NSAF Methodology Reports

1997 NSAF Impact of Census Undercount-Adjusted Weights on Survey Estimates

Report No. 14

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Preface

1997 NSAF Impact of Census Undercount–Adjusted Weights on Survey Estimates is the fourteenth report in a series describing the methodology of the 1997 National Survey of America's Families (NSAF). The NSAF is part of the *Assessing the New Federalism* project being conducted at the Urban Institute, in partnership with Child Trends. The data collection for the NSAF was conducted by Westat.

The NSAF is a major new survey focusing on the economic, health, and social characteristics of children, adults under the age of 65, and their families. During the first round of the survey in 1997, interviews were conducted in over 44,000 households, yielding information on over 100,000 people. The NSAF sample is representative of the nation as a whole and of 13 states in particular, and therefore has an unprecedented ability to measure differences between states.

<u>About the Methodology Series</u>

This series of reports has been developed to provide readers with a detailed description of the methods employed to conduct the 1997 NSAF. The early reports focus on the topics described below:

- No. 1: An overview of the NSAF sample design, data collection techniques, and estimation methods
- No. 2: A detailed description of the NSAF sample design for both telephone and in-person interviews
- No. 3: Methods employed to produce estimation weights and the procedures used to make state and national estimates for *Snapshots of America's Families*
- No. 4: Methods and results of computing sampling errors
- No. 5: Processes used to complete the in-person component of the NSAF
- No. 6: An assessment of several measures of child and family well-being
- No. 7: Studies conducted to understand the reasons for nonresponse and the impacts of missing data
- No. 8: Response rates obtained (taking the estimation weights into account) and methods used to compute these rates
- No. 9: Methods employed to complete the telephone component
- No. 10: Data editing techniques and imputation techniques for missing variables
- No. 11: Documentation to accompany the Child Public Use File
- No. 12: 1997 NSAF Questionnaire
- No. 13: 1997 NSAF MKA (Most Knowledgeable Adult) Public Use File documentation and codebook
- No. 14: Impact of census undercount–adjusted weights on survey estimates
- No. 15: 1997 NSAF comparisons with other national surveys
- No. 16: 1997 NSAF articles and papers on survey methods
- No. 17: 1997 NSAF Non-MKA (Other Adult) Public Use File documentation and codebook

About this Report

This report completes the methodological discussion of 1997 NSAF estimation that was begun in report no. 3 in this series. Here our main goal is to describe how we brought the 1997 NSAF up to census undercount–adjusted control totals and what difference this made. In report no. 3, we described the first estimation approaches used for the 1997 survey. (report no. 3 also explains how we brought the survey up to census-level controls.)

The use of census-undercount controls allows researchers to more readily compare NSAF results with those of most other large national surveys, like the Current Population Survey (CPS) or the National Health Interview Survey (NHIS). As will be seen, this reestimation effort also allowed us to refine some other steps taken earlier.

When the original *Snapshots* publication was under preparation, there was concern about what difference using undercount-adjusted results would make. The belief was widely held, however, that for the NSAF statistics expressed as percentages (as they were in *Snapshots*), the change in conventions would make little impact. But, we needed to check this, and the current report does just that–confirming, it might be added, that the conjecture was basically correct.

We also take the opportunity in this report, among other things, to look at the impact of editing differences on the *Snapshots* results, since it has been over a year since the *Snapshots* were prepared, during which editing continued. For the most part, these editing changes, as we also conjectured, have proved to be minor. This is not to say that individual researchers do not have to beware of further data problems, because some undoubtedly exist on the public use files being released. Still, we are relatively comfortable with the work done on the 1997 NSAF. It should be a sound baseline for comparison with the 1999 survey results coming out this year.

For More Information

For more information about the National Survey of America's Families, *contact Assessing the New Federalism*, Urban Institute, 2100 M Street, NW, Washington, DC 20037, telephone: (202) 261-5377, fax: (202) 293-1918, Web site: http://newfederalism.urban.org.

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Chapter 1
Introduction and Background

This report completes the methodological discussion of 1997 NSAF estimation that was begun in report no. 3 in this series. Here our goal is to describe how we brought the 1997 NSAF up to census undercount-adjusted control totals. In report no. 3, a description was given of the first estimation approaches used for the 1997 survey. (Report no. 3 also explains how we brought the survey up to census-level controls.)

The use of census-undercount controls allows researchers to more readily compare NSAF results with those of most other large national surveys, like the Current Population Survey (CPS) or the National Health Interview Survey (NHIS). As will be seen in chapter 2, this re-estimation effort also allowed us to refine some other steps taken earlier.

Chapter 2 explains the new weighting procedures used to revise the weights described in report no. 3 and how we produced new weights that could be used to make undercount-adjusted estimates from the 1997 NSAF data. The most significant revision was a change in the population control totals used in the last step of raking.

The revised weights developed here have controls that reflect the undercount of the population from the 1990 census. The undercoverage represents the population not enumerated in the 1990 census. This population is carried forward to 1997 through special adjustments on specific age-race groups. About four million persons residing in the United States on census day were not enumerated in 1990. As already noted, the result of the undercount adjustment is to make NSAF estimates more consistent with estimates from other large national surveys.

As described in chapter 2, two other adjustments in the weighting procedures were implemented. One of these was to better account for households that did not have anyone who spoke either English or Spanish. There was also an attempt to improve national estimates of American Indians and Asian/Pacific Islanders. These two “fixes” were implemented prior to the third and final fix, which was the use of the new control totals. We believe that for the low-income population, especially the minority population, the new 1997 NSAF results will be somewhat improved by the above three refinements.

In the last section of chapter 2, we describe several additional sets of weights that were developed to support special analyses. As was done in Report no. 3, we simplify the presentation by showing the results for one study area, Michigan.

An important concern in any survey is the extent to which all the members of the target population have an opportunity to be interviewed. This is called survey coverage. Any omission of units from the sampling process is called undercoverage. Chapter 3 considers coverage in the 1997 NSAF.

In chapter 3, coverage ratios are defined and examined. First, overall NSAF coverage ratios for different population groups are reviewed. The chapter then goes on to examine the household and within household-components of coverage. Comparisons of NSAF coverage to the coverage in other household surveys are also made.

National coverage ratios in the NSAF are 94 percent for children and 86 percent for adults. Coverage in the CPS is about the same for children and somewhat better for adults. The poorer adult coverage in the NSAF may be due partly to incorrectly classifying some childless households as having only persons 65 and over. Possible weaknesses in the nonresponse adjustment may also be a factor.

Coverage ratios for blacks are noticeably lower (70 percent for children and 67 percent for adults) than for Hispanics and for non-Hispanic non-blacks. Black coverage is also lower than in the CPS. Again, more examination of the NSAF nonresponse adjustments may be a place to look for possible causes.

Examining the components of undercoverage, it appears that the undercoverage in the NSAF is primarily whole-household undercoverage rather than within-household undercoverage. However, no firm conclusion about the magnitude of the two components is possible.

When the original *Snapshots* publication was under preparation, there was concern about what differences using undercount-adjusted results would make. The belief was widely held, however, that for NSAF statistics expressed as percentages (as they were in *Snapshots*), the change in conventions would make little difference. But we needed to check this and chapter 4 does just that.

To this end, in chapter 4 each of the January 1999 *Snapshots* tables has been reproduced – both as originally published and after adjusting the survey as described in chapter 2. Graphical and tabular comparisons are made. These confirm, it might be added, that the conjecture of only minor differences was basically correct

We also took the opportunity in chapter 4 to look at the effect of editing differences on the *Snapshots* results. Over the year since the *Snapshots* were prepared the 1997 survey files were being changed as more was learned. We wanted to let researchers see what impacts this had on the original results. The data changes, also as we conjectured, have proved to be mostly of minor importance. This is not to say that individual researchers do not have to beware of further data problems. Some undoubtedly exist on the public use files being released. Still, we are relatively comfortable with the work done on the 1997 NSAF. It should be a sound baseline for comparison with the 1999 survey results coming out this year.

One final comparison in chapter 4 looks at what would have happened if we had *not* done a special sample of non-telephone households and had relied only on a random-digit-dialing (RDD) design. Large differences exist between this RDD approach and the original *Snapshots* estimates, especially for households under 200 percent of poverty. An RDD-only approach would not have worked at all well for this most important group.

The basic results about NSAF coverage have already been summarized in report no. 1 in the 1997 methodology series. Report no. 3 in this series has already been mentioned. The papers on estimation published separately in report no. 16 may be worth further examination.

Chapter 2
Revised Weights

This chapter explains the weighting procedures that were used to revise the weights described in report no. 3 and to produce new weights that can be used to make estimates from the 1997 NSAF data. The most significant revision was a change in the population control totals used in the last step of raking. The revised weights have controls that reflect the undercount of the population from the 1990 census.[1] As a result of this change in control totals, the NSAF estimates will be more consistent with estimates from other surveys such as the CPS or SIPP, that are adjusted for the undercount.

Two other adjustments in the weighting procedures were implemented to better account for language minority households and for national estimates of American Indians and Asian/Pacific Islanders. These were implemented prior to the use of the new control totals.

The first section of this chapter describes the adjustment for language problem cases. The second section is the adjustment for American Indian and Asian/Pacific Islanders. The third is the adjustment to the new control totals. The fourth section details how the control totals were developed. The final section describes additional sets of weights that were developed to support special analyses. As was done in report no. 3, we simplified the presentation by showing the results for one study area, Michigan.

2.1 Adjustment for Language Problems

The NSAF interviews were conducted in both Spanish and English. Households in which all persons spoke only languages other than English and Spanish were not interviewed. In the original weights, these households were considered nonrespondents and handled like all other types of nonrespondents. The revised weights used a separate adjustment for the cases that were classified as being "language problem" cases (those households not speaking either English or Spanish). The adjustment assumes that the members in language problem households are eligible for the NSAF and they are not born in the United States. One of the objectives of this adjustment is to improve the estimates of the number of foreign-born persons.

At the screener level, the screener disposition codes assigned to households where there was not a member in the household who spoke either English or Spanish are LM, LH, and LP (See report no. 9, *1997 NSAF Telephone Survey Methods*). The total number of screened households coded as language problem (or LP) nonrespondents is presented in table 2-1.

[1] The undercoverage represents the population not enumerated in the 1990 census. This population is carried forward to 1997 through special adjustments on specific age-race groups. About four million persons residing in the United States on census day were not enumerated in 1990.

Table 2-1.
Number of Households Coded as "Language Problems"

Screener Disposition Code	Label	Description	Number of Households
LH	Final Language Problem-Hearing/Speech	Two call to this respondent resulted in a hearing or speech communication problem	81
LM	Max Call Language Problem	Questionnaire had an additional language problem in its call history and has reached the maximum calling algorithm	340
LP	Final Language Problem	Two calls to this respondent resulted in a communication problem (i.e., hearing or speech problem or non-English)	1,243
Total			**1,664**

During the screener nonresponse adjustments (at the household level), the LP households were combined with other types of nonresponse (e.g. refusals, break-off calls, etc.) forming weighting cells as described in chapter 2 of report no. 3. These households are then adjusted within cells. Doing so implicitly assumes that the LP households are similar to any other households within the cell. However, this assumption does not hold for LP households. One difference is the reason for nonresponse–it is not possible conduct the interview with the language problem households while most other nonresponse is associated with the unwillingness to interviewed. Another reason is that the composition of the LP households is different. The members of LP households are more likely to be foreign-born, since they are non-English non-Spanish speakers.

The revised approach is to create weighting adjustment cells containing households from both responding and nonresponding households where most of the members are non-English/non-Spanish speakers. However, this information about the household and the language spoken in the household is not available until the extended interview, and then it is only available for respondents. As a result, the adjustment cannot be done at the screener level.

To solve this problem, the extended interview weights of the persons who speak a language other than English or Spanish are adjusted to account for the persons who do not speak English or Spanish in LP households. Two quantities are computed for this adjustment: the proportion of eligible households[2] that are LP, and the proportion of persons who speak a language other than English or Spanish.

The proportion of eligible households that are LP is computed at the screener level, using the estimate of the proportion of *eligible* households that are LP households $\left(PEHH_{LP}\right)$. However,

[2] Eligible household is a residential household with at least one occupant less than 65 year old.

this proportion is not easily computed. We use the proportion of LP households an approximation as follows

$$PEHH_{LP} \approx PHH_{LP} = \frac{\sum_{LP} w_i}{\sum_{R} w_i}$$

where

w_i	=	Base weight for household *i* (adjused for for NA and NM)
R	=	Residential households
LP	=	Households with language problems

Table 2-2 shows the values of PHH_{LP} for the number of households considered to have language problems. For example, in Michigan there are 46 households that were consided to have a language problem during the screener interview. The estimated number of LP households accounts for 0.295 percent of the residential households in Michigan.

Table 2-2.
Number and Percentage of Language Problem Households, by Study Area

Study Area	Number of Language Problem Household	Percentage with Respect to All Screener Households (PHH_{LP})
Alabama	16	0.145%
California	265	1.888%
Colorado	68	0.404%
Florida	112	0.640%
Massachusetts	226	0.927%
Michigan	46	0.295%
Minnesota	40	0.242%
Mississippi	14	0.160%
New Jersey	261	0.904%
New York	306	1.709%
Texas	59	0.448%
Balance of U.S.	83	0.383%
Washington	82	0.505%
Wisconsin	86	0.237%
Total	**1,664**	

The second proportion needed is the number of persons who speak a language other than English or Spanish (P_{NE}). This proportion is computed at the person level separately for each set of weights, child weights (CW) and adult weights (random adult or RAW, and adult pair or APW). The expression for this proportion is

$$P_{NE} = \frac{\sum_{NE} w_i}{\sum_{NE} w_i + \sum_{E} w_i}$$

where

w_i	=	Final person weight
NE	=	Non-English/non-Spanish speakers[3]
E	=	English or Spanish speakers

The next step is to adjust the weights of the non-English/non-Spanish persons by the factor LPF so that the estimated percentage of persons using the revised weight (*LPW*) equals the new percentage of non-English/non-Spanish speakers. The new percentage, P_{NE}^{*}, is

$$P_{NE}^{*} = P_{NE} + PHH_{LP} .$$

The adjustment factor, LPF, is

$$LPF = \begin{cases} \dfrac{P_{NE}^{*}}{P_{NE}} & \text{If the person speaks a language other than English or Spanish} \\ \dfrac{1-P_{NE}^{*}}{1-P_{NE}} & \text{Otherwise} \end{cases}$$

The factor increases the weights of the non-English/non-Spanish speakers so they represent the new estimated percentage, P_{NE}^{*}, while reducing the weights of the remaining of the population to keep the sum of weights constant within each study area.[4]

Finally, the LP adjusted weight is

$$LPW_{ci} = LPF_c \cdot w_{ci} ,$$

where

LPF_{ci}	=	Language problem adjustment factor for study area *c*
w_{ci}	=	Person weight

[3] Defined by the questionnaire item CNTRYLNG.

[4] Milwaukee and Balance of Wisconsin are considered as a single study area for these adjustments.

The first column in tables 2-3 and 2-4 shows the number of respondents who are non-English/ non-Spanish speakers. The second and third columns give the original and revised percentage of non-English/non-Spanish speakers (P_{NE} and P^*_{NE} , respectively) for the child and adult-pair files. Table 2-5 shows the adjustment factors by study area.

For example, table 2-3 shows there are 24 non-English/non-Spanish speaking children in Michigan. They account for 1.246 percent of the total number of children in the state. As indicated before, 0.295 percent of the households in Michigan are LP households. The weights of the 24 children are adjusted so they represent 1.541 percent of the children in the state (1.246% + 0.295%). The weights of the non-English/non-Spanish speaking respondents are adjusted by a factor of 1.237, while the remaining are adjusted by a factor of 0.997.

Table 2-3.
Number of Non-English/Non-Spanish Speakers in the Child File

Study Area	**Number of Non-English/Non-Spanish Persons**	**Original Percentage of Non-English/Non-Spanish Persons (P_{NE})**	**Revised Percentage of Non-English/Non-Spanish Persons $\left(P^*_{NE}\right)$**
Alabama	8	0.417%	0.560%
California	44	1.941%	3.829%
Colorado	36	1.435%	1.839%
Florida	29	1.386%	2.026%
Massachusetts	45	1.992%	2.920%
Michigan	24	1.246%	1.541%
Minnesota	21	1.052%	1.294%
Mississippi	7	0.416%	0.576%
New Jersey	40	1.832%	2.735%
New York	43	1.497%	3.207%
Texas	24	1.414%	1.863%
Balance of U.S.	29	0.825%	1.208%
Washington	64	2.385%	2.890%
Wisconsin	34	0.994%	1.231%

Table 2-4.
Number of Non-English/Non-Spanish Speakers in the Adult Pair File

Study Area	Number of Non-English/Non-Spanish Persons	Original Percentage of Non-English/Non-Spanish Persons (P_{NE})	Revised Percentage of Non-English/Non-Spanish Persons $\left(P^*_{NE}\right)$
Alabama	36	1.161%	1.306%
California	290	6.871%	8.76%
Colorado	132	2.347%	2.751%
Florida	123	3.221%	3.861%
Massachusetts	333	5.336%	6.263%
Michigan	98	1.794%	2.089%
Minnesota	127	2.217%	2.458%
Mississippi	13	0.301%	0.461%
New Jersey	313	5.088%	5.991%
New York	234	5.139%	6.848%
Texas	81	2.093%	2.541%
Balance of U.S.	122	1.666%	2.049%
Washington	232	3.861%	4.366%
Wisconsin	166	1.482%	1.719%

Table 2-5.
Language Problem Adjustment Factor (LPF)
for the Child and Adult Pair Files, by Study Area

Study Area	Child File		Adult File	
	Non-English/ Non-Spanish Speakers	English or Spanish Speakers	Non-English/ Non-Spanish Speakers	English or Spanish Speakers
Alabama	1.350	0.999	1.125	0.999
California	1.973	0.981	1.275	0.980
Colorado	1.282	0.996	1.172	0.996
Florida	1.462	0.994	1.199	0.993
Massachusetts	1.466	0.991	1.174	0.990
Michigan	1.237	0.997	1.165	0.997
Minnesota	1.230	0.998	1.109	0.998
Mississippi	1.386	0.998	1.534	0.998
New Jersey	1.493	0.991	1.178	0.990
New York	2.142	0.983	1.333	0.982
Texas	1.317	0.995	1.214	0.995
Balance of U.S.	1.464	0.996	1.230	0.996
Washington	1.212	0.995	1.131	0.995
Wisconsin	1.238	0.998	1.160	0.998

Since the number of respondents for when the weight is adjusted upward is small, the change in the estimates of the percentage of foreign-born persons resulting from this procedure will not be very large. The magnitude of adjustments is different for children and adults. For children, California and New York have the largest adjustment factors. For adults, Mississippi has the largest adjustment factor.

2.2 Adjustment for American Indian/Asian Pacific Islander Populations

The objective of the second weighting adjustment is to improve the estimates of total population of American Indians (AI) and Asian Pacific Islanders (API). In the original weights, no controls were implemented for these subgroups; and the number of AIs was overestimated and the number of APIs was underestimated.

Poststratification of the weights after the LP adjustment was used to make this adjustment. New control totals of the number of persons (children and adults, separately) were prepared for each census region. The control totals were the number of AI persons, the number of API persons, and the number of all other persons, derived using the 1997 population estimates by state, age, race, and Hispanic origin produced by the Census Bureau (http://www.census.gov/population/www/estimates/st_sasrh.html).[5] The adjustment is done separately for the child weight and the two adult weights.

The poststratification factor is

$$PS_c = \frac{CT_c}{\sum w_{ci}}$$

where c is the adjustment class,

CT_c = poststratification control total for cell c

w_{ci} = weight LPW_i for person i.

The adjusted weight, $AIASW$, is

$$AIASW_{i(c)} = PS_c \cdot w_{ci}$$

where

[5] The file is the 1990 to 1997 Annual Time Series of State Population Estimates by Age, Sex, Race, and Hispanic Origin. The file contains estimates of the population of the 50 states and the District of Columbia by single year of age (0 to 84, 85+), sex (male, female), modified race (white; black; American Indian, Eskimo, and Aleut; Asian and Pacific Islander) and Hispanic origin. There is one file for April 1, 1990, consistent with the 1990 Census of Population as corrected and one file for each year's estimates: July 1, 1990; July 1, 1991; July 1, 1992; July 1, 1993; July 1, 1994; July 1, 1995; July 1, 1996; and July 1, 1997.

w_{ci} = LP adjusted person weight (child or adult)

$AIASW_{i(c)}$ = adjusted weight for cell *c*

Table 2-6 shows the values of the poststratification factor for the child and adult weights. For example, the weight of the AI children in the Northeast is adjusted by a factor of 0.39. The net effect is a considerable reduction of the AI population and large increases of the API population in the South and West.

