Does Aggregated Returns Disclosure Increase Portfolio Risk Taking?

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Abstract: Many experiments have found that participants take more investment risk if they see returns less frequently, see portfolio-level returns (rather than each individual asset's returns), or see long-horizon (rather than one-year) historical return distributions. In contrast, we find that such information aggregation treatments do not affect total equity investment when we make the investment environment more realistic than in prior experiments. Previously documented aggregation effects are not robust to changes in the risky asset's return distribution or the introduction of a multi-day delay between portfolio choice and return realizations.

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A series of experiments has found that participants are more willing to invest in risky assets with positive expected returns if they receive information on aggregated returns rather than on individual component returns. Information aggregation along various dimensions produces this effect: (i) reporting long-horizon portfolio returns at infrequent intervals rather than short-horizon returns every period (Gneezy and Potters, 1997; Thaler et al., 1997; Gneezy, Kapteyn, and Potters, 2003; Bellemare et al., 2005; Haigh and List, 2005; Sutter, 2007; Langer and Weber, 2008; Fellner and Sutter, 2009; Erikson and Kvaløy, 2010a, 2010b; van der Heijden et al., 2012), (ii) reporting portfolio-level returns rather than returns for each individual asset separately (Anagol and Gamble, 2013), or (iii) reporting historical long-horizon return distributions of asset classes rather than historical one-year return distributions of asset classes (Benartzi and Thaler, 1999).

These results are predicted under certain conditions by 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). 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. When gambles each have positive expected value and are not perfectly correlated with each other, the sum of their outcomes is usually less likely to be a loss than is each outcome individually.[1] Thus, if aggregated information disclosure encourages participants to integrate multiple gamble outcomes into a single mental account and derive gain-loss utility from only their combined outcome, then such disclosure makes the gambles more attractive to a loss-averse individual.

The strength and consistency of the experimental results constitute compelling evidence that information aggregation can increase risk-taking. In this paper, we consider a related but

[1] 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.

separate question: Would a financial institution increase the portfolio risk-taking of its clients if it started disclosing returns at a more aggregated level? Numerous authors have extrapolated from the existing experimental literature to suggest that the answer to this question is “yes.” For example, Gneezy and Potters (1997) write, “Our results suggest that providing investors with less frequent information feedback about how a particular risky fund is doing might make the fund appear more attractive.” Haigh and List (2005) speculate that “market prices of risky assets might be significantly higher if feedback frequency and decision flexibility are reduced.”

However, the laboratory environments of previous experiments differ in many ways from the typical investment setting. For instance, the experiments to date have been conducted over the course of one short laboratory session, while the typical investment horizon is many years. Most previous experiments have used laboratory assets whose return distributions differ from the return distributions of typical financial assets. In this paper, we show using two experiments that aggregation effects on risk-taking are not robust to making the experimental environment closer to the typical investment environment. In particular, we can identify that modifying the risky asset’s return distribution or the delay between portfolio choice and return realization is able to nullify or even reverse aggregation effects.

Our first experiment simultaneously made many aspects of the experimental environment closer to the typical investing environment. We recruited 597 participants from the general U.S. adult population to participate in a year-long study from 2008 to 2009. Each participant 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. Participants were free to reallocate their portfolio throughout the year, just as if they were making real investments in these mutual funds. We paid each participant 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. The large per-participant payment ensured that participants remained interested in their experimental portfolio through the end of a one-year experiment.

We randomly varied the extent of information aggregation along four dimensions. The first treatment dimension varied how frequently participants saw their returns by paying half of participants to view their weekly returns on our study website once per week and paying the other half to view their biannual returns on our website once every six months. We paid participants to view their returns on our website rather than simply emailing them their returns because emails are easily ignored, which would create less variation in viewing frequency across conditions, reducing our ability to detect a viewing frequency effect on risk-taking. In our experiment, returns were viewed 87% of the time a viewing was incentivized in the weekly condition, and 74% of the time in the biannual condition. To the extent that real-world return disclosures are more likely to be ignored than our incentivized disclosures, varying return disclosure frequency outside our experiment will generate even less variation in return viewing frequency, resulting in even weaker potential effects on risk-taking.

The second treatment dimension varied the level of detail participants saw when they viewed their weekly or biannual returns. Half of the participants saw only their overall portfolio return over the last week or six months. The other half of participants saw the return over the last week or six months of each individual asset they were holding. Because a screen available to all participants showed the dollar value of each asset in their portfolio, participants 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 provide convenient access to these previous asset values, making this calculation more difficult. Similarly, participants 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 treatment dimension varied the historical returns information shown to participants. We showed some participants 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 participants no historical returns information at all in order to see whether allocations were affected by seeing any version of the returns graphs.

The fourth treatment dimension varied whether participants who saw the historical returns graphs could access information about the historical performance of portfolios that held any combination of the four asset classes. Some participants could only see historical return distributions of four "pure" portfolios, each invested 100% in one of the four asset classes offered. Other participants could, via a Web interface, see return distributions of portfolios invested in whatever mix of asset classes they wished. This latter treatment is the backward-looking analogue of the previously described overall portfolio return reporting treatment. Giving participants the option of seeing mixed portfolio returns might make more apparent the diversification benefits of holding multiple asset classes, thus encouraging greater investment in risky assets.

We find that none of the four aggregation treatments significantly increased risk-taking, as measured by the fraction of the portfolio invested in equities. We have enough statistical power to reject increases of more than 2 to 4 percentage points, depending on the treatment. Our null effects are unlikely to be due to our treatments having no effect on the information participants received. There is a significant difference between how often participants in the weekly versus biannual return viewing treatments viewed returns on our study website, and weekly viewing treatment participants are significantly more likely than biannual viewing treatment participants to report in an exit questionnaire that study participation made them see market returns more often. We additionally find that showing either the one-year or five-year historical returns graphs to participants increased their equity investment by 11 to 12 percentage points (relative to participants who were not shown any historical returns), indicating that the graphs changed participants' beliefs about future asset returns.

Our null effects instead appear to be due to the fact that the aggregation treatment effects documented in previous studies are sensitive to the risky asset's return distribution. Our experiment in fact replicates the Benartzi and Thaler (1999) result that showing participants long-horizon historical U.S. stock returns increases investment in U.S. stocks relative to showing participants one-year historical U.S. stock returns. But showing participants long-horizon historical *international* stock returns *decreases* investment in international stocks, so there is no change in participants' *total* equity allocations.

Our second experiment, run in 2013, demonstrates that the return viewing frequency effect is similarly sensitive to the return distribution. We conducted this additional experiment to more precisely identify the reasons that our first experiment's return viewing frequency results differ from those in the previous literature and to rule out the possibility that they were driven by the economic events of 2008-2009. We began by successfully replicating the return viewing frequency effect of the earlier literature: Seeing ongoing returns less frequently within a one-hour laboratory session increased risk-taking when the risky asset had a binary return distribution of +250% with 1/3 probability and -100% with 2/3 probability, as is the case in Gneezy and Potters (1997) and the many subsequent studies that adopt their experimental design (Gneezy, Kapteyn, and Potters, 2003; Bellemare et al., 2005; Haigh and List, 2005; Sutter, 2007; Langer and Weber, 2008; Fellner and Sutter, 2009; Erikson and Kvaløy, 2010a, 2010b; van der Heijden et al., 2012). Such a return distribution is much more extreme than that of most common financial assets, including the mutual funds in our first experiment. We find that the return viewing frequency effect disappeared when we changed the risky asset's return distribution to +25% with 1/3 probability and -10% with 2/3 probability while keeping the total potential dollars at risk the same. Return viewing frequency similarly had no effect when we told participants that the risky asset's return distribution matched the historical U.S. stock market return distribution.

Even when the risky asset had the binary return distribution of (250%, 1/3; -100%, 2/3), we find that the return viewing frequency effect disappeared if the amount of time that elapsed between the portfolio choice and the return viewings lengthened, as in our first experiment. In all previous studies, participants saw their returns immediately after making their portfolio choice. We ran experimental conditions where participants in the frequent viewing treatment saw their returns once per week for three weeks, while participants in the infrequent viewing treatment only saw the sum of their three returns three weeks in the future. Return viewing frequency had no effect on risk-taking when return disclosure was delayed in this way, regardless of whether the risky asset's return distribution was (250%, 1/3; -100%, 2/3), (25%, 1/3; -10%, 2/3), or matched the historical U.S. stock market return distribution.

1. First aggregation experiment design

1.1. Participant recruitment

We recruited participants in the summer of 2008 for a one-year investing experiment through the market research firm MarketTools. We requested that participants be at least 25 years old and have an annual income of at least $35,000 so that it was more likely that they would have some investable assets. All interaction with the participants occurred through the Internet; we had no direct contact with participants.

Figure 1 shows the number of participants 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 thin line in the top graph) and the VIX index of expected annualized S&P 500 volatility (the thick line in the bottom graph). Even though our experiment spanned the market collapse in the fall of 2008, 94% of our participants made their *initial* portfolio choices between June 23, 2008 and July 14, 2008. Ninety-nine percent had completed this task by July 30, 2008, and the remaining 1% had completed the task by August 30, 2008. The market's precipitous fall did not commence until after the September 15, 2008 bankruptcy of Lehman Brothers. The VIX averaged only 24.5% from June 23 to July 14, 24.1% from June 23 to July 30, and 22.6% from June 23 to August 29. These averages are not far from the annualized monthly return standard deviation of large-cap equities from 1926 to 2007 of 20.0% (as reported by Ibbotson Associates), and are far from the VIX levels later in 2008 that would herald the arrival of the financial crisis; the VIX rose to 31.7% on September 15, 2008 and peaked at 80.9% on November 20, 2008. Hence, our participants made their initial portfolio allocations in a non-crisis environment, many weeks *before* we entered a bear market of historic proportions. In Section 3.3, we will discuss evidence that our null treatment effects are not explained by the market's 18% decline from its October 2007 peak to the beginning of our recruiting period. In addition, the follow-up experiments presented in Sections 4 and 5 show that null aggregation effects can be reliably produced outside of a bear market.

