ONLINE-APPENDIX

A Details regarding the implementation of the experiment

Amazon Mechanical Turk Workers log on to AMT through an interface that displays a list of *Human Intelligence Tasks* (HITs), each with a title, an estimated duration, and an estimated remuneration rate. Other HITs include taking surveys, categorizing images, writing product descriptions, and identifying performers on music CDs.

To ensure that subjects were *technically* able to view the videos, we told them at the outset of the study that access to youtube.com was required. We also asked them to reproduce the last word spoken in the welcome video, and the last word of the title slide of whichever treatment video they viewed. Subjects who were not able to complete these tasks correctly were not allowed to continue with the study. The videos were embedded in the survey so that subjects could not find the other treatment videos used in this study.

We ensured that each subject participated in our study only once using the unique identifying numbers assigned by AMT.[83] A subject can only receive payment for participation in the study if she correctly provides this information, and hence has no incentive for misrepresentation. We also recorded the subjects' public IP addresses. These addresses are unique to local networks, such as university networks, public WLAN networks, and the like; but different computers within a local network will typically share a public IP. Hence, it is entirely possible that two subjects with non-unique IP-addresses never interacted with each other. Our data contain 30 subjects with non-unique IP addresses. We re-estimated our estimations excluding these observations, and found that all of our results remain virtually unchanged. They are reproduced in appendix E. Since some of our main results concern a failure to reject a null hypothesis, we report results based on all observations to maximize statistical power.

Attention to the Video Before subjects watched the treatment video, we informed participants that, with 25% probability, their earnings would be entirely determined by their performance on a test,[84] and that 'to be able to answer the questions in the test, you need to both understand and know the contents of the video.' We also explained that the video could help them make better decisions both during the experiment and in real life, inasmuch as it was made by 'internationally recognized academic experts on financial decision making.' Finally, we disabled the *continue* button for the duration of the video.

[83] Nonetheless, one subject managed to participate in our study twice. Both times, this subject exhibited multiple switching points, and hence is excluded from all analyses.

[84] Hastings et al. (2013) criticize most existing studies that use such test scores as outcome measures on the grounds that the tests are unincentivized. One of the few exceptions is Levy and Tasoff (2014).

Iterated Multiple Price List Each line of each price list was a binary choice between the future reward and a specified dollar amount to be received no more than two days after completion of the experiment. For the first price list, the immediate payment varied from \$0 to \$20 in increments of \$2. For the second price list, it varied from $\$x$ to $\$(x + 1.8)$ in increments of \$0.20, where $x + 2$ is the smallest amount chosen over the future reward in the first list. (See appendix F for screenshots of the computer interface.) If a subjects' payment was determined according to a price list, the randomization over lines proceeded as follows. A line was randomly selected from the first price list. If that line did not correspond to x (defined above), it was implemented. Otherwise, a random line from the second price list was selected, and the decision for that line was implemented. With this procedure, truthful revelation of preferences is optimal.

Our measure of response time is the number of seconds a subject took to complete the first of two price lists for each task.

Questionnaire Questions concerning decision strategies employed the following wording. Use of the rule of 72 in complexly framed problems: "Sometimes in this experiment, you were given a choice such as 'We will invest \$10 in an account with 1% interest per week. Interest is compounded weekly. We will pay you the proceeds in 72 days.' When deciding about this choice, did you use the rule of 72?"[85] Use of the rule of 72 in simply framed problems: "Sometimes in this experiment, you were given a choice such as 'We will pay you \$20 in 36 days.' When deciding about such a choice, did you use the rule of 72?" In both cases, subjects answered either "Yes", "No", or "I don't know the rule of 72." Number of problems for which the future reward was calculated explicitly: "In total, you were given 10 rounds in which one of the options was something like "we will invest \$... in an account with ...% interest per day. Interest is compounded daily. We will pay you the proceeds in... days". Out of these 10 rounds, how many times did you explicitly calculate the money amount that this investment would yield within the specified time?" Subjects responded by selecting an integer between 0 and 10. Use of external help on the test: "When you completed the test about the video on financial investing, did you use external resources (such as other websites, books, etc.) to find the right answers?" Subjects answered either "Yes" or "No."

We also asked subjects how much attention they had paid to their choices, how much attention they had paid to the video, whether they had any suggestions about the study, and whether they had experienced any technical difficulties. The overwhelming majority of subjects reported the highest level of attention in answer to both questions.

[85] The survey question incorrectly described the interest rate as pertaining to a week rather than a day. We believe the meaning of the question was nevertheless clear despite this typo.

B Demographics

B.1 Summary statistics

Table B.1 presents detailed demographics of our subject pool by treatment.[86] (The financial literacy questionnaire we administered at the beginning of the experiment is in table A.1.) Column 5 lists data for the representative US citizen. Demographic variables are taken from the 2010 US Census. Employment variables are for April 2014, and come from the Bureau of Labor Statistics. Financial literacy scores are from Lusardi (2011), and from Bricker et al. (2012) for stock holdings. (Representative data on financial literacy only exist for questions FL1 and FL3.) For empty cells, no representative data are available.

As reported in section 4, our sample is poorer,[87] better educated, and more likely to live in larger households than the average US citizen. While the incidence of full-time employment in our sample mirrors that of the general population, the fraction of respondents who classify themselves as employed part-time is double that of the general population. Our subjects are also disproportionately male and white, younger, slightly more urban, and more likely to have never been married than the representative US citizen.

Column 6 reports, for each variable, the p-value of an F-test for differences across treatments. The number of significant differences is well within the range we would expect given the number of tests performed.

B.2 Measures of decision-making quality by demographics

We investigate how our measure of financial competence varies with (self-reported) demographic characteristics of the respondents. We compare this to how the Big Five measure of financial literacy varies with these characteristics, and explore how much of the demographic variation in financial competence remains once we control for financial literacy.[88] Because our dataset is not representative of the general U.S. population, these results should be interpreted with caution.[89]

We measure financial literacy as the number of questions FL1 - FL5 answered correctly, and use only data from subjects in the Control treatment, so that our measures of financial competence are not affected by our treatment interventions.

[86] These statistics only include subjects who did not exhibit multiple switching points in any of the price lists.

[87] Median household income is just slightly lower than that of the representative US household, but our subjects live in markedly larger households.

[88] The demographic variables we use here are slightly coarser than those listed in table B.1. This is to ensure that each subgroup is adequately populated.

[89] We also note that financial literacy scores are based on five unincentivized questions whereas measures of financial competence represent 20 incentivized decisions. Thus, we would expect our measure of financial competence to exhibit a larger number of significant correlations with demographics than financial literacy scores.

Column 1 of table B.2 shows how financial literacy relates to demographics in our subject pool. Perhaps due to the relatively small number of subjects, we find only one demographic variable that is significantly related to financial literacy – on average, hispanics answer one fewer question correctly.

Column 2 shows that several demographic variables are significantly related to the framing distortion, $d_{j,r,t}$. The bias is attenuated for males, for higher income individuals, and for African Americans. Moreover, the bias is exacerbated for subjects in 2-person households. Of these variables, only income is significantly related to welfare, $-(d_{j,r,t})^2$ (see column 3). Notably, the smaller exponential growth biases among males and African Americans do not translate into higher welfare.

The demographic patterns we observe partly align with those for financial knowledge that have been noted in the literature (see Lusardi and Mitchell (2014)). In particular, the literature finds that both males and the more highly educated perform better on tests of financial knowledge, while African-Americans, Hispanics, and people residing in rural areas perform worse. It is unclear why in our experiment the smaller exponential growth bias for the more highly educated translates into welfare gains whereas the smaller exponential growth bias for men does not. In contrast to the literature, we fail to find any relation to age. This might be an artifact of the limited age range observed in our sample.[90]

We also investigate how financial literacy relates to financial competence, and how controlling for financial literacy changes the relationships between demographics, framing distortions, and welfare. The results are in columns 4 and 5. We first note that financial literacy is significantly positively related to the framing distortion $d_{j,r,t}$, perhaps because the average individual is subject to exponential growth bias, and this bias is attenuated for subjects with higher financial literacy scores. Moreover, the relationship between financial literacy and welfare is positive and significant, which shows that financial literacy and financial competence tend to measure related attributes. Controlling for financial literacy, however, does not substantially affect the relationships between demographics and either the average framing distortion or welfare.

B.3 Treatment effects by demographics

To test how demographics affect treatment effects, we run the following regression separately for each treatment $\tau \in \{\text{Full, Substance, Rhetoric}\}$ on the subsample of subjects in treatment τ and in the Control.

$$y_{j,r,t} = X_j'\beta_1 + X_j'\beta_2^{\tau}\mathbb{I}_j(\tau) + \epsilon_{j,r,t} \tag{4}$$

[90] 84.2% of our subjects are between 20 and 40 years of age. The literature finds a hump-shaped relation between financial literacy and age. Age variables remain insignificant in all of the specifications in table B.2 even when we add a quadratic term.

Here, y is either exponential growth bias, d, or welfare, $-d^2$. Moreover, X_j is a vector of demographic variables and a constant.

Table B.3 displays the results. For brevity, it only reports vector β_2.[91] We first consider the effect of demographics on average exponential growth bias $d_{j,r,t}$, displayed in columns 1 - 3. Higher income subjects are significantly less affected by the Substance-Only and Rhetoric-Only treatments. Each of our treatments has a significantly smaller effect on $d_{j,r,t}$ for African-Americans, and we note that African Americans' $d_{j,r,t}$ are significantly closer to zero than other races' (see column 2 of table B.2). We do not find significant effects of education or marital status. Surprisingly, individuals in larger households tend to react less to the Full treatment.

