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Compensating for Noncoverage of Nontelephone Households in Random-Digit-Dialing Surveys: A Comparison of Adjustments Based on Propensity Scores and Interruptions in Telephone Service

K.P. Srinath[1], *Martin R. Frankel*[2], *David C. Hoaglin*[3], *and Michael P. Battaglia*[4]

Noncoverage of nontelephone households is a limitation of random-digit-dialing surveys, because households without telephone service may differ from telephone households on key survey measures. Poststratification is the most common method of compensating for the exclusion of nontelephone households. One alternative approach uses data on interruptions in telephone service from telephone households. Another method uses logistic regression to model households' propensity to be a nontelephone household. We evaluate the three methods using data from the National Health Interview Survey, a large in-person interview survey conducted by the National Center for Health Statistics. Our results show that the interruption-in-telephone-service method generally has the lowest mean squared error. A variant of the interruption method is suggested for the situation where no independent estimate of telephone coverage is available for the target population.

Key words: Noncoverage bias; National Health Interview Survey; mean squared error; poststratification.

1. Introduction

It is well-known that telephone surveys are subject to coverage bias from noncoverage of nontelephone households. Though the percentage of households not having telephone service is small nationally, it can be substantial in some geographic areas and socioeconomic groups. For example, not having telephone service is more common among low-income households than other income groups (Thornberry and Massey 1988). Therefore, failure of the sample to adequately represent nontelephone households introduces the possibility of bias in the survey estimates. The bias may be large for variables related to telephone status as compared to variables that are not related. It is important to adjust the sampling weights for this noncoverage. Recently, another source of bias in telephone surveys is the lack of coverage of households with only cellular

[1] Abt Associates Inc., 4550 Montgomery Avenue, Suite 800 North, Bethesda, MD 20814, U.S.A.
Email: K.P._Srinath@abtassoc.com
[2] Baruch College, City University of New York, One Bernard Baruch Way, New York, NY 10010, U.S.A.
Email: martin_frankel@baruch.cuny.edu
[3] Abt Bio-Pharma Solutions, Inc., 181 Spring Street, Lexington, MA 02421, U.S.A.
Email: david.hoaglin@abtbiopharma.com
[4] Abt Associates Inc., 55 Wheeler Street, Cambridge, MA 02138, U.S.A. Email: mike_battaglia@abtassoc.com
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telephones. Coverage problems with cellular-only households are briefly discussed in this article.

Among methods of adjusting the survey weights to reduce bias in the estimates from noncoverage of nontelephone households, the most common is poststratification. In this article, we investigate two alternative methods of adjustment and compare them to poststratification. The first method, introduced by Brick, Waksberg, and Keeter (1996) and refined by Frankel et al. (1999), uses data from the RDD survey on interruptions in household telephone service during the year to adjust the weights of households with interruptions in telephone service so as to compensate for noncoverage of nontelephone households.

The second method, propensity adjustment (Ferraro and Brick 2001), uses a logistic regression model to predict the probability of a household's being a nontelephone household. These propensity scores are used to adjust the weights of telephone households.

In Section 2 we describe simple poststratification. Section 3 gives a description of the interruption method. Details of the propensity-score method are given in Section 4. Results of applying these methods to data from the 2001–2002 National Health Interview Survey (NHIS) are given in Section 5. Section 6 presents some conclusions.

2. Simple Poststratification

In a random-digit-dialing survey one generally calculates a base sampling weight and then makes further adjustments for multiple voice-use telephone lines in the household and for unit nonresponse. We refer to the resulting weight as a nonresponse-adjusted sampling weight. Poststratification entails adjusting the nonresponse-adjusted sampling weights so that the totals of the weighted sample on specified variables match external control totals. The control totals generally come from a source such as the U.S. Decennial Census, U.S. Census Bureau population estimates, the U.S. Current Population Survey, or a private company that offers population estimates. Often each control total corresponds to a cell in a cross-classification of age group, gender, and race categories. Poststratification ratio-adjusts the nonresponse-adjusted sampling weights so that the weighted count in each poststratification cell equals the control total for that cell. Simple poststratification is commonly used to adjust for unit nonresponse and noncoverage of nontelephone households in RDD samples. This approach does not make a separate adjustment to compensate for the exclusion of nontelephone households. Rather, the adjustments for nonresponse and noncoverage are combined in a single overall adjustment.

When control totals are not available for each poststratification cell in the cross-classification, but are available for the cells of lower-dimensional margins, the alternative procedure known as raking iteratively adjusts the sampling weights until they satisfy those control totals (within a specified tolerance) (Deming 1943, Chapter VII). In many situations, raking actually offers an advantage, because it allows the poststratification process to include additional variables, for which only single-variable control totals are available.

In this article we use simple poststratification as a baseline against which to compare the other methods. We simulate an RDD sample by using the telephone households in the

2001–2002 National Health Interview Survey. Our analysis focuses on the NHIS Person File, which contains data for all individuals in the sample households. The NHIS contains an interim weight that reflects the probability of selection of each person in the household and an adjustment for unit nonresponse. It uses 88 poststratification cells formed by age group, gender, and race/ethnicity. Because we combined two years of NHIS data, the interim weight for persons in telephone households was divided by two, and the two years' control totals for each poststratification cell were averaged. The interim weights for the persons in telephone households were ratio-adjusted to the control totals for the 88 cells.

3. Interruption Method

Empirical evidence suggests that telephone households with interruptions in telephone service are often more similar to nontelephone households than are either telephone households without interruptions or all telephone households (Keeter 1995; Frankel et al. 1999). To take advantage of this relationship, an RDD survey can collect information on whether each selected household experienced an interruption in telephone service of one or week or longer in the past twelve months. Then the weights of households with interruptions in telephone service can be separately adjusted to compensate for the noncoverage of nontelephone households.

A simple version of this adjustment proceeds as follows. Let N denote the total number of households (i.e., both telephone and nontelephone households). Let N_t denote the total number of telephone households and N_{nt} the total number of nontelephone households. Then $N = N_t + N_{nt}$. Let r denote the estimated proportion of telephone households in the survey having interruptions in telephone service. We estimate the total number of households in the population having interruptions in telephone service as $\hat{N}_{tI} = rN_t$. We adjust the weights of households with interruptions in telephone service to sum to the total $N_{nt} + \hat{N}_{tI}$. Also, we adjust the weights of households without interruptions in telephone service to sum to the total $N_t - \hat{N}_{tI}$. This method of adjustment differs from the one proposed by Brick, Waksberg, and Keeter (1996). Their proposal adjusts only the weights of households with interruptions in telephone service. If the survey makes standard poststratification adjustments involving known control totals, then these two totals can be used as an additional margin for raking the weights.

