NATIONAL CENTER FOR EDUCATION STATISTICS

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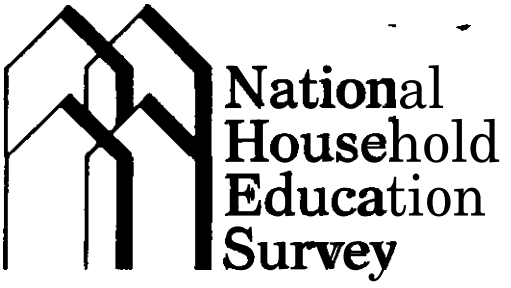
National Household Education Survey of 1993

Adjusting for Coverage Bias Using Telephone Service Interruption Data



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Introduction

The National Household Education Survey (NHES) is a data collection system of the National Center for Education Statistics (NCES), which has as its Legislative mission the collection and publication of data on the condition of education in the Nation. The NHES is specifically designed to support this mission by providing information on those educational issues that are best addressed by contacting households rather than schools or other educational institutions. The NHES provides descriptive data on the educational activities of the U.S. population and offers policymakers, researchers, and educators a variety of statistics on the condition of education in the United States.

The NHES is a telephone survey of the noninstitutionalized civilian population of the U.S. Households are selected for the survey using random digit dialing (RDD) methods, and data are collected using computer-assisted telephone interviewing (CATI) procedures. These procedures provide a cost effective means for quickly surveying households with telephones. Approximately 60,000 households are screened for each administration, and individuals within households who meet predetermined criteria are sampled for more detailed or extended interviews. The data are weighted to permit estimates of the entire population. The NHES survey for a given year typically consists of a Screener, which collects household composition and demographic data, and extended interviews on two substantive components addressing education-related topics. In order to assess data item reliability and inform future NHES surveys, each administration also includes a subsample of respondents for a reinterview,

The primary purpose of the NHES is to conduct repeated measurements of the same phenomena at different points in time, although one-time surveys on topics of interest to the Department of Education are also conducted. This has been done by repeating topical components on a rotating basis to provide comparative data across survey years. In addition, each administration of the NHES has benefited from experiences with previous cycles, resulting in enhancements to the survey procedures and content. Thus, while the survey affords the opportunity for tracking phenomena across time, it is also dynamic in addressing new issues and including conceptual and methodological refinements.

A new design feature of the NHES program implemented in the NHES:96 is the collection of demographic and educational information on members of all screened households, rather than just those households potentially eligible for a topical component. In addition, this expanded screening feature includes a brief set of questions on an issue of interest to education program administrators or policymakers. The total Screener sample size is sufficient to produce state estimates of household characteristics for the NHES:96.

Full-scale implementations of the NHES have been conducted in 1991, 1993, 1995, and 1996. Topics addressed by the NHES:91 were early childhood education and adult education. The NHES:93 collected information about school readiness and school safety and discipline. The 1991 components were repeated for the NHES:95, addressing early childhood program participation and adult education. Both components underwent substantial redesign to incorporate new issues and develop new measurement approaches. In the NHES:96, the topical components are parent/family involvement in education and civic involvement. The NHES:96 expanded screening feature includes a set of questions on public library use.

In addition to its topical components, the NHES system has also included a number of

methodological investigations. These have resulted in technical reports and working papers covering diverse topics such as telephone undercoverage bias, proxy reporting, and sampling methods. This series of technical reports and working papers provides valuable information on ways of improving the NHES, and may be useful to survey researchers more generally.

This report is a continuation of research on issues related to biases that result from the inability to survey persons who live in households without telephones. Two of the earlier NHES technical reports (Brick, Burke, and West 1992; Brick and West 1992) addressed this important subject. Another bias study involved adding certain questionnaire items to the NHES:93 to evaluate a different method of adjusting the estimates to reduce the bias associated with sampling only persons living in households with telephones. The method involves using data on interruptions of telephone service to adjust the weights of the respondents to the survey. The weights for households that report experiencing some periods of not having telephone service during the twelve months prior to the interview are increased whereas households reporting no breaks in telephone service receive their normal weights, The assumption behind this procedure is that households with interrupted telephone service are *more* like those without telephones than other telephone households. Although the goal of these adjustments is to reduce the bias due to excluding households without telephones at the time of the survey, a consequence of the adjustments is that the variances of the estimates increase. This analysis examines the benefits of the bias reduction in light of the variance increases and suggests situations in which the adjustments might be beneficial

The next section provides background information on telephone coverage bias, its implications for estimates from a survey such as the NHES, and previous research using data on telephone service interruptions to reduce coverage bias. Subsequent sections describe the estimates from the NHES:93 of the percentage of persons that experienced some interruption of telephone service, the procedures used to adjust the survey weights using these data, and the statistical implications of using the adjusted weights. The final section summarizes the findings and contains recommendations for use of this technique.

Background

Telephone surveys provide a relatively economical method of data collection compared with personal interviewing. However, telephone surveys are subject to art important source of bias that does not affect household surveys conducted with face-to-face interviewing only 94[1] percent of households nationally have telephone service at any given time. Moreover, for the children surveyed for the two components of the NHES:93, coverage rates are lower than 94 percent. Indeed, persons under 6 years of age have the lowest telephone coverage rate of all age groups in the U.S. (Thornberry and Massey 1988).

Weighting that includes poststratification based on demographic variables known to be associated with telephone coverage is effective in mitigating some of the consequences of coverage bias in telephone surveys generally, and has been shown to do so for marry items in the NHES (Brick, Burke, and West 1992).[2] But even when effective, weighting to known demographic totals dues not completely solve the problem of coverage bias. It undercompensates for some

[1] Estimate based on tabulations from the March, July, and November 1992 Current Population Survey.

[2] Postsurvey weighting is also used to compensate for nonresponse and other biases.

variables (Massey and Botman 1988) and overcompensates for others (Brick, Burke, and West 1992).

This report describes a study of an alternative method for adjusting telephone survey data to compensate for coverage bias. This method is based on the observation that telephone subscription not only varies across households in the population, but also within households over time. Keeter (1995) discusses this idea in some depth and demonstrates that a sizable number of U.S. households lose and gain telephone service during a given year. Because of this phenomenon, the telephone population at a given time includes households that recently were in the nontelephone population and excludes some households that were recently in the telephone population. Thus, weighting adjustments that use the data from households that have telephones only sometimes during the year might bean improvement over the current practice.

Despite considerable information on the size and characteristics of the nontelephone population, little is known about its dynamics over shorter time periods. Evidence from social workers, telephone companies, and others who deal with indigent households suggests that for marry families, telephone subscription is episodic. They have a telephone when they can afford it; the telephone is turned off when times are harder, when the bills get too large to manage, or both (Federal Communications Commission 1988). It is not known how many households change their telephone status and how long they stay in a particular status.

Keeter (1995) examined two household panel surveys to obtain estimates of the dynamics of telephone service subscription. Those households that changed telephone status (presence of a telephone in the household) from one wave to the next of the survey are called 'transient' households. For data collected 12 months apart, half of the 6 percent of all households without a telephone at either time were transient. For data collected only two months apart, one-fourth of the 6 percent of households without telephones at either point in time were transient. Since these estimates were based on observations at two points in time rather than continuous measurement, they underestimate the percent of households that are transient. Nevertheless, these results show that a substantial proportion of households without a telephone at a specific point in time are transient.

Another important condition that must be satisfied if the transient telephone households are to be useful in reducing coverage bias involves the characteristics of transient households and households without telephones at the time of the interview. If the two groups are not similar, then the adjustments will not be effective. Using the panel data and data from several Virginia surveys, Keeter (1995) showed that the characteristics of the transient households are more consistent with the nontelephone households than telephone households. Preliminary results on this comparison were presented by Keeter in a paper at the 1992 meetings of the American Association of Public Opinion Research. This presentation was the catalyst for the inclusion of the items on the interruption of telephone service in the NHES:93.

Estimates of interruptions of Telephone Service

In the NHES:93, 64,000 households completed the screening interview and nearly 30,000 interviews were conducted within those screened households. Two survey components were included: School Readiness (SR) and School Safety and Discipline (SS&D). Approximately 11,000 parents of 3- to 7-year-olds completed interviews on SR topics, including developmental characteristics of preschoolers,

school adjustment and teacher feedback to parents for kindergartners and primary students, home activities with family members, and health status. About 12,700 parents of children in grades 3 through 12 and about 6,500 youth in grades 6 through 12 were interviewed for the SS&D component. The topics for this component included the school learning environment, safety at school, and availability and use of tobacco, alcohol, and other dregs at school. For both components, characteristics of the family and household background information were collected.

The SR component included the 20 million children between the ages of 3 and 7 years as of December 31, 1992, and all other children through age 9 who were enrolled in kindergarten, first, or second grade. The SS&D component included the 35 million students in grades 3 through 12. The estimates of the population were derived from the October 1992 Current Population Survey (CPS).

For all households that completed an interview, one parent was asked if the household had experienced an interruption in telephone service in the last 12 months, where an interruption is any 24 hour period without telephone service. If the respondent said yes, he or she was asked how many days, weeks, or months the household was without service. This question was asked only of one parent in the household, even if there were multiple interviews in the household. (See Exhibit 1 for the interview questions.) The responses[3] to these items are the basis for the study of the effects of adjustments for telephone coverage discussed in the rest of this report.

Since the responses to these questions in the NHES:93 were only obtained for those households that completed either an SR or SS&D interview, this has implications for the analysis of the results. The data presented below pertain only to persons in certain households: those in which there was at least one child from preschool age (at least 3 years old) to the end of high school. Since the two eligible populations in the NHES:93 are not overlapping, the estimates are presented separately for the SR and SS&D children. In addition, the estimates are of children rather than households. This is an important distinction. Since the estimates from the NHES:93 generally refer to children rather than households, the impact of the coverage adjustment should he measured at the person level rather than the household level.

The estimated percentage of SR children in households that had a telephone interruption of 1 day or more was 12 percent of all children in telephone households at the time of interviews, while it was only 9 percent for the SS&D children. This estimated difference in the percentage with telephone service interruptions between the two populations is consistent with estimates that find lower telephone penetration for younger children. Thornberry and Massey (1988) reported that 12.3 percent of children under 6 years were in nontelephone households while only 8.5 percent of those 6 to 16 years were in nontelephone households.

Figure 1 shows the estimated percentage of persons in each population who bad a service interruption by the length of the interruption. The vertical lines in the figure arc 95 percent confidence intervals on the estimated percentages. Intervals constructed using these methods include the population value in 95 percent of all possible samples that could be selected. Both populations exhibit roughly the same pattern in the estimates by length of service. A substantial proportion of those with interruptions in telephone service experience only short interruptions of less than 1 week.

[3] The imputed responses were used for records with missing values. Only 123 of the 10,888 SR and 71 of the 12,680 SS&D values were imputed.

