

### Incentives for survey participation: research on intended and unintended consequences ; second annual Wildenmann lecture

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Veröffentlichungsversion / Published Version

Zeitschriftenartikel / journal article

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#### Empfohlene Zitierung / Suggested Citation:

Singer, E. (1998). Incentives for survey participation: research on intended and unintended consequences ; second annual Wildenmann lecture. *ZUMA Nachrichten*, 22(42), 7-29. <https://nbn-resolving.org/urn:nbn:de:0168-ssoar-208276>

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# INCENTIVES FOR SURVEY PARTICIPATION: RESEARCH ON INTENDED AND UNINTENDED CONSEQUENCES<sup>\*)</sup>

## SECOND ANNUAL WILDENMANN LECTURE

*Eleanor Singer*

I am honored to have been invited to give the second Rudolph Wildenmann lecture, following in the distinguished footsteps of Norman Bradburn last year. Although the topic of my talk is perhaps a little specialized, it is in the spirit of empirical social research, especially survey research, which Professor Wildenmann did so much to foster.

As you all know, sample surveys are increasingly used by government and the private sector to inform policy decisions in a wide variety of areas. They are also indispensable for theory building and theory testing in the social sciences.

But while reliance on the products of survey research is growing, the difficulty of obtaining cooperation with sample households in the United States and other developed countries is increasing as well (de Heer and Israëls, 1992). Even though the overall response rates of surveys may be relatively constant over time (Smith, 1995), the component of nonresponse due to refusals appears to be increasing unless efforts by survey organizations are substantially increased (Groves and Couper, 1996).

One of the strategies increasingly being used by survey organizations is the payment of monetary incentives. But payment of such incentives, either at the outset of the survey or in an effort to convert refusals later, raises a variety of long-term issues for survey organizations.

Shortly before I came to the Institute for Social Research at the University of Michigan in the fall of 1994, Professor James House, the Director of the Survey Research Center, asked Bob Groves and me to organize a research seminar that would discuss some of these issues. Following presentations by four distinguished speakers, including Barbara Bryant, a former director of the U.S. Census Bureau, and Tom Juster, an economist and

Senior Research Scientist at ISR, several of us met in an effort to formulate guidelines for an SRC policy on the payment of incentives to respondents. One of the few things the panel agreed on was the need for continued research, both on the effects of incentives and on public perceptions of, and reactions to, them.

Today's talk is about what I think we have learned since then about the role of incentives in motivating survey participation, at least in the United States. It remains to be seen whether the findings will generalize to other countries, with different traditions and different social norms. My hope is that this talk will stimulate a similar program of research at ZUMA, and elsewhere.

I apologize that this talk is in English rather than in German. But I am going to try to tell you a story with pictures, and those of you who are interested can read the lecture at your leisure. The lecture has three parts:

1. First, it reviews what is known about the effect of incentives on response rates in mail surveys, and describes recent research on how they affect face-to-face and telephone surveys.
2. Second, it discusses issues of equity that arise in connection with refusal conversion payments, and reviews recent research on how respondents perceive such issues and whether or not these perceptions affect their willingness to participate.
3. Third, it describes some empirical evidence on whether or not the payment of monetary incentives is having long-term effects on the motivation for survey participation.

Another way of stating this is to say that the lecture deals with *intended and unintended* consequences of paying incentives to respondents.

From the outset, I want to make it clear that I will be talking about a program of research involving the collaboration of many people, in particular my colleagues Bob Groves, Trivellore Raghunathan, John Van Hoewyk, and my research assistant, Amy Corning. The National Science Foundation and the Survey Research Center have provided indispensable financial support.

## **What Do We Know about How Incentives Work?**

### **Effects in Mail Surveys**

From a meta analysis of the effect of incentives on mail survey response rates (Church, 1993) we know that:

- 1) Prepaid incentives yield higher response rates than promised incentives;
- 2) Prepaid monetary incentives yield higher response rates than gifts;

- 3) Response rates increase with increasing amounts of money, although not all studies find a linear relationship;
- 4) Incentives promised after return of the questionnaire do not significantly increase response.

### **Effects in Telephone and Face-to-Face Surveys<sup>1)</sup>**

Initial incentives--that is, incentives offered to all respondents at the outset of a survey--are also being used increasingly in telephone and face-to-face surveys, but we know much less about their effectiveness in these modes. Comparison of the respondent's situation in mail and interviewer-mediated surveys suggests that, other things being equal, the need for a monetary incentive should be greater in mail surveys. This conclusion is based on the persuasive value generally attributed to the interviewer, as well as on the fact that the presence of an interviewer lessens the respondent's burden in completing the questionnaire. Thus, it seemed possible that the significantly positive effects of incentives documented for mail surveys would not be found in the case of interviewer-mediated surveys.

Nancy Gebler, John Van Hoewyk, Trivellore Raghunathan, Kate McGonagle and I brought together and quantitatively analyzed all the experimental studies we could locate (as of 1997) which used incentives in face-to-face and telephone surveys--37 studies in all--each containing two, and sometimes more, experimental conditions (Singer et al., forthcoming). We attempted to answer four basic questions:

- Do incentives improve response rates in telephone and face-to-face surveys? By how much?
- Are prepaid incentives more effective than promised incentives?
- Is money more effective than a gift?
- What is the effect of burden?

