Myopic Loss Aversion and Updating in the Presence of Safety

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Abstract

In 1997, two separate research teams, Thaler et al. (1997) and Gneezy and Potters (1997), conducted experiments to explore whether the theory of Myopic Loss Aversion could provide an explanation for the equity premium puzzle. Both teams found that subjects who were forced to commit for more time periods allocated more to the riskier asset class, providing evidence for the theory of Myopic Loss Aversion. However, in Thaler et al. (1997), subjects who were tasked with investing over longer periods became more risk-seeking over time. In contrast, subjects in Gneezy and Potters (1997) whose allocations held for more periods did not change their behavior over time. In this experiment, I replicate Thaler et al. (1997) while informing some subjects that one of the assets has a no-loss attribute in an effort to explore the discrepancy between the two experiments. I find that when informing subjects that the less risky asset is loss-less, those who trade the most frequently do choose to allocate more to the riskier asset initially. Additionally, those who trade less frequently, when provided with the information, do not increase their allocation to the riskier asset over the course of the experiment. Using these findings, I find evidence that the availability of a no-loss option in allocation decisions can either induce or inhibit updating behaviors depending on the task presented to subjects.

Introduction

How investors allocate between assets in repeated decisions across time is an important question in the study of financial markets. The equity premium puzzle, documented by Mehra and Prescott (1985), is an apparent violation of how agents typically allocate resources between interest-bearing, risky assets. In any financial market which contains multiple assets with varying levels of risk, a wedge exists in the returns demanded by investors between a relatively risky asset and a less risky asset. Over the century of financial data that Mehra and Prescott analyzed, the annual return on the S&P 500, an index of risky equities, was approximately 8%, while the average return on T-Bills, essentially risk free assets, was less than 1%. The magnitude of this difference in the returns between risky and less risky assets implies a coefficient of relative risk aversion of approximately 30, while most experimental evidence places the coefficient of relative risk aversion closer to 1 (Thaler et al. 1997). The equity premium puzzle, at its core, is the question of why this large divergence in the implied coefficients of relative risk aversion exists.

A survey of the potential explanations for the equity premium puzzle (Siegel and Thaler 1997) concluded that it is difficult to explain the equity premium puzzle without the incorporation of some degree of irrationality into agent decision making processes. One particular theory that could explain the equity premium puzzle is that of Myopic Loss Aversion. The Theory of Myopic Loss Aversion was first proposed by Bernatzi and Thaler (1995). The authors sought to provide an explanation for the equity premium puzzle using findings from behavioral economics. Bernatzi and Thaler proposed that the ways in which investors view their investment decisions across time, a mental accounting process, and the tendency for investors to overweight losses compared to gains, a phenomenon known as loss aversion, explain the tendency for investors to exhibit a relatively high degree of risk aversion.

To explain the behavior that Myopic Loss Aversion predicts in allocation decisions between risky assets, I use the problem posed by Paul Samuelson to a colleague in Samuelson (1963). Consider a gamble of the following nature: A fair coin is flipped. If the coin lands heads-up, you will receive $200, but if the coin lands tails-up, you lose $100. Samuelson's colleagues rejected the gamble, presumably because the expected utility they assigned to the lottery was negative. However, when Samuelson altered the gamble such that, instead of the coin being flipped only once, it was flipped 100 times, with the sum of the outcomes being the winnings of the participant, the colleague would accept the gamble. Bernatzi and Thaler (1995) proposed that investors in U.S. financial markets exhibited similar behavior to Samuelson's colleagues. Specifically, they predicted investors would be more willing to invest in assets with higher levels of risk if they were forced to commit their investments to a higher number of time periods, just as Samuelson's colleague was willing to accept the altered version of the gamble.

In 1997, two separate research teams, Thaler et al. (1997) and Gneezy and Potters (1997), conducted experiments to explore the theory of Myopic Loss Aversion. The experimenters tasked subjects with allocating resources between two risky assets, with some subjects being assigned to treatments where their allocations held for multiple periods. Both experiments found that decreasing the frequency at which investors observed the returns on their portfolios and were able to adjust their allocations led to subjects contributing more of their resources to the riskier asset. This was exactly the behavior predicted by the theory of Myopic Loss Aversion presented in Bernatzi and Thaler (1995). In the experiment conducted by Thaler et al. (1997), the authors found that subjects whose allocations held for multiple periods allocated a higher percentage of their funds to the risky option over time, while subjects whose allocations held for only one period did not allocate more to the risky option over time. In other words, subjects whose allocations held for only a few periods did not change from their initial allocation levels, but subjects whose allocations held for a high number of periods allocated progressively more to the riskier fund with each decision they made. However, in the experiment conducted by Gneezy and Potters (1997), no change in behavior over the course of the experiment was observed for either of the treatment groups. The explanation for this discrepancy has not been previously addressed or explored. This paper seeks to provide a partial explanation for this discrepancy.

After a review of the experimental designs, I identify a potential causal mechanism that may explain why updating behavior is observed in some tests of Myopic Loss Aversion but not others. Subjects in Gneezy and Potters (1997) were informed that they had the option of saving some of their available funds instead of allocating them to a risky gamble. Subjects in Thaler et al. (1997) had a similar option available to them by the way of one of the allocation options having a no-loss attribute; however, subjects were not informed of this fact. Importantly, subjects whose allocations held for only one period were the most likely to be aware of this attribute. Subjects in all treatment groups in Gneezy and Potters (1997) did not change their allocations over time, and the subjects who were the most likely to realize the no-loss nature of one of the allocation options in Thaler et al. (1997) did not change their allocations over time. I hypothesize that the knowledge of a no-loss option makes it such that individuals are less concerned with maximizing their returns when they know they have a sure way of avoiding losses.

