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Can Psychological Aggregation Manipulations Affect Portfolio Risk-Taking? Evidence from a Framed Field Experiment

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ABSTRACT

Consistent with the combination of loss aversion and mental accounting, previous laboratory experiments have found that subjects are more willing to invest in risky assets if they are given less frequent feedback about their returns, are shown their aggregated portfolio-level (rather than separate asset-by-asset) returns, or are shown long-horizon (rather than one-year) historical asset class return distributions. In this paper, we find that these manipulations do not significantly increase portfolio risk-taking when subjects are recruited from a broad swath of the population and have hundreds of dollars at stake which must be invested in real mutual funds over a one-year horizon. We do find that relative to when no historical return information is shown, subjects invest more in equities when they see either one-year or long-horizon historical return distributions, suggesting that many individual investors are unaware of how large the historical equity Sharpe ratio is.

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A remarkable series of laboratory experiments has found that subjects are more willing to invest in risky assets with positive expected returns if only aggregated returns are reported to them, rather than the individual component returns. Information aggregation along various dimensions produces this effect: reporting subjects' portfolio return over the last $n > 1$ periods rather than reporting single-period returns each period (Gneezy and Potters, 1997; Thaler et al., 1997; Gneezy, Kapteyn, and Potters, 2003; Barron and Erev, 2003; Langer and Weber, 2008; Bellemare et al., 2005; Haigh and List, 2006; Sutter, 2007; Fellner and Sutter, 2009), reporting subjects' portfolio-level returns rather than returns for each individual asset separately (Anagol and Gamble, 2009), or reporting historical long-horizon return distributions of asset classes rather than historical one-year return distributions of asset classes (Benartzi and Thaler, 1999).[1]

These results are consistent with investors suffering from myopic loss aversion (Benartzi and Thaler, 1995), which is the combination of loss aversion (Kahneman and Tversky, 1979) and mental accounting (Kahneman and Tversky, 1984; Thaler, 1985, 1990, 1999).[2] Aggregation frames encourage subjects to integrate multiple gamble outcomes into a single mental account. If these gambles are not perfectly correlated across time (or across assets) and the gambles have positive expected values, the resulting integration can lower the fraction of observations that are negative changes—i.e., losses.[3] Thus, integration makes the gambles appear more attractive to a loss-averse subject than if each gamble occupied its own separate mental account.

The strength and consistency of the experimental results constitute compelling evidence that myopic loss aversion is a real psychological phenomenon that responds to aggregation manipulations. In this paper, we consider a related but separate question: Can aggregation manipulations create large changes in portfolio risk-taking outside the laboratory?

Numerous authors have extrapolated from the existing experimental literature to suggest that the answer to this question is "yes." For example, Thaler et al. (1997) write, "Decisions made by employees covered by such [defined contribution pension] plans may vary considerably

[1] Guiso (2009) examines another aggregation manipulation that we do not test in this paper. He finds that asking subjects about their labor income risk before offering a hypothetical lottery makes them more likely to accept the lottery.

[2] Loss-averse agents derive utility and disutility directly from gains and losses, and the disutility of a loss is greater than the utility of a gain of equivalent magnitude. Agents engage in mental accounting when they evaluate outcomes within a subset of their wealth portfolio—the "mental account"—in isolation from outcomes outside the mental account.

[3] Aggregating gambles does not decrease the probability of an overall loss for all return distributions. But the loss probability does decrease, for example, when the aggregated gambles are drawn from the same normal distribution.

depending on how their investment opportunities are described and the manner and frequency with which they receive feedback on their returns." Gneezy and Potters (1997) observe that, "Manipulating the evaluation period of prospective clients could be a useful marketing strategy for fund managers." Haigh and List (2005) write that "institutions may have the ability to influence asset prices through changes in their information provisioning policies."

However, there is a substantial gap between the laboratory setting and the typical investment environment. Laboratory experimenters have their subjects' undivided attention, along with complete control over information flows during the experiment, whereas aggregation manipulations by a single institution in the field may have little power because of limited attention and interference from or interactions with background information flows. The laboratory experiments to date have been conducted over the course of one short session with small monetary stakes, artificial laboratory assets, and mostly[4] student subjects. The psychology of larger-stakes risk-taking outside the laboratory over many days, months, or years may differ. Investors may employ context-specific heuristics that mitigate their instinctual behavioral biases when dealing with familiar assets such as stocks, bonds, and mutual funds. Biases found among student subjects may not be present in the typical investor, who has more financial experience.

We narrow the gap between the laboratory and the field by conducting a framed field experiment (the terminology is that of Harrison and List, 2004). We recruited 597 subjects from the general U.S. adult population to participate in a year-long investment study. The monetary stakes were an order of magnitude higher than in previous laboratory experiments. Each subject allocated $325 among four real mutual funds that cover the U.S. equity, international equity, U.S. bond, and U.S. money market asset classes. Subjects were free to reallocate their portfolio throughout the year, just as if they were making real investments in these mutual funds. We paid each subject whatever the $325 would have been worth at the end of the year if the money had been invested according to his or her choices.

We test four aggregation manipulations which we randomly assigned to subjects.

[4] We know of three exceptions. Haigh and List (2006) use professional futures and options pit traders as laboratory subjects. Benartzi and Thaler (1999) include many non-student subjects in their studies. Anagol and Gamble (2010) recruit a broad subject population over the Internet, as we do, and tell subjects which real-world assets their artificial assets are designed to mimic.

The first manipulation varied how frequently subjects saw their returns by paying half of subjects to view their weekly returns once a week and paying the other half to view their biannual returns once every six months.

The second manipulation varied the level of detail subjects saw when they viewed their weekly or biannual returns. Half of subjects saw only their overall portfolio return over the last week or six months. The other half of subjects saw the return over the last week or six months of each individual asset they were holding. Because a screen available to all subjects showed the dollar value of each asset in their portfolio, subjects in the former group could, in theory, calculate their individual asset returns if they remembered the previous value of each asset. But we did not ourselves provide convenient access to these previous values, hindering this calculation. Similarly, subjects in the latter group could calculate their overall portfolio return from their individual asset returns, but we did not perform this calculation for them.

The third manipulation varied the historical returns information shown to subjects. We showed some subjects graphs depicting the distribution of real one-year returns for U.S. equities, international equities, U.S. bonds, and U.S. money markets from 1971 to 2007. Others were shown the distributions of real annualized five-year returns for the four asset classes over the same time period. We also gave some subjects no historical returns information at all in order to see whether allocations were affected by seeing any version of the returns graphs.

The fourth manipulation varied whether subjects who saw the historical returns graphs could also access information about the historical performance of mixed portfolios. Specifically, some subjects could only see historical return distributions of four "pure" portfolios, each invested 100% in one of the four asset classes offered. Other subjects could, via a Web interface, see return distributions of portfolios invested in whatever mix of asset classes they wished. The latter graphs might make more apparent the diversification benefits of holding multiple asset classes, thus encouraging greater investment in risky assets.

We find, contrary to the previous experimental literature, that none of the aggregation manipulations caused a significant increase in portfolio risk-taking. Seeing ongoing portfolio returns less frequently, seeing five-year instead of one-year historical return distributions, and having the ability to see historical returns of mixed portfolios do not affect the portfolio fraction allocated to equities. Seeing ongoing portfolio-level returns instead of ongoing asset-by-asset

returns *decreases* equity allocations initially—the opposite of Anagol and Gamble's (2009) result.

We identify subjects who are particularly loss averse and prone to mental accounting by offering subjects a one-time gamble. This gamble gave them an equal chance of adding $8 to or subtracting $5 from their up-front participation payment. Forty-seven percent of subjects rejected this gamble. Rabin (2000) and Barberis, Huang, and Thaler (2006) show that rejections of such small positive expected value gambles are difficult to explain without loss aversion and mental accounting. However, even within this subset of particularly myopically loss-averse subjects, aggregation does not significantly increase subjects' equity allocations.

We also find that relative to when no historical asset class return information is shown, subjects initially invest 11 to 12 percentage points more in equities when they see *either* one-year or five-year asset class return distributions. This suggests that many individual investors are unaware of how large the historical equity Sharpe ratio is. The effect of seeing return distributions is especially large among subjects who do not have a bachelor's degree.

The remainder of the paper proceeds as follows. Section I describes our experimental procedure. Section II presents the empirical results. Section III concludes.

I. Experimental procedure

A. Subject recruitment

We recruited subjects in late June and early July 2008 for a one-year investing experiment through the market research firm MarketTools. Figure 1 shows the number of subjects active in the experiment at each calendar date (the gray bars), as well as the level of the S&P 500 normalized by its July 2, 2007 value (the solid line). The S&P 500 was down only 18% from its October 2007 peak at the end of June 2008 when subject recruitment began; the market's precipitous fall did not commence until after the September 2008 bankruptcy of Lehman Brothers. Even though our experiment spanned the market collapse in the fall of 2008, all of our subjects' initial portfolio choices were made before it was known that we were in a bear market of historic proportions. We will discuss in Section II.C evidence that the market's decline prior to the experiment does not explain our null treatment effects.

We requested that our subjects be at least 25 years old and have an annual income of at least $35,000, so that it was more likely that they had some investable assets. All interaction with the subjects occurred through the Internet; we had no direct contact with them.

The initial invitation text introduced the faculty authors with our university affiliations in order to establish the credibility of the study. It then informed subjects that they would receive a $20 up-front participation fee for allocating $325 among four mutual funds. At the end of one year, we would pay them whatever their initial $325 portfolio was worth at that time, plus an additional amount for periodically checking their portfolio's return on the study website. The text concluded by telling the subjects that we expected the initial portfolio allocation to take thirty minutes to an hour, and that it would take no more than thirty minutes to an hour of additional time over the course of the next year to check their portfolio's return.

People interested in participating in the study clicked a link that took them to an informed consent page which described the task, the compensation scheme, and the expected time commitment again. The informed consent document also told subjects that they would periodically receive e-mails with a link that they could click to see their portfolio's return, and that we would pay them for clicking on these links.

Giving informed consent took subjects to a registration page where they supplied their name and contact information and chose a password. In order to prevent anybody from registering for the study more than once, we blocked any attempts to register multiple times from the same IP address. Upon registration, an e-mail was sent to each subject with a link to click on in order to activate his or her account.[5] The link then took them to a login screen.

We recruited 600 subjects, but three of them did not participate after registering. Therefore, our final sample consists of 597 subjects, whom we randomly assigned to one of eighteen experimental cells. Table 1 shows the distribution of our sample among the experimental cells. Table 2 groups the experimental cells in a different way to make clearer the comparisons we will be making in our analysis and the sample that is available for these comparisons. We will describe each experimental condition in further detail in Sections I.C and I.F.

