Reduction of Nonresponse Bias through Case Prioritization

Andy Peytchev[1], Sarah Riley[2], Jeff Rosen[1], Joe Murphy[1], Mark Lindblad[2]

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Abstract

Improving response rates to minimize nonresponse bias is often a key goal in survey research. How response rates are increased can determine the remaining nonresponse bias in estimates. Studies often target sample members that are most likely to be interviewed to maximize response rates. Yet the now widely accepted stochastic model for survey participation would suggest that this may not reduce nonresponse bias, further supported in the empirical literature. Counter to current practice, we suggest targeting of likely nonrespondents from the onset of the study with a different protocol, in order to minimize nonresponse bias. To achieve such targeting, various sources of information can be incorporated: paradata collected by the interviewers, demographic and substantive survey data from prior waves, and administrative data. Using these data, sample cases are identified on which a more effective, often more costly, survey protocol can be employed to gain respondent cooperation.

This paper describes the two components of this approach to reducing nonresponse bias, demonstrates methods used to create case priority assignments based on response propensity models, and presents empirical results from the use of a different protocol for prioritized cases. In a field data collection, a random half of cases with low response propensity received higher priority and increased resources to assure their completion. Resources for high-priority cases were allocated as interviewer incentives; random assignment was made within interviewer workload. The other half of these low response propensity cases served as the control group.

Introduction

Inference from probability surveys relies on the ability to obtain responses from all sample members. Invariably, this is hindered by unit nonresponse – the failure to interview all sample members. Response rates have been used as an indicator of the degree to which survey estimates may be biased by nonresponse. Response rates in household surveys have been declining (Groves and Couper 1998), while effort and cost has been increasing (Curtin, Presser, and Singer 2000). Survey organizations are under increasing pressure to increase response rates under cost constraints. This can lead to a blind pursuit of cases that are most likely to become respondents. Since nonresponse bias is a function of the association between the likelihood to respond (response propensity) and the survey variable, an uninformed approach to increasing response rates may not be successful in reducing nonresponse bias even if higher response rates are achieved – in fact, contrary to common expectation, nonresponse bias may be *increased* (Merkle and Edelman 2009).

The threat of this occurring in a blind pursuit of higher response rates is not impossible and more informed methods to target nonresponse bias can be constructed. Nonresponse bias in an estimate of the mean of a variable $\bar{y}$ based on survey respondents r can be expressed as the ratio of the covariance between the survey variable and the response propensity, $\sigma_{y,\rho}$, to the mean propensity, $\bar{\rho}$ (see Bethlehem 2002):

[1] RTI International (RTI International is a trade name of Research Triangle Institute)
[2] University of North Carolina Center for Community Capital

$$Bias(\bar{y}_r) \approx \frac{\sigma_{y,\rho}}{\bar{\rho}} \qquad [1]$$

Typically, survey effort is directed at increasing $\bar{\rho}$, under the assumption that $\sigma_{y,\rho}$ remains at least the same – that may often be inferred from parameterizing nonresponse bias in a mean as the expected value of the product of the nonresponse rate ($\frac{m_s}{n_s}$) and the difference between the means for the respondents and nonrespondents ($\bar{y}_r - \bar{y}_m$). This assumption is unrealistic and $\bar{y}_r - \bar{y}_m$ may actually increase to such a degree as to lead to greater bias in the estimate of $\bar{y}_r$ under the higher response rate (e.g., Merkle and Edelman 2009).

A more general fear in face to face surveys is that much of the control over which sample members are interviewed remains with the interviewer, whether through the ability to contact a sample member or to gain cooperation upon contact. Interviewers are often evaluated on their response rates and not on nonresponse bias in their sample – if all cases are equal in the evaluation of their performance, they should rightfully direct greater effort to sample members that they deem more likely to participate. Such effort may increase $\bar{\rho}$ by increasing the response propensity for sample members that tend to be likely respondents but have still not been interviewed, leaving those with very low propensities to remain nonrespondents – and the potential for leaving $\sigma_{y,\rho}$ relatively unchanged or even greater in magnitude.

