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Heaping at Round Numbers on Financial Questions: The Role of Satisficing

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ABSTRACT

Survey responses to quantitative financial questions frequently display strong patterns of heaping at round numbers. This paper uses two studies to examine variation in rounding across questions and by individual characteristics. Rounding was more common for respondents low in ability, for respondents low in motivation, and for more difficult questions, all consistent with theories of satisficing. Questions that require more difficult information retrieval and integration of information exhibit more heaping. The use of records, which lowers task difficulty, reduces rounding as well. Higher episodic memory is associated with less rounding, and standard measures of motivation are negatively associated with rounding. These relationships, along with the fact that longer response latencies are associated with less rounding, all support the idea that rounding is a manifestation of satisficing on open-ended financial questions. Rounding patterns also appear remarkably similar across the two studies, despite being fielded in different modes and employing different question order and wording.

JEL codes: C83, C81

Keywords: Data collection and estimation, consumer surveys, satisficing

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1. Introduction

Survey responses to open-ended quantitative questions frequently display strong patterns of heaping on round numbers; that is, numbers ending with one or more zeroes. Heaping has been documented for self-reported ages (Heitjan and Rubin, 1990), smoking (Wang and Heitjan, 2008), expectations (Manski and Molinari, 2010), consumption (Battistin et al., 2003), behavioral frequencies, personal characteristics, feeling thermometer reports (Holbrook et al., 2014), income (Schwabish, 2007), and wealth (Eggleston, 2015).

In contrast, heaping is not evident in administrative data on these financial topics. Figure 1 compares a histogram of self-reported mortgage balances in the Survey of Consumer Finances (SCF) to a histogram of mortgage balances in the Federal Reserve Bank of New York's Consumer Credit Panel/Equifax. While SCF data exhibit heaping at round numbers, the administrative data are much smoother. Similarly, administrative earnings data in the United States have a much smoother distribution (less heaping) than self-reported earnings on surveys (Riddles et al., 2016). These patterns imply that heaping on round numbers on financial questions likely reflects survey reporting behavior, and not the underlying distribution of true values. Furthermore, in survey data on brokerage account balances linked with the individual's actual account balances from their financial institution, precise survey responses are much closer to administrative values than imprecise responses (Gideon et al., 2015).

Differences between rounded survey responses and underlying true values can be understood in a total survey error framework, which provides a tool for thinking about the many sources of error in survey data. Survey designers must perform a careful balancing act to maximize survey quality and usefulness. One of many sources of error discussed in this framework is measurement error, an error of observation which arises at the point of interaction

between the survey respondent and the survey interview (Groves, Fowler, Couper, Lepkowski, Singer and Tourangeau, 2004, pp. 49-53). In this article, we focus on just one possible source of measurement error, error from providing rounded responses to quantitative financial questions. We examine predictors and possible cause of this type of measurement error. While this source of error may appear to be minor, error from rounding can potentially have great impact on common estimates and inference using household financial data from surveys.

Data that are heaped on round values are problematic for researchers because they are considered "coarse" (see Heitjan and Rubin, 1991), and valid inference and estimation depend on the coarsening mechanism, as is the case with missing data. Rounding behavior in response to survey questions is one possible coarsening mechanism. It may be correlated with unobservable individual characteristics and result in biased estimates of regression coefficients. Rounding can also lead to biased national statistics, particularly when they involve estimating the share of the population below (or above) a fixed threshold, including calculation of poverty rates and program eligibility. For example, the 2015 federal poverty threshold in the United States for a family of four was $19,096 in annual income (U.S. Census Bureau, 2015). Consider households with true incomes of $19,075 and how they respond to a government survey conducted to estimate the poverty rate. Rounding to the nearest 5,000 or 10,000 would incorrectly count them as out of poverty, rounding to the nearest 1,000 would correctly count them as in poverty, and rounding to the nearest 100 would again incorrectly count them as out of poverty. Arbitrarily small changes in the threshold from just below to just above a round number can lead to large changes in the poverty rate when measured using heaped data that would not occur if the underlying smooth true distribution were used.

Fixed thresholds, as are used for income tax brackets, can also create problems in economic research. Crossing from one tax bracket to the next leads to a discrete change in marginal tax rate. As discussed in Gideon (2014), measurement error in income leads to misclassification of "true" marginal tax rates, as economic researchers using survey data will often impute these tax rates using respondent-reported income. Whether thresholds are above or below rounded numbers or focal points favored by respondents will lead to asymmetric misclassification of marginal tax rates.

Despite widespread evidence of heaping, and the importance of its consequences if caused by rounding behavior, little is known about its causes. A common hypothesis is that heaping is the result of cognitive shortcuts taken by respondents. While Holbrook et al. (2014) found little evidence that heaping on non-financial quantitative questions occurred more frequently under conditions thought to foster satisficing, to our knowledge there are no studies analyzing the role of satisficing in response heaping on financial questions. In another related paper, Couper et. al (2013) find less round number reporting among respondents who consult records, though the paper does not directly address the role of satisficing.

Given the possible link between more precise responses and data quality, we test whether and to what extent round number responses are consistent with satisficing behavior using several open-ended financial questions on two surveys. Better understanding of heaping on financial questions has far-reaching implications, as such questions are used to compute federal economic statistics and have long been used in econometric studies to shed light on economic decisions and behavior.

Satisficing theory and heaped survey responses

Herbert Simon first proposed the idea of satisficing in the context of optimization in economic decision-making (Baumol, 1979). Applied to survey response, Krosnick (1991) posited that respondents provide a "good enough" answer when an optimal response requires substantial cognitive effort. He argued that satisficing could explain a wide range of respondent behaviors on attitudinal surveys, such as selecting the first reasonable response option, choosing the response that aligns with the status quo, or not differentiating among objects in ratings.

Many hypotheses about satisficing describe behavior on closed-ended questions but extend naturally to the case of open-ended, quantitative financial questions. These hypotheses suggest that satisficing may cause the observed patterns of heaping on round numbers. To analyze whether responding to financial questions with round numbers is a consequence of satisficing, we begin with the definition proposed by Krosnick (1991) that posits that the probability of satisficing may be expressed as:

$$p(satisficing) = \frac{a_1(task\ difficulty)}{a_2(ability) \times a_3(motivation)}.$$

Krosnick expected *task difficulty* to be related to the difficulty of the retrieval process and the complexity required to come up with an answer at the judgment phase of response. *Ability* was expected to be higher for those of high cognitive ability, especially for ability measures related to "retrieving information and making judgments," and the amount of practice an individual has in thinking about the question topic. *Motivation* was expected to be higher for respondents who think it is important to report precisely, or who enjoy thinking about or discussing the topic at hand. For example, Krosnick (1991) posited that respondents for whom the topic is particularly salient, those who believe the interviewer wants or the survey instructions seem to place importance on greater precision, and also those with higher *need for*

cognition (a measure of the extent to which individuals enjoy thinking or expending cognitive effort; see Cacioppo and Petty, 1982) would have higher motivation to provide optimal responses. Additionally, motivation may be decreasing in the time elapsed since the beginning of the interview and in the number of questions already asked (Krosnick, 1991).

Empirical studies have generally found evidence consistent with these hypotheses in closed-ended questions (see, e.g., Krosnick, Narayan, and Smith, 1996; Marcus and Schütz, 2005; Krosnick, et al., 2002; Holbrook et al., 2007; Bishop and Smith, 2001). While most of these studies focus on questions that have a fixed set of response alternatives, the framework extends naturally to questions with numerical responses. A paper closely related to ours examined heaping and satisficing on non-financial quantitative questions with a large set of response options in interviewer-administered surveys (Holbrook et al., 2014). Our paper aims to understand heaping on quantitative, open-ended questions, specifically on financial topics.

Rounding in the context of quantitative financial questions

To better understand the mechanisms at play, we discuss the role of question-level variation in *task difficulty*, and individual-level variation in *ability* and *motivation* from Krosnick's (1991) equation in the context of Tourangeau's (1984) four stages of response.

We define three types of quantitative financial questions, which we call knowable questions about aggregated values, knowable questions about single values, and unknowable questions. These questions have different levels of task difficulty at each stage of response, summarized in Table 1. We focus on the second through fourth stages of response, since we hypothesize that difficult comprehension of financial questions is primarily manifested in item nonresponse rather than through rounding on a response.

[Insert Table 1 here]

In the second stage, respondents retrieve information needed to answer the question. In all types of financial questions, information can be retrieved from memory, records, other resources (such as websites), or another person (such as a family member). Regardless of the retrieval method, the amount of information needed varies across types of questions, which affects the difficulty of the retrieval stage.

Knowable questions refer to specific values that can be directly verified through account records. These questions can be split into those asking about a single account, which require the retrieval of a single number, and those that ask about aggregate totals of multiple accounts, which require more information retrieval. For example, the value of monthly Social Security benefits is a single number that could be found relatively easily on a bank statement, if accessible, or drawn from memory. In contrast, questions about total annual household income require the retrieval of the amount earned from all sources of income during the time period. Therefore, information retrieval is likely least difficult for single-account questions. For simplicity, we treat knowability as a fixed attribute of the question, but it is possible that a question might be knowable for most respondents but unknowable for others.

Unknowable questions cannot be directly verified through records. In both studies, questions with "unknowable" values include opinions on home values, or estimates of weekly expenditures on food. Answering unknowable questions generally requires multiple pieces of information, but the respondent must subjectively assess what information she needs, as the question itself may not provide direction on how the respondent should approach the retrieval stage. How the difficulty of retrieval for unknowable questions compares to aggregated knowable questions likely depends on the specific question, and the particular financial situation of the respondent. For example, if asked about the value of one's home, respondents may retrieve

information in one or more of the following ways: consider the original purchase price and perceived changes in home value; use recent sales prices in the neighborhood; consult external sources like Zillow that provide estimated valuations of individual homes; some combination of the above; or something else entirely.

Next, respondents integrate the information retrieved. For knowable questions, integration is a concrete process, generally adding together values of knowable components. Questions about single accounts do not require additional integration beyond locating the single value, so may be less difficult. For unknowable questions, the integration stage is less concrete, since the value is typically not a sum of knowable components. Unknowable values require assessment, estimation, or inference, making this stage more difficult than for knowable questions. The idea of answers being unknowable is related to the concept of uncertainty. Ruud et al (2014) find experimental evidence that increasing uncertainty about an underlying answer induces more rounding.

Finally, during the response formulation stage, we expect the differences across questions types to stem from privacy concerns. If respondents are concerned about being identified by their responses, we expect people to want to round the most on questions involving individual accounts. On the other hand, if people have a general aversion to sharing financial information, even anonymously, then we do not expect differences in this response formulation process across question type.

