TECHNICAL PAPERS ON HEALTH AND BEHAVIOR MEASUREMENT

TECHNICAL PAPER 87

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A.M. Roman , E. Eggleston, C.F. Turner; S.M. Rogers, R. Crow; S. Tan,
J.R. Chromy, L. Ganapathi

Reference Citation

A.M. Roman , E. Eggleston, C.F. Turner; S.M. Rogers, R. Crow; S. Tan, J.R. Chromy, L. Ganapathi (2008) Effects of Sampling and Screening Strategies in an RDD Survey", In *JSM Proceedings, Survey Research Methods Section.* Alexandria, VA: American Statistical Association, pp 2712-2719.

Effects of Sampling and Screening Strategies in an RDD Survey

Anthony M. Roman[1], Elizabeth Eggleston[2], Charles F. Turner[3], Susan M. Rogers[2], Rebecca Crow[1], Sylvia Tan[2]
[1] Center for Survey Research, University of Massachusetts-Boston, 100 Morrissey Blvd., Boston, MA 02125
[2] Research Triangle Institute, 701 13th St. NW, suite 750, Washington, D.C. 20005
[3] Queens College and Graduate Center, City University of New York, New York City, NY

ABSTRACT

The Monitoring Sexually Transmitted Infections Project is an RDD telephone survey being conducted in Baltimore, Maryland from 2006 through 2009. The project screens households for people 15-35 years of age and then randomly selects one eligible person. To gain efficiency and reduce costs, three methodological changes were adopted after several months of data collection. These changes were 1) in the wording of the screening questions aimed at identifying households with someone 15-35, 2) implementing a sampling strategy that incorporated the use of lists that identified the ages of residents within some households, and 3) altering the probability of selection of an eligible respondent within a household to favor selecting someone who initially answered the telephone. This paper will examine the effects of these strategies on survey costs and sample composition.

KEY WORDS: RDD survey, dual frame sampling, survey costs

1. BACKGROUND

The Monitoring STIs Project is a random-digit-dialed (RDD) study being conducted in the city of Baltimore, Maryland. It is being conducted over a three year period from September 2006 through August 2009. The goal of the study is to monitor trends in three specific sexually transmitted infections (STIs), namely gonorrhea, chlamydia and trichomoniasis. The target population for the study is people 15-35 years of age inclusive.

Much has been written over the past few years about the current difficulties of conducting RDD surveys, including declining response rates [1,2] and potential biases due to the increase of cell phone only households [3,4]. Since the target population of this study includes people who are considered the most likely candidates to live in cell phone only households [5,6] and since young people are usually among the toughest to get to respond to surveys, the study was expected to be difficult to conduct. Add to these facts that the target population included people under 18 for whom parental permission would be required, the location of the study was entirely in a large metropolitan city, and the city has a large minority population, and the known difficulties of conducting the study become apparent.

Due to these known preexisting conditions, careful monitoring of all aspects of the study was performed from the outset. This paper will detail modifications made to the study design during the first year of the study to try to make the study more successful, and the results of these modifications. They include a change in the wording of screening questions, a change in the sample frame and a change in the methods used to randomly select a household respondent.

2. ORIGINAL DESIGN

The original design of the Monitoring STIs Project was a straightforward list-assisted RDD sample design using the GENESYS system from the Marketing Systems Group (MSG) [7]. In this design, all telephone exchanges known to cover the city of Baltimore were included in the sample, and then a simple random sample of telephone numbers was selected from this GENESYS frame. All telephone numbers were sent to be matched against reverse telephone directories to determine which numbers could be matched to a mailing address. An advance letter was sent to any address that could be obtained. Interviewers then called each number to determine if the number reached a residence, if the residence was indeed within the city of Baltimore and that at least one person between the ages of 15 and 35 currently resided in the household. In addition, the household needed to have a touch-tone telephone. The number of age eligible people within the household was then determined. If more than one eligible person resided in the household then one was randomly selected using a simple probability selection. In other words, if two eligible people were in the household then each had a 1/2 chance of selection, if three people were in the household then each had a 1/3 probability of selection and so on. Once a person was selected, an interview was attempted with them. If the selected person was under 18, parental permission was sought. The selected person had to speak English in order to conduct the interview. The interviewer explained that the study was about sexually transmitted infections and that the questions would be sensitive. They would be paid $20 for the interview. In addition, the interview would be conducted over a computer so that the person would not have to answer sensitive questions directly from an interviewer. This is why a touch tone telephone was required. If the person agreed, the interviewer would use Telephone Audio Computer Assisted Self Interviewing (TACASI) to conduct the interview. In other words, the interviewer would create a three way telephone connection among the interviewer, the respondent and the computer. Once it was certain that the connection was established and working, the interviewer would hang up. The computer then asked the sensitive questions and the respondent answered by hitting keys on the phone pad.

