Estimation Issues in Dual Frame Sample of Cell and Landline Numbers

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1. Introduction

The numbers of households and persons in the U.S. who have cell phones have greatly increased in the last few years. Estimates from a supplement to the February 2004 Current Population Survey (CPS) show that over 50% of households had one or more cell phones, and about 6% had only cell phones (Tucker, Brick, and Meekins 2005[**]. The National Health Interview Study (NHIS) showed a rapid growth in cell phone-only households from 2003 to 2004 (Blumberg, Luke, and Cynamon 2005). Assuming the percentage of households with landline telephone service continues to decrease, the potential for undercoverage bias in random digit dial (RDD) telephone surveys will increase because these surveys generally exclude most cell phone numbers. In addition to the undercoverage bias, nonresponse bias in RDD surveys may grow with the prevalence of cell phones because it may be more difficult to reach households with landline phones if they rely on cell phones for most of their calls. Thus, sampling cell phones may be necessary to gain access to a growing proportion of households that use cell phones exclusively or extensively.

In 2004, the Joint Program in Survey Methodology (JPSM) Practicum Survey was designed to evaluate issues associated with conducting surveys on cell phones. The study employed a dual frame methodology, sampling telephone numbers from frames of both cell phone numbers and landline numbers. The Land sample can be used to estimate the number and characteristics of households that have only landline telephones. The Cell sample can be used to estimate the number and characteristics of households that have only cell phones. Both samples can be used to make estimates of the overlap population—those households with both types of telephones. Steeh (2004) reported on a similar study she conducted in 2003 to evaluate the feasibility of collecting data from a cell phone sample.

In the sections that follow, the survey design, data collection procedures, and response rates for both the Cell and Land sample are described. The findings from experiments that sent text messages and offered different levels of reimbursement to the Cell sample are presented. The primary focus is on the nature of the nonresponse bias that was encountered in the survey and its implications for combining the two samples and producing unbiased estimates from the respondent data. The last section discusses the implications for future surveys.

2. The Survey

Development of the survey instrument was guided by a review of relevant literature and two focus groups with cell phone users. The instrument consisted of a screener interview to verify that the number was residential and that the person answering the telephone was at least 18 years old, and for the Land sample was a household member, and an extended interview about phone ownership and usage, attitude towards cell phones, social behaviors and demographics. The main study was conducted between July 10, 2004 and September 5, 2004. Extended interviews lasted an average of about 9 minutes.

Experiments for the Cell sample were constructed to evaluate the effect of two incentive amounts and of sending text messages. The lower incentive was set at $5 and the higher incentive was set at $10. Payments were postpaid once respondents provided their name and address so that money could be sent to them. Of the sampled Cell sample numbers, 86% were text message capable as determined by the carrier, these were randomly assigned to either an experimental or control group.

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[**] All the CPS estimates presented here have standard errors less than 0.5%.

A text message[***] was sent to the numbers in the experimental group. No messages were sent to numbers from the control group. The two experiments were assigned independently. Thus, roughly 25% of the Cell sample was assigned to each of the four conditions (text message/no text message crossed with $5/$10 incentive). No experiments were done in the Land sample.

2.1 Sampling

The samples for both the Cell and Land samples were drawn by Survey Sampling International (SSI) from the May 2004 Telcordia data base. The Cell sample was selected from 1000-blocks in the 50 states and D.C. that were cellular, after sorting the file by FIPS state and county, Carrier name, and 1000-block. A systematic sample of 8,500 telephone numbers was drawn. A random sample of 500 8,000 were for the main data collection.

The Land sample followed the standard SSI procedure for drawing an equal probability sample for a list-assisted RDD survey. A systematic sample of 4,688 telephone numbers was drawn, and 4,488 were assigned to the main data collection. A total of 1,590 numbers were pre-identified non-working and non-residential by SSI and addresses were obtained for 2,084 Land numbers, to which prenotification letters were mailed. About 84% of all telephone numbers that were eventually identified as residential had address matches.

For both the Land and Cell samples, any person answering the phone who was a household member and at least 18 years old could respond for the entire household. There was no sampling of persons within the household. Interviews for both samples were administered using Westat's computer-assisted telephone interviewing (CATI) system.