I conduct an experiment which tests various implications of my hypothesis using the proven experimental framework of Thaler et al. (1997). This experiment consists of a replication of the experiment by Thaler et al. (1997) to use as a reference and a treatment arm wherein subjects are informed of specific characteristics of one of the allocation options, specifically that the lower risk fund has a lower bound of 0 on its returns. My hypothesis implies that the added knowledge of the lower bound on the lower risk fund will inhibit subjects from changing their allocations over the course of the study when allocations hold for more periods. My hypothesis also implies that provision of the information about the lower bound should have no effect on subjects whose allocations hold for only one period. The latter implication follows from the fact that subjects whose allocations held for only one period likely knew about the lower bound on the less risky asset.

The results of my replication are consistent with the results of Thaler et al. (1997) and, the estimates are statistically significant even when clustering at the subject level. Additionally, I find that subjects in the highest frequency treatment group, after being informed of the lower bound on the returns of the low-risk fund, allocated more in their initial allocations compared to subjects who were not provided information, while subjects in the lower frequency treatment group did not increase their initial allocations to the low-risk fund compared to subjects who were not provided information. For subjects in lower frequency treatment groups who were provided information, I am unable to ascertain whether their updating behavior differed from their higher frequency counterparts. Finally, I find that the information provision to subjects in the highest frequency treatment group induces learning or updating behavior that was not found in either the original experiment conducted by Thaler et al. (1997). This is a surprising result as it directly contradicts the implications of my hypothesis, and these findings warrants more exploration.

Section I consists of a survey of the prior literature, both experimental and theoretical, to the experiment performed. Section II consists of a presentation of my own experimental design. Sections III and IV will report the results and discuss applications of behavioral theory which have the possibility of explaining these findings, respectively.

I. Prior Literature

The lab experiment conducted by Thaler et al. (1997) supports the hypothesis that Myopic Loss Aversion is a possible explanation for the Equity Premium Puzzle. The Equity Premium puzzle, as discussed previously, was first identified by Mehra and Prescott (1985). Theory of Myopic Loss Aversion, as described by Bernatzi and Thaler (1995), draws heavily on the concept of loss aversion and mental accounting. The Theory of Myopic Loss Aversion proposes that when an agent engages in multiple gambles or allocation decisions, the way in which that the agent aggregates the results from those decisions matters. The mechanism through which this aggregation may affect how an agent perceives a gamble is loss aversion. Loss aversion is the tendency for people to weigh losses more heavily than they do gains, and the typical estimate of the coefficient of loss aversion is 2 to 2.5, as noted in Tversky and Kahneman (1992). I will use 2.5 as the coefficient of loss aversion for this discussion, as both Bernatzi and Thaler (1999) and Thaler et al. (1997) did. If an agent exhibits loss aversion, assuming linearity of utility, they might have the following utility function:

$$U(x) = \begin{cases} x & if\ x > 0 \\ -\lambda x & \leq 0 \end{cases}$$

In the utility function shown above, λ is the coefficient of loss aversion. With this utility function, an agent would not take a single-shot gamble consisting of a 50% chance of a gain of \$200 and a 50% chance of a loss of \$100. This is consistent with the reluctance of Samuelson's colleague in Samuelson (1963) to engage in the gamble. However, as Bernatzi (1999) notes, if one considers the second version of Samuelson's gamble, the version where this gamble is played 100 times, the chance of a loss in this gamble is less than half a percent. Even without computation, using the above utility function, one can see the choice of playing 100 times is associated with a positive expected utility. The theory of Myopic Loss Aversion proposes that investors exhibit similar behavior to that of Samuelson's colleague, and that, when investors are restricted to evaluating an aggregation of lotteries, they will be more willing to take risks.

In the lab experiment conducted by Thaler et al. (1997), it was found that when subjects are forced to make allocation decisions between two assets over a longer time horizon, subjects, on average, choose to allocate a higher percentage of their funds to riskier assets. In other words, subjects were forced to commit to a gamble which involved a sum of multiple realizations of a gamble which, individually, had positive expected value. Subjects who were forced to commit to multiple periods allocated more to the riskier option. This is exactly what Bernatzi and Thaler (1995) predicted with their Theory of Myopic Loss Aversion. This finding was also confirmed in a lab experiment, conducted separately, by Gneezy and Potters (1997). Gneezy and Potters (1997) found that when subjects engaged in gambles in which they had to allocate resources to multiple plays of that gamble, they showed a preference for the riskier allocation option. This preference for risk when investing for multiple periods was further confirmed with professional traders in a field experiment by Haigh and List (2005). In fact, the findings in Haigh and List (2005) show that professional traders experience myopic loss aversion to an even greater degree than the undergraduate subjects in the experiments by Gneezy and Potters (1997) and Thaler et al. (1997). The increase in risk seeking behavior for subjects making allocation decisions which hold for multiple periods, or plays of a game, has been confirmed in other studies as well (e.g Bellemare et al. (2005)). However, the focus of this research will be on the experiments conducted by Thaler et al. (1997) and Gneezy and Potters (1997). I will firstly give a description of the experimental designs and procedures of the two experiments. I will then highlight what I believe to be the most important difference between the two experiments.