After the last survey question, the computer dialed the interviewer back and the interviewer reestablished contact with the respondent. At this point the respondent is told of the opportunity to earn $40 more if they agree to accept a urine cup in the mail, fill the cup and then mail it postage paid to a lab. At the lab, the urine will be tested for the three STIs and ONLY the three STIs. If a positive result is obtained, they will be notified and offered free treatment with antibiotics. The Maryland Dept. of Public will also be notified as is required by law. This was the entire originally designed protocol for conducting the study. With the sensitive subject matter to be addressed, the age of the respondents, a large metropolitan area, a high minority concentration, sometimes requiring parental permission, urine testing and health department notification, this had to be considered among the more difficult settings for a telephone survey. Initial situations encountered and modifications implemented will be discussed.

3. SCREENING QUESTION MODIFICATION

Initially, during the household screening process, the following question was used to screen for age of respondents:

“How many people aged 15 through 35 currently live in this household?”

From the initially released random sub-samples of telephone numbers (the sample was broken into random replicates which were then released over time as a method of controlling the sample over time), it was found that 21.3% (126/592) of all successfully screened households had at least one person aged 15-35. This was a cause for concern as the latest Census estimates were that 31.6% of all Baltimore households had someone of that age [9]. This large of a difference could imply potential biases due to the lack of being able to identify about 1/3 of the households that were expected to be in the city and contain an age eligible person. In addition, with a screening rate so much lower than expected, survey costs would be much higher. One initial thought was that this situation could be explained by the cell phone only household problem. Under this rationale, we could not contact about a third of all households that had someone 15-35 because they only had cell phones and no land lines.

In order to address this issue and to confirm that a problem indeed existed, we first modified the screening question script. Following is the revised script:

“How many people aged 36 or older currently live in this household?”

“How many people aged 15-35 currently live in this household?”

The goal was to ask the two questions in tandem, and reconcile any problems that come up with the answers. The respondent was never sure who we may or may not be looking for or even if it mattered. After the wording change, the percentage of households that had at least one person aged 15-35 jumped to 31.3% (343/1097). This was much more in line with Census estimates and made us feel more comfortable that the screening process could be successful. The age of the person was then later confirmed in the TACASI interview. The process confirmed the adage that you should never assume anything.

4. SAMPLE FRAME MODIFICATION

Originally the sample design was a pure list-assisted RDD design using GENESYS from MSG. In addition to the RDD sample frame, MSG also has list sample frames available for use. These list frames begin with white page telephone directory entries. To information from the phone books, zip codes are attached. Then information is merged from numerous other outside data sources including driver's license data, voter registrations, birth records, mail order information, survey respondent information, as well as proprietary data sources [8]. These other data sources provide information on household members including age. These lists alone obviously make up a biased sample frame, as it is impossible to determine the probability that any household is on the list. Therefore, a new dual frame design was proposed combining the lists and the RDD frame.

Overall for the city of Baltimore, the MSG list contained information on 149,119 households or 62.7% of the total 237,758 households in the city as reported by the 2006 American Community Survey (ACS) [9]. We decided to break the overall list of households into three distinct strata. They were:

1) Listed households that identified at least one household member 15-35 years of age (10,409 households)
2) Listed households for which age of household members was not available (85,089 households)
3) Listed households that identified age and no one 15-35 was listed (53,626 households)

To this listed household frame, we added a fourth stratum. This stratum was created by matching phone numbers from the entire listed sample (all three strata previously described), to the original RDD sample frame from Baltimore and removing all listed household numbers. In this way, four non-overlapping strata were created such that each household in Baltimore with a land line telephone was accounted for and existed in only one stratum. Multiple telephone lines into a household could exist, but this was asked about during the interview to reconcile this issue. We could then sample from within each stratum with rates to be determined.