The initial invitation text introduced the faculty authors with our university affiliations in an effort to augment the credibility of the study. It then informed participants 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 the participants whatever their initial $325 portfolio was worth at that time (we imposed no cap on possible payoffs), plus an additional amount for periodically checking their portfolio's return on the study website. The text concluded by telling the participants that we expected the initial portfolio allocation task 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 returns.

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

Giving informed consent took participants to a registration page where they supplied their name and contact information and chose a password. In order to prevent signing up for the study more than once, we blocked attempts to register multiple times from the same IP address. Upon registration, an e-mail was sent to participants with a link to click on in order to activate their accounts. Using an emailed activation link ensured that we had an active email account to which we could send the returns-checking links. The link then took subjects to a login screen.

We recruited 600 participants, but three of them did not continue after registering. Therefore, our final sample consists of 597 participants whom we randomly assigned to one of eighteen experimental cells. Table 1 shows the distribution of our sample among the experimental cells. Treatments varied on four dimensions: whether participants were induced to view their returns weekly or biannually; whether ongoing returns were reported to participants at the portfolio level or separately by asset; whether participants saw no historical returns graphs, one-year historical returns graphs, or five-year historical returns graphs; and whether participants who were shown graphs were able to see historical returns of only portfolios invested entirely in a single asset class or portfolios that held any mix of asset classes. All participants who did not see a historical returns graph had their ongoing returns reported to them at the aggregated portfolio level only.[2] The remaining treatment assignments are independent of each other. We will describe each experimental condition in further detail in Sections 1.3 and 1.6. Online appendix figures show additional representative screenshots from the experimental website.

1.2. Opening instructions screen

After logging in, participants received a more complete description of the study instructions. The instructions reiterated the nature of the portfolio allocation task and the compensation scheme, and informed participants that they could reallocate their portfolio any time during the year by logging into their account on the website. Participants 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 the conditions where relevant, participants were introduced to the historical returns graphing tool.

[2] We allocated extra subjects to the experimental cells where ongoing returns were reported at the portfolio level and no historical returns graphs were shown because these treatments were most similar to the Gneezy and Potters (1997) experiment. If we did not replicate their viewing frequency effect in the full sample, we could estimate the treatment effect using only these 120 subjects to see if interactions with the historical returns graphs and asset-by-asset return reporting were responsible for the non-replication. Results on this subsample are consistent with those on the full sample, so we do not report them later in the paper.

1.3. Historical returns graph treatments

For 80% of our participants, the bottom of the instructions screen described in the previous subsection introduced a graphing tool designed 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 participants did not see the graphing tool and did not receive any alternative information on historical returns. The graphs generated by the tool are closely 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 with its value labeled.[3] We used the S&P 500, MSCI EAFE, Barclays Capital 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 a return series to have a unique median, it must have an odd number of years. Therefore, we used the period from 1971 to 2007 for all four asset classes.[4]

Participants who had the graphing tool available to them were required to click 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

[3] A programming error caused the bar immediately to the left of the median return to be highlighted instead of the median for the first six months of the experiment, even though the correct median return value was displayed in the graph's label. The 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.

[4] In addition, the Barclays Capital index starts in 1976. We constructed 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 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.

five-year graph. Benartzi and Thaler (1999) used simulated 30-year returns in their long-horizon condition, which produces a starker contrast against one-year returns than the contrast between five-year returns and one-year returns. However, 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.

The second dimension was whether participants 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 by typing in the portfolio allocation on the website. The graphing tool allowed participants to see the return distributions of two different asset classes or portfolios side-by-side on the same graph. The vertical axis on all graphs was fixed to range between -40% and 70%, so that differential treatment effects across graphs could not be attributed to visual scaling effects.

Figure 2 contains a screenshot of the tool that showed one-year returns of pure portfolios, while Figure 3 contains a screenshot of the tool that showed five-year returns of arbitrary asset class mixes.

1.4. Initial portfolio allocation

Participants made their asset allocations by specifying portfolio percentages to be invested in each investment option. For participants 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 participants 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.

Participants 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. We provided links to each fund's prospectus. We also informed participants that the International Equity Index Fund charged a 2% redemption fee on the sale of shares held for less than thirty days. For participants 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 portfolio decisions. Participants could take as long as they wanted to make their portfolio decision. We did not (and could not) prevent participants from consulting information sources available outside of our website.

1.5. Post-allocation questionnaire

After participants 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. In order to assess which participants were most likely to suffer from myopic loss aversion, we also offered participants 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 International 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 up-front 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, participants who refuse the gamble are particularly likely to be loss averse and prone to engage in mental accounting. Fehr and Goette (2007) show that in a field experiment on labor supply, only workers who rejected a similar gamble (50% chance of winning 8 Swiss francs and 50% chance of losing 5 Swiss francs) exhibited a negative elasticity of effort per hour with respect to an exogenous increase in the piece wage rate, consistent with their daily labor supply being determined by loss-averse preferences that are evaluated each day with a reference point around a target daily income level.

Upon finishing the questionnaire, participants were taken to a page that showed their current investment allocation and total balance. At this point, participants could log out. On subsequent logins to the site that were not initiated by clicking an e-mailed link (described in Section 1.6), participants would see this portfolio status page first.

1.6. Ongoing returns viewing treatments

During the one-year duration of the experiment, half of participants 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 participants' 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 would earn an additional $52. The other half of participants 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 participants would have otherwise received their 26th and 52nd e-mails if they had been assigned to receive weekly e-mails. If participants receiving biannual e-mails clicked the link within a week of receiving the e-mail, we added $20 to their final payment. We offered only $20 per viewing for this group because we anticipated that participants 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 participants saw when they clicked on the e-mailed link. Half of participants saw a screen that showed the prior week or prior six-month return of each individual asset held as of the email date. The other half of participants saw a screen that showed only the overall prior week or prior six-month return of their portfolio. 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 participants receiving biannual e-mails did not see the returns screen more frequently than once every six months.

1.7. Treatment of interest, dividends, and trades

Dividends and interest were automatically reinvested in the fund that paid them.[5] This reinvestment policy was disclosed to participants directly above the boxes in which they entered their portfolio allocation. All participants 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. The reallocation screen showed the graphing tool relevant for participants' experimental condition, links to the fund prospectuses, the current percentage allocations across the four mutual funds, and a note about the international fund redemption fee. Four input boxes allowed participants to specify a new portfolio allocation. Trades were executed at the next close of the U.S. markets and could be cancelled by participants up to that time.

1.8. Exit questionnaire

At the end of the one-year investment period, we administered an exit questionnaire to participants. We will use in our analysis the questions that elicited beliefs about stock market return autocorrelations (to assess the extent to which the 2008 bear market might have affected participant choices) and the effect study participation had on participants' attention to market fluctuations (to confirm that our ongoing returns viewing treatments actually affected the frequency with which participants saw market returns). Of the 597 participants, 570 (95%) completed the exit questionnaire.

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[5] 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 email 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 email was sent) in accordance with the subjects' initially chosen asset allocation. This reallocation raised average equity allocations by 1.0 percentage point among subjects receiving weekly emails and 2.2 percentage points among subjects receiving biannual emails.

2. First aggregation experiment results

2.1. Participant characteristics

Table 2 displays demographic and financial summary statistics for our participants. This information was collected in the questionnaire administered immediately after the initial portfolio allocation. Men slightly outnumber women, and although all ages have substantial representation in our sample, the young are slightly overrepresented (33% of participants are 35 or younger). Our participants are relatively well-educated, with 56% reporting they hold a bachelor's degree or higher. The high average level of education may be due to our request for participants with annual incomes above $35,000; only 5% of participants report an income less than that threshold, and the median participant reports an income between $50,001 and $75,000. Total bank, brokerage, and retirement account assets exceed $75,000 for 38% of our sample, and 29% of the sample reports assets in excess of $100,000. Only 20% of our sample reports holding no stocks whatsoever in their personal portfolio.

Since the experimental setup was simple (from the perspective of an individual participant) and the assets were passively managed funds in familiar asset classes, participants did not necessarily need a long time to make a considered decision. The median participant submitted her initial portfolio allocation 13 minutes after login. A small number of participants took an extremely long time between first login and final portfolio choice. The longest gap was 58 days, but 96% of participants took less than 48 hours.

2.2. Average asset allocations

Participants initially allocated on average 65.7% of their portfolio to equities (with 34.8% invested in international equities and 30.9% invested in U.S. equities), 18.6% to bonds, and 15.8% to money markets. The relatively high allocation to international equities may be due to the strong performance of this asset class in the time period immediately preceding the experiment; the most recent one-year before-tax return reported in the fund prospectus was

25.8% for the international index fund versus 15.6% for the domestic equity index fund. The average participant made a positive allocation to 3.66 out of the four asset classes.

