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Diane K. Willimack
Howard Schuman
Beth-Ellen Pennell
James M. Lepkowski

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Survey Methodology Program • Institute for Social Research
University of Michigan • P.O. Box 1248 • Ann Arbor, MI 48106-1248
Phone (313) 763-2359 Fax (313) 764-8263

EFFECTS OF A PREPAID NONMONETARY INCENTIVE ON RESPONSE RATES AND RESPONSE QUALITY IN A FACE-TO-FACE SURVEY

DIANE K. WILLIMACK
HOWARD SCHUMAN
BETH-ELLEN PENNELL
JAMES M. LEPKOWSKI

Abstract We conducted a randomized experiment on a face-to-face interview survey in order to test the effects on response rates of a prepaid nonmonetary incentive. Results showed a statistically significant increase in response rates, mostly through reduction in refusal rates, in the half sample that received the incentive (a gift-type ballpoint pen) as compared with a no incentive control group. The effect appears to be due to greater cooperation from incentive recipients at the initial visit by an interviewer. Unexpectedly, the incentive group also showed a significantly higher rate of sample ineligibility, possibly due to easier identification of vacant residences or nonexistent addresses. In addition, evidence suggests greater response completeness among responding incentive recipients early in the interview, with no evidence of increased measurement error due to the incentive.

DIANE K. WILLIMACK is a survey methodologist at the National Agricultural Statistics Service of the U.S. Department of Agriculture. HOWARD SCHUMAN is professor of sociology and research scientist at the Institute for Social Research at the University of Michigan. BETH-ELLEN PENNELL is director of the Field Section, Survey Research Center, Institute for Social Research at the University of Michigan. JAMES M. LEPKOWSKI is senior study director at the Institute for Social Research at the University of Michigan.
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Incentives have long been used in mail surveys to increase response rates, and their effectiveness has been well documented in the published literature. See, for example, qualitative literature review articles by Armstrong (1975), Houston and Ford (1976), Kanuk and Berenson (1975), and Linsky (1975) or quantitative meta-analyses by Church (1993), Fox, Crask, and Kim (1988), Heberlein and Baumgartner (1978), Yammarino, Skinner, and Childers (1991), and Yu and Cooper (1983). Along with these reviews, experimental studies have shown that prepaid incentives are more effective than promises of rewards conditional upon survey completion (Berk et al. 1987; Gelb 1975; Goodstadt et al. 1977; Wotruba 1966), and they also suggest that monetary incentives are more successful than nonmonetary gifts (Goodstadt et al. 1977; Hansen 1980). Furthermore, response rates increase with the cash amount of a monetary incentive, but at a decreasing rate, such that larger amounts may not prove cost effective (Godwin 1979; James and Bolstein 1990, 1992; Kephart and Bressler 1958; Mizes, Fleece, and Roos 1984; Schewe and Cournoyer 1976). Since recent studies recommend the use of $1 (James and Bolstein 1990; Mizes, Fleece, and Roos 1984), especially when paired with sound mail survey practices of multiple follow-ups (James and Bolstein 1992), it appears that even token amounts are sufficient to influence response rates.

Some experimental evidence suggests that promised monetary incentives may also be used effectively in telephone surveys (Goetz, Tyler, and Cook 1984; Gunn and Rhodes 1981), although promised nonmonetary incentives were ineffective in one study (Pharr, Steufen, and Wilbur 1990). In addition, monetary incentives have been shown to be useful in surveys that place exceptional task burden on respondents, such as medical examinations (Findlay and Schaible 1975), educational testing (Chromy and Horvitz 1978), keeping daily expenditure diaries (Ferber and Sudman 1974; Walsh 1977), or panel participation (Berk et al. 1987). However, aside from these instances, use of incentives in survey administration modes other than mail is not well documented. This is especially true for face-to-face surveys, where interviewer presence at the door is considered the major factor motivating participation.

Our study advances understanding of incentive use by reporting the results of a split sample incentive experiment conducted in a face-to-face interview survey in the Detroit metropolitan area in 1991. The nonmonetary incentive item, a special gift-type ballpoint pen, was sent to a random half of the sample addresses in advance of initial interviewer contact. The Pen and No Pen half samples are compared for differences in response rates and for effects on the components of nonresponse, particularly refusals.