Exhibit 1
Telephone Interrupt Items

1. During the past 12 months, has your household ever been without telephone service for more than 24 hours?

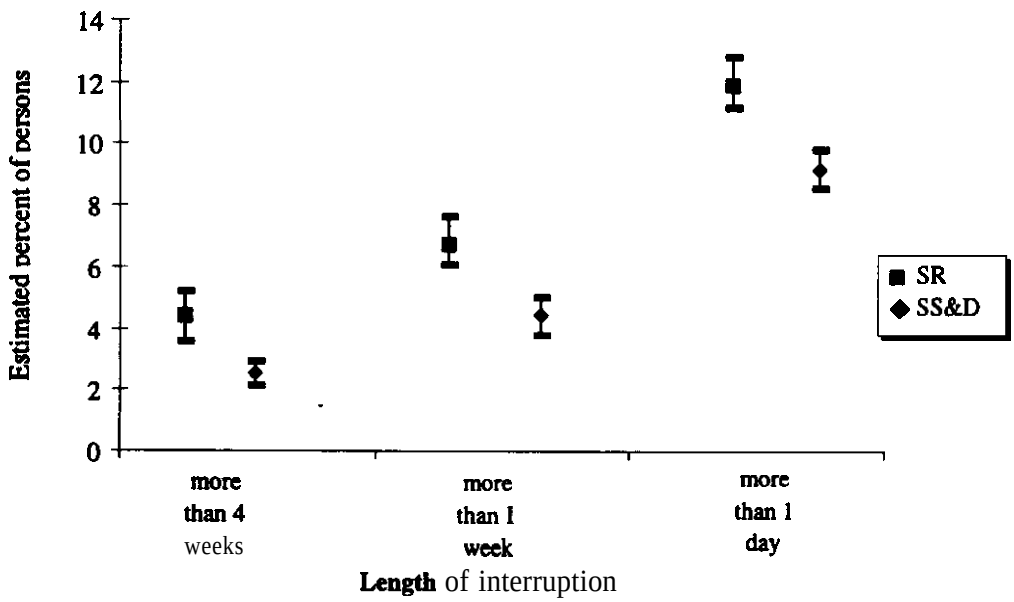
YES ..... 1 (GO TO 2)
NO 2
REFUSED ..... -7
DON'T KNOW ..... -8

2. What was the total amount of time your household was without telephone service in the past 12 months?

NUMBER ..... ☐☐

DAYS 1
WEEKS ..... -2
MONTHS ..... -3

Figure 1.-- Estimated 95 percent confidence intervals of the percentage of persons with interrupted telephone service during the previous 12 months, by length of interruption



SOURCE U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993

Characteristics of Those With Service Interruptions

The estimated percentage of children in households with any interruption in service is given in table 1 for the SR population and table 2 for the SS&D population. Characteristics collected for both populations are the first items shown in the tables.

The purpose of examining the telephone interruption estimates by the characteristics of the children is to evaluate the potential of using the data to adjust for nontelephone coverage bias. If the percentages of persons in households with telephone service interruptions are nearly the same for all persons across the characteristics, then little could be expected from using the items to reduce coverage bias.

The percentage distributions of persons with some interruption for the nine common items are relatively consistent for the SR and the SS&D populations. All of the items, except Census region and community mobility in the ZIP Code area, exhibit variation[4] in the percentage with interruptions. The characteristics associated with lower economic status have the highest percentage with interruptions in all of these common items: the percentage of black and Hispanic children in households with interruptions is higher than for white children; the percentage of households with interruptions for those renting is higher than for those who own; the percentage with household incomes less than $20,000 is higher than for those with larger incomes; the percentage for those from households with lower parental education levels (only completed high school or less) is higher than for those from households with higher parental education levels (college graduate or more); and the percentage of those living in ZIP Code areas with median household incomes of $15,000 or less is higher than for those in areas with household incomes of $25,000 or more.

The remaining items in table 1 were designed to address specific substantive issues associated with school readiness. For most of the items, the percentages of children in households with telephone interruptions are not statistically significant for different levels of the variables. The most striking differences are for the estimates of those respondents participating in Women, Infant, and Children (WIC) program and whose children participate in the free meal or lunch program at school. The higher estimates of the percentage with telephone interruptions for children participating in these programs designed for the economically disadvantaged is consistent with the estimates for the other economically related items. For other substantive items, the differences in the percentage of persons with some interruption in telephone service are statistically significant, but not large enough to be of great practical importance. For example, the difference in the percentage of children in kindergarten or primary school who attended a center-based program prior to school is statistically significant, but the estimates differ by less than 3 percent. A difference of this size may not be important for adjusting for coverage bias.

The estimates for the SS&D population in table 2 are similar. The differences across response categories for most of the items are either not statistically significant or so small that they have little practical importance. For example, the difference between children in public and private school is less than 2 percent and not likely to he important for adjustment purposes.

The estimates in tables 1 and 2 support the hypothesis that the chance of having telephone service interruptions is related to the economic situation in the household. Since race/ethnicity and economic status are highly correlated, a

[4]The statements in this report were tested at the 5 pa-cent significance level. Bonferroni adjustments were made to compensate for multiple levels of the response variables.

relevant question is whether the differences in the percentage of persons with telephone interruptions would be significant across categories of race/ethnicity after controlling for other variables related to economic status, such as household income or parental education since poststratification by income or education is frequently used in RDD surveys. To examine this, the percentage of the SR population with an interruption in telephone service in the last year by race/ethnicity and two categories of parental education was estimated and is shown in Figure 2. The figure shows 95 percent confidence intervals along with each of the point estimates. If education level accounted for all of the variability in the percentage with interruptions, then all three low education estimates should be equal and all three of high education estimates should be equal within sampling error. Clearly, this is not the situation; the estimates for blacks and Hispanics at the high parental education level are greater than the estimates for the nonblack, non-Hispanics. Race/ethnicity is art important correlate *even* after controlling for parents' education level. Tabulations controlling for household income instead of parental education were also prepared, and the estimates are also statistically significant. The same results also hold for the SS&D population controlling for either parental education or household income.

These findings indicate that interruptions in telephone service as estimated from the NHES:93 do vary by economic and demographic characteristics that have been identified as important correlates of telephone coverage. This condition is necessary for the telephone interruption data to be useful in adjusting for telephone coverage bias. The lack of important differences for marry of the substantive items, especially in the SS&D population, suggests the value of the adjustment may be less important for estimates of the substantive items.

Weighting Adjustments

In most sample surveys, the data collected from respondents are processed to make the estimates more representative of the population surveyed. A typical operation is to attach a survey weight to each observation and use these weights in the preparation of estimates. The weights are often the product of several steps. A base weight that is the reciprocal of the probability of including the respondent in the sample is first attached to each record. The base weight is then adjusted to account for nonresponse and noncoverage and to reduce the variability in the estimates by using auxiliary data.

Kalton and Kasprzyk (1986) discuss adjustments to the base weights, classifying the adjustments into four categories: population weighting adjustments, sample weighting adjustments, raking ratio adjustments, and response probability adjustments. In the NHES:93, sample weighting adjustments and raking ratio adjustments were used. Sample weighting adjustments were used to account for differential nonresponse from sampled persons. Raking ratio adjustments were then used to make the specified marginal distributions of the sample correspond to totals from the October 1992 CPS. One of the most important benefits of the type of raking ratio adjustment used in the NHES:93 is that it reduces the bias associated with the undercoverage of persons living in households without telephones because the CPS covers persons in both telephone and nontelephone households.

Table 1.-- Estimated percentage of persons in the School Readiness population[1] with interruptions in telephone service in last 12 months, by selected characteristics

Characteristic	Estimate	Standard error
Total	12.0	0.4
Race/ethnicity		
White, non-Hispanic	9.3	0.5
Black, non-Hispanic	19.8	1.5
Hispanic	17.2	1.5
Other	11.7	2.6
Tenure		
Own/other	7.9	0.5
Rent	18.4	1.0
Household income		
$10,000 or less	22.8	1.3
$10,001 to $20,000	19.9	1.4
$20,001 to $30,000	9.3	0.8
More than $30,000	5.5	0.5
Parental educational level		
Less than high school graduate	18.4	1.8
High school graduate or equivalent	15.4	0.8
Vocational/some college	11.8	0.7
College graduate	5.5	0.8
Graduate school	5.2	0.7
Mother's employment status		
No mother in household	17.6	3.5
Employed 35 hours/week or more	10.1	0.7
Employed less than 35 hours/week	9.6	0.9
Seeking employment	20.7	2.2
Not in labor force	13.1	0.7
Father's employment status		
No father in household	18.2	1.1
Employed 35 hours/week or more	8.7	0.4
Employed less than 35 hours/week	15.4	2.6
Seeking employment	19.6	3.4
Not in labor force	14.8	2.6
Census region		
Northeast	9.5	1.2
South	13.6	0.7
Midwest	11.1	1.0
West	12.5	0.9
Median household income in ZIP Code		
$15,000 or less	18.3	3.0
$15,000 to $25,000	15.8	0.9
More than $25,000	9.9	0.5
Mobility in ZIP Code		
High	13.0	1.9
Medium/high	13.1	1.3
Medium	12.2	0.9
Medium/low	11.0	0.8
Low	11.6	1.4
Time since doctor visit for routine care		
Less than 1 year	11.8	0.5
Over 1 year	13.1	1.0

Table 1.-- Estimated percentage of persons in the School Readiness population[1] with interruptions in telephone service in last 12 months, by selected characteristics (continued)

Characteristic	Estimate	
Birth weight		
5.5 pounds or less	12.0	1.6
Greater than 5.5 pounds	12.0	0.4
Child attending center-based program[2]		
Yes	9.3	0.7
No	13.7	1.1
Child ever attended center- based program[2]		
Yes	10.5	0.7
No	13.0	1.2
Child ever attended center-based program prior to school[3]		
Yes	11.7	0.6
No	14.4	1.2
Women, Infant, and Children program participant[2]		
Yes	18.2	1.3
No	8.0	0.6
Free meal at school or center[3]		
Yes	21.1	1.2
No	7.6	0.5
Repeated kindergarten[4]		
Yes	15.7	3.5
No	11.7	0.6
Family member read to child in last week[5]		
Not in last week	21.9	5.4
Once or twice	11.9	0.8
Three or more times	11.5	0.9
Family member taught child letters or words in fast week[5]		
Not in last week	12.7	1.8
Once or twice	10.5	Lo
Three or more times	12.6	0.6
Family member taught child songs or music in last week[5]		
Not in last k	12.3	1.0
Once or twice	11.1	0.9
Three or more times	12.4	Lo
Family member did arts or crafts with child in last week[5]		
Not in last week	14.9	1.0
Once or twice	10.9	0.9
Three or more times	10.4	0.9
Family member visited library with child in last month[5]		
Yes	10.2	0.8
No	13.2	0.7
Family member visited zoo with child in last month[5]		
Yes	10.6	1.3
No	12.3	0.6

[1]The SR population is approximately 20 million children from 3 to 7 years old.

[2]Estimate restricted to preschoolers.

[3]Estimate applies to all children except preschoolers.

[4]Estimate restricted to children in primary school.

[5]Estimate applies to all children except those in primary school.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household EducatiSurvey, spring 1993.

