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National Survey of Family Growth: Design, Estimation, and Inference

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This report describes the procedures used to select the sample, make national estimates, impute missing data, and estimate sampling errors for Cycle IV of the National Survey of Family Growth.

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Preface

This report presents a detailed description of the sample design, weighting procedures, variance estimation, and imputation procedures used in Cycle IV of the National Survey of Family Growth (NSFG). The survey was designed and conducted by Westat, Inc., of Rockville, Maryland, under a contractual arrangement with the National Center for Health Statistics (NCHS). The sample design was developed under the supervision of Joseph Waksberg of Westat, in cooperation with Dr. Owen Thornberry, Director of the Division of Health Interview Statistics of NCHS, and Dr. William F. Pratt, Chief of Family Growth Survey Branch and Project Officer for Cycle IV of the NSFG.

Some of this report is based on survey specification documents prepared by Westat, Inc., and on internal NCHS memoranda.

Dr. Christine A. Bachrach of the National Institute of Child Health and Human Development (NICHHD) and Thomas F. Moore of the Statistical Methods Division, U.S. Bureau of the Census, served as peer reviewers of this report and made many useful comments and suggestions.

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National Survey of Family Growth: Design, Estimation, and Inference

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Summary

The purpose of this report is to document the procedures used in the 1988 National Survey of Family Growth (NSFG) to select the sample, weight the data to produce national estimates, impute missing data, and estimate sampling errors. Therefore, this report necessarily contains a great deal of technical detail. For readers who do not need this level of detail, this summary briefly describes the procedures used.

The National Survey of Family Growth is conducted every few years by the National Center for Health Statistics (NCHS), a part of the U.S. Department of Health and Human Services. The purpose of the survey is to collect and publish data from a national sample of women on childbearing, factors affecting childbearing (such as contraception, sterilization, and infertility), and related aspects of maternal and infant health. Interviewing for Cycle IV of the survey was done in 1988 by Westat, Inc., under a contract with NCHS.

Personal interviews were conducted between January and August of 1988 with a national sample of 8,450 women in the civilian noninstitutionalized population of the United States. Interviews were conducted in person by trained female interviewers and lasted an average of 70 minutes. The interview focused on the woman's pregnancies, if any; her use of contraception; her ability to bear children (fecundity and infertility); her use of medical services for family planning, infertility, and prenatal care; her marriage and cohabitation history, if any; and a wide range of demographic and economic characteristics.

This report describes some of the main methodological aspects of the survey, including the sample design, weighting, sampling errors, and imputation of missing data. These topics will be described briefly and less technically in this summary. Each topic is discussed in more detail in the rest of the report.

Sample design

The 8,450 women interviewed for the NSFG were drawn from households in which someone had been interviewed for another NCHS survey, the National Health Interview Survey (NHIS), between October 1985 and March 1987. Women were sampled from 156 areas, called primary sampling units or PSU's. A PSU is a county or group of adjacent counties. The sampled PSU's were located in nearly every State and included all of the largest metropolitan areas in the United States. If the woman selected for the NSFG had moved since the NHIS interview, she was tracked to her new address and interviewed there.

Different numbers of women were available for the NHIS in 1985, 1986, and 1987, so the sampling procedure differed somewhat in each of these years. The NSFG sampling plan was designed to increase the reliability of data for black women by oversampling them and to increase the reliability of data for women who are not black by reducing the variations in their sampling rates. The NSFG sampled only one woman per household, even if more than one eligibile woman lived there. The sampled woman was selected in the following way: one black woman was selected for the NSFG from each of the households containing one or more black women interviewed in the NHIS from the last quarter of 1985 through the first quarter of 1987; of the women who were not black, one was selected per household from a much smaller proportion of households interviewed in the NHIS from the first quarter of 1986 through the first quarter of 1987.

A simple random sample of women 15–44 years of age in the United States would mean that every woman 15–44 years of age would have the same chance of being selected for the sample, regardless of her characteristics or where she lived. The NSFG sample is not a simple random sample for two reasons: Only some areas were chosen (by probability selection) to be in the sample and, within areas included in the sample, women were sampled at different rates. For example, black women were sampled for the NSFG at a higher rate than other women, so that reliable statistics could be produced for them. As a result, interviews were completed with 2,771 black women and 5,679 women of other races. Certain other women (described later in the report) were also sampled at higher rates. Therefore, the NSFG data must be weighted, and estimates of sampling errors should be made using the techniques discussed in this report.

The NHIS response rate was 96 percent. Of the sampled women selected from responding NHIS households for the NSFG, the NSFG simple response rate was 80 percent. However, this rate does not take into account the subsampling for nonresponse (described later in this report), which was a part of the intensive followup (the last stage of interviewing). NCHS prefers to take this subsampling into account when calculating the response rates. When this is taken into account, the NSFG response rate is 82 percent of the women in the NHIS. Thus, the total response rate is 96 percent times 82 percent, or about 79 percent.

Weighting

The NSFG is intended to produce national estimates of the number as well as the percent of women with certain characteristics—such as the number using the Pill, the number who are infertile, or the number who used family planning services in the last year. In order to produce these national estimates, each woman interviewed was given a sampling weight, which is the number of women in the population that she represents. The weights were determined in four main stages: The first stage was to determine the baseweight, which is the reciprocal of the probability that the woman was selected for the sample. For example, if the probability that a certain woman was selected was 1 out of 6,000, her baseweight would be 6,000. The second stage of weighting was trimming where cases with very large weights were reduced to a maximum value. The third stage of weighting was an adjustment for nonresponse, because certain categories of women were less likely than others to be found at home and interviewed. The fourth stage of weighting was poststratification, where NSFG totals were adjusted to independent estimates of women by age, race, marital status, and parity (the number of live births the woman has had) obtained from the U.S. Bureau of the Census.

Imputation

In any survey, not every question is answered by every person interviewed. Sometimes a respondent cannot remember the fact asked for in a question; occasionally the respondent may refuse to answer. Sometimes the interviewer will skip a question by mistake or forget to write down the answer. Such missing data create inconsistencies in estimates, which may be confusing for some users of the data. Filling in answers for these missing items is called "imputation"; imputation makes the data complete, more consistent, and easier to use. About 200 variables (only a small fraction of the total number used in the survey) were imputed. The percent of cases with imputed data is under 1 percent for most of the imputed variables, and it never exceeds 11 percent. The techniques used for imputation in the 1988 NSFG were "hot-deck imputation" (used most), imputation using models, and imputation by judgment (used least). On the public-use computer file of the NSFG data, variables with imputed data are identified, and the type of imputation used is also shown. Furthermore, the imputed variables are constructed variables (recodes), and all of the raw data may be found in their unimputed forms.

Variance estimation

Variance is a measure of how much a statistic (such as a percent or an average) can vary in different samples. In a simple random sample, the probability that a person will be selected for the sample is the same for all persons in the population. However, the NSFG was not based on a simple random sample; the probabilities of selection varied by area, by race, and by other variables. Therefore, variance estimates in many standard computer packages and many statistics textbooks will be too small when applied to the NSFG. The authors recommend that researchers use the weights, variance formulas, and variance estimation techniques described in this report to estimate sampling errors for the NSFG.

Variances for the NSFG were estimated using a technique called "balanced repeated replication" (BRR). This procedure estimates the standard error for survey estimates (such as percents or numbers) using parts of the whole sample. In the NSFG, tables were designed using a number of dependent and independent variables. Sampling errors were calculated for the weighted numbers in these tables. The ratios of the variances to the squares of the weighted numbers were plotted against the inverse of the weighted numbers, and a weighted least-squares line was fitted to those points. The intercept and slope for these lines are given in this report; they can be used to estimate the standard errors of percents and weighted numbers from the NSFG. If researchers wish to estimate variances for other statistics, such as averages or regression coefficients, they can use BRR, as described in this report.

The rest of this report covers sample design, weighting, imputation, and estimating sampling errors in more detail.

Background

The National Survey of Family Growth was established in 1971 by the National Center for Health Statistics, Division of Vital Statistics. The purpose of the survey is to provide current information on childbearing, factors affecting childbearing (such as contraception, sterilization, and infertility), and related aspects of maternal and child health. It is a periodic survey, conducted every few years. The first cycle was conducted in 1973, the second in 1976, the third in 1982, and the fourth in 1988.

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 or previously married. The only never-married women interviewed in Cycles I and II were those never-married mothers with offspring living with them at the time of interview. These women constitute only a small proportion of all never-married women.

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 represented the civilian noninstitutionalized population of women 15–44 years of age in the conterminous United States. This target population was expanded for the Cycle IV survey to include Alaska and Hawaii.

Data for all four cycles were collected from probability samples of women by means of personal interviews. For the first three cycles, the interviews lasted an average of 1 hour. For Cycle IV, the average interview was 70 minutes. The interviews provided information on fertility trends and differentials, contraception, breast feeding, family planning services, and other aspects of maternal and child health closely related to childbearing. Questions regarding knowledge of acquired immunodeficiency syndrome (AIDS), chlamydia, and genital herpes are among the new items in Cycle IV.

The sample design and data collection for Cycle I were done contractually by the National Opinion Research Corporation of the University of Chicago (1); and those for Cycles II through IV were done under contract by Westat, Inc., of Rockville, Maryland (2–4). Cycle IV is based on interviews with 8,450 women. The interviews were conducted between mid-January and mid-August of 1988. This report describes the sample design used to select the women, the techniques used to estimate population parameters (including weighting and imputation), and the procedures used to estimate sampling variances.

Design specifications

Efficient sample design must take into account the primary survey objectives, the available funds, logistical problems, time limitations, the size and characteristics of the population under study, and the costs of various design features. NCHS chose to select a subsample of the women that had been previously interviewed for another survey, the National Health Interview Survey (NHIS). The primary specifications for the National Survey of Family Growth (NSFG) were

● The target population was defined to be noninstitutionalized women 15–44 years of age who were living in households or group quarters in the United States, including Alaska and Hawaii. Women in the military and those confined to institutions such as prisons and mental hospitals were specifically excluded.

● Completed interviews were to be obtained from approximately 8,500 women, selected from households previously interviewed for the NHIS. It was to include about 2,800 black women, many more than could be expected by chance in a sample of 8,500. No more than one randomly selected eligible woman per household was to be interviewed.

● Data were to be collected from the sample women by means of personal interviews lasting an average of 70 minutes. No proxy interviews were to be accepted.

● All interviewers were to be female.

● The interviewer was to collect information on fertility, sexual experience and contraceptive use, sources and types of family planning services, knowledge of AIDS, and related aspects of maternal and child health by using a highly structured, printed questionnaire.

● The target interview completion rate was to be 85.0 percent among those who had already completed the NHIS. This meant achieving an overall completion rate of 81.6 percent, taking into account the fact that the NHIS had a completion rate of about 96 percent. Furthermore, these response rates were to be achieved both for black women and women of other races.

● The interviewing was to be completed in approximately 6 months.

● The contractor, in cooperation with NCHS, was to design and implement procedures to measure and control the quality of data collection and data preparation.

Sample design

Summary

The sample design for Cycle IV of the National Survey of Family Growth (NSFG) was a subsample of women whose households had participated in the National Health Interview Survey (NHIS). The NHIS is a continuous survey of the civilian noninstitutionalized population of the United States. When the full NHIS sample can be used, interviews are obtained at 47,600 housing units each year in a fixed set of 198 areas; some of these are metropolitan areas and others are clusters of nonmetropolitan counties. Data are collected for each household member on disabilities, health conditions, doctor visits, hospitalizations, and other health related topics. A new sample of households is interviewed each year.

NCHS provided computer files to Westat, Inc., of households that participated in the NHIS, together with address information, rosters, and some basic demographic data on household members. Households were included that had been interviewed for the NHIS any time between the fourth quarter of 1985 and the first quarter of 1987, inclusively. From these, Westat selected the NSFG sample. Households were drawn from 156 of the 198 primary sampling units (PSU's) in the NHIS design. In comparison, Cycle III was confined to 79 PSU's. Spreading the sample across more PSU's resulted in smaller sampling errors.

No more than one woman was selected per household. Interviewers attempted to locate the selected women, following them to new addresses, if necessary. After locating a sampled woman, the interviewer conducted a brief "screener" interview to confirm that she was the sampled woman and that she was eligible for the NSFG.

Design of the National Health Interview Survey

The NHIS sample design was redesigned in 1985 (5). As a result, it became possible for NCHS to transmit data on the NHIS sample households to private contractors for use in conducting followup surveys. These followup surveys are then said to be linked to the NHIS. The confidentiality of the transmitted data is protected under section 308(d) of the Public Health Service Act.

The NHIS sample is restricted to 198 county or multicounty PSU's. These sample PSU's were selected from a much larger set of PSU's (that covers the United States), using a stratified probability design. This means that the PSU's were grouped prior to selection to ensure that the selected PSU's would be broadly representative in terms of several demographic and economic characteristics. Some of these PSU's are so populous that they were included in the sample with certainty. These are called self-representing (SR) PSU's. There are 52 SR PSU's in the full NHIS design. The remaining 146 PSU's had a chance of not being selected. The selected PSU's represent both themselves and other PSU's that were not selected. Hence, they are called non-self-representing (NSR) PSU's.

To allow flexibility to conduct the survey with any of several different sample sizes, the PSU's are divided into four panels, each of which can be used to represent the Nation, if need be. The very largest SR PSU's are in all four panels. Medium-sized SR PSU's are in two panels. There are 62 PSU's in a single panel sample, 112 PSU's in a two panel sample, 156 PSU's in a three panel sample, and 198 PSU's in the full design.

Within each sample PSU, a sample of blocks (or small groups of blocks) was selected. In PSU's with between a 5- and 50-percent black population, blocks in enumeration districts (ED's) with high black populations were selected with a higher probability than other blocks. Within each block or blocks, a cluster of an expected eight housing units was selected. These housing units were spread as evenly throughout the block as possible.

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

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

Selecting the National Survey of Family Growth sample

Women interviewed in the National Survey of Family Growth were from households in which someone had been interviewed in the National Health Interview Survey. The procedure for selecting the NSFG sample from the NHIS sample was complex. In this section, factors motivating the design and the design features themselves are described in tandem. For those more interested in any effects of the design than in motivating factors, note that in some PSU's only black women were selected, neighborhood clusters of black women tend to be larger than clusters of other women, and households containing more than one eligible woman who was not black were selected at a higher rate than households containing just one. This last point reduces considerably the variability in the weights of women who are not black. The weights of black women vary more than the weights of other women.

The NSFG sample was drawn from women whose households had participated in the NHIS in the fourth quarter of 1985, any time during 1986, or in the first quarter of 1987. Because of variations in the level of funding for the NHIS, the 1985 NHIS sample was restricted to three panels (156 PSU's), and the 1986 NHIS sample to just two panels (112 PSU's). Funding was augmented for 1987; therefore, the 1987 NHIS sample is used in all 198 PSU's of the full NHIS design. Unfortunately, even combining all six of the available quarters did not provide as many black women as were selected for Cycle III of the NSFG. (However, even though fewer black women were selected in Cycle IV than in Cycle III, estimates for black women in Cycle IV have smaller sampling errors than those in Cycle III.) The decision was thus made to select as many black women as possible, subject to the restraint of selecting just one woman per household. The only black women who were not selected were those who resided in the 42 PSU's that were used by NCHS only in 1987. It was judged that the travel costs per completed interview would have been too high to interview the women in these PSU's.

Combining all six of the available quarters provided many more women who were not black than were required. In deciding how to subsample, the general preference was to take the most recently interviewed, because they would be the least likely to have moved since they were interviewed in the NHIS. It appeared, however, that the household information from the first quarter of 1987 might not be available for sampling in enough time; therefore, an initial decision was made to restrict the sample of women who were not black to those sampled in 1986. Subsequently, timing ceased to be as tight and more funding was made available; therefore, the sample of women who were not black was expanded to include some women from the first quarter of 1987.

The first step was to select households; the second was to select women from those households. In households with one woman eligible for the NSFG, that woman was selected with certainty. In households with two women eligible for the NSFG, only one was selected, so their within-household probability of selection was only one-half; if there were three eligible women, only one was selected, so their within-household probability of selection was only one-third. For example, if the overall probability of selecting a household in an area was about 1 in 5,000, for a household with only one eligible woman, the probability of selecting that woman is 1 in 5,000; for a household with two eligible women, the probability of selecting either woman is 1 in 10,000. This increases the variation in the probabilities of selection. To reduce this variability, households with two or more eligible women who were not black were oversampled, and households with only one eligible woman who was not black were undersampled. This disproportionate sampling of households that were not black was accomplished mainly by selecting households with more than one eligible woman for all 52 weeks of the 1986 NHIS and by selecting households with exactly one eligible woman from only 30 of those 52 weeks. Rules for selection of households are summarized in table A and listed below:

- All NHIS sample households in the 156 PSU's used in the fourth quarter of 1985 containing one or more eligible black women were selected.
- All NHIS sample households from 1986 containing one or more eligible black women were selected.
- All NHIS sample households from the first quarter of 1987 containing one or more eligible black women were selected if the women lived in one of the same 156 PSU's used in the fourth quarter of 1985.
- All NHIS sample households from 1986 containing more than one eligible woman who was not black were selected.
- Households from the 1986 NHIS with only one eligible woman who was not black were selected from 30 of the 52 weeks of 1986.

Table A. Rules for selecting households from the National Health Interview Survey (NHIS) sample for the National Survey of Family Growth, by year and quarter interviewed in the NHIS and race and number of eligible women living in the NHIS household: 1988 National Survey of Family Growth

Race and number of eligible women living in the NHIS household	Year and quarter interviewed		
	1985 fourth quarter	1986 all quarters	1987 first quarter
	Households selected for the NSFG		
Black			
At least 1	All in 156 PSU's	All in 112 PSU's	All in 156 PSU's
Other than black			
Exactly 1	None	30 weeks of every 52 in 112 PSU's	1 week of every 13 in 112 PSU's
Exactly 2	None	All in 112 PSU's	12 weeks of every 13 in 112 PSU's
At least 3	None	All in 112 PSU's	All in 112 PSU's

NOTE: PSU's are primary sampling units.

- A few of the NHIS sample households (1 week out of 13) in two of the available panels from the first quarter of 1987 containing exactly one eligible woman who was not black but no eligible black women were selected.
- Households assigned to 12 out of the 13 weekly NHIS subsamples in two of the available panels from the first quarter of 1987 containing exactly two eligible women who were not black but no eligible black women were selected.
- All NHIS sample households in two of the available panels from the first quarter of 1987 containing three or more eligible women who were not black but no eligible black women were selected.

Within a given household, all eligible women had the same probability of selection. (The probability of selection was simply one over the number of eligible women.) Eligibility was defined in terms of exact age on March 15, 1988. A woman had to be 15–44 years of age on that date. (There was one minor exception to these rules. Within multiracial households selected from the last quarter of 1985 and the first quarter of 1987, only black women had a chance of selection. Each of the black women in such a household had the same probability of selection.)