Instead, consider the consequence of reducing the variability in ρ. In an extreme case, $\sigma_\rho^2 = 0$, or that in a particular survey, $Var(\rho) = 0$. Since $\sigma_{y,\rho} = E[Corr(y,\rho)\sqrt{Var(y)Var(\rho)}]$, the covariance $\sigma_{y,\rho}$ will also be equal to zero and nonresponse bias in an estimate of a mean, as presented in Equation 1, is eliminated, regardless of the achieved response rate. Certainly, effort will not be made to reduce the likelihood of participation for sample members with above average response propensities in order to reduce σ_ρ^2. However, effort, or a more effective but costly protocol, can be directed at sample members with the lowest response propensities. If successful, the variability in the response propensities will be reduced, the association between propensities and survey variables can be reduced, reducing nonresponse bias. There is an added benefit from this approach: nonresponse weighting adjustments in surveys are the inverse of the likelihood of participation, whether at the sample member or subclass level. Reducing the variability of the likelihood of participation, will also reduce the variability in nonresponse adjustments, and likely reduce the loss in precision due to weighting – increasing the effective sample size and improving the precision of survey estimates.

The operationalization and implementation of such an approach is challenging and includes two essential components: estimation of the response propensity, ρ, and the design of an intervention for sample members with low $\hat{\rho}$ that will increase it. Surveys vary in the amount and type of information available on respondents and nonrespondents, that can be used to estimate ρ. Sources include data from the census, administrative data, data from a previous survey administration, and paradata – auxiliary information collected during the field period, such as interviewer observations and respondent behavior. The utility of different data in directing survey effort or expense to reduce nonresponse bias is not equal. Variables predictive of ρ, but also associated with the survey variables are of greatest interest – if only predictive of ρ, such variables will not help the differentiation of sample cases with low response propensities that if not interviewed, would induce nonresponse bias. In this respect, using survey variables measured at an earlier point in time are of great value in the estimation of response propensities.

The second component in this approach is the construction of a survey protocol that will increase participation among sample members with low estimated response propensities. A common design feature that is known to increase response propensities is the use of respondent incentives. Studies routinely use incentives and new methods need to be identified that could increase survey participation. While much focus has been given to understanding why respondents participate in surveys and how participation can be influenced, more attention could be given to the other actor in the social interaction that occurs in the survey request in face to face surveys, and who is also responsible for initially establishing contact with the respondent. Face to face interviewers are trained in techniques to locate sample members, how to make the survey request, avert refusals, and convert prior refusals. Certainly, developing better training protocols can help increase response rates (Groves and McGonagle 2001). Yet motivation somewhat similar to that used on respondents may help them implement the learned skills to a greater degree – providing an incentive for each completed interview in addition to their hourly compensation. Indeed, such performance-based incentives are common in other professions such as in the retail industry, and have demonstrated a positive effect on performance – greater sales volume (e.g., Banker et al. 1996; Banker, Lee, and Potter 1996). There are at least three ways through which such incentives could help survey outcomes: interviewers may improve their strategies in contacting respondents, they may make more call attempts, and they may be more persuasive at the doorstep once a household member is reached. Although per interview incentives have been intermittently implemented in studies such as the General Social Survey (Carr, Sokolowski, and Haggerty 2006), there has been no experimental evaluation of their effectiveness.

We test both components of the proposed approach to reducing nonresponse bias – identifying cases with lower likelihood to participate and using interviewer incentives to increase cooperation rates. Four research hypotheses are considered:

1. Using paradata, administrative, demographic, and survey data, groups can be defined prior to data collection that will differ in response rates.
2. Increasing interviewer incentives for completing interviews with sample members with low predicted likelihood of participation will increase participation among this group.
3. Increasing interviewer incentives for completing interviews with sample members with low predicted likelihood of participation will decrease the variability in response propensity.
4. Increasing interviewer incentives for completing interviews with sample members with low predicted likelihood of participation will decrease the correlations between response propensity and key survey variables.

Data and Methods

The data for the current study come from the Community Advantage Panel Survey (CAPS)[3]. CAPS evaluates the Community Advantage Program (CAP), a secondary mortgage market program. To qualify for the CAP, borrowers must meet one of three criteria: (1) have income of no more than 80% of the area median income (AMI); (2) be a minority with income not in excess of 115% of AMI; (3) or purchase a home in a high-minority (>30%) or low-income (<80% of AMI) census tract and have income not in excess of 115% of AMI. Two separate samples of households across the United States were selected, one of renters and another of home owners, matched on geographic location and demographic characteristics. As of the end of the 2007 data collection year, the CAPS owner and renter panels had completed five and four survey administrations, respectively. The 2008 data collection period, which focuses on wealth and assets, saving behavior, mortgages, and housing experiences, began in July 2008 and included 2,795 owners and 1,088 renters. Cases were assigned to

