Bias in Telephone Samples

Telephone surveys, the staple of many social surveys, face a number of difficulties that place into question the efficacy of the method. First, respondents are wary of calls from unknown individuals, in response to telemarketing and other security issues (Tourangeau 2004). Second call screening devices, such as caller identification (Caller-ID) and answering machines have grown in popularity. These devices allow potential respondents to decide when and for whom to answer the telephone, possibly avoiding the initial contact and survey request (Tuckel and O'Neill 1995, 1996, 2001). A third issue is the dedication of telephone lines to facsimile machines and Internet connections, thereby precluding possible contact (Tuckel and O'Neill 2001). Finally, there has been a general increase in cellular telephone usage in lieu of landline service, although the extent of this is still undetermined (Steeh 2004, Tourangeau 2004). These factors, and possibly others, have all probably contributed to the overall decline in telephone survey response rate (Steeh 1981, Groves and Couper 1998, Steeh et al 2001, Tuckel and O'Neill 2002). The decline in response rates may lead to increased nonresponse error, although the overall effect of this on telephone surveys is not clear (Keeter et al 2000).

Decreased landline ownership, however, will lead to coverage errors that could be removed by including cellular phones in a telephone sample. Whether this noncoverage will lead to a bias or little or no effects as in nonresponse studies is unknown. It is known that noncoverage of those without telephone service at all can bias results (Botman and Allen 1990, Groves 1989, Thornberry and Massey 1988, Hall et al 1999). The size of this population is small; in 2003, the estimated percentage of households with a telephone stood at 95.3 percent (FCC 2004). Changes in this number are likely to happen rapidly, as people choose cellular technology as their only source of telephone communication. Findings from the Consumer Expenditure

Interview Survey (CEIS) showed that households reporting a cell phone bill rose from two percent in 1994 to 25 percent in 2001, increasing to 47 percent in 2003 (Tucker et al 2004). Those reporting only a cell phone bill in the survey rose from one to four percent from 2001 to 2003 (Tucker et al 2004). Data from the February 2004 CPS supplement indicated that the cell only population stood at 6 percent, and 5.1 percent of households owned no phone at all (Tucker et al 2004). This is significant in that the cell only population now outnumbers the population with no phone. These numbers are not only changing fast, but the CPS data failed to measure all potential respondents due to a survey error (a faulty skip pattern which systematically did not ask questions of some of the sample), so new measures may be more accurate in estimating cell only prevalence (Tucker et al 2004).

Coverage error in telephone samples therefore now come from several sources, rather than just from those who do not own telephones. Each of these sources of error may have different effects on the estimates of interest. For example, Brick et al (1995) note that population members owning a phone but not covered in list-assisted designs are likely different than those not owning a phone at all. Similarly, those owning a cell phone only are likely to be different from landline owners that are not covered in list-assisted samples, or from those who own no phone at all. This follows the same idea that different sources of nonresponse leads to differential effects on estimates (Groves 1989). Following this logic, coverage error is parameterized by Equation 1:

$$\overline{Y} = \frac{N_c}{N}\overline{Y}_c + \frac{N_{cell}}{N}\overline{Y}_{cell} + \frac{N_{list}}{N}\overline{Y}_{list} + \frac{N_{nt}}{N}\overline{Y}_{nt} \quad (1)$$

Where Y is the statistic of interest for the full population;

N = total population of interest

N_c= number of population covered in the telephone frame,

$\overline{Y}_c$ = value of statistic of interest for population covered in the telephone frame

N_{cell}= number of population in cell phone only population

$\overline{Y}_{cell}$ = value of statistic of interest for population in cell phone only population

N_{list} = number of population not covered by list-assisted sample frame

$\overline{Y}_{list}$ = value of statistic of interest for population not covered by list-assisted sample frame

N_{nt} = number of population not owning a telephone

$\overline{Y}_{nt}$ = value of statistic of interest for population not owning a telephone

This equation is similar in structure to the one presented by Groves (1989) for nonresponse. Each source of error contributes to the equation depending on the size on the noncovered populations and difference between each one and the covered population. Research in Slovenia estimated a cell only population of about 10 percent, and that the effect of not including these cell only respondents changes the distributions of estimates (Vehovar et al 2004).

The equation can be altered as the sample design changes. If cellular numbers are included in the sample, then the portion of the equation in this equation is removed. Similarly, if a method alternative to list-assisted sampling is employed this term would drop. The term for those not owning a telephone would never drop in a telephone survey, unless it was included as a mixed-mode design that incorporated methods that could reach this population. In this case, all of the terms bias would be dropped.

