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

John Beshears
Harvard University

James J. Choi
Yale University and NBER

David Laibson
Harvard University and NBER

Brigitte C. Madrian
Harvard University and NBER

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Abstract: Consistent with myopic loss aversion, 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 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 component returns individually. Information aggregation along various dimensions produces this effect: reporting subjects' *n*-period portfolio returns once every $n > 1$ periods rather than reporting one-period returns every period (Gneezy and Potters, 1997; Thaler et al., 1997; Gneezy, Kapteyn, and Potters, 2003; Bellemare et al., 2005; Haigh and List, 2006), reporting subjects' portfolio-level returns rather than each individual asset's returns (Anagol and Gamble, 2008), 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 manipulations encourage the integration of more gamble outcomes into a single mental account. If these gambles are not perfectly correlated and have positive expected values, the resulting diversification can decrease the probability that the investor will end up with an overall loss in the mental account.[3] Therefore, the gambles appear more attractive than if each occupied its own separate mental account.

Given the strength and consistency of the experimental results, numerous authors have suggested that institutions may be able to affect risk-taking in portfolios outside the laboratory by changing the aggregation at which they report returns to investors. For example, Thaler et al. (1997) write, "Decisions made by employees covered by such [defined contribution pension] plans may vary considerably depending on how their investment opportunities are described and the manner and frequency with which they receive feedback on their returns." Haigh and List (2005) speculate that "institutions may

[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] This property is not true for all distributions, but it does hold, for example, when the two gambles are both drawn from the same normal distribution.

have the ability to influence asset prices through changes in their information provisioning policies."

However, the experiments to date have been conducted over the course of one short laboratory session with small monetary stakes, artificial laboratory assets, and mostly student subjects.[4] The substantial gap between the laboratory setting and the typical investment environment raises questions about how effective aggregation manipulations would be at raising portfolio risk-taking in the field. The psychology of large-stakes risk-taking over many days, months, or years may differ. Subjects may employ context-specific heuristics that mitigate behavioral biases when dealing with familiar assets such as stocks and bonds, but not realize that these heuristics are also applicable to abstract laboratory gambles. Aggregation manipulations by a single institution may have little power because of interference from or interactions with background information flows in the field. And biases found among student subjects may not be present in the typical investor, who has more financial experience.

In this paper, 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 they would if they were making real investments in these mutual funds. We paid each subject whatever the value of his or her portfolio is at the end of the year.

We test four aggregation manipulations, which were randomly assigned to subjects.

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.

[4] We know of two exceptions. Haigh and List (2006) use professional futures and options pit traders as subjects. Benartzi and Thaler (1999) include many non-student subjects in their studies.

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 of each individual asset they were holding over the last week or six months.

The third manipulation varied the historical returns information shown to subjects. We showed some subjects graphs depicting the distribution of real annual returns for U.S. equities, international equities, U.S. bonds, and U.S. money markets from 1970 to 2006. 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 simply by the presence of the graphs.

The fourth manipulation varied whether subjects who received the historical returns graphs could also access information about historical return covariances among the asset classes. Some subjects could only see historical return distributions of four portfolios, each invested 100% in one of the 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 condition 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 return covariances do not affect the portfolio fraction allocated to equities. Seeing ongoing portfolio-level returns instead of asset-by-asset returns *decreases* equity allocations—the opposite of Anagol and Gamble's (2008) result.

We identify subjects who are particularly loss averse by offering them a gamble that gave them an equal chance of adding $8 to or subtracting $5 from their experimental 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 myopic loss aversion. However, even

within this subset of particularly loss-averse subjects, aggregation does not significantly move subjects' equity allocations in the predicted direction.

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 particularly large among subjects who do not have a bachelor's degree.

These results are evidence that aggregation interventions that are effective in the laboratory will not have the same marginal effect in the field. Our findings are consistent with two interpretations. Perhaps myopic loss aversion is not an important determinant of equity portfolio shares in the field. Alternatively, myopic loss aversion may be important, but aggregation interventions do not alter investors' mental accounting in the anticipated direction. 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 (2008, 2009), investors receive prospect theoretic utility from returns only when they sell a security.

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

We recruited subjects in late June and early July 2008 through the market research firm MarketTools. The S&P 500 was down 18% from its October 2007 peak at the end of June 2008. Therefore, our subjects' initial portfolio choices were not made a time when it was known that we were entering a bear market of historic proportions. The market's precipitous fall did not commence until after the September 2008 bankruptcy of Lehman Brothers.

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 informed subjects that they would periodically receive e-mails with a link that they could click to see their portfolio's return, and 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 the subject with a link to click on in order to activate his or her account.[5] The link took them to a login screen.

After logging in, subjects received a fuller description of the study instructions. Figure 1 shows the screen subjects in one of our experimental conditions saw when they logged in for the first time. Subjects in other conditions 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

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

the year by logging into their account on the website. Subjects were also told about the inducement to view their ongoing returns, and in some conditions were introduced to the historical returns chart tool. We describe these latter two items in greater detail below.

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, if they clicked all of the e-mailed links they received during the one-year study, they could 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 e-mails were sent coincided with when these subjects would have otherwise received their 26th and 52nd e-mail if they had been assigned to the weekly viewing condition. If subjects in the biannual viewing condition clicked the link within a week of receiving the e-mail, we added $20 to their final payment.[6] We offered only $20 per viewing for this group because we anticipated that subjects in the weekly viewing condition would not click on every e-mail link, and we wanted to equalize average return-viewing payments across conditions based upon our best guess of weekly-condition compliance.

Within each of the above two conditions, 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 2, which showed only the overall return of their portfolio. The other half of subjects saw a screen like that in Figure 3, which showed the return of each individual asset they held.[7]

For 80% of our subjects, the bottom of the initial screen 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 graphs are modeled after those in Benartzi and Thaler (1999). Returns for an asset class over the historical sample period are sorted from lowest to highest and

[6] 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 $t + n$.

[7] In the asset-by-asset returns reporting condition, 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 it was held for only part of the time between e-mails.

