

# DO ADS INFLUENCE EDITORS? ADVERTISING AND BIAS IN THE FINANCIAL MEDIA<sup>†</sup>

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## Abstract

The independence of editorial content from advertisers' influence is a cornerstone of journalistic ethics. We test whether this independence is observed in practice. We find that mutual fund recommendations are correlated with past advertising in three personal finance publications but not in two national newspapers. Our tests control for numerous fund characteristics, total advertising expenditures, and past mentions. While positive mentions significantly increase fund inflows, they do not successfully predict returns. Future returns are similar for the funds we predict would have been mentioned in the absence of bias, suggesting that the cost of advertising bias to readers is small.

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# I. Introduction

Recently, there has been considerable interest in political media bias (Groseclose and Milyo [2004], Baron [2005], and Mullainathan and Shleifer [2005]). There is also growing interest in whether the media biases its content to benefit advertisers.<sup>1</sup> For their part, media outlets tend to strongly deny that such a pro-advertiser bias exists. For example, a 1996 article in *Kiplinger's Personal Finance* printed statements from editors at a number of personal finance publications (including the three in our study) claiming that advertisers have no influence over published content.<sup>2</sup> Despite the important role that the media plays in generating and disseminating information to consumers and investors, we are aware of few systematic attempts to test the accuracy of these claims.<sup>3</sup>

In this paper, we test for advertising bias within the financial media. Specifically, we study mutual fund recommendations published between January 1997 and December 2002 in five of the top six recipients of mutual fund advertising dollars. Controlling for observable fund characteristics and total family advertising expenditures, we document a positive correlation between a family's lagged advertising expenditures and the probability that its funds are recommended in each of the personal finance publications in our sample (*Money Magazine*, *Kiplinger's Personal Finance*, and *SmartMoney*). While we consider several alternative explanations below, the robustness of the correlation leads us to conclude that the most plausible explanation is the causal one, namely, that personal finance publications bias their recommendations—either consciously or subconsciously—to favor advertisers. In contrast, we find no such correlation between advertising and content in either national newspaper (the *New York Times* and *Wall Street Journal*).

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<sup>1</sup>For example, Baker [1994] and Hamilton [2004] both argue that the media biases its content to benefit advertisers, and Ellman and Germano [2005] model this bias as arising from advertisers committing to punish publications that run negative stories. A related bias is posited by Dyck and Zingales [2003], who document a positive correlation between the way earnings announcements are reported in a press release and the way they are reported in the media and argue that the correlation is consistent with reporters biasing articles in exchange for access to private information.

<sup>2</sup>Goldberg, Steven, “Do the ads tempt the editors? (influence of mutual fund advertising on personal finance publication editors),” *Kiplinger's Personal Finance*, May 1996. The article was written in response to an earlier article in *Fortune* accusing *Forbes* of “turning downbeat stories into upbeat stories in order to keep advertisers happy—even at the risk of misleading their own readers.”

<sup>3</sup>One exception is Reuter [2002], who asks whether advertising biases wine ratings. We briefly discuss his findings below. Another exception is Miller [2004], who examines a sample of firms that the SEC found guilty of accounting fraud and finds that the media is no less likely to break stories about firms in the 15 industries with the highest propensity to advertise, but acknowledges that the use of industry-level advertising data may reduce the power of this test. More generally, our work relates to other studies of correlations between expert opinion and business interests. For example, Lin and McNichols [1998] and Michaely and Womack [1999] find that sell-side analysts' buy and sell recommendations favor the companies with which their employers do investment banking business. Zitzewitz [2005] finds that figure skating judges are nationalistically biased and “sell” bias to colleagues by engaging in vote trading.

Mutual funds recommendations are a good laboratory in which to test for advertising bias. Product recommendations are a form of content that advertisers might expect to benefit most from biasing. Mutual funds are numerous and because they are financial assets their ex-ante and ex-post quality is relatively easy to observe. The availability of detailed data on funds' objective characteristics helps us control for differences in schools of thought about product quality in a way that would be difficult in other settings. For example, suppose we found that GM advertised more and received better reviews than Toyota in *Muscle Cars*. Here one might conclude that *Muscle Cars* simply likes muscle cars, and GM advertises there to reach its readers.<sup>4</sup> With mutual funds, differences in schools of thought about fund selection are largely over the relative importance of variables we observe in our data, such as past returns and expenses. What we interpret as possible evidence of bias is when, controlling for these and other factors, a publication is more likely to recommend funds from the mutual fund families that have advertised the most within its pages.

For mutual fund families to benefit from biased recommendations, at least some set of investors must rely upon them. Therefore, after testing for bias, we examine the impact that the mentions in our sample have on fund flows. controlling for past media mentions and a variety of fund characteristics, a single additional positive media mention for a fund is associated with inflows ranging from 7 to 15 percent of its assets over the following 12 months. While investors appear to respond to the media mentions, we find that the media mentions have little ability to predict future returns. Interestingly, this is not due to the pro-advertiser bias. When we predict which funds would have been mentioned in the absence of bias, we find little difference in future returns. However, we do find that the personal finance publications would perform significantly better by simply recommending funds with the lowest expense ratios in their investment objectives.

It is a puzzle why readers respond to recommendations that do not predict future returns. Conditional on their behavior, however, publications selecting funds to mention from the large number available have a valuable set of favors to bestow. In deciding whether to reward an advertiser, publications trade off the benefits (encouraging future advertising) with the costs (harming one's reputation by mentioning a lower-quality fund). When there is a wide selection of advertisers' funds to recommend, the cost of advertising bias

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<sup>4</sup>Reuter [2002] finds that advertisers in *Wine Spectator* receive slightly higher ratings than non-advertisers, controlling for ratings in *Wine Advocate*, which does not accept advertising. While *Wine Spectator* claims to use blind tastings to rate wines—a claim intended to increase reader confidence in the ratings—approximately half of the ratings difference is associated with the selective retasting of advertisers' wines. The rest of the rating difference may, in fact, be due to different schools of thought about how to rate wines.

may not be that large, especially if one does not internalize the reputational spillover to ones peers. This is precisely why the journalism profession developed an ethical sanction against advertising bias, because the returns to favoring advertisers might otherwise be high.

## II. Data

Our tests for advertising bias require data on advertising expenditures, media mentions, and mutual fund characteristics. Data on monthly mutual fund advertising expenditures by publication and fund family were purchased from Competitive Media Research (CMR).<sup>5</sup> According to these data, the mutual fund industry’s annual advertising expenditures averaged approximately \$307 million during our 1996-2002 sample period, with \$80 million (26%) going to national newspapers and \$119 million (39%) going to consumer magazines.

The publications we study include five of the six top recipients of mutual fund advertising between 1998 and 2002: the *Wall Street Journal* (\$48.5 million per year), *Money* (\$22.1 million), *New York Times* (\$14.0 million), *Kiplinger’s Personal Finance* (\$12.2 million), and *SmartMoney* (\$8.7 million). (We attempted to gather media mentions for *Mutual Funds* (\$14.0 million) but were unable to access its content electronically.) In total, the publications in our sample account for approximately 45 percent of the mutual fund industry’s advertising expenditures. Naturally, mutual fund advertising is a more important source of revenue for the personal finance publications than the national newspapers. Whereas mutual fund advertising accounts for 3.8 percent of advertising revenues at the *Wall Street Journal* and 1.1 percent at the *New York Times*, it accounts for 15 percent at *Money*, 16 percent at *SmartMoney*, and 28 percent at *Kiplinger’s*. We also gather media mentions from *Consumer Reports*, which does not accept advertising.