[3] CAPS is funded by the Ford Foundation.

either an in-person computer-assisted interviewing mode or a computer-assisted telephone interviewing mode based on baseline subject characteristics and interviewing modes from prior rounds. For the 2008 survey, 2,191 in-person interviews were conducted. For these cases, data collection began in July 2008 and ended in February 2009. The overall response rate for the 2008 field data collection was 92.3% (RR1, AAPOR 2008)[4] with 59% female, 54% home owners, 54% non-Hispanic White, 51% married, with partner, or with companion, 66% with some education beyond high school, and 62% from North Carolina, a substantially oversampled state.

To test the proposed approach to boosting retention and to minimizing bias resulting from the loss of participants, we implemented a case prioritization scheme during the 2008 data collection period. Participants who were least likely to complete the 2008 survey were assigned to a low propensity subsample, and these cases were randomly assigned to receive special treatment.

To assign a priority score to each survey participant, we estimated response propensities by predicting the 2007 outcome for respondents who were eligible to be surveyed in 2008. We fit separate logistic regression models for owners and for renters. As possible predictors of 2007 survey response, we considered demographic characteristics, substantive survey variables, and survey paradata, such as interviewer observations during previous interactions. Item missing data were imputed using sequential regression imputation in IVEware (Raghunathan et al. 2001). Significant predictors were age, race, education, gender, mortgage delinquency, the amount of time that had elapsed since loan origination, the amount of time since purchasing the home, whether the respondent had voted in the 2000 election, whether the respondent had said they were not interested in the prior wave, and whether the respondent had ever hung up during the introduction in the prior wave.

Predicted probabilities for completed interview in the prior wave were used to divide each sample into two equal groups: a low and a high response propensity. However, due to concerns about unequal opportunities for compensation of field interviewers, as high propensity cases will generally have lower compensation per interview while response propensities vary geographically, this division was done within each geographic sample area. The low propensity groups were then randomly assigned to high priority or control condition. Within each sample, 50% of sample households were classified as low propensity, of which half (25% overall) were randomly assigned to be subjected to the experimental manipulation.

The experimental manipulation involved the doubling of the per-complete interviewer bonus payments given in addition to interviewers' normal hourly wages for low propensity cases. We believed that the potential for greater reward for completion of high priority cases would motivate interviewers to devote extra effort in their strategic approach to these cases. For the 2008 CAPS, field interviewers received a $10 bonus payment for each completed control interview and a $20 bonus payment for each experimental interview during the first phase of data collection, which lasted 6 weeks. For the second phase of data collection interviewers received no bonus payment for control interviews and $10 for experimental interviews. Finally, for the third phase, conducted during the last 8 weeks of data collection, interviewers received the original $10 and $20 bonus payments for each completed interview. In order to avoid delayed action on the part of the interviewers, they were not notified in advance that bonuses in the third phase of data collection would be offered. In interviewer training, interviewers were instructed not to devote inordinate efforts to complete the experimental cases, but to note that these cases were very important for the successful completion of the project. Interviewers were told to work all cases, both control and experimental, with strong efforts.

Overall, the CAPS survey is designed to allow an evaluation of the effects of homeownership on health, social, and financial outcomes in the target population. In the 2008 CAPS interviews, key

[4] This response rate is conditional on sufficient participation in the previous waves and is based only on the sample in this wave; due to the complex criteria for inclusion in each wave of data collection, an overall response rate could not be calculated.

questions concerning health and financial characteristics were administered. Therefore, in our analysis of nonresponse bias, we consider such health-related measures as self-rated general health, whether the respondent had recently experienced a physical or mental disability, and whether the respondent participated in an employer-sponsored medical insurance plan. In addition, our selected financial measures concern financial literacy, financial control, financial well-being, and self-rated thriftiness, as well as financial habits, such as whether the respondent uses credit cards only for emergencies or sends money to friends and family.