Measurement error in questionnaires

Coverage error has often been estimated directly from survey data. Telephone ownership estimates often come from face to face surveys (Brick et al 1995, Federal Communication Commission 2003, Thornberry and Massey 1988). Estimates of coverage error in list-assisted frames for telephone samples have similarly come from survey data (Brick et al 1995). Survey data are often measured with error, however, which makes interpretation of the resulting estimates difficult (Biemer and Trewin 1997). When these data are used to estimate and model coverage error, the resulting estimate for coverage error will be inaccurate. For example, if one is estimate the value of N_{cell} from survey data, and that data is measured with error, then the estimate of N_{cell} becomes

$$\hat{N}_{cell} = \sum_i y_i = \sum_i (\mu_i + \varepsilon_i) = \sum_i \mu_i + \sum_i \varepsilon_i$$

where y_i is an indicator for population member *i*, 0 indicating owning a landline, 1 indicating owning only a cell phone. μ_i is the true value for population member *i* for the indicator, and ε_i is the measurement error of the true indicator for population member *i*. Since the true value is binary, the error terms can take on three values: -1 if μ_i =1 (i.e. respondent is cell only but reports owning landline); 1 if μ_i = 0 (i.e. respondent owns a landline, but reports being cell only); or 0, i.e. $\mu_i = y_i$, the respondent reports the true value. If the errors sum to zero, then the estimate on the number owning a cell phone only is equal to the true value. If however, it is not, then the analysis become more complicated. Let $\eta = \sum_i \mu_i$, i.e. the true population size, and $\zeta = \sum_i \varepsilon_i$, the sum of the errors. In this case, Equation 1 would be estimated by (assuming the remaining parts are known population estimates):

$$\hat{Y} = \frac{N_c}{N}\hat{Y}_c + \frac{\eta + \zeta}{N}\hat{Y}_{cell} + \frac{N_{list}}{N}\hat{Y}_{list} + \frac{N_{nt}}{N}\hat{Y}_{nt} =$$
$$\frac{N_c}{N}\hat{Y}_c + \frac{\eta}{N}\hat{Y}_{cell} + \frac{\zeta}{N}\hat{Y}_{cell} + \frac{N_{list}}{N}\hat{Y}_{list} + \frac{N_{nt}}{N}\hat{Y}_{nt} \quad (2)$$

where $\hat{Y}$ is the estimated population value. Equation 2 is the same as Equation 1 with the addition of $\frac{\zeta}{N}\hat{Y}_{cell}$. Thus, estimation of coverage error incorporates a term that is a function of the size of the errors when population size is estimated directly from survey data. In the likely event that the statistic of interest (i.e. Y) was also measured with error in the cell population, then Y_{cell} would be by a similar true and error value, and an additional two terms would be added to Equation 2.

A number of methods have been developed to estimate the prevalence of error in objective measures. These have relied heavily on remeasurement methods, where a second source of data (e.g. records, multiple questions within one survey, or multiple measures across time) is collected (Biemer 2004). The reliability of the multiple measures, κ, is an oft-used statistic that measures chance-adjusted agreement (Biemer 2004). Reinterview data that allows for ‘better’ methods and/or ability for reconciliation has been used as a gold-standard to assess error in the original measure (Sinclair and Gastwirth 1993). This technique often fails to meet the assumptions, which makes its efficacy uncertain (Sinclair and Gastwirth 1993). When such assumptions (and thus methods) fail, using Latent Class Models (LCM) may be advantageous (Biemer 2004). LCM are based on theory similar to that underlying factor analysis, but uses categorical data, where the latent ‘true’ measure(s) is extracted from multiple observed indicators (also categorical), which may be measured with error (Hagenaars 1993).

Error in categorical data is itself categorical. In binary data, error can be classified as false positive (i.e. y_i =1| μ_i =0) or false negative (i.e. y_i =0| μ_i =1). Let θ be the probability of a false positive and ϕ be the probability of a false negative. The complements are:1-θ, the specificity, and 1- ϕ the sensitivity of the test (Hui and Walter 1980). Let π represent the true probability (prevalence) of μ_i occurring in the population. Further, let A, B, C, etc., represent indicators of the underlying latent variable, with a,b,c, etc. be the value of the observed indicators obtained from the survey. Let X represent the latent 'true' value of μ_i, and x represent the possible values of the latent variable X. LCM estimates X, θ, and ϕ given the indicators A, B, C, etc. Depending on the number of indicators and on whether the multiple indicators were collected in one survey or across time, the models developed to date diverge and make different assumptions. All LCM can be seen in terms of log-linear analysis with latent categorical variables, and thus all belong to the same family of estimation procedures (Hagenaars 1993).

Using three indicators from one survey, Biemer and Witt (2001) estimate a log-linear model incorporating latent class variables to estimate error in drug use reports. For three measures across time, Markov assumptions have been placed on the LCM (Biemer and Bushery 1997)[1]. When only two indicators are available, the Hui-Walter LCM is most appropriate (Biemer 2004). This method has been applied to labor force data to estimate misclassification error. The data were collected over two interview periods and involved three category observed and latent variables (Sinclair and Gastwirth 1993). Biemer and Witt (1996) applied the method to drug use data prior to the redesign of the National Household Survey on Drug Abuse (NHSDA), with both indicators coming from the same survey data.

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[1] Markov assumptions are that the error in the first measure is independent of the error in the other two measures, the second measure error is conditionally independent on the first and only the first, and the third is conditionally independent on the first and second, but does not impact the other measure.