2.3. Return viewing frequency

Table 3 shows that our periodic e-mails to participants 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, participants who received weekly e-mails logged into the website 60.7 times on average versus only 18.2 times for participants who received biannual e-mails. Under the weekly e-mail treatment, 45.3 of those 60.7 logins occurred because participants 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, participants clicked 73.8% of links sent, so they saw the returns screen an average of 1.5 times. Participants in both treatments logged in about 16 times on average when not prompted by an e-mail.

The extra return viewings by participants 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 participants, “Did participating in this study make you see the ups and downs of the market more often than you otherwise would have?” Participants 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 participants reported that participation made them see the ups and downs of the market more often versus 57% of participants in the biannual e-mail treatment. Because these responses do not indicate *how much* more often study participation made them see returns, this 22% gap ($p < 0.01$) likely understates the effect that being in the weekly treatment had on return viewing relative to being in the biannual treatment. Only 1% of participants in the weekly e-mail treatment and 2% of participants in the biannual e-mail treatment reported that the study caused them to see market fluctuations less often.

The fact that a $1 payment was sufficient to induce participants to view their portfolio returns almost every week indicates that they did not find the time cost of such viewing to be high, nor the information revealed by such viewing to be particularly painful, even though the portfolio returns in this experiment were likely to be quite informative about the returns of their total financial portfolio. Andries and Haddad (2015) and Pagel (2016) present theoretical models where people are inattentive to their portfolios because they trade off the benefit of improved economic choices from better information against the fear of learning negative news. The high return-viewing compliance we obtain from a $1 payment places an upper bound on how strong this fear can be.

2.4. Effect of returns aggregation on asset allocations

The main dependent variable in our analysis of how aggregating returns information affects risk-taking is the fraction of the experimental portfolio that is invested in equities at the *beginning* of the experiment. Any effect of viewing the historical returns graphs would likely be most easily detected in the initial portfolio allocation, immediately after all participants in the historical graph conditions were required to view the graphs. Also, the previous literature on return feedback frequency finds that individuals who know they will receive frequent feedback reduce their demand for risky assets starting in the very first period of the experiments, indicating that they *prospectively* anticipate the disutility from disaggregated ongoing return disclosure (Gneezy and Potters, 1997; Gneezy, Kapteyn, and Potters, 2003; Bellemare et al., 2005; Haigh and List, 2005). In Section 3.2, we show that our results do not meaningfully change when we consider allocations at later dates.

The first column of Table 4 reports coefficients from OLS regressions where the dependent variable is the total fraction of the portfolio invested in equities (U.S. plus international) at the beginning of the experiment and the explanatory variables are treatment dummies. We find that anticipating biannual rather than weekly e-mails had a –0.03 percentage point effect on total equity shares. We can reject at the 95% confidence level an increase of more than 3.6 percentage points, which would be a treatment effect that is an order of magnitude smaller than the 28.7 percentage point increase Thaler et al. (1997) find when participants are shown yearly ongoing returns rather than monthly ongoing returns.[6] We also find that telling participants that they would see ongoing returns consolidated at the portfolio level rather than separately by each asset insignificantly decreased total equity investment by 1.7 percentage points, with a 95% confidence interval of -5.4% to 2.0%. This contrasts with Anagol and Gamble (2013), who find that portfolio-level ongoing returns reporting increases equity investment by 4.2 percentage points. (However, their finding is difficult to interpret because their randomization failed to produce a balanced sample across treatment groups.)

Being exposed to *any* historical returns graph significantly raised the initial equity share by 9.2 to 11.2 percentage points relative to not having access to a historical returns graph. This suggests that some participants were unaware of how attractive stock returns have been historically. But it does not matter for total equity allocations whether the distributions of one-year returns or five-year annualized returns are presented. In fact, our participants who saw the historical five-year return distributions initially allocated *less* to equities than participants who saw the historical one-year return distributions, although the difference is not statistically significant (95% confidence interval from -5.7% to 1.6%). This finding is contrary to the increases in equity allocations ranging from 19 to 41 percentage points found by Benartzi and Thaler (1999) when their participants were shown simulated 30-year return distributions rather than one-year return distributions. Nor does it seem to matter whether participants were able to see the historical 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 associated with an insignificant 0.5 percentage point increase in equity allocations (95% confidence interval from -3.2% to 4.2%).[7]

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[6] It is difficult to compare the magnitudes of our treatment effects with those of Gneezy and Potters (1997), Gneezy, Kapteyn, and Potters (2003), Bellemare et al. (2005), Haigh and List (2005), and van der Heijden et al. (2012), since the risky assets in their experiments had binary payoffs.

[7] In unreported regressions, we find no significant treatment interactions with holding equities outside the experiment on the total equity share chosen in the experiment.

The subsequent columns of Table 4 show the treatment effect estimates for each of the four asset classes separately. Less frequent ongoing return viewing and being able to see asset class mixes in the historical returns graph have no significant effect on allocations to any asset class. Portfolio-level reporting has a significant effect only on U.S. stock allocations, but with a negative sign, which is the opposite of what Anagol and Gamble (2013) predict. The most interesting results are for the one-year versus five-year historical returns graph treatments. We find that seeing the five-year graph instead of the one-year graph significantly ($p < 0.01$) increased allocations to U.S. stocks by 4.2 percentage points. Thus, we qualitatively replicate the Benartzi and Thaler (1999) result on aggregation of historical U.S. stock return distributions. However, aggregation in the graph also significantly ($p < 0.01$) *decreased* allocations to international stocks by 6.2 percentage points, which is why the overall equity allocation did not change significantly.

Figures 4 and 5 give some insight into why aggregation in the returns graphs decreased allocations to international stocks. Figure 4 shows returns for each asset class from 1971 to 2007, sorted from the worst one-year return to the best one-year return, while Figure 5 shows the analogous figure for five-year annualized returns. The data in these graphs match the data shown to participants in the graph treatments, although participants could only see information for one or two asset classes at a time.

In the one-year returns graph, international stock returns particularly stand out against the other asset classes in the right tail. The largest international stock return is twice the magnitude of the largest U.S. stock return, and the second-largest international stock return is 1.6 times the second-largest U.S. stock return. Everywhere else in the distribution, international stocks have returns that are mostly similar to U.S. stocks. In contrast, in the five-year return graph, the international stock returns in the right tail are closer to the U.S. stock returns—1.2 times at the maximum and 1.5 times at the second-largest value—and they are below U.S. stock returns in much of the remainder of the distribution. Hence, international stocks look relatively less attractive in the five-year graph.[8]

The historical graph aggregation effects contrast with what would be predicted if participants used valuations produced by a piecewise linear loss-averse value function or cumulative prospect theory (Tversky and Kahneman, 1992). To perform these calculations, we assume that: (i) participants who saw one-year graphs perceived each asset class to have 37 possible return realizations corresponding to the 37 annual returns shown in the one-year graphs; (ii) participants who saw five-year graphs perceived each asset class to have 33 possible return realizations corresponding to the 33 five-year annualized returns shown in the five-year graphs; and (iii) each possible realization occurs with equal probability. We use the preference parameter values estimated by Tversky and Kahneman (1992).

The first and third columns of Table 5 show the valuation of each asset class by an individual with piecewise linear loss aversion or cumulative prospect theory preferences, respectively, who perceives return distributions shown in the one-year graphs (Panel A) or the five-year graphs (Panel B). The valuations of all the asset classes rise in the five-year graph condition relative to the one-year graph condition. Since participants cannot increase allocations to every asset class as they move from the one-year to the five-year graph condition, valuations relative to the mean asset class valuation within a condition are more relevant for determining portfolio shares than the absolute valuation. The second and fourth columns show these relative valuations. Under both piecewise linear loss aversion and cumulative prospect theory, moving from the one-year graph to the five-year graph causes both U.S. equities and international equities to look relatively more attractive, which is contrary to our empirical finding that international stock allocations fall when going from the one-year graph to the five-year graph condition. The increase in stocks' attractiveness under piecewise linear loss aversion and

[8] The salience theory of Bordalo, Gennaioli, and Shleifer (2012) is motivated by intuitions about the disproportionate influence of salient states that are similar to what we have described here. A document describing the application of their theory to our experimental setting is available from the authors upon request.

cumulative prospect theory is driven by the dramatic increase in their returns conditional on losses when moving from the one-year to the five-year graphs.

3. First aggregation experiment robustness checks

3.1. Interactions with strength of loss aversion and mental accounting

Aggregating reported returns is thought to increase risk-taking because individuals are loss averse and engage in mental accounting, and because aggregation encourages the integration of multiple gambles into a single mental account. It is possible, then, that we would find positive aggregation effects on equity investment among a subset of individuals who are particularly prone to loss aversion and mental accounting even though there are no positive effects on average over our entire sample. We identify such individuals as the 47% of our sample who rejected the equal chance of winning $8 or losing $5, a choice that is difficult to explain in the absence of myopic loss aversion (Rabin, 2000; Barberis, Huang, and Thaler, 2006) and is associated with making labor supply decisions using a target daily income level as a reference point (Fehr and Goette, 2007).

