

Survey Methodology Program

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The Use of Incentives to Reduce Nonresponse in Household Surveys

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11.1 Introduction

A large number of experiments in recent years have manipulated incentives in telephone and face-to-face surveys, complementing the earlier, and even more voluminous, research literature on the effects of incentives in mail surveys. These experiments are based loosely on various forms of exchange theory (e.g. Adams, 1965; Berger et al., 1972; Homans, 1961, 1974), though many are quite atheoretical. This chapter synthesizes findings about the effects of incentives from both interviewer-mediated and mail surveys in order to increase their usefulness for practicing survey researchers and to stimulate more targeted research by survey methodologists. It reviews what is known about the *intended* effects of incentives on response rates in both types of surveys, drawing on two existing meta analyses (Church, 1993; Singer et al., 1999) as well as on subsequent work by the same and other authors. It also reviews what is known about such *unintended* consequences of incentives as effects on response quality and sample composition, concerns about equity, and the development of expectation effects (Groves

¹ Many people helped carry out the research on which much of this chapter is based. In alphabetical order, they are: Amy Corning, Nancy Gebler, Bob Groves, Patricia Maher, Kate McGonagle, Trivellore Raghunathan, and John Van Hoewyk. The National Science Foundation supported some of the experiments reported with Grant No. SBR - 9513219 to Robert M. Groves and Eleanor Singer. The collegiality of Richard Curtin, Director of the Surveys of Consumers, and the financial assistance of the Survey Research Center of the Institute for Social Research, University of Michigan, are also gratefully acknowledged. Mick Couper, Don Dillman, Bob Groves, Stanley Presser, and Howard Schuman read various versions of earlier drafts, and I am grateful for their comments even though I haven't always followed their advice.

et al., 1999; Singer, Groves, and Corning, 1999; Singer, Van Hoewyk, and Maher, 1998, 2000).²

Finally, it discusses issues of cost effectiveness and suggests avenues of further research.

Because of the additional complexities introduced in organizational surveys, they are not included in the present review.³

11.2 Why Do People Participate in Surveys?

Porst and von Briel (1995) point out that although a great deal is known about survey respondents--their demographic characteristics, as well as their answers to thousands of different survey questions, very little is known about why they choose to participate. Based on a content analysis of open-ended responses, their study of 140 participants in five waves of a German "Methods Panel" identifies three pure types of participants: those who respond for altruistic reasons (e.g. the survey is useful for some purpose important to the respondent, or the respondent is fulfilling a social obligation--31% of respondents); those who respond for survey-related reasons (e.g. they are interested in the survey topic, or find the interviewer appealing--38%); and those who cite what the authors call "personal" reasons (e.g. they promised to do it--30%).

More recently, Groves, Singer, and Corning (1999) outlined a theory describing the decision to participate in a survey as the interactive and additive resultant of a series of factors--some survey-specific, such as topic and sponsorship, others person-specific, such as concerns

² When studies are included in previous meta analyses, the present chapter refers only to the meta analytic conclusions. Incentive experiments carried out after 1991 were retrieved by searching a variety of electronic data bases (Sociofile, WILS, Public Affairs, A Matter of Fact, and Psych Info) as well as the American Statistical Association's *Proceedings* of the Section on Survey Research Methods and the annual programs of the American Association for Public Opinion Research.

about privacy, still others specific to the respondent's social and physical environment--each of which is associated with a weight and direction for a given person's decision, moving him or her toward or away from cooperation with a specific survey request.

From this perspective, monetary as well as nonmonetary incentives are an inducement offered by the survey designer to compensate for the absence of factors that might otherwise stimulate cooperation--e.g., interest in the topic of the survey or a sense of civic obligation. Although other theoretical frameworks such as social exchange theory (cf. Dillman, 1978), the norm of reciprocity (Gouldner, 1960), and economic exchange (e.g. Biner and Kidd, 1994) can also be used to explain the effectiveness of incentives--and, in particular, the greater effectiveness of prepaid over promised incentives--the present perspective is able to account for the differential effects of incentives under different conditions (e.g., for respondents with differing interest in the survey topic or with different degrees of community activism) in a way that other theories cannot easily do.

11.3 Intended Effects on Response Rates

In an effort to counter increasing tendencies toward noncooperation, survey organizations are offering incentives to respondents with increasing frequency, some at the outset of the survey, as has traditionally been done in mail surveys, and some only after the person has refused, in an attempt to convert the refusal.

