

I'LL CROSS THAT BRIDGE IF I GET TO IT:  
FOCUSING ON THE NEAR (CERTAIN) FUTURE\*

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**Abstract**

In many important economic decisions under uncertainty, the expected-utility maximizing choice is *ex post* optimal only if certain later states are reached. We investigate whether, outside of discounting, subjects are biased towards options faring well in early-period comparisons. We conduct neutral-context laboratory experiments to systematically investigate this bias and its interaction with an endowment bias. We find that the ordering of payoffs matters. Compared to a frame in which a single draw from a known distribution determines subject outcome, a frame making explicit the temporal ordering of payoffs decreases choice of the option whose *ex post* optimality requires reaching later states. We also find a strong endowment effect. We conclude with the implication of these findings on annuity choice by a recent retiree, as both of these biases work against annuity purchase by the 401(k)-endowed retiree.

*JEL Classification:* C91, D14, D81, G22, J26

*Key words:* uncertainty, experimental economics, behavioral, annuities

1 INTRODUCTION

The not always rational attraction to an option preferred in the short run over one whose benefits accrue in the long run is well documented. In this study, we consider such choices when the latter option, although perhaps *ex ante* optimal (depending on risk preferences), is *ex post* optimal only if certain later states are actually reached. In particular, we investigate whether, independent of discounting future utility, people place undue focus on early state comparisons relative to later states not reached with certainty.

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Our motivating example is the asset allocation decision of a recent retiree. If she is like many, insuring against the outliving her assets by annuitizing a significant portion of her retirement wealth offers significant expected welfare benefits (Yaari 1965, Davidoff, Brown and Diamond 2005). However, if she is like most, she will fail to annuitize (Johnson, Burman and Kibes 2004). A bias towards early period comparisons—plausibly underlying the commonly-expressed concern about getting “hit by a bus” shortly after annuity purchase—would work against annuity purchase. Assuming the annuity increases expected utility, *ex post* the annuity is going to have been the better decision only in the event of a long retirement. Due to likely higher early period consumption and other benefits of maintaining a lump sum of assets (such as a bequest motive), the lump sum likely offers better outcomes in early periods and in the event of a short retirement. A number of other interesting economic decisions share this relationship between payoff ordering and uncertainty. For example, the *ex post* optimality of saving for retirement may well depend on actually reaching retirement and being healthy enough to enjoy retirement consumption. Likewise, the payoff to car maintenance might depend on owning the car for the long haul. Finally, while additional studying for an exam may be optimal in expectation, its *ex post* optimality may depend on *ex ante* uncertain exam difficulty. Of course, the usual suspect for a preference for short-run utility is discounting, either by way of a high discount rate or present-biased preferences consistent with hyperbolic discounting. It is our hypothesis that when faced with temporally ordered risks, such as getting hit by a bus early in retirement versus the risk of a long retirement, undue weight on closer-in-time comparisons (essentially overweighting the risk of an unfortunate bus encounter) may lead to suboptimal decisions even when controlling for the discounting of future utility.

This risk-ordering bias, which has not been previously studied to the best of our knowledge, may stem from two sources. First, the decision maker may focus on earlier comparisons and risks because they are, well, earlier and perhaps more focal. Second, in cases such as saving for retirement or deciding whether to annuitize, the bias may also result from the compound, sequential nature of risks inherent in mortality. Surviving to age 70 requires surviving age 69, which requires surviving age 68, and so forth.<sup>1</sup> For perhaps both reasons, when faced with this sequence of risks, it is our hypothesis that some place too much salience on early-period utility comparisons over later-period comparisons. Note that we do not assume that the decision maker has incorrect beliefs about event likelihoods, nor that she overly discounts future utility. Rather, given event-likelihood beliefs and the (discounted) utility for each option and for each event, she is biased towards early comparisons. This bias is related to and may work in conjunction with, but is distinct from, hyperbolic discounting

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<sup>1</sup>We do in fact control for problems with sequential probabilities in some of our sessions.

and myopic loss aversion. We discuss their relation in Section 5.

We conducted two laboratory experiments to systematically investigate the role the risk-ordering bias in a setting removing discounting while controlling for probability beliefs. In both experiments, the number of Payout Phase rounds is uncertain, and the subject chooses between 2 payout options before uncertainty resolution. We compare option choice in a frame in which subjects must survive earlier periods to reach later ones (the Sequential frame) to a frame in which we remove the sequential ordering of risks (the Simultaneous frame) by determining Payout Phase outcome (i.e., the total number of rounds) with a single draw from a known probability distribution. Importantly, event likelihood does not differ across frames: a three-period Payout Phase in the Sequential frame has the same likelihood as Event C in the Simultaneous frame. As all subject payments occur at the end of the session, we remove discounting as a potential explanation for any cross-treatment differences.

In Experiment 2, we endow subjects with a Payout Phase option. In some applications, the decision maker may have an endowed or default option, and this default may influence her decision. Whether she is automatically enrolled in a 401(k) plan has been shown to affect her participation (Madrian and Shea 2001). In the annuity context, a retiree may be averse to exchanging the lump sum in her retirement account for a stream of payments. Attaching ownership to her endowed stock of wealth, she willingly forgoes some annuitization gains in order to ensure that she does not lose her endowment.<sup>2</sup> We investigate in Experiment 2 the interaction between any endowment effect and the risk-ordering bias. While we use a neutral context (avoiding words such as retirement, annuity, and death), the decision of whether or not to annuitize inspires our Experiment-2 design. In addition to varying the manner in which uncertainty is resolved, we vary across frames the denomination of experimental retirement assets. Subjects either a) earn a 401(k)-like account balance; b) earn a claim on annuity-like payment stream; or c) are not explicitly endowed with either option.

We find support for our risk-ordering hypothesis in both Experiments. Subjects in Sequential sessions select less frequently the option faring better only if later states are actually reached than subjects in the Simultaneous sessions. In Experiment 2, we also find an endowment bias. When subjects earn a lump-sum of assets, they select the annuity-like option less frequently than those in no-endowment sessions. Furthermore, when subjects earn a stream of payments, they are more likely to select this annuity-like option relative to those earning a comparable lump-sum of assets.

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<sup>2</sup>We do not in this study consider alternative endowment-effect causes, such as transaction costs associated with evaluating and carrying out a trade.

## 2 HYPOTHESES

We assume decision maker  $i$  must choose between two payout options  $j = \{0, 1\}$  whose payoffs depend on chance. We assume states  $t = \{1, 2, \dots, T\}$  are ordered:  $t$  is only possible if  $t - 1$  has been reached, with  $q_t$  the probability of reaching  $t$  and  $p_t > 0$  the probability of reaching exactly  $t$  states. Given option  $j$ , the decision maker accrues  $x_t^j$  if she reaches state  $t$ . When all uncertainty is resolved (i.e., she has reached exactly  $t$  states), she receives  $y_t^j = \sum_{\tau=1}^t x_\tau^j + b_t^j$ , where  $b_t^j$  is a final-period payoff perhaps equal to zero. Finally, we assume there exists a  $\bar{t}$  (with  $1 < \bar{t} < T$ ) such that  $y_t^0 > y_t^1$  for all  $t < \bar{t}$ , and  $y_t^0 < y_t^1$  otherwise. Expected utility for subject  $i$  for payout option  $j$  is:

$$\begin{aligned} EU_i^j &= \sum_{t=1}^T [q_t - q_{t+1}] u_i \left( \sum_{\tau=1}^t x_\tau^j + b_t^j \right) \\ &= \sum_{t=1}^T p_t u_i \left( \sum_{\tau=1}^t x_\tau^j + b_t^j \right) \\ &= \sum_{t=1}^T p_t u_i (y_t^j). \end{aligned} \tag{1}$$

Equation 1 expresses an option's expected utility in terms of the probability of outcome  $t$  and its total payoff ( $y_t^j$ ). Note that if  $x$  and  $b$  are discounted utils,  $u_i (y_t^j) = y_t^j$ . Defining  $\Delta_i \equiv \ln\left(\frac{EU_i^1}{EU_i^0}\right)$ , the decision maker chooses option 1 if  $\Delta_i \geq 0$ .

Uncertainty is resolved in one of two ways. Whereas in Sequential treatments the decision maker learns in state  $t - 1$  whether she proceeds to  $t$ , in Simultaneous treatments the decision maker is unaware of the ordering of states, knows  $p_t$ , and learns only her final state.

We first suppose that following Cumulative Prospect Theory (Tversky and Kahneman 1992), the weights that a subject assigns to different outcomes,  $\vec{\pi}$ , may deviate from actual probabilities,  $\vec{p}$ . Given these subjective weights, we define subjective expected utility

$$EU_i^j(\vec{\pi}) \equiv \sum_{t=1}^T \pi_t u_i (y_t^j). \tag{2}$$

Defining  $\Delta_i(\vec{\pi}) \equiv \ln\left(\frac{EU_i^1(\vec{\pi})}{EU_i^0(\vec{\pi})}\right)$ , a subject chooses option 1 if  $\Delta_i(\vec{\pi}) \geq 0$ . In particular, we make the following assumption about decision weights in our treatments.

**Assumption 1.** *In the Sequential treatments,  $\sum_{\tau=1}^{\bar{t}} \pi_\tau > \sum_{\tau=1}^{\bar{t}} p_\tau$  and  $\sum_{\tau=\bar{t}}^T \pi_\tau < \sum_{\tau=\bar{t}}^T p_\tau$ , with  $\sum_{\tau=1}^T \pi_\tau = 1$ . In the Simultaneous treatments,  $\vec{\pi} = \vec{p}$ .*

Assumption 1 states that the decision maker will put more weight on early period comparisons and less weight on later comparisons, and in particular more weight on those outcomes

where option 0 has a higher payout than option 1. Therefore, under Assumption 1, it will generically be the case that  $\Delta_i(\vec{\pi}) < \Delta_i$ . This leads to the following hypotheses concerning the effect of probability framing.