Gneezy and Potters (1997) recruited approximately 80 undergraduate students for their experiment, and the experiment was conducted on pen and paper in an in-person environment.. The problem that the subjects were tasked with was an allocation problem. In each decision that subjects made, they chose how much of their funds for that decision they would allocate to a gamble, and the remaining funds were saved. The number of decisions that subjects made varied, but the number of realizations of a lottery were the same. Specifically, subjects made decisions that held over a total of 9 periods; however, some subjects made only 3 decisions while others made 9 decisions. Subjects were split into two groups, each group's size being approximately 40 individuals, and assigned two different treatments. The subjects in the first group, as will be called Treatment H, were told that they were participating in a game with lotteries. A subject was tasked with deciding how much of their endowment, which was the same for each round at 200 cents, they would risk in a lottery. Subjects had a 1/3 chance of winning the lottery, and if they won, they would receive two and a half times the amount of money that they had risked in the gamble, if, on the other hand, subjects did not win the lottery, as happened 2/3rds of the time, they would lose all the money that they had gambled. Subjects in the second group, as will be called Treatment L, participated in a process very similar to Treatment H, however, subjects were given 600 cents, triple the amount in Treatment H, and they were told that they could bet any amount up to 600 cents. This amount of money was split equally between 3 realizations of the lottery. In other words, each time a subject made a decision, they committed to the same allocation to a lottery for 3 lotteries. Subjects in Treatment H made 3 decisions, and since each of those decisions held for 3 rounds, they experienced 9 realizations from the lottery. It is important to note that subjects were informed of the probabilities of winning the lottery. Finally, subjects were compensated based on their winnings held at the end of the experiment.

I will now summarize the experimental design of the experiment conducted by Thaler et al. (1997) which will later serve as the template for the experiment I conduct. The experiment recruited 80 subjects from a university student population and used a digital format. Subjects were told that they were to act as the manager of a university endowment, and they were tasked with maximizing the returns of this endowment. Subjects were told to allocate a percentage of the endowment's resources between two funds, Funds A and B. The returns for Fund A were drawn from a distribution with a mean of .25% and a standard deviation of .177%, with a lower bound of 0. This lower bound of 0, as I will explain later, is the starting for the motivations of my research. The returns for Fund B were drawn from a distribution with a mean of 1% and standard deviation of 3.54%. These distributions, respectively, were generated by analyzing the actual returns of 5-year bonds and a value-weighted stock index over 6.5 weeks. Subjects were assigned to one of four treatment groups with varying lengths for which their allocations held. More specifically, subjects were assigned to treatment groups where their allocations between the two funds held for 1 period, 25 periods, or 40 periods. Additionally, half of subjects who were assigned to the monthly treatment group were assigned to a sub-treatment which I will call the "inflated monthly" treatment. This treatment took the returns from Fund A and Fund B and translated them upwards by 10 percentage points. This had the effect of nearly eliminating the possibility that either of the funds would have a negative return.

In both experiments, regardless of the degree to which the risky and less risky options differed in their rewards and levels of risk, subjects in treatments whose decisions held for more periods had a greater appetite for allocating resources to the risky asset. This can be seen in the results of Thaler et al. (1997) and Gneezy and Potters (1997 as both provide strongly suggestive evidence that the frequency at which investors evaluate the portfolios has a negative association with an investor's appetite for risk. However, the behavior of subjects over the course of the experiments differs between the two experiments and within the treatment groups of the experiment performed by Thaler et al. (1997). As one can see from Thaler et al. (1997)'s results, as shown in Table 1, subjects in all treatment groups besides the monthly condition allocated more of their resources to the lower risk as they progressed through their allocation decisions. This updating behavior, as it will be called, did not occur at a significant level in the experiment conducted by Gneezy and Potters (1997), and this absence was noted by the authors.

The question that this paper aims to help answer is why updating behavior occurred in the experiment conducted by Thaler et al. (1997) but not in the experiment conducted by Gneezy and Potters (1997). To begin the discussion of the hypothesized mechanism for the difference in results, I will describe the returns experienced by the subject in Thaler et al (1997) in more detail. The following statistics are informed by simulations run using the distributions specified by the authors. In the monthly condition in the experiment conducted by Thaler et al. (1997), subjects observed a 0% return on the safe asset approximately 7.5% of the time. Additionally, subjects would, on average, observe this 0% return on the safe asset on their 12th decision. Since returns were displayed in percentage terms out to 2 decimal places, a "0.00" would be a strong signal that the safe fund's return was bounded below by zero. Importantly, in the pre-randomized returns that were used for the "yearly" conditions, no subject experienced a return of 0 on the low-risk fund.

Although an explicit model will not be developed here, a general intuition is necessary to understand the motivations for this research. The naïve motivation for the experiment I conduct will be described hereafter. Subjects in the experiment conducted by Gneezy and Potters (1997), on the treatment level, did not change their allocations to the gamble over time; importantly, every subject had complete knowledge of the distributions of both "funds", and specifically, they knew that any resource that they did not commit to the gamble had, in essence a return of 0. On the other hand, in the experiment conducted by Thaler et al. (1997) none of the subjects were provided any explicit information about the funds that they were allocating between. However, as noted previously, the subjects in the monthly condition frequently experienced returns that would indicate that there was a lower bound on the returns on Fund A. If a subject were to observe a "0%" return out to an arbitrary precision, that would be a strong signal, close to complete information, that the returns of Fund A were bounded below by zero.

To once again summarize, the theory here is relatively simple. In the experiment conducted by Gneezy and Potters (1997), where subjects knew that they could not lose on the funds they did not allocate to the risky option, updating behavior was not observed. As was mentioned before, every treatment group besides the monthly condition in Thaler et al. (1997) displayed updating behavior and likely were not aware of the lower bound on the returns from Fund A. My hypothesized causal mechanism for the lack of updating in Gneezy and Potters (1997) and the monthly condition of Thaler et al. (1997) is the shared knowledge of a loss-free option. The hypothesis implies that when the knowledge of a loss-free option is provided to subjects who make their allocation decisions monthly, there will be no change in the learning behavior. However, the hypothesis also implies that the provision of information to subjects who make decisions that hold for a high number of periods will not change their allocations over the course of the experiment.