The media mention data vary across publications and are summarized in Table I. Since these data had to be hand collected, for several publications we restrict attention to particular articles or columns. For the *New York Times*, we track funds mentioned in a column from the Sunday Business section titled “Investing With.” The column spotlights a particular fund, interviewing fund managers and providing details such

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<sup>5</sup>For print publications, CMR tracks the size of each advertisement and estimates a dollar cost for the advertisement based on the publication’s quoted advertising rates and any likely discount. Comparing CMR’s estimates of total print advertising revenue for the *New York Times* and *Wall Street Journal* to figures reported in the parent companies’ 10K filings, the CMR estimates often differ from the actual revenues by less than 10 percent.

as past returns, expense ratios, and the fund family’s contact information. For *Money*, we focus on the composition of the *Money 100* list, a list of recommended mutual funds published once a year between 1998 and 2002. Similarly, for *Consumer Reports* we focus on articles listing recommended equity funds, bond funds, or both. For each of these publications, we classify the mutual fund mentions as positive.

Since *Kiplinger’s Personal Finance* and *SmartMoney* do not publish lists of recommended funds analogous to the *Money 100* list, for these two publications we analyze every article containing the word “fund.” For articles that focus on mutual funds (rather than mention them in passing), we classify the article as making recommendations across investment objectives, within a particular investment objective, or within a particular mutual fund family. We also classify each mutual fund mention as positive or negative. As suggested by the representative article titles reported in Table I, this rarely involved close judgment calls. When we could not determine whether a mention was positive or negative, we dropped the mention from our sample; we did this for 8 of the 783 mentions in *Kiplinger’s* and 16 of the 2,417 mentions in *SmartMoney*. Finally, for the *Wall Street Journal*, we focus on a daily column titled “Fund Track,” that mentions funds either because they are the subject of news (such as manager turnover) or because their managers are being quoted on an issue. In the (pre-scandal) time period we study, being quoted on an issue in the *Wall Street Journal* is presumably positive exposure for the manager’s fund. However, since “Fund Track” rarely recommends a course of action for fund investors, mentions in the *Wall Street Journal* are potentially different from mentions in the other publications we study. At a minimum, mentions in the *Wall Street Journal* can be viewed as proxies for the news coverage of specific funds.

Data on U.S. mutual fund returns and characteristics come from CRSP. The unit of observation is fund  $i$  in month  $t$  and the sample period is January 1996 through December 2002.<sup>6</sup> Table II reports summary statistics for the full sample of mutual funds and for funds receiving mentions from a given publication in month  $t$ .<sup>7</sup> Univariate comparisons indicate that funds receiving positive media mentions tend to be larger,

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<sup>6</sup>Our sample includes all domestic equity funds, international equity funds, hybrid funds (which invest in debt and equity), and bond funds, but excludes money market funds because they are rarely mentioned in the publications we study. For mutual funds with multiple share classes, we calculate fund-level returns, inflows, and other characteristics, and include one observation per fund per month in our sample. Also, since we merge the media mention data with the CRSP Survivor-Bias Free US Mutual Fund Database using ticker, our sample is limited to funds for which we were able to locate a ticker for at least one share class.

<sup>7</sup>Because magazines are typically available on the newsstand in the month prior to the month stated on their cover, we code mentions in the month  $t + 1$  issue of *Money*, *Kiplinger’s*, *SmartMoney*, and *Consumer Reports* as occurring in month  $t$ . For example, we code mentions in the June 1998 issue of *Money* as occurring in May 1998. In contrast, mentions in a June 1998 issue of the *Wall Street Journal* or *New York Times* are coded as occurring in June 1998.

come from larger fund families, and have higher returns and inflows over the prior 12 months than their peers. They are less likely to charge investors a sales commission (load), but their expense ratios are roughly comparable. Relative to the actual distribution of mutual funds across investment objectives, mentions in the publications we study focus disproportionately on general domestic equity funds.

Funds receiving positive mentions belong to families that spend a greater percentage of family assets on both print and non-print advertising and, since these families are larger, spend much more than average in absolute terms. Interestingly, the sample of funds recommended by *Consumer Reports* also come from families that spend an above-average amount on advertising. This suggests that advertising may be correlated with characteristics that are unobservable to the econometrician but that the financial media uses to rank funds. Consequently, our tests for advertising bias control for fund families' general level of advertising. Examining the share of print advertising by publication reveals that funds receiving mentions from a publication tend to come from families with higher than average levels of advertising in that publication.

### III. Does Advertising Influence the Media?

#### A. Motivation and Empirical Framework

To motivate our tests for advertising bias, consider the mutual funds that appear on *Money* magazine's annual *Money* 100 list during our sample period. In an average year, 83.8 percent of families that spent more than \$1 million on advertising in *Money* over the prior 12 months are mentioned on the *Money* 100 list at least once. In contrast, only 7.2 percent of families that did not advertise in *Money* over the prior 12 months are mentioned. This difference partially reflects the fact that heavy advertisers tend to manage more mutual funds than non-advertisers. However, an individual fund from a heavy advertiser is more than twice as likely to be included on the *Money* 100 list as an individual fund from a non-advertiser (3.0 percent versus 1.3 percent). This difference is consistent with pro-advertiser bias, but obviously does not control for any of the mutual fund or mutual fund family characteristics that might lead publications to rank one mutual fund over another. In particular, one might worry that "high quality" mutual funds are both more likely to advertise and more likely to receive positive media mentions [Milgrom and Roberts, 1986]. To address this concern, we turn to multivariate tests for advertising bias.

Our general approach is to ask whether lagged publication-level advertising expenditures are correlated with the probability of receiving a media mention, controlling for all of the mutual fund and mutual fund family characteristics that publications might reasonably use to rank funds. Consider predicting positive mentions in a particular publication using the following specification:

$$Mention_{i,t} = \alpha + \gamma(Own-Publication\ Advertising_{i,t-1}) + \beta\mathbf{Z}_{i,t-1} + \delta_{k,t} + \varepsilon_{i,t}, \quad (1)$$

where  $Mention_{i,t}$  equals one if fund  $i$  receives a positive mention in the publication in month  $t$  and zero otherwise,  $Own-Publication\ Advertising_{i,t-1}$  measures lagged advertising expenditures in the publication by fund  $i$ 's family,  $\mathbf{Z}_{i,t-1}$  contains numerous control variables,  $\delta_{k,t}$  is an investment objective-by-month fixed effect, and  $\varepsilon_{i,t}$  is a fund-by-month disturbance term. To test whether advertising and content are related, we estimate equation (1) and test whether  $\hat{\gamma}$  is statistically different from zero. The identifying assumption required to give this test a causal interpretation is that advertising within a publication be uncorrelated with any unobserved fund characteristics that would cause its readers to want the publication to mention the advertiser's fund. For products whose quality is partially or totally subjective, the fact that advertising is endogenous would lead us to seriously question this assumption. However, in the context of mutual funds, where *ex post* product quality is objective and easily quantified, we believe the assumption may be reasonable.