2.2 Data Collection

The same calling protocol was used to dial both the Cell and Land samples. Up to 14 attempts were made to each case to establish contact. For the initial contacts, the scheduling algorithm scheduled calls over different times of the day and different days of the week. For both the Cell and Land samples if an answering machine was encountered and no prior call had contacted a person, a message was left identifying the survey and indicating callbacks would be forthcoming.

If a respondent refused to participate in the screener, a conversion attempt was made provided the refusal was not hostile. Refusal conversions were attempted for both the screener and the extended interviews. All Land sample refusals were attempted for conversion, and a random 75% of the initial screener refusals for the Cell sample were attempted. Conversions of the extended interview refusals were attempted for all Cell and Land sample cases.

An unanticipated problem arose in the Cell sample when the household respondent said they were not at least 18 years old. (In the Cell sample, these cases were coded "ineligible," while in the Land sample, the interviewer asked to speak to an adult). By the end of the survey period, 350 ineligible Cell sample cases had accumulated. A validation of a random sample of 50 ineligible cases found that only a few cases would have been classified as ineligible on the next call. While the validation was small-scale, it is clear that different methods are needed for handling this problem in the Cell sample.

2.3 Response Rates

Of the numbers dialed by interviewers (after the SSI purging of the Land sample), 41% of the Cell sample was residential and 51% of the Land sample was residential. The weighted screener response rate computed using AAPOR RR3, with weights adjusted for the subsampling of refusals in the Cell sample, was 26.5% for the Cell sample and 38.6%for the Land sample. These rates were computed using the CASRO method for the unresolved residency status numbers. The Cell sample cases that were originally coded as ineligible because the person answering the phone claimed to be less than 18 were treated as nonresponse for these computations, and this depresses the response rate for the Cell sample.

In each completed screener the household respondent (the adult answering the phone) was asked to respond for the household. For the main interviews, AAPOR RR2 was 83.5% for the Cell sample and 88.0% for the Land sample. The combined response rate (screener RR3 multiplied by extended interview RR2) was 22.1% for the Cell sample and 34.0% for the Land sample.

The response rates for the Cell sample experiments are reported here using the weighted RR3 rate for the screener and RR2 for the extended. The group assigned to the $10 incentive achieved a higher screener response rate than the $5 group (29.9% vs. 22.7%), and a higher extended rate (85.7% vs. 80.6%). The combined response rates over the screener and extended are substantially higher for the $10 group (25.8%) than for the $5 group (18.6%). The screener response rates for those in the group sent the text message and those in the group not sent the text message are not statistically different. Since the payment and the text messaging

[***] The message read: "Westat will call you for University of Maryland research study. $X for 10 minutes of your time." X was either 5 or 10 dependent on the treatment.

