VITAL & HEALTH STATISTICS

Integration of Sample Design for the National Survey of Family Growth, Cycle IV, With the National Health Interview Survey

Research was undertaken to develop alternative methods of selecting a sample of eligible women for the National Survey of Family Growth (NSFG) from the National Health Interview Survey (NHIS). This report presents estimates of the effects of alternative design options, obtained by statistical modeling techniques, for linking the NSFG with the NHIS. The estimated survey costs, lengths of data collection period, and projected response rates for alternative linked design options and for the unlinked design are compared for fixed precision. The findings indicate that substantial gains in the NSFG design efficiency could be realized if particular linked design options are adopted to replace the independent design.

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Symbols

- - - Data not available

. . . Category not applicable

- Quantity zero

0.0 Quantity more than zero but less than 0.05

Z Quantity more than zero but less than 500 where numbers are rounded to thousands

* Figure does not meet standard of reliability or precision

Figure suppressed to comply with confidentiality requirements

Foreword

Several years ago the National Center for Health Statistics (NCHS) embarked on a long-range program to integrate the designs of its national household sample surveys, including the National Health Interview Survey (NHIS), the National Survey of Family Growth (NSFG), the National Medical Expenditure Survey (NMES), and the National Health and Nutrition Examination Survey (NHANES). Each had been originally designed and conducted as an independent survey with the U.S. Bureau of the Census serving as the NCHS collection agent for NHIS, and consulting firms in the private sector serving as the collection agents for the other surveys.

The basic concept of the proposed integrated design strategy is that NHIS serve as the sampling frame for each of the other NCHS household surveys. This design strategy appears to offer opportunities for substantial gains in the overall design efficiency of the integrated surveys. NHIS is by far the largest of the surveys, and it collects a wealth of household information that would be available for designing the samples for the other surveys. However, this strategy would not have been feasible prior to 1985, the year in which the most recently redesigned NHIS was fielded. In prior years, decennial population listings served as one of the sampling frames for NHIS, and, therefore, access to the personal identifiers of the sample households was prohibited by the U.S. Bureau of the Census confidentiality restrictions imposed by the law (13 U.S.C. 8,9). Beginning in 1985, however, this restriction no longer applies because the redesigned NHIS is based solely on an area sampling frame without any use of decennial U.S. Bureau of the Census listings.

A research program is currently underway to evaluate the effects on sampling errors, response rates, respondent burdens, timeliness, and so forth of linking the designs of NSFG, NMES, and NHANES with NHIS. This research is intended to provide answers to two questions:

1. Should the NCHS independently designed household surveys be replaced by an integrated survey design?

If question 1 is answered affirmatively, then,

2. What kind of integrated survey design should NCHS adopt?

In carrying out this research program the questions posed will be addressed separately for the NSFG, NMES, and NHANES because each survey involves its own unique linked (integrated) design issues. However, a similar two-phase research strategy will be followed in each case. First, statistical modeling techniques will be used to obtain provisional estimates of the effects of alternative linked survey design strategies. In these investigations the unlinked design will be used as the standard for comparison. Subsequently, the most promising linked survey design options will be verified and refined by field testing. Finally, the design effects of the field-tested linked survey design options will be evaluated.

This publication presents provisional estimates of the effects of alternative design options for linking the NSFG with the NHIS that were obtained by statistical modeling techniques. The estimated survey costs, lengths of the data collection period, and projected response rates for alternative linked design options and for the unlinked design are compared for fixed precision. The findings are quite encouraging and indicate that substantial gains in the NSFG design efficiency could be realized if particular linked design options are adopted to replace the independent design.

I provided technical oversight to Westat, Inc., the contractor on this study. I commend them and, in particular, Joseph Waksberg and Doris Northrup for a job well done. A number of NCHS staff participated in this project. Dr. Andrew White conducted the technical review of this report. He worked closely with the authors in making technical revisions and also worked with the publications unit in making editorial revisions. Without his endeavor this report would not have appeared in the Vital and Health Statistics Series. Robert Fuchsberg, Director, Division of Health Interview Statistics, and Dr. William Pratt, Chief, Family Growth Branch, deserve special acknowledgment. Without their full support and cooperation this project would not have been possible.

Monroe G. Sirken
Associate Director for Research and Methodology

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by Joseph Waksberg and Doris R. Northrup, Westat, Inc.

Chapter 1 Introduction

The research discussed in this report was undertaken to develop alternative methods of selecting a sample of eligible women for Cycle IV of the National Survey of Family Growth (NSFG) from the National Health Interview Survey (NHIS), to consider survey methods that are possible with the samples, and to analyze the characteristics of these methods.

The alternatives consisted of the possible combinations of three basic variables:

1. A 200- versus 100-primary sampling unit (PSU) sample design.
2. A sample of eligible women in the NHIS (with movers tracked and interviewed at their new residences) versus a sample of addresses. In the latter case, the sample would include addresses with and without eligible women, although the addresses with eligible women would be sampled at a much higher rate. New construction, vacant housing units, and nonresponses in NHIS would be treated as noneligible addresses.
3. Accumulation of sample cases in the NHIS until the desired sample size was attained before starting the field operations versus continuous interviewing. Accumulating the sample before beginning interviewing will result in the length of the interview period being about the same as in earlier cycles of the NSFG, about 4 months. With continuous interviewing, the interview period would cover approximately the length of time necessary to accumulate the sample from the NHIS.

Therefore, eight possible basic designs exist consisting of all combinations of these three variables. Furthermore, these eight can be expanded considerably. Variable 2 provides for sampling housing units at different rates, depending on whether they contained eligible women in NHIS.

However, early in the course of analysis it became clear that some alternatives were unnecessary or impractical. With continuous interviewing, there seemed to be no point in having a housing unit sample. If the Cycle IV interview followed soon after the identification of a case in NHIS, almost all eligible women in NHIS would still be at the same residence, and there would be no need to incur the relatively high cost of selecting and screening housing units with no eligible women.

Another group of alternatives discarded early in the study was any combination of 100 PSU's and the housing unit samples. For these combinations, it was apparent that it would take over 3 years for NHIS to accumulate the sample size necessary for the required precision. This was not compatible with the NSFG time schedule.

Finally, the housing unit sample, which provided for taking all (or a very large sample) of the NHIS housing units with eligible women and a subsample of the rest, was restricted to three patterns. These involved subsampling housing units without eligible women at rates r one-half, one-third, or one-fourth the rates used for units with eligible women. Preliminary analysis indicated that the optimum for this procedure would be somewhere in this range.

The study, therefore, involved analyzing the characteristics of seven alternatives. The Cycle III model, consisting of sampling and survey procedures used in Cycles II and III, can be considered an eighth alternative because comparisons were made among the seven and also with the Cycle III model. The eight alternatives are as follows:

Design	*Description of design*
1.	Sample of persons, one-time interviewing, 100 PSU's
2.	Sample of persons, one-time interviewing, 200 PSU's
3.	Sample of persons, continuous interviewing, 100 PSU's
4.	Sample of persons, continuous interviewing, 200 PSU's
5.	Sample of housing units, $r = 1/2$, 200 PSU's
6.	Sample of housing units, $r = 1/3$, 200 PSU's
7.	Sample of housing units, $r = 1/4$, 200 PSU's
8.	Cycle III model

Six criteria were used to evaluate the various alternatives:

1. Sample size necessary to achieve fixed and identical levels of precision for all alternatives.

2. Alternative field and interviewing methods available for the sampling procedures.
3. Cost of implementing the designs.
4. Anticipated response rates.
5. Time schedules.
6. Potential administrative or operating problems.

A precision standard was set for four separate population groups. It is the precision that would result if the sample sizes shown in table 1 were used with the Cycle III sample design.

The various sample designs have different design effects and require different sample sizes to achieve the same precision. The design effects arise from a number of features of the sample design—principally, variability in sampling rates among certain segments of the population; subsampling in multieligible households; and the use of multistage sample designs involving between-PSU effects, between-segment effects, and variability in segment size.

Components of variance contributed by the first two features, variability in sampling rates and subsampling in multieligible households, were estimated in two steps. The first step was to develop the proportions of the population that would be sampled at each rate, in some cases using data from Cycle II and in others using U.S. Bureau of the Census sources. The second step was to use these estimates to calculate increases in design effects through use of a formula that is a good approximation of the increase under a broad range of conditions.

The components of design effects arising from multistage sampling were estimated by preparing special computer runs of Cycle II in which separate calculations were made of between-PSU variances, between-segment variances, and within-segment variances. For each sample design (including the Cycle III model), the components of variance were combined to produce an estimate of the total design effect. Sample sizes necessary to produce a fixed level of precision were then calculated.

All of the alternatives considered have sample sizes considerably below those required by the Cycle III model. They require sample sizes in the range of about 10,400 to 11,500, compared with the 14,000 needed for Cycle III procedures (not including an allowance for nonresponse). The savings were larger for the sample of black women than for others, varying from a low of 4,246 to a high of 4,719, compared with 6,200 for Cycle III. For white and other races, the sample sizes were between 6,152 and 6,729, compared with 7,800 for Cycle III. Reduced sample sizes were adequate for the alternatives because the Cycle III design had very different sampling rates between strata 1–3 (the concentrated black areas) and stratum 4; the difference was 5 or 6 to 1. Between-strata sampling rate variability was avoided or reduced in all of the alternatives. Other features of the designs, such as the enlarged number of PSU's and segments, also contributed to a reduction in sample size, but to a lesser extent.

The estimated sample sizes needed to achieve identical reliability among the alternative designs are shown in table 2. More detail, including a breakdown by race and marital status, is contained in the body of the report.

In regard to field procedures, the use of a sample of persons requires quite different operations to identify and locate eligible women than is required by a housing unit sample. For a person sample, eligible women must first be identified by name and address, and then movers must be tracked to their new residences. The number of movers depends on how long it takes to accumulate the NHIS sample before interviewing starts. With one-time interviewing, the percent of movers will vary from a low of about 7 percent for white women with the 200-PSU design to a high of 30 percent for black ever married women with the 100-PSU design. Locating and interviewing these women requires a considerable amount of tracking and travel. With continuous interviewing the problem is sharply reduced because only about 4–5 percent of both black and white women will have moved between the times of the NHIS and NSFG interviews.

There is, of course, no need for tracking with the housing unit approach; however, screening is required because housing units without eligible women are included in the sample. About 20,000–23,000 screenings would be necessary, depending on the subsampling rate for units without eligibles from the NHIS. About half the screened units would be housing units with eligible women in the NHIS; the other half would be used for subsampling of units without eligibles from the NHIS. With this procedure a small supplemental sample of new construction units also would be necessary. Although screening 20,000–23,000 units is a large increase in workload compared with a sample of persons, it still is considerably below the 54,000-unit screening necessary with the Cycle III model.

The nature of the field methods also is involved in comparing one-time interview procedures with continuous interviewing. With one-time interviewing (that is, during approximately a 4-month period), the field operations presumably would be set up about the same as for Cycles II and III. With continuous interviewing, the interview length would be about 15 months for the 200-PSU design and over 2½ years for the 100-PSU design. The workload during any month would be quite small, and the staffing pattern would have to accommodate these workloads. The most practical method seems to be to have traveling interviewers, each one covering three or four PSU's.

The important factors (including sample size, screening workload, cost of tracking, and increased travel either for visiting movers or for covering multiple PSU's) were considered in estimating the cost of each alternative. Although the costs should not be used to establish budgets, the estimates are satisfactory for the purpose of comparing the alternatives.

The sample of persons with one-time interviewing has the

Table 1. Sample sizes applicable with Cycle III sample design by race and marital status

Race and marital status	*Number of eligible women required for sample*
Total sample	14,000
Black	6,200
Ever married	3,600
Never married	2,600
White and other	7,800
Ever married	5,400
Never married	2,400

Table 2. **Principal features of alternative sample and survey procedures**

Design No.	Sample size	Households screened	Direct cost	Approximate response rate	Approximate interview periods
	Number		Dollars	Percent	
1	10,981	10,981	1,700,000	81	Oct. 1987–Feb. 1988
2	10,398	10,398	1,870,000	82	Apr.–July 1986
3	10,981	10,981	2,630,000	83	Apr. 1985–Dec. 1987
4	10,398	10,398	2,610,000	83	Apr. 1985–June 1986
5	10,623	23,113	2,020,000	84	July–Nov. 1986
6	11,023	21,017	2,020,000	84	Aug.–Dec. 1986
7	11,448	20,423	2,040,000	84	Sept. 1986–Jan. 1987
8	14,000	54,000	2,610,000	84	...

lowest cost. Estimates of the direct cost for this procedure are about $1,700,000 for a 100-PSU design and close to $1,900,000 for a 200-PSU sample. The cost for the sample of housing units is fairly close, costing about $2,000,000. Continuous interviewing costs much more, about $2,600,000, approximately the same as the Cycle III model.

Response rates can be approximated only roughly. Some of the reasons for this are a lack of experience with eligible women who have previously been interviewed in NHIS, unknown problems in tracking, the difficulty of following movers, and uncertainty about the public mood in regard to cooperation in surveys. However, approximations can be made of differences in response rate among the alternatives, and these differences are relevant for purposes of comparison.

Assuming that participation in the NHIS does not affect cooperation, it is likely that the housing unit sample procedures would have about the same nonresponse rates as the Cycle III model. This applies if screening is carried out in a personal visit immediately followed by the detailed interview, as was done in Cycles II and III. (If a different plan is followed, for example, telephone screening, the nonresponse rate would increase, probably by 1–3 percent.) The person samples would have somewhat higher nonresponse rates than the housing unit samples because of problems in tracking and in applying normal conversion procedures to movers. Rough estimates of the increases in nonresponse rates over the Cycle III rate are as follows: about 0.5 percent increase for continuous interviewing, 1–1½ percent for one-time interviewing with 200 PSU's, and 2–3 percent for one-time interviewing with 100 PSU's.

The various alternatives have important implications for the Cycle IV time schedule. The redesigned NHIS is not expected to start until January 1985. With the exception of continuous interviewing, fieldwork cannot begin until the entire sample has been accumulated. This time period is needed to create the sample of black eligible women, and it ranges from about 15 months for the 200-PSU person sample to 33 months for the 100-PSU design. Thus, for one-time interviewing, the earliest interviewing starting dates are April 1986 for the 200-PSU design and October 1987 for the 100-PSU design. The housing unit samples (in 200 PSU's) could begin in July–September 1986. The continuous interview alternatives could begin much earlier, probably about April 1985, because they only need part of the sample to get started. It is possible to begin a little later because a modified continuous operation can be envisioned in which interviewing starts later, near the end of 1985 or early 1986, and catches up over the next few months. However, regardless of the starting dates, the earliest survey completion for continuous interviewing is June 1986 for the 200-PSU design and December 1987 for the 100-PSU sample. The earliest ending dates for the one-time interviewing are roughly the latter half of 1986 for the 200-PSU procedures and the end of 1987 or early 1988 for the 100-PSU designs. Because the time period is so long with continuous interviewing, the possibilities of changing patterns of behavior exist and should be taken into account in deciding among alternatives.

Operational and definitional concerns that result from using the NHIS as a sampling frame are discussed in the body of the report. No major problems appear to exist that make any of the alternatives clearly unacceptable. Each of the alternatives has some advantages and disadvantages reflected in the estimated costs, response rates, or time schedules.

Two issues of particular note were identified. The first relates to the geographic coverage of NHIS and NSFG. The NHIS covers all 50 States and the District of Columbia, including Hawaii and Alaska, whereas until now the NSFG has excluded Hawaii and Alaska. This does not pose any serious problem for Cycle IV because the Alaska and Hawaii data can simply be excluded, and the remaining areas will be a probability sample of the 48 States. Some small amount of special weighting may be necessary, but this is the only complication. Alternatively, this may be an appropriate time to reconsider whether it is still advisable to exclude Hawaii and Alaska from the NSFG frame. Although the unit costs in these States would be higher, the total cost would be increased only slightly because only a few PSU's would be selected from these two States. The availability of the NHIS sample data would simplify expanding NSFG to represent the population of all 50 States.

The second issue is the advisability of some pilot studies to address particular aspects of the alternatives for which experience is lacking. Probably the most important subject for study is the effectiveness of different methods of tracking movers. A second subject is to ascertain whether the materials that the U.S. Bureau of the Census plans to make available are adequate to locate the sample persons or housing units and, if not, what kinds of maps, lists, or other materials are needed. Finally, it would be useful to explore the effect of the alternatives on nonresponse rates and to find out which population groups require more intensive work on refusal conversion.

Chapter 2
Description of The National Survey of Family Growth, Cycle III

Background

The National Survey of Family Growth (NSFG) was established in 1971 as an integral part of the National Center for Health Statistics, Division of Vital Statistics. The purpose of the survey is to provide current information on childbearing, contraception, and related aspects of maternal and child health. NSFG is a periodic survey, conducted every few years. Cycle I of the survey was conducted in 1973, Cycle II in 1976, and Cycle III in 1982.

The target population of Cycles I and II was the civilian household population of women 15–44 years of age in the conterminous United States who were currently married, previously married, or never married mothers with offspring living in the household at the time of interview.

