THE INFLUENCE OF INCREMENTAL INCREASES IN TOKEN CASH INCENTIVES ON MAIL SURVEY RESPONSE
IS THERE AN OPTIMAL AMOUNT?

NORM TRUSSELL
PAUL J. LAVRAKAS

Abstract The research reported is from a large-scale, national, mixed-mode survey (N = 49,675 addresses), in which token incremental, noncontingent incentives used in the second (mail) stage of the survey were tested in an experimental design. The levels of the incentives tested ranged from $0 to $10, in one-dollar increments, with the exception that there was no $9 condition. Furthermore, the experiment was conducted using three types of household addresses that originally were sampled in the first stage of the mixed-mode survey via a random digit dial (RDD) frame: (1) households that agreed to participate in the mail stage of the survey; (2) households that were never contacted in the RDD stage despite numerous contact attempts; and (3) households that had refused to participate when contacted in the RDD stage. It was determined that among those households that had previously agreed to participate, even those that received no cash incentive ($0) were more likely to return completed surveys than the groups of previously non-contacted and previously refusing households that received $10. It was also found that the nature of the outcome of the prior contact with the household mediated (that is, interacted with) the proportional effects of the incremental incentives, in ways only partially consistent with Dillman’s social exchange theory. The findings, which are consistent with leverage-salience theory, suggest that if there is an optimal amount for an incentive to use in a mail survey, then it is a variable amount best targeted to the individual household.

NORM TRUSSELL and PAUL J. LAVRAKAS are with Nielsen Media Research. The authors would like to express their considerable appreciation to the anonymous reviewers of the first version of this manuscript for their important suggestions, which led us to take an entirely different theoretical and analytic approach to our data. We also would like to thank Ken Steve, Jeff Stec, Scott Bell, and Heng Ling-Miean for their statistical advice. Address correspondence to Paul J. Lavrakas; e-mail: pjlavrakas@nielsenmedia.com.

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Introduction

As the survey research industry enters the eighth decade of research on the topic of incentives and response rates, there still appears to be no consensus on what constitutes an “optimal” incentive amount for a survey. A review of the available literature on monetary incentives used in mail surveys reveals many published studies that confirm cash incentives’ positive impact on response rates but little data on the optimum amount to maximize response rates at an affordable level.

The purpose of the present research was to use a large-scale experiment with a representative national sample of households to test the effects of varying the amount of a prepaid, noncontingent cash incentive on the quantity and quality of mail responses to a high-burden survey.1 Further, the experimentation was carried out with three separate types of households, all of which first had been sampled via an RDD telephone screening stage:

1. Households that had been contacted in the telephone stage, had cooperated with the telephone screening survey, and had agreed to participate in the mail survey;
2. Households that had not been contacted in the telephone stage, despite many callback attempts, but for which a telephone number/address matching process had located an address, thus allowing for the mail survey instrument to be sent; and
3. Households that had been contacted in the telephone stage but had refused to participate either in the telephone screening survey and/or the mail survey, but for which a telephone number/address matching process had located an address, thus allowing for the mail survey instrument to be sent.

Consistent with leverage-salience theory (Groves, Singer, and Corning, 2000), the effects of the amount of the incentive, which was sent along with the mail survey instrument, were hypothesized as being mediated by the outcome of the prior contact with the household—that is, whether or not the household had previously been contacted during the telephone sampling stage and, if so, whether or not the contact with the household had resulted in a positive (agreement to cooperate with the mail survey mode) or negative (refusal) outcome.

As explained below in more detail, the level of cash incentives that were mailed along with the survey instrument varied from $0 (no incentive) to $10, in one-dollar increments (with the exception that there was no $9 condition). This level of incentives was not large enough to provide equitable compensation for the burden (the amount of effort or time) required of respondents who completed the mail survey instrument, which was a relatively brief questionnaire.

1. Noncontingent monetary incentives are cash money paid to all prospective survey participants before they begin to complete the survey instrument. Contingent incentives are those that are promised to the respondent as a reward for completing and returning the survey instrument.
and a detailed, weeklong, television-viewing diary. Instead, and as theorized by Dillman (2000), providing a small cash incentive should stimulate compliance with the survey task according to social exchange reasoning, whereby the noncontingent “thank you” incentive at least partially obligates respondents to comply with the research request because of the common courtesy of reciprocating favorably to a request made by someone who has treated you positively (that is, given you money before you have done anything in return).

For households that already had complied in the telephone mode by agreeing to answer the questions asked at that stage and by agreeing to cooperate in the diary measurement stage, the relative effect of the size of the incentive could be hypothesized as being attenuated (suppressed) by the household’s a priori positive disposition to the mail survey. That is, these households already had said voluntarily that they would comply; thus the amount of the token noncontingent incentive they received could be expected to play a relatively smaller role in motivating their subsequent propensity to respond. In contrast, for the households that never were contacted during the telephone mode but for which addresses were matched to their telephone numbers, the effect of the size of the incentive would not have been attenuated by any a priori disposition or commitment. Finally, for those households that had refused to cooperate at the telephone stage but for which addresses had been matched to their telephone numbers, the effect of the size of the incentive could be hypothesized to be enhanced by the household’s a priori negative disposition to the mail survey. That is, these households already had said that they would not comply, and thus the amount of the noncontingent incentive they received could be expected to play a relatively larger role in motivating their subsequent propensity to respond.