Field adjustments

There were rare instances where the "sampled woman" was younger than 15 years of age, older than 44 years of age, or turned out to be male. (NHIS age and sex information were imputed if missing, causing some errors. Even where the data had not been imputed, other errors were found.) In these cases, the interviewer selected from among other eligible women then residing in the household. If there were no other eligible women, the case was dropped.

Subsampling for nonresponse followup

After all efforts to complete an interview were exhausted by local interviewers, a 50-percent subsample of all nonresponse cases was selected. The subsampling, which was designed to reduce interview costs, was accomplished in two ways. In six large-city PSU's, where there were large numbers of nonresponse cases, all nonresponse cases were sequenced by an identification number and a systematic sample of half of them was drawn. The remaining PSU's were sequenced in descending order by the number of nonresponding cases they contained. A 50-percent sample of these PSU's was selected systematically. Among the *selected* cases, those that appeared to be convertible were assigned to a corps of traveling interviewers and assistant supervisors for intensive followup. (Note that the subsampling was done *before* cases were screened for convertibility.)

Prior to the followup, the response rate was 77.9 percent. Of the 8,450 final respondents, 220 were obtained as a result of the nonresponse followup. Counting each of these 220 interviews twice (because each woman represents herself and one other woman) boosts the response rate from an unadjusted 80.0 percent (8,450/10,561) to an effective response rate of 82.1 percent ((8,450 + 220)/10,561).

Characteristics of the sample

Designated sample sizes and probabilities of selection

Table B shows the number of cases that were selected from the NHIS, by race and number of eligible women in the household; it also shows estimated numbers of such women in the Nation as a whole, average probabilities of selection, and average weights. Note that the probability of selection for black women is much higher than for other women. This is because of the deliberate oversampling of black women. Also note that the probability of selection for women in large households is lower than for women in smaller households. This is because of the selection of only one woman from each household, even where several are eligible. Lastly, for sampling, note that race was taken as reported for the NHIS and was thus subject to revision as a result of the NSFG interview.

Table B. Designated sample sizes, population sizes, probability of selection, and average weights, by race and number of eligible women: 1988 National Survey of Family Growth

Race and number of eligible women	Designated sample size	Population	Probability of selection	Average weight
		Number		
All women 15–44 years of age	10,694	57,900,000	0.00018470	5,414.25
Race				
Black.	3,566	7,679,000	0.00046438	2,153.39
Other than black.	7,128	50,221,000	0.00014193	7,045.59
Number of eligible women				
1	7,585	38,461,537	0.00019721	5,070.74
2	2,517	14,478,181	0.00017385	5,752.16
3	515	4,052,679	0.00012708	7,869.28
4	68	704,910	0.00009647	10,366.33
5 or more	9	202,693	0.00004440	22,521.39

Response rates

When calculating response rates, the nonresponse subsampling procedure must be taken into account. If this procedure is not taken into account, the numerator for the response rate is just the number of responding women, and the denominator is the number of eligible women. Because nonresponding households that were not selected to receive the special followup conversion procedures are counted in the denominator but cannot be counted in the numerator, this response rate is too small. A more appro-

priate response rate is calculated by using the same denominator but changing the numerator. The more appropriate numerator is computed by counting each responding household once if it was obtained through normal procedures and twice if it was obtained through the special followup procedure.

Calculating response rates this way, the weighted response rate among those who had responded to the NHIS was 82.5 percent. Taking into account an earlier nonresponse rate of 4 percent to the NHIS, the final overall response rate was 79 percent.

Table C shows response rates by race and age. As in table B, note that race was taken as reported for the NHIS. Age was taken as projected for March 15, 1988, from the birthdate reported during the NHIS. (In a few cases, the race or age reported during the NHIS was found to be in error; in those cases, the corrected race or age was used.)

Table C. Response rates for Cycle IV of the National Survey of Family Growth among completed cases in the National Health Interview Survey, by race and age: 1988 National Survey of Family Growth

Race and age	Completed interviews	Unweighted response rate	Weighted response rate[1]
	Number	Percent	
All women 15–44 years of age	8,450	80.0	82.5
Race			
Black	2,811	79.8	82.2
Other than black	5,639	80.1	82.6
Age			
15–19 years	1,254	77.8	80.5
20–24 years	1,307	77.9	79.7
25–29 years	1,593	79.7	84.9
30–34 years	1,713	82.3	84.5
35–39 years	1,426	82.0	83.8
40–44 years	1,157	79.6	81.1

[1]Weighted for nonresponse only.

Sample sizes, clustering, and variation in the probability of selection

Table D shows numbers of completed interviews by race and age as described above for table C. It also shows other characteristics of the sample that can affect the reliability of analysis.

Table D. Clustering and weight variation among completed cases in the 1988 National Survey of Family Growth, by race and age

Race and age	Completed Interviews	Clusters with 1 or more completes	Average number of completed interviews per cluster	Relative variance in unbiased weights
			Number	
All women 15–44 years of age	8,450	3,143	2.69	0.39
Race				
Black.	2,811	1,056	2.66	0.64
Other than black.	5,639	2,382	2.37	0.17
Age				
15–19 years	1,254	1,014	1.24	0.36
20–24 years	1,307	997	1.31	0.44
25–29 years	1,593	1,192	1.34	0.42
30–34 years	1,713	1,284	1.33	0.37
35–39 years	1,426	1,115	1.28	0.37
40–44 years	1,157	982	1.18	0.36

The average cluster size is the number of interviews of the indicated type that were obtained, on average, from the same neighborhood. As the category becomes more narrow, the average cluster size decreases. For example, the average cluster size for all women is 2.69, whereas the average cluster size for women 20–24 years of age is just 1.31. The average cluster size for currently pregnant women 20–24 years of age will be even smaller. The fact that multiple interviews were obtained from the same neighborhood reduced survey costs, but it also increased variances. In a sense, multiple interviews in a given neighborhood are slightly redundant; that is, the women in the neighborhood have similar characteristics. It is worth noting that the average cluster size is far smaller in Cycle IV than the comparable figure, 9.1, for Cycle III. Those who use the detailed datafile to do their own analysis need to remember that the data are clustered and discount their reliability statistics accordingly (by increasing standard error estimates, the width of confidence intervals, and the critical values of hypothesis tests). The variance estimation procedures discussed in this report will assist the user in this effort to calculate accurate measures of reliability.

The variation in the probabilities of selection is another important indicator of reliability. The variation means that some groups are underrepresented in the sample and that others are overrepresented. All reports published by NCHS from the NSFG weight the data appropriately; that is, cases from underrepresented groups are weighted more heavily than cases from overrepresented groups. Users of the detailed datafile are sometimes inclined to ignore the varying selection probabilities. Ignoring the variation in probabilities (analyzing the data without weights) can lead to increased biases and smaller variances. The last column of table D is the relative variance of the unbiased weights. This is computed by dividing the standard deviation of the weights, squared, by the mean weight squared. In a simple random sample, the relative variance of unbiased weights is 0/1 or 0 because all sample cases have the same probability of selection. The larger the value of the relative variance of the unbiased weights, the more the probabilities of selection vary. This decreases the efficiency (increases the sampling errors) of the estimates for all women 15–44 years of age or all women 20–24 years of age but increases the reliability of the data for the group being oversampled, primarily black women. It also points to the fact that ignoring the weights in analysis and significance testing may lead to erroneous conclusions. These issues will be discussed further in the section on variance estimation.

Estimation: Weighting procedures

Summary

The NSFG is designed to provide national estimates of the number of women with particular characteristics — for example, the number using the Pill, the number who are infertile, or the number who use family planning services. In order to make such estimates, each case was given a "sampling weight," which is the number of women in the population that she represents.

More precisely, the NSFG is designed to produce consistent estimates for the entire population of eligible women in the United States. In this context, "consistent" means that if both the population and the sample size were to be allowed to increase, with the same sort of sampling and estimation techniques, then the probability that the resulting sample estimates would vary from the true population parameters by more than a very small amount would approach zero. A weight has been assigned to each woman such that the weighted sum of any survey characteristic is a consistent estimate of the population total.

The weights were constructed in several steps. As a preliminary step, unbiased weights were calculated. Extremely large unbiased weights were trimmed to reduce variance. The trimmed weights were adjusted for nonresponse. Finally, there were further adjustments to force important statistics to agree with independent control totals.

Unbiased weights

Weighted tabulations with unbiased weights give unbiased estimates. Statisticians have a number of different meanings for unbiasedness. If the survey were repeated under the same general conditions on every possible sample using the same design, a particular estimate could then be produced under the same procedures from each of the possible samples. If the average of those hypothetical estimates is equal to the estimate that a census with comparable procedures (and comparable coverage and response rates) would yield, then the estimates are said to be "design-unbiased." For this report, design-unbiased is abbreviated to simply unbiased. Such estimates may or may not be "model-unbiased" (6).

The unbiased weight for a woman is the reciprocal of her probability of selection. The probability of selection is computed as the product of the probabilities of selection at each stage. There were several stages of selection for the NHIS, as described in the section on sample design. Westat obtained a weight from NCHS that reflected the probability of selection for the NHIS. (In fact, this weight also included an adjustment for nonresponse to the NHIS. If one household within a block could not be interviewed, then the remaining households in the block had their weights increased to represent the missing household. This adjustment is unbiased only if the nonresponding household can be viewed as having been randomly selected from the households within the block.)

As discussed in the section on sample design, Westat subsampled the NHIS completed cases by first selecting PSU's, then selecting weeks within selected PSU's, then households within selected weeks, and, finally, persons within selected households. It was assumed that the time of the NHIS interview had no effect on the data. The probability of selection at the "week" stage was thus taken to be the quotient of the number of weeks selected divided by the total number of weeks available. The probability of selection at the PSU stage was taken to be the quotient of the number of panels selected divided by four (the number of PSU panels available). The probability of selection within the household was taken to be the reciprocal of the number of eligible women in the household (according to NHIS data).

Special adjustments to the weights were necessary in the rare cases when either the originally selected woman was not eligible and a substitute woman was selected, or when the same woman was found in more than one NHIS household. (This could happen when a woman moved within the same neighborhood between NHIS interviews. It could also happen when the household was selected in one quarter through sampling of new construction permits and mistakenly selected in another quarter by area sampling.) If more than one eligible woman was available for substitution, the woman's probability of selection at the household stage was taken to be the reciprocal of the number eligible at that time. For the women interviewed twice during the NHIS, the preliminary unbiased weight was cut in half.

Women who were selected for the formal nonresponse followup had their preliminary unbiased weights doubled since they had only half the chance of being selected as cases in the prior stages of fieldwork.

Weight trimming

Extremely large weights can cause high variances even if they are unbiased. To reduce this potential for high variances, some of the weights were trimmed. To reduce the risk of bias, the trimmed weight was redistributed to other cases within the classes shown in table 1. These classes were formed based on characteristics that affected the probability of selection—race, number of eligible women in the household, and whether or not the woman was in the nonresponse followup.

Nonresponse adjustment

Summary

The linkage of the NSFG to the NHIS created the opportunity for a far more sophisticated nonresponse adjustment than had been possible in previous cycles of the NSFG. Each of the women who could not be reached for the NSFG or who declined to participate in the NSFG came from a household that had previously participated in the NHIS. (Most of the NSFG respondents were interviewed in the NHIS, but sometimes another member of the household provided data about her as part of the NHIS interview.)

Using the NHIS data, women were classified into groups with differing response rates. Some groups were easy to reach and had high response rates, so adjustments to their weights are small. Other groups were difficult to reach and had low response rates. Cases in these latter groups had their weights increased substantially. In general, each unbiased weight was divided by the probability of response. These weights are not design-unbiased, but they do substantially reduce the risk of nonresponse bias. (For additional details, see (7).)

Theory

The procedure adopted for Cycle IV is not the only possible procedure. The basic procedure is to inflate sampling weights by the inverse response rate within homogenous *groups* of sample cases known as nonresponse adjustment *cells*; however, there is wide latitude on how to form the cells. A particular set of cells is good if it eliminates or at least reduces nonresponse bias for the most important substantive variables. A set of cells eliminates nonresponse bias for a particular substantive variable if, within each cell, participation in the survey is independent of that one substantive variable. To eliminate nonresponse bias for all substantive variables it is necessary that, within cells, participation in the survey be independent of all substantive variables.

For the concept of independence to apply in this context, it is necessary to view participation in the survey as a random event. It may not be a true random event, but if it is a predetermined event, then it should be determined only by minor variables that are not measured by the survey, such as mood, reaction to the interviewer's appearance or behavior, or general attitude to surveys. Furthermore, within each cell, none of these minor variables should have an effect on substantive characteristics such as contraceptive use.

Forming the cells is, in general, a subjective procedure because it is impossible to directly measure the probability of nonresponse for each sample person. Two fairly objective procedures that have been developed are the predictive mean approach and the response propensity approach. The latter approach was used for Cycle IV.

The predictive mean approach is based on the theorem that if every sample person within a cell has the exact same value of the substantive variable, then nonresponse must be independent of the substantive variable. As applied to the NSFG, this strategy would have entailed grouping women together who are similar in terms of substantive variables. In this case, similarity would have been defined in terms of variables from the NHIS that are predictive of the critical variables of the NSFG, such as fertility and contraceptive use. The relevant predictive variables from the NHIS include race, ethnic origin, family structure, education of self and of parents, family income, marital status, number of own children in the household, personal health status, population density, distance to a major city, and region.

Models could have been developed to predict parity or use of contraceptives from these variables. Women with similar predicted parity or similar estimated probability of having ever used contraceptives could have been grouped together. Such a procedure would have worked well for one variable or the other. However, the groupings could be different for each substantive variable. This strategy thus requires placing one substantive variable above all the others. In this sense, the strategy is univariate.

The response propensity approach is based on the theorem that if every sample person within a cell has the exact same propensity to respond, then nonresponse must be independent of the substantive variable. With this strategy, women are grouped together according to their propensity to respond. Women with high propensity get grouped together and receive very small weighting adjustments because most of them respond. Women with low propensity also get grouped together but receive much larger weighting adjustments because they tend not to respond. The relevant variables from the NHIS include some of the same variables that would be used with the predictive mean strategy, such as education and size of place of residence, but the greatest emphasis is placed on variables that indicate resistance to participation in surveys. Such variables include refusing to give the telephone number of a contact person, refusing to give their social security number, refusing to give their own telephone number, and refusing to answer certain other questions. Another important set of variables indicates not resistance so much as unavailability (frequent travel, night shifts, multiple jobs). Such variables also include the number of calls that the U.S. Bureau of the Census interviewer had to make to get the NHIS interview.

To summarize the comparison of the two approaches, the criterion for elimination of nonresponse bias is the same: Nonresponse must be independent of all substantive variables within cells. The predictive mean approach tries to attain this goal by minimizing the variance of one or two particular substantive variables within cells. The response propensity approach tries to attain the same goal by minimizing the variance of the probability of nonresponse within cells. It does not seem plausible to believe that either method will completely succeed. The response propensity approach was favored because if it comes close to eliminating the variance on response propensity within cells, it reduces the nonresponse bias on all substantive variables, not just one or two. This feature was the deciding factor because the NSFG is very much a multivariate survey that measures a large number of important dependent variables. At least one comparison suggests that the procedure does indeed reduce nonresponse bias (7).

Methodology

Response rates were calculated for the 10,566 women selected from the NHIS as eligible for the NSFG sample. Completed interviews were obtained with 8,450 women. The first stage of the analysis of nonresponse was a series of cross-tabulations. These revealed 10 small cells (less than 400 cases each, or less than 4 percent of eligible women each) that had response rates of under 70 percent. (Many of these characteristics were associated with each other, however, so that when these 10 cells were specified in a mutually exclusive way, some of the response rates exceeded 70 percent.) These were the first 10 cells of the nonresponse adjustment matrix shown in table 2. These first 10 cells were defined hierarchically (with IF-THEN-ELSE statements), beginning with the lowest response rate (cell 1) to the highest (cell 10). Cases that fell into more than one group were classified into the first groups for which they were qualified. The tables were run first in more detail than shown in table 2; some smaller groups with similar response rates were combined.

With only one exception (cell 2), the first 10 groups have between 39 and 359 cases. The lowest response rate (33 percent) is in group 1—women whose education is unknown or who did not complete any years of school. Cell 2 taps a transient, marginal population that may not be covered well by most surveys. Rates were also relatively low for Asian and Cuban women (groups 8 and 9), and for those who were unemployed or worked without pay (group 10).

The results of the second and largest part of the analysis of nonresponse are shown in the rest of table 2 (cells 11–51). For this part of the analysis, tables of response rates were run by about 30 characteristics of women as measured in the NHIS. Chi-square values were computed for each table. Variables were included in the model in order by their chi-square per degrees of freedom. This approach was used at each stage of the analysis to identify a classification of the NSFG sample by response

rates that would be least likely to result from chance. The process stopped when there were not enough cases or when further tabulations found no more significant variables.

Cells 11–16 show women whose household refused to provide the NHIS contact person's telephone number and whose household refused to provide their own social security number.

Cells 17–22 show women 18 years of age or over who refused to give a telephone number for the NHIS contact person, but gave their own social security number. Among women who refused the contact person's phone number, but gave their own social security number (IIB in table 2), and who completed the NHIS interview after only one or two visits by the NHIS interviewer, response rates were lower in the Northeast and West (77 percent, cell 17) than in the Midwest or South (87 percent, cell 18).

Among women 18 years of age or over, the most important variable was whether they had children in the household. Thus, cells 25–34 of table 2 are for women who gave a phone number for the NHIS contact person, who were 18 years of age or over, and who had no children in the household at the time of the NHIS interview. For women without children in the household, education and region were found to affect response rates. Among women 18 years of age and over who had no children, who had a high school education or less, and who lived in the Northeast, response rates to the NSFG were much higher for those women who had completed the NHIS in one or two interviewer visits (74 percent, cell 25) than for those who required three visits or more (54 percent, cell 26).

Cells 35–43 of table 2 show response rates for women who provided the NHIS contact person's phone number, who were 18 years of age or over when the NHIS was conducted, and who had one or more children in the household when the NHIS was conducted (IIIA2b in table 2). Within this group, women living in central cities of large metropolitan statistical areas (MSA's) had a response rate of 82 percent. For these central city residents, response rates varied by education, from 79 percent in the lowest group (cell 35) to 88 percent among college graduates (cell 37). Among suburban residents who responded to the NHIS themselves, response rates were 81 percent for Hispanics (cell 38) and 88 percent for non-Hispanics.