Table 2.-- Estimated percentage of persons in the School Safety and Discipline population[1] with interruptions in telephone service in last 12 months, by selected characteristics

Characteristic	Estimate	standard error
Total	9.2	0.3
Race/ethnicity		
White, non-Hispanic	7.2	0.3
Black, non-Hispanic	14.7	1.1
Hispanic	14.1	1.1
Other	9.3	1.5
Tenure		
Own/other	6.6	0.3
Rent	15.3	0.8
Household income		
$10,000 or less	19.0	1.3
$10,001 to $20,000	15.7	1.1
$20,001 to $30,000	7.9	0.6
More than $30,000	5.0	0.3
Parental educational level		
Less than high school graduate	17.4	1.6
High school graduate or equivalent	11.0	0.8
Vocational/some college	8.6	0.5
College graduate	5.3	0.8
Graduate school	4.5	0.6
Mother's employment status		
No mother in household	12.8	1.9
Employed 35 hours/week or more	8.4	0.5
Employed less than 35 hours/week	7.8	0.6
Seeking employment	15.1	1.6
Not in labor force	10.3	0.7
Father's employment status		
No father in household	12.9	0.9
Employed 35 hours/week or more	6.8	0.3
Employed less than 35 hours/week	14.6	2.6
Seeking employment	17.3	2.5
Not in labor force	13.8	1.6
Census region		
Northeast	9.0	0.8
South	10.8	0.6
Midwest	7.3	0.7
West	9.2	0.8
Median household income in ZIP Code		
$15,000 or less	15.4	2.1
$15,000 to $25,000	11.6	0.8
More than $25,000	7.7	0.3
Mobility in ZIP Code		
High	7.6	1.7
Medium/high	9.7	0.9
Medium	9.1	0.6
Medium/low	9.2	0.5
Low	9.6	1.0
School control		
Public	9.4	0.4
Private	7.5	1.1
Visitors required to sign in at school		
Yes	9.4	0.4
No	8.6	0.7

Table 2.-- Estimated percentage of persons in the School Safety and Discipline population[1] with interruptions in telephone service in last 12 months, by selected characteristics (continued)

Characteristic	Estimate	Standard error
Students in fighting gangs at school[2]		
Yes	9.5	0.8
No	8.8	0.5
Ease of obtaining marijuana at school[2]		
Very or fairly easy	9.7	0.6
Hard	8.0	0.8
Nearly impossible	9.0	0.7
Had drug or alcohol ed program this year		
Yes	8.5	0.4
No	10.9	0.7
Fear of incident of crime at school		
None	8.7	0.4
Fear of theft or robbery[3]	9.4	1.0
Fear of bullying or assault[3]	11.5	1.2
Fear of two or more types of incidents[3]	10.6	0.9
Knowledge of crime at school		
None	8.9	0.5
Knowledge of theft or robbery[3]	7.5	0.8
Knowledge of bullying or assault[3]	10.8	0.9
Knowledge of two or more types of incidents[3]	9.6	0.5
Victimization by crime		
Not victimized	8.6	0.4
Victim of theft or m*	10.8	1.0
Victim of bullying or assault[3]	11.2	1.2
Victim of two or more types of incidents[3]	10.6	1.2
Witnessed crime at school		
None	8.8	0.5
Witnessed robbery	8.5	4.1
Witnessed bullying or assault[4]	10.1	0.5
Witnessed two or more of incidents		0.9

[1] The School Safety and Discipline population is approximatel y 35 million students in grades 3 through 12

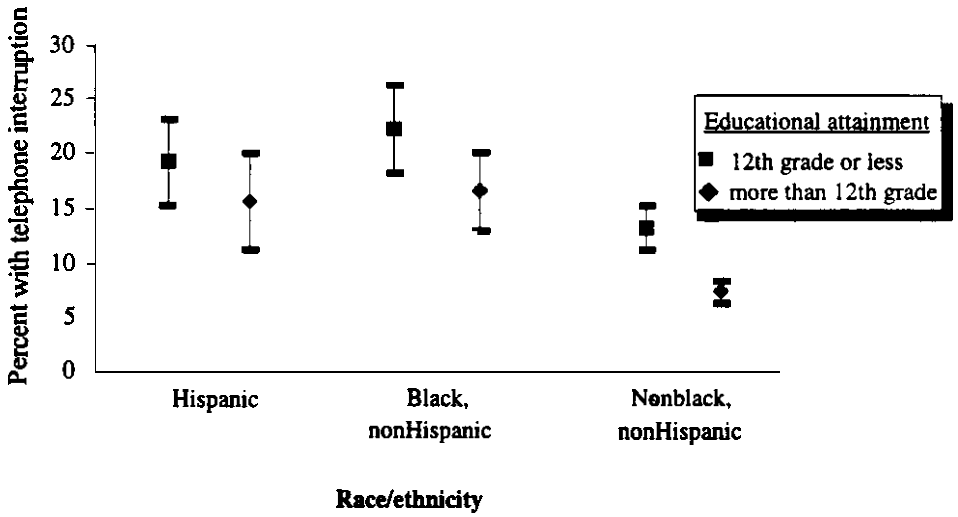
[2] Only asked for students in grades 6 through 12.

[3] For the fear of incident, knowledge of crime, and victimized by crime variables, the seared response category is used if either theft or robbery was reported but not both, the third response category is used if either bullying or assault was reported but not both.

[4] This response category is used if either bullying or assault was reported, but not both.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

Figure 2.-- Estimated 95 percent confidence intervals of the percentage of School **Readiness** population with service **interruption**



SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

The data on telephone service interruptions cart be used to make a response probability adjustment. Response probability adjustments are constructed by assuming that each sampled unit has a probability of responding to the survey, estimating that probability, and then using the inverse of the response probability as a weighting adjustment. The Politz and Simmons (1949) method is probably the best known application of the response probability adjustment procedure.

To apply this type of adjustment with the telephone service interruption data, assume that living in a telephone household is a dynamic phenomenon, and that a probability distribution can be associated with this status. Conceptually, a survey is conducted by sampling from this distribution and observing only those members that live in telephone households at the time of the survey. The probability of living in a telephone household must then be estimated for each respondent. The inverse of the estimated probability is the coverage adjustment. This model assumes that each person can be assigned a probability of being in a household with a telephone and that the probability is between zero and one (but not equal to zero).

For this analysis, the data on whether or not a household bad an interruption in telephone service and the length of that interruption are the basis for an adjustment, using methods suggested by Keeter (1995). Persons are divided into two categories: those in households with interruptions in service and those in households without interruptions in service. The probability is assumed to be one for persons in households without interruptions and their weights are not adjusted. The weights of persons in households with at least some interruptions in the last 12 months are adjusted to account for other households that have a probability of being covered of less than one. The adjustments may vary depending on the length of time they lived in nontelephone households and on other characteristics of the household. The purpose of having different adjustments is to account for the fact that some persons are more likely to live in nontelephone households than others.

Although the weighting adjustments may reduce the undercoverage bias, introducing adjustments

also typically increases the variances of the estimates. Kish (1992) discusses the reasons for unequal weights as well as the consequences from using them in a variety of situations. He advocates a common statistical approach of balancing the effect of the adjustments in reducing the bias of estimates against the increases in the variances of the estimates. If the weights reduce the bias of the estimates significantly, then it may be worthwhile accepting the variance increases. On the other hand, small reductions in bias associated with large variance increases are not recommended.

In the remainder of this section, the specific weighting adjustment procedures examined using the telephone service interruption data are described. The methods for creating the adjustments and applying them to the NHES:93 are presented in some detail. The statistical properties of the weights developed under four alternative adjustment scenarios are presented. The alternative weights are applied to the NHES:93 data and the estimated decrease in the bias of the estimates is compared with the increase in the variance of the estimates due to the unequal weighting.

Adjustment Schemes

The first step was to decide how to classify the length of interruption in telephone service. Various lengths of interruptions were examined to determine cut-offs which appeared to distinguish between temporary interruptions, not due to economic causes and others. It was decided to use two categories. for forming adjustment cells: 1 week or more and 1 month or more. When tables like tables 1 and 2 were created using the 1 week or more and 1 month or more criterion rather than any interruption, the estimates for the transients were still highly related to the economic and demographic variables identified in tables 1 and 2. A category for interruptions of less than 1 week was not used for adjustment because short-term interruptions may have been caused by factors, such as temporary weather-related outages, that are different from the longer term interruptions.

Within each of the length-of-service interruption categories, the children were classified into adjustment cells based on either parental education or tenure (home ownership). Race/ethnicity was used to form cells within the parental education and tenure categories. These cells were chosen because the percentage of persons with interruptions varied by these characteristics and the corresponding data were also available from the CPS. Four adjustment schemes were defined using these items:

- scheme Al-children in households that had a telephone service interruption of 1 week or more within categories defined by parental education (less than high school, high school diploma, college diploma or above) and race/ethnicity (Hispanic, black/non-Hispanic, white and other/non-Hispanic);
- Scheme A2---children in households that had a telephone service interruption of 1 month or more within categories defined by parental education and race/ethnicity;
- Scheme B1---children in households that had a telephone service interruption of 1 week or more within categories defined by tenure (own/other, rent) and race/ethnicity; and
- Scheme B2---children in households that had a telephone service interruption of 1 month or more within categories defined by tenure and race/ethnicity.

In these schemes, the children classified as living in households with interruptions of one month or more (A2 and B2) are a subset of those classified as having interruptions of one week or more. In other words, if the weight for a child was adjusted under scheme A2 or B2 it was also adjusted under scheme Al or B 1.

The adjustment factors for these schemes could not be obtained directly from the NHES:93 data because no data were collected from households without telephones. Instead, the adjustments were developed from CPS data and then applied to the NHES:93 weights, following the idea suggested by Keeter.

To explain the adjustment of the weights under the response probability model assumptions, consider petitioning the universe of persons at the time of the interview into four components: t_1 is the number of persons in *telephone* households with no *telephone interruptions* in the past year; t_2 is the number of persons in *telephone* households with *some telephone interruptions* in the past year; t_3 is the number of persons in *nontelephone* households with no *telephone interruptions* in the past year; i.e., persons who lived in nontelephone households throughout the entire year and t_4 is the number of persons in *nontelephone* households with some *telephone interruptions* in the past year. As noted above, the response probability model assumes $t_3=0$, *i.e., no* persons live in nontelephone households for the entire year. This assumption is clearly not true, but there are no sources to estimate the size of t_3. However, under the response probability model assumed all t_3 persons are included in the t_4 population.

Using the March 1992 CPS it is possible to estimate t, $+t_2$ (but not the separate quantities) and t_4; designate these estimates as $\hat{t}_1+\hat{t}_2$ and $\hat{t}_4$, respectively. Notice that $\hat{t}_4$ includes persons currently living in households without telephones, regardless of whether they had an interruption in service in the last year. Thus, it includes the t_3 population. From the NHES:93, t_1 and t_2 can be estimated separately; call these estimates t_1^* and t_2^*, respectively. The bias in the NHES:93 estimates arises because they do not include persons in nontelephone households (t_4). The goal is to reduce this bias by adjusting the NHES:93 weights of those persons living in telephone households with some telephone interruption.

A weight adjustment of $A=1+t_4/t_2$ would result in unbiased estimates of totals under the response probability model, since this model assumes t_4 and t_2 are members of the same population but in different telephone status at the time of the interview. However, this adjustment involves quantities that are unknown and must be estimated. Since t_2 can only be estimated separately from the NHES:93 and t_4 can only be estimated from the CPS, the adjustment is expressed in ratios to reduce the bias due to estimating the quantities from different surveys. The revised weight is

$$w_i = w_i\left(1+\delta_i\frac{\hat{t}_4}{\hat{t}_1+\hat{t}_2}\Big/\frac{t_2^*}{t_1^*+t_2^*}\right), \qquad (1)$$

where w_i is the NHES:93 weight adjusted for nonresponse of sampled persons but not yet raked to October 1992 CPS totals, $\delta_i=1$ if the person lives in a household that had an interruption of telephone service in the last year and is zero otherwise. The quantity in parenthesis in (1) is the weight adjustment.

Revised weights were computed separately for the SR and SS&D components, since these were handled as separate surveys. Rather than the overall adjustment as given in (1), the weight adjustments were computed within the cells defined for each of the four weighting schemes (A1, A2, B 1, and B2). Table 3 shows the resulting adjustment factors for the SR and SS&D components. The adjustments in the first column am those for schemes A 1 and B 1. The second column contains the adjustment factors for schemes A2 and B2. The adjustment factors for the schemes based on the 1 month or more

interruptions are greater than those based on the 1 week or *more* because the denominator of the ratio is smaller for this classification.

The last weighting step rakes the four alternative weights to the same October 1992 CPS totals used in raking the standard NHES:93 person-level weights. The result of this process is the standard NHES:93 weight and four alternative weights based on different adjustment schemes. All five of the weights conform to the same marginal totals. The only difference in the weights is the adjustment for the telephone service interruption.

Effect of Adjustments on Variance

As discussed before, the adjustment of the weights to reduce the bias increases the variability of the weights and the variance of the estimates. Kish (1992) gives an approximate expression for this increase in variance due to having weights that are not equal, the variance inflation factor (*VIF*). The *VIF* is a reasonable approximation if the population element variances of the persons sampled at different rates are roughly equal. The *VIF can be* written as

$$VIF = 1 + CV^2(\text{weights}) \qquad (2)$$

where *CV* is the coefficient of variation of the weights.

