COMPARING PROMISED AND PRE-PAID INCENTIVES FOR AN EXTENDED INTERVIEW ON A RANDOM DIGIT DIAL SURVEY

David Cantor, Westat, 1650 Research Blvd., Rockville, MD 20850; Kevin Wang, Natalie Abi-Habib, The Urban Institute

Keywords: Refusal Conversion; missing data; response distribution.

This paper reports on a test of the use of incentives on a Random Digit Dial (RDD) survey. While there has been quite a bit of research on incentives across all modes of interviewing, very little has been conducted for RDD surveys. Of the research that has been done, most relates to the use of incentives at the initial contact with the household. A set of experiments described by Singer, et al. (2000) present evidence that incentives are effective in an RDD context at the initial stage. They find that a pre-payment of $5 did significantly improve response rates in a series of experiments involving the Survey of Consumer Attitudes (SCA). A promised incentive did not increase response rates. Similarly, Cantor et al. (1997; 1998; Brick, et al., 1999) found that small, pre-paid, incentives work at both the initial and refusal conversion stages of the process. Promised incentives were not found to work.

It is common on a RDD survey to have both screening and extended levels to the survey process. A screening survey is administered to a general population to find units with specific characteristics selected. A longer, extended interview is then completed with the selected respondent. The issues that are of concern in this paper are the effectiveness of incentives at the extended level. Specifically, this paper examines four questions: 1) How does the effectiveness of a promised incentive at the extended interview compare with not using any incentive at all? 2) How does the effectiveness of a promised incentive at the initial contact compare to an advance incentive at refusal conversion? 3) Is the effectiveness of an incentive for an extended interview affected by the type of incentive offered at the screener? 4) Are there differences in data quality and/or the characteristics of respondents by the type of incentive offered at the extended interview?

Promised Incentives After Screening Households

The literature on incentives finds that pre-paid incentives are more effective than promised incentives (Singer, et al., 1999). According to social exchange theory (Dillman, 2000: 15-22), a pre-paid incentive works because it increases the social benefits to the respondent. It legitimizes the survey by showing the respondent that the researcher is willing to provide a benefit, no matter how small, before the respondent has actually completed any tasks. This invokes a "social obligation" on the respondent. Once making the incentive contingent on completing a task, as with a promised incentive, the exchange shifts from a social to an economic one. Once viewed as a purely economic exchange, the monetary rewards may not measure up to the perceived burden of the task.

One of the difficulties related to using promised incentives on an RDD survey is communication. Many of the refusals on an RDD survey occur within the first 10-15 seconds of the interaction. There isn't much time for the interviewer to communicate details either about the survey or how the incentive may be related to the associated task. Promises of money at this point may actually have a negative effect on cooperation because the offer may be confused with offers of money that some telemarketers make to get respondent's attention.

These communication problems should be less of an issue when asking for cooperation on an extended interview. At this point in the process, a screening interview has already been completed and the respondent is actively listening to what the interviewer is saying. The respondent is likely to have a bit more confidence in the credibility of the interviewer and the study.

Given the above, we pose several hypotheses:

Hypothesis 1: A promise of money for the extended interview will significantly increase response rates relative to not promising anything at all.

Hypothesis 2: A promise of money for the extended interview will increase response rates relative to sending a smaller amount of money in advance to the refusal conversion call.

Hypothesis 2 was tested because sending money at refusal conversion is an increasingly common practice for survey designers. Cantor et al., (1998) found that when used at the screener, this method produces response rates that are comparable to sending money in advance to the call. Providing the incentive at refusal conversion is consistent with

Dillman's (2000) idea of creating an increasing sense of reward for participating in the survey. When viewed in the context of all the contacts made with the household, offering a refusal payment recognizes the respondent's initial reluctance to participate in the study. Respondent's may appreciate the persistence of the interviewer and the idea that someone places such a high value on their views and time. This would predict that offering an incentive at this stage would be quite effective.

On the other hand, one might question offering an incentive at this later stage because it may change the exchange from a social to an economic one. This shift may occur because the sequence of contacts resembles a bargaining exchange. The respondent first refuses to participate, which leads to a monetary offer to cooperate. As noted above, once viewed as an economic, rather than social, exchange, the monetary rewards may not measure up to the perceived burden of the task. Related to this problem is that respondents may wonder why an incentive was not provided at the initial contact. They may become suspicious about the motives of the survey administrators.