In surveys, such as the NHIS, that collect data on persons in the selected households, the adjustment procedures are similar to the procedure described above. The two control totals are at the person level. Persons in telephone households without interruptions in telephone service form one control total, and persons in telephone households with interruptions in telephone service and persons in nontelephone households constitute the other control total. In applying this method to the NHIS, we used these two control totals in one margin and treated the totals for the 88 poststratification cells as a separate margin.

For some telephone surveys, it may not be possible to determine the number or exact percentage of nontelephone households. In such situations, we may approximate (from national or state-level information) the size of the nontelephone population relative to the size of the population of persons in households with interruptions in telephone service. From several national and state-level telephone surveys like the National Immunization Survey, we have found that the typical adjustment factor for nontelephone households falls in a range from 2.0 to 3.0. Thus, as part of our evaluation we have examined the properties of adjustments to the weights of persons in households with interruptions in telephone service under the assumptions that the size of the nontelephone population is 2, 2.5, and 3 times the size of the population of persons in interrupted households. The number between 2 and 3 is based on the estimated size of the interruption population and the known nontelephone population observed in more than one national survey. Such data (the size of the interruption population and nontelephone population) are available from the public-use data files of some large-scale national surveys in the U.S. such as the National Immunization Survey. In some surveys the actual adjustment factor may be larger than 3, but it seems prudent to restrict assumed values to more conservative choices.

When we estimate the size of the nontelephone population by multiplying the estimated size of the interruption-in-telephone-service population by a chosen constant (such as 2 or 3), we also consider the bias in the estimate, if the estimated size of the nontelephone population differs from its true size. Incorrectly estimating the proportion of the nontelephone population introduces an additional bias in the estimate of the population proportion of interest in the survey. First, we show the bias in the estimate based on interruption in telephone service. We show this at the person level rather than at the household level. As indicated earlier, the adjustments at the household level and person level are similar.

Assume that we are interested in estimating a certain population proportion P relating to persons (such as the proportion of persons who are up-to-date on a particular vaccination). Let $P_{t,i}$ denote this proportion among persons living in households with telephone and interruptions in telephone service. Let P_{nt} denote the same proportion among individuals in households without telephone service. It can be shown that the bias in the estimated proportion $\hat{P}$ based on estimating the population proportion from the sample of persons in households with interruptions in telephone service and using it for nontelephone households is

$$B(\hat{P}) = \frac{N_{nt}}{N}(P_{t,i} - P_{nt})$$

Suppose we incorrectly estimate the number of persons in nontelephone households from the number of persons in households with interruptions in telephone service. Then the bias is

$$B(\hat{P}) = \frac{N_{nt}}{N}(P_{t,i} - P_{nt}) + \frac{\left(N^{*}_{nt} - N_{nt}\right)}{N} P_{t,i}$$

where N^{*}_{nt} is the estimated size of the nontelephone population. The additional bias equals the difference between the estimated and true proportions of the nontelephone population multiplied by the proportion of interest in the population of persons with interruptions in telephone service. Therefore, it is important to use a factor that brings the estimated proportion of nontelephone population close to the true proportion of the nontelephone population.

4. Propensity Method

A number of telephone studies have proposed using propensity scores in post-survey weighting to decrease biases from deficient coverage (e.g., Battaglia et al. 1995; Hoaglin and Battaglia 1996; Duncan and Stasny 2001; Garren and Chang 2002). Propensity scores have also been used in surveys to address bias from partial response or nonresponse (e.g., Lepkowski, Kalton, and Kasprzyk 1989; Göksel, Judkins, and Mosher 1991; Smith et al. 2001). Apart from telephone surveys, propensity-score weighting has been used to adjust for late response (Czajka et al. 1992) and for nonprobability sample selection in Web surveys (Terhanian and Bremer 2000; Terhanian et al. 2000; Varedian and Forsman 2002; Lee 2006). The rest of this section reviews situations where propensity scores have been used to adjust design weights, assumptions underlying propensity-score weighting, and the mechanics of making the adjustments. Under the proper conditions, methods based on propensity scores offer an alternative way to adjust sampling weights for noncoverage of nontelephone households.

The estimated propensity scores come from a logistic regression model for the probability of a household's being a nontelephone household. Ideally, the predictor variables should have a substantive connection with the probability. To build such a model, however, one must have an external file of data on both nontelephone households and telephone households. Then one can apply the fitted model for the households in a telephone survey. Key requirements for this approach are that all the predictor variables in the model (based on the external file) must be available in the survey and that the definitions of those variables in the two sources must be consistent (e.g., categorical predictors should use the same categories). The results reported by Ferraro and Brick (2001) came from such a favorable situation. To evaluate methods of adjusting for nontelephone households, they used the 1997 National Survey of America's Families (NSAF), which collected data from both telephone and nontelephone households. As their external file they used the 1997 March supplement of the Current Population Survey. They built a logistic regression model on that file, applied it to the telephone households in the NSAF sample, and used the resulting propensities to form weighting classes (for use as part of a raking procedure).

Our analysis applied the nontelephone-propensity method to the persons in telephone households in the 2001–2002 NHIS Person File. To form the external file that includes telephone households and nontelephone households, we combined the 2001 and 2002 March CPS Supplements. As predictor variables in a household-level logistic regression model we used variables that were available and measured consistently in the 2001 and 2002 March CPS Supplements and in the 2001–2002 NHIS Person File:

(1) Census Region
(2) type of living quarters
(3) a household classification of race/ethnicity
(4) household income
(5) receipt of Food Stamps
(6) an age classification of household members
(7) household size
(8) highest level of educational attainment
(9) a household-level unemployment indicator.

We prefer a household-level model rather than a person-level model because telephone status is a household-level characteristic and, for example, we do not want to classify persons within the same household differently with respect to telephone status. For each person in the 2001 and 2002 March CPS Supplements, we used the logistic regression model to estimate the probability that the person resides in a nontelephone household. The predicted probabilities were ordered from lowest to highest, and quintiles were used to divide persons into five approximately equal groups (20th percentile = 0.014, 40th percentile = 0.022, 60th percentile = 0.037, and 80th percentile = 0.072). We formed nontelephone-propensity control totals by summing the CPS March Supplement weights for the persons in each of the five groups (weighting classes).