Second, we investigate the effect of demographics on welfare, reported in columns 4 - 6. The benefits of education are significantly smaller for higher income individuals and for those who were never married. We note that both of these groups suffer significantly smaller welfare losses than the other groups in the Control treatment. Finally, the Full treatment increases the welfare of Hispanics by a significantly larger margin, and the Rhetoric-Only treatment increases the welfare of the widowed or divorced to a significantly smaller extent.[92] The remaining demographic variables are not significantly related to the treatment effects.[93]

[91] By necessity, for columns 1 - 3, β_1 is the vector of coefficients in column 2 of table B.2, and for columns 4 - 6 it is the vector of coefficients in column 3 of table B.2.

[92] We caution the reader that table B.3 displays the results of 120 hypothesis tests. Hence we would expect about six coefficients to appear significant at the 5%-level merely by chance. We also note that while each individual in the Full, Substance-Only, or Rhetoric-Only treatment enters only two of the regressions shown in table B.3, the subjects in the Control treatment enter all of these regressions, which means that the coefficient estimates are not independently distributed.

[93] Goda et al. (2012) also find largely insignificant relationships between demographics and treatment effects.

C Additional Analyses

C.1 Treatment effects on simply framed choices

We assess whether the treatments affect choices in the simple frame. Table C.1 shows that only the Rhetoric-Only treatment has a significant effect on patience. Controlling for demographic characteristics slightly increases this effect. This change appears to be driven by subjects with high financial literacy, and by the decisions they make in problems for which the timeframe is 72 days.

C.2 Effects on answers to individual test questions

We analyze the effect of the treatments on answers to individual test questions in table C.2. The test questions differ by how closely they follow the material in education intervention, and by how easily they are answered without knowledge of the rule of 72.

Q1 is the only question for which the answer was explicitly given in the education video for the Full and Substance-Only treatments. These treatments also discussed an example similar, but not identical, to Q2.[94]

The remaining questions required more flexible thinking. Q3 and Q4 can easily be answered with the rule of 72. Knowledge of this rule, however, is not necessary to answer these questions correctly. Q3 can be answered by iteratively multiplying a starting value with 1.07, and counting the number of iterations required for the amount to increase to the desired value. Likewise, Q4 can be answered by calculating the factor by which an investment grows within 8 years at 9 percent interest (either iteratively, or using the compound interest formula), and then dividing 500 by this number. Q5 is a standard compound interest calculation, and parallels the calculations that need to be made in the complexly framed decision problems.

Table C.2 displays the treatment effects on the success rates for each of these questions. The significant effect of the Full and Substance-Only treatments on the total score seems to be due to questions Q1, Q2, and Q5. The fact that performance in Q5 increased in these treatments is reassuring, as it demonstrates that the increase in test scores is at least partly due to subjects increased ability to analyze previously unseen problems properly. At the same time it is puzzling, since any subject who is able to answer Q5 correctly should also be able to correctly assess the future value of interest-bearing investments in complexly framed decision problems. (See section 10 for a discussion of this issue.)

The increase in test scores for the Rhetoric-Only treatment seems to be due to Q2 and Q4.

[94] The example is: "To double your money in 10 years, what rate of return do you need? The answer: 10 times $X = 72$, so $X = 7.2$ percent."

C.3 Welfare analysis with choices that do not satisfy GARP

Most of our subjects' implied rates of time preference vary across reward sizes. The stochastic choice interpretation holds that this variation derives from well-defined preferences that satisfy GARP but are implemented with noise. Another interpretation is that this variation is due to differences in preferences over different reward amounts, or due to subtle differences in framing. To account for the latter, we employ the framework by Bernheim and Rangel (2009). That is, we treat all implied rates of time preference for a given horizon as normatively valid when analyzing any choice made with the same horizon, and live with the remaining ambiguity.

We let $\underline{\delta}_{j,t} = \min_r\{\delta^s_{j,r,t}\}$ and $\bar{\delta}_{j,t} = \max_r\{\delta^s_{j,r,t}\}$. We assume that, for any given simply framed choice with horizon t, subject j could manifest any $\delta \in [\underline{\delta}_{j,t}, \bar{\delta}_{j,t}]$. A subject's welfare from making a complexly framed choice with implied rate of time preference $\delta^c_{j,r,t}$ is then bounded from below by $W_L = \min\{-(\delta^c_{j,r,t} - \underline{\delta}_{j,t})^2, -(\delta^c_{j,r,t} - \bar{\delta}_{j,t})^2\}$, and bounded from above by $W_H = \max\big\{-(\delta^c_{j,r,t} - \delta)^2 : \delta \in [\underline{\delta}_{j,t}, \bar{\delta}_{j,t}]\big\}$. Notice that if $\delta^c_{j,r,t} \in [\underline{\delta}_{j,t}, \bar{\delta}_{j,t}]$, then $W_H = 0$; otherwise, $W_H = \max\{-(\delta^c_{j,r,t} - \underline{\delta}_{j,t})^2, -(\delta^c_{j,r,t} - \bar{\delta}_{j,t})^2\}$.

Table C.3 presents the effects of our treatments on each of these bounds, and splits the results according to financial literacy. Again, only the Rhetoric-Only treatment significantly improves welfare. This is driven by the effect on the lower bound W_L of the high financial literacy subjects.

C.4 Individual Consistency

There are at least two possible explanations for the observation that the Full and Rhetoric-Only treatments eliminate the mean exponential growth bias but are nevertheless unsuccessful in raising welfare. On the one hand, all participants may be subject to an exponential growth bias of a similar size, but their choices may be so noisy that our methodology cannot detect welfare improvements with statistical confidence. In this case, a debiasing intervention will be welfare improving despite the findings reported above. On the other hand, it is possible that subjects are heterogenous with respect to the extent of exponential growth bias. In this case, subjects who overestimate exponential growth will be made worse off by a debiasing intervention.

We distinguish between these hypotheses by examining the consistency exponential growth bias (EGB) across the ten choice pairs for each individual. If subjects are heterogenous in the extent to which they over- or underestimate exponential growth, then a subject's EGB measured on some choice pairs is predictive of the same subject's EGB for the remaining choice pairs. On the other hand, if

subjects are homogenous with respect to EGB, but made decisions in a noisy fashion, such predictive power should be absent.[95]

Formally, we calculate, separately for each treatment, the Cronbach-α of $d_{j,r,t}$. This statistic is 0.92, 0.92, 0.94, and 0.95 for the Control, Full, Substance-Only and Rhetoric-Only treatments, respectively.[96] These values compares favorably with the standard benchmark of 0.8,[97] indicating a high level of individual consistency for EGB.[98]

We conclude that the absence of measurable welfare effects in our setting is *not* due to noisy behavior, but rather due to individual heterogeneity in EGB.[99] While the Full and Rhetoric-Only interventions *do* make some subjects better off, they decrease the welfare of a similar number of subjects by comparable amounts, thus leaving average welfare nearly unchanged.

D Robustness Checks

In this section we demonstrate the robustness of our results so several additional concerns.

D.1 Noise and Inertia

Noise Noisy behavior could mask treatment effects. We classify subjects as *low noise* and *high noise*, depending on a median split of the standard deviation of subject's rates of time preference in the simply framed choice tasks. Specifically, for each simply framed decision we calculate the rate of time preference implied by the subject's choice. For each timeframe, we calculate the subject-level standard deviation of these rates of time preference, then average over timeframes. The average standard deviation is 8.14 percentage points amongst all subjects, and 4.25 percentage points amongst low noise subjects. In comparison, a pilot experiment with 38 Stanford undergraduates yielded a standard deviation in implied discount rates for the simply framed rewards of 5.22 percentage points.[100] Noisiness displays a modest but statistically significant correlation with financial literacy; the correlation coefficient is 0.17 ($p < 0.001$).

Table D.1 displays our main regressions, separately for low-noise and high-noise subjects.

[95] Note that under both hypotheses different realizations of the noise term will cause heterogeneity in within-subject averages of EGB. One cannot distinguishes between these hypotheses merely by pointing to that heterogeneity.

[96] For a vector $(X_1, \ldots, X_n)$ of random variables, Cronbach's alpha is defined as $\alpha(X_1, \ldots, X_n) = \frac{n}{n-1}\left[1 - \frac{\sum_i \text{var}(X_i)}{\text{var}(\sum_i X_i)}\right]$. Higher values signify higher internal consistency.

[97] See e.g., Kline (1999).

[98] For within-subject studies with random ordering of the choice tasks, individual consistency can be an artifact if subjects anchor on their first choice, and subsequently use an incomplete adjustment heuristic to make the remaining choices. In principle, this caveat could apply to our study. Because problems in complex and simple framing were presented in an intermingled fashion, however, such a mechanism can only explain consistency if it applies separately for the two framings, which strikes us as unlikely.

[99] Levy and Tasoff (2014) also find substantial consistency in individual-level EGB.

[100] The Stanford pilot used the timeframes 6, 9, and 12 weeks (i.e. 42, 63, and 84 days), and future values ranging from \$24 to \$36 in steps of 3. It was conducted in the Stanford Economics Research Laboratory (SERL) in March 2014.

The treatment effects on all dependent variables are similar across the two subsamples, with the exception that the effect of the Full treatment on W^+ and the effect of the Rhetoric-Only treatment on W^- are not significant for the high-noise subpopulations. Surprisingly, the high-noise subpopulation has a less severe mean framing distortion than the low-noise subpopulation.

Inertia Rather than behaving in a noisy manner, subjects who do not pay close attention to the experiment may fail to vary their responses appropriately in response to changing stimuli. Indeed, in each of our treatments, we found a single subject with no variation in switching points whatsoever. We therefore investigated the possible implications of inertia for our results.