II. Experimental Design

I conduct an experiment using the experimental design of Thaler et al. (1997) as a guide. Seventy subjects were selected from Cornell University's undergraduate student population and were randomly assigned to one of four treatment groups, with approximately the same number of subjects in each treatment. Subjects were asked to imagine that they were the portfolio manager for a college, and that they would be tasked with allocating a percentage of the college's funds between two assets, Fund A and Fund B. The choice would involve entering a number 0-100 that would correspond to the percentage of the college's funds allocated to one asset, Fund A, and whatever was not allocated to the first fund was allocated to the second fund, Fund B. Subjects were paid for their participation in the experiment. As part of the recruitment process, subjects were informed that they would be paid between \$5 and \$15 depending on their performance in the experiment. What payment a subject received was determined by comparing their average total return to their treatment group., with total returns being calculated by an average weighted by the allocation between Funds A and B. Specifically, if a subject was in the top half of average total returns for their treatment group, they would receive \$15, and if they were in the bottom half, they would receive only \$5. This payment structure is similar that used by Thaler et al. (1997), so the incentives for participants to perform well should not be of any meaningful difference between my replication and the original experiment.

Once again, subjects were asked to imagine that they were the portfolio manager of a college's funds, and that they would have to allocate a percentage of their funds between Funds A and B multiple times. Importantly, subjects were not told how many decisions they would have to make, but they were informed, in all treatments, of the number of financial periods that their decisions would hold for. Subjects in two of the treatment groups were told that their allocations would hold for 40 financial periods, while subjects in the other two treatment groups were told their allocations would hold for only a single period. Subjects in all treatments experienced 200 periods, but since half of all subjects made decisions that held for 40 periods, those subjects only made 5 decisions while subjects whose decisions held for only one period made 200 decisions. After making an allocation to Fund A, the remaining funds were automatically allocated to Fund B. Subjects were then shown a bar graph which depicted the percentage return for Fund A and Fund B over the periods to which their previous allocation held. They were also shown the return of their overall portfolio, with the return of the overall portfolio being a weighted average of the returns of Funds A and B, the weight on each fund being determined by the subject's percentage allocation to the funds. Along with the bars indicating the returns, the return out to 2 decimal places was presented for each of the funds and the return of the overall portfolio.

The percentage returns on the assets or funds that subjects allocated their resources between were both drawn from normal distributions. The returns for Fund A were drawn from a normal distribution with a mean of .25 percent and a standard deviation of .177 percent, with an important caveat. If a realization from this distribution was negative, it was transformed to a 0. This means that the mean was greater than .25 while the standard deviation was less than .177. The returns for Fund B were drawn from a normal distribution with a mean of 1 percent and a standard deviation of 3.54 percent. There was no elimination or transformation of negative returns for Fund B. The distributions from which returns were drawn are the same as the distributions from which returns were drawn in Thaler et al. (1997). Importantly, this includes the transformation of negative returns to 0 for Fund A. For the subjects whose returns held for 40 financial periods, the return shown to the subject was the result of compounding of the returns for those 40 periods.

I will now describe the nature of the treatments to which the participants were assigned. Approximately half of subjects were assigned to a "Monthly" condition where their allocations between the funds would only hold for one period. The other half of subjects were assigned to a "Five-Yearly" condition where their allocations held and returns compounded over 40 periods. The subjects in the "Monthly" and the "Five Yearly" conditions were each divided into two groups, for a total of four separate groups. Half of the subjects in the Monthly conditions were informed of the lower bound on the returns of Fund A, and half of the subjects in the Five Yearly condition were informed of the lower bound on the returns of Fund A. The condition where subjects' allocations held for only one period and were provided information will be referred to as the *Monthly Information* condition, the condition where subjects' allocations held for 40 periods and were not provided information will be referred to as the *Five-Yearly Without Information* condition, and so on.

III. Results

The primary regression specification I use is the same specification as used by Thaler et al. (1997). This regression allows the analysis of the existence and magnitude of changes in subject allocations over time. It is a simple OLS regression framework, as shown below, and estimates on the four treatment groups separately:

$$(1)\ Y_{it} = \beta_0 + \beta_1 X_{it} + \epsilon_{it}$$

- Where Y_{it} is the allocation for subject i to Fund A in period t
- X_{it} is the trial number for subject i for trial t
- β_1 is the coefficient on the trial number X_{it}; β_1 will measure the average change in allocation to Fund A in percentage points

There are several assumptions that are made in (1) that I will now discuss. First, I assume that there are no important differences between the types of subjects that participated in each treatment group. This assumption is likely to be valid because of the random assignment to treatment groups. Additionally, since all participants were drawn from the same pool, an undergraduate student population, I do not expect there to be meaningful differences between the types of participants in any of the treatment groups. Another assumption made is that the realized returns on Fund A and Fund B are, on average, the same across periods. Because the returns from Fund A and Fund B were drawn from the same distributions, subjects did not experience meaningful differences in terms of either the order they experienced returns or in the average returns from the funds. The final important assumption made is that learning behavior, if it is observed, will be constant across the subjects' decisions. This is a somewhat tenuous assumption as, if subjects are assumed to be Bayesians, they will asymptotically approach what is their utility maximizing allocation. In other words, it is likely that subjects learned more about the distributions in their first decision than their last decision. In a later section, I will address this issue with an alternate specification.

