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ESTIMATING THE EFFECT OF INCENTIVES ON MAIL SURVEY RESPONSE RATES: A META-ANALYSIS

ALLAN H. CHURCH

Abstract This article reports the results of a meta-analysis of 38 experimental and quasi-experimental studies that implemented some form of mail survey incentive in order to increase response rates. A total of 74 observations or cases were classified into one of four types of incentive groups: those using prepaid monetary or nonmonetary rewards included with the initial survey mailing and those using monetary or nonmonetary rewards as conditional upon the return of the survey. Results were generated using an analysis of variance approach. The overall effect size across the 74 observations was reported as low to moderate at $d = .241$. When compared across incentive types, only those surveys that included rewards (both monetary and nonmonetary) in the initial mailing yielded statistically significant estimates of effect size ($d = .347$, $d = .136$). The average increase in response rates over control conditions for these types of incentives was 19.1 percent and 7.9 percent, respectively. There was no evidence of any impact for those incentive types offering rewards contingent upon the return of the survey.

Introduction

Data collection in the form of mailed questionnaires, long accepted as the standard method for large sample surveys, has been implemented across such diverse fields as marketing, advertising, business, and the political and social sciences (Aiken 1988; Alwin and Campbell 1987; Dillman 1978; Greenberg and Manfield 1957; Groves 1987; Peterson

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1975; Shosteck and Fairweather 1979). While citing the mailed questionnaire's high degree of utility, however, many survey practitioners have been plagued by response rate problems (e.g., Eisinger et al. 1974). One popular method for increasing response rates that has received significant attention in the literature is the use of incentives.

Researchers and practitioners often implement some kind of a reward, compensation, or token value to increase the respondent's motivation to complete the survey (e.g., Armstrong and Overton 1971; Bevis 1948; Dohrenwend 1970; Gelb 1975; Gunn and Rhodes 1981; Lockhart 1984; Sudman and Ferber 1974; Wolfe and Treiman 1979). Variations on the types of rewards implemented across studies in the literature have been those of form (monetary and nonmonetary) and timing (sent initially with the questionnaire or contingent on the returned response). Although often referred to as a single factor in mail survey methodology, incentives can and should be classified into four distinct types for effect analyses, based on the crossed results of the two dimensions of form and timing. Thus the four groupings or types would consist of monetary and nonmonetary incentives mailed with the survey and monetary and nonmonetary incentives given on the return of the questionnaire (henceforth to be referred to as incentive or study types MI, OI, MR, and OR, respectively).

Despite the frequent usage of these kinds of rewards or incentives for increasing response rates in mail survey work, there has been little consistency among the specific effect sizes reported in the literature. This disparity of results, which is due in large part to the lack of differentiation among these four incentive types and their relative effects on response rates, makes it difficult for practitioners when planning their research.

Thus, the purpose of this article is to fill a need in the literature by providing an applied meta-analysis for the area of monetary and nonmonetary incentives. The results of this research will yield several estimates of the specific effects on increasing mail survey return rates for each type of incentive approach. Although other authors have provided similar kinds of incentive-related effect size summaries, ranging from purely qualitative (e.g., Kanuk and Berenson 1975; Linsky 1975) to more sophisticated quantitative approaches (e.g., Armstrong and Lusk 1987; Eichner and Habermehl 1981; Fox, Crask, and Kim 1988; Goyder 1982; Heberlein and Baumgartner 1978; Yu and Cooper 1983), none of these researchers has detailed the relative differential effects of response rates for each of the four incentive types. What also separates my study from those reported in the past is the larger number of observations, and therefore power, in the determination of the meta-effects of these four approaches to the use of incentives in mail surveys.

The initial hypotheses concerning the following meta-analysis of the effect of incentives on mail survey response rates were as follows:

H1. The overall effect for all incentives tested will yield significant differences from nonincentive controls or comparison groups.
H2. All four incentive types (MI, MR, OI, OR) will yield significant and/or meaningful increases in overall response rates relative to respective controls or comparison groups.
H3. Monetary incentives (MI and MR) will yield greater overall increases (effects) than nonmonetary incentives (OI and OR).
H4. Prepaid monetary incentives (MI) will yield the greatest effect over controls or comparison groups.
H5. These results will generalize across different populations and years in which studies were conducted.