The implied rate of time preference of a subject who is not sufficiently responsive to variations across the decision problems should be smaller the higher the reward amount. Hence, we estimate each subject's responsiveness by running the following regression, using data on the ten simply framed decision problems (recall that r is a dollar amount to be received in the future):

$$\delta^s_{j,r,t} = \beta^j_0 + \beta^j_1 r + \epsilon_{j,r,t} \tag{5}$$

Note that for a rational utility-maximizing agent with a linear rate of time preference, $\beta^j_1 = 0$. In contrast, for a subject for whom $V_{j,r,t}$ is constant across all decision problems, $\beta^j_1 < 0$.

We find that $\beta^j_1 \geq 0$ for 57.4% of our subjects. We separately investigate all treatment effects for those subjects who are sufficiently or overly responsive ($\beta^j_1 \geq 0$), and for those who are under-responsive ($\beta^j_1 < 0$). The table D.2 displays the results.

Our results are directionally similar for both subsamples. Unsurprisingly, perhaps, treatment effects tend to be smaller for the less responsive subjects, and they are often insignificant (but note that the subsample of insufficiently responsive subjects is smaller).

D.2 Welfare analysis adjusting for noise

Here we check the robustness of our results on welfare (section 7) with respect to procedures that account for stochasticity in choice. Decision noise could account for our null results regarding welfare through the two channels identified in the main text.

To account for decision noise, we implement two separate procedures. For the first, we impute the expected welfare loss each subject would incur if her choices in the complexly framed problems were just as noisy as those in the simply framed problems. Formally, we calculate the mean and standard deviation of each subject j's implied rate of time preference for each timeframe t. For each choice pair, we then replace the complexly framed choice by a draw from a normal distribution with the mean and standard deviation estimated for subject j in timeframe t, and calculate the quadratic deviation. We

repeat this calculation 10000 times, and thus obtain a Monte Carlo estimate, $l_{j,t}$, of the welfare loss that one would calculated for a complexly framed problems simply as a consequence of the decision noise present in the equivalent simply framed problem. We then redo the analysis of section V using $-(d_{j,r,t})^2 + l_{j,t}$ (in other words, the incremental welfare loss associated with complexly framing) as the dependent variable. The results are reported in table D.3. Our conclusions are qualitatively unchanged, with the exception that the Rhetoric treatment does no longer has a significant beneficial effect.

Now we turn to the second procedure through which we account for decision noise. First we calculate each subject's mean rate of time preference in the simply and complexly framed problems, $\bar{\delta}_j^s$ and $\bar{\delta}_j^c$, respectively. Decision noise thereby largely averages out. Our measure of welfare is then given by $-(\bar{\delta}_j^s - \bar{\delta}_j^c)^2$. Table D.4 displays welfare analysis for this alternative measure. Relative to the analysis in section 7, the Rhetoric-Only treatment has a more robustly significantly positive effect (although statistical power is not sufficient to detect them with confidence when the subject pool is split into subpopulations of subjects with high and low levels of financial literacy). This finding appears to be driven by the fact that the Rhetoric-Only treatment does not significantly increase mean implied rates of time preference that are already too high. The Full treatment, however, continues to increase those high rates of time preference, and therefore fails to improve mean welfare significantly.

D.3 Treatment effects on complexly framed choices

In field contexts, data on simply framed decisions are typically unavailable. Consequently, there is no standard against which choices with and without educational interventions can be compared. In such situations, one can only assess whether interventions affect behavior, and one must make strong assumptions to gauge the effect of the behavioral change on welfare.

Table D.5 shows our treatment effects on the implied rates of time preference for complexly framed choices. The table suggests that while the Full and Rhetoric-Only treatments raise these implied rates of time preference by similar amounts, the Substance-Only treatment leaves them approximately unchanged.

To approximate the welfare implications of the intervention without observing simply framed choices, one could compare the mean treatment effect to an estimate of the behavioral bias obtained from some other source.[101] Stango and Zinman (2009), for instance, model subjects' anticipated future values as $FV = V \times (1+i)^{\theta t}$, and estimate the median of θ to be 0.8.[102] Using this estimate, one might proceed on the basis of the assumption that an increase in the implied discount factors (starting from

[101] For instance, Goda et al. (2012) cite the literature on exponential growth bias to conclude that the increase in saving they observe due to an increase in the perceived interest rate implies that the intertemporal elasticity of substitution of their average participant must exceed 1.

[102] See footnote 24 in Stango and Zinman (2009).

the levels in the Control group) of up to 30.1 percentage points would increase the median subjects' welfare.[103] In fact, the Full treatment raises the mean implied rate of time preference with complex framing by $14.31/58.95 = 24.27\%$.

Thus, if one used more standard approaches that fail to elicit and employ appropriate normative benchmarks, one might (erroneously) conclude that the significant behavioral change due to the Full treatment is welfare-enhancing.

[103] Given the tasks in our experiment, a subject with $\theta = 0.8$ underestimates future values by a factor of 0.71. Hence, an increase in estimated future values by a factor of $1/0.71 = 1.401$. Discounting this by the mean time preference of 0.736, we arrive at 30.1 percentage points.

Table A.1: FINANCIAL LITERACY QUESTIONNAIRE

FL1. Suppose you had $100 in a savings account and the interest rate was 2 percent per year. After 5 years, how much do you think you would have in the account if you left the money to grow?

More than $102 (92.86%), Exactly $102 (3.37%), Less than $102 (1.98%), Do not know (1.79%)

FL2. Suppose you had $100 in a savings account and the interest rate is 20 percent per year and you never withdraw money or interest payments. After 5 years, how much would you have on this account in total?

More than $200 (72.62%), Exactly $200 (22.62%), Less than $200 (2.98%), Do not know (1.79%)

FL3. Imagine that the interest rate on your savings account was 1 percent per year and inflation was 2 percent per year. After 1 year, how much would you be able to buy with the money in this account?

More than today (8.33%), exactly the same (6.94%), less than today (1.15%), do not know (3.57%)

FL4. Assume a friend inherits $10,000 today and his sibling inherits $10,000 3 years from now. Who is richer because of the inheritance?

My friend (55.36%), his sibling (9.13%), they are equally rich (29.37%), do not know (6.15%)

FL5. Suppose that in the year 2015, your income has doubled and prices of all goods have doubled too. In 2015, how much will you be able to buy with your income?

More than today (4.76%), the same (89.29%), less than today (4.76%), do not know (1.19%)

Numbers in brackets indicate the percentage of subjects who chose a given answer.

Table B.1: DEMOGRAPHICS AND FINANCIAL LITERACY

Variable	(1) Control	(2) Full	(3) Substance	(4) Rhetoric	(5) US	(6) p-value
FL1	91.7	93.4	92.2	94.6	65	.81
FL2	73.4	81.1	73.4	70.5	-	.27
FL3	81.7	82.1	82.8	84.8	64	.92
FL4	64.2	57.5	50	58.9	-	.17
FL5	89.9	96.2	86.7	91.1	-	.03**
All questions FL1 - FL3 correct	63.3	70.8	61.7	61.6	-	.41
All questions FL1 - FL5 correct	45	47.2	34.4	40.2	-	.19
Male	56.9	56.6	60.9	50	49.2	0.40
Age (median)	32	28	29	29	37.2	0.05**
Household Income (median)[a]	35,000	45,000	45,000	45,000	53,046	0.69
Race						
African-american	5.5	7.5	7.8	4.5	13.1	.66
Asian	11	7.5	12.5	5.4	5.1	.18
Caucasian	72.5	81.1	71.9	76.8	63.0	.31
Hispanic	7.3	2.8	3.1	9.8	16.9	.08*
Other	3.7	.9	4.7	3.6	1.9	.19
Education						
Less than high school	0	.9	0	0	13.7	-
High school	11.9	13.2	14.8	14.3	31.0	.92
Vocational / technical	8.3	7.5	7.8	2.7	8.6	.11
Some college	36.7	34.9	32.8	43.8	19.3	.35
College	36.7	38.7	37.5	33.9	18.0	.9*
Graduate degree	6.4	4.7	7	5.4	9.3	.88
Employment						
Full time employed	49.5	50	47.7	42.9	48.2[b]	0.66
Part time employed	22.9	20.8	25.8	26.8	10.6[c]	0.74
Marital Status[d]						
Never married	65.1	64.2	64.1	64.3	26.9	0.99
Married	30.3	28.3	32	29.5	56.4	0.45
Widowed	0	0	0	0	6.3	-
Divorced	4.6	6.6	3.9	4.5	10.4	0.86
Urban / Rural						
Urban and suburban	83.5	83	89.1	83	80.7	0.38
Rural	16.5	17	10.9	17	19.3	0.38
Household size						
1	12.8	17.9	10.9	18.8	21.7	0.27
2	23.9	21.7	25	24.1	36.3	0.8
3	19.3	14.2	17.2	22.3	16.5	0.59
4 or more	44	46.2	46.9	34.8	25.6	0.24
Owns stocks	22.9	16	20.3	23.2	15.1	
N	109	106	128	112		

The sample includes all subjects who completed the study and did not exhibit multiple switching points in any of the treatments. Column 5 presents comparison values for the representative US citizen, whenever they are available. See text for data sources.

[a] In our survey, household income is interval coded. The values stated are the midpoints of the median intervals.
[b] Percentage of civilian noninstitutional population that is full-time employed.
[c] Percentage of civilian noninstitutional population that is part-time employed.
[d] Our questionnaire included the option "Prefer not to say". The three subjects who chose this response are not accounted for in this table.