Effect of Screener Incentives on the Extended Interview

There is very little research on the use of incentives at multiple points in the survey process. Similarly, for a cross-sectional survey, one might provide an incentive when screening for eligible respondents and a second incentive when doing an interview with the individual selected for the survey. While there is quite a bit of research on the best way to use incentives for the initial interview (e.g., screening interview for cross sectional survey), there is very little guidance on the use of incentives over multiple contacts and situations. For example, it is unclear whether it is better to provide a large, one-time incentive at the first wave of a panel or smaller incentives at each wave. There is a similar problem in many RDD surveys which require both a screening and an extended interview.

The third research question listed above concerns the interaction of the type of incentive at the screener and the extended interview. The specific concern is whether the sequencing of the screener incentive affects response rates at the extended interview. "Sequencing" refers to whether the incentive is offered when the screener is initially attempted or at refusal conversion. The hypothesis tested below is:

Hypothesis 3: A screener incentive offered at the initial contact will increase the response rate at the extended interview compared to screener incentives offered at refusal conversion.

As noted above, one argument against the use of incentives at refusal conversion is that it communicates an economic, rather than a social, exchange. Once doing this at the screener, there may be a tendency for respondents to view all subsequent contacts with the survey in this light.

Incentives and Response Distributions

Incentives may affect response distributions in a number of different ways. One is by changing the amount of missing data that occurs on the survey. Incentives may attract respondents who are less willing to participate in the survey and are more likely to provide poor quality data (e.g., by answering questions incorrectly or with too little thought, or refusing to answer questions). Another possibility is that incentives serve to motivate respondents and as a result, they provide better quality data (e.g., more carefully thought out answers, lower item nonresponse rates).

Similarly, incentives may be more attractive to certain kinds of respondents. For example, one hypothesis is that they will be most attractive to respondents in low income groups (Singer, 2002).

The final sections of the analysis explore these possibilities by analyzing the response distributions of key indicators (e.g., missing data on income; demographic characteristics) by the type of incentive that is offered.

Experimental Design

The experiment was conducted as part of Cycle 3 of the National Survey of America's Families (NSAF). The NSAF is a RDD survey funded by a consortium of private foundations in the United States. It was conducted by Westat for the Urban Institute. An important goal of the survey is to assess the impact of recent changes in the administration of a number of assistance programs for children and the poor.

The NSAF consists of both a screening and an extended interview. The screening interview consists of a 3-5 minute battery of questions that is designed to select the person that should be administered the extended interview. This involves determining if there are any persons under 65 years old in the household and whether or not the family is above or

below 200% of poverty. If there is someone in the right age-range and the household is sampled (based on poverty status) a respondent for the extended interview is selected. The extended interview is 25 - 50 minutes in length (depending on the type of interview) and covers a wide range of topics, including health, education, child care, income and receipt of social services. Approximately 42,000 to 45,000 extended interviews are completed in a typical cycle.

The design of the experiment is shown in Table 1. There were two experimental factors. The first was a screener incentive, which included sending a: 1) $2 incentive along with a letter prior to making the first call, 2) $5 incentive along with a letter prior to calling to convert refusals or 3) letter without an incentive. The second factor was the extended incentive, including 1) promising money when first requesting to do an extended interview and 2) sending $5 prior to trying to convert refusals and promising an additional $20 if the interview is completed.

Crossing these two factors yields four experimental groups, as shown in the first 4 columns of Table 1. The "promise" condition for the extended interview had two levels of incentives. The study was interested in offering an extra incentive to populations that were of special interest or had shown reluctance to participate in the past. Those individuals that did not report their income on the screener and those individuals that were not located in one of the 13 states with an over-sample were offered $20 (approximately 30% of the sample). All other persons in the sample were offered $10 (approximately 70% of the sample).

The fifth group (column) shown in Table 1 is the "no treatment" group which did not provide an incentive to any household at either the screener or extended level.

Methods

These experiments were conducted at the beginning of Cycle 3 of the NSAF. The field period for the NSAF lasted from February to October of 2002. Since the experiment was conducted with the initial release groups, most of these cases were finalized between February and July of 2002.

All interviewers administering the NSAF during this period participated. This is approximately 300 individuals, once the survey was fully staffed. Interviewers were aware of the different experimental manipulations.

