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BEHAVIORAL IMPEDIMENTS TO VALUING ANNUITIES:
EVIDENCE ON THE EFFECTS OF COMPLEXITY AND CHOICE BRACKETING

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Behavioral Impediments to Valuing Annuities: Evidence on the Effects of Complexity and Choice Bracketing

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ABSTRACT

This paper examines two behavioral factors that diminish people's ability to value a lifetime income stream or annuity, drawing on a survey of about 4,000 adults in a U.S. nationally representative sample. Our first main finding is that experimentally increasing the complexity of the annuity choice reduces respondents' ability to value the annuity. We measure lack of ability to value an annuity by the difference between the sell and buy values people assign to the annuity. Our second main result is that people's ability to value an annuity increases when we experimentally induce them to think jointly about the annuitization decision and about the decision of how quickly or slowly to spend down assets in retirement. Accordingly, we conclude that narrow choice bracketing is an impediment to annuitization, yet the impediment can be lessened with a relatively straightforward intervention.

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An Online Appendix Tables and Survey Instrument is available at
http://users.nber.org/~luttmer/annuityexperiment_apx.pdf

1. Introduction

The possibility of exhausting financial resources or having to curtail consumption severely at older ages is a significant risk to the well-being of older individuals, and annuities can be invaluable in helping people protect against outliving their assets. Nevertheless, there is relatively little demand for them (Mitchell, Piggott, and Takayama, 2011; Poterba, Venti and Wise, 2011). A voluminous literature reviewed in Brown (2009) explores rational explanations for why observed levels of annuitization are much lower than predicted by standard optimizing models such as those by Yaari (1965) and Davidoff, Brown, and Diamond (2005). Recent contributions to this literature include several papers that combine multiple deviations from the standard optimizing model. For instance, Ameriks, Caplin, Laufer, and Van Nieuwerburgh (2011) and Lockwood (2012) rationalize observed low annuity demand by combining a precautionary savings motive (for long-term care expenses when there is public care aversion) with a bequest motive; Reichling and Smetters (2015) do so as well by introducing stochastic mortality and correlated uninsured health care costs. Peijnenburg, Nijman, and Werker (2017) show that medical expenditure risk can rationalize low observed annuitization levels early in retirement, but not why many older people fail to buy annuities.

A different strand of literature explores whether behavioral factors contribute to low observed levels of annuitization. Several hypothetical choice experiments suggest that behavioral factors influence the demand for annuities including a set of studies showing that framing of the annuity choice affects the demand for annuities (Brown, Kling, Mullainathan, and Wrobel, 2008, 2013; Beshears, Choi, Laibson, Madrian, and Zeldes, 2014; and Brown, Kapteyn, and Mitchell, 2016). Similar findings arise in incentivized laboratory settings (Agnew, Anderson, Gerlach, and Szykman, 2008). Another source of evidence is research demonstrating that individuals in a hypothetical choice setting provide widely divergent valuations for small increases in annuitization versus small decreases in annuitization (Brown, Kapteyn, Luttmer, and Mitchell, 2017). The latter result is consistent with people having trouble assessing the value of an annuity stream and therefore requiring a high selling price and offering a low buying price, as they are reluctant to trade what they do not understand. There is also suggestive evidence from non-hypothetical choices that points to behavioral mechanisms. For instance, in 10 Swiss companies, Bütler and Teppa (2007) show that annuitization rates were much higher on average in the firms that offered an annuity as the default payout option, than in the one firm that paid out a lump sum as the default.

This finding suggests that annuitization rates are influenced by the default, implying a deviation from a standard rational model. Other papers that find patterns in observed annuitization choices that are suggestive of deviations from rational choice models include Hurd and Panis (2006), Chalmers and Reuter (2012), Previtro (2014), and Fitzpatrick (2015). Shepard (2011) and Bronshtein, Scott, Shoven, and Slavov (2016) use arbitrage arguments to show that, for many individuals, the annuitization decision implicit in when to claim Social Security benefits cannot be fully explained by a standard rational model.

While credible rational models can be constructed to match the low observed demand for annuities, our take from the literature on the annuity puzzle is that behavioral factors are also operative. In short, we share Brown's (2009, p. 185) assessment that while "it is possible to generate more limited annuitization by extending the rational model in several directions, such an approach does not seem to provide the complete answer to the puzzle" of low observed levels of annuitization. Similarly, Benartzi, Previtro, and Thaler (2011, p.161) conclude that the "tiny market share of individual annuities should not be viewed as an indicator of underlying preferences but rather as a consequence of institutional factors about the availability and framing of annuity options."

Despite the fact that many studies find that behavioral factors influence annuitization decisions, relatively little is known about the mechanisms driving this behavior. Brown et al. (2008, 2013) conclude that presenting annuities in terms of the consumption streams they generate leads to higher annuity demand, compared to presenting annuities as investment products. Brown et al. (2008) suggest that the adoption of a narrow decision frame, also referred to as choice bracketing (Thaler, 1985; Read, Loewenstein, and Rabin, 1999), may drive this finding: that is, people evaluate annuities based on the rate of return and variance of the payouts in isolation, rather than focusing on the level and variance of the consumption stream that results from holding an annuity (which is what matters for utility). It remains a leap of faith, however, to infer that the choice is more rational simply because demand is higher. Brown et al. (2017) establish that the deviation from rational choice, measured by the gap between peoples' sell versus buy price for annuities, is lower for individuals with better cognition scores. They take this as suggestive evidence that valuing annuities is cognitively challenging, because it is a complex task. Nevertheless, they do not claim that this is *causal* evidence of a mechanism, as they lack exogenous variation in the complexity of the annuitization decision.

In the present paper, we produce stronger evidence on behavioral mechanisms that may affect the annuitization decision. Rather than asking for a respondent's own hypothetical annuitization decision, we first describe a vignette where a hypothetical person faces an annuity decision, and we then ask our respondents to advise the vignette person. This alternative way of eliciting hypothetical annuitization choices allows us to experimentally vary characteristics of the vignette person that affect the complexity of the annuitization decision, but to hold the characteristics of the annuity itself constant. The annuitization decision faced by the vignette person is a choice between a lump sum amount and a change in Social Security benefits. We use the stream of Social Security benefits as the annuity in our experiment for two main reasons. First, most respondents are aware that Social Security payments last as long as they live (Greenwald, Kapteyn, Mitchell, and Schneider, 2010), which means they understand that Social Security provides an annuity even if they do not understand the term "annuity."¹ Second, because Social Security is a widely held annuity, it is natural to ask both about the value of decreases and increases in Social Security benefits, which allows us to measure the divergence between sell and buy valuations of the annuity. This divergence is our measure of deviations from rational decision making.

Specifically, we present respondents regularly interviewed by the nationally representative Understanding America Study (UAS) with a vignette in which a hypothetical person faces a choice between receiving a \$100 per month *increase* in Social Security benefits, versus receiving a lump-sum amount. We ask each respondent what the vignette person should choose and repeat the question for various values of the lump-sums until we find the lump-sum deemed equivalent in value to a \$100 per month increase in the Social Security annuity. We call this lump-sum amount the "sell" valuation, because the respondent advises the vignette person to sell a \$100 a month annuity for this lump-sum. At a different point in the experiment, we ask each respondent to advise the same vignette person on a choice between a \$100 per month *decrease* in Social Security benefits, versus paying a lump-sum. The lump-sum amount that is valued as much as the decrease in benefits is the "buy" valuation, as it represents the amount of money the respondent advises the vignette person to pay to avoid forfeiting a \$100 per month annuity. We refer to the absolute

¹ While policy risk reduces people's valuation of the stream of Social Security benefits (Luttmer and Samwick, 2017), this should reduce both the buy and sell valuation, leaving their differential unaffected.

difference between the log sell valuation and the log buy valuation as the “sell-buy spread,” and we use this to measure deviations from rational decision making.