[5] Using an e-mailed activation link ensured that we had an active e-mail account to which we could send the returns-checking links.

B. Opening instructions screen

After logging in, subjects received a fuller description of the study instructions. Figure 2 shows the screen that subjects in one of our experimental cells saw when they logged in for the first time. Subjects in other cells saw variations of this screen. The instructions reiterated the nature of the portfolio allocation task and the compensation scheme, and informed subjects that they could reallocate their portfolio any time during the year by logging into their account on the website. Subjects were also told about the inducement to view their ongoing returns, as well as the content and frequency of the ongoing returns they would be paid to see. In some conditions, subjects were introduced to the historical returns graphing tool.

C. Historical returns graph treatments

For 80% of our subjects, the bottom of the initial experimental screen (such as the one shown in Figure 2) introduced a graphing tool that was intended to help them understand the historical real return distributions of four asset classes: U.S. equities, international equities, U.S. bonds, and U.S. money markets. The remaining 20% of subjects did not see the graphing tool and did not receive any alternative information on historical returns. The graphs generated by the tool are modeled after those in Benartzi and Thaler (1999). Returns for an asset class during the historical sample period are sorted from lowest to highest and displayed as a bar chart. The lowest return is the leftmost bar, and the highest return is the rightmost bar. The median return is also highlighted and labeled with its value.[6] We used the S&P 500, MSCI EAFE, Lehman Brothers U.S. Aggregate Bond Index, and 30-day U.S. Treasury bill as our asset class proxies. Because the MSCI EAFE series starts in 1970, we cannot use returns prior to 1970 while maintaining identical sample periods for all asset classes. The most recent year of returns available at the start of the experiment was 2007. In order for each return series to have a unique median, we used the period from 1971 to 2007—which has an odd number of years—for all our asset classes.[7] Subjects who had the graphing tool available to them were required to click

[6] A programming error caused the bar immediately to the left of the median return to be highlighted instead for the first six months of the experiment, even though the correct median return number was displayed in the graph's caption. The paper's figures show the graphs with the shifted highlighting. The discrepancy was not visually apparent except in the one-year U.S. equities graph, where the median return was 10.61% but the highlighted bar corresponded to a 7.38% return.

[7] In addition, the Lehman Brothers index starts in 1976. We construct our own aggregate bond market index returns from 1971 to 1975 by weighting the returns of Ibbotson's long-term corporate bond, intermediate Treasury, and

through an animation that explained how to interpret and use the graph before they could proceed to the next part of the study. This animation could also be replayed in later screens where the graphing tool was shown.

The graphs varied across treatments along two dimensions. The first dimension was whether one-year return distributions or five-year annualized return distributions were shown. We used overlapping periods for the five-year distributions, so there were 33 bars shown on the five-year graph. The second dimension was whether subjects could see only the historical return distributions of four "pure" portfolios—each of which is invested 100% in a single asset class—or could see the return distribution of any asset class mix they wanted. Figure 3 shows an example of a graph where one-year returns are being shown and only four pure portfolio distributions are accessible. Figure 4 shows an example of a graph where five-year returns are being shown and any portfolio's return distribution can be seen. These two figures also demonstrate how the graphing tool allowed subjects to compare the return distributions of two different portfolios side-by-side.

D. Initial portfolio allocation

Subjects made their asset allocations by specifying portfolio percentages to be invested in each investment option. For the 80% of subjects who had access to the graphing tool, this choice was made after they saw the initial instructions screen and clicked through the animated explanation of the graphing tool. For the 20% of our subjects who did not see any historical returns graphs, the input boxes for the initial portfolio allocation were below the experimental instructions on the first screen.

Subjects could choose among four index funds offered by Northern Funds: the U.S. Stock Index Fund, the International Equity Index Fund, the Bond Index Fund, and the Money Market Fund.[8] We provided links to each fund's prospectus. We also informed subjects that the International Equity Index Fund charges a 2% redemption fee on the sale of shares held for less

long-term Treasury indexes by the total amount of each type of issue outstanding (as reported by the U.S. Treasury) at the end of the prior year.

[8] We chose Northern Funds because it was the largest fund family that offered U.S. equity index funds, international equity index funds, bond index funds, a money market fund, did not charge sales loads, did not impose redemption fees on non-international funds, and did not impose frequent trading restrictions.

than thirty days.[9] For subjects who were shown the historical returns graphs, the graphing tool remained accessible on the same screen in which the portfolio allocation was entered in order to aid their portfolio decision. Figure 5 shows this screen for one of our experimental conditions. Subjects could take as long as they wanted to make their portfolio decision. We did not (and could not) prevent subjects from consulting sources of information available outside of our website.

E. Post-allocation questionnaire

After subjects submitted their initial allocation, they completed a post-allocation questionnaire that elicited information on demographics, self-assessed investment knowledge, self-assessed confidence about their portfolio allocation, and time preference. As noted earlier, we also offered subjects a gamble with a 50% chance of winning $8 and a 50% chance of losing $5. The outcome of the gamble depended on whether the high temperature at San Francisco Airport on a future date, as reported on the National Weather Service website, was an odd or even integer. We applied the gains and losses from this gamble to the $20 participation fee. Expected utility maximizers with remotely reasonable risk aversion over large-stakes gambles should always accept such a small-stakes, positive-expected-value gamble (Rabin, 2000; Barberis, Huang, and Thaler, 2006). Therefore, subjects who refuse the gamble are particularly likely to be loss averse and prone to engage in mental accounting.

Upon finishing the questionnaire, subjects were taken to a page that showed their current investment allocation and total balance (see Figure 6). At this point, subjects could log out. On subsequent logins to the site that were not initiated by clicking an e-mailed link (the e-mails are described in Section I.F), subjects would see this portfolio status page first.

F. Ongoing returns viewing treatments

During the one-year duration of the experiment, half of subjects received e-mails once a week with a link they could click to view their previous week's return. These e-mails were sent on Saturdays, starting at the end of the subject's first full calendar week of participation. If they clicked the link within a week of receiving the e-mail, we added $1 to their final payment. Thus,

[9] We follow a first-in-first-out (FIFO) convention for determining which shares will incur the redemption fee, as real-life mutual funds do.

if they clicked all of the e-mailed links they received during the one-year study, they would earn an additional $52. The other half of subjects received e-mails once every 26 weeks with a link they could click to view their prior six-month return. The dates these biannual e-mails were sent coincided with when these subjects would have otherwise received their 26th and 52nd e-mails if they had been assigned to receive weekly e-mails. If subjects receiving biannual e-mails clicked the link within a week of receiving the e-mail, we added $20 to their final payment.[10] We offered only $20 per viewing for this group because we anticipated that subjects receiving weekly e-mails would not click on every e-mailed link, and we wanted to equalize average return-viewing payments across treatments based on our best guess of treatment compliance.

Within each of the above two treatments, we varied the level of detail subjects saw when they clicked on the e-mailed link. Half of subjects saw a screen like that in Figure 7, which showed the return of each individual asset they held. The other half of subjects saw a screen like that in Figure 8, which showed only the overall return of their portfolio.[11] These return screens were only accessible via the e-mailed link (i.e., they could not be reached by following links within the study website). If a link in a given e-mail had already been clicked, clicking it again later would not lead to the return screen; this was to ensure that subjects receiving biannual e-mails did not see the returns screen more frequently that once every six months.

G. Treatment of interest, dividends, and trades

Dividends and interest were automatically reinvested in the fund that paid them.[12] All subjects were free to reallocate their portfolio at any time during the year by logging into their account and clicking a button on the portfolio status page that took them to a reallocation screen.

[10] If an e-mail was sent on day *t*, the link reported returns through day *t*, even if the link was not clicked until day *t* + *n*.

[11] When ongoing returns were reported asset by asset, if the e-mail was sent on day *t*, only assets held on day *t* were included in the returns list. Returns on assets completely liquidated prior to day *t* were not reported. If a subject previously held no position in an asset but established a position sometime between e-mail send dates, the asset return reported was for the full period between e-mails (one week or six months) and did not adjust for the fact that the asset was held for only part of the time between e-mails.

[12] We used Yahoo! Finance for our dividend and price data. On July 1, 2008, Yahoo! erroneously reported a money market fund dividend of 28.8 cents per dollar invested, which was deposited into 339 of our subjects' accounts. The mean excess windfall was 4.5% of portfolio value. After the market close on July 31, 2008, we sent an e-mail to the affected subjects informing them of the error and (if applicable) how it had affected the July 5 weekly return reported to them. We let them keep the windfall but reallocated it (at the same time the e-mail was sent) in accordance with the subjects' initially chosen asset allocation. This reallocation raised average equity allocations by 1.0 percentage points among subjects receiving weekly e-mails and 2.2 percentage points among subjects receiving biannual e-mails.

The reallocation screen (see Figure 9) showed the graphing tool relevant for the subject's experimental condition[13], links to prospectuses, the current percentage allocations across the four mutual funds, and a note about the international fund's redemption fee. Four input boxes allowed subjects to specify what their new portfolio allocation should be. Trades were executed at the next close of the U.S. markets and could be cancelled by the subject any time up to then.

H. Exit questionnaire

At the end of the one-year investment period, we administered an exit questionnaire to subjects. We will use in our analysis the questions that elicited objective measures of financial literacy, beliefs about stock market return autocorrelations, and the effect study participation had on subjects' attention to market fluctuations. Of the 597 subjects, 569 (95%) completed the exit questionnaire.

II. Empirical results

A. Subject characteristics

Table 3 displays demographic and financial summary statistics on our subjects, which were collected in the questionnaire administered immediately after the initial portfolio allocation. Men slightly outnumber women, and the young are slightly overrepresented in our sample—33% of subjects are 35 or younger—although all ages have substantial representation. Our subjects are relatively well-educated, with 56% reporting holding a bachelor's degree or higher. The high average level of education is perhaps due to our request for subjects with annual incomes above $35,000; only 5% of subjects report an income less than that threshold, and the median subject reports an income between $50,001 and $75,000. The median subject reports total bank, brokerage, and retirement account assets of about $75,000, and 29% of our sample reports assets in excess of $100,000. Only 20% of our sample reports holding no stocks whatsoever in their personal portfolio.