The NSAF produces estimates for two different population groups. The first are families with at least one child age 0-17. To collect these data, the person selected to do the extended interview is the person who knows the most about the child that was sampled during the screener ("most knowledgeable adult" or MKA). The second population are all adults who are of working age (18-64). The respondent for this group is selected in several different ways. One method is to administer the extended interview to a randomly selected adult who is living in a household where there are no children present. Adults are also selected within households where there are children. The results reported below tabulate the results for the MKA and for the adults living in households without children (referred to as "Adult-Only" households).

Two rounds of refusal conversion were completed for most households. The results are weighted by the initial probability of selecting the telephone number, a non-response adjustment done at the screener level and the probability of selecting the household once the correct information was obtained. The latter included, for example, accounting for any over-sampling that was done for those under 200% of the poverty line. The non-response adjustment at the extended level was not included in the weights applied below. These weights do not account for the probability of selecting a particular respondent within the household. The weighted data are used to be able to generalize the results to a national population. The significance tests were calculated using *WESVAR 4©*, in conjunction with the JK2 estimation method.

To calculate response rates, the following formula was used:

SR = (CI)/(CI + R + ONR)
Where: CI = completed interviewer,

R = refusals

ONR = Other non-response. Includes non- contacts, broken appointments, answering machines, field period ending, language problems and other types of non-response.

Results

The results are discussed according to the research questions and hypotheses discussed above.

Does a promised incentive work at the extended level?

Initially, interest was in knowing whether the promise of money at the extended interview significantly increased the response rates relative to promising nothing at all (Hypothesis 1 above). This was addressed using the data displayed in Table 2, which provide the response rates once collapsing across the experimental groups with a common treatment at the extended interview. The first row is for interviews with MKAs (respondents reporting for a sampled child) and the second row is for a randomly selected adult in an adult-only household.[1] The first column is the promise of money, which adds together groups 1 and 2 in Table 1. The second column is the extended treatment using a $5 refusal conversion payment with the $20 promise for completing the survey (groups 3 and 4 in Table 1). The third column is for the group that was not offered any incentive at all (Column 5 of Table 1).

Strictly speaking, these groups are not entirely equivalent because the "no treatment" group did not have a screener incentive, while the other two each had some type of incentive treatment at the screener. As will be shown below, this may affect how the extended incentive is received in the household. Nonetheless, this comparison does provide an indication of whether a promise of money at the extended level has the potential to increase response rates.

For the MKAs, the promise of money is significantly different from not providing any incentive at all (84.9 vs. 75.8; p<.05; two-tailed test). This is not the case for the interviews with respondents in the Adult-Only households. In this case, the promised incentives are about the same level as not promising anything at all (82.3 vs. 85.4). A similar pattern is apparent for the other incentive treatment ($5 at refusal conversion with promise of $20). This is also significantly different for the MKAs from the no treatment group (82.2 vs. 75.8; p<.10; one-tailed test). It is not significantly different for the Adult-Only households.

The second hypothesis posed above was whether the extended treatments differed from one another. This can be tested by comparing the first two columns of Table 2. The effects of these different extended incentive schemes do not differ by the type of respondent. For the MKAs, the rates are 84.9 vs. 82.2 and for Adult-Only respondents the rates are 82.3 and 78.9. While the promised incentive is higher for both types of respondents, none of the differences are big enough to reach statistical significance.

Does the screener incentive affect extended interview response rates?

Hypothesis 3 above concerns whether the staging of the screener incentive interacts with incentives provided at the extended level. The initial hypothesis was that providing $2 to all households will have a bigger positive effect on subsequent procedures at the extended than using screener incentives at refusal conversion. The primary rationale being that the $2 treatment is pre-paid and establishes a clear social exchange prior to the initial contact with the survey, while the $5 may shift the motivation from a social to an economic one.

Table 3 provides support for this hypothesis. These data are the response rates disaggregated by both the screener treatments and extended treatments. These columns correspond to the five experimental groups shown in Table 1. From these data, there does seem to be an effect of the screener treatment on extended response rates. It is strongest for the Adult-Only households, where the rate for the promised incentive is 10% higher when the $2 screener treatment was used compared to the $5 refusal conversion treatment (83.6 vs. 73.0; p<.05; two-tailed test). A similar difference appears for the $5 refusal conversion extended incentive treatment, where the difference is also around 10% (83.2 vs. 73.6; p<..10; one-tailed test). These patterns carry over to the MKA's but are much smaller and not statistically significant (85.3 vs. 82.4; 83.7 vs. 80.9).