In the current application, two indicator variables from the same survey are used (A,B), to determine error in reports about belonging to the cell only population. This makes the Hui-Walter method most appropriate. Hui and Walter (1980) developed the model to examine the accuracy of a new diagnostic test (specifically, in medicine/epidemiology) compared to a standard test with unknown error rates. There are two indicators of X, A and B. In order to achieve a model that is identifiable, several assumptions are made. First, the sample must be divided into two mutually exclusive subpopulations (e.g. by gender). Let *g* denote the subpopulations, for g =(1,2). Further, these two subgroups are assumed to have the same error rates, i.e. $\theta_{g=1} = \theta_{g=2} = \theta$ and $\phi_{g=1} = \phi_{g=2} = \phi$. However, these two subgroups are assumed to have different prevalence rates, i.e. $\pi_{g=1} \neq \pi_{g=2}$. Finally, conditional on X, A and B are independent, i.e. $\pi_{ab|x} = \pi_{a|x}\pi_{b|x}$.

Previous research has assumed equality in error rates across the two groups used, with little examination of such an assumption (Sinclair and Gastwirth 1993, Biemer and Witt 1996). This research has applied only to two populations of interest. Different selections in populations should lead to different estimates of error and of the true underlying value. In this way, the Hui-Walter test can be examined for possible instabilities or possible violations of assumptions, as well as different effects of population types on error rates. If two groupings (4 populations, e.g. men/women and white/non-white) similar in size produce vastly different results, then two possibilities arise. One is that the groups are vastly different, e.g. that gender leads to less error than race. The other is that one of the two groupings violate the assumptions of the Hui-Walter method. Some groupings may not follow the assumptions laid out, while others may.

The Hui-Walter parameters are all estimated through maximum likelihood methods (see Hui and Walter [1980] p. 168 for the equation to be maximized) or Bayesian techniques using the EM algorithm (Pouillot et al 2002). There are in general, for *R* (i.e. A,B) indicators, $(2^R - 1)g$ degrees of freedom in which to estimate (2R+1)g parameters (Hui and Walter 1980). Thus, when there are two indicators and two subpopulation, the degrees of freedom and the number of parameters to estimate are both equal to 6, so the model is saturated. No model diagnostics are estimable with zero degrees of freedom. Further, the assumptions of all LCM, including the Hui-Walter, are not always tenable, especially the conditional independence of errors in indicators, in particular when the two indicators come from the same interview (Biemer 2004). For these reasons, the Hui-Walter and other LCM are most appropriate as diagnostic tools for individual questions, to identify those that are problematic (possibly error-prone) (Biemer 2004). The Hui-Walter method is therefore used in this analysis as a diagnostic tool to identify the more appropriate (less error-prone) indicator of the cell-only population. This is done in order to most accurately estimate the coverage error by not including cell only into a telephone sample, by minimizing the effect of $\frac{\zeta}{N}$.

Methods

The data to examine these errors came from a dual-frame sample national survey. The two frames covered all fifty states. The first used a standard list-assisted RDD sample design to reach the landline owning population. The other frame contained the known possible cell phone number universe, with sampled units selected randomly.[2] The RDD sample was stratified by county, and the cell sample was stratified by county and carrier. A total of 12,448 numbers were selected, 8,000 from the cell phone frame, and 4,448 from the RDD frame (using 1+ list assisted

[2] Survey Sampling International used a database creating 1000 blocks (as opposed to the standard RDD 100-blocks). The numbers were generated from known cell exchanges. This provided a universe of 282,722,000 potential cellular phone numbers, while the RDD frame had a universe of 264,362,500 possible numbers.

methods). Attempts to contact and complete surveys for sample members from both frames were by telephone from Westat's Telephone Research Center (TRC).[3] Portions of the samples were released the summer of 2004, between July 19 and September 5. The survey asked up to 48 questions and lasted an average of 8.3 minutes.

The survey designers expected a significantly lower response rate for the cell phones and so selected more numbers from the cell phone frame. However, the difference in response rates, while significant, was not as large as expected. The unweighted landline overall response rate (completed screener and completed survey) calculated AAPOR's response rate 3 (RR3) criteria was 31.9%, while the cell phone overall unweighted RR3 was 20.7%, giving a total overall RR3 across surveys of 24.3%.[4] This, in combination with similar rates of nonworking numbers/ineligible respondents (56.7% for landline, 52.4% for cell phones), resulted in more cell phone surveys being completed. There were 768 surveys completed on a cell phone and 556 surveys by landline, for 1324 completed surveys overall.

There were two questions from which estimates of the cell only population could be derived. The first was the second question asked of cell respondents, which asked "Is the cell phone your only phone or do you also have a regular telephone at home?" (Measure A).[5] Respondents answering yes to this question are coded '1', while those indicating owning a landline are coded '0'. The second question came in the middle of the survey, in a set of questions taken from the CPS February 2004 supplement on telephone usage. It asked of all respondents, "I would like to ask about any regular telephone numbers that your household has. These numbers can be used for different reasons, including making or receiving calls, for

[3] No provisions were made for non-English speaking respondents, or respondents using a telephone device for the deaf, so these portions of the population are not represented in this survey.
[4] The more stringent RR1 (not estimating ineligibles from the unknown eligibility numbers) for the landline overall was 29.0%, for cell 20.2%, for a total RR1 of 23.8%.

computer lines, or for a fax machine. How many different regular telephone numbers does your household have?" (Measure B).[6] Depending on their response to this question, respondents were routed to alternative questions asking about how many (or if) of these lines were answered by the respondent or anyone else in the household. An indicator of cell only status was created from these respondents, with respondents answering they owned zero 'regular' telephones coded '1', and those responded they owned one or more coded as a '0'.