For each experimental condition in each of the 37 studies, the following independent variables were coded:

- Amount of incentive
- Type of incentive (gift or money)
- Timing of incentive (paid before interview taken, or promised)
- Burden (coded as high if interview was longer than an hour or if any of the following were present: diary, test, sensitive questions, panel study; otherwise coded as low)

The dependent variable was the difference in response rates between the zero incentive condition (or the lowest incentive condition) and each higher incentive condition in the

same experiment. So, for example, if a study had three experimental conditions--a zero-incentive condition, a \$5 incentive, and a \$10 incentive--we computed two dependent variables for that experiment: the difference in response rate between the zero-incentive and the \$5 condition, and the difference between the zero-incentive and the \$10 condition. These differences were weighted by the inverse of the variance of the response rate difference in the analysis.

In order to eliminate confounding due to differential cooperation tendencies, we introduced a covariate which we call "panel status" into all the analyses reported below. Panel status simply refers to whether the subjects in the experiment were new respondents, nonrespondents to the current or a prior wave, or respondents to a previous wave. Most of the experiments were carried out with respondents who had not previously been approached for an interview. Because these variables were not significant in any of the models, we did not include them in the final models shown. Similarly, we included mode of administration as a control variable, but trimmed it from the models when it failed to show significant effects.

The results of the analysis are shown in Table 1. The first model shown in Table 1 compares one or more incentive conditions with a zero incentive condition in the same experiment. The model controls for type of incentive, timing of incentive, amount of incentive, and burden. As can be seen from Model 1, paying an incentive has a significant positive effect with these other variables controlled. As in the study by Yu and Cooper (1983) and Church (1993), the effects of incentives are linear; a term for the square of the incentive difference, introduced to test for curvilinearity, was not significant. Within the limits of incentives and response rates occurring in these experiments, more money results in higher response rates.

The difference in response rates produced by prepayment vs. promised payment is not significant in this model, but the direction of the differences is in accord with conventional wisdom--i.e., prepayment appears to be more effective than promised payments. When we look only at the five experiments that hold size of incentive constant and compare prepayment and promised payment directly, we find in every case that prepayment yields higher response rates than the promised payment condition. However, unlike Church (1993) but like Yu and Cooper (1983), we found a significant improvement in response rates with promised as well as prepaid incentives (data not shown).

Like Church (1993), we found that gifts are significantly less effective in eliciting response, even controlling for the value of the incentive. Increasing the burden of the interview significantly increased the difference in response rates between a zero

incentive and an incentive condition. The interaction between burden and the size of the incentive was not significant, however.

Are incentives necessary in low-burden telephone or face-to-face interviews? We cannot answer the question in that form, but we can ask whether incentives have a significant effect on response rates in such interviews. Restricting the analysis above to low-burden conditions, we find that the effect of incentives remains significant ( $b=0.5865$ ,  $S.E=0.2144$ ,  $p<.05$ ).

The second model in Table 1 includes, in addition to experiments with a zero-incentive condition, all those that compare the smallest with larger incentive conditions as well. These results parallel those shown in Model 1, except that here the effect of prepayment is not only positive but also significant. The effect of gifts is significantly negative (compared with cash), and the effects of burden are as described for Model 1.

**Table 1: Effects of Incentive Amount, Time of Payment, Type of Incentive and Burden on Differences in Response Rate**

	Model 1		Model 2	
	<i>Zero vs. Larger Incentive Conditions</i>		<i>Smallest vs. Larger Incentive Conditions</i>	
	<i>Beta</i>	<i>Standard Error</i>	<i>Beta</i>	<i>Standard Error</i>
Intercept	1.7852	1.2808	1.4492	1.2785
Incentive difference/dollar <sup>a</sup>	0.1479	0.0479***	0.1443	0.0481***
Prepayment	0.4599	2.0293	2.4462	1.5066*
Gift	-4.9357	2.1360**	-7.1428	1.4257***
Burden	3.2866	1.3308**	3.9550	1.2289***
Adj. R <sup>2</sup>	.36		.41	
N	(55)		(59)	

<sup>a</sup> „Incentive difference“ refers to the difference between the smallest (or zero) incentive and larger incentive amounts paid. Incentive amounts are modeled in constant 1983 dollars.

\*  $p = .11$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

Because some studies contributed more than one pair of conditions to the analysis, we reran these analyses using a random effects model with studies as random blocks. The parameter estimates are very similar to those in models that do not control for study, and the effects of all the independent variables except prepayment remain significant in these models.

As far as we can tell from the studies we examined, incentives do not appear to exact a price in quality (i.e., item nonresponse or number of words in response to open-ended

questions). Paying an incentive may alter the composition of the sample, however. In four studies, there is an indication that paying an incentive may be useful in obtaining higher numbers of respondents in demographic categories that might otherwise tend to be underrepresented in sample surveys (e.g., low income or nonwhite race). Six other studies reported no significant effects of incentives on sample composition, and in one study the results were mixed.