Method

BACKGROUND

The meta-analysis framework outlined by Hunter, Schmidt, and Jackson (1982) was implemented for hypothesis testing and literature synthesis. Meta-analysis techniques, although differing in their specifics, often yield important findings and allow for greater generalizability and application of the results to future research (Armstrong and Lusk 1987; Fox, Crask, and Kim 1988; Guzzo, Jette, and Katzell 1985; Houston and Ford 1976). Other methods of quantitative review, such as multiple regression analysis (Armstrong 1975; Goyder 1982; Heberlein and Baumgartner 1978) and cumulative chi-squares (Yu and Cooper 1983) have also been applied to divergent mail survey data sets with varying degrees of success.

PROCEDURE

Following Hunter, Schmidt, and Jackson's (1982) meta-analysis framework, the literature search consisted of locating all published studies concerning the use of incentives in mail surveys. Several seed sources (*Psychological Abstracts, Sociological Abstracts, Public Opinion Quarterly,* and the *Journal of Marketing Research*) were referenced manually, using abstracts and indexes to establish the initial set of studies to be included in the analysis. References from each study located were then used to determine further sources, following an iterative process until all potential sources of data were exhausted.

Although the potential error bias involved in missing the results of nonpublished data was recognized, it was not considered critical due to the wide range of effect sizes, nonsignificant results, and multiple factors and methodologies employed across the final set of studies collected. As Fox, Crask, and Kim (1988) have noted, many of these types of incentive-related studies report greater percentages of nonsignificant results relative to other types of academic research. Thus, the impact of this type of bias, although still unknown, was probably small to minimal on the findings presented below.

The initial criterion for inclusion in this meta-analysis stipulated that the study report at least one response rate in conjunction with monetary or nonmonetary incentives in a mailed survey. Monetary surveys consisted of those using cash or checks, while nonmonetary incentives were defined as those studies that used any extra item as an incentive above and beyond the normal procedure for most mail surveys. Those studies in which only the results of the survey were offered to the subject as an incentive to participate were not included in the analysis, since this is often considered good research practice and would therefore introduce an uncontrolled factor (Levine and Gordon 1958).

The final criterion for inclusion in the meta-analysis was the presence of a control or comparison group against which the incentive condition could be indexed. Although many studies reporting on the effects of incentives were collected and entered into the data base, only those with experimental control or quasi-experimental comparison groups were included in the final analysis for determining effect size estimates and variance proportions.

Once collected, all studies were coded for analysis using a method derived from prior research and theory (Armstrong and Lusk 1987; Eichner and Habermehl 1981; Fox, Crask, and Kim 1988; Heberlein and Baumgartner 1978; Hunter, Schmidt, and Jackson 1982; Yu and Cooper 1983). The result of this coding process yielded a unit of analysis consisting of the record or observation made from each reported pair of response rates. Because all observations included in the final data set were compared on a treatment versus control/comparison basis, the relative impact of other pertinent variables known to affect survey returns was minimized (Armstrong and Lusk 1987; Fox, Crask, and Kim 1988). Thus, the effects of survey length, content, return postage type, color, number of items, and so on, were theoretically controlled in the comparison score between control and incentive responses, since both conditions presumably had the same physical qualities and mailing procedures.

Information on these variables was still collected where possible, however, in order to test the degree to which these elements would serve as incentive effects moderators. Therefore, the categorical or character (i.e., nonnumeric) variables of author(s), journal or source of information, basic sample description, brief description of the manifest content of the survey, identified survey sponsor, type of nonmonetary incentive, and timing (initial or on return) of the incentive were included in each observation in the data base. Numerical variables consisted of year published, number of survey pages, total number of questionnaires sent and number returned, level of monetary incentive, control sample size, control group response rate, incentive group sample size, and incentive group response rate.

Although efforts were made to include other potentially useful variables (e.g., number of items in the questionnaire, sampling method, saliency of the survey topic), many of the authors were too scant in their methodological descriptions to determine these details with any accuracy. On analysis, these variables would have had values for only 30–40 percent of the total number of observations, rendering them useless for overall analyses. Similarly, two of the variables initially coded and entered (manifest content of the survey and the identified survey sponsor) were ultimately excluded from analysis as well, due to missing data. The variable representing the number of questionnaire pages was also dropped from the overall analyses with a missing data rate of 49 percent. This variable, however, was included in some simple correlation matrices to look for possible relationships between survey length and other factors in the data base.