For the first part of the discussion of results, the data will be examined using (1), the regression used by Thaler et al. (1997) in their experiment. The results from this regression are reported in Table 2. In comparing Table 2 and Table 1, the results of the replication seem to be remarkably similar to the results of Thaler et al. (1997). Subjects in the *Monthly Without Information* condition, as in the original experiment conducted by Thaler et al. (1997), did not change their allocations over the course of the experiment at a statistically significant level. This lack of a trend in allocations over time can be seen in Figure 1. The replication of the *Five-Yearly* condition showed that there was updating behavior over the course of the experiment as can be seen in Table 2, and this trend in allocations to Fund A over time can be seen visually in Figure 1. Once again, these results are consistent with the experiment conducted by Thaler et al. (1997). When a test of equality is performed, the coefficients on "Trial Number' for the *Monthly Without Information* and *Five-Yearly Without Information* conditions are not significantly different than their counterparts in my replication of Thaler et al. (1997). These tests are reported in Table 3.

I also perform a statistical analysis of the results from the *Information* treatments using (1), as can be seen in Table 2. A visualization of the average allocations to Fund A over time for subjects in the information conditions is reported in Figure 2. For the subjects in the Monthly *Information* condition, the coefficient on the "Trial Number" variable is negative indicating that over the course of the experiment, subjects increased the percentage of their resources allocated to Fund B. This is an unexpected result, and I will discuss this result further in later sections of this paper. I report statistics on the average initial and final allocations for subjects in the various conditions in Table 4. The initial allocation for *Monthly Without Information* condition is around 49 while the initial allocation of the regression for subjects in the *Monthly* Information condition is around 70. This indicates that on the first decision the subject made, the provision of information about Fund A induced them to allocate more to Fund A. This change in behavior is consistent with the phenomenon of ambiguity aversion (Ellsberg 1961), as I will address later. For the *Five-Yearly Information* condition, the point-estimate of the coefficient on Trial Number is negative, but I do not find statistical significance at any conventional level.

Clustering

One assumption that I did not mention above that applies to (1) is that of the robustness of the standard errors. The experiments conducted by Thaler et al. (1997) and Gneezy and Potters (1997) conducted regressions which did not account for the possible correlation of the errors between a subject's decisions. As the use of clustered standard errors was not common at the time of publication, this is to be expected. However, it has been shown that if a subject is asked to make multiple decisions in the same experimental environment, it is necessary to cluster the standard errors at the subject level (Kim 2020). Intuitively, the reason for this is that the decision of a subject made in period (t+1) is likely heavily determined by the decision that the subject made in period (t). Therefore, the errors for an individual subject's observations are likely to be strongly correlated. For this reason, I have included the clustered standard errors at the participant level in Panel B of Table 2 for the various conditions. As one can see, the coefficients on Trial Number that were significant without clustering, remain significant at a 5% level for all conditions besides the Monthly Information treatment. Since clustered standard errors do not change the point estimates of the coefficients, it is not correct to conclude that the information provision for subjects allocating monthly had no effect, rather, I lack the statistical power to reject the null hypothesis that there was no change in a subject's allocations over time. To ascertain whether or not the provision of information for subjects allocating monthly did definitively induce a change in allocations over time, a higher-powered study with more participants is needed.

Alternate Specification
In equation (2) I present a regression specification that seeks to capture differences in the rates of learning over time.

$$(2)\ Y_{it} = \beta_0 + \beta_1 X_{1it} + \beta_2 X_{2it} + \beta_3 X_{3it} + \beta_4 X_{4it} \ + \epsilon_{it}$$

Once again Y_{it} is the percentage allocation of a subject to Fund A. β_1 through β_4 are the coefficients on X_1 through X_4 with the coefficients representing the change in allocation since the initial allocation. The *X* variables are dummies that indicate the period-equivalent decision numbers, coded 1 or 0. So, subjects in the Monthly conditions had their 1^{st}, 41^{st}, 81^{st}, 121^{st}, and 161^{st} decisions included in the regression, while subjects in the Five-Yearly conditions had all their decisions included in the regression. To prevent collinearity, I do not assign a dummy to the first decision, so the estimate of β_0 should be viewed as the initial allocation. The reasoning for only including every 40^{th} decision for the subjects in the Monthly condition is twofold. First, the interpretation of the magnitude of the coefficients are the same across regressions, and secondly, it allows for an easier comparison of subject behavior across regression as their average allocations to Fund A can be evaluated at period-equivalent points. The results of these regressions are reported in Table 5. The reasoning for why this specification captures more nuance in subject behavior is simple. Imagine that instead of 5 decisions or 200 decisions, subjects instead made 10,000 decisions. It would be reasonable to suspect that even if subjects did exhibit learning behavior in the first few hundred decisions, they would likely arrive at a relatively stable desired allocation to Fund A. This would be true as well even if the allocation level oscillated around a certain value, as some learning models suggest. In this hypothetical experiment, the specification in (1) would not show significant learning behavior because there would be many decisions where the subject did not change their allocation.

Due to increase in the statistical power necessary for the results of the alternate specification to be significant, I cannot say definitively if learning behavior is different over the course of the experiment. However, from the point-estimates, it appears that majority of the change in allocations to Fund A that occurred in the *Five-Yearly Without Information* and *Monthly Information* conditions took place within the first few trials. This is an expected result, but due to lack of statistical power, I only find suggestive evidence that most of the observed learning behavior occurs in the first few decisions that subjects make in the *Monthly Information* and *Five Yearly Without Information* conditions.

IV. Discussion
One of the primary contributions of this research is to add to the body of experimental evidence that the phenomenon of myopic loss aversion exists, and

furthermore, that the findings of Thaler et al. (1997) do indeed hold up in other experimental settings. Although the experiment I conduct is modeled after the original experimental design, it is still notable that my results are consistent with the results of the original experiment. It is also of note that the coefficients on the regressions run on the replication treatments are still significant even when clustering at the participant level.