### **Does \$10 Equal \$10? The Effects of Framing on the Effectiveness of Incentives**

The theoretical literature on the norm of reciprocity (Gouldner, 1960; Cialdini, 1988) suggests that framing an appeal in such a way as to evoke a sense of diffuse obligation will result in higher response rates than framing it as payment for the respondent's time. The difference can, perhaps, also be thought of as a *Gemeinschaft* vs. *Gesellschaft* orientation (Toennies, 1955). The former is a reflection of a community of interests; the latter fosters a rational calculation of benefits and costs.

The question we addressed in the study I am about to describe is whether this theoretical principle depends on the type of incentive offered (Singer, Gebler, Van Hoewyk, and Brown, 1997). That is, we hypothesize that the use of a monetary incentive renders an appeal to reciprocity less effective, and that the use of a gift renders it more effective. In other words, we hypothesize that certain types of appeals are congruent only with certain types of incentives, and that they are less effective when this natural congruity is violated.

In order to test this hypothesis, we designed an experiment in which two types of incentives (a pen valued at \$3.75, and a check for \$10) were randomly paired with two different types of appeals.

One letter described the incentive as a "token of our appreciation"; the other referred to the incentive as "payment for your time." A random half of the group receiving each type of incentive received the "token" letter, the other half received the "payment" letter. Aside from the definition of the incentive as a token of appreciation or as payment, the two letters were identical in describing the content of the study (how to make surveys more effective), its sponsor (the University), and the organization carrying it out (the Survey Research Center at the University of Michigan). We predicted that describing a pen as a "token of appreciation for your help" would be perceived as an appeal to the norm of reciprocity, and would be more effective in eliciting agreement to participate in the study than a pen described as "payment for your time." Similarly, we predicted that a check for \$10 described as "payment for your time" would be more effective in eliciting agreement to participate than a check for \$10 framed as "a token of our appreciation."

Since most monetary incentives in face-to-face and telephone surveys are framed as promised rather than prepaid incentives, we also included a condition in which the \$10 check was described as a payment but promised to the respondent rather than sent ahead of time, so that we would be able to distinguish the effects of prepayment from the effects of appropriate framing. 125 undergraduates from the University of Michigan were randomly assigned to each of the five conditions.

Students were mailed a letter asking them to participate in a telephone survey being conducted by the Survey Research Center of the University of Michigan about how to improve survey practices. The survey was described as taking 45 minutes, and students were asked to return a card indicating their willingness to be interviewed. All students (both those who returned the card and those who did not) were followed up with a brief telephone interview. The questionnaires differed slightly between the two groups, but both contained similar sets of questions about their attitudes toward surveys and incentives and their reasons for returning or failing to return the card.

Looking only at those who returned a card indicating their willingness to be interviewed supported our framing hypothesis for money, but not for gifts (see Table 2, Column 1).<sup>2)</sup> As in other reviews of the role of incentives in mail as well as telephone and face-to-face surveys (Church, 1993; Singer et al., forthcoming; Yu and Cooper, 1983), we found that money is significantly more effective than a gift (57.6% vs. 32.0%;  $X^2=32.1$ ,  $df=1$ ,  $p<.01$ ), and promised payment is significantly less effective than a prepaid monetary incentive (44.8% vs. 57.6%;  $X^2=8.52$ ,  $df=1$ ,  $p<.01$ ).<sup>3)</sup>

As predicted, framing a check as payment is significantly more effective than framing it as a token of appreciation (64.0% vs. 51.2%;  $X^2=6.54$ ,  $df=1$ ,  $p<.05$ ), but there is no "boomerang" effect when a pen is framed as "payment" for time--perhaps because the incentive effect associated with the pen is so small.

As already noted, this conclusion is based on those expressing a willingness to participate by returning a card. In fact, however, we attempted to interview everyone in the sample, regardless of whether or not they had mailed back the card. Response rate to the survey can be seen as a measure of how firm the decision to participate or refuse, represented by mailing back the card, actually is.

Among those who had returned a card signifying willingness to participate, the response rate to the actual survey was 92%, indicating that mailing the card constitutes a high, but not perfect, commitment to complete the interview. But even among those who had not returned the card, 69% were ultimately interviewed.<sup>4)</sup> Thus, the negative commitment signified by failure to return the card is not very firm; persuasive interviewers can over-

come it in a large majority of potential respondents. We should note once again that these findings are based on a student sample, notoriously more cooperative than a general population sample, and that everyone in the sample either had received (80%) or was promised (20%) some kind of incentive.