**Hypothesis 1.** *Holding endowed payout option constant, the proportion of subjects choosing the option whose payouts are higher only in later state comparisons (option 1) will be greater in the Simultaneous treatment than in the Sequential treatment.*

We next suppose, once again following (Cumulative) Prospect Theory, that a subject evaluates uncertain outcomes relative to her initial endowment:  $v(y^j)$ . Given this value function, we define subjective expected value conditional on endowment  $J$ :

$$EU_i^j(\vec{\pi}, J) \equiv \sum_{t=1}^T \pi_t v_i(y_t^j). \quad (3)$$

We define  $\Delta_i(\vec{\pi}, J) \equiv \ln\left(\frac{EU_i^1(\vec{\pi}, J)}{EU_i^0(\vec{\pi}, J)}\right)$ , with  $J = \{0, 1\}$ . Endowed with the option 1, she keeps it if  $\Delta_i(\vec{\pi}, an) \geq 0$ , and if endowed with the option 0, she does not trade for option 1 if  $\Delta_i(\vec{\pi}, ls) < 0$ .

In the spirit of Prospect Theory, we make the following assumptions about the value function:

**Assumption 2.** *Endowed with option  $j$ , the value function  $v(\cdot)$ :*

- *is convex for  $y_t^{-j} < y_t^j$  (i.e., over losses relative to endowment);*
- *in concave and equal to  $u(\cdot)$  for  $y_t^{-j} \geq y_t^j$  (i.e., over gains relative to endowment);*
- *equal to  $u(\cdot)$  at  $y_t^{-j} = y_t^j = 0$ .*

Under Assumption 2, it will generically be the case that  $\Delta_i(\vec{\pi}, 1) > \Delta_i(\vec{\pi}, 0)$  (i.e., an endowment effect). This relationship will hold when  $\vec{\pi} = \vec{p}$ . This leads to the following hypothesis comparing choices across payout-option endowments.

**Hypothesis 2.** *Holding constant manner of uncertainty resolution, the proportion of subjects choosing option  $j$  option will be greater in those endowed with  $j$  than in those endowed with  $-j$ .*

### 3 EXPERIMENT 1

In Experiment I, we focus on testing our hypothesis of a bias towards early period comparisons. In all treatments, subject payment depends on the subject-chosen payout option and which of 5 equilikely events happens, with option choice preceding uncertainty resolution.

In the **Sequential** treatments, sequential survival determines the number of Payout Phase rounds (i.e., the event). The subject is initially presented with a bag containing 4 green and 1 red marble. She draws marbles *without replacement* until she draws the red marble. As she starts in round 1, her number of rounds is equal to her number of draws from the bag.<sup>3</sup> The probability of each Payout Phase length is 1 in 5.

[Figure 1 about here.]

In the **Sequential Ascending** treatment, a subject *accrues* money in each round (Figure 1a). If she selects the Orange option, she starts with \$7.00, and receives 50 cents in each round she reaches. If she selects the Blue option, she starts with no money, and receives \$3 in each round she reaches. In the **Sequential Descending** treatment, the subject *loses* money in each round (Figure 1b). If she selects the Orange option, she starts with \$10, and loses 50 cents each round she reaches. If she selects the Blue option, she starts with \$18, and loses \$3 in each round she reaches.<sup>4</sup> Note that each Sequential treatment has the same distribution of payouts for each option.

In **Simultaneous** sessions (the baseline), a single draw from a bag containing 5 lettered chips determined Payout Phase length. There was no mention of time or rounds. We identified Payout Phase lengths as Events A, B, . . . , E. For one-half of the subjects in this treatment, Event-A payments corresponded to a 1-round Payout Phase in the Sequential Ascending treatment, whereas for the other half, Event-A payments corresponded to a 1-round Payout Phase in the Sequential Descending treatment.

Subjects indicated their choices on treatment-specific Choice Sheets. For each event, the choice sheet indicated the payment for each option as well as event likelihood. For the Sequential treatments, Choice Sheets indicated each round’s payment or loss, as well as the conditional probability of continue to the next round.

Across treatments, we vary the manner in which uncertainty is resolved, but keep constant the distribution of payments for each option and the likelihood of each payment. While the Blue option has a higher expected payout, in the Sequential Ascending treatment it does poorly in early period comparisons, and the subject must survive to at least the third round in order for it to have been the *ex post* higher earning choice. We therefore expect fewer subjects to take the Blue option relative to the the Simultaneous treatment. In the Sequential Descending treatment, the Blue option does well in early period comparisons. We therefore expect more subjects to take the Blue option relative to the Simultaneous treatment.

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<sup>3</sup>Fatas, Lacomba and Lagos (2007) used a similar method to determine retirement length in their investigation of retirement timing.

<sup>4</sup>The experimenter did, in fact, give each subject a sum of money before the number of rounds were determined and added to it each round in Ascending sessions and took money back in Descending sessions.

Rationally, risk preferences ought to affect choice independent of treatment. Therefore, after all subjects made the choice of payout option, each subject completed a modified Holt-Laury risk-aversion assessment (Holt and Laury 2002) further described below. Subjects also completed a demographic questionnaire. Subjects were paid for either the Payout Phase or the Holt-Laury assessment, with the determination made by coin flip.<sup>5</sup>

We use a between-subject design, with 30 Williams College students in each treatment and an average of 10 subjects per session.<sup>6,7</sup> Sessions lasted approximately 30 minutes. The average payment was \$9.40.

### 3.1 Experiment 1 Results

[Figure 2 about here.]

In Figure 2, we depict the choice of the Blue option across treatments. The unconditional results are consistent with our hypothesis. In the Sequential Ascending treatment where the subject must survive early rounds to reach states in which the Blue option dominates, 40% chose the Blue option. In the Sequential Descending treatment where the Blue option is *ex post* optimal if the Payout Phase lasts 3 rounds or fewer, 90% chose the Blue option. Finally, in the Simultaneous treatment where the number of Payout Phase rounds is determined by a single draw, 63% chose the Blue option. Note that 10 out of 15 subjects chose Blue when Simultaneous treatment choice sheets ordered events as in Sequential Descending, while 9 out of 15 chose Blue when events were ordered as in Sequential Ascending. As we cannot reject the null hypotheses of equal Blue choice across these two groups ( $\chi^2$  test,  $p = 0.705$ ), we pool all Simultaneous subjects in further analyses.

**Result 1.** *Relative to the Simultaneous baseline, the Sequential Descending treatment increased Blue choice while the Sequential Ascending treatment weakly decreased Blue choice.*

**Support:** Using the  $\chi^2$  test of the null hypotheses of equal proportions, we weakly reject the hypothesis of equal likelihood of Blue choice across in the Simultaneous and Sequential Ascending treatment ( $p = 0.069$ ), and reject the hypothesis when comparing Simultaneous and Sequential Descending ( $p = 0.012$ ).

Formal testing supports our hypothesis. In all frames, we held constant outcome likelihood and each option’s payment across outcomes. Compared to the frame in which a single draw determined subject payment, ordering the outcomes so that a subject must survive

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<sup>5</sup>Payment for the Holt-Laury assessment was determined by two draws with replacement from a bag with 10 numbered chips: the first determined decision number, the second outcome.

<sup>6</sup>Subjects were recruited through the online recruitment system ORSEE (Greiner 2004).

<sup>7</sup>There were 31 subjects in Sequential Ascending. We drop the 1 subject who gave inconsistent responses on the Holt-Laury assessment. As this subject chose the Orange option, including this subject would only strengthen our results.

early rounds to reach outcomes in which the Blue *ex post* dominates weakly decreases Blue choice. Likewise, when the Blue option did well in early round comparisons, Blue choice increased significantly.

[Table 1 about here.]

While the unconditional tests provide evidence in favor of our hypotheses, cross-treatment differences in subject characteristics (Table 1) may over or under-state the treatment effect. We estimate a series of probit models of Blue choice to control for subject characteristics which may be correlated with option choice. In particular, the Orange option with its smaller payout variance will be more attractive to the relatively risk averse. Subject choices on the Holt-Laury instrument provide an independent measure of risk preferences. For each of 10 decisions, a subject chose between a safe option (where “Left” pays \$10.00 and “Right” \$8.00) and a risky option (where “Left” pays \$19.25 and “Right” \$0.75). The probability of left linearly increased from 1/10 in decision one to 10/10 in decision ten. As the probability of the good outcome (Left) increased monotonically from decision one to ten, consistency dictates only one switch from safe to risky option (or the risky option for all decisions).

In our analysis, we use two different measures of risk preferences. We define the **HL Score** as the decision number the subject first chose the risky option. Second, we construct an indicator (**Should Prefer Blue**) equal to one if the expected-utility-maximizing subject would have selected Blue given her HL Score. To construct this variable, we solve for the range of risk preferences consistent with each HL Score assuming CRRA utility. We also compute the coefficient of relative risk aversion consistent with indifference between the Blue and Orange options ( $\rho = 0.454$ ). A subject with  $\rho = 0.457$  would be indifferent between switching to the risky option at decision 5 ( $\frac{1}{2}$  chance of left) and switching at decision 6 ( $\frac{6}{10}$  chance of left). Therefore, we set **Should Prefer Blue** equal to 1 if the subject first switched to the risky option when the chance of left was less than or equal to  $\frac{1}{2}$ . We also control for demographic characteristics which may be correlated with attitudes towards or aptitude evaluating risk: age, gender, varsity athlete and whether the subject has taken a college-level statistics course.

[Table 2 about here.]

We estimate four probit models of Blue choice, reporting marginal estimates (Table 2). In model one, we include only indicator variables for our two treatments: Sequential Ascending and Descending. In models two and three, we add our risk-preference controls: HL Score in two; and Should Prefer Blue in three. In model four, in addition to a control for risk preferences, we include demographic controls.



We estimate robust treatment-effect coefficients across specifications. Compared to the Simultaneous baseline, the Sequential Ascending treatment decreases choice of the Blue option (*ex post* preferred only if subject survives to the last 3 rounds) by over 20 percentage points, an estimate weakly significant across specifications. The results are more striking when the highest payouts occur for short Payout Phases. The Sequential Descending frame increased Blue choice by over 30 percentage points in all specifications. While the coefficient for either risk-preference measure has the correct sign and is large and precisely estimated, demographic characteristics have little explanatory power.