In addition, responses are examined for effects of the incentive on

data quality. It has been argued that incentives may have positive or negative effects on response quality that are as important as their effects on response rates (Houston and Ford 1976). A number of studies have found positive incentive effects on questionnaire completeness, with no response bias, fewer response errors when some measure of validity was available, and more complete responses to open questions as reflected in a greater number of words written or more distinct items mentioned (Berk et al. 1987; Ferber and Sudman 1974; Godwin 1979; Goetz, Tyler, and Cook 1984; McDaniel and Rao 1980; Mizes, Fleece, and Roos 1984; Whitmore 1976; Wotruba 1966). Nevertheless, because of the potential trade-off between nonresponse reduction and measurement error, the effect of incentives requires evaluation beyond their use in obtaining survey participation.

Survey Design

The Detroit Area Study (DAS) is an annual survey administered by the Department of Sociology at the University of Michigan for the purposes of social science research and the training of students in survey methods. The sample design is a multistage area-based probability sample of housing units in the Detroit tricounty area (Wayne, Macomb, and Oakland counties).

For the 1991 survey, a two-stage sample of area segments and households was selected by the Sampling Section of the University of Michigan's Survey Research Center. In the first stage, 82 area segments were selected with probabilities proportional to size.[1] In the second stage, 1,451 addresses were selected systematically from segment listings with probabilities inversely proportional to segment size to yield an overall equal probability of selection for addresses (Steeh 1991).

Random assignment of treatments to individual addresses was achieved in the following manner. Each randomly selected area segment was first divided in half geographically. In the first sampled segment, sample addresses in the first half received the incentive, while those in the second half did not. In the second sampled segment, sample addresses in the second half received the incentive, while those in the first half did not. This rotated sequence was repeated for all remaining segments. This procedure provided some geographic distance between treatment groups in order to reduce the likelihood that people living at nonrecipient sample addresses might learn of the incentive

1. Originally, there were 83 segments drawn to represent the tricounty area, but one was withdrawn because the area posed concern for the safety of interviewers.

from those living at nearby recipient sample addresses. At the same time, it controlled for any effects due to location and its socioeconomic and ethnic correlates. The design produced 727 sample addresses in the incentive group and 724 sample addresses in the no incentive group.

The incentive was a ballpoint pen of the type typically found in gift shops. It had a blue matte finish and was imprinted in gold with the words, "The University of Michigan," along with the seal of the University. The pen was gift-boxed and enclosed with the presurvey notification letter mailed to each sampled address in the incentive group. Attached to the gift-box was a note stating, "We are enclosing a gift as a small token of appreciation for participating in our survey." Enclosure of the pen required use of a slightly larger envelope and additional postage, resulting in a total incremental cost of $3.98 per sampled address ($3.75 for the pen and an additional $0.23 for postage). The text of the presurvey notification letter itself was identical in both the Pen and No Pen half samples. All Pen or No Pen letters to a given sampled segment were mailed at the same time, less than 1 week before expected initial interviewer contact.[2]

The data were collected during face-to-face interviews from late April through mid-August 1991. One English-speaking person age 18 years or older per household was selected at random using a respondent selection table (Kish 1965, pp. 398–401) and was asked to participate in the survey. Prior to contact with the household, interviewers were unaware of whether or not a sampled address was in the incentive group, since our desire was to isolate the effect of the incentive on respondents and not have it confounded with interviewer behavior.

The Survey Research Center routinely uses a variety of methods to minimize nonresponse due to noncontacts or refusals. The number of follow-up visits was limited only by the budget and the length of the field period. Attempts were made to recontact most refusals, except those of a hard-core or threatening nature, in an effort to obtain completed interviews.

The subject of the 1991 survey was "Collective Memories." A variety of questions asked respondents to recall significant events from recent history and to explain why they considered these events to be important. Other questions asked about respondents' identification of their generation and their attitudes regarding various social issues. A set of demographic and socioeconomic variables were also collected. Thus, a variety of open questions and closed questions were asked, and interviews averaged just under 60 minutes in length.

2. This could not be perfectly controlled, however. In cases that required multiple visits before contact was accomplished, a few weeks may have passed between then and the initial mailing.

Sample Disposition

RESPONSE RATES AND REFUSAL RATES

Of the original 1,451 selected addresses, 1,334 qualified as eligible housing units and, thus, produced either a completed interview with the randomly selected respondent (78.1 percent) or a nonresponse (21.9 percent), the latter involving refusals and other sample noninterviews. The remaining 117 addresses were classified as nonsample: vacant houses, incorrect addresses, nonresidential or seasonal housing, and non-English-speaking households.

