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Monetary Incentives in U.S. Government Surveys

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In this article we examine the impacts of using a monetary incentive in a government mail survey on the following factors: (1) Response Rate; (2) Nonresponse Bias; (3) Data Quality; (4) Indicators of Respondents' Views of Incentives; and (5) Cost-effectiveness. The article contrasts two experimental groups differing only in terms of whether sample members received a small (5.00 USD) monetary incentive in the first mailing of a lengthy mail survey of moderate saliency. On the basis of the information in this study and the literature reviewed, the authors conclude that incentives can provide a cost-effective survey tool for use in government surveys when moderately high response rates are needed. Small monetary incentives in government surveys do not appear to increase nonresponse bias, decrease data quality, or create respondent ill will.

Key words: Nonresponse; data quality; response rate.

1. Introduction

Many studies have successfully used incentives to increase response rates (Ferber and Sudman 1974, Armstrong 1975, Kanuk and Berensen 1975, Linsky 1975, Heberlein and Baumgartner 1978, Hansen 1980, Kerachsky and Maller 1981, Yu and Cooper 1983; Goyder 1985, Berry and Kanouse 1987, Fox and Kim 1988, Hopkins, Hopkins, and Schon 1988, James and Bolstein 1990, Brennan 1992, Brennan, Hoek, and Astridge 1991, Groves, Cialdini, and Couper 1992, James and Bolstein 1992, Brennan 1992, Church 1993, Perneger, Etter, and Rougement 1993, Willimack, Schuman, and Lepkowski 1995, Baumgartner and Rathbun 1996, Dillman 1996, Groves and Couper 1998). Why incentives motivate greater participation has been explored through several theoretical perspectives. Dillman (1978) explains their effectiveness using social exchange theory. A small monetary incentive sent as a token of appreciation is viewed as placing the survey into a social context in which trust prevails and completing the survey is part of a social

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exchange. From this perspective, it is not simply the value of the incentive that is important, but the receiver's perception of the giver's intent. Groves, Cialdini, and Couper (1992) emphasize the importance of the norm of reciprocity in this exchange, i.e., there is a normative expectation in our society that the recipient of a favor (the incentive) will reciprocate by performing a favor for the provider (completing the survey). The metric of the exchange includes noneconomic intangibles such as ''thoughtfulness.''[3]

Biner and Kidd (1994) argue that economic exchange (or what they call the ''equity principle'') can also be an important part of the value of incentives, especially if the appeal is made explicit and the burden of the survey is consistent with the amount of incentive offered. From this perspective, the individual is motivated by the monetary gain (i.e., the incentive) associated with the task (completing the survey). The amount of the incentive is compared to the burden of completing the survey in a manner analogous to other economic decisions.

It is also possible that the use of incentives helps to convey the importance the sponsor places on survey completion. From this perspective, the incentive assists in evoking what Groves, Cialdini, and Couper (1992) refer to as the ''helping norm.''

These theoretical principles are not necessarily mutually exclusive, i.e., incentives **may** motivate some individuals through one mechanism while motivating others through a different mechanism. As Groves, Cialdini, and Couper (1992) point out, experienced interviewers are well aware that not everyone is equally persuaded by a particular approach and modify their behavior accordingly. Further, a given individual may be motivated by more than one factor. For example, an individual may view completing the survey as being rewarding economically and socially. Similarly, some individuals may complete the form in order to avoid the guilt associated with violating both the ''norm of reciprocity'' and the ''helping norm.''

If survey researchers were solely interested in maximizing response rates, there would be little need to do yet another study of incentives. However, response rates are only one factor affecting survey quality and costs. Concerns raised about incentives include the possibility that:

- Some individuals may perceive incentives to be ''undue pressure to comply'' (Groves, Cialdini, and Couper 1992) – concern that is especially strong among government administrators who worry about the government's image.
- Incentives may increase nonresponse bias, by increasing the differences between respondents and nonrespondents.
- Individuals who respond solely because an incentive is offered may be, on average, relatively careless, thereby reducing data quality.
- Incentives introduce a source of response bias into the data by increasing respondents' desire to respond in ways pleasing to the sponsor.
- The costs of incentives exceed their benefits.

Few of the empirical studies done to date have examined whether incentives actually have negative impacts on survey quality and survey costs. Further, little theoretical

[3] For example, a book on a topic of high interest to the recipient may have higher ''value'' in a social exchange than a more expensive book of little interest.

work has been done to understand why incentives do or do not impact survey quality. Although those few studies that have examined survey quality (Berry and Kanouse 1987, Hopkins, Hopkins, and Schon 1988, Willimack, Schuman, and Lepkowski 1995) report encouraging results, the evidence on these topics cannot yet be considered conclusive.

As a result of the concerns about the use of incentives, the 1992 pretest for the National Science Foundation's (NSF) 1993 National Survey of College Graduates (NSCG) was designed to test the impact of a prepaid monetary incentive on cooperation rates, nonresponse bias, data quality, and survey costs. The evidence presented suggests that incentives provide a decided cost advantage, assuming moderately high response rates are desired, without offsetting negative effects on survey quality.

2. Methodology

The pretest selected a sample of 3,200 college graduates from a three years old retired Current Population Survey (CPS) sample. Half were randomly selected from among those who reported having a science or engineering (S&E) occupation, while the other half were randomly selected from those not reporting an S&E occupation. With the sample stratified by S&E status, the 3,200 sample members were randomly assigned to one of four treatment groups:

- Long form – 20 page booklet[4] (no incentive)
- Long form (with incentive)
- Midlength form – 12 page booklet (no incentive)
- Screener (four pages) follow-up questionnaires – eight pages (no incentive).

