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Selection Neglect in Mutual Fund Advertisements

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Mutual fund companies selectively advertise their better-performing funds. However, investors respond to advertised performance data as if those data were unselected (i.e., representative of the population). We identify the failure to discount selected or potentially selected data as *selection neglect*. We examine these phenomena in an archival study (Study 1) and two controlled experiments (Studies 2 and 3). Study 1 identifies selection bias in mutual fund advertising by showing that the median performance rank for advertised funds is between the 79th and 100th percentile. Study 2 finds that both novice investors and financial professionals fall victim to selection neglect in a financial advertising task unless the advertisement makes the selective nature of available performance data transparent. Study 3 shows that selection neglect associated with a large well-known company can be debiased with a simple extrinsic sample space cue, although individual differences in statistical reasoning also matter. We argue that selection neglect results from a general tendency to ignore underlying sample spaces rather than a fundamental misunderstanding about the data selection process or the value of selected data.

Key words: selection bias; financial decision making; mutual fund ads; statistical heuristics; sample space

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1. Introduction

Louis Rukeyser, the long-time host of *Wall Street Week*, used to invite three of his two dozen regular panelists to appear on the annual New Year's show to offer stock recommendations for the coming year. Although Rukeyser's panelists offered stock recommendations every other week as well, the New Year's show commanded special attention. On this night, the invited panelists were the show's *best* stock pickers based on their performance during the prior year. Millions tuned in for tips from these proven seers. But how much confidence should investors have in the recommended stocks? After all, the chance that at least a few forecasters would have compiled terrific performance records—even if none had any real forecasting ability—increases as the number of forecasters increases. Consider the following analogy. If Katie flips a fair coin 100 times hoping to get lots of heads, there is a 3% chance that she will obtain 60 or more heads ("great success"). However, if she flips 50 coins 100 times each, there is a 76% chance that at least one of those coins will yield a great success. The point is that even in environments where ability is minimal or nonexistent, the laws of chance alone are likely to yield occasional great successes when there are many

opportunities to achieve success. But how many of those who watched the New Year's edition of *Wall Street Week*, pen in hand, thought about the size of the set of regular panelists from which the all-stars were selected? Would it have made a difference if they had? In a similar vein, when people learn that a touted mutual fund has a history of great success, do they consider that a company that offers many mutual funds is bound to have one or more funds that performed exceptionally well by sheer chance?

Mutual fund companies often advertise a subset of the total funds they operate. A *selection bias* is present if the advertised funds are chosen in ways that makes them atypical of the population of funds offered by the company. Selection bias research in other fields often focuses on how data become unrepresentative of populations and the problems that result when those data are used in statistical analyses (e.g., Heckman 1979). In contrast, we are primarily concerned with how people *respond* to data that are or may have been strategically selected. We predict that people respond to such data as if they were unselected (i.e., representative). We call this response tendency *selection neglect*. We also predict that selection neglect can often be overcome when people have sample space knowledge

and access to that knowledge. The access requirement is crucial. We argue that decision makers are likely to suffer from selection neglect unless their sample space knowledge is cued or otherwise made available.

We focus on selection neglect in the context of investors' responses to mutual fund advertisements. We begin with an archival study (Study 1) that asks whether companies that offer multiple mutual funds provide biased snapshots of their success by selectively advertising better-performing funds. After establishing that they do, we provide two selection neglect experiments (Studies 2 and 3). Study 2 examines the basic selection neglect phenomenon. Consistent with work by Nisbett et al. (1983) on statistical heuristics, this study shows that investors tend to appreciate selection when the sample space and/or selection process is transparent. However, when the sampling process is not made obvious, investors fall prey to selection neglect and fail to discount selected data. Study 2 also shows that financial professionals are as susceptible to selection neglect as ordinary investors. Study 3 addresses other selection neglect issues in the context of an advertisement from a large, well-known mutual fund company. First, we show that selection neglect can be overcome by subtly cueing sample space information that investors already know, but apparently do not access. Second, we test a process by which selection neglect may influence willingness to invest. Third, we consider the role that individual differences in statistical reasoning may play in debiasing selection neglect.

2. Reasoning with Biased Data: Selection Neglect

Behavioral research indicates that people often use sample data inappropriately to draw inferences about the populations from which the samples were derived (Tversky and Kahneman 1971). One reason that people make poor inferences from sample data is that the environment commonly offers up unrepresentative samples for their consideration (Fiedler 2000, 2008; Denrell 2003, 2007). For example, when informants (such as advertisers) control the flow of information and have a strategic interest in how others respond to that information, the likely result is a selection and presentation of unrepresentative data. Given that sample data presented in such situations are likely to be biased, why would people respond to such data as if they were unselected (i.e., suffer from "selection neglect")?

One reason is that people may not realize that the available data were strategically selected. This may occur when people mistakenly believe that informants are disinterested or have disclosed all relevant information about the sample and sampling process.

Although such effects undoubtedly occur on occasion, this explanation is incomplete, because research shows that people give great inferential weight to sample data even when they are expressly told that the data are atypical (Hamill et al. 1980).

A second reason decision makers might rely on biased data is that they recognize the potential for strategic selection in available data, but do not understand how selection should affect their use of those data. Undoubtedly, some people fail to discount obviously selected data for this reason. However, Nisbett et al. (1983) persuasively argue that people often possess sophisticated intuitive strategies, or "statistical heuristics" for reasoning effectively in various everyday tasks including, presumably, the evaluation of selected data.

A third reason for selection neglect is that people know that biased data should be discounted, but they fail to *access* and use this knowledge. An implication of this explanation is that people will assign less weight to selected data when the data selection process is made transparent or cued. The available research is most consistent with this explanation. People are prone to accept sample evidence at face value (Fiedler 2008), and such behavior may be automatic. As Daniel Gilbert and colleagues demonstrated, the mental representation of a proposition has a truth value and the default presumption is that the proposition is true (Gilbert et al. 1990). Extending this idea to quantitative data, the default assumption may be that available data are valid for inferential purposes. Of course, people sometimes reject propositions and selected data. However, doing so requires extra steps that the intuitive decision maker may not even consider.

This framework is consistent with a dual process theory of cognition in which an automatic, nondeliberative system (System I) quickly generates impressions of available stimuli, whereas a slower, more reflective system (System II) monitors the quality of those impressions through a more deliberative judgment process (Kahneman and Frederick 2002). However, System II may not intervene to override the automatic acceptance of sample data that System I yields in a data selection task. Effective System II deliberation in such tasks requires decision makers to construct and consider relevant sample spaces. Performing this task prompts decision makers to consider the role that sample bias may play and to discount the available data accordingly, but sample space construction is notoriously unnatural because it requires attention to *nonoccurrences* of the event of interest (Einhorn and Hogarth 1978). As Hearst (1991) explains, "[H]uman beings and other animals have trouble using the mere absence of something as a basis for efficient and appropriate processing of

information. They notice and recall additions much more readily than deletions” (pp. 432, 434).