PAST RESEARCH ON THE EFFECTS OF INCENTIVES IN SURVEYS

It has been argued that the proper use of monetary incentives is second only to the importance of multiple contacts in improving mail survey response rates (Dillman 1991, 2000). This is attributed to social exchange theory, which presumes that even a small monetary incentive is effective, as it makes the respondent feel a sense of social obligation—that is, “they gave me a monetary gift, so now I should complete the survey for them.” However, it appears that this advantage diminishes the closer the value of the incentive comes to the actual value of the task, which Dillman (2000) believes then becomes an economic exchange; therefore, the person may experience a lessened social obligation to reciprocate by cooperating as a respondent.

A review of the available literature on monetary incentives reveals many published studies that confirm the positive impact of such incentives on response rates but little data on the amount required to maximize response rates at an affordable level. The survey research industry appears to have no consensus as to an “optimal” incentive amount for a mail survey.
In the “tailored design method,” Dillman (2000) advised that a one-dollar bill is the smallest practical amount to send in a mail survey. He stated, “Support exists for somewhat larger amounts being more effective, but there are clearly diminishing returns, with far more of an increase coming from the first dollar than from five or ten times that amount” (p. 168). He did find $2 to be even more effective than $1 in increasing response rates. James and Bolstein (1992) found similar results with the $1 bill and with the relative value of $1 versus $2 in their earlier studies (for example, James and Bolstein 1990). One study (Warriner et al. 1996) found that a $5 cash incentive was more effective than a $2 incentive and as effective as $10. The sample size in this study was rather small, however.

There have been a few meta-analyses and literature reviews conducted over the years that deal with various aspects of incentive amounts. For example, Fox, Crask, and Kim (1988) found 30 published articles on experiments involving the manipulation of the incentive amount. Their analysis indicated that small monetary incentives (from 10 cents to $1) do indeed increase response rates, but they noted a diminishing returns effect as the amount is increased. Yammarino, Skinner, and Childers (1991) conducted an extensive meta-analysis of the literature on mail survey response behavior, including a review of four other meta-analyses (Armstrong and Lusk 1987; Fox, Crask, and Kim 1988; Heberlein and Baumgartner 1978; and Yu and Cooper 1983) involving monetary incentives, all of which found statistically significant, positive results on the use of a monetary incentive.

Kulka (1994) conducted an extensive review of empirical research and current research practices on the use of incentives, with particular emphasis on “hard to reach” respondents. He concluded that the literature supported the generally accepted belief that monetary incentives do increase response rates, results that can generally be attributed to the previously mentioned social exchange theory. He also supported Dillman’s assertion that “the importance of financial incentives is ‘second only’ (perhaps) to the use of follow-up mailings or prompts in improving response rates” (Kulka 1994, p. 262). Kulka’s review of the literature was limited in that most of the studies available for his review dealt with incentives of $1 or less (Armstrong 1975; Fox, Crask, and Kim 1988; James and Bolstein 1992; Jobber and Saunders 1988; Kanuck and Berenson 1975; Linsky 1975; Yammarino, Skinner, and Childers 1991; Yu and Cooper 1983). Kulka found relatively few studies that examined larger incentives amounts, and those that did were mostly studies that surveyed small, specialized groups and may not well generalize beyond the particular study design (Berry and Kanouse 1987; Gunn and Rhodes 1981; James and Bolstein 1992).

Shettle and Mooney (1999) concluded that monetary incentives in a mailed survey do not negatively influence data quality or result in respondent ill will. Like Kulka, they found that very few published studies appear to have been conducted on a large-scale, national random sample of the general population.
No reference could be found to a large, national experiment that examined the incremental impact of a large range of incentives from $1 to $10 and compared the results to a no incentive ($0) control condition. Nor could any study be found that tested whether the effect of the incentive amount was mediated by the nature of any prior contact with the respondent/household (positive, neutral, or negative).

In sum, the literature is virtually unanimous in the conclusion that the use of small, noncontingent monetary incentives will increase cooperation rates in mailed surveys and are more effective than a promised reward, even if the promised reward is of greater value, for completing the survey. However, there is very little explicit guidance as to how much of an incentive should be given or on what effect the nature of any prior contact with a household may have on the effects of the incentives. The experiment reported in this article, which used a large, representative, national sample of the general population of the United States, was conducted in a manner to shed light on these matters.

**Methodology**

_**Nielsen Diary Research Methodology**_

_Nielsen TV Ratings_ conducts large surveys to measure household television viewing through its National Station Index (NSI) Diary service, which is conducted several times each year in 210 local market areas throughout the United States. This process is commonly referred to as “the Sweeps.” Respondents in randomly sampled households throughout the United States are sent paper, television-viewing data collection questionnaire booklets, hereinafter referred to as a diary, to record demographic information and all household television viewing for a specified one-week period. Since the start of its diary service, Nielsen has recognized the value of a small noncontingent monetary incentive to increase cooperation rates and, as such, a noncontingent incentive always has been included in the diary packet.

The diary research process begins with the selection of a RDD sample of telephone numbers in each of 210 Designated Market Areas, hereinafter referred to as markets, across the United States. The first contact for those households with a matched address to their telephone number is a pre-recruitment postcard announcing the household’s selection and forthcoming phone call. The second contact (or first for those with no matched address) is a recruitment telephone call to request the household’s commitment to complete the diary.