The use of incentives has a long history in mail surveys (for reviews, see Armstrong 1975; Church 1993; Cox 1976; Fox, Crask and Kim 1988; Heberlein and Baumgartner 1978; Kanuk and Berenson 1975; Levine and Gordon 1958; Linsky 1975; Yu and Cooper 1983). In such surveys, incentives are one of two factors, the other being number of contacts, that have

consistently been found to increase response rates.

A meta analysis of the experimental literature on the effects of incentives in mail surveys (Church 1993) classifies incentives along two dimensions: whether the incentive is a monetary or nonmonetary reward; and whether it is offered with the initial mailing or made contingent on the return of the questionnaire. Analyzing 38 studies (yielding 74 comparisons between incentive and control conditions), Church concluded that:

- ▶ prepaid incentives yield significantly higher response rates whereas contingent (promised) incentives do not;
- ▶ prepaid monetary incentives yield higher response rates than gifts offered with the initial mailing;
- ▶ response rates increase with increasing amounts of money.

In Church's (1993) analysis, studies using prepaid monetary incentives yielded an average increase in response rates of 19.1 percentage points, representing a 65% average increase in response. Gifts, on the other hand, yielded an average increase of 7.9 percentage points. The average value of the monetary incentive was \$1.38; the average value of the gift could not be computed, given the great diversity of gifts offered and the absence of information on their cost. Reports similar to those of Church are reported by Hopkins and Gullikson (1992).

Incentives are also increasingly used in telephone and face-to-face surveys, and the question arises whether their effects differ from those found consistently in mail surveys. A meta analysis of 39 experiments in interviewer-mediated surveys by Singer et al. (1999) indicates that they do not, although the percentage point gains per dollar expended are generally smaller than those reported by Church. The main findings from the 1999 study are as follows:

- ▶ Incentives improve response rates in telephone and face-to-face surveys, and their effect does not differ by mode of interviewing. As expected, however, effects are much smaller than in mail surveys: on average, each dollar of incentive paid results in about a third of a percentage point difference between the incentive and the zero incentive condition. As in the analyses by Church (1993) and Yu and Cooper (1983), the effects of incentives are linear.
- ▶ Prepaid incentives do not differ significantly from promised incentives, although the differences are the expected direction. However, most studies in the meta analysis used promised incentives; in the four studies in which prepaid incentives were compared directly with promised ones, prepaid incentives resulted in significantly higher response rates.
- ▶ Money is more effective than a gift, even controlling for the value of the incentive.
- ▶ Increasing the burden of the interview increases the difference in response rates between an incentive and a zero-incentive condition. However, incentives have a significant effect even in low-burden studies.
- ▶ Incentives have significantly greater effects in studies where the response rate without an incentive is low. That is, they are especially useful in compensating for the absence of other motives to participate.

Some studies have found that the effect of incentives diminishes in mail surveys after repeated follow-ups (e.g. Hopkins, Hopkins, and Schon, 1988; Brennan, Hoek, and Astridge, 1991; James and Bolstein, 1992; Shettle and Mooney, 1999). However, Baumgartner and Rathbun (1997) found no reduction in the incentive effect despite implementation of a procedure

modeled on Dillman's (1978) "Total Design Method". Shettle and Mooney's (1999) experiment, involving a mail survey of college graduates that included telephone and personal follow-ups suggests that incentives work primarily by reducing refusals, rather than by reducing the noncontact rate. Similar findings are reported for the Survey of Consumer Attitudes (SCA), a random-digit-dialed (RDD) national survey of the general population with an *N* of 500 carried out monthly by the University of Michigan Survey Research Center (Singer, Van Hoewyk, and Maher, 2000).

11.3.1 Lotteries as Incentives

Some researchers, convinced of the value of incentives but reluctant to use prepaid incentives for all respondents, have advocated the use of lotteries as an incentive for stimulating response. The studies reported in the literature--all mail surveys or self-administered questionnaires distributed in person--have yielded inconsistent findings (e.g. positive effects by Balakrishnan et al., 1992; Hubbard and Little, 1988; Kim, Lee, and Whang, 1996; and McCool, 1991; no effects in four studies reviewed by Hubbard and Little, 1988 or in the experiment by Warriner et al., 1996). A reasonable hypothesis for further testing would seem to be that lotteries function as promised cash incentives with an expected value per respondent (e.g. a \$500 prize divided by 10,000 respondents would amount to an incentive of 50 cents per respondent), and that their effect on response rates would be predicted by this value as well as the fact that they are promised rather than prepaid.

