Public Opinion Quarterly, Vol. 73, No. 3, Fall 2009, pp. 537–550

REVISITING INCENTIVE EFFECTS
EVIDENCE FROM A RANDOM-SAMPLE MAIL SURVEY ON CONSUMER PREFERENCES FOR FUEL ETHANOL

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Abstract This study revisits the issue of monetary incentive effects utilizing data from a mail survey sent to a random sample of adults across the United States regarding preferences for fuel ethanol. The results reported here are consistent with those found in the literature regarding the effect of incentives on response rates: they improved them, with prepaid incentives performing relatively better. We also found that state of residence was significantly correlated with choosing whether to respond to a survey. Regarding the effect of incentives on sample composition, we found that incentives tended to bias the sample in favor of less educated respondents, and tended to attract respondents less familiar with the survey subject. Finally, results indicate that incentives had very little effect on item nonresponse. Instead, item nonresponse was driven by education level, gender, and familiarity with the survey subject. However, combining the findings on sample composition with those of item nonresponse, it appears that the use of incentives *indirectly* affects item nonresponse by recruiting relatively more respondents that are less educated and/or less familiar with the survey topic, who are then less likely to respond to all questions.

Introduction

This research note revisits the issue of monetary incentive effects on survey response rate, sample composition, and item nonresponse. These issues

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doi:10.1093/poq/nfp038 Advance Access publication July 21, 2009

have been addressed in the literature before: response rates (Peck and Dresch 1981; Berk et al. 1987; Fox, Crask, and Kim 1988; Yammarino, Skinner, and Childers 1991; Church 1993; Singer, Van Hoewyk, and Maher 2000; Ritter et al. 2005), sample composition (Peck and Dresch 1981; Shettle and Mooney 1999; Singer, Van Hoewyk, and Maher 2000; Groves, Presser, and Dipko 2004), and item nonresponse (Hansen 1980; Peck and Dresch 1981; Berk et al. 1987; Shettle and Mooney 1999; Singer, Van Hoewyk, and Maher 2000; Davern et al. 2003). In addition to providing additional evidence common to these issues, our analysis makes two additional contributions. First, we test whether geography plays a role in an individual's choice of whether to respond to a survey, and, second, we explicitly model respondent familiarity with the survey topic and respondent satisfaction with the survey itself to test if these have any significant effects on sample composition and item nonresponse. This study utilized data from a mail survey sent to a random sample of adults across the United States regarding preferences for fuel ethanol. Two incentive treatments were tested against a no-incentive control group: a $1 prepaid and a $5 postpaid incentive.

Survey Design and Data

A 10-page, 52-question contingent valuation (CV) mail survey was sent to a stratified (weighted by state population) random sample of 3,000 persons from all 50 U.S. states and Washington, D.C. The objective was to obtain data on consumer preferences for two ethanol-based fuels: E10, a combination of 10 percent ethanol and 90 percent regular gasoline, and E85, a combination of 85 percent ethanol and 15 percent gasoline. Surveys were mailed in April 2007, followed by a reminder letter two weeks later, and then a second mailing of the survey two weeks after that. Along with the survey, a personalized letter and postage-paid return envelope were included in each mailing. The survey had seven sections, with each section containing approximately seven questions, addressing household demographics, household vehicle fuel expenditures, familiarity with E10 and related fuels, specific environmental, economic, and national security issues related to ethanol production and use, and future E10 and E85 purchases. A complete version of the survey is available as an online appendix (please see the supplementary data online).

Incentive Effect on Response Rate

Dillman (2007) states that the use of monetary incentives can be effective in increasing response rates. For more detail on theoretical and other reasons for explaining this phenomenon, see Porter (2004). For the purposes of this analysis, our sample was subdivided into three independent stratified samples of

Table 1. Total Surveys Sent, Returned, and Response Rate for Each Incentive Treatment

Incentive	Total surveys sent	Total surveys returned	Response rate
$0	1,000	202	0.20
$1 prepaid	1,000	311	0.31[a,b]
$5 postpaid	1,000	235	0.24[c]
Total	3,000	748	0.25

NOTE.—Pearson's chi-squared statistic = 33.39 ($p = .00$).