The actual survey response rates, by experimental condition, are shown in Table 2, Column 2. Note that these cannot be taken as a pure measure of incentive effectiveness, since the incentive is, in every case, augmented by one or more contacts with an interviewer. Thus, Column 2 illustrates the additive and interactive effects of interviewers and different kinds of incentives. In the case of some of the experimental conditions, this "added value" is very large indeed: more than forty percentage points in the case of the pen incentive conditions and almost forty percentage points in the promised money condition.<sup>5)</sup>

**Table 2: Agreement to Be Interviewed and Actual Response Rates, by Experimental Condition**

<i>Experimental Condition</i>	(1)	(2)	<i>(N)</i>
	<i>Agreement to Be Interviewed</i>	<i>Response Rate</i>	
	%	%	<i>N</i>
Pen as a token of appreciation	30.4	76.0	125
Pen as payment for time	33.6	75.2	125
\$10 check as token of appreciation	51.2	79.2	125
\$10 check as payment for time	64.0	81.6	125
\$10 check as payment, promised	44.8	84.0	125

However, it is not simply true that the response rates after follow-up are higher than those without interviewer intervention. *The conclusions one would draw about the relative effectiveness of different incentives also differ.* None of the differences between conditions is significant after the calls by interviewers. Not only is there no support for the framing hypothesis when only the results of the actual survey are considered, but there is no support for the effects of cash vs. gifts or prepaid vs. promised incentives.

Because these results are based on an experiment with students, and because of several other ambiguities, including the absence of a control group, we are in the process of repeating this experiment with a national sample of telephone households for whom we were able to obtain a listed address. So far, the results resemble the results for the student sample before interviewer followup, except that the response rates, as one would expect, are significantly lower.

## Does Equity Matter? And to Whom?

So far, we have been looking at research on intended consequences of incentives. I turn now to some experiments involving *unintended* consequences.

There are two opposing views on the issue of paying refusal conversion payments to reluctant respondents. On one side are the economists, naive or otherwise, who argue that refusal is an *ipso facto* indication that the survey has less utility for the refusers, or is perceived as more burdensome by them. This being the case, it is seen as entirely appropriate to offer compensation to refusers but not to other respondents, who are more cooperative.

On the other side of the argument are the social psychologists, for whom refusal conversion payments represent a violation of equity expectations. For the social psychologists, reluctance to participate is not an *ipso facto* indication that the survey is more burdensome; and offering refusal conversion payments to reluctant respondents seems likely to reinforce their uncooperative behavior, making it more likely to recur in the future. In addition, such payments are believed likely to alienate cooperative respondents if they should become aware of the practice.

In the traditional formulations of equity theory (e.g. Adams, 1965; Adams and Freedman, 1976; Walster, Berscheid and Walster, 1976; Walster, Walster, and Berscheid, 1978; Homans, 1961, 1974), perceptions of equity are determined by the results of a comparison such as the following:

$$\frac{\text{Ego's Input}}{\text{Ego's Reward}} = \frac{\text{Alter's Input}}{\text{Alter's Reward}}$$

If the two ratios are similar, the outcome of the comparison is perceived as equitable. But if Alter is seen as differentially rewarded for a task similar to that performed by Ego, then the situation is likely to be perceived as inequitable, *provided that the other person is seen as "similar" to Ego*. Thus, offering incentives to respondents who have refused a prior survey request may be perceived as inequitable by those who have agreed to participate without such an incentive. Others, however, may not be defined as similar, or the task they perform may be seen as dissimilar. For example, potential respondents may recognize that although *they* are motivated to respond by altruistic concerns, others are not so motivated, and it is therefore appropriate to provide greater incentives to those others. Or, potential respondents may judge that the task is harder for reluctant respondents, and that it is therefore appropriate to offer greater incentives to them. Under those circumstances, if other respondents are not seen as similar, then reference

group theory (Hyman, 1942; Stouffer et al., 1949; Merton and Rossi, 1950; Hyman and Singer, 1968), cognitive dissonance theory (Festinger, 1957), and social comparison theory (Suls and Miller, 1977) all predict that they will cease to be defined as an appropriate object of comparison, and differences between Ego's reward and Alter's will cease to motivate Ego's behavior (Suls and Miller, 1977; Singer, 1981; Shepelak and Alwin, 1986).

If an expectation exists concerning appropriate rewards for survey participation, then it is reasonable to assume that a violation of that expectation by the survey organization will lead to a perception of inequitable treatment by the respondent. In turn, such a perception may lead to reluctant participation or outright refusal.<sup>6)</sup>

### **Focus Groups**

To explore potential respondents' reactions to equity issues involved in the payment of incentives, we held two exploratory focus group discussions in the fall of 1994. One consisted of black low-income participants, all but two women, in Detroit; the other, of middle-class, mostly white, men and women in Livonia, a suburb of Detroit. Respondents were recruited by Survey Research Center interviewing supervisors and promised \$15 for their participation.

These group discussions offered little evidence for widespread perceptions of inequity in survey organizations' incentive practices. But to provide better answers to this question, Bob Groves and I designed a study that relied in part on laboratory administration of videotaped vignettes of survey interactions and in part on a field experiment in the 1996 Detroit Area Study (Groves et al., 1997).