11.3.2 Incentives in Panel Studies

Assuring participation is especially important for panel studies, since participation at baseline usually sets a ceiling for the retention rate over the life of the panel. Although some

investigators (see, e.g., Presser, 1989) recommend returning to nonrespondents to one wave in subsequent waves, this is not often done. Even when it is, cooperation on a subsequent wave is generally predicted by prior cooperation. For this reason, investigators often advocate sizable incentives at the first wave of a panel study.

Analyzing the results of an incentive experiment on Wave 1 of the 1996 Survey of Income and Program Participation (SIPP), a longitudinal survey carried out by the U.S. Census Bureau to provide national estimates of sources, amounts, and determinants of income for households, families, and persons, James (1997) found that the \$20 prepaid incentive significantly lowered nonresponse rates in Waves 1-3 compared with both the \$10 prepaid and the \$0 conditions, but that the \$10 incentive showed no effect relative to the zero-incentive group. Mack et al. (1998), reporting on the results through Wave 6 using cumulative response rates, found that an incentive of \$20 reduced household, person, and item (gross wages) nonresponse rates in the initial interview and that household nonresponse rates remained significantly lower, with a cumulative 27.6% nonresponse rate in the \$0 incentive group, 26.7% in the \$10 group, and 24.8% in the \$20 group at Wave 6, even though no further incentive payments were made. (SIPP does not attempt to reinterview households that do not respond in Wave 1 or that have two consecutive noninterviews.) Differences between the \$10 incentive and the no-incentive group were not statistically significant.

Research on the Health and Retirement Survey (HRS) suggests that respondents who are paid a refusal conversion incentive during one wave do not refuse at a higher rate than other converted refusers when reinterviewed during the next wave (Lengacher et al., 1995).

11.3.3 Summary of Intended Effects

Incentives appear to accomplish their intended effect of increasing response rates to all kinds of surveys, though the size of the effect varies by mode and by other factors affecting willingness to respond. Prepaid incentives are clearly more effective than contingent ones.

There is no evidence that incentives are helpful in making contact with respondents in an RDD survey, nor any theoretical justification for believing that they would be. Thus, if the primary problem is one of finding people at home for such a survey, incentives may not be very useful. However, an experiment by Kerachsky and Mallar (1981) with a sample of economically disadvantaged youths suggests that prepayment may be helpful in locating members of a *list* sample, especially in later waves of a longitudinal survey. One reason, apparently, is that prepayment (and perhaps promised incentives from a trusted source) may be useful in persuading friends or relatives to forward the survey organization's advance letter or to provide interviewers with a current telephone number for the designated respondent.

11.4 Unintended Consequences of Incentives

11.4.1 Effects on Item Nonresponse

One question often raised about the use of incentives in surveys is whether they bring about an increase in response rate at the expense of response quality. This does not appear to be the case. On the contrary, what evidence there is suggests that the quality of responses given by respondents who receive a prepaid or a refusal conversion incentive does not differ from responses given by those who do not receive an incentive. They may, in fact, have less item-missing data and provide longer open-ended responses (Baumgartner et al., 1998; James and Bolstein, 1990; Singer, Van Hoewyk, and Maher, 2000; Shettle and Mooney, 1999; but cf.

Wiese, 1998). Experiments reported by Singer, Van Hoewyk, and Maher (2000) indicate that promised and prepaid incentives *reduce* the tendency of older people and nonwhites to have more Don't Know responses and item missing data, resulting in a net reduction in item nonresponse. However, incentives alone explained less than one percent of the variance in item nonresponse in that study.

Findings reported by Mason and Traugott (1999) suggest that persistent efforts to persuade reluctant respondents to participate may produce more respondents at the price of more missing data. But these authors did not use incentives, and motivational theory suggests that people who are rewarded for their participation would continue to give good information, whereas those who feel harassed into participation may well retaliate by not putting much effort into their answers.

We have found no studies of the effect of incentives on the validity or reliability of answers, and this aspect of quality is an important research question.

11.4.2 Effects on Response Distributions

Even more troubling, potentially, than an effect on item missing data is the effect of incentives on the distribution of responses. Does offering or paying incentives to people who might otherwise refuse affect their answers to the survey questions?

It is useful to think about the reasons for which effects on response distributions might come about. One is that the use of incentives brings into the sample people whose characteristics differ from those who would otherwise be included, and their answers differ because of those differing characteristics. If that is the case, the apparent effect on response distributions is really due to a change in the composition of the sample, and should disappear once the appropriate

characteristics are controlled. An example of the first process is presented Berlin et al. (1992), who demonstrate that the apparent effect of a monetary incentive on literacy scores can be accounted for by the disproportionate recruitment of respondents with higher educational levels into the zero-incentive group. There was no significant relationship between incentive level and the proportion of items attempted, indicating that the incentive influenced the decision to participate, but not performance on the test. Effects of incentives on sample composition are discussed further in the following section.