Table 6 adds interactions between the treatment dummies and a dummy variable for rejecting the small gamble to the Table 4 total equity share regression. We find no significant evidence of positive aggregation effects on equity allocations among the participants who reject the small gamble. The point estimate of the biannual e-mail treatment effect is 2.5 percentage points higher among gamble rejecters than gamble accepters, but this difference is not significant. The overall biannual e-mail effect among gamble rejecters, –1.6 + 2.5 = 0.9 percentage points, is not significantly different from zero. Reporting portfolio-level returns causes gamble rejecters' equity allocations to change by –2.9 + 2.5 = –0.4 percentage points, but this effect too is not significant. Gamble rejecters who saw the five-year graphs allocated a statistically insignificant 6.4 + 6.0 – (12.2 – 1.6) = 1.8 percentage points more to equities than gamble rejecters who saw the one-year graph, and gamble rejecters who were able to see graphed return distributions of asset class mixes allocated an insignificant –1.4 + 3.7 = 2.3 percentage points more to equities than gamble rejecters who could see only single asset class return distributions.

3.2. Do aggregation effects emerge with a delay?

The participants in the experiments conducted by Gneezy and Potters (1997), Gneezy, Kapteyn, and Potters (2003), Bellemare et al. (2005), and Haigh and List (2005) did not need to first experience disaggregated ongoing returns disclosure before reducing their portfolio risk. Instead, they reduced their demand for risky assets starting in the very first period of the experiments. It is nevertheless possible that our participants did not initially realize how disaggregated ongoing returns disclosure would affect their utility and instead learned gradually as they became exposed to these returns. This would lead to a relative decrease in the disaggregated groups' portfolio risk as the experiment progressed. Our participants were not inactive in their experimental accounts; the median number of days on which a participant made a reallocation is 2, and the average is 4.6.

We test for the gradual emergence of a positive aggregation effect on risk-taking by using the total equity share halfway into the experimental period as the dependent variable in the first column of Table 7, and the total equity share at the end of the experiment as the dependent variable in the second column. Equity share at the halfway point is measured eight days after participants receiving weekly e-mails got their 26th e-mailed returns-checking link, and eight days after participants receiving biannual e-mails got their first returns-checking link.[9]

The coefficient estimates 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 grows slightly from 0.03 percentage points at the beginning of the investment period (in Table 4) to 2.4 percentage points at the halfway

[9] By measuring allocations at this 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 available to weekly subjects each week for the prior six months.

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 from –1.7 percentage points (in Table 4) to an insignificant 0.1 percentage points at the halfway point and an insignificant –1.2 percentage points at the end of the experiment. Although having seen historical returns graphs continues to raise equity share by about 7 to 9 percentage points through the remainder of the experiment, it still does not matter whether one-year or five-year return distributions were shown. Those seeing five-year graphs hold 1.7 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 not significant. The effect of having a historical returns graphing tool that could show distributions for asset class mixes, which was 0.5 percentage points at the beginning of the experiment, increases slightly to an insignificant 1.5 percentage points halfway through the experiment and flips sign to an insignificant -1.0 percentage point at the end of the experiment.

3.3. Are our aggregation effects nullified by negative expected equity returns?

Could our null effects arise because some participants believed that the expected return of equities was negative due to the market's drop prior to the experiment? The same logic that causes gambles with positive expected returns to appear more attractive under aggregation causes gambles with negative expected returns to appear less attractive under aggregation.

The fact that our participants initially allocated an average of 65.7% of their portfolio to equities, split almost evenly between U.S. and non-U.S. stocks, suggests that they did not in fact believe that the expected return of equities was negative. We further test this story by running regressions of initial equity share on the treatment dummies and their interactions with a dummy for a participant believing that market returns are serially uncorrelated. The pre-experiment market decline is unlikely to cause somebody who believes in serially uncorrelated returns to forecast a negative equity premium. We classify a participant as believing in serially uncorrelated returns based on two questions in the exit questionnaire. The first asked, "Suppose during the month of **January** 2010, the stock market **falls** by 10%. What do you believe that tells you about the stock market's return during **February** 2010?" The second question asked about the scenario where the stock market rises by 10% in January 2010. We count the 49% of participants who chose the response, "The January 2010 stock market return tells me **nothing** about the February 2010 stock market return," for both questions as believing in serially uncorrelated returns.

Table 8 shows the regression results. Contrary to the hypothesis that our treatment effects are attenuated by the market's drop prior to the experiment, we find no significantly positive aggregation treatment effects among those who believe in serially uncorrelated returns.

4. Second aggregation experiment design

The results of our first experiment can be reconciled with the Benartzi and Thaler (1999) finding that seeing longer-horizon historical U.S. stock return distributions increases investment in U.S. equities, as we replicate their result on U.S. equity investments, but we also find that this result does not generalize to other return distributions. On the other hand, our first experiment offers no reconciliation with the previous literature's findings that more aggregated disclosure of ongoing returns increases risk-taking. This inconsistency is especially surprising for the return viewing frequency treatment, since many different experimenters have replicated this effect.

Our first experiment is not well-suited to identify what causes the return viewing frequency effect to disappear, since many aspects of the experimental design differ from the previous literature. In this section, we describe a follow-up experiment that starts with the classic Gneezy and Potters (1997) design and modifies it step by step to move closer to the design of our first experiment.[10] Our objective is to see which modifications eliminate the return viewing frequency effect.

[10] Like other authors in the myopic loss aversion literature, we choose to base these experiments on the Gneezy and Potters (1997) design rather than the Thaler et al. (1997) design because the former is a purer demonstration of myopic loss aversion, while the latter incorporates elements of memory and inference as well.

Our follow-up experiment allows us to study five dimensions along which our first experiment differs from the Gneezy and Potters (1997) experiment:

1. The risky asset in Gneezy and Potters (1997) has an extreme binary distribution: a 2/3 probability of a -100% net return and a 1/3 probability of a 250% net return (i.e., the investment amount plus 2.5 times the investment amount would be returned to the participant). Such return realizations do not resemble the historical returns of the diversified asset classes in our first experiment.
2. The risky asset in Gneezy and Potters (1997) is an artificial laboratory asset with which participants were unlikely to have had prior experience. Participants are much more likely to have contextual knowledge about the real financial assets offered in our first experiment, which could have caused them to apply context-specific heuristics such as "Allocate 100 minus my age to stocks," blunting the effect of aggregation manipulations on portfolio choices.
3. The risky asset realizations in Gneezy and Potters (1997) were shown within the course of a single 40-minute experimental session. The returns in our first experiment were revealed over the course of one year.
4. At the beginning of each round of Gneezy and Potters (1997), participants were given an endowment, and they could invest any portion of this endowment in the risky asset. After the risky asset's outcome was seen, participants were given a fresh endowment, and earnings from previous rounds were not available to invest in the current round. Therefore, each return-viewing event may have also been perceived as a portfolio liquidation event. In contrast, when participants viewed returns in our first experiment, they were not forced to liquidate their portfolios. In the realization utility models of Barberis and Xiong (2009, 2012), investors receive gain-loss utility from returns only when they *sell* a security, not when they view its returns. Therefore, the return viewing frequency effect of Gneezy and Potters (1997) may really be a liquidation frequency effect.

5. Gneezy and Potters ran their experiment on Tilburg University students, whereas our first experiment was run on a non-student population, which might be less prone to behavioral biases.

For our second experiment, we recruited experimental participants in two waves, the first in Spring 2013 and the second in Fall 2013 after we had analyzed the results of the Spring 2013 sample. Participants were paid a $5 participation fee plus any experimental earnings. The first wave consisted of students in the Harvard Decision Science Laboratory subject pool, making the sample closely comparable to the Tilburg University students used by Gneezy and Potters (1997). The second wave included both students and non-students in this subject pool, as well as participants recruited by distributing flyers on the Harvard campus. Only 38% of this second wave were full-time students. The S&P 500 rose 16.0% in 2012 and 32.4% in 2013, so the second experiment occurred in a bull market environment.

Figure 6 summarizes the design of our second experiment. Participants in the first wave participated in both an in-lab study and a post-lab study. Participants were randomly assigned to one of four in-lab conditions, and independently randomly assigned to one of four post-lab conditions. Within each condition, each participant was randomly assigned to a high-frequency viewing treatment or a low-frequency viewing treatment. Participants in the second wave participated only in the in-lab study. The online appendix contains the experimental instructions.

The first in-lab condition was a direct replication of Gneezy and Potters (1997). Participants made choices for nine rounds. Participants in the high-frequency treatment received a $2 endowment each round and could bet any amount from $0 to $2 in a gamble that had a 2/3 probability of a -100% net return and a 1/3 probability of a 250% net return. Participants kept any amount that was not bet. Gamble earnings and amounts not bet from previous rounds could not be bet in the current round. Participants knew that the outcome of each round's gamble would be revealed to them immediately after each round. Participants in the low-frequency treatment also received $2 per round, but they had to make investment decisions for three rounds at a time. For example, in Round 1, they chose how much they would invest in Rounds 1 through 3. Per-round investment amounts were constrained to be the same within each block of three. Participants knew that they would learn the outcomes of the three gambles for each block simultaneously instead of one by one.

The second in-lab condition, which we call the “scaled Gneezy-Potters” condition, modified the first in-lab condition by scaling down the percentage returns of the risky asset to a 2/3 chance of a -10% return and a 1/3 chance of a 25% return, which is within the realistic range of one-year returns on a diversified stock market investment. To keep the maximum possible dollars at risk the same, we increased the first period endowment to $20. In order to avoid creating significant wealth effects, we did not give participants a fresh $20 endowment each round. Instead, their balance at the end of round t constituted the maximum allowable investment amount in round $t + 1$. Participants kept whatever balance remained at the end of Round 9. Because the total balance available to bet changed from round to round, we had participants specify what percent of their endowment they wished to bet, rather than an absolute dollar amount as in the first condition. In the low-frequency treatment, this percentage was required to be constant within each three-round block.