The experimental findings are presented in table 1 along with associated Cochran-Mantel-Haenszel (CMH) tests. The CMH statistics (using a chi-square approximation) provide results that take into account the randomization of incentive receipt within segments, which eliminated some unwanted variation in assigning treatments to addresses (Schlesselman 1982).[3] The estimated odds ratios produced by the CMH test provide a measure of association controlling for segment. It should also be noted that because the CMH test is based on calculations within each segment, one at a time, it effectively takes into account the complex sample design, and further adjustment for clustering is not needed.

Our primary hypothesis about response rates was confirmed, as indicated in table 1. Those who received the incentive show a response rate nearly 5 percentage points higher than those who did not receive an incentive. The difference is significant at the .029 level when the CMH test is used to control for segment.

Furthermore, most of the 5 percentage point difference in the nonresponse rates is due to a difference of nearly 4 percentage points in refusals; the refusal rate in the Pen condition is 12.4 percent and in the No Pen condition, 16.1 percent ($p = .047$). The difference for other noninterview reasons (mainly noncontacts) is only 1.2 percent (7.0 percent for the Pen condition, 8.2 percent for the No Pen condition; data not shown in table 1). Thus it appears that the main effect of the incentive is to influence the willingness of people to grant an interview when they are contacted, although it may also have reduced slightly the tendency to avoid opening the door to an interviewer, which may account for the small difference in "other noninterviews." The source of refusals—whether from the selected respondent, from

3. The Cochran-Mantel-Haenszel statistic, for one segment, is equivalent to $[(n - 1)/n]\ \chi_p^2$, where χ_p^2 denotes the standard Pearson chi-square statistic. For more than one segment, the individual segment contributions are effectively weighted by the sample sizes in the segment. For our application of the CMH statistic, the individual segments are thus weighted by the number of cases in each segment.

someone prior to respondent selection, or from someone other than the respondent—did not differ by incentive group ($p > .90$).

NONSAMPLE RESULTS

Table 1 also presents an unexpected finding. The Pen condition appears to have produced a significantly higher nonsample result than the No Pen condition ($p = .009$). Thus, more addresses (9.8 percent) were classified as falling outside the eligible sample (mainly as vacant residences or nonexistent addresses) when a pen was sent to the address than when it was not (6.4 percent). It does not seem possible to explain this result as a direct effect of the incentive, since nobody was at a vacant residence or nonexistent address to receive the advance letter with or without the pen.

Our only substantive interpretation of this unexpected difference is that the letters containing the pen, which were packaged in larger envelopes and required more postage than the No Pen letters, may have been more readily returned by mail carriers to the sender in these cases. In addition, there is some evidence to suggest that some of the Pen nonsample cases, particularly the nonexistent addresses, were more readily confirmed by interviewers in the field, perhaps alerted by postal returns. Moreover, "not-at-home" noninterviews, which were slightly, though not significantly, more frequent in the No Pen condition, could have actually been vacant homes, leading to an understatement of the nonsample rate here. While each of these factors may have contributed to the observed nonsample difference, a replication of the experiment is needed to make certain that the nonsample result is reliable. Careful accounting of each nonsample disposition will then be necessary in order to determine how it occurred. If this unanticipated finding proves reliable, it has important practical implications for survey administration.

We also considered but rejected the possibility that the finding about the nonsample rate could have affected the predicted result for the response rate, for example, by shifting the classification of addresses between nonsample and nonresponse. However, the fact that it is the refusal component of nonresponse that produced the largest difference between the Pen and No Pen conditions argues against such an artifact. Thus, we believe that whatever the source of the unexpected nonsample difference, the effect of the pen incentive on response rate stands on its own.

INTERACTIONS WITH SOCIODEMOGRAPHIC VARIABLES

It would have been useful to have investigated interactions between the incentive effect on response rates and sociodemographic variables.