A second reason incentives might influence responses is if they influence people's opinions directly, or at any rate the expression of those opinions. A striking example of such influence involving sponsorship is reported by Bischooping and Schuman (1992) in their analysis of discrepancies among Nicaraguan pre-election polls in the 1990 election. Bischooping and Schuman speculate that suspicions that pre-election polls had partisan aims may have prevented many Nicaraguans from candidly expressing their voting intentions to interviewers, and tested this hypothesis by having interviewers alternate the use of three different pens to record responses: one carried the slogan of the Sandinista party; another, that of the opposition party; the third pen was neutral. The expected distortions of responses were observed in the two conditions that clearly identified the interviewers as partisan. Even in the third, neutral, condition, however, distortion occurred, because "polls were apparently not perceived as neutral by many respondents. In the Nicaraguan setting, after a decade of Sandinista rule, a poll lacking partisan identification was evidently regarded as likely to have an FSLN [Sandinista] connection (p. 346)"; the result was to bias the reporting of vote intentions, and therefore the results of the pre-election polls, which predicted an overwhelming Sandinista victory when in fact the opposition

candidate won by a large majority.

Still a third way in which incentives might affect responses is suggested by theory and experimental findings about the effects of mood (Schwarz and Clore, 1996). If incentives put respondents in a more optimistic mood, then some of their responses may be influenced as a result. Using 17 key variables included on the SCA, Singer, Van Hoewyk and Maher (2000) looked at whether the response distributions varied significantly by (a) the initial incentive or (b) refusal conversion payments.

The offer of an initial incentive was associated with significantly different response distributions (at the .05 level) on 4 of the 17 variables; a refusal conversion payment was also associated with significantly different response distributions on 4 of them. One variable was significantly affected by both types of incentives. In five of these cases, the responses given with an incentive were more optimistic than those given without an incentive; in two cases, they were more pessimistic. In the remaining case, respondents who received an incentive were somewhat more likely to respond “good” *and* “bad,” and somewhat less likely to give an equivocal reply. The effects do not disappear with controls for demographic characteristics; indeed, three additional variables show such effects with such controls. Thus, there is a suggestion that respondents to the Survey of Consumer Attitudes who receive an incentive *may give somewhat more optimistic responses* than those who do not. Similar findings have been reported by Brehm (1994). James and Bolstein (1990) reported that respondents receiving an incentive on a mail survey made more favorable comments about the survey sponsor than those who did not, but did not differ in their responses to fixed-alternative survey questions. Nor did Shettle and Mooney (1999) find significant differences in responses in their experimental investigation of incentives

in a mail survey of college graduates.

11.4.3 Effects on Sample Composition

Whether to think about the effects of incentives on sample composition as “intended” or “unintended” consequences depends on whether they exacerbate or counteract tendencies to underrepresent certain subgroups of the population. In a 1994 paper presented to a COPAFS workshop, Kulka reported some evidence suggesting that monetary incentives might be especially effective in recruiting into the sample low-income and minority respondents, groups that ordinarily would be underrepresented in a probability sample (Goyder, 1999, and the sources cited there; but cf. Groves and Couper, 1998). Reviewing a number of experimental interviewer-mediated studies that provided evidence on the issue of sample composition, including the studies discussed by Kulka, Singer et al. (1999) found that in three such studies, there was an indication that paying an incentive might be useful in obtaining higher numbers of respondents in demographic categories that otherwise tend to be underrepresented in sample surveys (e.g., low income or nonwhite race). Five other studies reported no significant effects of incentives on sample composition, and in one study the results were mixed. An early review of the mail literature by Kanuk and Berenson (1975) reported similarly mixed results.