In short, the third explanation for selection neglect suggests that decision makers’ failure to discount selected data is due to lack of knowledge *access* rather than lack of knowledge. Although many people realize, in the abstract, that selected sample data should be discounted, these same people may not discount such data at all because they do not automatically think about sample space and sampling matters that draw the biased nature of the data into focus. We contend that the attention of decision makers who are invited to make inferences from selected sample data must somehow be *directed* to sample space and sampling process considerations before people will access their statistical heuristics and discount selected data.

Our selection neglect theory has implications for how investors who read mutual fund ads use performance data in those ads to form beliefs about the advertising company and make investment decisions. We predict that investors who see ads that tout the recent strong performance of a few funds will not spontaneously consider the large sample space of unadvertised funds. Consequently, these investors are likely to be impressed by the ads because they will fail to consider that companies that operate many funds are bound to have some strong performers by chance alone. However, if the fund advertisement includes cues related to sample space (i.e., the total number of funds operated by the company), we expect investors to discount the potentially selected performance data.

3. Study 1: Selective Advertising of Mutual Funds

Mutual fund companies spend large amounts of money advertising their funds (Jordan and Kass 2002), and these expenditures are paying off. Funds that advertise attract 20% more new money than comparable funds that do not advertise (Jain and Wu 2000), but if the advertisements suffer from selection bias, such enthusiasm may be unwarranted.

Selection biases in mutual fund advertising may occur at various levels, some of which are hard to detect. They may occur at a company level (which fund companies are advertising), a content level (how is the fund advertised), or a product level (which funds are advertised). *Company-level selection bias* occurs when management companies that choose to advertise are unrepresentative of the general population of management companies. This may occur because small companies or companies that perform poorly lack the resources for vigorous advertising campaigns, or because strong performers are more motivated to call attention to their success. *Content-level selection bias* occurs when companies emphasize the most positive features of a product while

downplaying or ignoring other features. Mutual fund companies might advertise historical return data from a favorable time period or compare their performance against low-performing benchmarks or index funds. Recent Security and Exchange Commission (SEC) guidelines have curtailed time-period related selection biases, but other forms remain. *Product-level selection bias* occurs when companies advertise their most successful funds. When companies embrace this strategy, the value of performance information in their ads is likely minimal.¹ After all, if a company offers many funds, some are likely to outperform the benchmark by chance alone. Our studies focus on the existence of (Study 1) and reactions to (Studies 2 and 3) product-level selection bias in mutual fund advertisements.

3.1. Method

Study 1 examines whether companies selectively advertise their better-performing stock mutual funds. We investigate this issue by comparing the recent performance history of advertised stock mutual funds with the performance history of nonadvertised stock funds within the same management company. We reviewed mutual fund ads that appeared between 1998 and 2001 in *BusinessWeek* and *Fortune*. We restricted our analyses to ads from companies that operated multiple stock funds and that included performance data. We chose this time frame because it included favorable and unfavorable market periods and provided for a large data set. We examined these publications because they have the largest circulations among business periodicals over the past 10 years. Our data set included 341 advertised funds from 27 different companies. The mean ad contained performance data for 1.7 different funds.

We ranked each advertised fund’s performance relative to (a) the performance of other stock funds operated by the same company (company stock funds), and (b) the performance of other stock funds operated by the same company with the same objective, e.g., growth and income (same-objective company stock funds). We used the Center for Research in Security Prices (CRSP) mutual fund database to obtain relevant performance data.

SEC guidelines require that mutual fund ads that include performance data report 1-year, 5-year, and 10-year historical performance (SEC Rule 482; NASD Rule 2210-3). Funds less than 10 years old must report

¹ There is debate about the predictive value of mutual fund performance data even when those data are not strategically selected. Some argue that past performance provides no information about future success (Malkiel 1995, Carhart 1997). Others find that there is short-term persistence in high-performing funds (Hendricks et al. 1993, Brown and Goetzmann 1995). We are not concerned with this debate here. We focus on investors’ use of data rather than the predictive value of those data.

performance for the life of the fund, and ads may not report performance for periods less than one year. All 341 advertised funds in our data set disclosed 1-year performance data, 237 disclosed 5-year performance, and 132 disclosed 10-year performance. Our analyses required that the advertising company operate at least two stock funds for the company stock funds ranking and at least two same-objective stock funds for the same-objective company stock funds ranking. The *ns* for company stock funds rankings were 339 (1-year), 228 (5-year), and 121 (10-year); the *ns* for our same-objective company stock funds rankings were 306 (1-year), 193 (5-year), and 86 (10-year). The mean number of nonadvertised funds for the 1-year, 5-year, and 10-year time periods was 74, 34, and 21, respectively, for *all* company-operated stock funds and 12, 5, and 4 for *same-objective* company stocks funds.

3.2. Results and Discussion

We find that companies offer biased snapshots of their success by selectively advertising their higher-performing funds. The median 1-year, 5-year, and 10-year ranks for advertised stock funds were at the 79th, 88th, and 88th percentiles, respectively, for all company-operated stock funds, and at the 80th, 100th, and 100th percentiles for company-operated stock funds that share a common investment objective. The median rank of advertised funds relative to nonadvertised same-objective funds is 100% in the 5- and 10-year periods because the top performer was advertised 61% and 62% of the time, respectively. The results for 5- and 10-year performance are interesting because companies tend to close poorly performing funds. Indeed, ~6.5% of funds close each year (Clements 2002). The significance of this survivorship bias for our purposes is that it *reduces* the relative positioning of surviving funds. That is, if the closed funds were still open, the advertised funds would have even higher relative ranks. The higher longer-term performance percentiles are also interesting because they reflect an advertising strategy consistent with research that shows investors place great weight on long-term performance records (Wilcox 2003).

Our findings are consistent with Jain and Wu (2000), who found that advertised mutual funds outperformed various benchmarks immediately prior to the ad's appearance. However, Jain and Wu's results could be driven by *company-level* selection bias (i.e., companies that had good years chose to advertise). Our within-company analysis is more supportive of a *product-level* selection bias in which companies advertise funds that have performed particularly well during the past 5- and 10-year periods.

4. Study 2: Response to Selection Bias

Study 1 showed that investment companies selectively advertise their better-performing stock funds. Because

this practice is widespread, it is important to know whether potential investors appreciate the ads' selective nature. In Study 2, we examine the responses of novice and expert investors to selection bias in mutual fund ads. We compare their responses to the predictions of three simple selection bias sensitivity theories: complete sensitivity, complete insensitivity, and partial sensitivity. The *complete sensitivity* theory holds that investors are adept at recognizing selective data when they appear, and that they attach relatively less weight to selected data than to unselected data. The *complete insensitivity* theory holds that investors treat samples as representative of populations regardless of the process by which those samples were selected. Accordingly, investors make no distinction between selected and unselected data when drawing inferences. The *partial sensitivity* theory invokes a distinction between transparent and hidden selection processes. Investors who are partially sensitive to selection bias appreciate the shortcomings of selected data, but only when the selective nature of the data is transparent or otherwise cued. When the selective process remains hidden, partially sensitive investors give little thought to selection, and value the resultant data as much as they value data that they know to be unselected. Assuming that people are more apt to invoke statistical heuristics when the statistical aspects of the problem are clarified or cued (Nisbett et al. 1983), we favor the *partial sensitivity* theory.

