# TELEPHONE INTERVIEW INTRODUCTIONS AND REFUSAL RATES: EXPERIMENTS IN INCREASING RESPONDENT COOPERATION

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This paper reports on an investigation of one means of reducing nonresponse in telephone surveys. It was occassioned both by the increasing use of the telephone in academic survey research and by the comparative underdevelopment of methods to reduce nonresponse in telephone surveys especially in relation to mail surveys and to a lesser extent, in relation to personal interview surveys.

Overall, there are many strategies one might devise for reducing nonresponse in telephone surveys. They include, but are not necessarily limited to: Interviewer selection, selective retention of interviewers, manipulating interviewer norms and expectations, monitoring and reinforcing interviewer behavior, more field work to reduce noncontacts, prior notification letters (where feasible), and some sort of two-stage "foot in the door" technique.

A large proportion of refusals in telephone interviews occur in the first few minutes of the interaction. Consequently, any attempt to reduce refusal rates should carefully consider the nature of the interviewer's introductory remarks. These are the focus of this investigation.

In the course of most survey introductions, the respondent is typically told the purpose of the study, its importance, something about the organization conducting the research, and occassionally its sponsor. Frequently, this "interaction" is one-sided and interviewers often report uneasiness from lack of any verbal reaction on the part of the respondent until a rather substantial introductory explanation has been read. After the respondent has been told of the nature of the survey, however, the first several questions frequently appear totally unrelated to the subject: the phone number is verified, and rather detailed household information is collected (usually age, sex, and the relation of each household member to the respondent). Not uncommonly, interviewers are asked, "What does this have to do with the survey?" Even though there are good scientific reasons for asking these questions, it is easy to imagine that many respondents feel mislead at this point. This can be sufficient cause to refuse to continue the interview.

Several experiments were designed to test different approaches to increasing completion rates in telephone interviews by varying different aspects of the introductions. Three dimensions were tested in a split-ballot national probability telephone survey fielded between March and May 1979. All interviewing was conducted employing a computer assisted telephone interviewing system (CATI) at the University of Michigan's Survey Research Center.

## THE EXPERIMENTAL DESIGN.

All interviews were begun with the identical opening statement: "Hello this is the University of Michigan calling. As part of a research project we're interviewing people throughout the United States about two topics: their health and their television watching." After this introductory statement, the experimental forms of the questionnaire differed with respect to the presence of early substantive questions, an extended explanation, and verbal "feedbacks."

The first experimental variation was the presence or absence of substantively relevant questions immediately after the above introduction. Questions were intended to be (1) highly relevant to stated survey objectives, (2) applicable to all respondents and (3) easy to answer. They were "First, about health: would you describe your (family's) health as excellent, good, fair, or poor?", followed immediately by, "How do you feel about the medical care your family receives?".

The second variable involved the insertion or deletion of an extended description of the survey organization, the social utility of the research, and the need for a representative sample following the usual less extensive introduction. The exact wording of this statement was, "Before continuing, we'd like to tell you who we are and why we're calling. I am a professional interviewer from the Survey Research Center at the University of Michigan, and we conduct studies on important topics like health and the use of television. Our research is of interest to organizations like the United States Public Health Service. Because it's important to interview a representative sample of people through the United States, our computer has produced telephone numbers from area codes all over the country."

The third experiment tested the use of verbal "feedbacks" after all responses as means of operant conditioning of respondents

to increase their propensity to cooperate. These feedbacks were of two types. First, short feedbacks permissible were the words "Uh-huh," "I see," "Thanks," "Thank you," "That's useful" and "That's helpful". Interviewer discretion concerning which of these was used was permitted. No discretion was allowed, however, about when these were to be used; these were programmed. Second, for long feedbacks both the exact text and a specification of when they were to be used were programmed. An example of longer feedback would be, "Thanks that's the sort of information we're looking for."

These three dichotomous dimensions produce eight possible combinations of which six proved viable. They are indicated in Table 1. These were the six versions tested in this experiment.

TABLE 1

SAMPLE SIZES AND CONTENTS OF EXPERIMENTAL INTRODUCTIONS

| Form Number | Substantive Questions | Explanation of Survey | Feedback |
|---|---|---|---|
| 1 | X | | |
| 2 | X | X | |
| 3 | | | |
| 4 | X | | X |
| 5 | X | X | X |
| 6 | | | X |

| Form Number | Sample Size |
|---|---|
| 1 | 421 |
| 2 | 462 |
| 3 | 457 |
| 4 | 218 |
| 5 | 244 |
| 6 | 227 |
| | 2029 |

After the experimentally varied components of the introduction just described all interviews went through a sequence of questions which were designed to distinguish home and business telephones, verify the number reached, and gather sufficient household composition information to select a respondent. All treatments were administered first to the person who answered the telephone and, after a respondent was selected, to the respondent him/herself (if this was someone other than the initial phone answerer).