From a financial perspective, mutual fund investors should seek to maximize risk-adjusted returns on an after-expense basis. Therefore, within each investment objective, publications should seek to identify those funds with the highest expected future returns and the lowest expenses. Since Carhart [1997] finds low fund expenses to be a good predictor of future returns, we control for fund  $i$ 's lagged expense ratio. As other potential predictors of future returns, we include fund  $i$ 's log return over the prior twelve months, its lagged log return squared, and its Morningstar rating at the end of the prior calendar year.<sup>8</sup>

In addition to predictors of future returns, publications should also focus on the form of distribution

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<sup>8</sup>Mutual funds with multiple share classes can earn a different Morningstar rating for each share class. Therefore, to control for Morningstar rating we begin with five dummy variables that indicate whether one or more of fund  $i$ 's share classes earned a Morningstar rating of one, two, three, four, or five stars. We then scale each dummy variable by the fraction of dollars under management receiving each rating.

that most appeals to their readers. For example, to the extent that personal finance publications appeal to investors who prefer to purchase direct-marketed funds rather than employ a broker and pay a load, these publications should be more likely to recommend no-load funds. Since families of no-load funds should then be more likely to advertise in the personal finance publications,  $\mathbf{Z}$  includes a dummy variable indicating whether fund  $i$  charges a load; it also includes the level of fund  $i$ 's 12b-1 (marketing and distribution) fee. As additional measures of potential investor interest in fund  $i$ , we include log dollars under management within both fund  $i$  and the fund family to which it belongs, log net inflows into fund  $i$  over the prior twelve months, and the number of mentions in each of the other publications in our sample over the prior twelve months. Since mutual fund families that advertise may differ systematically from those that do not—either because advertisers have systematically higher expected future returns or because investors are more likely to value reviews of funds from families they learned about through advertising— $\mathbf{Z}$  also includes total print and non-print advertising expenditures by fund  $i$ 's family over the prior 12 months.

## B. Testing for Advertising Bias

In Table III, we estimate equation (1) separately for each type of media mention. For example, the dependent variable in the column titled “*SmartMoney* Positive” equals one if we coded fund  $i$  as receiving a positive mention in *SmartMoney* in month  $t$  and zero otherwise. Estimation is via logit and includes a separate fixed effect for each investment objective-by-month combination. The number of observations in this column reflects the number of mutual funds each month with the same investment objectives as those receiving a positive mention in *SmartMoney*.<sup>9</sup> The explanatory variable of interest is advertising expenditures by fund  $i$ 's family within *SmartMoney* over the prior 12 months, which we refer to as “own-publication advertising” expenditures. Standard errors are reported below the coefficients and cluster on mutual fund family [Moulton 1990].

Looking across the columns in Table III, the coefficients on own-publication advertising are positive and

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<sup>9</sup>Because funds with investment objectives that are not mentioned in the publication in month  $t$  are excluded from the estimation, our tests for advertising bias effectively condition on the investment objectives that publications choose to focus on each issue and ask, within these investment objectives, whether advertising expenditures influence which funds are mentioned. Since we observe advertising expenditures at the mutual fund family level and many families offer funds that span the set of investment objectives, we have insufficient statistical power to test whether the choice of investment objectives favors advertisers.



statistically significant at the 1-percent level for positive mentions in all three personal finance publications.<sup>10</sup> The coefficients are also economically significant. For *Money*, the marginal effect of \$1 million in family advertising expenditure is to increase the probability a of positive mention for each of its funds by 0.2% compared with a predicted probability (at sample means) of 0.5%. For *Kiplinger's*, those probabilities are 0.1% and 0.08%, respectively, and for *SmartMoney* they are 0.2% and 0.2%. Put differently, variation in own-publication advertising has more explanatory power for positive mentions in each of the personal finance publications than variation in fund expenses, and about the same explanatory power as past returns.

As another way of gauging the economic significance of our findings, we use the coefficients reported in Table III to predict the set of funds we would expect each publications to mention, first including the influence of own-publication advertising and then excluding it. For example, if *SmartMoney* mentioned 10 aggressive growth funds favorably in month  $t$ , we treat the 10 aggressive growth funds with the highest predicted values based on our estimates of equation (1) as predicted mentions that include the influence of own-publication advertising. We then repeat this exercise, setting the coefficient on own-publication advertising equal to zero. For the *Money 100* list, the overlap in the two sets of predicted mentions is 91.5%, suggesting that 8-9 funds were replaced on the list by advertisers' funds that had otherwise just missed the cutoff. For positive mentions in *Kiplinger's* and *SmartMoney*, the overlap is 77.0% and 77.9%, respectively.

In contrast to the results for the personal finance publications, the coefficient on own-publication advertising is a precisely estimated zero for the *Wall Street Journal* and negative, but statistically indistinguishable from zero, for the *New York Times*. Since the three personal finance publications receive between a much larger share of their advertising revenues from mutual funds than the newspapers, our findings are consistent with advertising expenditures influencing fund rankings in those publications relatively more dependent on mutual fund advertising. Of course, for *Wall Street Journal*, the lack of a statistically significant correlation between advertising and mentions could also reflect that mentions in the "Fund Track" column are a mixture of positive and negative, and driven primarily by news.

With respect to negative mentions, advertising bias predicts that  $\gamma$  will be negative, making publications less likely to include advertisers' funds in negative mentions. Here, evidence of bias is weaker. For

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<sup>10</sup>The correlations between advertising and content reported in Tables III and IV are robust to the inclusion of additional fund characteristics, such as fund age, manager turnover, and the standard deviation of fund returns over the prior 36 months.

negative mentions in *Kiplinger's*, the coefficient on own-publication advertising is negative but statistically indistinguishable from zero; for negative mentions in *SmartMoney*, the coefficient is also negative and statistically insignificant, but quite close to zero. Nevertheless, for both publications, we can reject the hypothesis that the marginal effects of own-publication advertising are equal for positive and negative mentions (at the 5-percent level). This fact casts doubt on one alternative explanation for our findings. Namely, if past advertising in a publication directly increases reader demand for information on advertiser's funds, we would expect advertising to predict more positive mentions and more negative mentions. However in Table III, we find evidence that advertising expenditures increase positive mentions more than negative mentions.

Before exploring the robustness of our main results, several of the coefficients on the control variables deserve mention. First, counter to our expectations, few of the coefficients on the total print and non-print advertising expenditure variables are statistically significant. The fact that the coefficients on total print advertising expenditures are positive for both types of negative mentions, suggests that *Kiplinger's* and *SmartMoney* may be responding to subscriber demand for negative reviews on funds they've seen advertised in general (rather than specifically in *Kiplinger's* or *SmartMoney*). Second, the probability of receiving both positive and negative mentions is increasing in the size of fund  $i$  and decreasing in the size of its family. Third, the probability of receiving both positive and negative mentions is increasing in the level of the fund  $i$ 's expense ratio for every publication except *Consumer Reports*. Fourth, funds experiencing inflows, good returns, and (though not reported) favorable Morningstar ratings over the prior 12 months are more likely to receive positive mentions, while outflows and low returns and ratings are associated with negative mentions. Fifth, with the exception of the *New York Times*, the probability of receiving a positive mention is lower for load funds than for no-load funds.<sup>11</sup>

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<sup>11</sup>As discussed above, load fund families are less likely to advertise in publications catering to do-it-yourself investors, and these publications are less likely to mention their funds. Including a load dummy variables controls for this effect, but as an additional robustness check, we restrict our sample to no-load funds and re-estimate equation (1) for mentions in the three personal finance publications. For positive mentions, the estimated coefficients on own-publication advertising are uniformly larger than those reported in Table III, and statistically significant at the 1-percent level. For negative mentions, both coefficients remain negative but statistically indistinguishable from zero.