### **The Videotaped Vignette Study**

The vignettes were constructed from three completely crossed factors: disclosure of differential incentives (vs. no disclosure), burden (high vs. low), and traditional persuasion (no prior refusal vs. prior refusal). In addition, we included two other factors that were only partially balanced within the design: a rationale for differential incentives in the disclosure condition (vs. no rationale), and an initial incentive (vs. no incentive). The result was 12 distinct vignettes, having the characteristics shown in Figure 1. These were videotaped, using one interviewer throughout, a second interviewer when a refusal conversion was involved, a female subject's voice, and a male subject's voice. Ninety-six subjects were recruited through the Parent-Teacher Organizations of two local elementary schools to which we made a cash contribution; subjects were not paid

individually. They were interviewed a week or so before viewing the videos with a questionnaire that asked about their political attitudes and community involvement, as well as about their attitudes toward surveys and their sensitivity to equity issues. Each subject viewed a randomly selected set of six vignettes and completed a brief self-administered questionnaire after seeing each one. The questionnaires included some manipulation checks along with the main dependent variable: "Would you be willing to take part in this interview?" Following the last video, they also completed a "final debriefing questionnaire" which repeated a few of the questions from the intake interview, primarily those about their perception of the fairness of differential payments to survey respondents.

**Figure 1: Subject Counts by Experimental Factors**

	No Incentive			
	Low Burden		High Burden	
	No Prior Refusal	Prior Refusal	No Prior Refusal	Prior Refusal
No Disclosure	48	48	48	48
Disclosure, No Rationale	48	48	48	48
Disclosure, Rationale	48	X	48	X
	Incentive			
	Low Burden		High Burden	
	No Prior Refusal	Prior Refusal	No Prior Refusal	Prior Refusal
No Disclosure	48	X	48	X
Disclosure, No Rationale	X	X	X	X
Disclosure, Rationale	X	X	X	X

The analysis controls for order-of-administration effects (not significant) as well as mode effects (25 subjects responded to written, rather than videotaped, versions of the vignettes). Although we also entered demographic and attitudinal variables into the logistic regressions, only one of these produced a significant effect, either alone or in interaction with the experimental variables, and is retained in the final model. The results of the final analysis, which was run in SUDAAN to adjust for multiple observations on the same respondent, are shown in Table 3.

The table shows some obvious things and some that are less obvious:

1. In the first place, interviews described as requiring an hour and a half are significantly less likely to elicit a willingness to respond than those described as requiring only half an hour.

2. Interviews described as offering an incentive ahead of time significantly increase the expressed willingness to respond.
3. Showing a vignette in which a respondent refuses twice, and is solicited three times by two different interviewers, *reduces* the expressed willingness to respond, as we had hypothesized; the effect is significant at the zero-order level at  $p=.09$ .
4. Subjects answering the written, rather than the videotaped, versions of the vignettes are *less* likely to express their willingness to respond, perhaps because of the absence of social desirability or identification factors, but there are no significant mode by independent variable interactions.
5. Subjects who believe respondents should be paid for survey participation were less likely to express a willingness to respond.
6. Finally, and most important from the point of view of equity theory, disclosure of differential incentives being paid to some but not all respondents significantly reduces subjects' expressed willingness to respond. However, if the vignette contains a rationale for why differential payments are necessary, the negative effect of disclosure is much reduced.

**Table 3: Logistic Regression Coefficients for Model Predicting High Likelihood of Participation <sup>1)</sup>**

	Coefficient	Standard Error
Intercept	.11	.28
Predictors		
Incentive	.94**	.27
Burden	-.58*	.26
Disclosure, Rationale Conditions:		
No Disclosure <sup>2)</sup>	---	
Disclosure, No Rationale	-.59**	.18
Disclosure, Rationale	-.25	.23
Refusal	-.49	.32
Mode	-.75*	.37
Compensation	-.85*	.40

1) Standard errors reflect the clustering of observations within subjects. The dependent variable is coded 0 (for responses 1, 2, 3) and 1 (for responses 4, 5). The *compensation* variable is coded "1" for "strongly agree" or "somewhat agree" and "0", otherwise. Asterisks denote p-values: \* -  $p<.05$ , \*\* -  $p<.01$ .

2) Omitted category in dummy variable regression.

Not shown in Table 3, but of interest nevertheless, is the fact that there is no independent or interactive contribution of a measure of perceived fairness on expressed willingness to participate. That is, although about 80% of subjects consider the paying of differential incentives unfair (unless justified by greater importance of the information or a longer interview), this attitudinal measure does not increase our ability to predict their willingness to participate in a survey in which differential incentives are being paid. Furthermore, the percentage of those considering the practice unfair declines somewhat from intake to debriefing interview.

### **The Field Experiment**

Because the vignette study was carried out in the laboratory with a rather homogeneous sample (most respondents were white married women with young children, members of two Parent-Teacher Organizations in Ann Arbor who had been recruited through the promise of a contribution of \$200 to the PTO), we designed a field experiment that, we hoped, would provide better external validity for these findings.

The vehicle for this investigation of the role of perceptions of equity in survey response was the Detroit Area Study (DAS), established by the University of Michigan over 40 years ago as a research and training laboratory in the community. The 1996 DAS was intended as a study of interracial contacts and attitudes, but the survey was also designed to permit an investigation of hypotheses concerning perceptions of equity in relation to the payment of incentives.

The 1996 DAS consisted of an area probability sample of 451 households, interviewed in person between April and August 1996 by graduate students supplemented, during the summer months, by professional interviewers from the Survey Research Center at the University of Michigan. The completion rate for the study, defined as the proportion of interviews completed divided by the number of households listed less vacants and non-sample households and those ineligible because of illness or language problems, was 66%. Seventy-two percent of the respondents were white, 21% were black, and 7% some other race. Ages ranged from 18-89 with a mean age of 44 years. Respondents had a mean of 13.5 years of education and the median income fell in the \$40,000-\$44,999 range.