Table 4 shows the *VIF* for the standard NHES:93 weights for each component. The SS&D component is broken down by the grade of the student, because youth were selected at different rates for these grade levels. Only one *VIF* is presented for the SR component because all children were sampled at the same sampling rate. The *VIF* for each of the components is about 1.4, indicating the variance is inflated by about 40 percent due to the variability in the standard weights. The *VIF* for the combined SS&D file is somewhat larger (1.5) because it includes youth sampled at different rates. The *VIFs* for many subdomains of children should be well approximated by the *VIF* for the full component.

The last four columns in table 4 are the ratios of the *VIF* for the four alternative weights to the *VIF* for the standard weight. These ratios show bow much greater the variances of estimates produced using the alternative weights are expected to be as compared to the variances of the standard NHES:93 weights.

Overall, the increase in variance due to the telephone interruption coverage adjustment are from 9 to 13 percent for schemes A 1 and B 1 in the SS&D component but up to 20 percent for the SR component. The ratios are larger for the schemes A2 and B2, ranging from 24 to 35 percent, with the largest ratio for Scheme A2 for the SR component. The larger ratios (hence *VIFs*) for the schemes based on interruptions of 1 month or more are a consequence of the larger and more variable factors shown in the second column of table 3. The ratios for the SR population are higher than the SS&D ratios.

Table 3.--Weighting cell adjustments factors, based on length of interruption of telephone service

Factor	Length of service interruption	
	1 week or more	1 month or more
School Readiness		
Cells defined by parental education and race/ethnicity (scheme A)		
Less than high school diploma; Hispanic	5.75	16.35
Less than high school diploma; black, non-Hispanic	5.10	6.72
Less than high school diploma; white and other, non-Hispanic	4.98	5.37
High school diploma; Hispanic	2.31	2.76
High school diploma; black, non-Hispanic	2.65	3.73
High school diploma white and other, non-Hispanic	2.16	2.79
Bachelor's degree or higher; Hispanic	1.34	2.33
Bachelor's degree or higher; black, non-Hispanic	1.77	2.64
Bachelor's degree or higher; white and other, non-Hispanic	1.58	2.09
Cells defined by tenure and race/ethnicity (scheme B).		
Rent Hispanic	3.74	5.15
Rent; black, non-Hispanic	3.23	4.54
Rent; white and other, non. Hispanic	2.43	2.96
Own/other; Hispanic	2.00	3.06
Own/other; black, non-Hispanic	2.53	3.46
Own/other; white and other, non-Hispanic	2.26	3.45
School Safety and Discipline		
Cells defined by parental education and race/ethnicity (scheme A)		
Less than high school diploma; Hispanic	4.89	8.52
Less than high school diploma; black, non-Hispanic	4.26	5.95
Less than high school diploma; white and other, non-Hispanic	3.81	4.86
High school diploma; Hispanic	2.67	4.51
High school diploma; black, non. Hispanic	3.06	4.71
High school diploma; white and other, non-Hispanic	2.18	3.09
Bachelor's degree or higher; Hispanic	1.96	8.22
Bachelor's degree or higher; black, non-Hispanic	1.35	8.83
Bachelor's degree or higher; white and other, non-Hispanic	1.91	3.48
Cells defined by tenure and race/ethnicity (scheme B)		
Rent Hispanic	3.58	6.08
Rent; black, non-Hispanic	3.38	4.95
Rent; white and other, non-Hispanic	2.99	4.00
Own/other; Hispanic	2.81	5.66
Own/other; black, non-Hispanic	2.90	6.11
Own/other; white and other, non-Hispanic	2.03	3.10

SOURCE U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993

Table 4.--Ratios of variance inflation factor due to coverage adjustment

Component	Sample size	*VIF** standard weight	Ratio of scheme's *VIF* to standard weight's *VIF*			
			Scheme AI	Scheme A2	Scheme B I	Scheme B2
School Readiness	10,888	1.36	1.20	1.35	1.16	1.26
School Safety and Discipline						
3rd through 5th graders	2,563	1.37	1:12	1.25	1.13	1.26
6th through 12th graders	10,117	1.39	1.13	1.27	1.09	1.24
3rd through 12th graders.....	12,680	1.49	1.12	1.26	1.11	1.25

● VIF is the standard inflation factor. It is the coefficient of variation of the weights _ plus one.

SOURCE U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993

Effect of Adjustments on Mean Square Error

In this section, the adjusted weights are applied to estimate the characteristics from the SR and SS&D components. Since four adjustments were constructed, five different estimates are computed: one from the standard NHES:93 weights and one for each of the four adjusted weights. The only difference in the methods used to compute the estimates for the five weights is the coverage adjustment. All five sets of weights include the final raking adjustment of the estimates to the totals from the CPS. Therefore, even the standard estimates are adjusted to account for undercoverage, but they do not have the telephone interruption adjustment.

The discussion of the alternative weighting schemes begins by considering the reduction in coverage bias, the difference between the estimate and the value that would have been obtained if households without telephones could have been surveyed. The bias corrections am then compared to the increase in variance associated with the adjustment procedures.

Coverage Bias Reduction

If estimates of the same characteristics as those produced from the NHES:93 were available from an independent source and these benchmark estimates were free of telephone coverage bias, then it would be possible to compare the five estimates to the benchmark. The comparisons could be used to evaluate the bias in the standard NHES:93 estimates and the bias remaining in the other estimates after the coverage adjustments. However, benchmark estimates comparable to the estimates from the two components of the NHES:93 do not exist. Consequently, other methods are needed to assess the bias-reducing potential of the coverage adjustments.

Due to of the lack of a benchmark, some model assumptions are required to assess the effectiveness of the adjustments. For this evaluation it is assumed that the adjustment procedures eliminate the coverage bias. As a result of this assumption, the difference between the standard estimate and the adjusted estimate is an unbiased estimate of the coverage bias resulting from using the procedures. In practice, the coverage bias is not completely eliminated by

any of the adjustment procedures. Even if the model were correct, the bias reductions estimated from the data would still be subject to sampling error. Despite the problems with this assumption, it is necessary to obtain some idea of the effectiveness of the adjustment. If the adjustment eliminates the bias, the mean square errors of the adjusted estimates are equal to the variances of the estimates, with no contribution from coverage bias. Therefore, the model assumption is favorable to the adjustment process, positing the adjusted estimates to be unbiased (any difference between the standard and adjusted estimates are attributed to bias). The impact of this assumption is discussed critically after evidence of the effectiveness of the method is presented.

The estimate from each scheme can be compared to the standard NHES:93 estimate, and the difference between the standard estimate and the adjusted estimate is an estimate of the reduction in the coverage bias. With four adjusted estimates, four different estimates of bias reduction are possible. The estimated reduction in bias is

$$b_a = \hat{p}_s - \hat{p}_a, \qquad (3)$$

where b_a is the estimated bias reduction using adjustment scheme a (a = A1, A2, B1, or B2), $\hat{p}_s$ is the estimate of the proportion using the standard estimate, and $\hat{p}_a$ is the estimated proportion using adjustment scheme a.

The estimated reductions in bias under each adjustment weighting scheme are given in table 5 for the SR characteristics and table 6 for the SS&D characteristics. It is important to understand that the estimates are of the amount of bias reduction in the standard estimate, assuming each adjustment scheme reduces the coverage bias. For example, the estimated bias reduction in the standard estimate of the percentage of preschool children attending a center-based program is 0.9 percent if scheme A1 is the assumed benchmark, and 0.3 if scheme A2 is used instead. The standard NHES:93 estimate of this percentage is 52.6 percent, so the scheme Al weight results in an estimate of 51.7 percent and the scheme A2 estimate is 52.3 percent. The bias reduction estimates in tables 5 and 6 are shown for all the items in the earlier tables, except for those items that were used in raking.[5] Since the raking procedure forces the estimates to the given marginal totals for these items, the adjusted estimates and the standard estimate are all equal for these characteristics.

The bias reduction estimates for most of the items in tables 5 and 6 are less than 1 percent and consistent across the schemes. Before summarizing the estimates, it is important to realize that the total number of children is constant for all the estimates due to the raking of the estimates to the CPS totals. The estimated reductions in bias across different response categories of an item, therefore, must sum to zero (positive bias reductions in response categories must be balanced by a negative estimates for other categories). As a result, the estimate and the bias reduction for the last category of a variable can be deduced from the estimates from the other levels.

The fixed total number of children across response categories has two consequences. It creates a negative correlation in the estimated reduction in bias across response categories (resulting from the zero sum nature of the total) and gives a false impression of the number of independent pieces of information in the tabled values. For example, for a dichotomous variable, the bias estimates are perfectly negatively correlated (the estimate in one category is the negative of the estimate for the other category). Thus, there is only one independent estimate for a dichotomous item.

[5]Raking was done to marginal totals by age, grade, tenure (own, rent), Census region, race/ethnicity, and household income.

The approach taken to address this problem in summarizing the estimates is to delete the estimate for one of the response categories for each item. For example, in the SS&D component estimates in table 6, rather than include both the estimate of the bias for the percentage of students in public school (-0.1 in scheme A 1) and private school (0.1 in scheme A1), only the public school estimate is retained for the summary. The "no" response category for all items with "yes" and "no" response categories is deleted. For other types of variables the response category with the smallest estimate is deleted. Of the 19 items in table 5 with estimates for 53 response levels, only 34 (53-19) are retained for the summaries below; of the 14 items with 48 response level estimates in table 6, the estimates for 34 (48-14) response levels are included in the summaries.

Figure 3 presents the reduction in bias estimated using scheme A 1 for the SR characteristics, and figure 4 is the same representation for the SS&D items. The reduction in bias estimates presented are the absolute values of the bias estimates shown in tables 5 and 6 after deleting the estimates for one response level per item. For both components, the bias reductions are small. The largest absolute bias is 1.3 percent for SR and 0.9 percent for SS&D. The mean and median of the bias reductions and the absolute values of the bias reductions were also computed for each scheme and each component. For the SR component, the mean and median of the absolute value of the estimated bias reductions are between 0.2 and 0.4 percent. For the SS&D, the mean and median of the absolute values are between 0.1 and 0.3.