Table B.2: FINANCIAL LITERACY, FRAMING DISTORTION, AND FINANCIAL COMPETENCE BY DEMOGRAPHICS

VARIABLES	(1) Financial literacy	(2) $100 \times d_{j,r,t}$	(3) $-100 \times (d_{j,r,t})^2$	(4) $100 \times d_{j,r,t}$	(5) $-100 \times (d_{j,r,t})^2$
Male	0.193	11.54**	1.242	10.79**	0.900
	(0.247)	(4.610)	(2.743)	(4.588)	(2.751)
Age	0.0124	-0.0982	-0.0143	-0.146	-0.0362
	(0.0148)	(0.248)	(0.142)	(0.245)	(0.141)
Income (in $1000)	0.00406	0.172**	0.126***	0.156**	0.119***
	(0.00452)	(0.0832)	(0.0438)	(0.0776)	(0.0420)
Rural	-0.120	-4.643	-0.849	-4.182	-0.637
	(0.329)	(5.948)	(3.323)	(5.745)	(3.375)
Race					
African American	-0.450	25.17***	-4.240	26.90***	-3.444
	(0.526)	(7.860)	(8.834)	(8.078)	(8.825)
Asian	-0.577	-3.533	-4.270	-1.308	-3.247
	(0.363)	(7.777)	(4.787)	(7.777)	(4.821)
Hispanic	-1.060**	1.021	-4.582	5.106	-2.704
	(0.435)	(7.832)	(3.086)	(8.439)	(3.132)
Other	-0.723	-7.762	-9.009	-4.975	-7.727
	(0.646)	(16.49)	(8.644)	(15.82)	(8.320)
Education					
High school or less	-0.356	11.00*	-2.613	12.37**	-1.982
	(0.583)	(6.082)	(4.988)	(6.202)	(4.948)
Vocational school or some college	0.227	-5.463	-3.500	-6.339	-3.902
	(0.507)	(5.421)	(3.463)	(5.626)	(3.394)
College degree	0.198	-3.947	-0.707	-4.710	-1.057
	(0.492)	(4.183)	(3.065)	(4.319)	(2.942)
Employment					
Full time employed	-0.0122	1.501	-0.0667	1.548	-0.0450
	(0.294)	(5.383)	(3.125)	(5.087)	(3.053)
Part time employed	0.0504	5.271	5.109*	5.077	5.020*
	(0.333)	(6.118)	(2.998)	(5.763)	(2.846)
Marital status					
Widowed or divorced	0.129	8.723	8.797*	8.226	8.569*
	(0.601)	(7.729)	(4.831)	(7.737)	(4.867)
Never married	-0.0696	9.968*	7.631**	10.24*	7.755**
	(0.307)	(5.709)	(3.205)	(5.692)	(3.182)
Household size					
2	0.287	-12.80**	-4.908	-13.91**	-5.416*
	(0.322)	(5.851)	(3.322)	(5.422)	(3.165)
3 to 5	-0.604	12.04	1.899	14.36**	2.969
	(0.460)	(7.345)	(4.518)	(7.206)	(4.521)
6 or more	-0.293	4.533	-1.605	5.663	-1.085
	(0.423)	(6.549)	(4.254)	(6.754)	(4.371)
Owns stocks	-0.0531	-2.362	-1.844	-2.158	-1.750
	(0.325)	(4.483)	(2.901)	(4.285)	(2.773)
Financial literacy score				3.854**	1.772*
				(1.738)	(0.970)
Constant	3.435***	-24.49*	-22.26***	-37.73**	-28.35***
	(0.800)	(13.80)	(7.815)	(15.28)	(8.895)
Observations	109	1,090	1,090	1,090	1,090
Subjects	109	109	109	109	109
R^2	0.199	0.155	0.081	0.170	0.089

*** p<0.01, ** p<0.05, * p<0.1

Excluded categories are *married*, *caucasian*, *urban or suburban*, *graduate degree*, *unemployed* and *single person household*. Only data from subjects in the Control treatment shown. Standard errors clustered by subject.

Table B.3: TREATMENT EFFECTS BY DEMOGRAPHICS

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	$100 \times d_{j,r,t}$			$-100 \times (d_{j,r,t})^2$		
Treatment	Full	Substance	Rhetoric	Full	Substance	Rhetoric
Male	-9.114	-11.21	-10.54	-3.243	3.407	1.193
	(6.899)	(6.801)	(6.395)	(4.426)	(3.892)	(3.609)
Age	-0.412	0.0569	0.145	0.00105	0.0483	0.0952
	(0.414)	(0.354)	(0.356)	(0.182)	(0.181)	(0.202)
Income	-0.0394	-0.197*	-0.339***	-0.141**	-0.123*	-0.167***
	(0.130)	(0.112)	(0.112)	(0.0640)	(0.0623)	(0.0534)
Rural	6.376	8.869	1.929	-0.567	2.815	-6.060
	(9.507)	(7.998)	(8.796)	(5.882)	(4.497)	(5.035)
Race						
African American	-27.74**	-24.36**	-36.98***	2.983	3.893	2.663
	(10.69)	(10.93)	(12.02)	(9.561)	(9.657)	(9.417)
Asian	6.099	-9.315	1.391	7.005	-2.521	8.185
	(10.62)	(10.87)	(15.11)	(5.929)	(6.708)	(6.079)
Hispanic	-6.041	-8.294	0.793	16.40***	7.042	1.909
	(10.46)	(11.24)	(9.756)	(5.400)	(5.292)	(4.129)
Other	-1.129	-4.777	-5.795	16.65	7.776	-9.492
	(20.33)	(19.52)	(20.85)	(11.92)	(10.02)	(15.19)
Education						
High school or less	3.349	-2.534	-15.92	-3.069	3.569	2.753
	(11.93)	(12.76)	(11.90)	(10.40)	(8.980)	(6.894)
Vocational school, or some college	13.92	5.190	1.773	-2.146	1.239	3.316
	(9.860)	(11.08)	(10.81)	(9.067)	(7.515)	(4.909)
College degree	8.836	7.143	0.222	3.490	3.030	5.677
	(7.488)	(10.71)	(9.958)	(7.239)	(7.383)	(4.574)
Full time employed	12.09	-2.196	2.997	-1.942	1.187	1.744
	(8.130)	(6.995)	(7.405)	(4.393)	(4.059)	(4.363)
Part time employed	2.521	1.727	-8.151	-5.872	-3.097	-0.836
	(9.570)	(8.814)	(8.012)	(5.058)	(4.668)	(4.551)
Marital status						
Widowed or divorced	-8.319	7.827	17.94	-11.80	-3.462	-24.89**
	(14.46)	(11.38)	(16.18)	(7.714)	(6.724)	(10.70)
Never married	-12.73	-8.853	-8.852	-9.622**	-10.32**	-11.43***
Household size						
	(8.472)	(8.354)	(7.561)	(4.642)	(4.867)	(4.359)
2	19.11**	0.0711	18.32**	6.745	5.723	1.987
	(9.452)	(8.203)	(7.355)	(5.524)	(4.771)	(4.440)
3 to 5	-28.47**	-14.18	-5.877	-0.306	-2.969	-1.838
	(11.74)	(10.74)	(9.698)	(8.513)	(6.383)	(5.580)
6 or more	-22.74**	-14.69	-2.653	2.107	0.210	2.077
	(10.56)	(9.917)	(8.779)	(8.520)	(5.811)	(5.288)
Owns stocks	0.653	6.332	3.289	3.173	3.756	1.240
	(7.393)	(6.584)	(7.082)	(4.695)	(3.902)	(3.995)
Constant	43.12**	31.77	37.73*	13.23	6.665	11.13
	(20.26)	(20.10)	(20.63)	(11.86)	(10.72)	(10.86)
Observations	2,130	2,350	2,160	2,130	2,350	2,160
Subjects	213	235	216	213	235	216
R^2	0.168	0.120	0.171	0.119	0.111	0.144

Excluded categories are *married*, *caucasian*, *urban or suburban*, *single person household*, *unemployed* and *high school or less*. Standard errors clustered by subject.

Table C.1: CHOICES IN SIMPLY FRAMED PROBLEMS

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	$\delta^s_{j,r,t}$	$\delta^s_{j,r,t}$	$\delta^s_{j,r,t}$	$\delta^s_{j,r,t}$	$\delta^s_{j,r,36}$	$\delta^s_{j,r,72}$
Sample	all	all	high FL	low FL	36 days	72 days
Demographic controls	No	Yes	No	No	No	No
Constant	72.26***	68.08***	75.08***	67.38***	75.87***	68.64***
	(2.089)	(7.414)	(2.155)	(4.214)	(2.013)	(2.244)
Full	0.402	-0.177	-2.224	4.792	-0.0725	0.876
	(2.990)	(2.914)	(3.246)	(6.076)	(2.890)	(3.186)
Substance-Only	0.0182	1.019	-4.670	7.895	-0.698	0.734
	(2.913)	(2.842)	(3.401)	(5.261)	(2.821)	(3.115)
Rhetoric-Only	5.368*	6.165**	8.483***	0.710	4.308	6.428**
	(2.975)	(3.000)	(3.021)	(5.773)	(2.843)	(3.198)
$P(\beta_{Substance}=\beta_{Rhetoric})$	0.0689	0.0859	0.000122	0.157	0.0764	0.0704
$P(\beta_{Full}=\beta_{Rhetoric})$	0.0998	0.0353	0.00100	0.489	0.130	0.0845
$P(\beta_{Substance}=\beta_{Full})$	0.897	0.677	0.495	0.566	0.827	0.964
P(joint insignificance)	0.202	0.000466	0.000342	0.371	0.268	0.162
Observations	4,550	4,460	2,920	1,630	2,275	2,275
Number of subjects	455	446	292	163	455	455
R^2	0.008	0.075	0.047	0.014	0.007	0.010

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered by subject.

Table C.2: INDIVIDUAL TEST QUESTIONS.