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.[8] We used the S&P 500, MSCI EAFE, Lehman Brothers U.S. Aggregate Bond Index, and the 30-day U.S. Treasury bill as our asset class proxies. Because the MSCI EAFE series starts in 1970, we used the period from 1970 to 2007 for all our asset classes.[9] Subjects were required to click through an animation that explained how to understand the graph before they could proceed to the next part of the study. This animation was also available for replaying in later screens where the graph was shown.

The graphs varied across subjects 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 that 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 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 4 shows an example of a graph where one-year returns are being shown and only four portfolio distributions are accessible. Figure 5 shows an example of a graph where five-year returns are being shown and any portfolio's return distribution can be seen. The figures show how the graphing tool allows subjects to compare the distributions of two different portfolios side-by-side.

After clicking through the explanation of the chart, subjects were asked to specify percentages to be invested in each investment option. 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.[10] We provided links to each fund's prospectus. We also informed subjects that the International Equity

[8] 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 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. stock graph, where the median return was 10.61% but the highlighted bar corresponded to a 7.38% return.

[9] In addition, the Lehman Brothers index starts in 1976. We construct our own aggregate bond market index returns from 1970 to 1975 by weighting the returns of Ibbotson's long-term corporate bond, intermediate Treasury, and 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.

[10] We chose Northern Funds because it was the largest fund family that offered U.S. equity, international equity, and bond index funds, offered 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.

Index fund charges a 2% redemption fee on the sale of shares held for less than thirty days.[11] In order to aid the portfolio decision, the graphing tool remained accessible on the same screen in which the portfolio allocation was entered. Figure 6 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.

For 20% of our subjects, no historical returns graphs were made available. These subjects saw the input boxes for their initial portfolio allocation below the experimental instructions on the first screen. Links to prospectuses and information about the international fund's redemption fee were also present on this screen.

Table 1 shows the distribution of our sample among all the experimental condition cells. We recruited 600 subjects, but three of them did not participate after registering. Therefore, our final sample consists of 597 subjects.

After subjects submitted their initial allocation, they completed a questionnaire that elicited information on demographics, self-assessed investment knowledge, and self-assessed confidence about their portfolio allocation. 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 odd or even. We applied gains and losses 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 likely to suffer from especially severe loss aversion and narrow mental accounting.

Upon finishing the questionnaire, subjects were taken to a page that showed their current investment allocation and total balance (see Figure 7). On subsequent logins to the site that were not initiated by clicking an e-mailed link, subjects would see this portfolio status page first. At this point, subjects could log out.

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

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 (see Figure 8). The reallocation screen showed the graphing tool relevant for the subject's experimental condition, 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.

II. Empirical results

A. Subject characteristics

Table 2 shows summary statistics on responses to the questionnaire that was 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—but all ages have substantial representation. Our subjects are also relatively well-educated, perhaps due to our request for subjects with a minimum income level of $35,000; only 5% of subjects report an income less than that threshold. 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. The median subject considers himself a "somewhat knowledge" 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.

Because the experimental setup was simple (from the perspective of an individual subject) and the assets were passively managed funds in familiar asset classes, subjects

[12] We used Yahoo! Finance for our dividend and price data. On July 1, 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, 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 equity allocations by 1.0 percentage points among subjects in the weekly returns-viewing condition and 2.2 percentage points in the biannual returns-viewing condition.

did not necessarily need a long time to make a considered decision. The median subject who received no historical returns information or who was only given the historical returns distribution of portfolios invested 100% in a single asset class took 14 minutes between login and submission of the initial portfolio allocation. The median subject who was able to see the historical returns distribution of any portfolio mix took 13 minutes.[13]

B. Effect of ongoing return presentation on equity shares

Table 3 shows that our periodic e-mails to subjects were successful at creating variation in the frequency with which they visited the study website and viewed their returns. During the one-year study 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. In the weekly condition, 45.3 of those logins occurred because subjects clicked on the e-mail link to view the screen with their ongoing returns. Thus, compliance with the link-clicking was high; 87.2% of weekly links sent were clicked within a week of receipt. In the biannual condition, subjects clicked 73.8% of links sent, so they saw the returns screen an average of 1.5 times. Subjects in both conditions logged in about 16 times on average when not prompted by an e-mail.

Our main dependent variable of interest is the total fraction of the portfolio invested in equities. In the first column of Table 4, the dependent variable is the equity share of the subject's initial allocation. We find that being in the biannual rather than the weekly viewing condition 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 aggregated yearly returns rather than monthly returns.[14] 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 viewing returns biannually decreases equity shares by as much as 2.5 percentage points.

[13] 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.

[14] 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.

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 5.0 percentage points, a statistically significant amount. Recall that Anagol and Gamble (2008) find that a similar manipulation *increases* portfolio risk-taking.

The second column of Table 4 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, so they should be the most susceptible to an aggregation manipulation. The point estimate of the biannual viewing treatment effect is 2.3 percentage points higher among gamble rejecters than gamble accepters, but this difference is not significant. The overall biannual viewing effect among gamble rejecters, $-0.4 + 2.3 = 1.9$ percentage points, is not significantly different from zero. Contrary to the prediction of 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 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 return disclosure.[15] It is nevertheless possible that our subjects initially did not realize how disaggregated return disclosure would affect their utility, but they gradually learned as they became exposed to these risks. This would lead to a progressive 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 closely enough, so they were not aware of the reporting regime.

We test these stories by using equity share halfway into the experimental period as the dependent variable in the third and fourth columns of Table 4. For subjects in the

[15] It is not possible to make a similar judgment about the subjects in Thaler et al. (1997) and Anagol and Gamble (2008). 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 (2008) make subjects play practice rounds before starting data collection, and they do not report the choices in the practice r\ounds.

weekly viewing condition, equity share is measured eight days after they receive their 26th e-mailed returns-checking link. For subjects in the biannual viewing condition, equity share is measured eight days after they receive their first returns-checking link. By measuring allocations at this later point, we capture both weekly and biannual subjects' allocations 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.