We introduce two experimental interventions to test for two types of behavioral impediments to valuing annuities.² First, we vary the complexity of the annuitization choice. Valuing an annuity stream is more difficult when there is greater uncertainty about longevity. We experimentally manipulate this uncertainty by telling the respondent what information the vignette person received about his or her longevity from a doctor. Valuing an annuity is also more difficult when the description of the annuity contains additional information that turns out to be irrelevant, but that nevertheless takes effort to process. This is an alternative means by which we vary complexity. Second, and independently, we randomize whether or not the respondent receives information about the benefits and drawbacks of spending down non-annuitized wealth during retirement more rapidly versus more slowly. This intervention occurs before the respondent advises the vignette person about annuitization. The purpose of the intervention is to induce people to think about the consumption consequences of holding an annuity during retirement. The “consequence message” intervention therefore has the potential to be a new instrument (besides framing) to reduce the narrow choice bracketing that Brown et al. (2008) identified as a behavioral mechanism.

Our experiment yields two main findings. First, we show that greater complexity causes the sell-buy spread to rise, indicating that complexity associated with annuities reduces people’s ability to assess the value of an annuity. This is the first causal evidence of complexity as a mechanism that impedes valuing annuities, and we consider this to be the first main contribution of our paper. This result supports the interpretation offered by Brown et al. (2017) that the cognitive challenge of assessing the value of an annuity makes people reluctant to either buy or sell an annuity, leading to a low buy price but a high sell price. Our finding is consistent with results from other contexts documenting that complexity reduces people’s responsiveness to incentives or the quality of their decision making, including in work decisions (Abeler and Jäger, 2015), portfolio choice (Carlin, Kogan, and Lowery, 2013; Carvalho and Silverman, 2017), EITC

² As described later in the paper, we have additional experimental interventions to test for anchoring and to test whether results are robust. All these experimental interventions are orthogonal to the two main interventions designed to test for behavioral impediments to valuing annuities.

benefit claiming (Bhargava and Manoli, 2015), and the selection of health insurance plans (Schram and Sonnemans, 2011; Besedeš, Deck, Sarangi, and Shor, 2012a, b).

Our second result is that the “consequence message” intervention reduces the sell-buy spread. Hence, individuals are better able to assess the value of an annuity if they think about the effect of the annuity on the distribution of their future consumption streams, versus when they do not make this connection. This finding supports results in Brown et al. (2008, 2013) on the role of choice bracketing in annuity decisions. Yet unlike Brown et al., here we measure a deviation from rational decision making by the discrepancy between the buy and sell price of a small change in annuitized wealth, which is a more objective indicator of lack of rational decision making than simply the level of annuitization. We consider this additional evidence on choice bracketing the second main contribution of this paper. This finding adds to the growing empirical evidence on choice bracketing based on experimental variation in the breadth of the decision frame. For example, Bertrand and Morse (2011) report that people take out smaller payday loans when they are experimentally induced to think more broadly about the consequences of taking out such loans, and Enke (2017) shows that people develop more accurate beliefs when they are experimentally induced to adopt broader mental frames.³

Evidence that behavioral mechanisms affect annuitization decisions has the important implication that one cannot infer how much people value annuities by simply observing their annuitization decisions. Specifically, the fact that observed voluntary annuitization levels are low does not necessarily imply that utility-maximizing levels of annuitization are low as well. In light of behavioral mechanisms affecting annuitization decisions, the fact that Social Security pays out benefits exclusively in the form of an annuity is particularly valuable to people that would otherwise underannuitize.

Evidence that complexity impedes annuitization decisions has the obvious implication that individuals’ annuitization decisions can be enhanced, to the extent that this complexity can be reduced. While it may be possible to make the decision less complex by presenting information about the annuity more clearly, we stress that much of the complexity is inherent in the

³ In addition, there is compelling empirical evidence that people do not treat money as fungible. Studies showing this include Kooreman (2000), Milkman and Beshears (2009), Feldman (2010), Hastings and Shapiro (2013), Beatty, Blow, Crossley, and O’Dea (2014), and Abeler and Marklein (2017). While these papers do not experimentally vary the breadth of the decision frame, a leading explanation of these findings is mental accounting, which is a form of choice bracketing.

annuitization decision itself: people need to jointly evaluate how much they will consume each future year with and without the annuity, how much they care about consumption fluctuations, and the probability that they will be alive in each future year. No matter how well the decision is presented, it remains a complex task. Similarly, evidence that inducing people to consider the consequences of annuitization decisions for their consumption streams enables them to better assess the value of an annuity is important because it provides clear guidance on how annuitization decisions should be presented. Still, while the consequence message limits the degree to which choice bracketing acts as an impediment to valuing an annuity, we emphasize that the sell-buy spread remains substantial even for those exposed to the consequences message.

The rest of the paper proceeds as follows. Section 2 describes our methodology and explains our experimental design. In Section 3, we present our empirical findings, and Section 4 concludes.

2. Methodology and Experimental Design

2.1 Understanding America Study

Our experiment is conducted using the Understanding America Study (UAS), a probability-based Internet panel of about 6,000 adults (age 18+) representative of the U.S. population. Panel members are recruited exclusively through address-based sampling, in which invitation letters are sent to randomly-selected households using address lists obtained from the U.S. postal service. This provides a broadly representative sample, since individuals lacking prior access to the Internet were provided with a tablet and broadband Internet. In addition, the UAS contains small oversamples (about 5% each) of Native Americans and of residents of Los Angeles County. Our experimental module was fielded between June and October of 2016, and all UAS panel members at the time were invited to participate. Panel members received \$10 for completing the survey, which took an average of 14 minutes, and they could also receive additional earnings depending on their answers to quiz questions. Of the 5,521 invited panel members, 83.2% opened the link to the survey. Of those who opened the link, 99.1% completed both annuity valuation questions for an overall response rate of 82.4% (4,549 respondents).

The UAS contains demographic characteristics for all respondents as well as detailed measures of cognitive capabilities and financial literacy (the latter for about 90% of respondents). Given that cognitive ability and financial literacy are important predictors of responses to annuity

questions, we limit the analysis sample to those observations with nonmissing measures of cognitive ability and financial literacy. In addition, we exclude 0.5% of observations with missing values for any of their demographic characteristics. The final analysis sample was therefore of 4,060 observations (89.2% of the total respondents who completed both questions and 73.5% of the panel members).

We recognize that a drawback of hypothetical choice data is that people may not put as much effort in making decisions as they might in real-life situations. As a result, their answers may contain more measurement error than would be true in the real world. Nevertheless, it seems unlikely that people can fully overcome cognitive biases simply by exerting more effort. Moreover, concerns about the reliability of willingness-to-pay responses in the UAS are allayed by Mas and Pallais (2017) who show that the distribution of willingness-to-pay for flexible work arrangements obtained in the UAS closely matched the willingness-to-pay distribution provided from a similar field experiment. In our case, using hypothetical choice data has the important advantage that we can elicit *both* a willingness-to-pay and a willingness-to-accept for the same person, permitting us to measure deviations from rational decision making. We know of no field setting that allows for the simultaneous measurements of willingness-to-pay and a willingness-to-accept for an annuity for the same person. Moreover, in our setting, we observe the valuations of all respondents, in contrast to most revealed preference approaches where only the valuations of marginal individuals can be observed and the valuations of inframarginal persons can only be bounded, absent functional form assumptions.

Table 1 provides summary statistics for our baseline sample and compares it to the Current Population Survey (CPS) of the same year. Compared to the CPS, our sample overrepresents respondents between the ages of 35 and 65 by 11 percentage points, females by 6 percentage points, married respondents by 7 percentage points, Nonhispanic whites by 11 percentage points, individuals with more than a high school education by 16 percentage points, households with annual incomes above \$75,000 by 3 percentage points, households with two or fewer members by 10 percentage points, and households with no children by 5 percentage points. While these differences are generally statistically significant, the two samples are reasonably similar in terms of economic magnitudes, with the absolute difference in the fraction of respondents in a category being 5 percentage points on average across the 25 demographic categories listed in Table 1. As such, we consider our sample to be broadly representative of the U.S. adult population.