The target population for the Cycle III survey was expanded to include women of all marital statuses and women living in group quarters. Thus the Cycle III survey represents the civilian noninstitutionalized population of women 15–44 years of age. Data for Cycles I–III were collected from probability samples by means of personal interviews lasting an average of 1 hour. The interviews provided information on fertility trends and differentials, contraception, breast feeding, family planning services, and aspects of maternal and child health closely related to family planning.

The sample design and data collection for Cycle I were contracted to the National Opinion Research Corp. of the University of Chicago. The sample design and data collection for Cycles II and III were contracted to Westat, Inc., of Rockville, Md. Descriptions of the Cycle I and II surveys can be found in other reports.[1,2] Cycle III is based on interviews with 7,969 women conducted between August 1982 and February 1983 and centered on October 1982. This report describes the sample design used to select the women.

Design specifications summary

Efficient sample design must take into account the primary survey objectives, the amount of funds available, logistic problems, time limitations, estimates of population characteristics and distribution, and operating costs. These requirements dictated a stratified multistage probability sample design for Cycle III, based on the following set of specifications:

1. The target population was defined as the noninstitutionalized population of women 15–44 years of age living in households or group quarters in the conterminous United States.
2. The sample would consist of approximately 7,600 women selected from an initial probability sample of households. It would include about 3,100 black women and 4,500 women of other races; by age, the sample would include about 2,000 women 15–19 years of age and 5,600 women 20–44 years of age. Trained field staff were to conduct a screening interview with a responsible member of each sample household to determine if there were any eligible women. No more than one eligible woman per household was to be interviewed.
3. Data were to be collected from the sample women by means of personal interviews lasting an average of 1 hour. No proxy respondents were to be accepted.
4. All interviewers would be female.
5. The interviewer would collect information on fertility, contraceptive use, sources and types of family planning services, and related aspects of maternal and child health using a preprinted questionnaire.
6. The fieldwork would be completed in approximately 4½ months.
7. The target interview completion rate for the total sample and both major subsamples by race was 90 percent of the expected number of women from all sample households (that is, screener and interview nonresponse combined ideally should be no more than 10 percent).
8. The contractor, in cooperation with the National Center for Health Statistics, would design and implement procedures to measure and control the quality of data collection and data preparation.

Sample design

The sample design for Cycle III of NSFG was a five-stage area probability design that incorporated oversamples of black and teenage women and a supplementary sample of women living in college dormitories and sororities.

The counties and independent cities that constitute the total land area of the conterminous United States were combined to form a frame of primary sampling units (PSU's). During the first stage of the sampling process, which involved extensive stratification, 79 PSU's were chosen from this frame. Census block groups (BG's) and enumeration districts (ED's) were then identified for each of the selected primary sampling units; during the second stage, these BG's and ED's were stratified into two groups according to the percent of their population that was black, and a systematic sample was drawn from each. The rate at which BG's and ED's were sampled varied from one second stage stratum to the next. These differential sampling rates were the first step in producing the desired racial composition of the final sample of women.

In the third stage, area segments (groups of houses or apartments) within sample BG's and ED's were identified and one segment was selected randomly from each district.

The fourth stage was to select households within sample segments. In segments from the "black" stratum (that is, enumeration districts with a 10-percent or greater black population), black households were selected at a higher rate than other households. These different rates of selection were obtained through a subsampling process (to be described later in this report) so that the desired proportions of black and other women would be included in the final sample.

In the fifth stage of sampling, women were selected from all eligible households. At each sample household an interviewer attempted to complete a Household Screener and identify women eligible for interview. In households containing at least one eligible woman, women were selected at rates dependent on the number of eligible women in the household, stratum, age, and marital status; no more than one woman was selected from any household.

Women living in households were selected in the manner described, but women living in college dormitories or sororities were selected from a sample of colleges with undergraduate female enrollment located within the 79 PSU's selected in the first stage of the sampling process. Within the selected colleges, women were systematically sampled from a list of all women living in college dormitories or sororities. A detailed description of the sample design can be found elsewhere.[3]

Chapter 3
Requirements of The National Survey of Family Growth, Cycle IV

It was assumed that Cycle IV was to consist of 14,000 interviewed women, it was assumed that the distribution of the sample by race and marital status would be the same as the original requirements for NSFG Cycle III, before the sample reduction and the oversampling of teenage women. This implies the sample distribution shown in table 1 in chapter 1. The requirements for sample size subsequently were modified so that for any sampling procedure being considered, the sample size should produce the same sampling variances as would be achieved with the current design's use of the sample sizes in table 1. Part of the present research is to ascertain these sample sizes.

Although most of the research is restricted to sample designs that have sampling errors consistent with table 1 sample sizes for the Cycle III sample design, it is prudent also to explore at least one alternative sample size. For this alternative, a sample size of 10,000 interviewed women with the same distribution by race and marital status was chosen. First, it will give some indication of the sensitivity of the sample design to the sample size requirements. Second, it will provide the design to be used in the event that there is a reduction in the funds available for the survey. At this level the sample distribution is as shown in table 3. The implications of using this sample size are discussed in chapter 6. In addition to consideration of feasible sampling strategies, the cost and operational features of various interviewing procedures have also been examined and are described in chapter 7.

Table 3. Sample distribution for a reduced sample of 10,000 women by race and marital status

Race and marital status	*Number of eligible women required for sample*
Total sample	10,000
Black	4,430
Ever married	2,570
Never married	1,860
White and other	5,570
Ever married	3,855
Never married	1,715

Chapter 4
Design effects in The National Survey of Family Growth with Cycle III design

Factors creating design effects

With the Cycle III design, the following features of the sample increase the variances above those expected in a comparable simple random sample:

1. Different sampling rates between strata 1–3 (predominantly black areas) and stratum 4 (low percent black areas).
2. Subsampling in multieligible households.
3. Between-PSU variances.
4. Between-segment variances.
5. Variability in segment size.

There are two other aspects of the current design that affect the variances. These two factors will, however, be applied in the new design in about the same way. The factors are subsampling of nonrespondents, which increases the variances, and ratio estimation, which tends to decrease them. Because these two design features will not affect decisions on alternative designs, they will not be considered in more detail in this report.

Estimation of parameters involved in design effects

The five features of the sample design listed above can be classified into two groups. The first group, consisting of items (1) and (2), involves the effects of portions of the eligible population with different probabilities of selection. The design effects attributable to these sources can be estimated with a fair degree of reliability because reasonably good information is available on the proportions of the population that will be sampled at the rates to be specified, either from Cycle II or from various U.S. Bureau of the Census sources. Estimates of the part of the design effects attributable to these factors are contained in the next two sections of this chapter.

The second group covers items (3)–(5). Estimates of these components of the total variance were not available previously (although the total variances for selected items were estimated by the National Center for Health Statistics (NCHS)). As part of this research, estimates of these components of variance for a select group of items have been prepared. The appendix to this report contains a description of the computational procedure and the results. These data have been used in development of estimates of the relevant parameters.

Strata 1–3 and 4 sampling rates

One of the main factors responsible for the large number of black women in stratum 4 in Cycle II was the fact that Cycle II was conducted more than 6 years after the 1970 census, and residential patterns had shifted during those 6 years. Cycle IV presumably will be conducted about 6 or 7 years after the 1980 census so that the movements of the population among strata after the census should be roughly similar to the situation that occurred in Cycle II. It has been assumed that the proportions of black women and others in the different strata in Cycle IV are the same as in Cycle II.

In Cycle II, the sample of white and other women in strata 1–3 was supplemented when it became clear that the total sample size was going to be lower than the target number. Such supplementation is not anticipated for Cycle IV. If Cycle IV were to be carried out in a manner similar to Cycles II and III, the sampling rates in strata 1–3 and 4 would be calculated to provide a reasonably efficient design. Because about 25 percent of black women will reside in stratum 4 (assuming that the Cycle II experience is repeated), the sampling rates probably should not differ by more than a factor of about 4, and this factor will be used in subsequent calculations. For white and other women, a higher rate in strata 1–3, but a much smaller disparity, probably will also be introduced. It is useful to calculate the design effects for both the Cycle II and IV sample designs (table 4). The effect of this disparity in weights is to create the design effects shown in table 5. (Under fairly general conditions, the relative increase in variance is equal to $(\sum P_i k_i)(\sum P_i/k_i) - 1$ where the values of P_i denote the proportions of the population

Table 4. Sampling rates by race, stratum, cycle, and marital status, and percent eligible women by race and stratum

Race and strata	*Sampling rate[1]*		*Percent of eligible women in strata[2]*
	Cycle IV	*Cycle II*	
White and other			
Strata 1–3	2r	3.64r	8
Stratum 4	r	r	92
Black			
Strata 1–3:			
Ever married	4r	5.60r	72
Never married	5r		
Stratum 4	r	r	28

[1]r = base sampling rate.
[2]National Center for Health Statistics, W. R. Grady: National Survey of Family Growth, Cycle II: Sample design, estimation procedures, and variance estimation. *Vital and Health Statistics*. Series 2, No. 87. DHHS Pub. No. (PHS) 81–1361. Public Health Service Washington. U.S. Government Printing Office. Feb. 1981.

Table 5. Design effects from sampling strata at different rates by race, marital status, and cycle

Race and marital status	Cycle IV	Cycle II
Black		
Ever married	1.44	1.76
Never married	1.65	1.76
White and other		
Ever married	1.04	1.14
Never married	1.04	1.14

sampled at different rates and the values of k_i are the ratios of the sampling rates to the rate used for the first group.[4])

Effect of subsampling in multieligible households

The proportion of women in single and multieligible households is estimated in table 6. It is assumed that the same plan will be followed in subsampling within households as the one currently employed in Cycle III; that is, never married women will have twice the probability of selection as ever married women, in households containing both. Consequently, subsampling within households will create sampling rates that vary as shown in table 7.

Using data in tables 6 and 7, the following design effects (DEFF's) for Cycle IV arise from subsampling within households:

Race and marital status	DEFF
Black:	
Ever married	1.36
Never married	1.18
White and other:	
Ever married	1.25
Never married	1.17

Table 6. Distribution of eligible women by number of eligible women in household, race, and marital status

Number of eligible women in household	Black: Ever married	Black: Never married	White and other: Ever married	White and other: Never married
	Percent			
1 woman	75.1	47.5	82.6	39.9
2 women:				
2 ever married	1.9	...	1.3	...
1 ever and 1 never married	15.6	16.6	12.3	24.1
2 never married	...	17.3	...	16.9
3 women:				
2 ever and 1 never married	1.0	0.5	0.3	0.2
1 ever and 2 never married	5.1	10.9	2.9	11.4
3 never married	...	2.5	...	3.5
4 women:[1]				
2 ever and 2 never married	---	---	0.1	0.2
1 ever and 3 never married	1.3	4.0	0.5	2.7
4 never married	...	0.7	...	1.1

[1]5 or more eligible-person households have been included with 4-person groups.

Table 7. Sampling rates by number of eligible women in household, race, and marital status

Number of eligible women in household	Black: Ever married	Black: Never married	White and other: Ever married	White and other: Never married
1 woman	r	r	r	r
2 women:				
2 ever married	0.50r	...	0.50r	...
1 ever and 1 never married	0.33r	0.67r	0.33r	0.67r
2 never married	...	0.50r	...	0.50r
3 women:				
2 ever and 1 never married	0.25r	0.50r	0.25r	0.50r
1 ever and 2 never married	0.20r	0.40r	0.20r	0.40r
3 never married	...	0.33r	...	0.33r
4 women:				
2 ever and 2 never married	---	---	0.17r	0.33r
1 ever and 3 never married	0.14r	0.28r	0.14r	0.28r
4 never married	...	0.25r	...	0.25r

NOTE: The sampling rates will not be the same for the 4 race-marital status groups, but because all rates are expressed relative to the 1 on the first line, the symbol r is used for all.

In Cycle II, the design effect from within-household subsampling was much smaller because the only single women eligible for the survey were those with their own children in the household at the time of screening. The design effects for Cycle II were as follows:

Race	DEFF
Black	1.07
White and other	1.01

In preparing the components of variance, the design effect for the within-segment component of variance was calculated. This design effect essentially should reflect the effects of variable sampling rates because without such variability, within-segment variances usually are very close to simple random sampling. It is useful to compare the sum of the two components for Cycle II (the values given for "Black" and "White and other" in the preceding paragraph and the values given for "Black" and "White and other" in table 5) with the within-segment design effects shown in the appendix. It is appropriate to use design effects for "never married women forced to a fixed total" because design effects imply a multiple of simple random sampling, which assumes the population is fixed and the variance is zero for $p = 100$ percent. The comparisons are given in table 8. The assumptions on which the formula $(\sum P_i k_i)(\sum P_i/k_i)$ is based obviously apply in this case.

Table 8. Comparison of Cycle II design effects calculated in this chapter with those of appendix by race

Source of data	Black	White and other
Sum of design effects in chapter 4	1.83	1.15
Within-segment design effects from appendix	1.825	1.151

Components of variance resulting from multistage sampling

Consistency with NCHS calculations

Westat prepared estimates of the various components of variance in Cycle II for use in estimating the intraclass correlations and other effects of multistage sampling. Before estimating these parameters, it is useful to compare the total design effects of the calculations with those previously prepared by NCHS. Although NCHS reports did not explicitly report design effects, it is easy to derive them from the data reported.

Design effects estimated by NCHS—In *Vital and Health Statistics,* Series 2, No. 87,[2] page 23, NCHS indicates that an estimator for the relvariance under simple random sampling of a proportion P' for Cycle II is

$$V_{P'}^2 = \frac{BQ'}{P'Z'}$$

where Z' is the total population to which P' applies, $Q' = 1 - P'$, and B takes the following values:

Race and marital status	B
Black women ever married	2,848.2
White and other women ever married	7,111.5

For the simple random sample, the relevant formula is, of course,

$$V^2 = \frac{Q'}{P'n}$$

where n is the sample size. The design effect DEFF is thus

$$\text{DEFF} = \frac{Bn}{Z'}$$

Values of Z', n, and DEFF from NSFG Cycle II are given in table 9.

Design effects from Westat computations—The design effects shown in table 9 are applicable to percent distributions, not to estimates of totals. The comparable Westat computations are the ones in which the population totals were held fixed because when this occurs, there is zero variance on estimates of 100 percent of the population and the relvariances of estimates of totals and of associated percents are identical. For a number of reasons, given in the appendix, the variances shown are not of the right dimensions and should not be used in comparisons. However, the design effects and the proportions of the variance attributable to the various stages of sampling are not affected by the dimension and can be used. The design effects shown in the tables of the appendix reflect only the effect of within-segment sampling. The total design effect can be obtained by dividing the within-segment design effect by the proportion of the variance arising from sampling households. The computations and comparisons with NCHS calculations are shown in table 10. The estimates for white and other races are a little further apart than expected. However, because of the rather small number of degrees of freedom used in both sets of computations, the difference probably is within sampling error. The Westat computations, therefore, will be used to estimate intraclass correlations.

Table 9. NCHS computations of Cycle II values for Z', n, and DEFF by race

Race	Z'	n	*DEFF*
Black	4,095,000	3,022	2.10
White and other	25,647,000	5,589	1.55

SOURCE: D. N. Krug, R. F. Slobasky, S. K. Hendricks, and J. Waksberg: *National Survey of Family Growth Cycle II Final Report.* Contract No. HRA–106–74–153. May 1977

Between-PSU and between-segment variances

Some manipulation of the appendix data is necessary to produce the PSU and segment intraclass correlations. The equation

$$\sigma_{x'}^2 = \sigma^2[1 + a^2 + b^2 + \bar{n}\rho_1 + (\bar{\bar{n}} - 1)\rho_2 + V_n^2]$$

is a reasonable approximation of the total variance. ρ_1 and ρ_2 are the PSU and segment intraclass correlation coefficients, respectively, and $\bar{n}$ and $\bar{\bar{n}}$ are the average number of sample persons per sample PSU and sample segment, respectively. However, this formula assumes that all PSU's contribute to the between-PSU component of variance. Where there are self-representing PSU's, the term $\bar{n}\rho_1$ should be replaced by $P\bar{n}\rho_1$, where P is the proportion of the relevant population in non-self-representing PSU's. In the expression for $\sigma_{x'}^2$, $1 + a^2 + b^2 =$ component from simple random sampling plus that from variability in the sampling rate; $P\bar{n}\rho_1 =$ effect of PSU sampling; and $(\bar{\bar{n}} - 1)\rho_2 + V_n^2 =$ effect of selecting segments; V_n^2 is the effect of variability in segment size, and $(\bar{\bar{n}} - 1)\rho_2$ is the effect of intraclass correlation within segments.

The values shown in table 11 are developed from the appendix. The values of P, $\bar{n}$, and $\bar{\bar{n}}$ come from the Cycle II sample design. They are shown in table 12 with the estimates of the desired parameters. It has been estimated (somewhat arbitrarily) that $V_n^2 = 0.0500$.

