Dual-Frame Estimators With Stand-Alone 1,000 Random Digit Dialing Estimator

Currently, the Centers for Disease Control and Prevention funds each State to collect health data from RDD surveys of 1,000 individuals. RDD estimators are subject to potential biases resulting from differences in a health characteristic between households with and without telephones. Therefore the mean square error and variance of dual-frame estimators were compared with the same measures of an RDD estimator based on 1,000 interviews per State. For these comparisons only 1 year of NHIS data was used assuming the proposed alpha option design. Estimates for all adults and for the six age-by-sex cells for adults were examined. A 95-percent response rate was assumed for NHIS and an 80-percent response rate for the RDD interviews.

Three different incidence rates were examined: 0.03, 0.10, and 0.30. This covers most of the incidence levels found in NHIS data and provides some measure of robustness to the results. The comparison of accuracy compared the relative root mean square errors (RRMSE's) of the two estimators. In the case of the dual-frame estimator, this is equal to the *cv*, but for the RDD estimator, it is a function of both variance and bias. *Trends in United States Telephone Coverage Across Time*

**Table 37. Variances of an RDD sample of 1,000 and 2,000 households per year for two designs, two different percentages, and for all adults and the rarest cell**

| Variance | One adult per household[1] | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | All adults | | | | 45–64 year old males[2] | | | |
| | 1 year | | 3 years | | 1 year | | 3 years | |
| | $p = .50$ | $p = .10$ | $p = .50$ | $p = .10$ | $p = 0.50$ | $p = 0.10$ | $p = 0.50$ | $p = 0.10$ |
| Variance ($n = 1{,}000$) | 0.00030 | 0.00011 | 0.00010 | 0.00004 | 0.00204 | 0.00073 | 0.00068 | 0.00024 |
| Standard error | 0.01732 | 0.01039 | 0.01000 | 0.00600 | 0.04518 | 0.02711 | 0.02608 | 0.01565 |
| *cv* | 0.03464 | 0.10392 | 0.02000 | 0.06000 | 0.09035 | 0.27105 | 0.05216 | 0.15649 |
| Variance ($n = 2{,}000$) | 0.00015 | 0.00005 | 0.00005 | 0.00002 | 0.00102 | 0.00037 | 0.00034 | 0.00012 |
| Standard error | 0.01225 | 0.00735 | 0.00707 | 0.00424 | 0.03194 | 0.01917 | 0.01844 | 0.01107 |
| *cv* | 0.02449 | 0.07348 | 0.01414 | 0.04243 | 0.06389 | 0.19166 | 0.03689 | 0.11066 |
| | All adults per household | | | | | | | |
| Variance ($n = 1{,}000$) | 0.00015 | 0.00005 | 0.00005 | 0.00002 | 0.00092 | 0.00033 | 0.00031 | 0.00011 |
| Standard error | 0.01211 | 0.00727 | 0.00699 | 0.00419 | 0.03032 | 0.01819 | 0.01751 | 0.01050 |
| *cv* | 0.02422 | 0.07265 | 0.01398 | 0.04195 | 0.06064 | 0.18192 | 0.03501 | 0.10503 |
| Variance ($n = 2{,}000$) | 0.00007 | 0.00003 | 0.00002 | 0.00001 | 0.00046 | 0.00017 | 0.00015 | 0.00006 |
| Standard error | 0.00856 | 0.00514 | 0.00494 | 0.00297 | 0.02144 | 0.01286 | 0.01238 | 0.00743 |
| *cv* | 0.01712 | 0.05137 | 0.00989 | 0.02966 | 0.04288 | 0.12864 | 0.02476 | 0.07427 |

[1]The design effect for sampling one adult per household is assumed to be 1.2. This is based on results of 18,500 interviews from the National Household Education Survey.

[2]Males 45 to 64 years old are the rarest age-by-sex cell and make up 14.7 percent of all adults (see Memorandum #56R).

[3]From Memorandum #66 the average number of adults per household is 1.85. Therefore this design anticipates interviews with 1,850 (3,700) adults. For the 45- to 64-year-old males there is no noticeable effect of clustering since there is only one such male in almost all households. For all adults there is an additional design effect to consider. Based on the calculations in Memorandum #40, a conservative estimate for the intrahousehold correlation is 0.1. This value was used for the bottom left part of this table.

NOTE: These variances are independent of the area sample size in the State.Therefore these estimates apply to all States using either the current or proposed sample designs.

**Table 38. The number of States, under the proposed redesign and *p* = 0.1, for which the unbiased composite estimates for certain cells will have *cv's* greater than 30 or 15 percent**

| Sample | Number of States with *cv* > 30 percent: Total adults | Number of States with *cv* > 30 percent: Males 45–64 | Number of States with *cv* > 15 percent: Total adults | Number of States with *cv* > 15 percent: Males 45–64 |
|---|---|---|---|---|
| One adult per 1,000 households, 1 year | 0 | 8 | 1 | 47 |
| One adult per 2,000 households, 1 year | 0 | 3 | 1 | 42 |
| All adults in 1,000 households, 1 year | 0 | 3 | 1 | 42 |
| All adults in 2,000 households, 1 year | 0 | 2 | 0 | 26 |
| One adult per 1,000 households, 3 years | 0 | 0 | 0 | 34 |
| One adult per 2,000 households, 3 years | 0 | 0 | 0 | 24 |
| All adults in 1,000 households, 3 years | 0 | 0 | 0 | 23 |
| All adults in 2,000 households, 3 years | 0 | 0 | 0 | 11 |

*and Subgroups* (22) provide estimates of the differences between telephone and nontelephone households for selected NHIS characteristics. The largest and smallest percent bias shown for (approximately) each of the three *p* values previously mentioned was determined (and the national bias was assumed to apply to all States). (These biases assume that no poststratification by income is done for the RDD estimator. To the extent that this is done, the impact of these biases will be reduced.) The minimum relative bias for all three incidence levels was near zero. The maximum bias varied by incidence rate. The maximum bias for $p = 0.03$ was 3.6 percent, for $p = 0.10$ it was 28.9 percent, and for $p = 0.30$ it was 16.7 percent.

Table 39 shows, for $p = 0.10$ and 0.30, the size RDD supplement needed in a State to combine with the NHIS area-permit sample and produce accuracy equivalent to that of a stand-alone RDD sample of 1,000 households. The key determinant of the size RDD supplement required by the dual-frame estimator is the level of the bias. For characteristics with small biases, the size supplements for a State are almost identical, regardless of incidence rate and of whether all six cells are wanted or only one. In fact, when the bias is zero, they are identical. For $p = 0.03$ the largest bias was only 3.6 percent; thus, the results closely matched the low-bias results in table 39.

When the bias is small, a dozen States do not require any telephone supplementation. That is, the NHIS sample in those States is more accurate than an RDD sample of 1,000 households. In 36 of the 51 States (including the District of Columbia), the entire supplement for all age and sex cells would be less than 1,000 households. In another 13 States, the supplement would have to be larger than 1,000; for 1 State it would exceed 7,000 households.

For two States, Alaska and Wyoming, no telephone sample could reduce the variability of the estimator sufficiently to match the accuracy of 1,000 telephone interviews. This is because the small number of NHIS interviews that must represent all of the nontelephone households in the State receive such a large weight that the design effect from differential weights keeps the variance of the dual-frame estimator larger than that of the 1,000 RDD interview estimator. To a lesser extent, it is this same variation in weights that causes the other 13 States' supplements to exceed 1,000 households.

For these 13 States whose supplements exceed 1,000 households, it is important to remember that the extra interviews are to ensure the desired level of accuracy, regardless of the actual telephone or nontelephone bias in the State. The stand-alone RDD estimator will achieve the desired accuracy only if its bias is no greater than that reflected by the national estimates. Given that this survey collects a great number of variables, each of which has different biases (which may vary over time), these are likely to be items, and States in which biases are much greater than would be anticipated from studies such as *Trends in United States Telephone Coverage Across Time and Subgroups* (22).

For characteristics with high biases, the supplemental sample sizes for the dual-frame estimator are even smaller. This is because the larger bias inflates the RRMSE of the stand-alone RDD estimator. For a given incidence rate and bias, the main determinant of the supplement size is the telephone coverage rate, not the NHIS sample size. In all of the high-bias situations, at least 45 of the States required supplements smaller than 1,000 households, and at least 20 needed no supplementation at all. When only one cell per State was needed, all States required supplements smaller than 1,000 households.

The next analysis compared the precision (variance) of the stand-alone RDD estimator with a minimum

**Table 39. Number of States, by the size RDD supplement needed to derive a dual-frame NHIS estimator of accuracy similar to stand-alone 1,000 RDD interviews**

| Size of RDD supplemental sample | Low bias: All six collapsed age/sex cells: *p* = 0.10 (Bias = 1.6) | Low bias: All six collapsed age/sex cells: *p* = 0.30 (Bias = 0.0%) | Low bias: All adults one combined cell: *p* = 0.10 (Bias = 1.6%) | Low bias: All adults one combined cell: *p* = 0.30 (Bias = 0.0%) | High bias: All six collapsed age/sex cells: *p* = 0.10 (Bias = 28.9%) | High bias: All six collapsed age/sex cells: *p* = 0.30 (Bias = 16.7%) | High bias: All adults one combined cell: *p* = 0.10 (Bias = 28.9%) | High bias: All adults one combined cell: *p* = 0.30 (Bias = 16.7%) |
|---|---|---|---|---|---|---|---|---|
| None needed | 12 | 12 | 12 | 12 | 20 | 25 | 33 | 35 |
| 1–999 | 24 | 24 | 24 | 24 | 25 | 22 | 18 | 16 |
| > 1,000 | 13 | 13 | 13 | 13 | 6 | 4 | 0 | 0 |
| No amount can help | 2 | 2 | 2 | 2 | 0 | 0 | 0 | 0 |

variance (but potentially biased) dual-frame estimator. This dual-frame estimator is different from that considered in earlier tables. All interviews are given equal weights to minimize the variance. This underrepresents households without telephones, however, because they are included only in the sample of NHIS households. Thus the estimator will only partially reduce the bias that is usually present in the stand-alone RDD estimator. (The percent reduction in bias is a function of the relative sample sizes from NHIS and RDD, independent of the actual bias in the RDD estimate.) By restricting the comparison to matching the estimators' variance, the RDD supplement will be between 0 and 1,000 interviews for each State. If the NHIS area sample in the State provides an estimate with smaller variance than that of the stand-alone RDD estimate, no RDD supplement is needed. If the NHIS area sample is practically nonexistent, the dual-frame estimator will resemble the stand-alone estimator and require 1,000 interviews.

Table 40 summarizes the results. No RDD supplement is required in 12 States, because the NHIS area sample in the States provides smaller variances than the stand-alone RDD sample. These are the same 12 States that did not require RDD supplementation in table 39 under the assumption of low nontelephone bias. When no supplementation is used, the bias is obviously completely eliminated because the NHIS sample is unbiased. For another 15 States, the supplemental sample size would be fewer than 500 interviews. The bias of the dual-frame estimator in these States will be from 46 to 83 percent less than that of the stand-alone estimator. In the remaining 24 States, where the supplement would require more than 500 interviews, the bias reduction is from 6 to 43 percent.

For the final analysis, two variables were chosen that are of concern as part of the Healthy People 2000 Objectives: Percent of persons currently smoking cigarettes and percent of persons under 65 years of age without private health insurance. Estimates from the 1985 NHIS are available for both of these variables for telephone and nontelephone households from *Trends in United States Telephone Coverage Across Time and Subgroups* (22). The latter variable represents a worst-case scenario for the stand-alone RDD estimator, because it has the highest relative bias (telephone households relative to all households) for variables reported with similar incidence rates. Cigarette smoking has a more moderate bias. For variables without any telephone coverage bias, examination of mean square errors would be equivalent to the variances used in this analysis. Information for 1985 (in percent) on these two variables, at the national level, is reported in table 41.

For each State, table 42 compares the RDD stand-alone estimator with a dual-frame estimator for the following characteristics: Coverage rate (telephone coverage rates are from the 1980 census, and NHIS coverage rate is assumed to be 100 percent); response rate (a weighted average of RDD and NHIS response rates weighted by the two actual sample sizes); variance (assuming $p = 0.30$, the same relative relationship between the variances would hold if any other incidence rate is assumed); mean square error (MSE) for currently smoking cigarettes; and MSE for persons under 65 years of age without private health insurance.

The dual-frame estimator used for the variance comparison is the minimum variance estimator also used in table 40. If the characteristic to be estimated was known not to suffer from significant bias, a minimum variance (potentially biased) dual-frame estimator could be used when comparing MSE's. If the characteristic (such as the two preceding) was biased, it is not obvious whether it would be better to use the minimum variance or unbiased versions of the dual-frame estimator. For this analysis the version with the best variance or MSE (depending on the table column) is used based on the telephone coverage rate and NHIS sample size in each State. In all but one case, the unbiased dual-frame estimator has a smaller MSE than the potentially biased dual-frame estimator. The one exception is for people currently smoking cigarettes in North Dakota. Cigarette smoking is subject to a smaller bias than percent without private health insurance (4.5, compared with 16.7, percent relative bias). North Dakota also has the third-smallest NHIS sample size combined with a high telephone-coverage rate. Thus, the overall bias is small, the bias in the State will be less than average, and the sample of NHIS households with telephones is small.

Examination of table 42 shows that for every State, the use of a dual-frame estimator (including NHIS data) can improve the estimates that would be obtained from a stand-alone RDD survey of 1,000 households. In general, the dual-frame estimator used in comparing variances in table 40 is not the same dual-frame estimator used in

**Table 40. Summary of the number of States and percent reduction in bias corresponding to different size RDD supplementation of a dual-frame estimator**

| Dual-frame RDD supplemental sample size | Number of States | Percent of reduction in bias of RDD with dual frame |
|---|---|---|
| 0 | 12 | 100 |
| 1–500 | 15 | 46–83 |
| 501–1,000 | 24 | 6–43 |

**Table 41. Differences between telephone and nontelephone households for percent of persons who smoke, and persons under 65 years old without private health insurance**

| Characteristic | Nontelephone households | Telephone households | All households |
|---|---|---|---|
| Persons who currently smoke cigarettes | 49.6 | 28.8 | 30.1 |
| Persons under 65 years old without private health insurance | 64.9 | 19.8 | 23.1 |

**Table 42. Comparison of 1,000 RDD interviews alone (sampling one adult per household), with dual-frame estimators (from proposed design), and *p* = 30 percent**

| Rank | State | Projected population year 2000 (000's)[1] | Actual NHIS sample using 1 year's data all adults[1] | Coverage rate (%) | | Response rate (%) | | Variance (× 1,000)[2] | | Persons who currently smoke cigarettes MSE (× 1,000)[3] | | Persons under 65 years old without private health insurance MSE (× 1,000)[3] | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | RDD | Dual frame | RDD | Dual frame | RDD | Dual frame | RDD | Dual frame | RDD | Dual frame |
| 1 | California | 33,500 | 9,394 | 95 | 100 | 80 | 93 | 0.252 | 0.036 | 0.372 | 0.036 | 0.778 | 0.030 |
| 2 | Texas | 20,211 | 5,673 | 91 | 100 | 80 | 94 | 0.252 | 0.054 | 0.634 | 0.054 | 2.010 | 0.046 |
| 3 | New York | 17,986 | 4,427 | 93 | 100 | 80 | 88 | 0.252 | 0.063 | 0.494 | 0.063 | 1.353 | 0.053 |
| 4 | Florida | 15,415 | 3,529 | 90 | 100 | 80 | 92 | 0.252 | 0.073 | 0.669 | 0.074 | 2.174 | 0.062 |
| 5 | Illinois | 11,580 | 2,577 | 95 | 100 | 80 | 89 | 0.252 | 0.089 | 0.370 | 0.090 | 0.768 | 0.076 |
| 6 | Pennsylvania | 11,502 | 2,016 | 96 | 100 | 80 | 91 | 0.252 | 0.097 | 0.327 | 0.098 | 0.564 | 0.083 |
| 7 | Ohio | 10,629 | 1,876 | 94 | 100 | 80 | 90 | 0.252 | 0.101 | 0.403 | 0.103 | 0.923 | 0.087 |
| 8 | Michigan | 9,250 | 1,761 | 96 | 100 | 80 | 90 | 0.252 | 0.107 | 0.325 | 0.108 | 0.555 | 0.092 |
| 9 | New Jersey | 8,546 | 1,906 | 95 | 100 | 80 | 87 | 0.252 | 0.107 | 0.349 | 0.109 | 0.669 | 0.092 |
| 10 | Georgia | 7,957 | 1,678 | 88 | 100 | 80 | 88 | 0.252 | 0.112 | 0.853 | 0.117 | 3.037 | 0.099 |
| 11 | North Carolina | 7,483 | 1,478 | 89 | 100 | 80 | 88 | 0.252 | 0.119 | 0.772 | 0.124 | 2.656 | 0.105 |
| 12 | Virginia | 6,877 | 1,366 | 92 | 100 | 80 | 90 | 0.252 | 0.124 | 0.548 | 0.129 | 1.604 | 0.109 |
| 13 | Massachusetts | 6,087 | 1,091 | 96 | 100 | 80 | 88 | 0.252 | 0.136 | 0.331 | 0.140 | 0.584 | 0.118 |
| 14 | Indiana | 5,502 | 952 | 93 | 100 | 80 | 88 | 0.252 | 0.143 | 0.441 | 0.150 | 1.104 | 0.127 |
| 15 | Missouri | 5,383 | 936 | 95 | 100 | 80 | 89 | 0.252 | 0.144 | 0.380 | 0.150 | 0.815 | 0.127 |
| 16 | Maryland | 5,274 | 1,149 | 96 | 100 | 80 | 86 | 0.252 | 0.137 | 0.329 | 0.141 | 0.576 | 0.119 |
| 17 | Tennessee | 5,266 | 970 | 90 | 100 | 80 | 88 | 0.252 | 0.143 | 0.674 | 0.154 | 2.198 | 0.130 |
| 18 | Washington | 4,991 | 849 | 94 | 100 | 80 | 86 | 0.252 | 0.150 | 0.392 | 0.157 | 0.871 | 0.133 |
| 19 | Wisconsin | 4,784 | 792 | 97 | 100 | 80 | 88 | 0.252 | 0.153 | 0.299 | 0.157 | 0.435 | 0.133 |
| 20 | Arizona | 4,618 | 1,041 | 89 | 100 | 80 | 88 | 0.252 | 0.147 | 0.758 | 0.161 | 2.591 | 0.136 |
| 21 | Louisiana | 4,516 | 1,021 | 89 | 100 | 80 | 88 | 0.252 | 0.145 | 0.762 | 0.157 | 2.613 | 0.133 |
| 22 | Minnesota | 4,490 | 686 | 97 | 100 | 80 | 87 | 0.252 | 0.158 | 0.302 | 0.164 | 0.449 | 0.139 |
| 23 | Alabama | 4,410 | 899 | 87 | 100 | 80 | 88 | 0.252 | 0.150 | 0.973 | 0.168 | 3.603 | 0.142 |
| 24 | South Carolina | 3,906 | 838 | 87 | 100 | 80 | 86 | 0.252 | 0.155 | 0.935 | 0.176 | 3.425 | 0.149 |
| 25 | Colorado | 3,813 | 797 | 94 | 100 | 80 | 84 | 0.252 | 0.161 | 0.422 | 0.172 | 1.013 | 0.146 |
| 26 | Kentucky | 3,733 | 610 | 88 | 100 | 80 | 86 | 0.252 | 0.167 | 0.861 | 0.194 | 3.074 | 0.164 |
| 27 | Connecticut | 3,445 | 672 | 97 | 100 | 80 | 86 | 0.252 | 0.168 | 0.302 | 0.175 | 0.450 | 0.148 |
| 28 | Oklahoma | 3,376 | 579 | 92 | 100 | 80 | 85 | 0.252 | 0.172 | 0.524 | 0.192 | 1.490 | 0.163 |
| 29 | Oregon | 2,877 | 480 | 83 | 100 | 80 | 85 | 0.252 | 0.181 | 1.449 | 0.240 | 5.839 | 0.203 |
| 30 | Mississippi | 2,877 | 654 | 93 | 100 | 80 | 86 | 0.252 | 0.170 | 0.436 | 0.186 | 1.080 | 0.158 |
| 31 | Iowa | 2,549 | 394 | 96 | 100 | 80 | 85 | 0.252 | 0.188 | 0.315 | 0.205 | 0.511 | 0.173 |
| 32 | Arkansas | 2,529 | 461 | 87 | 100 | 80 | 85 | 0.252 | 0.185 | 0.933 | 0.233 | 3.414 | 0.197 |
| 33 | Kansas | 2,529 | 445 | 95 | 100 | 80 | 85 | 0.252 | 0.186 | 0.363 | 0.206 | 0.735 | 0.175 |
| 34 | Utah | 1,991 | 330 | 95 | 100 | 80 | 84 | 0.252 | 0.199 | 0.379 | 0.231 | 0.809 | 0.195 |
| 35 | New Mexico | 1,968 | 587 | 86 | 100 | 80 | 87 | 0.252 | 0.185 | 1.098 | 0.238 | 4.189 | 0.202 |
| 36 | West Virginia | 1,722 | 260 | 89 | 100 | 80 | 84 | 0.252 | 0.205 | 0.745 | 0.286 | 2.532 | 0.242 |
| 37 | Nebraska | 1,556 | 254 | 96 | 100 | 80 | 83 | 0.252 | 0.208 | 0.314 | 0.240 | 0.507 | 0.203 |
| 38 | Hawaii | 1,345 | 239 | 95 | 100 | 80 | 84 | 0.252 | 0.212 | 0.357 | 0.260 | 0.707 | 0.220 |
| 39 | New Hampshire | 1,333 | 198 | 94 | 100 | 80 | 83 | 0.252 | 0.215 | 0.398 | 0.276 | 0.900 | 0.234 |
| 40 | Nevada | 1,303 | 271 | 90 | 100 | 80 | 84 | 0.252 | 0.211 | 0.663 | 0.301 | 2.146 | 0.254 |
| 41 | Maine | 1,271 | 186 | 93 | 100 | 80 | 82 | 0.252 | 0.216 | 0.491 | 0.301 | 1.337 | 0.255 |
| 42 | Rhode Island | 1,049 | 183 | 95 | 100 | 80 | 83 | 0.252 | 0.220 | 0.350 | 0.283 | 0.672 | 0.240 |
| 43 | Idaho | 1,047 | 178 | 93 | 100 | 80 | 83 | 0.252 | 0.221 | 0.471 | 0.318 | 1.242 | 0.269 |
| 44 | Montana | 794 | 120 | 92 | 100 | 80 | 82 | 0.252 | 0.228 | 0.520 | 0.386 | 1.473 | 0.326 |
| 45 | Delaware | 734 | 148 | 95 | 100 | 80 | 82 | 0.252 | 0.227 | 0.356 | 0.317 | 0.704 | 0.268 |
| 46 | South Dakota | 714 | 105 | 94 | 100 | 80 | 82 | 0.252 | 0.231 | 0.426 | 0.376 | 1.032 | 0.318 |
| 47 | Alaska | 687 | 112 | 83 | 100 | 80 | 81 | 0.252 | 0.231 | 1.459 | 0.616 | 5.887 | 0.521 |
| 48 | District of Columbia | 634 | 205 | 95 | 100 | 80 | 82 | 0.252 | 0.219 | 0.351 | 0.278 | 0.679 | 0.235 |
| 49 | North Dakota | 629 | 93 | 96 | 100 | 80 | 81 | 0.252 | 0.233 | 0.328 | 0.309 | 0.572 | 0.291 |
| 50 | Vermont | 591 | 87 | 93 | 100 | 80 | 81 | 0.252 | 0.234 | 0.449 | 0.426 | 1.139 | 0.360 |
| 51 | Wyoming | 489 | 84 | 92 | 100 | 80 | 81 | 0.252 | 0.236 | 0.554 | 0.515 | 1.634 | 0.435 |

[1]The actual NHIS sample sizes come from table 1 of Memorandum #56R.

[2]The RDD design effect for sampling one adult per household is assumed to be 1:2. This is based on results of 18,500 interviews from the National Household Education Survey. The State-specific design effects for the NHIS were computed in Memorandum #56R. The dual-frame estimator used for the variance comparison is the minimum variance (potentially biased) estimator also used in table 40.

[3]The dual-frame estimator used in these columns is the minimum of these unbiased and minimum variance estimators.

comparing mean square errors (except for North Dakota). The rationale is that, given either basis for choosing an estimator—minimizing variance or minimizing MSE—it is possible to construct a dual-frame estimator with significantly better properties than those of the stand-alone RDD estimator.

The bias discussed in this chapter arises from differences in the health characteristics of households with and without telephones. A second source of bias is adjusting for nonresponse. An estimator based on a higher response rate will minimize the impact of this second source of nonresponse. The following summarizes the findings comparing stand-alone RDD and dual-frame estimation:

- The coverage rate is from 3 to 17 percent higher.
- The response rate is from 1 to 14 percent higher.
- The dual-frame variance is always lower; for 12 of the States, it is less than one-half that of the RDD estimator.
- The dual-frame mean square error is always lower. For cigarette smokers in 32 States, and for those without private health insurance in every State, it is less than one-half that of the RDD estimator.

# Chapter 12. Empirical Comparison of Model-Dependent Estimators

This chapter describes an empirical comparison of model-dependent estimators that could potentially be used to produce State-level estimates from NHIS. Five model-dependent estimators were examined along with the direct design-unbiased estimator. Their point estimates for each State as well as their estimated mean square errors (MSE's) were compared using the data collected in the 1988 NHIS. The five estimators were the synthetic estimator originally developed for NHIS (23), a composite estimator of the synthetic and direct estimators (24), and three versions of the generalized synthetic estimator (GSE) (25).

The GSE loosens the restrictive assumptions on which the traditional synthetic estimator depends in order to produce unbiased estimates. In particular, although the synthetic estimator requires that the mean for a population subgroup (for example, white females 18–44 years of age) be the same in every State (if this assumption approximately holds, then the synthetic estimator will be approximately unbiased), the GSE requires only that they have the same expectation. The Bayesian statistical term for this is that the State/subgroup means for a particular variable be exchangeable (26). The GSE also allows one to incorporate prior information into the estimator and allows the subgroup means in some States to have smaller standard deviations than in others.

NHIS collects data on dozens of health items. For this analysis two items were examined: The mean of a count variable and a proportion. The count variable was the average number of doctor visits in the past 12 months, and the proportion was the percentage with a self-perceived poor health status. The verbatim questions on the NHIS questionnaire were as follows:

- During the past 12 months, how many times did (person's name) see or talk to a medical doctor or assistant? (Do not count doctors seen while an overnight patient in a hospital.)
- Would you say (your/his/her) health in general is excellent, very good, good, fair, or poor?

## Estimators

The exact formulas for the six estimators are described next.

**1. Direct, design-unbiased, inflation estimator**

$$\hat{y}_{1i} = \sum_j \sum_k w_{ijk}\, y_{ijk} \Big/ \sum_j \sum_k w_{ijk} \tag{11}$$

where

$y_{ijk}$ = response for the $k$th respondent, in subgroup $j$, in State $i$

$w_{ijk}$ = weight for the $k$th respondent, in subgroup $j$, in State $i$

$i = 1, 2, \ldots I$

$j = 1, 2, \ldots J$

$k = 1, 2, \ldots N_{ij}$

**2. Synthetic estimator.** Sixteen subgroups were used, four age categories (0–19 years, 20–44 years, 45–64 years, 65 years or over), two race categories (black, other than black), and two sex categories (male, female).

$$\hat{y}_{2i\cdot} = \sum_{j=1}^{16} \frac{N_{ij}\, \bar{y}_{.j.}}{N_{i.}} \tag{12}$$

where

$N_{ij}$ = 1990 census total population for subgroup $j$ in State $j$

$$\bar{y}_{.j.} = \sum_i \sum_k w_{ijk}\, y_{ijk} \Big/ \sum_j \sum_k w_{ijk}$$

$N_{i.}$ = 1990 Census total population for State $i$.

The predictive version of the synthetic estimator was also examined. This uses the observed values of the sampled respondents and the preceding synthetic estimator only for the nonsampled cases.

As with almost all national household surveys, the sampling fraction for NHIS is quite small, resulting in insignificant differences between the two forms of the synthetic estimator. Thus, only the results from the more common version given here are reported.

**3. Composite estimator.** This is a linear combination of the first two estimators where the weights are proportional to the mean square errors of the two estimators.

$$\hat{y}_{3i.} = t_i\, \bar{y}_{1i.} + (1 - t_i)\, \hat{y}_{2i} \tag{13}$$

where

$$t_i = \frac{\text{MSE}\,(\hat{y}_{2i.})}{\text{Var}\,(\bar{y}_{1i.}) + \text{MSE}\,(\hat{y}_{2i.})}$$

Because many States contain only one or two sampled PSU's, direct estimated variances would be highly unstable. Thus, for these analyses, the $\text{Var}\left(\hat{y}_{1i}\right)$ were developed separately for different types of questions (see the first section of chapter 3). The calculation of $\text{MSE}\left(\hat{y}_{2i.}\right)$ is described in the second section of this chapter.

Before discussing the final three estimators, it is necessary to discuss the equation for the GSE. (See *Small Area Estimation: A Bayesian Perspective* (25) and Small Area Estimation for the National Health Interview Survey (27) for more detail.) GSE is derived based on two assumptions. The first is that conditional on the mean and variance for State $i$ and subgroup $j$, the $Yijk$ are exchangeable within their State/subgroup and are independent between States/subgroups. The second assumption is that for each State/subgroup, the mean and variance are exchangeable across States, and they are independent between subgroups. Under these assumptions, GSE is the posterior mean for State $i$, given the sample $s$ containing data $y$.

$$E\left(\overline{Y_{..}} \,|\, (s,y)\right) =$$

$$\left[ n_{i.}\, \bar{y}_{i..} + \sum_{j=1}^{J} (N_{ij} - n_{ij}) \cdot \frac{\hat{\mu}_{.j}\, v_p + m_j^{*} v_s}{v_p + v_s} \right] \Big/ N_{i\cdot} \tag{14}$$

where

$n_i$ = sample size in State $i$

$\bar{y}_{i..} = \sum_i \sum_j w_{ijk} y_{ijk} / \sum_i \sum_j w_{ijk}$

$n_{ij}$ = sample size in subgroup $j$ and State $j$

$\hat{\mu}_{.j}$ = best linear unbiased estimate (BLUE) for the mean of subgroup $j$

$v_p$ = a priori variance on the prior mean for subgroup $j$

$m_j^*$ = prior mean for subgroup $j$

$v_s$ = expected sampling variance of the sample mean for subgroup $j$

As with any Bayesian prediction of finite populations, the GSE uses the observed data and estimates for only the unobserved values. The right-hand side of equation 14 is also seen to contain a weighted average of a sample estimate and a prior mean separately for each subgroup. If the a priori variance, $v_p$, is much larger than the expected sampling error, $v_s$, GSE reduces to

$$E(\bar{Y}_{i..}|(s,y)) = \left[ n_{i.}\,\bar{y}_{i..} + \sum_{j=1}^{J} (N_{ij} - n_{ij})\,\hat{\mu}_{.j} \right] / N_{i.} \tag{15}$$

In general, the BLUE for the mean of subgroup $j$ is a weighted average of every State average for subgroup $j$, $\bar{y}_{ij.}$, where the weights are inversely proportional to the sampling variance for that State/subgroup. If for any subgroup the elemental variances ($\sigma^2_{ij}$) are assumed to be equal in all States, the BLUE is simply equal to $\bar{y}_{.j.}$. In this case the GSE is equal to (the predictive version of) the synthetic estimator.

The remaining three estimators, all forms of the GSE, can now be described.

**4. GSE with heteroscedastic variances.** It is desired to examine the robustness of the traditional synthetic estimator in view of the assumption that the variances are equal across small areas. It can be shown that for proportions the State/subgroup variances must be homoscedastic if their means are assumed equal. Thus, this estimator applies only to the doctor visit variable. Assume the mean response for a given subgroup is the same for all small areas (to reflect the assumption used by the synthetic estimator) and that the prior variance is diffuse (at least relative to $v_s$). Assume that within a subgroup, there are differences in the distributions of doctor visits between those in central cities and those who live elsewhere. Estimates for subgroups in States that have either almost all of their population in central cities or not in central cities would be subject to less variation than for subgroups in States with a mixture. In particular, assume the mean and variance of each small area/subgroup is

$$[\mu_{ij},\ \sigma^2_{ij}\, z_{ij}\,(1 - z_{ij})]$$

where

$z_{ij}$ = the proportion of the State's black (or nonblack) population living in central cities (capped at 0.95)

Thus,

$$\overset{\triangle}{y}_{4i.} = \left[ n_{i.}\,\bar{y}_{i.} + \sum_{j=1}^{J} (N_{ij} - n_{ij})\,\hat{\mu}_j \right] / N_{i.} \tag{16}$$

where

$$\hat{\mu}_{.j} = \left[ \sum_{i=1}^{I} \frac{n_{ij}\,\bar{y}_{ij}}{z_{ij}\,(1 - z_{ij})} \right] \Bigg/ \left[ \sum_{i=1}^{I} \frac{n_{ij}}{z_{ij}\,(1 - z_{ij})} \right]$$

**5. GSE with balanced prior and sampling variances (16 subgroups).** A basic requirement for all Bayes estimators is that the weights associated with the BLUE and the prior mean must be nonnegative. Thus, $v_p$ and $v_s$ must be nonnegative. Further, when their exact formulas are examined, the relative magnitudes of these two terms are also constrained (24, 26), partially as a function of how evenly the population is spread across all States. Define $b_j$ as the midpoint in the allowable range in the relative sizes of the prior and sampling variances for each subgroup $j$. It is interesting to note that when the midpoint in the possible range of relationships for the two variances is chosen, the resulting estimator gives greater weight to the prior mean. This would be most appropriate when the prior is based on more data (or greater knowledge) than the current survey. In this empirical examination, subgroups are spread across 51 States (the following formula is a function of I-1 and $(I\text{-}1)^2$, where $I = 51$), and the prior mean is simply estimated by the mean of the previous year's NHIS.

$$\overset{\triangle}{y}_{5i} = \left\{ n_i \bar{y}_i + \sum_{j=1}^{J} (N_{ij} - n_{ij}) \cdot \left[ \frac{\hat{\mu}_{.j}\,(51/50_{b_j} - 1/50) + m_j^*(51/50_{b_j})}{(51/25_{b_j} - 1/50)} \right] \right\} / N_{i.} \tag{17}$$

**Table 43. Values of $b_j$ for each of the 16 age/race/sex subgroups used in estimator 5**

| Age | Race | Sex | $b_j$ |
|---|---|---|---|
| 0–19 | Black | Male | 21.46 |
| 0–19 | Black | Female | 21.43 |
| 0–19 | Nonblack | Male | 22.26 |
| 0–19 | Nonblack | Female | 22.37 |
| 20–44 | Black | Male | 20.56 |
| 20–44 | Black | Female | 20.61 |
| 20–44 | Nonblack | Male | 21.26 |
| 20–44 | Nonblack | Female | 22.00 |
| 45–64 | Black | Male | 20.21 |
| 45–64 | Black | Female | 19.85 |
| 45–64 | Nonblack | Male | 23.50 |
| 45–64 | Nonblack | Female | 23.43 |
| 65 and over | Black | Male | 21.86 |
| 65 and over | Black | Female | 21.33 |
| 65 and over | Nonblack | Male | 23.09 |
| 65 and over | Nonblack | Female | 23.51 |

Where for these analyses

$$\hat{\mu}_{.j} = \bar{y}_{.j.} \text{ from the 1988 NHIS}$$

$$m_j^* = \bar{y}_{.j.} \text{ from the 1987 NHIS}$$

Table 43 provides the values of $b_j$ for each of the 16 subgroups. A value near 1 indicates that the subgroup is concentrated in one State, and a value near 51 implies that the subgroup is evenly spread throughout the States.

**6. GSE with balanced prior and sampling variances (32 subgroups).** This estimator is of the same form as estimator 5. To examine the impact on the synthetic estimator when it is subject to greater variability, however, this estimator uses 32 subgroups. This increases the sampling errors associated with the subgroup means. The 32 subgroups are each of the 16 subgroups used previously, subdivided into central city and noncentral city. This examines the possibility of significant differences between members of a subgroup who live in a central city versus outside the central city. (It was not possible to separate the 1987 NHIS data by central city and noncentral city; thus the same prior means were used for estimator 6 as for estimator 5.) Table 44 provides the values of $b_j$ for each of the 32 subgroups. It is worth noting that although the $b_j$ values used for estimator 5 only vary from 19.85 to 23.51, the $b_j$ values used for estimator 6 vary from 14.30 to 25.92.

## State-Specific Mean Square Errors

Before discussing the empirical results, it is necessary to describe briefly a procedure for comparing the accuracy of the estimators. *Estimation of The Error of Synthetic Estimates* (28) introduced the average mean square error (across small areas) as a measure of accuracy for model-based small area estimates. Let $\bar{y}_{1i}$ be the design-unbiased inflation estimator for small area $i$; $\overset{\triangle}{y}_{fi}$ be any model-based estimator $f$ for small area $i$; and $\bar{Y}_{i.}$ be the true mean for small area $i$.