## 4 EXPERIMENT 2

Experiment 1 provides evidence of a bias towards early period comparisons. In many real world applications, such as saving for retirement and choice of retirement assets, option endowment may have an effect as well. In Experiment 2, we investigate the relationship between a bias towards early period comparisons and an endowment bias.

### 4.1 Experiment 2 Design

We chose a design inspired by the binary decision of whether or not to annuitize retirement wealth.<sup>8</sup> This relatively straightforward decision highlights our tradeoff of interest between an option preferred in early-period comparisons and one preferred in expectation but *ex post* only if certain later states are reached. Should a retiree die early in her retirement, maintaining the lump sum delivers higher utility due to bequest value<sup>9</sup> and (possibly) higher consumption utility in those early years. Should she live many years, the annuity delivers higher overall utility as risk pooling allows higher later-year consumption possibilities. We present our design and results as the choice between a lump sum of assets and a corresponding annuity. In experiment sessions, however, we used a neutral context, avoiding language such as retirement, bequests and death. In Section 4.3, we discuss the relative merits of this choice.

In Experiment 2, the **Payout Phase** lasted from 1 to 15 rounds, with each event (retirement length) equilikely. Prior to the Payout Phase, a subject chose between two payout schedules: one based on the utility outcomes available to the annuitizing retiree, the other based on the utility profile of the same retiree optimally consuming out of a lump sum of

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<sup>8</sup>In the real world, the decision to annuitize retirement wealth is not a binary decision. A retiree may choose to annuitize only a fraction of her wealth, and has some flexibility in purchase timing.

<sup>9</sup>We do not necessarily assume altruism and, correspondingly, intentional bequests. Instead, we simply assume utility from leaving a bequest. This accommodates the specification of Dynan, Skinner and Zeldes (2002) in which agents self-insure against uninsured medical expenses but receive positive bequest utility if the risk is unrealized.

retirement assets. The lump-sum (**Orange** in the Instructions and Payout Table) option’s declining per-period payment mimics the declining consumption path generally optimal for consumption out of a stock of wealth. The annuity (**Blue**) delivered a constant payment path. The lump-sum option paid more in the event of a short retirement, less in a long retirement, and had a lower expected payout.<sup>10</sup>

Payout tables listed a payment corresponding to consumption utility (the **Type-I** payment) and a payment corresponding to bequest utility (the **Type-II** payment) for both options. For simplicity, we set the annuity’s Type-II payments to zero. A subject choosing the lump sum started with an account from which Type-I payments were made.<sup>11</sup> After a subject’s final round, she received a Type-II payment equal to a fraction of the amount remaining in her account after subtracting Type-I payments received.

We did not allow the subject choosing the lump-sum option to actually choose per-period consumption. First, simplifying the subject’s decision problem allows us to focus on the behavioral hypotheses of interest. Given the complexity of the problem and the limited time to optimize, a subject may make serious mistakes in her allocation. If this subject chose the annuity, we would not know whether it is because she preferred the annuity’s earning path to the lump sum’s or whether she miscalculated the latter’s utility possibilities. Second, the subject choosing the annuity would make many fewer decisions than the subject choosing the lump sum. Preferences over number of decision may then drive choices.

[Figure 3 about here.]

As a step towards establishing robustness, we used two sets of payouts: Payouts A and B. In Payouts A, the expected payoff from the lump-sum option is 88% of the annuity’s. The difference in the maximal payoffs was also rather large, with the lump sum’s maximal payment only 60% of the annuity’s. We made expected payoffs in Payouts B more equal (the lump-sum’s expected payout is 97% of the annuity’s) and the difference in maximal payoffs smaller (lump-sum’s maximal payoff is almost 70% of annuity’s). We show in Figure 3 the base payment schedule for each payout option under Payouts A and B. While requiring subjects to earn endowments in some sessions introduced some endowment heterogeneity, payout options were always a proportional rescaling of either Payouts A or B.

To test our hypotheses, we varied conditions not affecting choices (for either the retiree or experiment subject) under the expected-utility-maximization assumption. In one dimension, we varied the denomination of retirement assets. In **No Endowment** sessions, we did not endow subjects with a particular payout option—we simply asked them to choose between

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<sup>10</sup>In Appendix A, we provide details on how we arrived at payout values.

<sup>11</sup>In those sessions in which the default option is the Lump-Sum Payout option, we referred to the account as *their* account.

the Annuity and Lump-Sum options. In the other two sets of sessions, a subject was endowed with either the annuity payout path (**Annuity Endowment**) or the lump-sum payout path (**Lump-Sum Endowment**). We then asked the subject, in essence, whether she would like to trade for the other payout path.

Previous experiments have found that hypothetical endowments do not always induce an endowment effect. Just as the real-world retiree earns her retirement assets, our subjects earned Payout Phase assets. In the **Earnings Phase**, subjects completed a timed memory task.<sup>12</sup> We translated memory-task points into Payout-Phase assets,<sup>13</sup> reported as either a per-round annuity payment (akin to a yearly Social Security statement) or a stock of wealth (akin to a quarterly 401(k) statement). To further foster a sense of ownership, we split the Earnings Phase into two four-minute periods, reporting current and projected per-round payments (in the Annuity sessions) or account balances (in the Lump-Sum sessions) between earning periods. In addition to fostering a sense of ownership, these procedures may solidify Payout Phase expectations. Ericson and Fuster (Forthcoming) find evidence of the importance of expectations in determining reference points, as proposed by Kőszegi and Rabin (2006).

After the Earnings Phase, we offered subjects the alternate payout option without further favoring the endowed option. While subjects earning  $n$  points in either earned-endowment treatment received the same payout table, these tables varied according to Earnings-Phase points.<sup>14</sup>

In the second treatment dimension, we varied the determination of the number of Payout-Phase rounds (i.e., retirement length) in the same manner as Experiment 1. In **Simultaneous** sessions, a single draw from a bag containing 15 lettered chips ( $\{A, B, \dots, O\}$ ) determined retirement length.<sup>15</sup> In **Sequential I** sessions, sequential survival determined retirement length. In each Payout-Phase round, the subject drew a marble *without replacement* from a bag of marbles to determine survival into the next round. The bag contained 14 green and 1 red marble in round 1. If she drew a green marble in round  $t$ , she received

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<sup>12</sup>A subject's monitor displayed five letters. She clicked okay after reviewing the letters and was presented with three letters. The subject then indicated whether all of the three letters were in the original five. Points earned equalled the number of correct responses minus the number of incorrect responses.

<sup>13</sup>A concave function from points into payouts mitigated inter-subject endowment variation. We attempted to control for inter-treatment variation by parameterizing so that the expected earned endowment equaled that of the No Endowment sessions. Prior to the Earnings Phase, subjects received a table indicating, for a range of points earned, either the per-round payment (Annuity Endowment sessions) or account balance (Lump-Sum Endowment sessions).

<sup>14</sup>A subject earning an \$18 account balance or the equivalent per-round payment received the exact same payout table as a No Endowment subject.

<sup>15</sup>Event A corresponded to a one-round payout phase. By listing outcomes associated with an early death first, we potentially introduce a bias. This bias ought to work in the same direction as the ordering bias, and thus works against finding a difference between treatments.

the round  $t + 1$  consumption (Type-I) payment and then drew another marble to determine survival into round  $t + 2$ . If she drew a red marble in round  $t$ , her Payout Phase ended and she collected no more per-round payments, but she did receive the round  $t$  bequest (Type-II) payment if she had chosen the lump-sum option.

[Table 3 about here.]

As in Experiment 1, there was no difference in event probabilities across treatments. For example, both Event G in the Simultaneous treatment and surviving exactly 7 periods in a Sequential treatment occurred with probability  $\frac{1}{15}$  and had the same subject payoff (conditional on having the same level of retirement assets). In Sequential I sessions, we presented the probability of surviving to the next period conditional on survival to the current period, but not the unconditional probability of surviving a given number of periods. While a subject in a Simultaneous session knew that the probability of Event G was  $\frac{1}{15}$ , a subject in a Sequential I session might not have known that she had this chance of surviving exactly 7 periods. While a lower rate of annuitization in Sequential I may be due to early events having more salience, it may also be due to an inability to calculate unconditional probabilities. To rule out this possibility, **Sequential II** sessions were identical to Sequential I sessions except that we provided both conditional and unconditional probabilities. In all Sequential II sessions, we did not explicitly endow subjects with either option, and thus subjects did not need to earn endowments.<sup>16</sup> In Table 3, we depict sample Payout Tables.

[Figure 4 about here.]

The main procedural difference between treatments was the need for subjects in the earned-endowment sessions to earn payout-phase assets. We present in Figure 4 a summary and timeline of session events.

After all subjects made the choice of payout option, each subject completed a standard Holt-Laury risk-aversion assessment (Holt and Laury 2002).<sup>17</sup> Subjects also completed a demographic questionnaire, and answered incentivized review questions before both the payout-option choice and the risk-aversion assessment. Subjects were paid for the outcome of their Payout Phase, the risk-aversion assessment, and the review questions.

[Table 4 about here.]

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<sup>16</sup>Nonetheless, we administered the timed memory task to these subjects after they had made their choices. We used their performance on the task as a proxy for cognitive ability.

<sup>17</sup>“Left” paid \$2 for the safe choice and \$3.85 for the risky choice. “Right” paid \$1.60 for the safe choice and \$0.10 for the risky choice.

We conducted sessions at George Mason University’s ICES laboratory. Approximately 14 subjects participated in a session, without duplication. Table 4 details the treatments and subject participation in Experiment 2. Participants were George Mason University students. Parts of the experiment (the Earnings Phase and the quizzes) were programmed and conducted with z-Tree (Fischbacher 2007). No Endowment sessions lasted approximately 60–75 minutes, and earned-endowment sessions approximately 75–90 minutes. The average payoff was \$23.45, including a \$7 show-up fee.

#### 4.2 Experiment 2 Results

[Figure 5 about here.]