Cells 44–51 are for women who provided the NHIS contact person's telephone number and who were under 18 years of age at the NHIS interview (IIIB). There were 1,041 of these women 15–17 years of age in the NHIS, and their response rate to the NSFG was 81 percent. For these teenagers, Hispanics had a higher response rate (91 percent, cell 44) than non-Hispanics (80 percent). This is the opposite of the pattern in cell 38, for women 18 years of age and over, and is a good illustration of the need to account for interactions when designing these nonresponse groups, or cells.

For a more detailed discussion of table 2, and of the uses of the data in the weighting procedure, see (7).

Poststratification

If independent estimates of the sizes of specific subpopulations are available that have smaller variances and no larger biases than estimates from a survey, then the data from the survey should be adjusted to match these independent estimates. This adjustment is called poststratification. The U.S. Bureau of the Census maintains data series on the age, race, and sex of the civilian noninstitutionalized population. These figures are based on the decennial censuses, immigration statistics, births, and deaths. Although these figures are subject to prediction error, they are not subject to sampling variability. Any survey that controls weights so that estimates of sex, age, and race agree exactly with these demographically modeled estimates eliminates the sampling variance on these statistics. Furthermore, any statistics that are strongly determined by sex, age, and race also benefit from this reduced variance.

The U.S. Bureau of the Census also adds a supplement to the Current Population Survey (CPS) each year to ask questions on parity and expected future births. The CPS and the NSFG were compared on two dimensions: variance and bias. Because the CPS has a much larger sample size than the NSFG, the CPS estimates have a smaller variance. Thus, the only potential reason not to poststratify to the CPS would be evidence of strong biases. Careful study did not reveal any evidence that the CPS estimates of marital status (ever married or never married) and parity (number of children ever born) are subject to any known biases stronger than those to which the NSFG is also subject. The decision was made, therefore, to poststratify according to marital status and parity as well as to age and race. (See appendix I for a detailed report on research of the poststratification question.) Conveniently, the CPS poststratifies on age and race, so poststratification to the CPS induced poststratification to the Bureau's demographically modeled estimates. The NSFG estimates of race (black versus other) and age (as of interview in 5-year cohorts) are thus not subject to sampling variance.

Poststratification was implemented as an iterative procedure where the NSFG weights were alternately adjusted to provide consistency with the CPS in terms of marital status and then parity within each combination of race and age. The result is double three-way, not four-way, consistency with the CPS. In other words, the NSFG estimates of parity groups agree with the CPS estimates by race and age, and so do the NSFG estimates of marital status groups. However, the NSFG cross-tabulations of parity and marital status do not agree exactly with the similar CPS cross-tabulations.

The NSFG data were first tabulated by race, age, and marital status using the nonresponse-adjusted weight. The CPS estimate for each combination of race, age, and marital status was divided by the NSFG estimate for the same combination. The NSFG weights were then multiplied by this quotient. At this point, the NSFG estimates would agree with the CPS estimates by race, age, and marital status but not by parity. The NSFG data were then tabulated again, using the new weight, by race, age, and parity. The CPS estimate for the same combination of race, age, and parity was divided by the NSFG estimate. The NSFG weights were then multiplied by this quotient. At this point, the NSFG estimates would agree with the CPS estimates by race, age, and parity, but not by marital status. However, the discrepancies on marital status within age and race were smaller than they had been at first. This process, called raking, continued, alternating between adjustment on parity within age and race and adjustment on marital status within age and race. After several rounds, consistency was obtained for both marital status and parity within age and race. However, the NSFG estimates of parity for a specific marital status will not agree exactly with the CPS estimates. The classes that were used for the poststratification are shown in table 3.

Estimating equation

The Cycle IV estimator of the number of women with a given characteristic is

$$y = \Sigma_i W_{si} I_i$$

where W_{si} is the final weight for the ith sample woman and

$$I_i = \begin{cases} 1 \text{ if the woman has the characteristic and} \\ 0 \text{ otherwise} \end{cases}$$

The Cycle IV estimator of the total number of events (such as births) associated with women with a given characteristic is

$$y = \Sigma_i W_{si} I_i x_i$$

where x_i is the number of events that the ith woman has experienced. This formula also works for continuous variables such as birth weight and income.

The Cycle IV estimator of the mean number of events (such as births) or the mean quantity of some continuous variable (such as income) associated with women with a given characteristic is

$$y = (\Sigma_i W_{si} I_i x_i)/(\Sigma_i W_{si} I_i)$$

Estimates of regression parameters and other complex statistics can also be computed using the weights. (That calculation is not shown here but can be seen in (8).) Note that the standard statistical packages such as SAS, SPSS, and BMDP offer options to compute numbers, percents, and other statistics using weights. The estimated population parameters that the packages supply will usually be good, but the estimated standard errors and p-values for the estimates will not be satisfactory. See the section on variances for more discussion of how to estimate variances.

Detailed derivation of sampling weights

The sampling weights were constructed in four basic steps:

- *Inflation*—By the reciprocal of the probability of selection. This weight is called the baseweight, or W_0. For example, if the probability of selection is 1 in 5,000, then W_0 is 5,000.

- *Trimming*—About 100 cases had extremely large baseweights (W_0) in the 1988 NSFG. In previous cycles, these large weights were left alone, but they could have adverse effects on results, especially in small categories. To reduce this problem, these large weights were trimmed, or reduced, to a maximum value of 8,000 for black women (about four times the average W_0 weight for black women) and 19,000 for women who were not black (about three times the average W_0 weight for women who were not black). This trimmed weight is called W_1. The trimming reduced the total weighted numbers to less than the 57.9 million U.S. women who were known to be 15–44 years of age in 1988. Therefore, the reduction in the weighted numbers was redistributed within each of 16 cells (table 1) to form a new weight, called W_2. This weight was trimmed again when it exceeded the maximum value. The resulting weight was called W_3.

- *Nonresponse adjustment*—For each of the 51 cells defined in table 2, the ratio of the weighted sum of all cases to the weighted sum of complete cases was applied to W_3 by cell. The new weight was called the "nonresponse adjusted weight," or W_4.

- *Poststratification*—A 72-cell matrix of categories of age by race (black versus other than black), by marital status (ever married versus never married), and by parity was defined. The control totals for these cells were obtained from the June 1988 Current Population Survey (CPS), conducted by the U.S. Bureau of the Census. The nonresponse adjusted weight, W_4, was forced to be equal (to the nearest thousand) to the CPS control total by raking parity against marital status within age-by-race categories. (Raking, described on page 13, is a procedure for iteratively adjusting sample data to independent marginal totals.) The resulting weight is called the "final poststratified weight," or W_5.

SAMPWGT is the NHIS noninterview-adjusted weight.

SUBSADJF is the factor that adjusts the weight for the subsampling of NHIS households. The factor varied depending on the quarter from which the household was selected, the size of the PSU it came from, the presence of eligible black women, and the number of eligible women who were not black. The first step was to calculate the probability of selection for a certain type of household relative to a full sample NHIS probability regardless of the quarter from which it was selected. The second step was to adjust that conditional probability for the quarter from which it was selected.

The ratios of the probability of selection for the NSFG to the probability of selection for a full-strength panel of the NHIS were computed as follows:

- *Households containing at least one eligible black woman in a large or medium SR PSU*—All such NHIS households from the fourth quarter of 1985 through the first quarter of 1987 were selected. However, in the fourth quarter of 1985, there were only three NHIS panels fielded. Furthermore, in all quarters of 1986, there were only two NHIS panels fielded. In the first quarter of 1987, all four panels were fielded. Thus, 15/16's of a full annual sample size was selected:

$$15/16 = [(3/4) + (1/2)4 + 1]/4$$

- *Households containing at least one eligible black woman in a small SR PSU or an NSR PSU*—The only difference in the probability of selection for these households from comparable households in the larger PSU's was that not all four panels of the first quarter of 1987 sample were taken even though they were available. From the first quarter of 1987, only three panels were selected. The proportion of a full annual sample size that was selected is thus:

$$7/8 = [(3/4) + (1/2)4 + (3/4)]/4$$

- *Households containing exactly one eligible woman who was not black and no eligible black women*—No such households from the fourth quarter of 1985 or 1986 were selected. Four weeks were selected from the second quarter of 1986 and all weeks from the third and fourth quarters of 1986. One week was selected from the first quarter of 1987, but only in two panels even though four were available. The proportion of a full annual sample size that was selected is thus:

$$31/104 = [0 + (1/2)(4/13 + 2) + (1/2)(1/13)]/4$$

- *Households containing exactly two eligible women who were not black and no eligible black women*—No such households were selected from the fourth quarter of 1985. All weeks were selected from all quarters of 1986. Twelve weeks were selected from the first quarter of 1987, but only in two panels even though four were available. The proportion of a full annual sample size that was selected is thus:

$$8/13 = [0 + (1/2)4 + (1/2)(12/13)]/4$$

- *Households containing more than two eligible women who were not black and no eligible black women*—No such households were selected from the fourth quarter of 1985. All weeks were selected from all quarters of 1986. All weeks from the first quarter of 1987 were selected, but only in two panels even though four were available. The proportion of a full annual sample size that was selected is thus:

$$5/8 = [0 + (1/2)4 + (1/2)]/4$$

Inversion of those probabilities gives the weights for the five categories relative to full panel NHIS weights:

$$16/15,\ 8/7,\ 104/31,\ 13/8,\ \text{and}\ 8/5$$

However, the NHIS weights were not full sample weights; they were quarterly weights. Moreover, they were quarterly weights that had already been adjusted by the U.S. Bureau of the Census to compensate for the reduced number of panels in the NHIS in 1985 and 1986. Adjustment to full strength quarterly weights gives relative weights smaller by a factor of four:

$$4/15,\ 2/7,\ 26/31,\ 13/32,\ \text{and}\ 2/5$$

The five relative weights were next adjusted to account for the number of panels actually fielded (rather than on the full potential sample). It was necessary to divide the weights for 1985 households by 4/3 to remove the adjustment inserted by the U.S. Bureau of the Census to account for a 3/4-strength sample and to divide the factor for 1986 households by 2 to remove the U.S. Bureau of the Census adjustment for a 1/2-strength sample.

The final household sampling adjustment factors can be summarized as follows:

Fourth quarter 1985

Households containing: At least one eligible black woman:	SUBSADJF
PSU is large or medium SR	$1/5 = (4/15)/(4/3)$
PSU is NSR or small SR	$3/14 = (2/7)/(4/3)$
No eligible black women	NA

All quarters 1986

Households containing: At least one eligible black woman:	SUBSADJF
PSU is large or medium SR	$2/15 = (4/15)/2$
PSU is NSR or small SR	$1/7 = (2/7)/2$
No eligible black women	NA
One eligible woman	$13/31 = (26/31)/2$
2 eligible women	$13/64 = (13/32)/2$
3 eligible women or more	$1/5 = (2/5)/2$

First quarter 1987

Households containing: At least one eligible black woman:	
PSU is large or medium SR	$4/15 = (4/15)/1$
PSU is NSR or small SR	$2/7 = (2/7)/1$
No eligible black women	NA
One eligible woman	$26/31 = (26/31)/1$
2 eligible women	$13/32 = (13/32)/1$
3 eligible women or more	$2/5 = (2/5)/1$

BASEWGT = (SAMPWGT)(SUBSADJF) (Number of women in the household with a chance of selection)

The number of women in the household with a chance of selection is usually the number of women determined from the NHIS sample data to be 15–44 years of age as of March 15, 1988. The only exceptions are for mixed-race households interviewed in the NHIS in the fourth quarter of 1985 and in the first quarter of 1987. If a household contained one or more eligible black women and one or more eligible women who were not black, only the black women were given a chance of selection for the NSFG.

The DCF (duplication control factor) corrects for multiple selection by the NHIS. It is equal to two if the woman was selected twice. Otherwise, it is equal to one.

ADBASEWT = (BASEWGT)(DCF)

$$\text{WEIGHT} = \begin{cases} 1 & \text{if the woman was interviewed during} \\ & \text{the initial fieldwork} \\ 2 & \text{if the woman was selected for the} \\ & \text{special followup} \\ 0 & \text{if the woman was not selected for} \\ & \text{the special followup} \end{cases}$$

CORRFAC = correction for special NHIS weighting

$$= \begin{cases} 1 & \text{for standard weighting} \\ 2/3 & \text{for panel 1 units with} \\ & \text{nonstandard weighting} \\ 4/3 & \text{for panel 2 and 3 units with} \\ & \text{nonstandard weighting} \end{cases}$$

NRSAMPWT = (WEIGHT)(ADBASEWT)(CORRFAC) = W_0

This completes derivation of the unbiased weights. Further adjustments follow.

The first trimmed weight is

$$W_1 = \begin{cases} \text{min. } \{W_0, 8{,}000\} & \text{for black women and} \\ \text{min. } \{W_0, 19{,}000\} & \text{for women who were not black.} \end{cases}$$

The trimmed weight after redistribution of trimmed weight (difference between W_1 and W_0) is

$W_2 = W_1 \Sigma W_{0i}/\Sigma W_{1i}$, where the summation is restricted to the cell containing the case in table 1.

The second trimmed weight is

$$W_3 = \begin{cases} \text{min. } \{W_2, 8{,}000\} & \text{for black women} \\ \text{min. } \{W_2, 19{,}000\} & \text{for women who were not black.} \end{cases}$$

$$\text{Let } I_{Ri} = \begin{cases} 1 & \text{if the } i\text{th case is a respondent} \\ 0 & \text{for nonrespondents.} \end{cases}$$

Let Σ_{Fi} denote summation over all cases (respondents and nonrespondents) in the cell from table 2 that contains the ith case.

The nonresponse adjusted weight is

$$W_4 = \begin{cases} W_3 & (\Sigma_{Fi} W_{3i}/\Sigma_{Fi} I_{Ri} W_{3i}) \text{ for respondents} \\ 0 & \text{for nonrespondents.} \end{cases}$$

The final weight is W_5 = maximum likelihood weights (raked weights) given marital status and parity marginals defined in table 3 within age-race cells.

Imputation

Introduction

In any survey, not every item is answered by every respondent. Sometimes the respondent cannot remember the answer and occasionally may refuse to answer. Also, interviewers sometimes forget to ask a question, skip it by mistake, or forget to write down the answer, so that some items are left blank when they should have been answered. Incomplete data create small inconsistencies in estimates, and these may create confusion. Filling in answers for these missing items (imputation) makes the data complete and consistent and, therefore, easier to use. In the NSFG, 201 important items have been forced to be complete. For these items, missing answers were imputed. These imputed answers may be thought of as educated guesses.

Generally, women with missing information were matched with similar women. The answer of a similar woman was then transferred to the woman with the missing answer.

In general, the frequency of missing values in Cycle IV was quite low. Missing values were imputed for the 201 variables shown in appendix 2. Some of these variables were not included on the public-use file because they were redundant; others were not included because of subsequent consistency checks and reprogramming of selected variables by NCHS; and others were left out for reasons of confidentiality. For the 173 imputed variables with imputation "flags" on the public-use file, the frequency of missing data was quite low. For 116 of these 173 variables, less than 1 percent of the cases had missing data (0–84 cases) and 39 other variables had 1 to 5 percent missing (85–423 cases). Only 13 variables had 5 to 10 percent missing, and 5 had 10 to 11 percent missing data. No imputed variable had more than 11 percent of cases imputed.

All but 1 of the 13 variables with 5 to 10 percent missing data were measures of use of family planning services, including age at first family planning visit, month of first visit, three measures of specific services received at the most recent visit, and five measures of ways that the most recent visit was paid for.

The five variables with 10 to 11 percent missing data were education of the respondent's mother (EDUC-MOM), month and year of first cohabitation or marriage (UNION1), type and outcome of first union (UNTYPE),

duration of first union (UNIONINT), and the ratio of family income to the poverty level (POVERTY).

For those researchers who have questions about the procedures used, there is always the option to use the microdata. All imputed data have been clearly marked on the microdata file so that researchers may do their own imputations if they wish. An excellent source for those interested in learning more about imputation is Little and Rubin (9, section 4.5).

Imputation procedures

The variables requiring imputation were put into 32 groups (sometimes called "modules," shown in appendix II); each group was imputed with a procedure known as "hot-deck imputation." Within a group, the hot-deck procedure sorted the file so that similar women were close to each other. Each woman's answers to the questions in the group were then examined in turn. Whenever a woman had *missing* values for one or more of the variables, she was given the values for the previous woman with *complete* values—in other words, the previous woman's answers were copied to her record. Only the missing variables were added; any complete (valid) values she had reported were left alone. The groups were imputed serially in separate computer runs so that each group was not imputed until the prior group was forced to be complete. The file was sorted by different variables for each group.

Additionally, tests, screens, and edits were used. A test was a logical examination of the case with a missing value to see if the missing value could be completed by logic. The tests usually included an examination of a variable that had just been completed by imputation. For example, if a woman was imputed to have never had intercourse, then a whole range of variables were set to blank (such as whether she had ever used a diaphragm), indicating "not applicable." A screen was a condition that cases with complete data had to meet to be considered as donors. For example, in imputing age at first intercourse, the only eligible cases are those that have had intercourse. Generally, tests and screens complemented each other so that recipients met the screening condition automatically. An edit was a check after imputation to make sure that the imputed value was sensible. If the imputed value did not make sense, it was replaced with a more plausible value. For example, if a woman was initially imputed to

have been separated from her first spouse after the date of her second marriage, then her age at dissolution was set equal to her age at second marriage, and the imputation flag for the age at dissolution was set to indicate model-based imputation. This did not occur often and its frequency may be seen in the tape documentation.

Imputing a group

The file was sorted so that women close to each other in the sort were predicted to have similar values of the variables being imputed. For example, in imputing the age at which a contraceptive was first used, the file was sorted by age at first intercourse. Sometimes, multiple variables were used for the sort. These variables are known as "sort keys." The first sort key is the primary determinant of the sort. Only where there are ties (equal values) on the first sort key do the secondary sort keys come into play. The second sort key is used to resolve ties on the first sort key. If there are still ties, the third sort key is used to resolve them. This continues until all ties have been resolved or until there are no more sort keys.

The hot-deck consists of the following two steps:

- If the woman has legitimate values for all variables in the group, then those values are stored in a vector.
- If the woman is missing one or more variables in the group, then the current contents of the vector are imputed to her.

Several points need to be noted. First, legitimately reported values were never replaced with imputed values. Thus, a woman who was missing two out of five variables in a group received imputed values only for the two missing variables. These values may be inconsistent with the values for the three reported variables. Even if the juxtaposition is not inconsistent, it may be rare. For example, a woman may report never having been treated for genital herpes but not know if her partner has ever been treated (appendix II, group 31). Because these variables are in the same module, there is a chance that the partner will be imputed as having been treated for herpes. Obviously, it is not inconsistent for only one partner to have herpes, but it is more common for neither or both to have had it. Imputing such rare combinations tends to wash out the relationships between variables. This phenomenon is known as "attenuation of correlation."