Table 1. Response Rates, Refusal Rates, and Nonsample Rates, by Incentive Group

	Total (%)	Pen (%)	No Pen (%)	Cochran-Mantel-Haenszel (CMH) Estimated Odds Ratio	CMH Test *p*-value
Total:					
Response rate[a]	78.1 (1,334)	80.6 (656)	75.7 (678)	1.34	.029
Refusal rate[b]	14.2 (1,334)	12.4 (656)	16.1 (678)	.73	.047
Nonsample rate[c]	8.1 (1,451)	9.8 (727)	6.4 (724)	1.71	.009
Detroit:					
Response rate[a]	76.7 (288)	77.5 (138)	76.0 (150)	1.08	.786
Refusal rate[b]	11.1 (288)	10.1 (138)	11.3 (150)	.92	.831
Nonsample rate[c]	15.8 (342)	19.3 (171)	12.3 (171)	1.85	.055

Suburbs:					
Response rate[a]	78.5	81.5	75.6	1.42	.020
	(1,046)	(518)	(528)		
Refusal rate[b]	15.1	12.7	17.4	.70	.038
	(1,046)	(518)	(528)		
Nonsample rate[c]	5.7	6.9	4.5	1.62	.072
	(1,109)	(556)	(553)		

NOTE. The numbers in parentheses are the base N for each rate calculation.

[a] $$\text{Response rate} = \frac{\#\text{ interviews}}{\#\text{ selected addresses} - \#\text{ ineligible addresses}} = \frac{\#\text{ interviews}}{\#\text{ interviews} + \text{refusals} + \text{noninterviews}}.$$

[b] $$\text{Refusal rate} = \frac{\#\text{ refusals}}{\#\text{ selected addresses} - \#\text{ ineligible addresses}} = \frac{\#\text{ refusals}}{\#\text{ interviews} + \text{refusals} + \text{noninterviews}}.$$

[c] $$\text{Nonsample rate} = \frac{\#\text{ ineligible addresses}}{\#\text{ selected addresses}} = \frac{\#\text{ vacant} + \#\text{ incorrect addresses} + \#\text{ nonresidential} + \#\text{ seasonal} + \#\text{ non-English speaking}}{\#\text{ selected addresses}}.$$

This would have required information on nonrespondents as well as respondents, which was impractical to obtain. At a later point, though, we do look at the sociodemographic composition of the Pen and No Pen responding samples and find no differences.

What we are able to do, however, is determine whether the incentive effect differed between city and surrounding suburbs (see table 1). The population of the city of Detroit was approximately 75 percent black in 1990, while the larger tricounty suburbs had a black population of only about 5 percent. Housing values in Detroit were approximately half those in the suburbs on the average, reflecting income differences of considerable magnitude. The effect of the pen on both the response and the refusal rates was noticeably higher in the suburbs than in the city. The relative odds of response were 42 percent higher among pen recipients in the suburbs as compared with only 8 percent among Detroit pen recipients, a fivefold difference in the effect. At the same time, the effect of the pen on the nonsample rate was slightly greater in the city than in the suburbs. The relative odds of a sample address being ineligible in Detroit were 85 percent higher in the Pen group than in the No Pen group, as compared with 62 percent in the suburbs.

However, none of the three-way interactions among the disposition variables of interest (nonsample, response, or refusal) with Pen/No Pen and city/suburbs approaches significance ($p > .10$). Thus we cannot be confident that the effect of the pen differs by location, though the fact that the incentive effect on response rates and refusal rates tends to be greater in the suburbs and the nonsample effect greater in the city of Detroit supports our belief that the two processes are quite likely independent.

Respondent Cooperation

Since the incentive increased the willingness of respondents to cooperate, it is useful to determine at what point the effect occurred. We expected that pen recipients would be more likely to grant an interview the first time an interviewer appeared at the door rather than putting it off or initially refusing, requiring later conversion. Table 2 presents completed interviews and cumulative response rates by number of interviewer visits for the Pen and No Pen conditions. Five percent more of the responding pen recipients granted an interview on the first visit than did respondents who had not received a pen. Although the mean number of visits did not differ by incentive condition (Pen = 4.3 vs. No Pen = 4.4, $t = 0.45$, $p = .653$) and the overall distributions in the table show no significant difference (likelihood ratio χ^2 ($df = 4$) $= 6.38$, $p = .173$), the expected difference between one and two or

Table 2. Percent of Respondents and Cumulative Response Rate Obtained at Each Interviewer Visit, by Incentive Group

Interviewer Visit	Percent of Respondents		Cumulative Response Rates (%)	
	Pen	No Pen	Pen	No Pen
1	21.9	17.0	17.7	12.9
2	18.1	21.2	32.2	28.9
3	14.2	12.3	43.7	38.2
4	9.5	12.5	51.3	47.7
5+	36.3	37.0	80.6	75.7
	100.0	100.0		
N	529	513	656[a]	678[a]

[a] Note that this is the base *N* for each response rate calculation—that is, the number of eligible addresses in the group.

more visits is confirmed (likelihood ratio χ^2 (df = 1) = 3.49, p = .062).[4] Thus the incentive appears to have had its greatest effect on the initial visit, resulting in a five-point increase in response rates that was maintained even on subsequent visits. That is, it appears that the boost in the response rate on the initial visit due to the incentive cannot be recouped simply by completing additional follow-up visits.