Ambiguity aversion was described early in the 20th century by John Maynard Keynes, but Ellsberg (1961) was one of the first papers to formalize the definition. To understand a specific example of ambiguity aversion, I will summarize one of the experiments that Ellsberg used as a canonical example of ambiguity aversion. Imagine that an individual is presented with 2 urns, both containing 100 balls. One of the urns is known to contain 50 red balls and 50 green balls, while the ratio of the color of the balls in the second urn is unknown. Subjects are presented with a lottery that gives a monetary reward if a red ball is picked from the urn and no winnings if a green ball is picked. If subjects are asked to choose which urn they would rather have the ball chosen from, they will pick the urn with the known ratio. There are several theories or explanations for this behavior, but for this paper, ambiguity aversion should generally be interpreted as a preference for the known distribution.

I observe evidence of the existence of ambiguity aversion in the *Monthly Info*rmation condition, as the initial allocation to the *Monthly Information* condition is significantly greater than the initial allocation to the *Monthly Without Information* condition. The initial allocation to Fund A is approximately 70% in the *Monthly Information* condition and approximately 50% in the *Monthly Without Information* condition, as can be seen in Table 4. In fact, as can be seen in Table 6, these values are significantly different from each other. As subjects have no information about either fund besides the knowledge about the lower bound in the information treatments, the increased allocation to the fund that subjects know more about is likely caused by ambiguity aversion, the preference for the known versus the unknown. This result on its own is not particularly of note. However, subjects in the *Five Yearly Information* condition did not allocate more initially than the subjects in the *Five Yearly Without Information* condition at any conventional level of statistical significance. This is suggestive evidence of subjects actively considering the number of periods they are told their investment decision will hold for. Furthermore, it suggests that knowledge of a loss-free option is not particularly valuable, or at the least is not incorporated into subject decision making processes, when the number of periods their allocation holds for is greater. In other words, subjects are aware of the effects of compounding the interest rate over those periods.

Even though the coefficient on Trial Number in the primary specification for the *Monthly Information* condition is not statistically significant when using clustered standard errors, the results are nevertheless suggestive that updating behavior may occur. Although I will not give a comprehensive explanation here, as such an explanation requires a more rigorous treatment of economic theory than considered in this paper, there are still some contributions that I can make. The point estimates of the final allocation to Fund A for the Monthly conditions, as reported in Table 4, have a difference of only 3 percentage points. Although this experiment is not equipped to evaluate these values at a degree of high precision, the point estimates suggest that they are reasonably similar to each other. This similarity may be a result of a phenomenon called bounded rationality, as described in Simon (1955). Bounded rationality, essentially, is the idea that individuals are satisfied with an outcome that is "good enough" and will not seek to maximize their utility beyond this level. Since the final allocations of the *Monthly Information* and *Monthly Without Information* condition are similar, near an allocation to Fund A of approximately 55, it might be the case that the environment in which subjects in these conditions participate makes it such this allocation decision is "good enough".

Although this speculation might be worthwhile, it is not the primary focus of this research, that focus being whether knowledge of certainty prevents the change of allocation between the two funds. I fail to provide a sufficient explanation for the lack of updating behavior in Gneezy and Potters (1997) and the monthly condition in Thaler et al. (1997). My hypothesis that updating behavior would not occur in *Monthly* Information condition seems to be false; however, I am unable to say this with certainty when clustering. Nevertheless, these findings indicate that knowledge of certainty might play a role in how agents update their decisions. Certainly, the question of how agents update their allocations in environments with a no-loss asset requires more study.

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Figure 1: Allocations to Fund A by Period for Subjects Without Information Provision

This figure presents that average allocation to Fund A in each period over the course of the experiment for subjects who were not provided explicit information about Fund A. The blue line corresponds to the average percent allocation to Fund A for subjects in the *Monthly Without Information* condition. The red line corresponds to the average percent allocation to Fund A for subjects in the *Five Yearly Without Information* condition.