Table 4 shows measures of our subjects' financial literacy. The subjects' self-assessments were collected in the post-allocation questionnaire at the beginning of the experiment, and the measures of objective knowledge were collected in the exit questionnaire administered at the end

[13] The graphs shown to a given subject remained constant throughout the experimental period. That is, they were not updated in real time to reflect new returns that had been realized since the start of the experiment.

of the one-year experimental period. The median subject considers himself a "somewhat knowledgeable" investor, "somewhat confident" that the portfolio decision was right for him, and "somewhat likely" to change his portfolio decision if he consulted a professional investment advisor. However, relative to the typical American, the subjects are very knowledgeable about basic financial concepts. Around 90% of subjects understand that $100 in a savings account yielding 2% interest per year would be worth more than $102 at the end of five years, that a stock mutual fund is safer than a single company's stock, and that stock returns fluctuate more than bond and savings account returns. In contrast, Lusardi and Mitchell (2009) report that American Life Panel respondents, who are drawn from a nationwide adult population, correctly answered the compound interest question only 69% of the time, the diversification question only 71% of the time, and the return fluctuation question 88% of the time. Although we doubt the relevance of the following scenario, it is possible that our subjects were able to answer these questions well at the end of the experimental period *because* of what they learned by participating in our experiment.

With regards to market return serial correlation, 55% of subjects believe that a 10% rise in the market in one month should not change their prediction of the subsequent month's return. Similarly, 55% believe that a 10% fall in the market in one month tells them nothing about the subsequent month's return. Among those who do not believe that the market follows a random walk, those who believe in positive serial correlation outnumber those who believe in negative serial correlation by a factor of three.

Since the experimental setup was simple (from the perspective of an individual subject) and the assets were passively managed funds in familiar asset classes, subjects did not necessarily need a long time to make a considered decision. The median subject who received no historical returns information took 14 minutes between login and submission of the initial portfolio allocation, as did the median subject who was only given the historical returns distributions of portfolios invested 100% in a single asset class. The median subject who was able to see the historical returns distribution of any portfolio mix took 13 minutes.[14]

[14] We report medians because of outliers for whom the time between initial login and initial portfolio submission was extremely long. These subjects likely made their allocation over the course of more than one sitting.

B. Average asset allocations

The average portfolio percentage subjects held in each asset class at the beginning of the experiment, halfway through their investment period, and at the end of their investment period are found in Table 5. For subjects who received weekly e-mails, equity share at the halfway point is measured eight days after they receive their 26th e-mailed returns-checking link. For subjects who received biannual e-mails, equity share at the halfway point is measured eight days after they receive their first returns-checking link.[15]

Subjects initially allocated 65.7% of their portfolio to equities—with 34.8% invested in international equities and 30.9% invested in domestic equities—18.6% to bonds, and 15.8% to money markets.[16] Subjects initially held positive amounts in 3.66 asset classes out of 4, on average. The average portfolio share invested in equities fell to 57.2% at the midpoint of the investment period, reflecting steeply dropping stock prices, and then recovered along with the stock market to 59.4% by the end of the investment period.

C. Effect of ongoing return presentation on equity shares

Table 6 shows that our periodic e-mails to subjects were successful at creating significant variation in the frequency with which they visited the study website and viewed their returns. During the one-year investment period, subjects who received weekly e-mails logged into the website 60.7 times on average, versus only 18.2 times for subjects who received biannual e-mails. Under the weekly e-mail treatment, 45.3 of those 60.7 logins occurred because subjects clicked on an e-mailed link to view the screen with their ongoing returns. Thus, compliance with the link-clicking requests was high; 87.2% of weekly links sent were clicked within a week of receipt. In the biannual e-mail treatment, subjects clicked 73.8% of links sent, so they saw the returns screen an average of 1.5 times. Subjects in both treatments logged in about 16 times on average when not prompted by an e-mail.

[15] By measuring allocations at this later point, we capture the allocations of subjects receiving both weekly and biannual e-mails right after they have been induced to see their returns on the website via an e-mailed link. It may be particularly convenient to reallocate one's experimental portfolio right after clicking on the e-mailed link. Therefore, biannual subjects have had a chance to adjust their portfolios in response to market movements and the reporting regime via the same convenient channel weekly subjects had had available each week for the prior six months.

[16] The relatively high allocation to international equities may have been due to this asset class's strong performance in the time immediately preceding the experiment. The most recent one-year before-tax return reported in the fund prospectus was 25.76% for the international index fund, versus only 15.56% for the domestic equity index fund.

The extra return viewings by subjects who received weekly e-mails did not merely crowd out or coincide with return viewing that they would have engaged in anyway. In the exit questionnaire, we asked subjects, "Did participating in this study make you see the ups and downs of the market more often than you otherwise would have?" Subjects could respond that participation made them see the fluctuations "more often," "less often," or that it had "no effect." In the weekly e-mail treatment, 79% of subjects reported that participation made them see the ups and downs of the market more often, versus 57% of subjects in the biannual e-mail treatment ($p < 0.01$). Only 1% of subjects in the weekly e-mail treatment and 2% of subjects in the biannual e-mail treatment reported that the study caused them to see market fluctuations less often.

In Table 7, we analyze the impact of the ongoing return presentation treatments on the total fraction of the portfolio invested in equities. The table reports coefficients from OLS regressions where the dependent variable is the total fraction of the portfolio invested in equities at the beginning of the experiment, halfway through the subject's investment period, or the end of the subject's investment period. All regressions control for a dummy for being sent biannual (instead of the weekly) e-mails and a dummy for being shown ongoing portfolio-level (instead of the asset-by-asset) returns.

We look first at the equity share of subjects' initial allocations (column 1). We find that being sent biannual rather than weekly e-mails raises the initial allocation to equities by only 0.6 percentage points, an increase not significantly different from zero. The point estimate is two orders of magnitude smaller than the 28.7 percentage point increase Thaler et al. (1997) find when subjects are shown yearly ongoing returns rather than monthly ongoing returns.[17] We can reject at the 95% confidence level the hypothesis that the increase is more than 3.7 percentage points, and we cannot reject the hypothesis that being sent e-mails biannually instead of weekly decreases equity shares by as much as 2.5 percentage points.

We also find that telling subjects that they would see ongoing returns consolidated at the portfolio level rather than separately by each asset *decreases* equity investment by a statistically significant 5.0 percentage points. Recall that Anagol and Gamble (2009) find that a similar manipulation in the laboratory significantly *increases* portfolio risk-taking.

[17] It is difficult to compare our treatment effect magnitudes with those of Gneezy and Potters (1997), Gneezy, Kapteyn, and Potters (2003), Bellemare et al. (2005), and Haigh and List (2006), since they only offer subjects assets with binary payoffs.

The second column of Table 7 adds interactions between the treatment dummies and a dummy variable for rejecting the equal chance of winning $8 or losing $5. Subjects who rejected this small 50-50 gamble have demonstrated that they are particularly loss averse and prone to narrow mental accounting, so they may be more susceptible to aggregation manipulations. The point estimate of the biannual e-mail treatment effect is 2.3 percentage points higher among gamble rejecters than gamble accepters, but this difference is not significant. The overall biannual e-mail effect among gamble rejecters, –0.4 + 2.3 = 1.9 percentage points, is not significantly different from zero. Contrary to the prediction of myopic loss aversion, gamble rejecters respond 0.7 percentage points more negatively to portfolio-level return reporting than gamble accepters, but this difference too is not significant.

The subjects in Gneezy and Potters (1997), Gneezy, Kapteyn, and Potters (2003), Bellemare et al. (2005), and Haigh and List (2006) did not need to first experience disaggregated ongoing return disclosure before reducing their portfolio risk. Instead, they reduced their demand for risky assets starting in the very first period of the experiments, indicating that they *prospectively* anticipated the disutility from disaggregated ongoing return disclosure.[18] It is nevertheless possible that our subjects initially did not realize how disaggregated ongoing return disclosure would affect their utility, but they gradually learned as they became exposed to these risks. This would lead to a progressive relative decrease in the disaggregated groups' portfolio risk. Alternatively, subjects may not have initially responded to how returns would be reported to them going forward because they did not read the experimental instructions carefully enough, so they were not aware of the reporting regime at first.

We test these stories by using the equity share halfway into the experimental period as the dependent variable in the third and fourth columns of Table 7, and equity share at the end of the experiment as the dependent variable in the fifth and sixth columns. Like in Table 5, equity share at the halfway point is measured eight days after subjects receiving weekly e-mails got their 26th e-mailed returns-checking link, and eight days after subjects receiving biannual e-mails got their first returns-checking link.

[18] It is not possible to make a similar judgment about the subjects in Thaler et al. (1997) and Anagol and Gamble (2009). Thaler et al. (1997) do not tell the subjects the return distributions of the experimental assets; subjects need to play numerous rounds in order to estimate these distributions themselves. Anagol and Gamble (2009) make subjects play practice rounds before starting data collection, and they do not report the choices in the practice rounds.

The coefficient estimates in the third and fifth columns indicate that throughout the investment period, reporting ongoing returns on an aggregated basis did not significantly increase portfolio risk-taking. The point estimate of the biannual e-mail treatment effect actually shrinks slightly to 0.5 percentage points at the halfway point, and it flips sign to –1.4 percentage points at the end of the experiment. The portfolio-level return reporting treatment effect also attenuates to an insignificant –2.8 percentage points at the halfway point and an insignificant –3.8 percentage points at the end of the experiment. The fourth and sixth columns show that none of the treatment interactions with the dummy for turning down the small gamble are significant at the halfway point or at the end of the experiment.

Could our null effects be driven by some subjects believing that the expected return of equities is negative due to the market's drop prior to the start of the experiment? Such subjects should allocate nothing to equities regardless of the aggregation regime,[19] attenuating any aggregation treatment effect that would be detectable in times when nearly all subjects believe the equity premium is positive. We test this possibility by running regressions (not shown in the tables) of initial equity share on the treatment dummies and their interactions with a dummy for a subject believing in market momentum. We classify a subject as believing in market momentum if he believes that a 10% increase in the market in one month should increase one's forecast of the market's return next month and that a 10% decrease in the market in one month should decrease one's forecast of the market's return next month (see Table 4). Contrary to the hypothesis that our treatment effects are attenuated by the 18% market drop prior to the experiment, we find no significantly positive aggregation treatment effects among those who do not believe in market momentum (and thus were unlikely to forecast a negative equity premium due to the pre-experiment market decline). In addition, the treatment interactions with the momentum belief dummy are not significant.

D. Effect of historical returns presentation on equity shares

Table 8 reports the results of regressions estimating the effect that the historical returns graphs had on the portfolio fraction invested in equities. We see in the first column that simply viewing any historical returns graph significantly raises the initial equity share by 11 to 12

[19] The same logic that causes some gambles with positive expected returns to appear more attractive under aggregation causes some gambles with negative expected returns to appear less attractive under aggregation.

percentage points relative to not viewing a historical returns graph. However, it does not appear to matter whether the distributions of one-year returns or five-year annualized returns are presented. In fact—contrary to the increases in equity allocations ranging from 19 to 41 percentage points found by Benartzi and Thaler (1999) when their subjects were shown simulated 30-year return distributions rather than one-year return distributions—our subjects who saw the five-year returns initially allocated *less* to equities than subjects who saw the one-year graph, although the difference is not statistically significant.[20] Nor does it seem to matter whether subjects were able to see the return distributions of any mix of asset classes instead of only portfolios invested entirely in a single asset class. Being able to see the mixed asset class distributions is actually associated with a slightly lower initial allocation to equities, although the –0.4 percentage point effect is small and not significant.