4.1. Method

4.1.1. Participants. We recruited 128 novice and 129 expert investors to read and respond to a mutual fund advertisement. We recruited novice investors from business classes at a top-10 ranked business school in exchange for class credit. About half (46%) of the novice investors had investing experience, and a large majority (90%) planned to invest in stocks in the near future. We recruited expert investors from 30 U.S.-based financial companies with the assistance of 20 professional contacts who worked at major financial institutions. Expert investors qualified for the study if they made investment decisions for others in a professional capacity and had at least three years of investing experience. Our experts were an elite group of professionals. They included chief financial officers, portfolio managers, financial consulting partners, fund managers, corporate valuation managers, directors, financial consultants, investment bankers, traders, investment sales representatives, and analysts. The mean expert investor had 10 years of personal investing experience, 6 years of professional investing experience, and spends five hours per week reading financial publications and watching financial programs on television. Our experts volunteered their time to complete the task and received a chocolate gift in return.

4.1.2. Materials and Procedure. We provided each participant, at random, with one of four versions (one control group, three experimental groups) of an advertisement for a fictitious mid-sized investment company called the “Allen Funds.” We told participants that the ad, which was modeled closely after an actual ad, appeared in a popular business publication. All four versions provided identical information about Allen Funds’ research strategy, fees and expenses, advisor, and risk profile. This information indicated that Allen Funds charged a 1% management fee for finding and investing in growth-oriented companies that can deliver high returns over the long term. This was the only information about Allen Funds provided to participants in the control group ($n = 60$).

For participants in the three experimental groups ($ns = 67\text{--}71$), the ad included statements related to past performance successes and an “Average Annual Total Returns” chart that documented these successes. The charts provided historical returns for the (a) *Allen MidCap Growth Fund*, (b) *Allen Growth Fund*, and (c) S&P 500 Index. The past performance data showed that the two featured funds outperformed the S&P 500 index by several percentage points in both the short term and long term.

The experimental conditions differed from one another in terms of a single statement in the ad pertaining to the number of funds operated by Allen Funds. The statement in one condition indicated that the funds named in the chart were “the only 2 funds operated by Allen Funds.” We refer to this as the *no-selection* (or “2 of 2”) condition because the advertised funds were not selected from a larger set of funds (i.e., two advertised funds out of two funds operated by the company in total). The statement in a second condition indicated that the funds were “2 of 30 funds operated by Allen Funds,” and that performance results for the other 28 funds could be obtained by contacting the company. We refer to this as the *transparent selection* (or “2 of 30”) condition because participants were explicitly told that the two advertised funds were selected from a larger set of funds. The statement in a third condition did not indicate how many funds Allen Funds operated. Instead, it noted that “performance results for all funds may be obtained by contacting Allen Funds.” We refer to this as the *hidden selection* (or “2 of ?”) condition because investors were not told the total number of funds that Allen Funds operated.² This third condition is

probably most representative of actual mutual fund advertisements.

After reviewing the ad, investors answered two questions about the quality of Allen Funds’ management company using 1–7 Likert-type response scales. Specifically, we asked investors whether they thought Allen Funds was (a) an extremely successful investment company, and (b) an investment company that deserves investors’ trust, where higher ratings indicated more positive assessments. We asked these questions to determine whether and when the past success of individual funds affected judgments about the provider company. Next, we informed investors that Allen Funds plans to introduce a new growth fund that would “have the same type of quality management team that you have come to expect from our funds.” We asked investors (a) how willing they would be to invest a portion of a \$10,000 gift in this new fund (1–7 Likert-type scale), and (b) what percentage of the \$10,000 they would be willing to invest in this fund. We asked investors about their willingness to invest in a new fund rather than in the funds named in the ad because people have wide-ranging beliefs about the dependency of fund performance across time periods. Such beliefs add undesirable “noise” to our study. We also asked investors to explain their willingness to invest. Finally, we included a manipulation check question that asked investors to indicate the total number of funds operated by Allen Funds and to complete several demographic questions that focused on investors’ financial experience.

4.2. Predictions

We predicted that investors would be *partially sensitive* to selection bias (i.e., sensitive to bias in performance data when the selective process was made transparent but not otherwise). Operationally, we anticipated that investors in the no-selection (2 of 2) and hidden selection (2 of ?) conditions would judge Allen Funds more favorably and report a greater willingness to invest than would investors in the transparent selection (2 of 30) condition. We did not expect to observe differences between the 2 of 2 and 2 of ? groups because investors in the latter group are unlikely to discount the impressive advertised performance data without a sample space prompt. We tested the partial sensitivity theory with planned comparisons between (a) the 2 of ? and 2 of 2 conditions versus the 2 of 30 condition, and (b) the 2 of ? versus the 2 of 2 condition.

Regarding the role that investment expertise plays in selection neglect, prior research shows that financial experts and novices often share cognitive shortcomings (Shapira and Venezia 2001, Wilcox 2003). One study found that financial professionals were *less* likely than novices to believe that financial advertisers would provide biased information (Diacon 2004).

² A more technically accurate label for the hidden selection condition is the unknown selection condition because it is possible that the complete sample space of funds is identical to the number of funds advertised (in which case there would be no selection). However, Study 1 indicated that 99.4% of mutual fund companies operate more funds than they advertise, and the mean (median) number of nonadvertised funds is 74 (41). We therefore refer to the unknown sample space condition as hidden selection.

Based on this evidence, we predict that the partial sensitivity pattern will hold for expert and novice investors alike. However, Barber et al. (2005) found that experienced investors assigned less weight to past performance data for mutual funds than novice investors. Accordingly, we also predict a main effect for expertise such that our experts are generally less willing to invest than the novices.

4.3. Results

The results support the partial sensitivity theory of selection bias for novice and expert investors on all key dependent measures. We first discuss the manipulation check and then analyze the effects of Experience and Selection on perceived company success, perceived company trustworthiness, willingness to invest, and percentage willing to invest. We also examine the control group responses.