2.2 Experimental Context

Rather than describing an unfamiliar hypothetical annuity product, we use Social Security benefits as the context for the analysis of payout annuities. Specifically, we asked respondents to make trade-offs between receiving higher or lower Social Security benefits (a change in a real annuity stream), and paying or receiving different one-time payments (lump sums). Our setting is policy relevant because past discussions of pension reforms around the world, including in the U.S., have included proposals to offer workers lump-sum payments in exchange for a reduction in their annuitized pension benefits (Maurer, Mitchell, Rogalla and Tschimetschek, 2016). Several U.S. corporations have also recently offered to buy back defined benefit pension annuities from retirees in exchange for lump sums (Wayland, 2012).

2.3 Elicitation of the Valuation of an Annuity Stream

Throughout the experiment, we used vignettes to describe trade-offs and asked respondents to give the hypothetical “vignette person” advice about annuitization decisions. This approach has several attractive features. First, we can directly manipulate the complexity of the annuitization decision by using different experimental treatments. Second, we control for the respondent’s own characteristics: unlike making a decision for one’s own situation (as in Brown et al. 2017), we need not worry about factors such as liquidity constraints or private knowledge that the respondent may have about his or her situation.

The vignette person in the control condition was described as follows:

Mr. Jones is a single, 60-year old man with no children. He will retire and claim his Social Security benefits at 65. When he retires, he will have \$100,000 saved for his retirement, and he will receive \$[SSB] in monthly Social Security benefits. Based on his current health and family history, doctors have told Mr. Jones that he will almost certainly be alive at age 75 but almost certainly will not live beyond age 85.

The gender and name of the vignette person was experimentally varied between respondents. The variable *SSB* represents the vignette person’s monthly Social Security benefits, and was randomized with equal probability between respondents to \$800, \$1,200, \$1,600 and \$2,000.

Our main outcome of interest is the respondent’s advice for how the hypothetical “vignette person” should trade off annuitized wealth and lump-sum amounts at retirement. All respondents

answer a series of questions that elicit either the equivalent variation (EV) of a \$100 increase in monthly Social Security benefits, or the EV of a \$100 decrease in monthly Social Security benefits. Each respondent was asked both questions, and the order in which they were asked was randomized.

The valuation of a \$100 increment in the annuity stream was elicited by asking a series of questions of the form:

What should Mr. Jones do?

(1) Receive a Social Security benefit of $[\$SSB+100]$ per month starting at age 65.

or

(2) Receive his expected Social Security benefit of $[\$SSB]$ per month and receive a one-time payment of $[\$LS]$ from Social Security at age 65.

The \$100 increment in benefits ($[\$SSB+100]$) was displayed as a single number on the screen. The variable *LS* represents the lump-sum amount that is traded off, which was randomized between respondents to start at \$10,000, \$20,000 or \$30,000. The question was subsequently asked four more times for different values of *LS*. For example, if the person declined a \$20,000 lump sum, we inferred that that the valuation must exceed \$20,000, and on the next question we used a higher value of *LS*, namely \$60,000. Had the person accepted the \$20,000 lump sum, we would have used a lower value of *LS*. Next, if the person accepted the \$60,000 lump sum, we inferred that the valuation must lie below \$60,000, and we asked the question three more times to further reduce the difference between the lower and upper bound of the person's valuation of the \$100 increment in the annuity stream. The exact sequence of values for *LS* is shown in the survey instrument in the Online Appendix. We refer to this question as the "sell" version because the person is receiving a payment in exchange for a smaller annuity stream.

The valuation of a \$100 decrement in the annuity stream was elicited by asking a series of questions of the form:

What should Mr. Jones do?

(1) Receive a Social Security benefit of $[\$SSB-100]$ per month starting at age 65.

or

(2) Receive his expected Social Security benefit of $[\$SSB]$ per month and make a one-time payment of $[\$LS]$ to Social Security at age 65.

As before, the question was asked five times for different values of LS until we could place the respondent's valuation of the annuity into one of 32 bins. We refer to this question as the "buy" version because the person is making a payment in exchange for a larger annuity stream.

Given that a \$100 change in the annuity stream is small relative to the average monthly benefit of \$1400, a rational respondent should value this change approximately the same whether it is an increase or a decrease. We therefore take the absolute difference of the sell and buy valuations to measure the deviation from rational decision making.

2.4 Experimental Design

Our experiment consisted of a 3x2 between-subjects design, summarized in Table 2. First, we experimentally varied the complexity of the vignette in one of two ways, either by increasing the uncertainty associated with length of life (*Complexity: Wide age range* treatment), or by adding extraneous information to the vignette that was not relevant to the decision (*Complexity: Added information* treatment). For example, in the control group respondents were told that the vignette person will "*almost certainly be alive at age 75 but almost certainly will not live beyond age 85.*" By contrast, in the *Complexity: Wide age range* treatment respondents were told that the vignette person "*has an 80% chance of being alive at age 70, a 50% chance of being alive at age 80, a 20% chance of being alive at age 90, and a 10% chance of being alive at age 95.*" Determining the value of an annuity is a more complex task when the variation in possible ages of death is more dispersed, as is the case in this second vignette. The extraneous information added to the *Complexity: Added information* treatment included information about Social Security qualification rules and described the circumstances because of which the vignette person qualifies. Here the increased complexity required the respondent to think about the additional information and determine whether it was relevant.

Second, prior to the advice decision, in half of the treatments we additionally provided a message about the consequences of spending down retirement savings (*Consequence message*). This message described an interaction between a different vignette person and his or her financial advisor. In the interaction, the advisor described the benefits and drawbacks of spending down savings relatively quickly (more likely to be able to use money in one's lifetime, but running a larger risk of running out of money while alive) versus relatively slowly (less likely to run out of money, but running a larger risk of not getting to enjoy one's money in one's lifetime). This

message was framed as neutrally as possible and designed to encourage the respondent to avoid narrow choice bracketing: by inducing respondents to think about the problem of how to spend down wealth in retirement, we intended that respondents consider the annuitization decision and the asset decumulation decisions jointly, rather than as disjoint decisions. To ensure that respondents paid attention to the message, respondents were further told that, at the end of the message, they would be asked two questions about the facts in the story and would receive an additional \$1 for each question they answered correctly. These factual questions were two multiple choice questions about the financial advisor’s explanation about the benefits and drawbacks under each scenario (spending down slowly or quickly). Of the respondents who were asked the two questions, 63% answered both correctly, 27% answered one correctly, and 10% answered neither correctly.

In summary, all respondents were asked to give advice to a primary vignette person about buying and selling a small fraction of the vignette person’s Social Security annuity. Between respondents, we had two main treatments: (1) the information about the vignette person, which was randomized between “*No added complexity*”, “*Complexity: Wide age range*”, and “*Complexity: Added information*”, and (2) whether we discouraged narrow choice bracketing, where we randomized between “*No consequence message*” and “*Consequence message*.” In addition, we had six secondary randomizations. We performed two randomizations to test for anchoring, which is another indication of lack of rational decision making: (3) the starting value for the lump-sum amount (\$LS=\$10,000, \$20,000, \$30,000) and (4) the order of the two annuity valuation questions. Finally, we randomized: (5) the name and gender of the primary vignette person (*Mr. Jones, Mrs. Jones, Mr. Smith, Mrs. Smith*) – the secondary vignette person, who was featured in the consequence message, has the opposite name and gender of the primary vignette person⁴ – (6) the Social Security benefit (\$SSB=\$800, \$1,200, \$1,600 or \$2,000), (7) the order of the options shown (option with lump sum always shown first, option with lump sum always shown last), and (8) whether the consequence message first discussed the consequences of spending

⁴ In short, the secondary vignette person was female if and only if the primary vignette person was male, and vice versa. Similarly, the secondary vignette person was named Jones if and only if the primary vignette person was named Smith, and vice versa. We did this to eliminate the possibility that the consequence message affected advice on annuity choices for the primary vignette person by respondents inferring the primary vignette person’s preferences or circumstances from information provided in the consequence message. Because the consequence message used a different person, it can only have altered the advice by the respondent through the respondent thinking differently about annuitization decisions rather than the respondent knowing more about the annuitant him- or herself.

wealth down quickly or whether it first discussed the consequences of spending down wealth slowly. These latter four manipulations were intended to verify that choices in the vignette that we assumed would be innocuous indeed did not matter for our results. All randomizations occurred across subjects and were mutually orthogonal. The options within each randomization had equal probability of being selected.