This article focuses on the first two treatment groups, which hold constant questionnaire length. Other than including a 5 USD incentive check in the initial mailing and language in the cover letter that identified the check as a token of appreciation for filling out the questionnaire, the same data collection procedures were used in all four treatment groups.

The mail procedures used in this study included a prenotification letter, followed by an initial questionnaire mailing (a ''user friendly'' questionnaire,[5] cover letter and postage-paid business reply envelope) about 7–10 days later, followed by a thank you/reminder letter approximately a week later. This reminder letter was followed by a second questionnaire mailing (questionnaire, a second cover letter and postage-paid business reply envelope) about five weeks after the initial questionnaire mailing. About a month after the second questionnaire mailing, telephone follow-up began. Because of budgetary constraints, follow-up was only conducted for a random sample consisting of two-thirds of the nonrespondents. Sample members were not reminded of or offered the incentive subsequent to the first mailing.

After the first mailing, the 16.5 percent of the questionnaires returned as ''undeliverable'' were assigned for aggressive in-person follow-up treatment. As expected, given random assignment, the percentage of undeliverable questionnaires varied little among

[4] The booklet consisted of 12 pages of questions, a four-page list of occupation and education codes needed to respond to several questions, and cover page.

[5] The questionnaire was designed by Mathematica Policy Research in conjunction with NSF and was reviewed by Don Dillman.

the treatment groups. Because the incentive was not offered during telephone or in-person follow-up, all of the ''undeliverables'' assigned to the aggressive in-person follow-up had to be removed from the experiment. As a consequence, the monetary incentive could not be evaluated using a traditional response rate definition. Instead, we evaluated the impact of incentives using a cooperation rate, defined as follows:

- Numerator = the number of ''useful'' questionnaires from both eligible and ineligible sample members
- Denominator = the original sample minus the undeliverables

For this analysis, a ''useful'' response was defined as one in which the questionnaire was at least partially completed or the respondent (or other informant) provided sufficient information to determine that the individual was out-of-scope.[6] This is mathematically equivalent to assuming that the out-of-scope rate for nonrespondents is the same as for respondents. An alternative approach, often used in calculating nonresponse rates, is to exclude out-of-scope individuals from both the numerator and the denominator. If all out-of-scope cases could be identified, this alternative method would provide a good estimate of the cooperation rate for in-scope individuals. We chose the first method because we believe it is a more accurate reflection of reality and because the information on whether the case is out-of-scope is useful in calculating nonresponse adjustments. Examination of a few key statistics using this alternate definition indicated that its use would not have had a material effect on the study conclusions.

Cooperation rates for each of the groups were recorded at key points during the survey process, using manual tallies. Estimates of cumulative cooperation rates weighted the phone and in-person returns by the inverse of the probability of selection into the sub-sample.

Information from returned mail questionnaires and phone interviews was keyed into the computer along with CPS information for respondents and nonrespondents. The resulting data file was used for the analyses for nonresponse bias and data quality.[7] Analyses of data quality were done for those respondents included in the pretest file except those classified as out-of-scope due to death, illness, institutionalization, or emigration from the country.

Analyses of the differences between the cooperation rates of the incentive and non-incentive groups were done, using *t*-tests. Analyses of the interaction effects between incentives and demographic variables used the GLM package in SAS to perform an Analysis of Variance. *t*-tests associated with this analysis used the Bonferroni test that adjusts for the fact that multiple tests of significance are being performed.

Since the dependent variable is a dichotomy, logistic regression analysis is technically a more appropriate technique to use; however, it is more difficult to interpret than linear regression. The relevant findings of this article were accordingly verified, using logistic regression analysis with the log of age included in lieu of the collapsed age variable

[6] Out-of-scope cases constituted seven percent of the nonincentive sample and six percent of the incentive sample.

[7] Unfortunately, this data file lacks information that would have permitted more extensive analyses of some of the issues raised in this article. For example, it does not contain a reliable indicator of completion mode, so that analyses of data quality cannot be performed for individuals who responded to the first mail request when the difference between the incentive and nonincentive response rates was at a maximum.

reported in this article. Since the findings did not vary substantially from those in the article, only the easier-to-understand linear results are presented.

Differences in item responses between the incentive and non-incentive groups used *t*-tests between mean responses for continuous variables (including dichotomous variables that were treated as dummy variables). Discrete variables were tested for significance, using chi-square tests. In order to avoid small cells, appropriate collapsing of categories was used in these tests.

Cost information was estimated for variable data collection costs only. These included the cost of the incentives themselves (minus the savings from uncashed cheques), the cost of processing the incentive cheques, costs associated with the mailings, CATI follow-up, and personal visit follow-ups. The costs were derived from preliminary cost estimates for the 1993 NSCG, using 1993 dollars.

All statistical tests between the incentive and nonincentive groups assumed a simple random sample, since individuals from the expired CPS sample were randomly assigned to either the incentive or the nonincentive group. These tests, therefore, indicate whether observed differences between the incentive and nonincentive groups can be attributed to chance differences in the assignment of individuals to these two groups.

It is necessary to emphasize that a major limitation of this study is that the sample is not truly representative of the entire U.S. population. First, and probably most important, the sample is restricted to individuals who have completed at least four years of college. Second, it is restricted to individuals who were part of the expired CPS sample. These individuals have had more prior experience, on average, with government surveys conducted by the U.S. Census Bureau than the general population has. Third, the sample over-represents those in science and engineering occupations. Fourth, CPS sampling weights were ignored in selecting individuals into the pretest sample; thus, individuals over-represented in the CPS sample – primarily individuals from states with small populations (U.S. Bureau of Labor Statistics 1997) – will also be over-represented in the pretest sample.