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2. Designated Market Areas are rigidly defined geographical areas used by Nielsen to identify television stations that best reach an area and attract the most viewers. Also known as a DMA market or area, a DMA market consists of all zip codes whose largest viewing share is given to stations of that same market area. Non-overlapping DMA markets cover the entire continental United States, Hawaii, and parts of Alaska.
Third, all households that agree to participate during the RDD call or that are not contacted or refuse, but have a matched address available, are sent a post-RDD advance postcard advising that diary materials will be mailed to them shortly.

Next, a diary packet, which includes a cover letter, a question and answer brochure, the small monetary incentive, and at least one diary booklet, is sent. At the time of this experiment, all diary packets were sent by first-class mail. The final contacts with each household, which occur during the diary-keeping week, consist of (1) two postcards and one phone call, or (2) one postcard and two phone calls, depending on the listed/unlisted status of the household’s telephone number.

Of note, no reference to the cash incentive that would be mailed was made in the RDD telephone contact. Further, interviewers were blind as to which of the incentive-level conditions a given household had been randomly assigned, with the exception that for telephone numbers in the $0 condition, interviewers were notified via the CATI system that in trying to gain cooperation from those households they were not to make any mention of the household receiving “a small thank you gift” along with the diary materials.

EXPERIMENTAL DESIGN

This experiment was conducted from March 1998 to May 1998, with the field period including both the RDD and mail stages. The experiment tested the effect on mail survey cooperation rates of sending television viewing diaries and materials to randomly assigned households with incentives of $0, $1, $2, $3, $4, $5, $6, $7, $8, or $10. The experimental treatments are outlined in table 1. No $9 incentive condition was used, as it was reasoned from previous Nielsen research that the expected cooperation rate for that amount could be modeled adequately using the data collected for the other experimental conditions, and the cost that would have been associated with the $9 condition instead was utilized to increase sample sizes for other experimental treatments.

Sampling Considerations. The sample sizes in this experiment were chosen to be large enough to detect any substantial effects achieved among the $0, $1, $2, $8, and $10 incremental treatments. Larger sample sizes were used with the $3 to $7 treatments to detect even small differences in cooperation. The largest sample sizes were used for the $4, $5, and $6 conditions, as past Nielsen research had suggested they would be most likely to result in increases in cooperation at levels that had a favorable cost-benefit ratio for the company to actually implement the treatment into its production procedures.

A random portion of 113,743 telephone numbers out of the total RDD sample of 1,054,894 was selected for inclusion in the experimental conditions. After recruitment phoning, households within the experiment sample were

3. For those households interviewed in the RDD stage, a diary booklet is sent for each television set in the household, up to a maximum of five diaries/sets.
randomly assigned to an experimental condition, based on the digits in their telephone number. The counts are shown in table 1.

**Incentives.** All cash incentives were sent in single bill denominations (for example, the $10 condition was paid as ten $1 bills). The test incentive amount was mailed in the diary packet.

**Dependent Variables.** Three dependent variables were created to investigate the effects of the experimental treatments. First, a “return” variable was created for all mailed households to represent whether any diary was received back from a household, regardless of whether it was completed correctly. Thus, the “return rate” for a given experimental condition is a measure of the percentage of all diaries returned compared to all households mailed. Second, a “cooperation” variable was created for all mailed households to represent whether a fully completed diary (or set of diaries) was received from the household. Thus, the “cooperation rate” for a given experimental condition is the proportion of households returning usable diaries compared to all households mailed. Third, a “quality of return” variable was created to indicate the proportion of returned diaries that were fully and correctly completed among all returned diaries. Thus, the “quality of return rate” for a given experimental condition is the proportion of households that returned valid diaries compared to all households that returned any diaries.

4. Due to the unique, multimode nature of our survey, it is not possible to adhere more closely to the American Association for Public Opinion Research (AAPOR) standard definitions guidelines for calculating more traditional response rates. This, in itself, should not lessen the value of the presented results, as the purpose of the dependent variables and the analyses in which they are used is to provide consistent and informative contrasts across the experimental conditions.
Results

RESULTS AMONG ACCEPTING HOUSEHOLDS

Table 2 shows the results across the experimental conditions for those households that had agreed to participate in the mail survey when they were contacted/sampled in the RDD stage. Cell by cell comparisons were made using a two-tailed t-test for difference of proportions. All the conditions in which a cash incentive was sent led to a significantly higher likelihood of the diaries being returned than the no-cash, $0 condition. The conditions in which at least $5 was sent showed a significantly higher likelihood of return compared to the conditions in which less than $5 was sent. Conditions in which $6, $7, or $10 were sent showed a significantly higher likelihood of return than did $5. There were no significant differences in likelihood of return among the $6–$10 conditions.