The second column shows that there are no significant interactions of the graph-viewing effects with whether the subject turned down the win $8/lose $5 gamble. Gamble accepters who saw the five-year graphs initially allocated a statistically insignificant 13.9 – 10.2 = 3.7 percentage points less to equities than gamble accepters who saw the one-year graph. Gamble rejecters who saw the five-year graphs initially allocated a statistically insignificant 10.2 + 2.2 – (13.9 – 3.1) = 1.6 percentage points more to equities than gamble accepters who saw the one-year graph.

These results do not change when we study asset allocation at the mid-point or at the end of the experiment. The last four columns of Table 8 report regressions in which the dependent variable is total equity share 27 weeks into experimental participation or at the end of the experiment. Although simply having seen historical returns graphs continued to raise equity share by about 9 percentage points through the remainder of the experiment, it again does not matter whether one-year or five-year return distributions were shown. Those seeing five-year graphs held 0.9 percentage points more in equities than those seeing one-year graphs at the halfway mark, and 1.5 percentage points less at the end of the experiment, but the differences are insignificant. Splitting the sample between small gamble accepters and rejecters reveals no

[20] Although using simulated 30-year returns in the long-horizon condition, as Benartzi and Thaler (1999) did, produces a stark contrast against one-year returns, simulated returns are difficult to explain to ordinary investors and are thus less likely to be employed in a real-world educational intervention. Reasonable five-year distributions can be computed from our 37-year historical sample period without resorting to simulation.

significant treatment effect differences between those who are more or less prone to myopic loss aversion.

Who is most affected by seeing the returns graphs? Table 9 shows estimates of graph treatment effect interactions with nine subject characteristics associated with greater financial sophistication: being more than 45 years old, having a bachelor's degree, having an annual income above $75,000, having over $75,000 in financial assets, having over 25% of one's non-experimental financial assets invested in stocks, answering the compound interest question correctly, answering the diversification question correctly, answering the asset return volatility question correctly, and believing that the stock market follows a random walk. Almost all of the interactions are negative as expected, indicating that the graphs had a smaller effect on initial equity allocations among the more sophisticated. However, only the interactions with having a bachelor's degree are consistently significant. Those without a bachelor's degree increased their initial equity share by 17 or 18 percentage points when they saw the graphs. Those with a bachelor's degree increase their initial equity share by only 7 or 8 percentage points. We also see in the first column that subjects older than 45 react significantly less to the five-year graph. The age interaction with the one-year graph treatment effect is negative as well, but not significantly different from zero. (On the other hand, it is not statistically distinguishable from the age interaction with the 5-year graph treatment either.)[21,22]

Table 10 shows how the graphs affected subjects' confidence in their investment decisions. The coefficients are from ordered probit regressions where the dependent variables are subjects' self-reported confidence in their investment decision, likelihood of changing their decision if they consulted with a professional investment advisor, and investment knowledge.[23] Choi, Laibson, and Madrian (2010) find that answers to these questions are correlated with

[21] Subjects cannot allocate more than 100% to equities, and when no graphs are shown, more sophisticated subjects tend to allocate more to equities than unsophisticated subjects. One might thus suspect that the negative interactions are driven by the fact that the sophisticated have less room to increase their equity share in response to the graphs. However, the results are similar when estimated using a tobit with left-censoring at 0% and right-censoring at 100%.

[22] We have also checked to see if there is an interaction between these sophistication measures and the ongoing return reporting treatment effects on initial equity shares. The only interaction significant at the 5% level is between having a bachelor's degree and the ongoing returns e-mail frequency; the more educated increase their equity share by 7.3 percentage points more than the less educated when e-mails are sent biannually instead of weekly, resulting in an overall treatment effect of 3.8 percentage points for the more educated that is significantly different from zero at the 10% level. However, this interaction attenuates to 3.6 percentage points and is no longer significant ($t = 0.83$) halfway through the investment period, and the overall treatment effect at six months for the more educated is 1.8 percentage points and insignificant ($t = 0.64$).

[23] See Table 3 for the exact wording of the questions used to elicit subjects' confidence and the distribution of answers to the questions.

higher-quality portfolio choices in the form of people choosing lower-fee S&P 500 index funds. The point estimates indicate that the graphs made subjects more confident in their decision, less likely to change their decision if they consulted with a professional advisor, and more knowledgeable as investors. However, the only statistically significant coefficient is the effect of the one-year graph on confidence in the investment decision.

E. Effects on trading frequency

In this subsection, we examine whether the aggregation treatments affected the tendency to trade during the one-year course of the experiment. The median number of days on which a subject made a reallocation is 2, and the average is 4.6.

We use the total number days on which a subject made a reallocation as the dependent variable in Table 11. In the first column, we control for all the treatment dummies. We find that subjects who were sent biannual e-mails to check their returns traded 4.3 fewer times than those who were sent weekly e-mails, a difference that is significant at the 1% level. Much of this effect is probably due to the fact that clicking on the e-mailed link is a convenient way to access the website, so the (already small) effort costs of entering a trade order are reduced for those who receive this link every week. However, simply seeing one's return may also stimulate trading. Evidence for this mechanism comes from the fact that holding the number of e-mail links sent constant, seeing the ongoing returns of each individual asset rather than overall portfolio returns causes subjects to trade an additional 2.0 times, a difference that is also significant at the 1% level.

Recall that 80% of subjects had access to the historical returns graphs on the screen in which they entered trade orders. Although these graphs had a large effect on initial equity shares (Table 8), the regression in the first column of Table 11 shows that they did not significantly affect trading frequency over the course of the experiment.

In the second column of Table 11, we add controls for a dummy for turning down the win $8/lose $5 gamble and an additional variable measuring subjects' one-week discount rate. The discount rate was elicited by asking subjects a series of questions of the following form, where X took on values of $10.10, $11, $12, $13, $14, and $15:

> Suppose an absolutely trustworthy person offered to give you either $10 today or **$X** in one week. Assume it's no more work for you to receive the money now versus later. Which would you prefer?

These questions, which we asked immediately following the initial portfolio allocation, were hypothetical: responses did not affect subjects' payments. We compute a subject's discount rate as the maximum weekly interest rate at which the subject chooses the earlier payment.[24] Barberis and Xiong (2009) predict that loss-averse mental accounters who derive direct utility from realizing gains and losses will be less likely to trade when they hold paper losses. Barberis and Xiong (2010) predict that mental accounters who are especially impatient will be less likely to trade when they hold paper losses because they gain the most from deferring the jolt of negative utility that comes from realizing a loss. At the end of our experiment, 95% of our subjects had portfolio values less than their initial $325 balance due to the bear market. The second column of Table 11 shows no significant evidence that especially myopically loss-averse, impatient subjects traded less frequently in a time period when most asset classes experienced large declines.

III. Conclusion

Many behaviors in the field are difficult to explain unless loss aversion and/or mental accounting are important determinants of economic choices. Such behaviors include aversion to small-stakes risks (Rabin, 2000; Rabin and Thaler, 2001), the tendency to sell stocks with paper gains and hold stocks with paper losses (Shefrin and Statman, 1985; Odean, 1998), and the failure to consider the asset allocation of non-salient accounts when making allocation decisions in a salient account (Choi, Laibson, and Madrian, 2009). Thus, it seems plausible that myopic loss aversion is responsible for depressing the demand for risky assets and driving up the equity premium (Benartzi and Thaler, 1995; Barberis, Huang, and Santos, 2001; Barberis, Huang, and Thaler, 2006). This raises the possibility of manipulating the boundaries of investors' mental accounts in order to increase their risky asset demand. The laboratory evidence to date has found that indeed, reporting only aggregated outcomes of multiple gambles increases subjects' willingness to take financial risks *ex ante*.

We have taken the experimental evidence several steps closer to the field by recruiting subjects from a broad swath of the population to invest hundreds of dollars in real mutual funds over a one-year horizon. We find that the aggregation manipulations that succeeded in increasing risk-taking in the laboratory are not effective in a setting closer to a field investment

[24] If the subject always chooses the later payment, we code her discount rate as 0%. The maximum possible discount rate is 50%.

environment. These results hold both at the start of our experiment, before the 2008 collapse in the global financial markets, and at the end, after our subjects have experienced the crash.

Many aspects of our experiment differ from prior studies. Therefore, we cannot say exactly what causes aggregation effects to disappear—the labeling of the assets, the fact that the assets are real mutual funds, the long horizon of the experiment, the relatively large stakes, or the composition of the subject pool. Moreover, we cannot distinguish between (1) the possibility that in the field, investors are able to overcome their innate loss aversion and mental accounting when choosing their equity portfolio shares, and (2) the possibility that myopic loss aversion is an important determinant of equity portfolio shares but aggregation interventions implemented by a single institution do not alter investors' mental accounting. Models that explain the equity premium with loss aversion (Benartzi and Thaler, 1995; Barberis, Huang, and Santos, 2001) find that investors must evaluate (and receive prospect theoretic utility from) returns only once a year in order to quantitatively match historical average equity returns. Because the typical investor probably sees stock market returns more frequently than once a year, these theoretical results suggest that simply seeing returns does not cause investors to evaluate them. In the models of Barberis and Xiong (2009, 2010), investors receive prospect theoretic utility from returns only when they sell a security. Future research should isolate what factors mediate information aggregation effects.[25]

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Anagol, Santosh, and Keith Jacks Gamble, 2010. "How segregated framing of portfolio results by asset affects investors' decisions: Evidence from a lab experiment." University of Pennsylvania working paper.

[25] Anagol and Gamble (2010), a revision of Anagol and Gamble (2009) that does not contain the original experimental results, imitate many aspects of our experimental design to test whether portfolio-level ongoing return reporting increases risk-taking. They recruit a broad swath of the U.S. population and tell subjects which real assets their laboratory assets are designed to mimic, but they conduct their experiment over a short session and pay subjects about $15 on average. Unlike in their original experiment with unlabeled assets and student subjects, they find no difference in subjects' risk-taking levels after subjects find out how returns will be reported. This suggests that the broad subject population and asset labels are important factors in mitigating the portfolio-level versus asset-by-asset ongoing return aggregation effect. However, their treatment and control groups choose substantially different risk levels *before* they find out how returns will be reported, raising concerns that their randomization failed to equalize innate risk-taking levels across their experimental groups.

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Table 1. Sample Size in Each Experimental Cell

This table reports the number of subjects that were assigned to each experimental cell. Panel A contains cells where ongoing returns were reported only at the aggregated portfolio level. Panel B contains cells where ongoing returns were reported separately by each asset held by the subject.