Thus, it is important that the items within a group be nearly independent for women close to each other in the sort. (For women close to each other in the sort, knowledge of one item should not help predict the other item.) The one exception to this rule is when missing data on one variable necessarily implies missing data on the other variable. Because the only cases that "donate" values within a group are complete within the group, multiple imputed values will always be consistent with each other. Group 24 in appendix II is an example of where this relationship was exploited.

Second, initial plausible values were developed for the event where no donor women were found before the first woman needing imputation was found. These initial plausible values are known as "cold-deck values." They were obtained by passing the file in the sort for the group until the first complete case was found. Third, a series of consecutive women with missing values was occasionally encountered. All of them received imputed values from the same donor woman.

Fourth, there are several points in the sorts where the women next to each other were not very similar. For example, in group 22.1 of appendix II, the first sort key was type of sterilization operation, the second was descending age at most recent birth, and the third was age on March 15, 1988. Among those women whose husbands or partners had vasectomies, the file for the youngest childless woman followed the file for the eldest woman among those who had their last birth at 17 years of age (the youngest observed age for last birth). That youngest childless woman did not know how old she was when her partner had undergone the vasectomy. As a result, she was imputed to have been much older at the event than her current age. This awkwardness was fixed by forcing her age at the event to equal her current age. The problem might have been more elegantly resolved by breaking the file into more pieces with a cold-deck value for each piece, but that would require more software development time and would lead to more frequent instances of women with complete data making multiple donations.

Group formation

The groups are defined in appendix II. In general, each group consists of variables that can be imputed with the same sort but are not too closely related. In deciding the order to impute related variables, variables missing less often were imputed earlier than variables missing more often, and background variables that shape fertility and contraceptive practices were imputed earlier than variables that actually measure fertility and classify contraceptive practices. Variables with very strong relationships were placed in separate modules to better preserve their relationships. Variables that were missing or complete as a group were always placed in a single module. Furthermore, variables in the early modules tend to determine variables in later modules.

Quite a few variables were complete or nearly so before imputation. These are listed in group 1 of appendix II. Those variables with less than 10 missing values were imputed manually using relevant information from the questionnaire. Manually imputed cases have a value of "3" in the corresponding imputation flags on the data file.

Group 2 consisted of Hispanic origin, region of birth, religious affiliation, education of mother, and age of the woman's mother when her first child was born. For this group of variables, the file was sorted by race, region, metropolitan residence, and rural-urban residence. This means that, if a case was missing information (the

"recipient" case) on Hispanic origin or education of mother, the case from which a reported value was borrowed (the "donor" case) would usually be in the same category of race, region, metropolitan residence, and rural-urban residence as the recipient case. For example, if the recipient is missing information on education of mother, and is white, living in the Northeast, in a metropolitan-urban area, then the donor case will usually also be white and live in the Northeast in a metropolitan-urban area.

Group 3 was age at first menstrual period. This variable was imputed separately for those with and without missing data on age at first intercourse. Cases with missing information on age at first intercourse were imputed within whole single years of age at first birth and current age. For example, if a recipient was missing information on age at first menstrual period, and also missing age at first intercourse, then the recipient's age at first menstrual period was taken from a donor with the same age at first birth and current age. If the recipient case did have data on age at first intercourse, then the donor case had to have the same age at first intercourse (to the nearest one-tenth of a year) and the same age in whole single years.

Group 4 was whether the respondent had ever had intercourse (SEXEVER). This variable has values of yes or no; only 13 women refused to answer this question. The specification "SCREEN" means that SEXEVER is always equal to "yes" unless the respondent has never been pregnant (PREGNUM = 0) and never been married (RMARITAL = 6), so SEXEVER only needs to be imputed if she has never been pregnant and never been married. The variable is sorted by race and age, which means that if a black woman 17 years of age refused to answer whether she had ever had intercourse, the donor case must also be black and 17 years of age as well as never married and never pregnant. When SEXEVER was missing, a large number of other variables related to contraceptive use and use of family planning services were taken from the same donor case, in order to ensure that all these variables (listed in appendix II, group 4) would be consistent for the recipient case.

Group 5 was age at first intercourse (SEX1AGE). This was imputed in hundredths of a year, so that the month and year of first intercourse could be calculated from it. The "TEST" specification means that if the recipient had never had intercourse, then age at first intercourse had to be blank. The "SCREEN" specification means that both donor and recipient cases had to have had intercourse (SEXEVER = 1) in order to impute an age at first intercourse.

If age at first conception was missing, but age at first birth was not missing, donor cases had the same values on 3 variables as the recipients ("SORT"): age at first menstrual period, age at first birth in tenths of a year, and age on March 15, 1988. If age at first conception was not missing, then donor and recipient cases had the same age at first menstrual period, age at first conception, and age

on March 15, 1988. The "SORT" on age at first menstrual period was "descending;" this forces the donor to have reached menarche at a later age than the recipient, which forces the donated age at first intercourse to be higher than the respondent's age at menarche.

The "CONSTRUCT" specification in group 5 means that the variable DATESEX1 (month and year of first intercourse) can be constructed, or calculated, from the age at first intercourse calculated to the nearest one-hundreth of a year. For example, if a woman was born in June 1970 and her age at first intercourse was 16.44 years, then her date of first intercourse (DATESEX1) was (June 1970 + 197.28 months = November 1986). Group 5 also contains an EDIT, which forces the age at first intercourse to be no higher than the age at first formal marriage.

Group 6 is age at first formal (or legal) marriage. The "SCREEN" specification means that both donors and recipients must be ever married (currently married or have been married at some time). Donor cases had the same age at first intercourse (in tenths of a year) and age on March 15, 1988, in hundredths of a year. The CONSTRUCT specification means that, once FMAR1AGE is known, four additional variables can be calculated (appendix II, Group 6).

Group 7.1 was whether the respondent had ever cohabited. The "SCREEN" specification means that cases who are currently cohabiting do not need to be imputed on this variable, because they have ever cohabited. Donor cases usually had the same values on legal marital status, whether the respondent had ever had intercourse, Hispanic origin, 5-year age group, and rural-urban residence.

Group 7.2 consists of the outcome of the first cohabitation (COHOUT) and the age at beginning of first cohabitation (COHAGEB). In this group, a new variable (ENDAGE2) is defined, the age at second marriage for those married two or more times (or age at interview if not married 2 or more times). The case is tested to see if she has never cohabited, in which case outcome of first cohabitation (COHOUT) is automatically blank. Cases are screened so that the donor has ever cohabited and ever had intercourse, and her date of first intercourse is earlier than or equal to her date of first cohabitation. COHOUT and COHAGEB are imputed in three groups: (A) Women never formally married (FMARITAL = 6); (B) Ever-married women who did not have premarital intercourse; and (C) Ever-married women who did have premarital intercourse. Three EDITS are done on the imputed values: to force the age at cohabitation to be less than or equal to the current age; to force the age at cohabitation to be greater than or equal to the age at first intercourse; and, if the initially imputed age at cohabitation is greater than the age at first marriage, then to force age at first cohabitation to be greater than the age at dissolution of the first marriage. Once these two variables are imputed, and the three EDITS done, then three more variables can be constructed.

A number of consistency problems occurred in groups 6 through 9 concerning dates at which unions were estab-

lished or dissolved. Detailed sorts, screening of donors, and breaking files into separate pieces still did not eliminate all inconsistencies. Part of the problem is in the reported data. For example, reporting errors by respondents resulted in some cohabitation intervals that were later than the date of interview. EDITS eliminated most of the inconsistencies from the imputed data, but some of these errors may still exist in the reported data.

Groups 10 and 11 concern information about pregnancies. In group 10, manual imputation was used to impute the ages of women at pregnancy outcomes. There were only two cases where no ages were reported at all. For the other cases, the women did remember ages for at least some pregnancy outcomes, making it fairly easy to guess at ages for other pregnancies. Pregnancy length in group 11 is another key item. After imputing it with the hot-deck, it was possible to construct many other recodes.

In group 12, a procedure different from the hot-deck was used. In this group, the variables being imputed concerned early contraceptive use. The variables indicating lifetime usage of methods were already complete at the end of group 4. To keep early usage consistent with lifetime usage would have required more detailed matching of donors with respondents than was possible with the basic sort. Instead, the 10 listed methods were ordered according to popularity for early usage. The method highest in that priority used at any time in the respondent's reproductive life was then imputed to be the first method that she ever used. That method was also generally imputed to have been used at first intercourse. However, if the imputed first method was unlikely to have been used at first intercourse (such as female sterilization) then it was imputed that no method was used at first intercourse. Women with a reported first method but missing method at first intercourse were imputed not to have used any method at first intercourse.

Groups 13, 14, and 16 included education, poverty, and labor force status. These were not imputed earlier because of the effects on them that early pregnancies can have. (Groups 15 and 18 were dropped.)

Group 17 consisted of variables on frequency of intercourse. The file was broken into five pieces to allow optimum sorts for each segment.

Group 19 consisted of date of first contraceptive use, which was imputed as an age and then translated to a date. If a method was used at first intercourse, then age at first use was set to age at first intercourse. Otherwise, a sort was established that forces first use to be between first intercourse and interview.

Group 20 consisted of age at first family planning visit. Occasionally a variable (TIMVIS1) was reported that indicated the rough timing of first family planning visit relative to first intercourse even where the exact age at the first visit was not reported. The average delay between first intercourse and first visit (which could be negative) was computed for each of the rough timing indicators among women with complete data. That average delay was then added onto the age at first intercourse to impute age

at first visit. Otherwise, a sort was used to force donors to be in the same category of pill usage at first intercourse. The idea was that women who used the Pill at first intercourse must have had a family planning visit before first intercourse. Beyond that rough categorization, secondary sort keys were used to force donors to be about the same age and to have had first intercourse at about the same age.

Group 21.1 consisted of variables about recent and current contraceptive usage. As in group 12, the relationships to methods ever used were too complex to rely upon the hot-deck. The current variables (METHCAL and CONSTAT) were not missing very often. Those few cases with missing data were imputed manually. Method used at last intercourse (LASTBC) was missing more often. It was imputed by ordering the methods from those used most often at last intercourse to those used least often (step 5 under "MODEL" in group 21.1 of appendix II). The method highest in that priority list that the respondent had ever used was then imputed to be the last method that she used.

Group 21.2 consists of the duration of current period of abstinence from sexual intercourse. It was not imputed in group 17 because the relationships to variables in that group were too strong.

Group 22.1 consists of age at sterilization. The "SCREEN" says that ages at sterilization were taken only from women who had been sterilized or whose husbands had been sterilized.

Group 22.2 consists of variables summarizing fecundity. Very few cases were missing values for these indicators. Most of those missing values were imputed by hand because of the wealth of questionnaire data on the topic. (Most of the missing data was caused by inconsistencies in the reported data rather than by the lack of reported data.) Only those cases that had refused to report whether they had ever had intercourse were imputed using the hot-deck.

Group 23 consists of variables about expectations and desires for additional children. The variable summarizing fecundity (FECUND Group 22.2) strongly restricts the possible values for these variables but does not determine them completely.

Group 24 consists of variables about services received to treat infertility and the source of those services. The variables were either all missing for a specific woman or all reported.

Groups 25, 26, 27.1, 27.2, and 28 consist of variables about source and types of first and last family planning services and visits in the preceding year to selected providers. In groups 25 and 26, the "SCREEN" says that FP1SRC is imputed only if the woman has had at least one family planning visit. In group 25, the file is sorted by race, poverty level, marital status, and age at first family planning visit.

In group 26, the file is sorted by whether the woman has ever used a contraceptive method (ANYMTHD), her first source of family planning services (FP1SRC, just

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imputed in group 25), the timing of the first visit in relation to first pregnancy (TIMY152), and poverty level.

In group 27.1, the number of visits for family planning services in the last 12 months by source of service was imputed. They are imputed separately depending on whether the total number of visits (FPYRTOT) was known.

In group 27.2, the most recent source of family planning services (RSOURCE) was imputed. The "SCREEN" says that it is imputed only if there is an age for her first visit (meaning that she has had at least one visit). For women who had visits in the last 12 months (FPYRTOT greater than 0), the file is sorted by whether she had visits in the last 12 months to a private doctor (FPYRMD), a clinic (FPYRCLIN), or a counselor (FPYRCOUN).

If she had no visits in the last 12 months and was under 25 years of age, (FPYRTOT = 0 and AGE 15–44), the file is sorted by the source of service for her first visit (FP1SRC).

If a woman had no visits in the last 12 months (FPYRTOT = 0) and she was 25–44 years of age, FP1SRC is not applicable, and the file is sorted by the main determinants of source of service (race, poverty level, etc.).

In group 28, the sources of payment for the most recent visit and the services received at that visit are imputed. The "SCREEN" says that these variables are imputed only if the woman has ever had a visit (FP1AGE is not blank). The file is sorted by a number of determinants of these variables.

Group 29 consists of variables about types and sources of gynecological services. Groups 30 through 32 consist of variables on sexually transmitted diseases.

Analysis of imputed variables

If researchers are interested in relating two variables, they should examine the group and sort key definitions in appendix II. If the two variables are in separate groups and if one variable was used as a sort key for imputing the group containing the other variable, then the variables may be safely related. However, if these conditions do not apply, then the imputed values will indicate a weaker relationship between the variables than what was observed just among cases who reported both variables. In this case, the researcher may wish to replace the imputed values with a custom imputation or to use an analysis technique that does not require imputation (9).

Simply repeating the analysis with and without cases having imputed values provides a general indicator of danger but can be misleading. Much depends on the detail of the analysis. If for example, the relationship of the two variables is examined at the total level, across all levels of all other variables, then it would not be surprising for analysis with and without imputed values to give different results. The analysis with the imputed values will, it is hoped, be better. On the other hand, if the effects of all known confounders are carefully controlled and there is still a difference between the two analyses, this could be a genuine indicator of a problem with the imputed cases. In developing a custom imputation, it will be useful to analyze the structure of the missing data to determine how frequently just one variable is missing, how frequently both are missing, and which variables seem to be associated with missing one or both variables. For more details, see (9).

Alternate imputation methodologies

Those familiar with imputation may wonder why hot-deck imputation was used instead of mean or regression imputation, among other possible alternatives. By using hot-deck imputation, the clustering of values associated with mean imputation or regression imputation was avoided. (The problems of value clustering can be solved for regression imputation by adding random residuals, but creating these models is very labor intensive. For more details, see (9).) The serial feature provided for far greater consistency between variables than what would have been possible with a single imputation run. Most of the inconsistencies that typically appear after imputation were prevented.

Variance estimation

Background

Variance is a measure of how unreliable a statistic is because of random events that could not be controlled by the data collector. Because different researchers have different conceptions of which events are fixed and which are random, the term "variance" can mean several different things. In this report, variance is defined in terms of the randomness that arises because a sample was interviewed rather than the whole population. This is known more technically as the "design variance." If the whole population were selected, this variance would be zero.

The standard error (SE) is the square root of the design variance and is primarily a measure of sampling error. The chances are about 68 in 100 (about 2 out of 3) that an estimate from the sample would differ from a complete census by less than the standard error. The chances are about 95 in 100 that the difference would be less than twice the standard error.

The relative standard error (RSE) of an estimate is obtained by dividing the standard error of the estimate by the estimate itself and is often expressed in percentage points. The RSE may be viewed as a measure of the reliability of an estimate. In NCHS reports based on the NSFG, estimates (percents) that have a relative standard error of 30 percent or more are marked with an asterisk to point out their unreliability.

Another variance may be thought of in terms of the basic unpredictability of human behavior. Models can be developed to predict whether a woman of given race and age with given education will or will not have children, but it is impossible to develop a model that will infallibly predict such behavior. Even if the whole population was interviewed, this variance, known as "model variance," would remain.

In this report, the primary concern is the estimation of design variance. This is consistent with the choice of weights to give design-unbiased estimates, as discussed in the section on estimation. The authors recommend that models be fitted taking the sample design into account (that is, by using the weights) and that design variances be calculated using the methods described below. It is important to note that use of the weights when fitting models increases the unreliability of estimated model parameters. When the weights have been used, it is thus particularly dangerous to rely on variances provided by standard statistical computer software packages or variances calculated with formulas from elementary textbooks. Even if the weights are not used in fitting models, the clustering of the sample induces dependencies between the observations that will render variance estimates based on the assumption of a simple random sampling too small.

Summary

Extra weights have been provided on the datafile that simplify the normally complex task of estimating design variances. These weights are known as "replicate weights." The user has only to carry out the following steps to estimate the variance on, for example, the number of women who use the Pill. First, an estimate is created using the regular final weight. Then, 100 additional estimates are created using each of the replicate weights. (The estimated number of women on the Pill will vary from replicate to replicate.) Each replicated estimate is subtracted from the full-sample estimate, and each difference is squared. The squared differences are added up. Finally, the sum of squared differences is divided by 100. This average squared difference is usually a reasonable estimate of the variance.

The formula for the variance estimator is

$$\frac{1}{100} \sum_{j=1}^{100} \left(\sum_{i=1}^{8,450} W_{ij} X_i - \sum_{i=1}^{8,450} W_{i0} X_i \right)^2$$

where W_{i0} is the final sample weight (labeled W_5 in this report and on the file layouts) for the ith case, W_{ij} is the jth replicate weight for the ith case, $\sum$ indicates summation, and X_i is the characteristic of interest for the ith case.

This method is very flexible; it may be used to estimate the variance on practically any statistic. All that is required is to estimate the statistic 101 times (once with the final weight and once for each of the 100 replicates) and then compute the average of the squared differences from the full sample estimate. Westat has software known as WESVAR and WESREG, which is a Westat-written SAS procedure, that will carry these steps out automatically. Westat will provide executable copies for IBM and VAX machines (no source code) at the cost of a

tape. The NCHS also has suitable software that can be used for this purpose.

Derivation of replicate weights

The task of estimating (design) variances has been rendered straightforward for Cycle IV through the provision of replicate weights. Generally, estimating (design) variances for surveys with complex designs and complex estimation procedures is very difficult. Use of the Cycle IV replicate weights avoids most of the difficult technical issues (10).

There are many competing techniques for estimating variances from complex samples. One that has met with considerable success is called "balanced half-sample replication" or "balanced repeated replication" (BRR). It is part of a larger family of schemes known as "resampling techniques." All of these techniques involve the assumption that the sample had been selected a little differently. The difference between the actual estimate and the hypothetical estimate can be manipulated to give a reasonable estimate of variance. Another type of procedure is known as Taylor linearization.

Recent research has shown that, although the jackknife and Taylor linearization techniques generally give the best results for many common statistics, they are not as robust to severe nonlinearity as BRR. The median is one example of a nonlinear statistic for which BRR provides better results than the jackknife or Taylor procedures. BRR was chosen over the jackknife because of its robustness and over Taylor linearization because of its flexibility. Users of the data would have to spend a great deal of effort on software development to implement Taylor linearization. For BRR (and other resampling methods), software development is minimal. On balance, it was felt that the savings in software development would offset the higher run-time expense of using BRR.