The coefficient estimates in the third column indicate that even over six months into the experiment, reporting returns on an aggregated basis does not significantly increase portfolio risk-taking. The point estimate of the biannual viewing treatment effect actually shrinks slightly to 0.5 percentage points. The portfolio-level return treatment effect also attenuates to –3.0 percentage points, and is no longer significant.

The fourth column shows treatment interactions with the dummy for turning down the small gamble. We see that among gamble accepters, the biannual viewing effect becomes more negative than the initial effect, dropping to –2.9 percentage points. Among gamble rejecters, the biannual viewing effect is –2.9 + 7.1 = 4.1 percentage points. However, neither of these effects are significantly different from zero or from each other. We also find that the effect of receiving ongoing returns aggregated at the portfolio level is 2.9 percentage points less negative for gamble rejecters than gamble accepters. This difference is also not significant, and the overall effect for gamble rejecters remains negative (–4.4 + 2.9 = –1.4 percentage points).

C. Effect of historical returns presentation on equity shares

Table 5 shows regressions estimating the effect the historical return graphs had on the portfolio fraction invested in equities. We see in the first column that simply viewing a historical returns graph significantly raises the initial equity share by 11 to 12 percentage points relative to not viewing a historical returns graph. However, it does not appear to matter whether the distribution of one-year returns or five-year annualized

returns are presented. In fact—contrary to the 19 to 41 percentage point increase in equity allocations found by Benartzi and Thaler (1999) when 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.[16] Nor does it seem to matter whether subjects were able to see the return distributions of any mix of assets 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 allocation to equities, although the –0.4 percentage point estimate is not significant.

The second column shows that there are no significant interactions of the graph-viewing effects with whether the subject turned down the small gamble. Less loss-averse subjects initially allocate a statistically insignificant 3.7 percentage points less to equities when they see the five-year graphs instead of the one-year graphs. More loss-averse subjects allocate a statistically insignificant 1.6 percentage points more to equities when seeing the five-year graph.

Given the historically high volatility and sharp drop in the stock market during the second half of 2008, did seeing more aggregated historical returns information encourage subjects to take a long-term view on stocks and maintain a higher equity allocation? The last two columns of Table 5, where the dependent variable is total equity share 27 weeks into experimental participation, indicate that it did not. Although simply having seen historical returns information continues to raise equity share at six months by 9 percentage points, it again does not matter whether one-year or five-year return distributions were shown. Those seeing five-year graphs hold 0.9 percentage points more in equities than those seeing one-year graphs, but the difference remains insignificant. Splitting the sample by loss aversion strength, the patterns we found in initial allocations remain qualitatively similar at six months: less loss-averse subjects who saw one-year graphs hold less equity than those seeing five-year graphs, and vice versa for more loss-averse subjects, but the differences are not significant.

[16] Although using simulated thirty-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 38-year historical sample period without resorting to simulation.

Who is most affected by seeing the returns graphs? Table 6 shows estimates of graph treatment effect interactions with five subject characteristics associated with greater 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, and having over 25% of one's non-experimental financial assets invested in stocks. All 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 increase their initial equity share by 17 or 18 percentage points when they see 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 5-year graph. The age interaction with the 1-year graph treatment effect is negative as well, but not significantly different from zero. (On the other hand, it is also not statistically distinguishable from the age interaction with the 5-year graph treatment.)[17]

Table 7 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 how knowledgeable of an investor they consider themselves to be. 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 an investor. However, the only statistically significant coefficient is the effect of the one-year graph on confidence in the investment decision.

D. Effects on trading frequency

In this subsection, we examine whether the aggregation treatments affected the tendency to reallocate one's account. The average number of days on which a subject

[17] Subjects cannot allocate more than 100% to equities, and more sophisticated subjects tend to allocate more to equities than unsophisticated subjects when they don't see the graphs. One might thus suspect that the negative interactions are driven by the fact that the graphs have less margin under 100% to operate among the sophisticated. However, the results are similar when estimated using a tobit with left-censoring at 0% and right-censoring at 100%.

reallocated his portfolio during the experiment is 4.6. The median number of reallocation days is 2.

We use the total number of reallocation days as the dependent variable in Table 8. In addition to treatment dummies, we include a dummy for turning down the small gamble and an additional variable measuring the subject's 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?

We compute the subject's discount rate as the maximum weekly interest rate at which the subject chooses the earlier payment.[18] Barberis and Xiong (2008) predict that loss-averse mental accounters who are especially impatient will be less likely to trade when they hold paper losses, since they would experience an immediate jolt of negative utility from realizing that loss. At the end of our experiment, 95% of our subjects had portfolio values less than their initial $325 due to the bear market.

We find that those who were sent biannual e-mails to check their returns traded on 4.4 fewer days 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 which is also significant at the 1% level.

Recall that 80% of subjects had access to the historical returns distribution charts on the screen in which they entered trade orders. However, the regression does not show that these charts mattered significantly for trading frequency. We also find no significant

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

evidence that especially loss-averse, impatient subjects traded less frequently in a time period where 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, 2008). 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 opens the possibility of manipulating the boundaries of investors' mental accounts in order to increase their risky asset demand. The laboratory evidence to date has suggested 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 and making them invest hundreds of dollars among 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 real-life investment environment. Because many aspects of our experiment differ from prior studies, we cannot say exactly what causes aggregation effects to disappear—the labeling of the assets, the fact that the assets are actual mutual funds, the long horizon of the experiment, the relatively large stakes, or the composition of the subject pool. Future research should isolate what factors mediate information aggregation effects.

In addition, our data do not speak to whether our manipulations were unsuccessful because they did not change our subjects' mental accounting, or because myopic loss aversion is simply not an important determinant of portfolio risk-taking. However, our

results do suggest that real-life financial institutions are unlikely to have a marginal impact on portfolio risk-taking by changing the aggregation of their return disclosures.