Do incentives affect the amount of missing data?

The amount of missing data was estimated for key items from across the extended interview treatment conditions (data not shown). For MKA interviews, there is a tendency towards higher levels of item nonresponse on earnings and income items for those being offered incentives relative to those not offered any incentives. This does not appear to be the case with the adult-only respondents. However, analysis that controls for other factors associated with item nonresponse should be carried out before concluding that the use of incentives on the extended interview increases levels of item nonresponse. It may be the case, for example, that respondents in the no-

[1] Analysis not discussed here found that there was not a significant difference between offering $10 or $20 at the extended level. Consequently, the results below aggregate together cases that were promised either $10 or $20 to do the extended interview.

treatment conditions answered different (e.g., fewer) income items, which may have led to a lower rate of missing data.

Do incentives at the extended interview affect respondent characteristics?

Another concern related to incentives is that it affects the types of respondents who agree to complete the survey. Analysis was conducted that examined key characteristics of respondents by each extended treatment group. In general, for demographic items that are used in the NSAF population weighting adjustments (home ownership, race/ethnicity, age, education), there were very few significant differences across the three treatment conditions.

A similar analysis was completed for key survey items across the three treatment conditions. Most of the differences in estimates across the treatment conditions were not statistically significant. For MKA interviews, there is some evidence that respondents in the incentive conditions tend towards higher socioeconomic status, especially with respect to employment. One possible explanation for this pattern is that for higher income respondents, incentives may compensate for a lack of interest in the subject matter of a survey that focuses primarily on the well-being of low income families. This pattern does not occur for respondents in households without children.

Discussion

This analysis was structured around three questions. The first compared the effectiveness of a promised incentive at the extended interview. The analysis above provides evidence that promises of money at this level do work for certain kinds of respondents. Significant effects were found for the MKA's. No effects were found for the Adult-Only group. These two groups of respondents differ demographically. For example, MKA's are more likely to be female and married. The survey procedures also treat these two groups differently. There is more discretion on the part of the screener respondent when selecting the MKA, since it is based on the respondent's judgement about who can answer questions about the sampled child. The Adult-Only respondent is selected at random from a list of persons living in the household. As a consequence, a higher proportion of the MKA's are also the screener respondent. One might expect that communication about the survey and the conditions surrounding participation could be different for those who are screener respondents and those who are asked to participate once the screener is completed by someone else.

A second possibility is that the weights used in the analysis did not fully account for the respondent's probability of selection. Each respondent was assigned a weight accounting for the households chance of selection. However this weight did not account for the chance of selection within the household. Those in larger households should have relatively higher weights than those in smaller households. The weights in the current analysis do not reflect this. Future analysis should re-compute the above response rates using the correct weights to assess whether this is related to the differences across Adult-Only and MKA respondents.

The second question was concerned with whether the effectiveness of an initial promised incentive at the extended interview is different than an advance incentive offered at refusal conversion. When these two treatments were compared, no differences were found with respect to the effects on response rates. This was true for both MKA and Adult-Only interviews. On it's face, this result is similar to that found for research at the screener level, where the use of incentives at either the initial or refusal conversion stages yield approximately the same response rates.

The third question was whether the treatment at the screener affected results at the extended level. The experiment tested whether the sequencing of the screener incentive (initial vs. refusal conversion) influenced the effects of the incentives at the extended level. The above analysis provides evidence that this was true for at least the Adult-Only respondents. Use of a refusal conversion treatment at the screener seemed to depress the extended interview rates, regardless of the type of incentive offered at the extended level. The worst combination seemed to be the use of refusal conversion treatments at both the screener and extended levels. A similar pattern was found for the MKA households, but the differences were not as large or statistically significant.

These last results suggest that application of incentives at early stages of a survey do have effects at later stages. They would further suggest that use of refusal conversion payments may be less effective from this perspective than a pre-paid incentive at the intial contact. It is unclear why this may be the case. It may be because the pre-paid incentive reaches all sample members and, thus, sets up a social exchange that is viewed favorably by many in the household. The refusal conversion payment reaches fewer people. Alternatively, it may be because the screener conversion payment makes it appear as if the survey is trying to buy the respondent's cooperation. Once

doing this, the motivation to cooperate at later stages may go down.