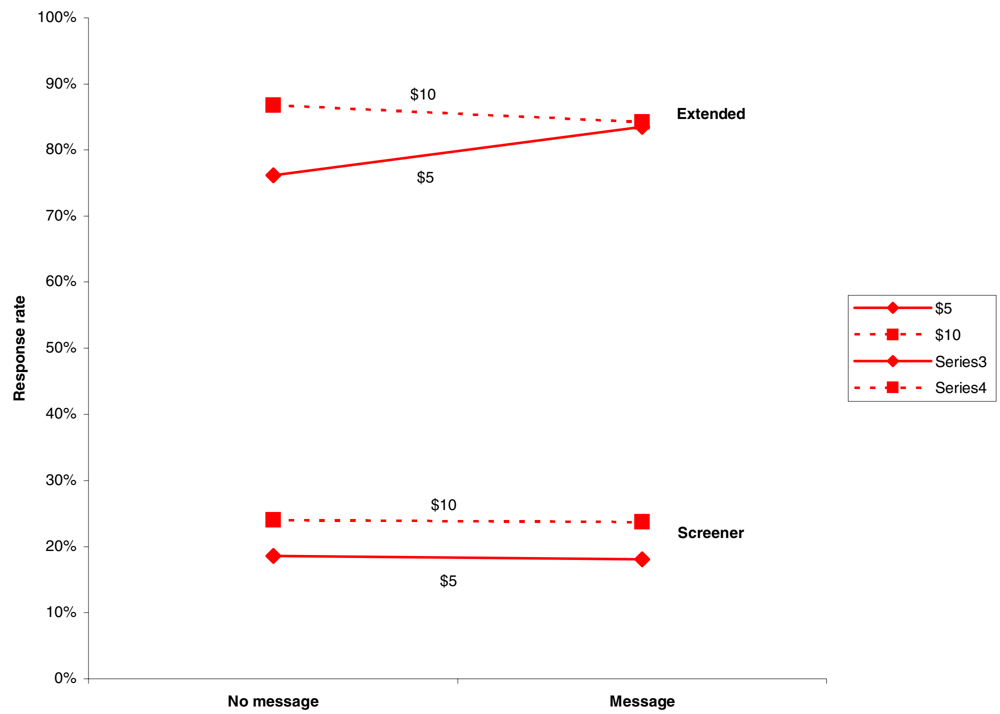

Figure 1. Cell sample screener and extended response rates for text message capable numbers, by experimental condition (includes only text message capable numbers)

treatments were crossed, we examined the response rates at the screener and extended level to assess whether there were any interactions. Figure 1 shows the response rates for the four conditions for both the screener and the extended levels for only those cases that were text message capable. For the screener, there is clearly no interaction between the payment level and the text message. However, at the extended level, there is an indication of an interaction.

3. Estimation Issues

The primary goal of weighting any survey is to produce approximately unbiased estimates of the population. For the Practicum Survey, the specific objective was to estimate household characteristics collected in the interview by telephone status (land-only, cell-only, and both). This section discusses the challenges associated with weighting the survey due specifically to nonresponse bias. A short review of the theory of dual frame estimation and the plans for implementing these methods are presented first. The evidence for nonresponse bias and the source of that bias are then discussed, along with the implications the bias has for estimation.

3.1 Background on Dual Frame Estimation

The Practicum sample was a dual frame, rather than a stratified sample, because some households could be sampled from both the cell and land frames (those households in the overlap population that have both cell and landlines). The non-overlap households (cell-only and landline-only households) could only be sampled from one of the two frames. Using the standard dual frames notation of Hartley (1962), let A be the landline frame and B be the cell frame. Assume that all households are in these two frames (households without any telephone service are not included). The set of households with landlines only are $a = A \cap B^c$ (where ^{c} denotes the complement), those with cell phones only are $b = A^c \cap B$, and those that have both types of lines are $ab = A \cap B$. The population totals for a characteristic of households in these sets are denoted Y_a, Y_b and Y_{ab}, respectively.

Since the samples from the two frames are independent, simple random samples, the inverse probability weighted estimator, y_a is unbiased for Y_a, and y_b is unbiased for Y_b. Three estimators of characteristics from the overlap for Y_{ab} are discussed: (1) the estimated total based on the units selected from frame A that are in the overlap, y'_{ab}; (2) the estimated total based on the units selected from frame B that are in the overlap, y''_{ab}, and; (3) a composite of these two means $y_{\lambda} = \lambda y'_{ab} + (1-\lambda) y''_{ab}$ where $0 < \lambda < 1$. All three estimators of the overlap are unbiased. If y_{λ} is used, the overall estimate is $y_{comp} = y_a + y_b + y_{\lambda}$.

The dual frame estimation methods assume that all the units sampled respond, and no auxiliary data are used to improve the precision of the estimates. Some research has explored using auxiliary data in conjunction with poststratification or raking estimators to improve the efficiency of the estimates. For example, see Bankier (1986), Skinner (1991), and Skinner and Rao (1996).

3.2 Weighting Approach

The base weights were simply the inverse of the probability of selection. The base weights were adjusted to account for those numbers that had unknown eligibility (never answered), and the Cell sample weights were further adjusted for refusal conversion subsampling. The weights for both samples were then adjusted to account for nonresponse using a standard weighting classes approach. The next adjustment was to account for households that had multiple chances of being sampled because they had more than one cell phone (Cell sample) or more than one landline (Land sample).

The plan was to combine the respondents from the two samples using composite weights for the overlap (dividing the weights for the overlap cases by two is equivalent to using $\bar{y}_{comp}$ with λ=0.5), and then to rake the combined respondent set to household control totals from the CPS. An alternative approach is to rake the Cell and Land samples separately using data from either the CPS Supplement of 2004 (Tucker et al. 2005) or from the NHIS (Blumberg et al. 2005) because these two sources reported detailed telephone status of households. This alternative was not pursued since an important goal of the study was to evaluate the estimates of the number of households by type of service from the dual frame survey.