To examine whether the propensity-score model has significant predictive ability, we computed the area under the receiver operating characteristic curve (ROC). This area, 0.77, is considered acceptable discrimination (Hosmer and Lemeshow 2000).

In theory, conditioning on the true propensity score ensures balance on the covariates on which it is based. That is, given the value of the propensity score, the distribution of the covariates is the same in the two groups (here, nontelephone and telephone households). It is customary to estimate the propensity score by logistic regression and to base the conditioning on the quintiles of the estimated propensity score. Thus, we assessed balance by first comparing the unconditional distributions of the nine covariates between nontelephone and telephone households and then comparing the conditional distributions within each of the quintiles. As the measure of balance, for each category of each covariate, we calculated the absolute standardized difference

$$d = \frac{100|p_t - p_{nt}|}{\sqrt{\dfrac{p_t(1-p_t) + p_{nt}(1-p_{nt})}{2}}}$$

where p_t is the percentage of telephone households within a category of a specific predictor variable and p_{nt} is the corresponding percentage of nontelephone households. A value of d larger than 10% is regarded as an indication of imbalance (Love 2005). On the unconditional distribution, all nine covariates had $d > 10\%$ in at least one category, and the total number of categories with $d > 10\%$ was 26 (83.9% of 31 categories). For the conditional distributions within the quintiles, the total number of categories with $d > 10\%$ was 23 (15% of $5 \times 31 = 155$). For six covariates out of nine, 80% of the values d were less than 10%. Thus, use of propensity score to form groups has substantially reduced (though not eliminated) the covariate imbalance, suggesting that the predictive power of the model is reasonable.

The coefficients of the logistic regression model were then used to assign a predicted probability of residing in a nontelephone household to each person residing in a telephone household in the 2001–2002 NHIS Person File. These predicted probabilities placed each person in one of five weighting classes defined by the quintile boundaries from the CPS. The NHIS interim weights were then raked to two margins. The first margin consisted of the 88 NHIS poststratification cells. The second margin consisted of the five CPS nontelephone-propensity weighting classes. The sum of the five weighting classes was adjusted so that it agreed exactly with the sum of the control totals for the 88 NHIS cells. The raking converged in 10 iterations under the fairly strict convergence criterion of a difference no larger than 10. The percentage value of 10 relative to the control total is less than 0.1%. The resulting nontelephone-propensity weight was then used in the analysis discussed below.

5. Application of the Adjustment Procedures in the NHIS

The 2001–2002 NHIS was well-suited for this analysis because it covered both telephone and nontelephone households. Thus, Person File estimates for all persons using the final NHIS weight are estimates for the entire population. The 182,154 persons in telephone households out of a total sample of 194,146 persons in the NHIS Person File can be considered as a proxy for an RDD sample. For each of these persons, we created a simple-poststratification weight, an interruption-in-telephone-service weight, and a nontelephone-propensity weight, as described in Sections 2, 3, and 4, respectively.

Previous work (Frankel et al. 2003) identified 12 Person File variables that were associated with telephone status (Table 1). We expected that the interruption and nontelephone-propensity methods would yield estimates that were closer to the full-sample estimates than the simple-poststratification estimates, but most telephone surveys will also include variables that have weak associations with telephone status. In order to assess whether these adjustment techniques can harm accuracy, as measured by the mean squared error, for variables unrelated to telephone status, we chose four additional Person File variables for analysis. By examining the association with telephone status of all remaining relevant variables in the Person File, we identified four variables that had essentially no association with telephone status (Table 1).

Table 1. 2001–2002 NHIS variables used in the analysis

Variables that are associated with telephone status
Family income below $20,000
Education less than 8th grade
Amount spent on medical care less than $2,000
No health insurance
Medicaid
Authorized to receive Food Stamps
Received interest from savings bank accounts
Private health insurance
Income from welfare
Ratio of family income to poverty threshold less than 0.50
Fair/poor health
No health care due to cost
Variables that are not associated with telephone status
Because of a health problem, had difficulty walking without any special equipment
Limited in any way because of difficulty remembering or experience periods of confusion
During the past 12 months a patient in a hospital overnight
During the past 12 months received care from doctors or other health care professionals 10 or more times

For the 16 variables Table 2 gives the estimates from the full 2001–2002 NHIS sample and the three adjustment methods. The bias in the estimates from the three adjustment methods is measured as the difference from the full-sample estimate, which includes both telephone and nontelephone households. The mean squared error (MSE) of each estimate is computed as the square of the standard error plus the square of the bias. The last column of the table gives the ratio of the MSE of the propensity estimate to the MSE of the interruption estimate. For 10 of the 12 variables related to telephone service, the propensity method has a larger mean squared error than the interruption method. The ratio of the mean squared errors ranges from a high of 13.0 to a low of 1.2 for the 10 variables, and 6 of those 10 ratios exceed 3.0. The interruption method does not perform as well for the variables not related to telephone service. Its mean squared error is smaller for only one of those four variables, but the ratios (of MSE for propensity method to MSE for interruption method: 0.63, 1.10, 0.28, 0.29) tend to be closer to 1 than those for the variables that are related to telephone service.

The interruption method has a lower mean squared error than simple poststratification for the exact same 10 variables related to telephone service. On the other hand, the propensity estimate has a marginally larger MSE than the simple-poststratification estimate for 9 of the 12 variables. One reason that the simple-poststratification adjustment does slightly better than the propensity method in the application to the NHIS may be its use of a large number of poststratification cells. It is possible that, within the 88 poststratification cells formed by the cross-classification of race/ethnicity, gender, and age, the characteristics of the telephone population and the small nontelephone population are somewhat similar. The propensity adjustment, which incorporates the adjustment to the 88 cells, appears to offer little additional bias reduction for the variables studied, and the greater variability in the weights leads to a slight increase in MSE.

As indicated earlier, though the percentage of households in the U.S. without telephone service is small overall, it is somewhat higher for low-income and minority households. The adjustment for nontelephone households accounts for a larger percentage of households in these two groups. Therefore, it is of interest to see how the three methods perform for these subgroups.