Given the variation in the difficulty of response across question types, we hypothesize that unknowable questions and aggregated questions will exhibit more rounding than single-account questions if rounding is evidence of satisficing, while single-account questions may exhibit more rounding if privacy considerations are particularly important.

Respondent-level variation in ability and motivation may affect satisficing behavior on open-ended financial questions. While the role of motivation is likely similar for financial questions as it is for close-ended questions, the role of ability might depend on the specific topic of the question. In particular, higher ability in the types of cognition most related to the survey topic may result in lower cognitive burden and therefore less frequent satisficing behavior. We hypothesize that quantitative reasoning ability, which has been shown to be associated with higher levels of financial literacy and wealth (McArdle, Smith, and Willis 2011), is negatively related to satisficing on financial questions. Specifically, satisficing on unknowable questions might display a stronger negative relationship to quantitative reasoning ability or education. Stronger episodic memory may predict easier recall, and be negatively related to satisficing in knowable questions.

In sum, heaping on round numbers is consistent with satisficing if it is positively related to measures of task difficulty and negatively related to measures of respondent ability and motivation.

2. The Current Research

We use two studies to evaluate potential causes of response heaping at numbers ending in zeroes in open-ended, quantitative financial questions. We will present and discuss data, methods and results by study, then discuss what can be learned by considering results from both studies together.

Measuring round number reporting

We propose a standardized measure that enables cross-question comparisons in level of rounding. This measure addresses the fact that the range of possible answers to quantitative financial questions is infinite, and yet some ranges are much more common for particular types

of questions than others (for example, weekly food expenditures are likely to fall between zero and a few hundred dollars, while home values may range from tens of thousands of dollars to millions of dollars). Specifically, we measure the roundness of numbers on a scale based on the number of trailing zeroes as a fraction of the total number of digits in the response. This roundness scale is defined as

$$rounding = \frac{m}{(m-1)} \times \frac{(m-n)}{m} = \frac{(m-n)}{(m-1)}.$$

Here, *n* stands for the number of significant digits reported, and *m* stands for magnitude, the maximum number of significant digits that *could be* reported. For example, if a survey response is *$3,000*, $rounding = \frac{4-1}{4-1} = \frac{3}{3} = 1$, while a response of $3,230 yields $rounding = \frac{4-3}{4-1} = \frac{1}{3} = 0.\overline{33}$, and a response of $3,233 yields $rounding = \frac{4-4}{4-1} = \frac{0}{3} = 0$.

There are a few important things to note about this measure. First, *rounding* ranges from zero to one, where a higher value for *rounding* indicates coarser values with more trailing zeroes, while a lower value indicates more precise values with fewer trailing zeroes. Second, our measure distinguishes between answers with the same number of zeros at the end by using the total number of digits in the response. This is in contrast to Holbrook, et al. (2014), who only look at the last digit of a response. This approach would assign the same level of rounding to responses of $30 and $124,740, whereas our approach assigns a value of 1 and 0.2, respectively. Next, for single digit responses, *rounding* is undefined. This is the case for 18 person-question response values in Study 2 (out of 5,288) and 237 out of 29,959 values in Study 1. In our analyses, we present results in which we have re-coded these values as 0. However, results are nearly identical if these observations are excluded. Last, we use the word "rounding" to describe the level of precision of dollar-value responses. The rounded response may be a result of mathematical rounding of the true ("optimal") value to the nearest ten/hundred/thousand dollars,

truncation of the true value, or some other process, and we do not make any assumptions about which process is used by respondents.

Using person-question level data, we analyze how *rounding* is influenced by task difficulty— which we hypothesize varies between knowable and unknowable questions, and, among knowable questions, and between single-account and aggregate value questions (Study 1 and 2). We also analyze the role of ability using detailed cognitive measures (Study 2), and the role of motivation using Need for Cognition (Study 2). We consider whether the respondent is responsible for managing finances, which affects the salience of the questions and therefore task difficulty, motivation and ability (Study 2). We also consider other potential determinants of *rounding*, including the use of financial records and demographic characteristics (Study 1 and 2).

Measuring rounding as a response strategy

Whether respondents employ rounding as a general response strategy can be measured by the intraclass correlation. A high intraclass correlation means that respondents who round highly for one question are more likely to round highly for other questions as well.

Study 1

We employed data from the 2013 wave of the Survey of Consumer Finances, a triennial cross-sectional survey sponsored by the Federal Reserve Board to measure the financial circumstances of U.S. households.

Methods

Respondents*:* Respondents were 5696 adults, drawn from a dual-frame sample that includes a multi-stage national area-probability sample and a separate stratified list sample designed to oversample wealthy Americans.

Procedures: Data were collected from April 2013 to March 2014 by about 200 interviewers. Interviews were primarily conducted face-to-face, though respondents were given the option of completing the survey by telephone. About one third of respondents chose telephone mode. Topics included household demographics, financial attitudes and expectations, and a detailed battery of questions on income, assets, liabilities and net worth. See Bricker, et al (2014) or http://www.federalreserve.gov/econresdata/scf/scfindex.htm for more information about the SCF.

Measures: We restricted our analysis to a set of financial questions comparable with those asked in the other survey used in this paper: the dollar value of new credit card charges, credit card charges outstanding, home value, mortgage outstanding, food consumed at home, food consumed away from home, the largest checking account, the largest savings account, Social Security income for self, and Social Security income for spouse. Question wording is presented in Appendix A.

We analyzed responses to dollar-value questions that were provided as "exact values"; that is, we excluded responses in the form of a range or via unfolding brackets. Responses that were missing due to skip logic were also excluded.

Interviewers coded how often respondents consulted records when completing the survey. Education was used as a proxy for respondent ability (see Narayan and Krosnick, 1996).

Analysis: We examined patterns of rounding by question type, using the *rounding* variable described above. We then estimated random-effects regressions of respondent-question level observations of rounding, by respondent, with interviewer-level fixed effects. Regressions were estimated separately for the full sample and subsamples of question types (single account

versus aggregated; knowable and unknowable questions). All regressions include variables for referring to records (never, rarely, sometimes, frequently); indicators for each question; indicators for number of total digits of the response, age category (less than 35, 35 to 44, 45 to 54, 55 to 64, 65 to 74, and 75 and over), education category (less than high school, high school diploma or GED, some college, college degree or more), marital status, and nonwhite status; log financial assets; and an indicator for telephone mode. As a robustness check, we estimated multi-level random-effects regressions of respondent-question level observations of rounding, with random effects for respondents as well as interviewers. We also analyzed tobit models, as well as logit and linear probability models using a binary form of *rounding* that takes the value of 1 when *rounding* is one (ie, the coarsest possible value) and 0 otherwise. Results from all robustness checks are reported in Appendix B.

Results

In the SCF, about half of the responses we analyzed were reported with a single significant digit— that is, *rounding* value of 1. The other half of responses were distributed across the rest of the range of *rounding*, from 0 to less than 1. Table 2 displays means of *rounding* for each question, as well as across question types. All questions exhibit rounding; Social Security income has the least rounding, and checking accounts exhibit the most.

[Insert Table 2 here]

Across question types with varying difficulty, knowable questions exhibited marginally less rounding than unknowable questions (means of 0.77 and 0.80). Single-account questions exhibited marginally less rounding than aggregate questions (means of 0.76 and 0.79). Though the differences are small, both two-tailed t-tests yielded p-values of less than 0.0001.

Unknowable questions exhibited slightly more rounding than aggregated-knowable questions ($p<0.10$).

Table 3 presents regression coefficients for random effects regressions, pooling together all questions, across respondents. Column (1) presents results using all questions, while columns (2) through (5) restrict the analysis to unknowable, knowable, aggregate, and single-account questions, respectively.

[Insert Table 3 here]

The intraclass correlation (rho), which indicates the level of correlation in *rounding* within respondents, was 0.07 for the entire sample, including all covariates. It was much higher for knowable questions (rho=0.15) and single-account questions (rho=0.17), with aggregate questions just slightly lower (rho=0.13), suggesting that people round to similar levels as a response strategy within each of these three types of questions. By contrast, the intraclass correlation was lowest for the unknowable questions (rho=0.08), suggesting that there is less consistency within person in the approach taken to unknowable questions.

Computing the unconditional intraclass correlations without covariates yielded similar results across the question groups. Estimating a multi-level model with both respondent- and interviewer-level random effects for all questions yielded an interclass correlation of 0.01 for interviewers, and 0.07 for respondents. The multi-level model, along with other robustness checks, yielded similar results for the various covariates; see Appendix B for full estimates.

The most consistent predictor of rounding across question type was consulting records. As seen in column (1), a respondent who frequently referred to records exhibited less rounding than someone who never consulted records (coefficient=-0.10, $p<0.01$), consistent with the patterns found by Couper et al (2013) using the 2009 Health and Retirement Study Internet

Survey. The use of records was particularly associated with less rounding for more difficult questions. When analysis was restricted to knowable questions (column 3), frequent use of records was associated with less rounding (coefficient=-0.15, $p<0.01$).

The SCF did not directly measure ability, so we followed other research and used respondent educational attainment to proxy for ability. In general, the results do not show clear patterns by education. Looking at all questions in column (1), more educated groups were statistically indistinguishable from the least-educated group. College educated respondents rounded more on knowable than unknowable questions, and they also rounded more for single than aggregate knowable questions.

We did not find strong demographic determinants of rounding. Married respondents did not consistently round more, even though married couples' finances are likely to be more complex than those of unmarried respondents. Some specifications (restricting the analysis to unknowable questions) suggested that older respondents round less, but this is not consistently statistically significant. Respondents with greater financial assets were more likely to round (coefficient=0.01, $p<0.01$, from column 1).

Respondents on telephone interviews rounded more than those on in-person interviews (coefficient=0.02, $p<0.01$, from column 1), and these effects were similar across all specifications.

Study 2

Our second study allowed us to delve more deeply into the roles of ability, motivation, and other factors. The Cognitive Economics Study (CogEcon) was designed to increase

understanding of the cognitive bases of economic decision-making.[2] For CogEcon respondents, we have measures of a number of individual-level characteristics, including direct measures of cognitive ability, which we used to analyze the extent to which ability and motivation predict rounding. We tested whether rounding was more common among respondents low in ability or low in motivation, and for more difficult questions.

Methods

Respondents: Respondents were 490 adults, drawn from a national area-probability sample age 54 or older, and their spouses regardless of age. The sample was constructed in 2007. At the time, the age criterion was 50 and older. The CogEcon 2011 sample included participants from the Cognition and Aging in the USA Study (CogUSA) who had also completed the 2008 or 2009 wave of the CogEcon questionnaire.

Procedure: Data were collected over several months beginning in October 2011. Individuals who completed the web version in 2008 or 2009 were invited to the web mode, as were participants who answered questions in previous waves saying they regularly use the Internet.