2.5 Data on Cognition

To investigate how the ability to value annuities varies by cognitive ability, we merged the data from our survey with existing data in the UAS, including a financial literacy survey (Lusardi and Mitchell, 2014). We also included four subtests of the Woodcock-Johnson Test of Cognitive Ability, a nationally normed test. The sub-tests included numeracy, number series, verbal analogies, and picture vocabulary. Whereas the first two sub-tests measure numerical ability, the second two tests measure lexical ability. We standardize the financial literacy measure and each of the four test scores. For the main analysis, we create a “cognition index” from these four tests and the financial literacy measure by taking their first principal component. In the robustness section, we demonstrate the robustness of the main results to using alternative measures of cognition.

3. Results

3.1 Baseline Sample and Randomization Check

As noted in Section 2.1, our baseline sample consists of respondents who answered both annuity valuation questions and who have nonmissing values for the cognition and demographic variables. We investigate whether the exclusion from the baseline sample due to missing data is balanced across the two key treatment conditions (see Appendix Table A1), and we find that neither the complexity treatment nor the consequence message treatment affect the likelihood that the respondent failed to answer the annuity questions (p -values: 0.322 and 0.491, respectively). The fraction of observations with missing demographic data is marginally significantly higher in the complexity treatment than in the control condition, and the fraction with missing cognition data is significantly higher in the complexity treatment than in the control condition. Since both demographic and cognition data were collected prior to randomization, these findings cannot logically be a consequence of the treatment, and we conclude they were a fluke of the

randomization. There are no significant differences in the fractions with missing demographics or cognition data between the consequence treatment and the control condition. In Section 3.5 below, we explore the robustness of the main results to including observations with missing demographic or cognition information.

We also test for balance on the control variables in the baseline sample by the two main treatments (Panel B, Appendix Table A1). Of the four dozen tests of differences in means across treatments for individual control variables, four are significant at the 10-percent level and one at the 5-percent level. This is roughly what one would expect by chance. Jointly, the control variables do not significantly predict the complexity treatment (p -value: 0.107) or the consequence message treatment (p -value: 0.788).

3.2 Annuity Valuation Distributions and Summary Statistics

Figure 1 shows the distribution of buy valuations for the subsample in which the buy valuation was asked first, and the distribution of sell valuations for the subsample in which the sell valuation was asked first. By focusing on valuations when the question was asked first, we avoid any influence of anchoring on a previously-asked valuation question. The figure clearly shows that the buy valuation is lower than the sell valuation throughout the distribution. Respondents advised our hypothetical vignette individuals to *buy* an annuity that pays \$100 per month for a median price of \$4,750 (s.e.: \$180) but advised them to *sell* this annuity for a median price of \$16,250 (s.e.: \$543). This represents a statistically significant difference (two-sample Wilcoxon-Mann-Whitney rank-sum test z -statistic=25.8, p -value<0.001).

Figure 2 shows the distribution of the buy and sell valuations in the entire baseline sample. Unlike Figure 1, Figure 2 includes responses to valuation questions that followed an earlier valuation question. Again, the figure clearly shows that the buy valuation is lower than the sell valuation throughout the distribution. The median buy valuation is now \$5,875 (s.e.: \$193) and the median sell valuation is \$16,250 (s.e.: \$483). These values are now slightly closer to each other than before, in line with the effects of anchoring. Still, the distributions of buy and sell valuations remain highly significantly different (Wilcoxon matched-pairs signed-rank test z -statistic=20.1, p -value<0.001).

Table 3 presents summary statistics for the key dependent variable in our analysis, namely the absolute value of the difference between the log buy price and the log sell price. We refer to

this variable as the *spread*, and we interpret it as a measure of the deviation from rational decision making. Ninety percent of respondents have a strictly positive spread. The table also shows the components of the spread, namely the log buy price and the log sell price. Anchoring mainly affects the buy price, which is significantly higher when asked after the (generally higher) sell price is elicited. The spread is slightly higher when the sell question was asked first (2.27 versus 2.16), but this difference is only marginally significant (p -value: 0.079). Because the spread is measured as an absolute log difference, an increase in the spread of 0.11 (from 2.16 to 2.27) can be interpreted as the difference between the higher valued annuity and the lower valued annuity increasing by 11 percentage points.

Our findings on the discrepancy between buy and sell valuations are in line with the results of Brown et al. (2017), who report sell valuations that are many times higher than buy valuations for respondents asked how much they *themselves* would buy or sell an annuity that paid out \$100 per month. This similarity is reassuring as it suggests that our elicitation of valuation advice to a vignette person (rather than asking about respondents' own valuations) does not meaningfully affect the responses. A further similarity is that we also find that the log buy valuation and the log sell valuations are negatively correlated (correlation coefficient: -0.11, p -value<0.001). Our use of vignettes allows us to vary the complexity of the annuity by experimentally altering the dispersion of ages of death, which would not be ethically feasible when asking about an annuity tied to the respondent's own life. As Brown et al. (2017) explain, people feel they may be taken advantage of when they trade a good that they cannot value accurately. Accordingly, it is can be a useful heuristic to be reluctant to trade such goods, and only to sell them at a very high price (or buy them at very low price). Such a heuristic predicts a sell-buy spread whenever it is difficult to accurately determine the value of a good, as is the case with an annuity.

3.3 Treatment Effects

In Table 4, we investigate our two main research questions. The first asks whether complexity inhibits respondents' ability to value an annuity stream. The second asks whether narrow choice bracketing contributes to respondents' difficulty in valuing an annuity stream. We measure respondents' inability to value an annuity stream by the spread between their sell and buy valuations, because the spread should be approximately zero for fully rational respondents. In all

regressions, we control for the experimental manipulations,⁵ the cognition index, and a common set of control variables (see Panel B, Appendix Table A1). In Table 4, we report only the coefficients of interest (the full set of coefficient estimates is provided in Appendix Table A2).

The estimate in the first row of Column 1 shows that the complexity treatment increases the sell-buy spread by 0.131, implying a 13.1 percent increase in the ratio of the higher-valued annuity to the lower-valued annuity. To our knowledge, this is the first causal evidence that the complexity of an annuity choice affects reported annuity valuations. The fact that complexity increases the spread between the buy and sell price indicates that complexity reduces individuals' ability to accurately value an annuity. The next two columns show the effect of the complexity treatment separately on the buy and the sell price. While the estimates seem to indicate that the complexity treatment primarily operates on the buy price, and hence reduces the average of the log sell and buy price, this is not a valid interpretation because we cannot reject that increase in the sell price and the decrease in the buy price are the same in absolute value (p -value 0.302). We also evaluate whether the two types of complexity treatments (*wide age range* vs. *added information*) have different effects on the spread. As reported in Appendix Table A3, this is not the case (p -value: 0.646), and we therefore pool the two complexity treatments.