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Table 1. Sample Size in Each Treatment Cell

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	30	30
5-year returns, portfolio mixes	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	30	30
5-year returns, portfolio mixes	30	29

Table 2. Subject Summary Statistics

% male	56%	Percent of financial assets invested in stocks	
Age		0%	20%
≤ 25	2%	1 - 25%	32%
26-35	31%	26 - 50%	17%
36-45	22%	51 - 75%	15%
46-55	19%	76 - 100%	8%
55-65	13%	Prefer not to answer	8%
≥ 66	13%		
Education		How knowledgeable an investor do you consider yourself to be?	
Some high school	1%	Very knowledgeable	2%
High school graduate	10%	Relatively knowledgeable	17%
Some college	23%	Somewhat knowledgeable	44%
Associate's degree	10%	Less than knowledgeable	28%
Bachelor's degree	28%	Not at all knowledgeable	9%
Some graduate school	7%		
Graduate degree	21%	How confident are you that the decision you made is the right one for you?	
Annual household income		Very confident	7%
< $35,000	5%	Relatively confident	39%
$35,000 - $50,000	21%	Somewhat confident	40%
$50,001 - $75,000	29%	Less than confident	12%
$75,001 - $100,000	19%	Not at all confident	2%
> $100,000	21%		
Prefer not to answer	5%	How likely is it that you would change your portfolio decision if you consulted a professional investment advisor?	
Financial assets in bank, brokerage, and retirement accounts		Very likely	17%
< $25,000	27%	Somewhat likely	66%
$25,001 - $50,000	13%	Not likely	17%
$50,001 - $75,000	10%		
$75,001 - $100,000	9%		
> $100,000	29%		
Prefer not to answer	12%		

Table 3. Website Visits After Initial Allocation

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

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

Table 4. 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 or 27 weeks into experimental participation. *Biannual viewing* is a dummy for whether the subject was paid to view his portfolio return 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 gamble we offered. Point estimates from an OLS regression are shown, with standard errors in parentheses.

	Initial allocation		27 weeks	
Biannual viewing	0.6	-0.4	0.5	-2.9
	(1.6)	(2.2)	(2.1)	(3.0)
Portfolio-level return reporting	-5.0**	-4.7*	-2.8	-4.4
	(1.7)	(2.3)	(2.2)	(3.0)
Loss averse		-2.2		-7.7
		(3.0)		(4.0)
Biannual viewing × *Loss averse*		2.3		7.1
		(3.3)		(4.3)
Portfolio-level return reporting × *Loss averse*		-0.7		2.9
		(3.3)		(4.4)
Constant	68.6**	69.7**	53.4**	57.2**
	(1.5)	(2.1)	(2.0)	(2.8)
Sample size	597	597	597	597

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

Table 5. 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 or 27 weeks into the experiment. The first two columns show coefficients from regressions run on the full sample. The last two columns show coefficients from regressions run only on subjects who rejected the 50-50 win $8/lose $5 gamble. *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 gamble we offered. Point estimates from an OLS regression are shown, with standard errors in parentheses.

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

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

Table 6. Historical Return Graph Effect on Initial Equity Allocation Interacted with Subject Characteristics

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, or allocating more than 25% of one's (non-experimental) financial assets to stocks. Point estimates from an OLS regression are shown, with standard errors in parentheses. All regressions include a constant term whose coefficient is not reported.

	Sophistication dummy used				
	Age > 45	Bachelor's degree	Income > $75,000	Assets > $75,000	Stock allocation > 25%
1-year graph	14.2**	18.0**	12.4**	12.7**	12.5**
	(2.9)	(3.4)	(2.8)	(2.8)	(2.7)
5-year graph	15.9**	16.9**	12.9**	13.5**	11.9**
	(2.9)	(3.4)	(2.8)	(2.7)	(2.7)
1-year graph × Sophistication dummy	-5.2	-9.6*	-1.0	-2.4	-2.7
	(4.4)	(4.4)	(4.4)	(4.4)	(4.4)
5-year graph × Sophistication dummy	-11.5**	-9.6*	-4.8	-6.7	-3.6
	(4.4)	(4.4)	(4.5)	(4.5)	(4.4)
Sophistication dummy	4.5	11.8**	5.6	9.2*	11.5**
	(3.6)	(3.6)	(3.6)	(3.6)	(3.6)
Sample size	597	597	597	597	597

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

Table 7. 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, the likelihood that they would change their portfolio decision if they consulted a professional investment advisor, and how knowledgeable of an investor they consider themselves to be. 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 8. Determinates of Trading Frequency

The dependent variable is the total number of days on which a portfolio reallocation was executed for a subject. *Biannual viewing* is a dummy for whether the subject was paid to view his portfolio return 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 gamble we offered. *Discount rate* is the maximum weekly interest rate at which the subject chooses the hypothetical earlier payment.

Biannual viewing	-4.35**
	(0.55)
Portfolio-level return reporting	-1.99**
	(0.62)
1-year graph	-0.69
	(0.87)
5-year graph	-0.92
	(0.87)
Asset class mixes shown	-0.86
	(0.62)
Loss averse	-0.22
	(0.56)
Discount rate	0.08
	(1.87)
Constant	8.20**
	(0.96)
Sample size	597

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

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

Figure 1. Initial Screen in Condition with Biannual Returns Viewing, Individual Asset Return Reporting, and Historical Individual Asset Class Five-Year Return Graphs