Question	Q1	Q2	Q3	Q4	Q5
Level in Control	0.330***	0.220***	0.514***	0.422***	0.477***
	(0.0380)	(0.0402)	(0.0478)	(0.0478)	(0.0474)
Treatment effects					
Full	0.566***	0.619***	0.0617	0.0214	0.174**
	(0.0541)	(0.0573)	(0.0681)	(0.0680)	(0.0674)
Substance-Only	0.584***	0.592***	-0.0372	0.0233	0.109*
	(0.0517)	(0.0548)	(0.0650)	(0.0650)	(0.0644)
Rhetoric-Only	0.0715	0.191***	0.0666	0.114*	0.0497
	(0.0534)	(0.0565)	(0.0671)	(0.0671)	(0.0665)
P(joint insignificance)	0	0	0.313	0.330	0.0587
$P(\beta_{Substance}=\beta_{Rhetoric})$	0	0	0.109	0.162	0.356
$P(\beta_{Full}=\beta_{Rhetoric})$	0	0	0.942	0.173	0.0645
$P(\beta_{Substance}=\beta_{Full})$	0.732	0.623	0.132	0.977	0.317
Observations	455	455	455	455	455
R^2	0.320	0.283	0.008	0.008	0.016

*** p<0.01, ** p<0.05, * p<0.1

Table C.3: WELFARE ANALYSIS WITHOUT GARP

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	$100 \times W_L$	$100 \times W_H$	$100 \times W_L$	$100 \times W_H$	$100 \times W_L$	$100 \times W_H$
Sample	all	all	high FL	high FL	low FL	low FL
Level in Control	-18.85***	-7.326***	-15.82***	-6.301***	-24.08***	-9.096***
	(2.346)	(0.986)	(1.949)	(1.154)	(5.352)	(1.776)
Treatment effects						
Full	1.611	-0.380	1.591	-0.282	-0.448	-1.329
	(3.134)	(1.603)	(3.169)	(1.960)	(6.346)	(2.616)
Substance-Only	3.099	1.073	4.461*	1.039	1.244	1.244
	(2.885)	(1.351)	(2.432)	(1.694)	(6.386)	(2.217)
Rhetoric-Only	4.770*	1.461	6.436***	1.654	2.462	1.276
	(2.860)	(1.312)	(2.309)	(1.482)	(6.384)	(2.424)
$P(\beta_{Substance}=\beta_{Rhetoric})$	0.476	0.760	0.302	0.692	0.805	0.988
$P(\beta_{Full}=\beta_{Rhetoric})$	0.233	0.230	0.0833	0.293	0.551	0.305
$P(\beta_{Substance}=\beta_{Full})$	0.578	0.354	0.322	0.512	0.729	0.272
P(joint insignificance)	0.358	0.538	0.0306	0.611	0.941	0.678
Observations	4,550	4,550	2,920	2,920	1,630	1,630
Number of Subjects	455	455	292	292	163	163
R^2	0.004	0.002	0.012	0.002	0.001	0.003

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered by subject.

Table D.1: ROBUSTNESS TO NOISE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Test score		$100 \times d_{j,r,t}$		$100 \times W$		$100 \times W^+$		$100 \times W^-$	
Noise	low	high	low	high	low	high	low	high	low	high
Level in Control	2.113***	1.821***	-16.56***	-10.23***	-10.80***	-12.53***	-9.851***	-8.664***	-0.954*	-3.870***
	(0.194)	(0.191)	(3.020)	(3.199)	(1.794)	(1.690)	(1.798)	(1.532)	(0.514)	(1.132)
Treatment effects										
Full	1.585***	1.155***	14.79***	14.31**	2.372	-4.318	5.397***	2.720	-3.025***	-7.038**
	(0.263)	(0.290)	(3.970)	(5.753)	(2.217)	(3.773)	(2.046)	(2.238)	(1.159)	(3.521)
Substance-Only	1.596***	1.055***	4.626	2.906	1.123	1.887	1.700	1.358	-0.577	0.529
	(0.272)	(0.254)	(4.179)	(4.146)	(2.542)	(2.217)	(2.502)	(2.016)	(0.898)	(1.453)
Rhetoric-Only	0.501*	0.469*	14.91***	11.76***	2.052	2.978	5.168**	5.369***	-3.116**	-2.391
	(0.270)	(0.272)	(4.184)	(4.104)	(2.382)	(2.432)	(2.135)	(1.657)	(1.377)	(2.023)
$P(\beta_{Substance}=\beta_{Rhetoric})$	5.80e-05	0.0228	0.0126	0.0171	0.698	0.630	0.0979	0.00629	0.0864	0.127
$P(\beta_{Full}=\beta_{Rhetoric})$	3.90e-05	0.0194	0.974	0.639	0.875	0.0561	0.880	0.131	0.956	0.214
$P(\beta_{Substance}=\beta_{Full})$	0.967	0.716	0.00924	0.0379	0.574	0.0919	0.0652	0.516	0.0559	0.0296
P(joint insignificance)	5.13e-05	0.000209	0.00583	0.729	0.221	0.0229	0.00133	0.0171	0.0914	
Observations	228	227	2,280	2,270	2,280	2,270	2,280	2,270	2,280	2,270
R^2	0.194	0.096	0.047	0.028	0.002	0.011	0.023	0.018	0.011	0.017

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered by subject in columns (3) - (10).

Table D.2: ROBUSTNESS TO INERTIA

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Test score		$100 \times d_{j,r,t}$		$100 \times W$		$100 \times W^+$		$100 \times W^-$	
Responsiveness	≥ 0	<0	≥ 0	<0	≥ 0	<0	≥ 0	<0	≥ 0	<0
Level in Control	1.951***	1.979***	-16.24***	-9.578***	-12.23***	-11.01***	-10.37***	-7.802***	-1.858***	-3.207**
	(0.185)	(0.204)	(2.870)	(3.420)	(1.642)	(1.864)	(1.658)	(1.623)	(0.551)	(1.287)
Treatment effects										
Full	1.580***	1.235***	15.99***	11.49*	2.432	-3.960	5.971***	1.744	-3.539**	-5.704*
	(0.258)	(0.298)	(3.892)	(5.917)	(2.287)	(3.740)	(1.927)	(2.308)	(1.506)	(3.421)
Substance-Only	1.584***	0.880***	5.174	2.469	2.470	0.191	2.241	0.710	0.229	-0.520
	(0.252)	(0.276)	(3.893)	(4.494)	(2.285)	(2.440)	(2.284)	(2.106)	(0.804)	(1.726)
Rhetoric-Only	0.818***	0.0421	18.73***	5.936	2.260	3.001	6.700***	3.346*	-4.441***	-0.345
	(0.257)	(0.289)	(3.876)	(4.447)	(2.375)	(2.368)	(1.886)	(1.896)	(1.642)	(1.816)
$P(\beta_{Substance}=\beta_{Rhetoric})$	0.00218	0.00290	0.000304	0.396	0.928	0.192	0.0144	0.114	0.00512	0.919
$P(\beta_{Full}=\beta_{Rhetoric})$	0.00295	9.65e-05	0.460	0.323	0.941	0.0517	0.585	0.403	0.666	0.119
$P(\beta_{Substance}=\beta_{Full})$	0.987	0.218	0.00396	0.111	0.986	0.251	0.0451	0.627	0.0138	0.126
P(joint insignificance)	2.34e-10	1.68e-05	1.34e-06	0.215	0.658	0.194	0.00102	0.231	0.00421	0.417
Observations	261	194	2,610	1,940	2,610	1,940	2,610	1,940	2,610	1,940
R^2	0.169	0.122	0.057	0.016	0.002	0.012	0.032	0.007	0.014	0.016

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered by subject in columns (3) - (10).

Table D.3: WELFARE ANALYSIS CONTROLLING FOR NOISE IN SIMPLY FRAMED CHOICES.

	(1)	(2)	(3)	(4)	(5)	(6)
Welfare function	$-100 \times (d_{j,r,t})^2$					
Sample	all	all	high FL	low FL	36 days	72 days
Demographic controls	no	yes	no	no	no	no
Level in Control	-8.997***	-10.90***	-7.697***	-11.24***	-9.377***	-8.617***
	(1.194)	(3.759)	(1.357)	(2.224)	(1.196)	(1.387)
Treatment effects						
Full	-0.780	-1.021	-0.300	-2.845	-0.653	-0.908
	(2.004)	(1.876)	(2.405)	(3.373)	(2.287)	(2.104)
Substance-Only	1.252	0.496	1.439	1.097	0.664	1.840
	(1.598)	(1.555)	(1.920)	(2.769)	(1.786)	(1.706)
Rhetoric-Only	1.800	1.657	2.255	1.225	2.255	1.344
	(1.570)	(1.507)	(1.736)	(2.951)	(1.614)	(1.786)
$P(\beta_{Substance}=\beta_{Rhetoric})$	0.710	0.417	0.639	0.960	0.353	0.741
$P(\beta_{Full}=\beta_{Rhetoric})$	0.177	0.143	0.260	0.204	0.193	0.247
$P(\beta_{Substance}=\beta_{Full})$	0.293	0.400	0.471	0.195	0.577	0.142
P(joint insignificance)	0.469	0.476	0.507	0.570	0.425	0.429
Observations	4,550	4,460	2,920	1,630	2,275	2,275
Number of subjects	455	446	292	163	445	445
R^2	0.002	0.031	0.003	0.004	0.002	0.003