Since then, additional evidence has accumulated suggesting that monetary incentives can be especially effective in recruiting and retaining minority respondents in interviewer-mediated studies. Mack et al. (1998) found that the use of a \$20 incentive in the first wave of a SIPP panel was much more effective in recruiting and retaining black households and households in poverty than it was in recruiting and retaining non-black and non-poverty households. And a subsequent study by Abreu, Martin, and Winters (1999) found that an incentive of \$20 significantly

increased the recruitment of low-income nonrespondents to a previous wave of a SIPP panel, whereas \$40 was required to produce a significant difference among nonrespondents with higher incomes. Both sets of results are in agreement with findings reported by Juster and Suzman (1995). They report that a special Nonresponse Study, in which a sample of people who refused “normal” refusal conversion efforts on the Health and Retirement Survey were offered \$100 per individual or \$200 per couple to participate, brought into the sample a group of people distinctly different from other participants: they were more likely to be married, in better health, and, particularly, they had about 25% more net worth and a 16% higher income than other refusal conversion households or those who never refused. (In that study, all nonrespondents were sent the incentive offer by FedEx mail; hence, it was not possible to separate the effect of the monetary incentive from the special mailing. In a subsequent small-scale experiment, money had a significant effect on converting refusals, whereas a FedEx mailing did not; Hill, personal communication.) Finally, analyses by Singer, Van Hoewyk, and Maher (2000) indicate that a \$5 incentive paid in advance to a random half of RDD households for which an address could be located brought a disproportionate number of low-education respondents into the sample; there were no significant differences on other demographic characteristics.

In other words, these studies suggest that, while monetary incentives are effective with all respondents, less money is required to recruit and retain low-income groups than those whose income is higher, and for whom the tradeoff between the time required for the survey and the incentive offered may be less attractive when the incentive is “small.” It should be noted that few, if any, of these studies (Mack [1998] is a notable exception) have explicitly manipulated both the size of the incentive and the income level of the population; the findings reported above

are based on *ex post facto* analyses for different subgroups, or on analyses of the composition of the sample following the use of incentives.

A number of other studies have also reported on the effects of incentives on sample composition. In some of these, it appears that incentives can be used to *compensate for lack of salience of, or interest in,* the survey by some groups in the sample. For example, the survey reported on by Shettle and Mooney (1999), the National Survey of College Graduates, is believed to be much more salient to scientists and engineers than to other college graduates, and in the 1980's the latter had a much lower response rate. Although this was also true in the 1992 pretest for the 1993 survey, the bias was less in the incentive than in the nonincentive group, though not significantly so. Similar findings are reported by Baumgartner and Rathbun (1997), who found a significant impact of incentives on response rate in the group for which the survey topic had little salience, but virtually no impact in the high-salience group; and by Martinez-Ebers (1997), whose findings suggest that a \$5 incentive, enclosed with a mail questionnaire, was successful in motivating less-satisfied parents to continue their participation in a school-sponsored panel survey. Berlin et al. (1992) found that people with higher scores on an assessment of adult literacy, as well as people with higher educational levels, were overrepresented in their zero-incentive group. Groves, Singer, and Corning (1999) also reported a similar result; in their study, the impact of incentives on response rates was significantly greater for people low on a measure of community involvement than for those high on community involvement, who tend to participate at a higher rate even without monetary incentives. In these studies, incentives function by raising the response rate of those with little interest, or low civic involvement; they do not reduce the level of participation of the highly interested or more altruistic groups.

In all of these studies, certain kinds of dependent variables would be seriously mismeasured if incentives had not been used. And the theory of survey participation outlined at the beginning of this paper (Groves, Singer, and Corning, 1999) suggests that the representativeness of the sample will be increased by using a *variety* of motivational techniques, rather than relying on a single one.

11.4.4 Expectation Effects

11.4.4.1 Effects on Interviewers

Are the consistent effects of incentives in telephone and face-to-face interviews attributable to their effect on respondents, or are they, perhaps, mediated by their effect on interviewers? Clearly, this question does not arise with respect to mail surveys, where incentives have also been consistently effective; but it seemed important to try to answer it with respect to interviewer-mediated surveys. It is possible, for example, that interviewers expect respondents who have received an incentive to be more cooperative, and that they behave in such a way as to fulfill their expectations. (For evidence concerning interviewer expectation effects, see Hyman 1954; Sudman et al. 1977; Singer and Kohnke-Aguirre 1979; and Singer, Frankel, and Glassman 1983; but cf. Lynn, 1999.) Or, they may feel more confident about approaching a household that has received an incentive in the mail, and therefore be more effective in their interaction with the potential respondent.

In order to separate the effects of incentives on interviewers from their effects on respondents, Singer, Van Hoewyk, and Maher (2000) randomly divided all respondents for whom addresses could be obtained into three groups. One third of the group for whom addresses could be obtained were sent an advance letter and \$5; interviewers were kept blind to this

condition. Another third also received a letter plus \$5, and still another third received the letter only. Interviewers were made aware of these last two conditions by information presented on their Computer-Assisted Telephone Interview (CATI) screens.