Figure 5 shows for each treatment the proportion of subjects choosing the annuity payout option. We make a few observations. First, in all treatments, a significant proportion of subjects chose the annuity option. While the annuitization rate in all treatments is greater than in the real world, our interest is in the treatment effects rather than absolute levels. Second, the ordering of risks seems to matter in the hypothesized direction, although the effect seems more pronounced in Experiment 1. Regardless of endowment, the proportion choosing the annuity is greater in the Simultaneous treatment than in the Sequential treatment. Third, Figure 5 suggests an endowment effect, as the proportion of subjects choosing the annuity payout option is smallest when endowed with the lump-sum payout option.

[Table 5 about here.]

We present in Table 5 the proportion of subjects choosing the annuity payout option by treatment. We also note for each null hypothesis the  $p$ -value of the  $\chi^2$  test. We now formally test our hypotheses.

**Result 2.** *When not explicitly endowed with a payout option, the proportion of subjects choosing the annuity payout option is greater in the Simultaneous treatment than in the Sequential treatment. The same difference is not significant when subjects are explicitly endowed with a payout option.*

**Support:** We present in the final column of Table 5 the  $p$ -values for the  $\chi^2$  test of the null hypotheses of equal proportions.

Result 2 provides partial support for the hypothesis that the temporal ordering of risks affecting decision making. In the No Endowment treatment, we can reject the hypotheses of annuity-choice equality at the 10% level of significance when comparing Simultaneous to Sequential I, and at the 5% level comparing it to Sequential II. Furthermore, we cannot reject the hypothesis of Sequential I versus II annuity-choice equality. This suggests that

the decreased likelihood of choosing the option doing better *ex post* only if later periods are reached is not due to problems with unconditional probabilities.<sup>18</sup> While annuity-choice proportion is greater in the Simultaneous frames than the Sequential in the earned-endowment treatments, we cannot reject the null of equality of proportions.

**Result 3.** *The proportion of subjects choosing the annuity payout option when endowed with the annuity payout option is greater than the proportion when endowed with the lump-sum option regardless of probability frame (Sequential or Simultaneous).*

**Support:** We present in the final column of Table 5 the  $p$ -values for the  $\chi^2$  test of the null hypotheses of equal proportions.

Result 3 suggests a fairly strong endowment effect in decisions akin to asset allocation in retirement. When there is no temporal ordering of retirement risks (Simultaneous), we reject the null hypotheses of proportion equality at the 5% level of significance, and reject the corresponding null hypotheses at the 10% level of significance when retirement risks are temporally ordered (Sequential I).

While we randomly assigned treatments to experiment sessions, subject characteristics could very well vary across sessions. These inter-treatment differences in subject characteristics could potentially exaggerate or understate differences attributable to treatment effects. Importantly, we ought to control for risk preferences. We based payout tables on the *utility* paths offered by the options for a representative retiree in an annuity-choice context. However, while the expected-utility-maximizing retiree cares only about expected utilities and not their variances, the subject cares about payment variance unless risk neutral. Therefore inter-treatment risk-preference differences could drive some of the Table 5 treatment differences.

As in Experiment 1, we elicited an ordinal measure of risk preferences (Holt and Laury 2002). We define the HL Score as the decision number the subject first chose the risky option, with higher scores corresponding to higher levels of risk aversion. We treat as missing the nearly 20% of subjects who switched back and forth multiple times between the riskier and safer option. As we presented subjects one of two sets of payout schedules, we do not use the Scores directly in our regression analysis. Instead, as in Experiment 1, we construct an indicator equal to one if the expected-utility-maximizing subject would have selected the annuity given her HL Score.

[Table 6 about here.]

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<sup>18</sup>Pooling these two treatments and comparing with Simultaneous, we reject the hypothesis of annuity-choice equality ( $p = 0.016$ ).

Just as inter-treatment heterogeneity in risk preferences may account for treatment differences, so may differences in other dimensions. In Table 6, we present subject characteristics across treatment groups. In addition to inter-treatment differences in risk preferences, we present other subject characteristics—such as age, education background, cognitive abilities, and U.S. nativity—plausibly affecting the valuation of the annuity relative to the lump sum. For example, a subject’s math background may provide information about ability to compute expected values, and ability to comprehend experiment instructions may be correlated with U.S. nativity.

We make a few observations. First, the proportion of subjects with HL Scores in the range that should prefer the annuity varies substantially across treatments. This proportion in the Lump-Sum treatment (in both Sequential and Simultaneous frames) is greater than in either the Annuity or the No Endowment treatment. This difference likely understates the effect of the Lump-Sum treatment on annuity choice. Likewise, in both the Annuity and Lump-Sum frames, the proportion of subjects in the prefer-annuity range is greater in Sequential sessions than in Simultaneous sessions. Again, this difference likely understates the effect of the Sequential treatment on annuity choice in those frames.

Second, we observe inter-treatment differences in memory-task points earned. Points in Sequential sessions were significantly lower than in Simultaneous sessions, and significantly lower in Lump-Sum than in Annuity sessions. Inter-treatment point differences may be important if performance in the memory task measures a dimension of cognitive abilities affecting annuity choice.

Third, about 15% of subjects, concentrated in the No Endowment sessions, did not answer experiment-instruction review questions. We did not administer these questions in the first sessions we ran. This inter-treatment difference could plausibly contribute to systematic differences across treatments in annuity choice.

Fourth, across treatments, subjects vary in gender, U.S. nativity, and educational background. However, there does not appear to be a clear pattern across treatments that would systematically bias the measured treatment effect in one direction or the other.

Fifth, we code as missing HL Scores for a reasonably large proportion of subjects. This proportion varies across treatments. Although we hold no priors that suggest subjects with missing scores would systematically behave differently from their counterparts with non-missing scores, the possibility exists.

[Figure 6 about here.]

Finally, most subjects the No Endowment sessions did not earn points. This was by construction since subjects in these sessions received, rather than earned, their endowments.

Given the wide distribution of points earned and the possibility that the variable absorbs important inter-treatment variation in cognitive differences (not absorbed by the other demographic variables), we impute for missing points earned.<sup>19</sup> We use a multiple imputation hot-deck method.<sup>20</sup> Hot-deck imputation preserves the distribution of points earned, while multiple imputations introduce variability, generating larger (better) standard error estimates than a single imputation.<sup>21</sup> In Figure 6, we show the cumulative distributions of imputed and actual points earned.

We now turn to regression evidence of treatment effects, controlling for observable inter-treatment variability. In all specifications, we control for treatment with indicators for Sequential, No Endowment, and Annuity Endowment sessions. The omitted categories are Simultaneous and Lump-Sum.

To account for inter-treatment differences in observable characteristics and experiment setup, we incrementally include control variables in a probit model. In all, we account for risk preferences (proxied by the indicator for whether the subject’s risk-aversion level is consistent with choosing the annuity), cognitive differences (proxied by points earned), a parsimonious set of demographic controls given the relatively small sample size, an indicator for whether the subject took the review questions, and relative payouts (by including an indicator if she faced Payouts B). The full sample includes 373 observations. Our sample size decreases when we add additional controls due to missing data.

[Table 7 about here.]

In Table 7, we report the coefficients we estimate for six regression models, reporting marginal estimates. In model one, our base estimate of treatment effects, we estimate a probit equation with only treatment dummies. In the second model, we include the risk-preference proxy whose distribution may lead to model one understating treatment effects. In model three, we add the demographic and experimental setup controls which may affect annuity choice: age and indicators for U.S. nativity, taken calculus, review questions, and Payouts B. In model four, we account for differences in points earned (using imputed values for the No Endowment sessions), which potentially proxies for cognitive differences.<sup>22</sup> We include the points earned and its squared term. In our fifth model, we account for possible

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<sup>19</sup>In later No Endowment sessions (60 subjects), we administered an incentivized memory-task phase after the risk-aversion assessment.

<sup>20</sup>It is reasonable to assume data are missing at random. There is no systematic organization of treatments in terms of session date or time, and recruitment emails do not differ across treatments.

<sup>21</sup>The standard errors from multiple imputations are constructed using the between- and within-imputation variation (Rubin 1987). We generate 10 complete imputed data sets due to the high missing rate. Due to relatively small cell sizes, we limit predictors for imputing to two: gender and U.S. nativity.

<sup>22</sup>We were unable to impute Earned Points for all subjects in the No Endowment sessions due to missing demographic values in the donor (predictor) pool.



selection by including subjects with missing HL scores. An indicator variable, set to 1, identifies missing HL scores. In our final model, we focus on earned-endowment sessions only.

There are four important points to take away from the results. First, subjects in the Sequential treatment are at least 10 percentage points less likely to choose the annuity option than subjects in Simultaneous treatment. This result is robust across all six specifications. Comparing estimates for models (1) and (2), the inclusion of the risk-preference proxy substantially increases the estimated effect of Sequential frame on annuity choice: from -11.3 percentage points to -15.3 percentage points. The effect remains with the inclusion of demographic and cognitive controls. Not surprisingly, accounting for missing HL scores reduced the magnitude of the estimated coefficient on the treatment variables. To the extent that subjects who had difficulty comprehending the HL risk assessment also had difficulty comprehending payout-option choice, we would not expect to observe a systematic pattern in annuity choice. Although our results are robust to the inclusion of subjects making inconsistent HL choices, our preferred specification would exclude those cases.

Second, subjects in both the No Endowment and Annuity Treatments are more likely to choose the annuity option than subjects in the Lump-Sum treatment. The estimated effect is large, precisely estimated and robust across all six specifications. The annuity-choice probability difference between the Annuity and Lump-Sum treatments is generally larger than the corresponding difference between the No Endowment and Lump-Sum treatments. In the Annuity vs. Lump Sum comparison, accounting for risk preferences increases the estimated effect of the endowment bias whereas accounting for selection absorbs some of the difference. In the No Endowment vs. Lump Sum comparison, the effect strengthens with additional controls.