The sample is divided into 200 clusters known as "variance clusters." These 200 clusters are arranged into 100 pairs known as "variance strata." One cluster from each pair is temporarily put aside, and the second cluster in each pair receives a double unbiased weight. Using the resulting half sample, all stages of estimation are repeated. This results in a new set of weights. This set of weights is called "replicate weight #1." The process is repeated with a different half sample resulting in replicate weight #2. With the 100 pairs, there are 2^{100} possible half samples (or 1.27×10^{30}). Mathematical theory shows, however, that it is only necessary to repeat the process for a special set of all half samples. Such a set is known as a "balanced set." A set of 100 balanced half samples was used to estimate variances.

How to form the variance clusters and group them into pairs (variance strata) are critical and complex decisions. Deciding on the overall number of variance clusters and strata is also critical. Ideally, each first-stage unit would be a variance cluster. That means that every NSR PSU and every neighborhood cluster (units from the same block or small cluster of blocks) in every SR PSU would be a separate variance cluster. This would, however, mean creating more than a thousand replicate weights. The costs of using that many weights would be prohibitive. As a compromise, 200 variance clusters were formed. One hundred replicates should result in very good variance estimates while not overburdening computer budgets.

Variance clusters and strata

A systematic procedure was used to assign clusters in SR PSU's to variance clusters and strata. The clusters were sorted by region and PSU and, within PSU, by the order of selection assigned by the U.S. Bureau of the Census when first selecting the clusters for the NHIS. Variance stratum codes were then assigned in the following pattern: 1, 1, 1, 1, 2, 2, 2, 2, 3, 3, 3, 3, . . ., 57, 57, 57, 57, 58, 58, 58, 58, 1, 1, 1, 1, ... Using the same sort, variance clusters were assigned in the following pattern: A, B, A, B, A, ... This rather unintuitive procedure reduces the variance on variance estimates for statistics that are geographically clustered. By following the systematic pattern that was used to select the sample in the first place, the variance estimate captures some of the increased reliability because of systematic selection. Groups of two can be used instead of groups of four. Groups of four were used in this instance to protect against sampling and nonresponse wiping out one variance cluster entirely.

As mentioned in the section on sample selection, some PSU's were only used to supplement the sample of black women. Leaving these PSU's aside, the assignment of NSR PSU's to variance strata was fairly straightforward because the NSR PSU's had previously been assigned to pseudostrata by NCHS for purposes of sample reductions. Each pseudostratum was generally taken to be a variance stratum. The only exceptions involved some special cases where one PSU in a pseudostratum was self-representing for the NSFG and the other was non-self-representing. The NSR PSU's from these pseudostrata were assigned to other variance strata. The assignment of general NSR PSU's to variance strata is shown in table 4.

Returning to the black-only NSR PSU's, one of these PSU's was selected from each pseudostratum. Leaving them there for variance estimation would result in variance estimates that were much too high, because the two halves of a variance stratum would be unbalanced with respect to black sample cases. To rectify this situation, these PSU's were assigned in pairs to the variance strata defined for general NSR PSU's, as shown in table E. One PSU from each pair was assigned to each variance cluster within each variance stratum to maintain balance. A further complication was caused by the odd numbers of black-only PSU's in one census region. Rather than collapsing across regions, these PSU's were assigned in triplets. The variance estimates resulting from this scheme are expected to be a little too high, but the effect is not thought to be serious.

Table E. Assignment of black-only primary sampling units to variance strata for the 1988 National Survey of Family Growth

Variance stratum	Variance cluster	
	A	B
61.	051	061
63.	071	081
65.	091	101
67.	161	171
69.	181	191
71.	$\left\{ \begin{matrix} 201 \\ 221 \end{matrix} \right\}$	211
73.	231	241
75.	251	261
77.	311	321
78.	341	351
80.	361	371
82.	381	391
84.	401	411
86.	421	431
88.	441	451
90.	461	471
92.	481	491
94.	501	511
95.	551	571
97.	581	591
99.	$\left\{ \begin{matrix} 601 \\ 621 \end{matrix} \right\}$	611

Perturbation factors

As mentioned above, the standard procedure for implementing BRR is to multiply the baseweights of each half sample by two. However, there were some exceptions. The NSR PSU's were originally selected in pairs for the NHIS with two from each stratum. To allow sample reductions, the strata were grouped into pseudostrata, each containing four sample NSR PSU's. The NSFG used three of these four PSU's, two for general sampling and one for sampling of blacks only. Because the PSU's were selected independently from disjoint strata, there is no between-stratum component of variance. However, assigning the NSR PSU's in a pseudostratum to opposite variance clusters results in variance estimates which include a between-stratum (within pseudostratum) component. This bias can be reduced by using factors different from two on the half samples. Also, when there are three PSU's in the same variance stratum, it is helpful to use a factor different from 2 on the half samples.

For variance strata 1 through 58, the factor was a constant 2 for both half samples. For example, if the trimmed baseweight is 2,000 for a case in variance stratum 3, then the perturbed weight will be either 4,000 or 0, depending on the replicate.

The factor was also exactly 2 for most of the black-only PSU's in variance strata 59 through 100. The only exceptions are pseudo-PSU's 201, 211, 221, 601, 611, and 621. These PSU's were added in triplets. For pseudo-PSU's 201, 221, 601, and 621, the factor is 1.50. For pseudo-PSU's 211 and 611, the perturbation factor is 3.

For the other NSR PSU's in variance strata 59 through 100, the factor varied across variance strata and clusters (but was constant across replicates for a given variance stratum and cluster). The factor for each pair or triplet of PSU's is given in table 4. These factors were calculated with the formula:

S_i/S_{iA} for cluster A and

S_i/S_{iB} for cluster B

where S_{iA} is the population of the strata represented by the PSU or PSU's in cluster A, S_{iB} is similarly defined, and S_i is the total population represented by the PSU's in the ith variance stratum ($S_i = S_{iA} + S_{iB}$).

Replicating stages of adjustment

Nonresponse adjustments were recomputed for each of the 100 replicates using the perturbed trimmed base-weights and the original nonresponse adjustment cells. A separate nonresponse propensity model could have been developed for each replicate, but this was not deemed to be worth the considerable effort. An additional reason for not replicating the model building was that the base-weights were not used in developing the model.

Poststratification was then repeated for each of the 100 replicate samples. An extra complication was caused by the fact that the CPS controls by marital status and parity are themselves subject to variance. Controlling every replicate to the same CPS controls would lead to underestimates of variance. To properly reflect the CPS variance, 100 sets of pseudo-CPS controls were developed that varied in about the manner that the CPS estimates would vary if the CPS baseweights were perturbed in the same fashion as the NSFG baseweights.

Current Population Survey: Pseudocontrols

The poststratification procedure only used the marginal controls of marital status and parity within race and age. It did not use the full four-way classification from CPS. However, to create the pseudocontrols, the full cross-tabulation was simulated. Let $c = (c_1, \ldots, c_{10})$ be the CPS estimates for parity by marital status within a specific race-by-age cell. (There is a maximum of 10; some cells have only 2 elements.) Using CPS generalized variance parameters, an estimate of Σ_c, the variance-covariance matrix of c was computed. Using a pseudo-random number generator, 100 observations were then obtained from the multivariate normal distribution with mean c and variance Σ_c. Negative estimates were reset to zero.

Summing the components of one of these 100-vector observations never yielded the exact control for the whole race-by-age cell. Because those controls are obtained by demographic methods not subject to sampling variance, a further adjustment was required. The 10 elements were ratio-adjusted to the demographic total. The needed marginals were then obtained by summing the ratio-adjusted elements.

Shortcut method for numbers and percents

For users who wish to obtain variance estimates for total numbers or percents of women or their pregnancies, there is an easier method than replicating estimates. Formulas are given in this section that will provide suitable estimates for many applications.

These estimates were derived by designing tables using a number of important dependent and independent variables. Sampling errors were calculated for the estimated numbers in these tables. The ratios of the variance to the square of the estimated number were plotted against the inverse of the estimated number, and a weighted least-squares line was fit to those points. The intercept and slope for these lines are given in this report; they can be used to estimate the standard errors of percents and weighted numbers from the NSFG.

To produce approximate standard errors for the NSFG estimates, first determine the type of characteristic to be estimated, that is, the parameter set in table F to be used. The reader must then determine the type of estimate that is needed. The type of estimate corresponds to three rules.

Table F. Estimated standard error parameters for the 1988 National Survey of Family Growth

Parameter set	Characteristic	Estimated parameters a	b
I	Number of pregnancies for women of all races, or women who were not black	−0.000047	13,216
II	Number of pregnancies for black women	−0.000961	4,407
III	Number of women of all races or of women who were not black	−0.00018	10,738
IV	Number of black women	−0.000626	5,181
V	Number of women in any combination of the poststratification cells in table 3.	0	0

Rule 1. Use for estimated number of women or pregnancies — For the estimated number of women for whom data are published in this report, there are two cases to consider. For the first case, if the estimated number is any combination of the poststratification cells in table 3, then its value has been adjusted to official U.S. Bureau of the Census figures and its standard error is assumed to be 0.0. This corresponds to parameter set V in table F. As an example, this would be the case for the number of women 15–44 years of age; the number of black women 15–44 years of age; the number of never-married or ever-married women; or the number of women in any 5-year age group. Although the race class "white" is not specifically adjusted to U.S. Bureau of the Census figures, it dominates the poststratification class of women who were not black; consequently, subgroups of white women can be treated as the corresponding subgroup of women who were not black in table L for the purpose of approximating standard errors.

For the second case, the standard errors for all other estimates of numbers of women or pregnancies, such as the number of women using the Pill, are approximated by using the parameters provided in table F and formula 1 below.

If the estimated number x for a characteristic has associated parameters a and b, then the approximate standard error for x, SE(x), can be computed by the formula

$$SE(x) = \sqrt{ax^2 + bx} \qquad (1)$$

See also tables G–K, in which this formula is evaluated at many common levels.

Table G. Approximate relative standard errors and standard errors for estimated number of women of all races or of women who were not black: 1988 National Survey of Family Growth

Size of estimate	Relative standard error in percent	Standard error
100,000	32.0	32,000
250,000	20.4	51,000
500,000	14.4	72,000
1,000,000	10.2	102,000
5,000,000	4.4	221,000
10,000,000	3.0	298,000
20,000,000	1.9	377,000
30,000,000	1.3	400,000
50,000,000	0.6	294,000
58,000,000	0.2	131,000

Table H. Approximate relative standard errors and standard errors for estimated number of black women: 1988 National Survey of Family Growth

Size of estimate	Relative standard error in percent	Standard error
100,000	22.0	22,000
250,000	14.0	35,000
500,000	9.8	49,000
1,000,000	6.7	67,000
5,000,000	2.0	101,000
7,500,000	0.8	60,000

Table J. Approximate relative standard errors and standard errors for estimated number of pregnancies to women of all races or to women who were not black: 1988 National Survey of Family Growth

Size of estimate	Relative standard error in percent	Standard error
100,000	36.0	36,000
250,000	22.8	57,000
500,000	16.2	81,000
1,000,000	11.4	114,000
5,000,000	5.1	254,000
10,000,000	3.6	357,000
20,000,000	2.5	495,000
30,000,000	2.0	595,000
50,000,000	1.5	737,000
75,000,000	1.1	852,000
100,000,000	0.9	922,000

Table K. Approximate relative standard errors and standard errors for estimated number of pregnancies to black women: 1988 National Survey of Family Growth

Size of estimate	Relative standard error in percent	Standard error
100,000	21.0	21,000
250,000	14.0	35,000
500,000	9.8	49,000
1,000,000	7.3	73,000
5,000,000	4.3	214,000
10,000,000	3.7	374,000
15,000,000	3.5	531,000

Example of rule 1—The estimated number of women using the Pill is 10,734,000. From table F, parameter set III, the *a* and *b* parameters for the numbers of women are -0.00018 and 10,738. Using formula (1), the estimated standard error is

$$\sqrt{(-0.00018)(10,734,000)^2 + (10,738)(10,734,000)} = 307,000$$

An approximate 95-percent confidence interval for the number of women using the Pill is 10,734,000 ± (1.96) (307,445).

Rule 2. For rates, proportions, and percents when the denominator is generated by the poststratification classes (table 3)—In this case, the denominator has no sampling error. For example, rule 2 would apply to the estimated percent of women using the Pill in a combination of the poststratification cells. Approximate standard errors for such estimates can be computed using the *a* and *b* parameters in table F along with formula (2) below.

If the estimate of a rate, proportion, or percent *p* is the ratio of two estimated numbers $p = x/Y$ (where *p* may be inflated by 100 for percents or 1,000 for rates per 1,000 women), with *Y* having no sampling error, then the approximate standard error for *p* is given by the formula

$$SE(p) = p\sqrt{a + \frac{b}{x}} \tag{2}$$

See also tables L and M, in which this formula is evaluated for many common values.

Example of rule 2—The estimated proportion of all women 15–44 years of age using the Pill in 1988 was 15.6 percent. From table F, parameter set III, the parameters *a* and *b* for number of women are -0.00018 and 10,738, respectively. Using formula (2), the estimated standard error for the percent is

$$15.6\sqrt{(-0.00018) + \frac{10,738}{10,734,000}} = 0.4$$

An approximate 95-percent confidence interval for the percent of women using the Pill is 15.6 ± (1.96) (0.4), or 14.8 to 16.4 percent.

Rule 3. Proportions and percents when the denominator is not generated by the poststratification classes—If *p* represents an estimated percent, *b* is the parameter from table F associated with the numerator characteristics and *y* is the number of persons in the denominator on which *p* is based, then the standard error of *p* may be approximated by

$$SE(p) = \sqrt{\frac{bp(100-p)}{y}} \tag{3}$$

(If *p* is a proportion, then the above formula can be used, but with 100 replaced by 1.0.) See tables N–Q, in which this formula is evaluated at many common levels.)

Example of rule 3—An estimated 30.7 percent of contraceptors were using the Pill in 1988. This percent is based on the estimated denominator of 34,912,000 women using contraception. From table F, parameter set III, parameter *b* is 10,738. Using formula (3), the standard error for the percent is

$$\sqrt{\frac{(10,738)(30.7)(100 - 30.7)}{34,912,000}} = 0.8 \text{ percent}$$

An approximate 95-percent confidence interval for the percent of contraceptors using the Pill is 30.7 ± (1.96) (0.8), or 29.1 to 32.3 percent.

Table L. Approximate standard errors for estimated percents expressed in percentage points for numbers of women of all races or of women who were not black: 1988 National Survey of Family Growth

Numerator of percent	Estimated percent									
	5	10	20	30	40	50	60	70	90	95
	Standard errors in percentage points									
100,000	1.6	3.3	8.5	9.8	13.1	16.4	19.6	22.9	29.5	31.1
250,000	1.0	2.1	4.1	6.2	8.3	10.3	12.4	14.5	18.6	19.6
500,000	0.7	1.5	2.9	4.4	5.8	7.3	8.8	10.2	13.1	13.9
1,000,000	0.5	1.0	2.1	3.1	4.1	5.1	6.2	7.2	9.2	9.8
5,000,000	0.2	0.4	0.9	1.3	1.8	2.2	2.7	3.1	4.0	4.2
10,000,000	0.1	0.3	0.6	0.9	1.2	1.5	1.8	2.1	2.7	2.8
50,000,000	0.0	0.1	0.1	0.2	0.2	0.3	0.4	0.4	0.5	0.6

Example of use of table L: If 20 percent of women in a category defined by 5-year age groups, race, ever versus never married, or parity had used family planning services in the last 12 months and the numerator of that percent was 5,000,000, then the 20-percent column and the 5,000,000 row indicate that one standard error is 0.9 percentage points and 2 standard errors are twice that, or 1.8 percentage points. Therefore, a 95-percent confidence interval on the percentage would extend from 18.2 to 21.8 percent (20.0 percent plus or minus 1.8 percent). In addition, the relative standard error of that 20-percent estimate is 4.5 percent (0.9 percent divided by 20 percent).

NOTE: The numerator but not the denominator is estimated from the survey.

Table M. Approximate standard errors for estimated percents expressed in percentage points for numbers of black women: 1988 National Survey of Family Growth

Numerator of percent	Estimated percent									
	5	10	20	30	40	50	60	70	90	95
	Standard error in percentage points									
100,000	1.1	2.3	4.5	6.8	9.0	11.3	13.6	15.8	20.4	21.5
250,000	0.7	1.4	2.8	4.3	5.7	7.1	8.5	9.9	12.8	13.5
500,000	0.5	1.0	2.0	3.0	3.9	4.9	5.9	6.9	8.9	9.4
1,000,000	0.3	0.7	1.3	2.0	2.7	3.4	4.0	4.7	6.1	6.4
5,000,000	0.1	0.2	0.4	0.6	0.8	1.0	1.2	1.4	1.8	1.9
7,000,000	0.1	0.1	0.2	0.3	0.4	0.5	0.6	0.7	1.0	1.0

Example of use of table M: If 40 percent of black women in a category defined by 5-year age groups, ever versus never married, or parity had used family planning services in the last 12 months and the numerator of that percent was 1,000,000, then the 40-percent column and the 1,000,000 row indicate that one standard error is 2.7 percentage points and 2 standard errors are twice that, or 5.4 percentage points. Therefore, a 95-percent confidence interval on the percentage would extend from 34.6 to 45.4 percent (40.0 percent plus or minus 5.4 percent). In addition, the relative standard error of that 40-percent estimate is 6.75 percent (2.7 percent divided by 40 percent).

NOTE: The numerator but not the denominator is estimated from the survey.

Table N. Approximate standard errors for estimated percents expressed in percentage points for numbers of women of all races or of women who were not black: 1988 National Survey of Family Growth

Base of percent	Estimated percent						
	2 or 98	5 or 95	10 or 90	20 or 80	30 or 70	40 or 60	50
	Standard error in percentage points						
100,000	4.6	7.1	9.8	13.1	15.0	16.1	16.4
250,000	2.9	4.5	6.2	8.3	9.5	10.2	10.4
500,000	2.1	3.2	4.4	5.9	6.7	7.2	7.3
1,000,000	1.5	2.3	3.1	4.1	4.7	5.1	5.2
5,000,000	0.6	1.0	1.4	1.9	2.1	2.3	2.3
10,000,000	0.5	0.7	1.0	1.3	1.5	1.6	1.6
20,000,000	0.3	0.5	0.7	0.9	1.1	1.1	1.2
30,000,000	0.3	0.4	0.6	0.8	0.9	0.9	0.9
50,000,000	0.2	0.3	0.4	0.6	0.7	0.7	0.7
58,000,000	0.2	0.3	0.4	0.5	0.6	0.7	0.7

Example of use of table N: If 30 percent of women in a specific category not defined by age, race, ever versus never married or parity, such as low-income women were using the Pill and the base (denominator) of that percent was 1,000,000, then the 30-percent column and the 10,000,000 row indicate that one standard error is 1.5 percentage points and two standard errors are twice that, or 3.0 percentage points. Therefore, a 95-percent confidence interval on the percentage would extend from 27.0 to 33.0 percent (30.0 percent plus or minus 3.0 percent). In addition, the relative standard error of that 30-percent estimate is 5.0 percent (1.5 percent divided by 30 percent).