[a]Null hypothesis that response rates for $0 and $1 prepaid groups are equal is rejected in a one-tailed pairwise t-test at $p = .01$ level.

[b]Null hypothesis that response rates for $1 prepaid and $5 postpaid groups are equal is rejected in a two-tailed pairwise t-test at $p = .01$ level.

[c]Null hypothesis that response rates for $0 and $5 postpaid groups are equal is rejected in a one-tailed pairwise t-test at $p = .05$ level.

1,000 persons based on respondent incentive: one-third of the sample received a $1 bill along with the survey (i.e., prepaid); one-third received a promise of a $5 bill upon return of the survey (i.e., postpaid); and one-third was given no incentive. Of the 3,000 surveys sent out, 748 were returned. We define response rate here using Response Rate 2 (RR2) as defined by the American Association for Public Opinion Research (2008), which counts both complete and partial interviews as respondents. Table 1 presents the response rate for each incentive treatment. Pearson's chi-squared statistic was calculated to test the null hypothesis that response rate was independent of incentive treatment, and was rejected. Additionally, pairwise null hypotheses were tested that response rates for the with-incentive treatments were equal to those of the no-incentive treatment, respectively, versus the alternative hypothesis that response rates were statistically higher for the with-incentive treatments. The null hypothesis was rejected for both the $1 prepaid and $5 postpaid treatment. Additionally, the null hypothesis that response rate for the $1 prepaid treatment was equal to that of the $5 postpaid treatment was rejected.

As an alternative, we also tested for incentive effects parametrically by estimating a probit regression model, using response as the dependent variable (i.e., $y = 1$ if survey returned, $y = 0$ otherwise), and incentive treatment as independent binary variables. This method also allowed us to incorporate the only other datum on respondents available: state of residence. Given this type of data, we adopted a random-effects probit model (Greene 2007), using state of residence as the group variable. The model was estimated in Stata (StataCorp 2008) using the "xtprobit" module. Table 2 contains the results of this estimation. The null hypothesis that the response rate among those assigned to the $1 prepaid incentive was not significantly different from those in the control group was rejected at the $p = .01$ level, and that of the respondents assigned to the $5 postpaid incentive, at the $p = .05$ level, in favor of the alternative

Table 2. Results of Random-Effects Probit Estimation for Survey Response

	Coef.	Std. err.	z	Marg. effects
\$1 prepaid	0.345[a]	0.061	5.61	0.115
\$5 postpaid	0.113[b]	0.063	1.79	0.037
Intercept	−0.807[c]	0.052	−15.46	
Rho	0.017	0.009		
LR test of rho = 0; chi-squared statistic = 9.86 ($p = .001$)				
Log likelihood = −1663.38 ($n = 3{,}000$)				
Wald's chi-squared statistic = 33.46 ($p = .0000$), groups = 38				

[a]Null hypothesis that the coefficient is equal to 0 is rejected in a one-tailed test at $p = .01$ level.
[b]Null hypothesis that the coefficient is equal to 0 is rejected in a one-tailed test at $p = .05$ level.
[c]Null hypothesis that the coefficient is equal to 0 is rejected in a two-tailed test at $p = .01$ level.

hypothesis that the coefficient is strictly positive. Marginal effects indicate that the presence of a \$1 prepaid or a \$5 postpaid incentive increased response rate 12 and 4 percentage points, respectively, relative to the no-incentive case. Results also indicate that state of residence was significant, as evidenced by the result of the likelihood-ratio test of the null hypothesis that rho = 0. The null hypothesis was rejected at the $p = .01$ level. Although we have no way of explaining why this result might have occurred, it does indicate that, for whatever reason, residents of the same state tend to choose to respond (or not) to surveys in a similar fashion.

In general, these results are consistent with the findings of Fox, Crask, and Kim (1988); Hansen (1980); Ritter et al. (2005); Shettle and Mooney (1999); and Yammarino, Skinner, and Childers (1991) that use of incentives increases response rate. More specifically, these results are consistent with the findings of Peck and Dresch (1981), who found that both prepaid and postpaid incentives increase response rates. These results contrast, however, with the findings of Berk et al. (1987); Church (1993); and Singer, Van Hoewyk, and Maher (2000) who found that postpaid incentives had no significant positive impact on response rates.