Cases were randomly assigned to treatment groups; approximately thirty interviewers conducted the survey, each interviewer used all forms of the introduction as part of an interpenetrated sample design.

RATIONALE FOR THE EXPERIMENTAL VARIATIONS.

The use of questions as a motivation to increase cooperation has two theoretical bases. The first is one of eliciting commitment on the part of the respondent to facilitate further cooperation. The second aims at reduction of respondent fear by educating them about the nature of the respondent role.[1] The first argument is enjoying active experimental testing, ranging from attempts to increase survey response rates (Reingen and Kernan, 1977) to experiments on increasing helping behavior in a variety of other settings. Many of these experiments have involved an initial contact where a small request is made followed somewhat later by the request for the actual task of interest. Our test of this approach has eliminated the time interval between the small request and the final request. This obviously is the least expensive implementation of the technique (because it eliminates the need for two contacts) but it may or may not stimulate perceived commitment on the part of the respondent.

The other conceptual argument supporting the positive effects of questions observes that many persons have little knowledge about what behavior will be required of them as respondents. The use of questions similar to those in the questionnaire may serve to instruct the respondent about the nature of the survey and of his role as a respondent, hopefully making cooperation a less threatening alternative.

The use of a rather lengthy explanation about the survey organization and the interviewer's role was suggested by observations similar to these. Some respondents may reject requests for an interview because of lack of information about the purposes of the survey. Suspicions about legitimacy, confusion with telephone marketing attempts that are disguised as surveys, and distrust of unanticipated telephone calls may sometimes be attacked through further explanations of the purposes of the survey and by an appeal to the scientific purposes of the work. For those not yet firmly committed to refusing, such information may have a positive effect.

The final experimental treatment is related to the procedures used within the subsequent questionnaire to test effects of interviewer behavior. It follows the work of Cannell and others (Cannell et al. 1977a, 1977b) in investigating programmed interviewer feedback to respondents after their answers have been given. This technique has been shown to provide greater reporting for topics that are often subject to underreporting bias (such as health-related events). It has been argued that greater effort on the part of the respondent to behave in a way that is rewarded by feedback is the cause of the increased reporting. In the context of the introduction to the survey, feedback is seen as supporting the answering of each question posed. Since each answer of the respondent stimulates interviewer feedback, it is argued that the respondent will be encouraged to continue the

interaction.

RESULTS.

Table 2 presents response rates and completion rates for the various introductions.[2] The response rates, calculated as indicated in the notes to Table 2, are conservative in including numbers never reached and numbers of undetermined status in the denominator. Given the number of callbacks made to these, past experience has indicated that a large percentage of these turn out to be nonworking numbers. This choice of conservative estimators does not affect cross-group comparisons, however. Since the proportion of numbers never reached and numbers of undetermined status should not be affected by experimental treatment, the completion rate is a more valid indicator of the effectiveness of these techniques.

TABLE 2
RESPONSE RATES AND COMPLETION RATES BY FORM OF INTERVIEW INTRODUCTION

| Form | Response Rate[a] (All Sample Cases) | Completion[b] Rate (Contacts only) |
|---|---|---|
| 1. Questions | .697 | .781 |
| 2. Questions, Explanation | .617 | .746 |
| 3. Nothing | .647 | .763 |
| 4. Questions, Feedback | .545 | .647 |
| 5. Questions, Explanation, Feedback | .660 | .789 |
| 6. Feedback | .601 | .721 |

[a]Defined as the number of completed interviews divided by the number of completions plus partials, plus callbacks, plus refusals, plus undetermined status, plus numbers never reached.

[b]Defined as the number of completed interviews divided by the number of completions, plus partials, plus callbacks, plus refusals.

A straightforward and convincing interpretation of these findings is elusive. If one collapses across the feedback dimension, it appears that the use of questions alone depressed the completion rate, but that used in conjunction with an extended explanation of the survey, its objectives, and the survey organization, it slightly increased the completion rate. The effects are even greater for the three separate "with feedback" conditions, but the direction of the effects is reversed. Thus, even with the substantial 14% difference in completion rates between the highest and lowest groups, there appear to be some inexplicable interactions between treatment effects that preclude a parsimonious explanation. In general, though, the use of questions and an extended explanation seem to have a positive effect on completion rates while feedback seems to have a negative one, although the highest completion rate of all was in the group with all three attributes present.