Table 2-6.
Adjustment Factors for the American Indian-Asian/Pacific Islander Poststratification Adjustments

	Child	Adult Pair	Random Adult
Adjustment Factors			
Northeast			
American Indian	0.36	0.59	0.63
Asian/Pacific Islander	1.07	1.23	1.23
Other	0.94	0.98	0.98
Midwest			
American Indian	0.67	0.48	0.48
Asian/Pacific Islander	0.85	0.87	0.88
Other	0.97	0.98	0.98
South			
American Indian	0.77	0.90	0.94
Asian/Pacific Islander	1.53	1.55	1.48
Other	1.01	1.03	1.03
West			
American Indian	0.57	0.67	0.70
Asian/Pacific Islander	1.43	1.49	1.53
Other	1.05	1.02	1.02

2.3 Adjustment to Control Totals with Undercount

After making the previous two adjustments, the third adjustment is to make the estimates consistent with the undercount-adjusted population totals. To do this, the person weights (adjusted as described in the previous sections) are raked to new derived control totals. The raking is done exactly as described in chapter 3 of report no. 3. As noted there, the child weights and adult weights are raked separately. The new controls represent the 1997 U.S. civilian noninstitutionalized population under 65 years old, adjusted for undercoverage. An explanation of how these controls were developed is presented in the next section.

The raked weight is the product of the weight adjusted for the poststratification to the AI and API populations and the raking factor. Using the notation from report no. 3, the new raked weight,[6] $NFRW_{i,cd}$, is

$$NFRW_{i,cd} = NRF_{cd} \cdot w_{i,cd} ,$$

where

NRF_{cd}	=	new raking factor for dimensions *c* and *d*
$w_{i,cd}$	=	the AIASW person weight
$NFRW_{i,cd}$	=	new raked final person weight

Tables 2-7 and 2-8 present the weighted average of the new raking factors for different domains for children and adults by study area, respectively. The adult factors are only given for the adult pair weight, but the random adult weight factors are very similar. These tables are identical in structure to the tables given in report no. 3 for the raking to the unadjusted totals. However, in this case the adjustments are much smaller because the weights had already been raked previously to the control totals with no undercount adjustment. For example, in Michigan, the average raking factor across all children is 1.04 with a minimum raking adjustment of 0.99 and a maximum factor of 1.16. The sum of the weights after the adjustment is 2,548,528.

[6] No trimming of the weights was needed in this round because the weights were already trimmed when they were raked to the control totals without the undercount adjustment.

Table 2-7.
Revised Raking Adjustment Factors for the Child Weights, by Study Area

	AL Alabama	CA California	CO Colorado	FL Florida	MA Massachusetts	MI Michigan	MN Minnesota
Sum of Weights After Adjustment	1,115,670	9,204,798	1,037,275	3,586,928	1,459,567	2,548,528	1,262,048
Mean Factor (weighted)	1.02	0.96	0.97	1.02	1.09	1.04	1.04
Min Factor	0.97	0.82	0.92	0.94	1.04	0.99	1.01
Max Factor	1.12	1.16	1.13	1.13	1.30	1.16	1.17
Factors by Race/Ethnicity							
Hispanic	1.07	1.02	1.02	1.05	1.15	1.07	1.12
Black, Non-Hispanic	1.07	1.11	1.10	1.08	1.20	1.11	1.14
Other	0.99	0.90	0.96	0.99	1.07	1.02	1.04
Factors by Sex							
Male	1.02	0.95	0.97	1.02	1.09	1.04	1.05
Female	1.02	0.96	0.97	1.02	1.08	1.03	1.04
Factors by Home Ownership							
Owner	1.02	0.95	0.97	1.02	1.08	1.03	1.04
Renter	1.02	0.96	0.98	1.01	1.09	1.05	1.05

(continued)

Table 2-7.
Revised Raking Adjustment Factors for the Child Weights, by Study Area (Continued)

	MS Mississippi	NJ New Jersey	NY New York	TX Texas	WA Washington	WI Wisconsin	U.S. Bal. U.S.	Total U.S.
Sum of Weights After Adjustment	787,546	2,011,423	4,642,640	5,749,528	1,487,708	1,364,322	34,942,945	71,200,924
Mean Factor (weighted)	1.027	1.07	1.09	1.02	0.96	1.04	1.04	
Min Factor	0.97	0.99	0.94	0.94	0.92	0.78	1.00	
Max Factor	1.11	1.23	1.32	1.10	1.17	1.53	1.18	
Factors by Race/Ethnicity								
Hispanic	1.07	1.12	1.16	1.04	1.03	1.10	1.09	
Black, Non-Hispanic	1.06	1.18	1.19	1.07	1.10	1.15	1.09	
Other	1.00	1.04	1.04	0.99	0.95	1.03	1.03	
Factors by Sex								
Male	1.03	1.07	1.09	1.01	0.97	1.05	1.04	
Female	1.03	1.07	1.09	1.03	0.96	1.04	1.04	
Factors by Home Ownership								
Owner	1.03	1.06	1.09	1.02	0.96	1.04	1.04	
Renter	1.03	1.10	1.09	1.02	0.97	1.05	1.04	

Table 2-8.
Revised Raking Factors for the Adult Pair Weights, by Study Area

	AL Alabama	CA California	CO Colorado	FL Florida	MA Massachusetts	MI Michigan	MN Minnesota
Sum of Weights After Adjustment	2,665,554	19,786,065	2,477,570	8,415,763	3,758,237	5,973,834	2,822,970
Mean Factor (weighted)	0.98	0.97	0.99	0.98	1.00	1.06	1.03
Min Factor	0.89	0.86	0.91	0.86	0.93	1.00	0.97
Max Factor	1.09	1.16	1.10	1.13	1.14	1.15	1.16
Factors by Race/Ethnicity							
Hispanic	1.07	1.07	1.04	1.04	1.09	1.12	1.10
Black, Non-Hispanic	1.02	1.08	1.06	1.03	1.09	1.10	1.10
Other	0.97	0.92	0.97	0.95	0.99	1.05	1.03
Factors by Sex							
Male	0.98	0.96	0.98	0.98	1.00	1.06	1.03
Female	0.98	0.97	0.99	0.97	1.00	1.06	1.03
Factors by Home Ownership							
Owner	0.98	0.97	0.99	0.98	1.00	1.06	1.03
Renter	0.98	0.97	0.98	0.97	1.00	1.06	1.02
Factors by Education							
Some High School	0.98	1.00	1.01	0.98	1.01	1.06	1.03
Finished High School	0.98	0.98	0.99	0.98	1.00	1.06	1.03
Some College	0.98	0.93	0.97	0.96	1.00	1.05	1.02

Table 2-8.
Revised Raking Factors for the Adult Pair Weights, by Study Area (Continued)

	MS Mississippi	NJ New Jersey	NY New York	TX Texas	WA Washington	WI Wisconsin	U.S. Bal. U.S.	Total U.S.
Sum of Weights After Adjustment	1,631,161	4,904,394	11,087,360	11,902,629	3,487,233	3,098,818	80,799,665	162,811,252
Mean Factor (weighted)	0.98	1.02	1.01	0.97	0.97	1.02	1.00	
Min Factor	0.92	0.93	0.89	0.87	0.91	0.64	0.95	
Max Factor	1.09	1.17	1.21	1.11	1.19	1.55	1.20	
Factors by Race/Ethnicity								
Hispanic	1.05	1.12	1.10	1.03	1.07	1.08	1.08	
Black, Non-Hispanic	1.01	1.10	1.09	1.01	1.08	1.08	1.04	
Other	0.96	0.99	0.98	0.94	0.96	1.02	0.99	
Factors by Sex								
Male	0.98	1.02	1.01	0.97	0.97	1.02	1.00	
Female	0.98	1.02	1.01	0.97	0.98	1.02	1.00	
Factors by Home Ownership								
Owner	0.98	1.01	1.01	0.97	0.97	1.02	1.00	
Renter	0.97	1.04	1.01	0.97	0.97	1.02	1.00	
Factors by Education								
Some High School	0.98	1.05	1.01	0.98	0.99	1.04	1.01	
Finished High School	0.98	1.02	1.02	0.97	0.97	1.02	1.00	
Some College	0.97	1.01	0.99	0.96	0.97	1.02	0.99	

2.4 Derivation of Control Totals

The methods used to produce the control totals of the 1997 noninstitutionalized civilian population of the United States without adjustments for the population undercount in the 1990 census[7] were described in report no. 3, chapter 3. Most other surveys, including the surveys conducted by the Census Bureau, such as the CPS and the SIPP, use control totals that do include adjustments for the undercount. This section describes how the new controls with the undercount adjustment were developed for the 1997 NSAF.

Since the 1997 March CPS estimates are adjusted to the desired control totals, we used these estimates as the basis for producing the control totals for the 1997 NSAF. It is important to note that the control totals are not based on the CPS, rather the CPS is adjusted to these totals so that it is a valid source for the numbers of persons. Control totals were obtained for the following groups: study area, sex, three race-ethnicity groups (Hispanic, black/non-Hispanic, and other), and seven age groups (0–5, 6–17, 18–24, 25–34, 35–44, 45–54, and 55–64) at the national level.

The creation of the new control for July 1997 involved the following steps:

1. We computed the civilian population total for July 1997 using the file produced by the Census Bureau (see section 2 of this chapter) with the July 1997 estimates of the civilian population broken down by state, sex, race-ethnicity, and age. These population totals include the institutionalized population, which is not eligible for the NSAF. In addition, these estimates are not adjusted to include the undercount. The Census Bureau's estimates of the total U.S. population is 267,636,061 persons.

2. We computed the total undercount for July 1997 using the file containing the net undercount for the 1990 census broken down by state, sex, race-ethnicity, and age. We assume that the undercount is the same for 1990 and July 1997. The total undercount is 4,046,555 persons.

3. We computed the total institutionalized population for July 1997 using an internal file provided by the Census Bureau, that contained the July 1996 institutionalized population totals broken down by state, sex, race-ethnicity, and age groups. We assume that the total institutionalized population is the same for July 1996 and July 1997. The total institutionalized population is 4,903,623 persons.

4. Combining the three previous files we consolidated a file that contained the totals for the July 1997 civilian noninstitutionalized population, adjusted for undercount at the site level and broken down by study area, race-ethnicity, and age groups. The total population is 266,778,993 persons, according to our calculations.

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[7] About 4 million persons residing in the United States on census day were not enumerated in 1990. This underenumeration is known as the "estimated net census undercount."

5. We estimated the March 1997 civilian noninstitutionalized population of the United States (adjusted for undercount) by scaling back the population estimates for July 1997 by the factor *F*, defined as

$$F = \frac{265{,}920{,}503}{266{,}778{,}993} = 0.996782,$$

   where 265,920,503 is the total population of the United States in March 1997 (computed using the March 1997 CPS). The total population after the adjustment is 265,920,503. Tables 2-9 and 2-10 show the dimensions used in poststratification.

6. We raked the population estimates to March 1997 CPS population totals using two dimensions shown in tables 2-9 and 2-10. The new population totals are consistent with the CPS estimates at the site level, and by gender, race-ethnicity, and age groups at the national level. The total population is 265,920,503.

We created two control files using these data, one for the child weighting and the other for the adult weighting. The first dimension is defined as the cross-tab of gender, race-ethnicity, and age groups within the study area. New control totals for tenure for children and tenure-education for adults were recreated as described in report no. 3, section 3.2.1.

Table 2-9.
First Dimension of Raking

No.	Race-Ethnicity	Sex	Age Group	Control Total
1	Hispanic	Male	0 – 5	2,103,015
2	Hispanic	Male	6-17	3,381,152
3	Hispanic	Male	18-24	2,007,690
4	Hispanic	Male	25-34	2,834,137
5	Hispanic	Male	35-44	2,223,691
6	Hispanic	Male	45-54	1,283,526
7	Hispanic	Male	55-64	690,342
8	Hispanic	Male	65+	646,320
9	Hispanic	Female	0 – 5	2,001,200
10	Hispanic	Female	6-17	3,055,320
11	Hispanic	Female	18-24	1,681,321
12	Hispanic	Female	25-34	2,585,273
13	Hispanic	Female	35-44	2,131,990
14	Hispanic	Female	45-54	1,277,672
15	Hispanic	Female	55-64	855,243
16	Hispanic	Female	65+	875,786
17	Black non-Hispanic	Male	0 – 5	1,846,386
18	Black non-Hispanic	Male	6-17	3,781,532
19	Black non-Hispanic	Male	18-24	1,668,304
20	Black non-Hispanic	Male	25-34	2,291,204
21	Black non-Hispanic	Male	35-44	2,396,070
22	Black non-Hispanic	Male	45-54	1,565,339
23	Black non-Hispanic	Male	55-64	906,389
24	Black non-Hispanic	Male	65+	1,013,523
25	Black non-Hispanic	Female	0 – 5	1,805,039
26	Black non-Hispanic	Female	6-17	3,672,372
27	Black non-Hispanic	Female	18-24	1,885,986
28	Black non-Hispanic	Female	25-34	2,839,271
29	Black non-Hispanic	Female	35-44	2,864,139
30	Black non-Hispanic	Female	45-54	1,889,733
31	Black non-Hispanic	Female	55-64	1,187,388
32	Black non-Hispanic	Female	65+8	1,566,315
33	Other	Male	0 – 5	8,262,812
34	Other	Male	6-17	17,054,960
35	Other	Male	18-24	8,718,701
36	Other	Male	25-34	14,565,783
37	Other	Male	35-44	16,937,385
38	Other	Male	45-54	13,223,149
39	Other	Male	55-64	8,628,432
40	Other	Male	65+	11,773,363
41	Other	Female	0 – 5	7,907,170
42	Other	Female	6-17	16,329,966
43	Other	Female	18-24	8,862,612
44	Other	Female	25-34	14,760,684
45	Other	Female	35-44	17,159,716
46	Other	Female	45-54	13,723,143
47	Other	Female	55-64	9,166,937
48	Other	Female	65+	16,033,020
Total				**265,920,503**

Table 2-10.
Second Dimension of Raking

Study Area	Control Total
Alabama	4,313,119
California	32,361,979
Florida	14,589,535
Massachusetts	6,011,230
Michigan	9,664,929
Minnesota	4,611,332
New Jersey	7,947,347
New York	17,977,050
Texas	19,485,171
Washington	5,587,901
Mississippi	2,731,979
Balance of the U.S.	131,660,585
Colorado	3,887,831
Wisconsin	5,090,517
Total	**265,920,503**

2.5 Special Purpose Weights

The procedures described in the previous sections resulted in revised weights for children, adults (the adult pair weight, the random adult weight, and the adult childless weight), and families. These weights and their uses were described in report no. 3. In addition to these weights, two additional weights were produced for special analytic purposes. These weights are designed for the analysis of data related to children only if the data were collected prior to the beginning of summer.

The 1997 NSAF data collection period went from the middle of February to November 2, 1997. As the data collection extended into the summer, some items were revised and a “summer” version of the questionnaire was administered. While some questions were revised to reflect the activities that were pertinent to summer, most other questions remained unchanged. Also, since some questions have the wording “in the last month,” the responses may have been different depending on the time when the person was interviewed. The effect on the responses is more evident in questions such as childcare arrangements, babysitting, and head start enrollment. This effect is known as the “summer effect.”

To deal with the summer effect, we created new weights for children (SCW) and the most knowledgeable adults or MKAs (SAW). In the new weights, data collected during the summer months are excluded and the weights for those who responded before the summer contain an additional nonresponse adjustment. The additional weighting adjustment or summer nonresponse adjustment (SNR) accounts for the effect of the children and MKAs interviewed during the summer. The data collected for summer children and MKAs are not used in the data analysis because they do not have a final weight.

The creation of the summer weights follows the same steps described in the previous chapters for both children and MKAs. The only difference is that the summer nonresponse adjustment is done after the multiple-telephone adjustment but before the first raking adjustment.[8] These adjustments are described in more detail below.

2.5.1 Summer Child Weights

The summer child weights are based on the child weights, with an additional adjustment to reflect the summer nonresponse. Staff at the Urban Institute examined the data and identified interviews that were conducted during the summer. These cases are considered nonrespondents for these special weights. The adjustment is applied to the multiple telephone-adjusted child weight[9] (CIW) within weighting classes. The summer nonresponse adjustment factor (SNR_c) is

$$SNR_c = \frac{\sum_{S} w_{ci} + \sum_{NS} w_{ci}}{\sum_{NS} w_{ci}}$$

where

w_{ci}	=	Multiple telephone-adjusted child weight (CIW_i) assigned to child i in adjustment cell c;
S	=	Children interviewed in the summer;
NS	=	Children not interviewed in the summer.

Table 2-11 shows the number of children who completed the extended interview, the number of children interviewed in the summer, and the weighted summer response rate at the study-area level. For example, for Michigan, 1,658 children completed the extended interview, and 485 children were interviewed in the summer. The response rate for Michigan for the summer-adjusted child interview is 80.17 percent. It is important to remember that this response rate is an additional level of nonresponse beyond the other sources of nonresponse for the child interview.

[8] See report no. 3, sections 2.2.1 and 2.2.2, for the first raking adjustment for children and adults.
[9] See report no. 3, section 2.2.1, for a description of the multiple telephone adjusted child weight.

Table 2-11.
Number of Children Interviewed in the Summer and Summer Response Rate, by Study Area

Study Area	Children who Completed the Extended Interview	Children Interviewed During the Summer	Summer Response Rate (Weighted)
Alabama	1,696	402	80.75%
Bal. of Wisconsin	1,827	493	78.61%
California	1,539	521	74.73%
Colorado	1,076	1,222	44.33%
Florida	1,615	448	77.43%
Massachusetts	1,811	570	73.67%
Milwaukee	1,451	353	80.17%
Michigan	1,658	485	76.46%
Minnesota	1,838	522	77.29%
Mississippi	1,598	386	79.98%
New Jersey	1,916	650	76.71%
New York	1,749	503	78.60%
Texas	1,801	448	81.54%
Balance of U.S.	2,722	670	80.26%
Washington	1,850	619	74.46%
Total	**26,147**	**8,292**	**78.33%**

The summer adjusted child weight SCW_i is

$$SCW_i = SNR_c \cdot CIW_{i(c)} .$$

where the terms were defined above.

As indicated before, the nonresponse adjustment is carried out within weighting cells. The categorical search algorithm CHAID[10] was used to create the cells. The variables used in the CHAID analysis are shown in table 2-12. The results of the CHAID analysis are shown in table 2-13 by study area. The variable SMPTYPE that indicates if the child is part of the area or telephone sample was included as the first variable in the analysis (irrespective of whether it would have been chosen as such by the CHAID algorithm) because most of the in-person sample was completed before the summer. The variables with numbers assigned were used to form adjustment cells.

[10] See report no. 3, section 2.1.2 for a description of CHAID.

Table 2-12.
Variables Used in the CHAID Analysis for Summer Weights

Variable Name	Description	Levels	Description
SMPTYPE	Sample type	1	Area sample
		2	RDD sample
MKAEDU	MKA education attainment	1	Less than high school
		2	High school
		3	More than high school
FMLYINCM	Family income	1	Poor using food stamps
		2	Poor no food stamps
		3	Missing income
		4	Near poor
		5	Low and middle
		6	Above $35,000
MKARACE	MKA race	1	White
		2	Black
		3	Other
CHLDAGE*	Child age	1	Preschool, age 0–4
		2	School age, age 5–12
		3	Teenager, age 13–17
MKA_AGE**	MKA age	1	Age 18–29
		2	Age 21–30
		3	Age 31–40
		4	Age 41–64

*Used in summer child CHAID analysis.
**Used in summer MKA CHAID analysis.

After the child weights were adjusted for summer nonresponse, the weights were created using the weighting procedures used for the child weights, including the revised weights described earlier in this chapter.