While this approach relies on both the ability to estimate response propensities and the degree to which they can be altered, the two components can be evaluated somewhat independently. We first look at the response rates for groups defined by estimated response propensity, i.e., how well $\hat{\rho}$ translates into the actual outcome, r. We then turn to differences in success rate in contacting and gaining cooperation among those with response propensity estimated to be low prior to the onset of data collection, i.e., whether the interviewer incentive manipulation can alter $\bar{\rho}$, as well as any differences in the amount of effort interviewers seem to be exerting. Finally, we turn to our ultimate objectives – whether the variance of the response propensity, $Var(\hat{\rho})$, can be lowered and whether the association between the response propensity and survey variables, $Cov(y, \rho)$, can be reduced, thus minimizing nonresponse bias in survey estimates of means and proportions. To achieve this, we created sample weights for two scenarios: increasing the weights for respondents in the control group by a factor of two and omitting the experimental group, and similarly, increasing the weights for respondents in the experimental group and omitting the control group.

Results

We first evaluated how predictive were the estimated response propensities of the actual outcomes in the survey. This comparison is limited by a "ceiling effect" as the overall response rate was 92.3%, yet the half of the sample with low estimated response propensities (assigned within interviewer workload[5]) had a significantly lower response rate (90.3%)[6] compared to the other half of the sample (94.3%), $\chi^2(1)=11.909$, $p<0.001$. In light of the high response rate, as many sample members who are less likely to participate have already attritted on earlier waves of data collection, these results can be seen as encouraging for the prediction of response outcome in a longitudinal survey, when data from multiple sources are used – the nonresponse rate was almost double among those with lower response propensities.

The second critical component of the approach is increasing participation among those with low estimated response propensities. Among sample members with low propensities, we found no significant difference in response rates between the control and experimental conditions, with 89.8% and 90.8% response rates, respectively ($\chi^2(1)=0.335$, $p=0.563$). Given the relatively long field period that was extended to over six months, which allowed ample time for all cases to be attempted multiple times and refusal conversion attempts made, we also looked at interview completion over the course of the data collection period.

The cumulative number of interviews completed by day in the field for the high propensity, low propensity control, and low propensity experimental conditions are presented in Figure 1. While the high propensity cases were completed much faster early into data collection, as expected, the rates of completion in the control and experimental condition are ostensibly identical.

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[5] Although somewhat surprisingly, the response rates were identical to these when low and high estimated response propensity group was assigned without balancing assignment across interviewers.

[6] Control and experimental conditions were pooled due to their similar rates, as reported in more detail in the following paragraph.

<Figure 1 about here>

We had identified different ways in which interviewer incentives could influence data collection and while increasing survey participation is a primary objective, it may at least increase obtaining contact with sample members. Since together in the control and experimental conditions only three cases were not contacted, we could once again examine the cumulative contact rates over the course of data collection. These curves, however, had the same pattern as for the interview rates in Figure 1, with virtually identical rates for the control and experimental conditions.

Even if the survey outcomes were not altered for this sample, it is possible that interviewers exerted more effort and gave higher priority to cases that would yield them greater compensation. Yet we find that the average number of call attempts in the two conditions are not different – 4.9 in the experimental condition and 5.0 in the control condition, *t*-test(1066)=.21, p=.837.[7]

Finally, we examined the key outcomes that would follow from the theoretical basis of our approach. Despite the lack of effect on response rates, we expected to find that the variance of the response propensities and the associations between the response propensities and survey variables had been reduced.

We found the hypothesized effect on the variance of the resulting response propensities in each group, with a standard deviation of $\hat{\sigma}_\rho = 0.070$ in the control group scenario[8] and a significantly lower standard deviation of $\hat{\sigma}_\rho = 0.054$ ($F(1650,1650)=1.66$, $p<.001$) in the experimental group scenario. This difference was not driven by differences in the mean propensity in each group, which was 0.915 in the control group and 0.917 in the experimental group ($t(3300)=0.97$, $p=.33$). These response propensities were estimated using the same predictors employed in the estimation of the response propensities prior to data collection, only using the outcome in the current wave. It is possible that, by chance, the two groups differed in their variability in propensity even prior to data collection. We found no such difference, with $\hat{\sigma}_\rho = 0.149$ in the control and $\hat{\sigma}_\rho = 0.146$ in the experimental condition ($F(1650,1650)=1.05$, $p=.32$). The results were almost identical for the samples of owners and of renters.

Unless the correlations between the response propensities and the survey variables were impacted so that they became greater in magnitude in the experimental group, we should expect less nonresponse bias in survey estimates through the experimental treatment. Unfortunately, a greater number of the correlations were significant in the experimental scenario (columns 4 and 5 in Table 1), and most of the correlations were significantly larger in the experimental scenario (column 6).[9]

<Table 1 about here>

With lower variation in the response propensities but larger correlations between the propensities and the survey variables under the experimental treatment, nonresponse bias can be

[7] Four cases in the priority condition and four cases in the control condition were excluded for having extremely high number of call attempts (over 25). Findings do not change when these cases are left in the analysis.