Incentive Effect on Sample Composition

The following analysis is based on the 748 returned surveys. Demographic data were collected on each respondent as well as data on respondent familiarity with ethanol and overall satisfaction with the survey. Table 3 contains distribution of respondent demographic, subject familiarity, and respondent satisfaction variables by incentive treatment. Pearson's chi-squared and/or Kruskal–Wallis (for variables that can also be considered ordinal) statistics were calculated to test the null hypotheses that proportions were independent of incentive

Table 3. Respondent Proportions by Demographic and Survey-Specific Indicators for Each Incentive Treatment

Variable description		*n*	Incentive treatment		
			$0	$1 prepaid	$5 postpaid
Age	(Mean)	573	59.16	58.88	57.54
City type	Rural/farm	655	0.20	0.18	0.18
	<10,000		0.15	0.15	0.13
	10,000–50,000		0.31	0.30	0.32
	>50,000		0.34	0.36	0.38
Education[†,*]	H. S. or less	662	0.20	0.28	0.27
	Some college		0.33	0.32	0.22
	Bachelor's		0.24	0.23	0.26
	Adv degree		0.23	0.17	0.26
Gender	Male	664	0.73	0.65	0.69
Household size	1	662	0.20	0.18	0.23
	2		0.50	0.46	0.40
	3		0.10	0.16	0.19
	4		0.11	0.12	0.12
	5+		0.09	0.08	0.06
Household vehicles	0	658	0.02	0.03	0.04
	1		0.21	0.25	0.25
	2		0.47	0.47	0.41
	3		0.17	0.16	0.19
	4+		0.13	0.10	0.12
Income	$0–19,999	523	0.10	0.08	0.08
	$20,000–39,999		0.11	0.19	0.18
	$40,000–59,999		0.26	0.21	0.21
	$60,000–79,999		0.18	0.15	0.17
	$80,000–99,999		0.07	0.13	0.11
	$100,000+		0.28	0.24	0.25
Political preference	Conservative	523	0.50	0.46	0.41
	Liberal		0.35	0.30	0.39
	Moderate		0.15	0.24	0.20
Race	White	639	0.90	0.90	0.91
E10 locally available	Yes	661	0.21	0.18	0.14
	No		0.51	0.55	0.61
	Not sure		0.28	0.27	0.25
Familiarity with E10[†,*]	Very	665	0.19	0.12	0.11
	Somewhat		0.51	0.51	0.62
	Not at all		0.31	0.37	0.27
Have read about ethanol[††,*,#]	Many times	661	0.53	0.47	0.49
	A few times		0.40	0.40	0.47
	Once/Never		0.06	0.13	0.04

Continued

Table 3. Continued

Variable description		*n*	Incentive treatment		
			\$0	\$1 prepaid	\$5 postpaid
Age	(Mean)	573	59.16	58.88	57.54
More satisfaction from E10	Yes	657	0.42	0.47	0.49
	No		0.26	0.23	0.20
	Not sure		0.31	0.30	0.31
Survey captures opinion	Yes	658	0.53	0.55	0.56
	No		0.15	0.18	0.14
	Not sure		0.33	0.28	0.30
Satisfaction with survey	Very much	655	0.43	0.39	0.39
	Somewhat		0.52	0.57	0.55
	Not at all		0.05	0.05	0.06

*Null hypothesis that proportions equal between \$0 and \$1 prepaid treatments rejected in two-tailed pairwise *t*-test at $p = 0.05$ level.

#Null hypothesis that proportions equal between \$1 prepaid and \$5 postpaid treatments rejected in two-tailed pairwise *t*-test at $p = 0.05$ level.

††,†Null hypothesis of independence rejected in Pearson's chi-squared test at $p = 0.01$, $p = 0.05$ levels, resp.

treatment. The null hypothesis was rejected for education, familiarity with E10, and number of times reading about ethanol only. As shown in the table, the no-incentive and \$1 prepaid treatments had a plurality of respondents with "some college," followed by those with a bachelor's degree or those with a high-school diploma or less, respectively. The \$5 postpaid treatment, however, had a plurality of respondents with a high-school diploma or less, followed by those with a bachelor's or advanced degree. Viewed another way, the no-incentive group had the smallest proportion of the least-educated respondents, whereas the \$1 prepaid treatment had the smallest proportion of the most-educated respondents. However, based on pairwise tests of equivalence of the means, these differences were statistically significant when comparing the no-incentive treatment to the \$1 prepaid treatment only. No other demographic indicators were found to be statistically significant across treatments.