*** p<0.01, ** p<0.05, * p<0.1

Table D.4: WELFARE ANALYSIS WITH INDIVIDUAL-LEVEL NOISE AVERAGED OUT.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	W	W	W	W	W^+	W^-
Sample	all	all	high FL	low FL	all	all
Controls	No	Yes	No	No	No	No
Level in Control	-7.134***	-8.205**	-5.401***	-10.12***	-6.421***	-0.713
	(1.054)	(3.729)	(1.330)	(1.680)	(0.846)	(0.711)
Treatment effects						
Full	0.610	0.582	-0.448	1.966	3.562***	-2.952***
	(1.501)	(1.498)	(1.843)	(2.543)	(1.205)	(1.013)
Substance-Only	1.375	0.966	1.375	1.375	1.684	-0.309
	(1.434)	(1.420)	(1.820)	(2.264)	(1.151)	(0.967)
Rhetoric-Only	2.913**	2.667*	2.631	3.574	4.375***	-1.463
	(1.480)	(1.476)	(1.881)	(2.334)	(1.189)	(0.999)
$P(\beta_{Substance}=\beta_{Rhetoric})$	0.281	0.238	0.402	0.483	0.0190	0.230
$P(\beta_{Full}=\beta_{Rhetoric})$	0.123	0.164	0.0958	0.522	0.497	0.139
$P(\beta_{Substance}=\beta_{Full})$	0.597	0.790	0.385	0.985	0.106	0.00695
P(joint insignificance)	0.229	0.309	0.353	0.505	0.00111	0.0141
Observations	455	446	292	163	455	455
R^2	0.010	0.095	0.011	0.015	0.035	0.023

*** p<0.01, ** p<0.05, * p<0.1

Table D.5: CHOICES IN COMPLEX FRAMING.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	$100 \times \delta^c_{j,r,t}$					
Sample	all	all	high FL	low FL	36 days	72 days
Demographic Controls	No	Yes	No	No	No	No
Level in Control	58.95***	57.71***	63.12***	51.76***	61.50***	56.40***
	(2.272)	(8.939)	(2.471)	(4.272)	(2.371)	(2.333)
Treatment effects						
Full	14.31***	13.41***	10.46***	20.74***	14.40***	14.23***
	(3.427)	(3.418)	(3.762)	(6.951)	(3.532)	(3.580)
Substance-Only	4.021	4.752	-1.366	13.17**	4.213	3.829
	(3.285)	(3.339)	(3.736)	(6.021)	(3.403)	(3.369)
Rhetoric-Only	18.59***	19.45***	20.06***	16.73**	17.64***	19.54***
	(3.595)	(3.506)	(3.845)	(6.754)	(3.675)	(3.682)
$P(\beta_{Substance}=\beta_{Rhetoric})$	7.96e-05	7.91e-05	2.67e-07	0.598	0.000342	3.26e-05
$P(\beta_{Full}=\beta_{Rhetoric})$	0.259	0.100	0.0196	0.597	0.399	0.178
$P(\beta_{Substance}=\beta_{Full})$	0.00340	0.0131	0.00327	0.277	0.00464	0.00451
P(joint insignificance)	1.62e-07	4.08e-08	8.62e-08	0.0123	1.08e-06	1.20e-07
Observations	4,550	4,460	2,920	1,630	2,275	2,275
Number of Subjects	455	455	292	163	455	455
R^2	0.047	0.094	0.075	0.037	0.044	0.051

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered by subject.

E Tables Including only Subjects with Unique IP addresses

In this section, we re-estimate all tables included in the main text on the subsample of subjects that have a unique IP address.

Table E.1: TEST SCORES AND SELF-REPORTED BEHAVIOR.

VARIABLES	(1) Test score compounding	(2) Test score control	(3) External help	(4) Uses R72 in complex framing	(5) Uses R72 in simple framing	(6) Explicit Calculation
Level in Control	1.979***	3.309***	0.237***	0.144***	0.103**	6.639***
	(0.147)	(0.109)	(0.0443)	(0.0425)	(0.0406)	(0.376)
Treatment effects						
Full	1.371***	-1.089***	-0.0371	0.546***	0.137**	1.341**
	(0.206)	(0.153)	(0.0622)	(0.0596)	(0.0570)	(0.527)
Substance-Only	1.281***	-1.351***	0.0402	0.612***	0.241***	1.554***
	(0.198)	(0.147)	(0.0597)	(0.0572)	(0.0547)	(0.506)
Rhetoric-Only	0.413**	-1.104***	0.0620	0.0706	0.0371	0.0805
	(0.203)	(0.150)	(0.0612)	(0.0587)	(0.0561)	(0.519)
$P(\beta_{Substance}=\beta_{Rhetoric})$	8.90e-06	0.0839	0.709	0	0.000144	0.00295
$P(\beta_{Full}=\beta_{Rhetoric})$	2.77e-06	0.923	0.103	0	0.0734	0.0147
$P(\beta_{Substance}=\beta_{Full})$	0.649	0.0725	0.192	0.243	0.0546	0.671
P(joint insignificance)	0	0	0.370	0	3.07e-05	0.00151
Observations	423	423	423	423	423	423
R^2	0.136	0.187	0.007	0.303	0.055	0.036

*** p<0.01, ** p<0.05, * p<0.1

Table E.2: FRAMING DISTORTION.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	$100 \times d_{i,r,t}$					
Sample	all	all	high FL	low FL	36 days	72 days
Demographic controls	No	Yes	No	No	No	No
Level in Control	-14.46***	-13.08*	-12.81***	-17.13***	-15.51***	-13.40***
	(2.365)	(7.721)	(2.582)	(4.553)	(2.419)	(2.508)
Treatment effects						
Full	15.36***	14.47***	14.27***	16.71**	15.88***	14.84***
	(3.505)	(3.379)	(3.907)	(6.963)	(3.666)	(3.686)
Substance-Only	5.035	5.663*	4.042	6.507	5.767*	4.303
	(3.073)	(2.932)	(3.376)	(5.986)	(3.298)	(3.127)
Rhetoric-Only	14.47***	14.76***	12.59***	17.51***	14.57***	14.38***
	(3.115)	(2.976)	(3.336)	(6.069)	(3.240)	(3.245)
$P(\beta_{Substance}=\beta_{Rhetoric})$	0.000899	0.00141	0.00517	0.0506	0.00487	0.000327
$P(\beta_{Full}=\beta_{Rhetoric})$	0.788	0.930	0.641	0.904	0.708	0.893
$P(\beta_{Substance}=\beta_{Full})$	0.00159	0.00545	0.00543	0.121	0.00463	0.00145
P(joint insignificance)	1.02e-06	6.76e-07	9.21e-05	0.0152	3.78e-06	2.16e-06
$P(\beta_0 = 0)$	2.22e-09	0.0911	1.25e-06	0.000240	3.85e-10	1.49e-07
$P(\beta_0 + \beta_{Full} = 0)$	0.728	0.853	0.618	0.937	0.894	0.596
$P(\beta_0 + \beta_{Substance} = 0)$	2.21e-06	0.335	7.21e-05	0.00702	1.73e-05	1.59e-06
$P(\beta_0 + \beta_{Rhetoric} = 0)$	0.993	0.819	0.917	0.924	0.661	0.635
Observations	4,230	4,230	2,720	1,510	2,115	2,115
Number of subjects	423	423	272	151	423	423
R^2	0.039	0.069	0.040	0.038	0.038	0.039

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered by subject.

Table E.3: FINANCIAL COMPETENCE

	(1)	(2)	(3)	(4)	(5)	(6)
Welfare function	$W = -100 \times (d_{i,r,t})^2$					
Sample	all	all	high FL	low FL	36 days	72 days
Demographic controls	no	yes	no	no	no	no
Level in Control	-12.16***	-15.72***	-10.18***	-15.36***	-12.31***	-12.01***
	(1.328)	(4.090)	(1.380)	(2.591)	(1.338)	(1.486)
Treatment effects						
Full	0.0318	-0.300	0.202	-1.770	-0.0264	0.0899
	(2.159)	(2.057)	(2.504)	(3.741)	(2.463)	(2.231)
Substance-Only	2.602	2.368	2.265	2.800	1.816	3.387*
	(1.746)	(1.666)	(1.947)	(3.222)	(1.904)	(1.822)
Rhetoric-Only	2.728	3.178*	3.539**	1.620	3.252*	2.205
	(1.802)	(1.650)	(1.780)	(3.553)	(1.846)	(1.977)
$P(\beta_{Substance}=\beta_{Rhetoric})$	0.939	0.602	0.474	0.704	0.440	0.481
$P(\beta_{Full}=\beta_{Rhetoric})$	0.198	0.0805	0.161	0.352	0.178	0.318
$P(\beta_{Substance}=\beta_{Full})$	0.209	0.163	0.410	0.169	0.456	0.0950
P(joint insignificance)	0.270	0.132	0.194	0.545	0.291	0.184
Observations	4,230	4,230	2,720	1,510	2,115	2,115
Number of Subjects	423	423	272	151	423	423
R^2	0.004	0.044	0.006	0.004	0.004	0.005

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered by subject.

Table E.4: ALTERNATIVE WELFARE FUNCTIONS

	(1)	(2)	(3)	(4)
VARIABLES	$100 \times W^+$	$100 \times W^-$	$-100 \times \lvert d_{i,r,t} \rvert$	$-100 \times \lvert d_{i,r,t} \rvert^3$
Level in Control	-9.728***	-2.428***	-25.09***	-7.592***
	(1.263)	(0.711)	(1.747)	(1.101)
Treatment effects				
Full	4.644***	-4.613***	1.890	-1.533
	(1.560)	(1.755)	(2.513)	(2.255)
Substance-Only	2.413	0.188	3.941*	1.695
	(1.659)	(0.920)	(2.272)	(1.452)
Rhetoric-Only	5.621***	-2.893**	4.956**	1.135
	(1.441)	(1.308)	(2.275)	(1.668)
$P(\beta_{Substance}=\beta_{Rhetoric})$	0.0126	0.0136	0.622	0.722
$P(\beta_{Full}=\beta_{Rhetoric})$	0.395	0.377	0.187	0.253
$P(\beta_{Substance}=\beta_{Full})$	0.115	0.00515	0.377	0.140
P(joint insignificance)	0.000484	0.00472	0.138	0.408
Observations	4,230	4,230	4,230	4,230
Number of subjects	423	423	423	423
R^2	0.021	0.012	0.006	0.002

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered by subject.