The third in-lab condition replaced the binary risky asset with an asset whose return distribution matched the historical U.S. stock market distribution. We explicitly labeled this asset the “stock market” in the instructions given to participants. Participants were told that each round corresponded to one month, and that we would randomly select a starting month between January 1923 and January 2010, with each month having an equal chance of being selected. If, for example, the starting month was March 1954, then the Round 1 stock market return would be the actual March 1954 U.S. stock market return, the Round 2 stock market return would be the actual April 1954 U.S. stock market return, etc. The instructions showed participants a histogram of historical monthly stock returns from 1923 to 2012.[11] There were 18 total rounds, and

[11] These histograms had the return magnitude on the horizontal axis and the percent of months with this return on the vertical axis. These differ from the historical return graphs shown in our first experiment.

participants began with a $20 endowment. In the high-frequency treatment, participants made an allocation choice at the beginning of each round. The instructions said, "At the end of each round, you will learn how much money you gained or lost and your resulting balance. Your stock market investment will then be completely sold, and you must decide what percent of your total balance you wish to invest in the stock market for the next round." Therefore, in this condition, we retained the coincidence of return viewing and portfolio liquidation found in the Gneezy and Potters design. At the end of each round, participants were told, "Your stock market investment has been completely sold." Although this liquidation has no economic meaning, it may be psychologically meaningful.[12] A participant's experimental earnings equaled her balance at the end of Round 18. In the low-frequency treatment, participants made portfolio choices at the beginning of Rounds 1, 7, and 13, and they saw only their cumulated six-round returns at the end of Rounds 6, 12, and 18. The six-round frequency matches the frequency of paid return viewings in our first experiment's biannual viewing treatment. Portfolios were not rebalanced within each six-round block, so the percent invested in stock would move with the risky asset's return, but the instructions told participants that their portfolio would be liquidated every six rounds.

The fourth in-lab condition was identical to the third, except that we did not force liquidation each time a return was viewed. The instructions said that after seeing returns, "you will have the option of holding onto your stock market investment from the last round or changing the percent of your balance invested in the stock market." The computer screen eliciting the participant's investment choice showed two radio buttons labeled "Keep current stock holdings" and "Change stock holdings." If the participant chose the second button, she was asked to specify what percent she wished to invest in the stock market.

The four post-lab conditions mirrored the in-lab conditions, except that the amount of real time that elapsed between rounds was one week. In the Gneezy-Potters replication condition,

[12] Weber and Camerer (1998) find that when shares are automatically sold at the end of each period in an experimental market, subjects' disposition effect is greatly reduced even though they can buy their previous position back costlessly, rendering the liquidation economically meaningless.

there were three post-lab rounds. At the beginning of the first round, which occurred at the end of the laboratory session, participants in the high-frequency treatment chose an amount between \$0 and \$2 to bet in an asset that had a 2/3 chance of a -100% return and a 1/3 chance of a 250% return. The investment principal was taken from their experimental earnings in the in-lab session excluding their \$5 participation fee.[13] At the end of each subsequent week, participants received an email with the outcome of the gamble and a link to an online survey. The survey asked the participant to enter the result of the gamble (to confirm that she had seen it) and how much she wished to bet in the next round. If the participant filled out the survey within five days of the email being sent, we added \$1 to her final payment. The next round's bet amount equaled the previous round's bet amount for participants who did not fill out the survey within five days. The third and final survey did not ask for a bet amount. After the last survey, we mailed participants a payment equal to their final balance plus any survey completion payments they earned.

Participants in the low-frequency treatment in the Gneezy-Potters replication condition chose a per-round gamble amount that was constrained to be identical across post-laboratory rounds. The cumulative outcome was only revealed to them in an email sent three weeks in the future. The email also contained a link to a survey that asked what the result of the gambles was. If participants filled out the survey within five days of the email being sent, we added \$3 to their final payment.

The second post-lab condition modified the asset to have a 2/3 chance of a -10% return and a 1/3 chance of a 25% return. Participants specified what percent of their ending in-lab balance (which does not include their \$5 participation fee) they wished to bet, and balances rolled over from round to round, as in the second in-lab condition. High-frequency treatment participants chose their bet percentages each week, while low-frequency treatment participants chose one bet percentage that applied to all three rounds.

[13] For subjects who earned less than \$6 in the in-lab session, the bet amount was limited to one-third of their earnings.

The third post-lab condition lasted for six weekly rounds, and the risky asset's returns were drawn from one historical monthly U.S. stock market return sequence, as in the third in-lab condition. Participants in the high-frequency condition were told that their investment would be completely liquidated at the end of each round, and they would have to choose a new allocation each week. These participants were paid $1 for completing each of six post-lab surveys about their realized return and new portfolio choice. Participants in the low-frequency condition were only told their six-round return at the end of six weeks, and they were paid $6 for completing one survey at that time.

The fourth post-lab condition was like the third, except it did not force liquidation at the end of each round in the high-frequency treatment. The post-lab surveys allowed participants to click a radio button to keep their current stock holdings. The low-frequency treatment was identical to the low-frequency treatment in the third post-lab condition.

5. Second aggregation experiment results

We recruited 320 participants in Wave 1 and another 320 participants in Wave 2. This allowed us to assign 80 participants to each in-lab condition (for Wave 1 only) and 80 participants to each post-lab condition (for both Wave 1 and Wave 2). Based on the means and standard deviations reported in Gneezy and Potters (1997), a sample size of 80 gives us a 79.9% probability of detecting an effect of their reported magnitude at 5% significance in the replication condition.

Panel A of Table 9 reports regression results from the in-lab conditions for Wave 1 participants. The dependent variable is the percent of the available endowment that is invested in the risky asset in each round. We find that we do not obtain the Gneezy-Potters viewing frequency result in our replication condition. The infrequent return viewing treatment dummy coefficient is 4.8%, which has the predicted sign but is insignificant. Scaling the risky asset payoffs down by a factor of ten causes the infrequent return viewing treatment effect to flip sign, so that infrequent viewing actually *reduces* risky asset investment by 7.8 percentage points, although this effect is not significant. Further modifying the risky asset payoffs to match the stock market return distribution does not resurrect the Gneezy-Potters effect, whether portfolio liquidation is forced upon return viewing or not. The treatment coefficients in these last two conditions are insignificant.

The Wave 1 in-lab results raised a question: Is our failure to find a viewing frequency effect in the replication condition the result of Type II error, or is the original Gneezy-Potters result a Type I error and the subsequent successful replications in the literature are due to publication bias? To answer this question, we re-ran the in-lab conditions on Wave 2 participants. Panel B of Table 9 shows that in this second sample, viewing returns less frequently *did* increase risk-taking robustly in the replication condition. The effect magnitude is 26.9 percentage points, which is much larger than the 16.9 percentage point effect reported by Gneezy and Potters (1997) and highly significant ($p < 0.0001$). If we pool the Wave 1 and Wave 2 samples, the effect is a 15.8 percentage point increase that is significant at the 1% level ($p = 0.002$). We believe that our failure to replicate in Wave 1 was due to Type II error, which we expected to occur with 20% probability. In the scaled-down Gneezy-Potters condition and the two stock conditions, we continue to find no significant effects, and each point estimate has the opposite sign of what we found in Wave 1.[14] Therefore, these three treatment effects appear to be truly zero.

In Table 10, we report regression results from the post-lab conditions, which were run only on Wave 1 participants. These regressions are analogous to the in-lab regressions in Table 9. In none of the four conditions do we find that infrequent return viewing significantly affects the fraction of the available endowment invested in the risky asset. The null effect in the post-lab Gneezy-Potters replication condition is not simply due to participants in the Wave 1 in-lab replication condition being less susceptible to infrequent return viewing. Because assignment to

[14] Pooling Waves 1 and 2 does not qualitatively change the significance of these three treatment effects.

the post-lab conditions was independent of assignment to the in-lab conditions, 75% of participants in the post-lab replication condition were not in the in-lab replication condition.

6. Second aggregation experiment discussion

Our follow-up experiment indicates that return viewing frequency affects risk-taking only when the risky asset has extreme percentage returns that are viewed immediately after the portfolio choice is made. Thus, both the less extreme returns and the long time horizon of our first experiment contribute to its null viewing frequency effects. Because the return viewing effect in the in-lab replication condition is stronger in Wave 2, which contains many more non-student participants than Wave 1, and neither wave shows significant return viewing effects in the other three in-lab conditions, the null results of our first experiment do not appear to be driven by the fact that its participants are not students.

The fragility of the viewing frequency effect leads us to hypothesize that the Gneezy-Potters effect arises from an idiosyncratic feature of their aggregation treatment interacting with their risky asset return distribution to make a particular chain of reasoning cognitively fluent—that is, easy to process mentally. Note that each possible asset return has a probability that is a multiple of 1/3, and the infrequent return viewing treatment groups together three rounds. The fact that the expected outcome for this grouping consists of an integer number of wins and an integer number of losses makes it very concrete and easy for subjects to conceptualize. In addition, it is very easy for the subject to calculate the expected gross return from investing a dollar in the risky asset in each of the three rounds—$(2 \times \$0) + (1 \times \$3.50) = 1 \times \$3.50 = \$3.50$—and see that it is greater than $3, because the first term can be dropped due to its being multiplied by zero. Finally, a subject who believes in the law of small numbers (Tversky and Kahneman, 1971)—the mistaken belief that a small sample will closely resemble the population from which it is drawn—would believe that this expected outcome over three rounds is a highly probable outcome. Thus, the three gambles together are a great deal, since they offer a positive expected return with low risk. This sequence of thought is easy to produce, and statements and scenarios that are easily processed are more likely to be regarded as true (Tversky and Kahneman, 1973; Schwarz et al., 1991; Reber and Schwarz, 1999; McGlone and Tofighbakhsh, 2000).