The multiple telephone adjustment was done separately for the Cell and Land samples, avoiding computing an overall probability of selecting a household by either a landline or cell phone as needed in single-frame estimators. For both samples, the weight was divided by the number of telephone numbers in the household. In the Land sample, the divisor was 3 if there were 3 or more lines (only 3% of the sample had more than 3 landline numbers). In the Cell sample, the divisor was allowed to be 4 (3% of Cell sample respondents had more than 4 cell numbers).

The nonresponse adjustment initially used classes based on region for both the Cell and the Land samples. However, after reviewing some of the estimates from the Cell sample, the weighting classes for this sample were modified to be the number of call attempts to first contact with the household. Forming classes based on a process variable like this is a bit unusual. This approach was adopted for this study because of concerns about serious nonresponse bias in the Cell sample.

3.3 Nonresponse Bias

Two sources of nonresponse bias, topic salience (Groves, Presser, and Dipko 2004) and respondent inaccessibility are explored in this section. In the Practicum survey, both the advance letter sent to the Land sample cases with addresses and the interviewer's introduction indicated that the survey was about new technologies and cell phones. The introduction was:

"Hello, my name is _____. I'm calling for a University of Maryland research study about new technologies such as cell phones."

Persons hearing this introduction should be more disposed to respond if they are interested in new technologies and cell phones. Two indicators of nonresponse bias support this hypothesis. The first is the estimated percentage of households with landlines that also had cell phones. The nonresponse adjusted estimate from the Land sample was 71.2% (se=1.9%), compared to 52.4% from the 2004 CPS reported by Tucker et al. (2005). Thus, compared to the CPS standard, households with cell phones are over-represented in the Land sample. Since response rates are typically lower for households with low socio-economic status in telephone surveys, we examined the difference by characteristics including home ownership, education, income, and Hispanic origin. Controlling for each of these characteristics separately, the Land sample consistently overestimated the percentage of landline households with cell phones. For example, the difference between the Land sample estimate and the CPS estimate was 17.7 percentage points for households that owned their home and 21.4 percentage points for those that rented.

In the Cell sample, the nonresponse adjusted estimate of the percentage of households with cell phones that also had landlines was 77.1% (se=2.0%)[±], which is 11.4 percentage points lower than the CPS estimate of 88.5%. It is unlikely this difference is related to topic salience. The more probable source of this bias is respondent inaccessibility, which we discuss later.

The second indicator of nonresponse bias that is consistent with a topic effect is the percentage of completed interviews in the Land sample that required refusal conversion. In households with only landlines, 23.9% of the sample was completed as a result of conversion efforts while the figure was only 18.7% in households with both cell and landlines (p=0.004). This result is consistent with the expectation from leverage-saliency theory that less effort is required to obtain the participation of those more interested in the topic (in this case presumably those with cell phones). For the Cell sample, the difference by type of service in the percentage that required refusal conversion (< 1%) was statistically insignificant, as would be expected since all the households in this sample had cell phones. Since this difference could be the result of many other causes of nonresponse, we examined the differences in the percentage of completed interviews resulting from refusal conversion in the Land sample by variables associated with SES, which is often correlated with nonresponse. Of the SES variables reviewed, only education was statistically significant. Within education levels defined as high school graduate or less and some college or more, the percentage of completed interviews in the Land sample that required refusal conversion was statistically higher for the landline-only households (p<.01).

Another source of nonresponse bias is related to being able to reach households at a telephone number. Tucker et al. (2005) reported that nearly one-third (31%) of the households with both land and cell phones received very few or none of their calls on their cell phones. When these households are sampled in the Cell sample, they may be less likely to respond to the call. For example, their cell phone may rarely be on to receive a call, or they may be unwilling to answer their cell phone when they are not expecting a call. This should lead to bias in estimating the percentage of households with different types of service. Indeed, we suspect that this largely accounts for the Cell sample estimate of the percent of households with cell phones that do not have a landline being 11.4 percentage points higher than the estimate from the CPS after excluding households without phone service (22.9% (se=2.2%) and 11.5%, respectively).