Table 3 gives estimates for the subpopulation consisting of persons in households with income below 200% of the U.S. federal poverty level. The interruption method has a smaller mean squared error than the propensity method for 11 of the 12 variables related to telephone service. The 12 ratios range from 0.9 and 1.2 to 4.0 and 7.3. It does not perform as well for variables not related to telephone service; the ratios for the four variables range from 0.6 to 0.9. A similar result was found for the lower-income subpopulation consisting of persons in households below the poverty level. Excluding the variable "Family income below \$20,000," for 8 of the 11 variables related to telephone service, the interruption method has a smaller mean squared error than the propensity method.

Table 4 gives the estimates for three race/ethnicity groups. The "Other" group includes all other races and Hispanics. The interruption method gives a smaller mean squared error for minority groups, for variables related to telephone service. The ratio of the mean squared errors depends on the variable, though the interruption method does better than the propensity method for all three groups. For example, for "Medicaid" and "No health insurance," the MSE ratio is higher for the White non-Hispanic group than for the other

Table 2. Bias and mean squared error of simple poststratification, propensity, and interruption estimates. The estimates are weighted percentages of persons in the specified category of each variable

Variable	Estimate and standard error				Bias			Mean squared error			
	FS	Simple PS	Prop score	Interrup.	Simple PS	Prop score	Interrup.	Simple PS	Prop score	Interrup.	Prop/ Inter
Family income below $20,000	16.9476 (.2170)	16.1015 (.2171)	16.0421 (.2168)	17.0292 (.2454)	−0.8460	−0.9055	0.0816	0.7629	0.8668	0.0669	12.9607
Education less than 8th grade	19.1635 (.1369)	19.3893 (.1384)	19.3746 (.1382)	19.5018 (.1422)	0.2258	0.2111	0.3383	0.0701	0.0637	0.1346	0.4728
Amount spent on medical care less than $2,000	71.6706 (.2439)	72.9888 (.2419)	72.9807 (.2421)	72.6523 (.2541)	1.3183	1.3102	0.9817	1.7964	1.7752	1.0283	1.7263
No health insurance	12.6505 (.1615)	12.0623 (.1591)	12.0487 (.1591)	12.5563 (.1705)	−0.5882	−0.6018	−0.0942	0.3713	0.3874	0.0379	10.2146
Medicaid	7.6721 (.1396)	7.2600 (.1342)	7.2473 (.1342)	7.7093 (.1554)	−0.4120	−0.4248	0.0372	0.1878	0.1984	0.0255	7.7738
Authorized to receive Food Stamps	93.8207 (.1300)	95.3604 (.1129)	95.3667 (.1127)	94.9544 (.1268)	1.5398	1.5460	1.1337	2.3836	2.4029	1.3014	1.8463
Received interest from savings bank accounts	68.2626 (.3243)	68.8390 (.3274)	68.7777 (.3284)	69.1592 (.3304)	0.5764	0.5150	0.8965	0.4394	0.3731	0.9130	0.4087
Private health insurance	68.5834 (.3013)	70.5932 (.2967)	70.6297 (.2967)	69.5238 (.3234)	2.0098	2.0463	0.9404	4.1271	4.2752	0.9889	4.3231
Income from welfare	1.3893 (.0496)	1.2877 (.0472)	1.2855 (.0471)	1.4159 (.0551)	−0.1017	−0.1039	0.0266	0.0126	0.0130	0.0037	3.4767
Ratio of family income to poverty threshold less than 0.50	3.1960 (.1101)	2.9272 (.1112)	2.9200 (.1108)	3.2299 (.1328)	−0.2688	−0.2760	0.0339	0.0846	0.0884	0.0188	4.7055
Fair/poor health	9.0624 (.1141)	8.8908 (.1145)	8.8716 (.1142)	9.1752 (.1248)	−0.1716	−0.1908	0.1128	0.0426	0.0494	0.0283	1.7470

Table 2. Continued

Variable	Estimate and standard error				Bias			Mean squared error			
	FS	Simple PS	Prop score	Interrup.	Simple PS	Prop score	Interrup.	Simple PS	Prop score	Interrup.	Prop/ Inter
No health care due to cost	4.6798 (.0739)	4.4808 (.0721)	4.4710 (.0720)	4.8654 (.0877)	−0.1990	−0.2088	0.1855	0.0448	0.0488	0.0421	1.1589
Because of a health problem had difficulty walking without any special equipment	3.4346 (.0575)	3.4155 (.0584)	3.4086 (.0582)	3.4874 (.0606)	−0.0191	−0.0260	0.0528	0.0038	0.0041	0.0065	0.6305
Limited in any way because of difficulty remembering or experience periods of confusion	1.9892 (.0435)	1.9350 (.0428)	1.9313 (.0427)	2.0371 (.0489)	−0.0541	−0.0578	0.0479	0.0048	0.0052	0.0047	1.1027
During the past 12 months a patient in a hospital overnight	8.5005 (.0833)	8.5506 (.0876)	8.5427 (.0875)	8.6582 (.0959)	0.0500	0.0421	0.1576	0.0102	0.0094	0.0340	0.2772
During the past 12 months person received care from doctors or other health care professionals 10 or more times	9.9065 (.0991)	9.9794 (.1001)	9.9719 (.1000)	10.1030 (.1049)	0.0729	0.0653	0.1965	0.0153	0.0143	0.0496	0.2876

FS = full-sample, PS = poststratification, Interrup. = Interruption, Prop Score = Propensity Score.

Table 3. Bias and mean squared error of simple poststratification, propensity, and interruption estimates for persons below 200% of poverty level. The estimates are weighted percentages of persons in the specified category of each variable