Table 2-13.
Variables That Were Predictive* of Nonresponse in the Summer Child File

Site	Number of cells	SMPTYPE	FMLYINCM	MKAEDU	MKARACE	CHLDAGE
Alabama	11	1	2, 4	3	4, 5	3 ,6
Bal. of Wisconsin	3	1			2	
California	4	1		3		2
Colorado	7	1	4	3	3	2
Florida	8	1	3	2, 4		2
Massachusetts	11	1	2, 4	3, 5		3
Michigan	9	1	2	3		3
Milwaukee	12	1	3	4	2	4
Minnesota	9	1	2, 4	3		3
Mississippi	9	1	2	3		2, 3
New Jersey	3	1				2
New York	9	1		4	2, 3	2
Texas	8	1	3	4	3	2
Balance of U.S.	10	1	3, 4	3, 5	4	2
Washington	3	1				2

* The number indicates how strongly the variable was found to be predictive of the response rate. A lower number indicates the more powerful predictor.

2.5.2 Summer MKA

The second special-purpose weight is for the MKA of children who were not interviewed during the summer. This summer MKA weight is based on the adult weight (APW), but has an additional nonresponse weighting adjustment for MKAs of children interviewed during the summer. This is similar to the summer child nonresponse adjustment. However, the summer MKA required a slightly different process at the raking stage because no control totals exist for MKAs. The MKA population is a subgroup of the adult population, but there are no separate control totals for MKAs. For the standard MKA weight, this concern does not arise because the adult pair weight is used for MKA analysis and no separate control totals for MKAs are needed.

To deal with this issue, sample-based control totals were computed using the final adult weights (NFAPW) using only MKA records. The MKA summer weights were then raked to these sample-based control totals.[11] In other words, the MKA summer weights were forced to be consistent with the MKA totals from the full sample. Since the control totals are subject to sampling variation (in most other applications the control totals are known without sampling error), we computed the sample-based control totals separately for each replicate in the adult weight file. Each replicate in the MKA file was then raked the corresponding replicate sample-based control total. An important additional benefit of this approach is that the sum of the weights for the MKA population created using the standard MKA weights and the summer MKA weights are consistent. The weighting steps[12] are now described in more detail.

The summer nonresponse adjustment is applied to the multiple telephone–adjusted adult weight[13] (NAW) within weighting classes. The adjustment is applied to all MKAs interviewed during non-summer months to account for the MKAs interviewed during the summer. As noted above, the other types of adults (those who are not MKAs) are included in this process these adults have an adjustment factor of 1. The summer nonresponse adjustment factor for MKAs (SNR_c) is

$$SNR_c = \begin{cases} \dfrac{\sum\limits_{S} w_{ci} + \sum\limits_{NS} w_{ci}}{\sum\limits_{NS} w_{ci}} & \text{If the adult is an MKA} \\ 1 & \text{Otherwise} \end{cases}$$

where

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[11] To accomplish this, all adult records except for those of the summer MKAs are included in the weighting procedures. The non-MKAs are dropped at the last stage of the weighting adjustment before the weights are raked to MKA totals.

[12] In checking the summer adjusted weights received from Westat, we discovered that 1,059 observations were missing a summer-adjusted weight in the non-summer MKA child care file. A "hot decking" procedure was employed rather than having Westat rerun the weighting procedure again. While hot decking is a common choice in imputing missing content variables, its use in imputing weights is clearly nonstandard. Basically we did this because the problem seemed small and any roughness in the approach was predicted to make little difference (as seems to be the case). Central to this thinking was the fact that the total number of observations with missing weights represents 5 percent of the observations in the nonsummer sample. In addition, for a wide selection of characteristics of MKAs without weights, the observations we looked at were very much like the observations with weights. Appendix A serves to further document the MKA weight creation process by taking up the weight creation process after Westat had finished its work.

[13] See report no. 3, section 2.2.2, for a description of the multiple telephone–adjusted adult pair weight.

w_{ci} = Multiple telephone–adjusted adult pair weight (NAW_i) assigned to adult i in adjustment cell c;

S = Adults interviewed in the summer;

NS = Adults interviewed some other time.

Table 2-14 shows the number of adults who completed the extended interview, the number of interviewed MKAs, and the weighted summer MKA response rate by study area level. For example, for Michigan, there are 4,677 adults who completed the extended interview; 457 are MKAs interviewed in the summer and 1,298 are MKAs interviewed some other time. The MKA response rate for Michigan is 69.48 percent. Again, keep in mind that this is an additional level of nonresponse.

Table 2-14.
Number of MKAs Interviewed in the Summer and Summer Response Rate, by Study Area

Study Area	Adults Who Complete the Extended Interview	MKAs Interviewed in the Summer	MKAs Interviewed Some Other Time	Summer MKA Response Rate (Weighted)
Alabama	4,182	391	1,328	77.72%
Bal. of Wisconsin	5,362	458	1,454	75.82%
California	4,282	498	1,114	70.03%
Colorado	5,344	1,055	824	42.67%
Florida	3,922	459	1,216	72.60%
Massachusetts	5,297	576	1,373	69.48%
Michigan	4,677	457	1,298	73.69%
Milwaukee	3,497	341	1,128	76.10%
Minnesota	5,557	483	1,432	75.19%
Mississippi	3,941	378	1,221	76.38%
New Jersey	5,940	655	1,423	70.82%
New York	4,291	494	1,310	73.17%
Texas	4,181	440	1,365	76.70%
Balance of U.S.	7,905	648	2,118	76.37%
Washington	5,790	572	1,418	71.19%
Total	**74,168**	**7,905**	**20,022**	**74.25%**

The summer adjusted MKA weight SAW_i is

$$SAW_i = SNR_c \cdot NAW_i$$

using the terms defined above.

The variables used for the creation of the cells are listed in table 2-12. The results of the CHAID analysis are shown in table 2-15 by study area. As we did for children, the variable SMPTYPE that indicates whether the MKA is part of the area or telephone sample was included as the first variable in the analysis because most of the area sample was completed before the summer. The variables with number assigned were used to form adjustment cells.

Table 2-15.
Variables That Were Predictive* of Nonresponse in the Summer MKA File

Site	Number of cells	SMPTYPE	FMLYINCM	MKA_AGE	MKARACE
Alabama	7	1	3	2, 4	2
Bal. of WI	3	1		2	
California	2	1			
Colorado	4	1		2	3
Florida	4	1	2	2	
Massachusetts	5	1	2	2	
Michigan	8	1	3	2, 5	4
Milwaukee	8	1	2	3	
Minnesota	2	1			
Mississippi	5	1	2	2	
New Jersey	6	1	3	2	3
New York	5	1	3	3	2
Texas	4	1	3	2	
Balance of U.S.	6	1	3	2	4
Washington	9	1	3	2	

* The number indicates the variable was found to be predictive of the response rate the variable was found to be. A lower number indicates a more powerful predictor.

After we adjusted for summer nonresponse, the weights follow the same stages of the adult weighting process except for the final raking. At the final adjustment, the weights of the MKAs are raked to control totals derived using the MKA records of the adult sample after all records for adults who are not MKAs are eliminated from the file. Sample-based control totals for each study area by sex/race-ethnicity/age group and region/tenure/education were created for each replicate. Each summer MKA replicate is raked to the corresponding replicate control total. In this way, the summer MKA weights reflect the sampling variation in the estimates of the MKA populations. The raking factor is

$$PS_{c(j)} = \frac{\hat{C}T_{c(j)}}{\sum w_{ci(j)}},$$

where c is the adjustment class,

$\hat{C}T_{c(j)}$ = Estimate of the control total for cell c for replicate j;

$w_{ci(j)}$ = The weight ($AIASAPW_i$) assigned to MKA i in replicate j.

The final MKA weight ($FSAPW$) is

$$FSAPW_{ic(j)} = PS_{c(j)} \cdot w_{ci(j)}.$$

Chapter 3
Coverage Analysis

An important concern in any survey is the extent to which all the members of the target population have an opportunity to be interviewed. This is called survey coverage. Any omission of units from the sampling process is called undercoverage. This chapter considers coverage in the 1997 NSAF.

The first section below defines coverage ratios and discusses how these ratios are related to coverage. The next section examines overall NSAF coverage ratios. Section 3.3 examines the household and within household components of coverage. Section 3.4 compares NSAF coverage rates to those of other surveys. The final section summarizes the findings.

3.1 Coverage and Coverage Ratio

There are a number of possible reasons for undercoverage in the 1997 NSAF. Household undercoverage could occur in the RDD sample if there are telephone households in 100-banks of telephone numbers in which none of the numbers is listed; if contacted telephone numbers are misclassified as being nonresidential; or if some telephone numbers cannot be categorized as residential or nonresidential (ring/no answer or answering machine). Household undercoverage can occur in the area frame from failing to list a residential unit, from misclassifying a unit as nonresidential or vacant, or because the sampling frame was truncated to those block groups (BGs) with relatively high 1990 census nontelephone rates. In addition, household undercoverage can occur in both the RDD and area frames because a household is mistakenly classified as only having persons 65 and over.

Undercoverage is possible even when whole households are completely covered. Within household undercoverage occurs in interviewed households if all eligible persons are not identified. A respondent may intentionally fail to mention a household member, may forget to mention a member, or may think someone who would not be included in any other household is not a household member. Interviewer error can also lead to within-household undercoverage. See Statistical Policy Office (1990) for a detailed discussion of types of coverage error.

One method to evaluate coverage is to look at coverage ratios. A coverage ratio is usually calculated as the ratio of a sample estimate based on weights that exclude any poststratification adjustment to the corresponding control total. If there is neither undercoverage nor overcoverage, then the expected value of the coverage ratio is 1.0. A coverage ratio significantly less than 1.0 indicates a shortfall in the coverage of the sample for reasons other than nonresponse. Shapiro et al. (1996) discuss the distinction between undercoverage and nonresponse.

Coverage ratios may be different from 1.0 even if there is complete coverage. This can occur if the control totals used in the ratios are not correct or the variables are not measured the same way in the survey and in the control totals. Differential nonresponse can also affect coverage ratios. For example, if small households are nonrespondents more often than large households, the household nonresponse adjustment will tend to overadjust by increasing the weights on large households to account for nonresponding small households. This will result in coverage ratios over 1.0. Thus, the coverage ratios given are not a simple complete measure of coverage. Despite these limitations, coverage ratios do provide valuable information about a survey's coverage of the target population.

3.2 Overall Undercoverage

Table 3-1 gives coverage ratios for children by study area and race/ethnicity. The numerator of each coverage ratio is the estimated total number of children using weights that do not include either the household or child poststratification factors. The weights include all other components of the estimation procedure, including household undercoverage adjustment factors for the truncation of the area sampling frame. The denominator is the corresponding control total count of the number of children used in the poststratification adjustment discussed earlier in this report. The control totals include the adjustment for the census undercount. These coverage ratios reflect undercoverage due to unknown sources. If the adjustments for nonresponse and undercoverage due to the truncation of the area frame completely accounted for these factors, these ratios should have an expected value of 1.0. The coverage ratios are also subject to sampling error because the numerator is a sample estimate. We have not computed sampling errors for these ratios, but clearly small differences are not statistically significant.

Table 3-1.
Overall Coverage Ratios for Children, by Race/Ethnicity and Study Area

Study Area	Hispanics	Blacks	Non-Hispanic, Non-black	All
Alabama	1.24	0.77	0.97	0.91
California	0.98	0.76	0.94	0.94
Colorado	0.92	0.73	1.00	0.97
Florida	0.91	0.68	0.87	0.83
Massachusetts	1.02	0.83	1.00	0.99
Michigan	1.13	0.77	0.95	0.92
Minnesota	1.12	0.96	0.97	0.97
Mississippi	1.88	0.73	0.89	0.82
New Jersey	0.93	0.79	1.04	0.98
New York	0.98	0.83	1.02	0.98
Texas	0.89	0.78	0.91	0.89
Washington	1.10	0.79	0.96	0.97
Milwaukee	0.89	0.60	0.93	0.80
Balance of Wisconsin	1.06	0.92	0.93	0.93
Balance of U.S.	0.92	0.64	1.03	0.96
U.S.-Weighted	0.95	0.70	0.99	0.94
U.S.-Unweighted	1.06	0.77	0.96	0.92

The coverage ratio for a national estimate of children is .94, or 94 percent.[14] The northeastern states (Massachusetts, New Jersey, and New York) have somewhat better coverage than those in the south (Alabama, Florida, Mississippi, and Texas) for non-Hispanic, non-black children.

Coverage of Hispanic children is not significantly different from coverage of all children (.95 versus .94). On the other hand, coverage of black children nationally is only .70. The national average for black children is depressed because of the low ratio in the balance of the United States. This study area has a large effect because it accounts for about 50 percent of the national total.

We investigated whether the low coverage for the balance of the United States was due to sampling error associated with the sampling of Primary Sampling Units (PSUs). If this particular sample of PSUs had a relatively low proportion of the black population we would expect undercoverage due to sampling. Since it contains many states, the balance of the United States contains PSUs with a wide range values for the of the proportion black. The PSUs in the balance of the U.S. were stratified on several variables, but not on proportion black. We found that the particular set of sample PSUs selected can be expected to result in an estimate of the black population that is only 86.4 percent of the actual black population. Thus, if estimates were based only on the area sample, one could expect a coverage ratio of .864 due to this factor alone. However, only an estimated 9 percent of the black population in the balance of the United States is without telephone. Thus, for the combined area/RDD sample, the expected a coverage ratio is .988 due to this factor (.09 * .864 + .91 = .988). Also, as discussed later, coverage of black adults, unlike coverage of black children, is not particularly low for the balance of the United States.

Table 3-2 gives coverage ratios for adults under 65, by sex and race/ethnicity. The national adult coverage ratio is .86, considerably lower than for children. As will be discussed in the next section, this appears to be due to poorer coverage of households without children but with adults under 65, rather than to poorer coverage of persons within households. In contrast to the child ratios, the coverage ratios have no obvious tendencies among study areas.

As is true in most household surveys, coverage is lower for males than for females and for Hispanics and blacks than for others. The lowest coverage ratio is for black males (.65 for the nation).

Hispanic coverage ratios are highly variable and not very meaningful for states that have a small Hispanic population. It has been postulated that in most surveys coverage for blacks is worse in large cities than in more rural areas. The NSAF results do not support this view, with low black coverage ratios in predominately rural Alabama and Mississippi.

[14] The unweighted averages across study areas are shown in the table as well as weighted national averages. For all children, the difference is slight (.94 versus .92).

Table 3-3, like table 3-2, gives coverage ratios for adults under 65, but by sex and age groups only for non-Hispanic, non-black persons. Younger age groups have lower coverage ratios, consistent with patterns typically found in household surveys, such as CPS (Hainer et al., 1998).

Table 3-4 also gives coverage ratios for adults under 65, but unlike the other tables includes the effect of the area frame truncation. In other words, the weights used to produce the estimates in the numerator do not include the undercoverage adjustment factor applied to the area sample weights. The U.S. weighted coverage ratio drops from .86 (table 3-2) to .85. The reduction in the coverage ratios is small for Hispanics and blacks, as well as for the total population. Thus, as expected, the frame truncation did not have a major effect on coverage since only a relatively small proportion of nontelephone households were not covered.

Table 3-2.
Overall Coverage Ratios for Adults Under 65, by Race/Ethnicity, Sex, and Study Area

	Hispanics			Blacks			Non-Hispanic, Non-black			All
Study Area	**Males**	**Females**	**Total**	**Males**	**Females**	**Total**	**Males**	**Females**	**Total**	**Persons**
Alabama	N/A	N/A	1.22	0.59	0.71	0.66	0.86	0.86	0.86	0.81
California	0.76	0.87	0.81	0.47	0.71	0.60	0.86	0.94	0.90	0.85
Colorado	0.85	0.88	0.86	0.54	0.77	0.66	0.90	0.90	0.90	0.89
Florida	0.72	0.74	0.73	0.55	0.68	0.62	0.86	0.97	0.92	0.84
Massachusetts	0.73	1.00	0.87	0.67	0.79	0.74	0.85	0.94	0.90	0.89
Michigan	1.01	0.67	0.84	0.54	0.70	0.63	0.84	0.87	0.85	0.82
Minnesota	1.32	0.91	1.12	0.60	0.78	0.69	0.89	0.93	0.91	0.91
Mississippi	1.73	2.04	1.89	0.64	0.70	0.67	0.84	0.87	0.85	0.80
New Jersey	0.80	0.84	0.82	0.61	0.68	0.65	0.90	0.97	0.93	0.88
New York	0.71	0.90	0.81	0.67	0.67	0.67	0.94	1.02	0.98	0.91
Texas	0.65	0.80	0.72	0.65	0.81	0.74	0.90	0.90	0.90	0.83
Washington	1.08	1.06	1.07	0.50	0.63	0.56	0.86	0.91	0.88	0.88
Milwaukee	0.74	1.07	0.90	0.61	0.61	0.61	0.85	0.86	0.85	0.80
Balance of Wisconsin	1.12	0.97	1.05	0.82	0.60	0.70	0.88	0.91	0.90	0.90
Balance of U.S.	0.79	0.86	0.83	0.70	0.67	0.68	0.86	0.91	0.89	0.86
U.S.–Weighted	0.75	0.85	0.80	0.65	0.69	0.67	0.87	0.92	0.90	0.86
U.S.–Unweighted	N/M	N/M	0.96	0.61	0.70	0.66	0.87	0.92	0.90	0.86

Note: NM = not meaningful.
N/A = not available.

Table 3-3.
Overall Coverage Ratios for Non-Hispanic, Non-black Adults Under 65, by Age Group, Sex, and Study Area

	Males					Females					All Persons					Males	Females	All
Study Area	**18-24**	**25-34**	**35-44**	**45-54**	**55+**	**18-24**	**25-34**	**35-44**	**45-54**	**55+**	**18-24**	**25-34**	**35-44**	**45-54**	**55+**	**All ages**	**All ages**	**persons**
Alabama	0.77	0.75	0.88	1.03	0.87	0.68	0.82	0.91	0.92	0.94	0.72	0.78	0.90	0.97	0.90	0.86	0.86	0.86
California	0.72	0.82	0.91	0.94	0.83	0.89	0.89	0.94	0.93	1.08	0.80	0.86	0.92	0.94	0.96	0.86	0.94	0.90
Colorado	0.77	0.97	0.98	0.88	0.78	0.77	0.93	1.00	0.85	0.88	0.77	0.95	0.99	0.87	0.83	0.90	0.90	0.90
Florida	0.68	0.74	0.93	1.00	0.87	0.92	0.92	0.94	1.02	1.05	0.81	0.83	0.93	1.01	0.97	0.86	0.97	0.92
Massachusetts	0.74	0.82	0.84	0.92	0.94	0.90	0.95	0.93	0.96	0.96	0.82	0.89	0.89	0.94	0.95	0.85	0.94	0.90
Michigan	0.82	0.80	0.87	0.84	0.85	0.79	0.83	0.89	0.92	0.88	0.80	0.81	0.88	0.88	0.86	0.84	0.87	0.85
Minnesota	0.78	0.88	0.90	0.91	0.97	0.74	0.91	0.98	0.96	0.99	0.76	0.90	0.94	0.93	0.98	0.89	0.93	0.91
Mississippi	0.77	0.87	0.79	0.87	0.91	0.78	0.80	0.84	0.95	0.98	0.77	0.84	0.82	0.91	0.95	0.84	0.87	0.85
New Jersey	0.94	0.83	0.95	0.87	0.91	0.81	0.98	0.99	0.97	1.01	0.88	0.91	0.97	0.92	0.96	0.90	0.97	0.93
New York	0.84	0.86	0.96	1.00	1.07	0.92	0.97	1.04	1.06	1.08	0.88	0.91	1.00	1.03	1.07	0.94	1.02	0.98
Texas	0.76	1.00	0.91	0.91	0.81	0.77	1.01	0.89	0.92	0.85	0.77	1.01	0.90	0.91	0.83	0.90	0.90	0.90
Washington	0.75	0.82	0.90	0.92	0.88	0.74	0.93	0.91	0.97	0.93	0.74	0.88	0.91	0.94	0.91	0.86	0.91	0.88
Milwaukee	0.86	0.79	0.81	1.02	0.81	0.68	0.87	0.91	1.01	0.74	0.77	0.83	0.86	1.01	0.77	0.85	0.86	0.85
Balance of Wisconsin	0.74	0.90	0.85	0.93	1.00	0.69	0.96	0.89	0.95	1.07	0.71	0.93	0.87	0.94	1.04	0.88	0.91	0.90
Balance of U.S.	0.78	0.82	0.86	0.93	0.90	0.75	0.93	1.01	0.92	0.86	0.77	0.88	0.93	0.92	0.88	0.86	0.91	0.89
U.S.–Weighted	0.78	0.83	0.88	0.93	0.90	0.79	0.93	0.98	0.94	0.92	0.78	0.88	0.93	0.94	0.91	0.87	0.92	0.90

Table 3-4.
Overall Coverage Ratios for Adults Under 65 Treating the Area Frame Truncation as Undercoverage, by Race/Ethnicity and Study Area

Study Area	Hispanics	Blacks	Non-Hispanic, non-black	All
Alabama	1.18	0.64	0.85	0.80
California	0.80	0.58	0.89	0.84
Colorado	0.83	0.63	0.89	0.87
Florida	0.73	0.61	0.91	0.84
Massachusetts	0.82	0.72	0.90	0.88
Michigan	0.83	0.61	0.85	0.82
Minnesota	1.00	0.65	0.90	0.89
Mississippi	1.75	0.64	0.85	0.78
New Jersey	0.80	0.63	0.93	0.87
New York	0.80	0.67	0.98	0.91
Texas	0.70	0.72	0.89	0.82
Washington	1.00	0.56	0.88	0.88
Milwaukee	0.90	0.61	0.85	0.80
Balance of Wisconsin	1.05	0.69	0.90	0.90
Balance of U.S.	0.82	0.68	0.88	0.85
U.S.-Weighted	0.78	0.66	0.89	0.85

3.3 Components of Undercoverage

This section considers the two components of undercoverage. Generally speaking, undercoverage can occur either because an entire household is missed or because some individuals in an enumerated household are missed. We first consider undercoverage at the household level. The separation of undercoverage by components is dependent on there being good controls of both households and persons, on nonresponse adjustment not affecting coverage ratios, and on household coverage adjustments not distorting estimates of within-household undercoverage. Because the conditions are difficult to meet, analysis of coverage components is problematic and definitive conclusions are rarely possible.