We analyzed data from web respondents who reported exact amounts instead of selecting a range of numbers on our questions of interest. In total, 670 were invited to take the web survey and 527 completed at least part of the web questionnaire. In the web instrument, respondents were asked to enter dollar values. If an answer was left blank, respondents received the option of

[2] The Cognitive Economics Study (CogEcon) is sponsored by the National Institute on Aging (grant number NIA P01 AG026571) and is conducted by the University of Michigan. These respondents also participated in the Cognition and Aging in the USA study, the source of the cognition variables used in our analyses, sponsored by NIA grant R37 AG007137, "Assessing and Improving Cognitive Measurements in the HRS," John J. McArdle, PI.

choosing a range. Finally, some questions asked for an exact value without the option of reporting a range.

We removed respondents with missing data: 16 did not provide any exact values for the financial questions used in our analyses, 9 were missing measures of cognition, 3 were missing information on the use of records and household financial decision-making, and 9 were missing data on total financial assets. This resulted in an analysis sample of 490 web respondents.

***Measures**:* Respondents were asked to answer detailed questions about income, assets, savings and spending. We selected 19 questions that were applicable to a large subset of the sample and covered similar concepts as Study 1, including 7 questions about income, 7 about assets and debts, 4 about spending and 1 about savings in defined contribution retirement plans. These questions have different magnitudes and ask about different time horizons; some ask about the respondents while others ask about their spouses, partners, or households. Appendix A presents the exact wording of each question.

Episodic memory and the Woodcock-Johnson III Number Series score were used as measures of respondent cognitive ability. The episodic memory test comes from the Health and Retirement Study (HRS), described in Ofstedal et al. (2005). Number Series measures quantitative reasoning—reasoning with concepts that depend upon mathematical relationships—and used unpublished items from the *Woodcock Johnson Psychoeducational Test Battery* (WJ-III; Woodcock, McGrew, and Mather, 2001; Woodcock and Mather, 2001).

Need for Cognition and Big 5 measures of conscientiousness and openness were used as measures of motivation. The Need for Cognition measure was constructed from an 18-item short form validated by Cacioppo, Petty, and Kao (1984). The Big 5 characteristics were measured via self-report using the 44-item Big Five Inventory (BFI; John & Srivastava, 1999). Using the

sample from our main specification, we calculate Cronbach's alpha = 0.873 for the Need for Cognition measure, Cronbach's alpha = 0.743 for the Big Five measure of Conscientiousness and Cronbach's alpha = 0.803 for the Big Five measure of Openness.

Response latencies (time spent answering each question) and self-reported use of records are used as measures of respondent effort and information retrieval methods, respectively. The introduction to the financial questions encouraged respondents to consult records: "In the next several sections of the questionnaire, we will ask questions about your income and assets. We recommend that you use account statements, tax returns, personal finance software (e.g., Quicken, Microsoft Money) or other account information." At the end of the survey respondents were asked whether they consulted records. See Appendix A for the exact question wording.

Lastly, we use responses to a question identifying the person most knowledgeable about the family's finances to determine if the respondent is the family's Chief Financial Officer (CFO), the person primarily responsible for handling the family's finances (Hsu, 2016).

***Analysis*:**

We examine patterns of rounding by question type, using the rounding variable described above. As with Study 1, we analyze all questions together, then separately analyzed specific question types: unknowable questions, knowable questions, aggregated questions, and single account questions. Using a set of random effects regressions with respondent-question level observations, we compare rounding across questions, testing for statistically significant differences in the mean across these subsets. We then examine the role of individual-specific characteristics. We include dummy variables for each question. Because we have an unbalanced panel, we use the Swamy and Arora estimator for the variance components. As robustness checks (estimates reported Appendix B), we analyzed tobit models using the original *rounding* variable, as well as logit and

linear probability regression models using a binary form of *rounding* that takes the value of 1 when *rounding* is one (ie, the coarsest possible value) and 0 otherwise.

Results

Table 4 displays means of rounding for each question, as well as across question types. All questions exhibited rounding, ranging from an average of 0.34 for respondents' monthly Social Security benefits to 0.88 for credit card balances. Knowable questions displayed slightly less rounding (mean=0.72) than unknowable questions (mean=0.73); a two-tailed t-test of the difference in means was marginally statistically significant, at the 0.1 level. Single-account questions exhibited less rounding (mean=0.50) than aggregate questions (mean=0.78), and the two means were statistically different at the 0.0001 level. A two-tailed t-test of the difference between unknowable questions and knowable aggregate questions also yielded a p-value of less than 0.0001.

[Insert Table 4 here]

Table 5 presents regression coefficients for random effects regressions, pooling together all questions across respondents. Column (1) presents results using all questions, while columns (2) through (5) restrict the analysis to unknowable, knowable, aggregate, and single-account questions, respectively.

[Insert Table 5 here]

The intraclass correlation (rho) was 0.112 for the entire sample. However, it was much higher for knowable questions (rho=0.166) and single-account questions (rho=0.191), suggesting that people round to similar levels as a response strategy within knowable and single-account questions. Similar to column (2) in Table 3, the intraclass correlation for unknowable questions (rho=0.115) was lower than that for other question types, again suggesting less individual

consistency in respondents' approach to such questions. Observable individual characteristics likely explains some, but not all, of the correlation within respondents, given that in column (1) rho was 0.164 without individual-specific predictors, compared to 0.112 including those covariates.

The most consistent predictors of rounding across question type were consulting records and the time spent answering the question. As seen in column (1), data from a respondent who referred to records while completing the survey exhibited less rounding than someone who never consulted records (coefficient=-0.08, $p<0.01$). Similar to our findings in Study 1, the use of records was strongly associated with reduced rounding for more difficult questions. When analysis was restricted to knowable questions (column 3), frequent use of records was associated with reduced rounding (coefficient=-0.10, $p<0.01$), in contrast to our findings for unknowable questions (coefficient=0.02, $p>0.1$; column 2). We found that more time spent on a question is associated with less rounding (coefficient=-0.06, $p<0.01$). The relationship held for all types of questions.

The evidence on cognition was mixed. We found no measurable relationship between the Number Series score and rounding. Still, a one standard deviation higher episodic memory was associated with less rounding (coefficient=-0.02, $p<0.05$ in column 1, with similar results in columns 3-5 and an imprecise but comparable estimate in column 2).

In our main specification (column 1), a one standard deviation higher Need for Cognition (which reflects both cognitive effort and cognitive enjoyment) was associated with less rounding (coefficient=-0.02, $p<0.05$). This effect was similar for unknowable (coefficient=-0.03, $p=0.104$), knowable (coefficient=-0.02, $p=0.110$), and aggregated questions (coefficient=-0.02, $p<0.01$). We did not find evidence that conscientiousness is associated with rounding, although

this effect might operate through record use or spending more time answering the question. Greater openness was associated with more rounding (coefficient=0.02, $p<0.05$).

We also have information about each household's chief financial officer (CFO), or the person most knowledgeable about the household's finances. We found that being the CFO is associated with reporting less-rounded financial values (coefficient=-0.04, $p<0.01$). As in the Study 1 analysis, we estimated various additional specifications as robustness checks. Results can be found in Appendix B, as they yielded nearly identical results to those presented above, and do not change any of the substantive conclusions.

Discussion

The extent of rounding on open-ended financial questions varies across respondents and across questions. Our results about determinants of rounding are largely consistent with Krosnick's (1991) model of satisficing: rounding is positively related to variables associated with task difficulty and negatively related to measures of ability and motivation.

Task difficulty: Question-level characteristics

We expect that knowable questions (and particularly single-account questions) are easier to retrieve information for, with little information to integrate relative to unknowable questions. Questions on unknowable topics generally require more integration than those on knowable topics, which are verifiable through records. Among knowable questions, aggregate questions by definition require retrieval and integration of more information than single-account questions. These differences are important whether respondents retrieve information through memory, records, asking another person, or other background data sources.

Results from both studies showing greater rounding on more difficult questions (those requiring more, or more challenging, information retrieval and/or integration) are consistent with

these theoretical differences. Differences in the difficulty of retrieval explain why consulting records, which in both studies is one of the strongest predictors of less rounding, appears important in mitigating task difficulty. Using records may also ease the information integration process, perhaps by reducing the need for mental calculations. Furthermore, questionnaire designers may consider mapping questions directly to records. For example, survey questions on income could be mapped to values of specific lines on an income tax return.

These results suggest that encouraging respondents to consult records may increase data quality by reducing satisficing. However, Couper et al. (2013) found that encouraging record use does not have a noticeable impact on data quality. It is possible that the effectiveness of encouraging record use might depend on characteristics of the questions. For example, encouraging respondents to use past income tax returns may have a bigger impact on reported income when the question asks about values from a calendar year rather than over the past week, month, or 12 months. Records might also be less helpful for frequently-consulted information (e.g., check book, bank statement) versus less-frequently consulted (e.g., balance of mortgage, value of retirement accounts).

Couper et al. (2013) also find that respondents tend to be consistent in their record use across waves of the survey, implying that individual characteristics may drive record use. It is possible that similar factors, such as unobserved characteristics of respondents, may influence both record use and rounding. While one might be concerned ex ante that any correlation between record use and rounding in Study 1 stems from heterogeneity across interviewers in their encouragement of record use, accounting for interviewer effects either as fixed effects or random effects in a multi-level model yields similar results on the use of records as a

specification omitting interviewer effects. Nevertheless, the effects of motivation or ability cannot be fully disentangled from the use of records.

Our intraclass correlation estimates shed light on the extent to which rounding might be used as a general response strategy for all financial questions. Both studies showed higher intraclass correlations for knowable and single-account questions. This is consistent with the idea that single-account questions generally require a single item of information for all respondents, so these questions exhibit higher correlations of rounding than aggregate questions.

The intraclass correlations are lower for unknowable and aggregate knowable questions, perhaps because each additional piece of information required provides an opportunity to satisfice during information retrieval. Similarly, each additional piece of information increases the computations needed to fully integrate this information. However, for questions requiring multiple steps of information retrieval and integration, some respondents' response tasks will vary in the number of steps needed, depending on the complexity of their financial situation. This means there is varying difficulty of information retrieval and variation across respondents in the integration stage of response. As a result, we expect the overall intraclass correlation to be smaller than in the case of only single-account questions for which the information needed to answer the questions is the same for each person and across questions.

Ability: Individual-level characteristics

Our results so far have shown that differences in difficulty across questions at various stages of response influence rounding. Similarly, individual characteristics may operate at different stages of response. Because some questions are more difficult at one stage versus another, some dimensions of ability will affect rounding more on some question types than others.