Estimates of design effects for the current 79-PSU design

Assuming

$$\text{DEFF} = 1 + P\bar{n}\rho_1 + (\bar{\bar{n}} - 1)\rho_2 + a^2 + b^2$$

Table 10. Comparison of Westat and NCHS computations of DEFF by race

Source of data	*Black*	*White and other*
Westat computations:		
Within-segment DEFF	1.825	1.151
Within-segment component of variance as proportion of total	0.890	0.653
Estimated total DEFF	2.05	1.76
NCHS computations: estimated total DEFF	2.10	1.55

Table 11. Values of terms used in calculation of total variance by race

Term	Black	White and other
$1 + a^2 + b^2$	1.8250	1.1511
$1 + a^2 + b^2$/total variance	0.8900	0.6535
$P\bar{n}\rho_1$/total variance	0.0075	0.1986
$(\bar{\bar{n}} - 1)\rho_1 + V_{\bar{n}}^2$/total variance	0.1025	0.1480
$P\bar{n}\rho_1$	0.0154	0.3498
$(\bar{\bar{n}} - 1) = V_{\bar{n}}^2$	0.2102	0.2607

Table 12. Estimates of Cycle II design effects from multistage sampling by race

Term	Black	White and other
$\bar{n}$	38.00	71.00
P	0.61	0.75
$\bar{\bar{n}}$	4.81	5.6
$P\bar{n}\rho_1$	0.0154	0.3498
$(\bar{\bar{n}} - 1)\rho_2 + V_{\bar{n}}^2$	0.2102	0.2607
ρ_1	0.0007	0.0066
$V_{\bar{n}}^2$	0.0500	0.0500
ρ_2	0.0422	0.0458

the effects for Cycle IV sample sizes as shown in table 13 can be calculated. In calculating the values of $\bar{n}$, the part of the sample expected in non-self-representing PSU's (P times the sample size) is divided by 54, the number of non-self-representing PSU's. To estimate $\bar{\bar{n}}$, the segment sizes are assumed to be the same as in Cycle III; that is, 24 households to be screened in strata 1 and 4, and 48 and 96 households in strata 2 and 3, respectively. This leads to the values of $\bar{\bar{n}}$ shown in table 13.

It will be useful for later consideration to calculate the design effects that would result if Cycle IV used a sample size of 10,000, as occurred in Cycles II and III. It is assumed that essentially the same design would be used, except that the number of segments would be reduced. This would leave $\bar{\bar{n}}$ unchanged, but reduce the values of $\bar{n}$ so that they were only 10/14 of those shown in table 13. All other parameters will be the same. The resulting design effects have been included in table 13.

Table 13. Components of design effects and design effects for Cycle IV, with Cycle III sample design, by size of sample, race, and marital status

Term	Black: Ever married	Black: Never married	White and other: Ever married	White and other: Never married
14,000 sample		Components of DEFF		
P	0.61	0.61	0.75	0.75
$\bar{n}$	41	29	75	33
$\bar{\bar{n}}$	4.3	3.1	5.2	2.3
ρ_1	0.0007	0.0007	0.0066	0.0066
ρ_2	0.0422	0.0422	0.0458	0.0458
$P\bar{n}\rho_1$	0.02	0.01	0.37	0.16
$(\bar{\bar{n}} - 1)\rho_2$	0.14	0.09	0.19	0.06
$V_{\bar{n}}^2$	0.05	0.05	0.05	0.05
a^2	0.36	0.18	0.25	0.17
b^2	0.44	0.65	0.04	0.04
DEFF	2.01	1.98	1.90	1.48
Sample size	3,600	2,600	5,400	2,400
Sample size with simple random sample	1,791	1,333	2,842	1,622
10,000 sample				
DEFF	2.00	1.98	1.79	1.43
Sample size	2,570	1,855	3,860	1,715
Sample size with simple random sample	1,285	937	2,156	1,199

Chapter 5
Design effects in alternative designs

Alternative designs considered

Six alternatives will be considered. Two will consist of whether to use all 200 PSU's versus using 100 PSU's. Two will vary in whether the sample should consist of eligible women in the NHIS (with movers being tracked) or whether the sample will consist of sample addresses. In the latter case, both addresses with and without eligibles will be included although the former will be sampled at a much higher rate. (New construction will be treated as part of the noneligible addresses.) Four of the alternatives will be the four combinations of the two types of plans. These four assume that data collection is performed in a moderately short time period, after the sample has been accumulated. The other two will consider the desirability of data collection over a more lengthy period, in effect, following closely behind the NHIS interviews. This method will be restricted to a sample of persons. When a sample of persons is used, there will be very little difference in design effects and sample sizes between the alternatives of continuous interviewing and carrying out the data collection in a limited period of time. Issues relating to the choice between these two will, therefore, be discussed in the section of this report on cost and operational features and not in the sample size sections.

In developing the properties of these designs, at least two other parameters will be investigated. One is the period of time to be used in the NHIS sampling frame. However, this will be an output of the calculations. Once the desired sample sizes are known, the minimum length of time for NHIS to supply this size can be determined. This is the time period that should be chosen. The other parameter is the subsampling rate to use in noneligible addresses, if that alternative is considered. This, in turn, will affect the NHIS period required. For the time being, the subsampling rate will be treated as a variable, to be determined later.

Population projections for 1987

Table 14 has current data on marital status, by race, for women 15–44 and projections to 1987. The current figures are those for March 1981[5] (from P–20, No. 372). March 1981 is the most recent U.S. Bureau of the Census report on marital status. The projections for 1987 have been derived by using U.S. Bureau of the Census projections for race and age[6] (P–25, No. 704) and assuming that the current proportions of never married women, by age, apply to 1987.

Table 15 summarizes the results, discounts the proportion that will be lost because of the restriction that only one person per household can be interviewed, and contains estimates of the number of households that need to be screened to locate one eligible woman.

Table 6 has been used to obtain the proportion of women retained after loss due to multieligible households. The total number of households in 1987 comes from the Series B projections in P–25, No. 805.[7] Black households were estimated as 12 percent of the total, a little higher than the 10.7 percent of the total reported by the U.S. Bureau of the Census in 1981[8] (see P–20, No. 371).

Table 14. Number of women 15–44 years, percent never married, and number never married, by age and race, 1981 and 1987 projections

	Black					*White and other*				
	1981			*1987*		*1981*			*1987*	
Age	*Women*	*Never married*		*Women*	*Never married*	*Women*	*Never married*		*Women*	*Never maried*
	Number in thousands	Percent	Number in thousands				Percent	Number in thousands		
Total	6,771	48.0	3,250	7,405	3,248	46,557	33.1	15,415	48,625	14,114
15–19 years	1,460	96.4	1,407	1,347	1,299	8,599	91.3	7,849	7,425	6,779
20–24 years	1,445	68.8	994	1,387	954	9,240	49.2	4,549	8,218	4,043
25–29 years	1,273	35.7	454	1,390	496	8,705	19.8	1,725	9,020	1,786
30–34 years	1,075	20.7	222	1,313	272	8,150	9.1	738	8,820	803
35–39 years	819	13.8	113	1,108	153	6,478	5.2	339	8,097	421
40–44 years	699	8.6	60	860	74	5,385	4.0	215	7,045	282

Table 15. Eligible women, number of households, and households per eligible woman, by race and marital status, percent eligible women retained after loss in multieligible households, 1987 projections

Characteristic	Black: Ever married	Black: Never married	White and other: Ever married	White and other: Never married
	Number in thousands			
Eligible women	4,157	3,248	34,511	14,114
Eligible women retained after loss in multieligible households	3,442	2,397	30,439	10,063
Households	11,021		80,820	
Households per eligible woman retained[1]	3.20	4.60	2.66	8.03
	Percent			
Eligible women retained after loss in multieligible households	82.8	73.8	88.2	71.3

[1] Data do not include any allowance for nonresponse or vacant housing units. Also, this screening ratio assumes the same subsampling within households as in Cycle III. For some alternatives, it may be possible to reduce the amount of subsampling.

Between-PSU variances

To estimate the between-PSU effect, it is necessary to estimate $\bar{n}$ and P for a 100- and a 200-PSU design. Some time ago Westat developed a 100-PSU design, and it is assumed that the U.S. Bureau of the Census 100-PSU sample will be generally comparable. This design was used to estimate P and the number of non-self-representing PSU's. Results also were extrapolated to a 200-PSU design.

The values of the required parameters are in table 16. Strictly speaking, these values of $P\bar{n}\rho_1$ apply when a sample size of 14,000 is used. Because some sample designs require smaller samples, from about 10,000 to 12,000 sample persons, the parameters will vary. However, because the difference is small, and to simplify the computations, the figures in table 16 will be used for all alternatives.

Between-segment variances

For the Cycle III design, the segment sizes were 24 housing units for strata 1 and 4, 48 for stratum 2, and 96 for stratum 3. However, the subsampling of white and other races was at such a rate as to produce an effective segment size of 24 in strata 2 and 3, as well as in strata 1 and 4.

The NHIS segment sizes will be eight housing units if 1-year NHIS is used, 16 housing units if 2 years are used, and so forth. The values of $(\bar{\bar{n}} - 1)\rho_2$ are given in table 17.

Subsampling within households

In general, the same within-household subsampling procedure will be applied as in the Cycle III design, and the values of b^2 as shown in table 13 will apply to the various alternatives. For some alternatives it may be possible to eliminate the within-household subsampling for white and other races. In that case, this component of variance will be eliminated.

Differential sampling by strata

With the use of NHIS as a sampling frame, there will not be any need to create the four strata used in the current design, and the values of a^2 used in table 13 will not apply. The sample designs being considered that involve following up persons who move will not have any differential sampling rates. However, the alternative designs, which include all housing units that have eligible women and a subsampling of the rest, will have strata with different rates. For these alternatives, the additional

Table 16. Parameters for estimation of between-PSU effect by number of PSU's, race, and marital status

Term	Black: Ever married	Black: Never married	White and other: Ever married	White and other: Never married
200 PSU's				
P	0.45	0.45	0.57	0.57
Number of non-self-representing PSU's	110	110	110	110
$\bar{n}$	15	11	28	12
ρ_1	0.0007	0.0007	0.0066	0.0066
$P\bar{n}\rho_1$	0.01	0.01	0.11	0.05
100 PSU's				
P	0.50	0.50	0.62	0.62
Number of non-self-representing PSU's	66	66	66	66
$\bar{n}$	27	20	51	23
ρ_1	0.0007	0.0007	0.0066	0.0066
$P\bar{n}\rho_1$	0.01	0.01	0.21	0.09

Table 17. Values of $(\bar{\bar{n}}-1)\rho_2$ by number of years of NHIS, race, and marital status

Race and marital status	$(\bar{\bar{n}}-1)\rho_2$ 1-year NHIS	2-year NHIS	3-year NHIS
Black			
Ever married	0.02	0.08	0.14
Never married	0.00	0.04	0.09
White and other			
Ever married	0.03	0.11	0.19
Never married	0.00	0.03	0.06

component of variance will depend on the subsampling rate and the number of years of NHIS.

Table 18 shows the mobility rates for selected segments of the population for time periods ranging from 1–5 years. An analysis of this table indicates that there is very little difference between black and white mobility. A common subsampling rate for previously ineligible households therefore will be used. This subsampling rate also will apply to vacant units and new construction. The increase in variance caused by subsampling is approximately

$$1+a^2=(p+qr)\left(p+\frac{q}{r}\right)$$

where r = subsampling rate,

p = proportion of population at addresses sampled and at rate used for households with eligibles in NHIS interview, and

$q=1-p$ = proportion of population at addresses subsampled.

With some algebraic manipulation, $1+a^2$ can be expressed as

$$1+a^2=1+pq\left(r-\frac{1}{r}\right)^2.$$

Values of p can be derived from data in table 18, modified by information in table 15. Table 18 can be used to estimate the proportion of housing units containing eligible women in which the eligible women will still reside after 1 year, 2 years, and so forth. The new residents at the sample addresses from which eligible women moved mostly will be ineligible, but in some cases new eligibles will have moved in and will be included in the sample at the higher sampling rate. This proportion can be estimated in the following way:

Number of mover households

$$=Q_1s_1N+Q_2s_2N+Q_3(1-s_1-s_2)N$$

where $Q_i=1-P_i$;

P_i = proportion of eligibles (retained) who do not move;

P_3 = proportion of noneligible households that do not move;

s_i = proportion of households with eligibles (retained), $i=1$, ever married, $i=2$, never married;

$s_3=1-s_1-s_2$; and

N = total households (black or white).

The total number of housing units into which a mover can move (H_m) is expressed as follows:

$$H_m=0.10N+Q_1s_1N+Q_2s_2N+Q_3(1-s_1-s_2)N$$

where $0.10N$ is the number of vacant units plus new construction units. The probability H_{e_i} that a unit containing a new resident contains an eligible of type i is as follows:

$$H_{e_i}=\frac{Q_is_i}{0.10+Q_1s_1+Q_2s_2+Q_3(1-s_1-s_2)}$$

where $i=1$ or 2. The number of eligibles in stratum taken with certainty E_c is as follows:

$$E_c=P_is_iN+\frac{Q_is_i\left(\sum_i^2 Q_is_i\right)N}{0.10+Q_1s_1+Q_2s_2+Q_3(1-s_1-s_2)}\ .$$

The number of eligibles in subsampled stratum E_s is

$$E_s=\frac{(Q_is_i)[Q_3(1-s_1-s_2)(N)+0.10N]}{0.10+Q_1s_1+Q_2s_2+Q_3(1-s_1-s_2)}\ .$$

The total number of eligibles $E=s_iN$; therefore, the proportion of eligibles in stratum taken with certainty

$$e_i=P_i+\frac{Q_i\left(\sum_i^2 Q_is_i\right)}{Q_1s_1+Q_2s_2+Q_3(1-s_1-s_2)+0.10}\ .$$

The values of s_i are the reciprocals of the number of households per eligible woman retained, shown in table 15 (for example, 1 per 3.20 for black ever married). The values of Q_i are the mover rates. Estimates of mover rates have been made by dividing the mobility rates shown in table 18 in half and rounding up. This was done because when a 1-year NHIS sample is used, the average length of time between NHIS and NSFG data collection will be half a year. The upward rounding is to account for the fact that there will be approximately an additional 2-month lag needed to select the sample and organize the material. The same value of Q_i is used for black and white households, but different values of s_i are used. (This estimation formula assumes that black households that move are replaced by other black households and, similarly, white and other households are replaced by other households of the same race. This is, of course, an oversimplification of what happens in practice, but it should provide a reasonably good estimate of the resulting proportion of households that will contain an eligible person.)

Using this method of estimation leads to the values of e_i shown in table 19. The value of r, the subsampling rate, is

Table 18. Mobility rates for selected population groups, 1975–80

Population group[1]	1 year	2 years	3 years	4 years	5 years
BOTH SEXES			Percent		
Total movers	17.7	27.4	35.5	41.7	47.0
Local movers	11.6	17.5	22.1	25.3	27.9
Long distance movers	6.1	9.9	13.4	16.4	19.1
White:					
Total movers	17.4	27.2	35.1	41.3	46.6
Local movers	11.1	17.0	21.4	24.3	27.1
Long distance movers	6.3	10.2	13.7	17.0	19.5
Black:					
Total movers	18.3	27.3	36.2	41.9	47.2
Local movers	14.8	21.4	27.6	31.4	34.9
Long distance movers	3.5	5.9	8.6	10.5	12.3
Persons 14–44 years[2]					
Total movers	23.7	36.0	45.4	52.6	59.2
Local movers	15.5	20.2	28.7	32.4	35.9
Long distance movers	8.2	15.8	16.7	20.2	23.3
Persons below poverty level[2]					
Total movers	26.7	37.1	45.2	50.0	55.0
Local movers	18.0	24.5	28.8	31.9	34.7
Long distance movers	8.7	12.6	16.4	18.1	20.3
Age 14–44:					
Total movers	35.6	48.0	56.3	61.3	68.2
Local movers	23.2	30.4	34.8	37.8	41.5
Long distance movers	12.4	17.6	21.5	23.5	26.7
FEMALES					
16–44 years[2]					
All females:					
Total movers	24.6	37.2	46.8	54.4	59.9
Local movers	16.3	24.1	29.9	34.1	36.9
Long distance movers	8.3	13.1	16.9	20.3	23.0
Never married:					
Total movers	20.9	27.5	37.6	43.9	47.7
Local movers	13.6	19.9	24.4	27.7	29.0
Long distance movers	7.3	7.6	13.2	16.2	18.7
Ever married:					
Total movers	26.2	41.3	51.1	59.8	65.7
Local movers	17.5	25.9	32.5	37.1	40.6
Long distance movers	8.7	15.4	18.6	22.7	25.1
15–19 years					
Total movers	19.1	24.8	31.5	36.0	41.0
Local movers	12.5	16.6	20.5	22.6	26.1
Long distance movers	6.6	8.2	11.0	13.4	14.9
HOUSEHOLDS					
Total	18.3	29.6	36.9	42.8	47.9

[1]Local movers are persons moving within the same standard metropolitan statistical area (SMSA) or within the same county if outside an SMSA.