**Table 44. Values of $b_j$ for each of the 32 central city/age/race/sex subgroups used in estimator 6**

| Central city | Age | Race | Sex | $b_j$ |
|---|---|---|---|---|
| Yes . . . . . . . . . . . . . . . . | 0–19 | Black | Male | 18.20 |
| Yes . . . . . . . . . . . . . . . . | 0–19 | Black | Female | 18.16 |
| Yes . . . . . . . . . . . . . . . . | 0–19 | Nonblack | Male | 14.83 |
| Yes . . . . . . . . . . . . . . . . | 0–19 | Nonblack | Female | 14.92 |
| Yes . . . . . . . . . . . . . . . . | 20–44 | Black | Male | 17.28 |
| Yes . . . . . . . . . . . . . . . . | 20–44 | Black | Female | 17.12 |
| Yes . . . . . . . . . . . . . . . . | 20–44 | Nonblack | Male | 14.30 |
| Yes . . . . . . . . . . . . . . . . | 20–44 | Nonblack | Female | 14.85 |
| Yes . . . . . . . . . . . . . . . . | 45–64 | Black | Male | 16.43 |
| Yes . . . . . . . . . . . . . . . . | 45–64 | Black | Female | 15.72 |
| Yes . . . . . . . . . . . . . . . . | 45–64 | Nonblack | Male | 16.17 |
| Yes . . . . . . . . . . . . . . . . | 45–64 | Nonblack | Female | 16.18 |
| Yes . . . . . . . . . . . . . . . . | 65 and over | Black | Male | 18.37 |
| Yes . . . . . . . . . . . . . . . . | 65 and over | Black | Female | 17.58 |
| Yes . . . . . . . . . . . . . . . . | 65 and over | Nonblack | Male | 16.81 |
| Yes . . . . . . . . . . . . . . . . | 65 and over | Nonblack | Female | 17.05 |
| No . . . . . . . . . . . . . . . . | 0–19 | Black | Male | 18.34 |
| No . . . . . . . . . . . . . . . . | 0–19 | Black | Female | 18.26 |
| No . . . . . . . . . . . . . . . . | 0–19 | Nonblack | Male | 25.17 |
| No . . . . . . . . . . . . . . . . | 0–19 | Nonblack | Female | 25.28 |
| No . . . . . . . . . . . . . . . . | 20–44 | Black | Male | 18.28 |
| No . . . . . . . . . . . . . . . . | 20–44 | Black | Female | 18.17 |
| No . . . . . . . . . . . . . . . . | 20–44 | Nonblack | Male | 24.01 |
| No . . . . . . . . . . . . . . . . | 20–44 | Nonblack | Female | 24.70 |
| No . . . . . . . . . . . . . . . . | 45–64 | Black | Male | 18.62 |
| No . . . . . . . . . . . . . . . . | 45–64 | Black | Female | 18.68 |
| No . . . . . . . . . . . . . . . . | 45–64 | Nonblack | Male | 25.92 |
| No . . . . . . . . . . . . . . . . | 45–64 | Nonblack | Female | 25.79 |
| No . . . . . . . . . . . . . . . . | 65 and over | Black | Male | 18.77 |
| No . . . . . . . . . . . . . . . . | 65 and over | Black | Female | 18.41 |
| No . . . . . . . . . . . . . . . . | 65 and over | Nonblack | Male | 24.85 |
| No . . . . . . . . . . . . . . . . | 65 and over | Nonblack | Female | 25.29 |

$$\frac{1}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i} - \overset{\triangle}{y}_{fi}\right)^2 =$$

$$\frac{1}{I}\sum_{i=1}^{I}\left[\left(\bar{y}_{1i} - \bar{y}_{i.}\right) - \left(\overset{\triangle}{y}_{fi} - \bar{y}_{i.}\right)\right]^2$$

$$= \frac{1}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i} - \bar{y}_{i.}\right)^2 + \frac{1}{I}\sum_{i=1}^{I}\left(\overset{\triangle}{y}_{fi} - \bar{y}_{i.}\right)^2$$

$$- \frac{2}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i} - \bar{y}_{i.}\right)\left(\overset{\triangle}{y}_{fi} - \bar{y}_{i.}\right) \tag{18}$$

By taking the expectation of both sides of equation 18

$$E\left[\frac{1}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i} - \overset{\triangle}{y}_{fi}\right)^2\right]$$

$$= \text{aveMSE}\left(\bar{y}_1\right) + \text{aveMSE}\left(\overset{\triangle}{y}_f\right)$$

$$- E\left[\frac{2}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i} - \bar{y}_{i.}\right)\left(\overset{\triangle}{y}_{fi} - \bar{y}_{i.}\right)\right]$$

If the number of areas $I$ is relatively large, the covariance-like last term on the righthand side will be quite small, because $E\left(\bar{y}_{1i} - \bar{y}_{i.}\right) = 0$ and as $I \to \infty$, $\bar{y}_{1i}$ and $\overset{\triangle}{y}_{fi}$ become approximately independent. Thus, for large $I$, this equation is approximately

$$E\left[\frac{1}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i} - \overset{\triangle}{y}_{fi}\right)^2\right] \approx$$

$$\text{aveVAR}\left(\bar{y}_1\right) + \text{aveMSE}\left(\overset{\triangle}{y}_f\right)$$

and the approximate (for relatively large $I$) average MSE may be written as

$$\text{aveMSE}\left(\overset{\triangle}{y}_f\right) \approx E\left[\frac{1}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i} - \overset{\triangle}{y}_{fi}\right)^2\right]$$

$$- \text{aveVAR}\left(\bar{y}_1\right) \tag{19}$$

and an approximately unbiased estimate of the average MSE is

$$\frac{1}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i}-\hat{y}_{fi}\right)^2 - \text{aveVAR}\left(\bar{y}_1\right) \tag{20}$$

To estimate the average MSE, the first term of equation 20 is computed by taking the average squared difference between the two estimators across all small areas. The second term can be computed for any item by calculating the variances of the direct estimator in each small area and taking their average. With many sample designs, however, there will be very few PSU's in each small area; therefore, the estimated variances for direct estimators for such small areas will be very unstable. Averaging the estimated variances across all small areas will provide a stable estimate of average variance.

The approximately unbiased estimate of the average MSE is simple to compute and provides a design-based overall measure of accuracy. Unfortunately, by averaging across all small areas, it overstates the error associated with areas where the model fits well or for which the sampling error is small (compared with an average small area). Similarly, it understates the error associated with areas where the model fails or for which the sampling error is large.

It would be far preferable to produce small area-specific mean square errors for model-based estimators. This would provide smaller MSE's in areas where the model fits well or for which the sampling error is small. It would also provide larger MSE's in small areas where the model fails or for which the sampling error is large. The following develops a new procedure for estimating State-specific MSE's. The definition of MSE for small area $i$ of model-based extimator $\hat{y}_f$ is

$$\text{MSE}\left(\hat{y}_{fi}\right) = \text{Var}\left(\hat{y}_{fi}\right) + \text{Bias}^2\left(\hat{y}_{fi}\right) \tag{21}$$

$\text{Var}\left(\hat{y}_{fi}\right)$ can be calculated using replicated variances (jackknife or balanced repeated replication). Replicated variances are frequently used for survey estimates based on complex survey designs and/or when weights vary within a stratum. The changes in the estimated statistic are examined as different PSU's are dropped from the analysis, with other PSU's simultaneously given additional weight. These procedures incorporate the survey's complex sample design into the estimated variance and produce estimates of precision for each small area. Replicated variances of model-based statistics are computed using the same procedure as for direct estimates of means or ratios.

Unfortunately, the lack of an estimate of the ''truth'' for potentially biased model-based estimators requires the use of an average bias, in conjunction with a State-specific variance. (If the true value were known for each small area, MSE's could be computed directly.)

$$\text{aveBias}^2\left(\hat{y}_f\right) = \text{aveMSE}\left(\hat{y}_f\right) - \text{aveVar}\left(\hat{y}_f\right) \tag{22}$$

Combining equations 19 and 22 gives

$$\text{aveBias}^2\left(\hat{y}_f\right) = E\left[\frac{1}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i}-\hat{y}_{fi}\right)^2\right] - \text{aveVAR}\left(\bar{y}_1\right) - \text{aveVAR}\left(\hat{y}_f\right) \tag{23}$$

Using this average bias (equation 23), we can produce small area-specific mean square errors by replacing the definition of MSE (equation 21) with

$$\text{MSE}\left(\hat{y}_f\right) = \text{Var}\left(\hat{y}_{fi}\right) + \text{aveBias}^2\left(\hat{y}_f\right). \tag{24}$$

To estimate equation 24, the computations for the design-based small area-specific MSE's involve the following six-step process:

Estimate

$$E\left[\frac{1}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i}-\hat{y}_{fi}\right)^2\right]$$

with

$$\frac{1}{I}\sum_{i=1}^{I}\left(\bar{y}_{1i}-\hat{y}_{fi}\right)^2 \tag{25}$$

and $\text{aveVar}\left(\bar{y}_1\right)$ with $\frac{1}{I}\sum_{i=1}^{I}\text{var}\left(\bar{y}_{1i}\right)$ (26)

$\text{Var}\left(\hat{y}_{fi}\right)$ are estimated using replicated variances, call these estimates $\text{var}\left(\hat{y}_{fi}\right)$ (27)

$$\text{Estimate aveVar}\left(\hat{y}_f\right) \text{ with } \frac{1}{I}\sum_{i=1}^{I}\text{var}\left(\hat{y}_{fi}\right) \tag{28}$$

Then

$$\text{aveBias}^2\left(\hat{y}_f\right) = (\text{eq. }25) - (\text{eq. }26) - (\text{eq. }28) \tag{29}$$

$$\text{MSE}(\hat{y}_{fi}) = (\text{eq. }27) + (\text{eq. }29) \tag{30}$$

An examination of equation 24 reveals that the six-step procedure will produce improvements (that is, differentiation in MSE's for small areas) only when the variance is a significant proportion of MSE. This can occur under either of two conditions. First, if the model used to produce the model-based estimates is a close approximation of reality, the bias term will be small. Second, if the model-based estimates depend heavily on the sample in that small area and the sample size of the survey in the small area is small, the variances will be large. It is important to note, however, that these model-based estimators can depend on factors other than the sample; thus, a small sample size does not necessarily imply a large variance. For example, if a form of the generalized synthetic estimator (equation 14) is used that relies heavily on the prior distribution, even when the sample size for each subgroup is small, the estimator will have little variance. Alternatively, if the form of the GSE weights the sample data heavily, the variance of the estimator will increase and will increase even more if the number of subgroups into which the data are split increases.

In the ideal situation for producing small area-specific mean square errors, the variance term will dominate the bias term. In such situations the root mean square error can be used to provide approximate confidence intervals for the model-based estimates.

# Empirical Results

## National Model

Tables 45 and 46 provide the estimates derived for each State for each of the estimators for number of doctor visits and perceived poor health status, respectively. The States are sorted in descending order of the estimates using estimator 1. Note that for two of the States, North Dakota and Nebraska, no sample was collected; and thus, no direct design-unbiased estimate exists. The largest and smallest estimates are shaded for each estimator. Although the model-based estimators are not consistent with the design-unbiased estimator, it is striking to note the similarities between the different model-based estimators. In particular for doctor visits, the standard synthetic estimator and all three generalized versions found Alaska to have the smallest estimated numbers. All but estimator 6 found Florida to have the largest estimated number (Florida was second largest for estimator 6). The different versions of the synthetic estimator had very little difference, generally less than 1 percent. This robustness of the synthetic estimator to heterogeneous variances and inclusion of data from the prior year would be comforting if this estimator were to be used to produce State-level estimates, and it is probably a result of the large sample sizes from each subgroup.

Similar results are observed for self-perceived poor health status. The three versions of the synthetic estimator each found Alaska to have the lowest incidence rate and the District of Columbia, the highest. It is intriguing to note that these two States had direct estimates that were very similar. Again, the robustness of the synthetic estimator for NHIS is a striking finding.

Many estimators based on exchangeable models produce estimates similar to those of composite estimators, that is, a weighted average of the direct estimator and the estimator developed conditional on the truth of a particular model (for example, the synthetic estimator). The GSE assumes exchangeability of State/subgroup means across States for a given subgroup. It therefore produces estimates that, in the absence of strong prior information, are more similar to the synthetic than to the composite estimator.

One other finding from these tables is the strong shrinkage toward the mean in all of the model-dependent estimators. Although the direct estimator spans a range of more than 3.0 doctor visits per year (2.685−6.036), each of the model-dependent estimators has a span of only 0.4 doctor visits. Similar results are found for self-perceived poor health.

Table 47 shows the results of the six-step process for computing MSE's for estimators 2 and 5 for average number of doctor visits. The third row (the Gonzalez-Waksberg average MSE) is calculated by subtracting the second from the first. The fourth row is the average of the replicated variances for the 51 States. The average bias (row 6, step 5) is found by subtracting row 4 from row 3. Unfortunately, the average variance across States is quite small compared with the average bias (less than 2 percent). As a result the largest and smallest State-specific MSE's are very similar to the average MSE. For the synthetic estimator, the aveMSE = 0.1703, while the smallest State-specific MSE is 0.1701 (Georgia) and the largest is 0.1732 (the District of Columbia). MSE's of similar magnitude with small variances were found for the other estimators and for perceived poor health. These results are not very surprising as numerous examples have shown that the magnitude of the bias of the synthetic estimator is bigger than its variability. This is compounded by the fact that the sample size of NHIS is so large (125,000 respondents) that the sampling variation is quite small. For most surveys where the sample size is likely to be much smaller, the sampling variation can be expected to play a more important role, increasing the utility of State-specific MSE's.

## Subnational Models

Given the large sample size of NHIS, it is possible to divide the Nation into groups of States having anticipated similar biases with respect to the variable of interest. When this is true, the differences in MSE's between these groups can be large; and, for States in groupings where the model fits well, the average bias will be small and the State-specific MSE's will vary. This allows for significantly smaller approximate confidence intervals for some States compared with other States. The range of estimated mean values may also increase when the computations are based on data from a restricted subset of States.

Three potential groupings of States are examined. They are based on the percent of the State's population living below the poverty level, the percent of the population that is not black living in central cities, and the percent of the black population living in central cities. Table 48 identifies which States were classified in the high, medium, and low categories in each of these groupings.

Although each State still only has 16 (or 32) subgroups, the national data set is being split into 48 (or 96) subgroups under each of these subnational models. Thus, the subgroup means will be more variable, and one would expect the State-specific MSE's to be more variable.

One limitation when using the average MSE of M. Gonzalez and J. Waksberg (28) is that its computation depends on taking the difference of two estimates. It is quite possible that, because of the errors associated with these two estimates, the estimated average MSE may be negative. Because MSE's are by definition nonnegative, it may be impossible to produce estimated MSE's for some data sets. The same holds for the six-step procedure described earlier for computing State-specific MSE's. Unfortunately, this problem is exacerbated when the Nation is split into multiple groupings. When the Nation is split into three groups of States (as is the case for each of the three groupings already mentioned), there is a much greater chance of producing negative estimated MSE's than when all States are combined into one estimate. Every time State-specific MSE's were calculated for each estimator for both variables using the poverty-based groupings, at least one of the three groups of States produced a negative estimated average MSE. Thus, the

**Table 45. Average number of doctor visits, by State**

| State | Design-unbiased inflation estimator (1) | Synthetic estimator (2) | Composite estimator (3) | Hetero-scedastic variances (4) | GSE with (16) balanced variances (5) | GSE with (32) balanced variances (6) |
|---|---|---|---|---|---|---|
| Vermont | 6.036 | 3.889 | 4.600 | 3.912 | 3.904 | 3.893 |
| Delaware | 5.648 | 3.856 | 4.178 | 3.898 | 3.895 | 3.873 |
| Colorado | 4.868 | 3.831 | 4.477 | 3.858 | 3.851 | 3.859 |
| District of Columbia | 4.706 | 3.852 | 4.121 | 3.967 | 3.987 | 4.084 |
| Michigan | 4.594 | 3.852 | 4.461 | 3.890 | 3.888 | 3.890 |
| Connecticut | 4.589 | 3.926 | 4.343 | 3.957 | 3.948 | 3.956 |
| Arizona | 4.562 | 3.889 | 4.350 | 3.915 | 3.906 | 3.927 |
| Nevada | 4.530 | 3.841 | 4.026 | 3.871 | 3.862 | 3.865 |
| Massachusetts | 4.481 | 3.933 | 4.362 | 3.960 | 3.951 | 3.959 |
| Rhode Island | 4.424 | 3.959 | 4.098 | 3.984 | 3.974 | 3.977 |
| Montana | 4.394 | 3.911 | 4.159 | 3.933 | 3.925 | 3.920 |
| Kentucky | 4.333 | 3.895 | 4.179 | 3.925 | 3.921 | 3.913 |
| Pennsylvania | 4.204 | 3.967 | 4.175 | 4.000 | 3.993 | 3.994 |
| Ohio | 4.201 | 3.896 | 4.162 | 3.930 | 3.926 | 3.930 |
| Wyoming | 4.156 | 3.827 | 3.912 | 3.849 | 3.844 | 3.842 |
| California | 4.111 | 3.817 | 4.093 | 3.848 | 3.841 | 3.850 |
| Oklahoma | 4.104 | 3.898 | 4.041 | 3.927 | 3.923 | 3.928 |
| New Mexico | 4.084 | 3.842 | 3.898 | 3.865 | 3.860 | 3.866 |
| Maine | 4.075 | 3.928 | 3.981 | 3.949 | 3.941 | 3.933 |
| Maryland | 4.045 | 3.811 | 3.970 | 3.863 | 3.861 | 3.841 |
| Florida | 3.981 | 4.003 | 3.985 | 4.041 | 4.030 | 4.025 |
| Tennessee | 3.950 | 3.876 | 3.931 | 3.916 | 3.915 | 3.924 |
| Washington | 3.869 | 3.871 | 3.870 | 3.896 | 3.888 | 3.890 |
| Arkansas | 3.840 | 3.908 | 3.867 | 3.949 | 3.949 | 3.937 |
| New York | 3.828 | 3.902 | 3.836 | 3.943 | 3.937 | 3.958 |
| Iowa | 3.821 | 3.957 | 3.878 | 3.980 | 3.971 | 3.973 |
| New Jersey | 3.806 | 3.913 | 3.828 | 3.950 | 3.943 | 3.930 |
| West Virginia | 3.734 | 3.975 | 3.888 | 3.999 | 3.992 | 3.981 |
| Oregon | 3.704 | 3.928 | 3.800 | 3.951 | 3.941 | 3.943 |
| Utah | 3.704 | 3.740 | 3.721 | 3.761 | 3.761 | 3.760 |
| Wisconsin | 3.692 | 3.894 | 3.732 | 3.920 | 3.912 | 3.920 |
| South Carolina | 3.677 | 3.780 | 3.730 | 3.838 | 3.843 | 3.801 |
| Virginia | 3.671 | 3.819 | 3.704 | 3.864 | 3.864 | 3.859 |
| Alabama | 3.638 | 3.846 | 3.698 | 3.899 | 3.903 | 3.893 |
| Hawaii | 3.575 | 3.850 | 3.796 | 3.875 | 3.864 | 3.869 |
| Kansas | 3.573 | 3.897 | 3.690 | 3.924 | 3.917 | 3.922 |
| Missouri | 3.549 | 3.913 | 3.655 | 3.947 | 3.942 | 3.940 |
| Mississippi | 3.489 | 3.763 | 3.674 | 3.829 | 3.840 | 3.793 |
| Texas | 3.450 | 3.789 | 3.482 | 3.824 | 3.823 | 3.838 |
| Illinois | 3.420 | 3.862 | 3.483 | 3.901 | 3.897 | 3.908 |
| Louisiana | 3.410 | 3.754 | 3.502 | 3.813 | 3.819 | 3.810 |
| Indiana | 3.380 | 3.885 | 3.516 | 3.915 | 3.910 | 3.920 |
| South Dakota | 3.334 | 3.921 | 3.702 | 3.924 | 3.933 | 3.932 |
| New Hampshire | 3.236 | 3.879 | 3.798 | 3.901 | 3.893 | 3.893 |
| Idaho | 3.162 | 3.857 | 3.616 | 3.877 | 3.871 | 3.864 |
| Alaska | 3.093 | 3.629 | 3.542 | 3.656 | 3.653 | 3.665 |
| Minnesota | 3.034 | 3.885 | 3.258 | 3.908 | 3.900 | 3.899 |
| Georgia | 3.000 | 3.755 | 3.190 | 3.809 | 3.812 | 3.787 |
| North Carolina | 2.685 | 3.842 | 2.949 | 3.890 | 3.891 | 3.874 |
| North Dakota | . . . | 3.908 | 3.908 | 3.931 | 3.920 | 3.923 |
| Nebraska | . . . | 3.913 | 3.913 | 3.938 | 3.931 | 3.938 |

. . . Category not applicable.

NOTE: Shading indicates the largest and smallest estimates in the column.

analyses are restricted to the latter two State groupings.

Tables 49 and 50 provide the estimates derived for each State for each of the estimators for number of doctor visits, based only on data from within that State's group of States, grouped by percent of other-than-black and black populations in central cities, respectively. As with table 46, the States are sorted in descending order of the estimates for estimator 1, with the largest and smallest estimate shaded for each estimator. Again, it is striking to note the similarities between the different model-based estimators. With States grouped by percentage of the population that is not black in central

**Table 46. Percentage with perceived poor health status, by State**

| State | Design-unbiased inflation estimator (1) | Synthetic estimator (2) | Composite estimator (3) | GSE with (16) balanced variances (5) | GSE with (32) balanced variances (6) |
|---|---|---|---|---|---|
| West Virginia | 7.17 | 2.87 | 5.28 | 2.86 | 2.87 |
| Mississippi | 6.28 | 3.19 | 5.28 | 3.18 | 3.23 |
| Alabama | 5.42 | 3.13 | 4.94 | 3.13 | 3.14 |
| Maine | 5.19 | 2.60 | 4.01 | 2.59 | 2.59 |
| Arkansas | 5.10 | 3.07 | 4.49 | 3.07 | 3.08 |
| Kentucky | 5.06 | 2.70 | 4.56 | 2.69 | 2.70 |
| North Carolina | 4.68 | 2.99 | 4.48 | 2.99 | 3.01 |
| Tennessee | 4.46 | 2.91 | 4.23 | 2.91 | 2.90 |
| Oklahoma | 3.81 | 2.75 | 3.61 | 2.74 | 2.74 |
| Louisiana | 3.52 | 2.96 | 3.44 | 2.96 | 2.97 |
| Georgia | 3.44 | 2.81 | 3.38 | 2.81 | 2.83 |
| South Carolina | 3.42 | 3.03 | 3.36 | 3.03 | 3.07 |
| Indiana | 3.40 | 2.69 | 3.33 | 2.68 | 2.68 |
| Arizona | 3.29 | 2.57 | 3.18 | 2.56 | 2.55 |
| Virginia | 3.29 | 2.80 | 3.25 | 2.80 | 2.80 |
| Texas | 3.02 | 2.49 | 3.00 | 2.48 | 2.47 |
| Florida | 2.96 | 3.29 | 2.97 | 3.30 | 3.30 |
| District of Columbia | 2.95 | 4.34 | 3.62 | 4.35 | 4.25 |
| Alaska | 2.87 | 1.76 | 2.13 | 1.74 | 1.74 |
| Idaho | 2.80 | 2.42 | 2.66 | 2.40 | 2.41 |
| Ohio | 2.79 | 2.81 | 2.79 | 2.80 | 2.80 |
| Michigan | 2.78 | 2.76 | 2.78 | 2.75 | 2.75 |
| Missouri | 2.64 | 2.88 | 2.66 | 2.87 | 2.87 |
| Delaware | 2.61 | 2.85 | 2.72 | 2.85 | 2.87 |
| Oregon | 2.50 | 2.65 | 2.53 | 2.64 | 2.64 |
| Wyoming | 2.41 | 2.32 | 2.36 | 2.30 | 2.30 |
| Illinois | 2.40 | 2.82 | 2.42 | 2.82 | 2.81 |
| California | 2.31 | 2.45 | 2.31 | 2.45 | 2.44 |
| New York | 2.31 | 2.96 | 2.33 | 2.96 | 2.94 |
| Kansas | 2.21 | 2.69 | 2.28 | 2.68 | 2.68 |
| Massachusetts | 2.21 | 2.71 | 2.24 | 2.70 | 2.69 |
| Montana | 2.10 | 2.59 | 2.27 | 2.58 | 2.58 |
| Vermont | 2.09 | 2.45 | 2.24 | 2.44 | 2.44 |
| New Jersey | 2.03 | 2.94 | 2.07 | 2.94 | 2.95 |
| South Dakota | 2.00 | 2.63 | 2.23 | 2.62 | 2.62 |
| Rhode Island | 1.97 | 2.78 | 2.19 | 2.77 | 2.77 |
| Maryland | 1.84 | 2.91 | 1.91 | 2.92 | 2.93 |
| Utah | 1.75 | 1.99 | 1.79 | 1.97 | 1.97 |
| Nevada | 1.74 | 2.56 | 1.92 | 2.56 | 2.56 |
| Pennsylvania | 1.69 | 3.02 | 1.72 | 3.01 | 3.01 |
| New Mexico | 1.67 | 2.36 | 1.79 | 2.35 | 2.35 |
| Iowa | 1.65 | 2.77 | 1.76 | 2.76 | 2.76 |
| Washington | 1.44 | 2.50 | 1.50 | 2.49 | 2.49 |
| Wisconsin | 1.38 | 2.64 | 1.45 | 2.63 | 2.62 |
| Connecticut | 1.27 | 2.82 | 1.38 | 2.82 | 2.81 |
| Colorado | 1.20 | 2.36 | 1.27 | 2.35 | 2.35 |
| Minnesota | 1.16 | 2.51 | 1.23 | 2.49 | 2.49 |
| Hawaii | 0.87 | 2.44 | 1.08 | 2.44 | 2.43 |
| New Hampshire | 0.68 | 2.42 | 0.86 | 2.40 | 2.40 |
| North Dakota | . . . | 2.59 | 2.59 | 2.58 | 2.58 |
| Nebraska | . . . | 2.67 | 2.67 | 2.66 | 2.66 |

. . . Category not applicable.

NOTE: Shading indicates the largest and smallest estimates in the column.

cities, the standard synthetic estimator and all three generalized versions find Alaska to have the smallest estimated number of doctor visits and West Virginia to have the largest estimated number. The four model-based estimators again agree on the States with the largest and smallest number of doctor visits when States are grouped by percentage of black population in central cities, but the extreme States are not the same as with the first grouping; instead, the District of Columbia is always the highest and Mississippi, the lowest.

A major difference between the estimates in tables 49 and 50 compared with those in table 45 is that the State-to-State range of model-dependent point estimates is much larger when the States are grouped than when all States used national subgroup estimates. Table 51 demonstrates that for number of doctor visits, the range of model-dependent estimates across the States doubles when the States are grouped. For example, when data from all States are combined, the GSE with 32 subgroups has a range of 0.41 (from 3.67 (Arkansas) to 4.08 (the District of Columbia). When data only from within a State's grouping are used, this range is increased to 0.76 for groupings that are not black (3.42 (Arkansas) to 4.18 (West Virginia)) and to 0.97 for black groupings (3.48 (Mississippi) to 4.45 (the District of Columbia)). This is important because one of the concerns about the synthetic estimator is that many analysts believe it ''overshrinks'' the estimates toward the national average, underrepresenting the true variability from small area to small area.

Similar results are observed for self-perceived poor health status. With States grouped by populations that are not black in central cities (table 52), the three versions of the synthetic estimator each find Alaska to have the lowest incidence rate and the District of Columbia the highest. It is again intriguing to note that these two States had direct estimates that were very similar. When grouped by black populations in central cities (table 53), Alaska still has the lowest rate, but Mississippi has the highest rate.

Table 54 examines the robustness of the GSE by comparing the variation among the different model-dependent estimators for each State. Small variation (as is found when national subgroup estimates are used) would indicate that the synthetic estimator is robust to its assumptions that are loosened in the various forms of the GSE that are examined. For doctor visits, the variation among the model-based estimators is similar when States are grouped by population other than black as when all States are combined. When grouped by percentage of the black population in central cities, there is significant variation among the model-dependent estimates for the 11 States with more rural black populations.

Grouping the States also increases the variation among model-dependent estimates for percent with poor health in each State. This variation is particularly pronounced for States with large percentages of its population that are not black, or small percentages of its black population, in central cities.

Thus, using subnational groupings of States results in model-dependent estimates that vary more from State to State and are, in general, still consistent across the form of estimator that is used. In some States, however, the form of the GSE can have a significant impact on the estimate. The remainder of this chapter examines the impact on MSE's of using the subnational groupings.

The six-step process for computing MSE's is recalculated averaging only across those States in the same grouping. The subgroup means are calculated separately for each grouping. Tables 55–57 are demonstrative of results for the two

**Table 47. State-specific mean square errors for number of doctor visits, using estimators 2 and 5**

| | | Synthetic estimator | GSE with (16) balanced variances |
|---|---|---|---|
| (1) | $E\,(\bar{y}_{1i.} - \hat{\bar{y}}_{fi.})^2$ . . . . . . . . . . . . . . . | .3777 | .3754 |
| (2) | aveVar $(\bar{y}_1)$ . . . . . . . . . . . . . . . . | .2074 | .2074 |
| | aveMSE $(\bar{y}_f)$ . . . . . . . . . . . . . . . | .1703 | .1680 |
| (4) | aveVar $(\bar{y}_f)$ . . . . . . . . . . . . . . . . | .0020 | .0003 |
| | $\frac{\text{aveVar}}{\text{aveMSE}}\,(\bar{y}_f)$ . . . . . . . . . . . . . . . | 1.2 percent | 0.1 percent |
| (5) | aveBias$^2$ $(\bar{y}_f)$ . . . . . . . . . . . . . . . | .1684 | .1678 |
| (6) | MSE $(\bar{y}_f)$ | | |
| | Smallest . . . . . . . . . . . . . . . . | .1701 (Georgia) | .1680 (many) |
| | Largest . . . . . . . . . . . . . . . . . | .1732 (District of Columbia) | .1684 (District of Columbia) |
| | RMSE $(\bar{y}_f)$ . . . . . . . . . . . . . . . . . | .412–.416 | .410 |

**Table 48. Groupings of States by common expected biases**

| | |
|---|---|
| Percentage of population below poverty in 1988: | |
| 0.0–8.7 . . . . . . . . . . . . . . . . . . . . | Connecticut, New Jersey, New Hampshire, Wisconsin, Kansas, Vermont, Massachusetts, Delaware, Nevada, Washington |
| 9.4–17.6 . . . . . . . . . . . . . . . . . . . | Iowa, Wyoming, Maryland, Rhode Island, Utah, Indiana, Nebraska, Pennsylvania, Oregon, Virginia, Alaska, Hawaii, Minnesota, North Dakota, Michigan, Ohio, Colorado, Idaho, North Carolina, Illinois, Michigan, California, Maine, New York, Florida, Georgia, Arizona, South Dakota, Montana, District of Columbia, South Carolina, Oklahoma, Kentucky |
| 17.9–27.2 . . . . . . . . . . . . . . . . . . | West Virginia, Tennessee, Texas, Alabama, Arkansas, Louisiana, New Mexico, Mississippi |
| Percentage of 1990 nonblack population in central city: | |
| 0–15 . . . . . . . . . . . . . . . . . . . . . . | Delaware, Vermont, South Carolina, Mississippi, West Virginia, Georgia, New Jersey, Maryland, Idaho, Maine, Kentucky, Michigan |
| 16–37 . . . . . . . . . . . . . . . . . . . . . | All 34 others |
| 38–100 . . . . . . . . . . . . . . . . . . . . | Alaska, New York, Texas, Arizona, District of Columbia |
| Percentage of 1990 black population in central city: | |
| 0–35 . . . . . . . . . . . . . . . . . . . . . . | Mississippi, South Carolina, Hawaii, Vermont, North Dakota, Idaho, West Virginia, Maine, Georgia, Delaware, North Carolina |
| 36–77 . . . . . . . . . . . . . . . . . . . . . | Arkansas, Maryland, Montana, Utah, Florida, Nevada, New Mexico, New Jersey, South Dakota, Alabama, Kentucky, Louisiana, Virginia, New Hampshire, Wyoming, Washington, California, Missouri, Oklahoma, Colorado, Alaska, Texas, Rhode Island, Kansas, Ohio, Arizona, Tennessee, Minnesota, Pennsylvania, Connecticut, Iowa |
| 78–100 . . . . . . . . . . . . . . . . . . . . | Illinois, Michigan, Oregon, Massachusetts, New York, Nebraska, Indiana, Wisconsin, District of Columbia |

**Table 49. Average number of doctor visits, with States grouped by percentage of nonblack population in central cities**

| State | Design-unbiased inflation estimator (1) | Synthetic estimator (2) | Composite estimator (3) | Hetero-scedastic variances (4) | GSE with (16) balanced variances (5) | GSE with (32) balanced variances (6) |
|---|---|---|---|---|---|---|
| Vermont | 6.036 | 4.055 | 5.241 | 4.079 | 4.099 | 4.097 |
| Delaware | 5.648 | 3.999 | 4.653 | 4.041 | 4.044 | 4.042 |
| Colorado | 4.868 | 3.831 | 4.100 | 3.822 | 3.861 | 3.876 |
| District of Columbia | 4.706 | 3.807 | 3.928 | 3.971 | 4.051 | 4.053 |
| Michigan | 4.594 | 3.997 | 4.554 | 4.035 | 4.052 | 4.090 |
| Connecticut | 4.589 | 3.923 | 4.099 | 3.919 | 3.958 | 3.969 |
| Arizona | 4.562 | 3.767 | 4.074 | 3.799 | 3.713 | 3.703 |
| Nevada | 4.530 | 3.839 | 3.888 | 3.832 | 3.872 | 3.881 |
| Massachusetts | 4.481 | 3.931 | 4.170 | 3.923 | 3.961 | 3.973 |
| Rhode Island | 4.424 | 3.956 | 3.995 | 3.947 | 3.982 | 3.986 |
| Montana | 4.394 | 3.908 | 3.997 | 3.895 | 3.930 | 3.921 |
| Kentucky | 4.333 | 4.050 | 4.290 | 4.080 | 4.101 | 4.110 |
| Pennsylvania | 4.204 | 3.961 | 4.107 | 3.959 | 3.997 | 3.995 |
| Ohio | 4.201 | 3.892 | 4.074 | 3.891 | 3.932 | 3.935 |
| Wyoming | 4.156 | 3.826 | 3.849 | 3.813 | 3.850 | 3.847 |
| California | 4.111 | 3.816 | 4.042 | 3.812 | 3.850 | 3.867 |
| Oklahoma | 4.104 | 3.894 | 3.963 | 3.890 | 3.929 | 3.938 |
| New Mexico | 4.084 | 3.840 | 3.855 | 3.829 | 3.866 | 3.879 |
| Maine | 4.075 | 4.091 | 4.081 | 4.115 | 4.132 | 4.134 |
| Maryland | 4.045 | 3.940 | 4.031 | 3.990 | 3.991 | 3.997 |
| Florida | 3.981 | 3.994 | 3.987 | 3.997 | 4.034 | 4.030 |
| Tennessee | 3.950 | 3.871 | 3.901 | 3.878 | 3.922 | 3.931 |
| Washington | 3.869 | 3.870 | 3.870 | 3.860 | 3.897 | 3.899 |
| Arkansas | 3.840 | 3.900 | 3.885 | 3.907 | 3.950 | 3.937 |
| New York | 3.828 | 3.801 | 3.820 | 3.857 | 3.779 | 3.781 |
| Iowa | 3.821 | 3.954 | 3.923 | 3.942 | 3.976 | 3.980 |
| New Jersey | 3.806 | 4.057 | 3.826 | 4.096 | 4.093 | 4.101 |
| West Virginia | 3.734 | 4.131 | 3.881 | 4.159 | 4.177 | 4.177 |
| Oregon | 3.704 | 3.925 | 3.876 | 3.912 | 3.947 | 3.951 |
| Utah | 3.704 | 3.741 | 3.734 | 3.729 | 3.766 | 3.763 |
| Wisconsin | 3.692 | 3.891 | 3.798 | 3.883 | 3.920 | 3.931 |
| South Carolina | 3.677 | 3.910 | 3.739 | 3.964 | 3.968 | 3.936 |
| Virginia | 3.671 | 3.817 | 3.754 | 3.827 | 3.872 | 3.868 |
| Alabama | 3.638 | 3.837 | 3.768 | 3.856 | 3.904 | 3.894 |
| Hawaii | 3.575 | 3.848 | 3.835 | 3.837 | 3.872 | 3.884 |
| Kansas | 3.573 | 3.894 | 3.806 | 3.887 | 3.924 | 3.930 |
| Missouri | 3.549 | 3.908 | 3.785 | 3.908 | 3.948 | 3.942 |
| Mississippi | 3.489 | 3.886 | 3.651 | 3.946 | 3.958 | 3.921 |
| Texas | 3.450 | 3.665 | 3.508 | 3.713 | 3.634 | 3.630 |
| Illinois | 3.420 | 3.858 | 3.613 | 3.863 | 3.904 | 3.917 |
| Louisiana | 3.410 | 3.747 | 3.624 | 3.772 | 3.824 | 3.816 |
| Indiana | 3.380 | 3.881 | 3.698 | 3.877 | 3.917 | 3.929 |
| South Dakota | 3.334 | 3.918 | 3.852 | 3.904 | 3.938 | 3.935 |
| New Hampshire | 3.236 | 3.879 | 3.860 | 3.865 | 3.902 | 3.901 |
| Idaho | 3.162 | 4.017 | 3.491 | 4.043 | 4.087 | 4.089 |
| Alaska | 3.093 | 3.456 | 3.437 | 3.495 | 3.416 | 3.417 |
| Minnesota | 3.034 | 3.884 | 3.567 | 3.872 | 3.908 | 3.905 |
| Georgia | 3.000 | 3.888 | 3.090 | 3.936 | 3.947 | 3.944 |
| North Carolina | 2.685 | 3.837 | 3.356 | 3.851 | 3.897 | 3.881 |
| Nebraska | . . . | 3.910 | 3.910 | 3.901 | 3.937 | 3.949 |
| North Dakota | . . . | 3.906 | 3.906 | 3.893 | 3.925 | 3.930 |

. . . Category not applicable.

NOTE: Shading indicates the largest and smallest estimates in the column.

different groupings of States examined for each of the two variables.

Table 55 demonstrates that grouping States with similar expected biases can result in State-specific MSE's where the variance is a nontrivial component of the MSE. For States with a high percentage of their population that is not black residing in central cities, the variance of the synthetic estimator is on average 21 percent of the MSE's. When all States are taken together, the root mean square error (RMSE) varies only from 0.412 to 0.416. When States are grouped by the distribution of population that is not black, RMSE's vary from 0.190 in Utah to 0.717 in West Virginia, reflecting the fact that

**Table 50. Average number of doctor visits, with States grouped by percentage of black population in central cities**

| State | Design-unbiased inflation estimator (1) | Synthetic estimator (2) | Composite estimator (3) | Hetero-scedastic variances (4) | GSE with (16) balanced variances (5) | GSE with (32) balanced variances (6) |
|---|---|---|---|---|---|---|
| Vermont | 6.036 | 3.471 | 5.369 | 3.703 | 3.856 | 3.859 |
| Delaware | 5.648 | 3.336 | 4.622 | 3.559 | 3.699 | 3.709 |
| Colorado | 4.868 | 3.897 | 3.897 | 3.898 | 3.893 | 3.900 |
| District of Columbia | 4.706 | 4.230 | 4.342 | 4.282 | 4.440 | 4.449 |
| Michigan | 4.594 | 3.877 | 4.410 | 3.934 | 3.878 | 3.873 |
| Connecticut | 4.589 | 3.992 | 3.992 | 3.991 | 3.991 | 3.998 |
| Arizona | 4.562 | 3.954 | 3.945 | 3.953 | 3.948 | 3.969 |
| Nevada | 4.530 | 3.904 | 3.904 | 3.903 | 3.903 | 3.907 |
| Massachusetts | 4.481 | 3.891 | 4.303 | 3.953 | 3.870 | 3.875 |
| Rhode Island | 4.424 | 4.028 | 4.028 | 4.026 | 4.020 | 4.023 |
| Montana | 4.394 | 3.978 | 3.978 | 3.976 | 3.968 | 3.963 |
| Kentucky | 4.333 | 3.961 | 3.961 | 3.961 | 3.961 | 3.953 |
| Pennsylvania | 4.204 | 4.033 | 4.033 | 4.032 | 4.033 | 4.033 |
| Ohio | 4.201 | 3.960 | 3.960 | 3.961 | 3.964 | 3.966 |
| Wyoming | 4.156 | 3.892 | 3.892 | 3.892 | 3.884 | 3.882 |
| California | 4.111 | 3.880 | 3.880 | 3.881 | 3.880 | 3.889 |
| Oklahoma | 4.104 | 3.962 | 3.962 | 3.962 | 3.964 | 3.968 |
| New Mexico | 4.084 | 3.907 | 3.907 | 3.906 | 3.900 | 3.906 |
| Maine | 4.075 | 3.506 | 3.940 | 3.740 | 3.887 | 3.882 |
| Maryland | 4.045 | 3.866 | 3.866 | 3.873 | 3.893 | 3.878 |
| Florida | 3.981 | 4.063 | 4.063 | 4.061 | 4.071 | 4.068 |
| Tennessee | 3.950 | 3.937 | 3.937 | 3.939 | 3.950 | 3.956 |
| Washington | 3.869 | 3.937 | 3.937 | 3.936 | 3.931 | 3.932 |
| Arkansas | 3.840 | 3.967 | 3.967 | 3.969 | 3.982 | 3.974 |
| New York | 3.828 | 3.934 | 3.845 | 3.993 | 3.937 | 3.948 |
| Iowa | 3.821 | 4.026 | 4.026 | 4.023 | 4.017 | 4.019 |
| New Jersey | 3.806 | 3.976 | 3.976 | 3.977 | 3.982 | 3.971 |
| West Virginia | 3.734 | 3.520 | 3.683 | 3.754 | 3.908 | 3.907 |
| Oregon | 3.704 | 3.873 | 3.795 | 3.934 | 3.845 | 3.845 |
| Utah | 3.704 | 3.801 | 3.801 | 3.804 | 3.797 | 3.795 |
| Wisconsin | 3.692 | 3.859 | 3.738 | 3.918 | 3.834 | 3.837 |
| South Carolina | 3.677 | 3.184 | 3.597 | 3.391 | 3.532 | 3.532 |
| Virginia | 3.671 | 3.878 | 3.878 | 3.882 | 3.896 | 3.893 |
| Alabama | 3.638 | 3.900 | 3.900 | 3.905 | 3.928 | 3.922 |
| Hawaii | 3.575 | 3.455 | 3.526 | 3.699 | 3.805 | 3.771 |
| Kansas | 3.573 | 3.961 | 3.961 | 3.960 | 3.960 | 3.963 |
| Missouri | 3.549 | 3.977 | 3.977 | 3.977 | 3.981 | 3.978 |
| Mississippi | 3.489 | 3.135 | 3.395 | 3.335 | 3.477 | 3.476 |
| Texas | 3.450 | 3.848 | 3.848 | 3.852 | 3.858 | 3.872 |
| Illinois | 3.420 | 3.890 | 3.518 | 3.947 | 3.891 | 3.894 |
| Louisiana | 3.410 | 3.801 | 3.801 | 3.810 | 3.841 | 3.836 |
| Indiana | 3.380 | 3.871 | 3.559 | 3.929 | 3.858 | 3.861 |
| South Dakota | 3.334 | 3.987 | 3.987 | 3.984 | 3.977 | 3.976 |
| New Hampshire | 3.236 | 3.949 | 3.949 | 3.947 | 3.937 | 3.937 |
| Idaho | 3.162 | 3.447 | 3.232 | 3.679 | 3.826 | 3.822 |
| Alaska | 3.093 | 3.668 | 3.688 | 3.692 | 3.689 | 3.702 |
| Minnesota | 3.034 | 3.952 | 3.952 | 3.950 | 3.944 | 3.942 |
| Georgia | 3.000 | 3.182 | 3.010 | 3.386 | 3.534 | 3.541 |
| North Carolina | 2.685 | 3.285 | 2.715 | 3.499 | 3.642 | 3.637 |
| Nebraska | . . . | 3.872 | 3.872 | 3.930 | 3.848 | 3.853 |
| North Dakota | . . . | 3.505 | 3.505 | 3.747 | 3.862 | 3.836 |

. . . Category not applicable.