4.3.1. Manipulation Check. We asked investors in the 2 of 2 and 2 of 30 conditions how many funds Allen Funds operated prior to introducing the new growth fund. All investors in the 2 of 2 condition correctly indicated that Allen Funds operated two funds. Nine investors in the 2 of 30 condition indicated an incorrect number of funds and are excluded from the remaining analyses.³

4.3.2. Beliefs About Fund Company: Success and Trustworthiness. We examined two facets of investors' beliefs about the fund company: their perceptions about the company's success and trustworthiness. We conducted separate analysis of variances (ANOVAs) for success and trustworthiness with Experience and Selection as independent variables.⁴ Both ANOVAs showed main effects for Experience (Success: $F_{1,178} = 17.03$, $p < 0.01$; Trustworthiness: $F_{1,177} = 27.24$, $p < 0.01$) and Selection (Success: $F_{2,178} = 3.24$, $p < 0.05$; Trustworthiness: $F_{2,177} = 5.91$, $p < 0.01$). There were no significant Experience $\times$ Selection interactions. The Experience main effects indicates that experts rated the company as less successful and less trustworthy than novices (Success: $M_{\text{Experts}} = 4.1$, $M_{\text{Novices}} = 4.9$; Trustworthiness: $M_{\text{Experts}} = 3.5$, $M_{\text{Novices}} = 4.5$). The pattern of means on the Selection main effects supports the partial sensitivity theory of selection bias. Follow-up contrasts show that investors in the 2 of 30 condition viewed the company as less successful and trustworthy than those in the 2 of 2 and 2 of ? conditions (Success: $F_{2,181} = 5.52$, $p < 0.01$, Effect Size (ES) = 0.4; Trustworthiness: $F_{2,180} = 3.19$, $p < 0.05$, ES = 0.5). Investors in the 2 of 2 and 2 of ? conditions did not differ on these measures (Success: $t_{122} = 0.17$, n.s.; Trustworthiness: $t_{121} = 1.08$, n.s.).

³ The results are similar when data from these nine investors are not excluded.

⁴ We obtained similar results and identical inferences with a multivariate analysis.

4.3.3. Willingness to Invest. We also asked investors to assess their willingness to invest (WTI) in a new fund offered by the same management company and the percent they were willing to invest (Percent). We conducted separate ANOVAs for WTI and Percent using Experience and Selection as independent variables. Both ANOVAs showed main effects for Experience (WTI: $F_{1,187} = 56.03$, $p < 0.01$; Percent: $F_{1,185} = 37.37$, $p < 0.01$) and Selection (WTI: $F_{2,187} = 5.51$, $p < 0.01$; Percent: $F_{2,185} = 3.36$, $p < 0.05$). The Experience main effect indicates that experts were less willing to invest in the company and invested a smaller percent than novices (WTI: $M_{\text{Experts}} = 3.3$, $M_{\text{Novices}} = 4.5$; Percent: $M_{\text{Experts}} = 18.3$, $M_{\text{Novices}} = 37.3$). This result follows from the finding that novices perceived the fund company as more successful and trustworthy than experts, as well as research that shows experts rely less on past performance data than novices (Barber et al. 2005). The pattern of means behind the Selection main effects for the investment measures supports the partial sensitivity theory (see Figure 1). Specifically, investors in the 2 of 30 condition were less willing to invest and invested a smaller percent than those in the 2 of 2 and 2 of ? conditions (WTI: $F_{2,190} = 5.47$, $p < 0.01$, ES = 0.5; Percent: $F_{2,188} = 3.33$, $p < 0.05$, ES = 0.4). Investors in the 2 of 2 condition and the 2 of ? condition did not

Figure 1 Novice and Expert Investors' Mean Willingness to Invest by Condition (Study 2)

differ on either of the investment measures (*WTI*: $t_{131} = 0.11$, n.s.; *Percent*: $t_{130} = 0.25$, n.s.).

4.3.4. Control Group. Investors in the control group read an ad for Allen Funds but did not receive information about past performance or number of funds operated. These investors responded similarly to those in the 2 of 30 condition on all four dependent measures (all $ps > 0.10$). Control condition investors also gave lower ratings to the company and were less willing to invest than investors in the 2 of 2 and 2 of ? conditions (all $ts > 2.0$, $p < 0.05$ for all). The finding that control condition responses closely parallel 2 of 30 condition responses supports the claim that investors in the 2 of 30 condition gave little weight to the excellent (but selected) performance data that they saw in the ad.

4.3.5. Written Explanations. Marginally, more investors in the 2 of 30 condition than the 2 of ? condition spontaneously commented on the absence of performance information on other funds operated by Allen Funds (34% versus 16%, $Z = 1.97$, $p < 0.10$). This result suggests that investors are more likely to be concerned about selection bias when the selective nature of the data is transparent.

4.4. Discussion

Study 2 indicates that novice and expert investors respond to selection bias in advertised mutual fund performance data when the selection process is obvious, but treat nonobviously selected data as if it were not selected at all. We made the data selection process obvious or not obvious by providing investors with fund performance data that either did or did not include information that directed readers' attention to the sample space of funds operated by the company. When the advertisements revealed that the company operated many funds, investors appeared to pay more attention to the possibility that the advertised funds had been strategically selected. These investors treated the favorable performance data as if they had little, if any, value. This is as it should be. Data that are selected from a broad sample space by parties who are motivated to communicate a positive image are less diagnostic than unselected data.

Investors were less likely to recognize selection bias in the more realistic condition in which the ads did not include sample space information. Investors in this hidden selection condition were just as enthusiastic about investing in the company's newest fund as investors who *knew* that the company operated exactly two funds, both of which were successful. Apparently, investors in the hidden selection condition did not spontaneously consider the possibility that the stellar data they saw may have been unrepresentative.

Overall, our financial experts were less impressed than novices by the quality of the advertising com-

pany and were less willing to invest in the company's latest fund. This may reflect a general skepticism that goes with working in the field. Nevertheless, experts exhibited the same selection neglect tendencies that we observed in novice investors. Apparently, expertise is an insufficient antidote to selection neglect.

A skeptic might argue that the pattern of data we report arose because investors in the hidden selection condition mistakenly assumed that Allen Funds only operated two funds, but this is unlikely. First, most investors in the hidden selection condition estimated that Allen Funds operated more than two funds ($M = 15$ funds). Second, investors in the hidden selection condition who *did* presume that Allen Funds operated just two funds gave responses that were similar to others in this condition. For example, those who assumed that there were two funds were not more willing to invest ($M_{2 \text{ funds}} = 4.1$, $M_{>2 \text{ funds}} = 4.2$, $t_{64} = 0.24$, n.s.), nor did they invest a greater percent ($M_{2 \text{ funds}} = 30.0$, $M_{>2 \text{ funds}} = 32.6$, $t_{63} = 0.42$, n.s.) than other investors in this condition. This finding supports the claim that investors in the hidden selection condition gave little, if any, thought to the total number of funds when they were evaluating the quality of the fund company and the attractiveness of the investment opportunity. If they had, then those who assumed that there were many funds would have responded with less enthusiasm.

We contend that selection neglect is a genuine phenomenon that arises because people do not ordinarily think about the sample space from which available data were chosen. In the absence of appropriate cues, people accept data without handicap and draw inferences accordingly. We do not necessarily contend that cued investors respond with the *appropriate* amount of enthusiasm. Even investors in our 2 of 30 conditions may have been too enthusiastic about the fund company after viewing the ad. Such a result would be consistent with Cain et al. (2005), who found that people fail to discount advice from a biased source as much as they should, even when the source's conflict-of-interest bias is fully disclosed. Our point is that, in the investment context, people who see ads that tout the performance of a few funds do not spontaneously consider the larger sample space of funds and selection process. However, when the ad includes cues related to sample space or sampling, people are more likely to invoke their statistical heuristics and recognize that selected data should be discounted.