Household Coverage

In the preceding section, we compared coverage ratios in which the area frame truncation was considered as undercoverage (table 3-4) to ratios in which the area frame truncation was not considered as undercoverage (table 3-2). The coverage ratios were very similar. As a result, we do not discuss this issue further here.

Table 3-5 gives estimates of household coverage, based on the household poststratification adjustments without the person-level poststratification adjustments.[15] Overall national coverage is

[15] In weighting, an adjustment for multiple telephones in a household was not made until after the household poststratification adjustments. To properly reflect the effect of multiple telephones on the level of household coverage, we adjusted for multiple telephones (a single factor per study area) rather than base the coverage ratios directly on the poststratification adjustments.

.86. Coverage is worse for households without children (.80 nationally) than for households with children (.94 nationally). Coverage is fairly consistent among study areas and does not show any obvious patterns.

Table 3-5.
Household Coverage Ratios, by Study Area

Study Area	Households with Children	Households without Children	All Households
Alabama	0.88	0.85	0.86
California	0.98	0.79	0.88
Colorado	0.95	0.82	0.87
Florida	0.92	0.81	0.85
Massachusetts	0.98	0.83	0.89
Michigan	0.88	0.79	0.83
Minnesota	0.95	0.88	0.91
Mississippi	0.81	0.86	0.84
New Jersey	1.02	0.79	0.88
New York	1.02	0.87	0.93
Texas	0.87	0.79	0.83
Washington	0.95	0.84	0.88
Milwaukee	0.86	0.81	0.83
Balance of Wisconsin	0.91	0.86	0.88
Balance of U.S.	0.95	0.78	0.85
U.S.–Weighted	0.94	0.80	0.86

The coverage ratios are lower than one might expect for a survey that is primarily RDD, since the vast majority of all households have telephones and are thus covered by the RDD sample. However, some domains with lower coverage also have a smaller percentage with telephones. Table 3-6 shows percentages of families without telephones by family income. About 25 percent of the poorest families, those with incomes under $5,000 in 1994, are without telephones.

Table 3-6.
Percent of Families without Telephones, by 1994 Income

	Family Income						
	<$5,000	$5,000-$14,999	$15,000-$34,999	$35,000-$49,000	$50,000-$74,000	>$75,000	Total
Percent without telephones	25.4%	12.6%	4.4%	1.6%	1.0%	1.0%	6.2%
Percent of all families by income	5.9%	21.8%	32.5%	15.8%	14.0%	10.1%	100%

Note: Data provided by Lee Giesbrecht.
Source: March 1995 Current Population Survey. This data is a correction to data appearing in table 2 of Giesbrecht et al. (1996).

One major reason for the low coverage ratios for households appears to be misclassification of households by household composition. Table 3-7 shows weighted national coverage ratios for the RDD sample by whether a household has only persons 65 or over, has children, or has no children and has persons under 65. These coverage ratios were computed using separate control totals for telephone and nontelephone households from the CPS, rather than the actual totals used in the poststratification. The table indicates a pronounced tendency for households without children to be classified incorrectly as having no persons under 65.

Table 3-7.
Coverage Ratio–Weighted National Average for RDD Sample, by Type of Household

With Children	No Children, Under 65	Only 65+	Total
1.05	0.88	1.25	1.00

One explanation for the apparent overcoverage of households with only persons 65 or over is respondent or interviewer error. A second explanation has to do with differential nonresponse. It is likely that households with only elderly persons had higher response rates than other households, since they only had to answer a few questions to become respondents. If this is true, the nonresponse adjustments would then overadjust households with only elderly persons and underadjust other households. These adjustments were not applied separately by household composition because it was not known for all telephone numbers. Thus, the household nonresponse for this domain is really higher than indicated by the nonresponse rates, and coverage rates are better than shown by the ratios. In other words, the nonresponse rate and coverage ratio are confounded.

Within-Household Coverage

Within-household coverage of adults is now examined. Table 3-8 provides within-household coverage ratios by study area, sex, and race/ethnicity. The coverage ratios are essentially the inverse of adult poststratification factors. They indicate the additional undercoverage after household undercoverage, and all types of nonresponse are taken into account.[16] The national within-household coverage ratio was 1.00. This is unexpected because research on the CPS and other surveys would lead one to expect some within-household undercoverage. For example, Shapiro et al. (1993) estimated that more than one-half the undercoverage in CPS was within household undercoverage.

The patterns of coverage are as expected: Blacks have poorer coverage than non-Hispanic non-blacks and males have poorer coverage than females. However, this pattern results in some groups having apparent within-household overcoverage. For example, the coverage ratio for non-Hispanic, non-black females is 1.08.

One possible explanation for these surprising results is that nonresponse is higher for households with few adults and children, and the application of nonresponse factors and household poststratification causes overadjustment of persons. However, the NSAF mean household size is nearly identical to the CPS mean household size (2.84 persons versus 2.83 persons), so this explanation is not supported.

[16] All types of nonresponse are included in order to avoid underestimation of coverage ratios, since the adjustment for multiple telephones was done after the application of the household poststratification factors. The table 3-8 coverage ratios are based on a special tabulation in which the multiple-telephone adjustment was not performed.

A second possible explanation is that the household control counts used in the household poststratification adjustment are too large. Census Bureau estimates of persons are generally better than those of households, so the household estimates could be in error by a few percentage points. Since the household coverage ratios presented in table 3-5 indicate higher levels of household undercoverage than one would expect, it is plausible that the household control estimates are too large. However, we have no specific evidence for this assertion.[17]

A third possible explanation for the overcoverage of non-Hispanic, non-black females is that nonminority households, especially those with a preponderance of females, experience lower nonresponse rates than other households. If this were the case, the household nonresponse adjustments would tend to overadjust such households and underadjust other households, resulting in differential coverage ratios, as shown in table 3-8. Report No. 7 in this methodological series examines nonresponse rates in several different ways. In one analysis, blacks appear to have relatively high nonresponse rates (implying low nonresponse rates for non-Hispanic, non-black households), but in another analysis both blacks and Hispanics appear to have relatively low nonresponse rates. In summary, it is possible that there is some differential nonresponse that would tend to result in coverage ratios over 1.0 for non-Hispanic, non-black females, but it is very unlikely that this is the full or main explanation.

As a further check of the premise that there is little or no within-household undercoverage and that there is overcoverage of non-Hispanic, non-black females, we examined two sets of correlations among the study areas. The correlations indicate that the coverage ratios in table 3-7 do not reflect actual within-household coverage.[18] This suggests that there is some within-household undercoverage not revealed by the ratios. This also means that there is not substantial overcoverage of non-Hispanic, non-black persons.

Table 3-9 shows coverage ratios by age group for non-Hispanic, non-black adults. Again, the pattern is consistent with that of CPS and other surveys, with younger groups generally having poorer coverage than older groups. The disparity between young and old, however, is somewhat greater than for the CPS. Further, the indicated national overcoverage is quite extreme for some age-sex groups, with coverage ratios as high as 1.16.

In summary, tables 3-8 and 3-9 imply that overall there is no within-household undercoverage, and that there is overcoverage for non-Hispanic, non-black females and for some age groups of non-Hispanic, non-black males. However, there are several confounding factors that make it likely that there is some within-household undercoverage. In particular, the household controls used in the household poststratification might be too large. As a result, one cannot accurately estimate the relative importance of whole-household and within-household undercoverage.

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[17] The direct CPS estimate of households is several percentage points *higher* than the control totals we used. This does not support the hypothesis that the control totals are too large, but shows the household controls are subject to variation.

[18] If the coverage ratios were correct, then study areas with high proportions of minorities should have lower coverage. Computed correlations between the overall coverage ratio and proportion nonminority indicate this is not true. Further, the coverage ratio for non-Hispanic, non-black persons would not be correlated with the proportion of non-Hispanic, non-black in the study area. However, the correlation between these two items is –0.66.

Table 3-10 gives coverage ratios for adults by tenure and education.[19] The patterns in table 3-10 are consistent with what would be expected from the coverage ratios in the earlier tables and in the CPS: People in renter-occupied units have poorer coverage than persons in owner-occupied units, and people with less educational attainment have relatively poor coverage.

[19] Unlike the coverage ratios by race/ethnicity, sex, and age, we did not do a special tabulation in which the multiple telephone adjustment was excluded. Instead, we simply made a crude adjustment so that the table 5-10 coverage ratios would be comparable in magnitude to those of tables 3-8 and 3-9.

Table 3-8.
Within-Household Coverage Ratios for Adults, by Race/Ethnicity, Sex, and Study Area

Study Area	Hispanics			Blacks			Non-Hispanics			All Persons		
	Males	Females	Total	Males	Females	Total	Males	Females	Total	Males	Females	Total
Alabama	N/A	N/A	1.39	0.70	0.83	0.77	1.00	1.00	1.00	N/A	N/A	0.94
California	0.81	0.90	0.85	0.55	0.81	0.69	1.01	1.11	1.06	0.92	1.03	0.97
Colorado	0.94	0.94	0.94	0.63	0.92	0.78	1.04	1.04	1.04	1.01	1.02	1.02
Florida	0.83	0.88	0.85	0.63	0.79	0.71	1.02	1.14	1.08	0.93	1.04	0.99
Massachusetts	0.77	1.02	0.90	0.76	0.92	0.85	0.95	1.07	1.01	0.93	1.06	1.00
Michigan	1.14	0.77	0.96	0.66	0.86	0.77	1.01	1.04	1.03	0.97	1.01	0.99
Minnesota	1.39	0.96	1.19	0.67	0.85	0.76	0.98	1.01	1.00	0.98	1.01	0.99
Mississippi	2.01	2.45	2.23	0.76	0.84	0.80	1.01	1.05	1.03	0.93	0.98	0.96
New Jersey	0.84	0.85	0.85	0.67	0.73	0.71	1.04	1.13	1.09	0.97	1.04	1.01
New York	0.73	0.90	0.82	0.74	0.69	0.71	1.03	1.10	1.06	0.95	1.00	0.98
Texas	0.75	0.93	0.83	0.81	0.99	0.91	1.11	1.11	1.11	0.97	1.05	1.01
Washington	1.16	1.16	1.16	0.58	0.71	0.64	0.98	1.03	1.00	0.98	1.02	1.00
Milwaukee	0.89	1.24	1.07	0.76	0.73	0.74	1.04	1.04	1.04	0.97	0.98	0.97
Balance of Wisconsin	1.23	1.05	1.14	0.96	0.70	0.82	1.00	1.03	1.02	1.00	1.03	1.02
Balance of U.S.	0.90	0.93	0.91	0.85	0.81	0.82	1.02	1.07	1.04	0.99	1.03	1.01
U.S.–Weighted	0.82	0.92	0.87	0.77	0.81	0.79	1.02	1.08	1.05	0.97	1.03	1.00

N/A – not available.

Table 3-9.
Within-Household Coverage Ratios for Non-Hispanic, Non-Black Adults, by Age, Sex, and Study Area

	Males					Females					Both Sexes					All
Study Area	18-24	25-34	35-44	45-54	55+	18-24	25-34	35-44	45-54	55+	18-24	25-34	35-44	45-54	55+	Persons
Alabama	0.89	0.84	1.02	1.19	1.02	0.79	0.91	1.07	1.09	1.10	0.84	0.87	1.04	1.14	1.06	1.00
California	0.87	0.96	1.03	1.14	1.03	1.09	1.03	1.06	1.16	1.32	0.98	0.99	1.04	1.15	1.18	1.06
Colorado	0.91	1.10	1.09	1.07	0.95	0.90	1.04	1.11	1.02	1.06	0.91	1.07	1.10	1.04	1.01	1.04
Florida	0.84	0.85	1.06	1.23	1.05	1.10	0.99	1.09	1.27	1.25	0.97	0.92	1.07	1.25	1.16	1.08
Massachusetts	0.84	0.91	0.89	1.07	1.09	1.10	1.03	1.02	1.14	1.10	0.97	0.97	0.96	1.11	1.10	1.01
Michigan	1.00	0.95	1.02	1.06	1.07	0.96	0.95	1.05	1.16	1.10	0.98	0.95	1.03	1.11	1.08	1.03
Minnesota	0.86	0.95	0.97	1.03	1.10	0.81	0.96	1.05	1.09	1.11	0.83	0.96	1.01	1.06	1.11	1.00
Mississippi	0.90	1.02	0.97	1.07	1.08	0.94	0.95	1.05	1.14	1.14	0.92	0.99	1.01	1.10	1.11	1.03
New Jersey	1.20	0.91	1.00	1.07	1.15	1.13	1.03	1.07	1.24	1.26	1.16	0.97	1.04	1.15	1.21	1.09
New York	0.95	0.91	1.00	1.12	1.21	1.07	1.01	1.09	1.18	1.18	1.01	0.96	1.05	1.15	1.19	1.06
Texas	1.00	1.17	1.10	1.17	1.03	1.02	1.18	1.08	1.17	1.06	1.01	1.17	1.09	1.17	1.04	1.11
Washington	0.86	0.91	1.00	1.08	1.03	0.86	1.02	1.01	1.13	1.09	0.86	0.96	1.01	1.11	1.06	1.00
Milwaukee	1.05	0.95	0.97	1.27	1.00	0.82	1.03	1.09	1.26	0.90	0.93	0.99	1.03	1.26	0.94	1.04
Balance of Wisconsin	0.86	1.00	0.95	1.07	1.16	0.81	1.06	1.00	1.10	1.22	0.83	1.03	0.97	1.08	1.19	1.02
Balance of U.S.	0.98	0.92	0.96	1.14	1.14	0.90	1.03	1.13	1.15	1.08	0.94	0.98	1.05	1.14	1.11	1.04
U.S.–Weighted	0.95	0.94	0.99	1.13	1.11	0.95	1.03	1.10	1.16	1.13	0.95	0.98	1.04	1.14	1.12	1.05

Table 3-10.
Overall Coverage Ratios for Adults by Tenure and Education

Study Area	Tenure		Education of Most Knowledgeable Adult			Tenure by Education						All Persons
						Renter			Owner			
	Renter	Owner	Less Than High School	High School Graduate	Some College	Less Than High School	High School Graduate	Some College	Less Than High School	High School Graduate	Some College	
California	0.92	1.02	0.78	0.96	1.16	0.72	0.92	1.23	0.91	0.99	1.12	0.97
Florida	0.81	1.08	0.67	1.04	1.09	0.62	0.87	0.85	0.71	1.12	1.18	0.99
New York	0.90	1.03	0.56	0.95	1.27	0.53	0.92	1.32	0.66	0.97	1.24	0.98
Texas	0.93	1.05	0.72	1.00	1.30	0.71	0.91	1.33	0.73	1.05	1.29	1.01
Remaining of U.S.	0.89	1.05	0.78	0.99	1.17	0.77	0.87	1.09	0.79	1.04	1.19	1.01
U.S.–Weighted	0.90	1.05	0.75	0.99	1.18	0.72	0.88	1.14	0.79	1.04	1.19	1.00

Table 3-11.
Coverage Ratios for NSAF and CPS, by Age, Race/Ethnicity, and Sex

Survey	Children[1]				Adults[2]									Total
					Hispanic			Black			Other[3]			
	Hispanic	Black	Other[3]	Total	Male	Female	Total	Male	Female	Total	Male	Female	Total	
NSAF	0.95	0.70	0.99	0.94	0.75	0.85	0.80	0.65	0.69	0.67	0.87	0.92	0.90	0.86
CPS	0.85	0.86	0.96	0.94	0.77	0.89	0.84	0.78	0.88	0.83	0.92	0.96	0.94	0.93

[1]For NSAF under age 18; for CPS under age 15.
[2] For NSA,F age 18-64; for CPS, age 16 and over.
[3] NSAF figures for non-Hispanic, non-black, CPS figures for white.
Source of CPS data: For children, unpublished data for December 1996; for adults, unpublished averages for January–April 1996.

3.4 Comparison with Other Surveys

Inevitably there are differences among surveys in methodologies, estimation, and how coverage ratios are computed. As a result, it is difficult to draw meaningful conclusions when comparing coverage ratios between surveys. We primarily compare the NSAF coverage ratios to those for the CPS, since coverage ratios are not routinely computed and published. Maklan and Waksberg (1988) examined coverage ratios for RDD surveys, but they only looked at surveys that covered less than the whole nation. Chu et al. (1999) also compare some of the coverage rates in the NSAF to other surveys.

Table 3-11 compares overall coverage between the NSAF and the CPS. As noted in the table footnotes, age groups and race/ethnicity definitions are not identical for the two surveys, but are sufficiently similar to allow us to draw conclusions. The NSAF ratios in the table are the weighted national averages given in tables 3-1 and 3-2 and do not reflect undercoverage due to truncation of the area frame to BGs with high nontelephone rates.

There are a number of important differences between the two surveys that affect coverage. The CPS is a monthly survey, conducted by the Census Bureau in which most of the sample is drawn from decennial census addresses, supplemented by a sample of new construction permits, whereas the 1997 NSAF sample consists mainly of an RDD telephone sample. In general, one might expect better coverage of households in the NSAF because RDD has better coverage of households with telephones than can be obtained using 1990 Census addresses. On the other hand, initial contacts in the CPS are made in person, rather than by telephone, as is the case for most of the NSAF. One possibility is that people are more reluctant to provide complete rosters of household members on a cold telephone contact, leading to poorer within-household coverage in telephone surveys. In summary, there are a number of major differences between the two surveys that can be expected to result in different coverage ratios between the two surveys. As a result, it is difficult to predict which survey should have better coverage.

Table 3-11 indicates that NSAF has about the same coverage of children overall (.94) as the CPS. Overall coverage of adults is somewhat worse (.86 versus .93). As discussed earlier, some misclassification of households with adults under 65 and no children might account for some of this difference. The magnitude of this misclassification could account for the lower NSAF coverage of adults and would explain why the NSAF coverage of children, but not of adults, is comparable to the CPS.

In examining population subgroups, the NSAF has worse coverage than the CPS for blacks (both children and adults), especially for black children (.70 versus .86) and black female adults (.69 versus .88). There is no obvious explanation for the poorer coverage of blacks. The NSAF has better coverage of Hispanic children and about the same coverage of Hispanic adults as the CPS. The superior Hispanic coverage may be due to inconsistencies in self-identification of Hispanic/non-Hispanic between the NSAF and the CPS. In the Health and Nutrition Examination Survey III conducted by Westat from 1988 to 1994, results were similar, with better coverage of Hispanic children and adults than of white/other children and adults (Montaquila et al., 1996).