[2]Distribution between local and long distance is partially estimated. Reports of the U.S. Bureau of the Census do not show data on the proportion of outside-SMSA movers who remain in the same county for these subgroups. We have estimated this by assuming that the percent of local movers for total population applies to each subgroup

NOTE: Denominators of percents are population totals at the period after the move and thus may differ a little from rates that would be calculated using denominators at the beginning of the period.

SOURCES: U.S. Bureau of the Census: Geographical mobility: March 1975 to March 1976. *Current Population Reports.* Series P–20, No. 305. Washington, D.C. U.S. Government Printing Office, 1977.

U.S. Bureau of the Census: Geographical mobility: March 1975 to March 1977. *Current Population Reports.* Series P–20, No. 320. Washington, D.C. U.S. Government Printing Office, 1978.

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Table 19. Design effect components arising from subsampling housing units without eligible women in NHIS by race and marital status

Term and NHIS time period	Black: Ever married	Black: Never married	Black: Ineligible (= Q_3)	White and other: Ever married	White and other: Never married	White and other: Ineligible (= Q_3)	Total households
s_i	1/3.20	1/4.60	1/2.13	1/2.66	1/8.03	1/1.69	...
Q_i							
1-year NHIS	0.15	0.12	0.07	0.15	0.12	0.08	0.10
2-year NHIS	0.22	0.15	0.13	0.22	0.15	0.13	0.16
3-year NHIS	0.26	0.20	0.14	0.26	0.20	0.15	0.19
e_i							
1-year NHIS	0.95	0.96	...	0.93	0.95	...	...
2-year NHIS	0.92	0.94	...	0.89	0.93	...	...
3-year NHIS	0.91	0.93	...	0.88	0.91	...	...
a^2							
$r = 1/2$:							
1-year NHIS	0.02	0.02	...	0.03	0.02	...	...
2-year NHIS	0.04	0.03	...	0.05	0.03	...	...
3-year NHIS	0.04	0.03	...	0.05	0.04	...	...
$r = 1/3$:							
1-year NHIS	0.06	0.05	...	0.09	0.06	...	...
2-year NHIS	0.10	0.08	...	0.13	0.09	...	...
3-year NHIS	0.11	0.09	...	0.14	0.11	...	...
$r = 1/4$:							
1-year NHIS	0.11	0.09	...	0.15	0.11	...	...
2-year NHIS	0.17	0.13	...	0.22	0.15	...	...
3-year NHIS	0.18	0.15	...	0.24	0.18	...	...

NOTE: $a^2 = e_i(i - e_i)\left(\sqrt{r} - \frac{1}{\sqrt{r}}\right)^2$

Table 20. Total design effects with alternative sample designs by race and marital status

Sample design	Black: Ever married	Black: Never married	White and other: Ever married	White and other: Never married
Current design	2.01	1.98	1.90	1.48
Sample of persons with tracking				
200 PSU's:				
1-year NHIS	1.44	1.24	1.44	1.27
2-year NHIS	1.50	1.28	1.52	1.30
3-year NHIS	1.56	1.33	1.60	1.33
100 PSU's:				
1-year NHIS	1.44	1.24	1.54	1.31
2-year NHIS	1.50	1.28	1.62	1.34
3-year NHIS	1.56	1.33	1.70	1.37
Sample of housing units, with subsampling				
200 PSU's:				
$r = 1/2$:				
1-year NHIS	1.46	1.26	1.47	1.29
2-year NHIS	1.54	1.31	1.57	1.33
3-year NHIS	1.60	1.36	1.65	1.37
$r = 1/3$:				
1-year NHIS	1.50	1.25	1.53	1.33
2-year NHIS	1.60	1.36	1.65	1.39
3-year NHIS	1.69	1.29	1.74	1.44
$r = 1/4$:				
1-year NHIS	1.55	1.33	1.58	1.38
2-year NHIS	1.67	1.41	1.74	1.45
3-year NHIS	1.74	1.48	1.84	1.51
100 PSU's:				
$r = 1/2$:				
1-year NHIS	1.46	1.26	1.57	1.33
2-year NHIS	1.54	1.31	1.67	1.37
3-year NHIS	1.60	1.36	1.75	1.41
$r = 1/3$:				
1-year NHIS	1.50	1.29	1.63	1.37
2-year NHIS	1.60	1.36	1.75	1.43
3-year NHIS	1.69	1.42	1.84	1.48
$r = 1/4$:				
1-year NHIS	1.55	1.33	1.68	1.42
2-year NHIS	1.67	1.41	1.84	1.49
3-year NHIS	1.74	1.48	1.94	1.55

treated as an undetermined variable; $r = 1/2$, $1/3$, and $1/4$ are considered as three potential values. Values of r less than $1/4$ would increase the variances to a level clearly inefficient.

Variability in segment size

Because NHIS will be based on area samples and will lag behind the 1980 census for about the same amount of time as the Cycle III design, the value of V_n^2 should be the same as estimated earlier, that is, $V_n^2 = 0.05$.

Total design effect

The total design effect is one plus the various components described above and shown in table 20. The design effects for the current sample design have been repeated in this table.

Chapter 6
Sample sizes for alternative designs

Sample sizes needed for specified reliability

Denote the design effect for any statistic with the current design as d and that for any particular alternative as d'. Let n be the sample size with the current design and n' for the alternative. Then, if n' is chosen to equalize the variances,

$$\frac{d\sigma^2}{n} = \frac{d'\sigma^2}{n'}$$

and

$$n' = \frac{d'}{d}n$$

Table 21 shows the sample sizes needed for each alternative to produce equal variances.

Sample size available from NHIS for the alternative designs

For the alternatives comprising samples of women (rather than households), the sample sizes will be the number of households available from NHIS divided by the number of households per eligible retained, as shown in table 15. The number of households to be visited is, of course, the same as the number of women, because there will be one sample woman per household. With a household sample, the household sample size will be the number of households containing eligible women in NHIS plus the appropriate subsample of the remaining NHIS households. These two components are shown separately in table 21. The sample of women will consist of the number that would be available from a sample of women.

The first two lines of table 21 contain estimates of the number of sample black and white and other households that will be in NHIS annually after the NHIS redesign. The data on number of eligible women available for NSFG Cycle IV during any specific period of time and the length of time necessary for NHIS to provide the samples required for each alternative are derived from these numbers. The assumptions used in making the household estimates are as follows:

1. The total number of interviewed households with viable addresses in NHIS will be 50,000 per year.
2. In the population as a whole, the proportion of black households will increase from the 1981 level of about 11 to about 12 percent in 1987, reflecting the increase in the black population.
3. The NHIS revised sample will oversample black households by 20 percent. This will bring the percent of black households in NHIS up to the level of a little over 14 percent of all households.
4. Ten percent of NHIS interviews will be carried out by telephone via random digit dialing and not be available for NSFG. It is assumed that the 10 percent loss will occur proportionally among black and white and other households. The loss will actually be a little greater among white households and a little less for black households because of the lower telephone penetration in black households, but this will probably approximately compensate for some upward rounding in the estimated percent of black households.

Three decisions are inherent in these assumptions—the total sample size, the 20-percent oversampling of black households, and the 10-percent subsample to be carried out by random digit dialing. Any policy change on these three issues will change some of the numbers. The only important item affected will be length of time necessary for NHIS to supply the needed sample. None of the other conclusions in this report will be affected in any appreciable way.

Number of years of NHIS required to provide needed sample size

Comparing the sample sizes in table 22 with the number available from NHIS in table 21 permits a calculation of the minimum number of years required for NHIS to provide the needed sample. This, in turn, can be used to calculate the sample sizes. These data are shown in table 23. The minimum time period in almost all cases includes fractional parts of years, and interpolation methods are necessary for their estimation, because tables 21 and 22 constitute full-year data. The following algorithms have been used for this estimation:

1. If the minimum period is less than 1 year, it is assumed that the sample will consist of a subset of full segments. For the designs using a sample of persons, the design effects will be the ones specified in table 20, and the fraction of NHIS necessary to produce the required sample sizes for 1-year NHIS will be used. For the designs with subsampling, the same procedure is used, although this probably will produce somewhat smaller sampling errors than the ones desired because the proportion of movers will be a little lower than the ones estimated. There are only a few instances in which this is of any importance, and the differences probably are well within the margin of error of the estimated parameters.

Table 21. Sample sizes required for equal reliability with alternative sample designs by race and marital status

Sample design	Black: Ever married	Black: Never married	White and other: Ever married	White and other: Never married	Total
Current design	3,600	2,600	5,400	2,400	14,000
Sample of persons with tracking					
200 PSU's:					
1-year NHIS	2,579	1,628	4,093	2,059	10,359
2-year NHIS	2,687	1,681	4,320	2,108	10,796
3-year NHIS	2,794	1,746	4,547	2,157	11,244
100 PSU's:					
1-year NHIS	2,579	1,628	4,377	2,124	10,708
2-year NHIS	2,687	1,681	4,604	2,173	11,145
3-year NHIS	2,794	1,746	4,832	2,222	11,594
Sample of housing units with subsampling					
200 PSU's:					
$r = 1/2$:					
1-year NHIS	2,615	1,655	4,178	2,092	10,540
2-year NHIS	2,758	1,720	4,462	2,157	11,097
3-year NHIS	2,866	1,786	4,689	2,222	11,563
$r = 1/3$:					
1-year NHIS	2,687	1,694	4,348	2,157	10,886
2-year NHIS	2,866	1,786	4,689	2,254	11,595
3-year NHIS	3,027	1,865	4,945	2,335	12,172
$r = 1/4$:					
1-year NHIS	2,776	1,746	4,491	2,238	11,251
2-year NHIS	2,991	1,852	4,945	2,351	12,139
3-year NHIS	3,116	1,943	5,229	2,449	12,737
100 PSU's:					
$r = 1/2$:					
1-year NHIS	2,615	1,655	4,462	2,157	10,889
2-year NHIS	2,758	1,720	4,746	2,222	11,446
3-year NHIS	2,866	1,786	4,974	2,286	11,912
$r = 1/3$:					
1-year NHIS	2,687	1,694	4,633	2,222	11,236
2-year NHIS	2,866	1,786	4,974	2,319	11,945
3-year NHIS	3,027	1,865	5,229	2,400	12,521
$r = 1/4$:					
1-year NHIS	2,776	1,746	4,775	2,303	11,600
2-year NHIS	2,991	1,852	5,229	2,416	12,488
3-year NHIS	3,116	1,943	5,514	2,514	13,087

2. If the minimum period is more than 1 year, linear interpolation will be used in the last year to estimate the sample size necessary for the fraction of the year required. For a sample between 1 and 2 years, if:

$$m_1 = \text{sample size needed in 1 year}$$
$$m_2 = \text{sample size needed in 2 years}$$
$$n_1 = \text{1-year NHIS}$$
$$n_2 = \text{2-year NHIS}$$
$$f = \text{fraction of the second year}$$
$$n_1 + f(n_2 - n_1) = \text{sample available in } 1 + f \text{ years}$$
$$m_1 + f(m_2 - m_1) = \text{sample needed in } 1 + f \text{ years}$$

then

$$f = \frac{m_1 - n_1}{m_1 - n_1 - (m_2 - n_2)}$$

The sample size is $n_1 + f(n_2 - n_1)$.

Obviously, the same algorithm applies for a sample between 2 and 3 years, with m_1 and n_1 defined as the second-year requirement and NHIS availability, and m_2 and n_2 comparable figures for the third year. The period is then $2 + f$.

An examination of table 23 reveals some of the major features of the alternative designs. For the alternatives requiring samples of persons with tracking, less than 1-year NHIS is needed for almost all of the sample of white women. For the samples of black women, about 1¼ years are needed for a 200-PSU design, and 2½–3 years for a 100-PSU design. A 100-PSU design will require about 6 percent more interviews than a 200-PSU design for the same reliability. For the alternatives in which housing units (or addresses) are the sample units, if one considers 3 years as the maximum period of NHIS that is practical, then a 100-PSU design cannot be used. A 200-PSU design will provide enough sample cases of both black and white women. With a housing unit sample, the number of interview cases needed is increased over the number required if a sample of persons is used, the increase ranging from 2–10 percent, depending on the subsampling rate.

Because it is assumed that a 3-year span is the maximum

Table 22. Sample sizes of households and eligible women available from NHIS for alternative sample designs by race and marital status of eligible women

Sample design	Black: Households[1] With previous eligibles	Black: Households[1] Subsample	Black: Eligible women Ever married	Black: Eligible women Never married	White and other: Households[1] With previous eligibles	White and other: Households[1] Subsample	White and other: Eligible women Ever married	White and other: Eligible women Never married
Total annual NHIS	7,200		2,250	1,565	42,800		13,250	4,242
Area sample annual NHIS	6,480		2,025	1,409	38,520		11,925	3,818
Sample of persons								
200 PSU's:								
1-year NHIS	3,434	...	2,025	1,409	15,743	...	[2]11,925	[2]3,818
2-year NHIS	6,868	...	[2]4,050	[2]2,818	31,486	...	23,850	7,636
3-year NHIS	10,302	...	6,075	4,227	47,229	...	35,775	11,454
100 PSU's:								
1-year NHIS	1,716	...	1,012	704	7,871	...	[2]5,962	1,909
2-year NHIS	3,432	...	2,024	1,408	15,742	...	11,924	[2]3,818
3-year NHIS	5,148	...	[2]3,036	[2]2,112	23,613	...	17,886	5,727
Sample of housing units with subsampling								
200 PSU's:								
$r = 1/2$:								
1-year NHIS	3,434	1,523	1,883	1,332	15,743	11,388	[2]10,912	[2]3,570
2-year NHIS	6,868	3,046	[2]3,605	[2]2,606	31,486	22,776	20,869	6,987
3-year NHIS	10,302	4,569	5,316	3,847	47,229	34,164	30,409	10,194
$r = 1/3$:								
1-year NHIS	3,434	1,015	1,836	1,306	15,743	7,592	[2]10,574	[2]3,487
2-year NHIS	6,868	2,030	[2]3,456	[2]2,536	31,486	15,185	19,875	6,771
3-year NHIS	10,302	3,045	5,063	3,720	47,229	22,777	28,620	9,774
$r = 1/4$:								
1-year NHIS	3,434	762	1,812	1,293	15,743	5,694	[2]10,405	[2]3,446
2-year NHIS	6,868	1,523	[2]3,382	[2]2,501	31,486	11,388	19,378	6,662
3-year NHIS	10,302	2,285	4,936	3,656	47,229	17,082	27,726	9,564
100 PSU's:								
$r = 1/2$:								
1-year NHIS	1,716	762	942	666	7,871	5,694	[2]5,456	1,784
2-year NHIS	3,432	1,523	1,802	1,303	15,742	11,388	10,433	[2]3,493
3-year NHIS	5,148	2,285	2,658	[2]1,923	23,613	17,082	15,204	5,097
$r = 1/3$:								
1-year NHIS	1,716	508	918	653	7,871	3,796	[2]5,287	1,743
2-year NHIS	3,432	1,015	1,728	1,268	15,742	7,593	9,937	[2]3,385
3-year NHIS	5,148	1,523	2,531	1,860	23,613	11,389	14,310	4,887
$r = 1/4$:								
1-year NHIS	1,716	381	906	646	7,871	2,847	5,202	1,723
2-year NHIS	3,432	762	1,691	1,250	15,742	5,695	[2]9,689	[2]3,331
3-year NHIS	5,148	1,143	2,468	1,828	23,613	8,542	13,863	4,782

[1]Does not include any allowance for vacant units, new construction, or nonresponse.
[2]Data for the minimum number of years needed to achieve the required sample size.

practical NHIS time period, a 100-PSU design cannot be used with a sample of housing units, and such designs are excluded from further consideration.

Of course, the choices do not have to be limited to a 100- or 200-PSU design. Intermediate values are possible, although with the planned structure of NHIS, they are somewhat awkward to construct, and computation of variances is somewhat more involved.

Sample design for alternatives using sample of persons

If a sample of persons is to be used, then table 23 describes the sampling procedures fairly completely. For example, if a 200-PSU design is planned, then a sample size of 10,398 women will have the same precision as 14,000 sample women with the current design. The 10,398 is achieved by having a sample of 2,610 black ever married women accumulated over about 1.29 years of NHIS. To get the required number of black never married women, 1.16 years of NHIS are needed, and so forth. Similarly, for a 100-PSU design, 2.73 years of NHIS are needed for black ever married women, and so forth. (The numbers of years shown are, of course, approximations. The actual period of time required to reach the particular sample sizes shown will be affected by such factors as differential response rates, sampling error in NHIS, and the accuracy of our projections of the population in the various race-marital status, and age group combinations. However, the time periods shown are believed to be reasonably close to what will be achieved.)