Table 2. Return Rates, Cooperation Rates, and Quality of Return Rates for Accepting Households Mailed

<table>
<thead>
<tr>
<th>Condition</th>
<th>Incentive Amount</th>
<th>Households Mailed</th>
<th>Return Rate (%)</th>
<th>Cooperation Rate (%)</th>
<th>Quality of Return Rate (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>a</td>
<td>$0</td>
<td>541</td>
<td>42.7&lt;sup&gt;B,C,D,E,F,G,H,I,J&lt;/sup&gt;</td>
<td>37.9&lt;sup&gt;B,C,D,E,F,G,H,I,J&lt;/sup&gt;</td>
<td>88.7</td>
</tr>
<tr>
<td>b</td>
<td>$1</td>
<td>896</td>
<td>56.9&lt;sup&gt;A,F,G,H,I,J&lt;/sup&gt;</td>
<td>50.6&lt;sup&gt;A,F,G,H,I,J&lt;/sup&gt;</td>
<td>86.2</td>
</tr>
<tr>
<td>c</td>
<td>$2</td>
<td>1,634</td>
<td>55.6&lt;sup&gt;A,E,F,G,H,I,J&lt;/sup&gt;</td>
<td>50.3&lt;sup&gt;A,E,F,G,H,I,J&lt;/sup&gt;</td>
<td>90.4</td>
</tr>
<tr>
<td>d</td>
<td>$3</td>
<td>2,132</td>
<td>58.3&lt;sup&gt;A,F,G,H,I,J&lt;/sup&gt;</td>
<td>51.3&lt;sup&gt;A,F,G,H,I,J&lt;/sup&gt;</td>
<td>87.9&lt;sup&gt;f&lt;/sup&gt;</td>
</tr>
<tr>
<td>e</td>
<td>$4</td>
<td>6,759</td>
<td>59.1&lt;sup&gt;A,C,F,G,H,I,J&lt;/sup&gt;</td>
<td>52.8&lt;sup&gt;A,C,F,G,H,I,J&lt;/sup&gt;</td>
<td>89.4</td>
</tr>
<tr>
<td>f</td>
<td>$5</td>
<td>7,085</td>
<td>61.6&lt;sup&gt;A,B,C,D,E,G,h,j&lt;/sup&gt;</td>
<td>55.2&lt;sup&gt;A,B,C,D,E,G,i&lt;/sup&gt;</td>
<td>89.6</td>
</tr>
<tr>
<td>g</td>
<td>$6</td>
<td>6,997</td>
<td>63.2&lt;sup&gt;A,B,C,D,E,i&lt;/sup&gt;</td>
<td>56.6&lt;sup&gt;A,B,C,D,E,i&lt;/sup&gt;</td>
<td>89.6</td>
</tr>
<tr>
<td>h</td>
<td>$7</td>
<td>2,408</td>
<td>63.7&lt;sup&gt;A,B,C,D,E,i&lt;/sup&gt;</td>
<td>57.0&lt;sup&gt;A,B,C,D,E&lt;/sup&gt;</td>
<td>89.5</td>
</tr>
<tr>
<td>i</td>
<td>$8</td>
<td>1,096</td>
<td>63.8&lt;sup&gt;A,B,C,D,E&lt;/sup&gt;</td>
<td>58.5&lt;sup&gt;A,B,C,D,E,F&lt;/sup&gt;</td>
<td>91.7&lt;sup&gt;d&lt;/sup&gt;</td>
</tr>
<tr>
<td>j</td>
<td>$10</td>
<td>692</td>
<td>65.5&lt;sup&gt;A,B,C,D,E,i&lt;/sup&gt;</td>
<td>58.2&lt;sup&gt;A,B,C,D,E&lt;/sup&gt;</td>
<td>89.0</td>
</tr>
</tbody>
</table>

*<i>p < .10 compared to $0</i>  
*<i>p < .10 compared to $1</i>  
*<i>p < .10 compared to $2</i>  
*<i>p < .10 compared to $3</i>  
*<i>p < .10 compared to $4</i>  
*<i>p < .10 compared to $5</i>  
*<i>p < .10 compared to $6</i>  
*<i>p < .10 compared to $7</i>  
*<i>p < .10 compared to $8</i>  
*<i>p < .10 compared to $10</i>

5. <i>p value = 2*(1 – [Normal cumulative distribution (ABS(R/SE)]) where R = difference in percentages, and SE = standard error of that difference.</i>
A nearly identical pattern of significant differences was observed for the impact of the incentive amount on cooperation. In terms of the quality of the return, there essentially were no significant differences across any of the incentive levels, other than a few that could be expected to occur merely by chance.

RESULTS AMONG MAILED, NONCONTACTED HOUSEHOLDS

Table 3 shows the results across the experimental conditions for those households that were mailed diaries despite never being contacted during the RDD stage. Thus, these households had neither agreed nor refused to participate in the mail stage of the survey.

In terms of the household’s likelihood of returning and completing survey materials, surprisingly there was no significant difference between the non-cash, $0 condition and any of the $1–$4 cash incentive conditions. The $5–$8 conditions were not different from each other in either return or cooperation likelihood. However, with the exception of the $7 condition, they all showed a significantly greater likelihood in eliciting the return of completed survey