NOTE: The numerator and the denominator are both estimated from the survey.

Table O. Approximate standard errors for estimated percents expressed in percentage points for numbers of black women: 1988 National Survey of Family Growth

Base of percent	Estimated percent						
	2 or 98	5 or 95	10 or 90	20 or 80	30 or 70	40 or 60	50
	Standard error in percentage points						
100,000	3.2	5.0	6.8	9.1	10.4	11.2	11.4
250,000	2.0	3.1	4.3	5.8	6.6	7.1	7.2
500,000	1.4	2.2	3.1	4.1	4.7	5.0	5.1
1,000,000	1.0	1.6	2.2	2.9	3.3	3.5	3.6
5,000,000	0.5	0.7	1.0	1.3	1.5	1.6	1.6
7,500,000	0.4	0.6	0.8	1.1	1.2	1.3	1.3

Example of use of table O: If 30 percent of black women in a category not defined by age, race, marital status, or parity (such as low-income black contraceptive users) were using the Pill, and the base of that percent was 1,000,000, then the 30-percent column and the 1,000,000 row indicate that one standard error is 3.3 percentage points and 2 standard errors are twice that, or 6.6 percentage points. Therefore, a 95-percent confidence interval on the percentage would extend from 23.4 to 36.6 percent (30.0 percent plus or minus 6.6 percent). In addition, the relative standard error of the 30-percent estimate is 11 percent (3.3 percent divided by 30 percent).

NOTE: The numerator and the denominator are both estimated from survey.

Table P. Approximate standard errors for estimated percents expressed in percentage points for pregnancies to women of all races or to women who were not black: 1988 National Survey of Family Growth

	Estimated percent						
Base of percent	2 or 98	5 or 95	10 or 90	20 or 80	30 or 70	40 or 60	50
	Standard error in percentage points						
100,000	5.1	7.9	10.9	14.5	16.7	17.8	18.2
250,000	3.2	5.0	6.9	9.2	10.5	11.3	11.5
500,000	2.3	3.5	4.9	6.5	7.5	8.0	8.1
1,000,000	1.6	2.5	3.4	4.6	5.3	5.6	5.7
5,000,000	0.7	1.1	1.5	2.1	2.4	2.5	2.6
10,000,000.	0.5	0.8	1.1	1.5	1.7	1.8	1.8
20,000,000.	0.4	0.6	0.8	1.0	1.2	1.3	1.3
30,000,000.	0.3	0.5	0.6	0.8	1.0	1.0	1.0
50,000,000.	0.2	0.4	0.5	0.7	0.7	0.8	0.8
75,000,000.	0.2	0.3	0.4	0.5	0.6	0.7	0.7
100,000,000.	0.2	0.3	0.3	0.5	0.5	0.6	0.6

Example of use of table P: If 60 percent of babies born to women in a certain category were breast fed and the base of that percent was 10,000,000 births, then the 60-percent column and the 10,000,000 row indicate that one standard error is 1.8 percentage points and 2 standard errors are twice that, or 3.6 percentage points. Therefore, a 95-percent confidence interval on the percentage would extend from 56.4 to 63.6 percent (60.0 percent plus or minus 3.6 percent). In addition, the relative standard error of the 60 percent estimate is 3 percent (1.8 percent divided by 60 percent).

Table Q. Approximate standard errors for estimated percents expressed in percentage points for pregnancies to black women: 1988 National Survey of Family Growth

	Estimated percent						
Base of percent	2 or 98	5 or 95	10 or 90	20 or 80	30 or 70	40 or 60	50
	Standard error in percentage points						
100,000	2.9	4.6	6.3	8.4	9.6	10.3	10.5
250,000	1.9	2.9	4.0	5.3	6.1	6.5	6.6
500,000	1.3	2.0	2.8	3.8	4.3	4.6	4.7
1,000,000	0.9	1.4	2.0	2.7	3.0	3.3	3.3
5,000,000	0.4	0.6	0.9	1.2	1.4	1.5	1.5
10,000,000.	0.3	0.5	0.6	0.8	1.0	1.0	1.0
25,000,000	0.2	0.4	0.5	0.7	0.8	0.8	0.9

Example of use of table Q: If 10 percent of births to black women in a certain category were low birth weight and the base of that percent was 5,000,000 births, then the 10-percent column and the 5,000,000 row indicate that one standard error is 0.9 percentage points and two standard errors are twice that, or 1.8 percentage points. Therefore, a 95-percent confidence interval on the percentage would extend from 8.2 to 11.8 percent (10.0 percent plus or minus 1.8 percent). In addition, the relative standard error of the 10 percent estimate is 9 percent (0.9 percent divided by 10 percent).

Hypothesis tests

An estimate of the standard error of the difference, $X-Y$, between any two aggregates or percents is given by

$$SE(X-Y) = \sqrt{SE(X)^2 + SE(Y)^2}$$
$$= \sqrt{(X^2)RSE\ (X)^2 + (Y^2)RSE(Y)^2}$$

This expression provides a good estimate of the standard error for uncorrelated statistics, but it can be considered only a rough approximation otherwise.

Because estimates from the 1988 National Survey of Family Growth are based on a large sample of women and because the variance estimates were based on such a large number of replicates (100), the test statistics

$$t = \frac{X - Y}{SE(X-Y)}$$

will be approximately normally distributed ((10), appendix B). Therefore, individual two-tailed significance tests of differences between statistics from Cycle IV data can be performed with an approximate significance level of alpha by computing t and comparing it with the two-tailed 1-α critical value for the normal distribution.

Example: In 1988, 33.92 percent of pregnancies to black women ended before 20 years of age, although the corresponding percent for other women was 18.51. To test whether this racial difference is significant at the 0.05 level of significance, compute

$$t = \frac{33.92 - 18.51}{\sqrt{(33.92)^2 \cdot RSE^2\ (33.92) + (18.51)^2 \cdot RSE^2\ (18.51)}}$$

Relative standard errors are computed using the appropriate values for b from table K as follows:

$$RSE\ (18.51) = \sqrt{\frac{(13,216)\ (100 - 18.51)}{(18.51)\ (80,842,470)}}$$
$$= 0.027$$

and

$$RSE\ (33.92) = \sqrt{\frac{4,407\ (100 - 33.92)}{(33.92)\ (14,481,078)}}$$
$$= 0.024$$

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thus

$$t = \frac{33.92 - 18.51}{\sqrt{(33.92)^2 (0.027)^2 + (18.51)^2 (0.044)^2}}$$

$$= 12.5$$

The two-tailed critical value for a normal statistic and a significance level of 0.05 is 1.96. Therefore, the difference is significant at the .05 level.

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List of detailed tables

Table 1. Cells for redistribution of trimmed weights, by race, substratum type, number of eligible women in household, and interview type: 1988 National Survey of Family Growth

Race, substratum type, number of eligible women, and interview type	Cell number
Black, substratum 2	
Multiple eligible women	
Special followup interview	1
Other.	2
One eligible woman	
Special followup interview	3
Other.	4
Black, substratum 0 or 1	
Multiple eligible women	
Special followup interview	5
Other.	6
One eligible woman	
Special followup interview	7
Other.	8
Other than black, substratum 2	
Three eligible women or more	
Special followup interview	9
Other.	10
One or two eligible women	
Special followup interview	11
Other.	12
Other than black, substratum 0 or 1	
Three eligible women or more	
Special followup interview	13
Other.	14
One or two eligible women	
Special followup interview	15
Other.	16

NOTE: ED is enumeration district. Substratum 0 consists of housing units selected from new construction permits. Substrata 1 and 2 consist of housing units selected by area listing techniques. Substratum 1 had higher concentrations of black women in 1980 than substratum 2 and was therefore oversampled for the National Health Interview Survey.

Table 2. Nonresponse adjustment cells used in the 1988 National Survey of Family Growth (NSFG)

Item	Number	Response rate (percent)	Item	Number	Response rate (percent)
I. Initial nonresponse cells			AND number of children in the household = 0.	2,271	80
1. Education is unknown or no completed grades .	91	33	(aa) AND education = 1–12 years	1,148	76
2. ELSE: Housing unit type is motel, hotel, or rooming house	21	48	AND region = Northeast	234	66
3. ELSE: Refusal on height, weight, education, health status, or major activity; or racial background is unknown	148	70	25. AND number of calls to complete the NHIS = 1 or 2	143	74
4. ELSE: Has telephone but no telephone number is given.	333	61	26. AND number of calls to complete the NHIS = 3 or more.	91	54
5. ELSE: Marital status, income, or class of worker is unknown	359	72	AND region = South	467	82
6. ELSE: The NHIS interview length was 15 minutes or less.	103	76	27. AND keeping house or attending school	150	87
7. ELSE: Interview is 2 or more weeks late	202	68	28. AND working	317	80
8. ELSE: Asian or Pacific Islander	182	72	AND region = Midwest	298	77
9. ELSE: Cuban	39	74	29. AND the NHIS respondent = self	196	83
10. ELSE: Never worked but in labor force or worked without pay	64	73	30. AND the NHIS respondent = proxy	102	64
			31. AND region = West.	149	72
II. The NHIS contact person's telephone number was refused:	1,587	74	(bb) AND education = 13–15 years.	662	82
			32. AND refused to give social security number . . .	74	72
A. AND refused own social security number	475	65	33. AND woman did not refuse to give social security number	588	83
AND class of worker = employed by government	62	74	(cc) 34. AND education = 16 years or more	461	88
11. AND number of own children in household = 0	28	61			
12. AND number of own children in household = 1 or more.	34	85	AND number of own children in household = 1 or more	4,125	87
AND class of worker = not in labor force	138	74	(aa) AND type of place = central city of MSA of 1 million	841	82
13. AND type of place = central city of an MSA of 1 million or more.	48	63	35. AND education = 1–12 years.	571	79
14. AND type of place = central city of an MSA of less than 1 million or in the suburban area of an MSA.	58	74	36. AND education = 13–15 years	177	85
15. AND type of place = nonmetropolitan or rural. .	32	91	37. AND education = 16 years or more	93	88
16. AND woman is employed in private sector or is under 18 years of age	275	59	(bb) AND type of place = suburb of large MSA or central city of smaller MSA	1,810	86
			AND respondent = self	1,555	87
B. AND gave own social security number:	873	79	38. AND woman is Hispanic	93	81
(a) AND number of visits to complete the NHIS interview = 1 or 2.	484	83	AND woman is not Hispanic.	1,462	88
17. AND region = Northeast or West.	207	77	39. AND woman is not in labor force or works for Government.	703	90
18. AND region = Midwest or South	277	87	40. AND sampled woman works in private sector or is under 18 years of age	759	86
(b) AND number of visits to complete the NHIS interview = 3–5	299	78	41. AND respondent for the NHIS = proxy	255	82
19. AND sampled woman completed 1–12 years of school	174	72	(cc) AND type of place = rural parts of MSA or nonmetropolitan	1,474	91
20. AND woman completed 13 or more years of school	125	85	AND respondent = self	1,271	92
(c) AND number of visits to complete the NHIS interview = 6 or more	90	63	42. AND woman refused social security number . .	240	83
21. AND marital status = married.	37	76	43. AND woman did not refuse social security number.	1,234	92
22. AND marital status = not married	53	55			
			B. AND woman is under 18 years of age	1,041	81
C. AND social security number not asked because woman is under 18 years of age	239	72	44. AND woman is Hispanic	66	91
23. AND respondent = 17 years of age	47	87	AND woman is not Hispanic	975	80
24. AND respondent is 15 or 16 years of age	192	68	(a) AND number of calls to complete the NHIS = 1 or 2	627	82
			45. AND the NHIS respondent = self	44	86
III. The NHIS contact person's telephone number was answered (NOT refused):	7,437	84	46. AND the NHIS respondent = proxy	85	72
			47. AND woman is under 17 years of age.	498	84
A. AND sampled woman is 18 years of age or older . . .	6,396	85	(b) AND number of calls to complete the NHIS = 3–5.	295	78
			48. AND type of place = central city of large MSA .	60	68
			49. AND type of place = suburb, or central city of smaller MSA.	130	75
			50. AND type of place = rural or nonmetropolitan. .	105	86
			(c) 51. AND number of calls to complete the NHIS = 6 or more	53	70

NOTES: Characteristics of sampled women were obtained from the National Health Interview Survey. Cells, or groups, actually used for the nonresponse adjustment are shown with numbers 1–51 in table 2. MSA is metropolitan statistical area. NHIS is National Health Interview Survey.

Table 3. Number of persons in poststratification cells, by race, age, parity, and marital status: 1988 National Survey of Family Growth

Age, race, parity, and marital status	Control total from the Current Population Survey	Age, race, parity, and marital status	Control total from the Current Population Survey
15–17 years of age		**30–34 years of age – Con.**	
Black	844,000	Black – Con.	
Other than black	4,471,000	Parity = 3	204,000
		Parity = 4 or more	154,000
18–19 years of age		by	
Black		Ever married	957,000
Parity = 0	395,000	Never married	444,000
Parity = 1 or more	147,000	Other than black	
Other than black		Parity = 0	2,465,000
Parity = 0	2,844,000	Parity = 1	1,943,000
Parity = 1 or more	242,000	Parity = 2	3,147,000
		Parity = 3	1,302,000
20–24 years of age		Parity = 4 or more	581,000
Black		by	
Parity = 0	690,000	Ever married	8,164,000
Parity = 1	358,000	Never married	1,273,000
Parity = 2	215,000		
Parity = 3 or more	112,000	**35–39 years of age**	
by		Black	
Ever married	322,000	Parity = 0	150,000
Never married	1,052,000	Parity = 1	269,000
Other than black		Parity = 2	302,000
Parity = 0	5,842,000	Parity = 3	211,000
Parity = 1	1,324,000	Parity = 4 or more	242,000
Parity = 2	778,000	by	
Parity = 3 or more	221,000	Ever married	881,000
by		Never married	292,000
Ever married	3,294,000	Other than black	
Never married	4,871,000	Parity = 0	1,551,000
		Parity = 1	1,458,000
25–29 years of age		Parity = 2	2,993,000
Black		Parity = 3	1,607,000
Parity = 0	444,000	Parity = 4 or more	804,000
Parity = 1	374,000	by	
Parity = 2	350,000	Ever married	7,706,000
Parity = 3	180,000	Never married	707,000
Parity = 4 or more	115,000		
by		**40–44 years of age**	
Ever married	698,000	Black	
Never married	765,000	Parity = 0	139,000
Other than black		Parity = 1	141,000
Parity = 0	4,134,000	Parity = 2	268,000
Parity = 1	2,123,000	Parity = 3	147,000
Parity = 2	2,098,000	Parity = 4 or more	186,000
Parity = 3	785,000	by	
Parity = 4 or more	237,000	Ever married	736,000
by		Never married	145,000
Ever married	6,984,000	Other than black	
Never married	2,392,000	Parity = 0	1,061,000
		Parity = 1	1,072,000
30–34 years of age		Parity = 2	2,602,000
Black		Parity = 3	1,538,000
Parity = 0	250,000	Parity = 4 or more	1,002,000
Parity = 1	384,000	by	
Parity = 2	408,000	Ever married	6,807,000
		Never married	467,000

Table 4. Assignment of non-self-representing primary sampling units (PSU's) to variance strata and clusters: 1988 National Survey of Family Growth

Variance stratum	Variance cluster A		Variance cluster B	
	Pseudo PSU	Perturbation factor	Pseudo PSU	Perturbation factor
59	313	2.64	343	1.61
60	{042 052}	1.52	053	2.92
61	062	2.06	063	1.94
62	072	2.04	073	1.96
63	082	2.01	083	1.99
64	092	1.68	093	2.46
65	102	2.29	103	1.77
66	162	1.76	163	2.32
67	172	2.25	173	1.80
68	182	1.84	183	2.19
69	192	2.49	193	1.67
70	202	2.36	203	1.74
71	212	1.63	213	2.59
72	222	2.19	223	1.84
73	232	2.18	233	1.85
74	242	2.27	243	1.78
75	252	1.99	253	2.01
76	262	1.83	263	2.20
77	322	1.91	323	2.10
78	352	1.96	353	2.04
79	362	1.79	363	2.26
80	372	2.26	373	1.79
81	382	1.99	383	2.01
82	392	1.97	393	2.03
83	402	1.97	403	2.03
84	412	2.46	413	1.68
85	422	1.93	423	2.07
86	432	1.76	433	2.32
87	442	2.05	443	1.95
88	452	1.92	453	2.09
89	462	2.03	463	1.97
90	472	1.95	473	2.05
91	482	1.88	483	2.13
92	492	1.92	493	2.09
93	502	2.00	503	2.00
94	512	2.44	513	1.69
95	572	2.69	{553 573}	1.59
96	582	1.67	583	2.49
97	592	2.36	593	1.74
98	602	2.29	603	1.77
99	612	1.92	613	2.08
100	622	2.01	623	1.99

Appendixes

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Appendix I
Use of Current Population Survey controls in weighting the National Survey of Family Growth

The June supplement to the Current Population Survey (CPS) has questions on fertility and marital status. Because the CPS is based on a far larger sample than the NSFG, the reliability of the NSFG estimates could be improved by adjusting the weights so that the weighted NSFG estimates of fertility and marital status are consistent with the CPS estimates. Sampling theory holds that the reliability of any NSFG estimate can be improved by "controlling" the weights in this manner, provided that the correlation of the characteristic with fertility and marital status is strong enough and that the CPS variance is smaller than the uncontrolled NSFG variance. On the other hand, controlling the weights in this fashion can "import" any biases of the CPS to the NSFG. It was therefore necessary to determine whether the CPS variances were, in fact, smaller than the NSFG variances and whether there was any reason to believe that the CPS biases might be more severe than the NSFG biases.

Current Population Survey background

Through 1988, fertility questions in the CPS were asked once per year, in June. Not everyone who completed the regular survey agreed to also complete the fertility supplement. After nonresponse, there are about 3,400 completed interviews with black women and 27,200 with other women. After controlling for age, the parity distribution is very stable from year to year. (A woman's parity is the number of children ever born to her as of the date of interview.) The distribution is also very consistent with vital statistics. Through 1988 females under 18 years of age were not eligible for the CPS supplement.