Table 23. Minimum years of NHIS necessary to achieve required sample sizes and resulting sample sizes by race and marital status

Sample design	Black: Ever married	Black: Never married	White and other: Ever married	White and other: Never married	Total
	Minimum years NHIS				
Sample of persons with tracking					
200 PSU's	1.29	1.16	0.34	0.54	...
100 PSU's	2.73	2.43	0.73	1.12	...
Sample of housing units with subsampling					
200 PSU's:					
$r = 1/2$	1.46	1.27	0.38	0.59	...
$r = 1/3$	1.59	1.34	0.41	0.62	...
$r = 1/4$	1.71	1.41	0.43	0.65	...
100 PSU's:					
$r = 1/2$	3+	2.75	0.82	1.23	...
$r = 1/3$	3+	3+	0.88	1.31	...
$r = 1/4$	3+	3+	0.92	1.39	...
	Number of interviewed women				
Current design	3,600	2,600	5,400	2,400	14,000
Sample of persons with tracking					
200 PSU's	2,610	1,636	4,093	2,059	10,398
100 PSU's	2,765	1,709	4,377	2,130	10,981
Sample of housing units with subsampling					
200 PSU's[1]					
$r = 1/2$	2,681	1,672	4,178	2,092	10,623
$r = 1/3$	2,793	1,725	4,348	2,157	11,023
$r = 1/4$	2,929	1,790	4,491	2,238	11,448

[1]Because 100 PSU's requires over 3 years' accumulation of NHIS data for black participants, it is not acceptable for Cycle IV.

The minimum number of years for white women will apply if the data collection is performed after the entire sample has been accumulated. If continuous interviewing is used, several options are available. One option is to start the fieldwork with only black women in the sample with the white women brought into the sample after a period of time has passed. The periods of time shown in table 23 apply to such a procedure. A second option is to interview both black and white women during the entire field period, subsampling white women to achieve the desired sample size.

Sample design for alternatives using sample of housing units

The housing unit sample alternatives reflect much more complicated situations. Because a different number of months is required for those ever married than for those never married, as well as for black versus white and other races, a decision is necessary on what set of households will be in the sample with certainty and what groups will be subsampled. The rules will have to be developed carefully so that they achieve several purposes:

1. They must provide unbiased samples of eligible women.
2. The amount of screening should be minimized.
3. Sampling procedures should be as simple as possible to avoid potential errors in execution.

There is an issue of possible biases in the sample arising from problems in making sure that movers have known probabilities of selection. The problem is illustrated in table 24, where *B* represents housing units occupied by black households and *W* stands for housing units occupied by white or other races. Time 1 is the time period used for the black sample only; time 2 is the time for both the black and white sample. Time 3 is the interview period. Let r be the NHIS sampling rate in times 1 and 2. All possible situations in residences during times 1–3 and the probabilities of selection of different types of movers are shown in the table, to illustrate the potential problem. Units of types 1 and 5, selected at time 2, will look

Table 24. Probability of selection of movers

Design No.	Time 1	Time 2	Time 3	Probability of selection
1	B	B	B	$2r$
2	B	B	W	$2r$
3	B	W	B	$2r$
4	B	W	W	$2r$
5	W	B	B	r
6	W	B	W	r
7	W	W	B	r
8	W	W	W	r

exactly the same at time 3. There is no way to distinguish them, but they have different probabilities of selection and should get different weights. A similar situation exists for types 2 and 6, and so on. The problem for the white sample could be eliminated by a change in the sampling rules. The new rule would exclude any white households selected at time 1 from the sample. This would give all white households the same probability of selection. However, we still would have the varying probabilities that cannot be identified for black households.

To avoid this bias, it is necessary to use the same period of time for both black and white households (and vacants and new construction). Subsampling of white households will be used to reduce the screening workload, but white households in both periods of time will need to be included.

Specific rules for subsampling are needed for implementing this plan and coming as close as possible to the oversimplified version of the sampling plan discussed earlier. Obviously, variations in the rules can be made and, if the variations are not too great, the analyses and inferences probably are not greatly affected. Major variations are likely to affect the variances or costs. One acceptable set of rules follows:

1. The time period to be used is the longest period needed for the four race-marital status groups. In practice, this means the time period required for black ever married women.
2. Housing units in the NHIS sample containing black ever married women will be included in NSFG with certainty. Housing units with exactly one white or other eligible woman will be subsampled, the subsampling rate consisting of the ratio of the time required for the particular group to the time for the black ever married sample.
3. Housing units containing two or more black eligible women will be retained with certainty. Units with two or more white eligible units will be subsampled at the rates required to bring the rates for these women as close as possible to the rates for women in white households with one eligible woman.
4. Vacant units and other addresses with no persons first will be classified as in areas that are predominantly black or predominantly white. They then will be subsampled at the same rate as noneligible units of the same race.
5. New construction will be sampled at the same rate as vacant units in white areas.

These rules deviate slightly from the assumptions made in estimating design effects and the resulting required sample sizes. For example, the design effects arising from subsampling within multieligible households will be reduced. Conversely, black eligible women moving to new construction will be subsampled at a lower rate than shown in the assumptions. The overall effect of the deviations seems to be fairly small and probably is smaller than errors in estimates of the parameters involved. Therefore, the sample sizes developed earlier will be used.

Screening workload with sample of housing units

Using information on the number of housing units in NHIS, the expected number with eligible women, and the sampling patterns described in the previous section, the screening workload can be estimated. A model of how this is done is shown in table 25.

Similar patterns can be developed for $r = 1/3$ and $1/4$ and for the 100-PSU designs. Table 26 summarizes the results. For purposes of comparability, data for the options using samples of persons (rather than housing units) have also been included. For these alternatives, the number of households is identical to the number to be interviewed. Table 26 contains a number of assumptions, but they affect only a small part of the workload. The assumptions include that new construction will be at the level of 1.6 million units a year and that the number of vacant and converted units constitutes 10 percent of the total in both black and white areas (in addition to the assumptions about number of eligible women in the population and proportions of multieligible households previously discussed). The total workload, including number of interviews and number of units screened, is shown in table 27.

Table 25. Estimation of screening workloads for 200 PSU's at $r = 1/2$ by race and type of housing unit

Race and type of housing unit	Annual NHIS no. of units	Time period required	Number in NHIS	Sub-sampling rate	Number to be screened
Black housing units		Years			
1 ever married	1,800	1.46	2,628	1.00	2,628
1 never married	1,060	1.46	1,548	0.87	1,347
Multiple eligibles	575	1.46	840	1.00	840
No eligibles	3,045	1.46	4,446	0.50	2,223
Vacant	655	1.46	956	0.50	478
White and other housing units					
1 ever married	11,025	1.46	16,097	0.26	4,185
1 never married	2,700	1.46	3,942	0.40	1,577
Multiple eligibles	2,015	1.46	2,942	0.66	1,942
No eligibles	22,780	1.46	33,259	0.20	6,652
Vacant	3,350	1.46	4,891	0.20	978
New construction	900	1.46	1,314	0.20	263

NOTE: The subsampling rates for 1 ever married or 1 never married are the ratios of the minimum number of years for the groups in table 23 to the minimum number of years for black ever married The sampling rate for no eligibles and vacants is r times the rate for the larger of ever married or never married for the race. Vacants will be classified by the dominant race of the area in which they are located.

Table 26. **Estimation of screening workloads for alternative sample designs with 200 PSU's**

Type of housing units	Sample of persons[1]	Sample of housing units[2]					
		r = ½		r = ⅓		r = ¼	
		Subsample rate	Number screened	Subsample rate	Number screened	Subsample rate	Number screened
Black housing units							
1 ever married	2,610	1.00	2,628	1.00	2,862	1.00	3,078
1 never married	1,636	0.87	1,347	0.84	1,416	0.82	1,495
Multiple eligibles	...	1.00	840	1.00	914	1.00	983
No eligibles	...	0.50	2,223	0.33	1,614	0.25	1,302
Vacant	...	0.50	478	0.33	347	0.25	280
New construction	...	...	...	...	...	...	...
White and other housing units							
1 ever married	4,093	0.26	4,185	0.26	4,520	0.25	4,741
1 never married	2,059	0.40	1,577	0.39	1,674	0.38	1,755
Multiple eligibles	...	0.66	1,942	0.65	2,083	0.63	2,171
No eligibles	...	0.20	6,652	0.13	4,709	0.10	3,895
Vacant	...	0.20	978	0.13	692	0.10	573
New construction	...	0.20	263	0.13	186	0.10	154

[1]The numbers shown for 1 ever married and 1 never married include women in households with multiple eligibles.
[2]1.46 years of NHIS for r = ½; 1.59 years of NHIS for r = ⅓; 1.71 years of NHIS for r = ¼.

Table 27. **Estimated total field workload for alternative sample designs by race and marital status**

Sample design	All races	Black		White and other		All races		Black		White and other	
		Ever married	Never married	Ever married	Never married	Interviewed women	Screened households	Interviewed women	Screened households	Interviewed women	Screened households
	Years NHIS required					Number					
Cycle III sample design	...	...	...	...	...	14,000	54,000	6,200	16,000	7,800	38,000
Sample of persons, 200 PSU's	...	1.16	1.29	0.34	0.54	10,398	10,398	4,246	4,246	6,152	6,152
Sample of housing units:											
r = ½	1.46	...	...	...	...	10,623	23,113	4,353	7,516	6,270	15,597
r = ⅓	1.59	...	...	...	...	11,023	21,017	4,518	7,153	6,505	13,864
r = ¼	1.71	...	...	...	...	11,448	20,423	4,719	7,138	6,729	13,289
Sample of persons, 100 PSU's	...	2.43	2.73	0.73	1.12	10,981	10,981	4,474	4,474	6,507	6,507

Sample size for alternative of a sample of 10,000 women with Cycle III sample design

If the budget is based on a sample of 10,000 with the Cycle III sample design, the design effects for the various alternative samples will be a little lower than for a 14,000 sample size because the PSU cluster size will be reduced. The reduction will be trivial for the black sample and about 5 percent for the white sample (see table 13). Because the two sets of design factors are so close, the differences can be ignored. This will cause a trivial overstatement in the required field workload for white women, and virtually no overstatement for black women. The sample sizes then will be 10/14 of the sample sizes for the major alternatives. The fieldwork for this sample size is shown in table 28.

Tracking workload with one-time versus continuous data collection

It is useful to consider the advantages and disadvantages of a one-time versus continuous field data collection procedure. With a one-time system, the NHIS sample cases would be accumulated until the required sample sizes are attained and then NSFG interviewing would start. With a continuous procedure, NSFG interviews would follow closely after the identification of a sample person in NHIS. The advantage of a continuous operation is that the mobility of the population would become a relatively minor matter. It is assumed that arrangements would be made with the U.S. Bureau of the Census to obtain lists of eligible sample persons within a few weeks after their inclusion in NHIS. Allowing a little additional time for a contractor to organize the material, subsample where necessary,

Table 28. Estimated total field workload for alternative sample designs with sample size of 10,000 by race and marital status

Sample design	All races	Black: Ever married	Black: Never married	White and other: Ever married	White and other: Never married	All races: Interviewed women	All races: Screened households	Black: Interviewed women	Black: Screened households	White and other: Interviewed women	White and other: Screened households
	Years NHIS required					Number					
Cycle III sample design	...	...	...	...	...	10,000	38,570	4,425	11,430	5,575	27,140
Sample of persons, 200 PSU's	...	0.83	0.92	0.24	0.39	7,427	7,427	3,033	3,033	4,394	4,394
Sample of housing units:											
$r=1/2$	...	...	...	...	...	7,588	16,510	3,109	5,369	4,479	11,141
$r=1/3$	...	...	...	...	...	7,873	15,012	3,227	5,109	4,646	9,903
$r=1/4$	...	...	...	...	...	8,177	14,592	3,371	5,099	4,806	9,492
Sample of persons, 100 PSU's	...	1.74	1.95	0.52	0.80	7,843	7,843	3,195	3,195	4,648	4,648

Table 29. Percent of movers in full sample of persons by distance of move, type of interviewing, race, and marital status

No. of PSU's, race, and marital status	Years NHIS required	Total movers: 1-time interviewing	Total movers: Continuous interviewing	Local movers: 1-time interviewing	Local movers: Continuous interviewing	Long distance movers: 1-time interviewing	Long distance movers: Continuous interviewing
200 PSU's		Percent[1]					
Total[2]	...	11.0	4.8	7.3	3.2	3.7	1.6
Black:							
Ever married	1.16	17.6	5.2	11.7	3.5	5.9	1.7
Never married	1.29	15.3	4.2	9.9	2.7	5.4	1.5
White and other:							
Ever married	0.34	6.8	5.2	4.6	3.5	2.2	1.7
Never married	0.54	7.5	4.2	4.9	2.7	2.6	1.5
100 PSU's							
Total[2]	...	18.8	4.9	12.4	3.2	6.4	1.7
Black:							
Ever married	2.43	30.7	5.2	20.0	3.5	10.7	1.7
Never married	2.73	23.9	4.2	16.4	2.7	7.5	1.5
White and other:							
Ever married	0.73	11.8	5.2	7.9	3.5	3.9	1.7
Never married	1.12	13.6	4.2	8.8	2.7	4.8	1.5

[1]For 1-time interviewing, we have assumed a 2-month lag in addition to the required number of years NHIS. The mobility rates then were those applicable to the midpoint of the time interval. For continuous interviewing, we have assumed a 2-month lag and that the mobility rate is 20 percent of 1-year mobility.
[2]The total is the weighted average of the 4 subgroups, with the weights proportionate to the sample size.

and send the material to the interviewers, it is believed that the NSFG interviews usually could be conducted about 2 months after identification in NHIS. The proportion of movers then would be quite low. As a result, there does not appear to be any need for a housing unit sample with continuous interviewing. Analysis of continuous and one-time interviewing is, therefore, restricted to person samples.

Table 29 shows the percent of sample women who, at the time of the NSFG interview, will no longer reside at the address at which they were interviewed in NHIS. The table contains data for the 200- and 100-PSU designs. Separate estimates are shown for local and long distance movers.

As can be seen, with 200 PSU's and one-time interviewing, an estimated 11 percent of the sample women no longer will live at their old addresses. Of this number, 3.7 percent will be long distance movers and 7.3 percent local movers. The comparable figures for continuous interviewing are 4.8, 1.6, and 3.2 percent. With continuous interviewing, therefore, tracking is necessary for less than half the number of cases required with a one-time data collection period. The differences are even greater with a 100-PSU sample design that will have about 18.8 percent movers with one-time interviewing contrasted with 4.9 percent with continuous data collection.

The number of people who need to be followed will, of course, affect the cost of the survey and probably the response rates. The costs of continuous versus one-time interviewing and management and administrative issues are discussed in chapter 7.

Chapter 7
Costs, response rates, and time schedule

Costs

In this section cost estimates for the various possible designs for Cycle IV of NSFG are presented. All cost estimates shown consist of direct costs only. Because the main purpose of the cost analysis was to arrive at relative costs of the various designs, it was thought best to deal with estimates of direct costs only. In this way, there was no need to speculate on the variations in indirect cost structures among survey organizations that might perform the task of NSFG data collection. It may be noted that indirect costs generally add another approximately 50 percent to the direct costs for a survey of this size and complexity.

No pretest or pilot costs have been included in any of the estimates. Regardless of the design chosen for Cycle IV, a pretest primarily designed to test the questionnaire, the field procedures, and the data preparation procedures would be conducted. There appears to be no reason to expect its cost to vary much among the several designs under consideration.

A pilot study might be in order, particularly with regard to the development of procedures and cost estimates for tracing "movers," an effort that will be required if a decision is made to use persons (that is, eligible respondents identified through NHIS) as the sampling unit. Other possible concerns that could be examined in pilot studies are discussed in chapter 8. However, no attempt was made as part of this report to estimate the cost of such an effort.

Computer costs have been included only for the management systems required to control a project of this magnitude and complexity. It is assumed that the NCHS computer would be used for data cleaning purposes.

All costs are estimated at first quarter 1983 price levels. The reader should use his or her own judgment in estimating how various categories of cost might be expected to inflate between early 1983 and the actual period of contract performance.