5. Study 3: Debiasing Selection Neglect

The weight of neglected decision variables can often be increased by drawing attention to them (Kahneman and Frederick 2002). Study 2 suggested that sample

space is such a decision variable. The transparent selection manipulation demonstrated one way to draw attention to sample space in a financial advertising task: Include this information in the ad. However, mutual fund companies are not required to disclose the number of funds they operate in their ads. Consequently, selection is hidden rather than transparent in real-world ads. In Study 3, we test whether an *extrinsic* sample space cue may debias selection neglect. We hypothesize that when investors consider the sample space, they are more likely to access their statistical heuristics, recognize the selection bias, and discount the available data accordingly.

We also explore a more global model that describes a *process* by which selection neglect might affect investment intentions. We propose that beliefs about the quality of the fund company and expectations about future performance mediate the relationship between selection condition and willingness to invest. In support, we offer a path analysis. Because path analyses have limited ability to identify causal relations (Holland 1988), we also provide separate individual analyses of the influence of selection condition on fund company beliefs, performance expectations, and willingness to invest.

Study 3 also examines whether individual differences in statistical reasoning affect efforts to debias selection neglect. We have argued that selection neglect occurs when investors fail to access their statistical heuristics. However, people vary in their reliance on statistical principles (Stanovich and West 2000, 2008). Consequently, cues that draw attention to a neglected sample space variable will not be universally effective. In this study, we test the idea that sample space cues are more likely to influence the investment judgments of participants who embrace statistical reasoning than those who do not.

Finally, Study 3 asks whether the partial sensitivity to selection observed in Study 2 will persist when investors are familiar with the company and know that it operates multiple funds. On the one hand, knowledge that a fund operates multiple funds is information that investors need to overcome selection neglect. On the other hand, this knowledge is unlikely to be used unless attention is directed to it. Therefore, we expect to observe selection neglect even for a large, well-known company.

5.1. Method

5.1.1. Participants. A total of 180 business students from a large state university read and responded to a mutual fund advertisement. Of these participants, 36% had prior investing experience, and 91% expected to invest in stocks within the next five years. Participants spent an average of two-and-a-half hours per week reading business-related periodicals and watching business television shows that

included financial advertisements. To test whether the effects of our manipulations vary by investing experience, we conducted ANOVAs for each of our six dependent variables, using investing experience as a covariate. We observe an insignificant experimental condition by investing experience interaction term for each dependent variable (all $p > 0.10$). This result suggests that investing experience did not influence the effects of the experimental manipulations.

5.1.2. Materials and Procedure. The stimuli and procedure in Study 3 were similar to those used in Study 2 with five key changes. First, we changed the name of the advertising company from Allen Funds (a hypothetical company) to Fidelity Investments (an actual company). We chose Fidelity because it is a large, well-known mutual fund that operates approximately 30 growth-oriented mutual funds. Second, we provided performance data from two Fidelity growth funds (Midcap Growth Fund and Growth Fund) that actually *did* outperform the S&P 500 index over the benchmark periods shown in the ad. Third, we updated the time frame and benchmark data (from the S&P 500 index) in the performance chart. Fourth, we assigned investors to one of two conditions—the hidden selection (2 of ?) and transparent selection (2 of 30) conditions. The no-selection (2 of 2) condition is inapplicable here because Fidelity operates many funds. Fifth, half the participants were asked to estimate the total number of growth funds Fidelity operates before answering the primary dependent measures (extrinsic cue). Participants in the uncued conditions were asked this question at the end of the task. The experimental design is 2 (selection: hidden, transparent) $\times$ 2 (extrinsic cue: yes, no) between subjects.

Following the experimental manipulations, participants answered questions related to their (a) beliefs about the quality of Fidelity (success and trustworthiness), (b) expectations for future performance (Fidelity overall and new fund), (c) willingness to invest (how willing to invest and percent willing to invest), (d) beliefs about how Fidelity decides which funds to advertise, (e) statistical reasoning, and (f) demographics and familiarity with Fidelity. As before, we used 7-point Likert-type scales for most questions. The performance expectation questions, advertising questions, and statistical reasoning questions were new. The performance expectation questions assessed participants' beliefs about whether Fidelity's mutual funds overall, and their new growth fund in particular, were likely to outperform the S&P 500. The advertising questions assessed participants' beliefs about how Fidelity selects funds to advertise. One question, which appeared on a separate page prior to the others, asked "How do you think Fidelity Investments decides which mutual funds to advertise?" A scale question that followed asked participants whether

they thought Fidelity deliberately advertises funds that have better-than-average historical performance. The purpose of these questions was to determine whether participants know that large companies like Fidelity do not provide representative fund performance data in their advertisements.

Finally, half of the participants answered six statistical reasoning questions. The questions asked for levels of agreement with the following statements: (1) investigators who think in terms of statistical probabilities are more likely to catch criminals than those who do not; (2) if 10 people predict the score of a lottery game and one gets the numbers exactly right, we should rely more on that person's predictions in future lottery games; (3) even an unskilled economist will occasionally make accurate predictions; (4) in a game of darts, a person who scores 6 bulls-eyes in 6 attempts is more likely to get a bulls-eye in her next attempt than one who scores 8 bulls-eyes in 30 attempts; (5) a customer can learn more about the overall quality of fruit in a store by examining 10 boxes of berries selected by the store manager than by examining 10 boxes of berries selected at random; and (6) a randomly selected professor at a university is more likely to be 40–45 years old than to be 70–75 years old. We reverse-coded responses to questions 2 and 5 so that higher responses always indicate a greater propensity toward statistical reasoning. After assessing the reliability of the underlying construct (Cronbach's $\alpha = 0.7$), we averaged responses to the six questions to form a composite statistical reasoning score for each participant.

5.1.3. Predictions and Analyses. We predict that selection neglect can be overcome with an extrinsic sample space cue. Operationally, investors in the *cued* hidden selection (2 of ?) condition (i.e., those who estimate the number of funds Fidelity operates prior to answering the key questions) will have lower beliefs about the company, lower performance expectations, and be less willing to invest than those in the *uncued* hidden selection (2 of ?) condition. We also predict that investors in the transparent selection (2 of 30) conditions will be unaffected by the absence or presence of the cue because sample space transparency within their advertisement already triggers their statistical heuristics.

In short, we predict the following relations on all of the key dependent measures: $M_{\text{Uncued 2 of ?}} > M_{\text{Cued 2 of ?}} = M_{\text{Uncued 2 of 30}} = M_{\text{Cued 2 of 30}}$. We test this pattern with planned contrasts (Buckless and Ravenscroft 1990), using a contrast coefficient of 3 for the uncued hidden selection condition and -1 for the other three conditions. We include a test of the between-group variance that is *unexplained* by the contrast. A lack of significance on this residual noncontrast between-group effect indicates that the planned

contrast explains all significant observed between-group variance (Rosnow and Rosenthal 1996). Finally, we predict that participants with lower composite statistical reasoning scores will give relatively high ratings on the dependent measures *regardless* of whether or not they receive the sample space cue.

5.2. Results

We find that the explicit sample space cue debiases selection neglect on the six key dependent measures. We also find that those with lower statistical reasoning scores were less likely to be influenced by our debiasing manipulations than those with higher scores. Before providing details, we discuss the manipulation checks.