NOTE: Shading indicates the largest and smallest estimates in the column.

the model-dependent estimators are able to produce estimates for some States with much greater accuracy than for others.

Table 56 demonstrates results that do not differentiate quite as strongly between States. Using the generalized synthetic estimator with 16 balanced variances to estimate percent with perceived poor health without subgrouping the States, the variance of the model-based estimator is only 0.4 percent of its MSE, and the RMSE varies only from 1.16 to 1.17 in any State. When the States are grouped according to the percent of the black population in central cities, however, the State-specific MSE's are different.

**Table 51. Variation in State estimates for average number of doctor visits**

| | Direct inflation | Synthetic | Composite | Hetero-scedatic variances | GSE with 16 groupings | GSE with 32 groupings |
|---|---|---|---|---|---|---|
| All States | 2.69–6.04 | 3.63–4.00 | 2.95–4.60 | 3.66–4.04 | 3.65–4.03 | 3.67–4.08 |
| Percent nonblack | 2.69–6.04 | 3.46–4.13 | 3.09–5.24 | 3.50–4.16 | 3.42–4.18 | 3.42–4.18 |
| Percent black | 2.69–6.04 | 3.14–4.23 | 2.72–5.37 | 3.34–4.28 | 3.48–4.44 | 3.48–4.45 |

**Table 52. Percentage with self-perceived poor health status, with States grouped by percentage of nonblack population in central cities**

| State | Design-unbiased inflation estimator | Synthetic estimator | Composite estimator | GSE with (16) balanced variances | GSE with (32) balanced variances |
|---|---|---|---|---|---|
| West Virginia | 7.17 | 3.34 | 6.02 | 3.44 | 3.43 |
| Mississippi | 6.28 | 3.37 | 5.67 | 3.56 | 3.56 |
| Alabama | 5.42 | 3.10 | 4.71 | 3.11 | 3.13 |
| Maine | 5.19 | 3.06 | 4.52 | 3.13 | 3.13 |
| Arkansas | 5.10 | 3.00 | 4.23 | 3.02 | 3.04 |
| Kentucky | 5.06 | 3.10 | 4.81 | 3.19 | 3.19 |
| North Carolina | 4.68 | 2.95 | 4.36 | 2.97 | 3.00 |
| Tennessee | 4.46 | 2.84 | 4.09 | 2.86 | 2.85 |
| Oklahoma | 3.81 | 2.65 | 3.48 | 2.67 | 2.67 |
| Louisiana | 3.52 | 2.95 | 3.40 | 2.97 | 2.98 |
| Georgia | 3.44 | 3.04 | 3.42 | 3.18 | 3.17 |
| South Carolina | 3.42 | 3.25 | 3.40 | 3.40 | 3.41 |
| Indiana | 3.40 | 2.59 | 3.27 | 2.62 | 2.61 |
| Arizona | 3.29 | 2.72 | 2.81 | 2.47 | 2.45 |
| Virginia | 3.29 | 2.75 | 3.21 | 2.77 | 2.78 |
| Texas | 3.02 | 2.59 | 2.78 | 2.34 | 2.34 |
| Florida | 2.96 | 3.19 | 2.98 | 3.23 | 3.24 |
| District of Columbia | 2.95 | 4.07 | 3.99 | 3.82 | 3.81 |
| Alaska | 2.87 | 1.88 | 1.91 | 1.68 | 1.68 |
| Idaho | 2.80 | 2.83 | 2.81 | 2.90 | 2.90 |
| Ohio | 2.79 | 2.72 | 2.78 | 2.75 | 2.74 |
| Michigan | 2.78 | 3.09 | 2.79 | 3.20 | 3.20 |
| Missouri | 2.64 | 2.79 | 2.66 | 2.82 | 2.82 |
| Delaware | 2.61 | 3.18 | 2.78 | 3.30 | 3.29 |
| Oregon | 2.50 | 2.53 | 2.51 | 2.56 | 2.56 |
| Wyoming | 2.41 | 2.20 | 2.28 | 2.22 | 2.22 |
| Illinois | 2.40 | 2.75 | 2.42 | 2.78 | 2.77 |
| California | 2.31 | 2.36 | 2.31 | 2.39 | 2.39 |
| New York | 2.31 | 3.03 | 2.63 | 2.77 | 2.78 |
| Kansas | 2.21 | 2.59 | 2.29 | 2.61 | 2.61 |
| Massachusetts | 2.21 | 2.60 | 2.25 | 2.62 | 2.62 |
| Montana | 2.10 | 2.46 | 2.27 | 2.49 | 2.49 |
| Vermont | 2.09 | 2.89 | 2.32 | 2.94 | 2.93 |
| New Jersey | 2.03 | 3.31 | 2.07 | 3.43 | 3.43 |
| South Dakota | 2.00 | 2.51 | 2.25 | 2.53 | 2.54 |
| Rhode Island | 1.97 | 2.67 | 2.24 | 2.69 | 2.69 |
| Maryland | 1.84 | 3.16 | 1.89 | 3.30 | 3.30 |
| Utah | 1.75 | 1.89 | 1.78 | 1.91 | 1.91 |
| Nevada | 1.74 | 2.46 | 1.97 | 2.49 | 2.49 |
| Pennsylvania | 1.69 | 2.92 | 1.74 | 2.95 | 2.94 |
| New Mexico | 1.67 | 2.25 | 1.82 | 2.28 | 2.28 |
| Iowa | 1.65 | 2.65 | 1.81 | 2.67 | 2.67 |
| Washington | 1.44 | 2.38 | 1.52 | 2.41 | 2.41 |
| Wisconsin | 1.38 | 2.52 | 1.48 | 2.55 | 2.55 |
| Connecticut | 1.27 | 2.72 | 1.43 | 2.75 | 2.74 |
| Colorado | 1.20 | 2.26 | 1.31 | 2.28 | 2.28 |
| Minnesota | 1.16 | 2.39 | 1.26 | 2.41 | 2.41 |
| Hawaii | 0.87 | 2.32 | 1.16 | 2.36 | 2.36 |
| New Hampshire | 0.68 | 2.30 | 0.95 | 2.32 | 2.32 |
| Nebraska | . . . | 2.56 | 2.56 | 2.58 | 2.58 |
| North Dakota | . . . | 2.47 | 2.47 | 2.50 | 2.49 |

. . . Category not applicable.

NOTE: Shading indicates the largest and smallest estimates in the column.

**Table 53. Percentage with perceived poor health status, with States grouped by percentage of black population in central cities**

| State | Design-unbiased inflation estimator | Synthetic estimator | Composite estimator | GSE with (16) balanced variances | GSE with (32) balanced variances |
|---|---|---|---|---|---|
| West Virginia | 7.17 | 3.94 | 6.09 | 4.40 | 4.39 |
| Mississippi | 6.28 | 4.32 | 5.82 | 4.75 | 4.75 |
| Alabama | 5.42 | 3.09 | 4.79 | 3.06 | 3.06 |
| Maine | 5.19 | 3.57 | 4.62 | 3.96 | 3.95 |
| Arkansas | 5.10 | 3.07 | 4.34 | 3.03 | 3.04 |
| Kentucky | 5.06 | 2.72 | 4.42 | 2.69 | 2.70 |
| North Carolina | 4.68 | 4.06 | 4.63 | 4.47 | 4.40 |
| Tennessee | 4.46 | 2.91 | 4.15 | 2.87 | 2.87 |
| Oklahoma | 3.81 | 2.77 | 3.55 | 2.74 | 2.74 |
| Louisiana | 3.52 | 2.91 | 3.41 | 2.88 | 2.89 |
| Georgia | 3.44 | 3.77 | 3.46 | 4.16 | 4.12 |
| South Carolina | 3.42 | 4.09 | 3.49 | 4.50 | 4.50 |
| Indiana | 3.40 | 2.28 | 2.85 | 2.21 | 2.21 |
| Arizona | 3.29 | 2.60 | 3.15 | 2.58 | 2.56 |
| Virginia | 3.29 | 2.78 | 3.23 | 2.75 | 2.76 |
| Texas | 3.02 | 2.49 | 2.99 | 2.46 | 2.45 |
| Florida | 2.96 | 3.30 | 2.98 | 3.28 | 3.28 |
| District of Columbia | 2.95 | 4.04 | 3.87 | 3.90 | 3.95 |
| Alaska | 2.87 | 1.77 | 2.22 | 1.74 | 1.74 |
| Idaho | 2.80 | 3.32 | 2.93 | 3.67 | 3.66 |
| Ohio | 2.79 | 2.82 | 2.79 | 2.79 | 2.79 |
| Michigan | 2.78 | 2.38 | 2.65 | 2.30 | 2.28 |
| Missouri | 2.64 | 2.89 | 2.67 | 2.86 | 2.86 |
| Delaware | 2.61 | 3.87 | 3.03 | 4.28 | 4.27 |
| Oregon | 2.50 | 2.21 | 2.33 | 2.13 | 2.13 |
| Wyoming | 2.41 | 2.35 | 2.38 | 2.32 | 2.32 |
| Illinois | 2.40 | 2.44 | 2.41 | 2.36 | 2.36 |
| California | 2.31 | 2.47 | 2.31 | 2.44 | 2.44 |
| New York | 2.31 | 2.56 | 2.36 | 2.48 | 2.50 |
| Kansas | 2.21 | 2.72 | 2.31 | 2.69 | 2.69 |
| Massachusetts | 2.21 | 2.28 | 2.23 | 2.20 | 2.21 |
| Montana | 2.10 | 2.63 | 2.32 | 2.60 | 2.60 |
| Vermont | 2.09 | 3.35 | 2.49 | 3.70 | 3.71 |
| New Jersey | 2.03 | 2.94 | 2.09 | 2.92 | 2.92 |
| South Dakota | 2.00 | 2.68 | 2.30 | 2.64 | 2.65 |
| Rhode Island | 1.97 | 2.82 | 2.26 | 2.79 | 2.79 |
| Maryland | 1.84 | 2.87 | 1.94 | 2.86 | 2.87 |
| Utah | 1.75 | 2.03 | 1.81 | 1.99 | 1.99 |
| Nevada | 1.74 | 2.58 | 1.98 | 2.56 | 2.56 |
| Pennsylvania | 1.69 | 3.04 | 1.74 | 3.00 | 3.00 |
| New Mexico | 1.67 | 2.40 | 1.84 | 2.37 | 2.36 |
| Iowa | 1.65 | 2.82 | 1.81 | 2.78 | 2.78 |
| Washington | 1.44 | 2.53 | 1.52 | 2.50 | 2.50 |
| Wisconsin | 1.38 | 2.22 | 1.63 | 2.14 | 2.15 |
| Connecticut | 1.27 | 2.84 | 1.42 | 2.81 | 2.81 |
| Colorado | 1.20 | 2.39 | 1.30 | 2.36 | 2.35 |
| Minnesota | 1.16 | 2.54 | 1.26 | 2.51 | 2.51 |
| Hawaii | 0.87 | 3.37 | 1.09 | 3.70 | 3.61 |
| New Hampshire | 0.68 | 2.46 | 0.93 | 2.42 | 2.42 |
| North Dakota | . . . | 3.59 | 3.59 | 3.96 | 3.89 |
| Nebraska | . . . | 2.23 | 2.23 | 2.16 | 2.17 |

. . . Category not applicable.

NOTE: Shading indicates the largest and smallest estimates in the column.

For States with a high percentage in central cities, the average variance is 12.8 percent of the MSE. Among these States, RMSE's vary from 0.411 in Michigan to 0.475 in the District of Columbia, and the State-specific RMSE for Mississippi (with a low percent of the black population in central cities) is 1.46, three and one-half times the RMSE for Michigan.

This is the greatest differentiation found between State RMSE's using this estimator. In general, the traditional synthetic estimator has a larger variance than do forms of GSE that rely on prior information. By incorporating prior information, the GSE subgroup means are no longer completely a function of the sampled data and therefore are less variable than the subgroup means of the synthetic estimator.

**Table 54. Robustness of the generalized synthetic estimator**

| Variable | State grouping | Range among estimators 2, 4, 5, and 6 | |
|---|---|---|---|
| Doctor visits | None | 6 percent | District of Columbia |
| | | 0–2 percent | All others |
| | | 6 percent | District of Columbia |
| | Nonblack | 3 percent | Arizona |
| | | 0–2 percent | All others |
| Black | | 10–11 percent | All 11 States with < 36 percent |
| | | 4 percent | District of Columbia |
| | | 0–2 percent | All others |
| Poor health | None | 0–2 percent | All states |
| | Nonblack | 7–12 percent | All 5 States with > 37 percent |
| | | 3–6 percent | 9 of 12 States with < 16 percent |
| | | 0–2 percent | All others |
| | Black | 10–12 percent | All 11 States with < 36 percent |
| | | 3–4 percent | 8 of 9 States with > 77 percent |
| | | 0–2 percent | All others |

**Table 55. State-specific mean square errors for number of doctor visits, using the synthetic estimator, with States grouped by percent nonblack population in central cities**

| | | Nonblack population in central cities[1] | | | |
|---|---|---|---|---|---|
| | | All States | Low | Moderate | High |
| (1) | $E(\bar{y}_{1i.} - \hat{\bar{y}}_{2i.})^2$ | .3777 | .7536 | .2203 | .3238 |
| (2) | aveVar $(\bar{y}_1)$ | .2074 | .2417 | .1842 | .2736 |
| | aveMSE $(\bar{y}_2)$ | .1703 | .5119 | .0361 | .0502 |
| (4) | aveVar $(\bar{y}_2)$ | .0020 | .0119 | .0029 | .0106 |
| | $\frac{\text{aveVar}}{\text{aveMSE}}(\bar{y}_2)$ | 1.2 percent | 2.3 percent | 8.0 percent | 21.1 percent |
| (5) | aveBias$^2$ $(\bar{y}_2)$ | .1684 | .5001 | .0333 | .0396 |
| (6) | MSE $(\bar{y}_2)$ | | | | |
| | Smallest | .1701 (Georgia) | .5097 (Georgia) | .0360 (Utah) | .0474 (Texas) |
| | Largest | .1732 (District of Columbia) | .5141 (West Virginia) | .0364 (Iowa) | .0586 (District of Columbia) |
| | RMSE $(\bar{y}_2)$ | | | | |
| | Smallest | .412 | .714 | .190 | .218 |
| | Largest | .416 | .717 | .191 | .242 |

[1]See table 48 for State groupings.

**Table 56. State-specific mean square errors for percentage with perceived poor health using the generalized synthetic estimator with 16 balanced variances, with States grouped by percentage of black population in central cities**

| | | Black population in central cities[1] | | | |
|---|---|---|---|---|---|
| | | All States | Low | Moderate | High |
| (1) | $E(\bar{y}_{1i.} - \hat{\bar{y}}_{6i.})^2$ | 1.727 | 2.752 | 1.276 | .431 |
| (2) | aveVar $(\bar{y}_1)$ | 0.369 | 0.641 | 0.312 | 0.254 |
| | aveMSE $(\bar{y}_6)$ | 1.358 | 2.111 | 0.964 | 0.177 |
| (4) | aveVar $(\bar{y}_6)$ | .0006 | .0898 | .0073 | .0227 |
| | $\frac{\text{aveVar}}{\text{aveMSE}}(\bar{y}_6)$ | 0.4 percent | 4.3 percent | 0.8 percent | 12.8 percent |
| (5) | aveBias$^2$ $(\bar{y}_6)$ | 1.357 | 2.022 | 0.957 | 0.154 |
| (6) | MSE $(\bar{y}_6)$ | | | | |
| | Smallest | 1.357 (Alaska, Utah) | 2.102 (Georgia) | 0.962 (Alaska) | 0.169 (Michigan) |
| | Largest | 1.360 (District of Columbia) | 2.125 (Mississippi) | 0.967 (Florida) | 0.226 (District of Columbia) |
| | RMSE $(\bar{y}_6)$ | | | | |
| | Smallest | 1.16 | 1.45 | 0.981 | 0.411 |
| | Largest | 1.17 | 1.46 | 0.983 | 0.475 |

[1]See table 48 for State groupings.

NOTE: All values are times 0.0001, except RMSE, which is times 0.01.

**Table 57. State-specific mean square errors for percentage with perceived poor health using the synthetic estimator, with States grouped by percentage of nonblack population in central cities**

| | | Nonblack population in central cities[1] | | | |
|---|---|---|---|---|---|
| | | All States | Low | Moderate | High |
| (1) | $E(\bar{y}_{1i.} - \hat{\bar{y}}_{2i.})^2$ | 1.724 | 3.011 | 1.080 | .0651 |
| (2) | aveVar $(\bar{y}_1)$ | 0.369 | 0.552 | 0.265 | 0.600 |
| | aveMSE $(\bar{y}_2)$ | 1.355 | 2.459 | 0.815 | 0.051 |
| (4) | aveVar $(\bar{y}_2)$ | .0005 | .0055 | .0007 | .0040 |
| | $\frac{\text{aveVar}}{\text{aveMSE}}(\bar{y}_2)$ | 0.4 percent | 2.2 percent | 0.9 percent | 78.4 percent |
| (5) | aveBias$^2$ $(\bar{y}_2)$ | 1.350 | 2.404 | 0.809 | 0.011 |
| (6) | MSE $(\bar{y}_2)$ | | | | |
| | Smallest | 1.353 (Alaska) | 2.446 (Georgia) | 0.813 (Utah) | 0.032 (Alaska) |
| | Largest | 1.374 (District of Columbia) | 2.475 (West Virginia) | 0.819 (Iowa) | 0.097 (District of Columbia) |
| | RMSE $(\bar{y}_2)$ | | | | |
| | Smallest | 1.16 | 1.56 | 0.902 | 0.179 |
| | Largest | 1.17 | 1.57 | 0.905 | 0.311 |

[1]See table 48 for State groupings.

NOTE: All values are times 0.0001, except RMSE, which is times 0.01.

Table 57 shows much more promising results. Using the synthetic estimator to estimate percent with perceived poor health without subgrouping the States yields results that are almost identical to those in the previous table. When the States are grouped according to the percent of the population that is not black in central cities, the State-specific MSE's are quite different. For States with a high percentage in central cities, the average variance is 78 percent of MSE. Among these States, the RMSE's vary from 0.179 in Alaska to 0.311 in the District of Columbia; thus, a confidence interval on the estimate for the District of Columbia would be almost twice as wide as one for Alaska. The State-specific RMSE for West Virginia is 1.57, almost 9 times the RMSE for Alaska.

In addition to examining the variation of MSE's from State to State, it is also of interest to examine when estimates produced by the synthetic estimator or GSE have smaller MSE's. By incorporating prior information, the GSE is based on more information and can therefore be expected to have smaller variance (unless the variance on the prior estimate is large). If the prior estimates are not consistent with the sample means for certain subgroups, however, the GSE will introduce an extra source of bias. Thus, it is possible for either estimator to produce smaller MSE's. The data used in this empirical analysis produced MSE's that were sometimes smaller for the GSE and at other times, for the synthetic estimator. Table 58 summarizes these findings for self-perceived poor health.

## Summary of Empirical Results

With a national model, where the subgroup averages are subject to little sampling error, the generalized synthetic estimator was quite robust to alternative model assumptions that were examined. That is, the point estimates of the traditional synthetic estimator were hardly changed when the small area and/or subgroup variances were allowed to vary across small areas or when prior information (in the form of the previous year's survey estimate) was included. The large sample sizes in the national model resulted in MSE's that were dominated by the bias term and thus showed little difference from State to State.

Doubling the number of subgroups in the national model from 16 to 32 did not have much impact on the estimates. The empirical findings were quite different when the sample size per subgroup was reduced by using subnational models. The form of the GSE was important in that the point estimates for a given State could differ by approximately 10 percent, depending on the form of the estimator. All of the model-dependent estimators had more State-to-State variation using the subnational models, reducing the amount of shrinkage toward the national mean.

By grouping the States on the basis of similar expected biases, it was possible to produce State-specific MSE's that varied markedly from group to group. For groupings where the average bias was a small component of MSE, the individual State MSE's also varied within the group. For example, in those States where a high percentage of the population that is not black lives in

**Table 58. Reduction in estimate of the MSE of the generalized synthetic estimator (16 subgroups) relative to the MSE of the synthetic estimator for percent with perceived poor health**

| Percent in central city | Grouped by percentage black | Grouped by percentage nonblack |
|---|---|---|
| Low | < 1 | 5–6 |
| Medium | < 1 | (1–2) |
| High–District of Columbia | 26 | (14) |
| High–other States | 5–7 | (128–226) |

NOTE: When the MSE of the GSE is larger, it is shown in parentheses.

central cities, the variance averaged to be almost 80 percent of MSE. This resulted in RMSE's for some of these States that were almost twice as large as those for other States in the same group. Across all States RMSE's varied by a factor of 9.

When the GSE incorporated prior information, the State-to-State variation in MSE's was not as significant. This results from the fact that, by basing the estimates only partially on the sample data, the variance is reduced. Because only the variance component of the MSE is State-specific, this minimizes the differences between States.

No consistent results were found for whether the GSE would produce smaller MSE's than the traditional synthetic estimator. When States were grouped by percent of black (or other-than-black) population in central cities, the GSE always had a smaller MSE for self-perceived poor health. When they were grouped by percent of black (or other-than-black) population in central cities, however, the reverse was true for two groups of States. This analysis was limited by the many occasions when the MSE procedure produced negative estimates for one or more groups of States. Although this limitation also arises when using average MSE's, it is more pronounced the greater the number of groups of small areas.

The GSE has the potential to improve significantly on the traditional synthetic estimator when the sampling error of the subgroup mean is not small—for example, when the number of subgroups is large. In such cases the synthetic estimator is sensitive to the assumptions that are generalized in the GSE. When this situation arises, the GSE can be used either to produce estimates based on assumptions with which the analyst feels more comfortable or to test the sensitivity of the traditional estimator to its assumptions.

The GSE appears to have potential for producing State-level estimates from NHIS for at least some variables. The procedure for producing State-specific MSE's may be useful for detecting differences between States. In either situation more research is needed on the covariates for the model-dependent estimators and the effects on the estimates of using different prior distributions.

# Part IV.
# Coordination With Surveys of Health Care Providers

# Chapter 13. Coordination at the Primary Sampling Unit Level

Locating all the surveys of health care providers (known collectively as the National Health Care Survey, or NHCS) in a single set of PSU's that is a subset of the NHIS PSU's was a possibility in which National Center for Health Statistics was extremely interested, as discussed in chapter 2. Two potential benefits of such coordination were identified and researched: One is cost savings; the other is enrichments to analysis. They are discussed separately in the following sections. A major potential drawback—variance increases—was identified and researched. After advantages and disadvantages of this approach are discussed, a conclusion is presented.

## Cost Savings

Placing all the component surveys of the NHCS into a single subset of NHIS PSU's could lead to certain synergies that could translate into cost savings. To achieve the synergies, however, it would be necessary to limit the flexibility of the combined design. At a broad level, the coordination would save money if a single small set of interviewers could work on all the surveys. Cost savings would be largest if the interviewers could work only in their home PSU's, thereby eliminating the cost of long-distance travel. For the U.S. Bureau of the Census to be able to retain such a cadre of local interviewers, it would be necessary to schedule the provider surveys well in advance in a pattern that allowed optimal staff utilization. Any breaks in the pattern could lead to loss of that staff, thereby requiring costly travel by backup interviewers. Overlaps in the pattern where local staff were stretched beyond their capabilities would also require expensive travel by backups. Given the uncertain nature of multiyear funding for these surveys, this sort of predictability seems unlikely; therefore, major cost savings also appear unlikely.

## Analysis Enrichment

Most health care policy research is done at the individual level. However, some research is done at the community level, some at the State level, and some at the national level. Examples of research at the individual level include studies linking smoking to heart disease, evaluating different treatments for breast cancer, and testing drugs. Examples at the broader levels consist mostly of trying to link different policies, such as per capita spending, with average health measures. The idea behind this research was that it might make sense to study linkages at the PSU level. For PSU's to be an interesting level to study, they should be defined as interesting entities in and of themselves.

It would be a promising start if PSU's could be defined as clearly distinct units with respect to patterns of health care delivery. Toward this aim, NCHS staff (29) defined a set of health care service areas (HCSA's) that were fairly well self-contained in terms of health care delivery; that is, the HCSA's were defined in such a way that for most of the areas, residents obtained their health care services from providers located in the same HCSA's.

If a sample of HCSA's was used as sample PSU's for NHIS and for the full set of health care provider surveys (NHCS), an analysis file could be set up in which each record corresponded to a different HCSA and each record contained average health statistics gathered from NHIS and average provider traits collected from NHCS.

It turned out, however, that HCSA's had to be defined as quite large areas for them to be self-contained. This resulted in a substantial reduction in the number of PSU's in the United States (about 800 rather than the 1,900 PSU's defined for NHIS in 1985). Even at this level, persons seeking health care services still participated in considerable crossing of HCSA borders. Furthermore, a clear and convincingly urgent set of hypotheses about linkages at the HCSA level was never set forth.

## Variance Increases

Confining NHIS and all the surveys of health care providers to a single set of PSU's will inevitably lead to increases in variance for analyses restricted to just one of the surveys. This is true because the optimal definition, stratification, and selection procedure of PSU's is different for each survey.

Because there are far fewer providers than households and far more work to be done by the interviewer at each sample provider than at each sample household, the optimal PSU size for several provider surveys is far larger than for NHIS. For example, it might be reasonable to define a substantial part of South Dakota as a single PSU for a survey of hospitals; for NHIS, however, the travel costs would be extremely high if more than a few rural counties were collapsed together to form a PSU.

Stratification is quite different, too, because no thought was devoted to trying to produce provider statistics at the State level. Trying to keep NHIS flexible for State expansions means that strata must respect State boundaries. The provider surveys have no parallel need. For example, Cheyenne, Wyoming; Boise, Idaho; Helena, Montana; and Billings, Montana, might make a reasonable stratum for one of the provider surveys.

In particular, the optimal number of sample PSU's is very different for various surveys. To provide flexibility for State expansions, the optimal number of PSU's for NHIS is quite large—at least 300. The optimal number of sample PSU's for health care provider surveys is closer to 100.

Even given the same PSU definitions and stratification, the optimal probabilities of selection are different across surveys. For each survey, the optimal probabilities of selection are proportional to a different measure of size. For NHIS, the best measure of size would be some weighted combination of population by domain, as discussed in chapters 4 and 15. For the National Hospital Discharge Survey, the optimal measure of size would be a recent count of hospital discharges. For the National Ambulatory Medical Care Survey, the optimal measure of size would be a recent count of ambulatory visits to doctors' offices. Similar statements can be made about the other provider surveys.

Unfortunately, it is impossible using currently available data to quantify accurately the extent of the variance increase for one survey to use the sample PSU's designed as optimal for a different survey. To solve this problem, it would be necessary to have accurate data on the characteristics of interest at the county level. Other than a question on disability, the decennial census does not contain health-related data of any sort, much less health care data. The health-related information that is available on a county basis covers such items as numbers of physicians, hospital beds, nursing homes, mortality from various causes, and so forth. Past study has shown that these do not correlate well with each other, making it unlikely that they would be efficient stratifiers or measures of size for different provider surveys. Consequently, research on alternate PSU definitions, stratification, and selection methodology is not very productive. The only other alternative is to conduct experiments at the national level where two independent designs are used with independent field staffs; to do so, of course, would be extremely expensive.

Nonetheless, a set of PSU's that would be reasonable for NHIS, with a measure of size that would also be reasonable for NHIS, was examined to see how satisfactory this set of PSU's would be for the provider surveys. The set consisted of those defined for the *Current Population Survey* outside New England in 1984 and those defined in New England for the Survey of Income and Program Participation in 1985. For this set of PSU's, the degree of overlap across the surveys was examined with respect to the subset of PSU's that should be self-representing; that is, selected with certainty. The correlations between the optimal measures of size at the PSU level were also examined among a common set of nonself-representing PSU's.

A finding of this study was that all the PSU's that should be self-representing for one provider survey or more (given 112 sample PSU's) would be contained within the set of NHIS self-representing PSU's (assuming 358 sample NHIS PSU's). In addition, high correlations were found between the alternate measures of size. Although it is true that the correlations became rather low for the smallest PSU's (in terms of population), only a small part of the national population lives in such PSU's. From these findings, it was concluded that the penalties to the provider surveys of being confined to a subset of NHIS sample PSU's would probably be modest.

## Conclusion

With respect to NHIS itself, the potential for enriching analysis and improving operational efficiencies for the provider surveys does not appear concrete enough to justify picking a set of PSU's that is less than optimal for NHIS. Furthermore, investigations of correlations of measure of size indicate that the provider surveys can probably fit within the NHIS sample PSU's with little precision loss, if that is desired.

# Chapter 14. Network Sampling of Providers Through the National Health Interview Survey

## Introduction

In its report, the Panel on the National Health Care Survey (30) made two recommendations related to network sampling. The first (recommendation 4–1) was that providers other than short-term hospitals, office-based physicians, and nursing homes be surveyed by network sampling based on nominations from patients of these providers who happened to be included in NHIS. The second (recommendation 4–2) was that NCHS study the utility of sampling even the short-term hospitals and office-based physicians in this manner. This chapter examines the circumstances under which network sampling based on NHIS could be more efficient than list-based sampling.

It is important to note that recommendations 4–1 and 4–2 by the panel concern only the types of data traditionally collected in NCHS provider surveys and not dual patient-provider data on individual health care events such as were obtained through the National Medical Expenditure Survey (NMES) of 1987 with its component Medical Provider Survey (MPS). Other recommendations in the panel's report deal with the analytic potential of those types of data and problems involved in collecting such data. These other issues are briefly discussed in chapter 15.

## List Quality

For an arbitrary class of health care providers, it is useful to define three grades of list quality. In the first case, it is possible that no list exists at all. This seems doubtful because the sorts of health care providers listed in the panel's report (psychologists, dentists, physical and occupational therapists, pharmacists, podiatrists, chiropractors, nurses, nurse practitioners, physician assistants, nurse midwives, and optometrists, among others) are typically State licensed or are associated with licensed practitioners or facilities. If a licensing procedure exists, a list can be constructed. Of course, it may be necessary to negotiate individually with each State to obtain a list of licensed providers within that State, and merging the State lists together into a national list would require some effort. (Also, classification and unduplication issues are likely to be severe, but that is true of any sampling approach.) The only types of entities that are sometimes viewed as health care providers that are probably not susceptible to list-based sampling are those that consider religious practices a form of health care. The panel did not recommend that coverage be extended to this class of caregivers, however.

At the next level of list quality, a list exists but contains no indication of practice size. This is frequently the case with membership lists of national voluntary associations of health care providers. This may be the most common situation for the types of health care providers under consideration. At the highest level of quality, a list exists with a quantification of the number of persons served by each provider.

Another dimension of list quality concerns coverage of the intended population. Lists of health care providers are likely to age fairly quickly. Providers move, retire, enter the business, form new partnerships, dissolve partnerships, and so on. If the State licensing procedure does not require annual renewals, the information may become extremely outdated. Furthermore, a small class of unlicensed providers may exist. Coverage gaps due to out-of-date lists and unlicensed providers do cause bias. Undercoverage is also a problem with network samples, however, as is explored below.

## Network Sample Undercoverage

A number of possible sources of undercoverage exist. The first source is undercoverage in NHIS itself. Coverage rates generally run between 60 and 100 percent depending on age, sex, race, and ethnic origin, with an overall figure around 90 percent (relative to the unadjusted decennial census). The second source is unit nonresponse in NHIS. For various reasons about 5 percent of eligible sample households are not interviewed. The third source is underreporting of health care events. Little empirical evidence currently exists on how often this occurs, but research at HCFA (matching self-reported events from the Medicare Current Beneficiary Survey against administrative records) may soon shed additional light in this area. This source of undercoverage can clearly be reduced by keeping the reference period short for qualifying events. (There could also be overreporting of events if steps are not taken to prevent telescoping.)

The fourth source of undercoverage is failure to name the health care provider and to provide enough information so that the provider can be located. Experience with the provider followup (MPS) to NMES is not directly relevant to projecting the rate at which this might occur but does contain some clues. NMES respondents were asked to sign consent forms in addition to naming the health care provider and supplying address information. Although just 81 percent of the NMES sample provided the signed consent forms and provider information, it could be that a network sample based on NHIS would encounter much more favorable rates. A reasonable projection of 95 percent overall coverage can be made on the assumption that consent forms would not be required because a random sample of patient records would be drawn at the provider's office and information would presumably be recorded anonymously; it is possible that the nominator would not even be in the sample. Coverage can be anticipated to be far worse for some providers, however. In particular, separately billing doctors associated with hospital visits are likely to be poorly covered because many patients do not remember their physicians' names or even know that they were in attendance.

All in all, it seems reasonable to project provider coverage of between 75 and 85 percent before provider nonresponse. (Implications for response rates are discussed in the section entitled ‘‘Other considerations.’’) Depending on State licensing procedures and/or the frequency of membership updates for professional societies, this range of coverage could be better or worse than what could be expected from a list. Within a provider type, coverage of those providers who serve predominantly black and Hispanic populations can be expected to be less well covered because of the undercoverage of these groups in NHIS itself. The decision of which approach would lead to higher coverage would probably have to be based on a thorough review of possible list sources for the specific provider to be surveyed. Of course, coverage alone will not determine the decision; there are still questions of variance and cost.

### Provider or Event Characteristics?

The variance structure of a sample survey of health care providers depends strongly on the nature of the survey’s focus. If primary interest is in personal and/or professional characteristics of the provider (e.g., office costs, technical equipment owned, fee for service, income), network sampling will almost always lead to very large variances. This is attributable to the highly variable probabilities of selection (arising from the fact that the probability will depend on the number of visits to the facility). The obstacles to list formation would have to be severe indeed for network sampling with such a substantive focus to be considered. Only if the primary interest is on volumes of various types of services provided (e.g., numbers of appendectomies, tranquilizer prescriptions, root canals, babies delivered at home, thin lenses ground), does network sampling have any appeal. This is because network sampling is similar to probability proportionate-to-size (PPS) sampling.

The range of provider sizes is an important consideration in whether PPS sampling or some other method of sampling is efficient. PPS sampling is more likely to be important in a survey of institutional providers than in a survey of individual professionals because of the much greater room for variation in workload. Because good lists are also more likely to be available for institutional providers, however, this criterion will not usually facilitate the decision between list-based sampling and network sampling.

### Areas With Rich Potential for Network Sampling

Taking these factors together, the chief potential for NHIS-based network sampling of providers appears to be in surveys where the focus is on event characteristics; the only available lists of providers have no adequate information on measures of practice size; and the providers are not associated with institutions for which good lists are available. We will assume that dentists are a potential domain of interest in order to simplify language in creating a model for costs and variances of network sampling. A structure is developed below for comparing variances and costs for network and list-based samples of dental visits. Analyses for other domains would follow similar lines.

## Model for Costs and Variance

### A Network Sample of Dentists

Suppose that a survey of dentists has been commissioned in which the main focus is on volumes of services such as prophylaxis, x rays, fillings, crowns, root canals, orthodontia, and gum surgery. Note that this sort of data would probably be most efficiently obtained from a dentist by abstracting individual patient records rather than by asking the dentist to keep count. Although gross revenue, expenses, and net income are examples of variables related to service volume where no abstraction is required, at least some abstraction is probably typical of provider surveys interested in service volumes and will be an assumed feature of the design.

Let

$N$ be the annual number of dental visits throughout the United States

$t$ be the length of the reference period in weeks preceding the NHIS sample week

$N_{iv}$ be the number of appointments kept by the $i$-th dentist within the $t$-week reference period immediately prior to the $v$th NHIS sample week (It is explicitly assumed here that appointments by the same person cannot reasonably be unduplicated and will be counted separately.)

$n$ be the number of dental visits reported in the NHIS sample during the $t$-week period preceding the $v$th sample week (assume constant across sample weeks for simplicity)

$m$ be the number of networks that are established for the $t$-week period preceding the $v$th sample week (assume constant across sample weeks for simplicity)

$\delta$ be the intraclass correlation for events at the same dentist and

$\lambda$ be the number of events sampled per network (may not include the event that brought the practice into sample)

The planned average sampling interval for persons who are neither black nor Hispanic in 1995 and beyond is about 1,900. Only 1 part in 52 of the sample can report on any $t$-week period, however. Also, only $t$ parts in 52 of annual visits occur during a $t$-week period. Thus, the expected number of dental visits to be detected in a $t$-week period is $n = Nt \big/ [(52^2)1{,}900]$. For example, suppose that $N = 500{,}000{,}000$ and $t = 1$. Then $n = 97$; that is, it is possible to form 97 network samples around 97 dental visits reported in 1 week’s sample. For ease of exposition, each of these network samples is referred to as a cluster. With 97 dental visits identified each week, a maximum of 5,044 clusters can be established over the course of a year. That would probably be more than necessary to achieve adequate precision.

Note that some dentists will be hit (nominated) more than once. Most often, this will occur when a patient has two visits during the reference period for related services. Siblings may often receive routine dental care in the same week. To reduce the number of multiple hits, it may be advantageous to identify a specific subset of NHIS that is allowed to make nominations. The objective of that subsample design would be to minimize the number of households with multiple potential nominators without increasing the variation in selection probabilities. This can be done with the same sort of subsampling that was used for Cycle IV of the National Survey of Family Growth. There, single-eligible households were selected at lower rates than multieligible households, and only one eligible was selected per household. (This would not be an option if all visits detected in a year had to be retained as cluster nuclei, as might be the case with a rarer domain such as endodontists.)

It is reasonable to assume that probabilities of selection for providers will be too difficult to determine, and that, therefore, it will not be possible to use the Horvitz-Thompson estimator. (The problem is made particularly complex by the systematic selection of blocks for NHIS. To establish the probability of selection for the provider, it would be necessary to geocode all patients who could have nominated the provider and then establish their relative positions in the systematic block sort (and within-block sort).) Instead, a multiplicity estimator will be required, and one is suggested in the following:

Let $i$ be the index dentist and $j$ the index appointments within the $t$-week reference period immediately before the $v$th NHIS sample week. If a person who made the $j$th appointment at the $i$th dentist happens to be interviewed for NHIS within the $v$th week of the year, let $W_{ivj}$ be the NHIS noninterview-adjusted weight for the person; otherwise, let $W_{ivj} = 0$. Assume that nonresponse to NHIS is ignorable given the adjustment cells employed by NHIS. Then an unbiased weight for $v$th week for the dental provider is

$$W_{Div} = \frac{52}{t} \sum_j W_{ivj}\, N_{iv}$$

This weight can be averaged over all 52 weeks to obtain an unbiased weight for the dental provider for the year. (Note that it is not necessary to know $N_{iv}$ for weeks where there are no hits because $\sum_j W_{ivj}\, N_{iv} = 0$ for such periods.)