Cost estimates (incorporating all the preceding caveats) are presented in summary form for eight possible designs in table 30. The sample sizes on which the costs are based are those that will provide the same reliability (in terms of sampling

Table 30. Comparisons of direct costs, schedules, number of interviews, and response rates for alternative sample designs

	Sample of persons				*Sample of housing units*			
	1-time interviewing		*Continuous interviewing*		*r = ½*	*r = ⅓*	*r = ¼*	*Cycle III model, 80 PSU's*
	100 PSU's	*200 PSU's*	*100 PSU's*	*200 PSU's*		*200 PSU's*		
Scheduling element	*Design 1*	*Design 2*	*Design 3*	*Design 4*	*Design 5*	*Design 6*	*Design 7*	*Design 8*
				Dollars				
Total	1,706,721	1,872,316	2,632,006	2,608,193	2,024,473	2,025,328	2,041,343	2,612,510
Professional labor	249,134	251,448	285,470	267,342	244,058	244,058	244,058	263,273
Clerical labor	173,146	156,762	145,197	145,182	173,948	176,707	179,488	211,201
Field labor	519,205	561,877	615,359	613,798	627,508	629,936	638,147	923,420
Travel	390,540	540,965	1,252,687	1,258,562	556,180	556,180	556,180	468,995
Other direct costs	374,696	361,264	333,293	323,309	422,779	418,447	423,470	745,621
				Months				
Schedule	18	18	45	27	18	18	18	20
				Number				
Screening interviews	0	0	0	0	23,113	21,017	20,423	54,000
Extended interviews	10,981	10,398	10,981	10,398	10,623	11,023	11,448	14,000
				Rate				
Estimated response[1]	0.81	0.82	0.83	0.83	0.84	0.84	0.84	0.84

[1]The extended interview response rate for Cycle III was 84.2 percent for 20–44-year-old women and 83.6 percent for the total sample, which included an oversampling of teenagers. Therefore, 84 percent was used as the estimated response rate for Cycle IV, Design No. 8. All other response rates have been estimated relative to the Cycle III model.

error) for all designs. For the convenience of the reader, each of the designs is described briefly again:

1. *Design 1.* Sample of persons; one-time data collection period; 100 PSU's. As shown in table 30, this design requires an $n = 10{,}981$. This estimate is for a design in which cases would be accumulated from NHIS until a sufficient sample is available and then the interviewing would be conducted over a period of about 4 months. The total project time would be about 18 months. A field force of 187 interviewers would be needed.
2. *Design 2.* Sample of persons; one-time data collection period; 200 PSU's. As shown in table 30, Design 2 requires an $n = 10{,}398$. Design 2 differs from Design 1 in that by doubling the number of PSU's, one would need to increase the number of interviewers to 300. Total study time would be about 18 months.
3. *Design 3.* Sample of persons; continuous data collection; 100 PSU's. As in Design 1, an $n = 10{,}981$ would be required. The only difference between Designs 1 and 3 is that in the latter the NSFG interviews would be conducted on a "flow" basis as eligible respondents are identified in NHIS. Because this results in a very thin workload per PSU per month, it is assumed that the interviewing would be carried out by a traveling corps of about 32 interviewers, each of whom covers several PSU's. The total project time would be about 45 months.
4. *Design 4.* Sample of persons; continuous data collection; 200 PSU's. This design is similar to Design 3 except the number of cases is 10,398; the number of interviewers is 66; and the total project time is 27 months.
5. *Design 5.* Sample of housing units; $r = ½$ (that is, using a 50-percent subsampling rate for housing units that did not contain an eligible person in the NHIS interviews); 200 PSU's. As shown in table 30, this design requires 10,623 extended interviews and would require screening 23,113 housing units. It is estimated that 300 interviewers and a total study length of 18 months would be needed.
6. *Design 6.* Sample of housing units; $r = ⅓$; 200 PSU's. Same as Design 5 except that the number of extended interviews becomes 11,023 and the number of screenings would be 21,017.
7. *Design 7.* Sample of housing units; $r = ¼$; 200 PSU's. Same as Design 5 except that the number of extended interviews becomes 11,448 and the number of screenings would be 20,423.
8. *Design 8.* Cycle III model. This area probability design using 80 PSU's would require the screening of 54,000 households and extended interviewing of 14,000 women. Two hundred interviewers and a total study time of 20 months would be required.

Table 30 indicates that among the seven designs using the NHIS sample, the least expensive would be a sample of persons in 100 PSU's who were interviewed during a one-time data collection effort (Design 1). The most expensive would be a sample of persons in 100 PSU's with continuous interviewing over the entire period of time needed to build the required sample (Design 3). The Cycle III model (Design 8) would cost almost as much as the most expensive NHIS-related design.

The factors that have the greatest effect on cost are overall time schedule, number of interviewers required, amount of travel, number of screening interviews, and number of extended interviews. The models requiring continuous interviewing (Designs 3 and 4) are very expensive because of the use of traveling interviewers; the longer overall schedules for these designs also make professional labor somewhat more expensive. All of the designs using a sample of persons (that is, Designs 1–4) require a certain amount of tracking of those respondents between the NHIS and the NSFG field periods, plus the extra costs of interviewing those who move out of a PSU. Tracking costs for these four designs, including extra costs for out-of-town movers, are as follows:

Design	*Cost*
1	$134,690
2	54,004
3	22,483
4	19,809

For the most part these costs are a function of time lapse. It takes longer to accumulate the sample in 100 PSU's (Design 1) than in 200 PSU's (Design 2); therefore, there is greater opportunity for respondents to move before the NSFG interview occurs. In the continuous interview alternatives (Designs 3 and 4), it is assumed that only a modest amount of moving occurs during the time needed for NHIS to turn a case over to the NSFG contractor and for the contractor to put the case into the field.

The designs that sample housing units (Designs 5–7) have no tracking costs associated with them. However, they have screening costs and hence require more interviewer, supervisor, and training time.

The Cycle III model (Design 8) is relatively expensive because the number of extended interviews is higher (14,000 compared with a range of 10,398 to 11,448), the number of screening interviews is much higher (54,000 compared with a range of zero to 23,113), and address listing is required in 1,800 area segments. Hence, the cost of field labor is much higher.

Response rates

A considerable amount of conjecture is involved in attempting to predict response rates for Cycle IV. One of the unknown factors is the public mood 4 or 5 years from now, and general attitudes toward surveys. A second unknown item is whether prior involvement with the NHIS interview will affect willingness to cooperate in NSFG. (Examining this issue in a pilot study is recommended; see chapter 8.) Consequently, serious reservations accompany estimates of response rates for Cycle IV made at this time.

The effect of the various procedures on response rates can be approximated roughly, however, leading to estimates of differences among response rates. Such differences are more relevant than data on total response rates in evaluating the quality of the various alternatives. It should be noted, however, that a fair amount of speculation went into the preparation of these estimates, and they should be treated as indications of the order of magnitude of differences in response rates.

It is likely that the housing unit sample procedures (Designs 5–7) will have about the same nonresponse rates as the Cycle III model (assuming that inclusion in NHIS has no effect on cooperation in NSFG). In interviewer-respondent interactions, Designs 5–8 are similar, consisting of a screening followed by the detailed interview. However, the same response rates would be expected for the four methods only if screening is performed in a personal visit, to be followed immediately by the detailed interview. If a different plan is followed for Designs 5–7 than for Design 8 (for example, telephone screening with a followup in the form of a personal interview in eligible households), the nonresponse rate is likely to increase. Experience with this kind of survey situation is limited but it is speculated that under these conditions response rates would decline by 1–3 percent.

It is likely that response rates for Designs 1–4 would be somewhat below those for Designs 5–7. The need to track movers almost certainly will lower response rates. One reason is that a few movers probably never will be located regardless of the effort made in tracking. Second, some of the movers now may reside in areas that are so far away from the sample PSU's that it is not practical to carry out the intensive conversion efforts normally applied.

The decline in response rates thus will depend partly on the proportion of movers in the sample. As can be seen from table 29, the expected percents of movers in Designs 1–4 are as follows:

Design	*Percent*
1	18.8
2	11.0
3	4.9
4	4.8

Using these data, it is speculated that the decline in response arising from tracking and followup of movers will be about as follows:

1. Designs 3 and 4—an increase of perhaps 0.5 percent in the nonresponse rate.
2. Design 2—an increase of about 1–1.5 percent.
3. Design 1—an increase of 2–3 percent.

Time schedule

There are two aspects of the Cycle IV time schedule that should be considered. One is the time period for essential survey operations, for example, questionnaire development, training, conduct of interview, data processing, and so forth. The other is the time required to accumulate the necessary sampling materials in the NHIS and organize it for transmittal to the Cycle IV survey operations group.

Table 30 contains, among other information, our estimates of the time period needed for survey operations. As can be seen, all designs except those for continuous interviewing can be completed in about the same period of time, 18 or 20 months. The Cycle III model takes a little longer than the others because of the additional time needed for sample selection, listing, and screening. However, the difference between Cycle III and Designs 1, 2, 5, 6, or 7 is quite small; also, these numbers should not be taken too literally because at present only approximate time schedules are possible.

Designs 3 and 4 will take much longer because of the nature of the continuous interviewing operation. The length of the actual interviewing period will be about 4–6 months for all the noncontinuous interview procedures. For continuous interviewing, it will be about 18 months for the 200-PSU design and 34 months for the 100-PSU design. The analytic implications of these increases in length of the interviewing period should be taken into account in choosing among the alternatives.

Chapter 8
Other research issues

Scope and definitions in NHIS and NSFG

The definitions for the two surveys are essentially the same. Both cover the civilian noninstitutionalized population, including all eligible persons in ordinary housing units and boarding houses; they exclude the institutionalized and military population. The residence rules for college students are different in the two surveys, but this does not affect the population covered. It simply means that college students selected in NHIS at one location may have to be interviewed in another.

There is one difference in the geographic coverage of the two surveys. NHIS covers all 50 States and the District of Columbia, including Hawaii and Alaska, whereas NSFG has excluded Hawaii and Alaska. If NSFG is to retain the same geographic coverage as in the past, it would not be complicated or difficult to modify NHIS to exclude these two areas. In the current NHIS design, Hawaii is a separate stratum. Eliminating the Hawaii PSU automatically will bring the estimates to the 49-State level. Alaska has been combined with other northwestern PSU's. If the U.S. Bureau of the Census follows the same policy in the redesigned NHIS, some reweighting of these PSU's will be needed. The adjustments will not be very large, and the effect should be trivial. This should not be an important consideration in deciding whether and how to use NHIS. It should be noted, however, that one or two NHIS PSU's may fall outside the sample. The 200-PSU sample thus may turn out to be about 198 PSU's.

This may be an appropriate time for the National Center for Health Statistics to reconsider whether Hawaii and Alaska should be excluded from the NSFG frame. There are some advantages in having the survey cover all 50 States, and thus be comparable with most demographic and social statistics produced by U.S. Government agencies. There is no intrinsic difficulty in conducting interviews in PSU's in these two States. The unit costs would be higher, stemming from such factors as increased costs of bringing interviewers to central locations for training, higher recruiting costs, and higher costs for quality control and general communication. However, because it is likely that these costs will affect only one or two PSU's, the effect on the total budget will be fairly small. The availability of NHIS sample data would simplify expanding NSFG to cover Hawaii and Alaska.

In this report it has been assumed for cost and sample size calculations that all 50 States are to be covered. However, this has only a minor effect on total costs and none of the conclusions would be changed if the survey were restricted to 48 States, as in the past.

Staffing

If the data collection period is to be concentrated within a few months, starting after the entire NHIS sample has been accumulated, then the interview process will be quite similar to the way Cycles II and III were carried out. In that case, using a staffing pattern similar to the one in Cycles II and III is suggested. This involves using local interviewers who are residents of the sample PSU's. The number of interviewers per PSU will depend on the expected workload. The work assignments would be made in more or less the same way as in Cycles II and III; that is, to minimize travel costs and, when feasible, to racially match respondents to interviewers.

With continuous interviewing, the monthly workload per PSU will be quite small; therefore, traveling interviewers, somewhat in the manner used in the NHIS, are recommended. In general, one interviewer would be recruited to handle two to four neighboring PSU's, depending on the sample sizes and distances between PSU's.

Timing of NHIS and NSFG

No need for special action because of movement into and out of NHIS-screened housing units or into and out of age groups is expected. Each of the alternatives discussed in this report has methods of handling persons who move that give all eligible persons known non-zero probabilities of selection. As far as age is concerned, the NHIS contains the ages of all sample persons by single years of age. Thus, it is possible to specify the ages at time of NHIS interview that will make the designated women age eligible at the time of NSFG interview.

It is, of course, not possible to control marital status in the same way as age. Some never married women will marry in the interval between the NHIS and NSFG interviews. This does not cause any bias, but it does introduce a small amount of uncertainty into the final sample sizes for ever married and never married women. In addition, it implies that a small number of ever married women will have been sampled as never married and carry their weights, and vice versa. Exact sample sizes never have been possible in NSFG, and this is not a cause for concern. In fact, for the alternative comprising samples of persons, use of NHIS should result in actual sample sizes being much closer to the designated sample sizes than was possible in the past.

Induction of sample persons

NHIS contains a considerable amount of information that could be used for stratification; for example, age of woman, income, number of children living in household, and educational attainment. Unfortunately, this information can be used for sample selection only to a limited degree. The ability to stratify depends on having a reasonably large pool of women from which a sample is to be selected. Creating such a pool implies using a long time period for NHIS during which the sample is accumulated. For the alternatives considered, however, long periods have deleterious effects. With samples of persons, a long period means that more persons have to be tracked with attendant costs and potential increases in nonresponse rates. With samples of housing units, the effective stratification of households into those containing eligibles and those not having eligibles will be weakened. The disadvantages are quite serious. In addition, the redesigned NHIS will only be in existence a limited period of time when it will be necessary to start NSFG. For these reasons, it is not practical to have a long time period in NHIS from which to choose the NSFG sample. The following are suggestions for subsampling the NHIS file:

1. There will be very little subsampling for black women; the NHIS time period used will be the minimum necessary to supply the sample of black women. All black ever married women in NHIS in that period will be designated for NSFG and about 90 percent of never married women. Stratification cannot be applied effectively.
2. With one-time interviewing of a sample of persons, it is recommended that the sampled white women be those most recently in NHIS. Stratification cannot be used for white women with this alternative.
3. With continuous interviewing, a subsample of white women will be selected for interviewing. At this time it is unclear whether the sample will be drawn by the U.S. Bureau of the Census, in its field offices, or whether the U.S. Bureau of the Census will provide the entire list of eligible women to a contractor who will select the sample. This is a detail that will have to be worked out with the U.S. Bureau of the Census.

   If the U.S. Bureau of the Census selects the sample, it is recommended that no stratification be imposed. With such decentralized sampling, it is more important to keep the procedures simple than to attempt to squeeze the maximum efficiency out of the process. Possible errors resulting from the added complexity of stratification could negate any statistical advantages. Items that appear to be effective modes of stratification can be introduced in the estimation procedure in the form of poststratification. Their effectiveness would be almost as great.

   If a contractor carries out the sample selection, stratification is feasible. It is recommended that items be used that seem to be important discriminators for fertility patterns, such as (*a*) for women 20 years of age and over, educational attainment and number of own children in household; (*b*) for women under 20 years of age, school enrollment and educational attainment of parents. However, Cycle III data should be analyzed, when available, for a final decision on modes of stratification.
4. If Cycle IV consists of a sample of housing units, units containing white eligible women also will be subsampled. The policy recommended in paragraph (3) is again recommended.

In regard to the method of notifying sample persons that they are to be included in NSFG, the situation is somewhat complicated. At the time of the NHIS interview, the U.S. Bureau of the Census interviewer will not know exactly who will be included in NSFG (or other followup surveys). It is assumed that the U.S. Bureau of the Census will wish to inform all respondents that they may be covered in other studies, without describing the other studies. In Cycles II and III, Westat sent a letter to respondents prior to the interviewers' visits. It is suggested that this practice be continued. With a sample of persons, the letters could be addressed directly to the sample eligible women instead of "occupant" and thus be somewhat more personal.

In addition, it may be useful to have interim mailings if the procedure to be used is a sample of persons with one-time interviewing. The purpose of the interim mailings would be to assist in tracking movers. The letters would ask potential respondents to notify the National Center for Health Statistics or the contractor of new addresses if they move between the NHIS and NSFG interview periods. Such actions seem to have helped tracking in other studies. This may not be necessary or feasible for white respondents with a 200-PSU design. The time interval between the two surveys will be quite short and interim mailings for this group may be more of a nuisance than a help. It is more useful with a 100-PSU sample and, of course, more important for the sample of black women than for white and other women because of the much longer time period used for the accumulation of the sample of black women.

The continuation of the Cycle III practice of asking for parental consent for teenagers at the time of the NSFG interviewers' visits is suggested. The success rate in Cycle III was quite good and no advantage could be anticipated by changing the procedure.

Pilot studies

There are several issues that should be addressed in a pilot study, or possibly several small studies. (It may be useful to distinguish between the concept of pilot studies and the pretests that have been conducted as part of Cycles II and III. Pretests essentially are for the purpose of improving questionnaires, procedures, and training methods that have been decided upon. They act as "debugging" devices. Pilot studies are conducted at an earlier stage of preparation. They test whether some approaches are feasible and search for the best among several alternatives. It is assumed that pretests will be carried out for Cycle IV as in the past.)

It would be useful to study the methods of tracking movers. The possibility of interim mailings for access to more current addresses of the sample persons has been suggested. There are, of course, other methods that can be used to locate movers, such as calling directory assistance, contacting the friends and relatives supplied by the respondents in the NHIS interview, and writing to the sample persons and asking the post office to forward the mail. It is likely that all these methods will be used to some extent, but it would be useful to get information on the unit costs of these procedures and the success rate of each. This would help in establishing an order of priority for the search operation. It also would help in budgeting and in anticipating nonresponse resulting from inability to locate some persons.

It also would be useful to investigate whether there are problems in locating the sample addresses with the materials supplied by the U.S. Bureau of the Census. There are several kinds of materials the U.S. Bureau of the Census uses in the course of developing area samples: maps, listing forms, addresses of sample units, and names of residents. It is likely that transmittal of street addresses and apartments is all that is needed in most urban areas to locate the sample person, although there may be some problems even in such areas in buildings that do not have clearly identifiable apartment numbers. Good street addresses do not exist in many rural areas. It would be helpful to ascertain whether information currently recorded by the U.S. Bureau of the Census is sufficient to locate virtually all sample addresses, or if some modification of the U.S. Bureau of the Census forms is necessary.