Table 5.-- Estimated reduction in bias and bias ratio for selected characteristics of the School Readiness component

characteristic	Standard estimate		Estimated reduction in bias[1]				Bias ratio[2]			
	Percent	standard error	Scheme A1	Scheme A2	Scheme B1	Scheme B2	Scheme A1	Scheme A2	Scheme B1	Scheme B2
Parental educational level										
Less than high school graduate	8.6	0.3	-1.7	-1.9	0.1	0.1	-5.7	-6.3	0.3	0.3
High school graduate or equivalent	33.9	0.8	0.4	0.3	-0.7	-1.0	0.5	0.4	-0.9	-1.3
Some college	57.5	0.7	1.3	1.6	0.6	0.9	1.9	2.3	0.9	1.3
Mother's employment status										
No mother in household	2.4	0.2	-0.1	-0.1	-0.1	-0.1	-0.5	-0.5	-0.5	-0.5
Employed 35 hours/week or more	34.3	0.5	0.5	0.8	0.2	0.5	1.0	1.6	0.4	1.0
Employed less than 35 hours/week	20.9	0.5	-0.1	-0.2	0.0	-0.2	-0.2	-0.4	0.0	-0.4
Seeking employment	6.6	0.4	0.0	-0.1	-0.1	-0.1	0.0	-0.3	-0.3	-0.3
Not in labor force	35.8	0.6	-0.4	-0.3	0.0	0.0	-0.7	-0.5	0.0	0.0
Father's employment status										
No father in household	26.3	0.5	-0.4	-0.6	0.0	-0.1	-0.8	-1.2	0.0	-0.2
Employed 35 hours/week or more	63.4	0.6	0.3	0.5	0.1	0.2	0.5	0.8	0.2	0.3
Employed less than 35 hours/week	3.8	0.3	0.0	-0.1	0.0	0.1	0.0	-0.3	0.0	0.3
Seeking employment	3.2	0.3	0.0	0.0	-0.1	-0.2	0.0	0.0	-0.3	-0.7
Not in labor farce	3.3	0.2	0.1	0.2	0.0	0.1	0.5	1.0	0.0	0.5
Median household income in ZIP Code										
$15,000 or less	4.0	0.4	-0.2	0.0	0.1	0.2	-0.5	0.0	0.3	0.5
$15,000 to $25,000	30.1	0.8	-0.6	-0.5	-0.3	-0.5	-0.7	-0.6	-0.4	-0.6
More than $25,000	66.0	0.8	0.8	0.6	0.4	0.4	1.0	0.7	0.5	0.5
Mobility in ZIP Code										
High	7.6	0.5	0.0	0.2	0.0	0.2	0.0	0.4	0.0	0.4
Medium/high	17.7	0.6	-0.1	-0.2	-0.3	-0.3	-0.2	-0.3	-0.5	-0.5
Medium	30.0	0.7	0.0	-0.1	0.0	0.0	0.0	-0.1	0.0	0.0
Medium/low	31.2	0.8	0.3	0.3	0.4	0.2	0.4	0.4	0.5	0.2
Low	13.5	0.5	-0.2	-0.2	-0.1	0.0	-0.4	-0.4	-0.2	0.0
Time since doctor visit for routine care										
Less than 1 year	84.1	0.4	0.4	0.4	0.2	0.1	1.0	1.0	0.5	0.2
Over 1 year	15.9	0.4	-0.4	-0.5	-0.2	-0.1	-1.0	-1.3	-0.5	-0.2

Table 5.-- Estimated reduction in bias and bias ratio for selected characteristics of the School Readiness component (continued)

Characteristic	Standard estimate		Estimated reduction in bias[1]				Bias ratio[2]			
	Percent	Standard error	Scheme A1	Scheme A2	Scheme B1	Scheme B2	Scheme A1	Scheme A2	Scheme B1	Scheme B2
Birth weight										
5.5 pounds or less	93.3	0.3	-0.1	0.0	0.0	0.1	-0.3	0.0	0.0	0.3
Greater than 5.5 pounds	6.7	0.3	0.1	0.0	0.0	-0.1	0.3	0.0	0.0	-0.3
Child attending center-based program[3]										
Yes	52.6	0.8	0.9	0.3	0.8	0.6	1.1	0.4	1.0	0.8
No	47.4	0.8	-0.9	-0.3	-0.8	-0.6	-1.1	-0.4	-1.0	-0.8
Child ever attended center-based program[3]										
Yes	62.9	0.8	0.5	0.3	0.4	0.3	0.6	0.4	0.5	0.4
No	37.1	0.8	-0.5	-0.3	-0.4	-0.3	-0.6	-0.4	-0.5	-0.4
Attended center-based program prior to school[4]										
Yes	73.5	0.5	0.6	0.7	0.5	0.6	1.2	1.4	1.0	1.2
No	26.5	0.5	-0.6	-0.7	-0.5	-0.6	-1.2	-1.4	-1.0	-1.2
Women, Infant, and Children program participant[3]										
Yes	33.8	1.0	-0.6	-0.1	-0.8	-0.7	-0.6	-0.1	-0.8	-0.7
No	66.2	1.0	0.6	0.1	0.8	0.7	0.6	0.1	0.8	0.7
Free meal at school or center[4]										
Yes	35.8	0.6	-0.9	-1.1	-0.5	-0.5	-1.5	-1.8	-0.8	-0.8
No	64.2	0.6	0.9	1.1	0.5	0.5	1.5	1.8	0.8	0.8
Repeated kindergarten[5]										
Yes	5.7	0.4	-0.3	-0.5	-0.2	-0.2	-0.8	-1.3	-0.5	-0.5
No	94.3	0.4	0.3	0.5	0.2	0.2	0.7	1.3	0.5	0.5
Family member read to child in last week[6]										
Not in last week	4.3	0.5	-0.3	-0.4	0.1	0.1	-0.6	-0.8	0.2	0.2
Once or twice	16.9	0.9	0.0	0.4	0.0	0.2	0.0	0.4	0.0	0.2
Three or more times	78.8	0.9	0.3	0.0	-0.1	-0.3	0.3	0.0	-0.1	-0.3
Taught child letters or words in last week[6]										
Not in last week	12.3	0.4	-0.4	-0.4	-0.3	-0.3	-1.0	-1.0	-0.7	-0.7
Once or twice	27.4	0.6	0.3	0.3	0.3	0.5	0.5	0.5	0.5	0.8
Three or more times	60.3	0.7	0.1	0.1	0.0	-0.2	0.1	0.1	0.0	-0.3
Taught songs or music in last week[6]										
Not in last week	33.3	0.6	0.6	0.2	0.3	-0.1	1.0	0.3	0.5	-0.2
Once or twice	30.1	0.7	-0.3	-0.1	-0.2	0.0	-0.4	-0.1	-0.3	0.0
Three or more times	36.7	0.7	-0.2	0.1	0.0	0.1	-0.3	0.1	0.0	0.1
Did arts or crafts with child in last week[6]										
Not in last week	32.2	0.7	-0.3	-0.7	-0.5	-1.0	-0.4	-1.0	-0.7	-1.4
Once or twice	35.7	0.7	0.1	0.3	-0.1	0.0	0.1	0.4	-0.1	0.0
Three or more times	32.1	0.6	0.2	0.4	0.6	1.0	0.3	0.7	1.0	1.7
Visited library with child in last month[6]										
Yes	39.4	0.9	0.5	0.4	0.4	0.5	0.6	0.4	0.4	0.6
No	60.6	0.9	-0.5	-0.4	-0.4	-0.5	-0.6	-0.4	-0.4	-0.6
Visited zoo with child in last month[6]										
Yes	16.8	0.7	0.3	0.1	0.2	0.1	0.4	0.1	0.3	0.1
No	83.2	0.7	-0.3	-0.1	-0.2	-0.1	-0.4	-0.1	-0.3	-0.1

[1]The estimated reduction in bias is the standard estimate of the percent minus the adjusted estimate of the percent.

[2]The bias ratio is the bias reduction estimate divided by the standard error.

[3]Estimate restricted to preschoolers.

[4]Estimate applies to all children except preschoolers.

[5]Estimate restricted to children in primary school.

[6]Estimate applies to all children except those in primary school.

NOTE: Percents may not add to 100 because of rounding.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993

Table 6.-- Estimated reduction in bias and bias ratio for selected characteristics of the School Safety and Discipline component

Characteristic	Standard estimate		Estimated reduction in bias[1]				Bias ratio[2]			
	Percent	Standar error	cheme Al	Scheme A2	scheme B1	Scheme B2	cheme Al	Scheme A2	Scheme B1	Scheme B2
Parental educational level										
Less than high school graduate	9.4	0.5	-1.2	-1.3	-0.3	-0.6	-2.4	-2.6	-0.6	-1.2
High school graduate or equivalent	32.7	0.6	0.3	0.0	-0.2	-0.6	0.5	0.0	-0.3	-1.0
some college	57.9	0.5	0.9	1.3	0.5	1.1	1.8	2.6	1.0	2.2
Mother's employment status										
No mother *in* household	3.5	0.2	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Employed 35 hours/week or more	46.2	0.5	0.0	0.1	-0.1	0.1	0.0	0.2	-0.2	0.2
Employed less than 35 hours/week	20.3	0.5	0.1	0.0	0.0	-0.1	0.2	0.0	0.0	-0.2
Seeking employment	4.5	0.3	-0.2	-0.2	-0.2	-0.2	-0.7	-0.7	-0.7	-0.7
Not in labor force	25.5	0.5	0.0	0.1	0.2	0.2	0.0	0.2	0.4	0.4
Father's employment status										
No father in household	26.8	0.6	-0.2	-0.2	-0.1	-0.2	-0.3	-0.3	-0.2	-0.3
Employed 35 hours/week Or mm-e	63.2	0.5	0.6	0.9	0.6	0.8	1.2	1.8	1.2	1.6
Employed less than 35 hours/week	3.1	0.2	-0.2	-0.2	-0.2	-0.2	-1.0	-1.0	-1.0	-1.0
Seeking employment	2.6	0.2	-0.2	-0.3	-0.2	-0.3	-1.0	-1.5	-1.0	-1.5
Not in labor force	4.3	0.3	-0.1	-0.1	-0.1	-0.1	-0.3	-0.3	-0.3	-0.3
Median household income in ZIP Code										
$15,000 *or less*	4.2	0.3	-0.1	-0.2	0.0	-0.1	-0.3	-0.7	0.0	-0.3
$15,000 to $25,000	31.3	0.7	-0.3	-0.4	-0.3	-0.4	-0.4	-0.6	-0.4	-0.6
More than $25,000	64.5	0.8	0.5	0.6	0.3	0.5	0.6	0.8	0.4	0.6
Mobility in ZIP Code										
High	5.5	0.3	0.2	0.1	0.1	0.1	0.7	0.3	0.3	0.3
Medium/high	17.1	0.6	0.0	0.1	0.0	0.0	0.0	0.2	0.0	0.0
Medium	29.4	0.6	-0.1	-0.1	-0.1	-0.2	-0.2	-0.2	-0.2	-0.3
Medium/low	33.9	0.7	-0.1	-0.1	0.0	-0.1	-0.1	-0.1	0.0	-0.1
	14.1	0.7	0.0	0.1	0.1	0.1	0.0	0.1	0.1	0.1
School control										
Public	91.2	0.3	-0.1	-0.1	-0.1	-0.1	-0.3	-0.3	-0.3	-0.3
Private	8.8	0.3	0.1	0.1	0.1	0.1	0.3	0.3	0.3	0.3
Visitors required to sign in at school										
Yes	79.9	0.5	0.1	0.4	0.0	0.2	0.2	0.8	0.0	0.4
No	20.1	0.5	-0.1	-0.4	0.0	-0.2	-0.2	-0.8	0.0	-0.4
Students in fighting gangs at school[3]										
Yes	22.3	0.5	-0.3	-0.4	-0.3	-0.5	-0.6	-0.8	-0.6	-1.0
No	77.7	0.5	0.3	0.4	0.3	0.5	0.6	0.8	0.6	1.0
Ease of *obtaining* marijuana *at school*[3]										
Very or fairly easy	39.2	0.6	-0.2	-0.3	-0.2	-0.3	-0.3	-0.5	-0.3	-0.5
Hard	29.7	0.5	0.1	0.1	0.2	0.2	0.2	0.2	0.4	0.4
Nearly impossible	31.1	0.6	0.1	0.1	0.0	0.1	0.2	0.2	0.0	0.2
Had drug or alcohol ed program this year										
Yes	68.5	0.7	0.6	0.8	0.7	0.9	0.9	1.1	1.0	1.3
No	31.5	0.7	-0.6	-0.8	-0.7	-0.9	-0.9	-1.1	-1.0	-1.3
Fear of incident of crime at school										
Nom	66.1	0.5	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Fear of theft or robbery[4]	11.9	0.5	-0.1	-0.2	0.0	-0.2	-0.2	-0.4	0.0	-0.4
Fear of bullying Or assault[4]	8.6	0.3	-0.1	-0.1	-0.1	-0.1	-0.3	-0.3	-0.3	-0.3
Fear of two or more types of incidents[4]	13.3	0.5	0.1	0.3	0.1	0.2	0.2	0.6	0.2	0.4
Knowledge of crime at school										
None	38.7	0.6	0.2	0.1	0.2	0.1	0.3	0.2	0.3	0.2
Fear of theft or robbery[4]	14.1	0.5	0.2	0.3	0.2	0.3	0.4	0.6	0.4	0.6
Fear of bullying or assault[4]	15.6	0.4	-0.5	-0.4	-0.4	-0.4	-1.3	-1.0	-1.0	-1.0
Fear of two or more types of incidents[4]	31.6	0.6	0.1	0.0	0.0	0.0	0.2	0.0	0.0	0.0
Victimization *by crime*										
Not victimized	73.0	0.5	0.3	0.2	0.3	0.2	0.6	0.4	0.6	0.4
Victim of theft or robbery[4]	10.9	0.3	-0.2	-0.1	-0.1	0.0	-0.7	-0.3	-0.3	0.0
Victim of bulling or assault[4]	8.9	0.3	-0.1	0.0	-0.2	-0.1	-0.3	0.0	-0.7	-0.3
Victim of two or more types of incidents[4]	7.2	0.3	0.0	0.0	0.0	-0.1	0.0	0.0	0.0	-0.3