The second row shows the treatment effects of the consequence message. The consequence message decreases the sell-buy spread by 0.141. This means that inducing respondents to think about how to spend down savings during retirement causes them to report an annuity sell price and a buy price that are closer together, which is consistent with being more able to value annuities rationally. Apparently, the consequence message reduces the degree to which respondents consider annuitization and the spending down of assets during retirement as two separate decisions, a form of narrow choice bracketing. While the consequence message moves the buy and sell value closer by 14 percentage points, this still leaves a substantial spread of $2.21 - 0.14 = 2.07$ log points that remains among respondents who received the consequence message. In short, decision making among those who receive the consequence message is still far from rational, given that their spread remains well above 0. The next two columns show that the consequence message has virtually no effect on the sell price but significantly increases the buy price. In fact, it marginally significantly

⁵ We do not control for the order in which the two blocks of consequence message treatment were shown because this variable is available for only half the sample. Within the half of the sample for which this order was randomized, the order has no significant effect on the spread (p -value: 0.758).

increases the average of the log buy and sell price (p -value 0.073), suggesting the consequence message not only increases the rationality of the annuity valuations but also raises them. The latter finding is what one would expect when people jointly consider the asset decumulation decision and how to value the lifetime income stream. In particular, annuities remove uncertainty in consumption associated with asset decumulation in the face of uncertain life spans.

The third row shows that the cognition index is a very strong predictor of the sell-buy spread, with a standard deviation increase in the cognition index decreasing the sell-buy spread by 0.788. This finding underscores the conclusion that cognitive limitations play an important role in people's inability to value an annuity. This limitation had been previously established in a different setting by Brown et al. (2017), but we now have causal evidence on two mechanisms by which cognition affects people's ability to value annuities: narrow choice bracketing, and the complexity of the annuity choice. The effect of cognition also allows us to put the magnitudes of the treatment effects in perspective. Each of our two treatments, which by coincidence each has the same absolute magnitude of around 0.14, has the same effect on the spread as roughly a 17% ($=0.14/0.79$) of a standard deviation change in cognitive ability.

The remaining rows examine the effects of our secondary randomizations. Consistent with earlier findings in the literature, and indicative of less-than-fully rational decision making, we find significant effects of anchoring. When we ask the sell valuation first (which typically has a higher valuation than the buy valuation), the buy valuation is significantly higher, consistent with the buy valuation being anchored on the sell valuation. We find no significant anchoring of the sell price on the buy price when the latter is asked first. The starting values (\$10,000, \$20,000, or \$30,000) of the lump sum amount used in the annuity value elicitation procedure also have a strong effect on the valuation reported; in fact, we can reject at the 1-percent level that the starting value has no effect on the sell price or the buy price. The starting value has a similar effect on the sell and buy price, resulting in no significant net effect on the spread. The remaining randomizations cover the various choices we made in the design of the experiment (whether the lump-sum amount was the first or second choice, the monthly Social Security benefit amount, and the name of the vignette person). We anticipated that these choices would be innocuous, but the randomizations allow us to test whether outcomes indeed are insensitive to them. The last three rows show that these choices had no significant effects on our main outcome variable, the sell-buy spread. With the exceptions

of the effect of vignette name and the benefit amount on the buy price, these choices also do not affect the sell or buy price.⁶

3.4 Heterogeneous Treatment Effects

In Table 5, we explore whether the impact of our two main treatments varies across respondent subgroups. The first column examines heterogeneity in the effect of the complexity treatment, and the second column investigates whether the consequence message has different effects across subgroups. For each specification, we create two subgroups that are as close as possible in size to each other in order to maximize statistical power.

The first two specifications examine interaction effects between our treatments. One might expect that the complexity treatment has a greater impact on the spread when people engage in narrow choice bracketing, because they do not recognize how annuities help in the asset decumulation process. In line with this prediction, the point estimate of the complexity treatment is larger for respondents who do not receive the consequence message than for those who do; nevertheless, this difference is not statistically significant (p -value: 0.408). The second specification is the flipside of the first, asking whether the consequence message has a greater impact on persons exposed to the complexity treatment. While the point estimates do go in this direction, this effect is not significant either (and the p -value is the same as in the first specification by construction).

The remaining specifications examine heterogeneity by cognition, gender, education, age, and income, respectively. In none of these 10 specifications do we find a difference in the treatment effect by demographic characteristic that is significant at the 5-percent level. Respondents age 50 or older are marginally significantly more affected by the complexity treatment than younger respondents, but we are reluctant to make much of this single marginally significant result given issues surrounding multiple hypothesis testing when running a dozen specifications.

3.5 Robustness

⁶ One might expect that people with a higher Social Security benefit amount to begin with put a lower value on a \$100 change in Social Security benefits. After all, they are already more highly annuitized. To test this, we ran an alternative specification in which the baseline Social Security benefit amount is included as a linear control instead of as a set of dummy variables. Both the buy and sell value decline in the baseline amount of Social Security benefits. The effect is not significant for the sell value (p -value 0.145) but there is a significant 2.5% decline in the buy value for each additional \$100 in baseline Social Security benefits.

Table 6 examines the robustness of the two primary treatments to different measures of cognition, to different ways of selecting the sample, to different sets of controls, and to topcoding. The first row reproduces our baseline specification from Column 1 of Table 4. All subsequent rows provide estimates on the two main treatments in specifications that are identical to the baseline specification except for the change noted in the row heading.

In Panel A, we examine the robustness to using different measures of cognition because cognition is a very strong predictor of the spread and because we saw in Appendix Table A1 that the cognition index is marginally significantly higher for those who received the complexity treatment than for those who did not. Rows (2) and (3) show that the point estimates and standard errors are not at all sensitive to the details of the construction of the cognition index: it does not matter whether we control for cognition by using the first principal component of the five available cognition measures, by taking a simple average of these five measures, or by entering all five measures separately. However, it is important for the significance of complexity treatment that we exploit information from all the cognition tests. If we control only for financial literacy, the point estimate on the complexity treatment declines moderately (by about a fifth) but loses statistical significance. If we control only for the two numeracy measures or only for the two verbal measures, the point estimate on the complexity treatment declines somewhat (by less than a fifth) but becomes only marginally statistically significant. In contrast, the point estimate on the consequence message is very stable, retaining statistical significance in all three specifications that use a subset of the cognition measures.

Panel B examines robustness to different sample definitions. Row (7) includes observations with missing demographic information, row (8) includes observations with missing cognition data, row (9) includes observations with any missing information (demographic or cognition), and row (10) excludes the oversamples of Native Americans and of Los Angeles county residents. We include observations with missing values in the regression by dummifying out the missing values. While the coefficient estimate of the complexity treatment is reasonably stable, it becomes only marginally significant once observations with missing cognition data are included or the oversample is excluded. The estimate of the treatment effect of the consequence message remains significant in all specifications of Panel B. Next, Panel C investigates robustness to excluding various controls. Given the earlier finding that cognition is not quite balanced across complexity treatments, it is not surprising that the complexity treatment is sensitive to having cognition

controls included. Excluding the controls for the secondary experimental manipulations makes the complexity treatment only marginally significant. The four remaining estimates of panel C are not sensitive in magnitude or significance to the exclusion of controls.

Our module tells the respondent that the vignette person will have saved \$100,000 for retirement when making the decision on buying an annuity. Hence, logically (unless there are other sources of savings), the vignette person cannot pay more than \$100,000 for an annuity. This implies that there is an implicit topcode of \$100,000 on the buy valuations, though respondents are able to give buy valuation in excess of \$100,000 and 9% of them do so. Nevertheless, we want to be sure that this implicit topcode does not drive key results. More generally, it is useful to know that the results do not hinge on a few respondents with very high buy or sell valuations. In the final specification check, therefore, we topcode all buy and all sell valuations at \$100,000. Panel D shows that the main results are not sensitive in terms of economic magnitude or statistical significance to such topcoding.

Overall, Table 6 shows that the results on the complexity treatment are reasonably stable in magnitude but somewhat sensitive in terms of statistical significance, which falls to marginal in 6 of the 13 specification checks and disappears in 2 of them. The sensitivity can largely be traced to the fact that the cognition control, which is a very strong predictor of the spread, was not balanced across the complexity treatment and control conditions. Hence, having good controls for cognition is important for the results of the complexity treatment. By contrast, the consequence message treatment is extremely robust and remains significant in all cases.