As table 3 shows, the \$5 postpaid treatment had the lowest proportion of respondents self-described as "very familiar," followed by the \$1 prepaid treatment. The no-incentive treatment had the highest proportion of "very familiar" respondents. Furthermore, the \$1 prepaid treatment had the highest proportion of respondents who had read about ethanol "once or never," and the no-incentive treatment had the highest proportion of respondents who had read about ethanol "many times." Based on pairwise tests of equivalence of the means, these differences are statistically significant only when comparing the

no-incentive treatment to the $1 prepaid treatment, and when comparing the $1 prepaid to the $5 postpaid treatments for the reading variable only.

These results indicate that the use of either the $1 prepaid or $5 postpaid incentive may influence sample composition. However, the null hypothesis that respondent satisfaction with the survey was independent of incentive treatment could not be rejected. These findings contradict those of Shettle and Mooney (1999) and Singer, Van Hoewyk, and Maher (2000), who found that the use of a prepaid incentive did not affect sample composition in terms of demographics, but are consistent with those of Groves, Presser, and Dipko (2004), who found that respondents interested in the survey topic were more likely to respond, but that monetary incentives mitigated this effect by attracting otherwise uninterested respondents.

Incentive Effect on Item Nonresponse

The following analysis is based on the 748 returned surveys as well. Each survey question was recoded as "1" if a response was recorded and "0" otherwise. Table 4 contains the proportions of item nonresponse for each survey question by incentive treatment. The null hypothesis that item nonresponse was independent of incentive treatment was rejected for 15 out of 56 questions, or roughly one-fourth of the total. For all questions, the $5 postpaid treatment resulted in the lowest proportion of item nonresponse, and in most cases, the $1 prepaid treatment resulted in the second-lowest proportion of item nonresponse. Based on pairwise tests of equivalence, however, the difference between the no-incentive and $1 prepaid treatment was not significant with one exception. The difference between the no-incentive and $5 postpaid incentive was significant for 41 questions, or roughly three-fourths of the total. Additionally, the difference between the $1 prepaid and $5 postpaid treatments was significant for 30 questions, or just over half of the total.

Thus, on the surface, results indicate that the $5 postpaid incentive significantly reduced item nonresponse, which would contradict previous findings. However, taking a cue from the results shown in table 4, we estimated a probit regression model for each question, with item response as the dependent variable ($y = 1$ if the question was answered, $y = 0$ otherwise). We included each incentive treatment (with "no incentive" as the omitted one), the number of times the respondent had read about ethanol (as a proxy for subject familiarity), gender, and education level as independent variables. Other demographic variables, including age, income, and race, were precluded from the models due to insufficient variation on item nonresponse. In other words, the age and income questions were the most frequently not answered; consequently, there were too few observations on which to test relationships. Race could not be tested sufficiently because non-whites were underrepresented in the sample to begin with. Additionally, it should be noted that education and subject

Table 4. Item Nonresponse Proportions for Each Survey Question by Incentive treatment and Results of Significance Tests ($n = 748$)