The no-incentive condition had a lower prevalence of missing data than the two incentive treatments. On its face, this implies that providing incentives may decrease motivation to respond. No strong differences were found in the response distributions across the different treatments.

References

Brick, J.M., Flores-Cervantes, I., and D. Cantor (1999), *1997 NSAF Response Rates and Methods Evaluation*, Report No. 8, March 1999, Washington, D.C.: Urban Institute.

Dillman, D.A. (2000) *Mail and Internet Surveys, The Tailored Design Method*. John Wiley: New York.

Singer, E., (2002) "The Use of Incentives to Reduce Nonresponse in Household Surveys," in R. Groves, D. Dillman, J. Eltinge, and R. Little (eds), *Survey Nonresponse. John Wiley: New York, pp. 163-177.*

Singer, E., Van Hoewyk, J., Gebler, N, Raghunathan, T. and K. McGonagle (1999) "The Effect of Incentives on Response Rates in Interiewer-Mediated Surveys." *Journal of Official Statistics* 15:217-230.

Cantor, D., Allen, B., Cunningham, P., Brick, J.M., Slobasky, R., Giambo, P., and Kenny, G. (1997), "Promised Incentives on a Random Digit Dial Survey," in A. Koch and R. Porst (eds), *Nonresponse in Survey Research*, Proceedings of the Eighth International Workshop on Household Survey Nonresponse, Sept. 24-26, 1997, ZUMA, Mannheim, Germany, pp. 219-228.

Cantor, D., Cunningham, P., and Giambo, P. (1998), "Testing the Effects of a Pre-Paid Incentive and Express Delivery to Increase Response Rates on a Random Digit Dial Telephone Survey," Paper presented at the 1998 Annual Meeting of the American Association for Public Opinion Research, St. Louis, MO., May 14-17.

Singer, E., Van Hoewyk, J. and M.P. Maher (2000) "Experiments with Incentives on Telephone Surveys." Public Opinion Quarterly 64: 171-188.

Table 1. Experimental design*

Treatments	Condition Number 1	2	3	4	5
Screening Interview					
Pre-pay $2	x		x		
Advance $5 before Refusal Conversion		x		x	
No incentive					x
Extended Interview					
Promise $10 to 13 sites; Promise $20 to balance of nation+	x	x			
Advance $5 at refusal Conversion* and promise $20 when completing			x	x	
No incentive					x

* = Only includes households where an address could be found; All treatments include sending a letter in advance to making the initial call.

+ = $10 offer was made to households that provided an answer to the screening income item and who are in the 13 targeted states
$20 offer was made to households that were not in the targeted 13 states or were missing on the income screening item.

Table 2. Extended Interview Response Rates By Incentive at the Extended

	Promise Money at Extended Interview	Pre-pay $5/Promise $20 at Extended	No Incentive at Extended	Significance
MKA Households				
Response Rate	84.9	82.2	75.8	1 vs. 3**; 2 vs. 3#
(unweighted n)	2245	649	300	
Adult Only Households				
Response Rate	82.3	78.9	85.4	ns
(unweighted n)	898	284	134	

+ Includes sample where an address was found

* - Significant at p<.10 two tailed test; ** - Significant at p<.05; # - Significant at p<.10 one-tailed test.

Table 3. Extended Interview Response Rates By Experimental Group

	Promise Money at Extended Interview		Pre-pay $5/Promise $20 at Extended		No Incentive at Extended	
	Screener Treatment		Screener Treatment		Screener Treatment	
	$2 at Initial	$5 at Conversion	$2 at Initial	$5 at Conversion	No Incentive	Significance
MKA Household 1920						
Response Rate	85.3	82.4	83.7	80.9	75.8	1 vs. 5**; 3 vs. 5*
(unweighted n)	325	323	326	300		
Adult-Only Households						
Response Rate	83.6	73.1	83.2	73.6	85.4	1 vs. 2*; 1 vs. 4*; 3 vs. 4#
(unweighted n)	773	125	146	138	134	2 vs. 3*; 2 vs. 5*;

+ Includes sample where an address was found

*Significant at p<.10, two tailed test. ** Significant at p<.05 two tailed test. # Significant at p<.10 one tailed test