<table>
<thead>
<tr>
<th>Condition</th>
<th>Incentive Amount</th>
<th>Households Mailed</th>
<th>Return Rate (%)</th>
<th>Cooperation Rate (%)</th>
<th>Quality of Return Rate (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>a</td>
<td>$0</td>
<td>99</td>
<td>13.1 $e, t, J</td>
<td>10.1 $e, t, J</td>
<td>76.9 $B, D, E, j</td>
</tr>
<tr>
<td>b</td>
<td>$1</td>
<td>188</td>
<td>14.4 $e, t, J</td>
<td>12.8 $t</td>
<td>88.9 $A, C, e, F, G, H, i</td>
</tr>
<tr>
<td>c</td>
<td>$2</td>
<td>305</td>
<td>12.5 $e, t, J</td>
<td>9.2 $t</td>
<td>73.7 $B, D, E, F, G, H, J</td>
</tr>
<tr>
<td>d</td>
<td>$3</td>
<td>408</td>
<td>16.9 $</td>
<td>14.5 $t</td>
<td>85.5 $A, C, e, F, G, H</td>
</tr>
<tr>
<td>e</td>
<td>$4</td>
<td>1,311</td>
<td>15.8 $e, t, J</td>
<td>13.3 $t</td>
<td>84.1 $A, B, C, F, G, H</td>
</tr>
<tr>
<td>f</td>
<td>$5</td>
<td>1,308</td>
<td>20.3 $a, b, C, E, J</td>
<td>16.6 $t</td>
<td>81.6 $B, C, d, e, H</td>
</tr>
<tr>
<td>g</td>
<td>$6</td>
<td>1,392</td>
<td>20.5 $a, b, C, E, J</td>
<td>16.7 $t</td>
<td>81.1 $B, C, D, E, H</td>
</tr>
<tr>
<td>h</td>
<td>$7</td>
<td>473</td>
<td>19.5 $C, t</td>
<td>14.8 $t</td>
<td>76.1 $B, D, E, F, G, H</td>
</tr>
<tr>
<td>i</td>
<td>$8</td>
<td>204</td>
<td>24.0 $a, b, C, E</td>
<td>19.6 $t</td>
<td>81.6 $B, C</td>
</tr>
<tr>
<td>j</td>
<td>$10</td>
<td>121</td>
<td>30.6 $A, B, C, D, E, F, G, H</td>
<td>26.4 $t</td>
<td>86.5 $A, C, H</td>
</tr>
</tbody>
</table>

*p < .10 compared to $0  A p < .05 compared to $0
*p < .10 compared to $1  B p < .05 compared to $1
*p < .10 compared to $2  C p < .05 compared to $2
*p < .10 compared to $3  D p < .05 compared to $3
*p < .10 compared to $4  E p < .05 compared to $4
*p < .10 compared to $5  F p < .05 compared to $5
*p < .10 compared to $6  G p < .05 compared to $6
*p < .10 compared to $7  H p < .05 compared to $7
*p < .10 compared to $8  I p < .05 compared to $8
*p < .10 compared to $10 J p < .05 compared to $10
materials (that is, the cooperation variable) compared to the $0, $2, and $4 conditions. The $5–$8 conditions also all showed improvements over the $1 and $3 conditions in terms of return and cooperation, but in most cases the differences did not achieve significance. The $10 condition was significantly more likely than any of the incentives below $8 to yield returned and completed survey materials. However, although the $10 conditions also performed much better than the $8, the difference was not significant due to the relatively small sample sizes in these conditions.

The quality of the returns varied inconsistently across conditions, with the $1, $3, and $10 conditions having the highest quality of return ratios, whereas the $0, $2, and $7 conditions had the lowest quality ratios.

RESULTS AMONG MAILED, REFUSING HOUSEHOLDS

Table 4 shows the results across the experimental conditions for those households that were mailed diaries even though they refused to cooperate during the RDD stage.

Of note, although the no-cash, $0 condition was significantly less likely than all the other cash conditions to return survey materials, there was no significant

<table>
<thead>
<tr>
<th>Condition</th>
<th>Incentive</th>
<th>Households Mailed</th>
<th>Return Rate (%)</th>
<th>Cooperation Rate (%)</th>
<th>Quality of Return Rate (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>a</td>
<td>$0</td>
<td>201</td>
<td>7.5&lt;sup&gt;B, C, D, E, F, G, H, I, J&lt;/sup&gt;</td>
<td>5.5&lt;sup&gt;C, D, E, F, G, H, I, J&lt;/sup&gt;</td>
<td>73.3&lt;sup&gt;B, C, D, E, F, G&lt;/sup&gt;</td>
</tr>
<tr>
<td>c</td>
<td>$2</td>
<td>742</td>
<td>16.7&lt;sup&gt;A, B, E, F, G, H, I, J&lt;/sup&gt;</td>
<td>11.0&lt;sup&gt;A, B, E, F, G, H, I, J&lt;/sup&gt;</td>
<td>61.6&lt;sup&gt;A, E, F, G, H, I, J&lt;/sup&gt;</td>
</tr>
<tr>
<td>d</td>
<td>$3</td>
<td>990</td>
<td>17.9&lt;sup&gt;A, B, E, F, G, H, I, J&lt;/sup&gt;</td>
<td>14.8&lt;sup&gt;A, B, C, D, E, H, J&lt;/sup&gt;</td>
<td>69.0&lt;sup&gt;A, B, C, D, E, H&lt;/sup&gt;</td>
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<tr>
<td>e</td>
<td>$4</td>
<td>3,004</td>
<td>22.8&lt;sup&gt;A, B, C, D, E, F, G, H, I, J&lt;/sup&gt;</td>
<td>16.8&lt;sup&gt;A, B, C, D, E, H, J&lt;/sup&gt;</td>
<td>71.3&lt;sup&gt;B, C, D, E, G&lt;/sup&gt;</td>
</tr>
<tr>
<td>f</td>
<td>$5</td>
<td>3,237</td>
<td>24.4&lt;sup&gt;A, B, C, D, E, F, G, H, I, J&lt;/sup&gt;</td>
<td>17.1&lt;sup&gt;A, B, C, D, E, H, J&lt;/sup&gt;</td>
<td>72.5&lt;sup&gt;B, C, D, E, G&lt;/sup&gt;</td>
</tr>
<tr>
<td>g</td>
<td>$6</td>
<td>3,294</td>
<td>25.4&lt;sup&gt;A, B, C, D, E, F, G, H, I, J&lt;/sup&gt;</td>
<td>21.2&lt;sup&gt;A, B, C, D, E, F, G&lt;/sup&gt;</td>
<td>74.3&lt;sup&gt;B, C, D, E, F, G&lt;/sup&gt;</td>
</tr>
<tr>
<td>h</td>
<td>$7</td>
<td>1,133</td>
<td>28.5&lt;sup&gt;A, B, C, D, E, F, G&lt;/sup&gt;</td>
<td>18.6&lt;sup&gt;A, B, C, D, E, F, G&lt;/sup&gt;</td>
<td>71.3&lt;sup&gt;B, C, D, E, G&lt;/sup&gt;</td>
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<td>i</td>
<td>$8</td>
<td>414</td>
<td>26.1&lt;sup&gt;A, B, C, D, E, F, G, I&lt;/sup&gt;</td>
<td>23.5&lt;sup&gt;A, B, C, D, E, F, G&lt;/sup&gt;</td>
<td>72.5&lt;sup&gt;B, C, D, E, G&lt;/sup&gt;</td>
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<td>j</td>
<td>$10</td>
<td>247</td>
<td>32.4&lt;sup&gt;A, B, C, D, E, F, G, I&lt;/sup&gt;</td>
<td>23.5&lt;sup&gt;A, B, C, D, E, F, G&lt;/sup&gt;</td>
<td>72.5&lt;sup&gt;B, C, D, E, G&lt;/sup&gt;</td>
</tr>
</tbody>
</table>