Let $m_{iv}$ be the number of networks that are established at the $i$th practice based on the $v$-th interview week. Then an unbiased weight for all the $\lambda m_{iv}$ sample events is

$$W_{Eiv} = W_{Div} \frac{N_{iv}}{\lambda m_{iv}}$$

Note that if the number of networks established at a practice were equal to the number of times that it was hit for NHIS, and if NHIS were self-weighting, we would have

$$W_{Eiv} = \frac{(52/t)\, BW}{\lambda}$$

where $BW$ was the common NHIS weight. Note, however, that clients of dentists with racially or ethnically integrated practices will have different probabilities of selection than those at mostly white or mostly minority practices.

The actual variance associated with using $W_{Eiv}$ to create a weighted estimate is extremely complicated, depending on patterns of multiple visits within NHIS segments. Nonetheless, a reasonable approximation can be obtained by treating the sample as a self-weighting sample of $52m$ clusters of $\lambda$ events each. The relative variance of such a sample is well known to be

$$v_w^2 = \frac{[1 + \delta(\lambda - 1)]\, V_w^2}{52m\lambda}$$

where $m = \Sigma_i\, m_{iv}$, the number of networks per week, is assumed not to vary by week; where $52m\lambda$ is the total number of sample events; and where $V_w^2$ is the relative variance within the population of events of the characteristic of interest. For example, if the interest lies in the proportion of dental visits that are for root canals, and the true proportion is 0.05, then $V_w^2 = 0.95/0.05 = 19$.

To allow for multiple hits and the variation in the NHIS sample, an adjustment factor $\xi$ is introduced:

$$v_w^2 = \frac{[1 + \delta(\lambda - 1)]\, \xi V_w^2}{52m\lambda}$$

Adding on the between-PSU relative variance, the final approximation is obtained:

$$\begin{aligned} v^2 &= v_w^2 + v_b^2 \\ &= \frac{[1 + \delta(\lambda - 1)]\, \xi V_w^2}{(52m\lambda)} + v_b^2 \end{aligned}$$

where $v_b^2$ is the between-PSU relative variance associated with the selected number of PSU's.

Of course, the most critical parameter to fix for the subsampling is the number of clusters to retain. Given either a fixed-cost or a fixed-precision requirement, that number is fixed by the cluster size. To identify the optimal cluster size, it is necessary to quantify some cost estimates for the various phases of data collection. Let

- $c_0$ be the cost of developing sampling and data collection procedures and training interviewers on the new procedures in the desired number of PSU's
- $c_1$ be the extra cost per NHIS household interview of screening for recent dental visits (equal to 0 if NHIS already asks about visits to this type of provider)
- $c_2$ be the cost associated with each network, such as obtaining from the NHIS respondent the name and address of the dentist and a consent form to release records, tracing and recruiting the nominated dentist, and sampling from his or her records
- $c_3$ be the cost per event of abstraction including data processing and editing

Then the total costs for the hypothetical survey of dentists would be

$$C = c_0 + c_1\,(55{,}000) + 52c_2m + 52c_3m\lambda$$

With these assumptions, for a given fixed cost $C$, the optimal cluster size is

$$\lambda = \sqrt{\frac{1-\delta}{\delta}}\sqrt{\frac{c_2}{c_3}}$$

and the optimal number of clusters is

$$m = \frac{C - c_0 - 55{,}000c_1}{52(c_2 + c_3\lambda)}$$

For a fixed-precision $v^2$, the optimal cluster size is unchanged and the optimal number of clusters is

$$m = \frac{[1 + \delta(\lambda - 1)]\,\xi V_w^2}{(v^2 - v_b^2)52\lambda}$$

Some plausible values for the parameters are shown next. Obviously, however, each parameter would have to be carefully reconsidered for a specific survey. For illustrative purposes, then, assume the following cost structure, variance structure, and precision requirement:

$c_0$ = \$300,000
$c_1$ = \$3
$c_2$ = \$300
$c_3$ = \$15
$V_w^2$ = 99
$v_b^2$ = 0.0005
$\xi$ = 1.1
$\delta$ = 0.15
$v^2$ = 0.01

(Note that $V_w^2$ is a population-relative variance, whereas $v_b^2$ is the relative between-PSU variance on a sample estimator—hence the difference in magnitude between $V_w^2$ and $v_b^2$.) Then $\lambda = 15$ and $m = 46$. The total annual sample size would then be 35,880 abstracted events and the total cost would be \$1.7 million.

# Comparison With List-Based Sampling

## A Comparable List Sample of Dentists

Cost implications are discussed first, followed by variance implications, and last by some operational implications that may affect response rates. The effort to patch together State lists would increase $c_0$, perhaps by as much as \$200,000. On the other hand, cost $c_1$ does not apply. Furthermore, cost $c_2$ might be lower because State lists might have better name and address information than what NHIS respondents can supply. State lists will probably contain enough dirty and incomplete records, however, to offset most of these savings. Thus, it seems reasonable to speculate that $c_2$ might decrease from \$300 to \$290. It is also possible that $c_3$ would increase somewhat because of variation in the number of cases to be abstracted per dental practice. (Note: It is being assumed here that the provider class of interest is one for which lists cannot be expected to contain useful measures of size.)

Assuming that the list will not contain any information on the number of patients served by the dentist, the relative variance from a list sample would be

$$v^2 = \frac{[1 + \delta(\bar{\lambda} - 1)]\,\xi V_w^2}{(52m\bar{\lambda})} + \frac{V_N^2}{(52m)} + v_b^2$$

where $V_N^2$ is the relative variation in practice size, $\bar{\lambda}$ is now the average cluster size, and $\xi$ is now the extra design effect attributable to variation in practice size that persists even when the characteristic of interest is a ratio in which the numerator and the denominator are highly correlated with practice size. If poststratification is possible (unlikely for a dental survey), or the main interest is in ratios such as the percent of patients who receive root canals, then the $V_N^2/(52m)$ term will disappear. (The term cancels out by subtraction after linearization because it affects both numerator and denominator.) However, if poststratification is not possible and the main focus is on totals such as the total number of root canals performed in the country or on ratios where the denominator is not correlated with practice size, such as the average number of root canals per dentist, then the $V_N^2/(52m)$ term does apply. In either case, the $\xi$ factor is likely to persist. Also note that $\xi$ does not approach 1 as $m$ becomes large. (This factor is discussed in *Sample Survey Methods and Theory* volumes I and II (31). It is written there as the ratio of $V^2$ with a caret to $V^2$ without a caret.)

For fixed precision, the optimal number of clusters is now

$$m = \frac{[1 + \delta(\bar{\lambda} - 1)]\,\xi V_w^2 + \bar{\lambda} V_N^2}{(v^2 - v_b^2)\,52\bar{\lambda}}$$

To illustrate, suppose the following distribution of weekly practice sizes:

| | Practices | Visits |
|---|---|---|
| 1–10 | 10,000 | 55,000 |
| 11–20 | 20,000 | 310,000 |
| 21–50 | 60,000 | 2,130,000 |
| 51–100 | 60,000 | 4,530,000 |
| 101–200 | 10,000 | 1,515,000 |
| 201–500 | 100 | 35,100 |
| 501–1,000 | 2 | 1,500 |
| Total | 160,102 | 8,576,600 |

Then the average practice size would be 53 and the population variance of practice size would be 1,326 for a population-relative variance of 0.47. This variation is probably a bit on the high side, assuming that, in multiple-dentist practices, the sample would be taken of all patients seen by any of the partners. Whether the sample could be stratified by individual dentists would depend on how lists are maintained in the office.

Assume then the following cost structure, variance structure, and precision requirement:

$c_0$ = \$500,000
$c_1$ = \$0
$c_2$ = \$290
$c_3$ = \$18
$V_w^2$ = 99
$v_b^2$ = 0.0005
$\xi$ = 0.47
$\delta$ = 1.2
$v^2$ = 0.01

Then $\bar{\lambda} = 10$ and $m = 57$. The total annual sample size would then be 29,640 abstracted events, and the total cost would be \$1.9 million.

## Other Considerations

Besides coverage, cost, and variance implications, other possible implications exist for response rates, other nonsampling errors, and elapsed time from survey authorization to release of data. With network sampling, it is more difficult to plan the work in advance. It is fairly likely that routine dental visits will be scheduled on the same day for some family members. Also possible, but somewhat less likely—neighbors may be scheduled for the same week. Multiple hits cause operational nuisances by requiring twice as much abstracting to keep the sample self-weighting (or *k* times as much if there are *k* hits in the same practice). It is also possible for the same practice to be hit multiple times over the year, thereby adding to variable and unpredictable response burden and resulting in irritation in some dentists toward the survey taker. The number of multiple hits will be difficult to predict and probably will vary considerably from one type of provider to another. For example, multiple hits seem unlikely to be much of a problem for chiropractors but do seem a likely problem for emergency rooms. If this unpredictability is a problem with providers, one solution would be to wait until an entire year of NHIS had made nominations before following up any of the nominations. This, of course, implies an extra time lag between the time the survey is approved and when data collection may begin. Another problem with such an approach would be the destruction or transfer to long-term storage of service records.

With list sampling, it is possible to plan the visits in advance, possibly leading to cost savings and improvements in response rates. On the other hand, if the list does not have a good measure of size, the number of records to be abstracted per practice becomes even more unpredictable than with network sampling. This might have unfavorable implications for costs and response rates. In the example of dentists just given, the number could vary from 1 to more than 100.

One problem inherent in both sampling approaches is the existence of joint practices. If the provider named by the list or nominated by the patient has a joint practice with one provider or more, establishing the exact definition of the sampling unit is an important and possibly difficult task. If separate appointment logs are kept, there is no real problem. If no separate logs are kept and each patient is simply seen by the next available provider, the entire practice may have to be viewed as the sampling unit. Alternatively, it may be possible to select an oversample of patient records and subsample them after inspection to just those of patients seen by the provider of interest. This problem's existence does not seem to favor either network or list-based sampling.

Last is the consideration of multiple provider surveys. Such surveys would undoubtedly involve some economies of scale. The cost $c_0$ for the network sample and the list sample would be lower per survey if multiple surveys were done. The reduction might be sharper for network sampling, however, than for list sampling.

## Conclusion

If a reasonable list with a reasonable measure of size can be found, list-based sampling is probably preferable; otherwise, it appears that network sampling should be considered. Factors that would tilt the balance in favor of network sampling include the following:

- Primary interest is on event characteristics, not provider characteristics.
- Considerable uncertainty exists about the quality of the lists, particularly with respect to coverage and currentness.
- Available lists do not have reasonable measures of size; and either:
  - The numbers of events per provider varies widely, control totals for poststratification are not available, and only a small number of providers can be sampled or
  - The numbers of events per provider varies widely, and variation in the number of records to be abstracted per provider increases abstracting costs or nonresponse rates.
- The cost of forming a list is projected to be greater than the cost of collecting nominations from NHIS.
- The difficulties in tracing nominated providers are thought to be less onerous than finding providers mentioned on State licensing lists (or lists obtained through other sources).
- Flexibility in scheduling visits to providers (including multiple visits) is not problematic, or an extra delay in the survey schedule can be accommodated.

Formulas have been provided in this report that will facilitate deciding which approach is less expensive for fixed precision. Some of the parameters will be extremely difficult to project; however, considerable room for judgment will remain.

# Chapter 15. The National Health Interview Survey as a Tool for Linking Beneficiary and Provider Perspectives on Health Care

A great deal of attention has recently been focused on the possibility of fundamentally redesigning NHIS in such a manner that it provides a more complete picture of the health care process, incorporating data from the beneficiaries and providers. Ideally, health economists would like to be able to use NHIS as a tool for assessing the beneficiaries' satisfaction with different treatment modalities for various conditions and the cost of those treatment modalities.

## Other Surveys

Several of the recommendations of the Panel on the National Health Care Survey, if implemented, would make NHIS resemble other Federal surveys more closely. This brings up the question of the proper goals for NHIS vis-a-vis those other surveys. Specifically, the panel's call for detailed medical cost and utilization data to be collected through longitudinal follow ups of the health care providers to NHIS respondents would make NHIS resemble the National Medical Expenditure Survey (NMES) rather closely. Along with its predecessors, the National Medical Care and Expenditure Survey and the National Medical Care Utilization and Expenditure Survey, and the Medicare Current Beneficiary Survey conducted by HCFA, NMES allows the linkage of health care costs with the outcomes of health care.

## National Health Interview Survey as a Baseline or as a Follow Up?

If NHIS is to be used to obtain dual-perspective data on health care (data about the same events from both the provider and the beneficiary), the question arises of whether it is better to track patients from a sample of provider records or to track providers from a sample of patient nominations. A different research effort (2) funded by NCHS has been underway to study the feasibility of the first approach. NMES stands as an example of how the second approach can work. Both approaches obviously involve problems. The Panel of the National Research Council favored the NMES approach. This report takes no position.

# Part V.
# Decisions on Sample Design and Contingency Options

# Chapter 16. Decisions on Sample Design

The recommended design is based on the assumption that the field budget for NHIS can be boosted (starting in 1995) by 50 percent (over the level needed to support 50,000 household interviews with the 1985–94 design) to pay for improved precision on health statistics about black and Hispanic persons. Starting in 1991, the U. S. Bureau of the Census undertook all necessary preparations for the recommended design. As discussed in chapter 17, there was not a budget increase in 1995. Also, the preparation costs in 1994 for the recommended design are substantial. Thus, an alternative was also designed that would lead to some improvement in the precision of Hispanic statistics under a level 1995 budget (equal to the 1988 budget after adjustment for inflation). The recommended design is referred to as the alpha option; the alternative is known as the beta option.

## Reconciliation of Objectives

It was recognized early in the research that there were some fundamental tensions between the objectives for the redesign. The desire for improved statistics on the black and Hispanic populations implied a need to increase the sample in the major metropolitan areas of the large States. This desire also implied a need to stratify PSU's by ethnic composition, with a lower priority for respect of State boundaries. Furthermore, for this objective, nonself-representing PSU's with significant numbers of minority persons should be oversampled. Last, for this objective, all household members should be interviewed in black and Hispanic households. Pulling in a second direction, the desire for improved State statistics implied a need to allocate a disproportionate share of the sample to small States where relatively few minority persons live, to define and stratify nonself-representing PSU's in such a manner that State boundaries were strictly respected, and to select nonself-representing PSU's solely in proportion to population within States. Pulling in a third direction, the desire to integrate the provider surveys with NHIS implied a need to define large PSU's as sometimes crossing State lines (so that most inhabitants of a PSU would receive their medical services within the boundaries of the PSU) and to oversample PSU's that tend to provide disproportionate amounts of medical services to nonresidents. Pulling in a fourth direction, the desire for reduced response bias through self-response implied a need to interview just one randomly selected adult per household.

These inherent conflicts were resolved by placing the highest priority on improving NHIS statistics about the black and Hispanic populations despite interviewing just one random adult per household. Deviations from optimal design features for this objective were tolerated where the effect on the primary objective was not severe.

## Primary Sampling Unit Definition and Measure of Size

The definition of PSU's and the measure of size used to select sample PSU's have important impacts on between-PSU variances. For PSU's with cost functions such as NHIS, it is important that there be more heterogeneity within PSU's than across PSU's. Thus, for example, it would be inefficient to define the suburbs of a metropolitan area as a PSU separate from the central city. With respect to the measure of size, the between-PSU variance on a particular statistic is closely related to the correlation between the measure of size and the PSU totals for the characteristic of interest. If the correlation is perfect, the between-PSU variance will be zero.

Unfortunately, it is extremely difficult to project the between-PSU variances that would result from alternate definitions and/or measures of size. The current survey can provide information only about between-PSU variance for the current definition and measure of size. To answer this question objectively, it is necessary to have precise county-level summary information on the characteristic of interest for every county in the Nation. Such information exists for characteristics tabulated in the decennial census, the Census of Agriculture, the Economic Census, Internal Revenue Service studies of payrolls, and other sources. Unfortunately, none of these sources has much information that is relevant to NHIS. NCHS also has the U.S. Department of Health and Human Services Area Reference File that contains county-level information on hospitals and doctors as well as other health providers. These, however, still have very little to do with the aspects of health about which the population is queried in NHIS. Some conclusions can be reached using basic demographic data from the census, but much of the work in this area is highly speculative and driven by expert opinion.

### Primary Sampling Unit Definition

A study conducted by the U. S. Bureau of the Census showed that using the Health Care Service Areas as PSU's would decrease survey precision, assuming that the budget was held constant (32). This is because the HCSA's tend to be much larger in terms of land area than traditional PSU's. The larger land area would mean more within-PSU travel expense. To offset that increase in cost, it would be necessary to reduce the number of sample PSU's. The decrease in the number of sample PSU's would decrease survey precision. Although there is some question about just how large a budget-neutral cut in the number of sample PSU's would have to be and some question about the impact of the cut on between-PSU variance, Westat, Inc., agreed with the overall thrust of this research.

Having ruled out HCSA's as PSU's, the next question was whether the PSU definitions used for any of the other current surveys at the U. S. Bureau of the Census might be suitable for NHIS. The PSU's for the *Current Population*

*Survey* respect State and metropolitan statistical area (MSA) boundaries (as defined in 1990). Given the desire to make NHIS flexible for State expansions, the NHIS PSU's need to respect State boundaries. So from that perspective, the CPS PSU's were attractive. Administrative data that might be useful in small area estimation projects, however, are generally available only at the county level, even in New England, where MSA's violate county boundaries. Thus, the CPS PSU's in New England were unsuitable for NHIS. The PSU's for the Survey of Income and Program Participation (SIPP) do not respect State boundaries outside New England and are thus unsuitable for NHIS, but the SIPP PSU's in New England respect State and NECMA (New England county metropolitan area) boundaries, which means they also respect county boundaries.

The final NHIS PSU's were therefore defined to be coincident with CPS PSU's outside New England and SIPP PSU's within New England. This resulted in 1,955 PSU's.

### Primary Sampling Unit Measure of Size

Based on the research described in chapters 4 and 13, the recommendation was made to use total population (all ages) as the measure of size. This is the same measure of size that was used in the 1985 redesign.

## Primary Sampling Unit Stratification

The stratification performed for the 1985 redesign was extremely efficient for regional and national statistics. Despite a drop in the number of sample PSU's from 376 in 1984 to 198 in 1987 and beyond (because of budget problems, not all of the 198 sample PSU's were used in 1985 and 1986), the level of between-PSU variance stayed about the same. (Estimates of between-PSU variances are quite unstable from both surveys; this instability may be masking some level of change.) The 1985 stratification was based on multivariate clustering techniques and used more than a dozen dimensions to define similarity between PSU's. This efficiency is particularly impressive given the fact that health characteristics were not among the dimensions used for stratification. The clustering program appears to have identified natural clusters that made for efficient strata for a wide range of statistics.

One type of statistic was not well served by the stratification, however. Statistics for individual States are subject to very high between-PSU variances in the 1985–94 design. This is because the strata do not respect State boundaries. (The strata for the 1973–84 design did not respect State boundaries either, leading to similar high between-PSU variances for State statistics.) PSU's with similar demographic, social, and economic characteristics were grouped together from different States. The *cv* on many State estimates is 100 percent or worse.

Because the flexibility to supplement NHIS for specific States was a major goal of the 1995 redesign, it was decided that the strata should strictly respect State boundaries. To counteract the increase in between-PSU variances that would affect regional and national statistics, it was also decided to force the strata to respect metropolitan status and to increase the number of sample PSU's to a level comparable to that of the 1973–84 design.

## Primary Sampling Unit Selection

The methodology used for selecting PSU's was much the same for 1995 as for 1985. In strata where two PSU's were selected, the U.S. Bureau of the Census selected two PSU's without replacement using the Brewer-Durbin methodology. The measure of size was total population (all ages). No attempt was made to maximize or minimize overlap with the old NHIS design or with the new design for the CPS or any other current survey at the U.S. Bureau of the Census. The only difference from 1985 was that some strata were quite small as a result of respecting State boundaries and metropolitan status. In these small strata, just one sample PSU was selected with probability proportional to total population.

## Selection of Blocks

As discussed in chapter 1, NHIS utilizes a mix of area and permit sampling rather than the list sampling used by most of the other demographic surveys conducted by the U.S. Bureau of the Census. To facilitate coordination across surveys and the preparation of field materials, dummy entities are set up to represent all the housing units (HU's) and group-quarter equivalents (GQE's) reported in the 1990 census to exist in a PSU. (A GQE is a collection of usually three persons in noninstitutional group quarters. This collection is counted as equivalent to a household for sampling purposes.) Census Bureau headquarters personnel select placeholders rather than HU's and GQE's directly for NHIS. Census Bureau field personnel then list the blocks that contain the sample placeholders. Census Bureau headquarters personnel then draw a correspondence between the sample placeholders and the physically listed HU's and GQE's to identify the final sample of NHIS.

The selection of placeholders involves two stages. First, blocks must be selected. As the first step in block selection, these placeholders were sorted by the characteristics of their parent blocks and, within that sort, by their block labels. One of the block characteristics used in the sort was minority density stratum, as described later.

Blocks with fewer than four known HU's and GQE's were combined with neighboring blocks and given a combined block label. A larger minimum combined block size was considered but rejected by U.S. Bureau of the Census staff because of concerns about coordination with other current surveys.

Each block was classified into one of the 24 minority density strata defined in table 59. A different optimal cluster size was identified for each minority density stratum. The possibilities were

**Table 59. Initial cluster sizes (in terms of placeholders)**

| Percent of block that is black | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 16 | 16 | 16 | 12 | 8 |
| 10–30 | 12 | 12 | 12 | 8 | 8 |
| 30–60 | 12 | 12 | 12 | 8 | 8 |
| 60+ | 8 | 8 | 8 | 8 | 8 |

8, 12, and 16. Although intermediate sizes, not multiples of 4, might have been even better for NHIS, coordination with the Bureau's other current surveys required the clusters be multiples of 4. The formula used to determine the optimal cluster size for the *i*-th stratum was

$$\lambda_i = 4\left[\frac{8}{1 - O_i(1 - r_i)} \times \frac{1}{4} + 0.5\right]$$

where $O_i$ is the proportion of the stratum that is neither black nor Hispanic (derived from tables 17 and 18), and $r_i$ is the planned retention rate for such other households (table 22). The idea of the formula is to inflate the cluster size enough so that eight households are still designated for full interviews after screening and subsampling. Although it would have been easier to equalize the number of placeholders per cluster, it was thought that field operations would be more efficient with a more uniform number of full interviews per cluster.

The placeholders were strung together into initial clusters of corresponding lengths. Each such cluster is also known as a segment. (The word ''segment'' has two common usages at the U.S. Bureau of the Census. It is also used to refer to the block or collection of blocks from which the HU's and GQE's are drawn corresponding to the placeholders. For example, a block might have 35 HU's, with 8 scheduled for interview in NHIS in a particular year. The 8 and the 35 are referred to as a ''segment.'') Consecutive groups of 10 initial clusters were strung together to provide enough sample for 10 years of interviewing. Thus, each string consisted of 80, 120, or 160 placeholders.

A systematic sample was then drawn of the strings from each PSU with a single sampling interval of $40 \times \Pr\{\text{PSU}\}$ and a single random start. (PSU's with a probability of selection less than 1/40 were grouped into rotating clusters.) This sampling plan will yield an overall sample of 1/40th of all U.S. housing. The number 40 was chosen so that the sample would yield 10 samples of 200,000 interviewed households each. The number 200,000 was chosen because it corresponds to 4 times 50,000, and the largest oversampling ratio specified in table 20 is 4.0. No measure of size was used in the selection, so long strings had the same probability of selection as short strings. (Because string length is inversely related to frequency, the probability of selection for an individual placeholder is 1/40 whether it is in a long or short string.) This resulted in a random clustered equiprobability sample comprising 2.5 percent of the universe. The placeholders in this sample are reserved for the sole use of NHIS even though NHIS will use only a subsample of them. The reason for not sharing the unused portion with other current surveys at the U.S. Bureau of the Census is that the unused NHIS subsample will not represent the entire U.S. population.

As is discussed in chapter 5, the best plan identified for nonelderly black persons and Hispanic persons involves a combination of oversampling and screening. The strata and oversampling rates desired for the oversampling are those shown in table 20. The initial string selection previously described resulted in an oversampling rate of 4 in all the strata. Thus, some strings had to be dropped in some strata. The string retention rates are shown in table 60. These rates were calculated by dividing the desired oversampling rates in table 20 by 4.

## Within-Block Selection

At the end of block selection, the U.S. Bureau of the Census had a list of sample blocks and, for each block, a count of how many placeholders were to be allocated to NHIS. They have procedures in place to list physically the residential structures in the block and to line them up with the placeholders. These procedures guarantee that the HU's and GQE's assigned to each survey will be nonoverlapping even if several current surveys have selected the block. To make this possible, all blocks that were selected by NHIS had to be converted to area-type sampling procedures for all the current surveys, even though good address registers exist for many of the blocks, and the other

**Table 60. Percent of initially selected strings to be retained for actual listing (all others are put aside)**

| Percent of block that is black | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 40.0 | 50.0 | 80.0 | 100.0 | 100.0 |
| 10–30 | 40.0 | 50.0 | 87.5 | 100.0 | 100.0 |
| 30–60 | 42.5 | 50.0 | 87.5 | 100.0 | 100.0 |
| 60+ | 48.8 | 50.0 | 87.5 | 100.0 | 100.0 |

surveys could have used them to select samples more cheaply. The listing began sometime in 1994.

Westat, Inc.'s, projection is that the listing operation for the year 2000 will yield 123,900 HU's and GQE's to be screened. Of course, there is considerable uncertainty when projecting the results even a few years into the future. Factors that can affect the accuracy of the projection include new housing construction rates, housing demolition rates, residential/nonresidential conversion rates, and the growth rate of the population living in group quarters such as dormitories. The details of the listing procedures can also affect the accuracy of the projections. It is important to note that the number of units listed each year is likely to increase from 1995 through 2004 and that as a result, some adjustment of the string retention rates might be required over time.

## Screening

The new sample was introduced in January 1995. Screening of the sample housing units began immediately. All households with black or Hispanic occupants will be retained for full interviews. Other households will be retained at the rates shown in table 22.

Of the 123,900 HU's and GQE's that are projected to be designated for screening in the year 2000, not all will have cooperative occupants, some will have no occupants at all, and some will not even be inhabitable. The U.S. Bureau of the Census made a projection that in the average year about 80 percent of the listed units will have occupants who will cooperate with the screening effort. These 99,100 successfully screened HU's are expected to consist of 12,300 black households, 12,500 Hispanic households, and 74,400 ''other'' households.

Turning to the mechanics of screening, the exact procedures that are used to cope with HU's that are difficult to screen will depend on whether laptop computers are used by the interviewers as they screen. The screening procedures are described separately for CAPI and paper-based screening.

(The next paragraph describes a proposed procedure for CAPI screening. However, when CAPI interviewing was implemented in 1997, this procedure was not implemented; the paper-based screening process described later was retained.)

With CAPI screening, the interviewer will approach the door with the computer to enter the race and ethnicity of the occupants. The computer will have preloaded instructions to guide the interviewer through the regular NHIS interview or to terminate the interview after ascertaining the race and ethnicity of all occupants. If any of the occupants are black or Hispanic, the decision will always be for a regular interview. If none of the occupants are either black or Hispanic, the computer will draw a random number to decide whether to proceed with a full interview or to terminate. The random decision will give the correct retention probability for these other households, but there will be some variability from expected yields. Consideration was given to coordinating the random draws across households, but this will probably not be feasible across interviewers, and it is not clear that it is worth the effort to coordinate across households screened by a single interviewer.

With paper-based procedures, interviewers will have an instruction for each assigned HU on whether to conduct a regular interview regardless of the race or ethnicity of the household occupants (an ''I'' instruction for interview) or whether to only conduct the regular interview if at least one of the occupants is either black or Hispanic (an ''S'' instruction for screen). These instructions will be made up in the central office after the listing phase for sample blocks and will ensure the desired retention probabilities. Furthermore, it will be possible to coordinate the sampling instructions across interviewers. However, there will still be some variance about expected yields as can be seen from the possibility that just by chance, the households that are neither black nor Hispanic could happen to fall into the ''I'' category at disproportionate rates.

With either procedure, allowance needs to be made for housing units that are difficult or impossible to interview, even just to get the race and ethnicity of the occupants. To keep response rates high without raising costs exorbitantly through many repeat visits, interviewers will be trained to use information from neighbors under some circumstances. (Screening using neighbors was discontinued in 1997.) After two unsuccessful attempts on different days to find anyone at home, interviewers will call on neighbors. With CAPI, interviewers will be required to obtain consistent information from two neighbors before accepting their reports on the race and ethnicity of the targeted household. Upon obtaining consistent information, the computer will decide whether additional attempts are needed to conduct a regular interview or whether the HU can be closed as successfully screened and subsampled out. In the latter case, considerable expense will have been saved. In the first case, the interviewer is not much worse off than prior to contacting the neighbors, and valuable information has been gathered to inform the weighting procedures for nonresponse adjustment even if the household is never interviewed. Note that the base weights are formed based on the neighbor information and are unbiased even if the neighbor information turns out to be wrong.

With paper-based procedures, the interviewer will never bother to contact neighbors for an HU that is marked ''I.'' For an HU marked ''S,'' the interviewer will start to contact neighbors after two unsuccessful attempts at direct contact on different days. If the first neighbor says that no one in the targeted household is black or Hispanic, the interviewer will close the case as successfully screened out. Otherwise, the interviewer will try a second neighbor. If both neighbors say that the household has some members who are black or Hispanic, the interviewer will resume attempts to contact the household directly. If the interviewer eventually succeeds and it turns out that the neighbor's information was wrong (there are not any members who are black or Hispanic), the interviewer will close the case as successfully screened out. Note that this procedure results in a slight bias in the baseweights, but this bias should be quite small. To make the paper-based procedure unbiased, it would be necessary to seek neighbor information on all the ''I'' households that are difficult to contact. It was the U.S. Bureau of the Census' decision that using neighbors for ''I'' households would be expensive and incomprehensible to interviewers.

Because there was some initial skepticism about using neighbors at all, the U.S. Bureau of the Census Bureau conducted a field test of screening procedures in the summer of 1993. They focused in particular on the accuracy of neighbor information on race and ethnic origin. The results were strongly supportive of the utility of neighbor contacts during screening. Information about race from neighbors was shown to be extremely accurate, at least for the dichotomy of black versus not black. Information about Hispanic origin was less reliable but still useful.

## New Construction

A plan was considered where new construction would be treated the same as the stratum for old construction least dense with minorities (under 10 percent black and under 5 percent Hispanic). This would have required taking a considerably larger sample of new construction and then screening it for minorities. This recommendation was rejected out of concern that the new construction sample would be more expensive to screen because it is difficult to cluster effectively. There was also a feeling that the new construction universe would contain even fewer minorities than the least minority-dense old construction stratum. (This supposition may be tested with the 1995 design after implementation. Such a study would provide useful input to the 2005 redesign.) Current plans are not specific on the sampling interval to be used in new construction, but it is likely to be larger than that used in the 1985–94 design.

## Dual-Frame Sampling for Elderly Minorities

The oversampling and screening plans described earlier will not yield satisfactory precision for elderly black persons or elderly Hispanic persons unless NCHS allows all elderly minority adults found in a household to be interviewed, in addition to a randomly selected adult between the ages of 18 and 64. These groups are simply too rare to allow within-household subsampling. As discussed in chapter 6, there are attractive dual-frame sampling systems employing SSA lists that could improve the precision for elderly black persons and Hispanic persons at reasonable cost, while keeping the restriction that only one adult be interviewed per household. Such a dual-frame system is not part of the formal plan for the 1995 design at this time. As of this writing, a dual-frame system may be implemented in 1996. Table 61 shows the projected effective sample sizes for elderly black persons and Hispanic persons from the planned design (the alpha option). It also shows the desired effective sample sizes, the projected effective sample sizes that could be attained with dual-frame sampling, and the numbers of additional screening interviews that would be necessary to obtain those effective sample sizes with dual-frame sampling.

## Panel Construction

This section begins with a review of panel formation in 1985 and continues with a review of the objectives for panels, a discussion of the additional complexities for 1995, a description of some adjustments to the methods used in 1985, and a couple of brief notes on implications for weighting.

### Panel Design for 1985

Four panels were defined in the 1985 design. To support panel definition, self-representing PSU's were divided into three classes: Large, medium, and small. The panels were defined at the PSU level for NSR PSU's and small SR PSU's. They were defined at the assignment level for large- and medium-sized SR PSU's. (This may not have been done in 1985 itself but was in place for 1986 and beyond.) An assignment was a collection of segments within a PSU that were scheduled to be interviewed in the same week. A PSU was defined as the first-stage unit in sample selection, not as the unit for interviewer assignment as used for field management and control. The panels were defined by first forming pairs of strata and then randomly assigning panel designations 1 and 2 to the two PSU's in one stratum and 3 and 4 to the two PSU's in the other stratum within the

**Table 61. Sample sizes for elderly minorities**

| Domain | Desired | Effective sample sizes: Projected under alpha option (1 household) | Projected under alpha option (all households) | With extra screened 2,800 cases from SSA (1 household) | With extra screened 5,800 cases from SSA (1 household) |
|---|---|---|---|---|---|
| Black | | | | | |
| Male | 1,000 | 600 | 1,150 | 1,000 | 1,000 |
| Female | 1,000 | 740 | 1,710 | 1,000 | 1,000 |
| Hispanic | | | | | |
| Male | 1,000 | 350 | 910 | 770 | 1,000 |
| Female | 1,000 | 495 | 1,270 | 920 | 1,000 |

superstratum. The random assignment was not independent across superstrata. Dependence was created to better equalize panel sizes. For example, if a PSU with a large workload was assigned to panel 1 in one superstratum, a PSU with a small workload was chosen for panel 1 in the next superstratum. The panels were exhaustive and mutually exclusive. Each panel was an equally valid national sample. (See *Design and Estimation for the National Health Interview Survey, 1985–94* (33) for more details on 1985 procedures.)

## Objectives

Panels had several uses in the 1985 design. The first use was sample reduction. When adequate funds were not available in 1985 and 1986 for the full sample design, the panels were a quick and efficient way to cut back the sample because whole NSR PSU's and complete assignments in SR PSU's were dropped.

The second use was advance selection of a conveniently sized PSU sample for occasional surveys such as the National Hospital Discharge Survey. Selecting general purpose designs in advance saves some time and effort if a survey comes along that needs approximately that number of PSU's. There are also some operational efficiencies in terms of knowing where the U.S. Bureau of the Census should maintain an interviewing potential. Subsamples could obviously be drawn when necessary, but it seemed convenient to have plans in place for such subsampling.

The third use of the panels was splitting retired NHIS samples up among competing followup surveys such as the National Survey of Family Growth. By stating that survey A could have panels 1 and 3, and survey B could have panels 2 and 4, none of the sample was wasted or used twice. It is not clear whether this capability was actually used. Whether this would have been the best way to split the sample between the two surveys is also unclear. For some pairs of surveys, it would probably have been better to randomize segments or even to split each segment between the competing surveys.

The fourth use of the panels was conducting randomized experiments with the NHIS sample. This potential does not appear to have been used. The panels would not necessarily have yielded a good experimental design anyway, because randomization would have been at a high level in the sampling, allowing between-PSU and between-interviewer variances to obscure the results. A better design for many purposes would be obtained by splitting each work assignment, or even each segment, between treatments.

Given the utilization patterns of the panel structure in the 1985–94 decade, it seems clear that the highest priority in designing panels for the next decade should be on the potential for orderly cuts in the sample. (Cutting PSU's makes sense if a desire exists to reduce costs while maintaining the same relative sampling rates for minorities as proposed for the full design. If, instead, the decision is made to drop back to an equiprobability sample, dropping PSU's is not the best way to cut the sample. See chapter 17 for more details.) Based on the cuts experienced in 1985 and 1986 (25 percent and 50 percent, respectively) and the fact that the number of PSU's in the 1995 design was chosen based on the assumption of a 50-percent budget increase, it is convenient to have panels that can make cuts of the following magnitudes easy: 25 percent, 33 percent, 50 percent, 67 percent. The second priority should be to have conveniently sized, first-stage designs available for the provider surveys and other new or occasional surveys; or at least there should be a clear mechanism for generating appropriately sized subsamples of the NHIS first-stage design with short lead times. The uses for splitting the sample for either follow-on surveys or experiments seem less important and should probably not be considered at all. It also seems reasonable to abandon all support for State estimates in subdesigns.

## Problems for 1995

The NSR strata in the 1985 design were roughly equal in size. Two PSU's were selected from each NSR stratum. Neither of these two statements applies to the 1995 design. To provide the flexibility in the 1995 design for State expansions, the NSR strata were defined by mostly State and metropolitan status. In slightly fewer than one-half of the State-by-metro-status domains, it was possible to stratify more deeply than that. The most strata that were defined within a State-by-metro-status domain was five. In States where either the NSR metropolitan population or the nonmetropolitan population is small, only one PSU was selected from the stratum instead of two. In larger strata, two PSU's were selected. Some smaller metropolitan areas were selected with certainty because they made up a large portion of the State metropolitan population. The total number of PSU's in the 1995 design is 358. (Consolidated metropolitan areas are just counted once, regardless of how many administrative PSU's they might be divided into. A number of smaller metropolitan areas are counted twice, however, because they cross State lines and multiple fragments were selected.)

To minimize the variation in baseweights for a reduced NHIS, it is important to form the superstrata in such a manner that the number of full-design sample PSU's per superstratum is constant and the original strata within a superstratum are nearly constant in size. Also, the full-design sample PSU's should be subsampled with equal probability within a superstratum. It does not matter for this objective whether the superstrata vary in size.

For the second objective of having the subdesigns serve as appropriate first-stage samples for provider surveys, however, the superstrata for subsampling PSU's should be nearly equal in size (population, health care providers, health care events, etc.). This means collapsing unequal numbers of original strata of various sizes together to form superstrata. For this objective, the full-design sample PSU's should be subsampled with probability proportionate to stratum size. If one superstratum contains seven sample PSU's and another contains three, there is no simple way of coming up with exhaustive and mutually exclusive sets of NSR PSU's.

## Panel Construction for 1995

(This section describes panel structures created by Westat, Inc., and/or the U.S. Bureau of the Census. Subsequently, NCHS created a panel structure consisting of four subsets; that is the panel structure that has been used in the 1995–2004 redesign.)

It was decided to construct two distinct panel structures. For the first of these, to be constructed immediately, it was decided that the same general procedures would be followed as in 1985, forming superstrata with a fixed number of sample PSU's per superstratum and then subsampling with equal probabilities. For the second structure, to be constructed when needed, it was decided to form superstrata with equal populations and then to subsample with probability proportionate to stratum population. The plans for each of these structures is discussed in more detail later. The first panel structure will serve the first, third, and fourth objectives just discussed. The second panel structure will efficiently satisfy the second objective. At the conclusion of this section, the penalties that would be suffered if the decision were reached to use the first panel structure to serve the second objective are discussed.

*Reduction panels*—The general procedures used in 1985 need only minor adjustments. The most significant of these adjustments is to provide the capability of cuts of 16.7, 25.0, 33.3, 50.0, 66.7, 75.0, and 83.3 percent. To provide this capability, two panel structures will actually be created for reduction purposes—one with four panels and one with six. Both will be created using the same methodology. Only the four-PSU case is discussed in detail here, but the generalization to six PSU's is straightforward.

The first adjustment to the old four-PSU procedure is that, rather than pairing only two 2-ps strata (strata in which two PSU's are selected per stratum), it will be necessary to group together either two 2-ps strata, one 2-ps stratum and two 1-ps strata, or four 1-ps strata. (If one 2-ps stratum is paired with two 1-ps strata, it is important that the 1-ps strata be roughly equal in size and half the size of the 2-ps stratum.) The second adjustment is that it may be more difficult to pair strata that are equal in size while still respecting regional and metropolitan category boundaries. On the one hand, original strata vary much more in size, and on the other hand, there are more original strata to work with. Although adequate data were not available at the time of this writing to project the numbers of SR PSU's by size class that the procedure will yield, this will be a routine task for staff at the U.S. Bureau of the Census. It is currently anticipated that the resulting set of panels could yield nested subdesigns with roughly 110 PSU's, 200 PSU's, 280 PSU's, and 358 PSU's. (Of course, the set of six panels will provide additional options beyond these.)