Panel A: Ongoing returns reported at portfolio level		
	Return viewing inducement frequency	
Historical return graph shown	Weekly	Biannual
None	60	60
1-year returns, single asset classes	30	30
5-year returns, single asset classes	29	30
1-year returns, portfolio mixes allowed	30	30
5-year returns, portfolio mixes allowed	30	30
Panel B: Ongoing returns reported separately by asset		
	Return viewing inducement frequency	
Historical return graph shown	Weekly	Biannual
1-year returns, single asset classes	30	29
5-year returns, single asset classes	30	30
1-year returns, portfolio mixes allowed	30	30
5-year returns, portfolio mixes allowed	30	29

Table 2. Sample Divisions for Experimental Comparisons

	Proportion of sample
How often are subjects induced to get feedback about their own ongoing returns?	
Weekly	50%
Biannually	50%
How granular is subjects' feedback about their own ongoing returns?	
Each asset's return reported separately	40%
Only aggregated portfolio-level returns reported	60%
Are historical returns graphs available?	
No historical returns graphs	20%
Historical returns graphs available	80%
How are historical returns data aggregated across time in the graphs?	
1-year return distributions shown	40%
5-year annualized return distributions shown	40%
How are historical returns data aggregated across asset classes in the graphs?	
Return distributions of single asset classes only are shown	40%
Return distributions of mixes of asset classes are shown	40%

Table 3. Subject Characteristics

Percent male	56%	Financial assets in bank, brokerage, and retirement accounts	
Age		< $25,000	27%
≤ 25	2%	$25,001 - $50,000	13%
26-35	31%	$50,001 - $75,000	10%
36-45	22%	$75,001 - $100,000	9%
46-55	19%	> $100,000	29%
55-65	13%	Prefer not to answer	12%
≥ 66	13%		
Education		Percent of outside financial assets invested in stocks at beginning of experiment	
Some high school	1%	0%	20%
High school graduate	10%	1 - 25%	32%
Some college	23%	26 - 50%	17%
Associate's degree	10%	51 - 75%	15%
Bachelor's degree	28%	76 - 100%	8%
Some graduate school	7%	Prefer not to answer	9%
Graduate degree	21%		
Annual household income			
< $35,000	5%		
$35,000 - $50,000	21%		
$50,001 - $75,000	29%		
$75,001 - $100,000	19%		
> $100,000	21%		
Prefer not to answer	5%		

Table 4. Subjects' Financial Literacy

How knowledgeable an investor do you consider yourself to be?	
Very knowledgeable	2%
Relatively knowledgeable	17%
Somewhat knowledgeable	44%
Less than knowledgeable	28%
Not at all knowledgeable	9%
How confident are you that the decision you made is the right one for you?	
Very confident	7%
Relatively confident	39%
Somewhat confident	40%
Less than confident	12%
Not at all confident	2%
How likely is it that you would change your portfolio decision if you consulted a professional investment advisor?	
Very likely	17%
Somewhat likely	66%
Not likely	17%
Suppose you had $100 in a savings account, and the interest rate was 2% 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	90%
Exactly $102	7%
Less than $102	3%
No response	0%
Buying a single company's stock usually provides a safer return than a stock mutual fund.	
True	5%
False	91%
No response	4%
Normally, which asset's returns fluctuate the most over time?	
Bonds	3%
Stocks	87%
Savings accounts	5%
All three about the same	5%
No response	0%
Suppose during the month of January 2010, the stock market rises by 10%. What do you believe this tells you about the stock market's return during February 2010?	
Increases return prediction	32%
Return prediction unchanged	55%
Decreases return prediction	10%
No response	2%
Suppose during the month of January 2010, the stock market falls by 10%. What do you believe this tells you about the stock market's return during February 2010?	
Increases return prediction	12%
Return prediction unchanged	55%
Decreases return prediction	31%
No response	2%

Note: The percentages for the compound interest, single stock versus mutual fund risk, asset volatility, and stock market momentum questions exclude from their denominator 28 subjects who did not respond to the exit questionnaire.

Table 5. Portfolio Allocations

This table shows the average portfolio allocation and average number of funds held by subjects at the start of the experiment, 27 weeks into experimental participation, and at the end of the experiment.

	Initial allocation	27 weeks	Final allocation
Money market	15.8%	20.6%	20.0%
Bonds	18.6%	22.2%	20.6%
U.S. equities	30.9%	29.3%	31.2%
International equities	34.8%	27.9%	28.2%
Total equities (U.S. + international)	65.7%	57.2%	59.4%
Number of funds held	3.66	3.56	3.52

Table 6. Website Visits After Initial Allocation

This table shows, by return viewing inducement frequency, the average number of total visits to the study website per subject, the average total viewings of the returns screens per subject, and the average fraction of the available returns screens that were viewed by each subject. "Total visits to the website" include visits that involved viewing a returns screen. Standard errors are in parentheses.

	Return viewing inducement frequency		*p*-value of difference
	Weekly	Biannual	
Total visits to website	60.7 (2.6)	18.2 (1.9)	0.000
Viewings of returns screens from e-mail links	45.3 (0.7)	1.5 (0.0)	0.000
Fraction of possible e-mail link returns screens viewed	87.2% (1.9)	73.8% (2.5)	0.000

Table 7. Ongoing Return Presentation Effects on Equity Allocations

The dependent variable is the percent of the portfolio allocated to equities at the start of the experiment, 27 weeks into experimental participation, and at the end of the experiment. *Biannual e-mail* is a dummy for whether the subject was sent an e-mail with a link to his ongoing returns biannually. *Portfolio-level return reporting* is a dummy for whether the subject's ongoing returns were reported only at the consolidated portfolio level. *Loss averse* is a dummy for whether the subject turned down the win $8/lose $5 gamble we offered. Point estimates from an OLS regression are shown, with standard errors in parentheses.

	Initial allocation		27 weeks		Final allocation	
Biannual e-mail	0.6	-0.4	0.5	-2.9	-1.4	-3.5
	(1.6)	(2.2)	(2.1)	(3.0)	(2.2)	(3.0)
Portfolio-level return reporting	-5.0**	-4.7*	-2.8	-4.4	-3.8	-4.9
	(1.7)	(2.3)	(2.2)	(3.0)	(2.2)	(3.1)
Loss averse		-2.2		-7.7		-5.3
		(3.0)		(4.0)		(4.1)
Biannual viewing × *Loss averse*		2.3		7.1		4.3
		(3.3)		(4.3)		(4.4)
Portfolio-level return reporting × *Loss averse*		-0.7		2.9		2.0
		(3.3)		(4.4)		(4.5)
Constant	68.6**	69.7**	53.4**	57.2**	62.4**	65.0**
	(1.5)	(2.1)	(2.0)	(2.8)	(2.0)	2.9
Sample size	597	597	597	597	597	597

* Significant at 5% level. ** Significant at 1% level.

Table 8. Historical Return Graph Effect on Equity Allocations

The dependent variable is the percent of the portfolio allocated to equities at the start of the experiment, 27 weeks into the experiment, and at the end of the experiment. *1-year graph* is a dummy for whether the subject was shown graphs with one-year historical returns. *5-year graph* is a dummy for whether the subject was shown graphs with five-year historical returns. *Asset class mixes shown* is a dummy for whether the subject saw a historical returns graphing tool that could show distributions of arbitrary asset class mixes. *Loss averse* is a dummy for whether the subject turned down the win $8/lose $5 gamble we offered. Point estimates from an OLS regression are shown, with standard errors in parentheses.

	Initial allocation		27 weeks		Final allocation	
1-year graph	12.2**	13.9**	8.5**	9.3*	9.6**	11.1*
	(2.4)	(3.3)	(3.2)	(4.5)	(3.2)	(4.5)
5-year graph	11.1**	10.2**	9.4**	7.9	8.1*	7.5
	(2.4)	(3.3)	(3.1)	(4.4)	(3.2)	(4.5)
Asset class mixes shown	-0.4	-1.9	1.2	-2.5	-1.0	-5.3
	(1.8)	(2.5)	(2.4)	(3.3)	(2.4)	(3.4)
Loss averse		-2.3		-6.2		-5.3
		(3.6)		(4.8)		(4.8)
1-year graph × *Loss averse*		-3.1		-1.1		-2.4
		(4.7)		(6.3)		(6.4)
5-year graph × *Loss averse*		2.2		3.4		1.8
		(4.7)		(6.3)		(6.4)
Asset class mixes shown × *Loss averse*		2.7		7.5		8.6
		(3.6)		(4.8)		(4.9)
Constant	56.8**	57.9**	44.4**	47.4**	52.7**	55.2**
	(1.8)	(2.5)	(2.4)	(3.3)	(2.4)	(3.4)
Sample size	597	597	597	597	597	597

* Significant at 5% level. ** Significant at 1% level.

Table 9. Historical Return Graph Effect on Initial Equity Allocation
Interacted with Subject Sophistication

The dependent variable is the percent of the portfolio allocated to equities at the start of the experiment. *1-year graph* is a dummy for whether the subject was shown graphs with one-year historical returns. *5-year graph* is a dummy for whether the subject was shown graphs with five-year historical returns. The definition of the sophistication dummy varies by column: being older than 45, having at least a bachelor's degree, having an annual income above $75,000, having financial assets in excess of $75,000, allocating more than 25% of one's (non-experimental) financial assets to stocks, correctly answering the compound interest question, correctly answering that a single company's stock is riskier than a stock mutual fund, correctly answering that stocks are more volatile than bonds or savings accounts, and answering that neither a 10% increase nor a 10% decrease in the stock market in a given month predicts the next month's stock market return. Point estimates from an OLS regression are shown, with standard errors in parentheses.