5.2.1. Manipulation Check and Related Tests.

Participants in all conditions indicated how many funds they believed Fidelity operated prior to the introduction of the new growth fund. When asked prior to the key dependent measure, this question was our selection neglect debiasing mechanism. This question also served as a manipulation check for participants in the 2 of 30 condition. Eleven participants in the uncued 2 of 30 condition and 18 participants in the cued 2 of 30 condition indicated an incorrect number of funds. These participants are excluded from the remaining analyses. Three participants in the 2 of ? conditions who gave responses that were more than 10 standard deviations above the mean are also excluded. Among the remaining participants in the 2 of ? conditions, the mean estimated number of funds was 36 in the uncued condition and 41 in the cued condition. The mean estimated number of funds in both the cued and uncued 2 of 30 conditions was 30. The similarity of estimates across the four experimental conditions ($F_{3,140} = 0.33$, n.s.) suggests that introduction of the cue did not affect participants' beliefs about how many funds Fidelity operates. It also rules out differential beliefs about the number of funds Fidelity operates as an explanation for differences on the dependent measures across experimental conditions.

We also asked investors how they think companies decide which funds to advertise. Study 1 showed that companies advertise their better-performing funds. If investors believe otherwise, then selection neglect may be more about financial advertising naiveté than cognitive bias. However, the data are inconsistent with naiveté. In response to an open-ended question, 77% of participants mentioned that Fidelity advertises their highest-performing stocks. No other factor was mentioned by more than 10% of participants. Moreover, all but one participant agreed with this statement: "When deciding which growth funds to advertise, Fidelity probably chooses funds with better-than-average historical performance."

Figure 2 Investors' Performance Expectations and Willingness to Invest by Condition (Study 3)

5.2.2. Beliefs About Fidelity: Success and Trustworthiness. Consistent with expectations, participants in the uncued 2 of ? condition believed that Fidelity was more successful than did participants in the other three conditions ($M_{\text{Uncued 2 of ?}} = 5.4$ versus $M_{\text{Cued 2 of ?}} = 4.6$, $M_{\text{Uncued 2 of 30}} = 4.9$, $M_{\text{Cued 2 of 30}} = 4.4$; contrast $F_{1,141} = 12.36$, $p < 0.01$; residual noncontrast $F_{2,141} = 2.19$, n.s.). We observed similar results for trustworthiness ($M_{\text{Uncued 2 of ?}} = 5.0$ versus $M_{\text{Cued 2 of ?}} = 4.5$, $M_{\text{Uncued 2 of 30}} = 4.5$, $M_{\text{Cued 2 of 30}} = 4.3$; contrast $F_{1,141} = 7.07$, $p < 0.01$; residual noncontrast $F_{2,141} = 0.34$, n.s.). In particular, participants in the cued 2 of ? condition judged Fidelity as less successful and trustworthy than those in the uncued 2 of ? condition (Success: $t_7 = 3.15$, $p < 0.01$; Trustworthiness: $t_7 = 2.01$, $p < 0.05$). These results suggest that our extrinsic sample space cue made the selection process more transparent for participants in the 2 of ? condition. When participants were cued to think about sample space, those in the 2 of ? condition were no more impressed with Fidelity than were those in the 2 of 30 conditions.

5.2.3. Performance Expectations and Willingness to Invest. Participants also indicated whether they expected Fidelity stock funds overall and the new growth fund in particular to beat the S&P 500 index (see panel A of Figure 2). Participants in the uncued 2 of ? condition were much more likely to believe that both Fidelity and its new fund would outperform the S&P 500 than participants in the other three conditions (Fidelity Expectations: contrast $F_{1,141} = 14.64$, $p < 0.01$;

residual noncontrast $F_{2,141} = 0.04$, n.s.; New Fund Expectations: contrast $F_{1,141} = 14.37$, $p < 0.01$; residual noncontrast $F_{2,141} = 0.29$, n.s.). As predicted, participants in the cued 2 of ? condition had lower expectations for Fidelity overall and for the new growth fund than those in the uncued 2 of ? condition (Fidelity Expectations: $t_7 = 3.30$, $p < 0.01$; New Fund Expectations: $t_7 = 3.534$, $p < 0.01$).

We observed a similar pattern in our willingness to invest measures (see panel B of Figure 2). Participants in the uncued 2 of ? condition were more willing to invest and invested a greater percent than those in the other three conditions (WTI: contrast $F_{1,141} = 12.59$, $p < 0.01$; residual noncontrast $F_{2,141} = 1.39$, n.s.; Percent: contrast $F_{1,141} = 11.72$, $p < 0.01$; residual noncontrast $F_{2,141} = 0.06$, n.s.). Finally, as expected, participants in the cued 2 of ? condition were less willing to invest and invested a smaller percent than those in the uncued 2 of ? condition (WTI: $t_7 = 3.71$, $p < 0.01$; Percent: $t_7 = 2.65$, $p < 0.01$). These results support our contention that an extrinsic sample space cue can help debias selection neglect by leading investors to think about the broader sample space and access their statistical heuristics.

5.2.4. Path Analysis. Thus far, we have examined selection neglect and a debiasing technique on investors' (a) beliefs about the fund company, (b) performance expectations, and (c) willingness to invest.⁵

⁵ To ensure that the questions that measure beliefs about the fund company, performance expectations, and willingness to invest

Figure 3 Path Analysis (Study 3)

Notes. This figure reports the standardized regression coefficients from our path analysis. Bold lines indicate significant paths (all $p < 0.01$). Dashed lines indicate insignificant paths (all $p > 0.10$).

We now offer a test of the relations among these variables using the AMOS structural equation modeling program. We have already demonstrated that investors' beliefs about the fund company differ by experimental condition. Specifically, participants who were in the three conditions that were cued to think about sample space rated Fidelity as less successful and trustworthy than those in the remaining condition. We expect these beliefs about Fidelity to drive participants' performance expectations. This prediction assumes that at least some investors believe in a "hot hand" model of mutual fund returns (i.e., past success predicts future success) rather than a random walk model. Research confirms that this is what investors believe (Capon et al. 1996) and that they act on those beliefs (Jain and Wu 2000).⁶ We expect participants' performance expectations to then drive willingness to invest. Figure 3 provides a graphical summary of this model. The data fit the model well. The model's comparative fit index is 0.99. Values greater than 0.95 indicate a good model fit (Hu and Bentler 1999).

The individual path coefficients are also as expected. Specifically, we find that the *Condition* → *Beliefs about Fund Company* link (coefficient = -0.28), the *Beliefs about Fund Company* → *Performance Expectations* link (coefficient = 0.75), and the *Performance Expectations* → *Willingness to Invest* link (coefficient = 0.79) are significant ($p < 0.01$ for all). Importantly, the *Condition* → *Willingness to Invest* link is significant when we omit *Beliefs about Fund Company* and *Performance Expectations* from the model (coefficient = -0.25 , $p < 0.05$); but when these mediating variables

are included, the *Condition* → *Willingness to Invest* link is not significant (coefficient = -0.06 , n.s.). Thus, our manipulated variables affect willingness to invest via their effects on performance expectations and beliefs about Fidelity.