TABLE 3
RESULTS OF FIRST CONTACT BY FORM OF INTERVIEW INTRODUCTION

| Form | Completed Interview | Partial Interview | Callback with Appointment |
|---|---|---|---|
| 1. | .317 | .024 | .301 |
| 2. | .332 | .039 | .194 |
| 3. | .313 | .012 | .284 |
| 4. | .189 | .005 | .378 |
| 5. | .335 | .005 | .281 |
| 6. | .320 | .020 | .290 |

| Form | Callback without Appointment | Refusal | Undetermined Status at First Contact | |
|---|---|---|---|---|
| 1. | .142 | .161 | .055 | 1.000 |
| 2. | .165 | .207 | .063 | 1.000 |
| 3. | .164 | .168 | .059 | 1.000 |
| 4. | .129 | .229 | .070 | 1.000 |
| 5. | .207 | .128 | .044 | 1.000 |
| 6. | .080 | .255 | .035 | 1.000 |

Since our hypotheses address the behavior of a respondent during the first moments of contact with the interviewer, it may be more revealing to examine separately the results of the first contact with each household. Table 3 presents the initial disposition of those cases where contact was made. Three things are striking about this table. First, there are no substantial differences between five of the six groups in the proportion of completed interviews (given in the first column of Table 3). Second, the low completion rate for group 4 (questions plus feedback without explanation) does indeed originate in low initial contact completion rates and high initial contact refusals. (This raises the question: does the use of questions and feedback, without adequate explanation of the survey organization and its purposes sound strained or artificial?). Third, differences between the other five groups are more in the distribution of types of initial noninterview dispositions than in rates of initial completions. And, as Table 4 indicates, first contact classification of noninterviews is highly predictive of the likelihood of ultimately obtaining an interview. So these might be seen as second-order effects that emerge as a result of varying success in converting initial noninterviews in spite of initial similarities in completion

rates.

TABLE 4
CONVERSION RATES: PROPORTION OF COMPLETED INTERVIEWS BY INITIAL CONTACT DISPOSITION BY FORM OF INTERVIEW INTRODUCTION

| Form | Partial Interview | Callback with Appointment | Callback without Appointment |
|---|---|---|---|
| 1. | .444 (9) | .790 (110) | .588 (51) |
| 2. | .467 (15) | .849 (73) | .561 (57) |
| 3. | .400 (5) | .811 (106) | .661 (65) |
| 4. | 1.000 (1) | .685 (73) | .346 (26) |
| 5. | .000 (1) | .679 (56) | .564 (39) |
| 6. | .000 (4) | .750 (48) | .938 (16) |

| Form | Refusal | Undetermined Status at First Contact |
|---|---|---|
| 1. | .230 (61) | .625 (16) |
| 2. | .342 (79) | .059 (17) |
| 3. | .118 (68) | .250 (16) |
| 4. | .209 (43) | .286 (14) |
| 5. | .192 (26) | .667 (9) |
| 6. | .167 (48) | .143 (7) |

A major remaining curiosity is that techniques designed to operate mainly within the context of an initial contact--particularly within the first few moments, even seconds, of an interaction--seem to produce their biggest effects not in the initial completion rates but in ultimate completion rates, after recontacts. Given that experimental treatments were not maintained after the first contact (but that there were no conscious differences between groups after the first contact) it appears most likely that final disposition differences are due mostly to differences in the distribution of initial noninterview types by introductory form. That is, final differences are due mainly to the differential ability of different introductions to produce "softer" (more persuadable) types of noninterviews. Some forms may also have yielded more useful information than others in aiding conversion attempts but relevant data have yet to be extracted.

OVERALL OBSERVATIONS.

Many of the differences we observed were smaller than we had hoped and the larger differences were arrayed in a pattern that defied parsimonious explanation. The apparent reasons for these two observations include the following: First, there was no "straw man" treatment form. Each form was designed to be the best possible prototype we could produce. Second, experimentation of this sort has little cumulative knowledge on which to build. And third, it may be unreasonable to expect more than incremental effects from anything so small as minor variations in introductory comments--a fact which may also explain the relative absence of cumulative knowledge in this area. The prospect of incremental effects makes for less enticing research.

The most striking observation about the effects of the three main experimental variations was the relatively high completion rates in the fully saturated form (questions plus explanation plus feedback) and in the "questions only" form. The lack of an overall consistent pattern and the lack of a ready explanation for the other interactions that were observed is ample cause for caution in overdrawing the implications of these findings. Most of the effects that were observed were second-order effects in that the techniques seemed to have a greater effect on the distribution of noninterview types than on the proportion of initial completions. Clearly the strongest predictor of the likelihood of a successful conversion is the initial noninterview classification. Strategies designed to soften refusals, rather than all-out direct attempts to reduce the proportion of initial noninterviews at all costs may therefore be the most productive over the long run. If this is correct, it indicates that the most successful approaches in this area will be those that treat the refusal problem as part of a multiple-call strategy where necessary and not simply a single-call effort.

FOOTNOTES:

[1] Since many interviewers report uneasiness at the lack of feedback (any verbal activity) from respondents during the first few moments of an interview, the use of questions early in an interaction may also stimulate interviewer motivation and satisfaction.

[2] A double sampling scheme for nonrespondent cases was applied; response and completion rates are weighted to reflect unequal probabilities of retention in the final sample.

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