4. Conclusion

Annuities allow people to smooth consumption in retirement when facing an uncertain age of death, yet annuity holdings are relatively low and only about 3% of individuals maximize their annual Social Security annuity payouts by delaying claiming benefits until age 70 (Social Security Administration, 2017). While these decisions may be rational for some people, in this paper we investigate whether behavioral factors impede people's annuitization choices. We do this in the context of a hypothetical choice experiment on a broadly representative sample of about 4,000 adults in the U.S. Such a setting confers two important advantages for our purposes. First, we can measure deviations from rational decision making by observing for each respondent both his willingness to pay to forgo a small decrease in annuitization and his willingness to accept to forgo

a small increase in annuitization. Second, we can experimentally vary the complexity of the annuitization decision. We also experimentally vary whether respondents are encouraged to jointly consider the annuitization decision and the asset decumulation decision during retirement (thus discouraging narrow choice bracketing), though this treatment could in principle also be applied in non-hypothetical choice settings.

Our first main finding is that increasing the complexity of the annuity decision reduces people's ability to value the annuity. This decreased ability manifests itself as an increase in the divergence of people's sell and buy price for a marginal change in annuitization. When the annuity decision becomes more complex, people tend to become more reluctant to buy or sell annuities, meaning they need greater inducements (lower buy prices or higher sell prices) to do so. Brown et al. (2017) document that a reluctance to trade annuities, as measured by the sell-buy price spread, is strongly negatively associated with cognitive ability, but of course, cognitive ability is not randomly assigned. In the present paper's setting, we experimentally vary the complexity of the annuitization decision, and thus we obtain the first causal evidence that more complex annuitization decisions reduce people's ability to place a value on an annuity, as measured by the sell-buy spread. Hence, people's low annuity holdings can at least in part be traced to the cognitive challenges of the complex task of valuing an annuity.

The second finding is that inducing people to jointly think about annuitization and how to draw down assets during retirement increases their ability to place a value on an annuity. We experimentally induce respondents to think about these decisions jointly by exposing them to a "consequence message" which explains the consequences of spending down assets more slowly or more rapidly during retirement. Respondents who are already thinking about this asset decumulation decision have a smaller sell-buy spread for annuities than respondents not exposed to the consequence message. This finding suggests that narrow choice bracketing, which the consequence message counteracts, is one behavioral mechanism that impedes people from placing a value on annuities.

Our results on the roles of complexity and cognitive ability offer relatively little scope for interventions to improve the quality of people's annuitization decisions. Cognitive ability is relatively immutable and so is the complexity of the annuitization decision for any given person's situation. While the complexity may be reduced somewhat by presenting the information about an annuity more transparently, most of the complexity stems from having to consider how the annuity

would alter consumption streams in different states of the world, which is an inherently complex task. In contrast, our finding on the role of narrow choice bracketing does offers scope for interventions that can improve people's decision making about annuities. We find that people provide more rational annuity valuations if they first consider the question of how to spend down non-annuitized wealth during retirement. We therefore conclude that annuitization decisions can be improved by inducing people to jointly think about annuitization and about spending down non-annuitized wealth.

Our paper adds to the evidence that behavioral factors influence annuitization decisions, and it also provides causal evidence on two specific mechanisms: narrow choice bracketing and cognitive limitations to dealing with complex decisions. Of course, our evidence on these two behavioral impediments to valuing annuities does not preclude other mechanisms (c.f., Brown 2009). Another avenue for future investigation might be to quantify the welfare effects of behavioral deviations from rational decision making in the context of annuitization decisions. Investigating both of these remains for future research.

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Figure 1: CDF of Sell Price and Buy Price in the Subsample without Anchoring