Data for each question had missing data. The first measure (A) had only one missing case, while the second (B) had ten missing cases. These cases were imputed by calculating probabilities of belonging to one of the two categories, cell only or landline only. Probabilities were calculated by first estimating a logistic regression using Measure A as the dependent variable. The equation was taken from a previous paper that examined this probability (Albaghal 2005)[7]. Since these variables were selected for Measure A, the same method and variables were applied to Measure B to remain consistent. Both equations and methods used can be found in Appendix A. In both cases, all missing data were imputed as '0', indicating these respondents were expected to own a landline.

Several demographic variables were also imputed. Race, income, and age were all imputed. There were 70 missing cases for age, 26 for race, and 270 for income. Age was imputed first, then race, and then income, with the each successive imputation including the previously imputed values. The complete methods can be found in Appendix A. The imputed values for these variables were used to estimate probabilities for the cell only variables as mentioned above.

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[5] Landline respondents were asked a similar question whether their landline was their only phone or whether they owned a cell phone as well.

[6] This question differed slightly from the version CPS asked in the Feb. 2004 supplement. This asked: "First I would like to ask about any regular, landline telephone numbers in your household. These numbers are for phones plugged into the wall of your home and they can be used for different reasons, including making or receiving calls, for computer lines or for a fax machine. How many different landline telephone numbers does your household have?"

[7] Conference paper, presented at 2005 Annual Midwest Political Science Association Conference.

While this can create the processing errors mentioned previously (Biemer and Lyberg 2003), it is not examined in the study. Further, due to the small number of data imputed on the dependent variables, the hope is that the error introduced is also small.

We treat landline respondents as if they have known true values on the measure of interest - that is, whether they are cell only or not (i.e. X=0) - and the probabilities of interest are fixed for these respondents. Therefore, for the examination of error, only cell phone responses (n=768) are used. When examining national estimates (weighted) the entire sample is used (n=1324), as the weights were developed for this complete data set.

Weights adjusted for unequal probabilities of selection and nonresponse, and post-stratified to the entire population. Unequal probabilities of selection arose for sample members who own more than one landline/cell phone and/or those who own a combination of the two devices. When presenting national estimates, I use weights applying Taylor series approximation to estimate standard errors. To calculate the LCM, I use the TAGS module developed for the R statistical package (Pouillot et al 2002).

Results

Table 1 presents the crosstabulation of responses to the two measures. This shows that although the majority of respondents show a concordance in response (the diagonal), a significant number gave contradictory responses (the off-diagonal). This discordance occurs exclusively in the lower off-diagonal cell, with respondents responding to the first measure as ‘cell-only’, but in the second measure indicating they owned a landline. These 43 cases make up about 5.5% of the cell phone respondents, and about 3.2% of the overall sample (n=1324).

Table 1: Response Distributions for Two Cell Only Measures (Cell Respondents)

Measure	**B = Landline Owner**	**B = Cell Only**	**Total**
A = Landline Owner	585 (100.0)	0 (0.0)	585 (100.0)
A = Cell Only	43 (23.5)	140 (76.5)	183 (100.0)

Estimates of the cell only population and the coverage error that arises from failure to include this population depends on how these cases are classified. Weighted data give national estimates of the cell only population as 10.5% of households if Measure A is used, and 8.6% from Measure B[8]. The weighted estimate, standard errors (through Taylor Series approximation), and confidence intervals for the national estimates of cell only households are given in Table 2. Although these numbers are not statistically significantly different from one another, they are practically different from one another. Using the point estimates leads to two percent variations in the estimates at the national level, and while using the confidence interval estimates would lead to overlap in estimates, large variations can still occur. For example, the lower 95% confidence interval for the Measure B national estimate is 6.8%, while the upper estimate from Measure A is 12.6%, a change of nearly six percent in estimates.

Table 2: National Estimates of Cell Only from Two Measures

	Measure A National Estimate	**Measure B National Estimate**
Estimate (Standard Error)	10.54% (1.03%)	8.62% (0.94%)
Lower 95% CI	8.51%	6.77%
Upper 95% CI	12.57%	10.47%

Table 3 presents a breakdown of the demographic characteristics of the cell only owners and their landline owning counterparts. These results divide the cell sample according to the results from Measure A. Dividing the sample by the second measure, on how many regular

[8] The weights are developed by Westat. The larger number, from Measure A, is the estimate given by Westat .

phones are owned results in similar distributions, none of these demographics estimates are different beyond what is accountable to sampling error. Cell only respondents are younger, are more likely to be men, make less money, are more likely to be a minority group, are more likely to live alone (although the average number per household is the same), and are less likely to be married or own their home. These findings parallel findings in Europe (Vehovar et al 2004). However, the differences in the percentage of blacks and Hispanics are not significant, and education level is the same, indicating these groups are not appropriate for use with the Hui-Walter method. The remainder of the divisions apparently meet the criteria that the two populations have different prevalence of cell only status.

