

Incentives in Random Digit Dial Telephone Surveys: A Replication and Extension

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Singer, Van Hoewyk, and Maher (2000) found that a \$5 prepaid incentive markedly increased response rates during the period 1996 to 1998 on the University of Michigan's monthly RDD Survey of Consumer Attitudes (SCA). Although this led the study to adopt that incentive, SCA's response rate has declined sharply since then. We examine (a) whether the effect on response rates of the \$5 prepaid incentive has diminished; and (b) whether prepaid incentives of \$10 would, as predicted from earlier research, show a larger effect. In addition, we look at the effect of incentives on number of calls to obtain an interview, item nonresponse, response distributions, and sample composition, thus replicating analyses of the earlier article. We also examine the cost-effectiveness of prepaid initial incentives versus promised refusal conversion payments and the effect of differences between cases for which addresses can and cannot be obtained, topics neglected in the earlier article. Finally, we discuss implications of incentives for nonresponse bias.

Key words: Response rate; nonresponse bias.

A little over a decade ago monetary incentives were used relatively rarely in household interviews. A conference convened in 1993 by the U.S. Office of Management and Budget concluded that no incentives should be offered to respondents for taking part in household interviews that would generally be considered nonintrusive and that could be concluded in one session at a time and place of the respondent's choosing (Council of Professional Associations on Federal Statistics 1993). Since then, declining response rates have led survey organizations to rely increasingly on such compensation, in the form of an incentive paid to all participants before or after their participation, as a payment offered to those who initially refuse the interview, or both.

Following Gouldner (1960), some hypothesize that incentives produce their effects by virtue of the norm of reciprocity (e.g., Cialdini 1988); others conceptualize the process more simply as an exchange of tangible benefits in return for cooperation (e.g., Dillman 1978). Either way, innumerable studies have demonstrated the effectiveness of incentives in mail surveys (for a meta-analysis, see Church 1993) and, more recently, in telephone and face-to-face surveys as well (Singer et al. 1999). Hardly anyone today would question the value of incentives in motivating response to

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surveys. Among the questions that remain are those about the minimum amount of money needed to induce cooperation in a given situation for different kinds of persons, whether it is preferable to use refusal conversion payments only or to combine them with incentives offered to all participants, and how effective incentives are in reducing non-response bias.

Some of the questions pertaining to the use of incentives in random digit dial telephone surveys were addressed in a series of experiments embedded over a period of 15 months in the Survey of Consumer Attitudes (SCA) between 1996 and 1998 by Singer, Van Hoewyk, and Maher (2000). They demonstrated, among other things, that (1) prepaid incentives of \$5 were more effective than promised incentives in increasing the response rate; (2) prepaid incentives do not increase item nonresponse – if anything, they reduce it among certain subgroups; and (3) prepaid incentives were cost-effective because they reduced the number of calls required to close out a case. They found no effect of prepaid incentives on willingness to participate in the same survey six months later; nor, with one exception, did prepaid incentives, in the size offered in these experiments, appear to alter the sample composition. The exception was education: people with less education were more likely to participate when they were prepaid \$5 than when they were sent a letter only, but the effect was small.

Since then, response rates to the SCA have shown a marked decline, from about 57 percent in 1997 to about 48 percent in 2003,⁴ in spite of the continued use of prepaid incentives to that portion of the SCA sample for which addresses could be obtained. Accordingly, we decided to investigate whether (a) prepaid incentives of \$5 continued to produce the same increase in response rates as they had five years earlier, or whether the effect of such incentives had declined; and (b) whether prepaid incentives of \$10 would, as predicted from earlier research, show a larger improvement in response than a \$5 incentive. In addition, we look at the effect of respondent incentives on the number of calls required to obtain an interview and to close out a case, on item nonresponse, on response distributions, and on sample composition, thus replicating key analyses of Singer, Van Hoewyk, and Maher (2000). We also examine two issues neglected in the earlier article: the cost-effectiveness of prepaid incentives vs refusal conversion payments; and the differences between that portion of the sample for which addresses (necessary to mail the prepaid incentive) can be obtained and that portion for which they cannot, together with the effect of such differences on the Index of Consumer Sentiments, the key variable derived from the survey.