Our experimental conditions that deviate from the original Gneezy-Potters design disrupt the above chain of reasoning. For the scaled Gneezy-Potters asset, the calculation of the expected gross return is (2 × $0.90) + (1 × $1.25). Because the first additive term does not drop out, it is more difficult for subjects to evaluate this expression, making the logical chain described above harder to mentally process. Therefore, the conclusion that the aggregated gambles are a great deal feels less compelling. If we instead delay the gambles' resolution by weeks, the collection of gambles no longer feels so low-risk because people are less likely to believe the law of small numbers applies when more time elapses between gambles (Gold and Hester, 2008).

This hypothesis explains why Moher and Koehler (2010) find that less frequent viewing of gambles that are similar but not identical to each other does not increase risk-taking. The gambles they offer subjects have the same possible returns as the Gneezy-Potters assets and the aggregation treatments group three gambles together, but the probability of losing varies from 30% to 37% across gambles. Both the variation in probabilities and the fact that they are not exact multiples of 1/3 makes it harder for subjects to calculate and envision the modal outcome across three rounds, preventing them from reaching the conclusion that the bundled outcome is low-risk and high-reward. Similarly, Langer and Weber (2005) find no significant increase in risk-taking in gambles where the probability of losing is 10% when returns are shown only once every three rounds instead of every round.[15] According to our theory, if the aggregation treatment groups n gambles together, an increase in risk-taking is most likely if the probability of a loss is a multiple of $1/n$.

[15] They find that risk-taking decreases in the low-frequency condition for both gambles they test, but the effect is insignificant for one gamble and significant only at the 10% level for the other, so these could be interpreted as collectively null results.

7. Conclusion

Many financial behaviors 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 with positive expected values (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). Myopic loss aversion has also been proposed as a resolution of the equity premium puzzle (Benartzi and Thaler, 1995; Barberis, Huang, and Santos, 2001; Barberis, Huang, and Thaler, 2006).

It seems plausible that the boundaries of investors' mental accounts could be shifted by changing information disclosure in order to increase risky asset demand. The experimental evidence to date has found that indeed, reporting only aggregated outcomes of multiple gambles increases participants' willingness to take risks. However, these previous experiments abstracted away from certain features of the real-world investment environment that may moderate such effects outside the laboratory. In particular, previous experiments have all taken place within a single laboratory session and have used laboratory assets. In order to gauge the potential impact of disclosure policy, our first experiment had participants invest in real financial assets over the course of an entire year.

We find that disclosing returns at a more aggregated level does not increase portfolio risk-taking. Further analysis and a second experiment indicate that the effect of aggregated returns disclosure is sensitive to the distribution of the risky asset's returns and the amount of time that elapses between the portfolio choice and the return disclosure. Aggregated returns disclosure does not appear to be a policy lever that can be used by financial institutions to robustly affect average portfolio risk choices.

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Table 1. Sample Size in Each Experimental Cell in the First Experiment

This table reports the number of subjects that were assigned to each experimental cell in the first experiment. 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 subjects.

Panel A: Ongoing returns reported at portfolio level		
	Return viewing inducement frequency	
Historical returns 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 returns 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. Participant Characteristics in the First Experiment

Percent male	56%	Financial assets in bank, brokerage, and retirement accounts	
Age		< $25,000	27%
≤ 25	2%	$25,000 - $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%		
High school graduate	10%		
Some college	23%	0%	20%
Associate's degree	10%	1 - 25%	32%
Bachelor's degree	28%	26 - 50%	17%
Some graduate school	7%	51 - 75%	15%
Graduate degree	21%	76 - 100%	8%
		Prefer not to answer	9%
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 3. Website Visits After Initial Allocation in the First Experiment

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 includes any visit 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	18.2	0.000
	(2.6)	(1.9)	
Viewings of returns screens from e-mail links	45.3	1.5	0.000
	(0.7)	(0.0)	
Fraction of possible e-mail link returns screens viewed	87.2%	73.8%	0.000
	(1.9)	(2.5)	

Table 4. Aggregation Effects on Initial Allocations in the First Experiment

The dependent variable is the percent of the portfolio allocated to equities in total (U.S. plus international) or to each asset class at the start 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. *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. Point estimates from an OLS regression are shown, with standard errors in parentheses.

	Total equity	U.S. stock	Int'l stock	Bonds	Money market
Biannual e-mail	-0.0	-0.5	0.5	0.5	-0.5
	(1.7)	(1.5)	(1.6)	(1.1)	(1.1)
Portfolio-level return reporting	-1.7	-4.1*	2.4	0.1	1.6
	(1.9)	(1.6)	(1.8)	(1.2)	(1.2)
1-year graph	11.2**	1.8	9.4**	-4.4**	-6.9**
	(2.6)	(2.3)	(2.5)	(1.7)	(1.7)
5-year graph	9.2**	6.0**	3.2	-3.2	-6.0**
	(2.6)	(2.3)	(2.6)	(1.7)	(1.7)
Asset class mixes shown in graph	0.5	2.4	-1.9	-0.2	-0.3
	(1.9)	(1.6)	(1.8)	(1.2)	(1.2)
Constant	58.3**	29.6**	28.8**	21.3**	20.3**
	(2.8)	(2.4)	(2.7)	(1.8)	(1.8)
Sample size	597	597	597	597	597

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

Table 5. Asset Class Valuations Under Piecewise Linear Loss Aversion and Cumulative Prospect Theory

The first and third columns of this table show the valuation of each asset class by an agent who has either piecewise linear loss-averse utility or cumulative prospect theory utility. In Panel A, this valuation is done assuming the agent perceives that each historical one-year return realization of the asset class, as shown in the one-year historical returns graphs, is equally likely in the future. In Panel B, the agent perceives that each historical five-year annualized return realization of the asset class, as shown in the five-year historical returns graphs, is equally likely in the future. The second and fourth columns show the deviation of each asset class's valuation from the mean asset class valuation in its graph type × utility function cell.

Panel A: One-year return graph				
	Piecewise linear loss aversion		Cumulative prospect theory	
	Valuation	Deviation from mean valuation	Valuation	Deviation from mean valuation
U.S. stocks	2.9%	0.7%	-2.5%	-2.3%
Int'l stocks	3.6%	1.3%	0.7%	0.8%
Bonds	1.8%	-0.5%	0.9%	1.0%
Money market	0.7%	-1.5%	0.3%	0.5%

Panel B: Five-year return graph				
	Piecewise linear loss aversion		Cumulative prospect theory	
	Valuation	Deviation from mean valuation	Valuation	Deviation from mean valuation
U.S. stocks	6.0%	1.9%	5.5%	1.3%
Int'l stocks	6.3%	2.1%	7.3%	3.1%
Bonds	3.3%	-0.9%	2.9%	-1.3%
Money market	1.1%	-3.1%	1.1%	-3.1%

Table 6. Aggregation Effects on Initial Equity Allocation
Interacted with Loss Aversion and Mental Accounting in the First Experiment

The dependent variable is the percent of the portfolio allocated to equities in total (U.S. plus international) at the start 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. *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.

Biannual e-mail	-1.6 (2.3)
Biannual e-mail × *Loss averse*	2.5 (3.4)
Portfolio-level return reporting	-2.9 (2.6)
Portfolio-level return reporting × *Loss averse*	2.5 (3.7)
1-year graph	12.2** (3.7)
1-year graph × *Loss averse*	-1.6 (5.3)
5-year graph	6.4 (3.7)
5-year graph × *Loss averse*	6.0 (5.3)
Asset class mixes shown in graph	-1.4 (2.6)
Asset class mixes shown in graph × *Loss averse*	3.7 (3.7)
Loss averse	-7.9 (5.5)
Constant	62.3** (3.9)
Sample size	597

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

Table 7. Aggregation Effects on Later Equity Allocations in the First Experiment

The dependent variable is the percent of the portfolio allocated to equities in total (U.S. plus international) 27 weeks into experimental participation or 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. *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. Point estimates from an OLS regression are shown, with standard errors in parentheses.

	27 weeks	Final
Biannual e-mail	2.4	-1.4
	(2.1)	(2.2)
Portfolio-level return reporting	0.1	-1.2
	(2.4)	(2.4)
1-year graph	7.4*	9.0**
	(3.4)	(3.4)
5-year graph	9.1**	7.5*
	(3.4)	(3.4)
Asset class mixes shown in graph	1.5	-1.0
	(2.4)	(2.4)
Constant	48.7**	54.6**
	(3.5)	(3.6)
Sample size	597	597

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

Table 8. Aggregation Effects on Initial Equity Allocation Interacted with Belief in Serially Uncorrelated Returns in the First Experiment

The dependent variable is the percent of the portfolio allocated to equities in total (U.S. plus international) at the start 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. *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. *Serially uncorrelated returns* is a dummy for whether the subject believes that neither a 10% increase in the stock market last month nor a 10% decrease in the stock market last month should affect one's forecast of the stock market's return next month. Point estimates from an OLS regression are shown, with standard errors in parentheses.