Table 6.-- Estimated reduction in bias and bias ratio for selected characteristics of the School Safety and Discipline component (continued)

Characteristic	Standard estimate		Estimated reduction in bias[1]				Bias ratio[2]			
	Percent	Standard error	Scheme A1	Scheme A2	Scheme B1	Scheme B2	Scheme A1	Scheme A2	Scheme B1	Scheme B2
Witnessed crime at school										
None	63.8	0.8	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2
Witnessed robbery[5]	0.6	0.1	0.0	.0.0	0.0	0.0	0.0	0.0	0.0	0.0
Witnessed bulling or assault[5]	24.1	0.8	-0.3	-0.3	-0.3	-0.3	-0.4	-0.4	-0.4	-0.4
Witnessed two or more types of incidents	11.4	0.4	0.0	0.1	0.0	0.0	0.0	0.2	0.0	0.0

[1]The estimated reduction in bias is the standard estimate of the percent minus the adjusted estimate of the percent.

[2]The bias ratio is the bias reduction estimate divided by the standard error.

[3]Item was only asked for students in grades 6 through 12.

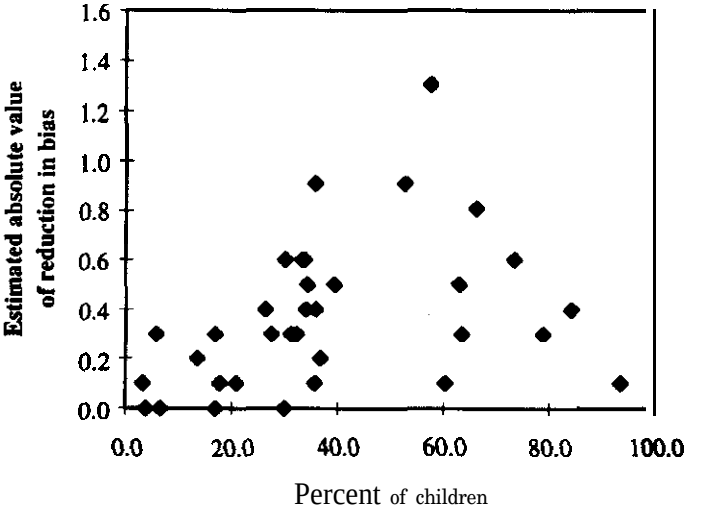
[4] For the fear of incident, knowledge of crime, and victimized by crime variables, the second response category is used if either theft or robbery was reported but not both, the third response category is used if either bullying or assault was reported but not both.

[5] This response category is used if either bullying or assault was reported, but not both.

NOTE: Percents may not add to 100 because of rounding.

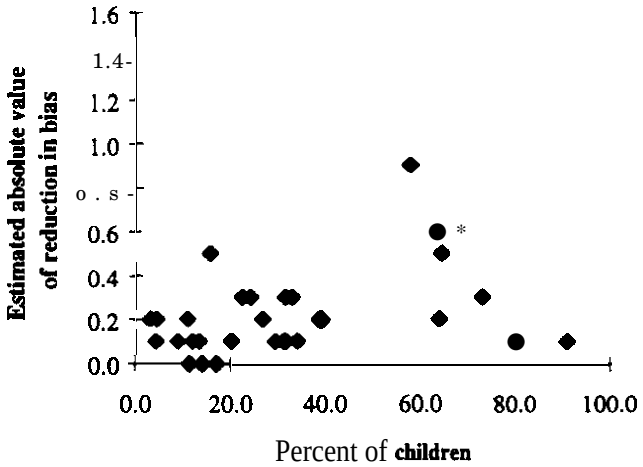
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

Figure 3.--Estimated reduction in absolute bias for School Readiness characteristics (scheme A1)



SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

Figure 4.-- Estimated reduction in absolute value of bias for School Safety and Discipline characteristics (scheme A1)



SOURCE U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

Bias Ratio

The size of the absolute reduction in bias is not a very useful statistical measure of the impact of the bias because it does not take the magnitude of the standard error of the estimate into account. Cochran (1977) discusses the impact on confidence intervals as the ratio of the bias to the sampling error varies. The bias ratio is defined as the bias divided by the sampling error of the estimate. For each scheme the bias ratio is given by

$$r_a = \frac{b_a}{se(\hat{p}_s)}, \qquad (4)$$

with the sampling error of the standard estimate as the denominator. As the bias ratio increases, the chance of covering the population value departs significantly from the nominal confidence interval. For example, a bias ratio of 0.1 baa very little impact on a 95 percent confidence interval, but a bias ratio of 1.0 results in a nominal 95 percent confidence interval that only covers the population value 83 percent of the time.

The bias ratios for all of the response categories for the SR items are given in table 5, and the ratios for the SS&D items are in table 6. The discussion of the bias ratios that follows is based on the ratios remaining after eliminating one response level for each item.

Many of the bias ratios for the SR items are large, even though the average and median ratios are near zero. Nearly half of the ratios are larger than 0.4 in absolute value. A ratio of 0.4 is large enough to reduce a nominal confidence interval from 95 percent to about 93 percent. For the SS&D items, the bias ratios are smaller. Only 5 of the 34 bias ratio estimates in table 6 are greater than 0.4.

The bias ratios show that the biases as estimated under the assumed model could have an effect on the inferences made from the survey estimates. The effect on the inferences is a greater problem for the SR component than for the SS&D component. The confidence intervals based on the standard estimates for some characteristics will not attain the nominal confidence intervals due to the undercoverage bias.

Consistency Across Adjustment Schemes

A review of the estimates in tables 5 and 6 shows that if the bias reduction estimated under one adjustment scheme is large and positive, the estimates under the other schemes tend to be large and positive. The correlations between the bias estimates under the four schemes are a measure of the consistency of the reduction in bias estimates across the schemes. In general, the correlations[6] are very high, as might be expected. For the SR component, the correlation between the estimates goes from a low of 0.6 between the estimates for schemes A2 and B 1, to a high of 0.9 for schemes Al and A2 and schemes B 1 and B2. The correlations for the bias reduction estimates from the characteristics of the SS&D component are uniformly high, with correlations nearly 0.9 between all the schemes.

The bias adjustments resulting from defining the cells by educational attainment within race/ethnicity are highly correlated with those formed by tenure within race/ethnicity. However, the consistency of the bias estimates does not imply that all of the adjustment schemes are equivalent in terms of their overall statistical properties. So far, the discussion has only been about the bias reduction, Their variance implications are presented below.

Variance Implications

The results above show that the standard estimates from the NHES:93 are subject to coverage bias under the assumed model. Since the ordinary measure of variation for an unbiased estimate, the variance, is not appropriate for biased estimates, the mean square errors of the biased estimates are considered. The mean square error of the estimate (MSE) is a frequently used statistic that reflects both the variation about the average and the bias of an estimate. The MSE is the sum of the variance and the square of the bias of the estimate.

The MSE of the standard estimate can be computed by using the standard variance and bias estimates presented above.[7] The estimated MSE can be written as

$$\text{mse}_a = \text{var}(\hat{p}_s) + b_a^2 \qquad (5)$$

where the terms have been defined before. Four different estimates of the mean square error can be formed, one corresponding to each of the weighting schemes. Drily the estimated mean square errors for scheme A 1 are shown in tables 7 and 8 because the results for the other schemes are so similar.

It is interesting to note that the mean square error of the estimate is functionally related to the bias ratio discussed previously. The relationship for the standard NHES:93 estimate is given by

$$\text{mse}_a = \text{var}(\hat{p}_s)(1 + r_a^2). \qquad (6)$$

This relationship explains why the value of the mean square error is close to the variance estimate unless the bias ratio is large.

[5] The correlations were computed after deleting one of the response categories for each item, as discussed above.

[6] An unbiased estimate of the MSE can be found by adding an unbiased estimate of the variance to an unbiased estimate of the hiss squared. The estimated bias squared is not technically an unbiased estimate of the squared bias, but the difference is extremely small in this case. Consequently, the squared bias estimates are used for this report.

Table 7.-- Estimated mean square error and mean square ratio for selected characteristics of the School Readiness component

Characteristic	Standard estimate			Mean square ratio[2]			
	Percent	Variance	MSE[1]	Scheme A1	Scheme A2	Scheme B1	Scheme B2
Parental educational level							
Less than high school graduate	8.6	0.1	3.0	3.6	4.1	3.5	3.8
High school graduate or equivalent	33.9	0.6	0.8	96.0	108.0	92.8	100.8
Some college	57.5	0.5	2.2	27.0	30.3	26.1	28.3
Mother's employment status							
No mother in household	2.4	0.0	0.1	96.0	108.0	92.8	100.8
Employed 35 hours/week or more	34.3	0.3	0.5	60.0	67.5	58.0	63.0
Employed less than 35 hours/week	20.9	0.3	0.3	115.4	129.8	111.5	121.2
Seeking employment	6.6	0.2	0.2	120.0	135.0	116.0	126.0
Not in labor force	35.8	0.4	0.5	83.1	93.5	80.3	87.2
Father's employment status							
No father in household	26.3	0.3	0.4	73.2	82.3	70.7	76.8
Employed 35 hours/week or more	63.4	0.4	0.4	96.0	108.0	92.8	100.8
Employed less than 35 hours/week	3.8	0.1	0.1	120.0	135.0	116.0	126.0
Seeking employment	3.2	0.1	0.1	120.0	135.0	116.0	126.0
Not in labor force	3.3	0.0	0.0	96.0	108.0	92.8	100.8
Median household income in ZIP Code							
$15,000 or less	4.0	0.2	0.2	96.0	108.0	92.8	100.8
$15,000 to $25,000	30.1	0.6	1.0	76.8	86.4	74.2	80.6
More than $25,000	66.0	0.6	1.3	60.0	67.5	58.0	63.0
Mobility in ZIP Code							
High	7.6	0.3	0.3	120.0	135.0	116.0	126.0
Medium/high	17.7	0.4	0.4	116.8	131.4	112.9	122.6
Medium	30.0	0.5	0.5	120.0	135.0	116.0	126.0
Medium/low	31.2	0.6	0.7	105.2	118.4	101.7	110.5
Low	13.5	0.3	0.3	103.4	116.4	100.0	108.6
Time since doctor visit for routine care							
Less than 1 year	84.1	0.2	0.3	60.0	67.5	58.0	63.0
Over 1 year	15.9	0.2	0.3	60.0	67.5	58.0	63.0
Birth weight							
5.5 pounds or less	93.3	0.1	0.1	108.0	121.5	104.4	113.4
Greater than 5.5 pounds	6.7	0.1	0.1	108.0	121.5	104.4	113.4
Child attending center-based program[3]							
Yes	52.6	0.6	1.5	53.0	59.6	51.2	55.6
No	47.4	0.6	1.5	53.0	59.6	51.2	55.6
Child ever attended center-based program[3]							
Yes	62.9	0.6	0.9	86.3	97.1	83.4	90.6
No	37.1	0.6	0.9	86.3	97.1	83.4	90.6
Attended center-based program prior to school[4]							
Yes	73.5	0.3	0.6	49.2	55.3	47.5	51.6
No	26.5	0.3	0.6	49.2	55.3	47.5	51.6
Women, Infants, and Children program participant[3]							
Yes	33.8	1.0	1.4	88.2	99.3	85.3	92.6
No	66.2	1.0	1.4	88.2	99.3	85.3	92.6
Free meal at school or center[4]							
Yes	35.8	0.4	1.2	36.9	41.5	35.7	38.8
No	64.2	0.4	1.2	36.9	41.5	35.7	38.8
Repeated kindergarten[5]							
Yes	5.7	0.2	0.3	76.8	86.4	74.2	
No	94.3	0.2	0.2	76.8	86.4	74.2	80.6
Family member read to child in last week[6]							
Not in last week	4.3	0.3	0.3	88.2	99.3	85.3	
Once or twice	16.9	0.8	0.8	120.0	135.0	116.0	126.0
Three or more times	78.8	0.8	0.9	108.0	121.5	104.4	113.4
Taught child letters or words in last week[6]							
Not in last week	12.3	0.2	0.3	60.0	67.5	58.0	63.0
Once or twice	27.4	0.4	0.4	96.0	108.0	92.8	100.8
Three or more times	60.3	0.5	0.5	117.6	132.3	113.7	123.5