Third, recall that based on the  $\chi^2$  test, we could not reject the hypothesis of equality of annuity-choice proportions between Sequential and Simultaneous frames when subjects earned a particular payout option. This is not the case after accounting for risk preferences. Model six includes only subjects in earned-endowment sessions. Our specification includes only those who had valid HL scores for the reasons discussed above. We estimate that those receiving the Sequential treatment were nearly 19 percentage points less likely to choose the annuity option than subjects in the Simultaneous treatment. Furthermore, when endowments were denominated as an annuity, subjects were 16 percentage points more likely to choose the annuity option than when denominated as a Lump-Sum.<sup>23</sup>

Finally, our specification-check variables assure us that neither maximal payoffs or the inability to compute unconditional probabilities drive our results. We varied the expected

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<sup>23</sup>This specification excludes cases with missing HL Scores. Treatment effects are smaller (13% and 13%) and less precisely estimated when we include cases with missing HL scores.

total payment and maximal payoff between the annuity and lump-sum options (Payouts A vs. B) in case these comparisons contributed to treatment differences. We also varied whether we included unconditional probabilities in the payout table in Sequential treatments (Sequential I vs. II), in case choices were largely driven by mistakes computing survival probabilities. Neither appear to be significant in our “annuity-choice” decision.

### 4.3 *Experiment 2 Discussion*

The perfect-world case for annuitizing retirement assets is strong. Since Yaari’s (1965) seminal work, a large literature has attempted to explain why observed annuitization rates are lower than generally predicted under standard neoclassical models. Even accounting for factors such as high loads (Mitchell, Poterba and Warshawsky 1999), pre-existing annuities, bequest motives (Friedman and Warshawsky 1990), and precautionary saving for uninsured late-life medical expenses (Sinclair and Smetters 2004, Turra and Mitchell 2004), these models are generally unable to explain fully the gap (Davidoff et al. 2005). A growing body of evidence suggests that behavioral biases influence financial decision making. These biases plausibly affect the annuity decision (Hu and Scott 2007), and thus might well be important components of the remaining annuity gap (Brown 2007, p. 3). Recent survey (Brown, Kling, Mullainathan and Wrobel 2008a, Brown, Kling, Mullainathan, Wiens and Wrobel 2008b) and experimental (Agnew, Anderson, Gerlach and Szykman 2008) studies find that framing has potentially important effects on annuity valuation and may explain, to some extent, why the market for private annuities is much thinner than expected given standard assumptions. We contribute to this line of inquiry, focusing on the oft-expressed yet previously untested concern of dying shortly after annuity purchase. Our hypotheses of an undue weight to earlier comparisons and a loss-aversion induced endowment effect map neatly into this concern.

We find support for both of these hypotheses in a laboratory setting capturing many of the salient aspects of the annuity decision. Our preferred explanation of the mechanism by which our sequential treatment reduced annuity choice is that the ordering of states induced subjects to effectively overweight early states. We do admit alternative explanations. For example, a person plausibly experiences disutility as she waits to find out if her decision paid off. In the simultaneous sessions, we resolved uncertainty immediately. In the sequential sessions, the annuity choice meant approximately 8 rounds of being at risk of having made the wrong choice.<sup>24</sup> Of course, the real-world annuitant faces the same situation.

We take care in extrapolating our results to the real-world retiree’s annuity decision, especially in considering the relative magnitude of these effects. While the laboratory allows us to cleanly investigate these biases in a context capturing key aspects of the decision, the

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<sup>24</sup>We thank Casey Rothschild for this insight.

laboratory differs from the wild in a number of potentially important ways.

First, we believe ourselves on firm ground describing the retirement-asset allocation decision as context rich (as we would any decision in which mortality plays such a large part). In the laboratory, we used a neutral context, avoiding language such as retirement, bequests and death. We did so because we believe context so important. In order to focus on the hypothesized biases of interest, we felt it necessary to control for additional biases that context may have introduced. For example, the neutral context allows us to control the value of the lump-sum at death. Identified as a bequest, we lose control of this valuation and must worry whether the influence of pre-existing notions and norms vary across treatments. Whether context-dependent biases mitigate or augment the identified biases is left for future research.

Second, we use university students in a relatively low-stakes environment to proxy for the very high-stakes decision made by retirees. One can naturally ask whether either age or increased stakes might overcome the identified biases, and we plan to do so in future work. We note that while those making the annuity decision are older, it is not clear why older subjects would be less prone to these biases. The annuity decision is largely one shot with little to no feedback, conditions conducive to the continued existence of biases (Thaler and Sunstein 2008). Furthermore, while an increase in stakes most surely decreases the likelihood and extent of less-than-perfectly-rational behavior, there is considerable evidence that it does not extinguish it.

The control offered by the laboratory environment does offer benefits. For example, it would be difficult to disentangle mistaken survival-probability beliefs from an overweighting of early events despite correct beliefs. In fact, recent evidence suggests that at least on average, retirees hold generally correct beliefs. Hurd and McGarry (2002) and Smith, Taylor and Sloan (2001) find subjective survival probabilities reasonably close, on average, to life tables, and Gan, Gong, Hurd and McFadden (2004) find even more optimistic beliefs. Our results suggest a mechanism by which the perceived unattractiveness of the annuity persists despite accurate or even optimistic survival beliefs.

Our results suggest a near-future bias, plausibly operating *in addition to* non-exponential discounting of future utility. This is important as the effect of hyperbolic discounting on annuity demand is ambiguous. The sophisticated hyperbolic discounter, realizing she will be present biased in future periods, receives great benefits from the annuity's commitment value (Laibson 1997, Diamond and Köszegi 2003). The naïve hyperbolic discounter, who believes her present bias only temporary, does benefit relative to the exponential discounter from bringing utility to the current period. However, the difference between the annuity valuations of the naïve hyperbolic discounter and her exponential counterpart is likely small. First, the naïvely present-biased retiree looks to bump up consumption today as opposed to

today and tomorrow. This is consistent with reducing the amount annuitized (to increase current-period consumption) as opposed not annuitizing.<sup>25</sup> Second, curvature of the utility function moderates overall utility gains from reallocating consumption to the current period. Our calculations suggest that at plausible (given the context) levels of risk aversion, the optimal first-period consumption for the naïve hyperbolic discounters does not greatly differ from her exponential counterpart, and therefore their relative valuations of even full annuitization do not greatly differ.<sup>26</sup> Taken together, we believe that the assumption of naïve hyperbolic discounting by an otherwise rational retiree can by itself only explain a very moderate reduction in the amount annuitized at retirement.

Given the annuity paradox and the impending bulge of largely non-annuitized retirees, a number of policies aimed at increasing annuitization rates have been proposed, with some receiving careful analysis (Gentry and Rothschild 2010). We believe our results have implications in this policy arena. Our finding of an endowment bias (stemming from loss aversion) in payout options suggests that changing the denomination of 401(k) assets from a lump sum to a claim on a per-period payment might increase annuitization rates. This idea has been proposed in policy circles and would be a relatively inexpensive and straightforward option to implement (Iwry and Turner 2009).

The policy implications stemming from the risk-ordering bias are not obvious as the risk of an early death naturally precedes the risk of outliving one's assets. However, whereas Agnew et al. (2008) show that explicitly highlighting the annuity's downsides in the event of an early death reduced annuity demand, our results suggest that such a focus may not require priming. We share Brown et al.'s (2008a) belief that a more thorough understanding of the frame through which annuities are sold is needed. We also wonder whether making a large annuity decision *at retirement* might exacerbate the fear of losing one's principal. If so, the retiree may find more palatable longevity (i.e., delayed payout) annuities (Milevsky 2005, Scott 2008).

## 5 DISCUSSION

In many interesting economic environments, a decision maker must choose between an option with higher earlier payoffs and one with higher payoffs in later states not reached with

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<sup>25</sup>Naïve hyperbolic discounting might also lead to a (permanent) delay in paying the transactions costs of annuitizing. This would further strengthen the importance of any endowment effect.

<sup>26</sup>For example, consider the environment faced by our Appendix A representative retiree with discounting but without a bequest motive. Ms. Exponential discounts future utility at  $0.944^{\tau-t}$  while Mr. Quasi-Hyperbolic naïvely discounts at  $(.7)0.957^{\tau-t}$  (Angeletos, Laibson, Repetto, Tobacman and Weinberg 2001). We calculate the increase wealth necessary to make utility from consumption out of a wealth stock equal to the utility from the actuarially fair annuity. With  $\rho = 2$ , the exponential discounters needs a 74% increase while the naïve quasi-hyperbolic discounters needs a 70% increase in assets.

certainty. Either a high discount rate or preferences consistent with hyperbolic discounting will surely favor high early rewards. In our experiments, we remove discounting, and relative to a frame in which we remove the temporal ordering of payoffs, still find a preference for higher earlier payoffs. This finding is consistent with some subjects approaching the decision in an “I’ll cross that bridge *if* I get to it” framework. Such an approach would lead to excessive focus on early period comparisons to the detriment of later period comparisons and comparisons of expected outcomes. We also find in Experiment 2 a significant endowment effect operating independently of the still present risk-ordering bias. While endowment effects have been well documented (Kahneman, Knetsch and Thaler 2004), to the best of our knowledge this is the first experimental evidence using lotteries with such large state spaces.

We note the overweighting of earlier comparisons in our experiment is not consistent with other results suggesting a difference between objective probabilities and decision weights. First, it does not appear that an inability to translate conditional probabilities into outcome likelihoods drove our results. Experiment-1 subjects had access to both, and Experiment-2 choices did not depend on explicitly informing subjects that events were equilikely. Further, experimental evidence (Bar-Hillel 1973) suggests a tendency to overestimate conjunctive events (e.g., the likelihood of drawing  $n$  consecutive green marbles) and underestimate disjunctive events (e.g., the likelihood of drawing a red marble in the first  $n$  draws). This bias works in the opposite direction. Second, our results are not predicted from the standard assumptions about the transformation of objective probabilities into decision weights. In both of our experiments, outcomes are equilikely, thus any translation from event probability to decision weight ought to be the same for all outcomes. Further our results are not consistent with traditional transformations of the *cumulative* probability functions as suggested by rank-dependent utility (e.g., Quiggin (1982)) or cumulative prospect theory (Tversky and Kahneman 1992). Such theories assume an over-weighting of “extreme” events. While it is not clear whether subjects viewed lasting all or only 1 Payout Phase rounds as more extreme, a more fundamental problem is that such a transformation would predict no difference between choices in Sequential Ascending and Descending treatments. We find a large a statistically significant difference.