The advantage of this dual frame stratified sample was that addresses could be available for a higher percentage of households so that more households received advance letters. Also, for much of the sample, screening rates could be reduced and sample efficiency gained. To begin, we decided upon the following sample breakdown by stratum:

22% in the list frame with households expected to contain someone 15-35
45% in the list frame with households with age of occupants unknown
33% from the RDD frame with listed households removed

The above percentages were based on estimated yields of eligible households from within each stratum and not on the distribution of sample telephone numbers. The distribution of telephone numbers dialed was 6% from households expected to have someone 15-35, 22% from households with unknown ages and 72% from the remaining RDD sample. In addition we drew a random sample of 100 telephone numbers from the stratum expected NOT to have anyone 15-35 to investigate the extent to which this assertion was true.

Table 1 displays results from dialing telephone numbers within each of the strata as well as from the original RDD sample.

Table 1: Results of Telephone Number Dialing by Stratum

Sample Source:	Rate of Connecting to Residential Households:	Rate at which Households had Age Eligible Respondent:	Overall Rate: Column 1 x Column 2:
Original RDD	30.80%	31.10%	9.58%
List with 15-35 person	78.05	61.94	48.34
List Age Unknown	62.07	41.01	25.46
RDD with lists removed	16.79	35.74	6.00
Combined Lists (15-35 and Unknown age)	65.27	45.46	29.67
Combined lists and RDD with lists removed	30.16	41.78	12.60

It can be seen that even with our rather conservative approach (i.e., not using a heavy reliance on the list with people expected to be 15-35), we still managed an overall 31% increase in sample efficiency (12.60/9.58). Altering sampling fractions can increase the gain in efficiency.

When the stratum in which listed households are not expected to contain anyone 15-35 was examined, the following results were obtained. The residential rate of the telephone numbers was 72.73%. The rate at which residences contained someone 15-35 was 18.37%. This meant an overall rate of 13.36%. Even this rate is higher than the original RDD rate, but it is obviously not efficient to use. Still, sample from this stratum must be used within any study using this type of design or biases may result from these households having no probability of selection. This is being done with the current study.

One additional way to examine this sampling strategy is to look at a few demographic characteristics of people interviewed from within these various strata and compare them to both the original RDD design as well as to estimates from the 2006 ACS. This is done in Table 2.

Table 2: Comparison of Demographic Characteristics

	Orig. RDD:	List 15-35:	List Age Unknown:	Combined Lists:	RDD with Lists Removed:	Combined Columns 2, 3 and 5:	2006 ACS:
Gender:							
Male	38.8%	37.4%	38.5%	38.3%	38.4%	38.3%	52.2%
Female	61.2	62.6	61.5	61.7	61.6	61.7	47.8
Race:							
Black	63.0	50.9	60.4	58.6	83.7	70.1	62.3
Non-black	37.0	49.1	39.6	41.4	16.3	29.9	37.7
Age:							
15-17	18.8	16.7	15.3	15.6	19.2	17.2	14.8
18-21	17.6	15.7	16.4	16.2	23.8	19.7	21.6
22-24	14.5	12.6	15.4	14.9	15.1	15.0	14.4
25-30	28.7	23.3	33.7	31.6	25.7	28.9	26.7
31-35	20.4	31.7	19.2	21.7	16.2	19.2	22.4
Education:							
Aged 25-35:							
< high school	10.3	2.8	12.5	10.5	16.7	13.0	11.3
High school*	45.0	46.2	35.4	37.6	59.2	46.2	44.8
4YR college +	44.7	51.0	52.1	51.9	24.1	40.8	43.9
Aged 18-24:							
< high school	16.9	7.7	30.2	26.1	16.9	21.4	20.2
High school*	68.7	82.3	53.2	58.5	80.1	69.6	69.0
4YR college +	14.4	10.0	16.6	15.4	3.0	9.0	10.8

* This row includes people with a high school diploma, a G.E.D. or some college beyond high school but less than a 4-year college degree.