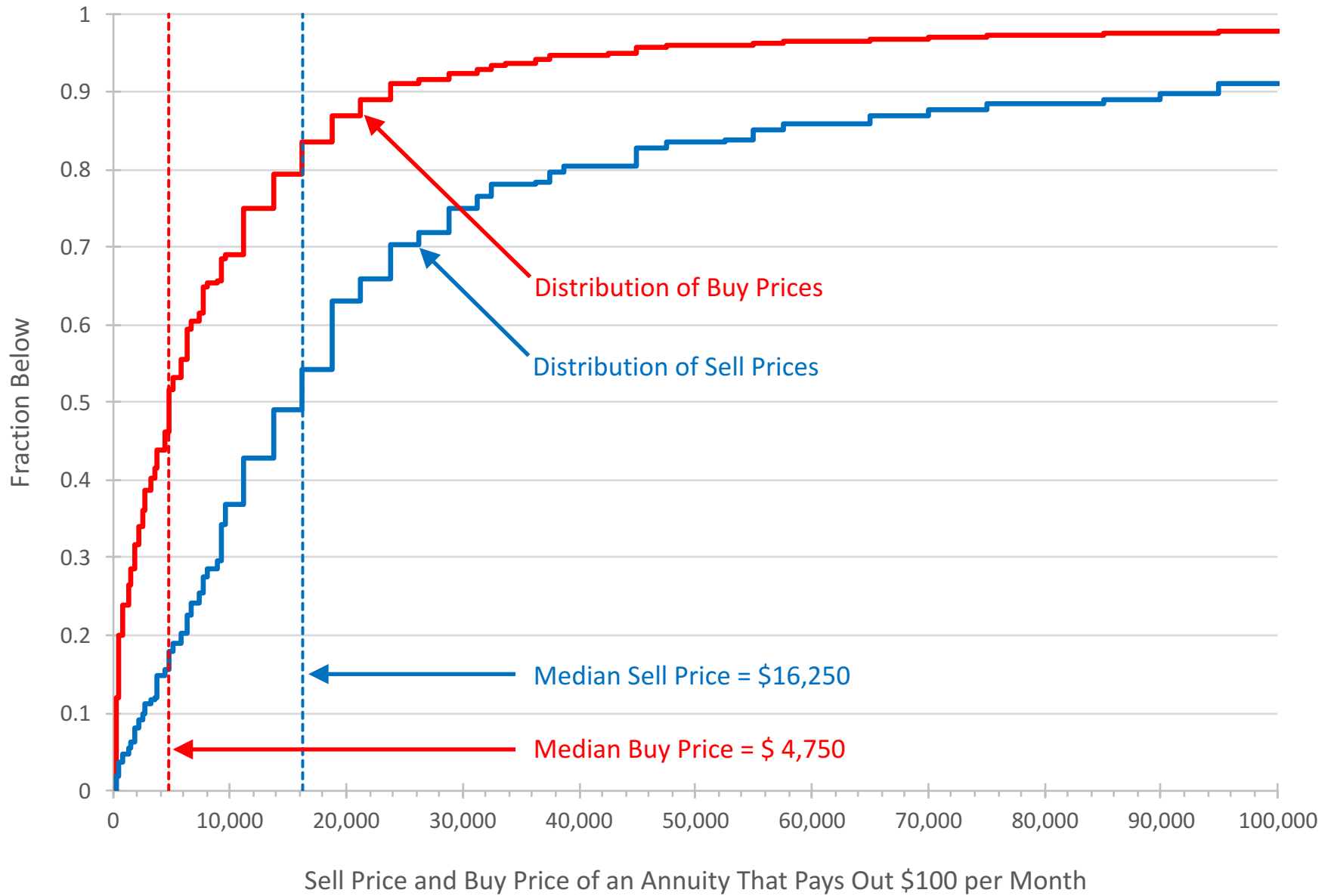


Figure 2: CDF of Sell Price and Buy Price in the Entire Baseline Sample

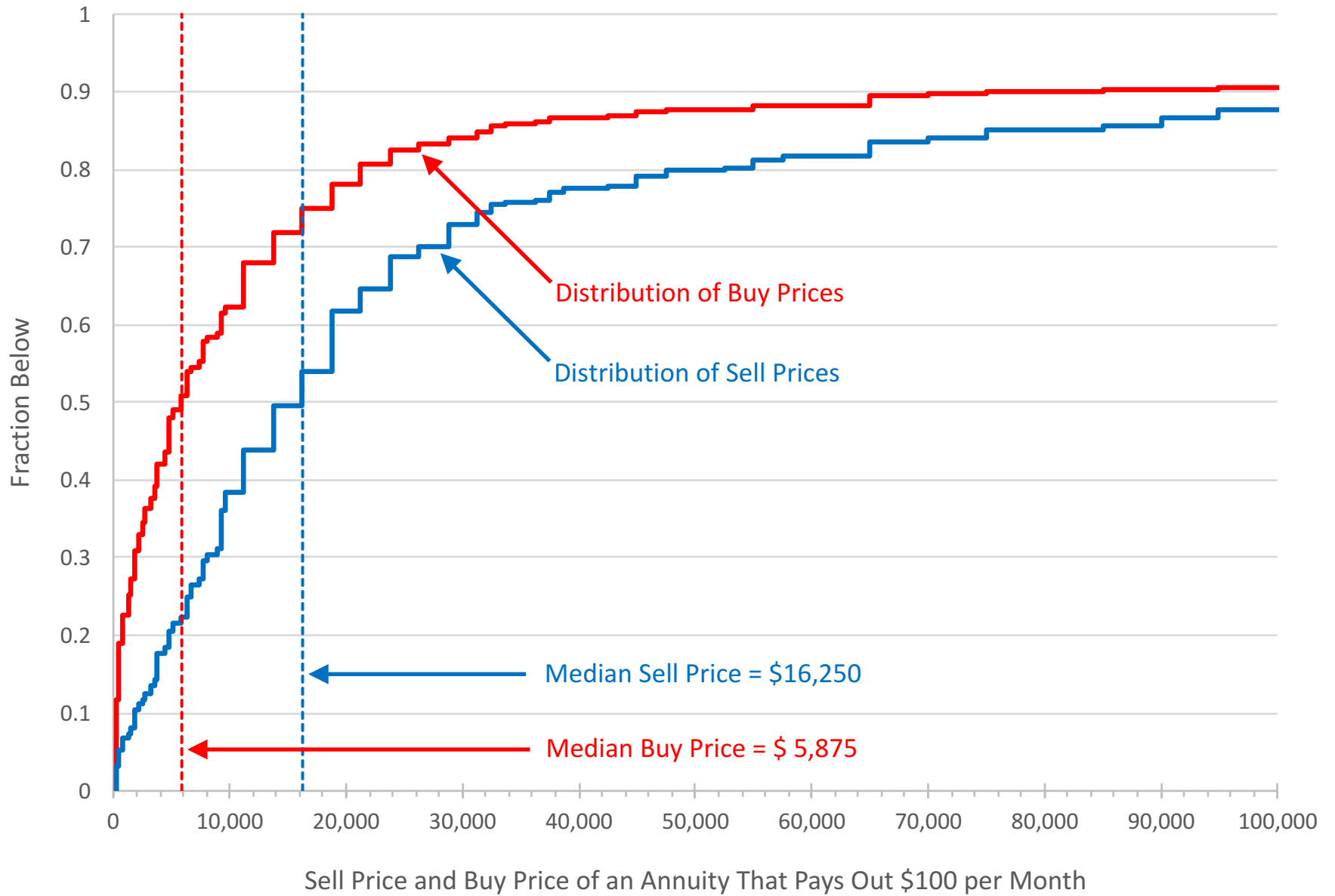


Table 1: Summary Statistics and Comparison to the CPS

Variable:	(1)	(2)	(3)
	Understanding America Survey: Ages 18+	Current Population Survey: Ages 18+	Difference
	Mean	Mean	
Age: 18-34	0.223	0.300	-0.077
Age: 35-49	0.296	0.248	0.048
Age: 50-64	0.317	0.258	0.060
Age: 65+	0.164	0.194	-0.030
Female	0.574	0.516	0.058
Married	0.597	0.527	0.070
Nonhispanic white	0.755	0.644	0.112
Nonhispanic black	0.081	0.118	-0.037
Nonhispanic other	0.078	0.080	-0.002
Hispanic	0.085	0.158	-0.072
High school dropout	0.053	0.117	-0.064
High school education	0.193	0.290	-0.096
Some college	0.388	0.286	0.102
Bachelor's degree	0.218	0.195	0.023
Graduate degree	0.148	0.112	0.036
Household Income: Less than 25k	0.166	0.161	0.005
Household Income: 25k-50k	0.176	0.205	-0.029
Household Income: 50k-75k	0.165	0.173	-0.008
Household Income: 75k-100k	0.130	0.138	-0.008
Household Income: Above 100k	0.364	0.324	0.040
Household size of one	0.201	0.145	0.057
Household size of two	0.390	0.342	0.048
Household size of three	0.174	0.191	-0.017
Household size of four or more	0.235	0.322	-0.087
Any kids	0.328	0.378	-0.050
Observations	4,060	134,420	

Notes: Column 1 shows the demographic characteristics of respondents in our baseline sample from the Understanding America Survey. The UAS data throughout the paper are unweighted. The Current Population Survey data tabulated in the second column come from the 2016 Annual Social and Economic Supplement and are weighted. The sample is limited to non-institutionalized respondents age 18 and older. With four exceptions, each demographic characteristic's mean is statistically significantly different at the 1 percent level between the two samples. The exceptions are for the means of the fractions "Nonhispanic other," "Household Income: Less than 25k," "Household Income: 50k-75k," and "Household Income: 75k-100k," which are not even marginally statistically significantly different.

Table 2: Experimental Design

	No consequences message	Consequences message
No added complexity	Vignette 1: Mr. Jones is a single, 60-year old man with no children. He will retire and claim his Social Security benefits at 65. When he retires, he will have \$100,000 saved for his retirement, and he will receive \$[SSB] in monthly Social Security benefits. Based on his current health and family history, doctors have told Mr. Jones that he will almost certainly be alive at age 75 but almost certainly will not live beyond age 85.	One of the vignettes 1-3 is offered with a consequence message preceding it, which is always the following. The order of the last two paragraphs was randomized. First, we will show you a story about Mrs. Smith. Please pay close attention to the story, because at the end we will ask you two questions about the story. You will receive an additional \$1 for each question you answer correctly. Mrs. Smith is a single, 65-year old woman with no children, and she is as healthy as the typical 65-year old woman. She just retired and receives her monthly Social Security check. She is talking with her financial adviser on how to spend her substantial savings in retirement. Her advisor explains that she could decide to spend down her savings relatively quickly. In this case, she will be more likely to be able to enjoy her money during her lifetime. But she also runs a risk of running out of money while alive and having to cut back on her spending as a result. Her advisor explains that she could also decide to spend down her savings relatively slowly. In this case, she will be less likely to run out of money. But now she runs a risk of not getting to enjoy all her money during her lifetime.
Complexity: Wide age range	Vignette 2: Mr. Jones is a single, 60-year old man with no children. He will retire and claim his Social Security benefits at 65. When he retires, he expects to have \$100,000 saved for his retirement, and expects to receive \$[SSB] in monthly Social Security benefits. Based on his current health and family history, doctors have told Mr. Jones that he has an 80% chance of being alive at age 70, a 50% chance of being alive at age 80, a 20% chance of being alive at age 90, and a 10% chance of being alive at age 95.	
Complexity: Added information	Vignette 3: Mr. Jones is a single, 60-year old man with no children. Social Security rules state that you need at least 40 credits, or 10 years of work, to qualify for Social Security – and Mr. Jones qualifies since he has worked for 30 years. Since Mr. Jones was born in 1956, his full retirement age is 66 years and 4 months, but he is eligible to start claiming starting at 62. He will retire and claim his Social Security benefits at 65. When he retires, he will have \$100,000 saved for his retirement, and he will receive \$[SSB] in monthly Social Security benefits. Based on his current health and family history, doctors have told Mr. Jones that he will almost certainly be alive at age 75 but almost certainly will not live beyond age 85.	This story is followed by two 4-option multiple-choice questions to induce the respondent to pay attention to the story. One question asks about the benefits and drawbacks of spending down wealth quickly while the other asks about the benefits and drawbacks of spending down wealth slowly. See the online appendix for the exact wording of these questions. The screen with the two test questions is followed by an advice question where the respondent is asked to advise the vignette person how quickly to spend down her wealth. This question was asked to induce the respondent to think about the problem of how to spend down wealth during retirement. See the online appendix for the exact wording of this question.

