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The Effect of Differential Incentives on Attrition Bias: Evidence from the PASS Wave 3 Incentive Experiment

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Abstract

Respondent incentives are widely used to increase response rates, but their effect on nonresponse bias has not been researched as much. To contribute to the research, we analyze an incentive experiment embedded within the third wave of the German household panel survey "Panel Labor Market and Social Security" conducted by the German Institute for Employment Research. Our question is whether attrition bias differs in two incentive plans. In particular, we want to study whether an unconditional €10 cash incentive yields less attrition bias in self-reported labor income and other sociodemographics than a conditional lottery ticket incentive. We find that unconditional cash incentives are more effective

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than conditional lottery tickets in reducing attrition bias in income and several sociodemographic variables.

It is well known that respondent incentives can increase response rates (Church 1993; James and Bolstein 1990; Mercer et al. 2015; Singer 2002; Singer and Ye 2013; Toepoel 2012; Willimack et al. 1995). It is also widely agreed that response rates per se are not a good indicator of survey quality and that other measures, such as nonresponse bias, should be taken into account (Groves and Peytcheva 2008; Singer and Ye 2013).

But it is much less clear what the effect of incentives on nonresponse bias is. An increasing response rate does not necessarily decrease nonresponse bias. However, if incentives increase the response rates of some groups more than others, nonresponse bias is likely to be affected.

An increase in response rates only for subgroups who are likely to participate anyway will increase nonresponse bias. Therefore, the desired effect of incentives is to bring people who are less likely to respond into the respondent pool.

Literature shows that cash incentives increase response rates more than in-kind incentives, unconditional incentives have a higher effect than conditional ones, and higher incentives increase response rates more than lower ones (Church 1993; Singer, Groves, et al. 1999; Singer and Ye 2013). Dillman et al. (2014), however, argue that the amount of the incentives should be chosen carefully as incentives that are too high might create suspicion rather than trust and might destroy cooperation.

The fact that unconditional incentives are found to increase response rates more than conditional ones might be explained by social exchange theory: Beyond economic interests, incentives can be used to establish trust, which is more important than the incentive value (Cantor et al. 2008; Dillman et al. 2014). The potential impact of incentives on underrepresented subgroups can be explained by leverage-saliency theory (Groves et al. 2000). Under this theory, the decision to participate in a survey depends on various features of the survey, their relative importance to the sample case, and how salient they are to the sample case. If an incentive is enough of a positive inducement to participate, overcoming the negative and less enticing features of the survey, it may pull people into the respondent pool who would not otherwise participate.

The subgroups most affected by a monetary incentive are thought to be those for whom money has a high importance. Economic models of survey response stress that incentive payments may be perceived as compensation for the time and effort a respondent provides (Philipson 1997); such models predict that a modest incentive should have a stronger effect on low-income respondents (because their opportunity cost of time is lower).

Empirically, these predictions are broadly confirmed; for instance, Mack et al. (1998) found that incentives of USD20 can disproportionally increase participation of respondents from poverty and black households. Groves et al. (2006) found that people who are less interested in surveys can be brought into the pool by monetary incentives that serve as compensation for lack of interest. In a meta-analysis of incentive experiments in face-to-face and telephone surveys, Singer, van Hoewyk, et al. (1999) find some evidence that incentives can improve sample composition by increasing the response propensity for people who are otherwise underrepresented, such as low-income people or nonwhites. Incentive effects on attrition in panel surveys appear to be very similar to the effects in cross-sectional surveys (Laurie and Lynn 2009): Unconditional incentives are more effectively increasing retention rates than conditional ones, cash incentives more than in-kind incentives, and higher incentives more than lower ones. As for attrition bias, Laurie and Lynn (2009) conclude that there is some evidence that incentives have the potential to reduce bias by disproportionally increasing response for respondents with lowest response propensities. None of these studies examines the effects of incentives on nonresponse or attrition bias of survey statistics directly.