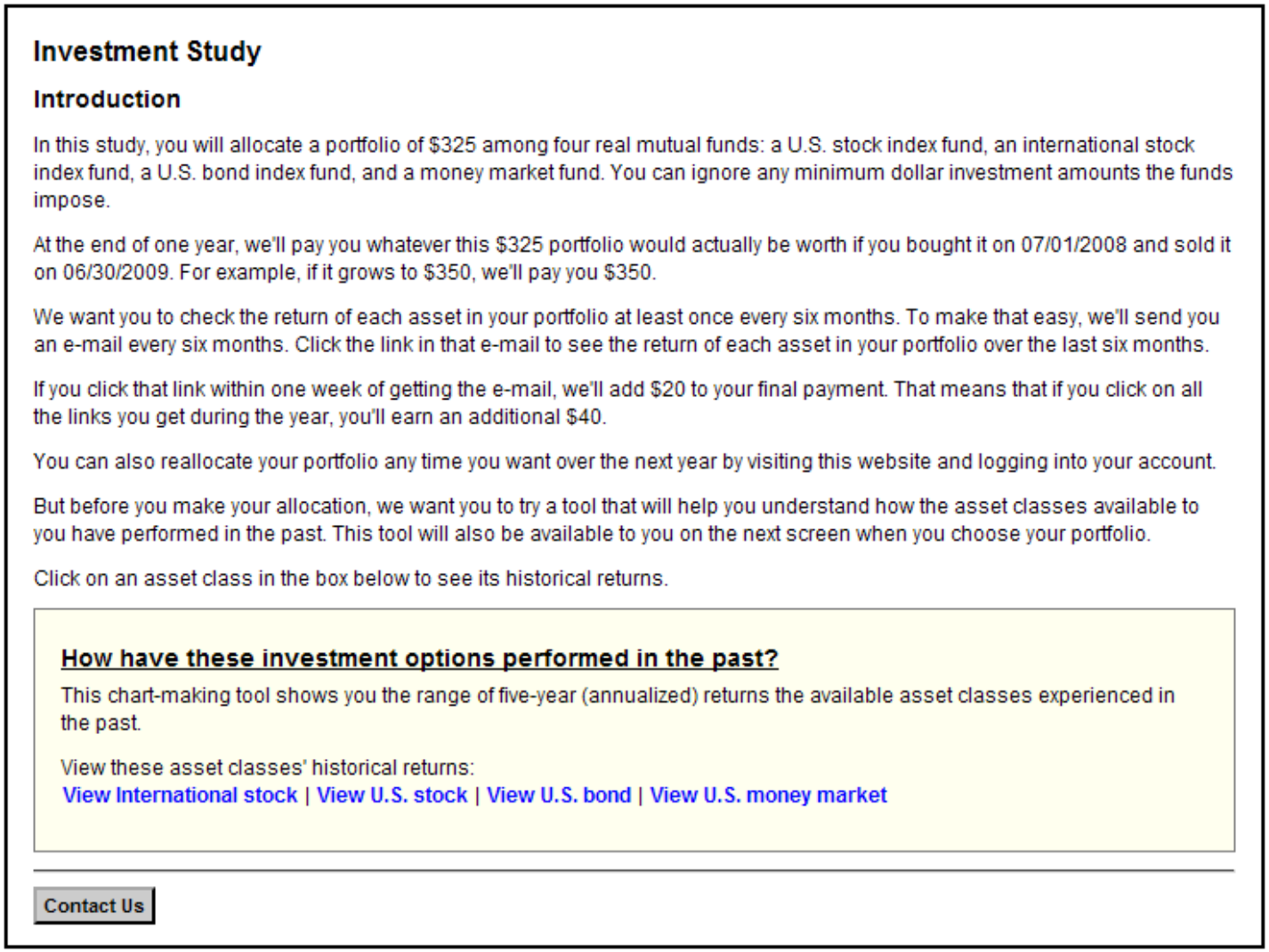


Figure 2. Recent Returns Screen in Weekly Viewing of Ongoing Individual Asset Returns Condition

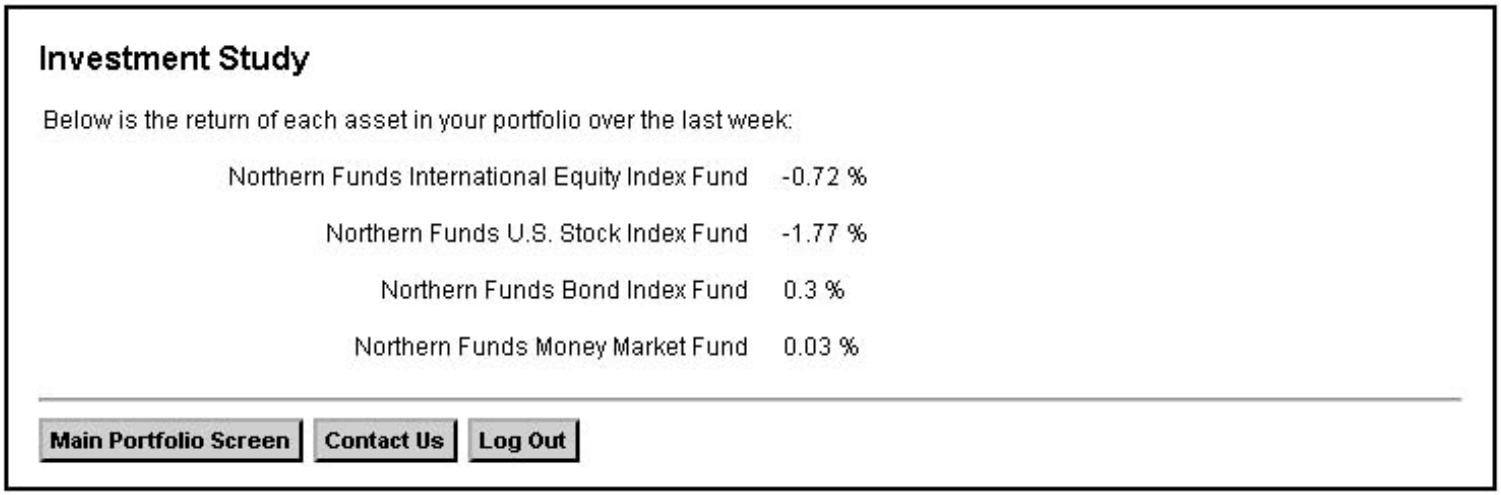


Figure 3. Recent Returns Screen in Biannual Viewing of Ongoing Overall Portfolio Returns Condition