[8] Recall that the control group cases are assigned twice the weight of the high propensity cases to compensate for the exclusion of the cases assigned to the experimental treatment, creating a control "scenario" with weights that sum to the full sample.

[9] This test assumes two independent groups – since the two scenarios overlap in the high propensity cases, this is a conservative test and does not affect the conclusions.

affected in either direction.[10] Unfortunately, in this instance, the lower variability in the propensities was not sufficiently lower, and the estimated nonresponse bias was somewhat higher for eight of the twelve estimates, shown in columns 7 and 8 of Table 1. This result, combined with the equally high response rates achieved in the control and experimental conditions, lead us to believe that the lower variability in response propensities found in the experimental group is a spurious effect. Thus, it leaves the question of whether nonresponse bias could be reduced through this approach in studies with lower response rates, awaiting further research.

Discussion and Conclusions

We were fairly successful in estimating response propensities prior to data collection that were predictive of the survey outcome that was later observed, achieving the first of the two steps in the proposed approach. Once cases were prioritized based on low response propensity, however, our manipulation of offering higher interviewer incentives for completed interviews was not successful. This null effect could have multiple explanations. It could be that the field period was sufficiently long that maximal effort was exerted on all sample cases. This explanation is unlikely, however, as the cumulative number of interviews during the early part of data collection was almost identical for the control and experimental conditions. It is much more likely that interviewers were well-trained and did their best regardless of which cases yielded higher compensation to them because they knew that they would get to attempt all cases in their workloads. It is also possible that higher interviewer incentives for some sample cases motivated interviewers to try those cases earlier and with greater effort, but that this effect was countered by a possible perception that those cases were more difficult and, therefore, were to receive special treatment. With almost identical response rates in the control and experimental conditions, differences in the variance of the response propensities were likely by chance, and a reduction in nonresponse bias could not be observed. Starting with a cross-sectional sample of the general population might help to alleviate some of these limitations. Certainly, obtaining data on all nonrespondents would relax the implicit assumption that the association between response propensities and the survey variables is the same for respondents and nonrespondents.

There are three lines of work that we think merit further pursuit. First, interviewer incentives are ill-understood and have received little attention in the research literature, relative to respondent incentives. The mechanisms though which they may act on interviewer response rates and nonresponse bias are different from those on those that act on respondents, as interviewers and respondents have very different roles in the social interaction at the doorstep. Further research is needed to explore how, and under what circumstances, interviewer incentives could help achieve survey goals.

Second, regardless of research on interviewer incentives, effective interventions for cases with low response propensities are needed in order to succeed in the second step of the proposed approach to reducing nonresponse bias. Such interventions are not limited to incentives, and their effectiveness varies across target populations, modes of data collection, and other major study design features. Methods to simply increase response rates are not sufficient, as likely nonrespondents can remain nonrespondents at the conclusion of the study.

Last, but not least, is a possible modification to the theoretical approach employed in the current study. We believe that focusing on cases with low response propensities, while theoretically justified to reduce nonresponse bias, may not be sufficiently efficient in some studies. Rather, especially in surveys with substantially lower response rates, it may be more efficient to identify cases

[10] A third term in the equation for nonresponse bias [1] is the variance of the survey variable – this was not different by experimental treatment for all but one of the survey variables, which is within the error rate allowed at α=.05.

with the greatest potential to induce nonresponse bias in selected survey variables. These cases are not only likely nonrespondents, but are also expected to have survey values different from those based on the likely respondents.

In sum, we believe that survey practitioners benefit from thinking in terms of nonresponse bias as opposed to response rates, and that the logical extension is to implement study designs that reduce nonresponse bias, not necessarily maximizing response rates. In this regard, it seems critical to recognize that not all cases are equal, and targeting of nonrandom, although stochastically selected, subsets of cases may help achieve the goal of minimizing nonresponse bias in survey estimates.