Variable	Estimate and standard error				Bias			Mean squared error			
	FS	Simple PS	Prop score	Interrup.	Simple PS	Prop score	Interrup.	Simple PS	Prop score	Interrup.	Prop/Inter
Family income below $20,000	57.5516 (.5720)	56.0344 (.6176)	55.9990 (.6174)	56.9362 (.6668)	−1.5172	−1.5526	−0.6154	2.6832	2.7918	0.8234	3.3907
Education less than 8th grade	29.6224 (.3206)	29.5621 (.3308)	29.5635 (.3309)	29.5866 (.3543)	−0.0603	−0.0589	−0.0358	0.1130	0.1129	0.1268	0.8905
Amount spent on medical care less than $2,000	68.8041 (.5017)	69.4338 (.5173)	69.4315 (.5170)	68.8986 (.5632)	0.6297	0.6274	0.0945	0.6641	0.6609	0.3262	2.0262
No health insurance	23.9808 (.3633)	23.0655 (.3631)	23.0952 (.3633)	23.5977 (.4041)	−0.9153	−0.8856	−0.3831	0.9696	0.9162	0.3101	2.9549
Medicaid	21.6910 (.4312)	20.9727 (.4272)	20.9905 (.4273)	21.6201 (.4911)	−0.7183	−0.7005	−0.0709	0.6985	0.6733	0.2463	2.7340
Authorized to receive Food Stamps	87.2718 (.3283)	88.1717 (.3185)	88.1627 (.3186)	87.2622 (.3501)	0.8999	0.8908	−0.0096	0.9113	0.8951	0.1227	7.2970
Received interest from savings bank accounts	89.5555 (.2689)	89.1600 (.2822)	89.1853 (.2821)	89.5139 (.2897)	−0.3955	−0.3702	−0.0415	0.2361	0.2166	0.0856	2.5297
Private health insurance	38.7963 (.5268)	40.4503 (.5477)	40.4200 (.5475)	39.4043 (.6098)	1.6540	1.6237	0.6080	3.0357	2.9362	0.7416	3.9594
Income from welfare	4.3501 (.1847)	4.1254 (.1834)	4.1305 (.1833)	4.4433 (.2061)	−0.2247	−0.2196	0.0932	0.0841	0.0818	0.0511	1.6000
Ratio of family income to poverty threshold less than 0.50	14.8196 (.4461)	13.8892 (.4671)	13.8957 (.4669)	14.5526 (.5261)	−0.9304	−0.9239	−0.2670	1.0839	1.0715	0.3481	3.0781
Fair/poor health	15.2647 (.2979)	15.0611 (.3022)	15.0358 (.3015)	15.3682 (.3338)	−0.2036	−0.2289	0.1035	0.1328	0.1433	0.1221	1.1732
No health care due to cost	9.4557 (.1957)	9.0583 (.1912)	9.0608 (.1913)	9.7124 (.2374)	−0.3974	−0.3948	0.2567	0.1945	0.1925	0.1223	1.5740

Table 3. Continued

Variable	Estimate and standard error				Bias			Mean squared error			
	FS	Simple PS	Prop score	Interrup.	Simple PS	Prop score	Interrup.	Simple PS	Prop score	Interrup.	Prop/Inter
Because of a health problem had difficulty walking without any special equipment	5.7026 (.1623)	5.7843 (.1655)	5.7666 (.1649)	5.7742 (.1741)	0.0817	0.0641	0.0716	0.0341	0.0313	0.0354	0.8835
Limited in any way because of difficulty remembering or experience periods of confusion	3.7799 (.1291)	3.7099 (.1302)	3.7024 (.1299)	3.9100 (.1513)	−0.0700	−0.0775	0.1301	0.0219	0.0229	0.0398	0.5749
During the past 12 months a patient in a hospital overnight	10.7390 (.1816)	10.7476 (.1895)	10.7359 (.1893)	10.8262 (.2138)	0.0086	−0.0031	0.0872	0.0360	0.0358	0.0533	0.6721
During the past 12 months person received care from doctors or other health care professionals 10 or more times	12.0190 (.2314)	12.0838 (.2383)	12.0678 (.2379)	12.1379 (.2520)	0.0648	0.0489	0.1190	0.0610	0.0590	0.0777	0.7596

FS = full-sample, PS = poststratification, Interrup. = Interruption, Prop Score = Propensity Score.

Table 4. Bias and mean squared error of simple poststratification, propensity, and interruption estimates by race/ethnicity. The estimates are weighted percentages of persons in the specified category of each variable

Variable	Race/ethnicity	Estimate and standard error				Bias			Mean squared error			
		FS	Simple PS	Prop Score	Interrup.	Simple PS	Prop Score	Interrup.	Simple PS	Prop Score	Interrup.	Prop/ Inter
Family income below $20,000	White, non-Hispanic	13.6241	13.0106	12.9395	13.7519	−0.6135	−0.6846	0.1279	0.4310	0.5229	0.0911	5.7422
		(.2386)	(.2337)	(.2330)	(.2734)							
	Black, non-Hispanic	27.8185	26.0425	26.0219	28.0084	−1.7760	−1.7966	0.1899	3.6368	3.7106	0.6855	5.4133
		(.7036)	(.6947)	(.6949)	(.8058)							
	Other	22.7418	21.6647	21.6174	22.5902	−1.0771	−1.1244	−0.1516	1.3647	1.4686	0.2559	5.7388
		(.4463)	(.4523)	(.4520)	(.4826)							
Education less than 8th grade	White, non-Hispanic	15.9201	16.1448	16.1264	16.2027	0.2246	0.2063	0.2825	0.0731	0.0651	0.1041	0.6254
		(.1485)	(.1506)	(.1502)	(.1559)							
	Black, non-Hispanic	21.8620	22.0914	22.0885	22.2385	0.2294	0.2265	0.3765	0.1473	0.1461	0.2543	0.5746
		(.3115)	(.3076)	(.3079)	(.3354)							
	Other	30.4597	30.7779	30.7568	31.0526	0.3182	0.2971	0.5928	0.2302	0.2175	0.4890	0.4448
		(.3516)	(.3591)	(.3595)	(.3709)							
Amount spent on medical care less than $2,000	White, non-Hispanic	72.7326	73.9766	73.9648	73.7066	1.2440	1.2322	0.9740	1.6273	1.5980	1.0353	1.5436
		(.2773)	(.2823)	(.2823)	(.2944)							
	Black, non-Hispanic	69.1778	70.9359	70.9390	70.2078	1.7581	1.7612	1.0300	3.4654	3.4763	1.5628	2.2243
		(.6383)	(.6119)	(.6120)	(.7085)							
	Other	69.1192	70.4045	70.4068	70.0847	1.2853	1.2876	0.9655	1.8877	1.8935	1.2114	1.5631
		(.4861)	(.4854)	(.4855)	(.5285)							
No health insurance	White, non-Hispanic	8.9007	8.3845	8.3693	8.8208	−0.5162	−0.5314	−0.0799	0.2885	0.3044	0.0342	8.9135
		(.1592)	(.1487)	(.1486)	(.1666)							
	Black, non-Hispanic	15.4056	14.8190	14.8184	15.6501	−0.5866	−0.5872	0.2445	0.4975	0.4984	0.2778	1.7941
		(.3941)	(.3917)	(.3919)	(.4669)							
	Other	25.9710	25.1916	25.1606	25.6389	−0.7794	−0.8104	−0.3321	0.8132	0.8628	0.3181	2.7119
		(.4428)	(.4537)	(.4540)	(.4559)							
Medicaid	White, non-Hispanic	4.7100	4.3554	4.3411	4.7363	−0.3546	−0.3688	0.0263	0.1416	0.1518	0.0228	6.6458
		(.1349)	(.1260)	(.1257)	(.1488)							