3. The Impact of Incentives on Cooperation Rates

First Mailing. As shown in Table 1, the overall first mailing cooperation rate was 67.8 percent for those who received incentives, as compared to 53.9 percent for those who did not receive incentives. The difference between these two groups is statistically significant at the .05 level.

Table 1. Mail cooperation rates, by whether incentive received

Group	First mailing			Second mailing			Combined first and second mailing		
	n	Percent	S.E.	*n*	Percent	S.E.	*n*	Percent	S.E.
No incentive	675	53.9	1.9	311	19.1	2.2	675	62.7	1.9
Incentive	674	67.8*	1.8	217	17.1	2.6	674	73.3*	1.7
Total	1,349	60.8	1.3	528	18.3	1.7	1,349	68.0	1.3

*Incentive and nonincentive groups significantly different at the .05 level.

Second Mailing. The cooperation rate for individuals receiving the second wave mailing was slightly lower for the incentive group than the nonincentive group (17 percent versus 19 percent). This difference is not statistically significant. After the second mailing, the cumulative cooperation rate for the incentive group was 73.3 percent, versus 62.7 percent for the nonincentive group. Thus, the incentive increased the overall mail cooperation rate by nearly 11 percentage points, as compared to the 14 percentage-point difference observed after the first mailing. This finding is consistent with that of other studies that have shown that the impact of incentives included decreases after subsequent follow-up (Hopkins, Hopkins, and Schon 1988, p. 173; Brennan, Hoek, and Astridge 1991).

The apparent diminished impact of the incentive on the second mailing is consistent with an assumption that individuals vary in their susceptibility to incentives. Since it is likely that most incentive-group individuals who are highly susceptible to incentives returned their original questionnaire, most incentive group members who received the second mailing presumably have a low susceptibility to this appeal. Further, since individuals were not reminded of the incentive at the time of follow-up, they may not have been motivated by the incentive, simply because they had not received it[8] or had forgotten it.

Telephone. Telephone follow-up of a randomly selected subgroup of mail nonrespondents began approximately eight weeks after the first questionnaire mailing. Because time restrictions precluded any locating beyond using directory assistance, interviews were only completed with those mail nonrespondents who could be easily located.

One of the advantages of phone contact over mail is that we can more precisely differentiate nonrespondents who refuse participation from those who are not contacted. As shown in Table 2, the incentive group had a refusal rate that was less than half that of the no incentive group (7.6 percent as compared to 16.3 percent) – a difference that was statistically significant. The impact of the incentive on the refusal rate is consistent with work by Willimack, Schuman, and Lepkowski (1995, p. 82) that indicated that in their personal interview survey, a reduction in refusals was the primary method by which the response rate was increased through the use of incentives. However, Brennan (1992) and Brennan, Hoek, and Astridge (1991) did not find a similar impact of the incentive on refusal rate in their New Zealand studies.

As was true for the second mailing, phone interviews were completed with a slightly smaller percent of individuals in the incentive group than in the nonincentive group – 31.3 percent of the no incentive and 28.0 percent of the incentive group. The difference of 3.3 percentage points was not statistically significant (Table 3).

At the end of the phone stage, the cumulative cooperation rate was 80.8 percent for the incentive group, as compared to 74.4 percent for the nonincentive (Table 3). The difference continues to be statistically significant, but has been reduced from the 14 percentage points observed after the first mailing to six percentage points. The cumulative noncontact rates were essentially the same for the two groups (17.2 percent for the incentive group versus 19.5 percent for the nonincentive).

[8] Although individuals whose mail was returned as undeliverable were deleted from the calculation of the cooperation rates, there are undoubtedly others in the incentive group who never received the incentive. Packages may have been mis-delivered by the post office, household members may have forgotten to give (or forward) the package to the addressee, or the person may not have opened the package.

Table 2. Phone cooperation, refusal, and noncontact rates, by whether incentive received

Group	*n*	Cooperated		Refused		Not contacted	
		Percent	S.E.	Percent	S.E.	Percent	S.E.
No incentive	166	31.3	3.6	16.3	2.9	52.4	3.9
Incentive	118	28.0	4.1	7.6*	2.4	64.4*	4.4
Total	284	29.9	2.7	12.7	2.0	57.4	2.9

*Incentive and nonincentive groups significantly different at the .05 level.

Table 3. Cooperation, refusal, and noncontact rates at end of phone follow-up, by whether incentive received

Group	*n*	Cooperated		Refused		Not contacted	
		Percent	S.E.	Percent	S.E.	Percent	S.E.
No incentive	675	74.4	2.1	6.1	1.1	19.5	1.9
Incentive	674	80.8*	1.9	2.0*	0.7	17.2	1.8
Total	1,349	77.6	1.4	4.1	0.7	18.4	1.3

*Incentive and nonincentive groups significantly different at the .05 level.

Personal Visits. Members of the follow-up sample not contacted by telephone were sent for personal visit follow-ups approximately three months after the second questionnaire mailing. In contrast to the situation at phone follow-up, refusals were lower in the no incentive group than in the incentive group – 4.6 percent, as compared to 11.8 percent (Table 4). However, the difference is not statistically significant.

As was true at phone follow-up, the completion and contact rates were higher for the no incentive group. Interviews were completed with 63.2 percent of the no incentive and 47.4 percent of the incentive group, a difference statistically significant at the .05 level. The contact rates were 68 percent versus 59 percent – a nonstatistically significant difference.