Further analysis of refusals by number of visits revealed a marginally significant tendency among incentive recipients to be more reluctant to refuse, requiring an average of 9.4 visits to achieve final disposition as a refusal, compared to 7.6 for nonrecipients (t = -1.72, p = .085). Refusals among nonrecipients tended to occur at earlier visits, overall, than among recipients (Wilcoxon Z = 1.59, one-tail p = .056).[5]

The Survey Research Center's policy of allowing repeated follow-up visits, limited only by time and budget constraints, likely contributed to these findings. For this reason, the cost savings from incentive use

4. The t-, Z-, and χ^2-statistics reported in this and subsequent sections were calculated using an estimated design effect equal to 1.175 as an adjustment factor in order to take into account the complex sample design. This design effect was calculated as the mean of 29 design effects computed by Willard Rodgers across a number of subgroups and reflecting a variety of analyses of these survey data.

5. An alternative interpretation is that, in some cases, interviewers, upon learning the identity of incentive recipients at an initial contact resulting in a refusal, may have tried harder to convert these nonrespondents by increasing the number of attempted contacts.

was not as great as might have been expected. However, it must be noted that this experiment was not set up to support separate cost analyses by incentive condition, and any such analysis at this point is ad hoc, relying heavily on assumptions. Nevertheless, it is clear that the incentive added less than $3,000 to data collection costs, but it resulted in a 5 percentage point increase in response rates.

Response Quality

Since methods used for nonresponse reduction may adversely affect measurement error, it is important to examine the data for effects on response quality. Any consideration of data quality must first recognize the possibility that the effect of the incentive on response rates might have altered the composition of the responding sample. For example, if the incentive encouraged disproportionate participation of individuals of high or of low educational level, this could have an indirect effect on answers related to education. However there are no differences approaching significance between the Pen and No Pen half samples in age, education, income, race, or gender ($p > .10$ in all cases).[6] The incentive seems to have operated in essentially the same way across the main social dimensions of the population. Thus any differences in response content or quality may be attributed to the effect of the incentive on respondents, rather than to the demographic types that became respondents.

Incentive recipients who responded exhibited a marginally significant tendency toward greater response completeness early in the interview.[7] They tended to identify more distinct items in response to the

6. Excluded from these and subsequent data quality analyses are 34 respondents who, toward the end of the survey period, were paid $10 for their participation. Since 16 of these cases were in the Pen group and 18 cases were in the No Pen group, response rates were not appreciably affected (Steeh 1991). However, their responses to survey questions may reflect systematic differences in their motivation. Also excluded were five partial interviews, all Pen recipients, with extremely limited usable data, leaving a sample size of 1,003 cases, 508 in the Pen group and 495 in the No Pen group. The finding that five partial interviews occurred in the Pen condition and none in the No Pen condition ($p < .05$) raises the possibility that incentive use may encourage participation by some well-meaning people who are nevertheless unable or unwilling to provide adequate answers to survey questions. However, results will show that, among interviews with complete data, quality is improved among pen recipients. Hence, even if pen receipt leads to slightly more partial interviews, we believe our major conclusion that incentive use encourages cooperation remains supported.

7. The experiment did not explicitly control for potential interviewer effects on data quality. The DAS utilizes both student and professional interviewers. Analysis shows that both types of interviewers were equally likely to have completed interviews with pen recipients as with nonrecipients. Thus statistical independence between incentive condition and interviewers is maintained. This, coupled with the fact that none of the

first substantive open question ($t = 1.85$, $p = .064$; see Question B1 in the Appendix), and they also appeared to describe these items more fully, using slightly more words (first item: 10.5 vs. 8.0 words, $t = 1.93$, $p = .054$; second item: 9.4 vs. 8.2 words, $t = 1.49$, $p = .136$).[8]

Despite the difference in response completeness, answers from recipients did not differ in content from those given by nonrecipients (first item: $p = .778$; second item: $p = .432$). Incentive recipients also did not appear to answer other substantive questions differently than nonrecipients (p-values ranged from .273 to .923 in comparisons of the response distributions on eight closed format attitudinal questions), nor were they more likely to adopt a response set that would have sped repetitive questioning to conclusion ($t = 0.12$, $p = .904$). Thus there appears to be no bias in responses due to the incentive.