1. Methods

The University of Michigan Survey Research Center has conducted the SCA on a regular basis for over 50 years. For three decades, beginning in the late 1940s, it was conducted as an area probability in-person survey. Steeh (1981) drew on SCA results from 1954 through 1976 to describe the nature of the substantial response rate declines during that period.

⁴ These response rates are from Curtin, Presser, and Singer (2005) and correspond to AAPOR's Response Rate 2. No change in survey methodology has occurred over this time period.

In 1977, the SCA was converted to a random digit dial telephone survey, which has now been repeated every month for more than a quarter of a century.

Each monthly survey now consists of 300 cases from a newly drawn sample and 200 drawn from a survey carried out six months earlier. We restrict our analysis to the newly drawn samples, as did Singer, Van Hoewyk, and Maher (2000). These are random digit dial samples from the coterminous United States, drawn using list-assisted procedures. One respondent is randomly selected from among all household residents aged 18 or older. Except for the constraint imposed by the month-long interviewing period, no limit is placed on the number of calls, and attempts are made to convert virtually all initial refusals.

Our analysis is based on surveys carried out in November and December 2003 and January and February 2004. The response rate denominators include all sampled phone numbers with the exception of those known to be ineligible (e.g., nonworking numbers and businesses), and the numerators include the small number of partial interviews that got through all the key items in the questionnaire. This corresponds to AAPOR's Response Rate 2 (RR2).

In each of the four months, we randomly assigned the portion of the sample for which addresses could be obtained (roughly 50 percent, using both Axcion and Genesys, two independent providers) to one of three experimental conditions: advance letter without an incentive; advance letter plus \$5 incentive; and advance letter plus \$10 incentive.⁵ Interviewers were not told which respondents received letters with money. Normal follow-up procedures, including promised refusal conversion payments, were employed in all of the experimental conditions as well as for those respondents who were not sent an advance letter. The SCA promises either \$25 or \$50 to convert a refusal. Although at least two "hard" refusals are ordinarily required before a conversion payment is offered, interviewers have discretion with respect to both the amount and the timing of these incentive offers.

The distribution of the sample is shown in Table 1. Also included in Table 1, for comparison purposes, are response rates for those for whom we could not locate an address. These people are, of course, not a random subgroup of all respondents. Indeed, as in all other such comparisons, the response rate for this group – some 50 percent of the sample – is much lower than for the portion of the sample for which addresses could be obtained. One reason for this may be that a large proportion of those without a listed address are really ineligible for the sample, but, because of the short field period, we could not establish this in the time available. A second reason may be that people without a listed number may differ in a number of respects from those whose telephone numbers are listed; we return to this in the concluding section. A fifth relatively small group, consisting of people for whom letters were returned by the post office because the person named on the envelope no longer lived at that address, had response rates almost as high, on average,

⁵ All telephone numbers in the RDD sample were sent to both Genesys and Axcion to locate addresses. No address was found by either service for 48.6 percent of the numbers. Most of the remaining numbers were matched to the same address by both Genesys and Axcion (34.2 percent). Axcion had a slightly higher rate of unique address matches than Genesys (11.2 vs 6.0 percent), but yielded a somewhat lower response rate (54.1 vs 59.2 percent).

Table 1. Description of Sample and Experimental Conditions (Interviews conducted November 2003 to February 2004)

	Total sample		Eligible sample		Eligible sample as percent of total	Interviews	
	Cases	Percent	Cases	Percent		Cases	RR2
Address	1,568	43.9	1,334	52.8	85.1	815	61.1
Letter with \$0	521	14.6	443	17.5	85.0	229	51.7
Letter with \$5	520	14.6	445	17.6	85.6	284	63.8
Letter with \$10	527	14.8	446	17.6	84.6	302	67.7
No address	1,774	49.7	1,045	41.3	58.9	299	28.6
Returned letter	227	6.4	149	5.9	65.6	68	45.6
Total	3,569	100.0	2,528	100.0	70.8	1,182	46.8

as the letter-zero incentive group. One hypothesis when it comes to explaining the relatively high response rate shown by this group is that the telephone number we had was associated with a listed address, either that of the previous occupant (who now lived at a different address) or that of the current occupant (who had a different name or had ported the telephone number from a previous address to the current address). Closer investigation supported this hypothesis.