	Sophistication dummy used								
	Age > 45	Bachelor's degree	Income > $75,000	Assets > $75,000	Stock allocation > 25%	Understands compound interest	Understands diversification	Knows asset volatilities	Believes random walk
1-year graph	14.2**	18.0**	12.4**	12.7**	12.5**	14.8*	3.9	14.5*	9.0**
	(2.9)	(3.4)	(2.8)	(2.8)	(2.7)	(7.1)	(6.9)	(6.3)	(3.2)
5-year graph	15.9**	16.9**	12.9**	13.5**	11.9**	19.9**	5.0	15.4*	11.5**
	(2.9)	(3.4)	(2.8)	(2.7)	(2.7)	(7.3)	(7.2)	(6.5)	(3.1)
1-year graph × Soph. dummy	-5.2	-9.6*	-1.0	-2.4	-2.7	-2.9	9.1	-2.7	6.0
	(4.4)	(4.4)	(4.4)	(4.4)	(4.4)	(7.4)	(7.3)	(6.7)	(4.5)
5-year graph × Soph. dummy	-11.5**	-9.6*	-4.8	-6.7	-3.6	-9.8	6.6	-4.9	-0.8
	(4.4)	(4.4)	(4.5)	(4.5)	(4.4)	(7.7)	(7.6)	(6.9)	(4.5)
Sophistication dummy	4.5	11.8**	5.6	9.2*	11.5**	9.0	-2.9	6.7	2.0
	(3.6)	(3.6)	(3.6)	(3.6)	(3.6)	(6.2)	(5.8)	(5.6)	(3.7)
Constant	54.9**	49.6**	54.5**	53.3**	52.6**	48.5**	59.1**	50.6**	55.5**
	(2.3)	(2.8)	(2.3)	(2.2)	(2.2)	(5.9)	(5.4)	(5.3)	(2.6)
Sample size	597	597	597	597	597	569	569	569	569

* Significant at 5% level. ** Significant at 1% level.

Table 10. Historical Return Graph Effect on Investment Confidence

The dependent variables are subjects' self-reported confidence that the investment decision they made was right for them, likelihood that they would change their portfolio decision if they consulted a professional investment advisor, and investment knowledge. The table shows coefficients from an ordered probit regression, with standard errors in parentheses. *1-year graph* is a dummy for whether the subject was shown graphs with one-year historical returns. *5-year graph* is a dummy for whether the subject was shown graphs with five-year historical returns. Thresholds 1 through 4 are the boundaries between categories estimated by the ordered probit.

	Confidence in decision	Likelihood of changing decision	Investment knowledge
1-year graph	0.278*	-0.207	0.218
	(0.119)	(0.129)	(0.119)
5-year graph	0.170	-0.149	0.115
	(0.119)	(0.129)	(0.119)
Threshold 1	-1.958**	-1.109**	-1.220**
	(0.151)	(0.114)	(0.111)
Threshold 2	-0.912**	0.811**	-0.212*
	(0.106)	(0.110)	(0.101)
Threshold 3	0.283**		1.017**
	(0.101)		(0.106)
Threshold 4	1.635**		2.191**
	(0.118)		(0.148)
Sample size	597	597	597

* Significant at 5% level. ** Significant at 1% level.

Table 11. Determinants of Trading Frequency

The dependent variable is the total number of days on which a portfolio reallocation was executed for a subject. *Biannual e-mail* is a dummy for whether the subject was sent an e-mail with a link to his ongoing returns biannually. *Portfolio-level return reporting* is a dummy for whether the subject's ongoing returns were reported only at the consolidated portfolio level. *1-year graph* is a dummy for whether the subject was shown graphs with one-year historical returns. *5-year graph* is a dummy for whether the subject was shown graphs with five-year historical returns. *Asset class mixes shown* is a dummy for whether the historical returns graphing tool could show distributions of arbitrary asset class mixes. *Loss averse* is a dummy for whether the subject turned down the win $8/lose $5 gamble we offered. *Discount rate* is the maximum weekly interest rate at which the subject chooses the hypothetical earlier payment.

Biannual e-mail	-4.35**	-4.35**
	(0.55)	(0.55)
Portfolio-level return reporting	-1.99**	-2.00**
	(0.62)	(0.62)
1-year graph	-0.05	-0.06
	(0.87)	(0.87)
5-year graph	0.17	0.17
	(0.87)	(0.87)
Asset class mixes shown	-0.88	-0.86
	(0.62)	(0.62)
Loss averse		0.22
		(0.56)
Discount rate		0.08
		(1.87)
Constant	8.31**	8.20**
	(0.91)	(0.96)
Sample size	597	597

* Significant at 5% level. ** Significant at 1% level.

Note: A discount rate of 50% is coded as 0.5.

Figure 1. Experimental Period

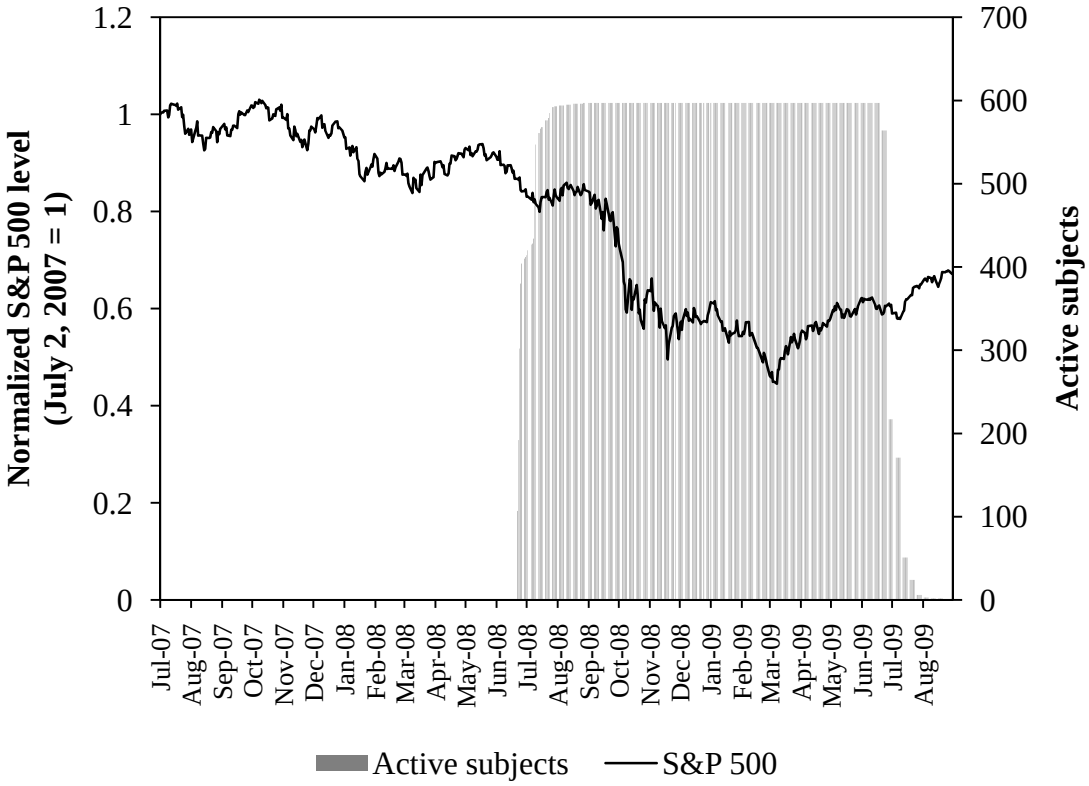
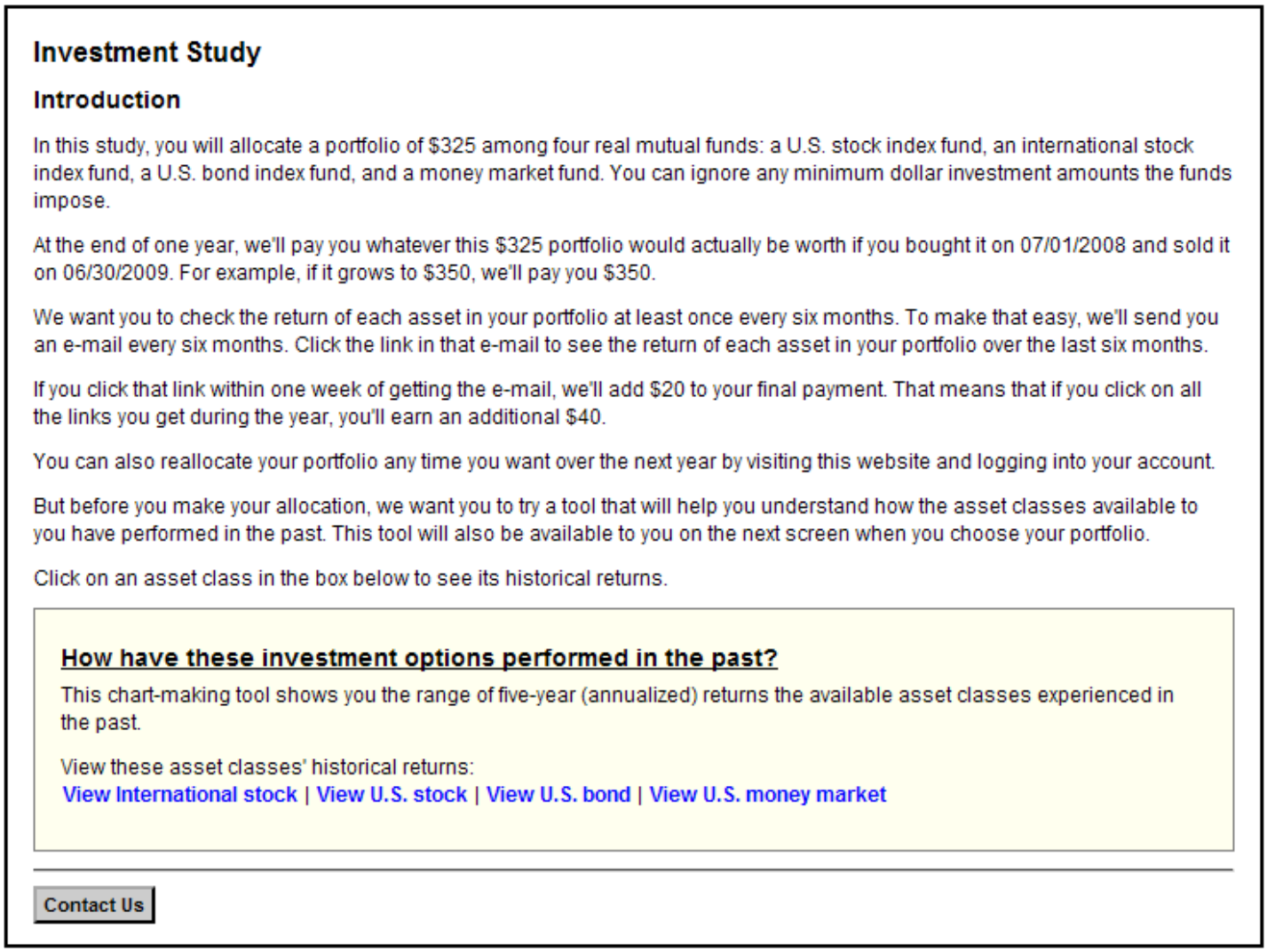


Figure 2. Initial Screen in Condition with Biannual Viewing of Asset by Asset Ongoing Returns and Single Asset Class Five-Year Historical Return Graphs



Investment Study

Introduction

In this study, you will allocate a portfolio of $325 among four real mutual funds: a U.S. stock index fund, an international stock index fund, a U.S. bond index fund, and a money market fund. You can ignore any minimum dollar investment amounts the funds impose.