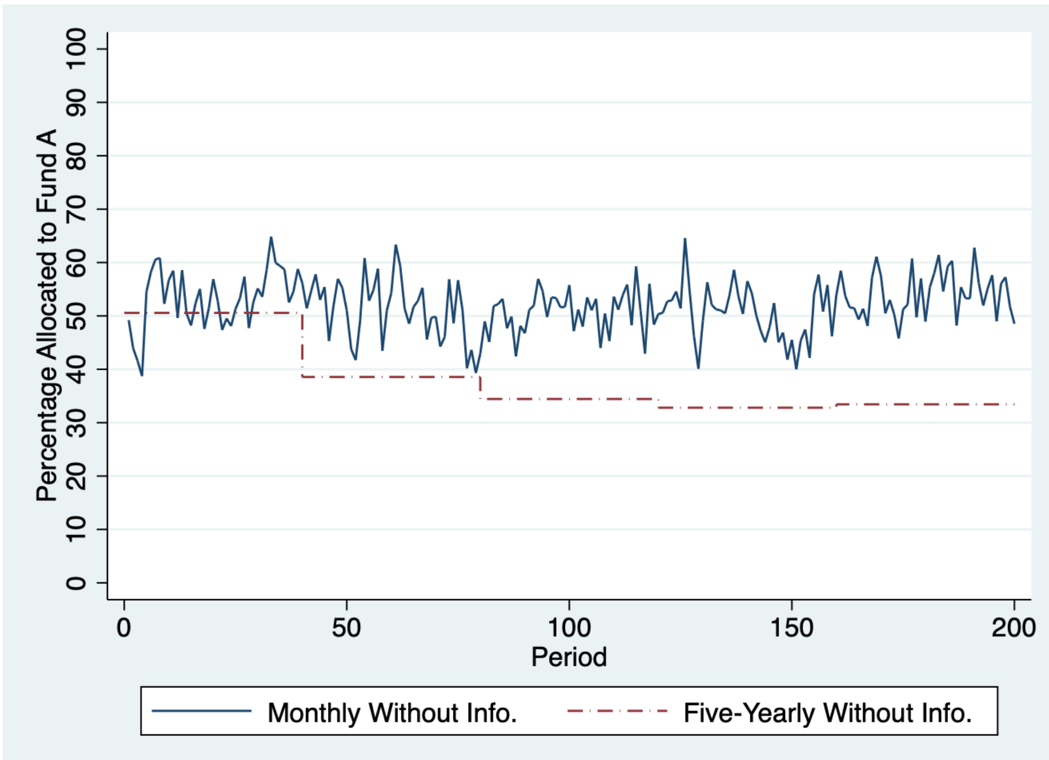

Figure 2: Allocations to Fund A by Period for Subjects With Information Provision

This figure presents that average allocation to Fund A in each period over the course of the experiment for subjects who were provided explicit information about Fund A, specifically the nature of the lower bound on Fund A's returns. The blue line corresponds to the average percent allocation to Fund A for subjects in the *Monthly Information* condition. The red line corresponds to the average percent allocation to Fund A for subjects in the *Five Yearly Information* condition.

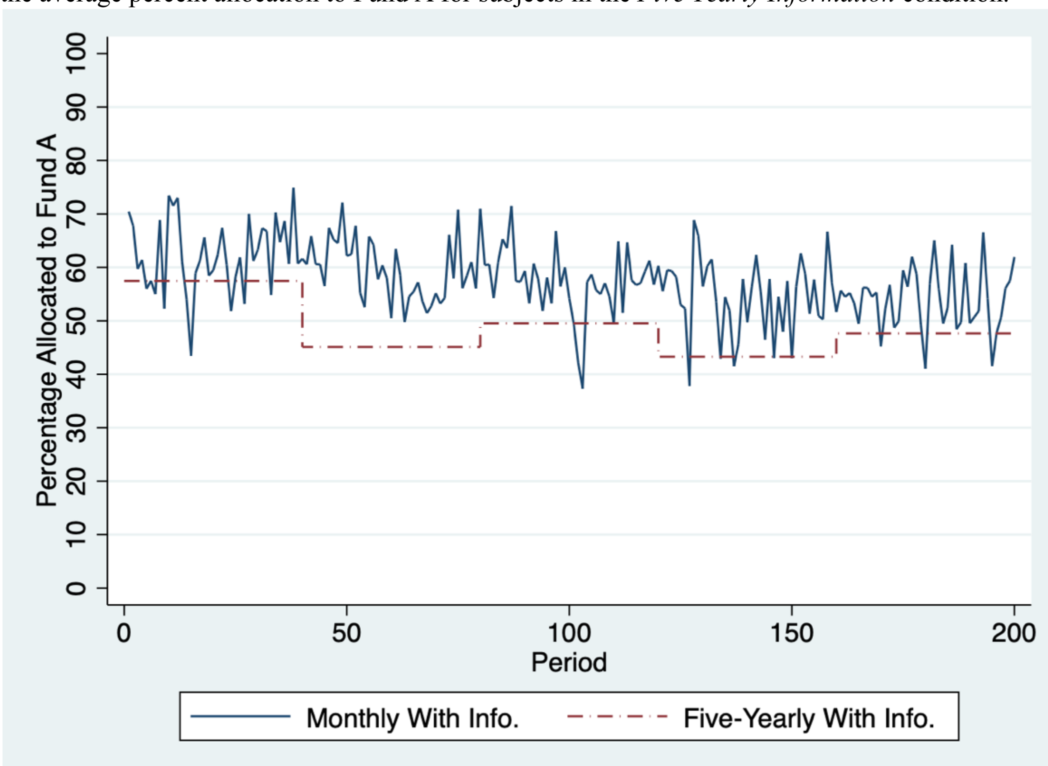

Table 1: Regressions Predicting Allocations to Fund A From Trial Number in Thaler et al. (1997)

The table below contains the results of Thaler et al. (1997)'s reported in Table II of their paper. The percentage of funds allocated to Fund A are regressed on the number which corresponds to the decision that subjects made in the experiment, and the coefficient on this trial number is reported for each of the treatment groups. The number of observations per treatment group are noted in the parentheses. *, *, and ** represent significance at the 10%, 5%, and 1% levels, respectively.

Variable		*B*	*SE B*
Monthly Condition (n = 4200)			
	Trial Number	-0.00	.01
	Intercept	55.9	.94
Yearly Condition (n = 550)		-0.71***	0.16
	Trial Number	46.2	2.31
	Intercept		
Five-Yearly Condition (n = 110)			
	Trial Number	-4.64***	1.52
	Intercept	48.1	5.04
Inflated monthly condition (n = 4200)			
	Trial Number	-.05***	.01
	Intercept	48.9	.93

Table 2: Regressions Predicting Allocations to Fund A From Trial Number in the Current Experiment

The regressions reported in the two panels below use the specification from (1), showing the estimated effect of the trial number on the percentage allocation to Fund A. The variable "Trial Number" refers to the number of the decision made a by a subject, not the allocation associated with a specific period. Panel A reports the estimates with unclustered standard errors while Panel B reports the estimates with standard errors clustered on the subject level. *, *, and ** represent significance at the 10%, 5%, and 1% levels, respectively.