A third item about which there is considerable uncertainty is what effect the new procedures would have on nonresponse rates. There is likely to be an increase in nonresponse caused by a number of factors: Some movers may not be able to be located in spite of intensive tracking efforts; a few may be located but are at such distances from sample PSU's that it will not be feasible to perform normal, intensive followup efforts; and the fact that the respondents were subject to an earlier NHIS interview may influence their willingness to cooperate. It is doubtful that good estimates of response rates are possible from a pilot study. (Pilot studies never seem to have the sample sizes; the time that is necessary for a full, intensive followup effort; the geographic distribution of the sample; or the commitment to carry out these operations as completely as necessary.) However, it should be possible to get some indication of the increase in nonresponse rates, and for what population groups more intensive work on conversion is useful. At a minimum, a pilot study on tracking should reveal how many cases will involve intensive and/or expensive efforts to locate, and what the increase in nonresponse would be if the efforts were relaxed.

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Appendix

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Appendix
Components of variance as calculated from Cycle II data

Computations for estimating components of variance, design effects, and intraclass correlations using National Survey of Family Growth (NSFG) Cycle II data were based on the following set of key statistics (each an estimate of the total number in the category listed):

1. Contraceptors.
2. Surgical contraceptors.
3. Nonsurgical contraceptors.
4. Women who have had a family planning visit within the last 3 years.
5. Women whose last family planning visit was to their own doctor.
6. Women whose last family planning visit was not to their own doctor.
7. Women who breast fed their first child for any duration.
8. Women who breast fed their first child for more than 3 months.
9. Women whose last live birth was wanted, then or sooner.
10. Women whose last live birth was wanted, but later.
11. Women whose last live birth was unwanted.
12. Women whose first child was born within first year of marriage.
13. Women whose first live birth was within first 7 months of marriage (premarital conception).

These statistics were chosen to be broadly representative of topics highlighted in *Vital and Health Statistics*, Series 23, Data from the National Survey of Family Growth, as well as to span a wide range of magnitudes.

Variance estimation for this project was done using balanced repeated replication. The goal in these computations was to estimate the between-primary-sampling-unit (PSU), between-segment, and within-segment components of variance. The 79-PSU design used for the NSFG also has a between-stratum component of variance which is not relevant to planning for Cycle IV. Preprocessing of the data file was done to eliminate this component of variance from the estimates. This was accomplished by first eliminating data from any noncertainty stratum that had only one PSU selected from it, leaving a two-PSU-per-stratum sample. The sample from the certainty strata was adjusted to a similar level by selecting a two-thirds subsample of segments.

This thinning of the sample necessitated an adjustment to the weights so that estimates of totals would be at the proper level. The weights of ever married women were ratio adjusted to the same Current Population Survey benchmarks by race and age class used by the National Center for Health Statistics in its original weighting scheme. The weights of never married women were inflated by a factor of 1.5 to reflect the two-thirds subsampling rate.

In balanced, repeated replication, an individual response is included in the estimate for a particular replicate based on a stratum and half-sample identifier (+ or −) assigned by the user. The different components of variance may be estimated by altering the assignment of these two identifiers. Table I shows how these identifiers were assigned for each type of variance estimate.

For each half sample the weights of the ever married women were ratio adjusted to match the Current Population Survey benchmarks. Those of the never married women were multiplied by 2.

The components of variance estimated directly were as follows:

1. $\hat{\sigma}^2$ between and within PSU.
2. $\hat{\sigma}^2$ within PSU.
3. $\hat{\sigma}^2$ within segment.

These are presented in tables II–IV. Subtraction produces estimates of the desired components:

1. $\hat{\sigma}^2$ between PSU $= \hat{\sigma}^2$ between PSU + within PSU − $\hat{\sigma}^2$ within PSU.
2. $\hat{\sigma}^2$ between segment $= \hat{\sigma}^2$ within PSU − $\hat{\sigma}^2$ within segment.
3. $\hat{\sigma}^2$ within segment.

Table I. Assignment of stratum and half-sample identifiers for estimating components of variance

Variance estimate	Self-representing PSU's		Non-self-representing PSU's	
	Stratum	+ or −	Stratum	+ or −
Between and within PSU	Sequential by PSU	Alternates by segment	Sequential by pairs of PSU's	Alternates by PSU
Within PSU	Sequential by PSU	Alternates by segment	Sequential by PSU	Alternates by segment
Within segment	Sequential by PSU	Alternates by dwelling unit number	Sequential by PSU	Alternates by dwelling unit number

Table II. Cycle II family growth variance estimates between and within PSU by race and categories of women

Race and category of woman	*Estimate*	*Variance*	*Standard error*	*Coefficient of variation*
Black				
Contraceptor	2,420,093	8,488,948,530	92,135	0.0381
Surgical contraceptor	448,770	2,031,145,030	45,068	0.1004
Nonsurgical contraceptor	1,971,323	6,507,505,590	80,669	0.0409
Had family planning visit within last 3 years	1,724,061	6,739,469,230	82,094	0.0476
Last family planning visit to own doctor	873,936	5,340,103,050	73,076	0.0836
Last family planning visit not to own doctor	732,971	5,476,464,200	74,003	0.1010
Breast fed first child for any duration	1,004,225	3,820,354,840	61,809	0.0615
Breast fed first child for more than 3 months	506,477	2,422,454,340	49,218	0.0972
Last live birth wanted, then or sooner	1,621,007	3,636,218,360	60,301	0.0372
Last live birth wanted, but later	1,040,170	3,370,875,700	58,059	0.0558
Last live birth unwanted	1,095,760	2,575,684,460	50,751	0.0463
First child born within first year of marriage	960,006	4,145,065,460	64,382	0.0671
Premarital conception	659,907	1,727,495,450	41,563	0.0630
White and other				
Contraceptor	19,588,131	87,681,683,800	296,111	0.0151
Surgical contraceptor	5,291,340	78,435,283,200	280,063	0.0529
Nonsurgical contraceptor	14,296,791	147,818,901,000	384,472	0.0269
Had family planning visit within last 3 years	14,048,920	116,884,028,000	341,883	0.0243
Last family planning visit to own doctor	11,340,357	130,425,806,000	361,145	0.0318
Last family planning visit not to own doctor	2,115,512	27,960,381,600	167,214	0.0790
Breast fed first child for any duration	8,559,417	80,478,878,300	283,688	0.0331
Breast fed first child for more than 3 months	3,885,143	38,372,987,700	195,890	0.0504
Last live birth wanted, then or sooner	14,457,698	90,793,742,000	301,320	0.0209
Last live birth wanted, but later	5,196,351	58,795,618,700	242,478	0.0467
Last live birth unwanted	3,283,521	25,776,525,600	160,551	0.0489
First child born within first year of marriage	8,161,022	72,369,758,400	269,016	0.0330
Premarital conception	4,132,640	44,789,841,600	211,636	0.0512

Table III. Cycle II family growth variance estimates within PSU by race and categories of women

Race and category of woman	*Estimate*	*Variance*	*Standard error*	*Coefficient of variation*
Black				
Contraceptor	2,420,093	7,787,095,290	88,245	0.0365
Surgical contraceptor	448,770	925,867,998	30,428	0.0678
Nonsurgical contraceptor	1,971,323	5,233,557,120	72,343	0.0367
Had family planning visit within last 3 years	1,724,061	4,803,545,380	69,308	0.0402
Last family planning visit to own doctor	873,936	3,776,200,390	61,451	0.0703
Last family planning visit not to own doctor	732,971	3,377,039,770	58,112	0.0793
Breast fed first child for any duration	1,004,225	3,281,294,840	57,283	0.0570
Breast fed first child for more than 3 months	506,477	2,453,951,620	49,537	0.0978
Last live birth wanted, then or sooner	1,621,007	3,122,518,040	55,879	0.0345
Last live birth wanted, but later	1,040,170	5,780,761,720	76,031	0.0731
Last live birth unwanted	1,095,760	2,740,103,030	52,346	0.0478
First child born within first year of marriage	960,006	3,131,131,450	55,957	0.0583
Premarital conception	659,907	1,404,540,040	37,477	0.0568
White and other				
Contraceptor	19,588,131	71,375,266,100	267,161	0.0136
Surgical contraceptor	5,291,340	42,819,579,600	206,929	0.0391
Nonsurgical contraceptor	14,296,791	116,922,269,000	341,939	0.0239
Had family planning visit within last 3 years	14,048,920	47,340,573,300	217,579	0.0155
Last family planning visit to own doctor	11,340,357	55,332,845,200	235,229	0.0207
Last family planning visit not to own doctor	2,115,512	29,834,095,700	172,725	0.0816
Breast fed first child for any duration	8,559,417	56,243,023,400	237,156	0.0277
Breast fed first child for more than 3 months	3,885,143	29,412,562,600	171,501	0.0441
Last live birth wanted, then or sooner	14,457,698	42,142,598,800	205,287	0.0142
Last live birth wanted, but later	5,196,351	45,471,006,200	213,239	0.0410
Last live birth unwanted	3,283,521	31,599,334,500	177,762	0.0541
First child born within first year of marriage	8,161,022	103,389,650,000	321,543	0.0394
Premarital conception	4,132,640	50,833,454,800	225,463	0.0546

Table IV. Cycle II family growth variance estimates within segment by race and categories of women

Race and category of woman	Estimate	Variance	Standard error	Coefficient of variation
Black				
Contraceptor	2,420,093	3,412,625,930	58,418	0.0241
Surgical contraceptor	448,770	682,522,083	26,125	0.0582
Nonsurgical contraceptor	1,971,323	3,068,381,090	55,393	0.0281
Had family planning visit within last 3 years	1,724,061	3,725,432,750	61,036	0.0354
Last family planning visit to own doctor	873,936	3,049,227,450	55,220	0.0632
Last family planning visit not to own doctor	732,971	3,410,762,670	58,402	0.0797
Breast fed first child for any duration	1,004,225	2,370,040,260	48,683	0.0485
Breast fed first child for more than 3 months	506,477	1,204,912,190	34,712	0.0685
Last live birth wanted, then or sooner	1,621,007	3,479,479,740	58,987	0.0364
Last live birth wanted, but later	1,040,170	5,176,962,610	71,951	0.0692
Last live birth unwanted	1,095,760	2,662,811,960	51,602	0.0471
First child born within first year of marriage	960,006	2,381,227,750	48,798	0.0508
Premarital conception	659,907	2,110,942,500	45,945	0.0696
White and other				
Contraceptor	19,588,131	40,609,652,600	201,518	0.0103
Surgical contraceptor	5,291,340	53,530,084,200	231,366	0.0437
Nonsurgical contraceptor	14,296,791	52,746,445,800	229,666	0.0161
Had family planning visit within last 3 years	14,048,920	50,224,706,100	224,109	0.0160
Last family planning visit to own doctor	11,340,357	46,314,146,000	215,207	0.0190
Last family planning visit not to own doctor	2,115,512	14,661,685,900	121,085	0.0572
Breast fed first child for any duration	8,559,417	64,122,743,700	253,225	0.0296
Breast fed first child for more than 3 months	3,885,143	38,126,388,100	195,260	0.0503
Last live birth wanted, then or sooner	14,457,698	80,367,974,800	283,492	0.0196
Last live birth wanted, but later	5,196,351	48,889,710,300	221,110	0.0426
Last live birth unwanted	3,283,521	27,654,888,100	166,298	0.0506
First child born within first year of marriage	8,161,022	35,522,609,200	188,474	0.0231
Premarital conception	4,132,640	31,363,390,400	177,097	0.0429

The percent of the total variance attributable to each component is presented in table V. (Note that some estimates are negative.) Average values calculated separately for the black and for the white and other races subgroups are presented in table VI.

Design effects were estimated for each item as

$$\text{DEFF} = \frac{\hat{\sigma}^2 \text{ within segment}}{(\text{Estimated total})^2} \div \frac{Q}{nP}$$

$$= \frac{\text{Cycle II relvariance}}{\text{Relvariance under simple random sampling}}$$

where n = effective sample size from the subpopulation (black or white and other eligible women)

P = estimated total expressed as a proportion of the subpopulation total

$Q = 1 - P$

(All the items for which variances were calculated are counts of women, and thus Q/nP is the variance estimator with simple random sampling.) These variables are presented item by item in table VII. Averages by race of DEFF's are given in table VIII.

As described earlier, ratio adjusting of sampling weights was performed only on the ever married group. In Cycle II of NSFG, the only never married women eligible for interviewing were those who had children and whose children lived with them in their home. There was no accepted count of these women nationally, so their estimated total in Cycle II could not be benchmarked.

In Cycle IV of NSFG, all women between the ages of 15 and 44 will be considered eligible without regard to marital status as was the case in Cycle III. Because there are variance implications associated within ratio adjusting and because in Cycle IV adjustments will be made for both the ever married and never married groups, the variance calculations were repeated and the weights were ratio adjusted within each replicate for both ever married and never married women. The never married group was benchmarked to the full dataset estimates by race and age class. The revised figures can be found in tables IX–XV. These variances most closely reflect the sampling and estimation procedure to be used in Cycle IV and are the ones used in the analyses.

While the estimated average design effect for both racial groups showed some reduction relative to the earlier estimates, the most dramatic change occurred in the estimated components of variance for the black subgroup. There the proportion of the total variance contributed by the between-PSU component dropped from 13.1 to 0.7 percent.

Table V. Cycle II family growth data components of variance by race and categories of women

Race and category of woman	Estimate	Variance	Between PSU	Between segment	Within segment
Black					
Contraceptor	2,420,093	8,488,948,530	0.0827	0.5153	0.4020
Surgical contraceptor	448,770	2,031,145,030	0.5442	0.1198	0.3360
Nonsurgical contraceptor	1,971,323	6,507,505,590	0.1958	0.3327	0.4715
Had family planning visit within last 3 years	1,724,061	6,739,469,230	0.2873	0.1600	0.5528
Last family planning visit to own doctor	873,936	5,340,103,050	0.2929	0.1361	0.5710
Last family planning visit not to own doctor	732,971	5,476,464,200	0.3834	−0.0062	0.6228
Breast fed first child for any duration	1,004,225	3,820,354,840	0.1411	0.2385	0.6204
Breast fed first child for more than 3 months	506,477	2,422,454,340	−0.0130	0.5156	0.4974
Last live birth wanted, then or sooner	1,621,007	3,636,218,360	0.1413	−0.0982	0.9569
Last live birth wanted, but later	1,040,170	3,370,875,700	−0.7149	0.1791	1.5358
Last live birth unwanted	1,095,760	2,575,684,460	−0.0638	0.0300	1.0338
First child born within first year of marriage	960,006	4,145,065,460	0.2446	0.1809	0.5745
Premarital conception	659,907	1,727,495,450	0.1870	−0.4089	1.2220
White and other					
Contraceptor	19,588,131	87,681,683,800	0.1860	0.3509	0.4631
Surgical contraceptor	5,291,340	78,435,283,200	0.4541	−0.1366	0.6825
Nonsurgical contraceptor	14,296,791	147,818,901,000	0.2090	0.4342	0.3568
Had family planning visit within last 3 years	14,048,920	116,884,028,000	0.5950	−0.0247	0.4297
Last family planning visit to own doctor	11,340,357	130,425,806,000	0.5758	0.0691	0.3551
Last family planning visit not to own doctor	2,115,512	27,960,381,600	−0.0670	0.5426	0.5244
Breast fed first child for any duration	8,559,417	80,478,878,300	0.3011	−0.0979	0.7968
Breast fed first child for more than 3 months	3,885,143	38,372,987,700	0.2335	−0.2271	0.9936
Last live birth wanted, then or sooner	14,457,698	90,793,742,000	0.5358	−0.4210	0.8852
Last live birth wanted, but later	5,196,351	58,795,618,700	0.2266	−0.0581	0.8315
Last live birth unwanted	3,283,521	25,776,525,600	−0.2259	0.1530	1.0729
First child born within first year of marriage	8,161,022	72,369,758,400	−0.4286	0.9378	0.4908
Premarital conception	4,132,640	44,789,841,600	−0.1349	0.4347	0.7002

Table VI. Cycle II family growth data components of variance by race

Race of woman and variable	Mean	Race of woman and variable	Mean
Black		White and other	
Between PSU	0.13140472	Between PSU	0.18926525
Between segment	0.14576017	Between segment	0.15053413
Within segment	0.72283512	Within segment	0.66020062