We analyze the effects of incentives on the attrition bias of survey statistics using data from a German household panel that was specifically designed to study people with low socioeconomic status. In the third wave of this panel survey, an incentive experiment was conducted in which households were randomly given either an unconditional cash incentive or a conditional lottery ticket, which has been the standard incentive in the previous two waves. The experimental groups differ by two characteristics of the incentives: conditional versus unconditional and cash versus in-kind incentives. Although the effects cannot be separated, they are known to influence respondents in the same way: unconditional incentives increasing response more than conditional ones and cash incentives more than in-kind incentives (Church 1993; Singer, van Hoewyk, 1999; Singer and Ye 2013).

Our question is whether unconditional monetary incentives affect attrition bias differently than a conditional lottery ticket. We are especially interested in the effect of incentives on estimates of personal income. This is usually untestable because no valid information on nonrespondents is available and therefore a gold standard for comparison is missing. For our study, we have administrative data on the target variables for both respondents and nonrespondents and we can compute attrition bias directly by comparing third-wave respondents to second-wave respondents serving as our gross sample. We address our research question by comparing attrition bias for the two experimental groups using administrative data.

Data

Panel Data

We use data from the first three waves of the German household panel survey "Panel Labor Market and Social Security" (PASS). PASS is conducted annually to study the effects of the German "Hartz"—reforms that came into effect in 2005 (see Trappmann et al. 2009). These reforms introduced a new welfare system at the household level called unemployment benefit II (UB II). In every wave, each sampled household in the survey receives a household questionnaire and personal questionnaires for all members aged 15 or older. The household questionnaire is completed by the head of the household, who is determined by the household in wave 1 and who serves as the contact person for the survey agency in the following waves. The questionnaire contains questions about household composition, dwelling, household income, and material deprivation, as well as received unemployment benefits. The personal questionnaire contains questions about the individual's employment status, employment history, and income. Households with completed household questionnaires are seen as respondents to the wave. The interviews for PASS were collected in sequential mixed-mode design (Computer Aided Telephone Interview [CATI] and Computer Aided Personal Interview [CAPI]). The first wave of data collection took place between December 2006 and July 2007, the second between December 2007 and August 2008, and the third was conducted from December 2008 to August 2009.

PASS consists of two different samples to compare benefit recipients and nonrecipients of the new UB II. About half of the sample from wave 1 is sampled from a register of UB II recipients at the Federal Employment Agency. This is the recipient sample because all households had received some kind of benefit by the date of sampling. The other half of the sample, called the population sample, is selected from a commercial database of residential addresses. In this sample, people with low socioeconomic status are oversampled by design because low-income households are of special interest for the PASS survey as they are under higher risk of being affected by changes in the welfare system. We analyze the recipient and population samples jointly. Due to the overrepresentation of UB II recipients and low-income individuals, we can analyze the effect of the two types of incentives on these groups who are often a small proportion of the sample in other surveys.

The incentive experiment was conducted in wave 3. In the first two waves, sample units received a thank-you card containing a stamp worth 55 cents with their advance letter, and responding households were given a German lottery ticket (“Aktion Mensch,” worth €1.50 in the first, and “ARD-Fernsehlotterie,” worth €5 in the second wave). In the experiment, panel households were randomly assigned to two treatment groups. The heads of household received two different invitation letters asking for their and their family members’ participation: Heads of household were either promised a lottery ticket (lottery group) for every family member who completed the questionnaire or were sent €10 with their cover letter (cash group; Büngeler et al. 2010). Note that the experiment compares a conditional in-kind incentive to an unconditional cash incentive. Also, the monetary values of each are different (€10 for the cash incentive and €5 for the lottery ticket). We restrict our analysis on the heads of household as they are the persons to be contacted by the field agency, are the ones to receive the invitation letters, and work as a gatekeeper for the whole household. In total, 16,091 households were part of the PASS wave 3 sample, including 4,031 wave 3 refreshment households who were not part of the incentive experiment. We exclude 4,793 cases from our analyses who have only responded to one of the prior waves and focus on 7,267 panel households who responded to both waves 1 and 2. Of those, a randomly selected 985 were part of another experiment and are omitted from our analyses.