Table 4. Continued

Variable	Race/ethnicity	Estimate and standard error				Bias			Mean squared error			
		FS	Simple PS	Prop Score	Interrup.	Simple PS	Prop Score	Interrup.	Simple PS	Prop Score	Interrup.	Prop/ Inter
	Black, non-Hispanic	16.9768	16.1008	16.0951	17.1324	−0.8760	−0.8818	0.1556	1.0206	1.0307	0.3757	2.7432
		(.5027)	(.5032)	(.5032)	(.5929)							
	Other	13.1101	12.8471	12.8280	13.1385	−0.2630	−0.2821	0.0284	0.1822	0.1925	0.1380	1.3947
		(.3341)	(.3362)	(.3360)	(.3704)							
Authorized to receive Food Stamps	White, non-Hispanic	95.3144	96.8166	96.8237	96.4875	1.5022	1.5094	1.1731	2.2701	2.2916	1.3948	1.6430
		(.1352)	(.1160)	(.1158)	(.1363)							
	Black, non-Hispanic	86.8801	89.0507	89.0524	88.2739	2.1706	2.1723	1.3938	4.8819	4.8891	2.1431	2.2813
		(.4454)	(.4128)	(.4127)	(.4477)							
	Other	92.6819	93.9068	93.9154	93.4586	1.2249	1.2335	0.7767	1.5502	1.5711	0.6712	2.3407
		(.2449)	(.2235)	(.2228)	(.2607)							
Received interest from savings bank accounts	White, non-Hispanic	61.5915	62.1406	62.0631	62.4457	0.5491	0.4715	0.8542	0.4478	0.3695	0.8799	0.4199
		(.3816)	(.3825)	(.3836)	(.3877)							
	Black, non-Hispanic	84.2247	85.1151	85.0988	85.6888	0.8904	0.8742	1.4642	1.0884	1.0604	2.4337	0.4357
		(.5741)	(.5436)	(.5443)	(.5385)							
	Other	84.0717	84.6744	84.6271	84.8184	0.6027	0.5554	0.7467	0.5568	0.5033	0.7555	0.6662
		(.4301)	(.4399)	(.4414)	(.4448)							
Private health insurance	White, non-Hispanic	76.0085	77.9572	77.9988	77.0159	1.9488	1.9904	1.0074	3.8825	4.0464	1.1229	3.6034
		(.3033)	(.2913)	(.2911)	(.3287)							
	Black, non-Hispanic	53.1892	55.5952	55.6011	53.7180	2.4060	2.4119	0.5288	6.2383	6.2672	0.8412	7.4503
		(.7010)	(.6703)	(.6706)	(.7494)							
	Other	49.2961	51.1060	51.1675	50.1580	1.8100	1.8714	0.8619	3.7211	3.9485	1.1935	3.3083
		(.6423)	(.6672)	(.6681)	(.6713)							
Income from welfare	White, non-Hispanic	0.7631	0.7092	0.7070	0.8256	−0.0539	−0.0561	0.0625	0.0047	0.0049	0.0068	0.7217
		(.0432)	(.0418)	(.0417)	(.0536)							
	Black, non-Hispanic	3.5011	3.2257	3.2236	3.4704	−0.2753	−0.2774	−0.0306	0.1202	0.1213	0.0522	2.3245
		(.2083)	(.2107)	(.2105)	(.2264)							

Table 4. Continued

Variable	Race/ethnicity	Estimate and standard error				Bias			Mean squared error			
		FS	Simple PS	Prop Score	Interrup.	Simple PS	Prop Score	Interrup.	Simple PS	Prop Score	Interrup.	Prop/Inter
	Other	2.4358	2.2728	2.2694	2.3627	−0.1630	−0.1665	−0.0731	0.0487	0.0497	0.0311	1.5995
		(.1600)	(.1488)	(.1483)	(.1604)							
Ratio of family income to poverty threshold less than 0.50	White, non-Hispanic	2.2576	2.0984	2.0903	2.3151	−0.1592	−0.1673	0.0575	0.0418	0.0444	0.0281	1.5772
		(.1255)	(.1284)	(.1280)	(.1575)							
	Black, non-Hispanic	6.4378	5.6583	5.6564	6.5414	−0.7794	−0.7814	0.1036	0.6959	0.6990	0.2115	3.3043
		(.2995)	(.2973)	(.2972)	(.4481)							
	Other	4.7093	4.3720	4.3627	4.6056	−0.3373	−0.3466	−0.1037	0.1659	0.1719	0.0716	2.4001
		(.2262)	(.2283)	(.2276)	(.2467)							
Fair/poor health	White, non-Hispanic	8.6136	8.4376	8.4134	8.6634	−0.1760	−0.2002	0.0498	0.0521	0.0611	0.0265	2.3034
		(.1463)	(.1455)	(.1451)	(.1551)							
	Black, non-Hispanic	12.2620	12.0048	11.9992	12.6118	−0.2573	−0.2628	0.3498	0.1960	0.1993	0.2729	0.7304
		(.3447)	(.3602)	(.3609)	(.3880)							
	Other	8.6098	8.5175	8.5093	8.8106	−0.0923	−0.1005	0.2008	0.0385	0.0401	0.0781	0.5135
		(.1677)	(.1733)	(.1732)	(.1943)							
No health care due to cost	White, non-Hispanic	4.3980	4.1763	4.1633	4.5287	−0.2217	−0.2347	0.1307	0.0569	0.0628	0.0281	2.2337
		(.0905)	(.0880)	(.0877)	(.1050)							
	Black, non-Hispanic	6.0689	5.9537	5.9530	6.5590	−0.1152	−0.1159	0.4901	0.0512	0.0513	0.3010	0.1704
		(.1964)	(.1948)	(.1946)	(.2464)							
	Other	4.8379	4.6746	4.6709	5.0317	−0.1634	−0.1670	0.1938	0.0517	0.0529	0.0714	0.7412
		(.1547)	(.1582)	(.1583)	(.1841)							
Because of a health problem had difficulty walking without any special equipment	White, non-Hispanic	3.6704	3.6527	3.6442	3.6992	−0.0177	−0.0262	0.0288	0.0053	0.0056	0.0060	0.9314
		(.0690)	(.0706)	(.0703)	(.0722)							
	Black, non-Hispanic	4.0650	4.0095	4.0060	4.1980	−0.0555	−0.0590	0.1330	0.0282	0.0286	0.0478	0.5979
		(.1583)	(.1585)	(.1584)	(.1734)							