A comparison of coverage ratios for the NSAF with those of a large recent RDD survey, the National Immunization Survey (NIS), is also possible. Coverage ratios for the NIS are only available children 19 to 35 months old. The coverage ratio in 1994 NIS was only .72, which is much lower than that in the NSAF (Shapiro et al., 1996).

3.5 Summary

Overall national coverage ratios in the NSAF are .94 for children and .86 for adults. Coverage in CPS is about the same for children and somewhat better for adults. The poorer adult coverage in the NSAF may be largely due to incorrect classification of some childless households as having only persons 65 and over. Coverage ratios for blacks are noticeably lower (.70 for children and .67 for adults) than for Hispanics and for non-Hispanic non-blacks, and are also lower than for blacks in the CPS. The truncation of the area frame accounts for little undercoverage.

Examining the components of undercoverage, it appears that the undercoverage in the NSAF is primarily whole-household undercoverage rather than within-household undercoverage. However, no firm conclusion about the magnitude of the two components is possible.

Chapter 4
Comparisons with *Snapshots* Statistics

As mentioned in chapter 1, when the original *Snapshots* publication was under preparation, there were concerns about what difference using undercount-adjusted results would make. Chapter 4 presents the impacts found. We also took the opportunity in this chapter to look at the effect of two other 1997 NSAF issues: the impact of data cleaning done the year after the *Snapshots* tables were produced, and what would have happened had we decided to conduct only an RDD survey.

The chapter consists of four brief sections with an extensive set of tabular material at the end. The first section discusses the effect of the additional data editing done after the publication of *Snapshots.* Section 4.2 goes over the general findings of our examination of the impact of the undercount adjustments made (as described in chapter 2). In the third section (section 4.3), we look at something we did not do–namely, use only an RDD approach in the NSAF. In the final section (section 4.4), we provide a unified national perspective and some advice to outside researchers who have concerns about their own analyses.

4.1 Editing Impacts on *Snapshots*

Over the year since the *Snapshots* were prepared, the 1997 survey files were changed as more was learned. We wanted to let researchers see what impact this made on the original results. The data changes, as we conjectured, have proved to be mostly of minor importance.

This is not to say that individual researchers do not have to beware of further data problems, because some undoubtedly exist on the public use files being released. Still we are a relatively comfortable with work done on the 1997 NSAF. It should be a sound baseline for comparison with the 1999 survey results coming out this year.

We do not discuss each of the 20 tables from the *Snapshots* report separately. Rather, we have taken just four of these as illustrative–one from each of the table sets A, B, C, and D. Each figure is shown as a histogram of differences, with seven classes centered at zero and defined by the intervals

–1.75 to –1.25	+0.25 to +0.75
–1.25 to -0.75	+0.75 to +1.25
–0.75 to –0.25	+1.25 to +1.75
–0.25 to +0.25	

State comparisons are being made for each of the 13 target NSAF states. Overall national comparisons are taken up mainly in section 4.4. Thus, for all figures there are only 13 comparisons being made, as is noted beside each figure. Also, beside every figure are the simple arithmetic means of the 13 state differences, not weighted by sample or population size, plus the comparable standard deviation (also calculated on this basis). The smooth bell-shaped curve, shown for contrast to the raw histogram, was calculated by treating the data as normally distributed with the mean and standard deviation shown.

One of the biggest sources of editing changes was the redetermination of family status, a very complex idea in the NSAF (See report no. 10 in this series). Even so, these changes had only minor impacts, as will be seen.

4.1.1 Editing Differences, A-2: Children Below the Poverty Level

The figures below show the distribution of differences between the preliminary data and the final edited data for *Snapshot* table A-2: "Children Below the Poverty Level". The first shows the distribution for data from two-parent families, and the second for that of all families.

Figure 4.1.1.1
Differences for Two-Parent Families, by State
(Preliminary Minus as Finally Edited)

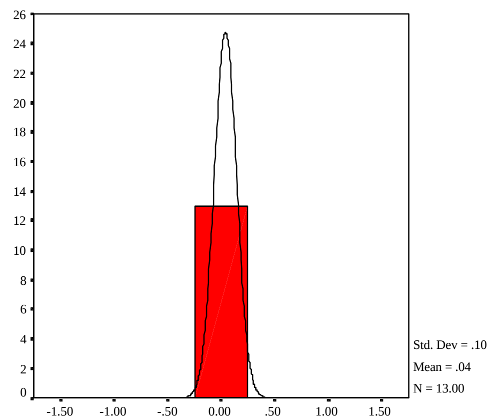
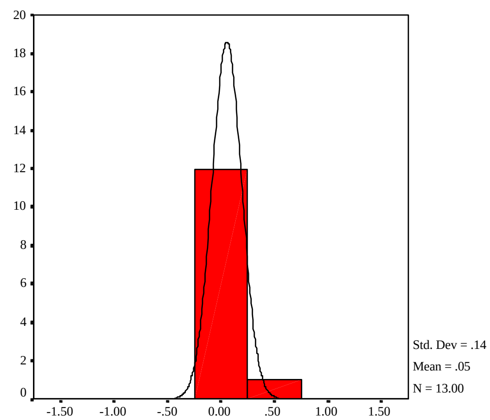


Figure 4.1.1.2
Differences for All Families, by State
(Preliminary Minus as Finally Edited)



These figures show that the 13 differences between the preliminary data and the final edited data are approximately symmetric, with a mean close to zero. As such, these differences do not affect the original conclusions drawn about poverty among children.

In both the preliminary data and the final edited data, child poverty rates exceeded the national average in five states (Alabama, California, Mississippi, New York, and Texas). Similarly, child poverty rates for both analyses were below the national average in seven states (Colorado, Massachusetts, Michigan, Minnesota, New Jersey, Washington, and Wisconsin). Further, both the preliminary data and the finally edited data illustrate that children living with one parent were much more likely to be poor than children living with two parents.

Finally, both versions of the data demonstrate the dramatic range of poverty rates in surveyed states, with one in ten children in Wisconsin living in poverty, compared to one in three children in Mississippi.

4.1.2 Editing Differences, B-3: Confidence in the Ability to Get Children Medical Care

The figures below show the distribution of differences between the preliminary data and the final edited data for *Snapshot* table B-3: "Confidence in the Ability to Get Medical Care". The first shows the distribution for data from families under 200 percent of the poverty level, the second for that of families above 200 percent of the poverty level, and the third for that of all families.

These figures show that the differences between the preliminary data and the finally edited data are approximately symmetric, with a mean close to zero. As such, these differences do not affect the original conclusions drawn about confidence in getting medical care.

Figure 4.1.2.1
Differences for Families Under 200percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)

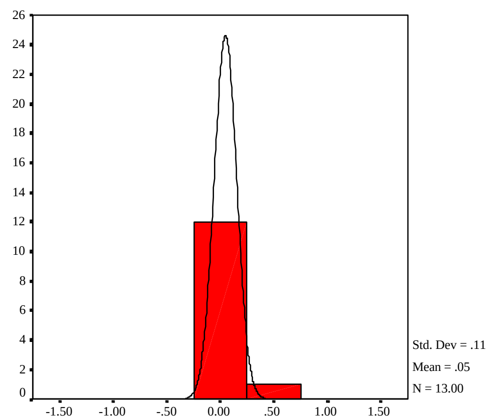
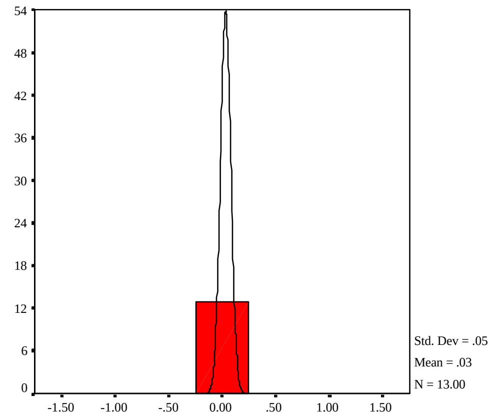


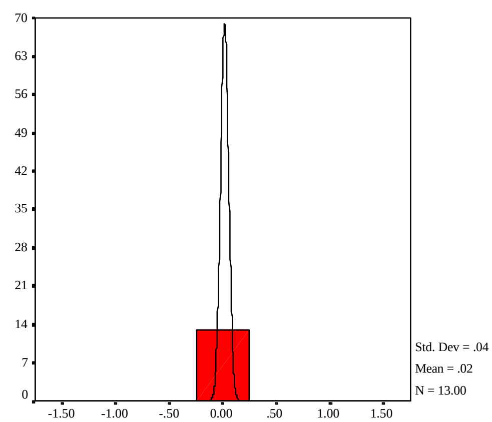
Figure 4.1.2.2
Differences for Families Above 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)



In both the preliminary data and the final edited data, there was a considerable difference in confidence between families with low incomes (below 200 percent of the federal poverty level) and those with higher incomes. Only 4 percent of children in higher-income families had parents who were not confident of their ability to obtain needed medical care, compared to 14 percent of children in low-income families.

Finally, both versions of the data demonstrate that while there was little variation in confidence among higher-income families, confidence in low-income families varied considerably more. In six states, low-income parents were less likely than the national average to lack confidence in their ability to get children medical care.

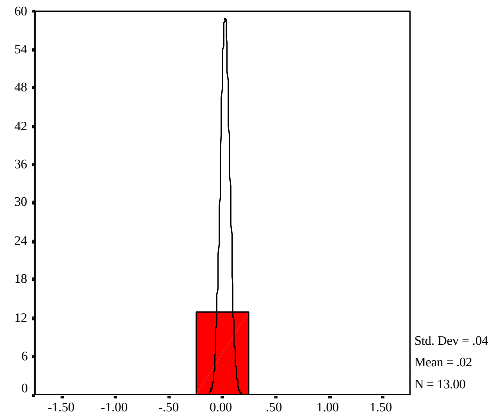
Figure 4.1.2.3
Differences for All Families, by State
(Preliminary Minus as Finally Edited)



4.1.3 Editing Differences, C-4: Reading and Telling Stories to Young Children

The figures below show the distribution of differences between the preliminary data and the finally edited data for *Snapshot* table C-4: "Reading and Telling Stories to Young Children." The first shows the distribution for data from families under 200 percent of the poverty level, the second for that of families above 200 percent of poverty, and the third for that of all families.

Figure 4.1.3.1
Differences for Families Below 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)



These figures show that the differences between the preliminary data and the final edited data are approximately symmetric with a mean close to zero. As such, these differences do not affect the original conclusions drawn about literacy activities.

Figure 4.1.3.2
Differences for Families Above 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)

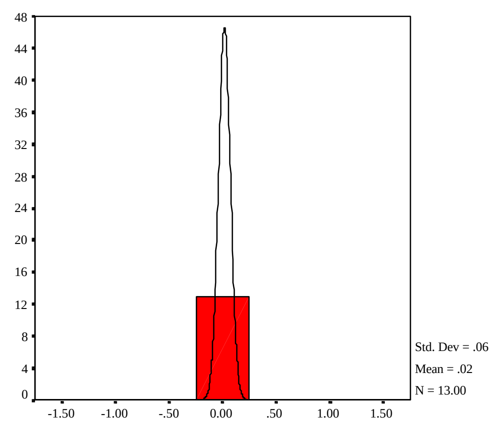
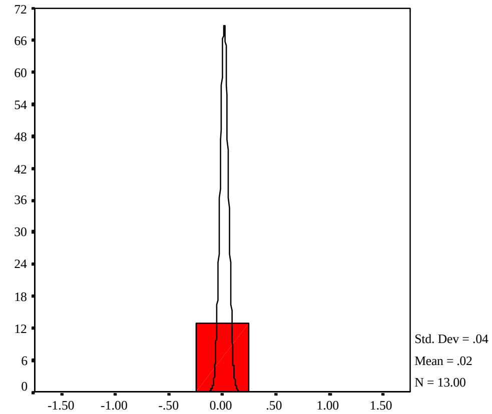


Figure 4.1.3.3
Differences for All Families, by State
(Preliminary Minus as Finally Edited)



Both the preliminary data and the finally edited data, show that, nationally, 17 percent of all children ages 1 to 5 were read to or told stories fewer than three days per week. Children in low-income families (below 200 percent of the poverty level) were more than twice as likely as other children to fall into this risk category (24 percent versus 10 percent).

Finally, both versions of the data demonstrate that among low-income children in the surveyed states, 16 percent to 33 percent were read to or told stories fewer than three times a week.

4.1.4 Editing Differences, D-2: Parental Aggravation

The figures below show the distribution of differences between the preliminary data and the finally edited data for *Snapshot* table D-2: "Parental Aggravation." The first shows the distribution for data from parents with a spouse, the second for that of parents without a spouse, and the third for that of all families.

Figure 4.1.4.1
Differences for Parents with a Spouse, by State
(Preliminary Minus as Finally Edited)

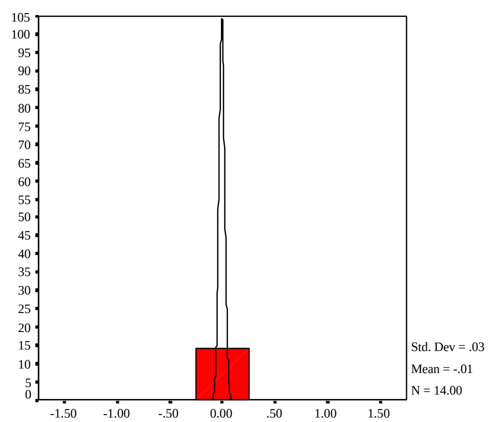
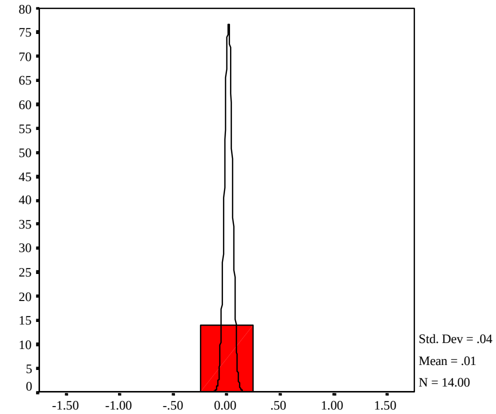
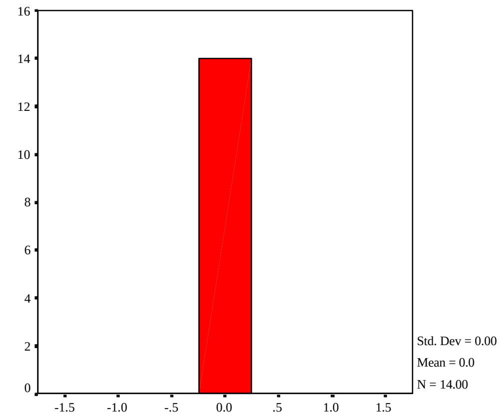


Figure 4.1.4.2
Differences for Parents without a Spouse, by State
(Preliminary Minus as Finally Edited)



From these figures it appears that the differences between the preliminary data and the final edited data are approximately symmetric, with a mean close to zero. As such, they are unlikely to affect the original conclusions drawn about parental aggravation, and this turns out to be the case.

Figure 4.1.4.3
Differences for All Parents, by State
(Preliminary Minus as Finally Edited)



Both the preliminary data and the finally edited data, show that, in the 13 states surveyed, 6 percent to 14 percent of children lived with a highly aggravated parent. In six of the states, the percentage was above the national average: Alabama, Florida, Mississippi, New Jersey, New York, and Texas.

Finally, both versions of the data demonstrate that in low-income families, 9 percent to 21 percent of children lived with a highly aggravated parent. In five states, that percentage was higher than the national average: Florida, Massachusetts, Mississippi, New Jersey, and New York. In Colorado and Washington, it was lower.

4.2 Undercount-Adjustment Impacts on *Snapshots*

In this section, a selection of tables from the January 1999 *Snapshots* have been compared, both as finally edited and after adjusting for the census undercount, as described in chapter 2. Graphical comparisons of the sort done for section 4.1 are made. These confirm, it might be added, that the conjecture of only minor differences was basically correct. There is one significant exception: We found that the percentage of the population in poverty was increased generally (this effect, while pervasive, was not great). For more on this, see section 4.4.

We do not discuss each of the 20 tables from the *Snapshots* report separately, although they are available at the end of this chapter. Instead, we have taken just four of these as illustrative–one from each of the table sets A, B, C, and D. These are the same tables we have already looked at for editing differences. Each figure, as earlier, is shown as a histogram of differences, with seven classes centered at zero and defined by the intervals

–1.75 to –1.25	+0.25 to +0.75
–1.25 to -0.75	+0.75 to +1.25
–0.75 to –0.25	+1.25 to +1.75
–0.25 to +0.25	

State comparisons are made for each of the 13 target NSAF states. Overall national comparisons are taken up mainly in section 4.4. Thus, for all figures there are only 13 comparisons being made, as is noted beside each figure. Again, beside every figure are the simple arithmetic means of the 13 state differences, not weighted by sample or population size, plus the comparable standard deviation (also calculated on this basis). The smooth bell-shaped curve, shown for contrast to the raw histogram, was calculated by treating the data as normally distributed with the mean and standard deviation shown.

4.2.1 Undercount Differences, A-2: Children Below the Poverty Level

The figures below show the distribution of differences between the census-level data and the undercount-adjusted data for *Snapshot* table A-2: “Children Below the Poverty Level.” The first shows the distribution for data from one-parent families, the second for that of two-parent families, and the third for that of all families.

These figures show that the differences between the census-level data and the undercount-adjusted data are somewhat asymmetric. This is unlike the editing differences examined, in the previous section. Also unlike the editing differences, here the mean difference is not as close to zero, being slightly negative. This slight negative mean difference reflects, as already noted, that the undercount adjustment (as expected) has increased the percentage of children in poverty. Even so, these differences do not affect the original conclusions drawn about poverty among children, particularly in terms of the state-to-state variation.

Both the census-level data and the undercount-adjusted data, show that child poverty rates exceeded the national average in five states (Alabama, California, Mississippi, New York, and

Texas), while child poverty rates were below the national average in seven states (Colorado, Massachusetts, Michigan, Minnesota, New Jersey, Washington, and Wisconsin.) Further, both the census-level data and the undercount-adjusted data illustrate that children living with one parent were much more likely to be poor than children living with two parents.

Finally, both versions of the data demonstrate the dramatic range of poverty rates in surveyed states, with one in ten children in Wisconsin living in poverty, compared to one in three children in Mississippi.

Figure 4.2.1.1
Differences for Families Below 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)

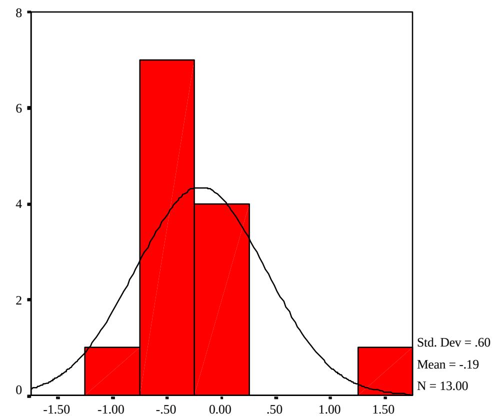


Figure 4.2.1.2
Differences for Families Above 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)

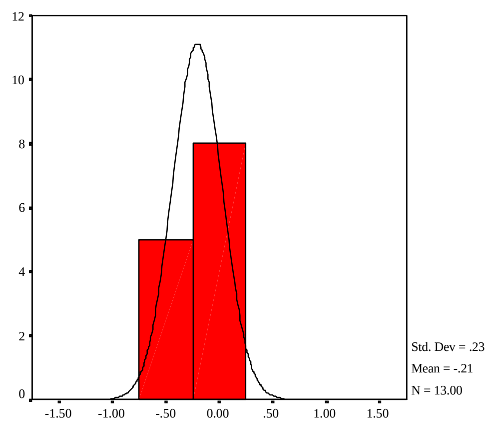
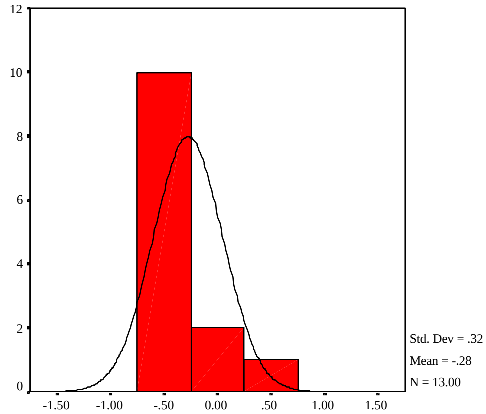


Figure 4.2.1.3
Differences for All Families, by State
(Preliminary Minus as Finally Edited)



4.2.2 Undercount Differences, B-3: Confidence in Ability to Get Children Medical Care

The figures below show the distribution of differences between the preliminary data and the undercount-adjusted data for *Snapshot* table B-3: "Confidence in the Ability to Get Children Medical Care." The first shows the distribution for data from families under 200 percent of the poverty level, the second for that of families above 200 percent of the poverty level, and the third for that of all families.