2. Results

2.1. *The Effect of Incentives on Response Rates*

Response rates by experimental condition are shown in Table 2.⁶ It is clear from Table 2 that a \$5 prepaid incentive made as much difference in the response rate in 2004 as it did in 1998: an average of 12.1 percentage points (cf. Singer, Van Hoewyk, and Maher 2000). However, the base response rate in 1998 was substantially higher among those receiving the letter only: an average of 65 percent from January through August 1998,⁷ compared with an average of 51.7 percent from November 2003 through February 2004. Thus, sending advance letters and prepaying \$5 kept the SCA response rate from declining even further than it otherwise would have. Increasing the prepayment to \$10 produced no additional significant gains in response.

2.2. *The Effect of Incentives on Survey Costs*

Could the SCA save money by sending advance letters without an incentive to everyone for whom an address can be obtained and offering refusal conversion payments to those who initially refuse?

⁶ By accident, the letter that was mailed to the December sample made no mention of the incentive, although the appropriate amount of money was enclosed. We examined results based on the three months excluding December as well as on all four months. Because there are virtually no differences, the analyses that follow are based on all four months of the experiment, including December.

⁷ Calculated from Tables 4 and 5 of Singer, Van Hoewyk, and Maher 2000.

Table 2. Response Rates by Experimental Condition

Experimental condition	Response rate (RR2)		Cases
Letter with no money	51.7		443
Letter with \$5	63.8		445
Letter with \$10	67.7		446
Significance tests			
\$0 versus \$5	$\chi^2 = 13.38$	d.f. = 1	$p < 0.01$
\$5 versus \$10	$\chi^2 = 1.50$	d.f. = 1	n.s.
\$0 versus \$10	$\chi^2 = 23.71$	d.f. = 1	$p < 0.01$

Brick et al. (2005) carried out an incentive experiment in the context of screening interviews required for an RDD survey. They created ten experimental conditions for all those for whom an address could be obtained (approximately half the sample) through different (not completely crossed) combinations of three factors: type of mailing (advance letters were all mailed first class, refusal conversion letters were randomly split between first class and priority mail), amount of prepaid incentive (\$0, \$2, \$5), and amount of refusal conversion incentive (\$0, \$2, \$5). Unlike the SCA, which makes refusal conversion payments contingent on an interview, Brick and his colleagues included these payments in the refusal conversion mailing.

Two conditions – where first class was used for both mailings and \$5 was included only in the refusal conversion letter, and where first-class letters included \$2 for both the initial and the refusal conversion – were the most cost-effective, yielding, respectively, final response rates of 69.5 percent and 69.9 percent. Brick et al. also note that the cost to the organization of completing a screener for a household that ever refused, exclusive of any costs associated with incentives or special mailings, is twice the cost for a household that never refused.

Unfortunately, we do not have information about all of the survey cost components needed to compute comparable costs for the SCA. We can, however, examine (a) the percent ever refusing in the \$0, \$5, and \$10 incentive conditions, (b) the mean number of calls required to obtain a completed interview in each of these three conditions, and (c) the average cost of all incentives – prepaid initial and promised refusal conversion – per interview in each of the three conditions. We hypothesized that eligible sample members receiving a prepaid incentive would be inclined to cooperate more readily – a hypothesis borne out by the earlier experiments – and that therefore, people in these conditions would be less likely to need refusal conversion payments in order to provide an interview.