Episodic memory is associated with reduced rounding for all questions. These results are consistent with the idea that stronger episodic memory enables respondents to retrieve information more easily and provide an optimal response, instead of resorting to satisficing. The effect of memory on rounding is larger for questions that ask for single-account values than aggregated, which suggests that memory is less important for questions that require both retrieval and integration of multiple pieces of information for a response.

The Number Series score, our measure of quantitative analytical ability, does not have a precisely measured relationship to rounding behavior. However, the size of the coefficient is substantially larger for unknowable questions, as is expected because unknowable questions require the most judgment on the types of information to retrieve and the most integration of such information. For other question types, analytical skill is likely trumped by episodic memory or being the CFO, perhaps due to less reliance on the judgment phase of response.

For all types of questions, respondents who are their family's CFO round less than those who are not. The family's CFO is the most knowledgeable about the family's finances and typically exhibits high domain-specific ability (Hsu, forthcoming). A CFO is likely to retrieve information more easily, whether through memory, records, or other data sources. That said, non-CFOs who asked someone else round at levels similar to CFOs than non-CFOs who did not ask someone for help, suggesting that non-CFOs might satisfice when answering without consulting the more knowledgeable member of their household.

Motivation: Individual-level characteristics

In our study, higher Need for Cognition, a source of respondent motivation, is associated with less rounding. Motivation likely leads respondents to exert more effort in responding, and our results are consistent with higher motivation leading to less satisficing. The relationship

between Need for Cognition and rounding is weakest for single-account questions, suggesting that motivation offsets task difficulty, particularly for more difficult questions, in the choice to provide round numbers.

Motivation: Question order

Figure 2 displays average rounding by question topic in the two studies, in order of the average magnitude of responses to each question. In both SCF and CogEcon, Social Security questions exhibit low levels of rounding whereas other questions like home values exhibit high levels of rounding. These patterns are similar in spite of the fact that the questions are in entirely different orders on the two surveys. This gives us confidence that variation in question difficulty yields similar rounding patterns across surveys. This is not surprising, as research on the influence of question order on another manifestation of satisficing—response order effects—has yielded mixed evidence depending on the length of the survey (Krosnick et al. 2002, Holbrook et al. 2007, Bishop and Smith 2001).

[Insert Figure 2 here]

Question order could theoretically also influence behavior through learning. Over the course of the survey, respondents who prefer not to give precise values may learn that refusing to provide an exact value would lead to multiple follow-up questions (for example, ranges or unfolding brackets), and therefore round more over the course of the survey. These effects are not precisely related to motivation, but in any case our analysis suggests that question order does not drive the patterns of rounding across questions that we find, regardless of whether motivation, learning or some other factor is most relevant.

Relatedly, low motivation and learning can be influenced by interviewers, and the analyses for Study 1 include interviewer fixed effects to control for such interviewer effects. The

robustness checks also include a multi-level model with interviewer random effects, as an alternative specification. Both sets of results are nearly identical to regressions omitting interviewer effects altogether. Furthermore, the fact that the multi-level models yielded intraclass correlations for respondents six to twelve times larger than that for interviewers indicates that rounding is only very slightly correlated within the same interviewer, but is more correlated within the same respondent and interviewer.

We also find in Study 1 that telephone respondents tend to round more than face-to-face respondents, consistent with Holbrook et al. (2003). One possible explanation is that interviewers may be less able to increase the motivation of respondents of telephone interviews. Study 1 respondents selected their preferred mode, so these results cannot be interpreted as causal.

Alternate explanations for rounded responses: Are results driven by sensitivity or privacy concerns?

Both studies yielded broad evidence that rounded responses on financial survey questions are consistent with satisficing. Still, it is possible that sensitivity or privacy concerns drive rounding behavior and that those concerns are coincidentally correlated with the same factors that also drive satisficing. To assess these competing explanations, we analyzed response latencies.

Since satisficing involves taking cognitive shortcuts, round number responses that reflect satisficing should be associated with shorter response latencies. But round numbers associated with privacy concerns should *not* have shorter latencies, since we would not expect cognitive shortcuts. That is, respondents would likely go through all the stages of response without shortcuts, but select a response in a final stage that is a round number to address their sensitivity

and privacy concerns. In fact, in CogEcon we found that longer response latencies are associated with less rounding after controlling for age, ability, consulting records, and other variables. This provides additional evidence that our results do in fact reflect satisficing. The question-level variation in rounding we explored earlier also suggests that respondents are not universally averse to providing personal information, which would have been reflected in similarly highly rounded responses across questions. In fact, we might expect that respondents with sensitivity concerns would provide less precision for single-account questions to hide potentially identifiable information, but we do not see this pattern in either study.

These results do not prove that sensitivity plays no role on financial surveys. For example, those with sensitivity concerns may skip a question, provide a range, or refuse to participate in a survey altogether. Examination of these possibilities is beyond the scope of this work.

In Appendix B, we present results from several robustness checks. First, we address the fact that our outcome variable, *rounding*, ranges from 0 to 1 inclusive (rather than being continuous over a larger range), and also takes a value of 1 for a large proportion of responses. In particular, we analyze Tobit models that account for the censoring or heaping at the top of the distribution of our outcome. We also transform *rounding* to a binary variable that takes a value of 1 when *rounding* is 1 (that is, the respondent gave the coarsest value possible), and 0 otherwise, and ran logit and linear probability model regressions, reporting coefficients for both and average marginal effects for the logit. All of these analyses yielded nearly identical results to the linear random-effects model, suggesting that our conclusions are robust across a variety of specifications of the model and the outcome.

Our results may appear to be at odds with Holbrook, et al. (2014), who found little evidence that heaping on a variety of non-financial numerical questions is more likely to occur under conditions predicted to foster satisficing. They concluded that heaping for objective questions may indicate use of estimation techniques and uncertainty, while for subjective questions it may actually reflect a more thoughtful response. While we study a wide range of financial questions, all of these are very different than the questions on behavioral frequencies, feeling thermometers, and personal characteristics discussed in Holbrook, et al. (2014). Therefore, it is plausible that respondents might employ different response strategies—satisficing or something else—for different types of questions. Indeed, we find differential levels of rounding for subsets of financial questions of varying difficulty.

3. Conclusion

This paper seeks to determine whether heaping on round numbers in financial questions reflects satisficing behavior. We use respondent-question level data from two surveys, the Survey of Consumer Finances and the Cognitive Economics Study, to analyze the determinants of rounding and test hypotheses drawn from applying Krosnick's (1991) satisficing theory to questions about financial values.

We find that questions that require more difficult information retrieval and integration exhibit more rounding. The use of records, which lowers task difficulty, reduces rounding as well. Higher measured episodic memory is associated with less rounding, but a measure of quantitative analytical ability displays no relationship with rounding. Standard measures of motivation are also negatively associated with rounding. These relationships, along with the fact that longer response latencies are associated with less rounding, all support the idea that rounding is a manifestation of satisficing on open-ended financial questions.

There are important limitations to this research. First, the samples are not necessarily representative of the population. Second, we observed, rather than experimentally manipulated, some of the factors that we found to be the most important—the use of records and time spent answering the questions. As a result, the effects of motivation or ability cannot be fully disentangled from the use of records. Third, the mode of data collection and interviewer effects likely impact both motivation and task difficulty, and therefore could impact response precision. We included controls for interviewer identifiers and modes, but a more direct analysis of these issues may be a fruitful area of future research.

Additionally, while we believe our question types capture differences in task difficulty, other distinctions could be important. For instance, the volatility of the underlying "true" financial value over time may affect rounding. At one end, Social Security payments are typically identical each month. At the other end, the value of checking accounts or credit cards can change daily, and respondents may differ on whether they report a number directly from records, or another number that may better reflect a "typical" value of that account. Alternatively, while we treat question attributes as fixed, there may be meaningful variation across respondents for the same question. For example, what is unknowable for one person may be knowable for another. Therefore, respondent heterogeneity may obscure underlying differences in rounding across question types.

Rounding patterns appear remarkably similar across the two surveys, despite being fielded in different modes and employing different question order and wording. This implies that our results may be generalizable to open-ended financial questions on other surveys. Also, our results imply that question order concerns may be less important to rounding behavior than formulating simple, clearly-written questions that are easy to answer. For some purposes, a round

response is "good enough," but for other common purposes of financial survey data, like group poverty rates, a round response may not be "good enough" at all. Survey designers, who may know the level of precision needed by their data users, may want to craft their surveys with this in mind and take steps to guide interviewers and respondents to a better sense of when rounding is indeed "good enough." Nevertheless, more work is needed to identify what characteristics of questions are susceptible to satisficing, and for which respondents.

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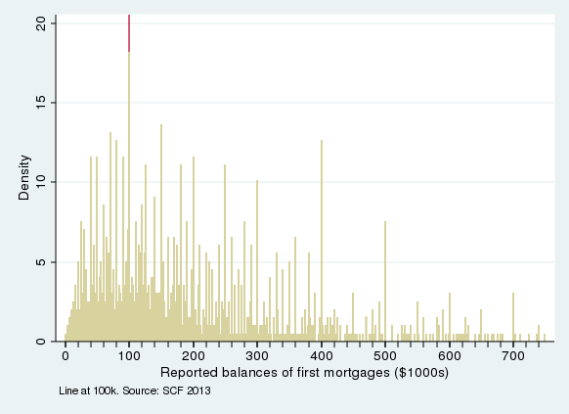
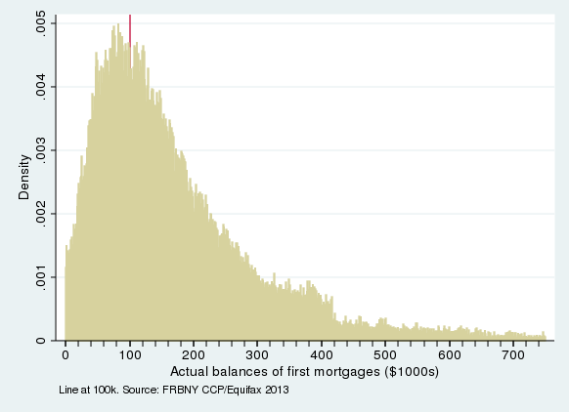
5. Figures and Tables

Figure 1: Histogram of mortgage balances
(a) Respondent-reported values, Survey of Consumer Finances (2013)

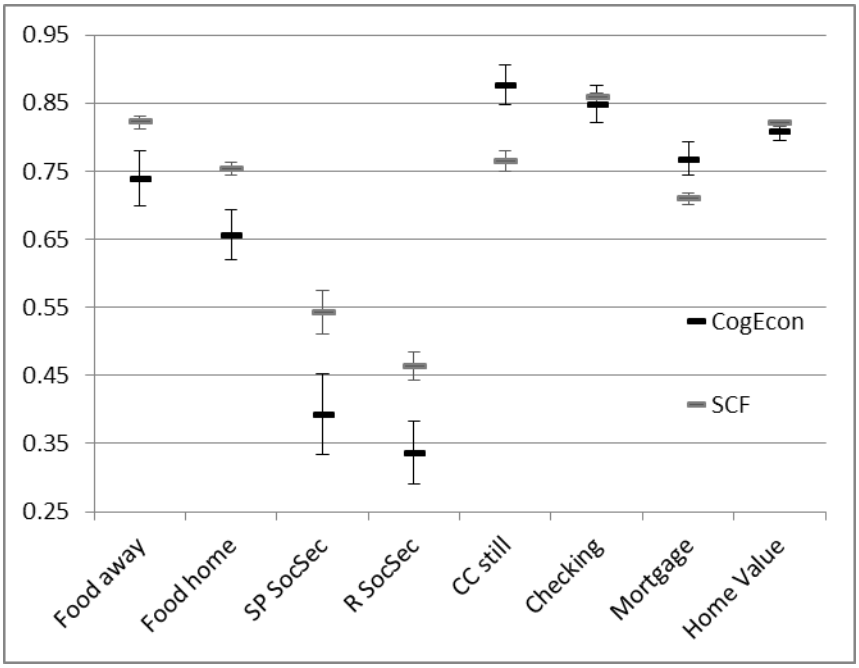


(b) Administrative Values, Federal Reserve Bank of New York Consumer Credit Panel/Equifax (2013)



Note: Displayed are distributions of mortgage balances in Survey of Consumer Finances (2013) and the FRBNY CCP/Equifax (2013).