Table 3. Breakdown of Cell Only and Landline Owners

	Owning Cell Phone Only	**Landline Owning**
Mean Age	32.1	40.8*
Median Income	$25,001-35,000	$50,001-75,000*
Median Education	Some college less than 4 year degree	Some college less than 4 year degree
Percent Female	40%	51%*
Percent White	63%	71%*
Percent Black	16%	15%
Percent 'Other' Race	21%	14%^
Percent Hispanic	11%	10%
Number in Household	2.2	2.3
Percent Single HH	34%	18%*
Percent Married	19%	55%*
Percent Owning Home	26%	69%*

^ difference significant at $p<.10$ level, *difference significant at $p<.05$ level. Baseline 768 cell phone responder cases, 183 cell only, 585 landline owning, although some cells have slightly smaller n due to missing data.

It is possible that in some of these groups the error rate is not uniform, which violates an assumption of the Hui-Walter method. There is, however, no reason to assume a priori that the contrasting groups suffer from differential error rates. To form two populations, age is grouped 18-39 years and 40 and over years old. Income is grouped as above or below $50,000. I examine two racial categories: white and non-white. Using level of education as a measure of cognitive ability, and thus propensity to err, very few differences occur across all demographic groups. The median in all populations is the same, “some college, less than 4-year degree”. Table 4 presents the results from applying the Hui-Walter method to the different groups that are significantly different in prevalence. The tables include estimates of percent of false positives for Measures A and B (False Pos-A,B), the percent of false negatives (False Neg-A,B), the ‘true’ value estimate from the LCM, and the actual estimates from Measures A and B for the two populations.

Table 4: Hui-Walter Estimates Based on Different Subgroups

Population	N	False Pos-A	False Pos-B	False Neg-A	False Neg-B	‘True’	Estimate- A	Estimate-B
Gender								
Men	394	0.0292	0.000	0.000	0.1537	0.2549	0.2766	0.2157
Women	374					0.1738	0.1979	0.1471
Race								
Nonwhite	236	0.000	0.000	0.000	0.2350	0.2839	0.2839	0.2034
White	532					0.2180	0.2180	0.1729
Age								
18-39	416	0.000	0.000	0.000	0.2350	0.3389	0.3389	0.2548
40 +	352					0.1193	0.1193	0.0966
Marital								
Not married	404	0.0222	0.000	0.000	0.1723	0.3469	0.3614	0.2871
Married	353					0.0787	0.0992	0.0652
Home owner								
Don’t own	312	0.0097	0.000	0.000	0.2061	0.4239	0.4295	0.3365
Own	449					0.0982	0.1069	0.0780
HH Size								
Single HH	164	0.0673	0.000	0.000	0.000	0.3659	0.3780	0.3659
Multi HH	599					0.1319	0.1987	0.1319
Income								
Below $50K	383	0.0075	0.000	0.000	0.2160	0.3397	0.3446	0.2663
Above $50K	449					0.1259	0.1325	0.0987

The results seemingly indicate that Measure B (asking about ‘regular phones’) is the more error prone of the two questions, with high levels of estimated false negatives amongst almost all populations. Except for household composition, the estimated rate of false negatives ranged from about 15% to 23.5% of the responses across populations. Contrasting households compositions, however, led to an estimate of no error in Measure B and an estimate of nearly 7%

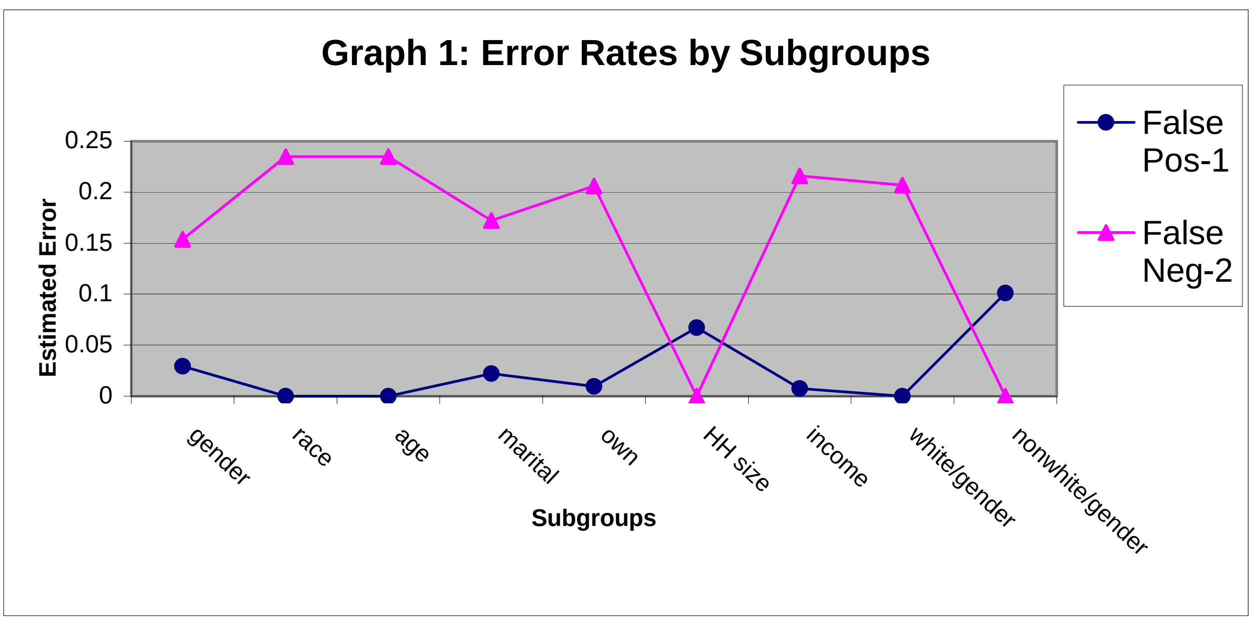
false positives in Measure A. This is higher than the estimates of false positives in Measure A across the remaining populations, which ranged from zero to about three percent. When estimates of no error for one measure occurred (i.e. race and age for Measure A, household composition for Measure B), the 'true' value is simply the estimate from the measure found to have no error.