Large differences were observed between the letter-only and the letter-plus-incentive conditions, but there is no evidence that this is due to the effect of incentives on interviewers. None of the differences between the condition in which interviewers were aware of the incentive and those in which they were not reached statistical significance. Thus, prepayment of a \$5 incentive substantially increases cooperation with an RDD survey, and the incentive appears to exert its effect directly on the respondent rather than being mediated through interviewer expectations. This conclusion is in accordance with research by Stanley Presser and Johnny Blair, at the University of Maryland, who also found substantial increases in response rates as a result of small prepayments to respondents to which interviewers were blind (personal communication).

11.4.4.2 Effects on Respondents

There are concerns that the payment of incentives, especially prepayment, will create expectations for future payment on the part of respondents. The effect of incentives may be direct--that is, arouse expectations on the part of respondents for payment the next time they are asked to participate in a survey--or both direct and indirect, creating a climate that affects even those members of the public who have not themselves been paid for their cooperation. In 1998, Singer, Van Hoewyk, and Maher reported that although people who had received a monetary incentive in the past were significantly more likely to endorse the statement that “people should be paid for doing surveys like this” than those who had not, they were actually

more likely to participate in a reinterview six months later, in spite of receiving no further payments. Subsequently, they extended this analysis, examining separately the effect of incentives offered at the outset of a survey and of refusal conversion payments, and controlling for the demographic characteristics of respondents as measured at the time of the initial survey.

The results of this analysis are shown in Table 1. Also included in each model are a series of demographic characteristics and interactions between them and the incentives. The effects of both kinds of incentives on response rates six months later are negative, but neither effect is significant.

The question remained, however, whether the absence of a significant effect of Time 1 incentives on Time 2 cooperation was because respondents construed the payment as covering the reinterview (by the same survey organization) as well as the initial interview, and whether the same results would be obtained if they were approached by a different survey organization. An experiment examining this question, reported by Singer, Groves and Corning (1999), suggests that they would, but respondents may not have construed the second request as coming from a different organization. Thus, this finding is clearly in need of replication.

11.4.5 Summary of Unintended Effects

Incentives do not appear to increase response rates at the expense of response quality, though research is needed on their effects on reliability and validity. However, there are indications that they do sometimes affect the distribution of responses as well as the composition of the sample, and more research is needed on the conditions under which these effects occur.

Incentives appear to increase response rates through their effect on respondents, not interviewers. There is no evidence that incentives create respondent expectations for future

payment on the *same* survey, but whether they create expectations for payment on unrelated surveys remains an open question, and may depend on the size of the incentive and the recency of the experience.

Table 5. Logistic Regression of Time 2 Participation on Time 1 Initial Incentives or Refusal Conversion Payments, Controlling for Demographic Characteristics and Interactions (n=4,055)

	Model 1		Model 2		Model 1		Model 2	
	Parameter Estimate	(S.E.)	Parameter Estimate	(S.E.)	Parameter Estimate	(S.E.)	Parameter Estimate	(S.E.)
Promised/prepaid incentives	-.073	(.077)	-.707	(.529)				
Refusal conversion payments					-.111	(.089)	-.668	(.616)
Age			.008 **	(.003)			.005 *	(.003)
Nonwhite			-.399 **	(.125)			-.390 **	(.122)
Hispanic			-.560 **	(.167)			-.431 **	(.165)
Education			.074 **	(.019)			.080 **	(.017)
Female			-.162	(.092)			-.094	(.084)
Income			-.000	(.000)			.000	(.000)
Age*Incentives/ Refusal Conversion			.001	(.005)			.014 *	(.006)
Nonwhite*Incentives/ Refusal Conversion			.037	(.231)			.012	(.241)
Hispanic*Incentives/R efusal Conversion			.546	(.319)			.098	(.323)
Education*Incentives/ Refusal Conversion			.020	(.032)			.004	(.038)
Female*Incentives/ Refusal Conversion			.209	(.158)			.003	(.184)
Income*Incentives/ Refusal Conversion			-.000	(.000)			.000	(.000)
Intercept	1.137**	(.045)	.018	(.307)	1.136 **	(.041)	-.068	(.281)

+ p=.06 * p<.05 ** p<.01

11.5 Issues in the Use of Differential Incentives

Some of the research reported above suggests that it may make economic sense to offer lower incentives to people with lower incomes and higher incentives to those who are economically better off. Another instance of differential incentives is the use of refusal conversion payments, in which respondents who have expressed reluctance, or who have actually refused, are offered payment for their participation whereas cooperative respondents are not. In both of these situations, the question arises how respondents who received lower, or no, rewards would feel if they learned of this practice, and how this might affect their future participation in this or another survey.