For this article, we analyze the remaining 6,282 households, 2,952 of which belong to the recipient sample and 3,330 to the population sample. Cases were randomly assigned to the experimental groups within these two subsamples. In total, 3,163 cases were part of the conditional lottery ticket incentive group, and 3,119 cases were part of the conditional cash incentive group. For our analysis, we include household information (UB II status) and personal information about the heads of household contained in the administrative data. We split personal income into terciles for our analyses (less than €980, €980 to €1629, and €1630 and more). The income variable contains earnings from own employment only—social benefits are not included. In addition, we examine sociodemographic variables that we expect to affect response, like gender, nationality (foreign or German), and employment status, whether the person has a job of up to €400 per month that is not taxed and exempt from social insurance payments (“mini job”) and age (younger than 30, 30–39, 40–49, 50–59, and 60 or older).

Administrative Data

We use administrative data from the "integrated employment biographies" (IEB) file provided by the Research Data Center of the Federal Employment Agency to compute the samples' true values. This data set contains detailed employment (e.g., type of employment and income) and benefit records (e.g., type of benefit) for the German workforce, excluding self-employed and civil servants and individuals who receive welfare. With these restrictions, administrative information is available for respondents and nonrespondents to the survey. IEB data have been found to be very reliable concerning employment status, wages, and transfer payments (Jacobebbinghaus and Seth 2007). Although we use these highly reliable variables, these data might contain some random measurement error. Administrative data are available as spell data. These contain multiple observations for each sample case that cover the beginning and the end of a span of time during which the case is in a certain state, like employed, unemployed, or benefit recipient. The variables of interest are constructed from these data using reference dates. For respondents, the date of the household interview is used. For all nonresponding cases who were contacted in CATI, the date of the last contact was used. We do not have contact data for cases contacted in CAPI, so we use the end of the field period as the reference date for those cases.

Estimation Sample

In total, 5,179 of the 6,282 cases (82%) who participated in waves 1 and 2 responded to the household interview in wave 3. For all units within the recipient sample, linkage to administrative data is straightforward as these data are part of the sampling frame. Administrative data could not be linked successfully for only five cases from the recipient sample. Most likely, these were wrong or temporary entries in the frame at the date of sampling and deleted from the records after the sample was drawn.

In contrast to the register-based recipient sample, cases from the population sample need to be searched for in the administrative records. However, due to data protection rules, this is only allowed if linkage consent has been given beforehand. In the population sample, 79.07% of the respondents gave their consent for data linkage, and 77.93% of them could be found in the records using probabilistic record linkage procedures. The cases who could not be found in the administrative records are most likely to be self-employed or civil servants as these groups are not covered by the

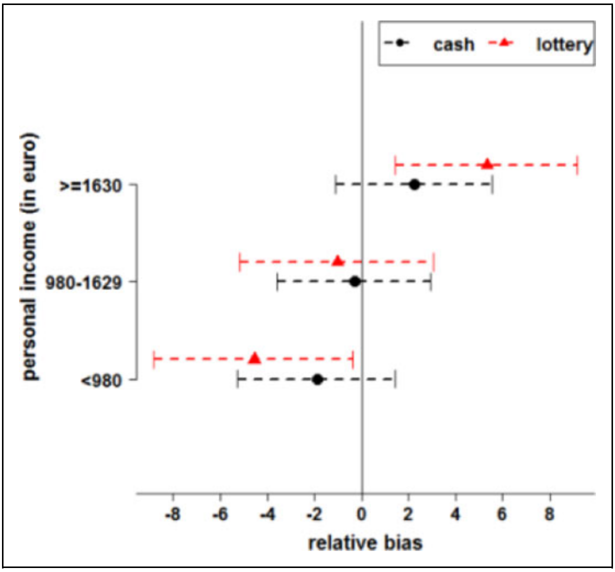

Figure 1. Relative attrition bias in income estimation.

administrative data. Sixty-two percent of the population sample cases could be linked successfully (see Figure 1 in the Online Appendix). Given the analyses from Sakshaug and Kreuter (2012), we do not expect much consent bias in these data. In their analyses of the same survey, consent bias is only found for age and foreign citizenship, and it is very small compared to other bias sources such as bias due to measurement error or nonresponse. Also, Beste (2011) finds that only respondents having a foreign citizenship and respondents who receive no income at all might be underrepresented in the linked data set. We do not expect that excluding respondents who could not be linked to the administrative data or did not agree to the linkage introduces significant biases into our analyses. For our analyses using the administrative data, we use the 2,947 cases of the recipient sample and the 2,049 cases of the population sample who were successfully linked to the administrative data—which makes 4,996 cases in all.