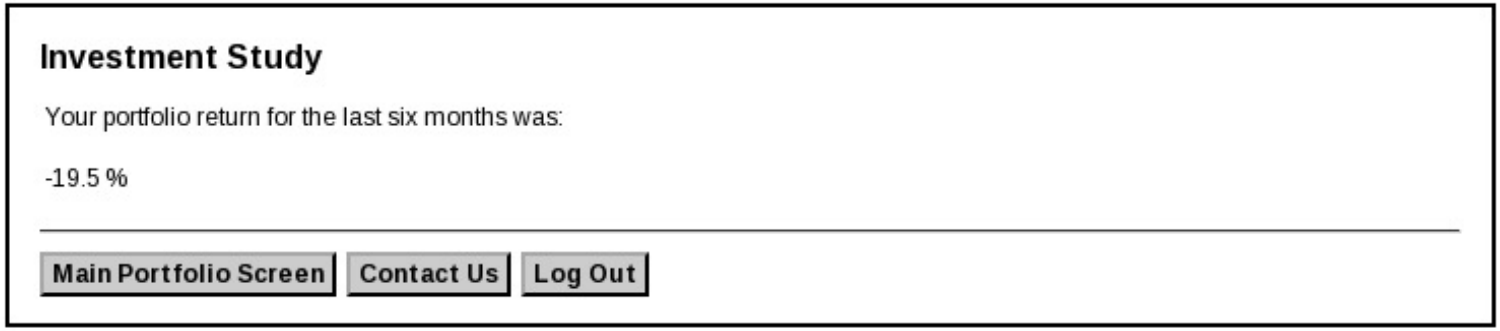


Figure 4. Historical Returns Graphing Tool in One-Year Return, Only Single Asset Class Returns Shown Condition

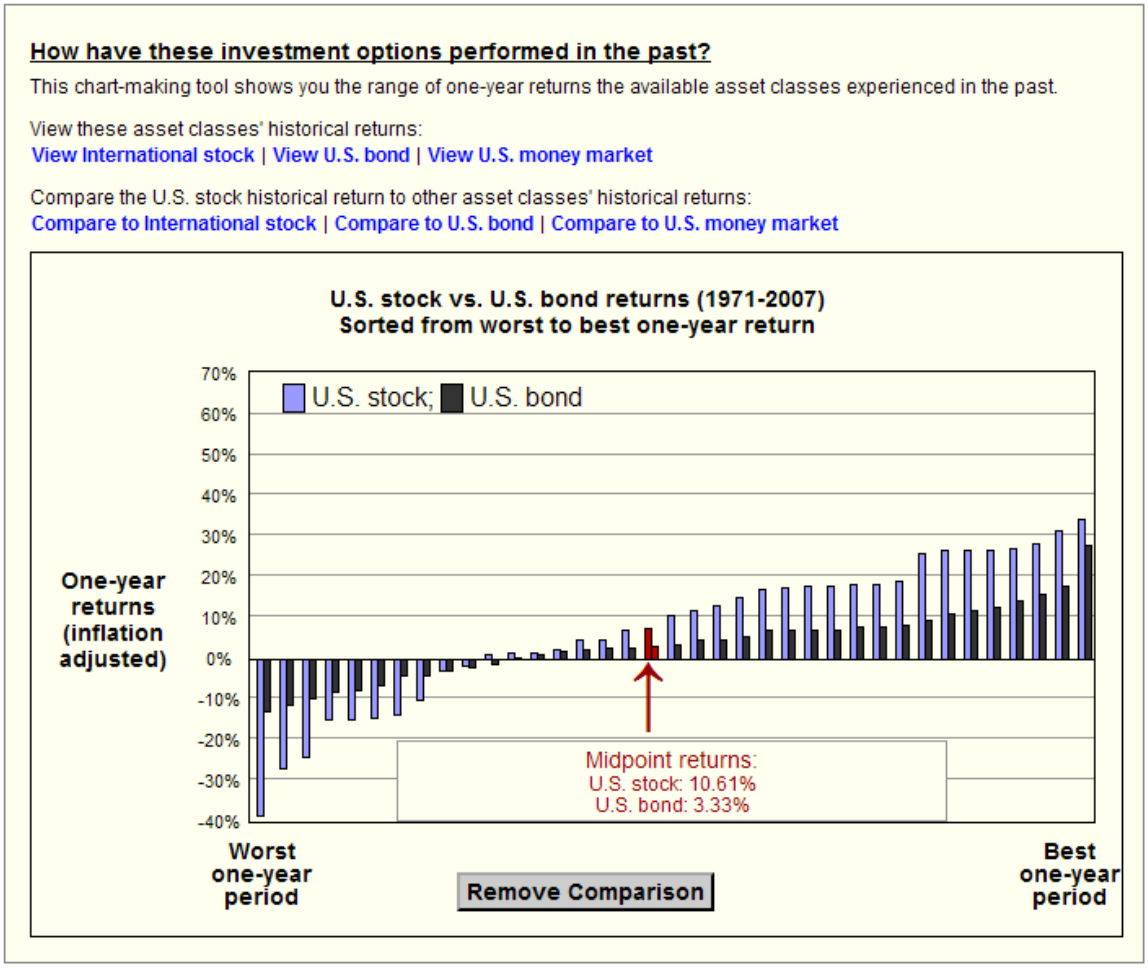


Figure 5. Historical Returns Graphing Tool in Five-Year Annualized Return, Arbitrary Portfolio Mixes Shown Condition