Prepaid incentives do, indeed, lead to significantly fewer refusals. The percentage of eligible sample members ever refusing was 44% in the \$0 condition, but only 33% and 31% in the \$5 and \$10 conditions (Table 3). However, as Table 3 shows, only respondents who received an advance payment of \$10 were somewhat less likely to have received refusal conversion payments. Differences among the three conditions were surprisingly small given the much higher level of initial refusals among those in the \$0 condition – perhaps because of the limited time (one month) for completing the survey and the need to maintain high response rates. As a result, the average cost of incentives per interview is substantially higher in the two prepaid incentive conditions – \$13.91 and \$19.65,

Table 3. *Percent Ever Refusing, Percent with Refusal Payment, and Average Incentive Cost per Interview (Interviewed Experimental Sample)*

	Prepayment		
	\$0	\$5	\$10
Percent ever refusing	44%	33%	31%
Percent with refusal payment	17%	17%	14%
Average incentive cost/interview	\$6.11	\$13.91	\$19.65
Response rate (RR2)	51.7%	63.8%	67.7%
Total interviewed cases	229	284	302

compared with \$6.11 in the condition where no prepayment was included with the advance letter. The response rates, of course, were also higher.

Although the mean number of calls required to complete an interview was marginally lower in the \$10 than in the \$5 condition, and significantly lower in the \$10 than in the \$0 condition, there was no significant difference between the \$0 and \$5 conditions (Table 4). Trends were similar in the number of calls required to close out a case (data not shown).

Other effects of prepaid incentives noted by Singer, Van Hoewyk, and Maher (2000) – e.g., fewer interim refusals, more interviews completed on the first call, and more appointments on the first call – may help to offset the higher costs of using prepaid incentives. The extent to which they do so undoubtedly varies with the size of the incentive, the size of the sample, and the structure of other costs borne by the survey organization. (For example, the SCA has not experimented with using prepaid incentives smaller than \$5, which might help reduce costs without greatly reducing the response rate. The option of using prepaid refusal conversion incentives might be difficult to implement on the SCA, because of the short field period.) But the decision to offer prepaid incentives will often involve a tradeoff between costs and response rates, as well as a decision about whether the gain in response is worth the added cost. We return to this issue in the concluding section.

2.3. *Prepaid Incentives, Refusal Conversion Payments, and Response Quality*

2.3.1. Incentives and Item Nonresponse

The question has been raised whether efforts to increase the response rate jeopardize response quality – whether people induced to respond by the offer of an incentive or by a persistent interviewer agree to the interview but offer ill-considered answers or none at all. Like Singer, Van Hoewyk, and Maher (2000), we found no evidence of such a tradeoff (Table 5). Because it examines the effects of refusal as well as prepaid incentives, the analysis in Table 5 is based on the entire interviewed sample instead of being restricted to that portion of the sample involved in the incentive experiment.⁸

⁸ Findings for the effect of incentives do not change if the analysis is restricted to the experimental portion of the sample.

Table 4. Mean Number of Calls to Interview by Experimental Condition

Experimental condition	Mean calls	Standard error	Cases
Letter with no money	7.32	0.54	229
Letter with \$5	7.19	0.49	284
Letter with \$10	6.03	0.39	302
Significance tests			
\$0 versus \$5	$t = 0.19$	n.s.	
\$5 versus \$10	$t = 1.84$	$p < 0.10$	
\$0 versus \$10	$t = 1.98$	$p < 0.05$	

Singer, Van Hoewyk, and Maher (2000) had found a significant negative effect on item nonresponse of both prepaid and refusal conversion incentives, which was reduced to insignificance when interactions with demographic variables were controlled.⁹ In the current study, prepayment had no significant effect on item nonresponse even without demographic controls, and we found only three variables with such significant effects. Older people and nonwhites were significantly more likely, and women marginally so, to have item missing data, whereas people with higher incomes were less likely to do so. The analysis revealed no significant interactions between the receipt of incentives and these demographic characteristics. These relationships between item nonresponse and demographic characteristics are very similar to those reported in the earlier study.