Total Cooperation/Refusal and Noncontact Rates. As shown in Table 5, after two mailings, a reminder postcard, telephone and personal visit follow-up, the cooperation rate for those receiving incentives was 88.4 percent, as compared to 86.4 percent for the nonincentive group. Thus, only two percentage points separated the incentive and no incentive groups – a difference that is not statistically significant. Although the difference in final refusal rates between the incentive and nonincentive groups was not statistically significant, it is interesting to note that it continued to favor the incentive group (4.0 percent versus 6.9 percent).

Table 4. Personal visit cooperation, refusal, and noncontact rates, by whether incentive received

Group	*n*	Cooperated		Refused		Not contacted	
		Percent	S.E.	Percent	S.E.	Percent	S.E.
No incentive	87	63.2	5.2	4.6	2.2	32.2	5.0
Incentive	76	47.4*	5.7	11.8	3.7	40.8	5.6
Total	163	55.8	3.9	8.0	2.1	36.2	3.8

*Incentive and nonincentive groups significantly different at the .05 level.

Table 5. Final cooperation, refusal, and noncontact rates, by whether incentive was received

Group	*n*	Cooperated		Refused		Not contacted	
		Percent	S.E.	Percent	S.E.	Percent	S.E.
No incentive	675	86.4	1.6	6.9	1.2	6.7	1.2
Incentive	674	88.4	1.5	4.0	0.9	7.6	1.3
Total	1,349	87.6	1.1	5.4	0.8	7.0	0.8

*Incentive and nonincentive groups significantly different at the .05 level.

4. Nonresponse Bias

Nonresponse bias is a function of both the percent of individuals responding to a survey and the difference between respondents and nonrespondents on the variable(s) of interest. This can be expressed mathematically as:

$$\bar{y}_n = \bar{y}_r + (m/n)(\bar{y}_r - \bar{y}_m) \quad (1)$$

where $\bar{y}_n$ equals the population mean for the total population, $\bar{y}_r$ equals the mean for the respondent population (i.e., all individuals in the population who would have responded if included in the survey), $\bar{y}_m$ equals the population mean for the nonrespondent population, and (m/n) equals the proportion of the population who are nonrespondents.

According to this equation, nonresponse bias approaches zero, as either the difference between respondents and nonrespondents approaches zero or the nonresponse rate approaches zero. However, techniques that decrease one component while increasing the other may lead to an overall increase rather than a decrease in the total amount of bias. Consequently, if incentives disproportionately motivate individuals already highly predisposed to respond, total nonresponse bias could increase with the use of incentives. Conversely, if incentives differentially motivate those generally disinclined to respond, they could decrease nonresponse bias by reducing $(\bar{y}_r - \bar{y}_m)$ as well as by reducing (m/n).

Only a limited number of studies have investigated the question of the differential motivating effect of incentives and their results have been contradictory. Dillman (1996) reports that in a study of Washington State drivers, an incentive improved the response rate among younger individuals while having relatively little impact upon older drivers. Since the original response rates increased dramatically with age, using incentives significantly decreased the difference between respondents and nonrespondents. He found some evidence that the incentive might also decrease response bias associated with whether the individual lived in a metropolitan area, but found no support for a differential motivating effect of the incentive by sex.

Baumgartner and Rathbun (1996) found incentives had a greater motivational impact on those who thought the survey was of little salience to them than on those for whom the survey was highly salient. In the high salience group, a 4 USD incentive increased the response rate from 86% to 88% as compared to the no incentive group, while in the low salience group the 4 USD incentive led to an increase from 63% to 78%. This is especially important since topic salience (as distinct from demographic characteristics) is difficult to measure and therefore not subject to correction through statistical adjustments for nonresponse. For many surveys, the salience of the survey topic is strongly associated with variables the survey is designed to measure.

Willimack, Schuman, and Lepkowski (1995, pp. 86–88) found nonmonetary incentives appealed more to suburban than urban individuals, though the impact was not statistically significant. They did not find any statistically significant differences between the incentive and nonincentive groups in terms of age, education, income, race, or gender. Tambor, Chase, Faden, Geller, Hofman, and Holtzman (1993) found that increasing response rates to a mailed physician survey through use of incentives and follow-up techniques did not have a significant impact on their key dependent variables, but did improve representativeness in terms of socio-demographic characteristics and in terms of variables related to the type of practice.

Berry and Kanouse (1987, p. 109) found prepayment in a study of physicians did not appear to increase (or decrease) differences between respondents and nonrespondents. Similarly, Hopkins, Hopkins, and Schon (1988, p. 174) failed to find a difference in the appeal of incentives to public librarians as compared to school librarians. Nor did Perneger, Etter, and Rougemont (1993) find significant differences in the effect of incentives by age or sex; however, the use of a postpayment incentive may have diminished the impact of the incentive overall and the differential within group impact.

James and Bolstein (1990) found mildly significant differences in the educational and income level of respondents in their treatment groups (which varied in accordance with whether an incentive was given and the size of the incentive for the incentive groups); however, since they did not have prior information on the socio-demographic data, it is not possible to determine whether incentives were increasing or decreasing the nonresponse bias and/or whether the impact was on response bias rather than nonresponse.