Within the large and medium SR PSU's, the reduction panels can be set up along the same lines as in 1985—that is, by systematically subsampling assignments. Current plans call for assignments to be all roughly equal in size, so there will be no need to incorporate a measure of size into the sampling. The sort for the subsampling should probably include region, PSU, administrative PSU, and an indicator of whether the assignment is predominantly central city or suburb. The systematic subsampling should probably be done across PSU's with alternating sorts on the indicators for central city status. The subsampling interval should be four so that the populations in SR and NSR PSU's have the same probabilities of selection.

An additional feature of the procedure for 1985 for assigning panel numbers to the four subsets of the PSU's will be retained: Either expected or actual NHIS sample sizes (in terms of segment counts) by sample PSU will be used to try to equalize total panel sample sizes.

*Subdesigns for other surveys*—For a number of reasons, it was decided to delay forming panel structures for other surveys. Although this work could be done in advance, the panel structures will be more efficient if designed after the actual requirements of other surveys are known. That will allow different sorts and specialized measures of size. For example, for a survey of hospices, data on local numbers and sizes of hospices could be used for stratification and also be built into the measure of size of each PSU to create a more efficient design.

As was discussed in chapter 3, further research is planned on the benefits of concentrating all the provider surveys into a single set of PSU's. Based on current research, however, and assuming that U.S. Bureau of the Census interviewers will be visiting all 358 NHIS PSU's (i.e., that there are no sample cuts), it is unclear whether there are substantial additional operational efficiencies from concentrating all the provider surveys into a single subset of NHIS. This suggests that it might be best to custom-select a different subset of the NHIS PSU's for each of the provider surveys.

It may be determined later that there are important benefits in having the provider surveys nested within each other. The rest of this section indicates a reasonable method for creating such a set of subdesigns.

Two surveys that are likely to share the NHIS design are NHDS and NAMCS. These have recently (the early 1990's) been carried out in the 112 PSU two-panel subdesign of the 1985 NHIS. Another recurring survey is the National Nursing Home Survey (NNHS). It has traditionally not been clustered. To ease into clustering so as to avoid large risks in the changeover process, it might make sense to have a design with as many as 200 PSU's for it. (With more than 1,200 sample facilities, even 200 PSU's should generate enough clustering to yield substantial operational efficiencies.) After having been fielded once with 200 PSU's, it would be possible to study the components of variance to see whether a smaller number of PSU's would serve survey objectives adequately. Other surveys of providers are planned but are brand new, so there is no basis for choosing an optimal number of PSU's for them. For demographic surveys, past (not publicly documented) experience has shown that between-PSU variance on minority estimates is important at 100 PSU's and small at 200 PSU's.

Interpolation between those extremes is uncertain and not attempted here.

The 52 largest SR PSU's in the full 1995 design (358 PSU's) were identified with the thought that they would be appropriate for a national survey of approximately 200 PSU's. Additional SR PSU's were identified for State purposes, but there is no guarantee that all the PSU's that should be SR for a national survey of more than 200 PSU's are in the sample. Thus, it is recommended that the largest subdesign contain 200 sample PSU's. These 200 should be selected in such a manner that each sample PSU represents an approximately equal number of persons. Once that subdesign has been selected, it will then be possible to split this set of 200 sample PSU's into panels in the same manner as was done for the 198 PSU's in the full 1985 design. In this manner a set of nested subdesigns can be created with 62 PSU's, 113 PSU's, 155 PSU's, and 200 PSU's.

The 306 full-design sample PSU's outside of the 52 largest consolidated metropolitan areas should be grouped into about 74 superstrata. This means an average of 4.2 sample PSU's per superstratum. The superstrata should be formed by applying the stratification software developed at the U.S. Bureau of the Census directly to the 306 PSU's. The measure of size for a PSU should be defined as the stratum population represented by the PSU (one-half of stratum population in the case of PSU's from 2-ps strata). Substantive characteristics of the sample PSU itself should be used to define similarity rather than stratum characteristics. (It is permissible and indeed most efficient to use the characteristics of the sample PSU itself rather than just those of the stratum that it represents. However, unbiased estimates of variance are then not possible, and reasonable estimates of variance components are likely to be particularly problematic. See ''Using Sample Information for Stratification'' (34).) The superstrata should be constrained to respect region and metro status and to be roughly equal in size. Respecting region and metro status is important because these are likely variables in NCHS reports. Keeping the superstrata sizes roughly equal is important because it reduces design effects and gives greater control over workloads. (Workload control is not that important for NHIS with its itinerant interviewers but may be for the provider surveys.) Of course, it is very likely that it will be difficult to find a stratification that respects all these constraints and has internally homogeneous strata. The phrase ''roughly equal'' already provides some slack, but it may be necessary to relax the constraint on strictly metro and nonmetro superstrata. This would not be catastrophic because the definitions of metropolitan areas change over time anyway.

Within each superstratum, two PSU's should be selected by the Brewer-Durbin method with probability proportionate to the measure of size defined earlier. Then the stratification program should be applied to the superstrata to form hyper-superstrata. The hyper-superstrata should also respect regional and metropolitan boundaries to the greatest extent compatible with roughly equal sizes for the hyper-superstrata. One superstratum should then be selected from each hyper-superstratum with probability proportionate to superstratum size. With four sample PSU's per hyper-superstrata, the four subpanels can then easily be identified.

An alternative that is less efficient but more elegant to describe is to form just 37 superstrata to begin with and then select four PSU's from each superstratum by either the Durbin-Sampford method or the Rao-Hartley-Cochran method with probability proportionate to the measure of size defined earlier. The Durbin-Sampford method is generally more efficient than the Rao-Hartley-Cochran method but is substantially more difficult to implement for $n = 4$.

Why not a unified set of panels? Two distinct panel structures have been recommended in this report because each will be more efficient for a particular objective and because no serious disadvantages are foreseen. To play the devil's advocate, however, suppose there were a cut in the NHIS sample size to 280 PSU's and there were a survey of dentists in 200 PSU's for which the U.S. Bureau of the Census could promise substantial budget savings if the sample were concentrated within NHIS PSU's. Because some of the 200 PSU's in the national subdesign for provider surveys would probably have been among the 78 PSU's dropped (in this example) by NHIS, some efficiency would have been lost. If the cost savings are great enough, however, it would be possible to pick a new national design of 200 PSU's from the 280 still in the NHIS.

If the smaller design effects and more stable workloads associated with uniform superstratum sizes are not important for a particular provider survey or for some other piggyback survey, however, the subdesigns prepared for NHIS cuts could also be used for such a survey. To recapitulate the sizes that will be available under the plans recommended here, it will be easy to reduce the NHIS to (roughly) either 85, 110, 140, 200, 250, 280, or 300 PSU's from the planned 358 PSU's; and subdesigns readily available for surveys with a requirement for more equal workloads will have 62, 113, 155, or 200 PSU's.

Note that the two 200-PSU subdesigns will have quite different characteristics. The NHIS will be self-weighting (within block strata) across the set of 200 PSU's designed for a reduced NHIS, but NHIS workloads will vary widely across those 200 PSU's. NHIS will not be self-weighting across the set of 200 PSU's designed for a piggyback survey nor will it have equal workloads across those PSU's (because of oversampling minority density strata). However, a uniform survey of total population could be made self-weighting with equal workloads across the set of 200 PSU's designed for a piggyback survey.

## Weighting Implications

If it is necessary to breach metropolitan boundaries when assembling superstrata, it would probably make sense to incorporate a first-stage adjustment with metro and nonmetro cells into the weighting. Also, if a piggyback survey uses one of the fixed subpanels designed for multipurpose use even though the ideal measure of size for the survey is very different from total population, a first-stage adjustment based on the ideal measure of size should be incorporated into the weighting for that survey.

## Summary of Design Characteristics

Figure 1 shows the projected sample sizes for the new design. It shows the total number of listing lines designated for screening, total number of screened households, number of interviewed households, and effective person-sample sizes for both options of interviewing all adults and for interviewing just one adult per household.

### Primary Sampling Units

PSU's were selected for the 1995 design. The PSU's are clusters of whole adjacent counties. In nonmetropolitan areas, the clusters were formed subject to minimum constraints on population and maximum constraints on land area. In metropolitan areas, the PSU's are complete 1990 MSA's except where MSA's cross State lines and in New England. Multistate MSA's were split into State components. PSU's in New England were based on NECMA's.

After the PSU's had been defined, they were stratified by State and metropolitan status. Large MSA's were designated SR and placed into separate strata. In States with large numbers of NSR MSA PSU's or large numbers of nonmetropolitan PSU's, the PSU's were further stratified by income with the constraint that the strata within a State and metropolitan category be approximately equal in size.

After stratification, one or two PSU's were selected from each stratum with probability proportionate to size. The measure of size was the 1995 estimated total resident population (all ages). Where two PSU's were selected, the selection was done without replacement using the Brewer-Durbin method. A total of 358 PSU's were selected.

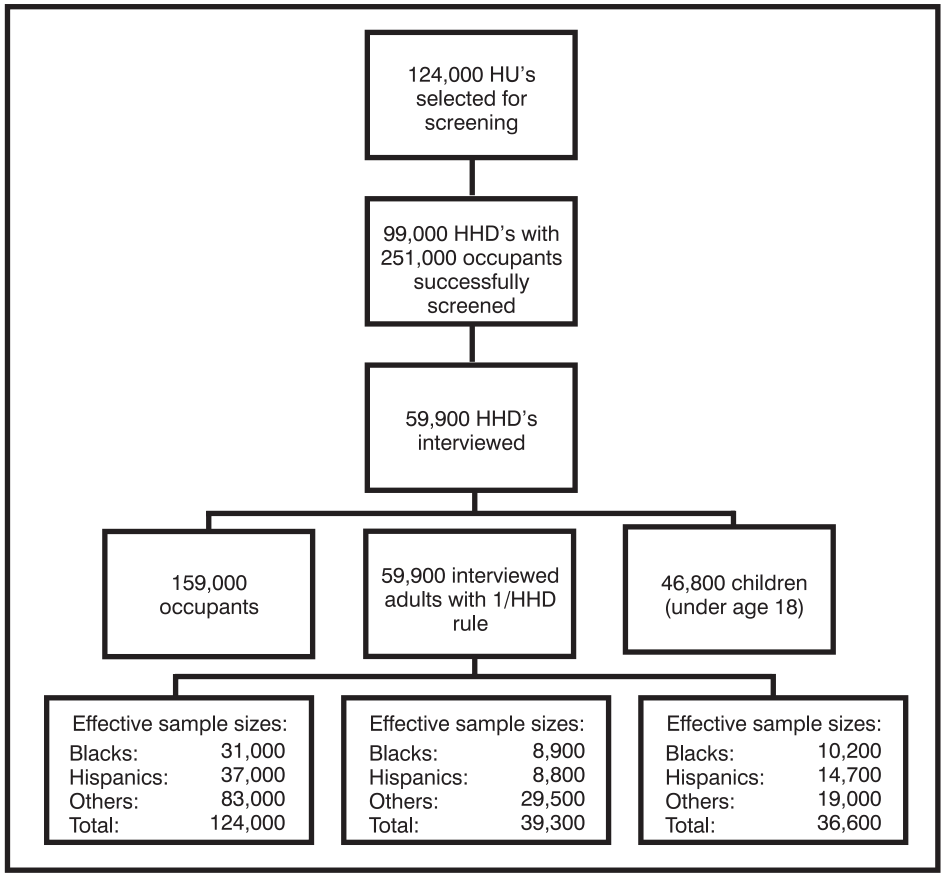


**Figure 1. Summary of sample sizes for new design (alpha option)**

### Household Clusters

Placeholders were created for all HU's reported in the 1990 census in a PSU. These placeholders were sorted by the characteristics of their parent blocks and, within that sort, by their block labels. (Blocks with fewer than four known housing units were collapsed with other blocks and given a combined block label.) One of the block characteristics used in the sort was whether there were substantial additional operational efficiencies from concentrating all the provider surveys into a single subset of NHIS. This suggests that it might be best to custom-select a different subset of the NHIS PSU's for each of the provider surveys.

# Chapter 17. Contingency Plans

At the time the research described in this report was performed, it was not clear whether the 50-percent budget increase needed to implement the recommended design would be available. Consequently, a number of contingency plans were developed. After intense study and discussion, one of these options was chosen as the fallback position. It is referred to as the beta option. (The intended redesign described in chapter 16 is referred to as the alpha option.)

In fact, in late 1994, a decision was made to implement the beta option rather than the alpha option. The beta option assumes the same level of funding as in 1988 (after adjustment for inflation). It calls for the continued oversampling of black persons at the current modest rate and for new oversampling of Hispanic persons at a rate high enough so that the precision for Hispanic estimates is comparable to that for black estimates.

Several other alternatives that were seriously considered are briefly described later for documentary purposes.

## The Beta Option

The beta option is similar to the recommended design but has a much lower level of screening. Instead of designating 123,900 HU's and GQE's for screening, only 71,500 will be designated. Also, the number of ''other'' households discovered during screening to be retained for full interviews will be reduced from 35,100 to 28,900. These two steps will reduce the cost from a level 50 percent over a survey with 50,000 regular interviews, to a level 5 percent below such a survey. Because the 1988 NHIS had about 47,500 regular interviews, these reductions will make the beta option close to budget-neutral relative to 1988, after correcting for inflation.

This cut was crafted in such a way that the precision for black statistics will be about the same as in 1988, the precision for Hispanic statistics will be sharply improved over 1988 levels to about the same level as the precision of black statistics, and the precision for all other statistics will drop fairly sharply relative to 1988. These decisions were guided by NCHS perceptions of precision needs for the domains. The basic directive was to hold the precision for black statistics steady, improve the precision for Hispanic statistics to bring them into parity with black statistics, and let the precision for other statistics fall into place as dictated by the budget.

Table 62 shows the oversampling rates that are projected to generate the required numbers of black and Hispanic households with an approximately optimal tradeoff between cost for screening and higher design effects for black and Hispanic statistics. These rates are relative to the sampling rate for a hypothetical equiprobability survey that would yield 50,000 completed interviews. These rates can be compared with those for the alpha option in table 20. Note that under the beta option, blocks with heavy concentrations of Hispanic persons and light concentrations of black persons are actually sampled more heavily than blocks with heavy concentrations of both minorities. This was arranged deliberately to gain parity between black and Hispanic effective sample sizes.

From a systems engineering standpoint, it was important that all sample clusters for the beta design be a subset of those selected for the alpha design. The U.S. Bureau of the Census had done a tremendous amount of work by the fall of 1994 to select and prepare the clusters for the alpha option. Any use of different clusters by the beta design would have resulted in at least a 2-year delay in the 1995 NHIS. Table 63 shows the percentages of the sample selected for the alpha option that will be needed for the beta option. These were calculated by the formula

**Table 62. Oversampling rates for the beta design (relative to a design that would yield 50,000 interviews)**

| Percent of block that is black | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 1.0 | 1.5 | 1.5 | 1.6 | 2.3 |
| 10–30 | 1.0 | 1.5 | 1.5 | 1.5 | 2.0 |
| 30–60 | 1.0 | 1.0 | 1.0 | 1.5 | 2.0 |
| 60+ | 1.0 | 1.0 | 1.2 | 1.5 | 1.5 |

NOTE: Compare with table 20 for the alpha design.

**Table 63. Percent of total sample reserved for alpha option to be retained for the beta option**

| Percent of block that is black | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 62.5 | 75.0 | 46.9 | 40.0 | 57.5 |
| 10–30 | 62.5 | 75.0 | 42.9 | 37.5 | 50.0 |
| 30–60 | 58.8 | 50.0 | 28.6 | 37.5 | 50.0 |
| 60+ | 51.3 | 50.0 | 34.3 | 37.5 | 37.5 |

$$keep_i = \frac{k_i'}{k_i}$$

where the numerator is the oversampling rate for the beta design from table 62 and the denominator is the oversampling rate for the alpha design from table 20.

The cut in the designated alpha sample will be achieved by a mixture of dropping whole clusters before listing and subsampling listed HU's in other clusters. Although it would have been possible to achieve the cut either by only dropping whole clusters or by only subsampling listed HU's within the whole set of clusters selected for the alpha option, the mixture approach will result in the smallest variation in the number of interviewed households per cluster. Table 64 shows the optimal cluster sizes for the beta design by stratum. These may be compared with those in table 59 and were calculated by the formulas given in chapter 16.

Let $y_i$ be the proportion of clusters selected for the alpha design that will be kept for the beta design. Let $z_i$ be the proportion of listed HU's (and GQE's) that would be used for the alpha design that still need to be used for the beta design. Let $s_i$ and $s_i'$ be the optimal cluster sizes for the alpha and beta designs, respectively. Then $y$ and $z$ are obtained by solving the set of equations

$$k_i' = y_i z_i k_i \quad \text{and}$$
$$s_i' = z_i s_i$$

The results of solving this system of equations are shown in tables 65 and 66.

Tables 67–70 parallel tables 21–24 showing persons in screened households by domain, retention rates for "other" households by stratum, effective sample sizes by domain when all household members are sampled, and effective sample sizes by domain when just one adult is sampled per household, respectively. These tables were derived using the same formulas and methods as discussed in chapter 5. As in chapter 5, the effective sample sizes reflect the reduced power of the sample (relative to a simple random sample of the same size) due to differential weights but not the reduced power due to clustering. The effective sample sizes are best compared with an equiprobability design that is subject to the same clustering as the 1985–94 design of NHIS.

The total number of screened households with cooperative occupants is projected to be about 57,200 in the year 2000 with a total of 144,000 occupants of all ages, of whom 106,000 will be 18 years and over. The distribution of these screened households across the domains of interest is shown in table 67. The total of 57,200 screened households is too many to retain for full interviews given an inflation-adjusted 1988 NHIS budget. According to the projection of a screening cost equal to one-third of a full interview cost, the total number of households that can be kept for full interviews is 41,400. This means that only 64.7 percent of the "other" households discovered during screening can be retained. The retention rates in table 68 restore the sample for "other"

**Table 64. Desired cluster sizes for beta design (in terms of placeholders)**

| | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| Percent of block that is black | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 12 | 12 | 12 | 12 | 8 |
| 10–30 | 12 | 12 | 12 | 8 | 8 |
| 30–60 | 8 | 8 | 8 | 8 | 8 |
| 60+ | 8 | 8 | 8 | 8 | 8 |

NOTE: Compare with table 59.

**Table 65. Percent of strings reserved for the alpha option to be retained for the beta option**

| | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| Percent of block that is black | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 83.3 | 100.0 | 62.5 | 40.0 | 57.5 |
| 10–30 | 62.5 | 75.0 | 42.9 | 37.5 | 50.0 |
| 30–60 | 88.2 | 75.0 | 42.9 | 37.5 | 50.0 |
| 60+ | 51.3 | 50.0 | 34.3 | 37.5 | 37.5 |

**Table 66. Percent of listing lines reserved for NHIS within strings retained for the beta option that are actually to be screened for the beta option**

| | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| Percent of block that is black | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 75 | 75 | 75 | 100 | 100 |
| 10–30 | 100 | 100 | 100 | 100 | 100 |
| 30–60 | 67 | 67 | 67 | 100 | 100 |
| 60+ | 100 | 100 | 100 | 100 | 100 |

**Table 67. Persons in screened households for the beta option**

| Race, Hispanic origin, and age | All adults in each household Male | Female | Total |
|---|---|---|---|
| **Black** | | | |
| Under 5 | 743 | 712 | 1,454 |
| 5–17 | 2,117 | 2,019 | 4,136 |
| 18–24 | 785 | 919 | 1,704 |
| 25–44 | 2,125 | 2,695 | 4,821 |
| 45–64 | 1,363 | 1,760 | 3,122 |
| 65 and over | 630 | 932 | 1,561 |
| Total | 7,762 | 9,036 | 16,798 |
| Total 18 and over | 4,903 | 6,305 | 11,208 |
| Households | . . . | . . . | 6,192 |
| **Hispanic** | | | |
| Under 5 | 1,275 | 1,221 | 2,496 |
| 5–17 | 3,064 | 2,926 | 5,989 |
| 18–24 | 1,161 | 1,123 | 2,284 |
| 25–44 | 3,243 | 3,165 | 6,408 |
| 45–64 | 1,457 | 1,650 | 3,107 |
| 65 and over | 525 | 736 | 1,261 |
| Total | 10,725 | 10,821 | 21,546 |
| Total 18 and over | 6,386 | 6,674 | 13,060 |
| Households | . . . | . . . | 6,340 |
| **Other** | | | |
| Under 5 | 3,080 | 2,919 | 5,999 |
| 5–17 | 9,306 | 8,825 | 18,131 |
| 18–24 | 4,476 | 4,517 | 8,993 |
| 25–44 | 15,450 | 15,670 | 31,119 |
| 45–64 | 12,693 | 13,178 | 25,871 |
| 65 and over | 6,265 | 9,055 | 15,320 |
| Total | 51,269 | 54,164 | 105,433 |
| Total 18 and over | 38,883 | 42,419 | 81,302 |
| Households | . . . | . . . | 44,671 |
| **Total** | | | |
| Under 5 | 5,098 | 4,852 | 9,950 |
| 5–17 | 14,486 | 13,771 | 28,257 |
| 18–24 | 6,422 | 6,558 | 12,981 |
| 25–44 | 20,817 | 21,530 | 42,347 |
| 45–64 | 15,512 | 16,588 | 32,100 |
| 65 and over | 7,420 | 10,722 | 18,142 |
| Total | 69,756 | 74,021 | 143,777 |
| Total 18 and over | 50,171 | 55,399 | 105,570 |
| Households | . . . | . . . | 57,204 |

. . . Category not applicable.

NOTE: Figures may not add to totals because of rounding.

**Table 68. Percentage of other households discovered during screening to be kept for full interviews, beta option**

| Percent of block that is black | Percent of block that is Hispanic < 5 | 5–10 | 10–30 | 30–60 | 60+ |
|---|---|---|---|---|---|
| < 10 | 71.2 | 47.4 | 47.4 | 44.5 | 30.9 |
| 10–30 | 71.2 | 47.4 | 47.4 | 47.4 | 35.6 |
| 30–60 | 71.2 | 71.2 | 71.2 | 47.4 | 35.6 |
| 60+ | 71.2 | 71.2 | 59.3 | 47.4 | 47.4 |

**Table 69. Effective sample sizes for beta design when all household members are sampled**

| Race, Hispanic origin, and age | All adults per household | | |
|---|---|---|---|
| | Male | Female | Total |
| Black | | | |
| Under 5 | 719 | 689 | 1,408 |
| 5–17 | 2,049 | 1,955 | 4,004 |
| 18–24 | 760 | 889 | 1,649 |
| 25–44 | 2,057 | 2,609 | 4,667 |
| 45–64 | 1,319 | 1,703 | 3,023 |
| 65 and over | 610 | 902 | 1,511 |
| Total | 7,514 | 8,748 | 16,262 |
| Total 18 and over | 4,746 | 6,104 | 10,850 |
| Households | . . . | . . . | 5,994 |
| Hispanic | | | |
| Under 5 | 1,179 | 1,129 | 2,307 |
| 5–17 | 2,831 | 2,704 | 5,535 |
| 18–24 | 1,073 | 1,038 | 2,111 |
| 25–44 | 2,997 | 2,925 | 5,922 |
| 45–64 | 1,346 | 1,525 | 2,872 |
| 65 and over | 486 | 680 | 1,165 |
| Total | 9,912 | 10,001 | 19,913 |
| Total 18 and over | 5,902 | 6,168 | 12,070 |
| Households | . . . | . . . | 5,859 |
| Other | | | |
| Under 5 | 1,993 | 1,889 | 3,882 |
| 5–17 | 6,021 | 5,710 | 11,731 |
| 18–24 | 2,896 | 2,922 | 5,818 |
| 25–44 | 9,996 | 10,138 | 20,134 |
| 45–64 | 8,212 | 8,526 | 16,738 |
| 65 and over | 4,053 | 5,859 | 9,912 |
| Total | 33,171 | 35,044 | 68,215 |
| Total 18 and over | 25,157 | 27,445 | 52,602 |
| Households | . . . | . . . | 28,902 |
| Total | | | |
| Under 5 | 3,335 | 3,173 | 6,508 |
| 5–17 | 9,606 | 9,128 | 18,734 |
| 18–24 | 4,305 | 4,410 | 8,716 |
| 25–44 | 14,080 | 14,601 | 28,681 |
| 45–64 | 10,715 | 11,434 | 22,149 |
| 65 and over | 5,165 | 7,469 | 12,634 |
| Total | 47,206 | 50,215 | 97,421 |
| Total 18 and over | 34,265 | 37,914 | 72,179 |
| Households | . . . | . . . | 37,821 |

. . . Category not applicable.

NOTE: Figures may not add to totals because of rounding.

households to an equiprobability basis. The design effects implied by the oversampling rates in table 62 are 1.033, 1.082, 1.000, and 1.095 for the black, Hispanic, ‘‘other,’’ and total populations, respectively. Application of these design effects to the raw sample sizes in table 67 and allowing for the random subsampling of ‘‘other’’ households leads to the effective sample sizes shown in table 69. These are the sample sizes that should be used to predict precision for the core questionnaire items that are asked of all household members. For questionnaire items that are administered to just one random selected adult per household, table 70 shows the effective sample sizes that can be used to predict precision.

Table 71 contrasts the alpha and beta options with both the old design (1988) and equiprobability samples. Note that under either option, the precision for black and Hispanic estimates will be comparable to each other. With the beta option, the effective sample size for ‘‘others’’ drops by about 20 percent, much steeper than the 4-percent drop under the alpha option. The effective sample size for total population declines by about 10 percent under the beta option, compared with about a 10-percent improvement under the alpha option. There are two equiprobability designs shown in the table. One consists of 47,000 interviewed households (the same size as the 1988 NHIS) and the other consists of 50,000 interviewed households (the design that was the basis of projections in chapter 4). The effective sample sizes for the black

**Table 70. Effective sample sizes for beta design when one adult is sampled per household**

| Race, Hispanic origin, and age | One adult per household | | |
|---|---|---|---|
| | Male | Female | Total |
| **Black** | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 328 | 384 | 712 |
| 25–44 | 888 | 1,126 | 2,014 |
| 45–64 | 569 | 735 | 1,305 |
| 65 and over | 263 | 389 | 652 |
| Total | . . . | . . . | . . . |
| Total 18 and over | 2,048 | 2,635 | 4,683 |
| Households | . . . | . . . | 5,994 |
| **Hispanic** | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 417 | 403 | 820 |
| 25–44 | 1,164 | 1,136 | 2,300 |
| 45–64 | 523 | 592 | 1,115 |
| 65 and over | 185 | 264 | 453 |
| Total | . . . | . . . | . . . |
| Total 18 and over | 2,292 | 2,395 | 4,688 |
| Households | . . . | . . . | 5,859 |
| **Other** | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 1,337 | 1,349 | 2,686 |
| 25–44 | 4,615 | 4,681 | 9,296 |
| 45–64 | 3,792 | 3,937 | 7,729 |
| 65 and over | 1,871 | 2,705 | 4,577 |
| Total | . . . | . . . | . . . |
| Total 18 and over | 11,616 | 12,672 | 24,288 |
| Households | . . . | . . . | 28,902 |
| **Total** | | | |
| Under 5 | . . . | . . . | . . . |
| 5–17 | . . . | . . . | . . . |
| 18–24 | 1,942 | 1,993 | 3,935 |
| 25–44 | 6,203 | 6,460 | 12,663 |
| 45–64 | 4,517 | 4,874 | 9,391 |
| 65 and over | 2,144 | 3,098 | 5,242 |
| Total | . . . | . . . | . . . |
| Total 18 and over | 14,805 | 16,425 | 31,230 |

. . . Category not applicable.

NOTE: Figures may not add to totals because of rounding.

population declines under either equiprobability design relative to the 1988 NHIS because of the oversampling of the black population that was done in the 1985–94 NHIS design. Because that oversampling was not very effective, the declines in the black effective sample size associated with a switch to an equiprobability design would be modest. The oversampling in the 1985–94 design also explains the increases in the effective sample sizes for the ‘‘other’’ and total populations under the equiprobability designs relative to the 1988 NHIS. The strong increases in the Hispanic effective sample sizes under the equiprobability design are mostly due to the projected growth of the American Hispanic population between 1988 and 2000. Finally, it is important to remember the caveats about the projections given in chapter 4. All the projections are very sensitive to undercoverage rates for the black and Hispanic populations, and it is well established that these rates fluctuate considerably from year to year for no known reason. Also, the projections of the growth of the Hispanic population are quite sensitive to immigration policies and economic conditions abroad and in the United States.

## New Construction Sampling

Sampling of permits for new residential buildings is being done about the same for the 1995 design as for the 1985 design. The permits carry no information on race or ethnicity of future occupants. Furthermore, it is extremely difficult to map individual permits into blocks defined for the 1990 decennial census. The main reason for

**Table 71. Effective adult sample sizes for the year 2000 by redesign option and population domain relative to the 1988 NHIS design when just one adult is sampled per sample household**

| | Budget | Total | Black | Hispanic | Other |
|---|---|---|---|---|---|
| 1988 NHIS | 95 | 35,600 | 4,500 | 2,200 | 30,900 |
| | | | Projections for the year 2000 | | |
| Alpha option | 150 | 38,400 | 8,900 | 8,800 | 29,600 |
| Beta option | 95 | 31,230 | 4,700 | 4,700 | 24,300 |
| Equiprobability | 95 | 39,500 | 4,100 | 3,000 | 32,400 |
| Equiprobability | 100 | 41,600 | 4,300 | 3,100 | 34,100 |
| | | | Percent change from 1988 | | |
| Alpha option | 150 | +8% | +97% | +299% | −4% |
| Beta option | 95 | −12% | +4% | +113% | −21% |
| Equiprobability | 95 | +11% | −8% | +35% | +5% |
| Equiprobability | 100 | +17% | −3% | +42% | +10% |

NOTE: Budget for 1988, adjusted for inflation, equals 95.

this difficulty is that much new construction takes place along new roads that did not exist at the last census. Also, sometimes the permits are identified in terms of legal language (e.g., plots, land grants) rather than by street addresses. Given that the occupants of newly constructed housing units tend to be disproportionately white and not Hispanic, and given the higher cost of screening permits, the decision was reached to sample permits at the same rate as ‘‘other’’ housing is sampled after screening. This means that new construction permits will be sampled at rather low rates and that any black or Hispanic households that happen to be found there will have rather large weights compared with other black and Hispanic households in the sample.

## Restoration of an Equiprobability Design at Level Cost and Other Contingency Plans

Another option seriously considered as a contingency plan was to drop back to an equiprobability sample that would cost the same as the 1988 survey after adjustment for inflation. (The idea here was an all-or-nothing approach on minority statistics.) One idea was to bring back dropped segments and then do a uniform cut. Theoretically, this would work because (as discussed in the fifth section of chapter 16) the U.S. Bureau of the Census initially selected a random clustered equiprobability 5-percent sample of the universe. Much of this sample in the lower minority density strata was set aside as not to be used. These ‘‘dropped’’ clusters are being reserved for NHIS use, but nothing is being done to make them ready for quick use. During the course of the redesign research, the U.S. Bureau of the Census estimated that 2 years or more of lead time would be required to restore any dropped clusters. This is a longer lead time than is compatible with quick response to funding contingencies. Thus, in designing a contingency plan to restore an equiprobability design, it was important to think of a way to do it without using the dropped clusters.

This is indeed possible. The key to the feasibility of restoring an equiprobability sample without using the dropped clusters is the fact that the proposed redesign includes large oversamples (relative to a design with 50,000 completes) in all strata for screening even after dropping initial clusters. In fact, after dropping clusters according to the alpha option, the sample is still 60 percent higher even in the lowest minority density stratum than under the old design. The only drawback to not being able to bring back dropped clusters on short notice is that the expected segment sizes will be larger than desired in several strata. The retention rates for an equiprobability sample (designed to yield about 48,500 completed household interviews) are shown in table 72.

Note that if the subsampling is done on the household level after listing in the low-minority-density strata rather than subsampling whole segments, the variation in segment size can be reduced considerably. The drawbacks of such a procedure would be decreased savings in listing cost and increased complexity in sampling systems. Tables 73 and 74 show retention rates at the segment and within-segment levels that would minimize the variation in segment size.

Projections of effective sample sizes for this plan are given in table 71. Note

**Table 72. Percent of sample originally selected that would be retained for an equiprobability design**

| Percent of block that is black | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 62.5 | 50.0 | 31.3 | 25.0 | 25.0 |
| 10–30 | 62.5 | 50.0 | 28.6 | 25.0 | 25.0 |
| 30–60 | 58.8 | 50.0 | 28.6 | 25.0 | 25.0 |
| 60+ | 51.3 | 50.0 | 28.6 | 25.0 | 25.0 |

**Table 73. Percentage of strings to keep in conjunction with post-listing subsampling for equiprobability design**

| Percent of block that is black | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 100.0 | 100.0 | 62.5 | 37.5 | 25.0 |
| 10–30 | 93.8 | 75.0 | 42.9 | 25.0 | 25.0 |
| 30–60 | 88.2 | 75.0 | 42.9 | 25.0 | 25.0 |
| 60+ | 51.3 | 50.0 | 28.6 | 25.0 | 25.0 |

**Table 74. Percentage of listed HU's to keep in retained strings in conjunction with string subsampling for equiprobability design**

| Percent of block that is black | Percent of block that is Hispanic | | | | |
|---|---|---|---|---|---|
| | < 5 | 5–10 | 10–30 | 30–60 | 60+ |
| < 10 | 62.5 | 50.0 | 50.0 | 66.7 | 100.0 |
| 10–30 | 66.7 | 66.7 | 66.7 | 100.0 | 100.0 |
| 30–60 | 66.7 | 66.7 | 66.7 | 100.0 | 100.0 |
| 60+ | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 |

that the effective sample size for black persons would drop relative to the 1988 design because they would not be oversampled. Effective sample sizes for Hispanic persons would rise simply because of their projected population growth.

By using the reduction panels described in chapter 16 in the section on 1995 panel construction, it is also possible to shrink the whole design proportionately.

# Chapter 18. Recommendations for Continuing Research

## Intercensal Tracking of Minority Populations Across Density Strata

As the decade progresses, it will be possible to use the NHIS to estimate the proportion of each domain living in each minority density stratum by using the technique used to estimate the 1988 distributions (as described in ''Cost Functions for NHIS and Implications for Survey Design'' (15)). Such tracking might indicate that adjustments should be made in the allocation of the sample across the strata. These adjustments can be made by adding and/or dropping segments (within the 2.5 percent reserved for NHIS) and by adjusting postscreening retention rates. A lag will occur, probably of several years, in implementing such adjustments. It is recommended that estimates of the proportion of minorities in the density strata be made every year. Furthermore, these should not be viewed as done only for research purposes but that consideration of modification of subsampling rates be performed at regular intervals.

## Updating Minority Density Areas

By the year 1998, the information on the distribution of minorities by density strata was 8 years old. Data from the 2000 census will not be available at the block level for another 3 years. Even when available, it will probably take several years to update the 1990 data about the blocks. Work comparing 1980 and 1988 showed significant movement of black persons out of areas that had strong black concentrations in 1980. Many of these migrants' new neighborhoods were also predominantly black by 1988, although they were mostly white in 1980. Similar trends may be expected for the 1990's. Black persons are predicted to continue to migrate out of heavily segregated inner-city neighborhoods into less segregated city and suburban neighborhoods. Some white persons are likely to leave these neighborhoods; if they do, new predominantly black neighborhoods will have formed. As this process continues, the oversample in the old black neighborhoods will yield smaller and smaller black samples, and the black persons who are found in the new, formerly white, areas will have large weights, reducing precision for minorities.

A middecade census with a crosswalk to 1990 blocks would largely solve this problem, but given the low probability of a mid-decade census, alternatives were sought to update the minority density of blocks, block groups, or even just tracts. Unfortunately, no viable alternatives are currently apparent. A study performed in a number of local jurisdictions found almost all of them do not maintain data bases that would be helpful (35). The sole exception was a school district involved in court-ordered desegregation.

It would be useful to repeat this study in 1998–99. It is possible that local area administrative requirements will make such data bases more common. They would permit updating of the density strata. The cost of this kind of research is quite modest.

## Coverage Improvement

As the research was underway, an aspect of NHIS emerged that had not been previously identified as a high-priority concern. NHIS coverage rates for adult black males (where coverage is defined as the proportion of the census ''adjusted'' total population that is covered in the survey) appears to be in the 60- to 75-percent range for most age groups. Coverage rates for Hispanic persons are not computed on a regular basis, but the U.S. Bureau of the Census' experience with *Current Population Survey* indicates that black and Hispanic male coverage rates are roughly comparable. Black female coverage is not as low as the male rates, but *Current Population Survey* evidence indicates that Hispanic female coverage is probably not much different than that for Hispanic males.

It is fairly obvious that the health and social characteristics of the uncovered population are quite different from those of the covered population. Furthermore, it is unlikely that the weighting procedures fully compensate for the differences. The resulting bias in the NHIS estimates could be quite serious, possibly more troubling than the bias due to accepting proxy response. NHIS research may have been focused too tightly on sampling error at the expense of bias. Unfortunately, this problem is extremely difficult to alleviate. At a meeting of staff members of NCHS, the U.S. Bureau of the Census, and Westat, Inc., a number of suggestions were made for additional research on the causes of the undercoverage. Westat, Inc., staff felt strongly that enough basic research had been done, however, and that it was time to start testing methods of changing NHIS procedures, using some of the ideas that had been put forward previously. The U.S. Bureau of the Census subsequently started to implement such research. For preliminary results, see ''Coverage Improvement from Experimental Residence Questions'' (36).