For all subpopulations, there were no estimated cases of false positives for Measure B or false negatives for Measure A. This follows as a result of the sampling zero in the upper off-diagonal cell. If the results are accurate, this indicates too many reported being cell only in response to Measure A, and not enough reported this status Measure B. Thus, in the case of accuracy of the Hui-Walter method, these two measures would be bounds on the range of the cell only population. The fact that some tests identify one measure as error free while others estimates error to be present in that measure, however signals the possibility of inaccuracy in estimates. Especially telling is the fact that Measure B is estimated to have no error in the comparison involving household composition (single person vs. multi-person household), while it is otherwise estimated to have a large amount of error (false negatives). This may be in part due to the unequal population sizes of the two types of households, but equality in size is not a requirement for the Hui-Walter test. The fact that there is no error found in Measure A in two of the tests is also questionable, but since the remainder of tests (sans household composition) show low levels of error associated with Measure A, this result is at least more plausible.

To examine these estimates further, two new populations were formed, based on race crossed with gender, which displayed different error distributions as separate subgroups. Thus white men are compared with white women, and non-white men compared with non-white women. Table 5 presents the result to the Hui-Walter estimates for these two groups. The differences between white and non-white males and females differ sharply, with all the estimated error in whites being false negatives to Measure B, and all the estimated error for non-white coming from false positives in Measure A. This does not follow the estimates found for race and gender separately, pointing to instability. The instability in error estimates across populations are clearly seen in Graph 1, with an almost random appearance.

Table 5: Hui-Walter Estimates Based on Gender by Race Subgroups

Population	N	False Pos-1	False Pos-2	False Neg-1	False Neg-2	'True'	Estimate- A	Estimate-B
White								
White Male	278	0.000	0.000	0.000	0.2069	0.2662	0.2662	0.2014
White Female	254					0.1654	0.1654	0.1417
Non-White								
Non-White Male	116	0.1011	0.000	0.000	0.000	0.2500	0.3017	0.2500
Non-White Female	120					0.1583	0.2667	0.1583



These discrepancies and instabilities in the estimates indicate possible inadequacies with the Hui-Walter method, at least in the current application. Violation of the assumptions of the method can make results uninterpretable and unstable. A further problem could be the sampling zero in the upper cell of the off-diagonal. We examine these possibilities by changing some of the 43 discordant cases to fall into the upper off-diagonal. Two new variable codings were created. The first recoded all of the female discordant cases (n=19) from the lower to the upper off-diagonal cell. This creates a clear violation of the Hui-Walter method, as the error rates in the in the two populations are completely different. The second coding randomly assigned the 43 discordant cases into either the upper or lower off-diagonal. This removes the sampling zero noted previously. In both cases, the men and women comprise the two populations. Table 6 presents the Hui-Walter estimates from the two different codings.

Table 6: Hui-Walter Estimates with Reassignment of Discordance Based on Gender

Population	N	False Pos-A	False Pos-B	False Neg-A	False Neg-B	'True'	Estimate- A	Estimate-B
Error by Gender								
Men	394	0.000	0.0315	0.000	0.1463	0.2738	0.2766	0.2157
Women	374					0.1471	0.1979	0.1471
Error Random								
Men	394	0.000	0.0331	0.000	0.1411	0.2538	0.2538	0.2386
Women	374					0.1684	0.1684	0.1765

The results further indicate difficulty with viewing the Hui-Walter method as accurate, due to fluctuations in findings. In both cases, all of the error came from Measure B. While this occurred in previous estimations, these two cases led to part of the error in Measure B estimated as coming from false positives. While changes in estimates are expected as the data was manipulated, it is difficult to understand why all of the error is estimated from B (thus Measure A equals the 'True' estimate). This instability may be more expected in the assignment of discordant cases by gender, as the assumptions are violated by choice, and because there is a sampling zero in both lower (for males) and upper (for females) off-diagonal cells. The error estimates, however, are almost identical for the random assignment of discordant cases, which may actually make the assumptions more tenable, and eliminates the sampling zeros. Since both cells of the off-diagonal have cases, it seems questionable that there is no error estimates in Measure A, especially when previous estimates of the error across genders found error in A (Table 4).