The percentages in table 2 are computed after weighting to adjust for differing probabilities of selection across strata, the probability of within household selection of an eligible respondent, survey nonresponse and the number of telephone lines into a household. They do not include the effects of any post-stratified weight adjustments. Overall, a gender bias among respondents exists as compared to the 2006 ACS, but that bias is very consistent across the original RDD and the list strata. Regarding race, the original RDD sample produced an estimate of the percentage black that was in complete agreement with the ACS. The list households do produce lower estimates of the black population. This is probably not that surprising. Whatever mechanisms are at work to identify households and get them on the list may have biases toward non-black households. Once the list and RDD strata are combined, the percentage black is actually a bit too high. This was later accounted for in post-stratified weight adjustments. When considering age, the original RDD had a slightly higher percentage of 15-17 year olds at the expense of mostly 18-21 year olds. The lists varied with households known as having someone 15-35 having a higher percentage of 31-35 year olds while list households where the age of residents was unknown had too few. Overall, when lists were combined with the RDD stratum, the resulting distribution by age was very similar to the original RDD.

The comparisons involving education contain the most interesting results. The original RDD sample produced estimates for the education that agreed mostly with the 2006 ACS. The lone exception involves 18-24 year olds in which the RDD had a bit too few people with less than a high school education (16.9 vs. 20.2 from the 2006 ACS). This occurred at the expense of having a bit too many people with 4-year college degrees (14.4% vs. 10.8%)

The list for households with someone 15-35 had a definite education bias with far too few people with less than a high school education (2.8% vs. 11.3% for 25-35 year olds and 7.7% vs. 20.2% for 18-24 year olds). However, when combined with the list stratum with ages unknown and the RDD with lists removed, this bias went away. Actually, the overall education distribution from the dual frame design was closer to the ACS than the original RDD design. It becomes obvious that reliance on just the lists for households with someone 15-35 would produce a biased education sample. However, the dual frame approach corrects the bias.

All this implies that when using the dual frame stratified approach and combining lists with RDD, the resulting sample had no observable biases that were not present in the original RDD sample. A reliance on the lists alone could produce some additional biases, especially in education. However, a proper mixing of lists with an RDD frame can increase sample efficiency without adversely causing additional bias.

5. RESPONDENT SELECTION MODIFICATION

Originally, the within household respondent selection was the simplest form of a probability sample of eligible respondents. The number of people 15-35 in the household was ascertained, and then one was randomly selected. For example, if two eligible people were in the household, half the time the interviewer asked to speak to the youngest and the other half to the oldest. In attempts to gain efficiency, a modified respondent selection procedure was implemented. This was because it became apparent that for this age group, getting people at home was time consuming and costly in that many call attempts were required. Therefore, a data collection model that would be probabilistic in nature, but lead to an increased probability of selecting the person answering the screening questions if that person was eligible seemed worthwhile. This is not a new idea, and has been suggested previously [10].

For our application, we created a model that would increase the probability of selecting an eligible respondent doing the screener by a known amount dependent upon the number of eligible people in the household. In the example previously cited with two eligible people, the selection probabilities became a third of the time asking for the youngest person 15-35, a third of the time asking for the oldest and a third of the time simply saying that the eligible respondent on the line was the one selected. This effectively meant that the eligible person currently on the phone line had a 2/3 probability of being selected instead of 1/2. Similar tactics were followed for other numbers of eligible people in the household. The result was selecting the phone answerer more often while retaining knowledge of the exact probability of selection.

Since the respondent selection procedures were put into place, a total of 2305 households have been successfully screened. Of these, 1236 had only one eligible respondent in the household. For these households, there is no difference between the original and modified respondent selection procedures. Of the remaining 1069 households with more than one eligible respondent, 314 were households in which the person completing the screener questions was not eligible for selection. For these households, there is also no difference between respondent selection procedures. Therefore, the new procedures were in effect for 755 households or 32.8% of all screened eligible households.

Within these 755 households, the person already on the phone doing the screener was selected as respondent 560 times or 74.2% of the time. If one assumes that an eligible person answering the phone is truly random among all possible eligible people in the household, then the expected number of times that the person doing the screening should have been selected under the probability model was 467 or 61.8% of the time. We ended up selecting the person doing the screening 1.20 times more often than expected. The most logical explanation for this is that the person doing the screening decides to say they are the selected person even if they are not.