# References


1. Parsons V, Botman S, Malec D. State estimates for the National Health Interview Survey. In: Proceedings of the section on survey research methods. American Statistical Association. 854–9. 1989.
2. Alternate survey design for the National Health Care Survey. Contract number 200–93-7043.
3. Botman SB. Gauging the effects of the NHIS respondent rule and within-household sampling strategy. In: Proceedings of the section on survey research methods. American Statistical Association. 196–201. 1991.
4. NHIS redesign memorandum number 40 from D Judkins to D Malec. Draft report on variance estimation. September 26, 1990.
5. Wolter K. Chapter 5. In: Introduction to variance estimation. Springer-Verlag: New York. 1985.
6. Valliant R. Generalized variance functions in stratified two-stage sampling. Journal of the American Statistical Association 82:499–508. 1987.
7. U.S. Bureau of the Census. Projections of the population of the United States by age, sex, and race: 1988 to 2080. Current population reports; series P-25, no 1018. 1989.
8. U.S. Bureau of the Census. Projections of the Hispanic Population: 1983 to 2080. Current population reports; series P-25, no 995. 1986.
9. NHIS redesign memorandum number 9 from J Waksberg to D Malec. Preliminary analysis of revisions in NHIS sample design required for subdomain statistics. December 13, 1989.
10. Fahimi M, Judkins D. PSU probabilities given differential sampling at second stage. In: Proceedings of the section on survey research methods. American Statistical Association. 538–43. 1991.
11. Waksberg J, Mohadjer L. Automation of within-household sampling. In: Proceedings of the section on survey research methods. American Statistical Association. 350–5. 1991.
12. Milliken C. Monitoring screening procedures for the Health Interview Survey. In: Proceedings of the section on survey research methods. American Statistical Association. 210–5. 1995.
13. Sirken MG, Graubard BI, McDaniel MJ. National network surveys of diabetes. Proceedings of the section on survey research methods. American Statistical Association. 631–5. 1978.
14. Judkins D, Waksberg J, Northrup D. Cost functions for NHIS and implications for survey design. In: Proceedings of the section on survey research methods. American Statistical Association. 34–43. 1990.
15. Judkins D, Massey J, Waksberg J. Patterns of residential concentrations by race and Hispanic origin. In: Proceedings of the section on social statistics. American Statistical Association. 51–60. 1992.
16. Ezzati TM, Hoffman K, Judkins DR, et al. A dual-frame design for sampling elderly minorities and persons with disabilities. Statistics in Medicine. 14:571–83. 1995.
17. Passel JS, Word DL. Constructing the list of Spanish surnames for the 1980 census: An application of Bayes' Theorem. Paper presented at the annual meeting of the Population Association of America. 1980.
18. Judkins D, Massey J, Smith V. Using surnames to oversample Hispanics from a list frame. Proceedings of the section on survey research methods. American Statistical Association. 847–52. 1992.
19. Wright D, Judkins D, Ryaboy S. Residential segregation and health characteristics of minority populations. In: Proceedings of the section on social statistics. American Statistical Association. 198–203. 1992.
20. Kish L. Survey sampling. New York: Wiley. 1965.
21. Brick JM, Waksberg J. Avoiding sequential sampling with random digit dialing. Survey Methodology. 17:27–41. 1991.
22. Thornberry OT, Massey JT. Trends in United States telephone coverage across time and subgroups. In: Groves RM, Biemer PP, Lyberg LE, et al., eds. Telephone survey methodology. New York: Wiley. 1988.
23. National Center for Health Statistics. Synthetic state estimates of disability. Public Health Service Publication No. 1759. Washington: Government Printing Office. 1968.
24. Schaible WL. A composite estimator for small area statistics. National Institute on Drug Abuse Research Monograph. 24:36–62. 1978.
25. Marker DA. Small area estimation: A Bayesian perspective (Unpublished doctoral dissertation). Ann Arbor, Michigan: University of Michigan. 1995.
26. Ericson W. Subjective Bayesian models in sampling finite populations. Journal of the Royal Statistical Society B. 31:195–233. 1969.
27. Marker DA. Small area estimation for the National Health Interview Survey. Proceedings of the section on survey research methods. American Statistical Association. 11–20. 1993.
28. Gonzalez M, Waksberg J. Estimation of the error of synthetic estimates. Paper presented at the first meeting of the International Association of Survey Statisticians. Vienna, Austria: International Association of Survey Statisticians. August 18–25, 1973.
29. Makuc DM, Haglund B, Ingram DD, et al. Use of cluster analysis to identify health care service areas. In: Proceedings of the section on social statistics. American Statistical Association. 260–5. 1990.
30. Panel on the National Health Care Survey. Toward a national health care survey: A data system for the 21st century. Wunderlich GS, ed. Washington, DC: National Academy Press. 1992.
31. Hansen MH, Hurwitz WN, Madow WG. Sample survey methods and theory. Volume 1, chapter 6, sections 1, 8, and 11. Volume 2, chapter 8, section 4. New York: Wiley. 1953.
32. Hicks LW, Keathley DH, Moore TF. Alternate PSU definitions for the redesigned National Health Interview Survey. In: Proceedings of the section on survey research methods. American Statistical Association. 732–4. 1992.
33. Massey JT, Moore TF, Parsons VL, Tadros W. Design and estimation for the National Health Interview Survey, 1985–94. National Center for Health Statistics. Vital Health Statistics 2(110). 1989.
34. Judkins DR, Huang H, Shapiro GM. Using sample information for stratification. In: Proceedings of the section on survey research methods. American Statistical Association. 248–53. 1984.
35. NHIS redesign memorandum number 98 from D Malec to J Waksberg. Feasibility of updating geographic distribution of minorities in postcensal years. August 20, 1992.
36. Kearny AT, Tourangeau R, Shapiro GM, Ernst LR. Coverage improvement from experimental residence questions. In: Proceedings of the section on survey research methods. American Statistical Association. 162–7. 1993.

# Appendix I
# List of the National Health Interview Survey Redesign Memorandums

| No. | Date | To[1] | From | Letter/Memo (if memo, subject) |
|---|---|---|---|---|
| 1 | October 3, 1989 | Malec | Judkins | (Letter) Draft Specifications for NHIS Extract for Westat |
| 2 | October 10 | Malec | Waksberg | (Letter) Specifications for NHIS Extract for Westat |
| 3 | October 17 | Malec | | Westat Research Plan |
| 4 | October 20 | Malec | Waksberg | Notes on October 12 Meeting Regarding NHIS Redesign |
| 5 | October 24 | Malec | Judkins | Letter (Draft NHIS Variance Estimation) |
| 6 | November 19 | Malec | Judkins | Letter (NHIS Variance Estimation) |
| 7 | November 29 | Malec | Waksberg | Letter |
| 8 | December 13 | Malec | Judkins | Letter (Merge of NHIS and Payroll Files) |
| 9 | December 13 | Malec | Waksberg | Preliminary Analysis of Revisions in NHIS Sample Design Required for Subdomain Statistics |
| 10 | January 3, 1990 | Malec | Waksberg | Preliminary Analysis of Revisions in NHIS Sample Design Required for State Statistics |
| 11 | January 11 | Malec | Waksberg | Numbering of Memoranda and Reports |
| 12 | January 12 | Malec | Judkins | Preliminary Report on Choice of First-Stage Probabilities |
| 13 | January 16 | Malec | Waksberg | Condition Items in NHIS |
| 14 | January 16 | Malec | Judkins | Preliminary Report on Network Sampling Application |
| 15 | January 17 | Malec | Judkins | Corrected Variance Specifications |
| 16 | January 24 | Malec | Waksberg | Further Analysis of Revision in NHIS Sample Design Required for Subdomain Statistics for Blacks and Hispanics |
| 17 | March 26 | Malec | Waksberg | Households as Sampling Units for Subdomain Statistics in the NHIS |
| 18 | March 28 | Malec | Waksberg | Interim Report on Oversampling Techniques |
| 19 | March 30 | Malec | Judkins | Preliminary Report on Dual Frame Sampling with Medicare Lists |
| 20 | April 6 | Malec | Waksberg | NHIS Sample Size at Constant Cost When Screening is Necessary for Oversampling of Subdomains |
| 21 | April 30 | Malec | Waksberg | Current Status of Westat's NHIS Design Research |
| 22 | June 7 | Malec | Judkins | The limits of oversampling without screening |
| 23 | June 12 | Malec | Judkins | Research on Minimum Sample Size for Desired Reliability |
| 24 | June 13 | Malec | Judkins | Supplemental Report on Minimum Sample Size for Desired Reliability |
| 25 | June 14 | Malec | Judkins | Variance Decomposition and Multi-Year Estimation |
| 26 | June 18 | Malec | Waksberg | Oversampling of Hispanic Spanish Origin Subgroups |
| 27 | June 18 | Malec | Judkins | State Grouping |
| 28 | June 21 | Malec | Judkins | Required Sample Sizes by Age |
| 29 | June 26 | Malec | Judkins | NHIS Redesign Research — Minutes and Feedback to Requests from Meeting of May 8, 1990 |
| 30 | June 26 | Massey | Waksberg | Census Bureau's Cost Estimates for Using Designated Respondents within Households in NHIS |
| 31 | July 3 | Malec | Marker | Hispanic Estimates by Geographic Area |
| 32 | July 9 | Malec | Judkins | NHIS Redesign Research — Comparison of Oversampling with Screening versus Oversampling by Itself |
| 33 | July 12 | Malec | Waksberg | First Draft of Executive Summary of NHIS Redesign Research Report |
| 34 | July 19 | Malec | Waksberg | Comparison of Sampling Plans that Produce Acceptable Race/Ethnicity Data for NHIS |
| 35 | July 26 | Malec | Judkins | More Thoughts on Oversampling Procedures |
| 36 | August 6 | Malec | Judkins | Revisions to Table in Memoranda 22, 32, and 33 |
| 37 | August 23 | Malec | Judkins | Revised Report on Network Sampling Applications |
| 38 | September 25 | Malec | Judkins | Variation in Segment Size |
| 39 | September 25 | Malec | Judkins | Comparison of Oversampling Strategies for Several Cost Assumptions |
| 40 | September 26 | Malec | Judkins | Draft report on variance estimation attached |
| 41 | October 19 | Malec | Waksberg | Robustness of Race/Ethnic Stratification Based on Decennial Census Data |
| 42 | November 7 | Malec | Marker | Model-Based Hispanic Estimates by Geographic Area |
| 43 | October 30 | Malec | Judkins | Draft Report on Subdomain and Design-Based Small Area Estimates |
| 44 | November 29 | Malec | Judkins | Oversampling Implications Tables |
| 45 | December 12 | Malec | Waksberg | Unlocatable Building Permit Records |
| 46 | December 19 | Malec | Judkins | Costs of Using HCFA Lists |
| 46A | December 19 | Malec | Marker | Subnational Research Plan for FY 91 |
| 47 | December 26 | Malec | Wright (David) | Master Area Reference File |
| 48 | January 11, 1991 | Massey | Waksberg | Sample Sizes, Screening Levels and Relative Costs of Alternative Sample Designs in Your December 28 Memo |
| 48R | January 4, 1991 | Massey | Waksberg | Sample Sizes, Screening Levels and Relative Costs of Alternative Sample Designs in Your December 28 Memo |
| 49 | January 11, 1991 | Massey | Waksberg | Information Requested in Your December 28 Memo that Was Not Covered in Our Memo Number 48 |
| 49R | January 8, 1991 | Massey | Waksberg | Information Requested in Your December 28 Memo that Was Not Covered in Our Memo Number 48 |

[1]The normal distribution list is: Westat, Inc.: Joe Waksberg, Mansour Fahimi, David Judkins, David Marker, and David Wright; CODA: Doris Northrup; NCHS: Dr. James Massey (2 copies), Dr. Owen Thornberry, Dr. Monroe Sirken, and Tommy McLemore; Census Bureau: Thomas Moore; and one copy for the chronological file. (Listing goes only to J.M., J.W., and chronological File.)

| No. | Date | To[1] | From | Letter/Memo (if memo, subject) |
|---|---|---|---|---|
| 50 | February 14, 1991 | Malec | Judkins | Design for a 50% Increase in Budget Authority |
| 50R | February 14, 1991 | Malec | Judkins | NHIS Redesign Research Design for 50% Increase in Budget Authority |
| 50A | February 15, 1991 | Massey | Judkins | Brief Description of Current Projects |
| 51 | February 19, 1991 | Massey | Waksberg | Use of Nonhousehold Respondent in NHANES III |
| 52 | February 19, 1991 | Malec | Marker | Comparison of Model-Based Estimators for States |
| 53 | March 12, 1991 | Malec | Fahimi/Judkins | Simulation Study on First-Stage Probabilities |
| 54 | March 27, 1991 | Massey | Judkins | Components of Variance for Minority and Nonmetropolitan Statistics |
| 55 | April 20, 1991 | Massey | Judkins | Integrated Survey Design |
| 56 | May 1, 1991 | Massey | Marker | Area Sample-Based and RDD Supplemented State Level Estimates |
| 56R | June 5, 1991 | Malec | Marker, Waksberg | Area Sample-Based and RDD Supplemented, State-Level Estimates |
| 57 | May 14, 1991 | Massey | Waksberg | Effect of Oversampling PSU's with High Proportions of Blacks or on Rural Statistics |
| 58 | May 30, 1991 | Massey | Waksberg, Wright, Judkins | Robustness of Distribution of the Population by Minority Density and Effect on Sample Designs Which Oversample Strata with High Minority Density |
| 59 | June 3, 1991 | Massey | Waksberg | Additional Tasks on NHIS Research |
| 60 | June 12, 1991 | Massey | Waksberg | Current Status of Tasks Listed in the Contract |
| 61 | June 12, 1991 | Massey | Judkins/Searls | Univariate Exploration of Health Characteristics by Minority Density Stratum |
| 62 | June 14, 1991 | Massey | Waksberg | Cost Estimates for Random Digit Dialing Surveys to Supplement NHIS |
| 63 | June 19, 1991 | Massey | Judkins | Allocation of Strata and PSU's given State Stratification |
| 64 | July 17, 1991 | Massey | Judkins | Use of Phone Number for Medicare Supplement |
| 65 | July 17, 1991 | Massey | Judkins | Sensitivity and Specificity of SSA Indicators for Institutionalization |
| 66 | August 2, 1991 | Massey | Judkins | NHIS Redesign Research — Revised Sampling Parameters |
| 67 | October 8, 1991 | Massey | Marker | Precision of RDD Supplementation for State Level Age/Sex Estimates |
| 68 | October 9, 1991 | Massey | Judkins | NHIS Redesign Research — Further Results of PL-94 Tabulations |
| 69 | October 14, 1991 | Massey | Judkins | Decomposition of NHDS Variances |
| 70 | October 17, 1991 | Massey | Judkins | NHIS Redesign — Census Screening System |
| 71 | November 12, 1991 | Massey | Marker | Comparison of Stand-Alone RDD with Unbiased Dual Frame Estimators |
| 72 | November 14, 1991 | Massey | Waksberg | Oversampling Density Strata for Statistics on Asian and American Indian Subdomains |
| 73 | December 9, 1991 | Massey | Marker | Comparison of stand-alone RDD with biased dual frame estimators |
| 74 | December 20, 1991 | Moore | Judkins | NHIS Redesign — Sample Selection Specifications for the Medicare Pilot Study |
| 75 | January 10, 1992 | Massey | Judkins/Edmonds | Documentation of Variance Estimation Software |
| 76 | January 15, 1992 | Massey | Göksel | Specifications for the Data File to Analyze Measures of Size for the NCHS |
| 77 | February 3, 1992 | Massey | Judkins | Restoration of an Equi-Probability Sample |
| 78 | February 4, 1992 | Massey | Judkins | Within Household Sampling Rules |
| 78R | March 4, 1993 | Massey | Judkins | Within Household Sampling Rules (Revised) |
| 79 | February 19, 1992 | Massey | Marker | Comparison of Stand-alone RDD with Dual Frame Estimators |
| 80 | February 21, 1992 | Massey | Judkins | New Construction Sampling |
| 81 | February 25, 1992 | Massey | Wright/Judkins | Logistic Regressions on Health Characteristics of Blacks and Hispanics by Minority 82 Density Strata |
| 82 | March 6, 1992 | Massey | Judkins | Questionnaire Implications of SSA Sampling |
| 83 | April 20, 1992 | Malec | Marker | Detailed plan for empirical comparison of small area estimators |
| 84 | April 27, 1992 | Thornberry | Marker | Comments on the Interviewer Assignment Period for the 1995 NHIS |
| 85 | May 29, 1992 | Massey | Göksel | The correlations among the health care service based and population based measures of size for NHIS |
| 86 | June 2, 1992 | Massey | Judkins | NHIS Redesign Research—Interim Report on the Use of List of Surnames to Oversample Hispanics |
| 87 | June 11, 1992 | Massey | Judkins | NHIS Redesign: Variance Implications of a Macro-level Incorporation of the SSA Disabled List Sample |
| 88 | June 11, 1992 | Massey | Judkins | Comments on Third Working Draft of UCF Specifications |
| 89 | June 24, 1992 | Massey | Waksberg | Current Status of Westat's Research on ISD Research Contract |
| 90 | July 7, 1992 | Massey | Judkins | NHIS Redesign: Utility of Hispanic Surname List for Elderly |
| 91 | July 31, 1992 | Massey | Waksberg | Interim Report on Oversampling Persons Whose Income Is Below the Poverty Level |
| 92 | July 8, 1992 | Malec | Edmonds | Gibbs Sampler Software Research |
| 93 | July 15, 1992 | Massey | Judkins | Interviewing Other Household Members in the 1992 SSA List Pilot for NHIS |
| 94 | July 15, 1992 | Massey | Judkins | Dual-Frame Estimation for 1992 Pilot |
| 95 | July 22, 1992 | Massey | Judkins | Construction of Panels for the 1995 NHIS Design |
| 96 | July 23, 1992 | Massey | Waksberg | NHIS Coverage Research |
| 97 | July 31, 1992 | Massey | Göksel | Variance Estimation for NAMCS |
| 98 | August 20, 1992 | Malec | Waksberg | Feasibility of Updating Geographic Distribution of Minorities in Postcensal Years |
| 99 | August 28, 1992 | Sirken | Judkins | Decisions from Meeting of July 30, 1992, on NHIS Panel Formation |
| 100 | August 31, 1992 | Malec | Marker | Update on Empirical Comparison of Model-Based Estimators |
| 101 | September 8, 1992 | Malec | Judkins | Assistance on Preparation of Sample Size Tables for Questionnaire Design |
| 102 | September 23, 1992 | Shimizu | Judkins | Network Sampling of Health Care Events Based upon the National Health Interview Survey |
| 103 | October 5, 1992 | Hoffman | Judkins | Comments on Draft Analytic Plan for SSA List Sample Pilot |
| 104 | October 6, 1992 | Malec | Judkins | Possible Benefits of a Mid-Design Update to the Minority Density Strata |
| 105 | October 12, 1992 | Sirken | Judkins | NHIS Redesign: Improvement Due to Screening and Equivalency to Multi-Year Reuse |

[1]The normal distribution list is: Westat, Inc.: Joe Waksberg, Mansour Fahimi, David Judkins, David Marker, and David Wright; CODA: Doris Northrup; NCHS: Dr. James Massey (2 copies), Dr. Owen Thornberry, Dr. Monroe Sirken, and Tommy McLemore; Census Bureau: Thomas Moore; and one copy for the chronological file. (Listing goes only to J.M., J.W., and chronological File.)

| No. | Date | To[1] | From | Letter/Memo (if memo, subject) |
|---|---|---|---|---|
| 106 | October 20, 1992 | Sirken | Judkins | NHIS Redesign: Oversampling/Screening Designs for a 15-Percent Increase in Budget Authority |
| 107 | October 27, 1992 | Massey | Waksberg | Updating Minority Density Areas |
| 108 | November 12, 1992 | Sirken | Judkins | NHIS Redesign: Changes in Effective Sample Sizes for Several Designs Options relative to the 1985–94 Design |
| 109 | January 25, 1993 | Massey | Judkins | County-Level Clustering for Surveys of Health Care Facilities |
| 109R | February 16, 1993 | Massey | Judkins | County-Level Clustering for Surveys of Health Care Facilities |
| 110 | December 3, 1992 | Massey | Judkins | NHIS Redesign: Changes in Effective Sample Sizes for Several More Design Options Relative to the 1985–94 Design |
| 111 | December 7, 1992 | Massey | Waksberg | Undercoverage in Sample Household Surveys |
| 112 | February 8, 1993 | Massey | Judkins | NHIS Redesign: Draft Outline for Final Report |
| 113 | February 16, 1993 | Massey | Judkins | Comments by Daniel Horvitz on Memorandum #102 |
| 114 | February 26, 1993 | Massey | Judkins | Tables on Benefits of SSA Sample of Disabled Beneficiaries |
| 102R | March 1, 1993 | Massey | Judkins | Network Sampling of Health Care Events Based upon the National Health Interview |
| 115 | August 5, 1993 | Massey | Göksel | Variance Estimation for NAMCS Report, cover letter |
| 116 | August 17, 1993 | Massey | Waksberg | Distribution of Poverty in Census Block Groups and Implications for Sample Design |
| 117 | December 6, 1993 | Milliken | Judkins | Draft NHIS Neighbor Screening Plan |
| 118 | September 20, 1994 | Moore | Judkins | Corrections to Specifications for the 1995 NHIS |
| 119 | September 27, 1994 | Moore | Judkins | Revised Retention Rates for the 1995 NHIS |

[1]The normal distribution list is: Westat, Inc.: Joe Waksberg, Mansour Fahimi, David Judkins, David Marker, and David Wright; CODA: Doris Northrup; NCHS: Dr. James Massey (2 copies), Dr. Owen Thornberry, Dr. Monroe Sirken, and Tommy McLemore; Census Bureau: Thomas Moore; and one copy for the chronological file. (Listing goes only to J.M., J.W., and chronological File.)

# Appendix II
# National Health Interview Survey Estimates and Coefficients of Variation

| | | | | Subdomain | | | | |
|---|---|---|---|---|---|---|---|---|
| Age | Category | Male | Female | Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
| Substantive variable: Number of chronic arthritis conditions | | | | | | | | |
| Aged under 5 years . . . . . . | Estimate | 10,834.15 | 0.00 | 0.00 | 10,834.15 | 0.00 | 0.00 | 10,834.15 |
| | *cv* (%) | 100.42 | 0.00 | 0.00 | 100.42 | 0.00 | 0.00 | 100.42 |
| Aged 5 to 17 years . . . . . . | Estimate | 20,936.29 | 102,472.95 | 2,989.95 | 88,779.63 | 31,639.65 | 0.00 | 123,409.24 |
| | *cv* (%) | 72.06 | 36.06 | 110.05 | 32.99 | 85.01 | 0.00 | 32.25 |
| Aged 18 to 24 years . . . . . | Estimate | 185,408.77 | 312,945.07 | 57,570.42 | 428,290.51 | 12,492.91 | 0.00 | 498,353.84 |
| | *cv* (%) | 28.64 | 20.29 | 33.17 | 19.97 | 99.51 | 0.00 | 18.67 |
| Aged 25 to 44 years . . . . . | Estimate | 1,704,849.19 | 2,992,162.51 | 462,040.58 | 3,843,956.66 | 391,014.45 | 24,536.29 | 4,672,475.41 |
| | *cv* (%) | 9.19 | 5.78 | 13.80 | 5.35 | 17.75 | 71.47 | 5.11 |
| Aged 45 to 64 years . . . . . | Estimate | 4,183,376.07 | 7,471,621.61 | 1,114,563.03 | 10,078,130.83 | 462,303.82 | 122,063.96 | 11,532,933.72 |
| | *cv* (%) | 4.99 | 3.85 | 8.86 | 3.42 | 15.44 | 31.80 | 3.31 |
| Aged 65 years or older . . . . | Estimate | 4,467,372.24 | 8,933,255.70 | 1,067,112.78 | 12,030,460.38 | 303,054.77 | 74,758.73 | 13,325,869.20 |
| | *cv* (%) | 4.72 | 3.76 | 8.75 | 3.52 | 23.39 | 48.58 | 3.31 |
| Total . . . . . . . . . . . . . . . | Estimate | 10,572,776.71 | 19,812,457.84 | 2,704,276.77 | 26,480,452.17 | 1,200,505.61 | 221,358.98 | 30,163,875.56 |
| | *cv* (%) | 2.87 | 2.54 | 5.24 | 2.30 | 10.90 | 24.08 | 2.19 |
| Age . . . . . . . . . . . . . . . . | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years . . . . . . | Estimate | 0.00 | 0.00 | 10,834.15 | 0.00 | 0.00 | 0.00 | 0.00 |
| | *cv* (%) | 0.00 | 0.00 | 100.42 | 0.00 | 0.00 | 0.00 | 0.00 |
| Aged 5 to 17 years . . . . . . | Estimate | 32,556.67 | 24,084.00 | 45,325.06 | 21,443.51 | 21,342.66 | 11,214.01 | 2,989.95 |
| | *cv* (%) | 55.18 | 103.70 | 45.66 | 70.41 | 71.02 | 87.10 | 110.05 |
| Aged 18 to 24 years . . . . . | Estimate | 90,312.81 | 117,621.35 | 153,598.07 | 136,821.61 | 12,749.08 | 77,563.72 | 93,995.48 |
| | *cv* (%) | 30.80 | 33.42 | 29.18 | 40.28 | 93.92 | 35.07 | 38.30 |
| Aged 25 to 44 years . . . . . | Estimate | 942,920.08 | 1,211,535.27 | 1,614,182.24 | 928,374.10 | 336,063.52 | 606,856.56 | 900,800.67 |
| | *cv* (%) | 12.38 | 9.10 | 9.00 | 13.36 | 26.65 | 15.06 | 11.08 |
| Aged 45 to 64 years . . . . . | Estimate | 2,241,755.71 | 3,160,059.56 | 4,285,532.46 | 1,967,649.95 | 463,956.43 | 1,777,799.28 | 2,402,975.30 |
| | *cv* (%) | 7.57 | 7.24 | 6.55 | 7.61 | 21.64 | 9.45 | 9.48 |
| Aged 65 years or older . . . . | Estimate | 2,742,113.80 | 3,636,458.13 | 4,917,022.86 | 2,105,033.14 | 709,838.86 | 2,032,274.94 | 2,696,136.12 |
| | *cv* (%) | 6.93 | 6.38 | 6.26 | 8.22 | 14.83 | 8.18 | 7.98 |
| Total . . . . . . . . . . . . . . . | Estimate | 6,049,659.07 | 8,149,758.31 | 11,026,494.85 | 5,159,322.31 | 1,543,950.55 | 4,505,708.52 | 6,096,897.52 |
| | *cv* (%) | 4.75 | 3.85 | 3.97 | 5.33 | 14.69 | 6.17 | 5.30 |
| Age . . . . . . . . . . . . . . . . | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years . . . . . . | Estimate | 0.00 | 0.00 | 0.00 | 10,834.15 | 0.00 | 0.00 | 10,834.15 |
| | *cv* (%) | 0.00 | 0.00 | 0.00 | 100.42 | 0.00 | 0.00 | 100.42 |
| Aged 5 to 17 years . . . . . . | Estimate | 21,094.04 | 22,659.13 | 0.00 | 22,665.93 | 21,443.51 | 0.00 | 123,409.24 |
| | *cv* (%) | 116.94 | 72.33 | 0.00 | 56.20 | 70.41 | 0.00 | 32.25 |
| Aged 18 to 24 years . . . . . | Estimate | 23,625.87 | 101,341.64 | 13,338.67 | 38,917.76 | 58,444.31 | 78,377.30 | 498,353.84 |
| | *cv* (%) | 66.51 | 35.79 | 101.16 | 59.05 | 53.65 | 49.02 | 18.67 |
| Aged 25 to 44 years . . . . . | Estimate | 310,734.60 | 560,452.09 | 306,884.03 | 746,846.13 | 236,257.40 | 692,116.70 | 4,697,011.70 |
| | *cv* (%) | 21.90 | 15.87 | 22.71 | 14.57 | 25.39 | 14.87 | 5.11 |
| Aged 45 to 64 years . . . . . | Estimate | 757,084.26 | 2,013,710.77 | 922,738.94 | 1,349,082.75 | 509,677.94 | 1,457,972.01 | 11,654,997.68 |
| | *cv* (%) | 16.23 | 10.23 | 21.37 | 14.03 | 19.66 | 10.86 | 3.22 |
| Aged 65 years or older . . . . | Estimate | 940,322.01 | 2,303,120.77 | 1,059,233.05 | 1,554,669.04 | 646,225.28 | 1,458,807.86 | 13,400,627.93 |
| | *cv* (%) | 13.04 | 12.19 | 14.31 | 12.95 | 20.29 | 9.46 | 3.29 |
| Total . . . . . . . . . . . . . . . | Estimate | 2,052,860.79 | 5,001,284.41 | 2,302,194.69 | 3,723,015.76 | 1,472,048.44 | 3,687,273.87 | 30,385,234.54 |
| | *cv* (%) | 12.09 | 8.89 | 12.69 | 11.02 | 14.03 | 7.39 | 2.15 |

| Age | Category | Male | Female | Subdomain | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | | Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
| | | | | Substantive variable: Number of acute digestive system conditions | | | | |
| Aged under 5 years | Estimate | 1,516,847.23 | 540,828.34 | 308,811.96 | 1,247,309.73 | 501,553.88 | 0.00 | 2,057,675.57 |
| | *cv* (%) | 22.01 | 27.00 | 44.33 | 19.84 | 46.42 | 0.00 | 17.40 |
| Aged 5 to 1 years | Estimate | 1,920,859.80 | 1,830,977.12 | 568,326.74 | 2,810,454.68 | 373,055.50 | 183,761.45 | 3,568,075.47 |
| | *cv* (%) | 15.11 | 19.63 | 28.24 | 14.95 | 57.91 | 61.36 | 12.30 |
| Aged 18 to 24 years | Estimate | 865,943.43 | 835,393.36 | 453,038.64 | 1,085,033.40 | 163,264.75 | 54,640.27 | 1,646,696.51 |
| | *cv* (%) | 26.72 | 22.64 | 30.41 | 21.64 | 53.71 | 99.10 | 17.34 |
| Aged 25 to 44 years | Estimate | 1,367,065.02 | 2,675,392.92 | 955,278.74 | 2,882,052.71 | 205,126.50 | 0.00 | 4,042,457.94 |
| | *cv* (%) | 20.76 | 15.25 | 23.59 | 15.09 | 50.84 | 0.00 | 12.09 |
| Aged 45 to 64 years | Estimate | 552,387.05 | 1,318,405.62 | 174,859.12 | 1,587,611.76 | 108,321.79 | 103,691.29 | 1,767,101.39 |
| | *cv* (%) | 31.39 | 18.72 | 50.71 | 18.40 | 70.59 | 71.34 | 16.61 |
| Aged 65 years or older | Estimate | 750,600.42 | 1,115,956.27 | 224,480.32 | 1,345,371.22 | 296,705.15 | 0.00 | 1,866,556.69 |
| | *cv* (%) | 28.53 | 23.45 | 52.74 | 20.76 | 52.73 | 0.00 | 17.99 |
| Total | Estimate | 6,973,702.97 | 8,316,953.63 | 2,684,795.51 | 10,957,833.50 | 1,648,027.58 | 342,093.01 | 14,948,563.58 |
| | *cv* (%) | 10.00 | 8.44 | 11.91 | 7.74 | 29.29 | 42.64 | 6.78 |

| Age | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
|---|---|---|---|---|---|---|---|---|
| Aged under 5 years | Estimate | 174,007.40 | 660,931.80 | 762,888.36 | 459,848.02 | 49,837.06 | 124,170.34 | 567,035.82 |
| | *cv* (%) | 52.10 | 37.50 | 28.41 | 36.44 | 99.77 | 60.55 | 40.53 |
| Aged 5 to 17 years | Estimate | 508,359.98 | 913,315.35 | 1,458,410.70 | 871,750.89 | 93,690.31 | 414,669.67 | 626,789.38 |
| | *cv* (%) | 31.30 | 29.43 | 16.58 | 28.79 | 64.51 | 35.49 | 36.98 |
| Aged 18 to 24 years | Estimate | 452,404.79 | 569,220.92 | 396,622.16 | 283,088.91 | 163,168.59 | 289,236.20 | 397,032.36 |
| | *cv* (%) | 31.89 | 30.55 | 36.32 | 44.57 | 38.62 | 44.76 | 38.38 |
| Aged 25 to 44 years | Estimate | 774,154.66 | 600,205.98 | 1,893,077.96 | 775,019.35 | 407,873.05 | 366,281.60 | 428,158.27 |
| | *cv* (%) | 25.25 | 29.86 | 16.71 | 32.45 | 34.55 | 37.13 | 34.73 |
| Aged 45 to 64 years | Estimate | 304,545.80 | 527,534.64 | 716,149.61 | 322,562.62 | 100,013.74 | 204,532.06 | 270,751.80 |
| | *cv* (%) | 40.88 | 31.23 | 26.46 | 36.36 | 65.91 | 51.58 | 43.01 |
| Aged 65 years or older | Estimate | 411,102.33 | 214,092.39 | 891,701.57 | 349,660.41 | 0.00 | 411,102.33 | 64,588.35 |
| | *cv* (%) | 48.04 | 46.51 | 24.95 | 27.98 | 0.00 | 48.04 | 71.87 |
| Total | Estimate | 2,624,574.96 | 3,485,301.08 | 6,118,850.36 | 3,061,930.19 | 814,582.75 | 1,809,992.21 | 2,354,355.97 |
| | *cv* (%) | 14.01 | 16.00 | 10.23 | 15.71 | 23.35 | 17.85 | 20.55 |

| Age | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
|---|---|---|---|---|---|---|---|---|
| Aged under 5 years | Estimate | 93,895.98 | 198,849.70 | 133,896.93 | 430,141.73 | 99,251.95 | 360,596.07 | 2,057,675.57 |
| | *cv* (%) | 99.42 | 50.43 | 55.59 | 41.35 | 70.40 | 42.88 | 17.40 |
| Aged 5 to 17 years | Estimate | 286,525.97 | 452,778.38 | 374,560.87 | 631,071.45 | 137,701.16 | 734,049.73 | 3,751,836.92 |
| | *cv* (%) | 41.11 | 35.04 | 56.55 | 34.02 | 56.94 | 31.64 | 12.70 |
| Aged 18 to 24 years | Estimate | 172,188.56 | 87,461.85 | 191,202.69 | 117,957.62 | 110,378.83 | 172,710.08 | 1,701,336.79 |
| | *cv* (%) | 53.00 | 63.08 | 56.30 | 64.06 | 71.11 | 57.47 | 16.94 |
| Aged 25 to 44 years | Estimate | 172,047.72 | 602,939.16 | 385,299.81 | 904,838.98 | 253,513.58 | 521,505.77 | 4,042,457.94 |
| | *cv* (%) | 58.20 | 32.93 | 37.61 | 24.80 | 45.28 | 40.47 | 12.09 |
| Aged 45 to 64 years | Estimate | 256,782.85 | 322,385.23 | 188,064.42 | 205,699.96 | 153,669.10 | 168,893.52 | 1,870,792.67 |
| | *cv* (%) | 45.37 | 40.08 | 49.83 | 59.82 | 49.25 | 53.27 | 16.23 |
| Aged 65 years or older | Estimate | 149,504.04 | 536,324.46 | 106,945.02 | 248,432.09 | 183,360.34 | 166,300.07 | 1,866,556.69 |
| | *cv* (%) | 58.91 | 30.98 | 70.13 | 51.93 | 49.92 | 61.82 | 17.99 |
| Total | Estimate | 1,130,945.11 | 2,200,738.79 | 1,379,969.75 | 2,538,141.83 | 937,874.96 | 2,124,055.23 | 15,290,656.60 |
| | *cv* (%) | 21.58 | 19.04 | 31.24 | 18.68 | 24.56 | 19.56 | 6.87 |

| | | | | Subdomain | | | | |
|---|---|---|---|---|---|---|---|---|
| Age | Category | Male | Female | Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
| Substantive variable: Number of acute bronchitis conditions | | | | | | | | |
| Aged under 5 years | Estimate | 1,132,721.10 | 932,992.47 | 174,278.96 | 1,681,713.59 | 209,721.03 | 0.00 | 2,065,713.57 |
| | *cv* (%) | 22.35 | 23.35 | 50.24 | 20.16 | 41.04 | 0.00 | 16.46 |
| Aged 5 to 17 years | Estimate | 863,924.92 | 939,198.84 | 230,337.38 | 1,480,137.09 | 92,649.29 | 0.00 | 1,803,123.76 |
| | *cv* (%) | 23.77 | 24.94 | 46.15 | 19.16 | 62.65 | 0.00 | 17.64 |
| Aged 18 to 24 years | Estimate | 111,977.36 | 342,820.50 | 62,544.24 | 328,439.51 | 63,814.11 | 0.00 | 454,797.86 |
| | *cv* (%) | 74.34 | 40.60 | 98.78 | 41.82 | 96.60 | 0.00 | 35.79 |
| Aged 25 to 44 years | Estimate | 726,719.19 | 1,313,878.85 | 198,576.82 | 1,842,021.22 | 0.00 | 0.00 | 2,040,598.04 |
| | *cv* (%) | 29.77 | 21.24 | 48.82 | 16.91 | 0.00 | 0.00 | 15.69 |
| Aged 45 to 64 years | Estimate | 283,565.40 | 479,666.93 | 0.00 | 737,641.85 | 25,590.48 | 0.00 | 763,232.33 |
| | *cv* (%) | 41.33 | 31.07 | 0.00 | 26.74 | 99.85 | 0.00 | 25.97 |
| Aged 65 years or older | Estimate | 604,458.85 | 349,479.40 | 0.00 | 953,938.25 | 0.00 | 0.00 | 953,938.25 |
| | *cv* (%) | 35.16 | 36.86 | 0.00 | 27.12 | 0.00 | 0.00 | 27.12 |
| Total | Estimate | 3,723,366.82 | 4,358,036.99 | 665,737.40 | 7,023,891.50 | 391,774.91 | 0.00 | 8,081,403.81 |
| | *cv* (%) | 11.82 | 10.42 | 29.54 | 8.24 | 33.03 | 0.00 | 7.77 |
| Age | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years | Estimate | 307,542.36 | 598,984.08 | 717,203.55 | 441,983.59 | 49,837.06 | 257,705.30 | 458,693.50 |
| | *cv* (%) | 38.13 | 32.18 | 27.15 | 27.27 | 102.98 | 40.64 | 39.54 |
| Aged 5 to 17 years | Estimate | 180,212.05 | 421,801.37 | 564,983.09 | 636,127.24 | 48,594.05 | 131,618.01 | 243,749.11 |
| | *cv* (%) | 48.53 | 36.05 | 29.23 | 30.06 | 83.39 | 59.20 | 45.28 |
| Aged 18 to 24 years | Estimate | 51,563.12 | 113,522.21 | 233,592.39 | 56,120.14 | 51,563.12 | 0.00 | 113,522.21 |
| | *cv* (%) | 99.76 | 70.60 | 50.94 | 99.80 | 99.76 | 0.00 | 70.60 |
| Aged 25 to 44 years | Estimate | 347,465.61 | 1,040,638.57 | 343,083.46 | 309,410.40 | 51,555.03 | 295,910.58 | 586,994.94 |
| | *cv* (%) | 41.49 | 22.84 | 39.53 | 33.61 | 99.11 | 45.45 | 27.21 |
| Aged 45 to 64 years | Estimate | 202,146.32 | 260,811.67 | 49,327.30 | 250,947.04 | 0.00 | 202,146.32 | 163,370.52 |
| | *cv* (%) | 51.08 | 44.90 | 101.54 | 44.78 | 0.00 | 51.08 | 57.67 |
| Aged 65 years or older | Estimate | 99,848.26 | 206,471.86 | 358,928.78 | 288,689.35 | 0.00 | 99,848.26 | 206,471.86 |
| | *cv* (%) | 71.44 | 50.50 | 37.96 | 56.76 | 0.00 | 71.44 | 50.50 |
| Total | Estimate | 1,188,777.72 | 2,642,229.77 | 2,267,118.57 | 1,983,277.76 | 201,549.25 | 987,228.47 | 1,772,802.14 |
| | *cv* (%) | 20.70 | 14.76 | 17.43 | 17.96 | 48.50 | 22.96 | 17.87 |
| Age | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years | Estimate | 140,290.58 | 140,473.88 | 242,401.58 | 334,328.09 | 91,121.73 | 350,861.85 | 2,065,713.57 |
| | *cv* (%) | 70.85 | 58.70 | 35.88 | 47.01 | 69.85 | 29.01 | 16.46 |
| Aged 5 to 17 years | Estimate | 178,052.27 | 149,421.31 | 101,987.37 | 313,574.41 | 241,046.73 | 395,080.52 | 1,803,123.76 |
| | *cv* (%) | 53.04 | 75.27 | 68.48 | 37.10 | 48.93 | 38.18 | 17.64 |
| Aged 18 to 24 years | Estimate | 0.00 | 55,451.51 | 0.00 | 178,140.88 | 0.00 | 56,120.14 | 454,797.86 |
| | *cv* (%) | 0.00 | 105.88 | 0.00 | 58.10 | 0.00 | 99.80 | 35.79 |
| Aged 25 to 44 years | Estimate | 453,643.63 | 233,637.93 | 0.00 | 109,445.53 | 96,819.49 | 212,590.91 | 2,040,598.04 |
| | *cv* (%) | 44.82 | 46.61 | 0.00 | 73.03 | 70.79 | 50.34 | 15.69 |
| Aged 45 to 64 years | Estimate | 97,441.14 | 0.00 | 0.00 | 49,327.30 | 48,694.13 | 202,252.91 | 763,232.33 |
| | *cv* (%) | 71.30 | 0.00 | 0.00 | 101.54 | 101.42 | 50.00 | 25.97 |
| Aged 65 years or older | Estimate | 0.00 | 157,907.61 | 0.00 | 201,021.17 | 47,540.24 | 241,149.11 | 953,938.25 |
| | *cv* (%) | 0.00 | 57.71 | 0.00 | 50.44 | 101.31 | 65.02 | 27.12 |
| Total | Estimate | 869,427.62 | 736,892.24 | 344,388.95 | 1,185,837.37 | 525,222.32 | 1,458,055.44 | 8,081,403.81 |
| | *cv* (%) | 35.44 | 35.64 | 38.90 | 23.40 | 33.18 | 20.97 | 7.77 |