<sup>a</sup> p < .10 compared to $0  <sup>b</sup> p < .10 compared to $1  
<sup>c</sup> p < .10 compared to $2  <sup>d</sup> p < .10 compared to $3  
<sup>e</sup> p < .10 compared to $4  <sup>f</sup> p < .10 compared to $5  
<sup>g</sup> p < .10 compared to $6  <sup>h</sup> p < .10 compared to $7  
<sup>i</sup> p < .10 compared to $8  <sup>j</sup> p < .10 compared to $10
difference between the $0 condition and the $1 condition among refusing households in the rate at which they returned completed survey materials. Refusing households in the $2 and $3 conditions were more likely than the $0 condition to return completed diaries. However, they were significantly less likely to return completed diaries than the $4–$10 conditions. With a few exceptions in the pattern of significance, the $7–$10 conditions were more likely to return completed diaries than the conditions in which less than $7 were given as incentives. The households in the $10 incentive condition significantly outperformed all the conditions except $7 and $8 in terms of both returned and completed diaries. Although the $10 condition outperformed the $7 and $8 conditions in terms of cooperation, the differences were not significant due to the relatively small sample sizes.

In terms of the quality of return across the various incentive levels, the $1–$3 incentive treatments did worse than the $0 condition and those of $4 or more. Surprisingly, the $0 condition had as high a quality of return ratio as the $7–$10 treatments.

COMPARING INCENTIVE EFFECTS ACROSS ACCEPTING, NONCONTACTED, AND REFUSING HOUSEHOLDS

First, it should be noted that regardless of the levels of token incentives that were sent, these results demonstrate conclusively that those respondents who previously agreed to comply with the researcher’s request did, in fact, comply at much higher rates than respondents who previously either had not been contacted or had refused the initial request. As shown in tables 2–4, even among the group of accepting households given no cash at all, return rates and cooperation rates were considerably higher than among the noncontacted and refusing households given $10.

Second, it was hypothesized that the relative effect of each incremental dollar in incentives would be mediated by the outcome of the RDD sampling stage of this research design. It was expected that increasing incentive values across the $0–$10 continuum would have the greatest proportional effects on refusing households and the least proportional effects on accepting households.

In table 4 it can be seen that there is 327 percent change in cooperation among refusing households, comparing the $0 condition to the $10 condition. The change among refusing households comparing the $1 condition with the $10 condition is 205 percent. In table 3 it can be seen that there is a 161 percent change in cooperation among noncontacted households, comparing the $0 condition to the $10 condition; comparing the $1 condition to the $10 condition, there is a 106 percent change in cooperation for the noncontacted households. For the accepting households (table 2), there is a change of 54 percent in cooperation, comparing the $0 condition to the $10 condition; comparing the $1 condition and the $10 condition, the change is 15 percent. Thus, considering only these simple comparisons of the proportional effects of
increasing the incentive from $0 to $10 or from $1 to $10, the hypothesis of the effect of prior contact mediating the incentive effects is strongly supported.