It is interesting to compare the above results to those from the original respondent selection procedure. For the random replicates for which we could accurately measure these numbers, we obtained the following results. A total of 653 households were successfully screened. Of those, 302 had more than one eligible respondent. This rate of 46.2% compares very favorably with the 46.4% (1069/2305) obtained under the revised respondent selection procedures. In addition, for 81 households with multiple eligible people, the person doing the screener was not an eligible respondent. This rate of 26.8% (81/302) again compared very favorably with the rate of 29.4% (314/1069) obtained under the revised procedures. Overall, respondent selection procedures were invoked in 221 households in which the person doing the screener was eligible under the old rules. This rate of 33.8% (221/653) again was very close to the 32.8% rate cited for the revised procedures. None of these percentages should have been affected by the change in respondent selection procedures, but it is still reassuring to see that they indeed were not.

For the 221 households under the old rules in which the person doing the screener was eligible, that person was selected 125 times or 56.6% of the time. Again assuming randomness for the person doing the screening, we would have expected this to happen 100 times or 45.3% of the time. Therefore, under the old respondent selection procedures, we ended up selecting the person doing the screener as the household respondent 1.25 more frequently than expected. This is actually slightly larger than the 1.20 factor obtained under the revised procedures. This shows that the underlying mechanism at work here did not change much across selection procedures.

Finally, the goal of implementing the revised respondent selection procedures was to reduce the number of calls required to interview a selected respondent. Considering call attempts made to all households found to have an eligible respondent, the average number of call attempts required to finalize these households under the original selection procedures was 12.31 call attempts. Under the revised procedures, this number was 8.88 call attempts. This reduction of 27.9% shows that the change in procedures certainly accomplished what it was intended to accomplish.

6. NET RESULTS OF MODIFICATIONS

The changes to the sample frame and the respondent selection procedures were incorporated into this study for the obvious reasons of attempting to increase response rates and decrease survey costs, while retaining a true probability sample. Regarding survey costs, this is best addressed by looking at changes in the number of interviewing hours required to get a successful interview. Costs per interviewer hour vary across survey vendors, but a reduction in survey hours must be accompanied by a relative reduction in survey costs. Prior to the modifications, when the original RDD sample and original respondent selection procedures were being used, interviewers averaged 4.64 hours to get one completed interview. This included all screening time. Once the modifications took effect, this was reduced to 4.15 hours per completed interview. This resulted in a 10.6% reduction in time and a similar reduction in survey costs. Added reliance on the lists by changing the sampling fractions can increase these savings.

Regarding response rates, Table 3 summarizes our findings:

Table 3: Response rate changes due to sampling modifications

	Interview Response Rate:	Agreed to receive Specimen Kit:	Returned Specimen:
Original design	55.04%	84.20%	78.26%
After modifications	59.65	86.14	85.04

The overall study involved more than simply an interview, but also agreeing to accept a urine specimen kit in the mail and then actually following through and sending the specimen to the lab for analysis. Improvements were observed in all three of these rates after the modifications were implemented. Although it is impossible to determine exactly what caused each increase, some speculation is warranted. Regarding the response rate increase, the overall percentage of households receiving advance letters increased from an estimated 51% to an estimated 70%. In addition, and probably more importantly, the person already answering the household screening questions was selected more frequently to continue on with the interview and therefore we did not have to “chase down” additional selected respondents. Regarding the agreement to accept the kit, it may be that listed households contain people more likely to be accepting of this type of offer. However, we also think that the respondent selection rules play into this as well. People who have answered the household screener questions and then follow up with the interview have had a longer time to form an opinion of the interviewer and the study. This increased rapport may have something to do with the increased percentage of people accepting the kit.

As far as actually sending the specimen to the lab, the most likely reason for this increase is something not as yet mentioned. At the time we instituted the other modifications we also began more aggressively pursuing people who did not return a specimen. Although we had always attempted to call people who had not returned a specimen, when the modifications went into effect, we also began offering $100 instead of $40 to send the specimen to the lab. This is the most likely reason for the increase in compliance. Since people had no way of knowing this would occur, it could not have affected any of the previous rates.

7. SUMMARY

Overall, the modifications put into effect in this study helped increase response rates as well as other compliance rates while reducing survey costs. Since all probabilities of selection are still known, it remains a true probability sample and one in which every household in the city of Baltimore with a land-line telephone has a known probability of selection. One additional analysis that needs to be performed is the effect on estimated variances of a few key statistics to see if the modifications have increased estimated standard errors. This will be done in the future and then optimal sampling fractions across strata can be determined.

ACKNOWLEDGMENTS

Support for this research was provided by NIH grant R01HD047163.

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