Question number and topic	Incentive treatment			Question number and topic	Incentive treatment		
	$0	$1 prepaid	$5 postpaid		$0	$1 prepaid	$5 postpaid
1. HH size	0.14	0.12	0.09[a]	25. Willingness to pay for E85	0.20	0.23	0.14[a,b,c]
2. No. of vehicles	0.15	0.13	0.09[a]	26. Follow-up question	0.51	0.54	0.46[d]
3. Paying for fuel	0.17	0.14	0.12	27. Flexible fuel update	0.41	0.41	0.33[a,d]
4. Recently paid price	0.17	0.13	0.12	28. Policy support	0.18	0.21	0.12[a,b,c]
5. Current gasoline situation	0.19	0.15	0.12[a]	29. Gender	0.14	0.12	0.07[b,d,e]
6. Zip code	0.15	0.13	0.09[a]	30. Citizenship status	0.14	0.12	0.07[b,d,e]
7. Locality	0.16	0.14	0.08[b,d,e]	31. Ethanol information	0.15	0.12	0.08[a]
8. Familiarity with E10	0.14	0.12	0.08[a]	32. Sources of Information	0.15	0.13	0.08[d,e]
9. Local availability of E10	0.14	0.13	0.09[a]	33. Education level	0.15	0.12	0.07[b,d,e]
10.1. General environmental impact	0.17	0.16	0.11[a]	34. Specialized degree (if applicable)	0.51	0.53	0.44[d]
10.2. General economic impact	0.18	0.16	0.12	35. Race	0.16	0.13	0.08[b,d,e]
10.3. General impact on national security	0.19	0.17	0.14	36. Age	0.19	0.13	0.09[d,e,f]
11. E10 versus regular gasoline	0.15	0.13	0.09[a,d]	37. Lifelong resident	0.20	0.14[g]	0.09[e,f]
12. E10 versus fuel made up of >10% ethanol	0.14	0.13	0.09[a]	38. Income	0.22	0.17	0.12[b,e]
13.1. Specific economic impact_q1	0.15	0.14	0.09[a,d]	39. Reason to support alternative fuel	0.22	0.17	0.14[a]
13.2. Specific economic impact_q2	0.16	0.15	0.09[a,d]	40. Voting eligibility	0.19	0.14	0.08[d,e,f]
13.3. Specific economic impact_q3	0.15	0.14	0.09[a,d]	41.1. Presidential vote (if applicable)	0.19	0.18	0.14
14. Specific environmental impact_q1	0.15	0.13	0.08[b,d,e]	41.2. State Governor vote (if applicable)	0.19	0.18	0.15
15. Specific environmental impact_q2	0.16	0.15	0.08[b,c,e]	41.3. US Senator vote (if applicable)	0.19	0.18	0.15
16. Specific environmental impact_q3	0.14	0.14	0.09[d]	42. Self-described political orientation	0.21	0.15	0.11[b,e]

Continued

Table 4. Continued

Question number and topic	Incentive treatment			Question number and topic	Incentive treatment		
	$0	$1 prepaid	$5 postpaid		$0	$1 prepaid	$5 postpaid
17.1. Spec. impact on national security_q1	0.14	0.13	0.08[a,d]	44. Fuel purchases[h]	0.22	0.19	0.14[d,e]
17.2. Spec. impact on national security_q2	0.14	0.13	0.08[a,d]	45.1. Dollars spent on fuel	0.49	0.52	0.52
17.3. Spec. impact on national security_q3	0.15	0.13	0.09[a]	45.2. Dollars spent on fuel	0.63	0.67	0.66
18. E10 awareness question 1	0.14	0.14	0.09[d]	45.3. Dollars spent on fuel	0.85	0.89	0.84[d]
19. E10 awareness question 2	0.14	0.16	0.11[d]	46.1. Number of fill-ups	0.48	0.45	0.38[a]
20. Willingness to pay for E10	0.14	0.15	0.09[d]	46.2. Number of fill-ups	0.59	0.59	0.56
21. Choice certainty	0.17	0.18	0.11[a,d]	46.3. Number of fill-ups	0.83	0.87	0.83
22. Follow-up question	0.43	0.41	0.33[a,d]	C1. Overall satisfaction with survey	0.15	0.13	0.1[a]
23. Choice certainty of follow-up question	0.42	0.40	0.32[a,d]	C2. Survey effective to capture opinion	0.15	0.12	0.09[a]
24. E85 awareness question	0.16	0.18	0.11[a,b,c]				

[a]Null hypothesis that proportions are equal between $0 and $5 postpaid treatments is rejected in a one-tailed pairwise *t*-test at $p = .05$ level.
[b]Null hypothesis of independence is rejected in Pearson's chi-squared test at $p = .05$ level.
[c]Null hypothesis that proportions are equal between $1 prepaid and $5 postpaid treatments is rejected in a one-tailed pairwise *t*-test at $p = .01$ level.
[d]Null hypothesis that proportions are equal between $1 prepaid and $5 postpaid treatments is rejected in a one-tailed pairwise *t*-test at $p = .05$ level.
[e]Null hypothesis that proportions are equal between $0 and $5 postpaid treatments is rejected in a one-tailed pairwise *t*-test at $p = .01$ level.
[f]Null hypothesis of independence is rejected in Pearson's chi-squared test at $p = .01$ level.
[g]Null hypothesis that proportions are equal between $0 and $1 prepaid treatments is rejected in a one-tailed pairwise *t*-test at $p = .05$ level.
[h]Question 43 is omitted due to complexity of the question structure.