Panel A

	Variable	*B*	SE *B*
Without Clustering	Monthly Without Information (n = 3800)		
	Trial Number	0.01	0.01
	Intercept	51.27	1.29
	Five-Yearly (n = 80)		
	Trial number	-4.00**	1.58
	Intercept	49.96	5.22
	Monthly Information (n = 3600)		
	Trial number	-0.03***	0.01
	Intercept	62.07	1.45
	Five-Yearly Information (n = 85)		
	Trial number	-2.15	2.06
	Intercept	55.05	6.85

Panel B

	Variable	*B*	S.E *B*
With Clustering	Monthly Without Information (n = 3800)		
	Trial Number	0.01	0.03
	Intercept	51.27	5.30
	Five-Yearly (n = 80)		
	Trial number	-4.00**	1.57
	Intercept	49.96	4.50
	Monthly Information (n = 3600)		
	Trial number	-0.03	0.03
	Intercept	62.07	5.24
	Five-Yearly Information (n = 85)		
	Trial number	-2.15	1.94
	Intercept	55.05	7.97

Table 3: Tests of Equality of Coefficients of Trial Number on Allocations

Presents tests of the equality of coefficients from regressions of the allocation to Fund A on trial number. The standard errors are included in the column labeled S.E, and they are clustered at the participant level. Panel A presents the tests of equality for coefficients of regressions on data from subjects with the same frequency but different information provision. Panel B presents the tests of equality for coefficients of regressions on data from subjects who had the same level of information provision but a different frequency at which they made allocations. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Panel A

	Without Information		Information		
Frequency	Coefficient on Trial Number	S.E	Coefficient on Trial Number	S.E	Test of Equality of Coefficients
Monthly	0.01	0.03	-0.03	0.03	1.65
Five-Yearly	-4.00**	1.57	-2.15	1.94	0.66

Panel B

	Monthly		Five-Yearly		
Information (Y/N)	Value	S.E	Value	S.E	Test of Equality of Coefficients
Y	-0.03	0.03	-2.15	1.94	0.70
N	0.01	0.03	-4.00**	1.57	5.33***

Table 4: Initial and Final Allocations to Fund A

This table presents the initial and final allocations to Fund A. Panel A shows the average allocations to Fund A in the first decision that subjects in each of the treatments made. Panel B shows the average allocations to Fund A in the final decision that subjects in each of the treatments made. Sample size is reported in the last column of both panels.

Panel A

Initial Allocation

Condition	Value	S.E	N
Monthly Without Information	49.21	3.71	19
Monthly With Information	70.44	4.04	18
Five-Yearly Without Information	50.56	3.59	16
Five-Yearly With Information	57.47	6.75	17

Panel B

Final Allocation

Condition	Value	S.E	N
Monthly Without Information	58.47	8.04	19
Monthly With Information	55.67	9.50	18
Five-Yearly Without Information	33.44	4.70	16
Five-Yearly With Information	47.65	6.30	17

Table 5: Estimating the effect of Trial Number on Allocation to Fund A with Dummy Variables

A report of the results of a regression run using equation (2), the alternate specification, as described in the text. The dependent variable in this case is the allocation to Fund A for the specified period. The number of observations for each regression are reported in parentheses next to the condition from which the data was collected. Columns (2) through Column (5) report the coefficients on dummy variables, coded 0 or 1. Standard errors are shown in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Condition	(1)	(2)	(3)	(4)	(5)
	β(0)	β(1)	β(2)	β(3)	β(4)
Monthly Without Information (n = 95)	49.21	2.21	-0.21	1.37	9.26
	(3.79)	(6.98)	(8.06)	(9.77)	(9.02)
Five-Yearly (n = 80)	50.56	-12.00	-16.13	-17.75	-17.13
	(3.68)	(4.37)	(7.60)	(9.07)	(5.76)
Monthly Information (n = 90)	70.44	-9.89	-10.00	-14.89	-14.78
	(4.14)	(6.83)	(7.60)	(9.23)	(10.20)
Five-Yearly Information (n = 85)	57.47	-12.35	-7.94	-14.18	-9.82
	(6.92)	(3.52)	(5.75)	(8.35)	(7.88)

Table 6: Reports of Two Sample T-Tests for Average Initial and Final Allocations

Reports tests of equality of the means for initial and final allocations. Panel A presents the t-tests for initial allocations between subjects with the same frequency but different information provision. Panel B presents the t-tests for initial allocations between subjects who had the same level of information provision but a different frequency at which they made allocations. Panels C and D present versions of Panels A and B, respectively, for final allocations. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Panel A

Initial Allocations					
	Without Information		Information		
Frequency	Value	S.E	Value	S.E	Test of Equality of Means
Monthly	49.21	3.71	70.44	4.04	3.87***
Five-Yearly	50.56	3.59	57.47	6.75	0.9

Panel B

Initial Allocations					
	Monthly		Five-Yearly		
Information (Y/N)	Value	S.E	Value	S.E	Test of Equality of Means
Y	70.44	4.04	57.47	6.75	1.65
N	49.21	3.71	50.56	3.59	0.26

Panel C

Final Allocations					
	Without Information		Information		
Frequency	Value	S.E	Value	S.E	Test of Equality of Means
Monthly	58.47	8.04	55.67	9.50	0.23
Five-Yearly	33.44	4.70	47.65	6.30	1.81*

Panel D

Final Allocations					
	Monthly		Five-Yearly		
Information (Y/N)	Value	S.E	Value	S.E	Test of Equality of Means
Y	55.67	9.50	47.65	6.30	0.70
N	58.47	8.04	33.44	4.70	2.69**