Method

In general, the bias of a statistic is given as the difference of the statistic's expectation and the true population value. Attrition bias in a mean statistic is defined as the difference of the estimated mean using the respondents only and the estimated mean of all sample cases. We estimate attrition bias directly using administrative data for respondents and nonrespondents of the survey as a gold standard:

$$\widehat{\text{bias}}(\bar{y}) = \hat{\bar{y}}_{\text{admin, respondents}} - \hat{\bar{y}}_{\text{admin, sample.}} \tag{1}$$

To be able to compare attrition bias statistics across variables, we use a measure of relative bias that equals the estimated bias standardized by the full sample mean of the respective variable.

$$\text{rel.}\widehat{\text{bias}}(\bar{y}) = \frac{\widehat{\text{bias}}(\bar{y})}{\hat{\bar{y}}_{\text{admin,sample}}} \times 100. \tag{2}$$

Based on estimates calculated as stated in equation (2), we will compare the relative biases between the lottery and the cash group. Confidence intervals are computed using bootstrap, and 10,000 bootstrap replicates are computed for each relative bias estimation. The bootstrap 0.025% and 0.975% empirical quantiles serve as the 95% confidence intervals.

Results

We find that the retention rate is higher for the unconditional cash group (85.54%) than for the conditional lottery group (79.39%), and the difference is statistically significant ($p < .001$). Concerning retention rates, we can see that in this experiment, incentives work in the expected direction. However, as an increasing retention rate does not ensure a decrease in attrition bias, we will next analyze relative attrition bias for personal income and several sociodemographic variables.

Figure 1 shows the relative attrition bias (including 95% confidence intervals) in income estimation for the two incentive groups using administrative data (for numbers, see Table 1 in the Online Appendix). While there is no significant relative attrition bias for any income category in the cash group, there is significant relative attrition bias for the lowest and highest income category in the lottery group. For the lottery group, the proportion of people in the high-income group is significantly overestimated and the proportion of people in the low-income group is significantly underestimated. Even though the relative attrition bias is significantly different from zero for these two income categories in the lottery group, the confidence intervals of the lottery and the cash group do overlap for all income categories and although overlapping confidence intervals do not necessarily imply that there is no significant difference (see, e.g., Schenker and Gentleman 2001), we take this conservative approach to evaluate differences between the two experimental groups.

We can see a clear age effect, but this is not significant for each single age category and incentive treatment: The proportions of the older age groups are overestimated while the proportions of younger age groups are underestimated (see Figure 2). We find significant relative attrition bias for the lowest (30 years and younger) and second highest (50–60) age group for both experimental groups, which is smaller for the cash than for the lottery group. The relative attrition bias for 60 and older, however, is only significantly different from zero for the lottery group.

As for income and age, the confidence intervals for all of the sociodemographic variables overlap for the two incentive groups. Also, relative attrition bias is in the same direction for both experimental groups, except for the proportion of females, which is not significantly different from zero for either incentive group. Proportions of people who are born outside Germany are only significantly underestimated in the cash group. There is no significant relative attrition bias for the proportion of people having a mini job, the proportion of people being employed, and the proportion of people receiving UB II.

Summary and Conclusions

Our findings confirm that unconditional cash incentives increase retention rates compared to conditional lottery tickets. But what is more important, they produce less attrition bias in some of the key variables of the survey at the same time. Cash incentives have proved useful to decrease attrition bias in this low-income and benefit-related survey. Although our analysis focuses on the aggregate survey outcome and we do not know how incentives work at the individual level, we think that both leverage-saliency and opportunity cost theories could explain these findings. According to leverage-saliency theory, the impact of cash incentives should be highest for people who lack other motivation to participate in the survey and for whom money has the highest importance. Therefore, low-income people might overproportionately be attracted by the cash incentive. They also have lower opportunity costs for survey participation, and, according to economic theory, a modest incentive of €10 will be more attractive to them as compared to people who have higher opportunity costs for participating in a survey.