Techniques described by Hunter, Schmidt, and Jackson (1982) and Hunter and Schmidt (1990) were used to compute the effect sizes and associated variance and error measures. Weighted means by sample size, unweighted means, and medians for several of the variables were also computed. An analysis of variance (ANOVA) approach (Cliff 1987; SAS Institute 1985; Tabachnick and Fidell 1989) was used for determining the overall significance of effects and for testing the different incentive type means.

DEPENDENT VARIABLES

Several different but related dependent measures were initially generated from the pair of response rates (control and incentive) entered into the data base for comparison purposes with previous articles and reviews. Each of these dependent variables was then examined for the extent to which it contributed any new information about the effects in question. Most of the variables did not. Thus, values for the relative incremental cost per individual response achieved (Berry and Kanouse 1987; Cox 1976; Hackler and Bourgette 1973; Kephart and Bressler 1958; Robinson and Agisim 1951; Zusman and Duby 1987), the percent decrease (*pdn*) in nonresponse rate (Armstrong 1975; Linsky 1975), the number of percentage points (*pntir*) increase in response rates (Kanuk and Berenson 1975; Zusman and Duby 1987), and the percent increase (*pir*) in response rate were computed for each observation but ultimately were dropped from further analyses.

The dependent variable indexing change actually used for analysis was the formula effect size (*d*) (Hunter, Schmidt, and Jackson 1982). Effect size, defined as the "difference between means in standard score form, i.e., the ratio of the difference between means to the standard deviation" (Hunter, Schmidt, and Jackson 1982, p. 97), was used because it is a standard measure of the impact of an experimental manipulation against which comparisons can be made across other types of research (Cohen 1977; Hedges and Olkin 1985). Thus, the effect size for a given study or series of studies answers the question, "How *large* was the treatment effect?" (Hunter and Schmidt 1990, p. 336), not just whether or not the observed effect was significantly different from zero. Independent variables included in the analysis consisted of study type (MI, MR, OI, OR), year of publication, sample type, and publication source of results.

Results

DATA BASE DEMOGRAPHICS

The literature search yielded a total of 38 studies reporting on the effects of different incentives. From this set of studies, 74 individual observations (records) had data for both incentive and comparison groups enabling the computation of the dependent measure *d*. Thus, 74 observations were coded and entered into the data base for subsequent analysis, representing information from a total of 38 different published sources (see the Appendix for a listing of studies included in the meta-analysis). All reported results are based on effects from these 74 records. The mean and median sample sizes used across observations were 664 and 329 survey respondents, respectively.

There was a wide variety of target populations for survey respondents across the 74 observations, ranging from urban household residents to out-of-state drivers. In order to test the differential effects of incentive type on these target populations, observations were classified into five groups based on the descriptions of the respondents. These groups consisted of general population (55 percent), students (8 percent), technical people (10 percent), business people—administrators and executives (16 percent), and medical personnel (11 percent).

The breakdown of data by study type was as follows: 43 records were taken from studies in which monetary incentives were mailed with the questionnaire (MI), 9 records were from studies using monetary incentives as contingent on the returned survey (MR), 12 records were from studies that implemented other nonmonetary incentives sent initially with the questionnaire (OI), and the final 10 observations were taken from research that used other incentives and required a returned response form (OR).

Listings of the data showed levels of monetary incentives offered by researchers ranging from \$.01 to \$5.00. When standardized and adjusted to 1989 dollars using the Consumer Price Index (CPI; 1974, 1989), the cash incentives showed considerable variation, ranging between \$.036 and \$9.29, with a median of \$.86 and a mean of \$1.38. The nonmonetary incentives were more interesting in their diversity, however, with such items as entry in a lottery, donations to charity, coffee, books, pens, key rings, golf balls, tie clips, stamps, and even a turkey (e.g., Knox 1951).

Observations used for analysis had been conducted across a time span of over 50 years, dispersed between time periods as follows: 13 percent before 1960, 19 percent between 1961 and 1970, 34 percent between 1971 and 1980, and 34 percent after 1981. Although no author or set of authors dominated in contributions to the data set (the most being 8 percent for any single researcher), over half the observations did originate from two of the seed journals: the *Journal of Marketing Research* (26 percent) and *Public Opinion Quarterly* (31 percent). The remainder of the records were well distributed among 14 other sources.