Figure 4.2.2.1
Differences for Families Below 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)

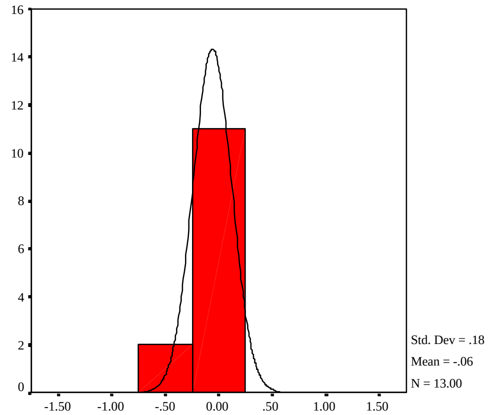


Figure 4.2.2.2
Differences for Families Above 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)

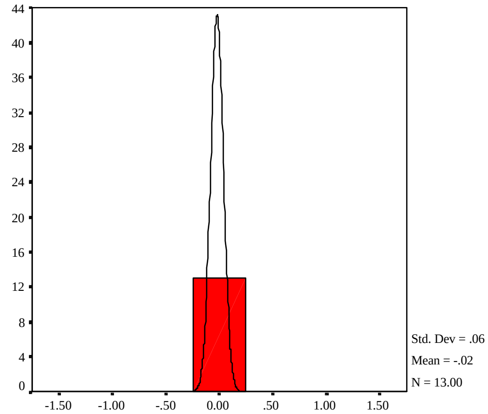
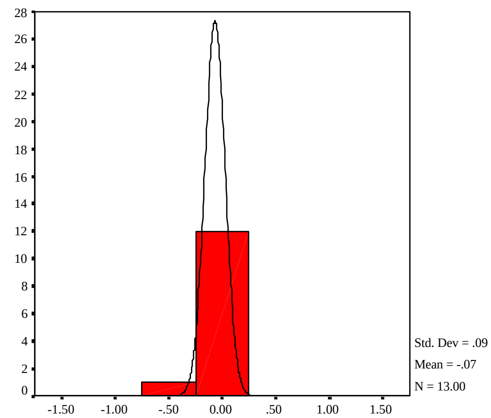


Figure 4.2.2.3
Differences for All Families, by State
(Preliminary Minus as Finally Edited)



These figures show that the differences in the preliminary data and the undercount-adjusted data are approximately symmetric with a mean close to zero (albeit, again, not as close to zero as in section 4.1 comparisons). As expected, these differences do not affect the original conclusions drawn about confidence in getting medical care.

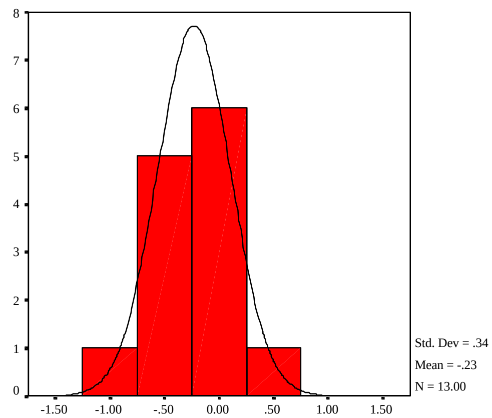
Both the preliminary data and the undercount-adjusted data show that there was a considerable difference in confidence between families with low incomes (below 200 percent of the federal poverty level) and those with higher incomes. Only 4 percent of children in higher-income families had parents who were not confident of their ability to obtain needed medical care, compared to 14 percent of children in low-income families.

Finally, both versions of the data demonstrate that while there was little variation in confidence among higher-income families, confidence in low-income families varied considerably more. In six states, low-income parents were less likely than the national average to lack confidence in their ability to get children medical care.

4.2.3 Undercount Differences, C-4: Reading and Telling Stories to Young Children

The figures below show the distribution of differences between the preliminary data and the undercount-adjusted data for *Snapshot* table C-4: “Reading and Telling Stories to Young Children.” The first shows the distribution for data from families under 200 percent of the poverty level, the second for that of families above 200 percent of poverty, and the third for that of all families.

Figure 4.2.3.1
Differences for Families Below 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)



These figures show that the differences between the preliminary data and the undercount-adjusted data are approximately symmetric, with a mean close to zero. As such, these differences do not affect the original conclusions drawn about literacy activities.

Both the preliminary data and the undercount-adjusted data, show that, nationally, 17 percent of all children age one to five were read to or told stories fewer than three days per week. Children in low-income families (below 200 percent of the poverty level) were more than twice as likely as other children to fall into this risk category (24 percent versus 10 percent).

Finally, both versions of the data demonstrate that among low-income children in the surveyed states, 16 percent to 33 percent were read to or told stories fewer than three times a week.

Figure 4.2.3.2
Differences for Families Above 200 Percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)

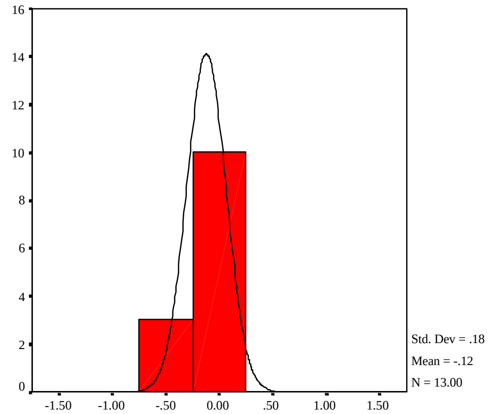
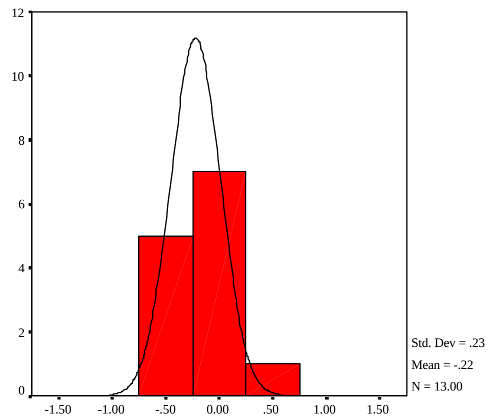


Figure 4.2.3.3
Differences for All Families, by State
(Preliminary Minus as Finally Edited)



4.2.4 Undercount Differences, D-2: Parental Aggravation

The figures below show the distribution of differences between the preliminary data and the undercount-adjusted data for *Snapshot* table D-2: "Parental Aggravation." The first shows the distribution for data from parents with a spouse, the second for that of parents without a spouse, and the third for that of all families.

These figures show that the differences between the preliminary data and the undercount-adjusted data are approximately symmetric, with a mean close to zero. As such, these differences do not affect the original conclusions drawn about parental aggravation.

Both the preliminary data and the undercount-adjusted data show that, in the 13 states surveyed, 6 percent to 14 percent of children lived with a highly aggravated parent. In six of the states, the percentage was above the national average: Alabama, Florida, Mississippi, New Jersey, New York, and Texas.

Finally, both versions of the data demonstrate that in low-income families, 9 percent to 21 percent of children lived with a highly aggravated parent. In five states, that percentage was higher than the national average: Florida, Massachusetts, Mississippi, New Jersey, and New York. In Colorado and Washington, it was lower than the national average.

Figure 4.2.4.1
Differences for Parents with a Spouse, by State
(Preliminary Minus as Finally Edited)

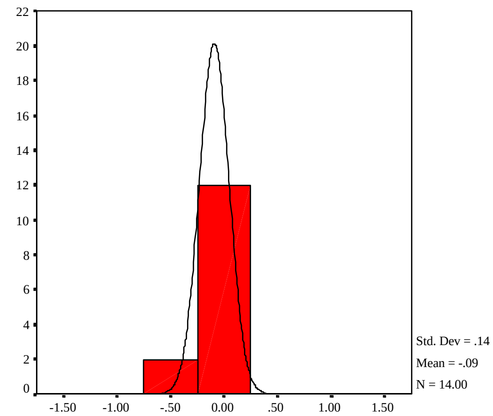


Figure 4.2.4.2
Differences for Parents without a Spouse, by State
(Preliminary Minus as Finally Edited)

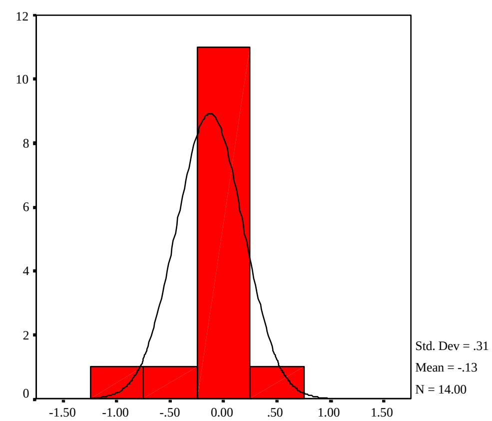
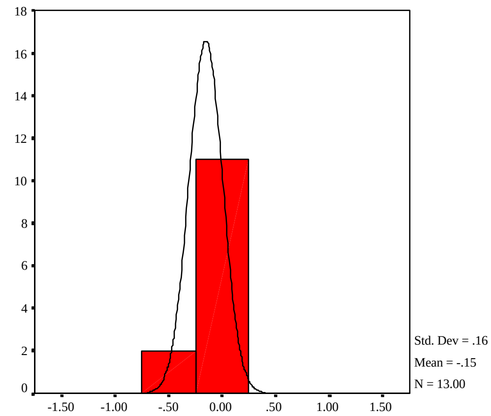


Figure 4.2.4.3
Differences for All Parents, by State
(Preliminary Minus as Finally Edited)



4.3 Hypothetical RDD Impacts on *Snapshots*

This section compares what would have happened, if we had not done a special sample of nontelephone households and had relied only on a random-digit-dialing (RDD) design. As will be seen, using only an RDD sample would have meant large differences in the statistics on the population, notably under 200 percent of poverty. An RDD-only approach would not have worked at all well for this most important group.

As earlier, we have not graphed the differences from all 20 *Snapshots* tables (they are shown in tabular form, however, at the end of this chapter.) Rather, we have taken just four of these as illustrative–one from each of the table sets A, B, C, and D. These are the same comparisons we have already made when examining editing and undercount-adjustment differences. Each figure, as earlier, is shown as a histogram of differences, with seven classes centered at zero and defined by the intervals

–1.75 to –1.25	+0.25 to +0.75
–1.25 to -0.75	+0.75 to +1.25
–0.75 to –0.25	+1.25 to +1.75

–0.25 to +0.25

State comparisons are made for each of the 13 target NSAF states. Overall national comparisons are mainly taken up next (in section 4.4). Thus, for all figures there are only 13 comparisons being made, as is noted beside each figure. Again, beside every figure are the simple arithmetic means of the 13 state differences, not weighted by sample or population size, and the comparable standard deviation (also calculated on this basis).

The smooth bell-shaped curve, shown for contrast to the raw histogram, was calculated by treating the data as normally distributed with the mean and standard deviation shown. Clearly, in many cases, the differences are not normal and large biases would have existed if we had relied solely on an RDD sample for the NSAF.

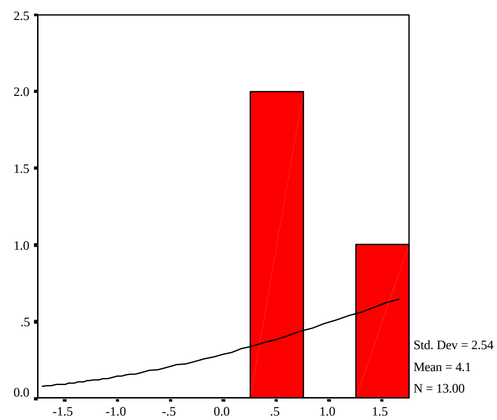
Unlike in the comparisons made earlier, the differences are too large to warrant the kind of detailed treatment presented above. For families above 200 percent of poverty, the approach may be barely workable in some cases, although even this is problematical. But for low-income families (and hence all families), it generally is not.

Instead of developing this point in detail, we present the results almost without comment. They speak for themselves. For alternatives (that might be more effective) to what was to reach nontelephone households in the 1997 and 1999 NSAF, see report no. 16.

4.3.1 RDD Comparisons A-2: Children Below the Poverty Level

First, the exclusion of nontelephone households from the sample leads to substantial understatement of child poverty rates in most of the NSAF states.

Figure 4.3.1.1
Differences for Families Below 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)

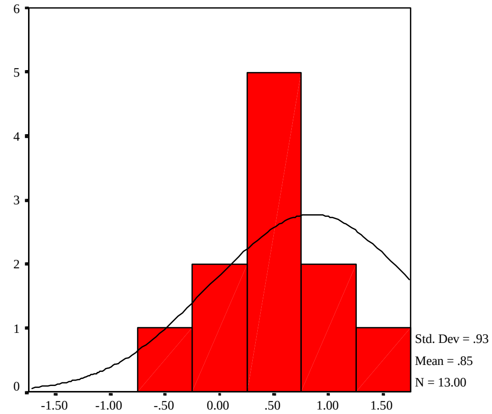


One might expect that the magnitude of the differences would vary by the percentage of nontelephone households in each state. Presumably, states with higher percentages of nontelephone households are more vulnerable to bias due to undercoverage of nontelephone households than states with lower percentages of nontelephone households. We see some evidence of this in the version of table A-2 that appears here.

States that have high percentages of nontelephone households, such as Mississippi and Alabama, show large biases in the percentage of children in families below the poverty level. Wisconsin and Massachusetts, on the other hand, have relatively low nontelephone rates, and show very small biases.

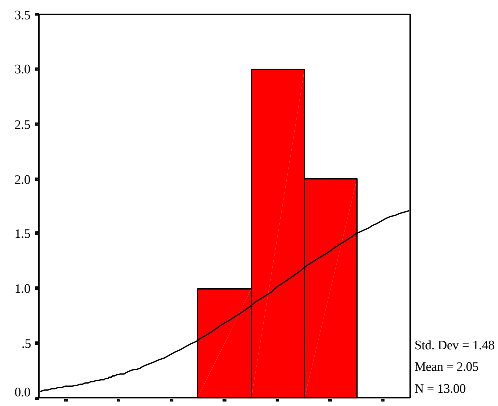
But nontelephone rates are not consistently associated with the understatement of poverty rates for children. Both California and Minnesota have low percentages of nontelephone households, yet in both states the child poverty rate is substantially underestimated when nontelephone households are excluded.

Figure 4.3.1.2
Differences for Families Above 200 percent of the Poverty Level , by State
(Preliminary Minus as Finally Edited)



Despite the underestimation of child poverty rates by state when only telephone households are included, the results of the significance tests employed in the original *Snapshot* remain unchanged. All states yield the same results of the hypothesis, that a given state estimate differs from the national estimate (using a significance level of .05 with no adjustment for multiple comparisons).

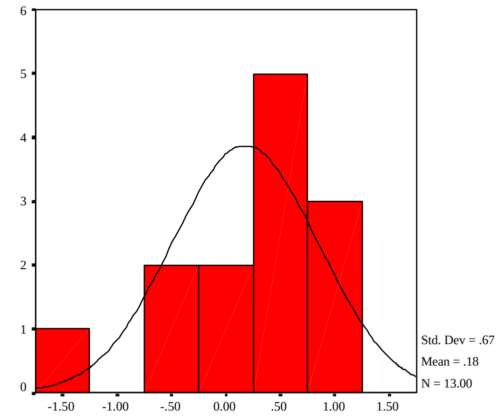
Figure 4.3.1.3
Differences for All Families, by State
(Preliminary Minus as Finally Edited)



4.3.2 RDD Comparisons B-3: Confidence in Ability to Get Children Medical Care

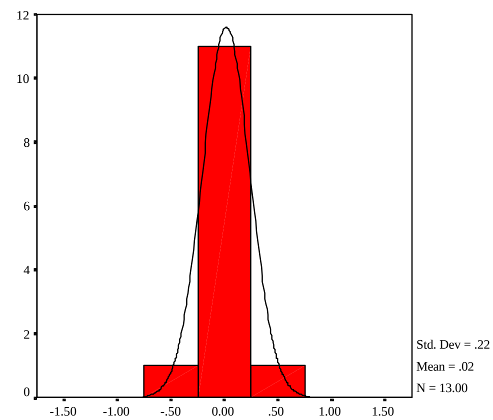
Differences between the telephone sample only estimates and the full sample estimates are very small. The largest difference is for Texan families below 200 percent of the poverty level (see figure 4.3.2.1).

Figure 4.3.2.1
Differences for Families Below 200 percent of the Poverty Level, by State (Preliminary Minus as Finally Edited)



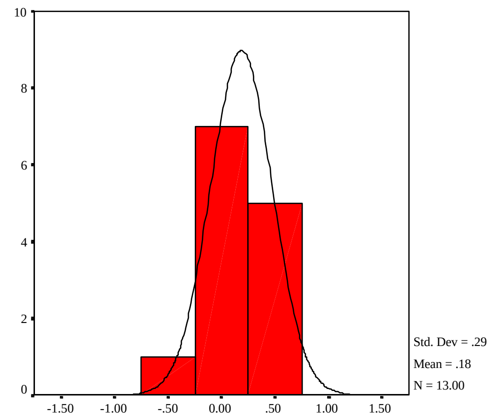
In contrast to the significance test results shown in the original *Snapshot* for this measure, the percentage of children with a most-knowledgeable adult (MKA) who is not confident about getting medical care for the family is significantly different from the national percentage (18.3 percent vs. 14.6 percent). Yet this was the only change in reproducing the significance tests in the *Snapshot* when using only the telephone sample. In addition, the telephone-only estimate of 18.3 percent is within sampling error (using a 95 percent confidence interval) of the full sample estimate.

Figure 4.3.2.2
Differences for Families Above 200 percent of the Poverty Level, by State (Preliminary Minus as Finally Edited)



The differences between the comparisons for this measure and those for the percentage of children in poverty illustrate a point made in the undercoverage analysis conducted by Hall et al (1999), in report no. 16. For univariate statistics for variables closely related to income, the exclusion of nontelephone households will result in substantial bias in the estimates. For measures less correlated with income, such as confidence in getting medical care, the bias is much smaller.

Figure 4.3.2.3
Differences for All Families, by State
(Preliminary Minus as Finally Edited)



In summary, only one difference emerged from replicating the tests of significance that were carried out in the original *Snapshot*. This occurred for children in families below 200 percent of poverty. For that group the estimated percentage of children whose MKA is either not confident at all or not too confident about getting medical care for their family is now statistically significant as compared to the national estimate.

4.3.3 RDD Comparisons C-4: Reading and Telling Stories to Young Children

At first glance, some of the differences between the telephone-only estimates and the full sample estimates appear to be large. For children in families with family incomes below 200 percent of poverty, large differences appear in the estimates for Florida, Michigan, Texas and Washington. The difference in Florida for children of all incomes also appears to be quite large. But all of these differences are within sampling error (which is larger than in the previously discussed tables due to the smaller sample sizes in this table).