Variance comparison

Prior to the intensive nonresponse conversion attempt for Cycle IV of the NSFG, there were 2,718 completed interviews with black women and 5,513 with other women. The CPS has a smaller design effect than the NSFG for several reasons—for example, the CPS does not have different sampling rates by race or by the number of women in the household, features which increase the design effect of the NSFG. Clearly, the CPS also provides a larger sample of women 15–44 years of age than the NSFG, especially for women who are not black (table I).

This larger sample size, along with its smaller design effect, means that the CPS estimates, especially for women who are not black, will have smaller variances than the NSFG estimates.

The sample sizes given in this table for Cycle IV do not include late interviews gained through the special nonresponse conversion effort. These cases reduce the risk of bias but have almost no effect on variance.

Bias comparison

Frame bias

Frame differences can arise from differences in the intended universe and from differences in how the intent is realized. The NSFG, as discussed more fully in the section on design, is based on the National Health Interview Survey (NHIS). The NHIS and the CPS have very similar intended universes; for example, the surveys have the same intended treatment of group housing quarters, temporary housing quarters, military personnel (except in March), immigrants (legal and illegal), homeless people, and most other special populations. The one exception is that the CPS has a lower age limit of 14 years of age for the regular survey and 18 years of age for the fertility supplement. There are no age limits for the NHIS interviews. However, given that frame construction is carried out by very similar personnel groups at the U.S. Bureau of the Census, the realization of frame intents are probably very similar between the two surveys. Very little, if any, difference is thus expected in frame biases between the NSFG and the CPS for those 18 years of age and over.

Nonresponse bias

The CPS and NHIS nonresponse rates are very similar; they are both at 5 percent or less. The NSFG nonresponse in going back to the NHIS households was 18 percent. The CPS nonresponse to the fertility supplement was well under 10 percent. There is thus no reason to think that the CPS nonresponse bias is more severe than the NSFG nonresponse bias. In fact, controlling to the CPS might reduce nonresponse bias as well as variance.

Response bias

Reporting of children ever born does not appear to be sensitive to the particularities of a survey. A wide range of

Table I. Sample sizes for the National Survey of Family Growth (NSFG) and the Current Population Survey (CPS)

	Sample size	
Survey	Black	Other than black
CPS.	3,400	27,200
NSFG IV	2,718	5,513
Ratio	1.3	4.9

surveys have reported parity distributions which are consistent with each other after controlling for various factors (11). According to Census Bureau experts, the CPS parity distributions also agree very well with those reported in the decennial census and with vital statistics.

Reporting of marital status may be more sensitive to the particularities of a survey. Some situations that can cause problems for either survey are common law marriages, annulments, cohabitation, separation, and divorce. The conditions of the interview (such as sex of interviewer, self or proxy response, wording of questions, training of interviewers, and privacy of interview) can easily influence how these situations are reported. Comparability of marital status from survey to survey also depends strongly on how categories are defined. Conversations with experts at NCHS and the U.S. Bureau of the Census led to the conclusion that the most reliable classifications are "ever married" and "never married." The following paragraphs describe the reasons for believing that the response bias for these categories is comparable between the CPS and the NSFG:

- In general, persons who have never been formally married but are cohabiting (unrelated persons of opposite sex living together without legal marriage but in a marriage-like relationship) are classified by both surveys as never married. The NSFG can further classify these persons into cohabiting or not cohabiting, but the CPS cannot. However, this distinction is not important for the categories "ever married" and "never married."

- Divorced persons (cohabiting or living alone) are usually classified by both surveys as ever married. However, there is unpublished evidence that the CPS underreports this category. The underreporting appears to be worse among men (which is irrelevant to the NSFG), but appears to also be present for women. The NSFG uses much more detailed probes to determine marital status than does the CPS. There was some hope that these probes might induce some of these divorced women to change their initial report from never married to ever married. However, after reviewing logbooks of reclassifications based on those probes, Westat found no evidence that more of these women accurately classified themselves in the NSFG than in the CPS. (The logbooks did show that the probing is helpful for distinguishing cohabitation from mere sharing of living quarters. This is, of course, irrelevant to this issue.)

- Separated women are usually classified as ever married by both surveys.

- If a respondent who has never been formally married reports that her relationship to her partner is a common law marriage, then both surveys report the woman as ever married. Neither survey inquires about this category but both report it when it is volunteered. (It is interesting to note that this concept changes from State to State and has no meaning for the Nation as a whole.)

- Neither survey inquires about annulments. However, if an annulment is volunteered, a difference in classification does ensue. The CPS treats it as never married. The NSFG treats it as divorced, and hence as ever married. However, annulments are rare.

One remaining concern was the greater use of proxy data in the CPS. In the CPS, one person is allowed to respond for household members not present at the time of the interview. The NSFG procedures, however, strictly reject all proxy response; only self-response is allowed. However, a consensus was reached that the emphasis on self-reporting is probably not important in distinguishing between ever married and never married. (It may be more important for reporting subcategories of marital status such as separated, but that is not relevant for the dichotomy between ever-married and never-married persons.)

Conclusion

The variances for the CPS are generally smaller than those for the NSFG. There is no reason to believe that the CPS biases are any worse than the NSFG biases, provided that marital status is defined in two categories: ever married and never married. However, despite the superiority of the CPS variances and the seeming comparability of biases, there was some reluctance to simultaneously control to the CPS's estimate for every combination of race, age, parity, and marital status. The reluctance was caused by the very small sample sizes upon which those CPS estimates are based. (For example, the number of married black female teenagers is small enough to make reliable estimation of the parity distribution difficult for the CPS.) As an alternative, an iterative procedure was used, which is described in full in the main text. This procedure ensures agreement with the CPS for race by age, for parity within race and age, and for marital status within race and age. No agreement is ensured for tabulations of marital status by parity.

The CPS estimates are themselves controlled to synthetic demographic estimates of age and race. This control to the CPS estimates induces simultaneous control of age and race to the synthetic controls.

Poststratification to the CPS also increased the consistency between two important Government data series. Such consistency, although not crucial, is desirable.

Appendix II
Groups of variables
used in imputation

GROUP 1
(Complete before imputation or imputed by judgment)

904	AGE315	Age on March 15, 1988
905	AGE	Age in single years
906	AGE5	Age in 5-year groups
911	RACE	Race
915	MARITAL	Marital status
916	FMARITAL	Legal (formal) marital status
955	METRO	Residence: Metropolitan vs. nonmetropolitan
956	RURAL	Residence: Rural vs. urban
936–939	YEARS OF RELIGIOUS SCHOOLING	
994	OUTCOME	Pregnancy outcome (live birth, abortion, miscarriage,stillbirth)
945	LIVING14	Living arrangements at 14 years of age
609	AGEBABY1	Age at birth of first child
410	BABY1MO	Month and year first baby was born
415	BABY12	Interval in months between first and second babies
326–340	ON PREGNANCY OUTCOMES	
980	PARITY	Number of live births
120–148	ON EVER USE OF SPECIFIC METHODS (such as birth control pills, IUD, and diaphragm)	
625	KIDSLT18	Number of children under 18 years of age in the household
630	BIOKIDS	Number of biological children under 18 years of age in the household
611	PREGOUT1	Outcome of first pregnancy
149	NONSURG	Ever (or never) used a method other than sterilization
150	ANYMTHD	Ever (or never) used any contraceptive method
925	REGION	Residence: Northeast, South, Midwest, or West
320	STRLOPER	Type of sterilization operation
518	FMARNO	Number of formal (legal) marriages
309	NINFERT	Number of months infertile
310	INFERT	Infertility status
311	INFERT2	Infertility status (second version)
730–738	ON AIDS TRANSMISSION	
377	LOW3	Low birthweight (for triplets)

GROUP 2

922	HISPANIC	Origin: Hispanic vs. non-Hispanic
926	BRTHPLCE	Birthplace: Region
935	RELIGION	Religious affiliation
962	EDUCMOM	Years of school completed by the respondent's mother
963	AGEMOMB1	Age of respondent's mother when first baby was born

SORT: RACE
              REGION
              METRO
              RURAL

GROUP 3

964   FSTMENS   Age at first menstrual period (age at menarche)

2 Steps:

1) Impute for cases with SEX1AGE = missing
    SORT: AGEBABY1   (Ascending sort, youngest first, in whole years)
              AGE   (Ascending sort, youngest first, in whole years)

2) Impute for cases with SEX1AGE NE missing
    SORT: SEX1AGE   (Ascending sort, youngest first, in tenths of a year)
              AGE   (Ascending sort, youngest first, in whole years)

GROUP 4

100   SEXEVER   Respondent has ever had intercourse (yes, no)

SCREEN: PREGNUM = 0 and RMARITAL = 6

SORT: RACE
              AGE   (Single years up to 25 years of age, then 5-year groups)

SPECIAL: SEXEVER is imputed only for the 13 cases who refused to answer the question on whether they had ever had intercourse. Whenever SEXEVER is imputed, also impute all of the following variables using the same donor as for SEXEVER: SEX1MTHD, LASTBC, METHOD1, DATEUMO, PILL, ..., OTHER, NONSURG, ANYMETHD, METHCAL, CONSTAT, SEXFREQ, DATESMO, SEX1AGE, FP1MTHD, ..., FPYRTOT, PAPSMEAR, ..., AIDSTEST, SEX1FOR, SEX1HUND, SEX1TEN (No need to check for reported values. They are all missing all of these variables.)

GROUP 5

965   SEX1AGE   Age at first intercourse

NOTE: Impute hundredths and then round down to whole years

TEST: If SEXEVER = 2, then SEX1AGE = blank

SCREEN: SEXEVER = 1

2 Steps:

1) Impute for AGECON1 = missing, but AGEBABY ≠ missing
    SORT: FSTMENS   (Descending, oldest first)
              AGEBABY1   (Ascending, youngest first, tenths of a year)
              AGE315

2) Impute for AGECON1 NE missing
    SORT: FSTMENS   (Descending, oldest first, whole years)
              AGECON1   (Ascending, youngest first, tenths of a year)
               AGE315   (Ascending, youngest first, whole years)

CONSTRUCT: (from SEX1AGE)
540   DATESEX1   Month and year of first intercourse

EDIT: If SEX1AGE > FMAR1AGE, then SEX1AGE = FMAR1AGE
            (If age at first intercourse is reported to be higher than age at first formal marriage, then make the two
            ages equal.)

GROUP 6

514   FMAR1AGE   Age at first formal (legal) marriage

SCREEN: FMARITAL = 1, 3, 4, or 5

SORT: SEX1AGE   (Descending sort, oldest first, tenths of a year)
            AGE315   (Ascending sort, youngest first, hundredths of a year)

CONSTRUCT:

511   SEX1FOR   Number of months between first intercourse and marriage
970   MAR1NOW   Number of years between first marriage and interview
976   FMAR1MO   Month and year of first legal marriage
406   FMARBBY1   Number of months between first legal marriage and birth of first child

GROUP 7.1

559   COHEVER   Respondent has ever cohabited

DEFINITION: COHEVER = 1 if ever cohabited
                     = 2 if never
                     = 9 if unknown

SCREEN: RMARITAL NE 2   (Woman is not currently cohabiting; COHEVER = 1 for those
                                         currently cohabiting.)

SORT: FMARITAL
            SEXEVER
            HISPANIC (yes or no)
            AGE5
            RURAL

GROUP 7.2

553   COHOUT   Outcome of first cohabitation
557   COHAGEB   Age at beginning of first cohabitation

NOTE: For all cases with reported COHAB1, calculate an age at beginning of first cohabitation to 1/100 of a
            year. The age variable will be imputed and then translated back to dates (for missing cases only—no
            need to translate reported dates into hundredths of a year and then back again).

DEFINE: ENDAGE2   = age (whole years) at second marriage if MAR2MO NE blank and NE '9999'
                        = age otherwise

TEST: If COHEVER = 2, then COHAB1 = COHOUT = blank

SCREEN: COHEVER = 1 and SEXEVER = 1 and DATESEX1 ≤ COHAB1

SORT:

  A: FMARITAL = 6:
  _____

      RMARITAL
      AGE   (Ascending, whole years)
      SEX1AGE   (Descending, hundredths of a year)

40

B: FMARITAL ≠ 6 and SEX1FOR = 0 or 996:

RMARITAL
AGE   (Ascending, youngest first)
AGEDISS   (Descending, oldest first)

C: FMARITAL ≠ 6 and SEX1FOR > 0:

Recode of RMARITAL:
    1: RMARITAL = 1, first marriage
    2: RMARITAL = 1, not first marriage
    3: RMARITAL = 2
    4: RMARITAL = 3, 4, or 5
FMAR1AGE   (Ascending, 5-year ranges)
ENDAGE2   (Ascending, youngest first, whole years)
SEX1AGE   (Descending, hundredths of a year)

NOTE: A, B, and C groups are imputed separately, then merged together.

EDIT: If COHAGEB > AGE100, then COHAGEB = AGE100 and FLAG = 2
EDIT: If COHAGEB < SEX1HUND, then COHAGEB = SEX1HUND and FLAG = 2
EDIT: If COHAGEB > FMAR1AGE then force COHAGEB > AGEDISS for AGEDISS = 9999

CONSTRUCT:
554   UNION1   Month and year of first union
552   COHSTAT   Cohabitational status (never, before marriage, after marriage)
550   COHAB1   Month and year of first cohabitation

GROUP 8
526   AGEDISS   Age at dissolution of first formal marriage
558   COHAGEE   Age at end of first cohabitation

TEST: If COHOUT = 1, then COHAGEE = AGE100
TEST: If COHSTAT = 2 and COHOUT = 2 or 3, then COHAGEE = FMAR1AGE
TEST: If COHEVER = 2, then COHAGEE = blank
TEST: If AGEDD1 ≠ blank or missing, then
            AGEDISS = maximum (FMAR1AGE, AGEDD1 − 1.08)

DEFINE: DISSEVER = 1 if FMARNO > 1 or if 2 < FMARITAL < 6
                       2 otherwise

DEFINE: ENDAGE4 = Age at second marriage if MAR2MO ≠ blank or missing;
                      AGE315 otherwise

SCREEN: DISSEVER = 1 or COHEVER = 1   (Ever married or ever cohabited)

SORT: (4 subgroups)

  A: COHSTAT = 1:   (Cohabitational status = never cohabited)
     FMAR1AGE   (Descending sort, oldest first, whole years)
     ENDAGE4   (Ascending sort, youngest first)

  B: COHSTAT = 2 and FMARNO > 0:   (Ever married; cohabited before marriage)
     DISSEVER   (Defined above)
     COHOUT   (Intact or dissolved marriage vs. dissolved cohabitation)

41

| FMAR1AGE | (Descending, whole years) |
| ENDAGE4 | (Ascending, whole years) |

C: <u>COHSTAT = 3</u>   (Cohabited after first marriage)

COHAGEB	(Ascending, whole years)
FMAR1AGE	(Descending, whole years)
ENDAGE4	(Ascending, whole years)

D: <u>COHSTAT = 2 and FMARNO = 0:</u>   (Never married; but has cohabited)

SPECIAL SCREEN: COHOUT = 4   (First cohabitation dissolved without legal marriage)

SORT:
| COHAGEB | (Descending, oldest first, whole years) |
| AGE315 | (Ascending, youngest first, whole years) |

EDITS: Force AGEDISS to be ≥ FMAR1AGE
Force COHAGEE to be ≥ COHAGEB
Force AGEDISS to be ≤ COHAGEB for COHSTAT = 3
Force COHAGEE to be ≤ FMAR1AGE for COHSTAT = 2

EDIT: If COHSTAT = 3 (cohabited after first marriage) and MAR2MO ≠ missing or blank and COHAB1 ≤ MAR2MO, then force COHAGEE ≤ COHAGEB + (MAR2MO − COHAB1)/12

EDIT: Force AGEDISS ≤ MAR2MO for MAR2MO ≠ blank or missing

CONSTRUCT:
525	MAR1DIS	Length of first marriage
524	DISFIRST	Month and year of dissolution of first marriage
535	AGEDD1	(AGED1SS + 1.08 or blank) Age at divorce or widowhood (first marriage)
555	UNTYPE	Type and outcome of first union
551	COHABINT	Length of first cohabitation in months
556	UNIONINT	Duration of first marriage or cohabitation (months)

GROUP 9

| 529 | MAR2MO | Month and year of second formal (legal) marriage |

DEFINE: LOWERB = MAXIMUM (DISFIRST, COHAB1 + COHABINT)
(Maximum of (1) the date of dissolution of first marriage, or (2) the beginning date of the first cohabitation, plus the length of the first cohabitation)

TEST: If FMARNO < 2, then MAR2MO = blank
(If she was not married 2 or more times then MAR2MO is blank)

SCREEN: FMARNO > 1 and LOWERB ≤ MAR2MO
(Number of marriages is greater than 1, and LOWERB is before the date of the second marriage)

SORT:
| LOWERB | (Descending, oldest first) |
| AGE315 | (Ascending, youngest first) |

CONSTRUCT:
| 530 | DIS1MAR | Interval in months between divorce or death and second marriage |

GROUP 10 (Interval File)

991   AGEPREG   Age of mother at outcome of pregnancy

Note: This variable was imputed by hand.

CONSTRUCT:

992	YRPREG	Year pregnancy ended
621	FMAROUT	Formal marital status when pregnancy ended
612	PREG1MO	Month and year when first pregnancy ended

GROUP 11 (Interval File)

341	PRGLENGTH	Length of pregnancy in months
372	PREGTEST[1]	Number of weeks pregnant at pregnancy test
373	PNCAREWK[1]	Number of weeks pregnant at first prenatal care
374	PNCARENO[1]	Number of visits for prenatal care
375	LOW1[2]	Low birthweight
376	LOW2[2]	Low birthweight for second baby (if twins) (Set equal to LOW1 if missing.)
426	WANTWIFE	Wantedness status of pregnancy according to the woman
427	WANTMAN	Wantedness status of pregnancy according to her husband or partner
614	LIVBABY1[2]	Living arrangements of first baby
615	LIVBABY2[2]	Living arrangements of second baby (if twins)
616	LIVBABY3[2]	Living arrangements of third baby (if triplets)

TEST: If PREGTEST = 0–31 and PNCAREWK = '99', then impute PNCAREWK = PREGTEST + 2

SORT:

A: OUTCOME = LIVE BIRTH and YRPREG < 84: BY YRPREG

B: OUTCOME = LIVE BIRTH and YRPREG 84: BY YRPREG AND FMAROUT

C: OUTCOME NE LIVE BIRTH: BY OUTCOME

CONSTRUCT:

613	LIV1CHILD	Living arrangements of first live birth
438	WTBIRTH1	Wantedness of first birth
439	WTLASTP	Wantedness of last pregnancy
440	WTLASTB	Wantedness of last birth
442	WANTP5	Number of wanted pregnancies in the last 5 years
605	AGECON1	Age at first conception
603	FMAR1CON	Interval in months between first marriage and first conception
604	CON1FMAR	Interval between first conception and first formal marriage
606	DATE1CON	Date of first conception
441	BIRTHS5	Number of births in the last 5 years

GROUP 12

110	SEX1MTHD	Method of birth control, if any, used at first intercourse
115	METHOD1	First method ever used

---

[1]Set = blank if OUTCOME was not a live birth, or if the year the
pregnancy ended was before 1984.