ESTIMATES OF EFFECT SIZE

Overall, an overwhelming majority of the total 74 observations (89 percent) yielded some improvement in response rates relative to the control or comparison group. Interestingly but not surprisingly, while only 1 percent provided absolutely no evidence of an effect, 10 percent of the incentive conditions actually yielded decreases in their survey returns. Table 1 shows the unweighted and weighted means and the maximum, minimum, and median values for the incentive and control response conditions as well as for the dependent measure of effect size (d) used in the subsequent analysis. The weighted formula effect size for all 74 observations was $d = .241$. This represents an overall average increase in response rate of 13.2 percentage points between the incentive and control conditions. The median effect size was slightly lower than the weighted mean effect at $d = .231$. The formula variance was computed to be $s^2 = .035$, with a sampling error of .0061. This yielded an estimate of the true standard deviation of the effect size at $s = .170$ (formulas for these computations were taken from Hunter, Schmidt, and Jackson [1982]). If the underlying effect was basically the same across all studies, this estimate of dispersion should have

Table 1. Descriptive Statistics for Meta-Analysis: All Valid Observations

Variable	Unweighted Mean	Weighted Mean	Median	Minimum	Maximum
Response incentive (%)	48.9	44.6	50.6	12.1	86.5
Response comparison (%)	35.8	33.0	34.5	13.0	64.0
Effect size (*d*)	.244	.241	.231	−.239	.643

NOTE.—$N = 74$.

been close to zero, or very small relative to the mean effect size. Since $d = .241$ is greater than $s = .17$ by a factor of less than two, these figures provided evidence for differential effects across incentive types, moderator variables, or large, unaccounted-for-error components.

A test for the homogeneity of effect sizes (Hedger and Olkin 1985; Hunter and Schmidt 1990) confirmed this finding, indicating that the 74 observations were probably not representing a common phenomenon and therefore should not be pooled into one overall estimate for analysis (i.e., some moderating variable or variables existed that were accounting for differential effects among various groups of observations). As Hedges and Olkin have also noted, however, when sample sizes are very large (ranging among these 74 observations from 20 to 5,000) and the *d* values do not vary greatly, "it is worth studying the variation in the values of *d*, since rather small differences may lead to large values of the test [homogeneity] statistic . . . [and] the investigator may elect to pool the estimates" (1985, p. 123). Thus, these data were subjected to further analyses to look for main effects, interactions, and moderating variables and their respective impact on response rates.

Simple correlations between the dependent measure (*d*) and the continuous independent variables of number of pages in survey, year published, and adjusted incentive value yielded no significant results at all. When examined by incentive type, however, the MI group of observations yielded significant correlations between the CPI adjusted incentive value and the dependent measure effect size ($r = .45$; $t = 3.23, p < .01, df = 1,41$). Again, there were no significant relationships between changes in response rate and either survey length or year of publication among these separate correlations computed by incentive type.

An analysis of variance (ANOVA) was conducted in order to (1) test the presence of any overall and/or interaction effects while including all possible independent variables for analysis among the 74 observations collected and (2) to best control for inflated Type I error rates (Cliff 1987; SAS Institute 1985; Tabachnick and Fidell 1989). The dependent variable used for this analysis was the formula effect side (*d*).

The independent variables included in the analysis consisted of study type (MI, MR, OI, OR), a coded version of year of publication, the type of respondents used in the mail survey, and the journal source from which the study was drawn (all of these variables were based on the distributions described above). The associated two-way interaction effects between these variables were also included for exploratory purposes. Incentive value and survey length were not included in these analyses because the data did not exist across all observations. Other than for those specific studies using monetary rewards (types MI and MR), very few researchers included the actual cash value for the incentive used. Survey length was simply a missing variable in many cases.

While the overall ANOVA yielded a significant $F(32,41) = 3.61$, $p < .001$, $R^2 = .737$, only the variable representing study type (MI, MR, OI, OR) resulted in a significant univariate main effect, with $F(3,41) = 28.11$, $p < .001$. Effect sizes were not significantly different across various types of respondent populations, journal source, or year of publication (as the simple correlational analysis suggested). Likewise, the test for the interactions among the independent variables were also nonsignificant. Thus, only study type was selected for further exploration of differences.