5.2.5. Individual Differences in Statistical Reasoning. Because the sample space cue was designed to improve access to statistical heuristics, it follows that nonstatistical reasoners are less likely to benefit from its introduction. We test this idea by examining whether participants' statistical reasoning scores predict their willingness to invest. We expect that relatively high statistical reasoners will fit the pattern $M_{\text{Uncued 2 of ?}} > M_{\text{Cued 2 of ?}} = M_{\text{Uncued 2 of 30}} = M_{\text{Cued 2 of 30}}$ on both the willingness to invest and percent willing to invest measures. Relatively low statistical reasoners are less likely to fit this pattern because the sample space cue is less likely to trigger statistical heuristics.

The data support these predictions. We perform a median split on statistical reasoning scores to identify those who are relatively more or less inclined to reason this way. We then conduct ANOVAs with *Statistical Reasoning* and *Experimental Condition* as independent variables, and *WTI* and *Percent* as dependent variables. We observe a significant *Statistical Reasoning* by *Experimental Condition* interaction term in both ANOVAs (*WTI*: $F_{3,60} = 5.63$, $p < 0.01$; *Percent*: $F_{3,60} = 2.75$, $p = 0.05$), suggesting the statistical reasoning has a greater effect on *WTI* and *Percent* in some experimental conditions than others. Follow-up tests show that, among participants with above-the-median statistical reasoning scores, the investment pattern was identical to the general finding: Investors in the uncued 2 of ? condition were more willing to invest than those in the other conditions that promoted access to statistical heuristics (*WTI*: $M_{\text{Stat. heuristics not accessed}} = 5.4$, $M_{\text{Stat. heuristics accessed}} = 2.6$, $t_{34} = 5.29$, $p < 0.01$; *Percent*: $M_{\text{Stat. heuristics not accessed}} = 51.3$, $M_{\text{Stat. heuristics accessed}} = 17.3$, $t_{34} = 5.55$, $p < 0.001$).

capture three separate underlying constructs, we conducted a factor analysis on the six questions. The resulting three-factor rotated component matrix shows that each question has its primary loading on the expected construct (all coefficients > 0.75).

⁶ Our predictions depend on investors' beliefs about the correct model rather than which model actually describes returns.

This pattern did not appear among participants with below-the-median statistical reasoning scores (*WTI*: $M_{\text{Stat. heuristics not accessed}} = 4.7$, $M_{\text{Stat. heuristics accessed}} = 4.4$, $t_{35} = 0.64$, n.s.; *Percent*: $M_{\text{Stat. heuristics not accessed}} = 48.5$, $M_{\text{Stat. heuristics accessed}} = 41.7$, $t_{35} = 0.74$, n.s.).

Correlations between statistical reasoning scores and *WTI* in each of the four conditions also illustrate the effects of individual differences in statistical reasoning in our task. The data show that statistical reasoning scores are negatively correlated with *WTI* and *Percent* for the three conditions that enhanced access to statistical heuristics (*WTI*: $r_{\text{Cued 2 of ?}} = -0.46$, $r_{\text{Uncued 2 of 30}} = -0.85$, $r_{\text{Cued 2 of 30}} = -0.69$, all $p < 0.05$; *Percent*: $r_{\text{Cued 2 of ?}} = -0.55$, $r_{\text{Uncued 2 of 30}} = -0.71$, $r_{\text{Cued 2 of 30}} = -0.49$, all $p < 0.05$). This is the expected result if people who have lower statistical reasoning tendencies do not access corrective statistical heuristics that dampen enthusiasm for investing with Fidelity. But what about the lone condition that was *not* conducive to accessing statistical heuristics (the uncued 2 of ? condition)? Here the negative relationship between statistical reasoning scores and *WTI* disappears (*WTI*: $r_{\text{Uncued 2 of ?}} = 0.25$, n.s.; *Percent*: $r_{\text{Uncued 2 of ?}} = -0.02$, n.s.). This result provides further support for the theory that investment enthusiasm in the uncued 2 of ? condition arises because people do not automatically think about sample space. Even the high statistical reasoners in this condition did not access their statistical heuristics.

Finally, the results suggest that selection neglect is not confined to hypothetical situations or to situations in which investors are unfamiliar with the advertising company. Even though investors knew that the advertising company operated numerous funds, those who were not *explicitly reminded* of this fact were more impressed with the company and more willing to invest than those who received the reminder in the advertisement itself. Apparently, one's background knowledge about the number of funds operated by a company provides insufficient protection against the perils of selection neglect.

5.3. Discussion

People are relatively insensitive to selection bias unless the sample space or sampling process is called to their attention. Sometimes task stimuli contain sufficient sample space or sampling cues. These cues help many people recognize a data selection situation, access statistical heuristics, and discount accordingly. However, when data providers have an interest in the inferences others draw from those data, sample space cues may not be available. In Study 3 we found that, for many people, a simple extrinsic sample space cue (asking people to estimate the total number of funds a company operates) debiased selection neglect in a financial advertising setting. This cuing technique

may also be useful for debiasing selection neglect in other settings. For example, when medical personnel or drug representatives tout the success of a new treatment with before-and-after pictures, treatment evaluators might be asked to consider how many people in total received the treatment. Importantly, Study 3 also showed that it may be harder to debias selection neglect with such prompts among people who are less inclined to reason statistically.

6. General Discussion

Financial magazines and advisors frequently counsel investors to consider the track record of mutual funds before investing (Chamberlain et al. 2009). However, this seemingly prudent advice can backfire if (a) companies selectively advertise their most successful funds, and (b) investors fail to appreciate this fact. We investigated these issues in three studies and found selection bias in mutual fund advertisements and selection neglect in investors. The finding that mutual fund companies provide biased snapshots of their success by selectively advertising their better-performing funds (Study 1) comes as no surprise. More surprising, perhaps, is that the judgments and decisions of novice and professional investors alike are influenced by advertised performance data that almost certainly arose through a selective (i.e., biased) process. Unless cued to attend to sample space, investors apparently gave little thought to selection bias and relied on potentially biased data (Studies 2 and 3). It may seem surprising that professional investors' knowledge and experience fail to inoculate them from selection neglect. However, experience is an insufficient teacher (Einhorn and Hogarth 1978), and cognitive errors may not yield to expertise, particularly in the financial realm (Wilcox 2003).

We had success debiasing selection neglect with subtle manipulations, although the bias persisted among those who scored lower in statistical reasoning. This group might benefit from a stronger and more direct debiasing manipulation. For investors who scored higher in statistical reasoning, enthusiasm for the company and its offerings dampened when their attention was directed to the broader sample space from which the advertised funds were selected. This finding supports our conclusion that *there is a disconnect between investors' understanding of the limited value of selected data and their ability to access and apply this understanding in situations where the sample space or selection process is not expressly cued*.