## C. Robustness

In this section, we subject our tests for advertising bias to four robustness checks. Our primary concern is that own-publication advertising may proxy for unobserved fund quality or demand for information from the publication’s readers. First, in unreported tests, we find that controlling for future fund returns (measured from month  $t$  through month  $t + 11$ ) has relatively little impact on the magnitude or statistical significance of the coefficients on own-publication advertising for any of the publications in our sample. These findings are consistent with our finding in Section IV that recommendations are not informative about future returns, and suggest that own-publication advertising does not proxy for unobserved fund quality.<sup>12</sup>

Second, we re-consider the possibility that past advertising in a specific publication reflects (or increases) the demand of that publications’ readers for articles on advertisers’ funds.<sup>13</sup> To address this concern, we re-estimate the specifications in Table III, instrumenting past own-publication advertising in personal finance publication  $k$  with past advertising in the two other personal finance publications. Advertising in these other publications is a valid instrument if it is related to a families’ general propensity to advertise in personal finance publications but not to any unobservable preferences of publication- $k$  readers for its funds.<sup>14</sup>

We follow Angrist and Krueger’s [2001] advice and switch to a linear probability specification to avoid the difficulties associated with estimating limited-dependent-variable IV models. In Panel A of Table IV, we report the coefficients on own-publication advertising for both OLS and IV. The OLS results are qualitatively similar to those in Table III, with positive and significant coefficients for the positive mentions and negative and insignificant coefficients for the negative mentions. The IV results are similar to those obtained using OLS, which suggests that own-publication advertising does not proxy for publication-specific interest in advertisers’ funds.

Third, to control for observable variables in a less parametric manner, we re-estimate equation (1)

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<sup>12</sup>We thank an anonymous referee for suggesting this robustness check.

<sup>13</sup>George and Waldfogel [2003] present evidence that newspaper content responds to the demographic mix of consumers within their market. Similarly, Hamilton and Zeckhauser [2004, p. 5] find that media coverage of CEOs is increasing in the number of common shares outstanding and conclude that this reflects “the desire of reporters to write about firms with a wide audience of investors.” These papers suggest that tests for advertising bias need to consider the possibility that both advertising and content respond to underlying subscriber demands.

<sup>14</sup>This identifying assumption is subject to a criticism analogous to the critique of estimating demand instrumenting with prices from other markets [Bresnahan 1997]. If certain families persistently appeal to readers of *all* personal finance publications in a way that is uncorrelated with observables (including the general level of print advertising), then other-publication advertising is not a valid instrument.

on matched samples. For each publication, we match funds from families that advertised over the prior 12 months to non-advertiser funds with approximately the same propensity to be mentioned (based on predicted values from equation (1) when we set the coefficient on own-publication advertising to zero). In unreported tests, the coefficients on own-publication advertising are economically and statistically significant for positive mentions in all three personal finance publications.

Fourth, we attempt to control for any long-run affinity of a publication’s readers for a particular fund family by analyzing changes in advertiser status. We classify a family as starting to advertise in publication  $k$  in month  $t$  if the family has positive advertising expenditures in the publication in month  $t$  but not in months  $t - 5$  through  $t - 1$ . Similarly, we classify a family as stopping advertising in publication  $k$  in month  $t$  if the family has positive advertising expenditures in month  $t - 5$  but not in months  $t - 4$  to  $t$ . In unreported tests, we find that families that start advertising in *Money* in the six months prior to the *Money 100* list are more likely to appear on the list than families that do not start advertising.<sup>15</sup> Similarly, we find evidence that families that start advertising in *SmartMoney* in month  $t$  are more likely to receive positive mentions in *SmartMoney* over the following six months relative to families that do not advertise. Finally, we find weak evidence (at the 20-percent level in a two-sided test) that families that stop advertising in *Kiplinger’s* are less likely to receive positive mentions over the following six months.

Overall, our findings are consistent with own-publication advertising expenditures influencing fund rankings at the three personal finance publications in our sample. Without purely exogenous variation in advertising, we cannot entirely eliminate the possibility that the correlations we observe are being driven by the endogeneity of advertising. However, based on the robustness of the positive correlation between own-publication advertising and content for the personal finance publications—and the absence of a positive correlation for the newspapers, which receive considerably less advertising revenues from mutual funds and otherwise serve as a falsification test—we believe that the most likely explanation is the causal one.

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<sup>15</sup>As an alternative robustness test suggested by an anonymous referee, we examined whether *Money* receives a disproportionate share of mutual fund print advertising in the months preceeding the publication of the *Money 100*, as one might expect if advertisers were attempting to influence its composition. We find that *Money* receives 8.9% of advertising in the three months prior to the publication of the list, 11.7% in the publication month, and 7.7% in the three months after. The difference between the pre and post-list window is statistically significant and robust to window lengths of 1, 2, and 4 months, or to the inclusion of month fixed effects (that we can identify since the month the list is published varies from year to year).

## IV. Does Advertising’s Influence on Content Matter?

### A. Do Investors Respond to Media Mentions?

Studies on the determinants of flows into U.S. mutual funds typically focus on the relation between measures of past performance and other characteristics and future inflows (see, for example, Ippolito [1992] and Chevalier and Ellison [1997]). However, as Sirri and Tufano [1998] argue, it is costly for investors to gather and process information on the universe of available funds, suggesting that the media may play an important role in disseminating this information. Consistent with investors having lower search costs for mutual funds they have been exposed to through the media, Sirri and Tufano find that mutual funds receiving more media attention receive correspondingly higher inflows. Similarly, Jain and Wu [2000], Cronqvist [2004], and Gallagher, Kaniel, and Starks [2005] provide evidence that mutual fund flows respond to fund-level and family-level advertising, while Del Guercio and Tkac [2005] provide evidence that flows respond to changes in Morningstar ratings. Collectively, these papers suggest that investors rely on both advertising and the media when deciding which mutual funds to buy. Since media mentions are only valuable to mutual fund families to the extent that they influence investor behavior, in this section, we ask whether the media mentions we study have a significant impact on mutual fund inflows.

In Table V, we estimate the impact of media mentions on fund inflows using Fama-MacBeth [1973] style regressions. Each month between January 1997 and December 2001, we estimate a separate cross-sectional regression. We then report the time-series means and standard errors associated with these cross-sectional estimates. Our dependent variable measures net inflows into fund  $i$  between months  $t$  and  $t+11$ . The number of media mentions that fund  $i$  receives in month  $t-1$  is measured separately for positive and negative mentions in each publication. To test whether family-level advertising expenditures are systematically related to fund-level inflows, we include advertising expenditures on print and non-print media over the prior twelve months, normalized by the average dollars under management within the fund family over this period. In addition to the fund characteristics used to predict media mentions in Table III, we also control for the raw return of fund  $i$  between months  $t$  and  $t+11$ . Finally, we include fixed effects for each investment objective within each monthly cross-sectional regression.

Since the control variables are highly persistent, we estimate their standard errors from the time-series of estimated coefficients via Newey-West [1987] and allow 12 monthly lags. Since media mentions within a publication are not persistent across months, and since for many of the media mentions we are unable to estimate a coefficient for each of the 60 months, we estimate the standard errors for the media mention variables via White [1980]. (When we estimate standard errors for mentions in the *Wall Street Journal* and positive mentions in *SmartMoney* via Newey-West with 12 lags, they are quantitatively quite similar to those reported in Table V.)