Investigating the data by study type showed differential outcomes for each incentive group. Effect sizes for the four types were computed at $d = .347$, $d = .085$, $d = .136$, and $d = .020$, for the incentive groups MI, MR, OI, and OR, respectively. Table 2 contains the associated computational elements and variance estimates, as well as the weighted mean incentive and control response rates for each of the four incentive conditions. These effect sizes represent comparable average increases in incentive versus control response rates of 19.1, 4.5, 7.9, and 1.2 percentage points for the four respective types of rewards.

Interestingly, further analysis also revealed that the least-squared effect size means (or marginal means, as they are often called; SAS Institute [1985]) calculated for incentive types MR and OR, both offering rewards on return, were not significantly different from zero ($t = 1.25$ and $t = 0.56$, respectively). Thus, these two types of incentives

Table 2. Effect Size, Associated Variance Estimates, and Weighted Mean Response Rates for Four Types of Incentives

	Monetary		Nonmonetary	
	Initial (MI)	Return (MR)	Initial (OI)	Return (OR)
Effect size (*d*)	.347**	.085	.136**	.020
Observed variance	.018	.016	.010	.014
Sampling error	.006	.015	.006	.004
Estimate of true standard deviation	.112	.032	.057	.100
Response incentive (%)	53.0	41.1	36.8	30.1
Response comparison (%)	34.2	29.1	28.9	33.1
N	(43)	(9)	(12)	(10)

** $p < .001$.

had no statistically significant effect or substantive impact on response rates. Next, post hoc comparisons were conducted using a Bonferroni approach, whereby simple *t*-tests were controlled for inflated error rates by adjusting the *p* values for acceptance by the number of comparisons being made (Howell 1982). These *t*-test comparisons of the four incentive type means showed that the computed effect for the MI group of observations (d = .347) was significantly greater than for all three other types. The effect size (d = .136) for the nonmonetary initial mailing studies (OI) was significantly greater than the effect (d = .020) for the nonmonetary incentives (OR) provided on return as well, but not for the mean effect (d = .085) of the monetary on return (MR) studies. Details of these comparisons can be found in table 3.

Discussion

Clearly, the findings of the present meta-analysis have demonstrated that incentives do indeed have substantial positive effects on mail survey return rates. The results of the analysis of variance indicated a significant overall effect for the use of any incentive in increasing mail survey responses, thus supporting H1.

An examination of the appropriate means and variances, however, suggested that H1 is not particularly meaningful given the degree to which effect size estimates differed among the four incentives types. Any overall effect for incentives could only be meaningful if all those types of rewards had some degree of positive impact on response rates. Given this criterion, the results of the more detailed study type mean

Table 3. Results of Multiple Comparisons for Effect Size by Incentive Type

Effect size (d)	t
MI vs. MR	3.67**
MI vs. OI	4.82**
MI vs. OR	8.13**
MR vs. OI	−.64
MR vs. OR	.85
OI vs. OR	2.21*

NOTE.—(df = 1,41).
* $p < .05$.
** $p < .001$.

comparisons indicated that it is, in fact, inappropriate and incorrect to assume that any reward or incentive used in a mail survey will result in improved response rates. Rather, there was evidence of significant effects, that is, meaningful increases in response rates, only for the two initial mailing incentive conditions (MI and OI) and not for those where the incentive was made contingent on returned responses (MR and OR). Thus, only incentives provided with the initial mailing of the survey instrument had any significant or meaningful positive impact on response rates, which served to disprove H2.

Similarly, there was no support for H3, stating that monetary-related improvements in response rates would be greater overall than those based on nonmonetary incentives. The obtained pattern of significant results solely for MI and OI suggests that the relative timing of the incentive is more important than the nature or form of what is included. It appears that people respond more favorably to incentives that are included with the questionnaire rather than those that are offered as contingent on the completed return and good faith of the mail survey practitioner. This is, perhaps, the most important finding of this meta-analysis.

Hypothesis 4 was supported by the analysis results, replicating other findings in the literature (e.g., Linsky 1975; Yu and Cooper 1983). Those studies in the data base offering prepaid monetary incentives yielded by far the greatest benefits over comparison groups, with an average increase of 19.1 percentage points and an effect size of d = .347. This difference between incentive and control conditions represents a 65 percent mean increase in response when using a monetary incentive with the initial mailing. In comparison, Yu and Cooper (1983), using a more limited number of observations, reported an average response enhancement of 16 percentage points, or an average increase of around 58 percent.