More important, figures 1–3 show the trend lines for cooperation between $1 and $10 (with a linear regression line plotted) for each of the three types of respondent households. Here it can be seen that the slope (the rate of change) of the regression lines in figure 2 versus figure 1 is significantly different (odds ratio = 1.048, \( p = .036 \)), with a slightly greater rate of change among noncontacted households than among accepting households. The slope of the regression line in figure 3 versus figure 1 also is significantly different (odds ratio = 1.084, \( p < .001 \)), with a slightly greater rate of change among refusing households than among accepting households. Finally, although the slope for refusing households was slightly greater than that for noncontacted households (odds ratio = 1.034, \( p = .187 \)), the difference was not statistically significant. This provides more definitive evidence of the differential effect that an incremental change in incentives has among those who previously agreed to cooperate compared to those who have not previously agreed to cooperate. In

6. These are results from a logistic regression analysis.
Influence of Token Cash Incentives

These data demonstrate the general trend that raising the level of token incentives has proportionally more effect on respondents who previously had refused to cooperate, and proportionally less effect on those who had previously agreed to cooperate.

Discussion

Heretofore in the research literature no consensus has developed about an optimal incentive amount, or even an optimal range, for a mail survey. However, there is solid agreement that a noncontingent incentive is better at eliciting survey cooperation than a contingent incentive, and that sending at least $1 is better than sending no cash at all. In addition, Dillman (2000) has advised that the relative effect of the first incentive dollar—that is, the effect on cooperation of $1 versus $0—is the greatest of any one-dollar increment thereafter. Finally, there is no report of any previous studies that were designed to investigate the impact of prior contact with respondents on the effects of subsequent incentives or incremental increases in their levels.

Figure 2. Cooperation among noncontacted households by incentive level.
The findings reported here provide results from the first reported, large-scale, national experiment on the effects of one-dollar increments from $0 to $10 (with the exception of there not being a $9 condition) on survey response and cooperation. Furthermore, the experiment was designed in a manner that allowed for an investigation of the effects of whether prior contact with the household—in this case via the RDD telephone mode—was positive, neutral, or negative.

As found in previous work, sending at least a $1 noncontingent incentive, compared with not sending any incentive whatsoever, increases the chance that respondent households will send back survey materials and that these materials will be properly completed. Among the three groups of respondents in this experiment, the difference in cooperation of $1 versus $0 ranged from approximately 2 to 13 percentage points, although the difference was not statistically significant among two of the groups. Similarly, with very few aberrations, the overall pattern of results across the three groups was that each additional dollar raised response and cooperation in a monotonic fashion.

It also should be noted that respondents who agreed to cooperate in the RDD stage, before they had received or knew about any incentive, returned completed diaries at higher rates, even when given no incentive at all, than did
Influence of Token Cash Incentives

those who were given as much as $10 but previously had not agreed to cooperate. This in itself shows the power of a voluntary commitment to affect survey response as compared to paid inducement.

However, considering the incremental effects of noncontingent incentives, there was a striking difference in the effect of each additional dollar sent depending on the nature of the outcome of prior contact that had been made with the respondent household. That is, consistent with leverage-salience theory (Groves, Singer, and Corning 2000), the outcome at the RDD stage interacted with the effects of the increments.

For households that had had a positive outcome when they were contacted in the RDD sampling stage, the first dollar sent performed essentially as well as the second, third, and fourth dollars sent. It was not until the $5 condition that response and cooperation were consistently and significantly higher than under the $1 condition. Moreover, after $6, the $7–$10 conditions did not significantly enhance response or cooperation. Thus, the implication that reasonably could be drawn from these results for researchers looking to raise response rates is to simply send $1, or to send $5 or $6 if that can be afforded and if the potential gain justifies the considerable increase in incentive costs. For this group, Dillman’s observation about the profound proportional impact of the first dollar sent certainly holds; it is the first dollar that has the greatest cost-benefit effect on respondent cooperation.

In contrast, from the results with the two other respondent groups—those that were never directly contacted in the RDD stage, and thus ostensibly had a neutral previous experience with the researchers, and those that had refused to participate in the RDD stage and had a negative previous contact experience—a very different conclusion would be drawn. As shown in tables 3 and 4, and in figures 2 and 3, each incremental dollar essentially has a slightly larger impact on response and cooperation beyond the previous incentive amount that was one dollar less in value. In particular, within these two groups Dillman’s observation that the first dollar generates proportionally the largest effect in cooperation does not hold. For these groups that previously had not made a commitment to complete and return the survey materials, each additional dollar helped by a fairly similar amount, as shown by the regression line fitted to the scatterplots. As such, these results would recommend to a researcher that if there is previous contact with respondents and that contact is not positive (for example, it does not elicit an agreement to subsequently cooperate), then one should send as much as one’s budget can afford by way of a token noncontingent incentive (defined here as up to $10).

Granted, leverage-salience theory would suggest that a respondent’s previous disposition toward a request to participate in a mail survey would interact with the effect of incentives given in the mail survey on response propensity. But why should this particular pattern of results be expected?

As noted previously, social exchange theory suggests that providing any token cash incentive “indebts” the respondent to respond favorably to the
researcher’s request. In most mail surveys, respondents do not have *formal* prior contact with the researcher and thus whatever forces may be operating to affect cooperation are essentially unknown to the researcher. However, even without formal prior contact by the particular researcher, a respondent brings her or his own prior dispositions or stereotypes to the notion of research and participating as a research subject/respondent. Some people are positively disposed; others are neutral; still others are negative. All these dispositions are muddled together in the respondent pool that is sampled for most mail surveys. As such, the notion that these prior dispositions toward research are interacting with, thus mediating, any incentive that is sent has been largely “invisible” to researchers.