Conclusions and Discussion

Face-to-face surveys have relied primarily on interviewers to persuade people to become respondents, and presumably for that reason have seldom provided incentives with presurvey letters in order to encourage cooperation. Our research shows that a prepaid nonmonetary incentive—a gift-type ballpoint pen included with an advance letter requesting cooperation—can have a noticeable effect on increasing the response rate. The effect seems to be mainly one of leading some respondents to grant an interview rather than to refuse, particularly on the interviewer's initial visit. Moreover, since interviewers in our experiment did not know the identity of incentive recipients, the observed effect of the incentive may be understated if interviewer behavior is altered by incentive use.

Our results suggest design considerations that may lead to cost reduction with incentive use. Since the effect of the incentive appears

interviewers had advance knowledge of incentive recipient identity, suggests that differences in recorded answers are unlikely to be associated with type of interviewer.

8. There is some evidence to suggest a statistical interaction such that the effect of the incentive on response completeness differed by education level of respondents (likelihood ratio χ^2 ($df = 6$) $= 12.31$, $p = .055$). Less educated respondents who received a pen tended to identify one item rather than none at all, but were as likely as nonrecipients to provide two answers. On the other hand, among better educated respondents, pen recipients and nonrecipients did not differ in the proportion choosing not to answer the question. However, better educated pen recipients showed a greater tendency than nonrecipients to identify two items rather than only one. Incentive recipients also used more words in answering than did nonrecipients, but the difference decreased as education level increased, raising speculation that less educated incentive recipients tried harder than they otherwise would have to answer a challenging open question early in the interview. Details of this analysis are available from Diane K. Willimack.

to occur largely at the time of the first visit, the need for costly follow-up visits is eliminated for these additional respondents. Also, since incentive recipients appear to have been more reluctant to refuse, additional cost savings would likely be realized by limiting the number of contact attempts. Furthermore, it is important to note that, unlike the trade-off between incentive use and follow-up contacts demonstrated in mail surveys (James and Bolstein 1990), an effect equivalent to that of the incentive apparently cannot be achieved simply through additional follow-up visits.

There is no reliable evidence of a change due to the incentive in the demographic composition of the final responding sample. However, the incentive effect on response and refusal rates shows a nonsignificant trend to be greater in the largely white suburbs than in a heavily black city.

In addition to reducing nonresponse, some effect of the incentive continues even after the interview begins, by stimulating fuller responses to open questions early in the questionnaire. This does not lead to differences in the content of responses, nor is there any evidence that the incentive adds to measurement error.

Our research produced one unexpected finding: mailing a pen incentive is associated with an increase in the number of addresses classified as nonsample (vacant residences and nonexistent addresses). This may have had to do with the way postal authorities handled a larger first-class package containing a material incentive. Postal returns may have alerted field staff to more reliably confirm nonsample addresses. Such a surprising nonsample difference, along with differences due to the incentive that were in line with predictions, deserves to be tested again. If the result is reliable, this has important implications for classification of addresses as nonsample.

Finally, our results are consistent with the social norm of reciprocity, which is typically set forth as the basis for incentive use in surveys (Groves 1989). Although it is difficult to know the mechanism through which the incentive operated, unconditional receipt of a token gift in the presurvey letter may have differentiated it from other mail, drawing attention to the survey request and legitimizing the interviewer's visit. Thereupon, incentive recipients, influenced by the general norm of reciprocity (Gouldner 1960), may have wished to repay the gift by responding to, rather than refusing, a personal request for survey participation. It is also possible that the unsolicited gift simply created a more positive view of the survey, which in turn affected a potential respondent's tendency to be helpful through survey cooperation (Isen 1987; Isen and Levin 1972). Our present research cannot distinguish between these two explanations, and there may be other explanations as well. We hope that future experimental work will contribute to

better understanding of the process through which incentives affect survey participation.

Appendix

DAS Survey Questions Examined for Response Completeness (Section B: Local Events)

B1. There have been a lot of events and changes over the past half century in the tricounty area of Wayne, Macomb, and Oakland counties, including the city of Detroit—say, from about 1930 right up until today. Would you mention one or two events or changes in the tricounty area that seem to you to have been especially important over the past 50 or so years?
B2. What was it about (1st event) that makes it seem to you especially important?
B3. What was it about (2nd event) that makes it seem to you especially important?

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