The risk-ordering bias we identify is related to, but distinct from, myopic loss aversion (MLA) (Benartzi and Thaler 1995) in which a decision maker focuses on gambles sequentially as opposed to holistically. Consider a gamble with a positive expected value despite the possibility a loss. Rejecting each instance of a sequence of individually presented gambles, but accepting the *collected* sequence, is consistent with MLA. A number of studies have found an increase in risk aversion when gamble outcomes are revealed sequentially (Gneezy

and Potters 1997, Thaler, Tversky, Kahneman and Schwartz 1997, Haigh and List 2005).<sup>27</sup> There are a number of clear differences, however, between the environment considered in these previous studies and ours. Notably, in the MLA studies per-period outcomes are stochastic and the number of periods known, whereas in our environment the per-period outcomes are known while the number of periods is stochastic.

We also point out that overweighting of earlier-in-time comparisons is closely related to discounting, such as hyperbolic discounting, that overweights current-period *utility* relative to classical models with exponentially discounted utility. (See Frederick, Loewenstein and O'Donoghue (2002) for an excellent overview.) In fact, Sheshinski (2007) notes the near equivalence of hyperbolic discounting and pessimistic survival beliefs. Empirically, it would be difficult to disentangle placing undue salience on early period utility comparisons from under-weighting the *utility* of later periods. This is less of an issue in the laboratory. Our experimental design allows us to focus on the former as subject payment occurs at the same time regardless of Payout Phase length.

We view our results as complementing time-inconsistent discounting in explaining present bias, joining other studies in taking a more nuanced and richer view. In particular, we join others in suggesting that some present bias may be due to biases in terms of what we focus on rather than discounting. While we speculate that near-in-time comparisons may receive undue weight, Kőszegi and Szeidl (2011) develop a model in which people focus on attributes in which options differ more, and show that this can lead to present bias. Karlan, McConnell, Mullainathan and Zinman (2010) find support for the hypothesis that some observed under-saving is due to overlooking future lumpy expenditures. Our study also complements those looking at the interplay of discounting and uncertainty. Andreoni and Sprenger (2010) find evidence of a disproportionate preference for certainty regardless of whether now or later,<sup>28</sup> suggesting a leading role for certainty in explaining present bias given the future's inherent uncertainty. Our results complement these findings, suggesting that earlier (and more certain) comparisons may receive undue weight relative to those later and less certain.

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<sup>27</sup>Langer and Weber (2005) show that whether myopia decreases the attractiveness of the sequence depends on the risk profile.

<sup>28</sup>See, for example, Neilson (1992) and Schmidt (1998).

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## A OPTIMAL CONSUMPTION

### A.1 No Endowment Treatments

We assume no inflation and set the risk-free interest rate equal to zero. We further assume that a representative retiree enters retirement with a stock of wealth  $W$ , and can survive from 1 to 15 periods with each retirement length equilikely. Under these assumptions, and letting  $q_t$  be the probability of surviving at least to period  $t$ , the actuarially fair annuitization of  $W$  pays

$$y = \frac{W}{\sum_{t=1}^{15} q_t} = \frac{W}{8}$$

each period starting in the first period (Creighton and Piggott 2006). To simplify matters, we assume that the retiree consumes her entire annuity payment in each period ( $c_t = y$ ).<sup>29</sup>

We assume that the retiree who does not annuitize retirement wealth optimally consumes from her stock of wealth  $W$ . The solution to this optimization problem will depend on her utility function as well as survival probabilities. We assume constant relative risk aversion (CRRA), with  $u_t(c_t) = \frac{c_t^{1-\rho}}{1-\rho}$  the per-period utility function of our representative retiree with  $\rho$  the coefficient of relative risk aversion. We implement a rational attraction to maintaining a stock of wealth (i.e., make reasonable a “hit by a bus” concern) by assuming a bequest motive.<sup>30</sup> We assume that the value of a bequest of wealth is  $v(w_t) = \beta \frac{w_t^{1-\rho}}{1-\rho}$ , where  $w_t$  is wealth remaining as of time  $t$ . The retiree thus solves the following:

$$\max_{c_t} E(U) = u(c_1) + \sum_{t=2}^{15} \left[ q_t \cdot u(c_t) + \left(1 - \frac{q_{t+1}}{q_t}\right) v(w_t) \right] \quad (4)$$

subject to:

$$w_t = W - \sum_{\tau=1}^t c_\tau;$$

$$c_{15} = w_{15}; \text{ and}$$

$$q_{16} = 0;$$

where

$$u(c_i) = \frac{c_i^{1-\rho}}{1-\rho} = \frac{v(\cdot)}{\beta}.$$

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<sup>29</sup>Under our assumptions, reallocating consumption between retirement periods subsequent to annuitization decreases utility. However, a retiree with a bequest motive might find it optimal to not consume her entire annuity payment.

<sup>30</sup>Our specification follows the “warm glow” model of Andreoni (1989) rather than an altruistic model that incorporates children’s utility, based on evidence in Kopczuk and Lupton (2007).

We consider  $\rho = \frac{1}{3}$ ,<sup>31</sup>  $\beta = 0.865$ ,<sup>32</sup> and  $W = 1000$ . We solve for optimal consumption, and scale utility by dividing by 20.

[Figure 7 about here.]

In Figure 7 we depict the cumulative utility for the retiree who annuitizes and the retiree who consumes out of the lump sum of assets. We base our Payouts A on these utility paths, paying \$1 per util with the following caveats. In our **No Endowment** sessions, we desired to present all payoffs in multiples of \$0.05. We also desired to translate the payoffs, particularly those arising from the lump-sum option, into rules easily explainable to subjects. We therefore offered subjects an annuity payment of \$2.00 per period as opposed to \$1.88. We set the lump sum account value equal to \$18.00, with round 1 Type-I (consumption) payment equal to \$2.25 in the first round. The “consumption” payment decreases by \$0.15 with each passing round. Type-I payments are subtracted from the account balance, and the subject choosing the lump-sum payout option receives a Type-II (bequest) payment equal to 30% of the amount remaining in the account as of the final round.

As a first step toward checking the robustness of our results to changes in the relative values of our subject payments, we slightly alter Payout A. We decrease the Type-I payment subject to annuitization to \$1.75 (akin to moving the annuity away from actuarially fair). Further, we decrease the annuity weight by decreases the fraction of the account balance received by the subject from 30% to 20% if the subject choose the lump sum. We depict the changes in Figure 3.

### A.2 Endowment Treatments

In the treatments in which a subject must earn her retirement endowment, we translate points earned in the Earnings Phase into either per-round payments or account balances in the Payout Phase.

We start by noting the following about the payoffs in the No Endowment treatments. First, for both Payouts A and B, we calculate the amount by which we need to multiply the per-round annuity payment to recover the round-one lump-sum Type-I payment:  $\alpha_A == \frac{2.25}{2.00}$ ,

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<sup>31</sup>We choose this level of risk aversion to match median and modal levels of risk aversion exhibited by experiment subjects. Holt and Laury (2002) find a median level of risk aversion in the range of  $0.15 < \rho < 0.41$  for low-stakes gambles (safe choice pays approximately \$1.80) and  $0.41 < \rho < 0.68$  for high-stakes gambles (safe choice pays approximately \$36.00).

<sup>32</sup>We initially choose this high weight on bequest motives for a few reasons. First, we are interested in decisions where both annuities and the lump-sum distribution are attractive. With  $\rho = \frac{1}{3}$ , without regard for a loss of assets due to death (i.e.,  $\beta = 0$ ), the expected utility of the optimal consumption of the lump-sum is only 81% of the actuarially fair annuity’s. Furthermore, we desire that both options deliver the same utility should the subject live for eight periods. This is the case with  $\beta = 0.865$ , and expected utility from the optimal consumption of the lump sum is 95% of the the actuarially fair annuity’s.

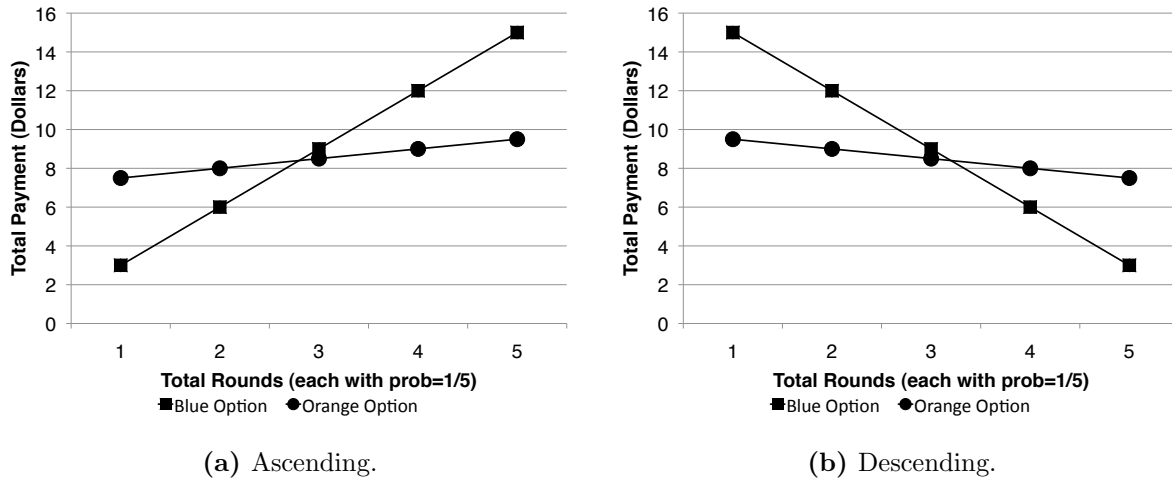
and  $\alpha_B = \frac{2.25}{1.75}$ . Second, letting  $x_1$  be the round-one Type-I payment subsequent to choosing the lump sum, payments decrease each round by  $\delta = \frac{x_1}{15}$ . We use these relationships between the Annuity and Lump-Sum payouts in the No Endowment treatments in deriving payments for the endowment treatments.