All households were sent an advance letter before the interviewer contacted them; two thirds of these letters contained a \$5 bill and one third did not.<sup>7)</sup> Following one refusal by anyone in the household, these two portions of the sample were again split in half. Half of the refusers in each group--those who had received an initial incentive, and those who had not--were sent a persuasion letter only; the other half were offered \$25 to

convert their refusal. Thus, ultimately, there were four groups of respondents, of unequal size: Those who had been offered nothing and who had given an interview; those who had given an interview after being offered \$5; those who had given an interview after being offered \$25, and those who consented after receiving \$5 and being offered \$25.

Near the end of the interview, we introduced a split-ballot experiment into the survey. After a few questions about the present survey, half the subjects in each of the four experimental groups above were informed that because of the importance of including everyone in the sample, some of those who had expressed reluctance to participate had been offered \$25, while others received nothing; and they were asked whether they considered this practice fair or unfair. For the other half of the sample, this piece of information, and the question about fairness, were omitted. Then, all subjects were asked about their willingness to take part in a future interview by the same organization.

We had expected those to whom the payment of differential incentives was disclosed to be less willing to participate in a future survey by the same organization, because they would consider the practice unfair. However, at the zero-order level, there were no differences in expressed willingness to participate between the group to which differential payments had been disclosed and the group to which it had not--about a quarter of each group said they would "definitely" be willing to participate in another survey by the same organization, even though a large majority--72.7%--of respondents to whom the payment of differential incentives was disclosed considered the practice to be unfair.

We had also hypothesized that respondents to whom differential incentive payments were disclosed *and who perceived these payments to be unfair* would be less willing to participate in future surveys than those who perceived them as fair. Although a somewhat greater percentage of those who considered the practice fair than of those who considered it unfair were "definitely" willing to participate in a future survey by the same organization (32.8% vs. 25.8%), this difference was not statistically significant ( $X^2=1.08$ ,  $df=1$ ,  $p<.30$ ).

Because it seemed possible that some of the variables specified above might interact, we estimated several logistic regression equations from the data controlling for these other variables as well as a series of demographic characteristics. None of them altered the basic conclusions drawn from the bivariate relationships.

A little more than a year later, all the original DAS respondents for whom we could locate an address were sent a mailed questionnaire on the topic of assisted suicide. The return address on the letter differed from that used for the original Detroit Area Study survey, complicating the inferences that can be drawn from the experiment about the

effect of concerns about equity.<sup>8)</sup> The questionnaire was preceded by an advance letter; a random half of the respondents received a \$5 prepaid incentive with the letter, whereas the other half did not. A week after the questionnaires had been mailed, everyone in the sample was sent a reminder postcard. We did not engage in further efforts to stimulate the response rate.<sup>9)</sup>

The response rate to the self-administered survey was 41.4% for those who did not receive an incentive and 65.1% for those who did--a large and statistically significant difference. Our real interest in the study, however, was the consequence of disclosing differential payments to respondents for their survey participation a year later. Such disclosure appears to have some effect on reducing participation a year later: 50% of those to whom differential payments were disclosed participated in the new, self-administered survey, compared with 55% to whom such payments were not disclosed, but this difference, while in the expected direction, is not statistically significant. When we looked at the relationship between disclosure and participation in a logistic regression framework, adding controls for receipt of an incentive in the DAS, receipt of a refusal conversion payment offer, and a series of demographic variables, the effect of disclosure remained constant but did not reach a conventional level of statistical significance ( $b=-.318, s.e.=.23, p=.17$ ).

### **Are Incentives Transforming the Motivation for Survey Participation?**<sup>10)</sup>

I come now to the last topic of the lecture: Are incentives transforming the motivation to participate in surveys?

Because of concerns about declining cooperation rates, the Survey Research Center at the University of Michigan decided in the fall of 1995 to begin monitoring the changing climate for survey research in the United States by adding five evaluative questions at the end of the Survey of Consumer Attitudes, a national telephone survey administered monthly to a sample of roughly 500 respondents. Of these 500, 300 are newly selected RDD households and the remaining 200 are reinterviews of respondents first interviewed six months earlier. Because of concerns about their possible biasing effects, the five evaluative questions were asked of only the reinterviewed portion of the sample. The questions were added to the survey in January and February of 1996 and repeated in February and March of 1997 in order to measure changes in the climate for survey research over the approximately 12-month period. In principle, one could use changes in the responses to these questions as leading indicators of changes in the climate for survey research, and take proactive steps to counteract such changes.

### **Response Rate**

The analysis involves comparing two independent samples of reinterviewed respondents. The initial response rate of those reinterviewed in January and February of 1996 was 70.3% six months earlier; their reinterview rate averaged 77.6%. Thus, the effective response rate of the 1996 sample is 54.5%. For the sample reinterviewed in February and March of 1997, the initial response rate averaged 69.3% and the reinterview rate, 76.8%; thus, the effective response rate for the 1997 sample was a slightly lower 53.2%. (The response rate excludes only nonsample cases from the denominator, and is thus a fairly conservative estimate. Noninterviews for reasons of illness or language, for example, are retained in the denominator.) The questions are, thus, asked primarily of cooperative respondents; presumably, the attitudes toward surveys among nonrespondents would be more negative.