Estimating the size of the science and engineering (S&E) population is a key statistic for the NSCG survey and one that posed serious problems for the survey series in the 1980s because the non-S&E population had much lower response rates than the S&E population. The authors believe that perceived lower salience among those with non-S&E occupations accounted for the high nonresponse bias in the 1980s surveys. While special care was taken to make the survey more salient to nonscientists and engineers in the 1992 pretest, it was not possible to completely eliminate factors likely to make the survey more salient to the S&E population.[9] Consequently, the NSCG pretest was specifically designed to examine whether the alternate methodologies under consideration would impact this nonresponse bias.

As expected, individuals identified as scientists and engineers in the CPS frame were more likely to respond to the mail survey than were nonscientists and engineers (Table 6).[10] Also, as expected, the bias was lower for the incentive group than the nonincentive group – 7.1 percentage points as compared to the 9.8 percentage point difference. However, this difference was not statistically significant.

[9] The survey title, cover letter, and questions were designed to make them appealing to the non-S&E population as well as the S&E population. However, sponsorship by the National Science Foundation was likely to make the survey seem more salient to the S&E population; further, NSF's informational needs required leaving some S&E bias in the instrument itself (e.g., more detailed education and occupation codes for the S&E versus non-S&E fields).

[10] Note that identification of scientists and engineers in the CPS frame is based solely on current (or for unemployed individuals, most recent) occupation – a variable that is known to be subject to considerable measurement error. Alternate definitions were unavailable in the CPS frame. Because of this measurement error, it is quite likely that the observed associations between science and engineering status and response are understated.

Table 6. Cooperation rates for scientists and engineers (S&E) compared to nonscientists and engineers (non-S&E) for incentive and nonincentive groups

Group	*n*	Mail		Phone		Final	
		Percent	S.E.	Percent	S.E.	Percent	S.E.
No incentive:							
S&E	149	67.6	3.1	77.4	3.4	86.3	2.8
Non-S&E	151	57.8	3.3	71.1	3.7	86.6	2.8
Difference		9.8*	4.5	6.3	5.0	−0.3	4.0
Incentive:							
S&E	148	76.9	2.8	83.6	3.0	90.8	2.4
Non-S&E	152	69.8	3.0	77.7	3.4	86.5	2.8
Difference		7.1*	4.2	5.9	4.5	4.3	3.6

*Difference between S&E and non-S&E groups statistically significant at .05 level.

At the end of the phone phase, the observed S&E bias was smaller than it was after the mail phase and was essentially the same for the incentive and nonincentive groups – 6.3 percentage points as compared to 5.9 percentage points.

By the time the personal visit phase was completed, response rate differences between the S&E group and the non-S&E group were small and not statistically significant for either the incentive or the nonincentive treatments. Thus, the incentive did not appear to be effective in reducing the nonresponse bias associated with topic saliency.

Additional analyses of the impact of incentives on subgroup response rates were done, using information about the cases available from the CPS on the pretest sample frame. These supplemental analyses are based on response rates to the survey at the end of the phone follow-up.

Of greatest interest in this regard, in light of Dillman's (1997) recent work, is the examination of the impact of incentives on nonresponse bias related to age. The response rate increased steadily with age for those in both the incentive and the non-incentive groups. Indeed, if anything, the incentive was less effective with the younger age groups than with the older ones. In the 28 or younger group, the incentive group had a response rate of 42 percent, as compared with the 41 percent response rate for the nonincentive group. As seen in Table 7, response rates are consistently higher in the incentive group than the nonincentive group, when controlling for sex, education, employment status, and science and engineering status. However, we found no evidence that small monetary incentives had different motivating effects within the subgroups examined in this article. However, it is possible that greater diversity would have been found if the initial target population had been more diverse, especially in terms of socio-economic status variables. It is also possible that we would have observed larger interaction effects if we had done these analyses at an earlier stage of the data collection.[11]

The demographic variable that comes closest to having an apparent interaction effect with the incentive offer is race/ethnicity. Response rates are higher for non-Hispanic Whites than for the minority groups for both the incentive and nonincentive groups.[12] If

[11] Unfortunately, the database design did not permit such tests.

[12] None of the comparisons among the minority race/ethnic groups were statistically significant; however, all were statistically different from the non-Hispanic White group.

Table 7. Response rates by incentive status and demographic categories after phone follow-up

Variable/Subgroup	With incentives		Without incentives	
	N	Response rate	*N*	Response rate
Age				
28 or under	148	41.9%	147	40.8%
29–35	189	60.8	196	59.2
36–45	229	71.2	240	60.4
46–55	129	77.5	110	65.5
56+	115	78.3	116	70.7
Sex				
Men	541	66.5	533	59.7
Women	269	63.2	276	56.9
Race/ethnicity				
Asian	52	50.0	48	39.6
Black	35	40.0	32	53.1
Hispanic White	23	60.9	26	26.9
Non-Hispanic White	700	68.0	703	61.5
Education				
16–17 years	529	62.6	513	55.4
18 or more years	281	70.8	296	64.5
Employment status				
Not in Labour Force	93	68.8	96	63.5
Unemployed	11	54.5	13	38.5
Working	706	65.2	700	58.4
Science and engineering status				
Not S&E	404	64.1	404	56.2
S&E	406	66.7	405	61.2

one pools the minority groups in Table 7, one obtains a response rate of 49 percent for minorities as compared with 68 percent for the non-Hispanic White group with incentives. For the nonincentive groups, the comparable response rates were 41 percent and 61 percent. Thus, the difference between the minorities and the non-Hispanic Whites is essentially the same in both the incentive and nonincentive groups. If there is an interaction effect between race/ethnicity and incentives, it is presumably due to a differential appeal of incentives within the minority groups. However, the sample sizes of these groups makes it difficult to interpret the pattern of responses for them.