We assume a linear relationship between points earned in the Earnings Phase and the stock of wealth brought into retirement by our representative retiree,  $W_i = points \times \gamma$ . An actuarially fair annuity pays  $y_i = \frac{W_i}{8}$ , yielding scaled per-period utility (and Type-I payment)  $u_1 = .05 \frac{y_i^{1-1/3}}{1-1/3}$ . The round-one Type-I payment subsequent to choosing the lump sum is  $x_1 = u_1 * \alpha$ , subsequent Type-I payments declining by  $\frac{x_1}{15}$  each round. The subject's account balance is the summation of Type-I payments over all rounds.

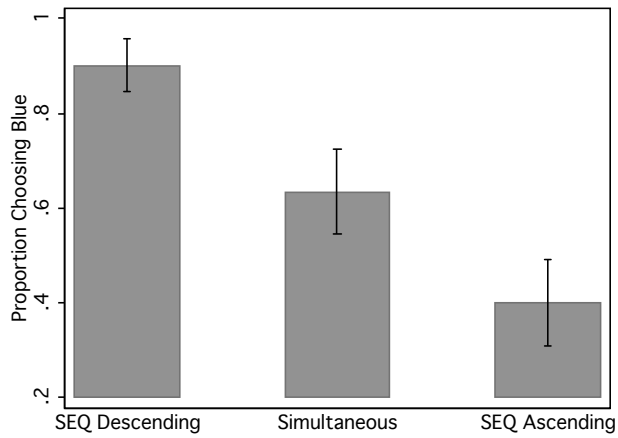
We wanted most earned-endowment subjects to have payouts in line with those faced by No Endowment subjects. We projected that the median subject would earn 90 points in the Earnings Phase. For example, setting  $\gamma = 10$  results in  $W = 900$ , with an actuarially fair annuity paying 112.5 and scaled utility equal to 1.75, exactly the per-round payout for Payout B.

In the experiment, 40% of subjects earned between 77 and 104 points, thus placing them within  $\pm 10\%$  of the No Endowment payouts, and 56% earned between 71 and 111 points, placing them with  $\pm 15\%$  of the No Endowment payouts.

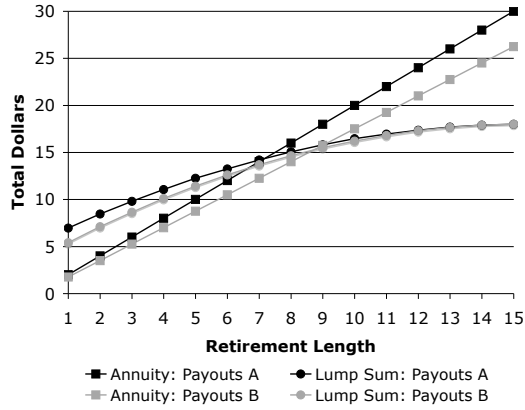
## B FIGURES



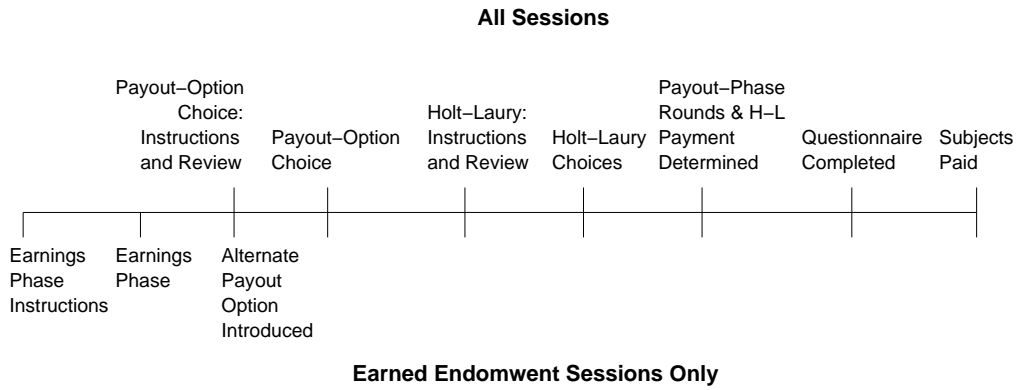
**Figure 1:** Payments in Experiment 1 Sequential Treatments.



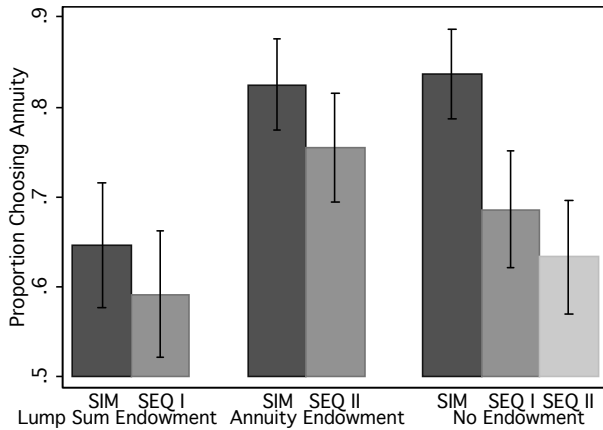
**Figure 2:** Experiment 1: Proportion of subjects choosing blue, by treatment, with standard error bars.



**Figure 3:** Experiment 2: Payment to subject surviving exactly a given number of rounds under Payouts A and B in sessions where endowments are not earned.

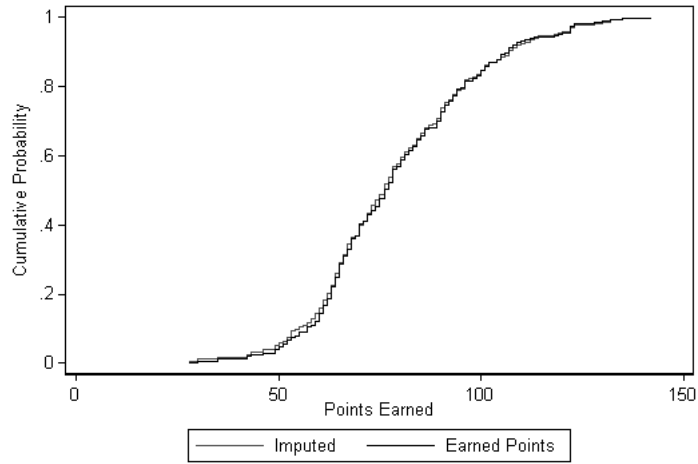


**Figure 4:** Summary and timeline of Experiment-2 session events. We list events occurring in all sessions above the timeline, and those occurring only in Earned-Endowment sessions below.

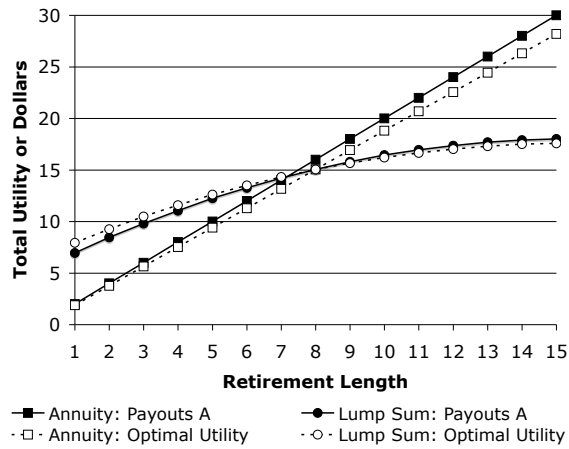


**Figure 5:** Experiment 2: Proportion of subjects choosing annuity, by treatment, with standard error bars.





**Figure 6:** CDFs of Earned and Imputed Memory Task Points.



**Figure 7:** Representative retiree utility vs. Subject Payouts A.

C TABLES

	<b>Sequential Descending</b>	<b>Simultaneous</b>	<b>Sequential Ascending</b>	<b>Total</b>
Mean Risk (HL) Score	6.20	5.67	5.77	5.88
(std dev)	(1.35)	(1.18)	(1.07)	(1.22)
Prop in Prefer Blue Range	0.30	0.37	0.33	0.33
Male	0.60	0.47	0.63	0.57
Born in US	0.80	0.80	0.87	0.82
White	0.73	0.60	0.57	0.63
Age	19.23	19.53	20.07	19.61
(std dev)	(1.22)	(1.22)	(1.34)	(1.30)
Is Athlete	0.23	0.20	0.20	0.21
Ever taken Calculus	0.87	0.93	0.87	0.89
Ever taken Statistics	0.33	0.47	0.43	0.41
N	30	30	30	90

**Table 1:** Experiment 1 subject characteristics across treatments.

VARIABLES	(1) isBlue	(2) isBlue	(3) isBlue	(4) isBlue
<b>Sequential Ascending</b>	-0.220* (0.124)	-0.221* (0.127)	-0.215* (0.128)	-0.227* (0.130)
<b>Sequential Descending</b>	0.304*** (0.106)	0.334*** (0.102)	0.424*** (0.087)	0.361*** (0.099)
<b>Should Prefer Blue</b>		0.276*** (0.094)		0.296*** (0.090)
<b>Age</b>				0.020 (0.044)
<b>Male</b>				-0.074 (0.108)
<b>White</b>				-0.082 (0.107)
<b>Varsity Athlete</b>				-0.062 (0.131)
<b>Taken Statistics</b>				-0.106 (0.109)
<b>HL Risk Score</b>			-0.190*** (0.050)	
Observations	90	90	90	90
$\chi^2$	15.46	22.38	26.71	27.23

*Notes:* Significant at: \* 10-percent level; \*\* 5-percent level; \*\*\* 1-percent level. Robust standard errors in parentheses. Omitted group is Simultaneous treatment. HL Scores refer to Holt-Laury risk assessment scores, and Should Prefer Blue is an indicator variable equal to 1 if subject ought to choose Blue given HL Score under assumption of CRRA utility.