Table 7.-- Estimated mean square error and mean square ratio for selected characteristics of the School Readiness component (continued)

characteristic	Standard estimate			Mean square ratio[2]			
	Percent	Variance	MSE[1]	Scheme A1	Scheme A2	Scheme B1	Scheme B2
Taught songs or music in last week[6]							
Not in last week	33.3	0.4	0.7	60.0	67.5	58.0	63.0
Once or twice	30.1	0.5	0.6	101.4	114.1	98.0	106.4
Three or more times	36.7	0.5	0.5	110.9	124.8	107.2	116.5
Did arts or crafts with child in last week[6]							
Not in last week	32.2	0.5	0.6	101.4	114.1	98.0	106.4
Once or twice	35.7	0.5	0.5	117.6	132.3	113.7	123.5
Three or more times	32.1	0.4	0.4	108.0	121.5	104.4	113.4
Visited library with child in last month[6]							
Yes	39.4	0.8	1.1	91.7	103.2	88.6	96.3
No	60.6	0.8	1.1	91.7	103.2	88.6	96.3
Visited zoo with child in last month[6]							
Yes	16.8	0.5	0.6	101.4	114.1	98.0	106.4
No	83.2	0.5	0.6	101.4	114.1	98.0	106.4

[1]MSE is the estimated variance plus the square of the bias estimate using scheme A1.

[2]The mean square ratio is the mean square error of the adjusted estimate divided by the mean square error of the standard estimate.

[3]Estimate restricted to preschoolers.

[4]Estimate applies to all children except preschoolers.

[5]Estimate restricted to children in primary school.

[6]Estimate applies to all children except those in primary school.

NOTE: Percents may not add to 100 because of rounding.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

Table 8.-- Estimated mean square error and mean square ratio for selected characteristics of the School Safety and Discipline component

characteristic	Standard estimate			Mean square ratio[2]			
	Percent	Variance	MSE[1]	Scheme A1	Scheme A2	Scheme B1	Scheme B2
Parental educational level							
Less than high school graduate	9.4	0.3	1.7	16.6	18.6	16.4	18.5
High school graduate or equivalent	32.7	0.4	0.5	89.6	100.8	88.8	100.0
Some college	57.9	0.3	1.1	26.4	29.7	26.2	29.5
Mother's employment status							
No mother in household	3.5	0.0	0.0	112.0	126.0	111.0	125.0
Employed 35 hours/week or more	46.2	0.3	0.3	112.0	126.0	111.0	125.0
Employed less than 35 hours/week	20.3	0.3	0.3	107.7	121.2	106.7	120.2
Seeking employment	4.5	0.1	0.1	77.5	87.2	76.8	86.5
Not in labor force	25.5	0.3	0.3	112.0	126.0	111.0	125.0
Father's employment status							
No father in household	26.8	0.4	0.4	100.8	113.4	99.9	112.5
Employed 35 hours/week or more	63.2	0.3	0.6	45.9	51.6	45.5	51.2
Employed less than 35 hours/week	3.1	0.0	0.1	56.0	63.0	55.5	62.5
Seeking employment	2.6	0.0	0.1	56.0	63.0	55.5	62.5
Not in labor force	4.3	0.1	0.1	100.8	113.4	99.9	112.5
Median household income in ZIP Code							
$15,000 or less	4.2	0.1	0.1	100.8	113.4	99.9	112.5
$15,000 to $25,000	31.3	0.5	0.6	94.6	106.4	93.8	105.6
More than $25,000	64.5	0.6	0.9	80.5	90.6	79.8	89.9
Mobility in ZIP Code							
High	5.5	0.1	0.1	77.5	87.2	76.8	86.5
Medium/high	17.1	0.4	0.4	112.0	126.0	111.0	125.0
Medium	29.4	0.4	0.4	109.0	122.6	108.0	121.6
Medium/low	33.9	0.5	0.5	109.8	123.5	108.8	122.5
Low	14.1	0.5	0.5	112.0	126.0	111.0	125.0
School control							
Public	91.2	0.1	0.1	100.8	113.4	99.9	112.5
Private	8.8	0.1	0.1	100.8	113.4	99.9	112.5
Visitors required to sign in at school							
Yes	79.9	0.3	0.3	107.7	121.2	106.7	120.2
No	20.1	0.3	0.3	107.7	121.2	106.7	120.2
Students in fighting gangs at school[3]							
Yes	22.3	0.3	0.3	83.1	93.4	80.1	91.2
No	77.7	0.3	0.3	83.1	93.4	80.1	91.2
Ease of obtaining marijuana at school[3]							
Very or fairly easy	39.2	0.4	0.4	101.7	114.3	98.1	111.6
Hard	29.7	0.3	0.3	108.7	122.1	104.8	119.2
Nearly impossible	31.1	0.4	0.4	109.9	123.6	106.1	120.6
Had drug or alcohol ed program this year							
Yes	68.5	0.5	0.8	64.6	72.6	64.0	72.1
No	31.5	0.5	0.9	64.6	72.6	64.0	72.1
Fear of incident of crime at school							
None	66.1	0.3	0.3	112.0	126.0	111.0	125.0
Fear of theft or robbery[4]	11.9	0.3	0.3	107.7	121.2	106.7	120.2
Fear of bullying or assault[4]	8.6	0.1	0.1	100.8	113.4	99.9	112.5
Fear of two or more types of incidents[4]	13.3	0.3	0.3	107.7	121.2	106.7	120.2
Knowledge of crime at school							
None	38.7	0.4	0.4	100.8	113.4	99.9	112.5
Fear of theft or robbery[4]	14.1	0.3	0.3	96.6	108.6	95.7	107.8
Fear of bullying or assault[4]	15.6	0.2	0.4	43.7	49.2	43.3	48.8
Fear of two or more types of incidents[4]	31.6	0.4	0.4	109.0	122.6	108.0	121.6
Victimization by crime							
Not victimized	73.0	0.3	0.3	82.4	92.6	81.6	91.9
Victim of theft or robbery[4]	10.9	0.1	0.1	77.5	87.2	76.8	86.5
Victim of bulling or assault[4]	8.9	0.1	0.1	100.8	113.4	99.9	112.5
Victim of two or more types of incidents[4]	7.2	0.1	0.1	112.0	126.0	111.0	125.0

Table 8.-- Estimated mean square error and mean square ratio for selected characteristics of the School Safety and Discipline component (continued)

Characteristic	standard estimate			Mean square error ratio[2]			
	Percent	Variance	MSE[1]	Scheme A1	Scheme A2	Scheme B1	Scheme B2
Witnessed crime at school							
None	63.8	0.6	0.7	105.4	118.6	104.5	117.6
Witnessed robbery	0.6	0.0	0.0	112.0	126.0	111.0	125.0
Witnessed bulling or assault[5]	24.I	0.6	0.7	98.2	110.5	97.3	109.6
Witnessed two or more types of incide n ts	11.4	0.2	0.2	112.0	126.0	111.0	125.0

[1]MSE is the estimated variance plus the square of the bias estimate using scheme A1.

[2]The mean square ratio is the mean square error of the adjusted estimate divided by the mean square error of the standard estimate.

[3]Asked only for students in grades 6 through 12.

[4] For the fear of incident, knowledge of crime, and victimized by crime variables, the second response category is used if either theft or robbery was reported but not both, the third response category is used if either bullying or assault was reported but not both.

[5]This response category is used if either bullying or assault was reported, but not both

NOTE: Percents may not add to 100 because of rounding.

SOURCE: U.S. Department Of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

The estimated mean square errors of the estimates can be used to contrast the bias and variance in the standard estimate with the variance in the adjusted estimates. As before, the telephone service adjustments are assumed to reduce the coverage bias. If bias were the only factor to be considered, then the adjusted estimates would clearly be preferred and the only decision would be which of the four adjustments should b e implemented. However, t h e adjustments increase the variability of the estimates. The trade-off between the bias reduction and the variance increase associated with the adjustments is discussed below.

The size of the variance increase from adjusting the weights using the telephone service interruption data was expressed earlier as the variance inflation factor (*VIF*). *The* relative *VIFs* for each of the four schemes are given in table 4. Multiplying the standard variance estimates by the relative *VIF* for the appropriate adjustment factor yields an approximate variance for the adjusted estimates. The variance estimates for the standard estimates are shown in the second column in tables 7 and 8. Multiplying these estimates by the appropriate ratios for the adjustment scheme give the approximate variances of the estimates for each adjustment scheme.

To aid in comparing the weighting procedures, the ratio of the estimated variance of the adjusted estimate to the estimated mean square error for the standard estimate was tabulated. This estimate is called the mean square ratio. It can be expressed as

$$msr_a(\hat{p}) = \frac{100 \times relative VIF_a \times var(\hat{p}_s)}{mse_{A1}(\hat{p})} \quad (7)$$

Note that the mean square error is derived using the bias estimated from scheme Al only, but it is used to compute the mean square ratios for all four schemes. This is done to make the ratios comparable across the schemes. The estimates were also computed using scheme B2 and the results are not sensitive to the scheme used for the bias.

The mean square ratios are useful in assessing the effectiveness of the adjustments since they include contributions from both the reduction in bias (in the mean square error estimates) and the variance (in the *VIF*). When the mean square ratio is 100, the variance of the adjusted estimate is exactly equal to the mean square error of the biased, standard estimate. This is the break-even point. A ratio less than 100 indicates that the bias reduction of the adjustment is greater than the variance increase so the adjusted estimate has a smaller mean square error than the standard estimate. A mean square ratio over 100 means that the variance increase associated with the adjustment is greater than the bias reduction and the unadjusted estimator has the smaller mean square error.

The mean square ratios for the selected items for the SR and SS&D components are given in tables 7 and 8, respectively. These ratios are summarized below. As before, the estimate for one response category for each item was deleted before summarizing the estimates. To give a more complete representation of the distributions, figure 5 displays the mean square ratios for the SR items and figure 6 displays the mean square ratios for the SS&D items. For both of these figures, the horizontal axis is the estimated percentage of children in the category, The break-even line, when the mean square ratio is 100, is shown on the charts.