As shown in Table 8, using Analysis of Variance (incorporating main effects for all of the independent variables plus all two-way interactions involving incentives), age, race/ethnicity, and whether an incentive was offered had statistically significant effects on response rates. Education level and science and engineering status were not statistically significant, but were close to being so ($P = .07$). None of the interaction effects between incentives and the remaining independent variables were statistically significant. The closest to being statistically significant was race/ethnicity ($P = .11$).

In sum, this analysis does not support Dillman's (1997) finding that incentives are especially valuable in motivating young sample members, nor Baumgartner and Rathbun's

Table 8. Impact of incentives and demographic variables on response rates at the end of the phone follow-up

Variable	Sum of Squares	*df*	Mean Square	*F* Value	Probability
Main effects					
Incentive	0.8736	1	0.8736	3.99	0.0461*
Age	15.6369	4	3.9092	17.84	0.0001*
Sex	0.0494	1	0.0494	0.23	0.6350
Race/ethnicity	6.1571	3	2.0524	9.36	0.0001*
Education	0.7407	1	0.7407	3.38	0.0662
Employment status	0.3548	2	0.1774	0.81	0.4454
S&E status	0.7226	1	0.7226	3.30	0.0696
Interaction effects					
Incentive and:					
Age	0.7204	4	0.1801	0.82	0.5112
Sex	0.0011	1	0.0011	0.01	0.9434
Race/ethnicity	1.3111	3	0.4370	1.99	0.1130
Education	0.0087	1	0.0087	0.04	0.8421
Employment status	0.3829	2	0.1915	0.87	0.4177
S&E status	0.1094	1	0.1094	0.50	0.4801

*Statistically significant at .05 level.

(1996) finding that incentives are especially valuable in motivating individuals for whom the survey has low saliency. Further, none of the other demographic variables were associated with the strength of the motivating effect of incentives. It is, therefore, reasonable to assume that increasing the response rates through the use of incentives will lead to a decrease in nonresponse bias. This finding should reassure those concerned that the use of incentives might differentially motivate those already inclined to respond, thereby increasing nonresponse bias.

5. Data Quality

Kerachsky and Maller (1981, p. 258) indicate that the effect of incentives on data quality is not known. On the one hand, it can be hypothesized that incentives will increase the care that respondents take in completing the survey. On the other hand, it can be argued that if the sole reason for responding is the incentive, the respondent may have little motivation for completing the survey instrument carefully. In their survey of disadvantaged youths they determined that data quality, measured by item nonresponse, was either the same for the incentive group as for the nonincentive group, or better.

More recently, Willimack, Schuman, and Lepkowski (1995, p. 88) found that incentive recipients tended to give more complete answers to open-ended questions at the beginning of the interview than did individuals not receiving an incentive. James and Bolstein (1990, p. 358) claim, based on their own research and that of others, that ''Factual information, and opinions about survey topics do not generally seem to be affected by incentives.'' They also found that ''Larger monetary incentives tended to produce . . . a greater degree of effort expended in completing the questionnaires, as measured by the number of short answers and comments provided, and the number of words written . . .'' (p. 346).

Table 9 shows the five indices used to measure how carefully NSCG respondents completed the mail or phone survey instrument. For four of the measures, differences

Table 9. Impact of incentives on indices of care taken in completing the survey

	Error rate (%)		Difference	*t*
	Without incentives	With incentives		
Percent of questions answered that should have been skipped	3.9	3.6	0.3	0.51
Percent of multiple response yes/no questions no answered	15.2	16.8	−1.6	−1.00
Percent of ''other specify'' responses omitted	0.5	2.5	−2.0	−1.84
Percent of contact information not provided	31.2	25.6	5.6	2.62*
Percent of other questions not answered	13.7	12.9	0.8	1.23
Total error rate	16.2	14.9	1.3	1.64

*Statistically significant at .05 level.
NOTE: Measures of data quality are for cases completed by mail or phone.

between those receiving incentives and those not receiving incentives were trivial and not statistically significant. These measures were the percent of: (1) items not skipped that should have been skipped according to the instructions; (2) yes/no options requested for multiple response questions that were not provided;[13] (3) requested ''other specify'' responses that were omitted; and (4) other unanswered content questions. The fifth indicator, the percent of information requested to assist the survey conductors in contacting respondents for future surveys, however, was statistically significant. Incentive group members failed to complete 26 percent of these items, as compared with 31 percent not completed by members of the nonincentive group. What is especially interesting about this index is that it implies that individuals in the incentive group were more willing to be re-interviewed than were individuals in the nonincentive group. While this does not necessarily indicate better quality data in the initial baseline survey, provision of additional contact information is likely to contribute to higher response rates, lower nonresponse bias, and lower data collection costs in future survey waves of a longitudinal survey.

Another aspect of data quality raised in the literature is 'favorability' (James and Bolstein, 1990 and 1992). According to this theoretical perspective, incentives may lead respondents to view the sponsor in a favorable light, resulting in responses biased towards the survey sponsor's perceived desires. This particular experiment did not provide a direct test of how favorably the respondent perceived the U.S. Census Bureau or the National Science Foundation. However, it was possible to determine whether statistically significant differences existed in how individuals responded to questions between the incentive and nonincentive groups. Analysis of the responses to 148 questions found only 8 questions (5.4 percent) with a statistically significant difference between the responses of the incentive and nonincentive groups at the .05 significance level. This rate is consistent

[13] For example, respondents who had changed employers or occupations in the preceding five years were asked to indicate whether each of a series of possible reasons had led to the change. Many respondents only provided ''yes'' responses, presumably implying that the remaining responses were ''no''.