2.3.2. Incentives and Response Distributions

There are suggestions in some earlier research that paying incentives to respondents alters the responses they would otherwise have given, quite aside from the possible effects of incentives on the composition of the sample. For example, experimental findings by Schwarz and Clore (1996) about the effects of mood suggest that if incentives put respondents in a more optimistic mood, some of their replies may be influenced as a result. More optimistic replies after receipt of incentives were reported by Brehm (1994) and by James and Bolstein (1990), but not by Shettle and Mooney (1999). Singer, Van Hoewyk, and Maher (2000) found that for five of 18 key variables on the SCA, respondents who received either a prepaid or a refusal conversion incentive, or both, gave a more optimistic response to the question, controlling for demographic characteristics; on two others, they gave more pessimistic responses; and on the third, there were more optimistic as well as more pessimistic responses and fewer neutral replies. In the present study, we investigated the effect of prepaid and refusal conversion payments on the Index of Consumer Sentiment, since this is the most important measure derived from the SCA.

Table 6 examines the effect of incentives in the presence of demographic controls, again in the entire interviewed sample. People with higher incomes are significantly more optimistic, and older people, nonwhites, and women significantly more pessimistic, in their economic outlook. Neither prepaid nor refusal conversion incentives had a

⁹The Index of Item Nonresponse consisted of the percentage of “don’t knows” and “no answers” with regard to 17 key questions on the Survey of Consumer Attitudes, indicating, for example, respondents’ assessments of their current and future family finances and income, the nation’s business and employment conditions, and the government’s role in the country’s economy.

Table 5. Log Index of Item Nonresponse Regressed on Incentives and Demographics

Variables	Parameter estimate (Standard error)	
	(1)	(2)
Advance letter with money – dummy	– 0.049 (0.060)	– 0.031 (0.059)
Promised refusal payment – dummy	0.029 (0.078)	0.089 (0.075)
Age		0.012* (0.002)
Nonwhite		0.220* (0.073)
Education		– 0.014 (0.013)
Female		0.109 (0.059)
Log income		– 0.152* (0.038)
Intercept	0.654 (0.045)	0.744* (0.230)
Adj RSQD	.000	0.072
Cases	1,152	1,152

Note: *Indicates $p < .05$. The Index of Item Nonresponse has been logarithmically transformed to correct for skew.

statistically significant effect on responses to the ICS, but the analysis uncovered an important interaction between nonwhite and receipt of an advance incentive: nonwhites who received such incentives provided significantly more optimistic responses.

2.4. The Effects of Incentives on Sample Composition

One argument for offering incentives to potential respondents – whether prepaid or in the form of refusal conversion payments – is that this may bring into the sample groups not otherwise disposed to respond, for example the less well educated, nonwhites, and people with lower incomes, and some studies claim to have found such effects (for a review, see Singer and Kulka 2002, pp. 115–116). As a result, surveys paying incentives may actually produce less biased samples than those that fail to do so. A similar argument has been made for the usefulness of incentives in reducing nonignorable unit nonresponse due, for example, to a differential sense of community involvement or differing interest in the topic of the survey (Singer and Kulka 2002, pp. 116–117; Groves, Presser, and Dipko 2004).

Because no demographic or attitudinal information is available for nonrespondents to the SCA, this survey is not well suited for testing these hypotheses. It is, however, possible to investigate the effect of *prepaid* incentives on the demographic composition of the resulting samples, since these incentives are administered to random subsamples of those for whom an address could be obtained. Among those interviewed, there were no significant demographic differences between those who had and those who had not been sent an incentive ahead of time;¹⁰ these results parallel those reported in Singer, Van Hoewyk, and Maher (2000). As noted there, larger effects have been reported in some studies; all of these, however, have used both larger incentives and larger samples.

¹⁰The demographic variables tested were age, education, income, sex, race, and ethnicity.