Discussion

While these estimates may be confusing and unstable, this does not mean they have no use. As Biemer (2004) points out, even in the face of the stringent assumptions, such models can point to flawed questions post-data collection. As such, it seems apparent from the majority of results that Measure A more accurately gauges the cell only population and is more appropriate for use in data collection for estimation of coverage error. Only in two groupings did the error in Measure A (false positives) exceed that for Measure B.

It is possible that the error arises from the fact that ‘regular telephone’ is not defined in Measure B. The 43 respondents with discordant answers to Measures A and B have a lower median education level (4.5, in between high school and some college, to 5, some college) and significantly fewer college graduates (about 33% to 14%) than the remainder of the population, which may indicate the potential for greater error. The discordant responders were also significantly younger than the remainder of the cell respondents, 28.8 to 39.3 mean age. A younger generation may think of cellular telephones as ‘regular’ as they have become an integral part of their daily life.

There is some support this possibility. Following the question about numbers and usage of ‘regular telephones’ a similar series of questions were asked about cellular phones in the household. In 14 of the 43 discordant cases, the difference between the number of ‘regular telephones’ and cellular phones was zero. This indicates the possibility that these individuals view cellular telephones as 'regular', and that when responding to these two questions, the understood reference is the same set of telephones.

There is, however, evidence that Measure B may be better if the goal is to measure telephone coverage of households, further placing into question the efficacy of the Hui-Walter test in this instance. First, two of the discordant respondents indicated that of the 'regular telephones' they identified, none them were answered. Since they were answering by cell phone, it can be assumed they meant landline telephones when referring to 'regular telephones', and therefore gave a false positive for Measure A.

Second, the two measures differ in the exact concepts they measure. Measure A asks whether the *respondent's* cell phone is his or her only phone or whether he or she has another 'regular' phone. This wording may also serve the purpose of cueing the respondent as to what 'regular' refers to, decreasing the likelihood errors are made in reference to what 'regular' means in later in the survey when Measure B is asked. Measure B, conversely, asks about telephones in the *household*. Children living at home with their parents, shared rental units, and students in communal facilities may have landline telephones in their households, but not feel they themselves have access to that connected line. Indeed, only two of the 43 discordant cases live by themselves. The remaining members of a household also are likely to use some telephone technology, other than the cell phone used during the interview. It could easily be that the discrepancies in questions led to the discordance in answers. If so, when calculating *household* coverage rather than *personal* coverage, as this is the purpose of this survey, Measure B may actually be more accurate, indicating a lower prevalence than estimates from Measure A.

Conclusions

The growing difficulties in telephone surveys should lead researchers to examine different ways to overcome these problems. Researchers could find alternate modes of surveys to employ, or to alter techniques in telephone surveys. Adding cell phones to survey samples does some of both. However, the prevalence and potential for the coverage error incurred by not including these depends on estimates that likely to come from surveys themselves. If the measure of prevalence also contains error, then estimates of coverage error become difficult to interpret and inaccurate.

The Hui-Walter method for estimating the true prevalence and error rates (false positives and negatives) when two indicators but no gold standard is available provides a possible tool to assess measurement error in surveys. There are a number of assumptions placed on the data, which possibly may be violated in survey data. Still, the tests may provide an insight into which questions may be most flawed. It may still be necessary, however, to examine additional variables that are measured in the same survey context that may give clues which questions were the most flawed.

This study continues initial research in the potential coverage error of cell phones and measurement error generally in surveys. More research should focus on accurate measurements of the cell only population, and the potential it has to bias results by not including it in survey samples. More work on the efficacy of methods such as the Hui-Walter and other latent class models to estimate measurement error in surveys is also worthwhile. It is possible that such assumptions made the models are untenable in a survey setting, and thus are inappropriate for this application. The potential benefits seem clear, however, and if barriers do exist, researchers should develop techniques to overcome them.

Appendix A. Imputation Method.

Age
To impute data, the method began with the age variable. Categorical means were calculated first using the following set of variables:

Education, Gender, Hispanic origin, Black, Other Race, Ownership of Home, Occupational Status (last week), Marital Status

This led to 550 combinations. If a respondent with missing data had matching values to any of these 550 combinations, their age was imputed to equal the mean of that combination. Since missing data on age still existed, using the data set with the newly imputed data, means were again calculated, except using the set of variables above minus one, in this case it was both racial categories. Again, age for missing cases was imputed to equal the mean of the combination they matched. Using the new data, the means were recalculated replacing marital status with racial categories. The method continued to use the above variables in all combination until all missing data for age was imputed.

To multiply impute race, the data set created with complete age data was used. A propensity equation was calculated for both black and other races using logistic regression. The models initially used the variables that were found to be related through chi-square tests.