Table 10. Estimated per case variable cost of incentive versus nonincentive approach

	Nonincentive				
	Percent of cases subject to procedure	Cost/case subject to procedure, USD	Cost/case in original sample, USD	Cumulative cost per case, USD	Cumulative cooperation rate, percent
First Mailing	100.0	3.12	3.12	3.12	53.9
Second Mailing	54.5	1.74	0.95	4.07	62.7
Phone Follow-Up	38.1	18.31	6.97	11.04	74.3
Personal Visit	19.9	72.10	14.31	25.35	86.4

with the number of statistically significant differences expected by chance (5 percent $\pm$ 3.6 percent). Thus, it seems unlikely that the incentive had a major impact on how individuals responded to the content questions on the survey.

In sum, evidence from this study does not support the contention that using an incentive increases response bias among college-educated individuals. The only noted effect of incentives on the responses was a greater willingness on the part of the incentive group to provide information helpful in tracking individuals for future waves of the survey – an effect that is of benefit to those doing longitudinal surveys.

6. Costs

Calculating the cost-effectiveness of a data collection method is not as straightforward as it may seem. One simple method is the cost per completed case (e.g., Bolstein and James 1992). As noted by Hopkins, Hopkins, and Schon (1988, pp. 174–175) and Bolstein and James (1992), however, the question is not simply which is less costly, but whether the extra cost is justified by the extra quality. Evaluating this trade-off is subjective. An alternate methodology that comes close to the approach taken in federal agencies that need to respond to survey guidelines enforced by the U.S. Office of Management and Budget is to compare the costs of incentive and nonincentive survey designs that produce similar response rates. Few researchers have taken this approach to evaluating cost-effectiveness.

James and Bolstein (1990) present a view of cost-effectiveness based on two alternate methodologies that both resulted in the same response rate of 88 percent. The two-wave/1.00 USD incentive group had costs per return that were somewhat higher than the four-wave/no incentive group (3.97 USD versus 3.65 USD). This argues that follow-up mailings are more cost-effective than incentives in an all-mail survey. However, as they note, there is also a ''timeliness'' cost associated with the additional mailings that may, in many cases, justify the additional expense.

Several other studies argue that increased costs for incentives are justified by the increase in quality (Hopkins, Hopkins, and Schon 1988, Brennan, Hoek, and Astridge 1991, Brennan 1992, and Willimack, Schuman, and Lepkowski 1995).

While precise cost information for incentive versus nonincentive options was not available for this study, approximate variable costs for data collection efforts were estimated (Table 10). Restricting the analysis to cases with presumed valid addresses, the incentive itself costs approximately 3.83 USD per case selected (adjusting the 5.00 USD payment

Incentive				
Percent of cases subject to procedure	Cost/case subject to procedure, USD	Cost/case in original sample, USD	Cumulative cost per case, USD	Cumulative cooperation rate, percent
100.0	6.95	6.95	6.95	67.8
40.2	1.74	0.70	7.65	73.3
27.0	18.31	4.95	12.59	80.6
17.2	72.10	12.41	25.00	88.4

for those failing to cash cheques[14] and for processing costs). On the other hand, introducing incentives saved approximately 4.18 USD per selected case in follow-up costs, providing a net cost of approximately 35 cents per selected case for the incentive group. This difference is negligible, given the approximate per selected case cost of 25 USD and the impression associated with the cost estimates.

Suppose, however, that the cooperation rate goal had not been in the mid-80 range. If the goal had been an 80 percent cooperation rate, personal visit follow-ups could have been avoided for the survey by using incentives. The per case variable cost for incentives under this scenario would have been approximately 12.59 USD. Assuming half of the nonincentive cases were sent to the personal visit phase to achieve the 80 percent cooperation rate, the cost for the nonincentive process would have been 18.19 USD. Further, elimination of the personal visit phase would result in the elimination of additional fixed costs (e.g., development of training materials for field interviewers) that are not included in these cost estimates.

If the goal had been a cooperation rate in the mid-70 range, the advantage of the incentive treatment would also have been clear. The incentive treatment attained a 73 percent cooperation rate after two mailings at a per selected case cost of 7.65 USD, as compared with a 74 percent cooperation rate at the end of the phone phase at a per case cost of 11.04 USD. Again, the savings would be even greater if the fixed costs associated with the phone follow-up were included in the calculations. Note that this particular scenario has another major non-cost benefit in that the incentive group would not be subject to the potential response bias problems that can arise in a mixed-mode survey.

If a cooperation rate in the low 60s had been acceptable to the survey sponsors, the nonincentive procedures would have been more cost-effective, since a two-wave mailing methodology would yield a 63 percent cooperation rate for a cost of 4.07 USD per selected case, as compared with the 6.95 USD cost of a one-wave incentive treatment resulting in a 68 percent cooperation rate. However, the one-wave mailing would result in faster data availability and would have eliminated some costs related to the more complex two-wave design that are not adequately reflected in these cost estimates.

In sum, the cost-effectiveness of incentives versus follow-up is likely to depend on the desired cooperation rate. If low rates are acceptable or very high rates are desired, incentives may not be cost-effective. However, if the survey sponsor requires cooperation

[14] 72 percent of those presumed to have received the original letter cashed their incentive cheques. Processing costs were assumed to be 25 cents per case.

rates that cannot be achieved by mail techniques without an incentive, it may be possible to make substantial savings by using an incentive rather than additional phone or personal visit follow-up. Even if the personal visit follow-up cannot be avoided due to the need for a very high cooperation rate, the incentive does not necessarily add to the cost of the survey, due to the reduction in cases needing expensive phone or personal visit follow-up.