Table 5. Selected Probit Coefficient Estimates for Item Nonresponse Analysis

	Incentive treatment				
Question number and topic	$1 prepaid	$5 postpaid	Read	Female	Educ
2. No. of vehicles					0.209[a]
3. Paying for fuel			0.216[a]		
8. Familiarity with E10					0.262[a]
9. Local Availability of E10					0.222[a]
10.1. General environmental impact				0.374[a]	0.183[a]
10.2. General economic impact				0.369[a]	0.177[a]
10.3. General impact on national security			0.199[a]	0.404[a]	0.142[a]
13.1. Specific economic impact_q1					0.362[b]
13.2. Specific economic impact_q2					0.414[b]
13.3. Specific economic impact_q3					0.371[b]
14. Specific environmental impact_q1		0.702[c]			0.446[b]
15. Specific environmental impact_q2					0.245[a]
17.3. Specific impact on national security_q3					0.261[a]
18. E10 awareness question 1					0.420[b]
19. E10 awareness question 2	−0.611[c]				0.271[b]
20. Willingness to pay for E10	−0.511[c]			0.441[a]	0.250[a]
21. Choice Certainty					0.248[b]
22. Follow-up question		0.267[c]		0.339[b]	
23. Choice certainty of follow-up question		0.233[c]		0.332[b]	
24. E85 awareness question					0.367[b]
25. Willingness to pay for E85					0.343[b]
27. Flexible fuel update			0.176[a]		

Continued

Table 5. Continued

Question number and topic	Incentive treatment		Read	Female	Educ
	$1 prepaid	$5 postpaid			
28. Policy support	−0.465[c]				
29. Gender		0.394[d]	Omitted	Omitted	Omitted
31. Ethanol information			Omitted		0.324[a]
34. Specialized degree (if applicable)					1.668[b]
36. Age	0.480[c]	0.538[c]		0.485[a]	
37. Lifelong Resident	0.403[c]	0.436[c]		0.392[a]	
40. Voting eligibility		0.600[c]			
41.1. Presidential vote (if applicable)				0.333[a]	0.158[a]
41.2. State Governor vote (if applicable)					0.192[b]
41.3. US Senator vote (if applicable)				0.355[a]	0.186[b]
42. Self-described political orientation					0.177[a]
44. Fuel purchases			0.179[a]		
45.2. Dollars spent on fuel				0.337[b]	
46.1. Number of fill-ups					0.142[b]
46.2. Number of fill-ups (2nd vehicle)				0.527[b]	0.184[b]
C2. Survey effectiveness to capture opinion			−0.335[a]		

NOTE.—Only significant coefficients are reported.
[a]Null hypothesis that the coefficient is equal to 0 is rejected in a two-tailed test at $p = .05$ level.
[b]Null hypothesis that the coefficient is equal to 0 is rejected in a two-tailed test at $p = .01$ level.
[c]Null hypothesis that the coefficient is equal to 0 is rejected in a one-tailed test at $p = .05$ level.
[d]Null hypothesis that the coefficient is equal to 0 is rejected in a one-tailed test at $p = .01$ level.

familiarity were positively correlated, but not to the extent that the model was compromised, and results were substantially the same whether one or both were included.

This approach allowed us to determine if any other factors influenced item nonresponse other than incentive. All models were estimated in Stata (StataCorp 2008) using the "probit" module. Table 5 contains the results. For brevity and ease of interpretation, we suppress all extraneous regression output; only coefficient values and statistical significance levels for variables found to be significantly different from 0 are reported. Thus, rows are omitted for questions for which there were no significant variables. As is clear from the table, the $1 prepaid incentive reduced item nonresponse significantly in only five instances, and the $5 postpaid, in seven instances. Results were dominated by the education variable: item nonresponse was significantly negatively related to education level (i.e., that this factor made a higher contribution to the likelihood of a response). Additionally, for several questions, gender and familiarity with ethanol was significant. Females were more likely to respond to questions, as were those respondents who had read more about ethanol. Thus, although nonparametric results indicated that incentive effect was a clear determinant of the likelihood of item nonresponse for as many as three-fourths of all questions, these latter results indicate that its effect was much more limited, having a significant impact for only a handful of questions, and that the effect was more likely (and more often) driven by differences in education level, and, to a lesser extent, gender and subject familiarity.