| | | | | Subdomain | | | | |
|---|---|---|---|---|---|---|---|---|
| Age | Category | Male | Female | Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
| Substantive variable: Number of acute urinary conditions | | | | | | | | |
| Aged under 5 years . . . . . . | Estimate | 49,837.06 | 201,295.03 | 0.00 | 251,132.09 | 0.00 | 0.00 | 251,132.09 |
| | *cv* (%) | 100.69 | 49.98 | 0.00 | 44.91 | 0.00 | 0.00 | 44.91 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.00 | 319,296.08 | 88,479.72 | 230,816.36 | 0.00 | 0.00 | 319,296.08 |
| | *cv* (%) | 0.00 | 38.68 | 73.83 | 44.73 | 0.00 | 0.00 | 38.68 |
| Aged 18 to 24 years . . . . . . | Estimate | 83,394.94 | 799,442.55 | 81,427.18 | 688,390.59 | 113,019.73 | 0.00 | 882,837.49 |
| | *cv* (%) | 77.78 | 31.83 | 75.66 | 35.09 | 69.76 | 0.00 | 30.56 |
| Aged 25 to 44 years . . . . . . | Estimate | 587,183.97 | 2,134,905.33 | 377,161.26 | 2,081,547.32 | 263,380.73 | 49,899.27 | 2,672,190.03 |
| | *cv* (%) | 37.06 | 15.80 | 34.88 | 17.20 | 44.48 | 99.71 | 14.91 |
| Aged 45 to 64 years . . . . . . | Estimate | 444,449.11 | 974,334.29 | 72,700.28 | 1,346,083.12 | 0.00 | 0.00 | 1,418,783.40 |
| | *cv* (%) | 33.31 | 23.99 | 74.84 | 20.44 | 0.00 | 0.00 | 19.78 |
| Aged 65 years or older . . . . | Estimate | 444,834.40 | 807,739.79 | 138,542.87 | 1,051,811.77 | 62,219.54 | 0.00 | 1,252,574.19 |
| | *cv* (%) | 35.96 | 23.26 | 48.54 | 22.00 | 102.89 | 0.00 | 18.86 |
| Total . . . . . . . . . . . . . . . | Estimate | 1,609,699.47 | 5,237,013.08 | 758,311.31 | 5,649,781.24 | 438,620.00 | 49,899.27 | 6,796,813.28 |
| | *cv* (%) | 21.15 | 10.84 | 23.24 | 10.65 | 38.96 | 99.71 | 10.02 |

| Age . . . . . . . . . . . . . . . . | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
|---|---|---|---|---|---|---|---|---|
| Aged under 5 years . . . . . . | Estimate | 58,172.05 | 96,937.60 | 46,185.39 | 49,837.06 | 0.00 | 58,172.05 | 51,115.62 |
| | *cv* (%) | 99.37 | 69.27 | 101.34 | 100.69 | 0.00 | 99.37 | 99.21 |
| Aged 5 to 17 years . . . . . . | Estimate | 88,479.72 | 0.00 | 135,191.30 | 95,625.05 | 0.00 | 88,479.72 | 0.00 |
| | *cv* (%) | 73.83 | 0.00 | 57.64 | 71.04 | 0.00 | 73.83 | 0.00 |
| Aged 18 to 24 years . . . . . . | Estimate | 118,529.80 | 344,193.21 | 309,007.16 | 111,107.31 | 53,585.91 | 64,943.90 | 281,350.14 |
| | *cv* (%) | 71.10 | 58.95 | 41.85 | 70.99 | 100.19 | 100.52 | 53.44 |
| Aged 25 to 44 years . . . . . . | Estimate | 238,666.14 | 692,642.11 | 1,270,746.34 | 520,034.72 | 154,706.16 | 83,959.98 | 431,523.91 |
| | *cv* (%) | 42.14 | 27.31 | 25.08 | 26.71 | 48.65 | 79.83 | 33.93 |
| Aged 45 to 64 years . . . . . . | Estimate | 365,603.96 | 509,122.92 | 494,821.01 | 49,235.51 | 0.00 | 365,603.96 | 301,864.91 |
| | *cv* (%) | 40.93 | 31.64 | 32.16 | 100.27 | 0.00 | 40.93 | 40.73 |
| Aged 65 years or older . . . . | Estimate | 0.00 | 298,918.98 | 612,170.27 | 341,484.94 | 0.00 | 0.00 | 157,433.20 |
| | *cv* (%) | 0.00 | 47.06 | 24.62 | 37.48 | 0.00 | 0.00 | 59.42 |
| Total . . . . . . . . . . . . . . . | Estimate | 869,451.67 | 1,941,814.83 | 2,868,121.47 | 1,167,324.59 | 208,292.07 | 661,159.60 | 1,223,287.78 |
| | *cv* (%) | 24.97 | 17.39 | 14.41 | 20.08 | 44.58 | 32.12 | 20.27 |

| Age . . . . . . . . . . . . . . . . | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
|---|---|---|---|---|---|---|---|---|
| Aged under 5 years . . . . . . | Estimate | 45,821.98 | 0.00 | 46,185.39 | 0.00 | 0.00 | 49,837.06 | 251,132.09 |
| | *cv* (%) | 96.52 | 0.00 | 101.34 | 0.00 | 0.00 | 100.69 | 44.91 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.00 | 89,982.98 | 0.00 | 45,208.32 | 51,177.33 | 44,447.73 | 319,296.08 |
| | *cv* (%) | 0.00 | 70.56 | 0.00 | 99.14 | 100.36 | 100.32 | 38.68 |
| Aged 18 to 24 years . . . . . . | Estimate | 62,843.07 | 103,181.26 | 58,890.15 | 146,935.75 | 0.00 | 111,107.31 | 882,837.49 |
| | *cv* (%) | 114.54 | 69.39 | 99.10 | 61.19 | 0.00 | 70.99 | 30.56 |
| Aged 25 to 44 years . . . . . . | Estimate | 261,118.20 | 576,214.70 | 222,177.88 | 472,353.76 | 100,789.16 | 419,245.56 | 2,722,089.30 |
| | *cv* (%) | 46.63 | 32.91 | 77.76 | 36.15 | 69.13 | 32.56 | 14.74 |
| Aged 45 to 64 years . . . . . . | Estimate | 207,258.01 | 236,053.19 | 0.00 | 258,767.82 | 49,235.51 | 0.00 | 1,418,783.40 |
| | *cv* (%) | 49.67 | 47.35 | 0.00 | 43.82 | 100.27 | 0.00 | 19.78 |
| Aged 65 years or older . . . . | Estimate | 141,485.79 | 204,100.43 | 144,310.85 | 263,758.98 | 139,487.22 | 201,997.72 | 1,252,574.19 |
| | *cv* (%) | 74.36 | 45.11 | 39.81 | 40.26 | 57.76 | 49.35 | 18.86 |
| Total . . . . . . . . . . . . . . . | Estimate | 718,527.05 | 1,209,532.57 | 471,564.28 | 1,187,024.62 | 340,689.22 | 826,635.38 | 6,846,712.55 |
| | *cv* (%) | 28.48 | 22.96 | 39.15 | 24.64 | 37.67 | 26.01 | 10.00 |

| Age | Category | Male | Female | Subdomain: Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
|---|---|---|---|---|---|---|---|---|
| Substantive variable: Number of chronic color blindness | | | | | | | | |
| Aged under 5 years . . . . . . | Estimate | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| | *cv* (%) | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Aged 5 to 17 years . . . . . . | Estimate | 360,797.82 | 11,117.47 | 0.00 | 347,494.82 | 24,420.47 | 0.00 | 371,915.28 |
| | *cv* (%) | 17.34 | 99.21 | 0.00 | 17.58 | 70.63 | 0.00 | 16.99 |
| Aged 18 to 24 years . . . . . . | Estimate | 217,425.86 | 11,899.18 | 0.00 | 215,055.95 | 14,269.09 | 0.00 | 229,325.04 |
| | *cv* (%) | 28.12 | 100.14 | 0.00 | 28.28 | 100.29 | 0.00 | 27.17 |
| Aged 25 to 44 years . . . . . . | Estimate | 1,011,047.67 | 94,985.16 | 42,279.77 | 978,638.48 | 85,114.58 | 26,619.79 | 1,079,413.04 |
| | *cv* (%) | 10.65 | 33.58 | 55.55 | 10.79 | 42.11 | 70.74 | 10.47 |
| Aged 45 to 64 years . . . . . . | Estimate | 631,363.19 | 51,355.20 | 37,329.58 | 633,092.92 | 12,295.89 | 12,295.89 | 670,422.51 |
| | *cv* (%) | 13.07 | 49.73 | 48.93 | 13.45 | 100.58 | 100.58 | 12.93 |
| Aged 65 years or older . . . . | Estimate | 309,106.73 | 52,550.96 | 14,983.71 | 346,673.97 | 0.00 | 0.00 | 361,657.69 |
| | *cv* (%) | 19.06 | 45.98 | 70.01 | 17.52 | 0.00 | 0.00 | 17.05 |
| Total . . . . . . . . . . . . . . . | Estimate | 2,529,741.27 | 221,907.97 | 94,593.07 | 2,520,956.15 | 136,100.02 | 38,915.68 | 2,712,733.56 |
| | *cv* (%) | 7.08 | 23.34 | 33.12 | 7.17 | 29.70 | 57.68 | 6.97 |
| Age . . . . . . . . . . . . . . . . | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years . . . . . . | Estimate | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| | *cv* (%) | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Aged 5 to 17 years . . . . . . | Estimate | 80,121.74 | 158,797.64 | 22,949.32 | 110,046.58 | 11,171.90 | 68,949.85 | 112,792.10 |
| | *cv* (%) | 37.90 | 27.65 | 70.34 | 36.34 | 99.44 | 40.99 | 33.08 |
| Aged 18 to 24 years . . . . . . | Estimate | 0.00 | 130,077.01 | 49,724.29 | 49,523.74 | 0.00 | 0.00 | 67,149.61 |
| | *cv* (%) | 0.00 | 40.00 | 46.17 | 49.46 | 0.00 | 0.00 | 42.56 |
| Aged 25 to 44 years . . . . . . | Estimate | 241,773.09 | 398,900.33 | 262,240.34 | 203,119.08 | 57,600.08 | 184,173.01 | 290,119.27 |
| | *cv* (%) | 19.63 | 17.38 | 24.80 | 25.41 | 43.19 | 22.73 | 21.27 |
| Aged 45 to 64 years . . . . . . | Estimate | 210,933.25 | 161,375.08 | 203,393.67 | 107,016.40 | 57,416.53 | 153,516.72 | 127,973.21 |
| | *cv* (%) | 25.46 | 26.77 | 23.04 | 31.88 | 33.82 | 30.88 | 29.54 |
| Aged 65 years or older . . . . | Estimate | 131,820.55 | 115,353.27 | 45,505.75 | 68,978.12 | 35,697.90 | 96,122.65 | 93,819.43 |
| | *cv* (%) | 30.64 | 31.41 | 50.01 | 40.51 | 57.08 | 36.19 | 34.93 |
| Total . . . . . . . . . . . . . . . | Estimate | 664,648.63 | 964,503.32 | 583,813.38 | 538,683.91 | 161,886.40 | 502,762.23 | 691,853.62 |
| | *cv* (%) | 12.03 | 11.41 | 17.06 | 15.10 | 27.37 | 14.37 | 12.91 |
| Age . . . . . . . . . . . . . . . . | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years . . . . . . | Estimate | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| | *cv* (%) | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Aged 5 to 17 years . . . . . . | Estimate | 46,005.54 | 22,949.32 | 0.00 | 0.00 | 56,502.03 | 53,544.54 | 371,915.28 |
| | *cv* (%) | 50.48 | 70.34 | 0.00 | 0.00 | 44.41 | 58.94 | 16.99 |
| Aged 18 to 24 years . . . . . . | Estimate | 62,927.40 | 11,849.17 | 0.00 | 37,875.13 | 25,665.47 | 23,858.27 | 229,325.04 |
| | *cv* (%) | 62.06 | 100.45 | 0.00 | 52.18 | 71.24 | 68.22 | 27.17 |
| Aged 25 to 44 years . . . . . . | Estimate | 108,781.05 | 118,806.37 | 92,391.81 | 51,042.17 | 103,037.96 | 100,081.12 | 1,106,032.84 |
| | *cv* (%) | 36.06 | 36.90 | 43.17 | 43.94 | 36.93 | 31.57 | 10.42 |
| Aged 45 to 64 years . . . . . . | Estimate | 33,401.86 | 93,450.63 | 28,806.17 | 81,136.88 | 35,220.62 | 71,795.77 | 682,718.39 |
| | *cv* (%) | 61.51 | 38.29 | 60.53 | 36.84 | 49.79 | 40.67 | 12.56 |
| Aged 65 years or older . . . . | Estimate | 21,533.84 | 34,466.02 | 0.00 | 11,039.73 | 33,202.75 | 35,775.36 | 361,657.69 |
| | *cv* (%) | 72.01 | 57.22 | 0.00 | 101.13 | 58.40 | 56.96 | 17.05 |
| Total . . . . . . . . . . . . . . . | Estimate | 272,649.69 | 281,521.51 | 121,197.98 | 181,093.90 | 253,628.84 | 285,055.07 | 2,751,649.24 |
| | *cv* (%) | 27.85 | 27.01 | 33.11 | 25.32 | 23.77 | 20.82 | 6.87 |

| Age | Category | Male | Female | Subdomain: Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
|---|---|---|---|---|---|---|---|---|
| Substantive variable: Number of chronic epilepsy conditions | | | | | | | | |
| Aged under 5 years | Estimate | 54,167.69 | 10,658.17 | 8,326.56 | 33,322.19 | 23,177.11 | 0.00 | 64,825.86 |
| | *cv* (%) | 44.47 | 100.01 | 104.12 | 56.63 | 70.05 | 0.00 | 40.54 |
| Aged 5 to 17 years | Estimate | 16,635.28 | 69,835.91 | 16,635.28 | 57,747.66 | 12,088.25 | 0.00 | 86,471.19 |
| | *cv* (%) | 77.13 | 39.51 | 77.13 | 42.90 | 100.78 | 0.00 | 33.08 |
| Aged 18 to 24 years | Estimate | 55,308.01 | 24,206.85 | 17,707.41 | 48,242.72 | 13,564.73 | 0.00 | 79,514.85 |
| | *cv* (%) | 44.57 | 69.99 | 71.36 | 49.36 | 99.89 | 0.00 | 43.97 |
| Aged 25 to 44 years | Estimate | 144,131.23 | 183,932.50 | 57,489.96 | 240,461.87 | 30,111.90 | 12,845.05 | 315,218.68 |
| | *cv* (%) | 35.51 | 25.20 | 47.67 | 25.15 | 59.54 | 99.72 | 21.50 |
| Aged 45 to 64 years | Estimate | 113,211.68 | 133,631.76 | 24,478.67 | 210,732.62 | 11,632.16 | 0.00 | 246,843.45 |
| | *cv* (%) | 31.71 | 29.69 | 48.89 | 23.09 | 100.34 | 0.00 | 21.08 |
| Aged 65 years or older | Estimate | 56,905.22 | 59,396.44 | 22,686.32 | 58,557.18 | 35,058.16 | 0.00 | 116,301.66 |
| | *cv* (%) | 35.34 | 44.73 | 58.03 | 39.34 | 58.11 | 0.00 | 28.89 |
| Total | Estimate | 440,359.11 | 481,661.63 | 147,324.20 | 649,064.24 | 125,632.30 | 12,845.05 | 909,175.69 |
| | *cv* (%) | 16.50 | 17.20 | 24.56 | 14.16 | 30.41 | 99.72 | 11.90 |
| Age | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years | Estimate | 23,177.11 | 13,084.48 | 28,564.27 | 0.00 | 0.00 | 23,177.11 | 13,084.48 |
| | *cv* (%) | 70.05 | 99.66 | 55.81 | 0.00 | 0.00 | 70.05 | 99.66 |
| Aged 5 to 17 years | Estimate | 0.00 | 11,961.48 | 63,848.35 | 10,661.35 | 0.00 | 0.00 | 11,961.48 |
| | *cv* (%) | 0.00 | 100.59 | 36.81 | 102.01 | 0.00 | 0.00 | 100.59 |
| Aged 18 to 24 years | Estimate | 36,733.13 | 30,836.97 | 0.00 | 11,944.75 | 0.00 | 36,733.13 | 20,194.85 |
| | *cv* (%) | 74.51 | 60.24 | 0.00 | 97.13 | 0.00 | 74.51 | 75.13 |
| Aged 25 to 44 years | Estimate | 66,198.97 | 76,137.71 | 116,095.06 | 69,632.00 | 11,138.18 | 55,060.79 | 52,692.82 |
| | *cv* (%) | 59.58 | 38.90 | 33.55 | 40.74 | 83.50 | 69.77 | 45.76 |
| Aged 45 to 64 years | Estimate | 47,773.59 | 61,273.37 | 88,991.71 | 48,804.77 | 23,162.50 | 24,611.10 | 37,552.77 |
| | *cv* (%) | 49.25 | 44.49 | 33.95 | 46.08 | 68.91 | 70.18 | 59.05 |
| Aged 65 years or older | Estimate | 12,183.05 | 23,077.76 | 58,629.13 | 22,411.73 | 0.00 | 12,183.05 | 23,077.76 |
| | *cv* (%) | 100.71 | 70.90 | 41.03 | 54.19 | 0.00 | 100.71 | 70.90 |
| Total | Estimate | 186,065.84 | 216,371.77 | 356,128.52 | 163,454.61 | 34,300.68 | 151,765.16 | 158,564.17 |
| | *cv* (%) | 31.32 | 23.09 | 20.78 | 25.33 | 53.82 | 36.40 | 27.10 |
| Age | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years | Estimate | 0.00 | 8,326.56 | 13,448.12 | 6,789.59 | 0.00 | 0.00 | 64,825.86 |
| | *cv* (%) | 0.00 | 104.12 | 89.17 | 92.43 | 0.00 | 0.00 | 40.54 |
| Aged 5 to 17 years | Estimate | 0.00 | 11,214.93 | 23,013.69 | 29,619.73 | 0.00 | 10,661.35 | 86,471.19 |
| | *cv* (%) | 0.00 | 104.51 | 70.71 | 55.24 | 0.00 | 102.01 | 33.08 |
| Aged 18 to 24 years | Estimate | 10,642.12 | 0.00 | 0.00 | 0.00 | 11,944.75 | 0.00 | 79,514.85 |
| | *cv* (%) | 98.62 | 0.00 | 0.00 | 0.00 | 97.13 | 0.00 | 43.97 |
| Aged 25 to 44 years | Estimate | 23,444.89 | 66,752.50 | 17,835.18 | 31,507.38 | 34,640.88 | 34,991.11 | 328,063.73 |
| | *cv* (%) | 71.66 | 46.49 | 74.33 | 61.60 | 57.85 | 57.16 | 21.03 |
| Aged 45 to 64 years | Estimate | 23,720.60 | 30,218.27 | 32,359.11 | 26,414.33 | 11,712.11 | 37,092.66 | 246,843.45 |
| | *cv* (%) | 66.44 | 61.00 | 58.61 | 56.98 | 100.17 | 51.50 | 21.08 |
| Aged 65 years or older | Estimate | 0.00 | 19,042.88 | 27,415.07 | 12,171.18 | 11,125.20 | 11,286.53 | 116,301.66 |
| | *cv* (%) | 0.00 | 73.19 | 59.97 | 80.84 | 42.73 | 99.48 | 28.89 |
| Total | Estimate | 57,807.61 | 135,555.15 | 114,071.16 | 106,502.21 | 69,422.94 | 94,031.67 | 922,020.74 |
| | *cv* (%) | 44.28 | 33.62 | 40.41 | 37.19 | 38.11 | 33.94 | 11.97 |

| | | | | Subdomain | | | | |
|---|---|---|---|---|---|---|---|---|
| Age | Category | Male | Female | Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
| Substantive variable: Number of moving motor vehicle injuries | | | | | | | | |
| Aged under 5 years . . . . . . | Estimate | 0.00 | 91,643.96 | 0.00 | 91,643.96 | 0.00 | 0.00 | 91,643.96 |
| | *cv* (%) | 0.00 | 99.89 | 0.00 | 99.89 | 0.00 | 0.00 | 99.89 |
| Aged 5 to 17 years . . . . . . | Estimate | 172,150.80 | 435,241.14 | 53,453.18 | 499,541.00 | 54,397.76 | 0.00 | 607,391.94 |
| | *cv* (%) | 48.70 | 35.24 | 70.09 | 32.68 | 99.64 | 0.00 | 28.22 |
| Aged 18 to 24 years . . . . . . | Estimate | 737,646.81 | 432,467.37 | 91,416.57 | 853,135.17 | 225,562.44 | 0.00 | 1,170,114.18 |
| | *cv* (%) | 37.76 | 34.30 | 67.82 | 34.40 | 50.45 | 0.00 | 28.03 |
| Aged 25 to 44 years . . . . . . | Estimate | 769,740.68 | 774,975.18 | 224,596.54 | 1,158,288.58 | 161,830.73 | 101,395.75 | 1,443,320.10 |
| | *cv* (%) | 31.41 | 26.45 | 50.96 | 24.27 | 61.67 | 100.30 | 20.83 |
| Aged 45 to 64 years . . . . . . | Estimate | 172,682.31 | 260,628.76 | 50,214.68 | 328,234.13 | 54,862.26 | 48,694.13 | 384,616.93 |
| | *cv* (%) | 58.90 | 42.60 | 71.03 | 41.51 | 98.85 | 100.60 | 36.85 |
| Aged 65 years or older . . . . | Estimate | 203,736.01 | 109,345.86 | 48,149.92 | 208,638.38 | 56,293.58 | 0.00 | 313,081.87 |
| | *cv* (%) | 46.82 | 70.54 | 62.72 | 50.23 | 99.49 | 0.00 | 46.07 |
| Total . . . . . . . . . . . . . . . | Estimate | 2,055,956.60 | 2,104,302.26 | 467,830.89 | 3,139,481.21 | 552,946.77 | 150,089.88 | 4,010,168.98 |
| | *cv* (%) | 20.37 | 16.62 | 32.59 | 16.16 | 32.18 | 75.26 | 13.91 |
| Age . . . . . . . . . . . . . . . . | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years . . . . . . | Estimate | 0.00 | 0.00 | 0.00 | 91,643.96 | 0.00 | 0.00 | 0.00 |
| | *cv* (%) | 0.00 | 0.00 | 0.00 | 99.89 | 0.00 | 0.00 | 0.00 |
| Aged 5 to 17 years . . . . . . | Estimate | 97,230.71 | 46,289.09 | 366,611.44 | 97,260.70 | 45,547.58 | 51,683.12 | 0.00 |
| | *cv* (%) | 65.72 | 100.48 | 33.73 | 70.86 | 83.53 | 99.87 | 0.00 |
| Aged 18 to 24 years . . . . . . | Estimate | 109,813.31 | 233,824.37 | 391,893.97 | 434,582.53 | 58,743.07 | 51,070.24 | 176,929.60 |
| | *cv* (%) | 69.12 | 53.49 | 49.98 | 38.01 | 99.93 | 97.84 | 58.87 |
| Aged 25 to 44 years . . . . . . | Estimate | 440,352.42 | 149,464.55 | 546,029.86 | 408,869.02 | 201,656.79 | 238,695.63 | 149,464.55 |
| | *cv* (%) | 35.38 | 56.77 | 38.04 | 39.90 | 48.15 | 51.20 | 56.77 |
| Aged 45 to 64 years . . . . . . | Estimate | 5,0703.93 | 130,989.73 | 148,061.01 | 103,556.39 | 50,703.93 | 0.00 | 130,989.73 |
| | *cv* (%) | 100.00 | 61.77 | 61.18 | 70.40 | 100.00 | 0.00 | 61.77 |
| Aged 65 years or older . . . . | Estimate | 108,160.19 | 0.00 | 101,202.21 | 103,719.48 | 0.00 | 108,160.19 | 0.00 |
| | *cv* (%) | 99.77 | 0.00 | 59.39 | 71.70 | 0.00 | 99.77 | 0.00 |
| Total . . . . . . . . . . . . . . . | Estimate | 806,260.56 | 560,567.74 | 1,553,798.49 | 1,239,632.08 | 356,651.38 | 449,609.18 | 457,383.87 |
| | *cv* (%) | 30.90 | 31.94 | 22.18 | 23.29 | 44.47 | 39.64 | 34.62 |
| Age . . . . . . . . . . . . . . . . | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years . . . . . . | Estimate | 0.00 | 0.00 | 0.00 | 0.00 | 91,643.96 | 0.00 | 91,643.96 |
| | *cv* (%) | 0.00 | 0.00 | 0.00 | 0.00 | 99.89 | 0.00 | 99.89 |
| Aged 5 to 17 years . . . . . . | Estimate | 46,289.09 | 47,891.53 | 103,080.35 | 215,639.55 | 50,575.91 | 46,684.79 | 607,391.94 |
| | *cv* (%) | 100.48 | 87.17 | 70.71 | 45.62 | 100.57 | 100.39 | 28.22 |
| Aged 18 to 24 years . . . . . . | Estimate | 56,894.78 | 289,788.91 | 0.00 | 102,105.05 | 164,983.75 | 269,598.78 | 1,170,114.18 |
| | *cv* (%) | 117.04 | 62.89 | 0.00 | 70.47 | 58.48 | 53.39 | 28.03 |
| Aged 25 to 44 years . . . . . . | Estimate | 0.00 | 59,956.38 | 123,477.46 | 362,596.02 | 150,941.13 | 257,927.89 | 1,544,715.85 |
| | *cv* (%) | 0.00 | 99.97 | 99.81 | 42.94 | 57.88 | 52.84 | 20.63 |
| Aged 45 to 64 years . . . . . . | Estimate | 0.00 | 0.00 | 66,351.30 | 81,709.72 | 54,862.26 | 48,694.13 | 433,311.06 |
| | *cv* (%) | 0.00 | 0.00 | 99.98 | 75.82 | 98.85 | 100.60 | 34.57 |
| Aged 65 years or older . . . . | Estimate | 0.00 | 0.00 | 53,052.29 | 48,149.92 | 0.00 | 103,719.48 | 313,081.87 |
| | *cv* (%) | 0.00 | 0.00 | 99.56 | 62.72 | 0.00 | 71.70 | 46.07 |
| Total . . . . . . . . . . . . . . . | Estimate | 103,183.87 | 397,636.83 | 345,961.39 | 810,200.27 | 513,007.00 | 726,625.07 | 4,160,258.86 |
| | *cv* (%) | 78.71 | 49.56 | 48.08 | 28.20 | 37.41 | 30.30 | 13.66 |

| | | | | Subdomain | | | | |
|---|---|---|---|---|---|---|---|---|
| Age | Category | Male | Female | Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
| Substantive variable: Percentage unable to carry out major activity | | | | | | | | |
| Aged under 5 years . . . . . . | Estimate | 0.49 | 0.59 | 1.08 | 0.35 | 1.10 | 0.00 | 0.56 |
| | *cv* (%) | 17.20 | 21.70 | 25.01 | 19.08 | 40.12 | 0.00 | 13.03 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.31 | 0.41 | 0.48 | 0.33 | 0.37 | 0.00 | 0.37 |
| | *cv* (%) | 17.18 | 15.22 | 23.77 | 12.94 | 30.69 | 0.00 | 11.66 |
| Aged 18 to 24 years . . . . . . | Estimate | 1.86 | 1.41 | 2.46 | 1.41 | 2.17 | 1.91 | 1.62 |
| | *cv* (%) | 9.57 | 9.44 | 15.11 | 9.05 | 19.80 | 63.53 | 6.61 |
| Aged 25 to 44 years . . . . . . | Estimate | 2.99 | 2.42 | 4.80 | 2.36 | 3.18 | 1.40 | 2.74 |
| | *cv* (%) | 4.11 | 5.16 | 7.08 | 3.92 | 11.25 | 29.27 | 3.32 |
| Aged 45 to 64 years . . . . . . | Estimate | 10.05 | 7.18 | 14.40 | 7.82 | 9.09 | 3.87 | 8.65 |
| | *cv* (%) | 3.09 | 3.85 | 4.44 | 2.67 | 11.36 | 25.76 | 2.38 |
| Aged 65 years or older . . . . | Estimate | 11.79 | 9.71 | 19.67 | 9.67 | 11.91 | 10.26 | 10.57 |
| | *cv* (%) | 4.02 | 3.29 | 6.05 | 2.91 | 13.30 | 28.15 | 2.68 |
| Total . . . . . . . . . . . . . . . | Estimate | 4.35 | 3.72 | 5.89 | 3.83 | 3.21 | 1.88 | 4.08 |
| | *cv* (%) | 1.90 | 2.34 | 3.08 | 1.82 | 7.10 | 17.81 | 1.56 |
| Age . . . . . . . . . . . . . . . . | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years . . . . . . | Estimate | 0.51 | 0.37 | 0.60 | 0.69 | 0.30 | 0.58 | 0.46 |
| | *cv* (%) | 34.48 | 31.18 | 23.15 | 34.21 | 80.38 | 37.86 | 32.26 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.18 | 0.35 | 0.47 | 0.33 | 0.26 | 0.16 | 0.36 |
| | *cv* (%) | 37.57 | 23.45 | 17.22 | 22.44 | 73.88 | 42.15 | 25.30 |
| Aged 18 to 24 years . . . . . . | Estimate | 1.24 | 1.72 | 1.47 | 2.20 | 1.68 | 1.07 | 1.75 |
| | *cv* (%) | 19.72 | 12.90 | 13.36 | 13.33 | 30.47 | 27.83 | 13.74 |
| Aged 25 to 44 years . . . . . . | Estimate | 2.69 | 2.46 | 2.82 | 2.80 | 2.75 | 2.67 | 2.61 |
| | *cv* (%) | 7.64 | 7.31 | 5.53 | 6.07 | 15.42 | 7.63 | 7.68 |
| Aged 45 to 64 years . . . . . . | Estimate | 7.74 | 7.98 | 9.85 | 7.91 | 7.63 | 7.77 | 8.16 |
| | *cv* (%) | 5.57 | 4.47 | 4.08 | 6.48 | 11.70 | 6.32 | 5.65 |
| Aged 65 years or older . . . . | Estimate | 9.15 | 9.59 | 12.35 | 10.31 | 8.67 | 9.33 | 10.09 |
| | *cv* (%) | 5.72 | 5.68 | 4.14 | 6.91 | 9.16 | 7.10 | 6.36 |
| Total . . . . . . . . . . . . . . . | Estimate | 3.84 | 3.68 | 4.51 | 3.83 | 3.78 | 3.85 | 3.83 |
| | *cv* (%) | 3.86 | 3.22 | 2.83 | 4.40 | 8.15 | 4.41 | 3.81 |
| Age . . . . . . . . . . . . . . . . | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years . . . . . . | Estimate | 0.13 | 0.50 | 0.36 | 0.78 | 1.46 | 0.34 | 0.54 |
| | *cv* (%) | 101.48 | 38.37 | 86.87 | 27.54 | 41.34 | 47.58 | 13.01 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.33 | 0.63 | 0.13 | 0.46 | 0.53 | 0.25 | 0.36 |
| | *cv* (%) | 40.58 | 24.13 | 67.35 | 26.68 | 33.99 | 32.69 | 11.61 |
| Aged 18 to 24 years . . . . . . | Estimate | 1.64 | 1.19 | 1.86 | 1.64 | 1.73 | 2.38 | 1.63 |
| | *cv* (%) | 32.36 | 22.93 | 25.23 | 19.77 | 29.59 | 15.05 | 6.34 |
| Aged 25 to 44 years . . . . . . | Estimate | 2.08 | 2.37 | 4.25 | 2.74 | 3.27 | 2.62 | 2.70 |
| | *cv* (%) | 15.44 | 8.44 | 10.94 | 9.82 | 8.65 | 8.18 | 3.30 |
| Aged 45 to 64 years . . . . . . | Estimate | 7.47 | 8.53 | 12.86 | 9.96 | 7.89 | 7.92 | 8.55 |
| | *cv* (%) | 7.66 | 6.60 | 8.64 | 8.53 | 15.20 | 5.96 | 2.33 |
| Aged 65 years or older . . . . | Estimate | 8.25 | 10.62 | 15.83 | 13.09 | 9.78 | 10.50 | 10.57 |
| | *cv* (%) | 10.32 | 5.96 | 8.83 | 6.59 | 15.21 | 7.48 | 2.61 |
| Total . . . . . . . . . . . . . . . | Estimate | 3.26 | 4.05 | 6.00 | 4.38 | 3.87 | 3.81 | 4.03 |
| | *cv* (%) | 5.34 | 3.86 | 5.60 | 6.56 | 9.66 | 4.51 | 1.53 |

| Age | Category | Male | Female | Subdomain: Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
|---|---|---|---|---|---|---|---|---|
| | | Substantive variable: Percentage with interval since last doctor visit more than 5 years | | | | | | |
| Aged under 5 years | Estimate | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| | *cv* (%) | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Aged 5 to 17 years | Estimate | 1.83 | 1.84 | 2.33 | 1.50 | 3.30 | 3.27 | 1.79 |
| | *cv* (%) | 7.79 | 9.12 | 16.02 | 8.89 | 11.98 | 36.33 | 6.68 |
| Aged 18 to 24 years | Estimate | 4.64 | 1.41 | 3.59 | 2.47 | 6.02 | 6.61 | 2.88 |
| | *cv* (%) | 6.97 | 12.13 | 12.25 | 7.65 | 13.62 | 23.38 | 6.65 |
| Aged 25 to 44 years | Estimate | 6.58 | 2.05 | 4.11 | 4.06 | 6.63 | 8.90 | 4.14 |
| | *cv* (%) | 3.19 | 5.13 | 9.02 | 3.20 | 9.30 | 14.02 | 2.86 |
| Aged 45 to 64 years | Estimate | 6.17 | 4.11 | 4.12 | 5.17 | 5.83 | 6.37 | 5.07 |
| | *cv* (%) | 3.84 | 4.34 | 9.50 | 3.52 | 9.71 | 16.77 | 3.20 |
| Aged 65 years or older | Estimate | 4.10 | 3.26 | 3.35 | 3.55 | 5.89 | 4.78 | 3.60 |
| | *cv* (%) | 6.78 | 5.11 | 15.22 | 4.66 | 17.77 | 62.55 | 4.13 |
| Total | Estimate | 4.57 | 2.36 | 3.18 | 3.33 | 4.80 | 6.07 | 3.36 |
| | *cv* (%) | 2.29 | 2.95 | 6.46 | 2.35 | 5.82 | 11.62 | 2.09 |
| Age | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years | Estimate | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| | *cv* (%) | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Aged 5 to 17 years | Estimate | 0.65 | 1.05 | 2.61 | 2.51 | 0.78 | 0.60 | 0.81 |
| | *cv* (%) | 24.07 | 16.72 | 8.86 | 11.19 | 58.31 | 23.11 | 18.10 |
| Aged 18 to 24 years | Estimate | 2.47 | 2.11 | 3.57 | 3.57 | 0.71 | 3.15 | 1.87 |
| | *cv* (%) | 12.87 | 13.20 | 10.54 | 13.57 | 20.84 | 13.53 | 15.03 |
| Aged 25 to 44 years | Estimate | 3.99 | 4.23 | 4.16 | 4.78 | 3.85 | 4.05 | 4.24 |
| | *cv* (%) | 6.08 | 5.56 | 5.31 | 5.84 | 10.69 | 7.41 | 6.14 |
| Aged 45 to 64 years | Estimate | 5.33 | 5.23 | 4.94 | 4.95 | 6.30 | 5.01 | 5.33 |
| | *cv* (%) | 7.14 | 6.38 | 6.23 | 6.87 | 10.62 | 8.49 | 7.13 |
| Aged 65 years or older | Estimate | 3.53 | 4.01 | 3.39 | 3.58 | 3.72 | 3.45 | 4.19 |
| | *cv* (%) | 9.40 | 8.20 | 8.44 | 8.85 | 18.56 | 11.21 | 8.82 |
| Total | Estimate | 3.19 | 3.22 | 3.53 | 3.74 | 3.16 | 3.20 | 3.19 |
| | *cv* (%) | 4.17 | 4.08 | 4.12 | 4.65 | 7.51 | 5.11 | 4.15 |
| Age | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years | Estimate | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| | *cv* (%) | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Aged 5 to 17 years | Estimate | 1.66 | 1.58 | 3.36 | 3.34 | 3.17 | 2.24 | 1.83 |
| | *cv* (%) | 30.33 | 17.83 | 17.92 | 13.84 | 19.64 | 13.67 | 6.64 |
| Aged 18 to 24 years | Estimate | 2.86 | 2.66 | 2.50 | 5.23 | 1.96 | 4.23 | 2.98 |
| | *cv* (%) | 23.10 | 15.42 | 30.62 | 13.60 | 28.08 | 15.20 | 6.22 |
| Aged 25 to 44 years | Estimate | 4.20 | 3.44 | 3.89 | 5.10 | 4.41 | 4.92 | 4.27 |
| | *cv* (%) | 11.51 | 7.02 | 10.39 | 8.64 | 8.30 | 7.58 | 2.67 |
| Aged 45 to 64 years | Estimate | 4.94 | 4.15 | 6.15 | 5.33 | 5.86 | 4.63 | 5.10 |
| | *cv* (%) | 11.64 | 9.78 | 13.15 | 10.57 | 15.05 | 7.77 | 3.09 |
| Aged 65 years or older | Estimate | 3.52 | 3.04 | 3.67 | 3.74 | 3.71 | 3.54 | 3.61 |
| | *cv* (%) | 19.48 | 13.10 | 24.51 | 12.33 | 20.52 | 11.88 | 4.14 |
| Total | Estimate | 3.29 | 2.86 | 3.79 | 4.21 | 3.69 | 3.76 | 3.43 |
| | *cv* (%) | 9.02 | 5.98 | 8.82 | 5.92 | 8.71 | 5.75 | 2.05 |