Table 4. Continued

Variable	Race/ethnicity	Estimate and standard error				Bias			Mean squared error			
		FS	Simple PS	Prop Score	Interrup.	Simple PS	Prop Score	Interrup.	Simple PS	Prop Score	Interrup.	Prop/ Inter
	Other	2.0237 (.0982)	2.0148 (.1014)	2.0133 (.1013)	2.1112 (.1138)	−0.0089	−0.0104	0.0875	0.0104	0.0104	0.0206	0.5029
Limited in any way because of difficulty remembering or experience periods of confusion	White, non-Hispanic	2.0270 (.0549)	1.9625 (.0547)	1.9581 (.0545)	2.0498 (.0614)	−0.0644	−0.0689	0.0228	0.0071	0.0077	0.0043	1.7960
	Black, non-Hispanic	2.5589 (.1159)	2.5409 (.1198)	2.5394 (.1197)	2.7100 (.1324)	−0.0180	−0.0195	0.1511	0.0147	0.0147	0.0404	0.3646
	Other	1.4287 (.0728)	1.3872 (.0738)	1.3854 (.0736)	1.5033 (.0843)	−0.0415	−0.0434	0.0746	0.0072	0.0073	0.0127	0.5766
During the past 12 months a patient in a hospital overnight	White, non-Hispanic	8.7611 (.1035)	8.8579 (.1071)	8.8473 (.1068)	8.9394 (.1147)	0.0968	0.0863	0.1784	0.0209	0.0188	0.0450	0.4190
	Black, non-Hispanic	8.9104 (.2331)	8.9260 (.2470)	8.9259 (.2472)	9.1164 (.2735)	0.0156	0.0155	0.2061	0.0613	0.0614	0.1173	0.5233
	Other	7.1463 (.1481)	7.0187 (.1547)	7.0178 (.1548)	7.1784 (.1758)	−0.1276	−0.1285	0.0321	0.0402	0.0405	0.0320	1.2665
During the past 12 months person received care from doctors or other health care professionals 10 or more times	White, non-Hispanic	10.8310 (.1250)	10.9189 (.1266)	10.9104 (.1264)	11.0382 (.1310)	0.0879	0.0794	0.2072	0.0237	0.0223	0.0601	0.3705
	Black, non-Hispanic	9.0147 (.2296)	8.9940 (.2356)	8.9916 (.2356)	9.1658 (.2478)	−0.0207	−0.0232	0.1510	0.0560	0.0560	0.0842	0.6654
	Other	6.7742 (.1410)	6.8273 (.1488)	6.8239 (.1487)	6.9441 (.1646)	0.0530	0.0497	0.1699	0.0250	0.0246	0.0559	0.4392

FS = full-sample, PS = poststratification, Interrup. = Interruption, Prop Score = Propensity Score.

two groups, whereas for “Authorized to receive Food Stamps,” the MSE ratio is higher for the Black and Other groups than for White non-Hispanic. For “Income from Welfare” the ratio is smaller than 1.0 for the White non-Hispanic group and larger than 1.0 for the other two groups, but this pattern is reversed for “Fair/Poor health.” Thus, for variables that are correlated with telephone service, the interruption method does well in compensating for noncoverage.

In situations where it is not possible to determine the size of the nontelephone population, one may be able to apply the interruption-based adjustment with an assumed ratio of the size of the population of nontelephone households to that of households with interruptions. As described in Section 3, we studied the interruption-based adjustment with an assumed ratio of 2, 2.5, and 3. Table 5 gives the mean squared errors of these three estimates. The mean squared error of the estimate based on a ratio of 3 appears to come closest to the regular interruption method (for the 2001–2002 NHIS the actual ratio was 3.4).

6. Discussion

Tables 2–5 show that the interruption method, which involves a straightforward adjustment, reduces bias in the estimates for variables related to telephone service and generally has a smaller mean squared error than either simple poststratification or the propensity method. Thus, the interruption-in-telephone-service method works well for variables related to telephone service. It is simple and does not need an external file for its implementation.

Even if the exact size of the nontelephone population is not known, a reasonable approximate adjustment based on estimated size may lead to estimates that have a smaller mean squared error than simple poststratification. Thus the interruption method can be applied to surveys covering geographic areas for which no independent estimates of telephone coverage exist.

Evidence from previous studies (Keeter 1995; Brick, Waksberg, and Keeter 1996; Frankel et al. 1999) indicates that households with telephones at the time of the survey and with interruptions in telephone service have similar characteristics to households without telephones at the time of the survey. We think the reason that the interruption method does better than the propensity approach is this strong similarity between the two groups. The interruption method has some drawbacks: (1) a question or questions on interruptions in telephone service must be included in the survey questionnaire, (2) a reliable estimate of the number of nontelephone households or the number of individuals living in nontelephone households is required, and (3) the method depends on one variable (the number reporting interruptions in telephone service in the survey).

We speculate that propensity-score models may not do well because they do not include the interruption-in-telephone-service variable. We believe that one of the limitations of using a propensity-score model for nontelephone adjustment is finding the right variables for the model and having them available for the survey. Also, the propensity method has the disadvantage of being able to use (in the logistic regression model) only variables that are available in the survey and in an external file and are measured in the same way in both. In the absence of such files, it is not possible to apply

Table 5. Bias and mean squared error of alternative interruption estimates that use an assumed ratio of the population number of nontelephone households to the population number of households with interruptions. The estimates are weighted percentages of persons in the specified category of each variable