Table E.5: USE OF SIMPLE INTEREST FORMULA

	(1)	(2)	(3)
VARIABLE		$V^c_{i,r,t}/V^s_{i,r,t}$	
Sample	all	high FL	low FL
$\beta_1^{Control}$	0.371***	0.262**	0.453**
	(0.110)	(0.121)	(0.210)
β_1^{Full}	0.0310	0.0177	0.0102
	(0.0345)	(0.0304)	(0.128)
$\beta_1^{Substance}$	0.0754**	0.0600*	0.279
	(0.0378)	(0.0358)	(0.205)
$\beta_1^{Rhetoric}$	0.0305	-0.00260	0.159
	(0.0354)	(0.0333)	(0.109)
$\beta_0^{Control}$	0.665***	0.760***	0.498**
	(0.0938)	(0.106)	(0.201)
β_0^{Full}	0.985***	0.993***	0.972***
	0.0248	0.0212	0.0987
$\beta_0^{Substance}$	0.917***	0.930***	0.743***
	0.0282	0.0265	0.168
$\beta_0^{Rhetoric}$	0.983***	1.003***	0.892***
	0.0211	0.0180	0.0865
$P(\beta_1^{Control} = \beta_1^{Full})$	0.00316	0.0500	0.0723
$P(\beta_1^{Control} = \beta_1^{Substance})$	0.0110	0.109	0.554
$P(\beta_1^{Control} = \beta_1^{Rhetoric})$	0.00319	0.0349	0.215
Observations	4,230	2,720	1,510
Subjects	423	272	151

*** p<0.01, ** p<0.05, * p<0.1

Estimated using median regression. Standard errors clustered by subject.

Table E.6: RESPONSE TIMES AND EFFECT OF DOUBLINGS

	(1)	(2)	(3)	(4)	(5)
VARIABLES	$\tau^c_{i,r,t}$	$\tau^s_{i,r,t}$	$-(d_{i,r,t})^2$	$-(d_{i,r,t})^2$	$-(d_{i,r,t})^2$
Doublings			1	[2 3 4]	2.5
Level in Control	50.93***	22.51***	-7.319***	-13.58***	-12.72***
	(2.882)	(1.155)	(1.591)	(1.501)	(2.093)
Treatment effects					
Full	2.578	-0.568	0.231	1.933	-5.872
	(4.718)	(1.832)	(2.050)	(2.191)	(4.227)
Substance-Only	20.99***	-0.400	2.446	3.426*	0.284
	(7.560)	(1.498)	(1.735)	(1.972)	(2.911)
Rhetoric-Only	9.646**	1.174	2.602	4.940**	-3.780
	(4.753)	(2.310)	(1.743)	(1.931)	(3.509)
$\beta_{Full} - \beta_{Rhetoric}$			-2.371	-3.007	-2.092
$\beta_{Substance} - \beta_{Rhetoric}$			-0.156	-1.514	4.064
Observations	4,230	4,230	846	2,538	846
Number of Subjects	423	423	423	423	423
R^2	0.006	0.000	0.009	0.009	0.006

*** p<0.01, ** p<0.05, * p<0.1

Columns (1) and (2) show the effect of the treatments on mean response times for the complexly and simply framed problems, respectively. Columns (3) - (5) show the effect on welfare for problems that vary depending on how many times the investment amount doubles over the investment period in the complex framing. Standard errors clustered by subject.

F Instructions

This is a research study run by the department of economics at Stanford University.

IMPORTANT

This study may take up to ONE AND A HALF HOURS to complete. Please start this study only if you do have that much time in a single session.

If you do not complete the study, or if the HIT times out on you, we will not be able to pay you. (The HIT is set to time out in 3 hours.)

You will earn $10 just for completing this study. In addition, you will receive up to $20, depending on the decisions you make in this study.

Do not start this study if you do not have access to youtube.com. Some browsers will block embedded videos. Please make sure your browser will display them, as you may otherwise not be able to complete this study.

Click here to start the study: https://stanforduniversity.qualtrics.com/SE/?SID=SV_0GPXNo1f9TX5YIR

Provide the survey code here:

WELCOME

This is a research study run by the department of economics at Stanford University.

IMPORTANT

This study may take up to ONE AND A HALF HOURS to complete. Please start this study only if you do have that much time in a single session.

If you do not complete the study, or if the HIT times out on you, we will not be able to pay you. (The HIT is set to time out in 3 hours.)

You will earn $10 just for completing this study. In addition, you will receive up to $20, depending on the decisions you make in this study.

Do not start this study if you do not have access to youtube.com. Some browsers will block embedded videos. Please make sure your browser will display them.

By clicking the button below, you consent to participating in this research study.

Questions, Concerns, or Complaints: If you have any questions, concerns or complaints about this research study, its procedures, risks and benefits, you should ask the Protocol Director, Sandro Ambuehl, sambuehl@stanford.edu

Independent contact: If you are not satisfied with how this study is being conducted, or if you have any concerns, complaints, or general questions about the research or your rights as a participant, please contact the Stanford Institutional Review Board (IRB) to speak to someone independent of the research team at (650)-723-2480 or toll free at 1-866-680-2906. You can also write to the Stanford IRB, Stanford University, Stanford, CA 94305-5401

>>

[Some browsers will ask you whether you want to display this content. Please click "display all content".]

[There should be a video here. If if does not load, please click here]

Links to researchers' personal homepages

Professor B. Douglas Bernheim

Sandro Ambuehl

To continue, please enter the LAST word that Doug Bernheim said in this video. A continue button will appear after the duration of the video.

>>

Before we start this study, we would like to ask you a few questions about yourself. Please answer these questions truthfully. Your answers will not affect your payment from this experiment.

What is your gender?

- male
- female

What is your age?

What is your ethinicity?

- African-American
- Asian
- Caucasian
- Hispanic
- Other

Please indicate the highest level of education you completed.

- Elementary School
- Middle School
- High School or equivalent
- Vocational/Technical School (2 year)
- Some College
- College Graduate (4 year)
- Master's Degree (MS)
- Doctoral Degree (PhD)
- Professional Degree (MD, JD, etc.)

What is your current marital status?

- Divorced
- Living with another
- Married
- Separated
- Single
- Widowed
- Prefer not to say

Which of the following best describes the area you live in?

- ◯ Urban
- ◯ Suburban
- ◯ Rural

Please indicate your current household income in U.S. dollars

- ◯ Under $10,000
- ◯ $10,000 - $19,999
- ◯ $20,000 - $29,999
- ◯ $30,000 - $39,999
- ◯ $40,000 - $49,999
- ◯ $50,000 - $74,999
- ◯ $75,000 - $99,999
- ◯ $100,000 - $150,000
- ◯ Over $150,000
- ◯ Prefer not to say

Please choose the option that describes your situation best

- ◯ I am unemployed
- ◯ I am employed part-time
- ◯ I am employed full-time

How many people other than you live in your household?

Do you own stocks or bonds?

- ◯ Yes
- ◯ No

>>

Please answer the following questions as well as you can. Your answers to these questions will not affect your payment from this study.

Imagine that the interest rate on your savings account was 1 percent per year and inflation was 2 percent per year. After 1 year, how much would you be able to buy with the money in this account?

- More than today
- Exactly the same
- Less than today
- Do not know

Suppose you had $100 in a savings account and the interest rate is 20 percent per year and you never withdraw money or interest payments. After 5 years, how much would you have on this account in total?

- More than $200
- Exactly $200
- Less than $200
- Do not know

Assume a friend inherits $10,000 today and his sibling inherits $10,000 3 years from now. Who is richer because of the inheritance?

- My friend
- His sibling
- They are equally rich
- Do not know

Suppose you had $100 in a savings account and the interest rate was 2 percent per year. After 5 years, how much do you think you would have in the account if you left the money to grow?

- More than $102
- Exactly $102
- Less than $102
- Do not know

Suppose that in the year 2015, your income has doubled and prices of all goods have doubled too. In 2015, how much will you be able to buy with your income?

- More than today
- The same
- Less than today
- Do not know

>>

You will now watch a

12-MINUTE VIDEO ABOUT FINANCIAL INVESTING.

Please follow this video carefully.

Please watch the ENTIRE video.

(a "continue" button will appear after 12 minutes.)

Doing so will be useful to you for three reasons:

1. *TEST with PAYMENT FOR CORRECT ANSWERS.*

Your earnings from this experiment may be entirely determined by a test on this video. **The final part of this experiment is a test about the contents of this video. There is a one in four chance that your earnings from this experiment are wholly determined by your performance in this test. The test has 10 questions. For each question you answer correctly, you will receive $1 within at most two days from today. For each question you answer incorrectly, you will receive $0. To be able to answer the questions in the test, you need to both *understand* and *know* the contents of the video. You may scroll back to watch parts of the video multiple times if you wish.**

2. *REMAINDER OF THIS STUDY.*

The video may help you with your decisions in the remainder of this experiment.

In each remaining part of this experiment, you will make financial investment decisions. There is a three in four chance that one of these decisions wholly determines your earnings from this experiment.

3. REAL LIFE

The video may help you with your decisions in real life.

This video was made by internationally recognized academic experts on financial decision making (Burton G. Malkiel, Charles D. Ellis, and B. Douglas Bernheim). This video may help you make financial decisions in your life in general.