Biannual e-mail	1.2
	(2.4)
Biannual e-mail × *Serially uncorrelated returns*	-1.4
	(3.4)
Portfolio-level return reporting	-3.9
	(2.7)
Portfolio-level return reporting × *Serially uncorrelated returns*	4.4
	(3.8)
1-year graph	7.4
	(3.8)
1-year graph × *Serially uncorrelated returns*	7.0
	(5.4)
5-year graph	8.5**
	(3.7)
5-year graph × *Serially uncorrelated returns*	1.8
	(5.4)
Asset class mixes shown in graph	-1.4
	(2.7)
Asset class mixes shown in graph × *Serially uncorrelated returns*	4.3
	(3.8)
Serially uncorrelated returns	-2.6
	(5.6)
Constant	59.2**
	(3.9)
Sample size	570

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

Table 9. In-Laboratory Viewing Frequency Experiment Results

This table shows regression results where the dependent variable is the fraction of the endowment invested in the risky asset in each round and the explanatory variable is a dummy for the subject having returns reported infrequently. Every choice by a subject is a separate observation, and standard errors are clustered by subject.

	Gneezy-Potters replication	Scaled Gneezy-Potters	Stock condition, forced liquidation	Stock condition, no forced liquidation
		Panel A: Wave 1		
Infrequent return viewing	4.8 (7.8)	-7.8 (8.0)	-0.8 (7.0)	4.0 (6.9)
Constant	48.2** (5.7)	49.5** (5.8)	48.7** (4.9)	43.1** (4.9)
N	480	480	840	840
Subjects	80	80	80	80
		Panel B: Wave 2		
Infrequent return viewing	26.9** (6.0)	0.7 (6.7)	5.4 (7.0)	-5.7 (5.9)
Constant	32.9** (4.5)	32.9** (5.0)	36.7** (4.6)	34.8** (4.5)
N	480	480	840	840
Subjects	80	80	80	80

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

Table 10. Post-Laboratory Viewing Frequency Experiment Results

This table shows regression results where the dependent variable is the fraction of the endowment invested in the risky asset in each round and the explanatory variable is a dummy for the subject having returns reported infrequently. Every choice by a subject is a separate observation, and standard errors are clustered by subject.

	Gneezy-Potters replication	Scaled Gneezy-Potters	Stock condition, forced liquidation	Stock condition, no forced liquidation
Infrequent	-2.8	6.6	-4.8	-10.0
return viewing	(7.7)	(8.9)	(7.2)	(7.2)
Constant	55.7**	40.9**	46.0**	50.4**
	(5.1)	(6.0)	(4.6)	(4.9)
N	160	160	280	280
Subjects	80	80	80	80

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

Figure 1. Experimental Period

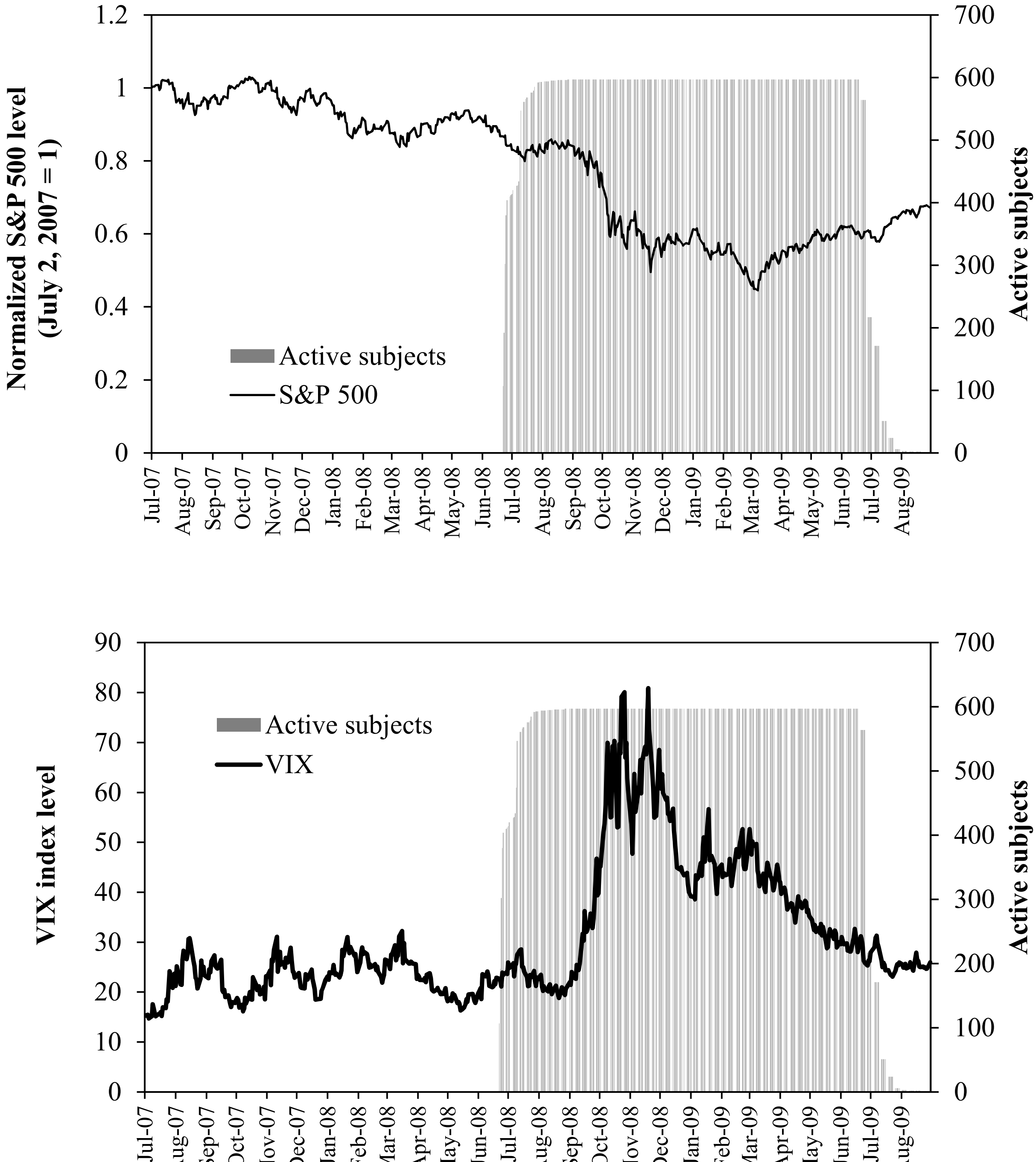

Figure 2. Historical Returns Graphing Tool that Shows One-Year Returns of Single Asset Classes Only

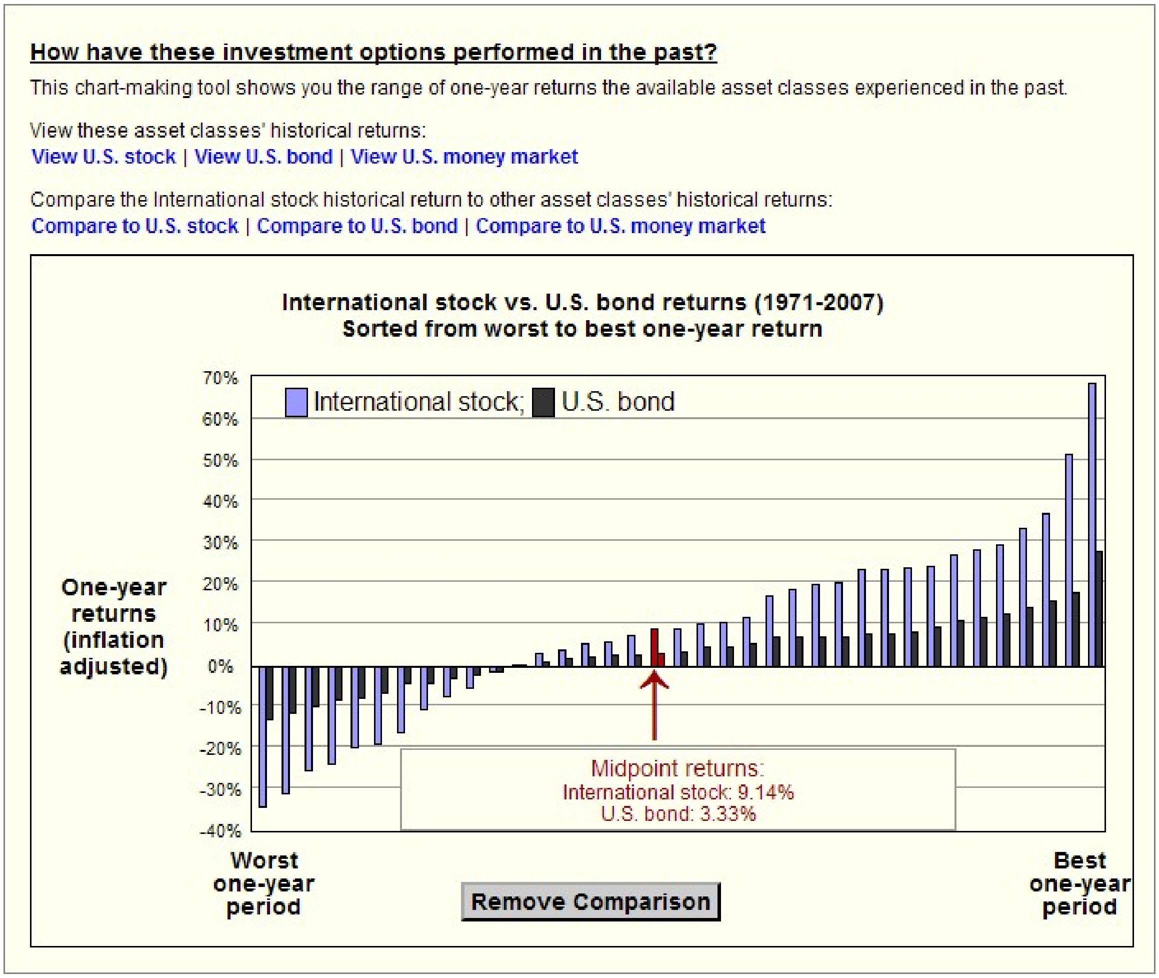

Figure 3. Historical Returns Graphing Tool that Shows Five-Year Annualized Returns of Arbitrary Portfolio Mixes