Figure 4.3.3.1
Differences for Families Below 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)

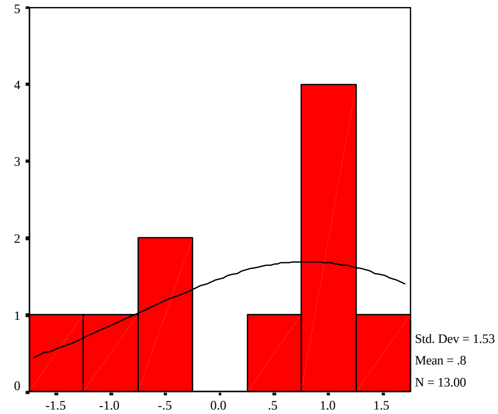
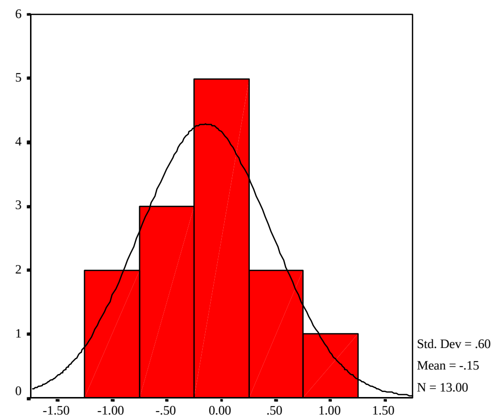
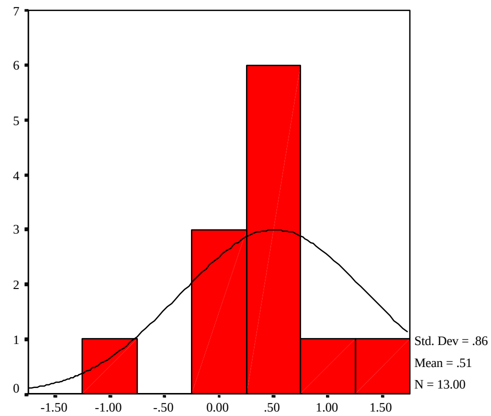


Figure 4.3.3.2
Differences for Families Above 200 percent of the Poverty Level, by State
(Preliminary Minus as Finally Edited)



What is also interesting is that for these estimates, which have the largest differences between the telephone-only and full samples, the results of the significance tests conducted in the original *Snapshot* are unchanged. For four estimates, the significance test results are changed from those shown in the original *Snapshot* (Mississippi and Wisconsin, for children in families with income below 200 percent of poverty; Florida, for children in families with income above 200 percent of poverty; and Colorado, for children in all families). In all of these states, the bias from excluding the nontelephone sample is small.

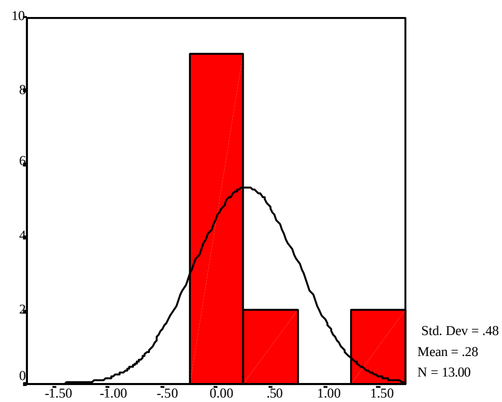
Figure 4.3.3.3
Differences for All Families, by State
(Preliminary Minus as Finally Edited)



4.3.4 RDD Comparisons D-2: Parental Aggravation

Most of the differences between the telephone sample only estimates and those using the full sample are small in this *Snapshot*. Across all three figures shown here, the percentage of children with an MKA who reports being highly aggravated is understated when using only the telephone sample. The size of the differences does not vary much by whether the MKA has a spouse.

Figure 4.3.4.1
Differences for Parents with Spouses Present, by State
(Preliminary Minus as Finally Edited)



For estimates of all MKAs, the results of the significance tests employed in the original *Snapshot* change for Washington. The Washington estimate of 7.5 percent of children with a highly aggravated MKA is not statistically significant from the national estimate of 8.6 percent.

Figure 4.3.4.2
Differences for Parents without Spouses, by State
(Preliminary Minus as Finally Edited)

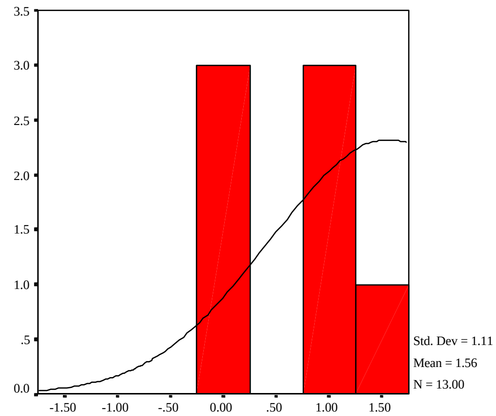
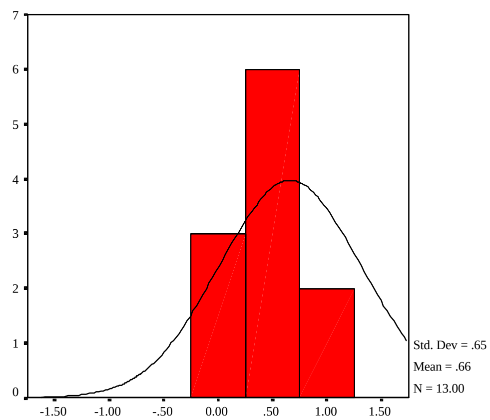


Figure 4.3.4.3
Differences for All Parents, by State
(Preliminary Minus as Finally Edited)



4.4 Overall Comparison Summary for *Snapshots* Statistics

In this section we make a national or overall comparison. Earlier we have looked at table-by-table and state-by-state differences due to editing, adjusting for the undercount, or treating the RDD portion of the sample as representing the whole population. In this section, we look at what is happening across all the state-by-state differences in the *Snapshots* tables combined (subsections 4.4.1 to 4.4.3). A complete tabular summary at the U.S. total level is found in table 4.4.1, which concludes the section.

Unlike earlier sections, in which we looked at individual tables, here the graphs, follow aggregate differences from all 20 *Snapshots* tables. Each plot is again shown as a histogram of differences, with seven classes centered at zero and defined by the intervals

–1.75	to	–1.25	+0.25	to	+0.75
–1.25	to	-0.75	+0.75	to	+1.25
–0.75	to	–0.25	+1.25	to	+1.75

–0.25 to +0.25

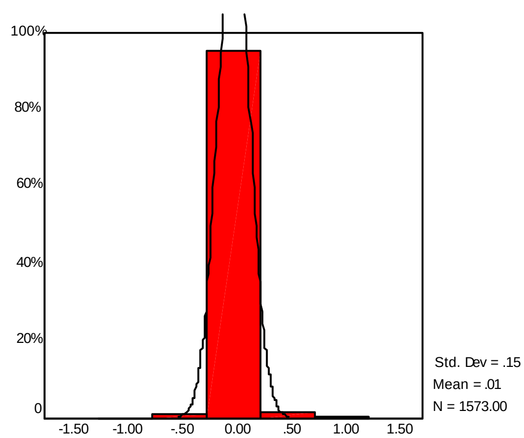
Unlike in the earlier displays in this chapter, the vertical dimension is plotted as a percentage and not as a simple count. The shapes can, of course, all be compared in any case.

Beside every figure, as usual, are the simple arithmetic means of the combined state differences, not weighted by sample or population size, and the comparable standard deviation (also calculated on this basis). Finally, the smooth bell-shaped curve, shown for contrast to the raw histogram, was calculated by treating the data as normally distributed with the mean and standard deviation shown.

4.4.1 Overall Editing Impacts on *Snapshots*

As shown in figure 4.4.1 below, the smallness of the editing differences we found earlier holds up overall. There are virtually no differences of importance resulting from the editing of the *Snapshots* variables. See also table 4.4.1 below for the U.S. summary.

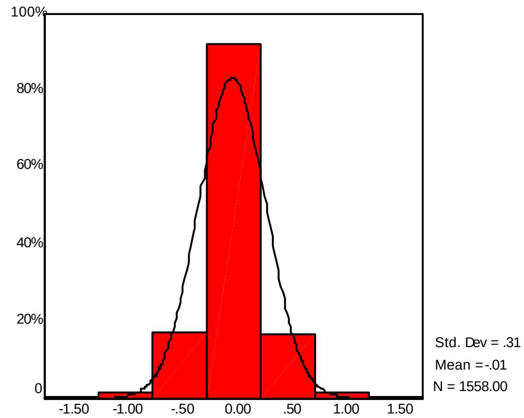
Figure 4.4.1
Editing Differences for All Tables and All States from
Original *Snapshots* Results



4.4.2 Overall Undercount-Adjusted Impacts on *Snapshots*

Figure 4.4.2 shows that most differences between the undercount-adjusted estimates and those produced originally for *Snapshots* heap close to zero. This is the same pattern we saw earlier with editing but the data here are somewhat more spread out. Even so, the differences found were insufficient to change the overall bottom–line conclusions in the *Snapshots*. It might be added that this was true despite the fact that the undercount adjustment, as expected, did have a greater impact on minorities and the low-income population. See also table 4.4.1below for the U.S. summary.

Figure 4.4.2
Undercount Differences for All Tables and All States from Original *Snapshots* Results

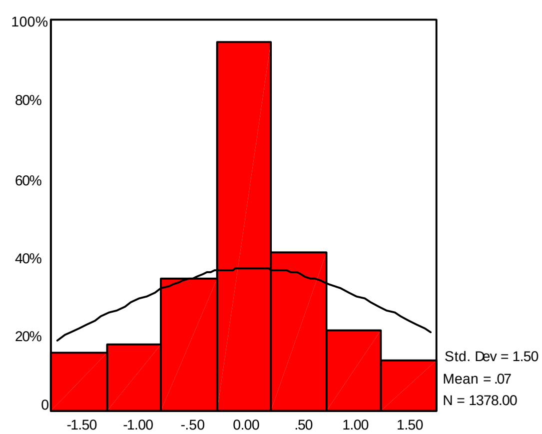


4.4.3 Overall RDD Impacts on *Snapshots*

The results from attempting to use just the RDD sample to represent the entire population have already been shown to be unsatisfactory, as we illustrated earlier in section 4.3. Here we have the same pattern but with the overall set of differences (figure 4.4.3), not just those from specific tables.

Clearly the differences are quite large and many of the observations we made in the *Snapshots* report would not have been made had we not made the effort to draw a large sample of nontelephone households.

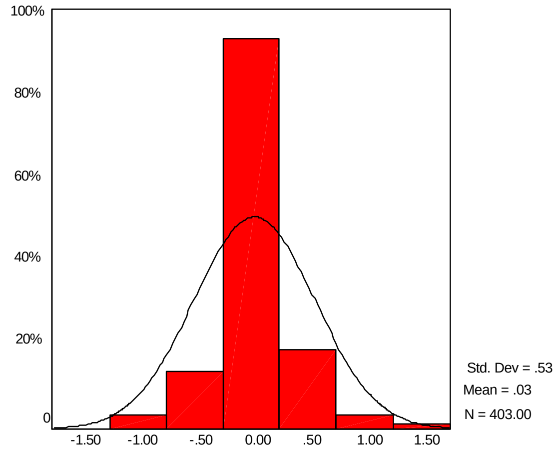
Figure 4.4.3
RDD Differences for All Tables and All States from Original *Snapshots* Results



Figures 4.4.4 and 4.4.5 allow us to look a little deeper into the major differences between the RDD and complete NSAF sample. In fact, as we see in figure 4.4.4 the RDD sample tracks the

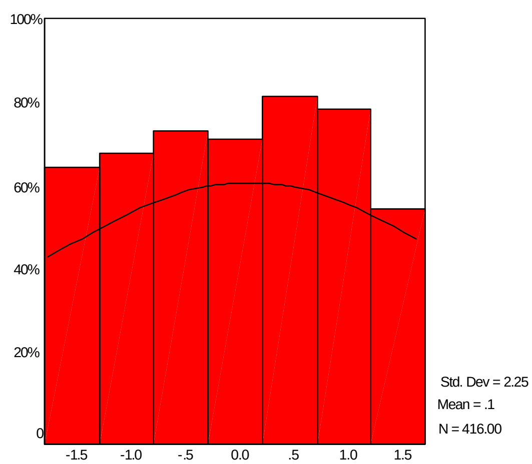
total survey quite well for households with incomes that bring them above 200 percent of poverty. This is expected since almost all such people have telephone service.

Figure 4.4.4
RDD Differences for All Families Over 200 Percent of Poverty and All States from Original *Snapshots* Results



The results for persons under 200 percent of poverty are very different. Notice how spread out the differences are. The histogram is almost flat across the entire range plotted. The RDD estimates are clearly unreliable and do not capture this segment of the population effectively. This is the very group that the NSAF was designed to focus on, and the results more than vindicate the decision to employ a special nontelephone sample.

Figure 4.4.5
RDD Differences for Families Under 200 Percent of Poverty and All States from Original *Snapshots* Results



In summary, the comparisons made above look at what would have happened, hypothetically, if we had not done a special sample of nontelephone households and had relied only on a random-digit-dialing (RDD) design. As we have seen, there are large differences in the statistics on the population, notably for families under 200 percent of poverty. An RDD-only approach would not have worked well at all for this most important group. See table 4.4.1 below for the U.S. summary.

The practice of a separate nontelephone sample was continued for the 1999 NSAF, and we intend to continue such a sample in later rounds, as well. We may be modifying our approach, however, by also employing what is called a “Keeter adjustment.” The Keeter technique treats telephone households with interrupted service as representative of all nontelephone households. We have already confirmed that this could work with the NSAF (see report no 16). We will use it while continuing a direct nontelephone sample. The main difference is that the nontelephone household sample could then be smaller in size.

Table 4-1
U.S. Summary Differences Between Editing, Undercount, and RDD from Original Snapshot Results

Snapshot	**Editing**			**Undercount**			**Telephone**		
	Prelim-inary	Diff (1) - (3)	As Finally Edited	Census Level	Diff (4) - (6)	Under-Count	Total Sample	Diff (7) - (9)	Phone Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A-1: Poor and Low-Income Non-elderly Americans									
Under 100% of Poverty	14.8	0.0	14.8	14.8	-0.1	14.9	14.8	1.6	13.2
Under 200% of Poverty	33.2	0.1	33.1	33.1	-0.3	33.4	33.1	1.6	31.5
A-2: Children Below the Poverty Level									
One-Parent Families	44.1	-0.5	44.6	44.6	0.0	44.6	44.6	5.4	39.2
Two-Parent Families	10.5	0.0	10.5	10.5	-0.1	10.6	10.5	1.1	9.4
All Famlies	20.5	0.1	20.4	20.4	-0.2	20.6	20.4	2.6	17.8
A-3: Adults Age 25-54 Employed Full- or Part-Time									
All Parents Under 200% of Poverty	65.0	0.0	65.0	65.0	-0.1	65.1	65.0	-2.1	67.1
All Parents Over 200% of Poverty	87.1	0.0	87.1	87.1	-0.1	87.2	87.1	-0.1	87.2
Unmarried Parents, All incomes	73.6	0.0	73.6	73.6	0.0	73.6	73.6	-3.5	77.1
All Parents, All incomes	79.8	-0.1	79.9	79.9	0.0	79.9	79.9	-1.0	80.9
All Adults, All Incomes	81.5	0.0	81.5	81.5	0.0	81.5	81.5	-0.9	82.4
A-4: Parents with Problems Paying their Mortgage, Rent, or Utility Bills									
Parents Under 200% of Poverty	28.4	0.0	28.4	28.4	0.2	28.2	28.4	2.2	26.2
Parents Over 200% of Poverty	9.1	0.0	9.1	9.1	0.0	9.1	9.1	0.1	9.0
Parents of All Incomes	16.0	0.0	16.0	16.0	0.1	15.9	16.0	1.2	14.8
Childless Adults Under 200% of Poverty	17.1	-0.2	17.3	17.3	0.1	17.2	17.3	2.1	15.2
Childless Adults Over 200% of Poverty	5.4	0.0	5.4	5.4	0.0	5.4	5.4	0.1	5.3
Childless Adults of All Incomes	8.3	0.0	8.3	8.3	0.0	8.3	8.3	0.6	7.7
A-5: Children Living in Families that Worried about or Experienced Difficulty Affording Food									
Under 200% of Poverty, No Food Problems	46.1	0.1	46.0	46.0	-0.2	46.2	46.0	-3.0	49.0
Under 200% of Poverty, Some Food Problems	53.9	-0.1	54.0	54.0	0.2	53.8	54.0	3.0	51.0
Over 200% of Poverty, No Food Problems	84.6	0.0	84.6	84.6	0.1	84.5	84.6	0.1	84.5
Over 200% of Poverty, Some Food Problems	15.4	-0.1	15.5	15.5	0.0	15.5	15.5	-0.1	15.6
All Incomes No Food Problems	68.2	0.0	68.2	68.2	0.1	68.1	68.2	-2.0	70.2
All Incomes Some Food Problems	31.8	0.0	31.8	31.8	-0.1	31.9	31.8	2.0	29.8
B-1: Children Covered by Health Insurance									
Under 200% of Poverty, Private	39.7	0.0	39.7	39.7	0.0	39.7	39.7	-3.4	43.1
Under 200% of Poverty, Public	39.0	-0.1	39.1	39.1	-0.2	39.3	39.1	3.1	36.0
Under 200% of Poverty, Uninsured	21.3	0.0	21.3	21.3	0.3	21.0	21.3	0.4	20.9
Over 200% of Poverty, Private	90.1	-0.1	90.2	90.2	0.3	89.9	90.2	0.0	90.2
Over 200% of Poverty, Public	5.0	0.0	5.0	5.0	-0.1	5.1	5.0	0.0	5.0
Over 200% of Poverty, Uninsured	4.9	0.0	4.9	4.9	0.0	4.9	4.9	0.1	4.8
All Incomes Private	68.6	0.0	68.6	68.6	0.2	68.4	68.6	-2.6	71.2
All Incomes Public	19.5	0.0	19.5	19.5	-0.3	19.8	19.5	2.0	17.5
All Incomes Uninsured	11.9	0.0	11.9	11.9	0.1	11.8	11.9	0.6	11.3
B-2: Non-elderly Adults Covered by Health Insurance									
Under 200% of Poverty, Private	43.5	0.1	43.4	43.4	0.0	43.4	[redacted]		
Under 200% of Poverty, Public	19.9	0.0	19.9	19.9	0.1	19.8			
Under 200% of Poverty, Uninsured	36.6	-0.1	36.7	36.7	-0.2	36.9			

Over 200% of Poverty, Private	88.0	0.1	87.9	87.9	0.0	87.9	[redacted]		
Over 200% of Poverty, Public	3.3	0.0	3.3	3.3	-0.1	3.4	[redacted]		
Over 200% of Poverty, Uninsured	8.7	-0.1	8.8	8.8	0.0	8.8	[redacted]		
All Incomes Private	75.0	0.0	75.0	75.0	0.2	74.8	[redacted]		
All Incomes Public	8.1	0.0	8.1	8.1	-0.1	8.2	[redacted]		
All Incomes Uninsured	16.9	0.0	16.9	16.9	-0.1	17.0	[redacted]		
B-3: Children Whose Parents Are Not Confident of Getting Them Medical Care									
Under 200% of Poverty	14.2	0.0	14.2	14.2	-0.1	14.3	14.2	-0.5	14.7
Over 200% of Poverty	3.9	0.1	3.8	3.8	-0.1	3.9	3.8	0.0	3.8
All Incomes	8.3	0.0	8.3	8.3	0.0	8.3	8.3	0.1	8.2
B-4: Children and Non-elderly Adults with No Usual Source of Health Care									
Children Under 200% of Poverty	9.7	0.0	9.7	9.7	-0.3	10.0	9.7	0.2	9.5
Adults Under 200% of Poverty	26.6	-0.1	26.7	26.7	-0.4	27.1	[redacted]		
Children Over 200% of Poverty	3.3	0.0	3.3	3.3	-0.1	3.4	3.3	0.1	3.2
Adults Over 200% of Poverty	13.8	0.0	13.8	13.8	-0.1	13.9	[redacted]		
Children of All Incomes	6.0	0.0	6.0	6.0	-0.2	6.2	6.0	0.2	5.8
Adults of All Incomes	17.6	0.0	17.6	17.6	-0.2	17.8	[redacted]		
B-5: Children and Non-elderly Adults In Fair or Poor Health									
Children Under 200% of Poverty	8.2	0.0	8.2	8.2	0.0	8.2	8.2	0.4	7.8
Adults Under 200% of Poverty	23.1	-0.1	23.2	23.2	0.0	23.2	[redacted]		
Children Over 200% of Poverty	1.9	0.0	1.9	1.9	0.1	1.8	1.9	0.1	1.8
Adults Over 200% of Poverty	7.6	0.0	7.6	7.6	-0.1	7.7	[redacted]		
Children of All Incomes	4.6	0.0	4.6	4.6	0.0	4.6	4.6	0.4	4.2
Adults of All Incomes	12.1	0.0	12.1	12.1	-0.1	12.2	[redacted]		
C-1: Children Living in Various Family Structures									
Two-Parent Families	62.6	0.0	62.6	62.6	0.2	62.4	62.6	-1.0	63.6
Blended Families	7.6	-0.2	7.8	7.8	0.1	7.7	7.8	0.1	7.7
One-Parent Families	26.7	0.2	26.5	26.5	-0.2	26.7	26.5	0.8	25.7
No-Parent Families	3.2	0.0	3.2	3.2	0.0	3.2	3.2	0.2	3.0
C-2: Children Born outside of and within Marriage									
Unmarried	18.2	-0.4	18.6	18.6	-0.2	18.8	18.6	0.9	17.7
Married	81.8	0.4	81.4	81.4	0.2	81.2	81.4	-0.9	82.3
C-3: Children Highly Engaged in School									
Under 200% of Poverty, Age 6-11	38.2	0.0	38.2	38.2	0.1	38.1	38.2	-0.7	38.9
Under 200% of Poverty, Age 12-17	29.8	0.0	29.8	29.8	0.2	29.6	29.8	-0.3	30.1
Under 200% of Poverty, Age 6-17	34.4	0.0	34.4	34.4	0.1	34.3	34.4	-0.6	35.0
Over 200% of Poverty, Age 6-11	47.1	0.0	47.1	47.1	-0.2	47.3	47.1	0.0	47.1
Over 200% of Poverty, Age 12-17	43.8	0.0	43.8	43.8	0.1	43.7	43.8	0.1	43.7
Over 200% of Poverty, Age 6-17	45.4	0.0	45.4	45.4	-0.1	45.5	45.4	0.0	45.4
All Incomes Age 6-11	43.3	0.0	43.3	43.3	0.0	43.3	43.3	-0.4	43.7
All Incomes Age 12-17	38.6	0.0	38.6	38.6	0.2	38.4	38.6	-0.3	38.9
All Incomes Age 6-17	41.0	0.0	41.0	41.0	0.1	40.9	41.0	-0.4	41.4
C-4: Children Age 1 to 5 Read to or Told Stories Fewer than Three Days per Week									
Under 200% of Poverty	24.0	-0.1	24.1	24.1	-0.1	24.2	24.1	0.0	24.1
Over 200% of Poverty	10.5	0.1	10.4	10.4	0.1	10.3	10.4	0.0	10.4