Figure 2: Rounding by question topic, Study 1 (SCF) and Study 2 (CogEcon)



NOTE: Results in grey are for Study 1 (SCF), results in black are for Study 2 (CogEcon). For this figure we ordered the questions in terms of the smallest mean magnitude (Food away) to the largest mean magnitude (Home Value). The mean magnitude for each question is reported in Table 6. This figure includes only topics that are in both Study 1 and Study 2. The dashed lines are 95% confidence intervals constructed using the standard deviation and number of observations. They do not account for complex survey sampling procedures and are meant for ease of interpretation rather than inference.

Table 1: Differences in stages of response, by question type

	QUESTION TYPE		
STAGES OF RESPONSE (Tourangeau, 1984)	**Knowable-Aggregate** e.g., total income (wages+interest+…)	**Knowable-Single** e.g., one checking account	**Unknowable** e.g., home values
(1) Comprehension	NA	NA	NA
(2) Information retrieval	Multiple pieces of information	One piece of information	Multiple uncertain pieces of information
(3) Integration	Concrete-difficult	Concrete-easy	Abstract-difficult
(4) Response formulation	Privacy less important	Privacy more important	Privacy less important

Table 2: Rounding on financial questions, Study 1 (SCF)

	Rounding			Question type	
Question	Mean	St dev	N	Knowable?	Single account?
Credit card – new charges	0.796	0.315	3152	Yes	No
Credit card – balance outstanding	0.764	0.295	1477	Yes	No
Home value	0.820	0.148	3419	No	
Mortgage	0.709	0.214	2143	Yes	Yes
Food at home	0.753	0.354	5426	No	
Food away	0.822	0.345	5017	No	
Checking	0.857	0.261	4618	Yes	Yes
Savings	0.806	0.301	2756	Yes	Yes
Social Security income (self)	0.463	0.366	1204	Yes	Yes
Social Security income (spouse)	0.543	0.386	577	Yes	Yes
Overall	0.779	0.313	29789		
All knowable questions	0.766	0.311	15927		
All unknowable questions	0.795	0.314	13862		
All single-account questions	0.758	0.312	11298		
All aggregated questions	0.787	0.309	4629		

Note: Displayed are summary statistics for rounding on financial questions, ordered by position in the survey. Statistics are displayed first for each question, and then for each question type. Data source: Survey of Consumer Finances, 2013.

Table 3: Effect of question and individual characteristics on rounding, Study 1 (SCF)

Questions analyzed:	(1) All questions	(2) Unknowable questions	(3) Knowable questions	(4) Aggregated questions	(5) Single-account q's
Frequency of referring to records					
Never	omitted	omitted	omitted	omitted	omitted
Rarely	-0.011*	-0.001	-0.021***	-0.027*	-0.017**
	(0.0062)	(0.0090)	(0.0077)	(0.0143)	(0.0083)
Sometimes	-0.031***	-0.009	-0.049***	-0.051***	-0.048***
	(0.0053)	(0.0078)	(0.0066)	(0.0122)	(0.0071)
Frequently	-0.098***	-0.032***	-0.147***	-0.145***	-0.148***
	(0.0065)	(0.0096)	(0.0080)	(0.0144)	(0.0086)
Question indicators					
Credit cards: new charges	omitted	omitted	omitted	omitted	omitted
Credit cards: bal. outstanding	-0.026***	0	-0.015*	-0.027***	0
	(0.0090)	(.)	(0.0082)	(0.0103)	(.)
Home value	0.064***	0.157*	0	0	0
	(0.0108)	(0.0938)	(.)	(.)	(.)
Mortgage	-0.048***	0	-0.022**	0	0.269***
	(0.0107)	(.)	(0.0105)	(.)	(0.0143)
Food at home	-0.008	-0.118*	0	0	0
	(0.0068)	(0.0691)	(.)	(.)	(.)
Food away	0.078***	-0.036	0	0	0
	(0.0071)	(0.0688)	(.)	(.)	(.)
Checking (largest account)	0.078***	0	0.088***	0	0.371***
	(0.0066)	(.)	(0.0060)	(.)	(0.0112)
Savings (largest account)	0.051***	0	0.067***	0	0.352***
	(0.0077)	(.)	(0.0070)	(.)	(0.0119)
Social Security income (self)	-0.327***	0	-0.325***	0	-0.049***
	(0.0098)	(.)	(0.0090)	(.)	(0.0120)
Social Security income (spouse)	-0.259***	0	-0.276***	0	0
	(0.0129)	(.)	(0.0116)	(.)	(.)
Total digits of response					
Single digit	omitted	omitted	omitted	omitted	omitted
2 digits	0.741***	0.802***	0.595***	0.566***	0.592***
	(0.0186)	(0.0451)	(0.0204)	(0.0900)	(0.0218)
3 digits	0.794***	0.831***	0.796***	0.749***	0.809***
	(0.0183)	(0.0454)	(0.0186)	(0.0888)	(0.0185)

4 digits	0.834***	0.877***	0.800***	0.765***	0.808***
	(0.0184)	(0.0498)	(0.0187)	(0.0890)	(0.0186)
5 digits	0.825***	0.673***	0.777***	0.757***	0.780***
	(0.0189)	(0.0771)	(0.0193)	(0.0898)	(0.0194)
6 digits	0.746***	0.592***	0.686***	0.799***	0.689***
	(0.0202)	(0.0789)	(0.0209)	(0.1362)	(0.0209)
7 digits	0.807***	0.657***	0.760***	0	0.756***
	(0.0231)	(0.0804)	(0.0276)	(.)	(0.0272)
8 digits	0.766***	0.642***	0.669***	0	0.672***
	(0.0418)	(0.0933)	(0.0592)	(.)	(0.0567)
Age					
<35	omitted	omitted	omitted	omitted	omitted
35-44	-0.006	-0.007	0	0.025	-0.01
	(0.0071)	(0.0103)	(0.0091)	(0.0167)	(0.0098)
45-54	-0.006	-0.018*	0.012	0.029*	0.004
	(0.0068)	(0.0098)	(0.0087)	(0.0159)	(0.0094)
55-64	-0.01	-0.012	0	0.007	-0.003
	(0.0070)	(0.0101)	(0.0089)	(0.0163)	(0.0096)
65-74	-0.012	-0.026**	0.008	-0.006	0.014
	(0.0077)	(0.0112)	(0.0098)	(0.0181)	(0.0106)
75+	-0.01	-0.024*	0.008	0.017	0.007
	(0.0089)	(0.0129)	(0.0114)	(0.0218)	(0.0122)
Female	-0.018***	-0.013	-0.025***	-0.039**	-0.018*
	(0.0068)	(0.0099)	(0.0087)	(0.0167)	(0.0093)
Education					
<High school	omitted	omitted	omitted	omitted	omitted
High school grad/GED	-0.009	-0.022*	0.004	-0.013	0.01
	(0.0088)	(0.0128)	(0.0113)	(0.0241)	(0.0119)
Some college	-0.001	-0.009	0.01	-0.003	0.016
	(0.0094)	(0.0137)	(0.0120)	(0.0248)	(0.0127)
College grad or more	0.004	-0.012	0.023**	0.017	0.026**
	(0.0091)	(0.0132)	(0.0115)	(0.0239)	(0.0122)
Married	0.004	0.018**	-0.012	-0.02	-0.011
	(0.0060)	(0.0088)	(0.0077)	(0.0143)	(0.0083)
Ln(financial assets)	0.007***	0.004***	0.011***	0.008***	0.012***
	(0.0009)	(0.0012)	(0.0011)	(0.0022)	(0.0012)
Nonwhite	0.007	0.013*	0.001	-0.01	0.006
	(0.0051)	(0.0075)	(0.0065)	(0.0120)	(0.0070)
Telephone interview	0.020***	0.020***	0.020***	0.024**	0.018***
	(0.0050)	(0.0074)	(0.0063)	(0.0114)	(0.0069)
Constant	-0.075*	0	-0.087*	0.093	-0.431***

	(0.0407)	(.)	(0.0496)	(0.1198)	(0.0552)
Observations	29789	13862	15927	4629	11298
Individuals	5698	5648	5371	3540	5194
Overall R2	0.183	0.068	0.326	0.182	0.400
Rho	0.068	0.080	0.151	0.134	0.167
Rho (no covariates)	0.106	0.088	0.209	0.235	0.185

Note: Displayed are coefficient estimates of random-effects models using data with multiple observations of *rounding* (one for each survey question) for each respondent. Indicators for interviewer identifiers included. Standard errors are in parentheses. Data source: Survey of Consumer Finances, 2013. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Rounding on financial questions, Study 2 (CogEcon)

	Rounding			Question type	
Question	Mean	Std dev	N	Knowable?	Single account?
Household income	0.751	0.248	461	yes	no
Social Security income (self)	0.337	0.352	232	yes	yes
Social Security income (spouse)	0.394	0.384	167	yes	yes
Pension income (self)	0.439	0.383	179	yes	yes
Pension income (spouse)	0.487	0.394	116	yes	yes
Earnings (self)	0.708	0.285	274	yes	no
Earnings (spouse)	0.761	0.250	186	yes	no
Home value	0.810	0.164	441	no	
Mortgage	0.768	0.196	262	yes	yes
Assets in tax-favored retirement accts	0.781	0.237	368	yes	no
Assets outside tax-favored ret accts	0.844	0.219	379	yes	no
Check, Savings, CDs	0.849	0.231	276	yes	no
Credit Card –balance outstanding	0.877	0.173	148	yes	no
Other non-housing debt	0.826	0.227	160	yes	no
401k contributions	0.770	0.238	161	yes	no
Food at home	0.657	0.397	474	no	
Food away from home	0.739	0.427	446	no	
Health insurance (household)	0.663	0.337	407	yes	no
Health spending out-of-pocket	0.814	0.267	429	yes	no
Overall	0.720	0.326	5,564		
All knowable questions	0.715	0.315	4205		
All unknowable questions	0.733	0.357	1361		
All single-account questions	0.502	0.376	956		
All aggregated questions	0.778	0.264	3249		

Note: Displayed are summary statistics for rounding on financial questions. Statistics are displayed first for each question, and then for each question type. Data source: Cognitive Economics Study, 2011.