The CPS also estimates that 8.9 percent of households with both types of telephone service receive all or almost all their calls on cell phones. These households may be less accessible on their landlines and this could lead to overestimating the percentage of households with landlines only from the Land sample by roughly 4 to 5 percentage points. As noted above, this is the opposite of what we observed: the Land sample underestimates the percentage of households with only landlines. This finding supports the hypothesis that nonresponse bias due to topic salience is probably the major contributor to the observed difference in the Land sample.

Further evidence of the nonresponse bias arising from inability to access some households can be obtained by comparing responses to questions in both the CPS and the Practicum samples about the percentage of telephone calls received on cell phones in households with both types of service. The CPS question with its response categories was:

"Of all the phone calls that members of your household receive, about how many are received on a cell phone? Would you say…All or almost all calls/ More than half/ Less than half/ or Very few or none?"

The Practicum question, with its response categories, was:

"Now how about receiving calls – on your cell phone, do you receive…Many more calls/ Somewhat more call/ Somewhat fewer calls/ or Many fewer calls on your cell phone, as compared to your regular home phone? (if volunteered About the same)"

Using the top two categories ("all or almost all calls" plus "more than half" from the CPS and "many more calls" plus "somewhat more calls" from the Practicum) produces estimates of "frequent" cell phone users of 33% for the CPS, 45% (se=2.3%) from the Cell sample, and 26% (se=2.2%) from the Land sample. Thus, compared to the CPS, the Cell sample respondents are more likely to be frequent cell users, and the Land sample respondents are less likely to be frequent cell users.

Taken together, this evidence suggests that both topic salience and household inaccessibility produce substantial biases in the estimates of type of telephone service. The implications of these biases for producing overall estimates are examined next.

[±] This nonresponse adjusted estimate is about 1.5 percentage points higher than the estimate computed when region (instead of the number of calls) was used to form classes for nonresponse adjustment.

3.4 Nonresponse Bias Implications for Estimation

As discussed earlier, an unbiased composite estimate is the average of two unbiased estimates of the overlap population, namely $y_\lambda = \lambda y'_{ab} + (1-\lambda) y''_{ab}$. However, the nonresponse biases in the estimated percentages of households by type of telephone service appear to be substantial for both y'_{ab} and y''_{ab}, and this will lead to biased composite estimates.

The most commonly used compositing scheme is to composite nonresponse adjusted estimates from two samples for the overlap population. This type of nonresponse composite estimate was formed for the Practicum Survey using $\lambda = 0.5$. When the nonresponse composite estimates are compared to the CPS estimates for a variety of statistics related to telephone status, they are clearly biased. For example, the estimated percentage of telephone households with cell phones is 72% (se=1.7%) compared to the CPS estimate of 55%, and the estimated percentage of telephone households with only cell phones is 14% (se=1.2%) compared to the CPS estimate of 6%. Thus, for estimating the percentage of households by telephone service and statistics highly correlated with this characteristic, the nonresponse composite estimates are highly biased.

An alternative weighting approach is to use the estimates of the number of households by telephone service from the CPS and rake the Practicum Survey weights to be consistent with these numbers. As mentioned earlier, this approach was not implemented for several reasons. A variant of this approach that makes use of some CPS estimates for raking was implemented. Rather than compositing the nonresponse adjusted weights and then raking them to the full telephone service status estimates from the CPS, the estimates from the two samples were separately raked and then composited. The Land sample weights were raked to the CPS number of households with landlines. The Cell sample was raked to the CPS number of households with cell phones[±±]. This procedure does not control the number of cell-only, land-only, or households with both types of service, making comparisons with the CPS estimates more meaningful. The raked weights for the two samples were composited using $\lambda = 0.5$ to form raked composite weights. The effect of this approach as compared to the nonresponse composite weighting scheme was to increase the estimated percentage of households with both land and cell phones, and decrease the percentages for the land-only and cell-only households. The raked composite weights yield an estimated percentage of households with cell phones of 79% (se=1.6%), and an estimated percentage of households with cell phones only of 10% (se=1.0%). The cell-only estimate from this scheme is closer to the CPS estimate, but the percentage of households with cell phones is even further from the CPS.