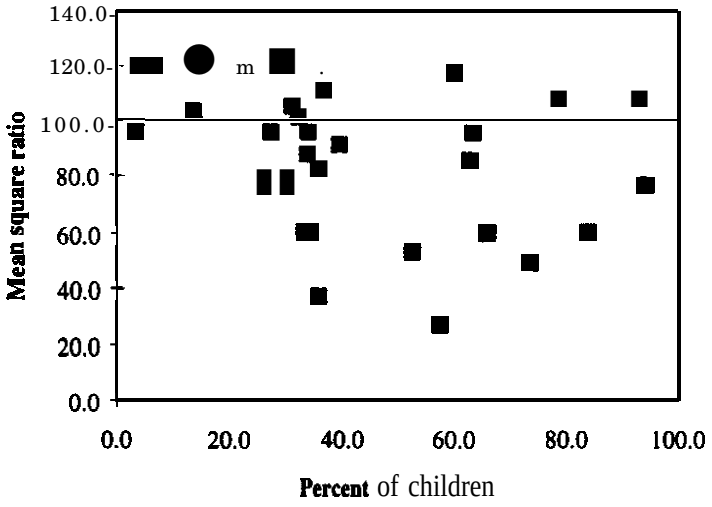
The distributions of mean square ratios for both components are very similar. The medians for schemes A 1 and B 1 (those based on interruptions of 1 week or more) arc near the break-even point of 100. The means for these schemes are close to 90 and the figures confirm that this is due to the skewed distributions of the mean square ratios.

Table 9.– Summaries of distribution of mean square ratios for selected characteristics of School Readiness and School Safety and Discipline components

	Scheme			
	Al	AZ	B1	B2
School Readiness				
mean	89.8	101.0	86.8	94.2
median	96.0	108.0	92.8	100.8
minimum	27.0	30.3	26.1	28.3
maximum	120.0	135.0	116.0	126.0
School Safety and Discipline				
mean	93.3	104.9	92.2	103.9
median	100.8	113.4	99.9	112.5
minimum	26.4	29.7	26.2	29.5
maximum	112.0	126.0	111.0	125.0

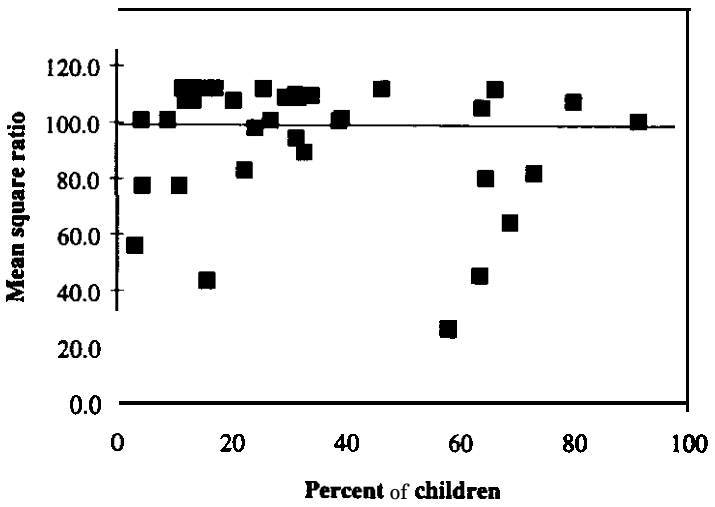
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

Figure 5.--Estimated mean square ratios for selected **School Readiness items (scheme A1)**



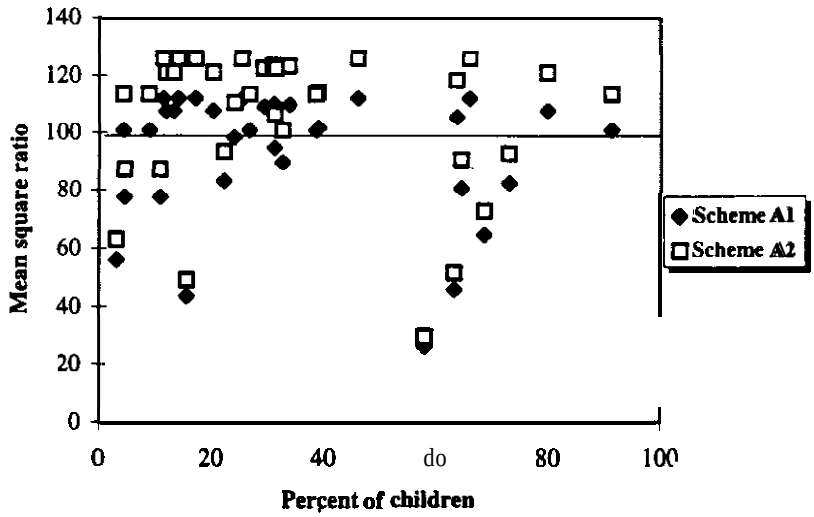
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

Figure 6.--Estimated mean square ratios for selected **School** Safety and **Discipline items (scheme A1)**



SOURCE: U.S. Department of Education, National Center for Education Statistics. National Household Education Survey, spring 1993.

Figure 7.--Estimated mean square ratios for selected School Safety and Discipline items, schemes Al **and A2**



U.S. Department of Education, National Center for Education Statistics, National Household Education Survey, spring 1993.

Another important observation about the distributions of the mean square ratios for schemes Al and B 1 is the size of the ratios at the extremes of the distribution. The maximum mean square ratios for both components is 120, while some ratios are as small as 26. This means the maximum increase in the mean square error of the estimates is 20 percent, while the reductions in mean square error for other estimates are large. Thus, the penalty associated with adjusting even when the estimate is not biased is modest, but the benefits of adjusting when it is needed can be quite large.

The distributions for the mean square ratios for schemes A 1 and B 1 are approximately equal, and the choice of which of these schemes should be used may be determined by nonstatistical issues, such as availability of data and the other types of adjustments required in the survey. The mean square ratios show that the adjusted weights reduce the mean square error for about half the estimates considered when compared with the standard weights.

The distributions of the mean square ratios for schemes A2 and B2 (those based on interruptions of 1 month or more) have medians and means that are greater than 100. Essentially, these mean square ratios are shifted upward when compared with those of schemes Al and B 1. This is clear from figure 7, which displays the ratios for schemes A 1 and A2 for the SS&D component estimates. Because estimates with smaller mean square ratios have lower mean square errors, the upward shift indicates that adjusting using the shorter telephone interruption period of 1 week or more (schemes Al and B 1) is preferable to the longer period (schemes A2 and B2). This result shows that in the trade-off between the variance inflation (the longer time period has a larger variance) and the bias reduction (the longer time period has smaller bias), the variance inflation has a bigger impact on the mean square error.

Since the mean square ratios were computed using the estimated bias from scheme A 1 (see equation 7), this might favor those schemes with shorter telephone interruption periods. As mentioned above, the mean square ratios were also computed using the bias estimates from scheme B2. The distributions of the mean square ratios using the scheme B2 bias estimates are very similar to those using the Al bias estimates.

For example, in the SR component the means of the distributions of mean square ratios using scheme B2 bias estimates are 95.7 for scheme A1, 107.7 for scheme A2, 92.5 for scheme B 1, and 100.5 for scheme B2. The adjustments using the shorter time periods (A1 and B 1) still dominate those using the longer time periods (A2 and B2).

Conclusions

In most surveys conducted solely by telephone, the potential bias introduced by excluding persons living in nontelephone households is a major concern. If the percentage of the target population living in nontelephone households is relatively large and the characteristics of those persons are different from those who live in telephone households, then the estimates may be susceptible to significant coverage bias.

One method of addressing this problem without resorting to other modes of data collection is to adjust the estimates using data collected from the responding households with telephones. Since having a telephone in the household is not a static phenomenon, a reasonable response propensity model leads to adjustments based on data on interruptions in telephone service. In essence, households that have bad interruptions in service are assumed to be similar to households without telephones at the time of the survey. Actually, the model only assumes that the relationship between persons living in households with interruptions in service and those in households without telephones is closer than between persons in all telephone households and those in households without telephones. The weights for persons in households reporting an interruption in telephone service are increased to adjust for those without telephones.

In the NHES:93, households were asked about interruptions in telephone service during the past 12 months. Estimates of the percentage of children living in households with any interruptions in telephone service were computed separately for the two NHES:93 components. The estimated percentage of children differed somewhat for the two populations, with 12 percent of the younger children (the SR component) living in households with some interruption in service and 9 percent of the older children living in households with this characteristic. The response to the questions about interruptions in telephone service were then used to adjust the standard weights. Four alternative weights were created depending on the length of the telephone interruption (at least 1 week or at least 1 month) and demographic characteristics of the household. The alternative weights were then used to produce estimates of the bias reduction in the standard estimates.

The bias reduction estimates computed under the assumed - model showed that the coverage adjustments for the SR component could have an important effect on the inferences from the survey estimates. The estimates of the ratio of the bias to the standard error of the estimate demonstrated that confidence intervals for some of the estimates based on the standard estimates were not likely to attain the nominal confidence intervals due to the undercoverage bias. On the other hand, many of the bias ratios were small and not substantively important.

Although the adjustments reduced bias, they also increased the variability of the estimates. The trade-off between the bias reduction and the variance increase was examined by comparing the estimated mean square error of the standard estimates (which were assumed to be biased because of the coverage problem) to the variance estimates from the alternative weights (assumed to be unbiased). These ratios are referred to as mean square ratios.

The alternative weighting schemes performed differently with respect to the mean square ratios, even though they were consistent in terms of bias reduction. The schemes baaed on interruptions

of telephone service of 1 week or more (schemes A1 and B1) were better than the schemes based on interruptions of 1 month or more (A2 and B2). The bias adjustments resulting from using educational attainment by race/ethnicity categories (A1) were roughly equivalent to those using tenure by race/ethnicity (B1). Either scheme A1 or B1 could be used if the adjustment procedure were adopted.

The distributions of the mean square ratios show that about half the estimates could be improved using the telephone service interruption adjustments. Furthermore, even for those estimates that were less accurate due to the variance increases associated with the differential weights, the magnitude of the increases were not large. In other words, the penalty for adjusting when it did not reduce the coverage bias was not very great. These findings suggest that the adjustments should be seriously considered.

The size of the sample is another factor that should be considered when evaluating the use of the telephone service interruption adjustment. Bias ratios increase with the sample size because the bias is not affected while the sampling error of the estimate (the denominator of the bias ratio) decreases. Thus, the adjustments should be more beneficial in surveys with large sample sizes where the bias ratios might be expected to be large.

While the results of this study suggest that the adjustments could be useful for many estimates from telephone surveys, other studies are needed before the adjustments are recommended. As discussed earlier, the estimates of the mean square errors in this study were based on the assumption that the adjusted estimates reduced the bias of the estimates. This model assumption could not be verified because of the lack of benchmark data for comparison. The assumed model may be beneficial to the adjusted estimates in the sense that it results in lower estimates of the mean square errors for the adjusted estimates. Thus, the findings of this study should be taken as an indication that adjustment using data on interruptions in telephone service is a feasible method that requires further study and evaluation.

The questions about interruptions in telephone service were included in the NHES:95 to further evaluate this method of adjustment for coverage bias. The NHES:95 has a survey component on adult education so that data on service interruptions will be obtained for virtually all types of households rather than being restricted to households with children as was the case with the NHES:93. In addition, the questions recently were added to the National Health Interview Survey, a survey conducted by the Census Bureau for the National Center for Health Statistics. The findings from this survey should he ¯even ˘more useful in evaluating the method because the survey covers households without telephones by in-person interviews, eliminating the need for the critical model assumption used in this study.

In summary, the findings of this study are:

- The coverage bias associated with households without telephones could be important for some statistics, even after the ordinary poststratification adjustments;
- Data collected on telephone interruptions can be used to reduce this bias by using a response probability type of adjustment;
- The benefits of the bias reduction appear to he large enough to offset the variance increases due to increased variability in the weights in this study, although the results may differ for different size samples; and
- The findings are tentative because they rely on a variety of assumptions, and some of them are favorable to the adjustment. Further research is needed before the procedure can be recommended.

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