Note: The experiment featured 6 main treatment cells: one of Vignettes 1-3 without the consequences message, or one of the same three vignettes with a consequence message preceding it, as described in the “Consequences message” column. Additionally, we include experimental variation in the name and gender of the vignette person: Mr./Mrs., Smith/Jones, and the order of the last two paragraphs of the consequence message (spending quickly first, versus spending slowly first) are independently randomized across respondents. A different name and gender was used in the consequence message from that in the vignette. Finally, there is experimental variation in the monthly Social Security benefit value ($SSB = \$800, \$1,200, \$1,600, \text{ or } \$2,000$).

Table 3: Descriptive Statistics on the Sell Price, Buy Price, and Spread

	(1)		(2)		(3)	(4)	
	Sell Question First		Buy Question First			Entire Baseline Sample	
	Mean	Standard Deviation	Mean	Standard Deviation	p-value on difference	Mean	Standard Deviation
Sell value (log)	9.65	1.53	9.71	1.96	0.257	9.68	1.76
Buy value (log)	9.06	2.43	8.28	1.68	0.000	8.67	2.12
Sell-Buy Spread	2.27	2.04	2.16	2.21	0.079	2.21	2.13
N	2,009		2,051			4,060	

Notes: Whether the buy valuation or sell valuation was asked first was randomized for each respondent. The p-value corresponds to the test that the mean in column 1 is equal to the mean in column 2. The Sell-Buy Spread is defined as the absolute difference between the log sell price and the log buy price for an annuity stream of \$100 per month.

Table 4: Treatment Effects on the Sell-Buy Spread and its Components

	(1)	(2)	(3)
Explanatory variables:	Sell-Buy Spread	Sell price (log)	Buy price (log)
Complexity treatment	0.131** (0.065)	0.050 (0.057)	-0.137** (0.068)
Consequence message treatment	-0.141** (0.062)	0.011 (0.055)	0.133** (0.065)
Cognition index	-0.788*** (0.043)	-0.188*** (0.038)	0.098** (0.046)
Sell question first	0.166*** (0.062)	-0.043 (0.055)	0.777*** (0.065)
P-value on lump-sum starting values	0.623	0.000	0.000
P-value on lump-sum shown first	0.633	0.425	0.316
P-value on SS benefit amounts	0.249	0.363	0.000
P-value on vignette names	0.375	0.552	0.033
Demographic controls	Yes	Yes	Yes
R ²	0.157	0.035	0.067
N	4,060	4,060	4,060

Notes: The Sell-Buy Spread is defined as the absolute difference between the log sell price and the log buy price for an annuity stream of \$100 per month. Each column displays the results from a single OLS regression, with the dependent variable listed in the column heading. Coefficient estimates on the secondary experimental treatments and the control variables are reported in Appendix Table A2. Robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5: Heterogeneity in Treatment Effects

Dependent Variable: Sell-Buy Spread						
Specification:	(1) Complexity Treatment		(2) Consequence Message Treatment		R ²	N
	Coeff.	(S.E.) [p-value]	Coeff.	(S.E.) [p-value]		
(1) By Consequence Message					0.1569	4,060
No consequence message	0.185**	(0.094)				[1,998]
Consequence message	0.078	(0.089)				[2,062]
P-value on test of equal coefficients		[0.408]				
(2) By Complexity Treatment					0.1569	4,060
No complexity treatment			-0.071	(0.104)		[1,409]
Complexity treatment			-0.178**	(0.077)		[2,651]
P-value on test of equal coefficients				[0.408]		
(3) By Cognition					0.1574	4,060
Below median cognition index	0.132	(0.103)	-0.167*	(0.099)		[2,030]
Above median cognition index	0.133*	(0.077)	-0.117	(0.074)		[2,030]
P-value on test of equal coefficients		[0.988]		[0.682]		
(4) By Gender					0.1568	4,060
Female	0.126	(0.089)	-0.152*	(0.086)		[1,729]
Male	0.139	(0.093)	-0.125	(0.088)		[2,331]
P-value on test of equal coefficients		[0.917]		[0.826]		
(5) By Education					0.1569	4,060
Some college or less	0.135	(0.085)	-0.179**	(0.082)		[2,577]
Bachelor's degree or more	0.122	(0.098)	-0.074	(0.092)		[1,483]
P-value on test of equal coefficients		[0.923]		[0.397]		
(6) By Age					0.1577	4,060
Below median (less than 50)	0.022	(0.091)	-0.191**	(0.086)		[2,107]
Above median (50 or more)	0.252***	(0.092)	-0.083	(0.089)		[1,953]
P-value on test of equal coefficients		[0.075]		[0.383]		
(7) By Income					0.1573	4,060
Below median (less than \$75k)	0.074	(0.097)	-0.220**	(0.091)		[2,054]
Above median (\$75k or more)	0.186**	(0.086)	-0.060	(0.083)		[2,006]
P-value on test of equal coefficients		[0.387]		[0.196]		

Notes: The Sell-Buy Spread is defined as the absolute difference between the log sell price and the log buy price for an annuity stream of \$100 per month. Each row reports the results from a single OLS regression in which the two main experimental treatments are interacted with the characteristics listed in the row header. Robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6: Robustness of the Main Treatment Effects

Dependent Variable: Sell-Buy Spread	(1)	(2)	(3)	(4)
Specification:	Coefficient on Complexity Treatment	Coefficient on Consequence Message Treatment	R ²	N
(1) Baseline	0.131** (0.065)	-0.141** (0.062)	0.1568	4,060
Panel A: Changing Cognition Measures				
(2) Cognition index is the simple average of the 5 cognition measures	0.131** (0.065)	-0.141** (0.062)	0.1554	4,060
(3) All five components of cognitions score entered separately	0.131** (0.065)	-0.137** (0.062)	0.1614	4,060
(4) Financial literacy is the only cognition measure	0.107 (0.066)	-0.128** (0.063)	0.1146	4,060
(5) Numeracy measures are the only cognition measures	0.121* (0.065)	-0.153** (0.062)	0.1495	4,060
(6) Verbal measures are the only cognition measures	0.111* (0.066)	-0.152** (0.063)	0.1174	4,060
Panel B: Sample Selection				
(7) Include observations with missing demographics (dummied out)	0.130** (0.065)	-0.139** (0.062)	0.1585	4,081
(8) Include observations with missing cognition index (dummied out)	0.118* (0.062)	-0.120** (0.059)	0.1422	4,528
(9) Include observations with any missing values (dummied out)	0.117* (0.062)	-0.118** (0.059)	0.1441	4,552
(10) Exclude Native American and LA county oversamples	0.113* (0.068)	-0.169*** (0.064)	0.1632	3,704
Panel C: Different Controls				
(11) No cognition controls	0.087 (0.068)	-0.159** (0.064)	0.0825	4,060
(12) No demographic controls	0.137** (0.065)	-0.140** (0.062)	0.1465	4,060
(13) No secondary experimental controls	0.125* (0.065)	-0.138** (0.062)	0.1534	4,060
Panel D: Topcoding				
(14) Buy and sell valuations topcoded at \$100,000	0.111** (0.054)	-0.108** (0.051)	0.1427	4,060

Notes: Each row displays the results of a single OLS regression that is identical to the baseline regression shown in Column 1 of Table 4 except for the difference described in the row header. In the baseline regression, the cognition index is the first principal component of five standardized cognition measures: financial literacy, a numeracy score, a number series score, a verbal analogies score, and a picture vocabulary score. Robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.