Table 5: Effect of question and individual characteristics on rounding, Study 2 (CogEcon)

Questions analyzed:	(1) All questions	(2) Unknowable questions	(3) Knowable questions	(4) Aggregated questions	(5) Single-account q's
Individual characteristics					
Number Series	0.009	0.022	0.003	0.004	-0.003
	(0.008)	(0.014)	(0.008)	(0.008)	(0.016)
Episodic Memory	-0.017**	-0.014	-0.018**	-0.016**	-0.025*
	(0.007)	(0.013)	(0.007)	(0.007)	(0.014)
Need for Cognition	-0.019**	-0.026	-0.015	-0.015*	-0.01
	(0.009)	(0.016)	(0.009)	(0.009)	(0.018)
Big Five- Conscientiousness	-0.005	0	-0.008	-0.006	-0.015
	(0.006)	(0.011)	(0.006)	(0.006)	(0.012)
Big Five- Openness	0.018**	0.01	0.022***	0.022***	0.022
	(0.008)	(0.014)	(0.008)	(0.008)	(0.016)
Household CFO	omitted	omitted	omitted	omitted	omitted
Not CFO-NOT asked someone	0.047***	0.049*	0.044***	0.031*	0.084***
	(0.016)	(0.029)	(0.017)	(0.016)	(0.031)
Not CFO- asked someone	0.033*	0.066**	0.017	0.012	0.04
	(0.018)	(0.033)	(0.019)	(0.019)	(0.036)
log(financial assets)	0.003*	-0.003	0.006***	0.004*	0.009**
	(0.002)	(0.003)	(0.002)	(0.002)	(0.004)
Non-white or hispanic (=1)	0.027	-0.005	0.043**	0.044**	0.048
	(0.019)	(0.036)	(0.021)	(0.020)	(0.041)
Female (=1)	-0.023*	-0.027	-0.021	-0.023*	-0.009
	(0.013)	(0.024)	(0.014)	(0.013)	(0.027)
Married (=1)	-0.01	-0.028	-0.001	0.004	-0.005
	(0.016)	(0.029)	(0.017)	(0.016)	(0.035)
College Graduate (=1)	0.014	-0.009	0.022	0.015	0.047*
	(0.013)	(0.024)	(0.014)	(0.013)	(0.026)
Age categories					
45=<age<55	omitted	omitted	omitted	omitted	omitted
55=<age<45	-0.021	-0.011	-0.026	-0.03	-0.039
	(0.025)	(0.045)	(0.027)	(0.025)	(0.063)
65=<age<75	-0.051*	-0.045	-0.054*	-0.069***	-0.032
	(0.026)	(0.047)	(0.028)	(0.026)	(0.066)
75=<age	-0.054*	0.004	-0.076**	-0.079***	-0.058
	(0.028)	(0.051)	(0.031)	(0.029)	(0.071)
Response behavior					

log seconds answering question	-0.061***	-0.041***	-0.058***	-0.062***	-0.061***
	(0.005)	(0.015)	(0.005)	(0.005)	(0.012)
Used records	-0.075***	-0.019	-0.095***	-0.081***	-0.150***
	(0.012)	(0.022)	(0.013)	(0.012)	(0.025)
Question Indicators					
Household income	omitted		omitted	omitted	
Social Security income (self)	-0.437***		-0.451***		0.181
	(0.026)		(0.023)		(0.200)
Social Security income (spouse)	-0.411***		-0.426***		0.201
	(0.028)		(0.026)		(0.200)
Pension income (self)	-0.363***		-0.374***		0.263
	(0.028)		(0.025)		(0.199)
Pension income (spouse)	-0.339***		-0.352***		0.28
	(0.032)		(0.029)		(0.200)
Earnings (self)	-0.089***		-0.094***	-0.093***	
	(0.021)		(0.018)	(0.017)	
Earnings (spouse)	-0.069***		-0.069***	-0.071***	
	(0.024)		(0.021)	(0.020)	
Home value	0.001	-0.380***			
	(0.020)	(0.143)			
Mortgage	-0.053**		-0.051***		0.534***
	(0.021)		(0.019)		(0.207)
Retirement Assets	0.006		0.009	0.002	
	(0.019)		(0.017)	(0.019)	
Nonretirement Assets	0.076***		0.075***	0.073***	
	(0.019)		(0.017)	(0.019)	
Check,Savings,CDs	0.132***		0.126***	0.133***	
	(0.021)		(0.019)	(0.021)	
Credit cards: bal. outstanding	0.038		0.035	0.024	
	(0.027)		(0.024)	(0.027)	
Other debt	-0.003		-0.009	-0.007	
	(0.026)		(0.023)	(0.026)	
401k contributions	-0.041		-0.054**	-0.045*	
	(0.026)		(0.024)	(0.027)	
Food at home	-0.093***	omitted			
	(0.025)				
Food away	0.027	0.135***			
	(0.029)	(0.027)			
Health insurance	-0.110***		-0.127***	-0.127***	
	(0.022)		(0.020)	(0.019)	

Health out-of-pocket	0.02		0.001	-0.006	
	(0.023)		(0.020)	(0.020)	
Total digits of response					
Single digit	omitted	omitted	omitted	omitted	omitted
2 digits	0.672***	0.700***	0.385***	0.771***	-0.128
	(0.067)	(0.103)	(0.108)	(0.139)	(0.208)
3 digits	0.789***	0.816***	0.713***	1.048***	0.332*
	(0.067)	(0.105)	(0.100)	(0.133)	(0.179)
4 digits	0.815***	1.104***	0.724***	1.038***	0.374**
	(0.067)	(0.226)	(0.100)	(0.132)	(0.178)
5 digits	0.800***	1.335***	0.696***	1.008***	0.424**
	(0.068)	(0.181)	(0.100)	(0.132)	(0.190)
6 digits	0.770***	1.288***	0.658***	0.985***	0.348*
	(0.069)	(0.177)	(0.100)	(0.132)	(0.192)
7 digits	0.780***	1.376***	0.652***	0.977***	0.323
	(0.076)	(0.196)	(0.106)	(0.136)	(0.288)
8 digits	0.534***	0	0.405**	0.754***	0
	(0.205)	(.)	(0.200)	(0.209)	(.)
Constant	0.239***	0.099	0.318***	0.036	omitted
	(0.077)	(0.132)	(0.106)	(0.133)	
Observations	5566	1361	4205	3249	956
Individuals	490	485	487	486	427
Overall R2	0.266	0.112	0.347	0.212	0.33
Rho	0.112	0.115	0.166	0.151	0.191
Overall R2 (no predictors)	0.172	0.031	0.23	0.055	0.204
Rho (no predictors)	0.164	0.137	0.24	0.244	0.27

Note: Displayed are coefficient estimates of random-effects models using data with multiple observations of *rounding* (one for each survey question) for each respondent. Measures of number series, episodic memory, need for cognition, conscientiousness and openness are standardized to ease interpretation. Standard errors are in parentheses. Data source: Cognitive Economics Study, 2011. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Summary statistics for magnitude of responses (total number of digits)

	SCF		CogEcon	
	Mean	Std Dev	Mean	Std Dev
Credit card- balance outstanding	3.87	0.76	4.22	0.73
Home Value	6.05	0.58	5.86	0.53
Mortgage	5.71	0.57	5.45	0.63
Food home	2.74	0.48	2.72	0.46
Food away	2.49	0.59	2.17	0.44
Checking	3.96	1.04	4.92	0.78
Social Security income (self)	3.80	0.60	3.70	0.52
Social Security income (spouse)	3.69	0.63	3.66	0.54
Savings	4.20	1.31		
Credit card – new charges	3.55	0.82		
Pension income (self)			3.63	0.60
Pension income (spouse)			3.54	0.57
Earnings (self)			4.99	0.62
Earnings (spouse)			4.94	0.66
Tax-favored retirement acct assets			5.63	0.80
Non tax-favored retirement acct assets			5.18	0.95
Other non-housing debt			4.66	0.59
401k contributions			4.28	0.65
Health insurance			3.90	0.52
Health out-of-pocket			3.63	0.56

Data source: Cognitive Economics Study, 2011, and Survey of Consumer Finances, 2013. Sample sizes are the same as reported in Table 2 (for SCF) and Table 4 (for CogEcon).

6. Appendix A

Question wording for Study 1 (SCF 2013)

Text of questions used in our paper, along with the range of values allowed in their response, are displayed below. Please see the Survey of Consumer Finances codebook, available at http://www.federalreserve.gov/econresdata/scf/files/codebk2013.txt, for more details, including the text of other questions in the survey. Text in italics are clarifying notes which are not part of the instrument.

Checking account: X3506 How much is in this account? (Probe: What was the average over the last month)

Savings account: X3730 How much is in this account? *["this account" refers to the largest savings or money market account, if the respondent has more than one]*

Credit cards (new charges): X412 On your last bill(s), how much were the new charges made to (this account/these accounts)? *["this account" or "these accounts" refer to Visa, MasterCard, Discover, or American Express cards that can be paid off over time; "this account" is read only if the respondent has only one of these cards]*

Credit cards (still owed): X413 After the last payment(s) (was/were) made, what was the total balance still owed on (this account/all these accounts)?

Home value: X716 What is the current value of this (home and land/apartment/property)? I mean, without taking any outstanding loans into account, about what would it bring if it were sold today?

Mortgage: X805 How much is still owed on this loan? *["this loan" refers to the first mortgage, if any, on the respondent's residence]*

Social Security (own): X5306(#1) How much (do you {personally})receive each month or year?

Social Security (spouse/partner): X5311(#2) How much (does he/does she/does he or she) receive each month or year?

Food at home: X3024 Now I have some questions about your spending. How much do you (and everyone else in your family) spend on food that you use at home in an average week? (What is your best estimate?)

Food away from home: X3029 About how much do you (and everyone else in your family) spend eating out?