Since the nonresponse bias in the Cell sample is so pronounced, another approach that might be considered is to directly adjust the Cell sample for nonresponse due to inaccessibility before compositing. For example, respondents with cell phones were asked the question:

"When you are at home, how often is your cell phone turned on? Would you say… Always/Most of the time/Some of the time/Rarely/ or Only when you make a call? (if volunteered Never)"

A potential nonresponse adjustment scheme is to reduce the weights for households that answered always, and increase the weights for those that answered rarely or never. While such an approach might be reasonable and could also be applied to the Land sample, data from a face-to-face survey such as CPS or NHIS would be needed to calibrate the adjustment. Unfortunately, no survey regularly collects data of this type, and there are no current plans to repeat the CPS supplement in 2004 that did collect these types of items.

In summary, none of the adjustments for the Practicum Survey responses provide reliable and approximately unbiased estimates of telephone service that are comparable to the CPS estimates. The next section contains a more general discussion of possible solutions to estimating from dual frame surveys of telephone numbers.

4. Discussion

The 2004 JPSM Practicum Survey revealed a great deal about the operational and statistical characteristics of a dual frame survey from landline and cell phone numbers. From an operational perspective, the study showed that, consistent with the earlier study by Steeh (2004), sampling and data collection from the cell phone frame is feasible. In both the 2004 Practicum Survey and Steeh's 2003 survey the cell phone numbers had lower response rates, and refusal conversion efforts were not very effective for cell phone numbers. Both studies also found that the respondents in the cell phone samples were much more likely to be cell-only than expected. The Practicum survey also showed that some important operational issues require more attention. For example, procedures for handling cell phone numbers used by persons under 18 years old need to be further refined. The Practicum Survey also showed that

[±±] Both samples were raked to CPS estimates of the number of households in three dimensions: (1) the reference person was Hispanic or not; (2) the number of adults by marital status (1 adult, 2 adults and married, 2 adults and not married, and more than 2 adults); (3) the home was owned or rented.

monetary incentives can improve response rates from cell phone samples, even though the incentives are not prepaid. On the other hand, text messaging did not appear to be effective in increasing the response rate. Text messaging is a still evolving technology that is becoming more popular and may be subject to legislation that would limit its use for this type of application. As a result, text messaging as a response enhancing method deserves to be re-evaluated as the environment changes.

A more serious problem is the potential for nonresponse bias. In the 2004 study, both topic salience and household inaccessibility appear to have led to large biases in the estimates. Topic bias can be avoided in other surveys by using introductions that do not refer to technology or telephones.

The bias due to inaccessibility is more problematic, especially for cell phones since many households have cell phones yet answer them rarely. The characteristics of households that use cell phones in this way are apt to be very different from cell-only households, making these biases more difficult to address.

Current RDD samples drawn from landline numbers may also be suffering from this kind of bias, but the data suggest the problem is not severe. Nevertheless, the coverage problem due to households only having a cell phone is exacerbated by nonresponse bias due to households that have landline telephone numbers but never or rarely answer those lines because they rely on their cell phones.

In future dual frame surveys, simply using control totals of numbers of households by type of service may not adequately reduce bias if household characteristics differ by type of telephone service. As discussed in the weighting section, this type of adjustment may increase the variability of the weights and the estimates, and not even reduce the biases. To address these biases, it may be necessary to collect data on how often households receive calls on land and cell phones or how often they have their cell phone on when they are at home in both the survey and in a source of control totals such as the NHIS. More research on effective ways of collecting the data is required.

While the evidence from the Practicum clearly shows that nonresponse bias poses a major problem for estimating the percent of households by type of telephone service, for many survey topics the biases may not be serious. If differences in characteristics by type of telephone service are a function of the person or household characteristics that are handled in weighting, then the problem is greatly reduced. For example, Keeter (2005) showed that estimates of voting preference in cell-only households for the 2004 election were essentially eliminated when age was used in the weighting. It is likely that for many dual frame surveys the direct effect of the biases in telephone service may not be large. To understand the nature of these biases, other characteristics could be included in weighting adjustments.

The Practicum was a household level survey and thus did not have to consider sampling persons within households to make estimates of individuals. While having any adult report for the entire household is not uncommon for federal government surveys, in most RDD surveys one or more persons within a household is sampled and responds as an individual. It is not clear if using the household as a sampling unit as has been the standard for RDD surveys is appropriate and feasible when calling cell phone numbers. This is a new and important area of research in dual frame surveys that must be addressed. Future developments in this area are essential to being able to conduct surveys from cell phone numbers.

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