As posited in our introduction, if a respondent is previously disposed positively toward the researcher’s request, then the effects of a noncontingent incentive sent along with a mail survey, when the respondent was not expecting it, would in theory have relatively little impact on the respondent’s decision to cooperate. Instead, whatever originally motivated the respondent to agree to participate in the research study should continue to hold more sway in determining cooperation than the subsequent receipt of an unexpected token cash incentive. However, as shown in table 2 and figure 1, this does not mean that the social exchange theory has no influence under these circumstances. Rather, it merely has a reduced power to affect response and cooperation.

In contrast, for those respondents who hold either a neutral or negative prior disposition toward research or the researcher, social exchange theory appears highly relevant. That is because for these respondents the noncontingent cash incentive is not competing with (that is, not being mediated by) a prior commitment to cooperate. Instead the incentive now takes on a special power, in that it is a means whereby the researcher can “tip the scales” so as to indebt such respondents who are otherwise feeling no prior motivation or commitment to comply.

These results appear highly relevant to researchers who deploy multistage, mixed-mode surveys such as the one used in this experiment. Whenever there is a previous contact with a respondent, that contact should be expected to mediate the effects of any subsequent contact with the respondent. As such, the prudent researcher will investigate the nature of the effects of the prior contact and take them into consideration when planning the next stage of contact. Simple common sense dictates that for those respondents who have previously cooperated without the inducement of cash incentives, the future role that cash incentives need to play in stimulating response is likely to be less than for respondents who have never previously cooperated without cash. In fact, as soon as cash is given to respondents who previously were positively disposed to cooperate with the researcher, one might reasonably fear that the nature of the “social contract” between the researcher and the respondent is changed for the worse. Subsequent to that, if too much cash is given as an incentive, the freely given good will of those respondents may erode to the point where the respondent comes to expect cash.
In contrast, for those respondents who previously do not have a positive disposition or commitment to cooperate, the value of a cash incentive takes on special power. And, the findings here suggest that the more cash that is given, the higher the rate of response will be. By implication, if the researcher can learn about a respondent’s prior willingness to cooperate in the absence of a cash inducement, then the researcher should be able to use that knowledge to make an informed decision about how best to target the use of cash incentives to a particular respondent. By further implication, the notion that one “optimal” level of incentive should be sent to all respondents makes no theoretical sense whatsoever (although in many cases it may be the only feasible alternative). Instead, the findings presented here are a call to researchers to begin the more sophisticated deployment of token cash incentives through differential deployment at the level of the individual respondent, thereby utilizing one’s finite resources in a more cost-beneficial manner.

Appendix

NIELSEN DIARY RESEARCH METHODOLOGY ADDENDUM

The Nielsen Station Index (NSI) was established in 1954 and provides local market television audience measurement ratings in all 210 Designated Market Areas (DMAs) in the United States, covering approximately 110 million television households and continuous metered-market overnight measurement in 56 major markets (nearly 75 million television households).

In recent years, the survey research industry has experienced a steady deterioration in telephone and mail survey cooperation rates. The Nielsen TV Ratings has also endured a decline in cooperation rates at the mail survey stage of its diary service. Overall cooperation rates for the diary service ranged in the mid- to upper-50 percent range in the 1960s and early 1970s. By the late 1980s, cooperation rates had dropped to the 45–50 percent range, and by the late 1990s, they had declined to the low 30s. This continual erosion in overall response rates has had a negative impact on sample representation, with cooperation among younger aged households declining faster than among older households.

The diary research process begins with the selection of a random digit dial (RDD) sample of telephone numbers in each of 210 Designated Market Areas across the United States. These telephone numbers are matched to a database of available addresses and demographic information by a third-party vendor. Records are then processed through address verification software in an effort to improve accuracy of the matching process. For households with an address match, the first contact is a pre-recruitment postcard announcing the household’s selection and forthcoming phone call from the research interviewer. The second contact (or first for those with no address associated with their record) is a recruitment call requesting commitment to complete the diary. Third, all households that (1) agree to participate during the RDD call, or (2) are not contacted or refuse, but have a valid address available, are sent an advance postcard advising them that diary materials will be mailed to them shortly.
Next, a diary packet is sent, which includes a cover letter, a question and answer brochure, a small monetary incentive, and, if the number of television sets is known, a diary booklet for each television (up to five), or a single diary if the number is not known. (At the time of this experiment, all diary packets were sent by first-class mail.) The diary booklet itself is designed to be a self-mailer for return to Nielsen. The final contacts with each household, which occur during the diary-keeping week, consist of (1) two postcards and one telephone call or (2) one postcard and two phone calls, depending on the status of the household. These final contacts are intended to remind the diary keeper to start the diary on Thursday and to return it at the end of the one-week survey period.

Standard Nielsen incentives and treatments at the time of this experiment depended on the listed/unlisted status of the RDD telephone number of the household, the size of the market, and the number of televisions in the household. Standard incentives for nonethnic households in the 100 largest markets were:

$1 for listed phone number households with one to four television sets; number of TVs unknown households (refusals and noncontacts); and unlisted telephone number households with one or two TVs.

$2 for listed telephone number households with five TVs; and unlisted telephone number households with three or four TVs, and

$3 for unlisted telephone number households with five TVs.

Standard incentives for nonethnic households in smaller markets (also known as 101+) were:

$3 for unlisted telephone number households with five TVs; and

$2 for all other households.

References