Variable	Estimate and standard error				Bias			Mean squared error		
	FS	Intrp 2.0	Intrp 2.5	Intrp 3.0	Intrp 2.0	Intrp 2.5	Intrp 3.0	Intrp 2.0	Intrp 2.5	Intrp 3.0
Family income below $20,000	16.9476	16.5096	16.7053	16.8959	−0.4380	−0.2423	−0.0517	0.2431	0.1129	0.0602
	(.2170)	(.2264)	(.2327)	(.2399)						
Education less than 8th grade	19.1635	19.4383	19.4620	19.4854	0.2748	0.2985	0.3219	0.0947	0.1086	0.1234
	(.1369)	(.1384)	(.1394)	(.1408)						
Amount spent on medical care less than $2,000	71.6706	72.8408	72.7698	72.7006	1.1702	1.0992	1.0301	1.4292	1.2695	1.1241
	(.2439)	(.2446)	(.2474)	(.2510)						
No health insurance	12.6505	12.2784	12.3827	12.4847	−0.3721	−0.2678	−0.1658	0.1647	0.0988	0.0556
	(.1615)	(.1620)	(.1646)	(.1678)						
Medicaid	7.6721	7.4575	7.5523	7.6447	−0.2146	−0.1197	−0.0274	0.0660	0.0357	0.0237
	(.1396)	(.1415)	(.1461)	(.1514)						
Authorized to receive Food Stamps	93.8207	95.1821	95.0964	95.0129	1.3614	1.2757	1.1922	1.8671	1.6419	1.4367
	(.1300)	(.1169)	(.1201)	(.1238)						
Received interest from savings bank accounts	68.2626	68.9786	69.0463	69.1126	0.7160	0.7837	0.8499	0.6200	0.7219	0.8309
	(.3243)	(.3277)	(.3284)	(.3294)						
Private health insurance	68.5834	70.1247	69.8989	69.6784	1.5413	1.3155	1.0950	2.4693	1.8279	1.3005
	(.3013)	(.3062)	(.3120)	(.3185)						
Income from welfare	1.3893	1.3441	1.3712	1.3975	−0.0452	−0.0181	0.0082	0.0045	0.0030	0.0029
	(.0496)	(.0495)	(.0513)	(.0534)						
Ratio of family income to poverty threshold less than 0.50	3.1960	3.0612	3.1250	3.1868	−0.1348	−0.0711	−0.0092	0.0322	0.0202	0.0166
	(.1101)	(.1184)	(.1232)	(.1287)						
Fair/poor health	9.0624	9.0152	9.0753	9.1340	−0.0471	0.0129	0.0716	0.0161	0.0146	0.0202
	(.1141)	(.1180)	(.1202)	(.1228)						
No health care due to cost	4.6798	4.6489	4.7301	4.8096	−0.0309	0.0503	0.1297	0.0069	0.0091	0.0240
	(.0739)	(.0773)	(.0808)	(.0847)						
Because of a health problem had difficulty walking without any special equipment	3.4346	3.4467	3.4619	3.4768	0.0121	0.0273	0.0422	0.0036	0.0043	0.0054
	(.0575)	(.0589)	(.0594)	(.0600)						

Table 5. Continued

Variable	Estimate and standard error				Bias			Mean squared error		
	FS	Intrp 2.0	Intrp 2.5	Intrp 3.0	Intrp 2.0	Intrp 2.5	Intrp 3.0	Intrp 2.0	Intrp 2.5	Intrp 3.0
Limited in any way because of difficulty remembering or experience periods of confusion	1.9892 (.0435)	1.9793 (.0448)	2.0009 (.0462)	2.0221 (.0477)	−0.0098	0.0118	0.0329	0.0021	0.0023	0.0034
During the past 12 months a patient in a hospital overnight	8.5005 (.0833)	8.5975 (.0904)	8.6202 (.0922)	8.6425 (.0943)	0.0970	0.1197	0.1420	0.0176	0.0228	0.0290
During the past 12 months person received care from doctors or other health care professionals 10 or more times	9.9065 (.0991)	10.0333 (.1013)	10.0594 (.1024)	10.0850 (.1038)	0.1267	0.1528	0.1784	0.0263	0.0338	0.0426

FS = full-sample; Intrp 2.0, Intrp 2.5, and Intrp 3.0 denote the interruption estimates that use 2.0, 2.5, and 3.0, respectively, as the assumed ratio of the population number of nontelephone households to the population number of households with interruptions.

this method. For example, if the external file does not contain information on health insurance though the survey collects this information, it cannot be used as a predictor of telephone status though it is an important variable for predicting telephone status. Also, household income, a variable that has a strong association with telephone status, may be available in the telephone survey and the external file, but the survey may only use a single income question, whereas the external file may use multiple questions to determine income by source, making the use of household income in the model problematic, especially if the level of income item nonresponse is high in the telephone survey. Even if one is considering using the propensity method, it is advisable to include the interruption-in-telephone service questions in the survey as a fall-back method to guard against the possibility that the propensity model will not have acceptable predictive ability. The interruption method may slightly increase the mean squared error for variables not related to telephone status, but it substantially decreases the bias in the estimates of variables related to telephone service.

Further evaluation of the interruption-in-telephone-service method is needed because the number of households that only have cellular telephone service has been increasing. Blumberg and Luke (2007) report that the percentage of households that only have cellular telephone service increased by a large amount, from 3.2% in January–June 2003 to 12.8% in July–December 2006. During this same time period the percentage of households without any telephone service remained about the same, 2.0% and 2.2%, respectively. Although the size of the nontelephone population has not declined in recent years, the percentage of households that have only landline telephone service has dropped substantially, from 43.0% in January–June 2003 to only 29.6% in July–December 2006, while the percentage of households with landline and cellular telephone service has increased by only a small amount, from 42.4% to 44.3%. In July–December 2006 landline RDD telephone surveys excluded 15.0% (12.8% + 2.2%) of households in the U.S. This coverage problem is expected to increase in the next several years, and the percentage of households that are excluded from RDD surveys is probably considerably higher in some states and sub-state areas and among population subgroups such as young adults. One approach to reducing bias from noncoverage of cellular-only households is to also draw a random sample of telephone numbers from dedicated cellular 1,000 banks (Link et al. 2007). The sample could be screened to identify households that only have cellular telephone service. Another approach is to identify households in a landline RDD sample that also have cellular telephone service and ask whether they had an interruption in landline telephone service during the year. Households with an interruption in landline service might be used as a proxy for households that have only cellular telephone service. Finally, if one conducts a landline RDD survey and a cellular telephone number survey, it may be possible to ask appropriate sample groups questions on interruptions in landline service and/or interruptions in cellular service, and use this information to make a better adjustment for households without any telephone service. Another possibility is to identify a subgroup of persons with both landline and cellular service similar in demographic characteristics to those with only cellular service. This subgroup can be used as a proxy to adjust for cellular-only persons. This method assumes some knowledge of the cellular-only population.

7. References

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