Table VII. Cycle II family growth data DEFF's by race and categories of women

Race and category of woman	*Estimated total*	*Base*	n	*Within segment*	*Relvariance under simple random sampling*	*Cycle II relvariance*	*DEFF*
Black							
Contraceptor	2,420,093	4,144,464	2,164	3,412,625,930	0.0003	0.0006	1.7696
Surgical contraceptor	448,770	4,144,464	2,164	682,522,083	0.0038	0.0034	0.8905
Nonsurgical contraceptor	1,971,323	4,144,464	2,164	3,068,381,090	0.0005	0.0008	1.5500
Had family planning visit within last 3 years	1,724,061	4,144,464	2,164	3,725,432,750	0.0006	0.0013	1.9319
Last family planning visit to own doctor	873,936	4,144,464	2,164	3,049,227,450	0.0017	0.0040	2.3086
Last family planning visit not to own doctor	732,971	4,144,464	2,164	3,410,762,670	0.0022	0.0063	2.9517
Breast fed first child for any duration	1,004,225	4,144,464	2,164	2,370,040,260	0.0014	0.0024	1.6264
Breast fed first child for more than 3 months	506,477	4,144,464	2,164	1,204,912,190	0.0033	0.0047	1.4151
Last live birth wanted, then or sooner	1,621,007	4,144,464	2,164	3,479,479,740	0.0007	0.0013	1.8407
Last live birth wanted, but later	1,040,170	4,144,464	2,164	5,176,962,610	0.0014	0.0048	3.4695
Last live birth unwanted	1,095,760	4,144,464	2,164	2,662,811,960	0.0013	0.0022	1.7249
First child born within first year of marriage	960,006	4,144,464	2,164	2,381,227,750	0.0015	0.0026	1.6856
Premarital conception	659,907	4,144,464	2,164	2,110,942,500	0.0024	0.0048	1.9866
White and other							
Contraceptor	19,588,131	28,891,218	3,790	40,609,652,600	0.0001	0.0001	0.8446
Surgical contraceptor	5,291,340	28,891,218	3,790	53,530,084,200	0.0012	0.0019	1.6247
Nonsurgical contraceptor	14,296,791	28,891,218	3,790	52,746,445,800	0.0003	0.0003	0.9581
Had family planning visit within last 3 years	14,048,920	28,891,218	3,790	50,224,706,100	0.0003	0.0003	0.9129
Last family planning visit to own doctor	11,340,357	28,891,218	3,790	46,314,146,000	0.0004	0.0004	0.8819
Last family planning visit not to own doctor	2,115,512	28,891,218	3,790	14,661,685,900	0.0033	0.0033	0.9810
Breast fed first child for any duration	8,559,417	28,891,218	3,790	64,122,743,700	0.0006	0.0009	1.3965
Breast fed first child for more than 3 months	3,885,143	28,891,218	3,790	38,126,388,100	0.0017	0.0025	1.4873
Last live birth wanted, then or sooner	14,457,698	28,891,218	3,790	80,367,974,800	0.0003	0.0004	1.4597
Last live birth wanted, but later	5,196,351	28,891,218	3,790	48,889,710,300	0.0012	0.0018	1.5049
Last live birth unwanted	3,283,521	28,891,218	3,790	27,654,888,100	0.0021	0.0026	1.2465
First child born within first year of marriage	8,161,022	28,891,218	3,790	35,522,609,200	0.0007	0.0005	0.7958
Premarital conception	4,132,640	28,891,218	3,790	31,363,390,400	0.0016	0.0018	1.1617

Table VIII. Cycle II family growth data mean DEFF's by race

Race of woman	*DEFF*
Black	1.93470594
White and other	1.17350327

Table IX. Cycle II family growth variance estimates between and within PSU by race and categories of women

[Never married women forced to fixed total]

Race and category of woman	*Estimate*	*Variance*	*Standard error*	*Coefficient of variation*
Black				
Contraceptor	2,420,093	4,516,232,360	67,203	0.0278
Surgical contraceptor	448,770	2,004,473,540	44,771	0.0998
Nonsurgical contraceptor	1,971,323	3,415,714,360	58,444	0.0296
Had family planning visit within last 3 years	1,724,061	2,686,936,320	51,836	0.0301
Last family planning visit to own doctor	873,936	4,800,288,060	69,284	0.0793
Last family planning visit not to own doctor	732,971	3,993,605,650	63,195	0.0862
Breast fed first child for any duration	1,004,225	3,763,437,140	61,347	0.0611
Breast fed first child for more than 3 months	506,477	2,426,146,010	49,256	0.0973
Last live birth wanted, then or sooner	1,621,007	3,780,847,770	61,489	0.0379
Last live birth wanted, but later	1,040,170	2,451,591,340	49,514	0.0476
Last live birth unwanted	1,095,760	1,850,537,520	43,018	0.0393
First child born within first year of marriage	960,006	4,145,065,460	64,382	0.0671
Premarital conception	659,907	1,727,495,450	41,563	0.0630
White and other				
Contraceptor	19,588,131	95,751,713,700	309,438	0.0158
Surgical contraceptor	5,291,340	78,083,948,300	279,435	0.0528
Nonsurgical contraceptor	14,296,791	158,745,583,000	398,429	0.0279
Had family planning visit within last 3 years	14,048,920	125,502,539,000	354,263	0.0252
Last family planning visit to own doctor	11,340,357	132,831,276,000	364,460	0.0321
Last family planning visit not to own doctor	2,115,512	29,032,111,000	170,388	0.0805
Breast fed first child for any duration	8,559,417	77,039,281,400	277,560	0.0324
Breast fed first child for more than 3 months	3,885,143	36,729,640,200	191,650	0.0493
Last live birth wanted, then or sooner	14,457,698	88,573,985,100	297,614	0.0206
Last live birth wanted, but later	5,196,351	56,225,567,600	237,119	0.0456
Last live birth unwanted	3,283,521	24,888,345,400	157,760	0.0480
First child born within first year of marriage	8,161,022	72,369,758,400	269,016	0.0330
Premarital conception	4,132,640	44,789,841,600	211,636	0.0512

Table X. Cycle II family growth variance estimates within PSU by race and categories of women

[Never married women forced to fixed total]

Race and category of woman	*Estimate*	*Variance*	*Standard error*	*Coefficient of variation*
Black				
Contraceptor	2,420,093	5,159,636,950	71,831	0.0297
Surgical contraceptor	448,770	844,823,872	29,066	0.0648
Nonsurgical contraceptor	1,971,323	3,748,930,430	61,229	0.0311
Had family planning visit within last 3 years	1,724,061	3,903,121,880	62,475	0.0362
Last family planning visit to own doctor	873,936	3,487,773,580	59,057	0.0676
Last family planning visit not to own doctor	732,971	3,104,228,040	55,716	0.0760
Breast fed first child for any duration	1,004,225	3,315,059,750	57,577	0.0573
Breast fed first child for more than 3 months	506,477	2,427,483,540	49,269	0.0973
Last live birth wanted, then or sooner	1,621,007	2,659,492,080	51,570	0.0318
Last live birth wanted, but later	1,040,170	4,967,963,800	70,484	0.0678
Last live birth unwanted	1,095,760	2,044,526,360	45,216	0.0413
First child born within first year of marriage	960,006	3,131,131,450	55,957	0.0583
Premarital conception	659,907	1,404,540,040	37,477	0.0568
White and other				
Contraceptor	19,588,131	79,155,147,200	281,345	0.0144
Surgical contraceptor	5,291,340	42,272,158,500	205,602	0.0389
Nonsurgical contraceptor	14,296,791	119,739,680,000	346,034	0.0242
Had family planning visit within last 3 years	14,048,920	41,347,863,800	203,342	0.0145
Last family planning visit to own doctor	11,340,357	53,762,263,200	231,867	0.0204
Last family planning visit not to own doctor	2,115,512	28,310,826,900	168,258	0.0795
Breast fed first child for any duration	8,559,417	53,231,064,100	230,719	0.0270
Breast fed first child for more than 3 months	3,885,143	28,630,347,800	169,205	0.0436
Last live birth wanted, then or sooner	14,457,698	43,494,391,800	208,553	0.0144
Last live birth wanted, but later	5,196,351	45,284,049,800	212,800	0.0410
Last live birth unwanted	3,283,521	31,274,608,700	176,846	0.0539
First child born within first year of marriage	8,161,022	103,389,650,000	321,543	0.0394
Premarital conception	4,132,640	50,833,454,800	225,463	0.0546

Table XI. Cycle II family growth variance estimates within segment by race and categories of women

[Never married women forced to fixed total]

Race and category of woman	Estimate	Variance	Standard error	Coefficient of variation
Black				
Contraceptor	2,420,093	3,342,992,790	57,819	0.0239
Surgical contraceptor	448,770	711,714,981	26,678	0.0594
Nonsurgical contraceptor	1,971,323	2,827,500,300	53,174	0.0270
Had family planning visit within last 3 years	1,724,061	3,571,004,210	59,758	0.0347
Last family planning visit to own doctor	873,936	3,088,666,370	55,576	0.0636
Last family planning visit not to own doctor	732,971	3,188,446,300	56,466	0.0770
Breast fed first child for any duration	1,004,225	2,155,316,810	46,425	0.0462
Breast fed first child for more than 3 months	506,477	1,065,358,850	32,640	0.0644
Last live birth wanted, then or sooner	1,621,007	3,127,954,880	55,928	0.0345
Last live birth wanted, but later	1,040,170	4,103,678,480	64,060	0.0616
Last live birth unwanted	1,095,760	2,899,132,390	53,844	0.0491
First child born within first year of marriage	960,006	2,381,227,750	48,798	0.0508
Premarital conception	659,907	2,110,942,500	45,945	0.0696
White and other				
Contraceptor	19,588,131	40,857,676,400	202,133	0.0103
Surgical contraceptor	5,291,340	52,396,704,800	228,903	0.0433
Nonsurgical contraceptor	14,296,791	53,350,409,400	230,977	0.0162
Had family planning visit within last 3 years	14,048,920	47,757,689,000	218,535	0.0156
Last family planning visit to own doctor	11,340,357	45,803,570,000	214,018	0.0189
Last family planning visit not to own doctor	2,115,512	14,749,006,000	121,445	0.0574
Breast fed first child for any duration	8,559,417	66,834,192,700	258,523	0.0302
Breast fed first child for more than 3 months	3,885,143	38,629,152,300	196,543	0.0506
Last live birth wanted, then or sooner	14,457,698	77,281,034,900	277,995	0.0192
Last live birth wanted, but later	5,196,351	40,196,187,900	200,490	0.0386
Last live birth unwanted	3,283,521	28,130,809,400	167,722	0.0511
First child born within first year of marriage	8,161,022	35,522,609,200	188,474	0.0231
Premarital conception	4,132,640	31,363,390,400	177,097	0.0429

Table XII. Cycle II family growth data components of variance by race and categories of women

[Never married women forced to fixed total]

Race and category of woman	Estimate	Variance	Between PSU	Between segment	Within segment
Black					
Contraceptor	2,420,093	4,516,232,360	−0.1425	0.4022	0.7402
Surgical contraceptor	448,770	2,004,473,540	0.5785	0.0664	0.3551
Nonsurgical contraceptor	1,971,323	3,415,714,360	−0.0976	0.2698	0.8278
Had family planning visit within last 3 years	1,724,061	2,686,936,320	−0.4526	0.1236	1.3290
Last family planning visit to own doctor	873,936	4,800,288,060	0.2734	0.0831	0.6434
Last family planning visit not to own doctor	732,971	3,993,605,650	0.2227	−0.0211	0.7984
Breast fed first child for any duration	1,004,225	3,763,437,140	0.1191	0.3082	0.5727
Breast fed first child for more than 3 months	506,477	2,426,146,010	−0.0006	0.5614	0.4391
Last live birth wanted, then or sooner	1,621,007	3,780,847,770	0.2966	−0.1239	0.8273
Last live birth wanted, but later	1,040,170	2,451,591,340	−1.0264	0.3525	1.6739
Last live birth unwanted	1,095,760	1,850,537,520	−0.1048	−0.4618	1.5666
First child born within first year of marriage	960,006	4,145,065,460	0.2446	0.1809	0.5745
Premarital conception	659,907	1,727,495,450	0.1870	−0.4089	1.2220
White and other					
Contraceptor	19,588,131	95,751,713,700	0.1733	0.4000	0.4267
Surgical contraceptor	5,291,340	78,083,948,300	0.4586	−0.1297	0.6710
Nonsurgical contraceptor	14,296,791	158,745,583,000	0.2457	0.4182	0.3361
Had family planning visit within last 3 years	14,048,920	125,502,539,000	0.6705	−0.0511	0.3805
Last family planning visit to own doctor	11,340,357	132,831,276,000	0.5953	0.0599	0.3448
Last family planning visit not to own doctor	2,115,512	29,032,111,000	0.0248	0.4671	0.5080
Breast fed first child for any duration	8,559,417	77,039,281,400	0.3090	−0.1766	0.8675
Breast fed first child for more than 3 months	3,885,143	36,729,640,200	0.2205	−0.2722	1.0517
Last live birth wanted, then or sooner	14,457,698	88,573,985,100	0.5089	−0.3815	0.8725
Last live birth wanted, but later	5,196,351	56,225,567,600	0.1946	0.0905	0.7149
Last live birth unwanted	3,283,521	24,888,345,400	−0.2566	0.1263	1.1303
First child born within first year of marriage	8,161,022	72,369,758,400	−0.4286	0.9378	0.4908
Premarital conception	4,132,640	44,789,841,600	−0.1349	0.4347	0.7002

Table XIII. Cycle II family growth data components of variance by race

[Never married women forced to fixed total]

Race of woman and variable	*Mean*	*Race of woman and variable*	*Mean*
Black		White and other	
Between PSU	0.00749961	Between PSU	0.19855842
Between segment	0.10249919	Between segment	0.14796341
Within segment	0.89000120	Within segment	0.65347817

Table XIV. Cycle II family growth data DEFF's by race and categories of women

[Never married women forced to fixed total]

Race and category of woman	*Estimate*	*Base*	n	*Within segment*	*Relvariance under simple random sampling*	*Cycle II relvariance*	*DEFF*
Black							
Contraceptor	2,420,093	4,144,464	2,164	3,342,992,790	0.0003	0.0006	1.7335
Surgical contraceptor	448,770	4,144,464	2,164	711,714,981	0.0038	0.0035	0.9286
Nonsurgical contraceptor	1,971,323	4,144,464	2,164	2,827,500,300	0.0005	0.0007	1.4283
Had family planning visit within last 3 years	1,724,061	4,144,464	2,164	3,571,004,210	0.0006	0.0012	1 8519
Last family planning visit to own doctor	873,936	4,144,464	2,164	3,088,666,370	0.0017	0.0040	2.3385
Last family planning visit not to own doctor	732,971	4,144,464	2,164	3,188,446,300	0.0022	0.0059	2.7593
Breast fed first child for any duration	1,004,225	4,144,464	2,164	2,155,316,810	0.0014	0.0021	1.4790
Breast fed first child for more than 3 months	506,477	4,144,464	2,164	1,065,358,850	0.0033	0.0042	1 2512
Last live birth wanted, then or sooner	1,621,007	4,144,464	2,164	3,127,954,880	0.0007	0.0012	1.6548
Last live birth wanted, but later	1,040,170	4,144,464	2,164	4,103,678,480	0.0014	0.0038	2.7502
Last live birth unwanted	1,095,760	4,144,464	2,164	2,899,132,390	0.0013	0.0024	1.8780
First child born within first year of marriage	960,006	4,144,464	2,164	2,381,227,750	0.0015	0.0026	1.6856
Premarital conception	659,907	4,144,464	2,164	2,110,942,500	0.0024	0.0048	1 9866
White and other							
Contraceptor	19,588,131	28,891,218	3,790	40,857,676,400	0.0001	0.0001	0.8498
Surgical contraceptor	5,291,340	28,891,218	3,790	52,396,704,800	0.0012	0.0019	1.5903
Nonsurgical contraceptor	14,296,791	28,891,218	3,790	53,350,409,400	0.0003	0.0003	0.9691
Had family planning visit within last 3 years	14,048,920	28,891,218	3,790	47,757,689,000	0.0003	0.0002	0 8680
Last family planning visit to own doctor	11,340,357	28,891,218	3,790	45,803,570,000	0.0004	0.0004	0 8722
Last family planning visit not to own doctor	2,115,512	28,891,218	3,790	14,749,006,000	0.0033	0.0033	0 9868
Breast fed first child for any duration	8,559,417	28,891,218	3,790	66,834,192,700	0.0006	0.0009	1.4555
Breast fed first child for more than 3 months	3,885,143	28,891,218	3,790	38,629,152,300	0.0017	0.0026	1.5070
Last live birth wanted, then or sooner	14,457,698	28,891,218	3,790	77,281,034,900	0.0003	0.0004	1.4036
Last live birth wanted, but later	5,196,351	28,891,218	3,790	40,196,187,900	0.0012	0.0015	1.2373
Last live birth unwanted	3,283,521	28,891,218	3,790	28,130,809,400	0.0021	0.0026	1.2680
First child born within first year of marriage	8,161,022	28,891,218	3,790	35,522,609,200	0.0007	0.0005	0.7958
Premarital conception	4,132,640	28,891,218	3,790	31,363,390,400	0.0016	0.0018	1.1617

Table XV. Cycle II family growth data mean DEFF's by race

[Never married women forced to fixed total]

Race of woman	*DEFF*
Black	1.82503382
White and other	1.15115422

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