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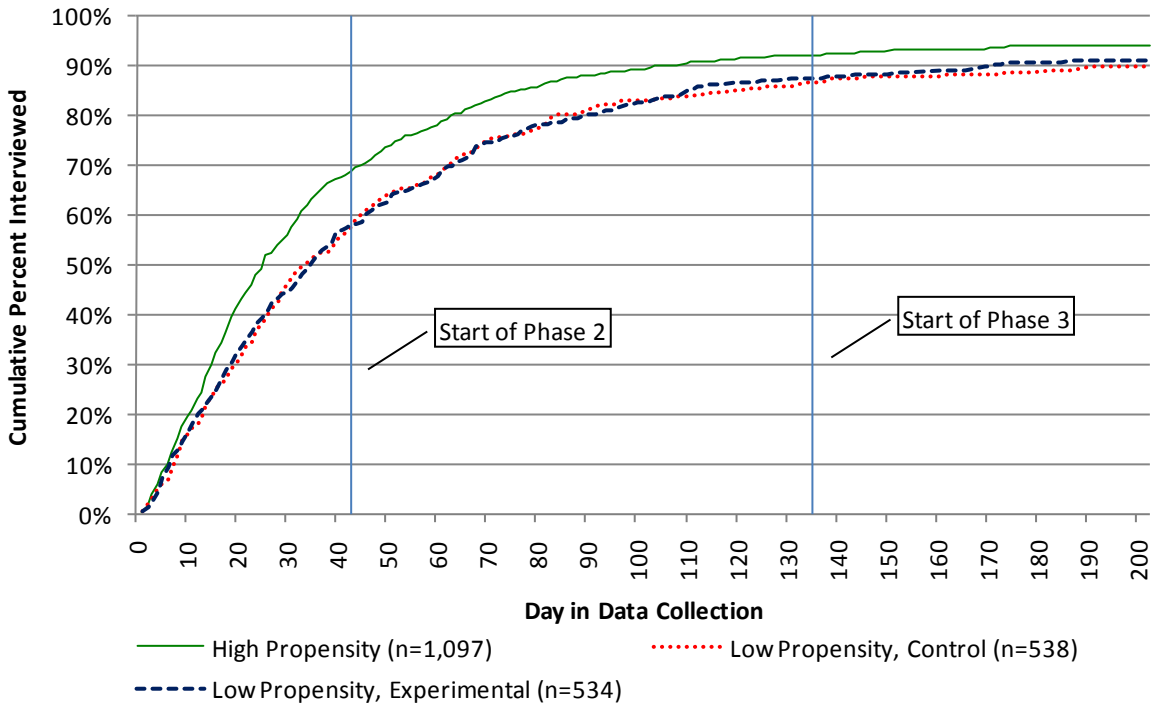

Figure 1. Cumulative Number of Interviews Completed by Day in the Field for the High Propensity, Low Propensity Control, and Low Propensity Experimental Conditions.

Table 1. Weighted Correlations between Response Propensities and Survey Variables, and Estimated Nonresponse Bias in the Control and Experimental Scenarios.

Survey Variable	**n**		$\sqrt{\hat{\sigma}_{y,\rho}}$			$\hat{B}ias(\bar{y}_r)100 \approx \frac{\hat{\sigma}_{y_r,\rho}}{\bar{\rho}}100$	
	Control	**Exper.**	**Control**	**Exper.**	**Abs(Diff.)**	**Control**	**Exper.**
Health							
Self-rated health	1,520	1,522	0.056*	-0.093***	0.149***	0.446	-0.579
Limited physical activity	1,519	1,521	-0.030	0.050*	0.080*	-0.083	0.109
Recent emotional problem	1,518	1,519	-0.035	-0.028	0.007	-0.089	-0.058
Employer-provided health ins.	1,518	1,522	0.008	-0.156***	0.164***	0.031	-0.429
Finances and Wealth							
Financial status vs. parents	1,502	1,504	0.027	-0.060*	0.087*	0.253	-0.431
Feels informed about finances	1,519	1,520	0.038	-0.046	0.084*	0.214	-0.206
Feels thrifty	1,518	1,520	0.029	0.081**	0.052	0.213	0.477
Feels on top of finances	1,517	1,519	-0.026	-0.109***	0.083*	-0.209	-0.678
Credit cards only for emergency	1,507	1,512	0.052*	0.131***	0.079*	0.465	0.924
Sent money to others	1,517	1,518	-0.063**	0.076**	0.139***	-0.211	0.197
# of relatives who own a home	1,517	1,518	0.037	-0.151***	0.188***	0.239	-0.764
Household # of vehicles	1,519	1,521	-0.121***	-0.020	0.101**	-1.114	-0.137

* Significant at α=.05; ** Significant at α=.01; *** Significant at α=.001