### **Monitoring Questions**

The five monitoring questions asked on the surveys are as follows:

1. If you had it to do over again, would you have agreed to do the interview or would you have refused?
2. For each of the following, please tell me whether you agree strongly, agree somewhat, disagree somewhat, or disagree strongly. Surveys like this one provide useful information for decision makers?
3. Surveys like this one are a waste of people's time.
4. People should get paid for doing surveys like this.
- 4a. How much should they get paid?
5. Everyone has a responsibility for answering surveys like this. Do you agree strongly, agree somewhat, etc.

The five questions were systematically rotated during their administration.

### **Changes over Time**

As already noted, the reason for asking these questions at two points in time was to capture changes in the climate for surveys. Responses to the five questions (and one subquestion) in 1996 and 1997 are shown in Table 4; they represent two cross-sectional measurements of attitudes toward surveys rather than answers by the same respondents at two different times.

Table 4 indicates that on three of the questions, no significant changes took place from one year to the next. Three others, however, show a significant change: Significantly

more respondents (45.7% in 1997, compared to 29.7% in 1996) said that respondents should be paid for doing a survey like this, and the amount they stipulated showed a significant increase as well. At the same time, significantly more respondents (51.4% in 1997, compared with 44.5% in 1996) agreed that everyone had a responsibility to answer a survey like this.

We had anticipated changes in answers to the question about payment for two reasons. First, the practice of paying incentives to respondents in telephone and face-to-face surveys appears to be increasing, and this may be influencing expectations among the general public. Second, a large number of respondents to the March 1997 survey had themselves received an initial incentive payment of \$5 six months earlier, and a smaller number of respondents in three of the four months had received \$20 or \$25 to convert an initial refusal.

**Table 4: Responses to Evaluative Questions, by Year**

	1996 (%)	1997 (%)
R Should Get Paid **		
Agree	29.7	45.7
Disagree	70.3	54.3
(n)	(411)	(396)
How Much *		
0-5	19.6	22.2
6-10	32.4	19.4
11-20	19.6	35.4
Over 20	28.4	22.9
(n)	(102)	(144)
Surveys Are Useful		
Agree Strongly	34.6	39.7
All Other	65.4	60.3
(n)	(405)	(401)
Everyone's Responsibility *		
Yes	44.5	51.4
No	55.5	48.6
(n)	(409)	(403)
Will Do Again		
Yes	76.9	76.1
No	23.1	23.9
(n)	(407)	(406)
Waste of Time		
Disagree Strongly	28.6	30.4

All Other (n)	71.4 (402)	69.6 (395)
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\* p <.05, \*\*p <.01

In order to separate the effect of generalized expectations from that of personal experience, we looked at 1997 responses to the "People should get paid" question among those who had and those who had not been offered an incentive. The results are shown in Table 5. In both years, those who had received an incentive were much more likely to say that people should be paid than those who had not; the differences are significant in both years, and are especially large for people who received the (larger) refusal conversion payments. Among people who did *not* receive any kind of incentive in either year, there is a slight increase from 1996 to 1997 in the percentage saying people should get paid for doing surveys like this, but this difference is not significant;  $X^2=1.77$ ,  $df=1$ ,  $p=.18$ . Nor is the difference between 1996 and 1997 among people who received a refusal conversion payment significant. Thus, Table 5 demonstrates that the changed expectations apparent in Table 4 are due almost entirely to the responses of those who had themselves received an initial incentive--in other words, to personal experience rather than diffuse social norms.

The question of interest for this paper, however, is whether these responses should be understood as *reflecting changed expectations for the future*, or as *normative statements justifying past behavior*.

**Table 5: Responses to Evaluative Questions, by Incentive and Year**

	1996			1997			
	Incentive			Incentive			
	0	\$20-25		0	5\$	\$20-25	
R Should Get Paid	26.0%	91.3%	***	31.0%	51.3%	77.6%	***
Surveys Are Useful	32.9%	63.6%	***	35.2%	48.7%	40.6%	
Everyone's Responsibility	44.8%	39.1%		45.7%	59.0%	58.0%	*
Will Do Again	77.6%	65.2%		74.5%	83.3%	76.7%	
Waste of Time (Disagree Strongly)	26.6%	63.6%	***	30.0%	38.5%	26.5%	
N	(393)	(23)		(242)	(81)	(70)	