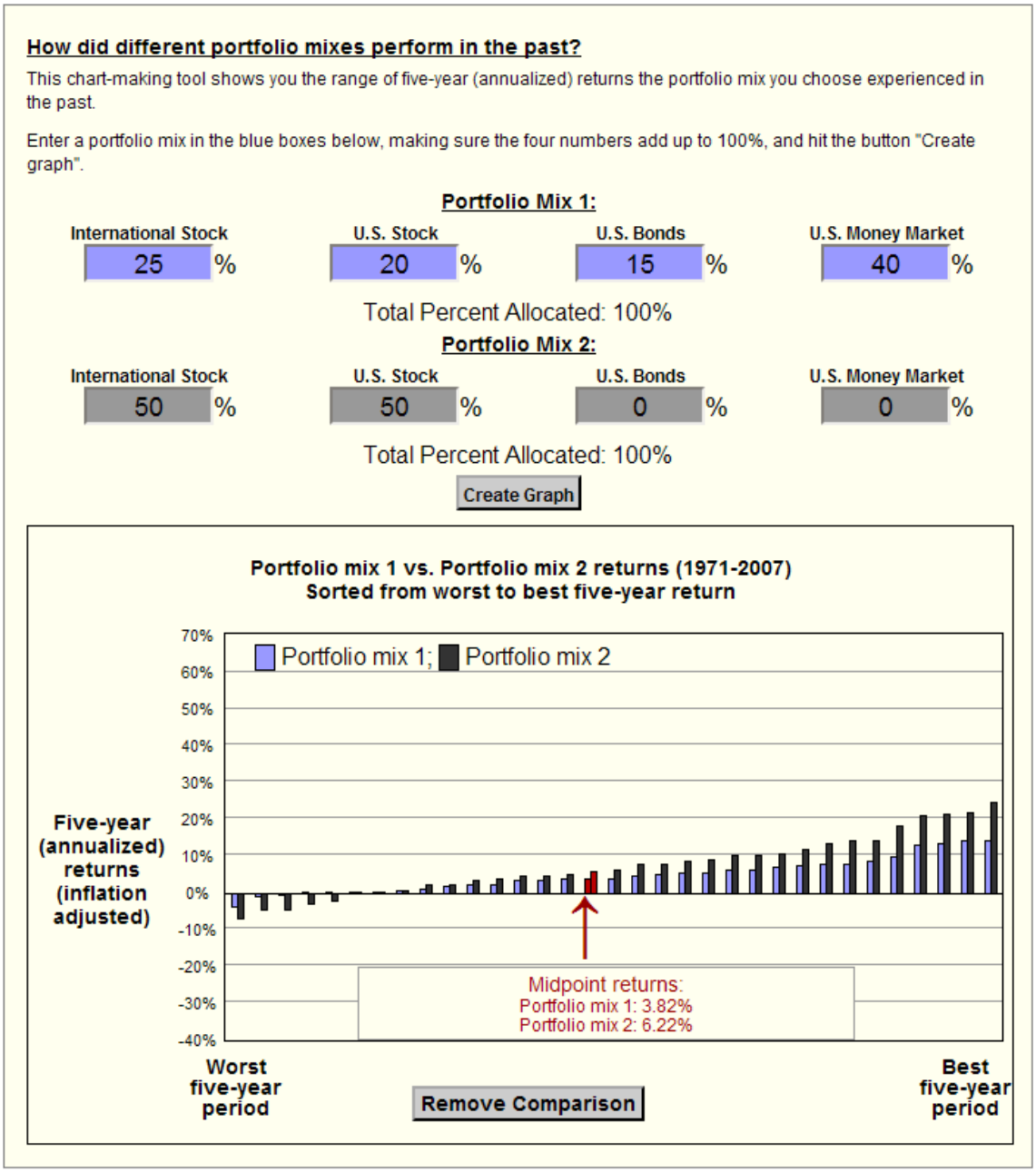


Figure 6. Initial Portfolio Allocation Screen in Five-Year Annualized Return, Only Single Asset Class Returns Shown Condition