Looking across the columns in Table V, we see that media mentions are associated with future flows in the direction we would expect. The magnitudes are largest in our baseline specification in column (2) and decline slightly when we control for fund  $i$ 's lagged Morningstar rating (column (3)), fund  $i$ 's prior media mentions (column (4)), and the prior media mentions of other funds in fund  $i$ 's family (column (5)). Overall, positive mentions in personal finance publications and *Consumer Reports* are associated with an economically significant 7-8 percent increase in fund size over the next 12 months, while a positive mention in the *New York Times* is associated with a 15 percent increase. Negative media mentions in both *Kiplinger's* and *SmartMoney* yield estimated future outflows of 1–2 percent, but neither coefficient is statistically significant.

The estimated coefficients on the control variables are largely consistent with those in the existing literature. For example, the positive and statistically significant coefficients on past fund returns and past fund returns squared capture the convex relation between inflows and performance first documented by Ippolito [1992]. Interestingly, while fund-level inflows are increasing in non-print advertising expenditures, they do not appear to be increasing in print advertising expenditures; in none of the columns is the coefficient on the print advertising ratio statistically different from zero. If we interpret the correlations in Tables III and V causally, this suggests that the returns to print advertising by mutual funds may be coming largely via biased content.

## B. Do Media Mentions Contain Information About Future Returns?

In this section, we compare the future returns of funds receiving media mentions to those of other funds. The question of interest is whether investors can benefit from buying funds that receive positive mentions

and selling (or avoiding) funds that receive negative mentions.

As such, we contribute to a large literature that seeks to predict future mutual fund performance using lagged fund and fund manager characteristics, and measures of past fund performance. Beginning with Jensen [1968], this literature generally finds it difficult to identify funds that will persistently outperform others.<sup>16</sup> That said, recent academic research provides some evidence of predictability, suggesting that full-time industry experts should be able to identify better than average future performers. For example, funds with the highest expenses persistently underperform funds with low expenses [Elton, Gruber, Das, Hlavka, 1993]. Chevalier and Ellison [1999] find that some fund manager characteristics are associated with consistently higher risk-adjusted returns. Carhart [1997] finds some persistence of past performance, especially of bad past performance, and Kacperczyk, Sialm, and Zheng [2005] find that their ability to predict underperformance increases if they focus on holdings-adjusted returns (i.e., the difference between actual returns and the returns of the fund's initial holdings). Therefore, it is an open question whether the media mentions in our sample help investors to choose funds with above average future returns.

We begin by calculating the relative return of fund  $i$  in month  $t$  as its monthly return minus the equal-weighted average monthly return of funds with the same investment objective. This measure is positive when fund  $i$  outperforms the average fund in its asset class and negative when it does not. For each type of media mention, we then regress monthly relative returns on a dummy variable that indicates whether fund  $i$  received the specified media mention at least once over the past twelve months. Specifically, for the personal finance publications and *Consumer Reports*, which are typically available shortly before the month listed on their cover, we assume that investors purchase funds on the first trading day of the month the issue is dated (for example, June 1, 1998 in the case of the first *Money 100* list) and sell them on the last trading day of the month twelve months later (May 31, 1999). For mentions in the *Wall Street Journal* and *New York Times*, which occur throughout the month, we assume that investors purchase funds on the first trading day of the month after the mention and hold them for twelve months.

In Table VI, we report estimated coefficients for the media mention holding period dummy variables

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<sup>16</sup>For example, Jain and Wu [2000] find little evidence that funds which advertise in *Barron's* or *Money* earn higher future returns than non-advertisers, and Blake and Morey [2000] find little evidence that Morningstar ratings help predict future fund performance.

using relative returns from January 1997 through December 2002. Panel A focuses on the full sample of U.S. mutual funds; the top of Panel B focuses on the sample of domestic equity funds.<sup>17</sup> Standard errors cluster on investment objective and month. Looking across publications, we find that funds mentioned in the *New York Times* outperform their peers by 22–33 basis points per month (significant at the 5-percent level), while funds receiving negative mentions in *SmartMoney* underperform their peers by 21–36 basis points per month (significant at the 1-percent level). In other words, based on relative returns, it appears that investors would have benefited from using positive mentions in the *New York Times* and negative mentions in *SmartMoney* to choose amongst funds. In contrast, for *Consumer Reports* (which does not accept advertising), the negative and significant coefficient of -13 basis points per month in Panel B suggests that investors would have earned higher returns picking domestic equity funds at random. For other publications, the coefficients on the media mention holding period dummy variables are statistically indistinguishable from zero, implying that the funds they mention are neither better nor worse than the average fund within each asset class.

Since relative returns ignore possible differences in risk within each investment objective, in the bottom half of Panel B, we examine the risk-adjusted returns of mentioned funds using portfolio-level returns. Specifically we adjust returns for correlation with the market (in the “CAPM” specification), as well as for the the size and book-to-market portfolios of Fama and French [1993] and the momentum portfolio of Carhart [1997] (in the “Four Factor” specification). Using risk adjusted returns does not affect our conclusion for negative mentions in *Smart Money*, but the coefficients for the *New York Times* and *Consumer Reports* lose statistical significance.

To determine whether advertising bias harms investors, Table VII extends the analysis of fund-level relative returns for positive mentions in the personal finance publications to three sets of predicted mentions. The first two sets revisit the comparison with and without advertising bias from Section III.B. Comparing the coefficients in rows (a) and (b), we find that future returns are quite similar across the two sets of predicted mentions. This suggests that the future returns of mentioned advertisers’ funds are similar to those of funds we predict would have been mentioned in the absence of any advertising bias. Therefore, it does not appear

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<sup>17</sup>Recall that we exclude money market funds and mutual funds for which CRSP does not report a ticker for at least one share class during our sample period. The number of observations is higher than in previous tables because we only require that fund  $i$  report a return and investment objective in month  $t$ .



that advertising bias is responsible for the inability of positive mentions to identify funds with above average future returns.

To test whether investors would benefit from personal finance publications putting more weight on fund expenses and less weight on other fund characteristics, we consider a third set of predicted mentions that ranks funds within each investment objective based solely on their expense ratios. In row (c), we see that these predicted mentions outperformed their equal-weighted peers by a statistically significant margin for *Kiplinger's* and *SmartMoney*. For *Money*, the coefficient is negative but close to zero (reflecting the fact that picking 100 funds in the same month based on expense ratios leads to higher expense ratios than if you pick 8-9 funds per month). Overall, the failure of the published recommendations to predict returns appears related not to advertising bias, but to how the publications choose to weight other fund characteristics when ranking funds.

## V. The (Behavioral) Economics of Media Bias

To summarize our results, investors respond to mutual fund recommendations despite the fact that these recommendations generally fail to predict future returns. Moreover, this failure is not due to advertising bias, but rather to recommendations overweighting factors such as past returns and Morningstar ratings, and underweighting expenses. An advantage of this weighting scheme, however, is that it gives publications a wider selection of funds to recommend, which allows them to more easily accommodate any pro-advertiser bias. The personal finance publications, who are more dependent on mutual fund advertising (and arguably have less reputation at stake), exhibit such a bias, while the national newspapers do not.