Race
For black respondents these variables were: age, education, cell response, Hispanic, Occupational Status, South region, West region

For ‘other’ respondents these variables were: age, education, cell response, Hispanic, West region

The equations estimated were used to calculate probabilities of respondents being either black or other based on their given data on the independent variables. The threshold was set at 0.3, i.e. those above the 0.3 threshold were then counted as black or other. Since not all these data were available, again different combinations were calculated, similar to the method used in age. The final equations were:

1.
black = ((1)/(1+e**-(-.4475 -(.0228*age) - (.1244*educ)+ (.3346*cell)-(1.2746*hisp) - (.3850*work status) + (.5986*south) -(.8765*west))))

other = ((1)/(1+e**-(-2.2609 -(.0330*age) - (.0893*educ)+ (.5693*cell)-(2.6346*hisp) +(1.3262*west))))

2.
black = ((1)/(1+e**-(-.6021 -(.0213*age) - (.1144*educ)+ (.3110*cell)- (.3757*work status) + (.5448*south) -(.9882*west))));

other = ((1)/(1+e**-(-1.2914 -(.0348*age) - (.0042*educ)+ (.5366*cell) + (1.3926*west))));

3. (LAST STEP NEEDED TO COMPLETE 'OTHER')
black = ((1)/(1+e**-(-1.057 -(.0221*age) + (.3170*cell) - (.4457*work status) + (.5444*south) - (1.0102*west))));

other = ((1)/(1+e**-(-1.3450 -(.0336*age) + (.5255*cell) +(1.3951*west))));

4. (LAST STEP NEEDED TO COMPLETE BLACK)
black = ((1)/(1+e**-(-1.2875 -(.0214*age) + (.2429*cell)+ (.5764*south) -(1.0041*west))));

There were five cases where the respondent self-identified as both black and some other race. In order to achieve mutual exclusive classification, these respondents were assigned to either the black or other group based on the probability calculated from equations used for imputation. This led to four of the five categorized as black, with the remaining one classified as 'other'.

Income

Income was done in two stages. First, whether the respondent made above or below $50,000 was imputed, as there was more complete data on these variables, and thus less error prone data to be used on the second stage. The method used here combined the method used for race and age. Equations were first estimated for the probability of above $50,000 with the threshold set at 0.5, i.e. those with probabilities greater than 0.5 were counted as making above, those with probabilities equal to 0.5 or below were counted as below. The second stage followed the same exact procedure as was used in age, except the median instead of mean was used, and the combination of variables were different. However, the variables used in both stages of imputation for income were used. These variables were selected by examining chi-square tests from the data set incorporating the imputed values for age and race. After the data was fully imputed for the first stage, data were separated by the above/below variable, and the second stage was completed on both sets using the same categorizing variables. After imputation, there were 658 cases below and 666 cases above. The variables:

Age, Education, Black, Other race, Gender, Occupational Status Marital Status Ownership of Home

The equations for stage one:

1.income = ((1)/(1+e**-(-3.4615 -(.0110*age) +(.4823*educ) -(.4873*black2)-(.3933*race2) - (.2516*gender) + (.5075*work status) + (.6338*married) + (1.3033*own))));

2. income = ((1)/(1+e**-(-3.5818 -(.0137*age) +(.4893*educ) -(.4787*blacknew)-(.3933*other) -(.2995*gender) + (.4980*work status) + (0.6316*married) + (1.3036*own))));

3.income = ((1)/(1+e**-(-3.5818 -(.0137*age) +(.4893*educ) -(.4787*blacknew)-(.3933*other) - (.2995*gender) + (.4980*work status) + (0.6316*married) + (1.3036*own))));

4. income = ((1)/(1+e**-(-3.5384 -(.0149*age) +(.5788*educ) -(.5528*blacknew)-(.4013*other) -(.4426*gender) + (0.6848*married) +(1.3981*own))));

5. income = ((1)/(1+e**-(-3.5366 -(.0012*age) +(.5377*educ) -(.6411*blacknew)-(.5188*other) -(.2707*gender) + (0.8244*married) +(0.6084*work status))));

6. income = ((1)/(1+e**-(-2.8651 -(.0151*age) +(.5814*educ) -(.5623*blacknew)-(.4133*other) -(.4521*gender) + (1.6504*own))));

7.income = ((1)/(1+e**-(-3.0983 -(.0046*age) +(.5620*educ) -(.6606*blacknew)-(.5219*other) -(.3965*gender) + (0.8154*married))));

8. income = ((1)/(1+e**-(-2.5926 -(.0009*age) +(.5422*educ) -(.7003*blacknew)-(.4832*other) -(.2847*gender) + (0.5666*work status))));

9. income = ((1)/(1+e**-(-2.1269 -(.0029*age) +(.5605*educ) -(.7404*blacknew)-(.5097*other) -(.3974*gender))));

10. income = ((1)/(1+e**-(-0.0085 -(.0154*age) +(1.5857*own) -(.6343*blacknew)-(.5097*other) -(.4110*gender))));

11. income = ((1)/(1+e**-(-0.4705 -(.0050*age) +(0.8325*married) -(.7556*blacknew)-(.5332*other) -(.3609*gender))));

12. (FINAL STEP) income = ((1)/(1+e**-(0.5027 -(.0029*age) + -(.8394*blacknew)-(.4956*other) -(.3541*gender))));

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