From an economic perspective, the fact that some people refuse to be interviewed may be an indication that the survey is more burdensome for them and that therefore the payment of incentives to such respondents (but not others) is justified. Nevertheless, some researchers are concerned that using incentives in this way will be perceived as inequitable by cooperative respondents, and, that, if they learn of the practice, this will adversely affect their willingness to cooperate in future surveys (Kulka 1994).

These unintended consequences were the focus of two studies (Singer, Groves, and Corning, 1999; Groves, Singer, Corning, and Bowers, 1999), the first done in the laboratory with community volunteers, using self-administered responses to videotaped vignettes, the second conducted as part of the Detroit Area Study, using face-to-face interviews.

The laboratory experiment resulted in significant negative effects of disclosing differential incentives to subjects on their expressed willingness to participate in the survey. However, this finding was not replicated in the field study. Most respondents in that study (75%) believed survey organizations are currently using incentives to encourage survey participation,

and these beliefs were affected by personal experience, so that those who themselves received an incentive in the survey were much more likely to believe this was a common practice. Only half of those who were aware of the use of incentives believed that payments are distributed equally to all respondents; and a large majority of respondents--74%--considered the practice of paying differential incentives unfair.

However, disclosure of differential payments to a random half of the respondents had *no* significant effect on their expressed willingness to participate in a future survey. About a quarter of each group said they would “definitely” be willing to participate in another survey by the same organization. Even those to whom differential payments were disclosed *and who perceived these payments as unfair* did not differ significantly in their expressed willingness to participate in another survey by the same organization, although the trend in responses was as predicted: 25.8% vs. 32.8% expressed such willingness. Nor were respondents to whom differential incentives had been disclosed significantly less likely to respond to a new survey request, from an ostensibly different organization, a year later, although again the differences were in the hypothesized direction. Given the small sample sizes involved in this experiment, and the fact that rapport with the interviewer may have affected some of the results, this finding of no effects is probably best regarded as tentative.

11.6 Are Prepaid Incentives Cost-Effective?

For a variety of reasons, including those discussed in the section immediately above, prepaid incentives to everyone in the sample may be preferable to refusal conversion or other differential payments.

One reason is that interviewers like them. Knowing the household is in receipt of an

advance payment, modest though it may be, they feel entitled to ask the respondent to reciprocate with an interview. Furthermore, prepaid incentives are equitable--they reward equally everyone who happens to fall into the sample, and they reward them for the "right" behavior--i.e., for cooperation, rather than refusal. Both of these advantages are likely to make modest prepaid incentives an attractive alternative to refusal conversion payments in many types of surveys.

There is also indirect evidence that the use of refusal conversion payments to persuade reluctant respondents leads to increasing reliance on such payments within an organization. Steeh (1999), for example, found that prior to 1995, the linear increase in the percentage of interviews resulting from refusal conversion was .05 each quarter, compared to .287 percent per quarter after 1995. This is also the period in which the SCA substantially increased its use of refusal conversion payments.

Still, the question arises whether prepaid incentives are cost-effective. On the face of it, it would appear that paying a small number of refusal conversion payments to reluctant respondents would be cheaper than paying everyone, even if those initial payments are smaller.

Several studies have concluded that prepaid incentives are cost-effective in mail surveys. For such surveys, the comparison ordinarily has been among incentives varying in amount or in kind, or in comparison with no incentive at all, rather than with refusal conversion payments. Two recent investigations of cost-effectiveness, by James and Bolstein (1992) and by Warriner et al. (1996), have included information on the relative effectiveness of various incentives. James and Bolstein (1992) found that a prepaid incentive of \$1 was the most cost-effective, yielding nearly as high a return as larger amounts for about one quarter of the cost. Warriner et al. (1996:9) conclude that for their study, a \$5 prepaid incentive was the optimal amount, resulting

in a saving of 40 cents per case (because the same response rate could be achieved as in a no-incentive, two-follow-up condition). The \$2 incentive resulted in costs per case only a dollar less than the \$5 incentive, while yielding a response rate 10 percentage points lower. Similar findings have been reported by Asch, Christakis, and Ubel (1998) in a mail survey of physicians.

For interviewer-mediated studies, as noted above, the comparison is much more likely to be with refusal conversion payments, and the answer is likely to depend on the nature of the study and the importance of a high response rate, on how interesting the study is to respondents (i.e., how many of them are willing to participate even without a prepaid incentive), on whether or not prepaid incentives reduce the effort required, and on a variety of other factors.