The greatest puzzle in our results is that investors react to recommendations that are largely uninformative about future returns. There are at least three possible explanations, but none is fully satisfying. First, if there are diseconomies of scale in the production of returns, the inflows that follow a mention could drive down relative fund returns [Berk and Green, 2004]. However, given recent estimates of scale diseconomies in portfolio management [Chen, Hong, Huang, and Kubik, 2004], the effect of a 7-15 percent inflow on returns should be quite small. Second, investors may receive entertainment from purchasing recommended funds along with other magazine readers. Third, some investors may have out-of-equilibrium beliefs about the

quality of the recommendations, and you might expect these to be the investors who self-select into reading and relying on the personal finance publications.

Regardless of its source, investors' willingness to follow recommendations with little apparent regard for their quality provides publications with the ability to bestow a favor on someone. When deciding whether to bestow this favor on a past advertiser, publications must tradeoff the benefits (rewarding past advertisers and thus encouraging future advertising) with the costs.<sup>18</sup> The most obvious cost of recommending the otherwise less attractive funds of an advertiser is its potential effect on readers' perceptions about the magazine's quality. But there are reasons to suspect that reader learning will be slow. To begin, investors who have self-selected into reading the personal finance publications are probably the least skeptical about their value. In addition, returns are noisy. In Table VI, we analyze seven years of data and about 500 recommendations each for *Money* and *Kiplinger's*, and our estimates of the recommended funds' annualized relative returns have standard errors of about 1 percent. A typical investor following the performance of only the recommended funds they invested in would require much more data before drawing any conclusions about the quality of the recommendations.

Presumably, the more important costs of advertising bias are the ethical costs. Journalists (like academics) typically earn less money than they could in alternative careers, accepting lower salaries because they receive utility from providing a public good. For many journalists, consciously biasing coverage in favor of advertisers would defeat the purpose of being a journalist in the first place. Yet there is probably heterogeneity in this distaste for bias, and one might expect journalists with less distaste to sort into publications where the economic returns to biasing were higher. This might explain why we find such different results for the national newspapers and the personal finance publications.

Of course, these issues raise the question of the extent to which any bias we document is conscious.<sup>19</sup>

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<sup>18</sup>Examining the relation between future advertising and past mentions, we find evidence that families that receive positive (negative) mentions in a publication tend to increase (decrease) future advertising in that publication. While consistent with advertising bias, this tendency may simply reflect families wanting to reinforce one positive message with another (and not reinforce negative messages). However, we also find evidence that these effects are strongest for families that already advertise in the publication, suggesting that mentioning past advertisers is likely to generate more incremental advertising revenues than mentioning non-advertisers.

<sup>19</sup>The line between conscious and unconscious bias may be blurry. For example, a former reporter from one of the personal finance publications in our sample describes a common fund selection procedure as first running database screens and then selecting which of several eligible funds to include based, in part, on the existence of high-level contacts available for quotation. The reporter noted many large advertisers were fairly proactive about meeting reporters when visiting their offices for business reasons. In other words, own-publication advertising may proxy for unobserved publication-specific efforts of mutual fund families to establish relationships with their reporters. To the extent that these relationships influence media mentions, they

We attempted to determine the mechanism at work by examining how advertiser bias varies by article type (articles making general fund recommendations, articles about an asset class or investment theme, articles about a single fund or family). We hypothesized that bias in the latter type of article could be imposed by an editor or publisher suggesting stories, while bias in the former two types would be more difficult to impose without reporters' knowledge. Results were mixed, however. For *Kiplinger's* we found the strongest evidence of bias in single-family articles, but we also found evidence of bias in within-investment-objective articles, while for *SmartMoney* we found evidence of bias only in general and within-asset-class articles. We also tested whether advertising bias increased during the 2001–2002 advertising recession (since mutual fund advertising declined by 67 percent from 2000 to 2002), perhaps because reporters facing layoffs were more willing to support efforts to sell advertising. Again the evidence was mixed, we found the strongest evidence of bias for *SmartMoney* in 2002, but that evidence of bias in *Kiplinger's* and the *Money 100* list peaked during the 1999–2000 boom.

Where we are certain the “bias” is conscious is in the personal finance publications' emphasis of past returns over expenses. As one former mutual fund reporter has written: “Mutual fund reporters lead a secret investing life. By day we write ‘Six Funds to Buy NOW!’ We seem delighted in dangerous sectors like technology. We appear fascinated with one-week returns. By night, however, we invest in sensible index funds.”<sup>20</sup> The alternative, however, of focusing on low expense funds might make for boring journalism since the set of low expense funds is relatively stable. It might also depress the returns to advertising, since it would make investors less responsive to ads for (and media mentions about) high-expense ratio funds. While we have treated the personal finance publications' decisions about how to weight fund characteristics as a potential valid “school of thought,” it might also constitute a form of advertising bias. If we counted it as such, then our conclusion about advertising bias not harming readers' returns would change.

## VI. Conclusion

We find that the personal finance publications in our sample are more likely to recommend the funds of their advertisers, even after controlling for the observable fund characteristics that their readers might

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clearly generate a pro-advertiser bias. However, whether this bias is conscious or unconscious is unclear.

<sup>20</sup>Anonymous, “Confessions of a Former Mutual Funds Reporter,” *Fortune*, April 1999.

value. Without exogenous variation in advertising, we cannot completely rule out the possibility that the positive correlations we observe between advertising and content are driven by the endogeneity of advertising. However, based on the robustness of these correlations for all three personal finance publications—and the absence of positive correlations for the two national newspapers, which receive much smaller fractions of their advertising revenues from mutual funds—we believe that the most likely explanation is the causal one. While positive mentions in these publications significantly increase fund inflows, they do not successfully predict returns. However, future returns are similar for the funds we predict would have been mentioned in the absence of bias, suggesting that the cost of advertising bias to readers is small.

Despite the fact that biased mutual fund recommendations may not harm the readers of personal finance publications, our findings raise questions about the reliability of content in advertiser-supported media more generally. While ideological media bias has received more popular and academic attention, the impact of ideological bias is likely to be mitigated by the presence of outlets on both sides of the spectrum (Dewatripont and Tirole [1999]; Mullainathan and Shleifer [2005]). In contrast, pro-advertiser bias is unlikely to be offset by anti-advertiser bias. Unfortunately, outside of our carefully chosen setting, many of the most important questions about advertising bias are extremely difficult to answer.