| Age | Category | Male | Female | Subdomain: Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
|---|---|---|---|---|---|---|---|---|
| Substantive variable: Percentage reporting poor health | | | | | | | | |
| Aged under 5 years . . . . . . | Estimate | 0.55 | 0.59 | 0.43 | 0.52 | 1.08 | 0.00 | 0.59 |
| | *cv* (%) | 19.83 | 24.32 | 37.53 | 19.61 | 37.14 | 0.00 | 16.09 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.22 | 0.37 | 0.49 | 0.23 | 0.43 | 0.00 | 0.30 |
| | *cv* (%) | 25.15 | 14.87 | 35.42 | 16.75 | 31.29 | 0.00 | 13.93 |
| Aged 18 to 24 years . . . . . . | Estimate | 0.31 | 0.55 | 0.54 | 0.37 | 0.83 | 0.00 | 0.45 |
| | *cv* (%) | 25.58 | 17.67 | 33.78 | 18.81 | 32.16 | 0.00 | 15.62 |
| Aged 25 to 44 years . . . . . . | Estimate | 1.29 | 1.29 | 2.44 | 1.10 | 1.61 | 0.76 | 1.30 |
| | *cv* (%) | 6.65 | 5.87 | 8.87 | 4.75 | 14.32 | 42.31 | 4.60 |
| Aged 45 to 64 years . . . . . . | Estimate | 5.87 | 5.05 | 9.64 | 4.94 | 5.51 | 3.18 | 5.49 |
| | *cv* (%) | 4.95 | 4.38 | 6.37 | 4.12 | 14.92 | 33.64 | 3.44 |
| Aged 65 years or older . . . . | Estimate | 9.71 | 9.43 | 19.24 | 8.64 | 9.67 | 4.07 | 9.60 |
| | *cv* (%) | 3.88 | 3.07 | 5.12 | 2.65 | 13.01 | 34.38 | 2.42 |
| Total . . . . . . . . . . . . . . . | Estimate | 2.63 | 2.82 | 4.09 | 2.60 | 1.99 | 0.99 | 2.77 |
| | *cv* (%) | 2.45 | 2.58 | 4.10 | 2.36 | 8.09 | 24.81 | 2.00 |
| Age . . . . . . . . . . . . . . . . | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years . . . . . . | Estimate | 0.27 | 0.35 | 0.92 | 0.52 | 0.00 | 0.36 | 0.44 |
| | *cv* (%) | 45.75 | 35.98 | 20.45 | 42.16 | 0.00 | 45.45 | 38.58 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.16 | 0.25 | 0.41 | 0.27 | 0.17 | 0.16 | 0.25 |
| | *cv* (%) | 28.20 | 27.54 | 22.66 | 28.47 | 66.78 | 40.01 | 32.54 |
| Aged 18 to 24 years . . . . . . | Estimate | 0.22 | 0.50 | 0.52 | 0.44 | 0.13 | 0.26 | 0.39 |
| | *cv* (%) | 39.50 | 28.61 | 23.92 | 32.22 | 95.90 | 43.04 | 34.50 |
| Aged 25 to 44 years . . . . . . | Estimate | 1.08 | 1.14 | 1.69 | 1.03 | 0.98 | 1.12 | 1.26 |
| | *cv* (%) | 10.52 | 10.40 | 6.65 | 9.95 | 24.00 | 12.84 | 11.93 |
| Aged 45 to 64 years . . . . . . | Estimate | 3.54 | 4.96 | 7.45 | 4.66 | 4.11 | 3.36 | 5.37 |
| | *cv* (%) | 7.74 | 6.67 | 5.76 | 8.49 | 18.18 | 9.57 | 7.39 |
| Aged 65 years or older . . . . | Estimate | 7.04 | 7.97 | 12.95 | 8.35 | 7.51 | 6.87 | 8.46 |
| | *cv* (%) | 6.91 | 6.19 | 4.27 | 7.05 | 14.92 | 8.30 | 6.48 |
| Total . . . . . . . . . . . . . . . | Estimate | 2.07 | 2.35 | 3.68 | 2.22 | 2.17 | 2.03 | 2.53 |
| | *cv* (%) | 5.21 | 4.35 | 2.90 | 5.64 | 13.86 | 6.25 | 4.88 |
| Age . . . . . . . . . . . . . . . . | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years . . . . . . | Estimate | 0.13 | 0.65 | 1.14 | 1.11 | 1.17 | 0.23 | 0.57 |
| | *cv* (%) | 102.85 | 41.73 | 42.25 | 27.81 | 50.58 | 62.30 | 16.13 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.24 | 0.37 | 0.57 | 0.37 | 0.14 | 0.33 | 0.29 |
| | *cv* (%) | 55.23 | 42.85 | 34.87 | 33.91 | 61.83 | 30.58 | 13.93 |
| Aged 18 to 24 years . . . . . . | Estimate | 0.81 | 0.38 | 0.51 | 0.71 | 0.39 | 0.46 | 0.44 |
| | *cv* (%) | 43.10 | 41.68 | 43.66 | 36.19 | 61.34 | 38.23 | 15.64 |
| Aged 25 to 44 years . . . . . . | Estimate | 0.81 | 1.21 | 2.79 | 1.78 | 1.02 | 1.03 | 1.29 |
| | *cv* (%) | 21.81 | 13.29 | 14.41 | 8.54 | 22.74 | 11.46 | 4.59 |
| Aged 45 to 64 years . . . . . . | Estimate | 3.83 | 6.76 | 9.99 | 6.99 | 4.58 | 4.69 | 5.44 |
| | *cv* (%) | 12.06 | 9.15 | 12.31 | 9.57 | 14.87 | 9.52 | 3.42 |
| Aged 65 years or older . . . . | Estimate | 6.67 | 12.07 | 16.39 | 12.52 | 8.97 | 8.12 | 9.55 |
| | *cv* (%) | 13.44 | 5.79 | 9.89 | 8.54 | 11.62 | 8.37 | 2.40 |
| Total . . . . . . . . . . . . . . . | Estimate | 1.90 | 3.40 | 5.04 | 3.40 | 2.22 | 2.23 | 2.73 |
| | *cv* (%) | 7.98 | 5.35 | 4.36 | 7.04 | 13.42 | 5.13 | 1.99 |

| | | | | Subdomain | | | | |
|---|---|---|---|---|---|---|---|---|
| Age | Category | Male | Female | Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
| | | Substantive variable: Percentage reporting at least 2 hospitalizations in the past year | | | | | | |
| Aged under 5 years . . . . . . | Estimate | 1.47 | 1.15 | 1.26 | 1.36 | 1.05 | 0.00 | 1.35 |
| | *cv* (%) | 14.02 | 14.10 | 23.41 | 11.10 | 24.87 | 0.00 | 9.56 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.32 | 0.35 | 0.21 | 0.39 | 0.13 | 0.00 | 0.35 |
| | *cv* (%) | 16.36 | 16.34 | 35.06 | 12.12 | 49.27 | 0.00 | 11.49 |
| Aged 18 to 24 years . . . . . . | Estimate | 0.50 | 1.65 | 1.24 | 1.05 | 1.18 | 0.58 | 1.11 |
| | *cv* (%) | 17.97 | 10.77 | 19.18 | 10.82 | 25.45 | 71.12 | 9.35 |
| Aged 25 to 44 years . . . . . . | Estimate | 0.79 | 1.40 | 1.51 | 1.05 | 0.99 | 0.09 | 1.13 |
| | *cv* (%) | 8.57 | 5.81 | 12.14 | 5.47 | 18.27 | 100.07 | 4.68 |
| Aged 45 to 64 years . . . . . . | Estimate | 2.32 | 1.94 | 2.75 | 2.08 | 1.60 | 0.21 | 2.16 |
| | *cv* (%) | 6.74 | 7.20 | 9.95 | 5.06 | 23.71 | 98.96 | 4.55 |
| Aged 65 years or older . . . . | Estimate | 4.75 | 4.26 | 4.71 | 4.44 | 4.44 | 3.18 | 4.48 |
| | *cv* (%) | 6.65 | 5.57 | 11.58 | 4.35 | 21.31 | 45.49 | 4.12 |
| Total . . . . . . . . . . . . . . . | Estimate | 1.41 | 1.71 | 1.61 | 1.61 | 1.03 | 0.29 | 1.60 |
| | *cv* (%) | 3.55 | 3.94 | 6.16 | 2.97 | 9.97 | 33.41 | 2.71 |
| Age . . . . . . . . . . . . . . . . | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years . . . . . . | Estimate | 0.95 | 1.64 | 1.67 | 0.66 | 1.01 | 0.93 | 1.80 |
| | *cv* (%) | 23.17 | 19.28 | 14.99 | 26.99 | 42.01 | 27.77 | 18.37 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.15 | 0.40 | 0.41 | 0.28 | 0.18 | 0.14 | 0.40 |
| | *cv* (%) | 30.94 | 19.00 | 20.39 | 26.60 | 71.78 | 32.12 | 22.54 |
| Aged 18 to 24 years . . . . . . | Estimate | 0.77 | 1.25 | 1.19 | 1.05 | 0.58 | 0.85 | 1.13 |
| | *cv* (%) | 20.81 | 15.94 | 17.86 | 19.40 | 46.80 | 24.11 | 21.03 |
| Aged 25 to 44 years . . . . . . | Estimate | 1.06 | 1.23 | 1.16 | 0.89 | 1.00 | 1.09 | 1.31 |
| | *cv* (%) | 10.23 | 10.28 | 7.14 | 10.89 | 17.73 | 12.41 | 12.13 |
| Aged 45 to 64 years . . . . . . | Estimate | 2.06 | 2.31 | 2.23 | 1.78 | 2.11 | 2.04 | 2.49 |
| | *cv* (%) | 9.27 | 9.06 | 7.18 | 13.11 | 16.39 | 11.19 | 9.35 |
| Aged 65 years or older . . . . | Estimate | 3.55 | 4.41 | 5.27 | 4.15 | 3.26 | 3.66 | 4.32 |
| | *cv* (%) | 8.46 | 9.09 | 6.85 | 10.14 | 15.53 | 10.58 | 9.49 |
| Total . . . . . . . . . . . . . . . | Estimate | 1.40 | 1.68 | 1.75 | 1.28 | 1.34 | 1.43 | 1.72 |
| | *cv* (%) | 5.03 | 5.97 | 4.85 | 7.05 | 9.34 | 6.02 | 6.49 |
| Age . . . . . . . . . . . . . . . . | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years . . . . . . | Estimate | 1.25 | 1.74 | 3.04 | 1.07 | 1.03 | 0.49 | 1.31 |
| | *cv* (%) | 56.07 | 16.51 | 27.56 | 37.20 | 29.57 | 37.47 | 9.57 |
| Aged 5 to 17 years . . . . . . | Estimate | 0.41 | 0.24 | 0.86 | 0.40 | 0.40 | 0.23 | 0.34 |
| | *cv* (%) | 36.48 | 39.90 | 33.09 | 34.66 | 32.30 | 34.39 | 11.51 |
| Aged 18 to 24 years . . . . . . | Estimate | 1.62 | 0.53 | 1.43 | 1.91 | 1.09 | 1.04 | 1.09 |
| | *cv* (%) | 20.46 | 27.49 | 40.42 | 19.51 | 33.67 | 24.24 | 9.40 |
| Aged 25 to 44 years . . . . . . | Estimate | 1.02 | 0.91 | 2.10 | 1.06 | 0.98 | 0.86 | 1.10 |
| | *cv* (%) | 17.52 | 15.08 | 10.72 | 13.67 | 21.05 | 13.38 | 4.67 |
| Aged 45 to 64 years . . . . . . | Estimate | 1.80 | 2.00 | 2.24 | 2.52 | 2.45 | 1.53 | 2.12 |
| | *cv* (%) | 23.32 | 12.04 | 16.01 | 11.09 | 26.16 | 12.79 | 4.52 |
| Aged 65 years or older . . . . | Estimate | 4.66 | 4.34 | 6.25 | 6.10 | 5.57 | 3.63 | 4.46 |
| | *cv* (%) | 21.67 | 10.47 | 15.00 | 9.21 | 16.72 | 11.50 | 4.11 |
| Total . . . . . . . . . . . . . . . | Estimate | 1.55 | 1.46 | 2.36 | 1.82 | 1.60 | 1.15 | 1.57 |
| | *cv* (%) | 11.97 | 6.38 | 8.98 | 8.51 | 10.79 | 7.52 | 2.71 |

| | | | | Subdomain | | | | |
|---|---|---|---|---|---|---|---|---|
| Age | Category | Male | Female | Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
| | | Substantive variable: Percentage reporting at least 1 doctor visit in the past year | | | | | | |
| Aged under 5 years . . . . . . | Estimate | 91.08 | 90.47 | 87.68 | 91.89 | 87.63 | 90.55 | 90.79 |
| | *cv* (%) | 0.50 | 0.48 | 1.23 | 0.43 | 1.39 | 2.34 | 0.36 |
| Aged 5 to 17 years . . . . . . | Estimate | 72.97 | 74.54 | 66.95 | 76.21 | 66.80 | 68.40 | 73.88 |
| | *cv* (%) | 0.69 | 0.74 | 1.77 | 0.65 | 1.54 | 3.99 | 0.57 |
| Aged 18 to 24 years . . . . . . | Estimate | 59.37 | 80.23 | 65.50 | 71.97 | 61.73 | 59.53 | 70.36 |
| | *cv* (%) | 1.14 | 0.78 | 2.16 | 0.71 | 2.79 | 5.97 | 0.67 |
| Aged 25 to 44 years . . . . . . | Estimate | 60.55 | 79.10 | 69.24 | 70.69 | 64.14 | 60.36 | 70.28 |
| | *cv* (%) | 0.68 | 0.44 | 1.22 | 0.45 | 1.35 | 2.87 | 0.39 |
| Aged 45 to 64 years . . . . . . | Estimate | 69.22 | 78.62 | 75.41 | 74.21 | 70.47 | 68.56 | 74.25 |
| | *cv* (%) | 0.68 | 0.49 | 1.26 | 0.54 | 1.84 | 3.18 | 0.47 |
| Aged 65 years or older . . . . | Estimate | 81.69 | 85.27 | 82.29 | 84.00 | 81.38 | 76.37 | 83.85 |
| | *cv* (%) | 0.68 | 0.45 | 1.17 | 0.44 | 2.61 | 5.40 | 0.44 |
| Total . . . . . . . . . . . . . . . | Estimate | 69.12 | 79.97 | 72.02 | 75.72 | 68.70 | 66.65 | 74.92 |
| | *cv* (%) | 0.35 | 0.29 | 0.86 | 0.29 | 0.98 | 2.05 | 0.26 |
| Age . . . . . . . . . . . . . . . . | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years . . . . . . | Estimate | 93.80 | 91.62 | 89.05 | 89.98 | 96.67 | 92.78 | 91.66 |
| | *cv* (%) | 0.82 | 0.53 | 0.68 | 0.92 | 0.97 | 1.02 | 0.64 |
| Aged 5 to 17 years . . . . . . | Estimate | 81.47 | 75.89 | 69.52 | 71.53 | 85.15 | 80.21 | 76.71 |
| | *cv* (%) | 0.98 | 1.16 | 1.00 | 1.12 | 2.45 | 1.13 | 1.26 |
| Aged 18 to 24 years . . . . . . | Estimate | 69.80 | 72.12 | 68.92 | 69.76 | 76.58 | 67.19 | 71.23 |
| | *cv* (%) | 1.59 | 1.14 | 1.33 | 1.69 | 2.09 | 1.86 | 1.23 |
| Aged 25 to 44 years . . . . . . | Estimate | 69.27 | 71.65 | 69.20 | 70.10 | 72.23 | 68.15 | 71.73 |
| | *cv* (%) | 0.92 | 0.78 | 0.83 | 0.83 | 1.96 | 1.05 | 0.91 |
| Aged 45 to 64 years . . . . . . | Estimate | 74.32 | 74.19 | 73.26 | 75.32 | 74.91 | 74.13 | 74.78 |
| | *cv* (%) | 0.85 | 0.91 | 0.77 | 0.88 | 1.52 | 1.07 | 1.10 |
| Aged 65 years or older . . . . | Estimate | 85.58 | 83.68 | 83.29 | 82.61 | 86.47 | 85.25 | 83.91 |
| | *cv* (%) | 0.80 | 0.84 | 0.77 | 0.81 | 1.48 | 0.95 | 1.02 |
| Total . . . . . . . . . . . . . . . | Estimate | 76.28 | 75.96 | 73.20 | 74.20 | 78.98 | 75.31 | 76.17 |
| | *cv* (%) | 0.53 | 0.50 | 0.49 | 0.61 | 0.92 | 0.61 | 0.56 |
| Age . . . . . . . . . . . . . . . . | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years . . . . . . | Estimate | 91.53 | 88.83 | 90.37 | 88.76 | 87.27 | 91.19 | 90.78 |
| | *cv* (%) | 1.15 | 1.11 | 1.42 | 1.00 | 1.79 | 1.06 | 0.37 |
| Aged 5 to 17 years . . . . . . | Estimate | 73.69 | 71.23 | 69.07 | 67.94 | 69.62 | 72.32 | 73.73 |
| | *cv* (%) | 2.78 | 1.91 | 2.20 | 1.53 | 2.29 | 1.32 | 0.57 |
| Aged 18 to 24 years . . . . . . | Estimate | 74.84 | 69.63 | 70.15 | 67.44 | 70.48 | 69.47 | 70.06 |
| | *cv* (%) | 2.36 | 1.92 | 3.20 | 1.82 | 2.73 | 2.17 | 0.67 |
| Aged 25 to 44 years . . . . . . | Estimate | 71.43 | 68.99 | 71.54 | 68.44 | 69.74 | 70.24 | 70.01 |
| | *cv* (%) | 1.32 | 1.32 | 1.41 | 1.27 | 1.55 | 1.02 | 0.40 |
| Aged 45 to 64 years . . . . . . | Estimate | 72.58 | 73.73 | 74.06 | 72.22 | 73.74 | 75.88 | 74.13 |
| | *cv* (%) | 1.84 | 1.12 | 1.89 | 1.57 | 2.36 | 0.96 | 0.48 |
| Aged 65 years or older . . . . | Estimate | 83.06 | 82.62 | 83.77 | 84.00 | 81.75 | 82.93 | 83.78 |
| | *cv* (%) | 1.59 | 1.17 | 1.49 | 1.24 | 1.97 | 0.96 | 0.44 |
| Total . . . . . . . . . . . . . . . | Estimate | 75.42 | 73.57 | 74.19 | 72.32 | 73.26 | 74.57 | 74.72 |
| | *cv* (%) | 1.05 | 0.82 | 0.97 | 0.66 | 1.20 | 0.72 | 0.27 |

| Age | Category | Male | Female | Subdomain: Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
|---|---|---|---|---|---|---|---|---|
| | | | | Substantive variable: Average number of doctor visits per capita | | | | |
| Aged under 5 years | Estimate | 4.49 | 4.35 | 3.25 | 4.69 | 4.15 | 3.56 | 4.45 |
| | *cv* (%) | 2.28 | 2.65 | 3.80 | 2.12 | 6.68 | 8.34 | 1.71 |
| Aged 5 to 17 years | Estimate | 2.43 | 2.52 | 1.81 | 2.71 | 1.88 | 1.61 | 2.50 |
| | *cv* (%) | 2.14 | 2.57 | 4.85 | 2.12 | 3.96 | 10.28 | 1.83 |
| Aged 18 to 24 years | Estimate | 1.93 | 4.32 | 2.59 | 3.34 | 2.51 | 1.67 | 3.20 |
| | *cv* (%) | 4.21 | 3.03 | 5.88 | 2.77 | 5.32 | 13.72 | 2.47 |
| Aged 25 to 44 years | Estimate | 2.76 | 4.69 | 3.33 | 3.85 | 3.23 | 2.65 | 3.78 |
| | *cv* (%) | 2.98 | 1.83 | 3.72 | 1.98 | 5.26 | 7.41 | 1.77 |
| Aged 45 to 64 years | Estimate | 3.94 | 4.88 | 5.37 | 4.28 | 4.99 | 2.92 | 4.46 |
| | *cv* (%) | 3.67 | 2.63 | 6.25 | 2.46 | 13.72 | 16.61 | 2.35 |
| Aged 65 years or older | Estimate | 5.60 | 5.84 | 6.47 | 5.61 | 7.73 | 5.59 | 5.74 |
| | *cv* (%) | 3.42 | 2.54 | 5.94 | 2.17 | 21.01 | 20.16 | 2.26 |
| Total | Estimate | 3.26 | 4.43 | 3.45 | 3.98 | 3.31 | 2.58 | 3.89 |
| | *cv* (%) | 1.43 | 1.08 | 2.41 | 1.11 | 4.20 | 6.50 | 0.94 |
| Age | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years | Estimate | 5.04 | 4.42 | 4.14 | 4.34 | 4.95 | 5.08 | 4.51 |
| | *cv* (%) | 6.10 | 3.74 | 2.85 | 3.14 | 7.60 | 7.32 | 5.04 |
| Aged 5 to 17 years | Estimate | 2.63 | 2.66 | 2.25 | 2.49 | 2.76 | 2.59 | 2.72 |
| | *cv* (%) | 3.10 | 3.69 | 3.09 | 3.85 | 6.16 | 3.85 | 4.04 |
| Aged 18 to 24 years | Estimate | 2.91 | 3.33 | 2.80 | 3.80 | 3.41 | 2.71 | 3.41 |
| | *cv* (%) | 4.30 | 3.97 | 3.89 | 7.08 | 5.48 | 6.03 | 5.06 |
| Aged 25 to 44 years | Estimate | 3.79 | 3.64 | 3.54 | 4.14 | 4.28 | 3.60 | 3.80 |
| | *cv* (%) | 2.86 | 2.84 | 3.26 | 3.31 | 7.06 | 3.73 | 2.96 |
| Aged 45 to 64 years | Estimate | 4.70 | 4.16 | 4.29 | 4.70 | 5.19 | 4.54 | 4.36 |
| | *cv* (%) | 5.48 | 3.93 | 4.50 | 5.03 | 16.02 | 4.46 | 4.58 |
| Aged 65 years or older | Estimate | 6.38 | 5.31 | 5.42 | 6.13 | 7.23 | 6.06 | 5.49 |
| | *cv* (%) | 5.99 | 4.12 | 3.36 | 5.28 | 20.08 | 4.43 | 4.92 |
| Total | Estimate | 4.12 | 3.77 | 3.63 | 4.11 | 4.56 | 3.96 | 3.90 |
| | *cv* (%) | 2.24 | 1.69 | 1.68 | 1.99 | 5.73 | 2.17 | 1.96 |
| Age | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years | Estimate | 4.19 | 4.08 | 4.25 | 4.16 | 4.59 | 4.22 | 4.42 |
| | *cv* (%) | 3.08 | 3.90 | 7.00 | 4.64 | 5.15 | 3.63 | 1.68 |
| Aged 5 to 17 years | Estimate | 2.50 | 2.10 | 2.89 | 2.13 | 2.66 | 2.42 | 2.48 |
| | *cv* (%) | 9.30 | 3.91 | 6.66 | 5.70 | 8.23 | 5.05 | 1.82 |
| Aged 18 to 24 years | Estimate | 3.10 | 2.92 | 2.48 | 2.82 | 4.63 | 3.46 | 3.16 |
| | *cv* (%) | 7.01 | 6.17 | 6.71 | 4.72 | 17.27 | 4.54 | 2.42 |
| Aged 25 to 44 years | Estimate | 3.23 | 3.52 | 3.75 | 3.47 | 4.39 | 4.05 | 3.74 |
| | *cv* (%) | 6.23 | 4.71 | 5.02 | 4.78 | 7.85 | 3.10 | 1.75 |
| Aged 45 to 64 years | Estimate | 3.64 | 4.04 | 4.77 | 4.36 | 5.02 | 4.58 | 4.43 |
| | *cv* (%) | 6.01 | 6.40 | 6.92 | 8.47 | 12.73 | 4.79 | 2.35 |
| Aged 65 years or older | Estimate | 4.85 | 5.28 | 5.68 | 5.50 | 5.97 | 6.18 | 5.74 |
| | *cv* (%) | 7.86 | 4.22 | 4.25 | 5.43 | 13.38 | 4.82 | 2.26 |
| Total | Estimate | 3.42 | 3.57 | 3.91 | 3.56 | 4.36 | 4.02 | 3.86 |
| | *cv* (%) | 3.10 | 2.48 | 3.13 | 2.48 | 4.79 | 2.03 | 0.94 |

| Age | Category | Male | Female | Subdomain: Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
|---|---|---|---|---|---|---|---|---|
| | | | | Substantive variable: Average number of bed days per capita | | | | |
| Aged under 5 years . . . . . . | Estimate | 3.23 | 3.33 | 2.61 | 3.40 | 3.33 | 2.26 | 3.31 |
| | *cv* (%) | 4.15 | 5.65 | 9.54 | 4.16 | 7.87 | 15.48 | 3.59 |
| Aged 5 to 17 years . . . . . . | Estimate | 2.54 | 3.14 | 1.99 | 3.07 | 2.42 | 1.21 | 2.88 |
| | *cv* (%) | 3.65 | 3.41 | 7.75 | 2.92 | 9.02 | 12.53 | 2.73 |
| Aged 18 to 24 years . . . . . . | Estimate | 2.36 | 4.14 | 3.35 | 3.35 | 2.58 | 1.57 | 3.32 |
| | *cv* (%) | 5.24 | 4.78 | 14.35 | 3.75 | 9.05 | 16.08 | 3.76 |
| Aged 25 to 44 years . . . . . . | Estimate | 3.66 | 5.25 | 5.90 | 4.33 | 3.92 | 2.16 | 4.54 |
| | *cv* (%) | 4.11 | 2.98 | 5.60 | 2.96 | 8.41 | 15.14 | 2.54 |
| Aged 45 to 64 years . . . . . . | Estimate | 7.16 | 7.95 | 1.16 | 7.22 | 6.49 | 3.71 | 7.65 |
| | *cv* (%) | 4.84 | 4.34 | 8.35 | 4.03 | 17.47 | 28.93 | 3.64 |
| Aged 65 years or older . . . . | Estimate | 0.76 | 1.67 | 9.59 | 0.55 | 0.26 | 8.47 | 1.32 |
| | *cv* (%) | 5.43 | 5.15 | 10.19 | 4.15 | 15.72 | 33.60 | 4.12 |
| Total . . . . . . . . . . . . . . . | Estimate | 4.64 | 6.00 | 6.32 | 5.35 | 3.89 | 2.45 | 5.42 |
| | *cv* (%) | 2.43 | 2.35 | 4.20 | 2.23 | 6.10 | 10.78 | 2.03 |

| Age . . . . . . . . . . . . . . . . | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
|---|---|---|---|---|---|---|---|---|
| Aged under 5 years . . . . . . | Estimate | 3.52 | 2.95 | 3.39 | 3.27 | 3.48 | 3.54 | 3.08 |
| | *cv* (%) | 8.74 | 7.41 | 6.31 | 7.16 | 8.84 | 11.15 | 9.28 |
| Aged 5 to 17 years . . . . . . | Estimate | 3.01 | 2.79 | 2.69 | 2.97 | 3.39 | 2.88 | 2.90 |
| | *cv* (%) | 7.45 | 4.69 | 4.99 | 4.30 | 7.57 | 9.97 | 5.68 |
| Aged 18 to 24 years . . . . . . | Estimate | 3.13 | 3.34 | 3.16 | 3.52 | 3.32 | 3.05 | 3.45 |
| | *cv* (%) | 7.61 | 7.44 | 7.58 | 5.63 | 15.46 | 10.31 | 9.07 |
| Aged 25 to 44 years . . . . . . | Estimate | 4.58 | 3.84 | 4.84 | 4.52 | 4.91 | 4.46 | 4.11 |
| | *cv* (%) | 6.28 | 5.10 | 4.58 | 4.49 | 8.18 | 8.32 | 6.17 |
| Aged 45 to 64 years . . . . . . | Estimate | 7.69 | 7.12 | 8.05 | 7.19 | 9.00 | 7.26 | 7.80 |
| | *cv* (%) | 7.57 | 7.85 | 6.41 | 7.75 | 14.76 | 8.55 | 9.16 |
| Aged 65 years or older . . . . | Estimate | 0.30 | 8.68 | 3.67 | 1.53 | 8.83 | 0.87 | 9.39 |
| | *cv* (%) | 7.75 | 8.63 | 6.84 | 8.77 | 17.04 | 8.96 | 9.55 |
| Total . . . . . . . . . . . . . . . | Estimate | 5.47 | 4.70 | 5.79 | 5.25 | 5.68 | 5.40 | 5.03 |
| | *cv* (%) | 3.47 | 4.67 | 3.31 | 3.70 | 6.63 | 4.62 | 5.54 |

| Age . . . . . . . . . . . . . . . . | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
|---|---|---|---|---|---|---|---|---|
| Aged under 5 years . . . . . . | Estimate | 2.62 | 3.03 | 4.04 | 3.50 | 3.64 | 3.11 | 3.28 |
| | *cv* (%) | 9.40 | 7.70 | 12.91 | 12.22 | 9.90 | 9.25 | 3.60 |
| Aged 5 to 17 years . . . . . . | Estimate | 2.51 | 2.42 | 2.97 | 2.83 | 3.46 | 2.77 | 2.83 |
| | *cv* (%) | 9.26 | 8.59 | 7.14 | 7.36 | 8.63 | 4.85 | 2.71 |
| Aged 18 to 24 years . . . . . . | Estimate | 3.02 | 2.63 | 4.34 | 3.24 | 3.48 | 3.54 | 3.27 |
| | *cv* (%) | 10.80 | 9.45 | 24.17 | 8.20 | 10.28 | 6.85 | 3.73 |
| Aged 25 to 44 years . . . . . . | Estimate | 3.13 | 4.01 | 7.42 | 4.74 | 4.59 | 4.49 | 4.47 |
| | *cv* (%) | 7.26 | 7.21 | 9.49 | 5.75 | 7.36 | 5.62 | 2.54 |
| Aged 45 to 64 years . . . . . . | Estimate | 5.27 | 6.11 | 0.71 | 9.17 | 6.51 | 7.44 | 7.57 |
| | *cv* (%) | 12.01 | 8.49 | 11.05 | 11.58 | 10.95 | 9.41 | 3.64 |
| Aged 65 years or older . . . . | Estimate | 6.83 | 1.00 | 8.87 | 4.90 | 9.18 | 2.40 | 1.29 |
| | *cv* (%) | 16.34 | 10.00 | 15.11 | 8.79 | 17.68 | 9.96 | 4.08 |
| Total . . . . . . . . . . . . . . . | Estimate | 3.79 | 4.81 | 7.97 | 5.97 | 4.96 | 5.36 | 5.34 |
| | *cv* (%) | 6.55 | 5.05 | 6.74 | 5.49 | 5.51 | 4.63 | 2.02 |

| Age | Category | Male | Female | Subdomain: Black not Hispanic | Other not Hispanic | Hispanic | Asian | Not Asian |
|---|---|---|---|---|---|---|---|---|
| Substantive variable: Average number of conditions per capita | | | | | | | | |
| Aged under 5 years | Estimate | 2.17 | 1.90 | 1.64 | 2.15 | 1.84 | 1.24 | 2.06 |
| | *cv* (%) | 2.76 | 3.00 | 6.10 | 2.63 | 6.75 | 15.81 | 2.18 |
| Aged 5 to 17 years | Estimate | 2.19 | 2.12 | 1.64 | 2.32 | 1.79 | 0.84 | 2.19 |
| | *cv* (%) | 2.02 | 2.22 | 4.07 | 1.69 | 5.45 | 18.72 | 1.57 |
| Aged 18 to 24 years | Estimate | 2.06 | 3.00 | 1.97 | 2.73 | 1.87 | 1.35 | 2.57 |
| | *cv* (%) | 2.81 | 2.45 | 5.26 | 2.05 | 6.95 | 23.30 | 1.80 |
| Aged 25 to 44 years | Estimate | 3.06 | 4.08 | 3.40 | 3.68 | 2.84 | 1.89 | 3.63 |
| | *cv* (%) | 1.66 | 1.18 | 2.61 | 1.25 | 4.15 | 8.43 | 1.08 |
| Aged 45 to 64 years | Estimate | 5.68 | 6.62 | 7.05 | 6.12 | 5.37 | 3.58 | 6.23 |
| | *cv* (%) | 1.57 | 1.41 | 2.95 | 1.29 | 5.27 | 8.88 | 1.21 |
| Aged 65 years or older | Estimate | 8.80 | 0.25 | 1.11 | 9.52 | 9.36 | 4.95 | 9.69 |
| | *cv* (%) | 1.71 | 1.32 | 3.56 | 1.25 | 5.68 | 11.21 | 1.28 |
| Total | Estimate | 3.78 | 4.78 | 3.86 | 4.50 | 2.93 | 1.97 | 4.36 |
| | *cv* (%) | 0.85 | 0.72 | 1.92 | 0.78 | 2.83 | 6.02 | 0.66 |
| Age | Category | Northeast | Midwest | South | West | New England | Mid-Atlantic | East North Central |
| Aged under 5 years | Estimate | 1.84 | 2.14 | 2.03 | 2.10 | 2.00 | 1.78 | 2.09 |
| | *cv* (%) | 6.09 | 4.51 | 3.80 | 4.99 | 11.04 | 7.08 | 5.04 |
| Aged 5 to 17 years | Estimate | 2.02 | 2.34 | 2.01 | 2.27 | 2.24 | 1.94 | 2.38 |
| | *cv* (%) | 3.85 | 2.98 | 3.28 | 3.18 | 6.82 | 4.53 | 3.61 |
| Aged 18 to 24 years | Estimate | 2.09 | 2.70 | 2.52 | 2.84 | 2.31 | 2.00 | 2.71 |
| | *cv* (%) | 3.81 | 3.67 | 2.99 | 5.00 | 8.03 | 4.61 | 4.21 |
| Aged 25 to 44 years | Estimate | 3.19 | 3.78 | 3.58 | 3.74 | 3.40 | 3.11 | 3.82 |
| | *cv* (%) | 2.34 | 2.11 | 2.33 | 2.06 | 4.44 | 3.15 | 2.37 |
| Aged 45 to 64 years | Estimate | 5.19 | 6.33 | 6.74 | 6.07 | 5.64 | 5.05 | 6.54 |
| | *cv* (%) | 2.55 | 2.30 | 2.50 | 2.30 | 4.80 | 3.00 | 2.68 |
| Aged 65 years or older | Estimate | 8.38 | 9.73 | 0.57 | 9.38 | 9.27 | 8.04 | 9.88 |
| | *cv* (%) | 2.12 | 2.18 | 2.64 | 2.90 | 3.27 | 2.78 | 2.88 |
| Total | Estimate | 3.87 | 4.43 | 4.47 | 4.26 | 4.22 | 3.75 | 4.51 |
| | *cv* (%) | 1.39 | 1.39 | 1.50 | 1.51 | 2.87 | 1.74 | 1.74 |
| Age | Category | West North Central | South Atlantic | East South Central | West South Central | Mountain | Pacific | Total |
| Aged under 5 years | Estimate | 2.27 | 1.75 | 2.70 | 2.06 | 2.10 | 2.10 | 2.04 |
| | *cv* (%) | 7.81 | 6.45 | 9.45 | 5.19 | 7.35 | 6.32 | 2.14 |
| Aged 5 to 17 years | Estimate | 2.26 | 1.78 | 2.53 | 2.02 | 2.57 | 2.15 | 2.15 |
| | *cv* (%) | 4.77 | 4.67 | 6.96 | 5.05 | 5.88 | 3.73 | 1.59 |
| Aged 18 to 24 years | Estimate | 2.68 | 2.35 | 2.79 | 2.61 | 3.10 | 2.74 | 2.54 |
| | *cv* (%) | 8.41 | 5.08 | 8.16 | 5.08 | 6.62 | 6.39 | 1.85 |
| Aged 25 to 44 years | Estimate | 3.65 | 3.13 | 4.74 | 3.61 | 4.04 | 3.63 | 3.58 |
| | *cv* (%) | 5.02 | 2.80 | 6.73 | 2.88 | 4.36 | 2.66 | 1.09 |
| Aged 45 to 64 years | Estimate | 5.78 | 6.12 | 8.48 | 6.61 | 6.54 | 5.90 | 6.17 |
| | *cv* (%) | 3.64 | 3.18 | 5.36 | 3.16 | 4.44 | 2.51 | 1.20 |
| Aged 65 years or older | Estimate | 9.32 | 9.54 | 3.72 | 0.50 | 0.37 | 9.02 | 9.65 |
| | *cv* (%) | 3.26 | 3.75 | 6.39 | 3.04 | 6.01 | 3.20 | 1.26 |
| Total | Estimate | 4.24 | 4.11 | 5.75 | 4.32 | 4.55 | 4.14 | 4.30 |
| | *cv* (%) | 3.21 | 2.03 | 3.80 | 2.11 | 3.43 | 1.71 | 0.67 |

# Vital and Health Statistics series descriptions

SERIES 1. **Programs and Collection Procedures**—These reports describe the data collection programs of the National Center for Health Statistics. They include descriptions of the methods used to collect and process the data, definitions, and other material necessary for understanding the data.

SERIES 2. **Data Evaluation and Methods Research**—These reports are studies of new statistical methods and include analytical techniques, objective evaluations of reliability of collected data, and contributions to statistical theory. These studies also include experimental tests of new survey methods and comparisons of U.S. methodology with those of other countries.

SERIES 3. **Analytical and Epidemiological Studies**—These reports present analytical or interpretive studies based on vital and health statistics. These reports carry the analyses further than the expository types of reports in the other series.

SERIES 4. **Documents and Committee Reports**—These are final reports of major committees concerned with vital and health statistics and documents such as recommended model vital registration laws and revised birth and death certificates.

SERIES 5. **International Vital and Health Statistics Reports**—These reports are analytical or descriptive reports that compare U.S. vital and health statistics with those of other countries or present other international data of relevance to the health statistics system of the United States.

SERIES 6. **Cognition and Survey Measurement**—These reports are from the National Laboratory for Collaborative Research in Cognition and Survey Measurement. They use methods of cognitive science to design, evaluate, and test survey instruments.

SERIES 10. **Data From the National Health Interview Survey**—These reports contain statistics on illness; unintentional injuries; disability; use of hospital, medical, and other health services; and a wide range of special current health topics covering many aspects of health behaviors, health status, and health care utilization. They are based on data collected in a continuing national household interview survey.

SERIES 11. **Data From the National Health Examination Survey, the National Health and Nutrition Examination Surveys, and the Hispanic Health and Nutrition Examination Survey**—Data from direct examination, testing, and measurement on representative samples of the civilian noninstitutionalized population provide the basis for (1) medically defined total prevalence of specific diseases or conditions in the United States and the distributions of the population with respect to physical, physiological, and psychological characteristics, and (2) analyses of trends and relationships among various measurements and between survey periods.

SERIES 12. **Data From the Institutionalized Population Surveys**—Discontinued in 1975. Reports from these surveys are included in Series 13.

SERIES 13. **Data From the National Health Care Survey**—These reports contain statistics on health resources and the public's use of health care resources including ambulatory, hospital, and long-term care services based on data collected directly from health care providers and provider records.

SERIES 14. **Data on Health Resources: Manpower and Facilities**—Discontinued in 1990. Reports on the numbers, geographic distribution, and characteristics of health resources are now included in Series 13.

SERIES 15. **Data From Special Surveys**—These reports contain statistics on health and health-related topics collected in special surveys that are not part of the continuing data systems of the National Center for Health Statistics.

SERIES 16. **Compilations of Advance Data From Vital and Health Statistics**—Advance Data Reports provide early release of information from the National Center for Health Statistics' health and demographic surveys. They are compiled in the order in which they are published. Some of these releases may be followed by detailed reports in Series 10–13.

SERIES 20. **Data on Mortality**—These reports contain statistics on mortality that are not included in regular, annual, or monthly reports. Special analyses by cause of death, age, other demographic variables, and geographic and trend analyses are included.

SERIES 21. **Data on Natality, Marriage, and Divorce**—These reports contain statistics on natality, marriage, and divorce that are not included in regular, annual, or monthly reports. Special analyses by health and demographic variables and geographic and trend analyses are included.

SERIES 22. **Data From the National Mortality and Natality Surveys**—Discontinued in 1975. Reports from these sample surveys, based on vital records, are now published in Series 20 or 21.

SERIES 23. **Data From the National Survey of Family Growth**—These reports contain statistics on factors that affect birth rates, including contraception, infertility, cohabitation, marriage, divorce, and remarriage; adoption; use of medical care for family planning and infertility; and related maternal and infant health topics. These statistics are based on national surveys of women of childbearing age.

SERIES 24. **Compilations of Data on Natality, Mortality, Marriage, Divorce, and Induced Terminations of Pregnancy**—These include advance reports of births, deaths, marriages, and divorces based on final data from the National Vital Statistics System that were published as supplements to the *Monthly Vital Statistics Report* (MVSR). These reports provide highlights and summaries of detailed data subsequently published in *Vital Statistics of the United States*. Other supplements to the MVSR published here provide selected findings based on final data from the National Vital Statistics System and may be followed by detailed reports in Series 20 or 21.

For answers to questions about this report or for a list of reports published in these series, contact:

Data Dissemination Branch
National Center for Health Statistics
Centers for Disease Control and Prevention
6525 Belcrest Road, Room 1064
Hyattsville, MD 20782-2003

(301) 436–8500
E-mail: nchsquery@cdc.gov
Internet: www.cdc.gov/nchswww/

**DEPARTMENT OF**
**HEALTH & HUMAN SERVICES**

Centers for Disease Control and Prevention
National Center for Health Statistics
6525 Belcrest Road
Hyattsville, Maryland 20782-2003

STANDARD MAIL (B)
POSTAGE & FEES PAID
CDC/NCHS
PERMIT NO. G-284

OFFICIAL BUSINESS
PENALTY FOR PRIVATE USE, $300

DHHS Publication No. (PHS) 99-1326, Series 2, No. 126
8-0781 (7/99)