At the end of one year, we'll pay you whatever this $325 portfolio would actually be worth if you bought it on 07/01/2008 and sold it on 06/30/2009. For example, if it grows to $350, we'll pay you $350.

We want you to check the return of each asset in your portfolio at least once every six months. To make that easy, we'll send you an e-mail every six months. Click the link in that e-mail to see the return of each asset in your portfolio over the last six months.

If you click that link within one week of getting the e-mail, we'll add $20 to your final payment. That means that if you click on all the links you get during the year, you'll earn an additional $40.

You can also reallocate your portfolio any time you want over the next year by visiting this website and logging into your account.

But before you make your allocation, we want you to try a tool that will help you understand how the asset classes available to you have performed in the past. This tool will also be available to you on the next screen when you choose your portfolio.

Click on an asset class in the box below to see its historical returns.

How have these investment options performed in the past?

This chart-making tool shows you the range of five-year (annualized) returns the available asset classes experienced in the past.

View these asset classes' historical returns:
View International stock | View U.S. stock | View U.S. bond | View U.S. money market

Contact Us

Figure 3. Historical Returns Graphing Tool that Shows One-Year Returns of Single Asset Classes Only

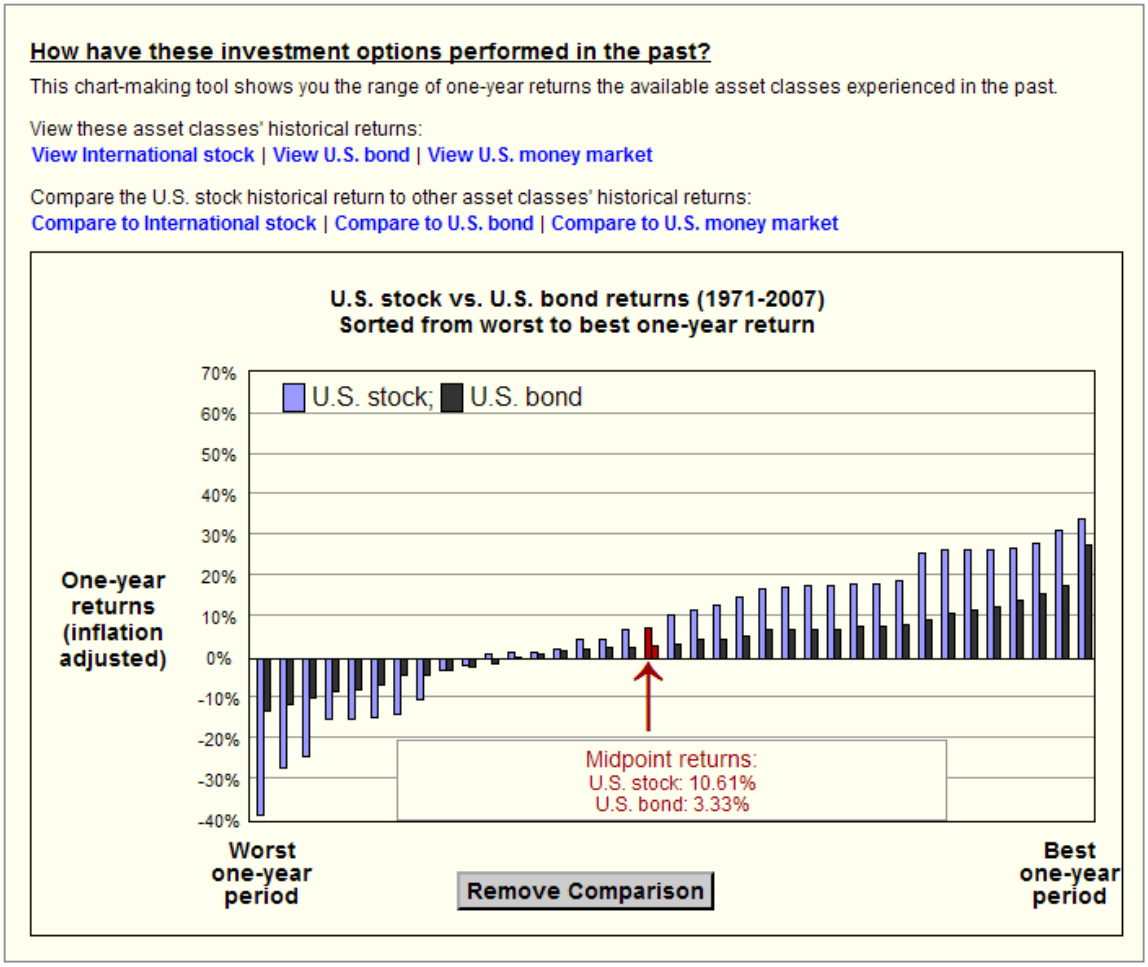


Figure 4. Historical Returns Graphing Tool that Shows Five-Year Annualized Returns of Arbitrary Portfolio Mixes

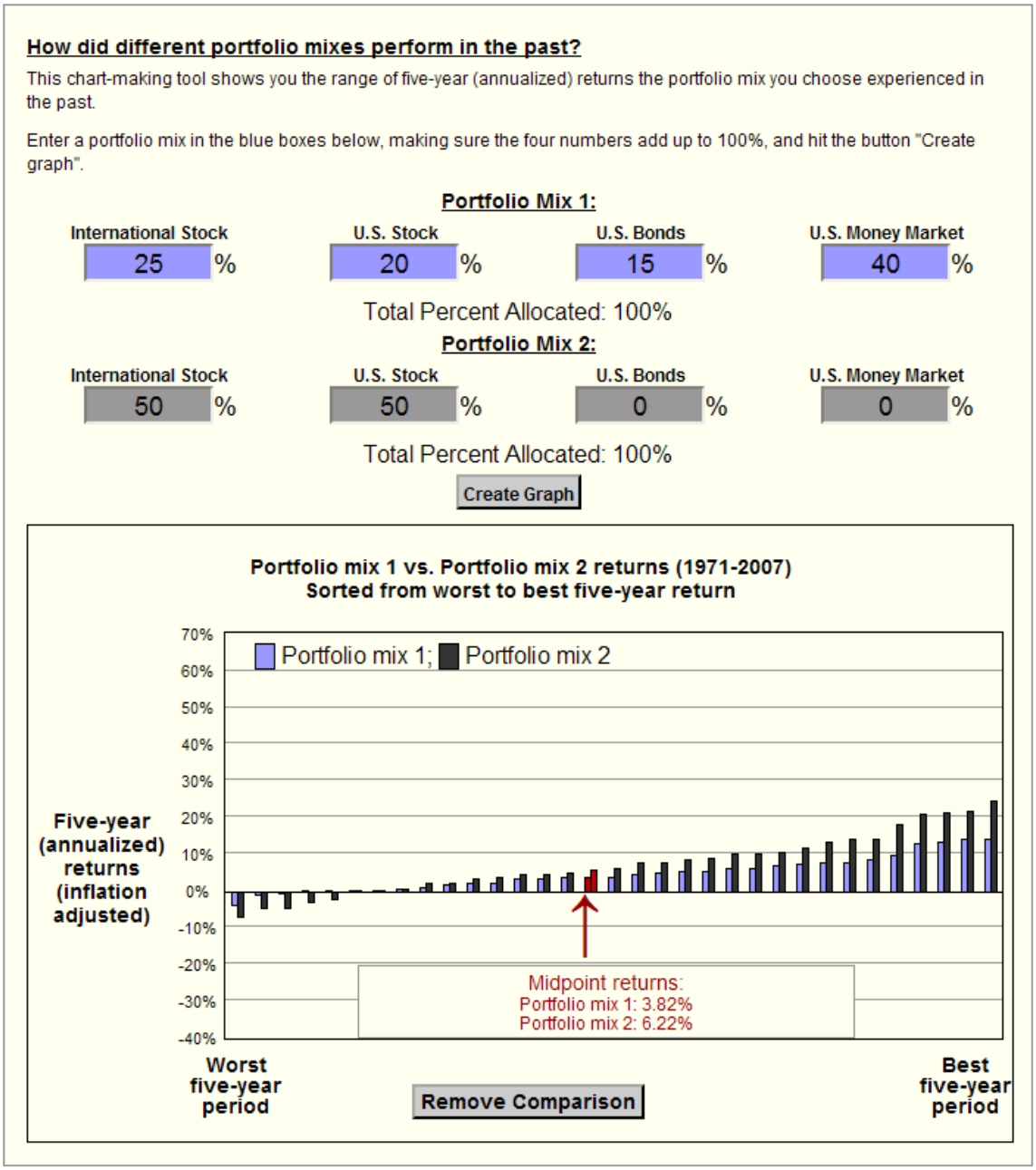
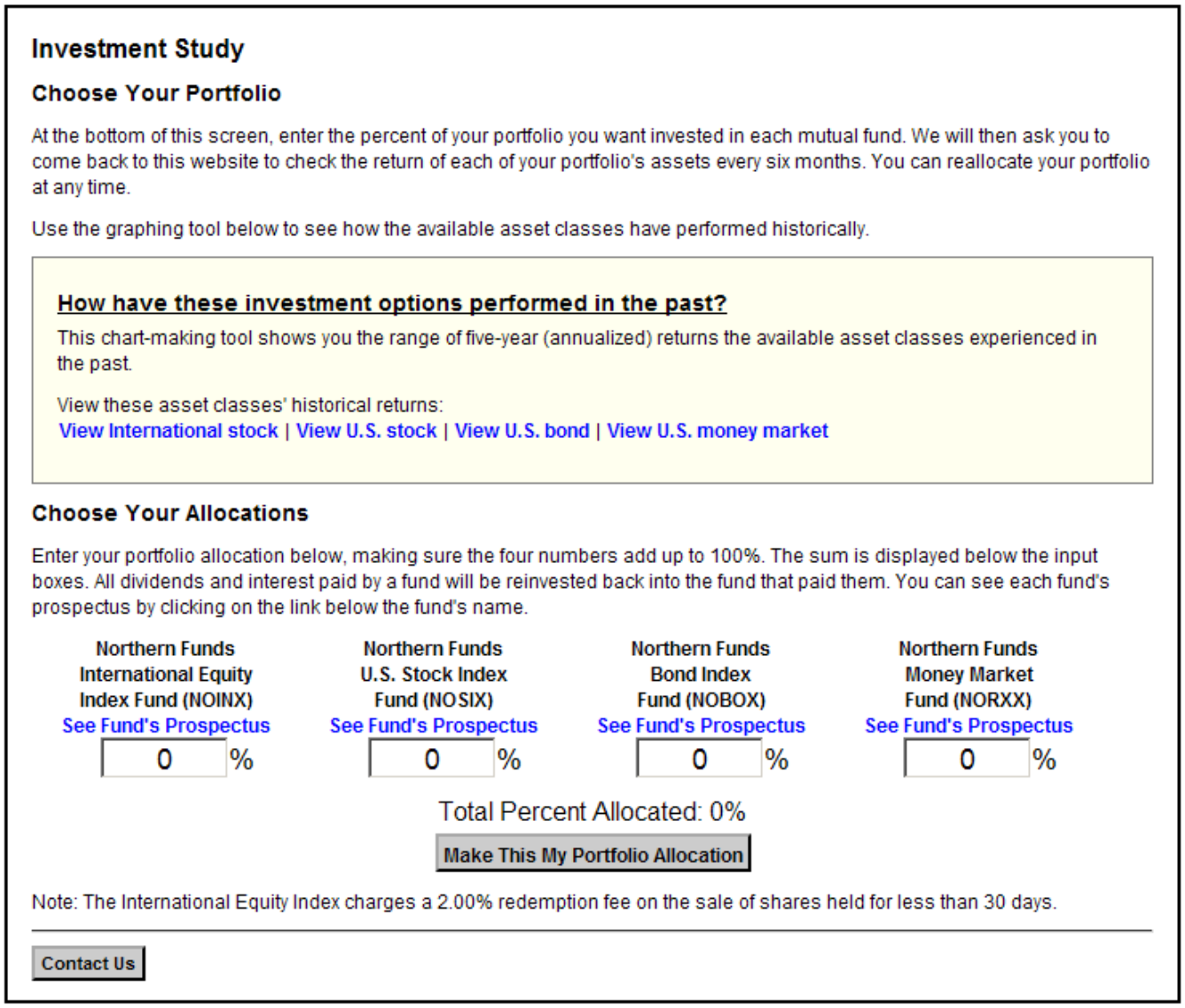