Several face-to-face surveys have reported that promised monetary incentives are cost-effective. Berlin et al. (1992), for example, reported that use of a \$20 incentive in a field test experiment with the National Adult Literacy Survey, which entails completion of a test booklet by the respondent, resulted in a cost saving per interview over the \$0 and \$35 incentive conditions when all field costs were taken into account. Similarly, Chromy and Horvitz (1978) reported (in a study of the use of monetary incentives among young adults in the National Assessment of Educational Progress) that when the cost of screening for eligible respondents is high, the use of incentives to increase response rates may actually reduce the cost per unit of data collected.

Singer, Van Hoewyk, and Maher (2000) investigated this problem in the SCA. They found that a \$5 incentive included with an advance letter significantly reduced the number of telephone calls required to close out a case (8.75 calls when an incentive was sent, compared with 10.22 when it was not; $p=.05$), and significantly reduced the number of interim refusals

(.282 refusals when an incentive was sent, compared with .459 when it was not). As expected, there was no significant difference between the incentive and the no-incentive condition in calls to first contact. The outcome of the first call indicates that compared with the letter only, the addition of a \$5 incentive resulted in more interviews, more appointments, and fewer contacts in which resistance was encountered.

Given the size of the incentive and the average cost per call aside from the incentive, sending a prepaid incentive to respondents for whom an address could be obtained was cost effective for the SCA. However, this conclusion depends on the size of the incentive as well as the structure of other costs associated with a study for a given organization, and should not be assumed to be invariant across organizations and incentives.

An argument that can be raised against the use of prepaid incentives is that they may undermine more altruistic motives for participating in surveys. Indeed, prepaid incentives have smaller effects on survey participation for people who score high on a measure of community activism (Groves, Singer, and Corning, 1999) than on people low on this characteristic. But this is because groups high in community activism already respond at a high rate. There is no evidence that people high on community activism who are offered a prepaid incentive respond at a *lower* rate than they would have had they not been offered the incentive. Although there is anecdotal evidence that some people are offended by the offer of an incentive, going so far as to return the incentive to the survey organization, by all accounts such negative reactions are few.

Conclusions

Prepaid incentives have been common in mail surveys for many years, although the

amounts used are ordinarily quite modest (see Church 1993). Because of increasing refusal rates in interviewer-mediated surveys and the widely documented success of incentives in counteracting them, we suspect that the use of incentives in such surveys will increase as well. But caution is needed in generalizing the results reported in this chapter, for two reasons. First, most of the experiments were not designed to test theoretically derived hypotheses. Second, many of the findings are based on one or a few experiments, and may not be replicable over time and across survey contexts. For example, most of the experiments agree in reporting smaller effects on response rates for gifts than cash. But there may well be situations in which gifts are more effective, because they are particularly well chosen to motivate participation--for example, umbrellas or beach balls (depending on the weather) handed out, with a smile, to travelers arriving at a local airport by interviewers conducting a tourist/travel survey (Edith de Leeuw, personal communication).

Thus, a great deal of specification and replication is needed. Here, I offer only a few modest suggestions for further research, noting that such research ought to be grounded in a theory of survey participation. The number of incentive experiments that could be designed is legion; unless they are guided by theory, they will not contribute to generalizable knowledge.

1. One question often asked is how large an incentive should be for a given survey. The issue here is the optimum size of an incentive, given other factors affecting survey response. If experiments varying the size of the incentive are designed in the context of a theory of survey participation that allows for changes in motivation over time, some generally useful answers to this question may emerge. In the absence of such theoretically based answers, pretesting is the only safe interim solution.

2. This chapter has documented effects of incentives on response distributions and sample composition. Research is needed to specify the conditions under which these effects occur.

3. Research is also needed on how paying respondents for survey participation affects both respondent and interviewer expectations for such payments in the long run.

4. Although a meta analysis of incentive effects in interviewer-mediated surveys found no differences by mode, this conclusion is in need of further research. For example, promised incentives may play a more useful role in face-to-face surveys, where the presence of the interviewer may engender trust and the delay in payment is relatively brief, than they do in RDD surveys.

5. Research is needed on the conditions under which incentives not only increase response rates but produce a meaningful reduction in nonresponse bias. Because they complement other motives for participating in surveys--such as interest in the survey topic, deference to the sponsor, or altruism--it is reasonable to hypothesize that incentives would serve to reduce the bias attributable to nonresponse. Whether the use of incentives for this purpose is cost-effective is less easily answered, however, and research is needed on this topic, as well.

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