7. Negative Reactions to Incentives

Few government-sponsored studies have used incentives. As noted earlier, Groves (1989) and others claim that there is a belief among some that responding to government surveys is part of one's ''civic duty.'' This pilot survey did not, however, encounter hostile reactions or negative feedback to the use of incentives. None of the sample members who called the number provided on the questionnaire cover letters or reminder letter complained about the incentive; none of the questionnaires were returned with angry notes; and none of the sample members contacted by telephone or personal visits raised this issue. Furthermore, the final refusal rates for the incentive and nonincentive groups actually favored the incentive group, although the difference was not statistically significant.

8. Summary

Many studies have demonstrated that small monetary incentives motivate survey response. However, many federal survey sponsors in the U.S. and survey practitioners hesitate to use incentives out of concern that the incentives may increase nonresponse bias, data quality, respondent ill will and/or survey costs. Although the small number of studies examining these possible negative impacts of incentives do not demonstrate that these concerns are warranted, there have not been a sufficient number of such studies for the results to be viewed as conclusive. The 1992 pretest for the National Science Foundation's (NSF) 1993 National Survey of College Graduates (NSCG) was accordingly designed to test the impact of a prepaid monetary incentive on cooperation rates, nonresponse bias, data quality, and survey costs. The evidence presented suggests that, at least for this population and this survey content, incentives provide a decided cost advantage, assuming moderately high response rates are desired, without offsetting negative effects on survey quality.

This pretest has confirmed what Sudman and Bradburn (1982) and others have suggested: a well-designed mail survey with a burdensome questionnaire can achieve reasonable cooperation rates in a highly educated population – without an incentive. However, using incentives can significantly increase respondent cooperation and reduce nonresponse bias in mail surveys with a fairly burdensome questionnaire and limited follow-ups.

The pretest for the National Survey of College Graduates also confirmed the results of previous research by James and Bolstein (1990), Nederhof (1983), and Heberlein and Baumgartner (1978), indicating that an incentive's power to motivate participation diminishes with successive follow-ups. This decline is consistent with the theoretical assumption articulated at the beginning of the article, that the impact of incentives can be attributed to several factors, likely to vary in their ability to motivate specific individuals – or even the same individual over time. These motivating factors include potential social and economic advantages perceived to accrue from the incentive and freedom from guilt associated with violating the societal norms of reciprocity and helping.

It is also important to note that in this experiment, as well as in the other studies examined, the incentive was only used in the first mailing. Because some of the first mailing packages may not have been received and opened by the addressee and because some incentive recipients may have forgotten receiving the incentive in this particular study, this approach does not fully test the potential usefulness of incentives.

The study did not demonstrate that incentives differentially motivate respondents on the basis of age, sex, race/ethnicity, educational level, employment status, or science and engineering status. Thus, although the incentive did not reduce the difference between respondents and nonrespondents on these variables, it also did not increase the difference between the two groups – a source of concern to those hesitant to use incentives. From a theoretical perspective, this result is somewhat surprising. Cultural groups typically differ on the nature and strength of specific norms and values and it is, therefore, reasonable to expect them to react somewhat differently to incentives. Of course, it is possible that this finding is attributable to the relative homogeneity of a college-educated population, the moderate sample size or our inability to examine respondent differences after the first mail wave when the cooperation rates for the two groups were most different.

The study also failed to confirm the concerns of some investigators that incentives might lead to careless responses and high response error rates. For most of the indices examined, there was little difference between the incentive and nonincentive groups. However, individuals who received an incentive were more cooperative in providing information needed to track their whereabouts for a successive wave of the survey – an important consideration in longitudinal surveys.

The study also failed to reveal serious concerns about the use of incentives in a government survey on the part of the sample members. Especially important in this regard is the fact that the cumulative refusal rate for the incentive group was lower than for the nonincentive group.

One major advantage of incentives over aggressive follow-up emerged from this analysis. Incentives are likely to be highly cost-effective, if one requires a high but not extraordinarily high cooperation rate. In this particular study, incentives would have been cost neutral when a cooperation rate in the mid 80 percent range was set as a goal, but would have been highly cost-effective if a cooperation rate between 70 and 80 percent were the goal. If cooperation rates in the 60s were acceptable, a mail-only procedure without an incentive would have been more cost-effective than using the incentive.

In sum, on the basis of the information in this study and the literature reviewed, the authors believe that incentives **can** provide a cost-effective survey tool for use in government surveys when moderately high response rates are needed. This study and the other studies reviewed do not indicate that the potential savings of incentives are paid for through increased nonresponse bias, decreased data quality, or respondent ill will.

9. Future Directions

This study points to three major directions for future research on incentives:

- Studies should be explicitly designed to further our understanding of how incentives motivate respondent behavior.

- Additional empirical work is needed on the impacts of incentives on factors contributing to total survey error and to cost-effectiveness.
- Experiments should be done in which the sample members are reminded of the incentive and/or are re-offered the incentive during survey follow-up.

In addition, future research should be designed to explore what Jerry Coffey (1996) has pointed out should be major concerns of surveys with low response rates – the impact of nonresponse on estimates of sampling errors and correlations between variables. His concern is based, in large part, on studies (such as that by Choi, Ditton, and Matlock 1992; Dolsen and Machlis 1991) in the leisure field that have not generally come to the attention of government statisticians and on work by Goudy (1978) on the impact of response rate on the size of correlation coefficients.

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