>>

PLEASE FOLLOW THIS VIDEO CAREFULLY

PLEASE WATCH THE ENTIRE VIDEO

[Some browsers will ask you whether you want to display this content. Please click "display all content".]

[There should be a video here. If it does not load, please click here.]

To continue, enter the FOURTH word of the FIRST slide of this video. A continue button will appear after the duration of the video.

>>

PLEASE READ THESE INSTRUCTIONS CAREFULLY

The remainder of this experiment consists of 20 rounds of decision making.

Your payment may be determined entirely by ONE RANDOMLY CHOSEN decision you make in this part of the experiment.

This will happen with a three in four chance. Otherwise, your payment is determined by your performance in the test about the video you just watched.

Hence, you should make every decision as if it is the one that counts, because it might be!

>>

PLEASE READ THESE INSTRUCTIONS CAREFULLY

In each round, you will be presented with two lists. The first list will be like the following:

	you will get the specified dollar amount within two days from today	Option X
$20	○	○
$18	○	○
$16	○	○
$14	○	○
$12	○	○
$10	○	○
$8	○	○
$6	○	○
$4	○	○
$2	○	○
$0	○	○

Option X will vary from round to round. For instance, option X may be "get $15 in 8 weeks".

YOUR TASK:
Decide, on each line, whether you prefer the option on the left, or the option on the right.

Most people begin a decision list by preferring the option on the left, and then switch to the option on the right, for instance like this:

	you will get the specified dollar amount within two days from today	Option X
$20	◉	○
$18	◉	○
$16	◉	○
$14	◉	○
$12	◉	○
$10	◉	○
$8	◉	○
$6	○	◉
$4	○	◉
$2	○	◉
$0	○	◉

After you have filled in the first list, you will be shown the second list. This list will have *different payment amounts*, for instance like this:

	you will get the specified dollar amount within two days from today	Option X
$ 7.80	○	○
$ 7.60	○	○
$ 7.40	○	○
$ 7.20	○	○
$ 7	○	○
$ 6.80	○	○
$ 6.60	○	○
$ 6.40	○	○
$ 6.20	○	○
$ 6	○	○

Again, your task is to decide, on each line, whether you prefer the option on the left, or the option on the right.

Read this paragraph if you want to know how the options on the second list are determined.

The options on the second list are determined by the point at which you switched from the left option to the right option in the first list. The second list will display payment amounts that lie between the two amounts at which you switched in the first list. In the above example, you switched between the amounts $6 and $8. Hence, the second list shows amounts between $6 and $8.

>>

PLEASE READ THESE INSTRUCTIONS CAREFULLY

Our payment procedure is designed such that it is in your best interest to choose, on each line of each decision list, the option you genuinely prefer.

Here's why: You'll get exactly what you chose, for one randomly drawn decision.

Read this paragraph if you want to know more details.

Question: When will I be paid according to the first decision list, and when will I be paid according to the second decision list in a round?

Answer: Suppose you filled in the *first* decision list of a round as follows:

	you will get the specified dollar amount within two days from today	Option X
$20	◉	○
$18	◉	○
$16	◉	○
$14	◉	○
$12	◉	○
$10	◉	○
$8	◉	○
$6	○	◉
$4	○	◉
$2	○	◉
$0	○	◉

If the line randomly selected on the *first* list is NOT the line corresponding to $6, you will be paid according to the *first* decision list. Otherwise, you will be paid according to the *second* decision list.

That is, you are paid according to the FIRST decision list whenever the line randomly selected on that list is NOT the first line at which you chose the option on the right. Otherwise, you are paid according to the SECOND decision list.

>>

YOU WILL NOW MAKE YOUR DECISIONS

It is in your best interest to choose as you genuinely prefer. Please think about your choices carefully.

There are no right or wrong choices!

>>

Please choose, on each line, the option you genuinely prefer.

If you pick the option on the LEFT,

you will get the specified dollar amount within two days from today.

If you pick the option on the RIGHT,

we will invest $4.50 in an account with 2% interest per day. Interest is compounded daily. We will pay you the proceeds in 72 days.

You may switch from left to right at most once.

This is the
first
decision list for these options.

	you will get the specified dollar amount within two days from today	we will invest $4.50 in an account with 2% interest per day. Interest is compounded daily. We will pay you the proceeds in 72 days.
$20	○	○
$18	○	○
$16	○	○
$14	○	○
$12	○	○
$10	○	○
$8	○	○
$6	○	○
$4	○	○
$2	○	○
$0	○	○

>>

Please choose, on each line, the option you genuinely prefer.

If you pick the option on the LEFT,

you will get the specified dollar amount within two days from today.

If you pick the option on the RIGHT,

we will invest $4.50 in an account with 2% interest per day. Interest is compounded daily. We will pay you the proceeds in 72 days.

You may switch from left to right at most once.

This is the
second
decision list for these options.

	you will get the specified dollar amount within two days from today	we will invest $4.50 in an account with 2% interest per day. Interest is compounded daily. We will pay you the proceeds in 72 days.
$ 9.8	○	○
$ 9.6	○	○
$ 9.4	○	○
$ 9.2	○	○
$ 9	○	○
$ 8.8	○	○
$ 8.6	○	○
$ 8.4	○	○
$ 8.2	○	○
$ 8	○	○

>>

TEST

You will now participate in a test about the video you have watched at the beginning of the experiment. The test has 10 questions.

There is a one in four chance that your earnings from this study are entirely determined by your performance in this test.

IF you are randomly chosen to be paid according to this test, THEN: For each question you answer correctly, you will earn $1. For each question you answer incorrectly, you will earn $0. You will be paid within at most two days from today.

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What is an "indexing" investment strategy?

- Buying index funds, which hold assets that have been indexed as particularly profitable by financial experts
- Buying index funds, which hold stocks of companies that provide information about the stock market as a whole (stock market indices)
- Buying index funds, which hold the market portfolio
- Buying index funds, which hold optimally diversified, custom tailored portfolios

Paul had invested his money into an account which paid 9% interest per year (interest is compounded yearly). After 8 years, he had $500. How big was the investment that Paul had made 8 years ago?

- $200
- $210
- $220
- $230
- $240
- $250
- $260
- $270
- $280
- $290
- $300
- $310
- $320
- $330
- $340
- $350
- $360
- $370
- $380
- $390
- $400

if the interest rate is 10% per year (interest is compounded yearly), how many years does it take until an investment doubles?

- 7 years
- 7.2 years
- 7.4 years
- 7.8 years
- 8 years

If an investment grows at 8 percent per year (interest is compounded yearly), by how much has it grown after 4 years?

- by 30%
- by 31%
- by 32%
- by 33%
- by 34%
- by 35%
- by 36%
- by 37%
- by 38%
- by 39%
- by 40%

Which of the following quotes is attributed to Benjamin Franklin?

- Compound interest is the most powerful force in the universe
- Youth is wasted on the young
- Money makes money. And the money that money makes, makes money

What percentage of mutual funds tends to be outperformed by the market (S&P 500 Index) each year?

- between 10 and 30%
- between 30 and 50%
- between 50 and 70%
- between 70 and 90%

If the interest rate is 7% per year (interest is compounded yearly), about how long does it take until an investment has grown by a factor of four (i.e. is four times as large as it was originally)?

- about 5 years
- about 10 years
- about 15 years
- about 20 years
- about 25 years
- about 30 years
- about 25 years
- about 30 years
- about 35 years
- about 40 years

Which quote is attributed to the author Upton Sinclair

- Only liars manage always to be out of the market during bad times and in during good times.
- It is difficult to get a man to understand something when his salary depends upon his not understanding it.
- There are three classes of people who do not believe that markets work: the Cubans, the North Koreans, and active managers.
- Nobody knows more than the market

If somebody tells you an investment should double in four years, what rate of return (per year) is he promising?

- 15%
- 16%
- 17%
- 18%
- 19%
- 20%

Professional investors as a whole are responsible for what percentage of stock market trading?

- 30%
- 50%
- 70%
- 90%

>>

Please answer the following questions truthfully. Your answers to these questions DO NOT AFFECT YOUR PAYMENT for this study.

How much attention did you pay to your choices?

- ◯ I paid quite a bit of attention for all of my choices.
- ◯ For some choices I paid attention, for others I didn't pay much attention
- ◯ I clicked through most of the choices without paying much attention.

At the beginning of the experiment, we asked you to watch a video about financial investing. Please indicate which of the following describes your situation best

- ◯ I watched the entire video, and paid close attention
- ◯ I watched the entire video, but sometimes didn't pay attention
- ◯ I skipped parts of the video, because I already knew the material
- ◯ I skipped parts of the video, because it was boring (but I did not already know the material)
- ◯ I did not watch the video.

Sometimes in this experiment, you were given a choice such as "We will invest $10 in an account with 1% interest per day. Interest is compounded weekly. We will pay you the proceeds in 72 days." When deciding about this choice, did you use the rule of 72?

- ◯ Yes
- ◯ No
- ◯ I don't know the rule of 72

Sometimes in this experiment, you were given a choice such as "We will pay you $20 in 36 days." When deciding about such a choice, did you use the rule of 72?

- ◯ Yes
- ◯ No
- ◯ I don't know the rule of 72

In total, you were given 10 rounds in which one of the options was something like "we will invest $... in an account with ...% interest per week. Interest is compounded weekly. We will pay you the proceeds in … days". Out of these 10 rounds, how many times did you explicitly calculate the money amount that this investment would yield within the specified time?

When you completed the test about the video on financial investing, did you use external resources (such as other websites, books, etc.) to find the right answers?

- ◯ Yes
- ◯ No

Do you have any suggestions for us about this experiment?

Did you experience any technical difficulties with this study?

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