Table 6. Determinants of Index of Consumer Sentiment

Variables	Parameter estimate (Standard error)		
	(1)	(2)	(3)
Advance letter with money – dummy	3.546 (2.297)	3.987 (2.271)	– 0.346 (2.551)
Promised refusal payment – dummy	3.223 (2.983)	1.317 (2.885)	– 0.025 (3.256)
Age		– 0.937* (0.361)	– 0.931* (0.359)
Age ²		0.005 (0.004)	0.005 (0.004)
Nonwhite		– 6.026* (2.796)	– 15.124* (3.840)
Education		0.524 (0.491)	0.428 (0.490)
Female		– 6.154* (2.275)	– 6.460* (2.266)
Log income		6.805* (1.465)	6.712* (1.458)
Nonwhite * letter with money			19.671* (5.417)
Nonwhite * refusal payment			4.001 (6.870)
Intercept	92.93* (1.73)	97.028* (10.79)	101.569* (10.808)
Adj RSQD	0.001	0.084	0.093
Cases	1,182	1,152	1,152

Note: * indicates $p < 0.05$

We performed a similar analysis for promised refusal conversion payments, asking which, if any, demographic characteristics predicted the acceptance of a promised refusal conversion payment. We again used the entire interviewed sample instead of the smaller experimental sample since all sample members were eligible to receive the refusal conversion payment. With the exception of age, there were no significant differences (see Table 7); older people were significantly less likely to receive refusal conversion payments. This result differs from that reported in Singer, Van Hoewyk, and Maher (2000), where those who received refusal conversion payments differed significantly from those who did not on virtually all the demographic categories we examined (Hispanic, nonwhite, education, and age), and differences reflected a greater likelihood of payments to groups more likely to refuse. But because that analysis was based on 15 months of

Table 7. Demographic Predictors of Receipt of Refusal Conversion Payment

Variables	Parameter estimate (Standard error)
Age	– 0.016* (0.005)
Nonwhite	– 0.236 (0.193)
Education	– 0.050 (0.033)
Female	– 0.096 (0.155)
Log income	– 0.003 (0.099)
Intercept	0.087 (0.085)
Adj RSQD	0.012
Cases	1,152

Note: * indicates $p < 0.05$

experiments rather than four and therefore on a much larger number of refusals, we do not know whether the differences are due to the reduced power of the statistical tests or to changes in the time of measurement or survey conditions.

2.5. *The Impact of Address Matching on Sample Composition and Response Distributions*

Relying on address matching, advance letters, and prepaid incentives results in increased response rates to a survey. However, as a consequence, an increasingly disproportionate part of the sample comes from those for whom an address could be obtained. Although it is known that such respondents differ in some respects from those with unlisted numbers, the impact of using address matching on sample composition and response distributions has not been extensively investigated because no information is ordinarily obtained from the noninterviewed portion of the sample (but see Parks, Kennedy, and Hecht 1994 and Johnson et al. 2006, who used neighborhood characteristics as proxies for such demographic information about individuals, and King 1998 who proposed a similar method). Instead, we adopted the second-best expedient of examining the demographic correlates of having a mailable address (as a result of having a listed telephone number) among the interviewed portion of the sample. The results of a logistic regression equation predicting the availability of an address are shown in Table 8, which indicates that an address was significantly less likely to be available for nonwhites and significantly more likely to be available for better educated and higher-income respondents.

An examination of the effect of address availability on the Index of Consumer Sentiment, controlling for demographic characteristics, reveals no significant net effect of address (Table 9).

There are, however, significant interactions between some of the demographic variables and the availability of an address, though with the exception of age, these involve differences in size of the coefficient and not in sign (Table 10). Weighting on demographics alone is unlikely to compensate for this, since having a listed address is not simply a function of demographic characteristics. It may, in addition, be associated with greater trust, greater cooperativeness, and less mobility – qualities that may be correlated with responses to the questions making up the Index of Consumer Sentiment. If, in other words, those with unlisted telephone numbers, estimated to be on the order of 35 percent

Table 8. *Demographic Predictors of Address Availability*

Variables	Parameter estimate (Standard error)
Age	0.002 (0.023)
Age ²	0.0003 (0.0002)
Nonwhite	-0.513* (0.155)
Education	0.058* (0.029)
Female	-0.055 (0.136)
Log income	0.184* (0.087)
Constant	-1.275* (0.630)
Adj. R ²	0.099
Cases	1,152