[2]Set = blank if OUTCOME was not a live birth.

TEST: If ANYMTHD = 2, then SEX1MTHD = 995 (Never used a method) and
            METHOD1 = blank

NOTE: Model-based imputation was used for this module. There are two submodels.

A: METHOD1 = missing and SEX1MTHD = '996' or missing:

Assign whatever methods she has ever used, in the following order:

Withdrawal
Condom
Pill
Foam
IUD
Rhythm (Calendar)
Condom and foam
Diaphragm
Female sterilization
Suppository

B: METHOD1 NE missing and SEX1MTHD = missing

Assign SEX1MTHD = '996' (did not use a method at first intercourse)

(SEX1MTHD is missing when the woman knew what method she first used but did not remember exactly when she used it.)

GROUP 13

930   EDUCAT   Education   (years of education completed)

SCREEN: RELIGSCH = 0   (Education is imputed by hot-deck only if she had no years of religious schooling; see note below.)

SORT:

A: AGE < 23:

AGE   (Ascending, youngest first, whole years)
AGEBABY1   (Ascending, youngest first, whole years)
RACE   (Black, other)

B: AGE > 22:

AGEBABY1   (Ascending, whole years)
RACE   (Black, other)
EDUCMOM

NOTE: There were two cases with missing education that had attended religious schools for some years. The average numbers of total years of education completed for students who had attended religious schools for the same number of years were used for these two women.

GROUP 14

931   DIPLGED   Receive high school diploma or GED
932   DROPOUTN   Number of times left school
933   TIMEOUT   Duration of first absence from school
934   AGELSTED   Age when last enrolled in school

Pass 1:

IMPUTE: DIPLGED

SORT: EDUCAT
AGE   (whole years)

Pass 2:

IMPUTE: DROPOUTN

SCREEN: AGE315 = under 25 years of age

SORT: EDUCATION
Special Recode:
1 = first pregnancy outcome before last enrollment
2 = never pregnant or first pregnancy outcome after last enrollment
RACE

Pass 3:

IMPUTE: TIMEOUT

SCREEN: DROPOUTN > 0

SORT: PREGOUT1
RACE

NOTE: Group 15 was dropped.

GROUP 16

| 940 | POVERTY | Ratio of family income to poverty level |
| 950 | LABORFOR | Current labor force status |

SORT:

RCURPREG	(Indicates current pregnancy)
KIDSLT18	(Number of own children living with her)
Recode of RMARITAL:	
1: RMARITAL = 1 or 2	(Married or cohabiting)
2: RMARITAL = 3–5	(Widowed, divorced, or separated)
3: RMARITAL = 6	(Never married)
RACE	
METRO	
Recode of EDUCAT:	
1: EDUCAT = 0–11 years	
2: EDUCAT = 12 years	
3: EDUCAT = 13–15 years	
4: EDUCAT = 16 years or more	

GROUP 17

380	NOSEX12	Number of months R didn't have intercourse in the last 12 months
322	NOSEX36	Number of months R didn't have intercourse in the last 36 months
520	SEXFREQ	Frequency of intercourse in the last 3 months

TEST: If SEXEVER = 2, then NOSEX12 = NOSEX36 = "96" and SEXFREQ = blank

SCREEN: SEXEVER = 1   (Impute these variables only if the woman has had intercourse. They are all blank if she has never had intercourse.)

SORT:

A: <u>RMARITAL = 1 and MAR1NOW < 4:</u>   (Currently married less than 4 years)

    SEX1FOR

B: <u>RMARITAL = 1 and MAR1NOW > 3:</u>   (Married 4 years or more)

    MAR1NOW

C: <u>RMARITAL = 2 and COHOUT = 1:</u>   (Currently cohabiting)

    COHABINT

D: <u>RMARITAL = 2 and COHOUT NE 1:</u>   (Cohabiting, but not first cohabitation)

    AGE

E: <u>RMARITAL = 3, 4, 5 or 6:</u>   (Formerly married or never married)

    RMARITAL
    AGE

NOTE: Group 18 was dropped.

GROUP 19

118   DATEUSE Date (month and year) of first contraceptive use

NOTE: Translate to age at first use and impute this age. Translate imputed ages back to imputed dates.

TEST: If SEX1MTHD = 01–16, then DATEUSE = DATESEX1

SCREEN: SEX1MTHD = 996 and ANYMTHD = 1 and DATEUSE > DATESEX1
    (Did not use a method at first intercourse but did use some method at least once)

SORT: AGE   (Ascending, youngest first, whole years)
    SEX1AGE   (Descending, oldest first, hundredths of a year)
    RACE   (Black, other)

FORCE: DATEUSE ≥DATESEX 1 (Date of first method use must be same as or after date of first intercourse.)

    DATEUSE ≤ Date of interview

GROUP 20

225   FP1AGE   Age at first family planning visit

TEST: If FP1AGE is missing but TIMVIS1 is not, then use average delay past SEX1AGE given TIMVIS1. (Set flag = 2)

SCREEN: SEX1EVER = 1 and FPEVER = 1   (Impute only if she has had intercourse and had a family planning visit; otherwise FP1AGE is blank.)

SORT: Recode of SEX1MTHD:
    1: SEX1MTHD = Pill
    2: SEX1MTHD ≠ Pill
    AGE   (Ascending, whole years to 34, then 35–44)
    SEX1AGE   (Descending, whole years)

CONSTRUCT:

226	MONVIS1	Month and year of first family planning visit
227	TIMVIS1	Timing of first visit in relation to first intercourse
228	TIMVIS2	Timing of first visit in relation to first pregnancy

FORCE: FP1AGE ≤ AGE   (Age at first visit must be less than or equal to age at interview.)

GROUP 21.1

112	LASTBC	Method used at most recent intercourse
158	METHCAL	Method calendar entry for the month of interview
160	CONSTAT	Contraceptive status at date of interview

NOTE: Imputation of group 4 nearly completes METHCAL and CONSTAT. The few remaining cases were imputed by hand. A model was used to impute LASTBC because of the complexity of the consistency required with all the variables, regarding the "ever use" of specific methods.   .

MODEL:
1) If ANYMTHD = 2 (never used method), then LASTBC = 995 (never used a method).
2) If CONSTAT = 01–17 (using a method), then the code for CONSTAT is translated into the code for LASTBC.
3) If CONSTAT indicates surgical sterilization, then LASTBC is set to sterilization (male or female as appropriate).
4) If CONSTAT = 20, 21, or 22 (pregnant, postpartum, or seeking pregnancy) then LASTBC = 996 (did not use a method)
5) Otherwise, the variables indicating whether specific methods have ever been used are searched in the order shown below. The first method with a positive response determines LASTBC.

FEMSTER	Ever/never used female sterilization
MALESTER	Ever/never used male sterilization
PILL	Ever/never used pill
CONDOM	Ever/never used condom
DIAPHRAGM	Ever/never used diaphragm
WITHDRAW	Ever/never used withdrawal
RHYTHM	Ever/never used rhythm
IUD	Ever/never used IUD
CONFOAM	Ever/never used condom and foam together

GROUP 21.2

| 385 | NOSEXDUR | Duration of the current period of nonintercourse, in months |

TEST: If SEXEVER = 2 (never had intercourse), then NOSEXDUR = blank
         If CONSTAT = 05–17 or 21 (using a coitus-dependent method or seeking pregnancy), then NOSEXDUR = 996
         If CONSTAT = 33 (had intercourse only once), then NOSEXDUR = date of interview − DATESEX1
         If METHCAL NE 5000 (not having intercourse), then NOSEXDUR = 996

SCREEN: METHCAL = 5000

SORT: NOSEX36

GROUP 22.1

| 325 | STERLAGE | Age of woman when sterilization operation was done |

SCREEN: STRLOPER NE 5   (Impute STERLAGE only if R or husband had a sterilization operation; if neither had a sterilization operation, then STERLAGE is blank.)

47

SORT: STRLOPER   (Type of sterilization operation)
             NEWKID   (Age at birth of youngest child)
                             (Descending, oldest first)
             AGE315   (Ascending, youngest first)

FORCE: STERLAGE ≤ AGE100
             STERLAGE ≥ NEWKID

CONSTRUCT: PPSTER (322) Sterilization operation was postpartum (yes, no)

GROUP 22.2

360   FECUND   Fecundity status

NOTE: Most cases were imputed by hand. The only cases imputed by hot-deck were those that had refused
             SEXEVER.

SORT: FMARITAL
             RMARITAL
             INFERT
             CONSTAT

CONSTRUCT: FECUND2 (361) Fecundity status, second version

GROUP 23

304   WANTKID1   R wants another baby (yes, no)
420   ADDEXP   Number of additional births expected

TEST: If FECUND = 1, 2, or 3, or if CONSTAT = 01 or 02 or 23–29, or
             if INFERT = 1, or if STRLOPER ≠ 5, then ADDEXP = 000 and INTENT = 2

SORT: FECUND
             RMARITAL
             CONSTAT
             AGE

CONSTRUCT: INTENT (985) R intends to have a baby in the future (yes, no)
           WANTKID2 (305) R wants another baby — second version (yes, no)

GROUP 24

298   INFSRC   Source of infertility services
288–297   Infertility Services, specific services received

SORT: RMARITAL
             FECUND
             NINFERT
             AGEBABY1

GROUP 25

230   FP1SRC   Source of service at first family planning visit

TEST: If FP1AGE = blank and AGE < 25, then FP1SRC = 6

SCREEN: FP1AGE ≠ blank and AGE < 25

48

SORT: RACE

             Recode of POVERTY:

               1:   0–99%
               2:   100–199%
               3:   200–299%
               4:   300% or more

             RMARITAL

             FP1AGE

GROUP 26

205	FP1MTHD	R got a new method at first family planning visit (yes, no)
210	FP1OMED	R got other medical services at first visit (yes, no)
215	FP1FPADV	R got advice on birth control at first visit (yes, no)
220	FP1DADV	R got other advice at first family planning visit (yes, no)

SCREEN: FP1SRC ≠ blank or 6   (R has had at least one family planning visit.)

SORT: ANYMTHD

             FP1SRC

             TIMVIS2

             Recode of POVERTY:

               1:   0–99%
               2:   100–199%
               3:   200–299%
               4:   300% or more

GROUP 27.1

270	FPYRMD	Number of family planning visits to private doctors in last 12 months
272	FPYRCLIN	Number of family planning visits to clinics in last 12 months
274	FPYRCOUN	Number of family plannning visits to counselors in last 12 months
276	FPYRTOT	Total number of family planning visits in last 12 months

2 Steps:

1) Impute 270, 272, and 274 for cases with FPYRTOT NE missing

    SCREEN: FPYRTOT NE blank

    SORT: FPYRTOT
                      FP1SRC

2) Impute 270–74 for cases with FPYRTOT = missing

    SCREEN: FPEVER = 1   (ever had a family planning visit)

    SORT: CONSTAT
                      RACE
                      POVERTY   (same four levels as in Group 25)
                      AGE5
                      FP1SRC

CONSTRUCT: FPYRTOT

GROUP 27.2

| 266 | RSOURCE | Most recent source of family planning services |

SCREEN: FP1AGE ≠ blank   (ever had a family planning visit)

DEFINITIONS:

Let b_1 = 1 if FPYRMD > 0   (R had 1 or more visits to private doctors in the last year.)
      0 otherwise

b_2 = 1 if FPYRCLIN > 0   (1 or more visits to clinics)
      0 otherwise

b_3 = 1 if FPYRCOUN > 0 (1 or more visits to counselors)
      0 otherwise

Let FPYRPATT = $4(b_1) + 2(b_2) + b_3$

SORT:

A: <u>FPYRTOT > 0:</u>

FPYRPATT

<u>FPYRTOT = 0 and AGE 15–24:</u>

FP1SRC

<u>FPYRTOT = 0 and AGE 25–44</u>

RACE   (two categories)
POVERTY (four levels)
RMARITAL
FP1AGE

GROUP 28

260–264   Sources of payment for most recent visit
240–255   Services received at most recent visit

SCREEN: FP1AGE $\neq$ blank   (Impute only if R ever had a visit; if not, the variables in group 28 are blank.)

SORT: RSOURCE
           RACE   (two categories)
           MEDMTHD (16 categories)*
           POVERTY (four levels)
           RMARITAL
           EDUCATN (four levels)
           AGE   (whole years)

*Combinations of ever use of pill, IUD, diaphragm, female sterilization

GROUP 29

280   Pap smear
281   Pelvic exam
282   Breast exam
283   Blood Press Test
284   Urinalysis
285   VD test
286   AIDS test

Step 1:   Impute only X's (see NOTE). These must not be imputed to be 2's.

    SCREEN: Each of variables 280 to 286 $\neq$ 2.

    SORT: RACE
               AGE5
               EDUCATN

Step 2:   Impute only "Y"'s. These must not be imputed to be 1's.

    SCREEN: Each of variables 280 to 286 = 1

    SORT: RACE
                      AGE5
                      EDUCATN

Step 3:   Impute Z's:

    SORT: RACE
                      AGE5
                      EDUCATN

NOTE: An "X" denotes that the woman definitely did not receive the service as part of a general check up or other medical visits. A "Y" denotes that the woman definitely did not receive the service as part of a family planning visit. A "Z" indicates that she may or may not have received the service in either setting.

GROUP 30

714	KNOWHERP	Have you ever heard of genital herpes?
722	KNOWCLMY	Have you ever heard of chlamydia?
728	KNOWAIDS	Have you ever heard of AIDS?

SORT: EDUCATN   (four levels)
               RMARITAL
               RACE   (black, other)
               SEXFREQ

GROUP 31

705	PIDTRAT	Ever/never been treated for PID
710	WARTS	Ever/never had genital warts
712	GONORRH	Ever/never had gonorrhea
716	RHERPES	Ever/never had genital herpes
718	HHERPES	Husband ever/never had genital herpes
720	GETHERP	Chances of getting genital herpes
724	RCHLAMYD	Ever/never had chlamydia
726	GETCLMD	Chances of getting chlamydia
750	STDMTHD	R uses method to prevent sexually transmitted diseases (yes, no)

TEST: If KNOWHERP = 2, then RHERPES = HHERPES = GETHERP = blank

TEST: If KNOWCLMY = 2, then RCHLAMYD = GETCLMD = blank

SCREEN: KNOWHERP = KNOWCLMY = 1

SORT: RMARITAL
               RACE   (black, other)
               SEXFREQ
               CONDOM3
               POVERTY   (four levels)

GROUP 32

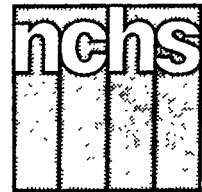| 740 | VIRUS | You can get AIDS from someone who has the virus but not the disease (yes, no) |
| 745 | GETAIDS | R's chances of getting AIDS |

NOTE: Don't impute "don't know's".

SCREEN: KNOWAIDS = 1   (has heard of AIDS)

SORT: EDUCATION   (four levels)
            RMARITAL
            RACE   (black, nonblack)
CONSTRUCT: AIDSMTHD (755) (Do you use a method to prevent AIDS?)
            (From KNOWAIDS and STDMTHD)

. . .   .

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Fecundity and Infertility in the United States, 1965-88

A report published recently by the National Center for Health Statistics (NCHS) notes that in 1988 about 1 in 12 U.S. women ages 15-44 years had physical conditions that made it difficult to have children.

The report, "Fecundity and Infertility in the United States, 1965-88," is the latest among a series of reports that present data from the 1988 National Survey of Family Growth (NSFG), conducted by NCHS.

According to data in the report, close to 4.9 million women ages 15-44 years had an impaired ability to have children (impaired fecundity) in 1988. Of this number, 2.2 million had no previous births and the other 2.7 million had one or more births before they became impaired.

The data on impaired fecundity are based on responses to a series of questions asked in the 1988 NSFG. The responses were used to classify women into three major groups: (1) those who were surgically sterile; (2) those with impaired fecundity (sterile for reasons other than surgery, or difficult or dangerous to have a baby); and (3) those who were fecund (had no known physical problem).

The report provides trend data on the number of infertile couples in the United States. The number of infertile couples in 1988, 2.3 million, was very similar to the number reported in 1982, 2.4 million. About 3 million couples were found to be infertile in 1965.

Infertility, the inability to conceive after 12 months or more of intercourse without contraception, is used as a screening device to decide when couples should begin to receive treatment. It is not a measure of sterility. Infertility was measured only for married couples in the NSFG.

The 1988 NSFG was based on personal interviews with a national sample of 8,450 women 15-44 years of age in the noninstitutionalized population of the United States.
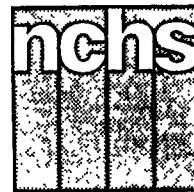
Single copies of this report can be obtained by contacting the Scientific and Technical Information Branch at the address listed above.

Cohabitation, Marriage, Marital Dissolution, and Remarriage: United States, 1988

Advance Data No. 194
(PHS) 91-1250

Authors: London, K.

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Hyattsville, MD 20782

Tel: (301) 436-8500

A new report by the National Center for Health Statistics (NCHS) describes current living arrangements of women of childbearing ages in the United States. Among these arrangements is the practice of cohabitation, the sharing of living quarters with a sexual partner without a formal marriage. Data contained in the report indicate that about one-third of women 15–44 years of age in 1988 had "cohabited," or had lived with a boyfriend or partner at some time without being married to him.

Discussions of cohabitation and other changes in living arrangements are provided in the report "Cohabitation, Marriage, Marital Dissolution, and Remarriage: United States, 1988." According to the report, changes in living arrangements can be attributed to three factors: (1) increases in the number of deferred marriages, (2) a high U.S. divorce rates, and (3) the declining rates of remarriages.

In 1988, only about 40 percent of women under 30 years of age had been married. Additionally, more than a third of first marriages among women 15–44 years had already ended in separation, divorce, or widowhood. The report further notes that only 16 percent of women whose first marriages ended in widowhood

or divorce in 1980–84 had remarried within a year, compared with 33 percent of women who divorced or were widowed in 1965–69.

All of the data included in the report are based on responses to interviews conducted as part of the National Survey of Family Growth, a periodic NCHS survey. The survey is designed to study the lifestyles of women 15–44 years of age in the noninstitutionalized population of the United States. Questions are formulated to obtain information on childbearing, contraceptive practice, and other aspects of maternal and child health.

Single copies of "Cohabitation, Marriage, Marital Dissolution, and Remarriage: United States, 1988" can be obtained by contacting the Scientific and Technical Information Branch. The address is listed above.

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