Question wording for Study 2 (CogEcon 2011)

Please see questionnaire documentation at http://ebp-projects.isr.umich.edu/CogEcon/docs/C3_questions.pdf for more detail.

Before asking about income and assets, we introduce the set of questions with the following:

In the next several sections of the questionnaire, we will ask a number of questions about your income and assets. We recommend that you use account statements, tax returns, personal finance software (e.g., Quicken or Microsoft Money), or other account information.

We need good estimates but not necessarily exact values if they are not readily available. If you have trouble coming up with a value, in many cases you may answer with "ranges," as indicated in the question.

Household income C2: What is the total combined income of all members of your family (living here) the last 12 months? *This includes wages or salary, net income from business, farm or rent, pensions, dividends, interest, Social Security payments, and any other money or income. Please provide an exact or approximate value. If you **cannot** provide an approximate value, please check a range.* [Ranges were listed next to question]

Social Security income (self) C5: How much do you receive in Social Security or Railroad Retirement benefit payments each month? Please give the amount you receive, before any deductions for Medicare. Approximately $_____ per month.

Social Security income (spouse) C11: How much does your spouse/partner receive in Social Security or Railroad Retirement benefit payments each month? Please give the amount he/she receives, before any deductions for Medicare. Approximately $_____ per month.

Pension income (self) C15-C16: In some pension plans, a worker is entitled to receive regular retirement payments for as long as the worker lives. The amount of these payments is most often determined by a formula as a percentage of final or average pay. Do you currently receive payments from an employer- or union-provided pension plan like this? C16: How much do you receive each month? Approximately _____ per month

Pension income (spouse) C21: How much does your spouse/partner receive each month? Approximately $ _____ per month

Earnings (self) C33: [Did you have at least one job for which you got paid (or received self-employment income) during 2010? If YES…] During 2010, what were your total earnings before taxes from all jobs? *Include only your earnings—do not include any other household member's earnings. If you **cannot** provide an approximate value, please enter a "range letter" from the list below.* [Ranges were listed next to question]

Earnings (spouse) C42: [Did your spouse/partner have at least one job for which he/she got paid (or received self-employment income) during 2010? If YES…] During 2010, what were your spouse's/partner's total earnings before taxes from all jobs? *Include only your spouse's/partner's earnings. If you **cannot** provide an approximate value, please enter a "range letter" from the list below.* [Ranges were listed next to question]

Home value D5: What would your primary residence be worth if sold today? *If you **cannot** provide an approximate value, please enter a "range letter" from the list below.*

Mortgage D7: [Do you (or your spouse/partner) have any mortgages or home equity lines of credit on your primary residence?] If so, what is the total balance owed on all such accounts? *If you **cannot** provide an approximate value, please enter a "range letter" from the list below.*

Assets in tax-favored retirement accts D15: Do you (or your spouse/partner) hold any **tax-advantaged retirement accounts** (for example, 401(k) plans, 403(b) plans, Keoghs, traditional IRAs or Roth IRAs)? If so, what is the total value of all the financial assets you (and your spouse/partner) hold in these accounts? Approximate total value $ _____ OR range letter

Assets outside tax-favored ret accts D16: What is the total value of all the financial assets you (and your spouse/partner) hold outside of tax-advantaged retirement accounts? *Your other financial assets could include bank accounts, money market funds, cash, CDs, bonds, stocks, mutual funds, or any other types of financial assets we have not yet mentioned.*

Check, Savings, CDs D16a: ***Please give us the breakdown of the total given above.*** *Check yes for each asset you hold. Please give an approximate value for each asset you hold, or indicate a range letter from the inside back cover if you cannot give an approximate value. Check no if you do not hold the asset. If you do not know whether you hold the asset, leave the checkbox blank.* D16a: Short-term assets such as cash, bank accounts, money market funds, CDs, and short-term Treasury bills?

Credit card (balance outstanding) D31: What is the total balance on all credit cards that you (and your spouse/ partner) carried over from last month to this one? Approximately _____.

Other non-housing debt D34[Aside from mortgages, other home equity lines of credit, and credit card balances, do you (or your spouse/partner) have other debts, including vehicle loans, student loans, overdue taxes, other personal loans, or debt for medical expenses?] What is the total outstanding balance on all of these loans or debts?

401k contributions E3: [E2: Did you (or your spouse/partner) contribute to any employer-sponsored tax-advantaged retirement accounts such as 401(k) or 403(b) accounts in 2010? If yes…] Excluding employer contributions, approximately how much did you (and your spouse/partner) contribute to these accounts in 2010? Approximately $_____.

Food at home E19: About how much does your household spend on food that you use at home in an average **week**, including any food delivered to the door? Approximately $_____ per week.

Food away from home E20: About how much does your household spend in an average **week** eating out? Approximately $____ per week

Health insurance E21: About how much did your household spend for health insurance in the **last 12 months**? *Please include amounts you paid for employer-provided health insurance and any premiums for insurance you bought on your own, including Medigap policies.* Approximately $_____ in the last 12 months

Health spending out-of-pocket E22: About how much did your household spend "out-of-pocket" for health care in the **last 12 months**? *Do not include the insurance premiums that were in the previous question. Please do include co-pays, prescription and nonprescription medications,*

health care services (cost of hospital care, doctor services, lab tests, eye, dental, and nursing home care), and medical supplies. Approximately _____ in the last 12 months

Record use H1: What sources of information did you use to assist you in answering the questions about your finances in this questionnaire? *Please check all that apply.* [Response options: Account statements, Tax returns, Personal finance software (e.g., Quicken or MS Money), Asked someone, Other (specify): ____, I did not use anything.]

7. Appendix B: Robustness checks

Alternative specifications for Study 1 robustness checks

Appendix Table 1: Alternative specifications using all questions

	(1)	(2)	(3)	(4)	(5)	(6)
Model	Linear, with Interviewer and respondent random effects	Tobit	Logit	Tobit	Logit	Linear with respondent RE
Reported	Coeff/marginal effects	Marginal effects	Marginal effects	Coefficients	Coefficients	Coefficients
Frequency of referring to records						
Never	omitted	omitted	omitted	omitted	omitted	omitted
Rarely	-0.014**	-0.035***	-0.022**	-0.035***	-0.115**	-0.022**
	(0.0059)	(0.0125)	(0.0089)	(0.0125)	(0.0467)	(0.0088)
Sometimes	-0.033***	-0.070***	-0.041***	-0.070***	-0.215***	-0.041***
	(0.0051)	(0.0106)	(0.0076)	(0.0106)	(0.0397)	(0.0075)
Frequently	-0.098***	-0.184***	-0.105***	-0.184***	-0.540***	-0.104***
	(0.0062)	(0.0127)	(0.0093)	(0.0127)	(0.0476)	(0.0090)
Question indicators						
Credit cards: new charges	omitted	omitted	omitted	omitted	omitted	omitted
Credit cards: Balance ounstanding	-0.027***	-0.071***	-0.054***	-0.071***	-0.262***	-0.069***
	(0.0090)	(0.0192)	(0.0145)	(0.0192)	(0.0697)	(0.0142)
Home value	0.060***	0.080***	0.026	0.080***	0.127	-0.007
	(0.0108)	(0.0228)	(0.0172)	(0.0228)	(0.0861)	(0.0169)
Mortgage	-0.051***	-0.103***	-0.154***	-0.103***	-0.747***	-0.152***
	(0.0107)	(0.0224)	(0.0191)	(0.0224)	(0.0903)	(0.0168)
Food at home	-0.007	-0.050***	-0.071***	-0.050***	-0.344***	-0.066***
	(0.0068)	(0.0148)	(0.0110)	(0.0148)	(0.0541)	(0.0107)
Food away	0.080***	0.161***	0.056***	0.161***	0.281***	0.055***
	(0.0071)	(0.0160)	(0.0119)	(0.0160)	(0.0592)	(0.0112)
Checking (largest account)	0.077***	0.200***	0.136***	0.200***	0.714***	0.141***
	(0.0066)	(0.0149)	(0.0101)	(0.0149)	(0.0540)	(0.0105)
Savings (largest account)	0.049***	0.118***	0.090***	0.118***	0.460***	0.086***
	(0.0077)	(0.0170)	(0.0118)	(0.0170)	(0.0619)	(0.0122)

Social Security income	-0.329***	-0.557***	-0.406***	-0.557***	-2.249***	-0.438***
	(0.0098)	(0.0198)	(0.0134)	(0.0198)	(0.0950)	(0.0155)
Social Security income	-0.259***	-0.459***	-0.312***	-0.459***	-1.587***	-0.341***
	(0.0129)	(0.0259)	(0.0186)	(0.0259)	(0.1086)	(0.0204)
Total digits of response						
Single digit	omitted	omitted	Not Estimable	omitted	omitted	omitted
2 digits	0.741***	1.120***		1.120***	2.608***	0.790***
	(0.0185)	(0.0358)		(0.0358)	(0.2996)	(0.0290)
3 digits	0.796***	1.158***		1.158***	2.207***	0.714***
	(0.0182)	(0.0350)		(0.0350)	(0.2944)	(0.0286)
4 digits	0.838***	1.150***		1.150***	1.632***	0.604***
	(0.0184)	(0.0353)		(0.0353)	(0.2910)	(0.0288)
5 digits	0.829***	1.040***		1.040***	0.910***	0.453***
	(0.0189)	(0.0364)		(0.0364)	(0.2877)	(0.0297)
6 digits	0.753***	0.880***		0.880***	0.13	0.306***
	(0.0201)	(0.0392)		(0.0392)	(0.2847)	(0.0316)
7 digits	0.816***	1.000***		1.000***	0.657**	0.418***
	(0.0230)	(0.0459)		(0.0459)	(0.2917)	(0.0361)
8 digits	0.779***	0.873***		0.873***	0	0.280***
	(0.0418)	(0.0851)		(0.0851)	(.)	(0.0656)
Age						
<35	omitted	omitted	omitted	omitted	omitted	omitted
35-44	-0.004	-0.003	-0.002	-0.003	-0.011	-0.002
	(0.0069)	(0.0149)	(0.0106)	(0.0149)	(0.0553)	(0.0104)
45-54	-0.004	0.002	0.009	0.002	0.05	0.009
	(0.0066)	(0.0143)	(0.0101)	(0.0143)	(0.0530)	(0.0100)
55-64	-0.007	0.003	0.013	0.003	0.07	0.012
	(0.0067)	(0.0146)	(0.0103)	(0.0146)	(0.0541)	(0.0102)
65-74	-0.01	-0.006	0.008	-0.006	0.042	0.008
	(0.0075)	(0.0160)	(0.0114)	(0.0160)	(0.0597)	(0.0112)
75+	-0.011	0.012	0.042***	0.012	0.221***	0.041***
	(0.0086)	(0.0186)	(0.0130)	(0.0186)	(0.0695)	(0.0130)
Female	-0.018***	-0.041***	-0.028***	-0.041***	-0.147***	-0.028***
	(0.0066)	(0.0144)	(0.0103)	(0.0144)	(0.0539)	(0.0100)
Education						
<High school	omitted	omitted	omitted	omitted	omitted	omitted
High school grad/GED	-0.013	-0.036**	-0.031**	-0.036**	-0.160**	-0.029**
	(0.0085)	(0.0183)	(0.0131)	(0.0183)	(0.0690)	(0.0128)
Some college	-0.004	-0.02	-0.022	-0.02	-0.114	-0.02