Given the latter analysis, our results are indeed consistent with the findings of Davern et al. (2003); Peck and Dresch (1981); Shettle and Mooney (1999); and Singer, Van Hoewyk, and Maher (2000), who found no effect of incentive on item nonresponse. There is no evidence, however, to support Hansen's (1980) finding that incentives actually increase item nonresponse. Additionally, these results call into question the findings of Berk et al. (1987), who found that a prepaid incentive reduced item nonresponse, because it does not appear that they controlled for other factors, such as demographics.

Summary and Conclusions

The results reported here are consistent with those found in the literature regarding the effect of incentives on response rates: they improve them, with prepaid incentives performing relatively better. We also found that, for whatever reason, residents of the same state tend to choose to respond to a survey in similar fashion.

Regarding effect of incentives on sample composition, we found some evidence that incentives tended to bias the sample in favor of less educated respondents; we also found that the prepaid incentive resulted in the lowest proportion of highly-educated respondents. This result is in contrast, somewhat, to the

literature, which indicated that the use of incentives has no significant effect on sample composition.

We also found that incentives tended to attract respondents less familiar with the survey subject; this can be interpreted to mean that use of an incentive attracts those otherwise "uninterested" respondents. This result can have substantial implications for contingent-valuation (CV) surveys, the object of which is to estimate values for non-market goods using hypothetical willingness-to-pay questions. It is held that most CV value estimates are probably biased upward because those that respond to the survey are typically those interested in (and in favor of) the subject. Thus, it is typical for nonresponses to be counted as "no" votes when calculating a lower-bound estimate. The results here indicate that the use of incentives may mitigate this issue because, as Groves, Presser, and Dipko (2004) state, "surveys with monetary incentives should show lower tendencies for the "interested" to respond at higher rates than others" (p. 26).

Our results indicate that incentives have a limited effect on item nonresponse, being driven more often by other factors; in our case, education level, gender, and familiarity with the survey subject. However, combining the findings on sample composition with those of item nonresponse, it appears that the use of incentives *indirectly* affects item nonresponse by recruiting relatively more respondents that are less educated and/or less familiar with the survey topic, who are then less likely to respond to all questions.

Possible confounding factors remain that were not explicitly tested here and may call into question the validity and/or robustness of these findings. The first is instrument readability. Our instrument was scored at a ninth-grade reading level, based on the Flesch-Kincaid Grade Level score. In their analysis of U.S. voter ballots, Kimball and Kropf (2005) report that the reading level of their ballots ranged from 4th to 12th grade, with an 8th-grade average. Admitting voter ballots as reasonable benchmarks, this anecdotal evidence indicates that our survey might have been somewhat difficult for the average respondent, and this may at least partially explain the effects presented here.

Related to readability is the context of the survey subject, which may be particularly obscure to less educated and less familiar respondents, and thus had a relatively larger impact on response rates than normal. The fact that education level and subject familiarity were significant supports this argument. Context may also explain the significance of state of residence. Preferences for ethanol may follow regional patterns according to areas of corn and ethanol production and/or consumption. This effect would be consistent with the findings of Groves, Presser, and Dipko (2004), who found higher cooperation rates among respondents interested in the topic.

Finally, the results shown here may be a function of the magnitude of the incentive payments, which, for this study, were relatively small, and consequently, perhaps not robust. One would expect response rates to favor the higher dollar promised incentive relative to the prepaid incentive, all else equal, as the

difference between them increased. However, little evidence exists in the literature on this possibility. Although Church (1993) found a strong correlation between response rate and incentive magnitude, Fox, Crask, and Kim (1988) found that there were decreasing marginal improvements. These last issues are left for further research.

Supplementary Data

Supplementary data are available online at http://poq.oxfordjournals.org/

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