Figure 5. Initial Portfolio Allocation Screen in Condition with Biannual Viewing of Asset by Asset Ongoing Returns and Single Asset Class Five-Year Historical Return Graphs



Investment Study

Choose Your Portfolio

At the bottom of this screen, enter the percent of your portfolio you want invested in each mutual fund. We will then ask you to come back to this website to check the return of each of your portfolio's assets every six months. You can reallocate your portfolio at any time.

Use the graphing tool below to see how the available asset classes have performed historically.

How have these investment options performed in the past?

This chart-making tool shows you the range of five-year (annualized) returns the available asset classes experienced in the past.

View these asset classes' historical returns:
View International stock | View U.S. stock | View U.S. bond | View U.S. money market

Choose Your Allocations

Enter your portfolio allocation below, making sure the four numbers add up to 100%. The sum is displayed below the input boxes. All dividends and interest paid by a fund will be reinvested back into the fund that paid them. You can see each fund's prospectus by clicking on the link below the fund's name.

Northern Funds International Equity Index Fund (NOINX)	Northern Funds U.S. Stock Index Fund (NOSIX)	Northern Funds Bond Index Fund (NOBOX)	Northern Funds Money Market Fund (NORXX)
See Fund's Prospectus	See Fund's Prospectus	See Fund's Prospectus	See Fund's Prospectus
0 %	0 %	0 %	0 %

Total Percent Allocated: 0%

Make This My Portfolio Allocation

Note: The International Equity Index charges a 2.00% redemption fee on the sale of shares held for less than 30 days.

Contact Us

Figure 6. Portfolio Status Page

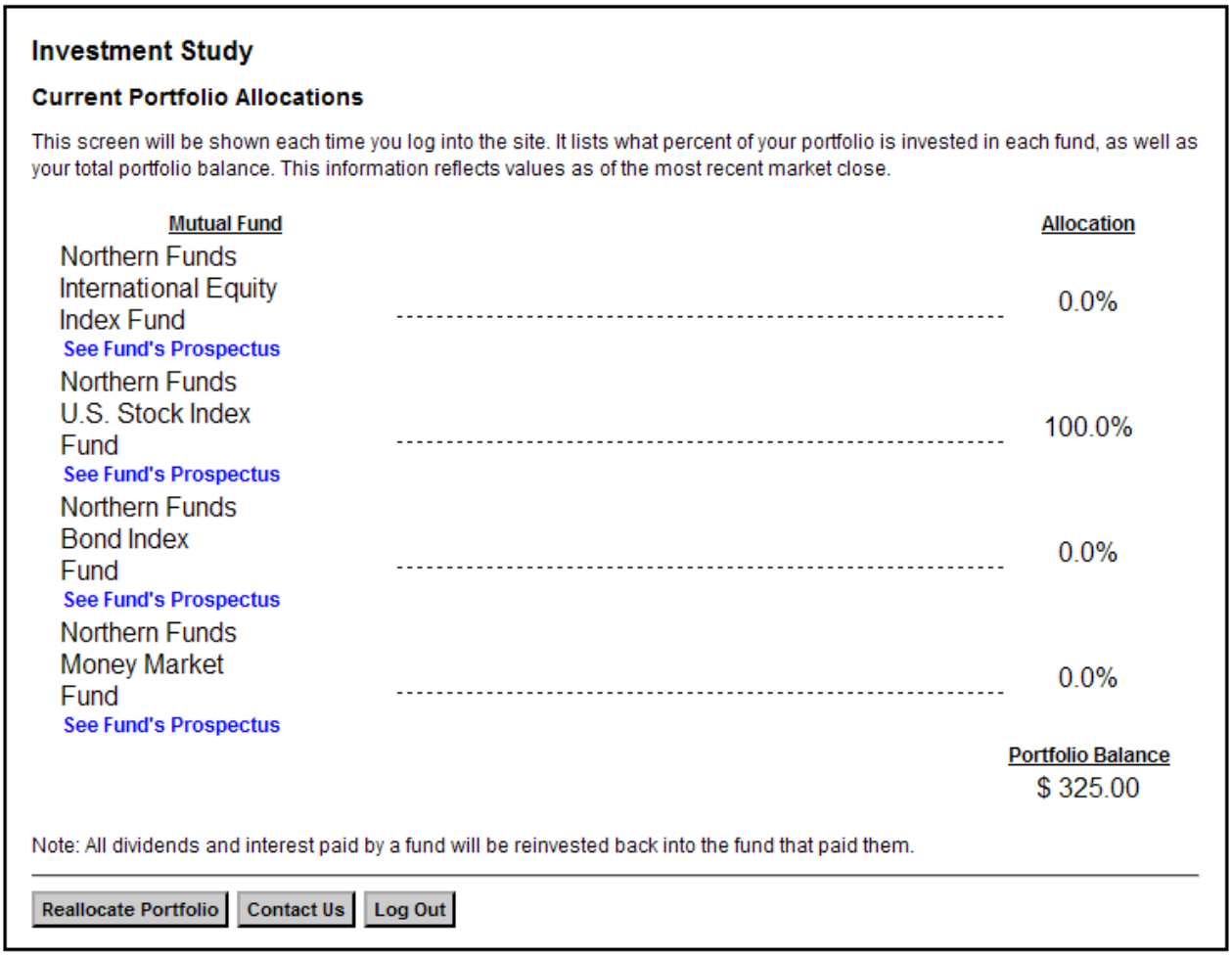


Figure 7. Recent Returns Screen in Conditions with Biannual Viewing of Asset by Asset Ongoing Returns

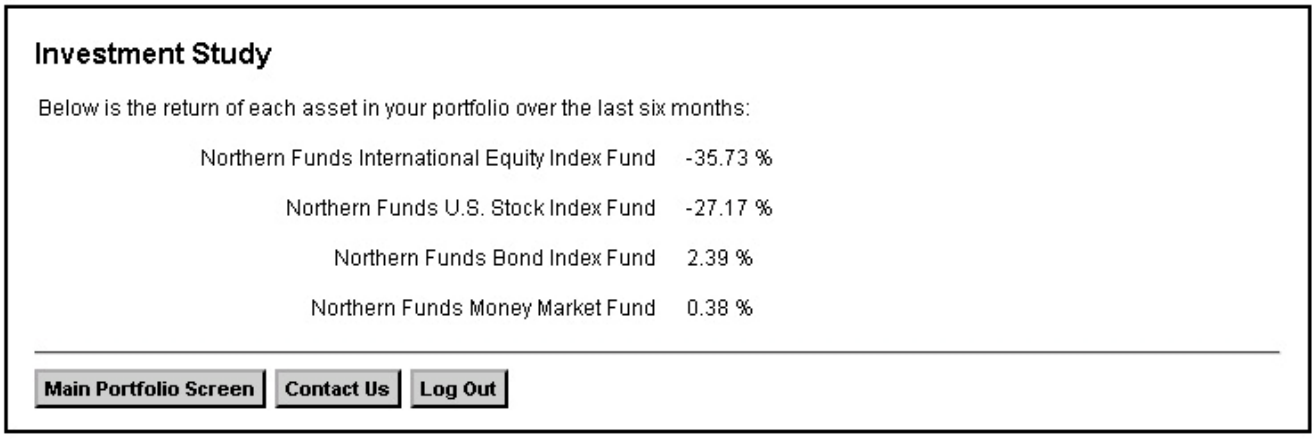


Figure 8. Recent Return Screen in Conditions with Weekly Viewing of Ongoing Overall Portfolio Returns

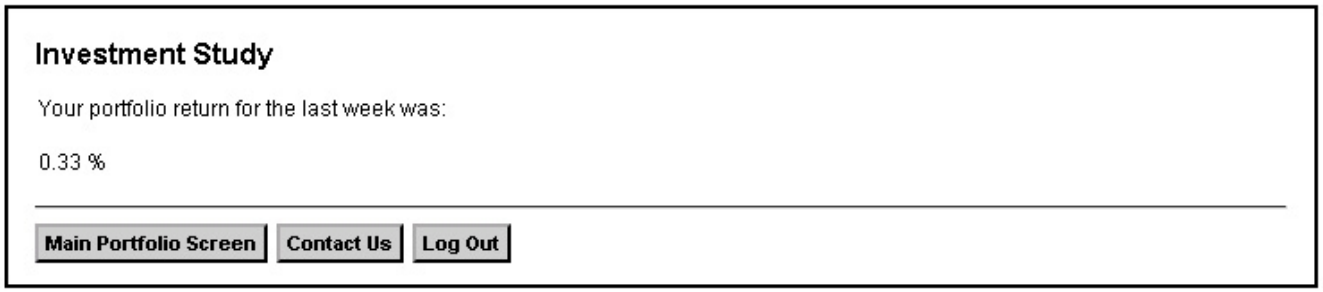


Figure 9. Portfolio Reallocation Screen in Conditions with Historical Returns Graphing Tool that Shows One-Year Returns of Arbitrary Portfolio Mixes