Note: * indicates $p < 0.05$

Table 9. Predictors of Index of Consumer Sentiment

Variable	Parameter estimate (Standard error)	
	(1)	(2)
Listed address – dummy variable	1.494 (2.479)	3.102 (2.507)
Age		– 0.959* (0.361)
Age ²		0.005 (0.004)
Nonwhite		– 6.102* (2.801)
Education		0.513 (0.492)
Female		– 6.133* (2.275)
Log income		6.819* (1.466)
Intercept	94.24* (2.06)	97.82* (10.71)
Adj. R ²	0.001	0.083
Cases	1,182	1,152

Note: * indicates $p < 0.05$

(Linda Piekarski, personal communication), were represented in the SCA in proportion to their actual presence in the population, the ICS might be somewhat lower than it is in these data.

3. Discussion and Conclusion

Prepaid incentives of \$5 continued to produce the same increase in response rates in 2004 as they had in 1998, namely about 12 percentage points.¹¹ This gain was nearly equal to the overall decline of about 13 percentage points in response rates among sample members who had received an advance letter but no incentive during the same time period. Although an increase in the prepaid incentive to \$10 did increase the response rate above the \$5 incentive condition, the additional gain of 4 percentage points was not statistically significant. The data thus indicate that the decisions of potential respondents were not very sensitive to a doubling of the incentive to \$10. This is consistent with earlier findings of diminishing effects of increasing amounts of incentives.

Whether incentives are cost-effective depends on the evaluation of five essential elements: the costs of incentives, other costs of conducting the survey, the increase in response rates, reduction in sampling variance, and the reduction in nonresponse bias. The added costs of the incentives need to be balanced against the savings from the reduced number of calls and the avoidance of refusals. The data indicated that fewer calls were required to complete the interview and fewer calls to close out a case with prepaid incentives compared with the letter-only condition, although the reduction was significant only with a \$10 incentive. But prepaid incentives were very effective at averting refusals, with a reduction of 11–13 percentage points in the number of sample members refusing at least once, compared with the letter-only condition.

¹¹ The increase of 12 percentage points was based on the use of RR2 and thus represents the minimum estimate of the increase. RR2 was used to avoid issues concerning the estimate of eligibility of noncontacts.

Table 10. Predictors of ICS by Presence of Listed Address

Variable	Parameter estimate (Standard error)		
	All	Listed address	No listed address
Age	-0.946* (0.361)	-1.341* (0.436)	-0.015 (0.692)
Age ²	0.005 (0.004)	0.009* (0.004)	-0.006 (0.007)
Nonwhite	-6.464* (2.786)	-2.888 (3.474)	-11.870* (4.821)
Education	0.550 (0.491)	0.290 (0.605)	0.757 (0.854)
Female	-6.165* (2.276)	-3.704 (2.654)	-11.399* (4.375)
Log income	6.931* (1.464)	8.697* (1.803)	3.588 (2.547)
Intercept	98.42* (10.70)	102.91* (13.44)	92.79* (18.81)
Adj. R ²	0.083	0.089	0.080
Cases	1,152	800	352

Note: * indicates $p < 0.05$

The relative advantage of refusal conversion payments versus prepaid incentives plus refusal conversion payments was also examined. Contrary to our hypothesis, although we found a large and statistically significant reduction in the percentage ever refusing in those conditions, we found no reduction in the percentage of those who accepted a promised refusal conversion payment in the prepaid incentive conditions compared with the letter-only condition. As a result, based on the additional costs of the incentives, the higher response rates in the prepaid incentive conditions are accompanied by a higher average incentive cost per interview.

Higher response rates are desired because they may enhance the representativeness of the results and are the best defense against potential nonresponse bias. We approached this issue by separately examining the impact of incentives on unit and item nonresponse as well as measurement error.