All Incomes	16.8	0.0	16.8	16.8	0.0	16.8	16.8	0.4	16.4
C-5: Children Participating in Extracurricular Activities									
Under 200% of Poverty, Age 6-11	72.5	0.0	72.5	72.5	0.7	71.8	72.5	-1.4	73.9
Under 200% of Poverty, Age 12-17	73.4	0.1	73.3	73.3	-0.1	73.4	73.3	-2.0	75.3
Under 200% of Poverty, Age 6-17	72.9	0.0	72.9	72.9	0.3	72.6	72.9	-1.6	74.5
Over 200% of Poverty, Age 6-11	90.4	0.0	90.4	90.4	0.2	90.2	90.4	0.2	90.2
Over 200% of Poverty, Age 12-17	89.9	0.0	89.9	89.9	-0.1	90.0	89.9	0.0	89.9
Over 200% of Poverty, Age 6-17	90.2	0.0	90.2	90.2	0.1	90.1	90.2	0.2	90.0
All Incomes Age 6-11	82.7	0.0	82.7	82.7	0.5	82.2	82.7	-0.8	83.5
All Incomes Age 12-17	83.7	0.0	83.7	83.7	-0.1	83.8	83.7	-0.9	84.6
All Incomes Age 6-17	83.2	0.0	83.2	83.2	0.2	83.0	83.2	-0.9	84.1
C-6: Children with High Levels of Behavioral & Emotional Problems									
Under 200% of Poverty, Age 6-11	9.6	0.0	9.6	9.6	0.0	9.6	9.6	0.8	8.8
Under 200% of Poverty, Age 12-17	14.9	0.0	14.9	14.9	0.1	14.8	14.9	1.2	13.7
Over 200% of Poverty, Age 6-11	4.2	0.0	4.2	4.2	-0.1	4.3	4.2	0.0	4.2
Over 200% of Poverty, Age 12-17	5.2	0.0	5.2	5.2	-0.1	5.3	5.2	-0.1	5.3
All Incomes Age 6-11	6.5	0.0	6.5	6.5	-0.1	6.6	6.5	0.4	6.1
All Incomes Age 12-17	8.8	0.0	8.8	8.8	0.0	8.8	8.8	0.5	8.3
D-1: Children Living with a Parent Who Participated in Volunteer or Religious Activities									
Under 200% of Poverty, Volunteer	30.5	0.0	30.5	30.5	0.1	30.4	30.5	-1.1	31.6
Under 200% of Poverty, Religious	54.7	0.0	54.7	54.7	-0.2	54.9	54.7	-1.2	55.9
Over 200% of Poverty, Volunteer	43.3	0.0	43.3	43.3	0.0	43.3	43.3	0.2	43.1
Over 200% of Poverty, Religious	61.9	-0.1	62.0	62.0	0.1	61.9	62.0	0.1	61.9
All Incomes Volunteer	37.8	0.0	37.8	37.8	0.0	37.8	37.8	-0.7	38.5
All Incomes Religious	58.8	0.0	58.8	58.8	-0.1	58.9	58.8	-0.7	59.5
D-2: Children Living with a Parent Who Felt Highly Aggravated									
Under 200% of Poverty, With a spouse	10.5	0.0	10.5	10.5	-0.1	10.6	10.5	0.7	9.8
Under 200% of Poverty, Without a spouse	18.0	0.0	18.0	18.0	-0.2	18.2	18.0	0.3	17.7
Under 200% of Poverty, All Parents	13.7	0.0	13.7	13.7	-0.2	13.9	13.7	0.6	13.1
Over 200% of Poverty, With a spouse	4.8	0.0	4.8	4.8	0.0	4.8	4.8	0.0	4.8
Over 200% of Poverty, Without a spouse	11.2	0.0	11.2	11.2	-0.2	11.4	11.2	0.3	10.9
Over 200% of Poverty, All Parents	5.6	0.0	5.6	5.6	-0.1	5.7	5.6	0.0	5.6
All Incomes, With a spouse	6.6	0.0	6.6	6.6	-0.1	6.7	6.6	0.2	6.4
All Incomes, Without a spouse	16.0	0.0	16.0	16.0	-0.2	16.2	16.0	0.5	15.5
All Incomes, All Parents	9.0	0.0	9.0	9.0	-0.2	9.2	9.0	0.4	8.6
D-3: Children Living with a Parent Whose Symptoms Suggested Poor Mental Health									
Under 200% of Poverty, With a spouse	20.9	-0.1	21.0	21.0	0.1	20.9	21.0	2.0	19.0
Under 200% of Poverty, Without a spouse	31.6	0.0	31.6	31.6	0.2	31.4	31.6	0.4	31.2
Under 200% of Poverty, All Parents	25.4	-0.1	25.5	25.5	0.1	25.4	25.5	1.4	24.1
Over 200% of Poverty, With a spouse	8.7	0.0	8.7	8.7	-0.1	8.8	8.7	0.1	8.6
Over 200% of Poverty, Without a spouse	19.6	0.0	19.6	19.6	-0.4	20.0	19.6	0.2	19.4
Over 200% of Poverty, All Parents	10.1	0.0	10.1	10.1	-0.2	10.3	10.1	0.0	10.1

All Incomes, With a spouse	12.7	0.0	12.7	12.7	-0.1	12.8	12.7	0.8	11.9
All Incomes, Without a spouse	28.1	0.0	28.1	28.1	0.0	28.1	28.1	0.7	27.4
All Incomes, All Parents	16.6	0.0	16.6	16.6	-0.1	16.7	16.6	0.9	15.7
D-4: Parents Who Agreed or Strongly Agreed with Statements Regarding Welfare and Working Mothers									
Under 200% of Poverty: Welfare makes people work less	73.5	0.0	73.5	73.5	0.0	73.5	73.5	-1.0	74.5
Under 200% of Poverty: Working Mother can establish a relationship	75.0	0.0	75.0	75.0	0.3	74.7	75.0	0.4	74.6
Under 200% of Poverty: Mothers should not work outside the home	53.3	0.0	53.3	53.3	0.0	53.3	53.3	-0.2	53.5
Over 200% of Poverty: Welfare makes people work less	81.1	0.0	81.1	81.1	0.1	81.0	81.1	0.0	81.1
Over 200% of Poverty: Working mother can establish a relationship	79.4	0.0	79.4	79.4	0.2	79.2	79.4	0.1	79.3
Over 200% of Poverty: Mothers should not work outside the home	46.7	0.0	46.7	46.7	0.0	46.7	46.7	-0.1	46.8
All Incomes: Welfare makes people work less	78.1	0.0	78.1	78.1	0.0	78.1	78.1	-0.5	78.6
All Incomes: Working mother can establish a relationship	77.6	0.0	77.6	77.6	0.2	77.4	77.6	0.1	77.5
All Incomes: Mothers should not work outside the home	49.4	0.0	49.4	49.4	0.0	49.4	49.4	0.1	49.3
Black boxes indicate that an RDD weight was not available for adults on that variable.									

Appendix
Imputation of Missing Special Adjusted Summer MKA Weights

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A.1. Introduction

This appendix serves to further document MKA weight creation by taking up the process after Westat had finished its work. As noted earlier in chapter 2, a mistake was made in selecting the MKAs to be given special summer-adjusted weights. To remedy this, an adjusted MKA summer weight was imputed. Because this approach was unusual, several checks were done on the new weight.

In checking the summer-adjusted weights received from Westat, we discovered that there were 1,059 observations that had inadvertently not been sent to Westat and hence were missing a summer-adjusted weight in the nonsummer MKA child care file. To deal with this problem, a "hot-decking" approach was employed rather than having Westat rerun the weighting procedure. While hot decking is a common choice in imputing missing content variables, its use in imputing weights is clearly nonstandard. Why did we do it?

Basically we did this because the problem seemed small and any roughness in the approach was predicted to make little difference (as seems to be the case). Central to this thinking was the fact that the total number of observations with missing weights represents 5 percent of the observations in the nonsummer sample. In addition, for a wide selection of characteristics of MKAs, the observations without weights we looked at were very much like the observations with weights.

In the hot-deck process, individuals with certain characteristics who had weights are matched to individuals with similar characteristics who did not. Since all observations (those missing weights as well as those not missing weights) had an original weight (the old weight), we were able to use this weight, along with other information from the survey to match observations and assign weights to those observations whose weights were missing. We used essentially the same process to assign replicate weights to these observations. In the four sections, that follow, we discuss the details of this process, including how we checked it:

- In section A.2 we describe how the new weights were created for each recipient observation.
- The creation of replicate weights for the recipient observations is covered in section A.3.
- Rescaling the new set of weights to population totals is dealt with in section A4.
- Finally, some results from the analyses done of the hot decked weights are presented in section A5.

A.2. Creating a New Weight for Each Recipient Observation

The first step of the process was to match “recipient” observations with “donor” observations. For example, if a 30-year-old, high school–educated female from Michigan did not have a weight, we attempted to match the observation with an observation with a similar set of characteristics that did have a weight. Once these observations were matched, the donor weight was supplied (as described below) to the recipient observation. Because the original weighting process done by Westat was based on a number of key variables, we attempted to match observations using all of the same variables. The variables used to match observations, therefore, were of two types: (1) variables used in creating the summer adjustment and (2) variables used in scaling the sample to population totals. These variables and their values are outlined in table A-1.

Table A-1.
Variables Used to Find Matches for 1,059 Observations that Needed Weights

Variable	Values	Variable	Values
Site	Alabama	**Income**	zero income
	California		< $15K and no food stamps
	Florida		< $15K and has food stamps
	Massachusetts		Between $15K and $25K
	Michigan		Between $26K and $35K
	Minnesota		Greater than $35K
	New Jersey		
	New York	**Race/Hispanicity**	Asian/PI, Hispanic
	Texas		Asian/PI, Non-Hispanic
	Washington		Black, Hispanic
	Mississippi		Black, Non-Hispanic
	Milwaukee County		Native American, Hispanic
	Balance of Wisconsin		Native Am., Non-Hispanic
	Balance of the US		White, Hispanic
	Colorado		White, Non-Hispanic
Sampling Type	Random-Digit Dialing	**Own or Rents Home**	Owns
	Area Sample		Rents
Sex	Male	**Region**	South
	Female		West
			Midwest
MKA Age	18-24 years		Northeast
	25-34 years		
	35-44 years	**Mother’s Education**	No HS diploma or GED
	45-54 years		HS diploma or GED
	55-64 years		Bachelor’s Degree or Higher

We began the matching process with the 20,220 donor observations that already had weights. For each of these observations we then created a variable that measured the change between the summer-adjusted weight and the original weight. This was simply

$$(\text{factor}) = (\text{new weight}) / (\text{old weight}).$$

Once matched (as described below), this factor would then be applied to the old weight of the recipient case, to create a new weight for that MKA. Two issues arose when matching observations and applying the factor:

(1) Recipient observations could match with more than one donor observation. In some cases, these donors had different factors. A possible reason for the different adjustment factors for observations sharing the exact same characteristics is that there may be other variables used in Westat's weighting that are different across these observations. Most combinations (about 80 percent), however, had a *unique* factor. The remaining 20 percent of combinations had *multiple* factors. For these observations, there were two to four different adjustment factors (most had two) associated with the same set of characteristics.

(2) Not every recipient had a donor match, in other words, there could be a Native American, 20-year-old, male, area sample case from Colorado in need of a weight, but no match of this person among donors with weights. Thus, we needed to devise a way to find matches for these remaining observations. About 80 percent of all observations had exact matches on all characteristics.

In light of these issues, we devised a four-step method to supply weights to all 1059 recipient observations. The more steps necessary for any individual observation, the less precise the process. The distribution of which step was used for what number of observations is shown in table A-2:

Step 1. If there was an exact match of donor to recipient on all variables in table A-1 and there was a *unique* adjustment factor, then the recipient's new weight is simply the recipient's old weight multiplied by the donor's factor. This is the easiest and most exact method. It worked for about 50 percent of the 1,059 observations.

Step 2. If there was an exact donor-recipient match (as in step 1), but there were multiple adjustment factors to chose from, then we checked for an "old weight match." This means that the old weights of the donor and the recipient are the same. If an old weight match was found, we used the factor of the matched donor. As with step 1, the recipient's new weight was the recipient's old weight multiplied by the donor's adjustment factor.

Step 3. If there was an exact donor-recipient match and multiple adjustment factors to chose from (as in step 2), but no "old weight match," a weighted average adjustment factor was used. This method takes all donors in the set and uses the following formula to produce a weighted average adjustment factor:

$$\frac{OldWt_1 + OldWt_2 + \dots + OldWt_n}{NewWt_1 + NewWt_2 + \dots + NewWt_n}$$

In this case, the recipient's new weight is the recipient's old weight multiplied by the multiple donors' weighted average adjustment factor.

Step 4. For those recipient observations without an exact donor match, we used a more complex nested hierarchical hot-deck approach (see report no. 10 in this methodology series for more on this method in a different setting). The process was identified a donor match by progressively re-coding and dropping variables from the list in table A-1. A simple example would be a 20-year-old college-educated male from Minnesota who did not have a match. However, an under-30 college-educated male from Minnesota was found as a match. We re-coded variables first, then began dropping the less important variables, followed by more important variables, until a match was found. The order that variables were re-coded and dropped is shown in table A-3. Very few observations remained after the first couple of rounds, and the distribution of how weights were assigned for all 1,059 observations is shown in table A-2.

Table A-2.
Which Method Was Used to Find
Weights for 1,059 Observations

Method	Number of Observations	Percent of Observations
Step1	539	50.9
Step 2	140	13.2
Step 3	168	15.9
Step 4		
1st round	64	6.0
2nd round	32	3.0
3rd round	15	1.4
4th round	39	3.7
5th round	34	3.2
6th round	21	2.0
7th round	0	0
8th round	5	0.5
9th round	2	.02
Total	1,059	100

Table A-3.
The Order in Which Variables Were Coarsened
and Dropped from the Set of Characteristics in Step 4

Round Number	Variable	What Was Done to the Variable
1st round	Mother's Age	re-coded to 3 categories
2nd round	Income	re-coded to 3 categories
3rd round	Race	re-coded to 3 categories
4th round	Sex	dropped
5th round	Own or Rent	dropped
6th round	Mother's Education	dropped
7th round	Region	dropped
8th round	Mother's Age	dropped
9th round	Race	dropped

Note: Site, Random-Digit Dialing/Area Sample, and Income were never dropped

A.3. Creation of Replicate Weights for the Recipient Observations.

Replicate weights were computed in a manner similar to the above process for all 60 replicate weights. The process was repeated for each replicate separately from start to finish. The distributions of how weights were found for each replicate look similar to table A-2 above.

One issue in creating replicates involved the fact that observations are dropped to create the replicate weights. When replicate weights are generated, a group of observations is dropped from the sample, and the weights of all these "dropped" observations are set equal to zero. This dropped group is different for each replicate. The issue was to figure out in which of the replicates should the recipient observation be part of the dropped group (i.e., weight should be zero for that replicate). In testing the donor observations, we found that in every instance that the donor observation had a zero in the old set of replicate weights, it also had a zero in the set of adjusted replicates. Therefore, where recipient observations had zero in the set of old replicate weights, the replicate weight was set to zero in the newly created replicate.

This method worked well on all but 54 cases where a replicate weight was needed. For these 54 cases, the recipient replicate needed a nonzero value, but the donor match had zero for that replicate. When this happened we continued the matching until a nonzero donor match was found.

A.4. Rescaling the Weights to Population Totals

The last step in this procedure was to rescale the final set of weighted observations to population totals. This was necessary because the donor weights were originally poststratified to national totals, and adding the 1,059 weighted recipient observations to the data set meant that the new population totals were greater than the actual national totals.

We rescaled using the poststratification variables used by Westat. We took the full MKA data set of both summer and nonsummer observations (donors + recipients + summer observations) and divided it into cells by each of poststratification variables. By doing this, we determined the population total (i.e. weighted frequency) for each cell. We also did this for the data set consisting of only donor and recipient observations (i.e., all observations except summer observations).

We then determined the population totals for the same cells, using the summer-adjusted weight. For the summer-adjusted observations in each cell, we multiplied the observation's summer adjusted weight by a factor created by dividing the population total given the full sample weight by the population total given the summer-adjusted weight (from the current data set). In other words,

$$\text{Scaled Weight} = \text{Unscaled Weight} * (\text{Population total}_{\text{full sample}} / \text{Population total}_{\text{summer adjusted}})$$

Here we also ran into one problem. The problem was that, because the full (not-summer adjusted) sample was larger than the nonsummer sample, some cells that were in the full sample were not represented in the nonsummer sample. Thus, after rescaling, we ended up with a population total that was 20,000 weighted observations below the correct total of 37,552,964. This difference was due to only 14 observations in the full data set. To account for this slight difference we multiplied each weight by the correct total divided by the current total. This rescaled the weights to correct population totals.

A.5. Checks Done on the Hot-Decked Weights.

In order to check the accuracy of the hot-decked weights, we ran several tests on the weights, both statistical descriptions of old, donor, and donor + recipient sets, and checks on the main child care variables using the different weights. The results of these tests are presented below.

Table A-4
MKA Statistical Checks

Item	Full Sample Weight	Scaled	New Weight (with 1,059 missing)	Scaled	New Weight (all non-summer)	Scaled
N	27,297		20,022		21,081	
Mean	1,335.63	1	1,875.58	1	1,781.36	1
Minimum Value	15.66246	.0117	17.54	0.0093	17.33368	.0097
Maximum Value	38,430.3	26.77	36,586	19.51	35,601.34	19.99
Standard Deviation	2,311.16	1.73	3,172.36	1.69	3018.54	1.69
Correlation (only those that have all 3 weights)	.	n/a	.99809	n/a	.99809	n/a
	.97640	n/a	.	n/a	.97640	n/a
	.97520	n/a	.97520	n/a	.	n/a
Population Projection	37,300,028	n/a	37,552,946	n/a	37552946	n/a

Table A-5
Child Care Variable Checks

	Old Weight	New Weight (with 1,059 missing)	New Weight (after hot-decking)	Change between Old Weight and Hot-Decked New Weight	Change between New Weights before and after Hot-Decking
Percent Paying for Child Care	56.6	57	56.3	.3	.7
Mean Monthly Expenses of Those Paying for Care	283.8	287.6	286.1	2.3	1.5
Percent of Earning Used for Child Care	9.3	9.2	9.2	.1	0