\* p <.10, \*\* p <.05, \*\*\* p <.01

We can test these alternative interpretations by examining the cooperation rate of people to the March 1997 survey. Among people recontacted in March 1997, 139 had received an incentive 6 months earlier and 98 had not. If the earlier payment of an incentive led

to (unmet) expectations for payment in the future, cooperation rates (without an additional incentive) in March should be lower among those who had received an initial incentive the preceding September than among those who had not. However, among those who had received an initial incentive in September and who were contacted by interviewers, 81.0% were reinterviewed without an additional incentive in March; among those who had received a refusal conversion incentive in September, the cooperation rate in March without an additional incentive was 79.5%; and among those who had received no incentive in September, the cooperation rate without an additional incentive in March was 66.3%. The difference between those receiving no incentive in September and those receiving \$5 is significant;  $X^2 = 5.43$ ,  $df=1$ ,  $p<.05$ ; those who had received a five dollar incentive six months earlier were significantly *more* likely to cooperate in March than those who had received no incentive. The difference in March cooperation between the no-incentive group and the group receiving a refusal conversion payment in September is not significant. Thus, these data provide no support for the interpretation that responses to the question about whether or not respondents should get paid reflect expectations about future behavior on the part of the survey organization.

## Summary and Conclusions

What do these investigations tell us about the anticipated and unanticipated consequences of incentives?

1. As far as anticipated consequences are concerned, incentives appear to have similar effects in interviewer-mediated surveys as they do in mail surveys. They increase response rates, even for low-burden surveys. They do not appear to have adverse effects on the quality of response, although they may alter the composition of the sample. Conclusions about the effects of framing await further testing, but results so far suggest they are relatively unimportant.
2. As far as unintended consequences are concerned, the evidence is more ambiguous. Our research suggests that equity matters to respondents, but it is not a very salient issue for them. Thus, although large majorities consider the practice of paying refusal conversion payments unfair, awareness of this practice does not seem to loom large in their expressed willingness to participate in future research by the same organization or in their actual participation a year later. Nor does the payment of incentives at the outset of a survey appear to have negative consequences for participation in a second wave of the same survey without additional incentives. On the contrary, people who received such an incentive six months earlier were actually more likely to cooperate than those who did not. However, payment of incentives is still a rather novel experience for respondents to

telephone or personal interviews. Whether the absence of negative results observed in the present study will survive the more widespread use of incentives in such interviews remains very much an open question, and one deserving continued research.

3. The general conclusion, therefore, should come as no surprise. Although we have not so far seen evidence that the payment of incentives, either to some or to all respondents, has dramatic unintended consequences, we cannot rule out the possibility of such effects in the future. Nor can we be sure that the effects would be the same in countries with different cultural norms about equitable treatment and payment for survey participation. Therefore, experiments along the lines reported here should be carried out in other countries, and we should continue to monitor attitudes toward surveys both in the U.S. and in other countries where surveys are routinely carried out and increasingly relied on by governments, businesses, and academics.

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### **Notes**

\*) Anmerkung des Herausgebers: Der Beitrag ist die überarbeitete Fassung einer Vorlesung, die Eleanor Singer im Rahmen der Wildenmann-Gastprofessur bei ZUMA hielt.

1) A fuller report of the research in this section will appear as Singer et al., „The Effects of Incentives on Response Rates in Face-to-Face and Telephone Surveys“, in a forthcoming issue of the *Journal of Official Statistics*. We would like to thank the anonymous reviewers of that journal, as well as associate editor Nancy Mathiowetz, for their helpful comments.

2) A handful of letters were returned as undeliverable. These did not cluster in any one experimental condition, and are included in the denominator.

3) As noted earlier, some students selected for the sample lived in the same dormitory. As a precaution, we asked, in the course of the telephone survey, whether or not the respondent knew anyone who had been selected for the survey. Eighty respondents--16.2% of the interviewed sample, and 12.8% of the original sample--claimed they did. When the responses of these students are excluded, the attitudinal results do not change

in any important way. We cannot, of course, analyze the effect of this variable on participation in the survey.

4) We stipulated that interviewers were to call back at least 4 times, at different times of the day and on different days, and to make one attempt to convert refusals by those who had not returned the card.

5) In a mail survey, follow-up mailings would serve a similar function.

6) What is considered „equitable“ today may, as a result of exposure to different practices, come to be perceived as „inequitable“ in the future, and vice versa. Thus, the practices of some organizations with respect to incentives may affect the climate in which all surveys are carried out. We return to this topic below.

7) Households who received the \$5 incentive were significantly more likely to respond than those who did not; the difference in response rates was 8 percentage points.

8) The questionnaire and the return address on letters identified the research organization as the Program on Public Opinion and Health Policy at the Survey Research Center, University of Michigan. Hence, respondents may not have identified the mail survey with the organization that had interviewed them more than a year earlier.

9) The number of respondents to the original Detroit Area Study was 451; the follow-up request was sent to 433 of those respondents (18 respondents were omitted because we were unable to locate an address for them). Completed questionnaires were returned by 202 respondents, 4 respondents refused to participate, and 172 respondents did not return questionnaires; 53 questionnaires were undeliverable, and 2 respondents were deceased or could not be contacted for other reasons. The completion rate, as defined earlier, was 53.4%.

10) For a fuller discussion of the research reported here, see Eleanor Singer, John Van Hoewyk, and Patricia Maher, Does the Payment of Incentives Create Expectation Effects? *Public Opinion Quarterly*, Summer 1998. We would like to thank Vincent Price and three anonymous reviewers for their helpful comments on the manuscript.

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