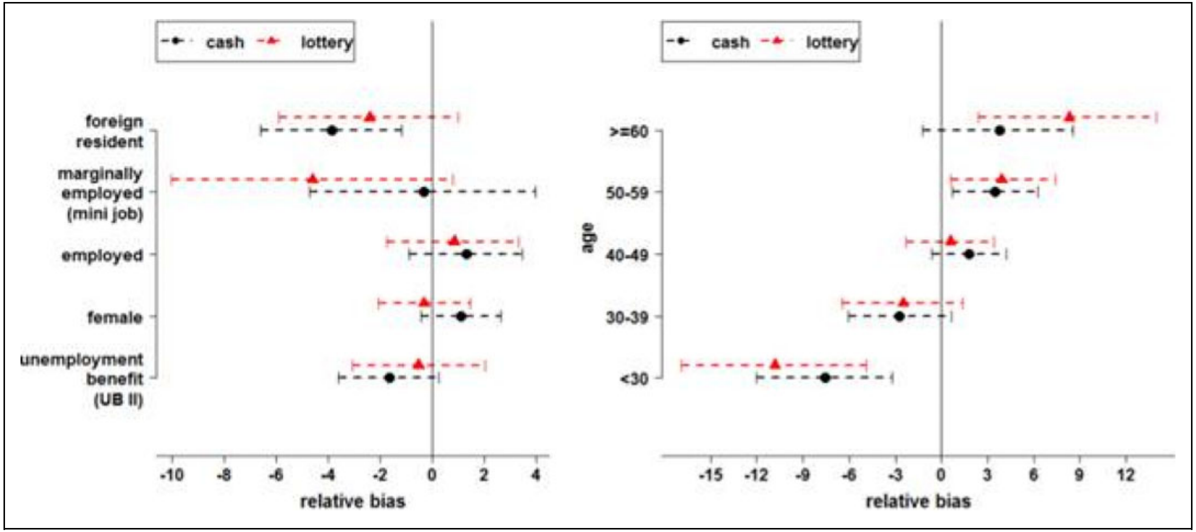

Figure 2. Relative attrition bias in the estimation of the proportions of people belonging to the sociodemographic groups (left-hand panel) and people falling into the five age categories (right-hand panel).

We do not have a clear interpretation for the unexpected finding that the proportion of foreign residents is significantly underrestimated for the cash group only. A possible explanation could be that non-German residents did not understand why they were sent money and became suspicious.

Our empirical approach has some limitations. The experiment varies two incentive characteristics that cannot be separated, namely, conditional versus unconditional and cash versus in-kind incentives. Further research is needed, ideally implementing a fully crossed experimental design comparing the four possible combinations of cash versus in-kind and conditional versus unconditional incentives. The third-wave attrition bias we analyzed in this article might be different from nonresponse bias in the first wave of a panel or in a cross-sectional survey as incentives might work differently on initial nonresponse and attrition. As Laurie and Lynn (2009) argue, panel attrition in previous waves leaves a sample of loyal respondents showing high interest in the survey already and therefore be less responsive to extrinsic incentives.

Future research should be conducted to show whether our findings hold for cross-sectional studies and whether incentive effects are found to be stronger or even found to affect demographic groups differently than for a panel study. Furthermore, the panel survey in which the incentive experiment was implemented oversamples low-income individuals. If incentive effects are mainly driven by economic interests, one could expect respondents with very high income, who are strongly underrepresented in our study, to be less responsive to incentives. The somewhat surprising finding that the proportions of high-income groups are overestimated in our panel might also be explained by oversampling of low-income groups. Our highest income tertile includes respondents who might achieve a middle income compared to the general population. Replicating the experiment for a high-income or general population sample may help us understand the mechanisms underlying the incentive effects better. In addition, we were only able to link sample cases from the commercial sample to the administrative data who gave consent. Since consent bias is usually found to be very small (Sakshaug and Kreuter 2012), and cases were randomly assigned to the experimental groups, differences between groups are not expected to be affected by consent bias.

Our findings are in line with the general finding that incentives have the greatest effect on respondents who show the lowest response propensity. Thus, we are confident that the findings are not unique to our study and will hold across countries. We do not think these general relations will change over time. However, future researchers could increase the incentive amount to counteract inflation when replicating the experiment.

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Supplemental Material

Supplementary material for this article is available online.

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