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TABLE I  
SUMMARY OF HAND-COLLECTED MEDIA MENTION DATA, 1996–2002

Publication	Article Type	Content/Sample Title	Nature	Frequency	Number of Articles	Number of Fund Mentions
<i>Wall Street Journal</i>	“Fund Track” column	Industry news and issues	News	Daily	1,096	3,527
<i>New York Times</i>	“Investing With” column	Profile of funds and managers	Positive	Weekly	201	201
<i>Money</i>	“Money 100 List”	Recommended funds	Positive	Annual	5	500
<i>Kiplinger’s</i>	All articles mentioning funds			Varies	144	783
	General recommendations	“Best Funds to Buy Now”	Positive		31	295
		“Hall of Shame”	Negative		11	63
	Within-asset-class articles	“Six Ways to Own the World”	Positive		49	300
		“The Wild Bunch”	Negative		3	12
	Single fund/family articles	“Magellan’s Driven Boss”	Positive		38	69
		“Is It Time to Leave Magellan?”	Negative		12	36
<i>SmartMoney</i>	All articles mentioning funds			Varies	686	2,417
	General recommendations	“Retire Ten Years Early”	Positive		232	1,047
		“The Underachievers Club”	Negative		65	319
	Within-asset-class articles	“Four Great Energy Funds”	Positive		116	453
		“It’s Not Easy Being Green”	Negative		46	188
	Single fund/family articles	“How Janus Got It’s Groove Back”	Positive		171	279
		“What is Janus Thinking?”	Negative		56	115
<i>Consumer Reports</i>	Mutual fund review issue	Recommended funds	Positive	Annual	11	812

Media mention data were hand collected from the *Wall Street Journal*, *New York Times*, *Money*, *Kiplinger’s*, *SmartMoney*, and *Consumer Reports*. Data from the *Wall Street Journal*, *New York Times*, *Money*, and *Consumer Reports* come from specific, recurring articles; data from *Kiplinger’s* and *SmartMoney* come from the universe of articles that focus on mutual funds. In general, data cover the period January 1996 through December 2002, although data for *Money* and *Consumer Reports* cover shorter intervals because their mutual fund articles appear less frequently (as reported in Table II). The number of fund mentions only counts the first mention of each mutual fund within each article. Eight of the *Kiplinger’s* mentions and 16 of the *SmartMoney* mentions could not be classified as clearly positive or negative.

TABLE II  
SUMMARY STATISTICS FOR MUTUAL FUNDS RECEIVING MEDIA MENTIONS, 1996–2002

Sample: Nature of Mention:	All Funds		WSJ News	NYT Positive	Money Positive	Kiplinger's		SmartMoney		Consumer Reports	
						Positive	Negative	Positive	Negative	Positive	Positive
Fund-Level Media Mentions With Ticker in CRSP			3,527	201	500	664	111	1,779	622	812	
With Ticker and Lagged Characteristics	319,006		2,985	185	497	618	106	1,694	601	738	
Date First Mention Collected			2,634	170	474	520	106	1,527	542	707	
Date Last Mention Collected			Jan-96	Feb-96	May-98	Jan-96	Jan-96	Jan-96	Jan-96	Apr-97	
			Dec-02	Nov-02	Jul-02	Dec-02	Nov-02	Nov-02	Nov-02	Dec-02	
Fund-level characteristics											
Ln Fund TNA (\$ millions)	4.90		6.89	5.87	7.48	6.77	6.15	6.87	6.59	6.76	
Ln Family TNA (\$ millions)	8.98		10.03	8.67	9.67	9.22	8.95	9.66	9.70	9.72	
Expense Ratio (%)	1.29		1.27	1.35	1.07	1.07	1.84	1.20	1.50	0.91	
12b-1 Fee (%)	0.21		0.17	0.18	0.11	0.09	0.12	0.15	0.19	0.02	
Load Fund Dummy	0.55		0.45	0.49	0.28	0.23	0.38	0.41	0.54	0.19	
Ln Net Flows Over Prior 12 Months (%)	11.87		24.05	36.01	21.38	43.45	-14.35	41.29	3.77	22.10	
Relative Return Over Prior 12 Months (%)	-0.21		12.15	15.30	1.68	8.63	-16.84	17.41	-1.33	6.52	
Family Advertising Expenditures Over Prior 12 Months Divided by Average Family TNA											
Print Advertising Ratio (basis points)	0.11		0.15	0.10	0.09	0.10	0.16	0.18	0.09	0.11	
Non-Print Advertising Ratio (basis points)	0.01		0.02	0.02	0.02	0.01	0.02	0.02	0.02	0.02	
Share of Print Advertising by Publication (%)											
<i>Wall Street Journal</i>	17.47		17.87	14.79	10.96	12.72	20.35	17.60	19.89	20.80	
<i>New York Times</i>	4.28		3.95	4.46	3.17	3.08	2.11	3.72	4.89	4.35	
<i>Money</i>	5.30		8.62	7.02	9.44	8.90	8.70	9.09	7.56	9.28	
<i>Kiplinger's</i>	3.06		4.47	3.11	4.67	5.96	5.29	4.90	3.68	5.52	
<i>Smart Money</i>	2.09		3.49	2.16	3.76	3.52	5.08	4.12	2.95	3.91	
Share of Funds by Broad Asset Class (%)											
General Domestic Equity	40.99		62.72	68.82	73.21	62.12	73.58	59.46	71.22	71.29	
Specialized Domestic Equity	17.79		17.92	12.35	8.44	11.73	5.66	18.14	15.13	5.80	
International/Global Equity	16.00		12.91	16.47	18.35	11.73	9.43	13.88	10.33	13.72	
Bonds	25.22		6.45	2.35	0.00	14.42	11.32	8.51	3.32	9.19	

This table compares the characteristics of U.S. mutual funds that receive media mentions to a sample of mutual funds for which the CRSP Mutual Fund Database reports all the lagged characteristics used in our analysis. We exclude money market funds and funds for which CRSP does not report a ticker for at least one share class. The unit of observation is fund  $i$  in month  $t$ . Mutual fund characteristics are aggregated across share classes. Because magazines are typically available in the month prior to the month stated on their cover, we code mentions in the month  $t+1$  issue of *Money*, *Kiplinger's*, *SmartMoney*, or *Consumer Reports* as occurring in month  $t$ . (For example, we code the June 1998 issue of *Money* as occurring in May 1998.) We code mentions in the *Wall Street Journal* or *New York Times* in month  $t$ , as occurring in month  $t$ . For mentions in month  $t$ , we report fund characteristics as of month  $t-1$ . Because we do not observe advertising expenditures in 1995, the summary statistics for the lagged advertising ratios are reported for January 1997 through December 2002. The share of print advertising by publication is reported for the subset of funds belonging to families with positive advertising expenditures in one or more publication.