Also, based on the strong correlation (r = .45) between effect size and cash value of the incentive, it would seem that the greater the value, the greater the increase in the response rate. Interestingly, Yu and Cooper (1983) in their analysis noted an even stronger positive correlation between incentive value and increases in returns (r = .61). While they concluded that the relationship between these variables was very strong and linear in nature, other researchers have posited a diminishing returns model to best represent this effect (e.g., Armstrong 1975; Fox, Crask, and Kim 1988). Further analysis and modeling still needs to be conducted, however, to clearly delineate and provide a more refined estimable function of the true relationship between incentive value and increased response rates.

Although the magnitude of the prepaid monetary effect of d = .347 may seem small to medium-size relative to standard qualitative conventions (Cohen 1977) and other reported meta-analysis effects coefficients in the experimental literature (e.g., Guzzo, Jette, and Katzell 1985), simple exploratory comparisons between hypothesized incentive and control return rates suggested that differences of 70 or more percentage points would be necessary to yield effect sizes greater than 1.0. Thus, the very nature of the percentage statistic provides an upper limit to the maximum effect size value obtainable from research using response rates as the primary dependent variable. Furthermore, meta-analyses conducted on other mail survey response enhancers (e.g., first class postage, prenotification by mail, university sponsorship, and follow-up letters) have produced effect sizes of this magnitude as well (Armstrong and Lusk 1987; Fox, Crask, and Kim 1988; Yu and Cooper 1983). From this perspective, an effect size of $d = .347$ seems impressively large. And, given that it represents an average increase of 19 percentage points, it is certainly meaningful enough for most practitioners to consider adopting as a response rate enhancement methodology.

The last hypothesis, H5, proved somewhat difficult to test given the previously cited problems with missing values and scanty documentation of methods. It was possible, however, to test the relative contribution and possible interaction effects of year of publication, study type, and sample composition to the overall reported effects from the 74 observations. As noted in the results and originally hypothesized, only incentive or study type yielded a significant contribution to understanding the variability in effect sizes. None of the other main or interaction effects of the independent variables in the analysis was significant. Thus, the results of this meta-analysis do seem to generalize across different samples and time periods.

It is important to remember, however, that the relative effects of other variables not tested in these analyses could have interacted with incentive type to enhance or inhibit the results (Jones 1979; Jones and Lang 1980; Wiseman 1973). The inability to test these variables or factors is simply a problem of missing data. Those authors that have attempted complex regression models in the past, predicting response rates from numerous indicators, have also encountered this problem, often using reduced sets for analysis, which result in data fragmentation and severe multicollinearity problems (Eichner and Habermehl 1981; Goyder 1982; Heberlein and Baumgartner 1978). Unfortunately, this problem of the relative contribution of related variables will continue until there are enough studies in the literature with fully detailed and documented methodology sections to test the specific combinations of compounded effects.

In conclusion, the results of this meta-analysis suggest that both monetary or nonmonetary incentives mailed with the survey instrument should provide improved return rates worth the investment of time and effort involved in their implementation. It is clear, however, that monetary incentives included in the initial mailing (MI) should be the method of choice for improving respondent return rates. The use of prepaid cash rewards for completing surveys had the most significant impact on increasing response rates among the observations in this meta-analysis.

There is also adequate support for including nonmonetary incentives with the initial mailing. Even though there were practically as many kinds of incentives offered as studies reviewed, there is a sizable if moderate effect (an additional 7.9 percent average increase in returns over control conditions) when including some token of appreciation with the survey. The decision is left to the mail survey practitioner, however, as to whether this additional 7.9 percent is worth investing in the use of a nonmonetary incentive.

It is also apparent from the results of this meta-analysis that practitioners should avoid using incentive systems that offer rewards, either monetary or otherwise, as contingent upon a returned questionnaire. These types of incentive plans are simply not worth the energy involved. They offer neither statistical nor meaningful enhancements to response rates with any consistency.

Appendix

Studies Included in Meta-Analysis

Biner, P. M. 1988. "Effects of Cover Letter Appeal and Monetary Incentives on Survey Response: A Reactance Theory Application." *Basic and Applied Social Psychology* 9:99–106.

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