Because prepaid incentives were offered only to those sample members for whom we could obtain an address, we compared the demographic composition of those with and without listed addresses. A listed address was less likely to be available for nonwhites, and more likely to be available for better-educated respondents and those with higher incomes. Thus, as used in RDD surveys, prepaid incentives disproportionately bring into the sample those already predisposed to respond (cf. Parks, Kennedy, and Hecht 1994, who note the same effect for advance letters). Refusal conversion payments have the potential for counteracting this effect, but only if the refusal payments are targeted at specific demographic groups that are known to be underrepresented in the sample. However, the composition of the interviewed sample was altered neither by the use of prepaid incentives nor, with one exception, as a result of refusal conversion incentives, suggesting that as used here, incentives do not counteract any compositional bias.

No significant effects from the incentives were found on item nonresponse. The data did indicate that prepaid incentives had a significant impact on the values for the Index of Consumer Sentiment among nonwhites, leading to more optimistic responses, but no such effect was present for the larger refusal conversion payments.

We also examined the effect of address availability on responses to the Index of Consumer Sentiment. Although we found no significant main effect of having an address available, we did find significant interactions between address availability and several of

the demographic variables. Since a disproportionate share of respondents consists of those with addresses, this finding suggests that respondents should be appropriately weighted, not only by demographic characteristics but also by whether or not their telephone number could be matched to an address.

These findings highlight a central question faced by survey designers: namely, the impact of response rate on measurement error. Earlier attempts to address this question for the SCA (Curtin, Presser, and Singer 2000) and for several other surveys (Keeter, Miller, Kohut, Groves, and Presser 2000; Merkle and Edelman 2002; Groves, Presser, and Dipko 2004; Keeter et al. 2006) suggest that within a fairly wide range, reduced response rates do not lead to nonignorable nonresponse.

Nevertheless, leverage-salience theory (Groves, Singer, and Corning 2000) posits that different motives (leverages) for survey participation exist, and that these can be activated by, for example, emphasizing different topics or sponsors in the survey introduction or in an advance letter, as well as by offering a monetary incentive to persuade otherwise unmotivated sample members to respond. The effects of such differential motives on participation have been demonstrated in a number of studies (e.g., Baumgartner and Rathbun 1997; Groves, Singer, and Corning 2000; Groves, Presser, and Dipko 2004; Groves et al. 2006). In some of these studies, key variables would have been misestimated if incentives had not been used, and the conditions under which nonresponse can be expected to lead to errors in estimation have been spelled out in Groves, Presser, and Dipko 2004 and Groves 2006. Leverage-salience theory argues that incentives, which provide an alternative motive for responding, can at times counteract such potential nonresponse bias.¹²

But which combination of incentive strategies is most likely to counteract nonresponse bias? We argue that a strategy that merely brings into the sample more of those already predisposed to respond will not accomplish this purpose. The finding that there are no observable differences in the demographic composition of the sample as a result of the use of incentives, and no consistent differences in the response distributions, suggests that the combination of prepaid incentives and nontargeted use of refusal conversion payments raises the response rate without counteracting such nonresponse bias as potentially exists.

If the investigators know the distribution of the dependent variable in the eligible sample, or the distribution of characteristics strongly correlated with this variable, they might target for refusal conversion only those underrepresented in the interviewed sample. For example, African-Americans are known to be much more concerned about privacy than Whites. They are also less likely to be included in survey sampling frames, and they are less likely to respond. In a survey of privacy attitudes, therefore, special efforts should be made to contact and convert refusals by African-Americans.

In most surveys, however, the distribution of the dependent variable is not known. In such surveys, it may be more useful to (a) use prepaid incentives in order to supply an initial alternative motive to respond for those who do not find the topic or the sponsor (or other characteristics of the survey) intrinsically appealing and (b) to concentrate refusal conversion efforts, including refusal conversion payments, on those who did *not* receive

¹²The persuasive abilities of the interviewer are another such countervailing influence, but, as has been shown in this and many other studies, the amount of effort required from interviewers can be significantly reduced through the use of incentives.

the prepaid incentive. While this strategy will not yield the highest response rate, it is most likely to bring into the sample at least some of those least motivated to respond, and therefore most likely to bias the results by their absence.

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