	(0.0091)	(0.0194)	(0.0139)	(0.0194)	(0.0732)	(0.0136)
College grad or more	0.002	-0.002	-0.005	-0.002	-0.026	-0.003
	(0.0087)	(0.0186)	(0.0132)	(0.0186)	(0.0702)	(0.0130)
Married	0.004	0.015	0.016*	0.015	0.083*	0.016*
	(0.0059)	(0.0127)	(0.0090)	(0.0127)	(0.0475)	(0.0089)
Ln(financial assets)	0.007***	0.016***	0.011***	0.016***	0.058***	0.011***
	(0.0008)	(0.0017)	(0.0013)	(0.0017)	(0.0067)	(0.0012)
Nonwhite	0.011**	0.032***	0.023***	0.032***	0.122***	0.023***
	(0.0048)	(0.0102)	(0.0073)	(0.0102)	(0.0382)	(0.0071)
Telephone interview	0.023***	0.066***	0.044***	0.066***	0.232***	0.043***
	(0.0046)	(0.0091)	(0.0064)	(0.0091)	(0.0339)	(0.0063)
Constant	-0.066***			-0.178***	-1.902***	-0.136***
	(0.0225)			(0.0447)	(0.3220)	(0.0346)
Observations	29789	29789	29526	29789	29526	29789
Individuals	5698	5698	5697	5698	5697	5698
Rho		0.079	0.073	0.079	0.073	0.043
ICC (Respondents)	0.0100					
ICC (Interviewers)	0.0700					

Note: Displayed are estimates of alternative specifications of the first column of Table 3 using data with multiple observations (one for each survey question) for each respondent. Column (1) presents coefficients from a linear multi-level model with respondent-level and interviewer-level random effects, which can also be interpreted as marginal effects. Columns (2) and (3) present marginal effects for a Tobit model and Logit model (using a binary form of *rounding*), respectively, both with respondent-level random effects. Columns (4) and (5) present coefficients for the Tobit and Logit models. Column (6) is identical to the first column in Table 3--a random effects model with respondent-level random effects-- but using a binary form of rounding; coefficients can be interpreted as marginal effects. Standard errors are in parentheses. Data source: Survey of Consumer Finances, 2013. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Alternative specifications for Study 2 robustness checks

Appendix Table 2: Alternative specifications using all questions

	(1)	(2)	(3)	(4)	(5)
Model	Tobit	Logit	Tobit	Logit	Linear with respondent RE
Reported	Marginal effects	Marginal effects	Coefficients	Coefficients	Coeff/Marginal Effects
Individual characteristics					
Number Series	0.017	0.014	0.017	0.075	0.015
	(0.012)	(0.011)	(0.012)	(0.059)	(0.011)
Episodic Memory	-0.031***	-0.032***	-0.031***	-0.173***	-0.032***
	(0.011)	(0.009)	(0.011)	(0.052)	(0.009)
Need for Cognition	-0.031**	-0.027**	-0.031**	-0.146**	-0.027**
	(0.014)	(0.012)	(0.014)	(0.066)	(0.012)
Big Five-Conscientiousness	-0.002	0.006	-0.002	0.032	0.006
	(0.009)	(0.008)	(0.009)	(0.045)	(0.008)
Big Five- Openness	0.028**	0.019*	0.028**	0.104*	0.020*
	(0.012)	(0.011)	(0.012)	(0.059)	(0.010)
Household CFO	omitted	omitted	omitted	omitted	omitted
Not CFO-NOT asked someone	0.082***	0.079***	0.082***	0.425***	0.077***
	(0.024)	(0.022)	(0.024)	(0.116)	(0.021)
Not CFO- asked someone	0.047*	0.035	0.047*	0.193	0.035
	(0.028)	(0.025)	(0.028)	(0.134)	(0.024)
log(financial assets)	0.005*	0.003	0.005*	0.016	0.003
	(0.003)	(0.003)	(0.003)	(0.015)	(0.003)
Non-white or hispanic (=1)	0.05	0.047*	0.05	0.255*	0.047*
	(0.031)	(0.027)	(0.031)	(0.145)	(0.026)
Female (=1)	-0.031	-0.009	-0.031	-0.05	-0.009
	(0.020)	(0.018)	(0.020)	(0.098)	(0.017)
Married (=1)	-0.009	0.004	-0.009	0.022	0
	(0.025)	(0.022)	(0.025)	(0.119)	(0.021)
College Graduate (=1)	0.03	0.036**	0.03	0.198**	0.038**
	(0.020)	(0.018)	(0.020)	(0.096)	(0.017)
Age categories					
45=<age<55	omitted	omitted	omitted	omitted	omitted
55=<age<45	-0.037	-0.033	-0.037	-0.179	-0.034
	(0.039)	(0.034)	(0.039)	(0.181)	(0.033)
65=<age<75	-0.079*	-0.05	-0.079*	-0.268	-0.052

	(0.041)	(0.036)	(0.041)	(0.192)	(0.035)
75=<age	-0.081*	-0.047	-0.081*	-0.253	-0.048
	(0.045)	(0.039)	(0.045)	(0.212)	(0.038)
Response behavior					
log seconds answering question	-0.088***	-0.072***	-0.088***	-0.394***	-0.065***
	(0.007)	(0.008)	(0.007)	(0.045)	(0.007)
Used records	-0.120***	-0.089***	-0.120***	-0.483***	-0.089***
	(0.019)	(0.016)	(0.019)	(0.089)	(0.016)
Question Indicators					
Household income	omitted	omitted	omitted	omitted	omitted
Social Security income (self)	-0.627***	-0.423***	-0.627***	-3.080***	-0.480***
	(0.041)	(0.030)	(0.041)	(0.295)	(0.042)
Social Security income (spouse)	-0.599***	-0.383***	-0.599***	-2.540***	-0.441***
	(0.045)	(0.033)	(0.045)	(0.283)	(0.046)
Pension income (self)	-0.546***	-0.374***	-0.546***	-2.446***	-0.423***
	(0.044)	(0.033)	(0.044)	(0.274)	(0.045)
Pension income (spouse)	-0.522***	-0.355***	-0.522***	-2.252***	-0.408***
	(0.051)	(0.036)	(0.051)	(0.298)	(0.052)
Earnings (self)	-0.141***	-0.117***	-0.141***	-0.629***	-0.111***
	(0.033)	(0.035)	(0.033)	(0.189)	(0.034)
Earnings (spouse)	-0.113***	-0.093**	-0.113***	-0.492**	-0.089**
	(0.038)	(0.038)	(0.038)	(0.208)	(0.039)
Home value	-0.002	-0.025	-0.002	-0.132	-0.016
	(0.031)	(0.033)	(0.031)	(0.173)	(0.032)
Mortgage	-0.092***	-0.102***	-0.092***	-0.542***	-0.092***
	(0.034)	(0.036)	(0.034)	(0.193)	(0.035)
Retirement Assets	0.033	0.065**	0.033	0.343**	0.079**
	(0.031)	(0.032)	(0.031)	(0.169)	(0.031)
Nonretirement Assets	0.177***	0.181***	0.177***	0.988***	0.197***
	(0.031)	(0.030)	(0.031)	(0.164)	(0.030)
Check,Savings,CDs	0.257***	0.245***	0.257***	1.384***	0.268***
	(0.035)	(0.031)	(0.035)	(0.180)	(0.034)
Credit cards: bal. outstanding	0.086*	0.044	0.086*	0.232	0.07
	(0.047)	(0.045)	(0.047)	(0.234)	(0.044)
Other debt	0.005	0.013	0.005	0.07	0.029
	(0.042)	(0.041)	(0.042)	(0.215)	(0.041)
401k contributions	-0.089**	-0.079*	-0.089**	-0.418*	-0.075*
	(0.043)	(0.042)	(0.043)	(0.227)	(0.043)

Food at home	-0.188***	-0.192***	-0.188***	-1.055***	-0.173***
	(0.042)	(0.037)	(0.042)	(0.225)	(0.041)
Food away	0.032	-0.067	0.032	-0.354	-0.019
	(0.049)	(0.048)	(0.049)	(0.262)	(0.047)
Health insurance	-0.208***	-0.162***	-0.208***	-0.883***	-0.175***
	(0.036)	(0.034)	(0.036)	(0.195)	(0.036)
Health out-of-pocket	0.021	-0.017	0.021	-0.091	0.006
	(0.038)	(0.037)	(0.038)	(0.198)	(0.037)
Total digits of response					
Single digit	omitted	Not estimable	omitted	omitted	omitted
2 digits	0.897***		0.897***	2.443***	0.706***
	(0.101)		(0.101)	(0.253)	(0.107)
3 digits	1.034***		1.034***	2.264***	0.672***
	(0.102)		(0.102)	(0.197)	(0.107)
4 digits	1.025***		1.025***	1.470***	0.540***
	(0.103)		(0.103)	(0.159)	(0.108)
5 digits	0.918***		0.918***	0.498***	0.345***
	(0.104)		(0.104)	(0.112)	(0.109)
6 digits	0.848***		0.848***		0.247**
	(0.105)		(0.105)		(0.111)
7 digits	0.836***		0.836***		-0.094
	(0.118)		(0.118)		(0.123)
8 digits	0.464		0.464		-0.156
	(0.310)		(0.310)		(0.330)
Constant			0.345***	0.265	0.248**
			(0.118)	(0.332)	(0.120)
Observations	5566	5476	5566	5476	5566
Individuals	490	490	490	490	490
Rho	0.11	0.096	0.11	0.096	0.053

Note: Displayed are estimates of alternative specifications of the first column of Table 5 using data with multiple observations (one for each survey question) for each respondent. Columns (1) and (2) present marginal effects for a Tobit model and Logit model (using a binary form of *rounding*), respectively, both with respondent-level random effects.with respondent-level random effects, respectively. Columns (3) and (4) present coefficients for the Tobit and Logit models. Column (5) is identical to the first column in Table 5--a random effects model with respondent-level random effects-- but using a binary form of rounding; coefficients can be interpreted as marginal effects. Standard errors are in parentheses. Data source: Cognitive Economics Study, 2011. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.