**Table 2:** Experiment 1 Probit Models of Blue Choice, reporting marginal effects.

Chip	BLUE OPTION			ORANGE OPTION			Chance You Draw this Chip
	Type I Earnings	Type II Earnings	TOTAL Earnings for this Chip	Type I Earnings	Type II Earnings	TOTAL Earnings for this Chip	
A	\$2.00	\$0.00	\$2.00	\$2.25	\$4.70	\$6.95	1/15
B	\$4.00	\$0.00	\$4.00	\$4.35	\$4.10	\$8.45	1/15
C	\$6.00	\$0.00	\$6.00	\$6.30	\$3.50	\$9.80	1/15
D	\$8.00	\$0.00	\$8.00	\$8.10	\$2.95	\$11.05	1/15
E	\$10.00	\$0.00	\$10.00	\$9.75	\$2.50	\$12.25	1/15
F	\$12.00	\$0.00	\$12.00	\$11.25	\$2.00	\$13.25	1/15
G	\$14.00	\$0.00	\$14.00	\$12.60	\$1.60	\$14.20	1/15
H	\$16.00	\$0.00	\$16.00	\$13.80	\$1.25	\$15.05	1/15
I	\$18.00	\$0.00	\$18.00	\$14.85	\$0.95	\$15.80	1/15
J	\$20.00	\$0.00	\$20.00	\$15.75	\$0.70	\$16.45	1/15
K	\$22.00	\$0.00	\$22.00	\$16.50	\$0.45	\$16.95	1/15
L	\$24.00	\$0.00	\$24.00	\$17.10	\$0.25	\$17.35	1/15
M	\$26.00	\$0.00	\$26.00	\$17.55	\$0.15	\$17.70	1/15
N	\$28.00	\$0.00	\$28.00	\$17.85	\$0.05	\$17.90	1/15
O	\$30.00	\$0.00	\$30.00	\$18.00	\$0.00	\$18.00	1/15

(a) Simultaneous Table; Payouts A

Round	BLUE OPTION			ORANGE OPTION			Number of Green Marbles in Bag	Number of Marbles in Bag	Chance of Continuing to Next Round
	Type I: Payment this Round	Type II Payment Last Round	TOTAL Earnings if Last Round	Type I: Payment this Round	Type II Payment Last Round	TOTAL Earnings if Last Round			
1	\$1.75	\$0.00	\$1.75	\$2.25	\$3.15	\$5.40	14	15	14/15
2	\$1.75	\$0.00	\$3.50	\$2.10	\$2.75	\$7.10	13	14	13/14
3	\$1.75	\$0.00	\$5.25	\$1.95	\$2.35	\$8.65	12	13	12/13
4	\$1.75	\$0.00	\$7.00	\$1.80	\$2.00	\$10.10	11	12	11/12
5	\$1.75	\$0.00	\$8.75	\$1.65	\$1.65	\$11.40	10	11	10/11
6	\$1.75	\$0.00	\$10.50	\$1.50	\$1.35	\$12.60	9	10	9/10
7	\$1.75	\$0.00	\$12.25	\$1.35	\$1.10	\$13.70	8	9	8/9
8	\$1.75	\$0.00	\$14.00	\$1.20	\$0.85	\$14.65	7	8	7/8
9	\$1.75	\$0.00	\$15.75	\$1.05	\$0.65	\$15.50	6	7	6/7
10	\$1.75	\$0.00	\$17.50	\$0.90	\$0.45	\$16.20	5	6	5/6
11	\$1.75	\$0.00	\$19.25	\$0.75	\$0.30	\$16.80	4	5	4/5
12	\$1.75	\$0.00	\$21.00	\$0.60	\$0.20	\$17.30	3	4	3/4
13	\$1.75	\$0.00	\$22.75	\$0.45	\$0.10	\$17.65	2	3	2/3
14	\$1.75	\$0.00	\$24.50	\$0.30	\$0.05	\$17.90	1	2	1/2
15	\$1.75	\$0.00	\$26.25	\$0.15	\$0.00	\$18.00	0	1	0

(b) Sequential I Table: Payouts B

**Table 3:** Examples of Experiment-2 Payout Tables. We depict Payouts A in the top table, and Payouts B in the bottom table. Tables for Sequential II follow the same format as Sequential I, but with final column of Simultaneous table added.

	Simultaneous	Sequential I	Sequential II	Total
No Endowment	27 Payout A	26 Payout A	32 Payout A	85 Payout A
	28 Payout B	25 Payout B	28 Payout B	81 Payout B
	<b>55 Total</b>	<b>51 Total</b>	<b>60 Total</b>	<b>166 Total</b>
Lump Sum Endowment	26 Payout A	25 Payout A		51 Payout A
	22 Payout B	24 Payout B		46 Payout B
	<b>48 Total</b>	<b>49 Total</b>		<b>97 Total</b>
Annuity Endowment	29 Payout A	28 Payout A		57 Payout A
	28 Payout B	25 Payout B		53 Payout B
	<b>57 Total</b>	<b>53 Total</b>		<b>110 Total</b>
TOTAL	82 Payout A	79 Payout A	32 Payout A	193 Payout A
	78 Payout B	74 Payout B	28 Payout B	180 Payout B
	<b>160 Total</b>	<b>153 Total</b>	<b>60 Total</b>	<b>373 Total</b>

**Table 4:** Experiment 2: Number of subjects in each treatment.

Endowment	Probability Frame	Annuity Choice Proportion	$\chi^2$ Test	
			$H_0$	$p$ -value
No Endowment	Simultaneous	83.6%	NoE:Sim=NoE:Seq I	0.069
	Sequential I	68.6%	NoE:Seq I=NoE:Seq II	0.558
	Sequential II	63.3%	NoE:Sim=NoE:Seq II	0.014
Lump Sum	Simultaneous	64.6%	LS:Sim=LS:Seq I	0.584
	Sequential I	59.2%	LS:Sim=Ann:Sim	0.037
Annuity	Simultaneous	82.5%	Ann:Sim=Ann:Seq I	0.368
	Sequential I	75.5%	LS:Seq I=Ann:Seq I	0.079

**Table 5:** Experiment 2: Annuity choice proportions by treatment and  $\chi^2$  test results.

	Total Obs.	No		Endowments			
		Endowment SIM	SEQ <sup>(1)</sup>	Annuity SIM	SEQ	Lump-Sum SIM	SEQ
Mean Risk (Holt-Laury) Score	303	6.41	7.25	6.57	6.46	6.54	6.68
Prop. in Prefer Annuity Range	303	0.51	0.42	0.41	0.48	0.49	0.57
Prop. with Missing HL Scores	373	0.25	0.18	0.14	0.13	0.19	0.24
Prop. Took Review Questions	373	0.78	0.70	1.00	0.72	1.00	1.00
Prop. Received Payouts B <sup>(2)</sup>	373	0.51	0.48	0.49	0.47	0.46	0.49
Points Earned (Std Dev)	207	n.a.	n.a.	85.63 (21.25)	79.32 (21.75)	77.52 (20.80)	76.65 (20.10)
Male	350	0.75	0.62	0.70	0.74	0.67	0.53
Born in the US <sup>(3)</sup>	348	0.47	0.40	0.47	0.47	0.44	0.61
Mean Age	347	21.46	21.60	21.42	21.41	21.69	21.36
Graduate Student	373	0.36	0.41	0.33	0.42	0.38	0.33
Working for Pay	362	0.57	0.49	0.67	0.61	0.91	0.65
Ever Taken Calculus	357	0.87	0.82	0.84	0.90	0.75	0.92
Ever Taken Trigonometry	357	0.91	0.89	0.86	0.92	0.87	0.92
Ever Taken Statistics	364	0.69	0.70	0.82	0.72	0.77	0.75

Notes:

(1) Includes 60 SEQ II subjects. Group's mean Points Earned is 76.07, with a std dev of 20.09.

(2) Payouts B has smaller differences in expected and maximal payoffs than Payout A.

(3) 60% of foreign born are graduate students compared to 6% of US born.

**Table 6:** Experiment 2 subject characteristics across treatments.

	(1) dy/dx	(2) dy/dx	(3) dy/dx	(4) dy/dx	(5) dy/dx	(6) dy/dx
<b>Treatments</b>						
Sequential I	-0.113** (0.05)	-0.153*** (0.05)	-0.137** (0.06)	-0.154*** (0.06)	-0.096* (0.06)	-0.188** (0.08)
No Endowment	0.111** (0.06)	0.112* (0.06)	0.145** (0.07)	0.161** (0.07)	0.182** (0.07)	
Annuity Endowment	0.159*** (0.05)	0.195*** (0.06)	0.190*** (0.06)	0.190*** (0.06)	0.158** (0.06)	0.163** (0.07)
<b>Risk Preferences</b>						
Should Prefer Annuity		0.136*** (0.05)	0.139** (0.06)	0.144** (0.06)	0.115* (0.06)	0.109 (0.08)
<b>Environment</b>						
Payouts B			-0.008 (0.07)	0.017 (0.07)	-0.058 (0.06)	-0.075 (0.08)
Sequential II			-0.061 (0.09)	-0.042 (0.09)	-0.095 (0.09)	
<b>Other Controls</b>						
Demographic	no	no	yes	yes	yes	yes
Imputed Earned Points	no	no	no	yes	yes	n.a.
Missing HL Indicator	no	no	no	no	yes	no
Observations	373	303	291	279	344	158
LR $\chi^2$	13.1	23.3	26.8	28.1	25.4	18.7
Prob> $\chi^2$	0.004	0.000	0.003	0.003	0.013	0.009

*Notes:* dy/dx is for a discrete change of indicator variable from 0 to 1. Significant at: \* 10-percent level; \*\* 5-percent level; \*\*\* 1-percent level. Standard errors in parenthesis. Omitted groups are Simultaneous and Lump-Sum Endowment. HL Scores refer to Holt-Laury risk assessment scores, and Should Prefer Annuity is based on HL Score and Payouts A vs. B. Payouts B has smaller difference in expected total payment and maximal payoff than A. Imputed earned points include the main and squared terms. Sequential II was only administered in the No Endowment frames. Demographic controls: age, US born, ever taken calculus. Missing demographic variables reduce sample sizes in models (3-6).

**Table 7:** Experiment 2 Probit Estimates: Marginal Effects of Sequential and Endowment Treatments on Annuity Choice.