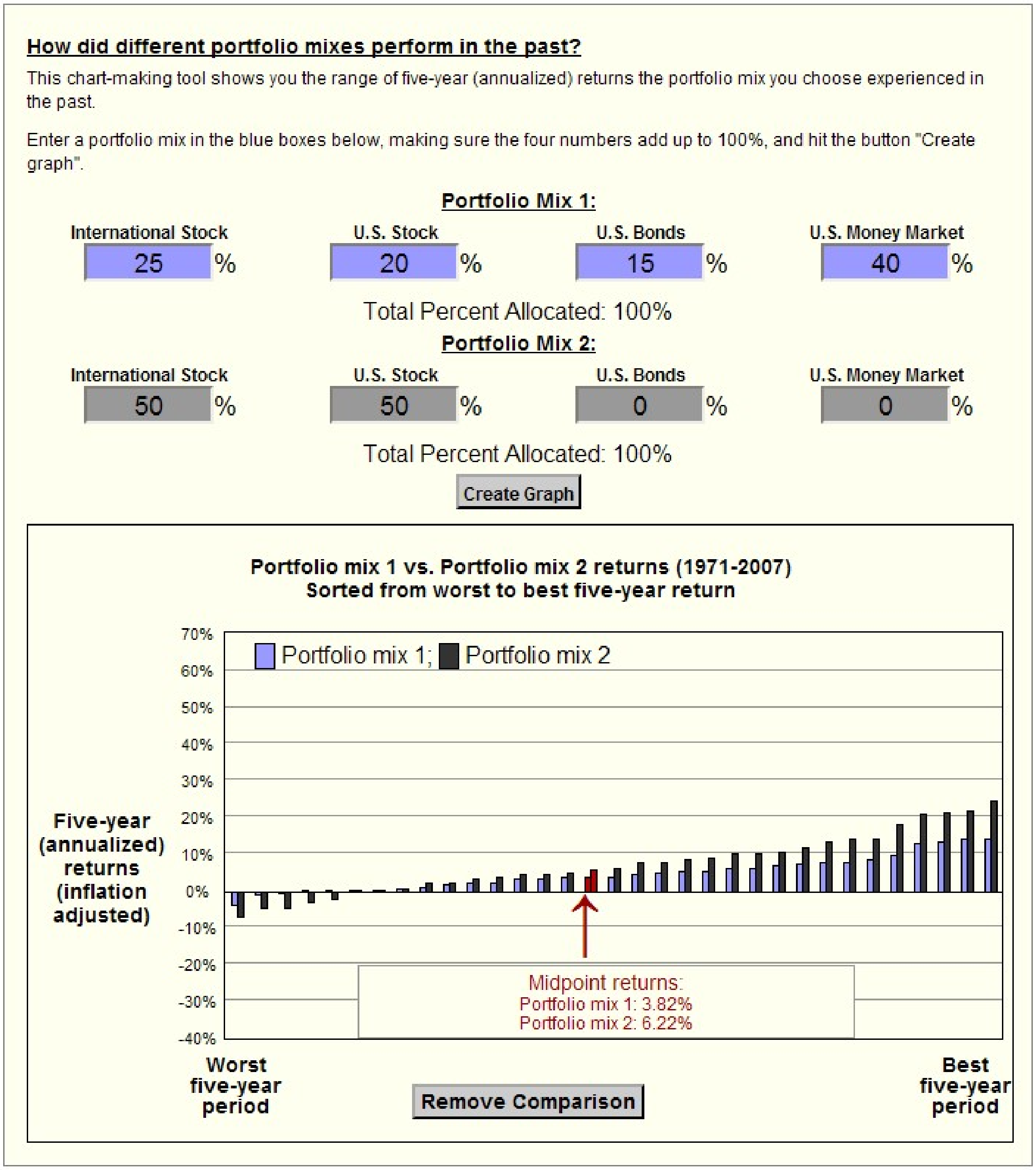

Figure 4. 1-Year Asset Class Returns, 1971-2007, Sorted From Worst to Best

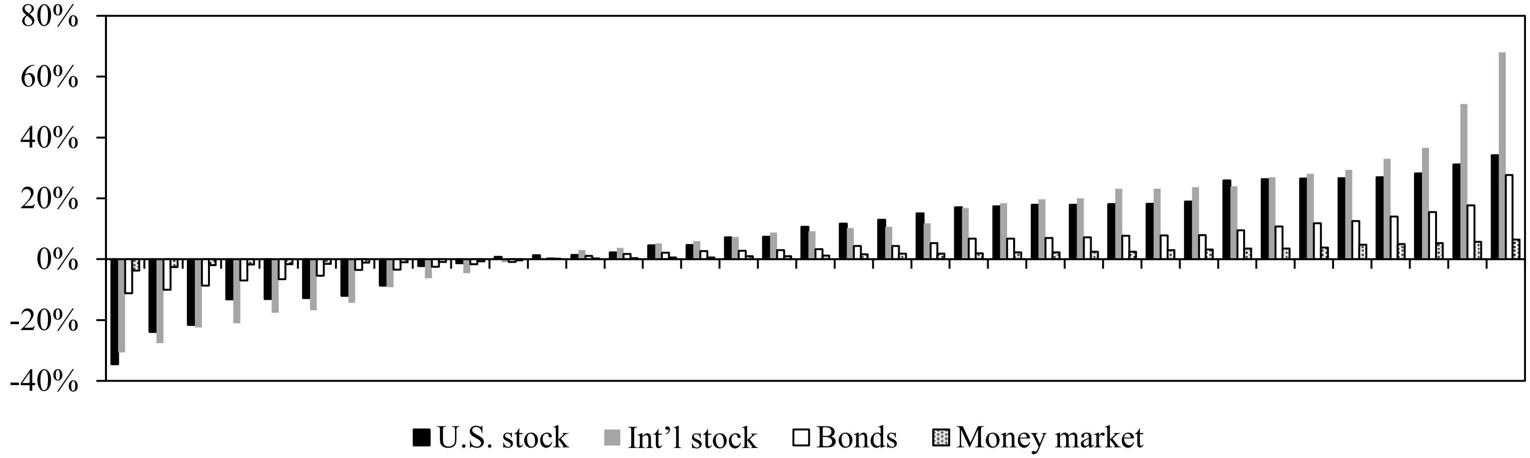

Figure 5. 5-Year Annualized Asset Class Returns, 1971-2007, Sorted From Worst to Best

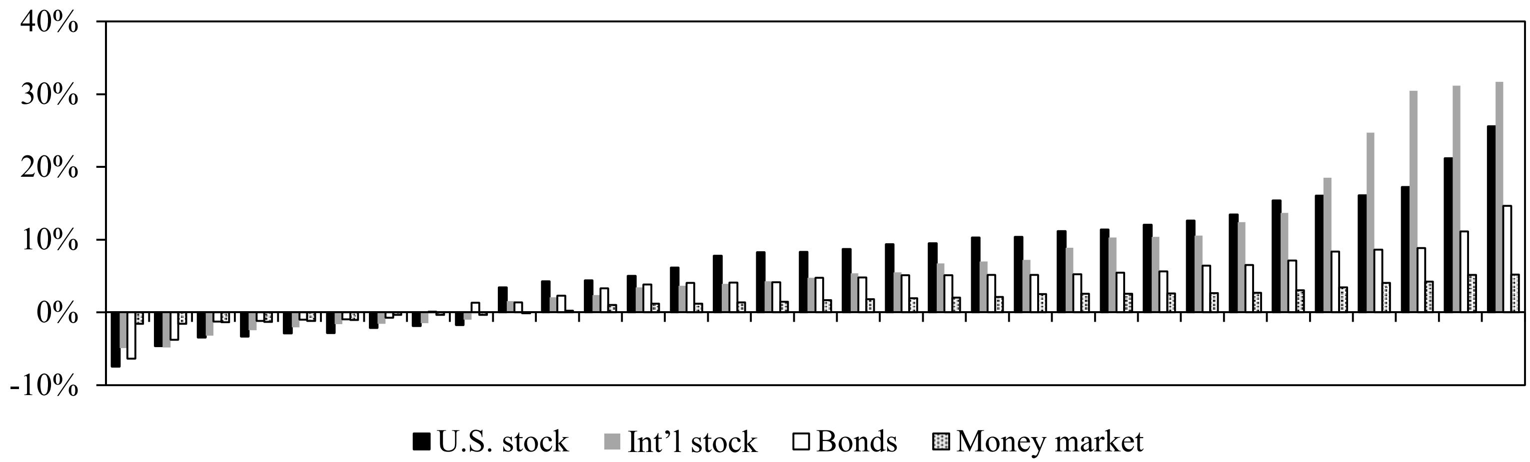

Figure 6. Second Experiment Design