Research on statistical reasoning finds a similar disconnect in other areas. For example, most people understand that base rates (i.e., background probabilities) provide relevant information for case predictions. However, people sometimes give little weight to base rates (for a review, see Koehler 1996). Similarly, people may give great weight to anecdotes and vivid

testimonials in reasoning tasks at the expense of more systematic evidence (Nisbett and Ross 1980). Consider, for example, quotes from satisfied customers on a company flier or examples of successful stock picks on a financial adviser's website. Such information disconfirms extreme, but irrelevant, hypotheses such as "all customers are dissatisfied," or "none of the advisor's recommendations were winners." However, the value of this information for assessing quality is indeterminate because the data may consist of self-serving outliers. This is not a complicated or controversial point, yet it merits attention because studies show that untutored intuition commonly treats sample data as near-perfect representations of the populations from which they were derived (Rabin 2002). This error does not arise from a conscious belief that samples are perfectly representative of populations (Fiske and Taylor 1991). Instead, *questions of sample space and sample adequacy simply do not surface in the minds of decision makers absent an appropriate cue.*

6.1. Selection Neglect or Strategic Behavior Neglect?

One might argue that investors' failure to discount the selected performance data contained in mutual fund ads is due to strategic behavior neglect (i.e., a failure to appreciate the strategic nature of advertisements) rather than selection neglect (i.e., a failure to appreciate the larger sample space of funds). To rule out this possibility, we asked Study 3 participants an open-ended question about how managers decide which funds to advertise. Most participants (77%) spontaneously mentioned that Fidelity advertises its highest-performing stocks. This result is inconsistent with a claim that investors' ignorance about advertisers' strategic behavior drives our effects. Further, our effects do not seem to arise from a failure to access knowledge about advertisers' strategic behavior. Participants who received performance data that were not strategically selected (i.e., those in the 2 of 2 condition) were less willing to invest than those who received data that were strategically selected (i.e., those in the 2 of 30 condition). If participants failed to access their knowledge about strategic behavior, judgments in the two conditions would have been the same. Instead, participants in the 2 of 30 condition apparently recognized that a company that operates 30 funds is likely to advertise its top performers rather than its typical performers, and they discounted the advertised data accordingly. These results suggest that investors understand the strategic nature of fund advertisements and can access this knowledge when judging the fund company.

As further evidence that selection neglect rather than strategic behavior neglect best explains our data, we demonstrated selection neglect in a setting devoid

of strategic behavior. We provided 94 business students similar to those in Study 3 with a list of the 25 U.S. mutual funds with the highest returns over the last 3-, 5-, and 10-year periods. Participants were informed that the list was compiled by *Morningstar* (an independent company that tracks mutual fund performance). Reliance on this source eliminated concerns about strategic selection and presentation by a particular fund company. The list showed that Fidelity Investments had a significant number of top-performing mutual funds over each time period. Fidelity is one of the largest mutual fund companies in the United States, and thus could have a significant number of funds on the list by chance alone. However, if investors fall prey to selection neglect, they will not access their knowledge about Fidelity's large sample space of funds unless prompted. To test this idea, we asked all participants to estimate the total number of mutual funds that Fidelity operates and to express their willingness to invest in a new Fidelity fund. We varied the order of these questions: Half of the participants indicated their willingness to invest *after* estimating the total number of Fidelity funds (Cued condition), and half indicated their willingness to invest *before* estimating the number of funds (Uncued condition). Whereas a theory of strategic behavior neglect does not predict differences between the Cued and Uncued conditions in terms of their willingness to invest, the selection neglect theory does. In support of selection neglect, participants in the Cued condition were significantly less willing to invest and invested a smaller percentage than those in the Uncued condition (WTI: $\text{Mean}_{\text{Cued}} = 4.2$, $\text{Mean}_{\text{Uncued}} = 5.5$, $t = 4.67$, $p < 0.001$; Percent: $\text{Mean}_{\text{Cued}} = 37.5$, $\text{Mean}_{\text{Uncued}} = 52.3$, $t = 3.28$, $p < 0.01$).

6.2. Lessons and Solutions

The lessons of partial sensitivity to selection bias (i.e., sensitivity to selection bias when the biased sampling process is made transparent, but not otherwise) depend on whether one is the provider or recipient of information. The lesson for information providers is that selective advertising works. By advertising selectively, information providers may find it easier to promote a positive image and make a persuasive case for its products and services. In the mutual fund world, companies might also exploit investors' insensitivity to hidden selection bias by increasing the number of funds they operate, increasing the volatility of the individual funds, and decreasing performance correlation across funds. This strategy would improve companies' chances of having a few funds that perform well above the benchmarks by chance alone. These funds could then be advertised selectively.

If markets are efficient, no lesson is necessary for information recipients, because those who mistakenly rely on the selected data in mutual fund ads

will not incur any real costs. However, prior studies show that markets are not efficient in this setting. Jain and Wu (2000) find that advertised mutual funds underperformed the S&P 500 by an average of 7.9% in the year subsequent to the advertisement. If this result persists, investors who are more willing to invest in a fund after viewing its advertised performance will be worse off, on average. This suggests that when evaluating advertised performance data, investors should consider the (typically undisclosed) number of chances the advertising company had to portray favorable data. They should consider how many funds a company operates and how those funds performed in different time frames. The more opportunities a company has to present positive outcome data, the more skeptical investors should be of advertisements that depict outstanding performance. In sum, the lesson for information recipients is that they should consider advertisers' incentives to manipulate available cues (Daniel et al. 2002) and the ease with which they are able to do so.

From a regulatory standpoint, it may be appropriate to modify the rules that dictate financial advertising practices. Rules 482 and 34b-1 of the SEC and Rules 2210 and 2211 of the National Association of Securities Dealers (NASD) regulate mutual fund advertising. Companies that advertise must provide "a sound basis for evaluating the facts in regard to any particular security," and may not "omit any relevant material fact or qualification if the omission, in light of the context of the material presented, would cause the communications to be misleading" (NASD Rule 2210(d)(1)(A)). Although none of the companies in our study disclosed the total number of funds it offers in its ads, we submit that such disclosure is a relevant material fact. Failure to disclose this information may mislead potential investors.

6.3. Future Research

This paper focused on *product-level* selection bias and selection neglect in a financial advertising context. Future researchers may wish to examine other forms of selection bias such as which companies advertise in the first place (company-level selection bias) or which product features companies reveal (content-level selection bias). Recent SEC rulings have clamped down on one form of selection bias by requiring a uniform set of disclosures for advertised products. Still, creative companies will find ways to draw attention to selected funds and time frames without violating SEC standards.

Researchers interested in selection neglect more broadly may wish to investigate ways to sensitize people to sample space in contexts where regulatory agencies are unlikely to help. How can we convince admissions committees that letters of recommendation provided by persons of a candidate's choosing

have minimal diagnostic value? How can we remind jurors that they are hearing a strategically selected set of evidence (Koehler and Thompson 2006)? How can we remind readers everywhere that the phrase "for example"—which we use five times in this paper—signals a forthcoming illustration that is more likely to be extreme than typical? Selection bias may be everywhere, but the tendency to fall victim to its unwelcome influence is treatable.

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