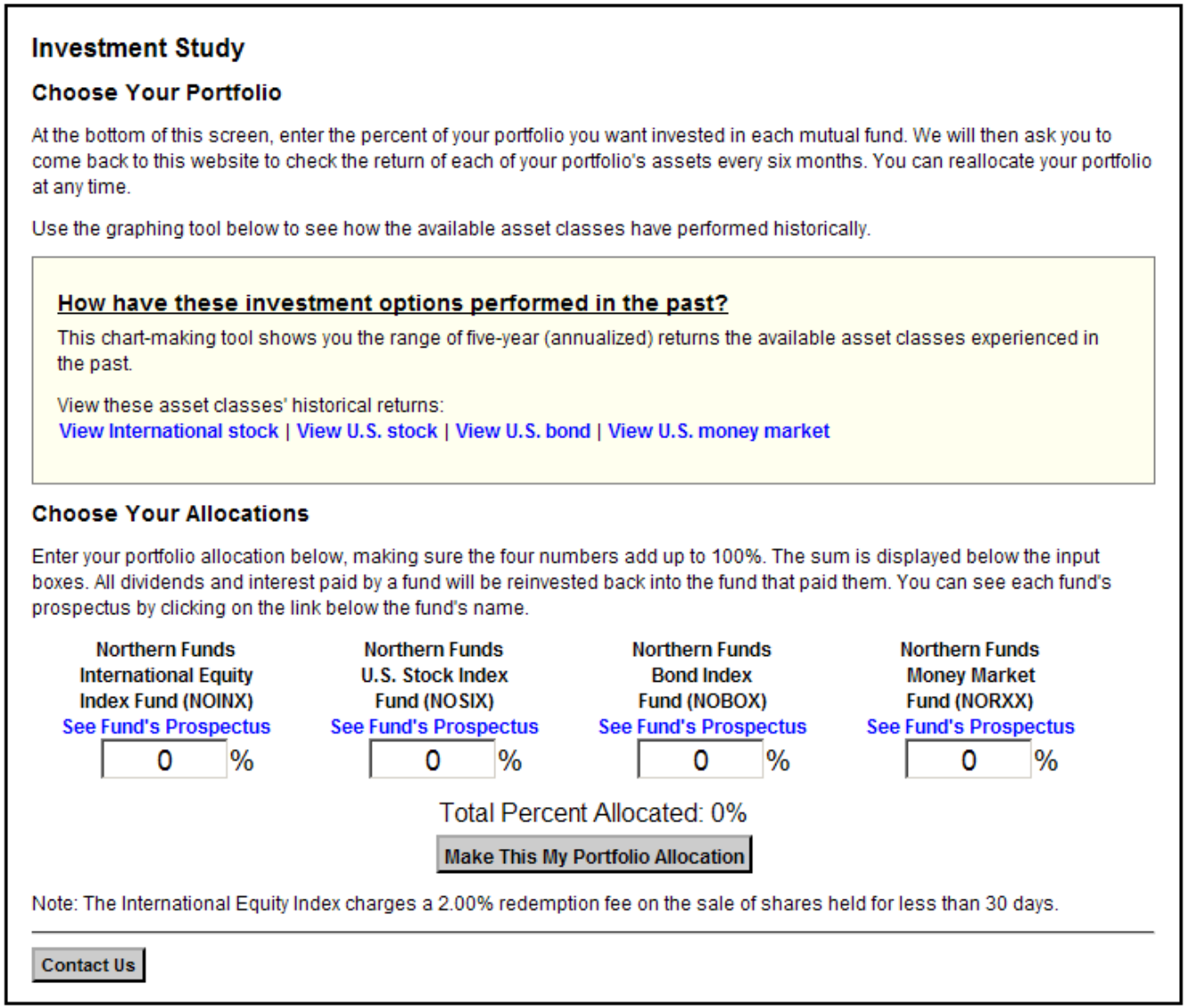
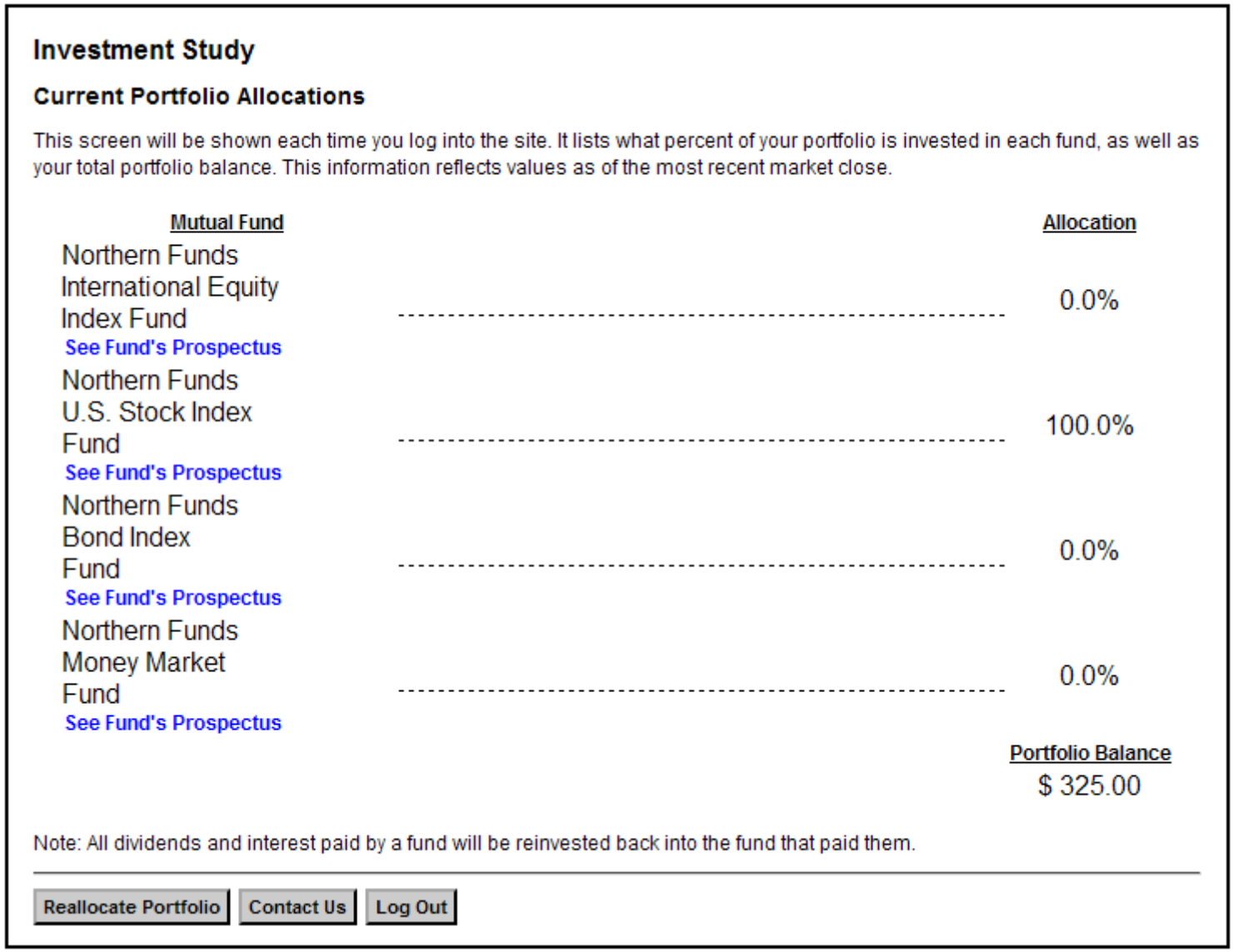


Figure 7. Portfolio Status Page



Investment Study

Current Portfolio Allocations

This screen will be shown each time you log into the site. It lists what percent of your portfolio is invested in each fund, as well as your total portfolio balance. This information reflects values as of the most recent market close.

Mutual Fund	Allocation
Northern Funds International Equity Index Fund See Fund's Prospectus	0.0%
Northern Funds U.S. Stock Index Fund See Fund's Prospectus	100.0%
Northern Funds Bond Index Fund See Fund's Prospectus	0.0%
Northern Funds Money Market Fund See Fund's Prospectus	0.0%

Portfolio Balance
$ 325.00

Note: All dividends and interest paid by a fund will be reinvested back into the fund that paid them.

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Figure 8. Portfolio Reallocation Screen in One-Year, Arbitrary Portfolio Mixes Shown Condition