TABLE III  
LOGIT REGRESSIONS PREDICTING MEDIA MENTIONS, 1997-2002

Media mention in month $t$ : Nature of mention:	WSJ News	NYT Positive	Money		Kiplinger's		SmartMoney		Consumer Reports Positive
			Positive	Negative	Positive	Negative	Positive	Negative	
Family-Level Advertising Expenditures in Millions of Dollars ( $t-12$ to $t-1$ )									
Own-Publication	0.004 (0.035)	-0.361 (0.281)	0.294*** (0.088)	0.819*** (0.249)	-0.220 (0.663)	0.730*** (0.136)	-0.015 (0.256)		
Total Print	0.017* (0.009)	0.017 (0.033)	-0.044** (0.019)	-0.004 (0.013)	0.070*** (0.027)	-0.014 (0.010)	0.025** (0.012)	0.026* (0.014)	
Total Non-Print	0.024 (0.015)	0.007 (0.027)	-0.045** (0.022)	-0.019* (0.011)	-0.045 (0.042)	-0.015 (0.010)	0.029** (0.013)	0.039** (0.018)	
Ln Fund TNA ( $t-1$ )	0.551*** (0.036)	0.198*** (0.063)	1.041*** (0.093)	0.598*** (0.080)	0.645*** (0.126)	0.541*** (0.038)	0.504*** (0.053)	0.375*** (0.052)	
Ln Family TNA ( $t-1$ )	-0.076 (0.051)	-0.138*** (0.051)	-0.313*** (0.061)	-0.283*** (0.057)	-0.174** (0.078)	-0.147*** (0.043)	-0.082* (0.043)	-0.091 (0.067)	
Expense Ratio ( $t-1$ )	0.153*** (0.024)	0.170*** (0.049)	0.291*** (0.054)	0.172*** (0.038)	0.156*** (0.052)	0.146*** (0.031)	0.096*** (0.016)	-0.086 (0.243)	
12b-1 Fee ( $t-1$ )	-0.366 (0.257)	-0.485 (0.337)	-1.204** (0.521)	-0.950 (0.827)	-1.910** (0.934)	-0.230 (0.341)	-0.136 (0.253)	-6.177*** (1.037)	
Load Fund Dummy ( $t-1$ )	-0.357*** (0.124)	0.049 (0.173)	-0.872*** (0.246)	-0.981*** (0.300)	-0.486 (0.314)	-0.277* (0.144)	-0.020 (0.162)	-0.649*** (0.256)	
Ln Net Flows ( $t-12$ to $t-1$ )	0.101** (0.049)	0.209*** (0.087)	0.157* (0.089)	0.323*** (0.072)	-1.253*** (0.271)	0.293*** (0.048)	-0.363 (0.257)	0.451*** (0.088)	
Ln Returns ( $t-12$ to $t-1$ )	0.526*** (0.122)	5.434*** (1.026)	-0.365 (0.338)	2.374*** (0.573)	-6.778*** (2.384)	2.512*** (0.416)	-4.406*** (0.583)	1.773** (0.583)	
Ln Returns Squared ( $t-12$ to $t-1$ )	1.044*** (0.111)	-2.801*** (1.087)	-2.795*** (1.024)	0.187 (0.630)	-0.968 (0.915)	-0.331 (0.412)	-1.439** (0.669)	-1.893* (0.988)	
Include Prior Mentions in Other Publications ( $t-12$ to $t-1$ )?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Include Lagged Morningstar Ratings?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Objective-by-Month Fixed Effects	552	140	33	178	44	420	201	52	
Sample Size	197,350	71,515	13,645	74,880	20,814	154,052	79,139	19,261	

Each column reports estimated coefficients from a logit regression estimated for a particular media mention. We include a separate fixed effect for each investment objective each month. "Ln Returns" ( $t-1$  to  $t$ ) is defined as the natural logarithm of one plus the return of fund  $i$  between months  $t-1$  and  $t$ . "Ln Net Flows" ( $t-1$  to  $t$ ) is defined as Ln Fund TNA ( $t$ ) minus Ln Fund TNA ( $t-1$ ) minus Ln Return ( $t-1$  to  $t$ ). Morningstar ratings from December of the prior year are used to create five dummy variables (corresponding to ratings between one and five stars). Since Morningstar ratings are awarded at the share class level, these dummy variables are then multiplied by the fraction of fund  $i$ 's dollars under management that receive each rating. Standard errors cluster on mutual fund family and are reported in parentheses. Significance at the 10-percent, 5-percent, and 1-percent levels are denoted by \*, \*\*, and \*\*\*.

TABLE IV  
 PREDICTING MEDIA MENTIONS WITH A LINEAR PROBABILITY MODEL AND IV, 1997–2002

	Sample Size	Objective- by-Month FEs	Own Publication Advertising	Sample Size	Objective- by-Month FEs	Own Publication Advertising
	<i>Kiplinger's</i> (Positive)			<i>SmartMoney</i> (Positive)		
OLS	74,880	178	0.0102** (0.0045)	154,052	420	0.0131*** (0.0036)
IV	74,880	178	0.0082*** (0.0019)	154,052	420	0.0174*** (0.0035)
	<i>Kiplinger's</i> (Negative)			<i>SmartMoney</i> (Negative)		
OLS	20,814	44	-0.0014 (0.0053)	79,139	201	-0.0006 (0.0019)
IV	20,814	44	-0.0004 (0.0085)	79,139	201	-0.0005 (0.0028)
	<i>Money 100</i> (Positive)					
OLS	13,645	33	0.0172*** (0.0062)			
IV	13,645	33	0.0221*** (0.0058)			

In this table, we estimate a linear probability model using the same set of investment objective-by-month fixed effects and observations as in Table III. We then re-estimate the linear probability model instrumenting own-publication advertising expenditures in one personal finance publication with advertising expenditures in the other two personal finance publications (for example, instrumenting own-publication advertising expenditures in *Money* from  $t-12$  to  $t-1$  with advertising expenditures in *Kiplinger's* and *SmartMoney* over the same period). Only the coefficient on own-publication advertising is reported. Standard errors cluster on mutual fund family and are reported in parentheses. Significance at the 10-percent, 5-percent, and 1-percent levels are denoted by \*, \*\*, and \*\*\*.

TABLE V  
FAMA-MACBETH REGRESSIONS OF FUTURE NET INFLOWS ON MEDIA MENTIONS, 1997–2001

Dependent Variable:	Ln Net Flows ( $t$ to $t+11$ )					
	N	(1)	(2)	(3)	(4)	(5)
Ln Fund TNA ( $t-1$ )	60	−0.0537*** (0.0023)	−0.0548*** (0.0022)	−0.0573*** (0.0011)	−0.0593*** (0.0012)	−0.0595*** (0.0012)
Ln Family TNA ( $t-1$ )	60	0.0225*** (0.0006)	0.0226*** (0.0005)	0.0230*** (0.0005)	0.0234*** (0.0006)	0.0197*** (0.0010)
Ln Net Flows ( $t-12$ to $t-1$ )	60	0.1294*** (0.0141)	0.1286*** (0.0140)	0.1086*** (0.0153)	0.1076*** (0.0150)	0.1057*** (0.0149)
Ln Returns ( $t-12$ to $t-1$ )	60	0.6007*** (0.0559)	0.5967*** (0.0568)	0.5484*** (0.0555)	0.5453*** (0.0578)	0.5474*** (0.0566)
Ln Returns Squared ( $t-12$ to $t-1$ )	60	0.7558** (0.3579)	0.7470** (0.3556)	0.7136** (0.3294)	0.7137** (0.3302)	0.7221** (0.3343)
Ln Returns ( $t$ to $t+11$ )	60	0.6682*** (0.0619)	0.6687*** (0.0615)	0.6996*** (0.0522)	0.6977*** (0.0531)	0.6972*** (0.0540)
Expense Ratio ( $t-1$ )	60	−0.0185*** (0.0052)	−0.0189*** (0.0052)	−0.0107* (0.0063)	−0.0109* (0.0065)	−0.0119* (0.0066)
12b-1 Fee ( $t-1$ )	60	0.0814*** (0.0108)	0.0829*** (0.0111)	0.0828*** (0.0104)	0.0858*** (0.0112)	0.1005*** (0.0133)
Load Fund Dummy ( $t-1$ )	60	0.0060 (0.0061)	0.0076 (0.0061)	0.0176*** (0.0049)	0.0209*** (0.0052)	0.0274*** (0.0052)
Family Print Advertising Ratio ( $t-12$ to $t-1$ )	60	−0.0015 (0.0022)	−0.0016 (0.0022)	−0.0019 (0.0022)	−0.0014 (0.0021)	−0.0020 (0.0018)
Family Non-Print Advertising Ratio ( $t-12$ to $t-1$ )	60	0.0456*** (0.0140)	0.0452*** (0.0139)	0.0508*** (0.0148)	0.0438*** (0.0138)	0.0499** (0.0225)
Fund-Level Media Mention ( $t-1$ )						
<i>WSJ</i> “Fund Track” (News)	60		0.0458*** (0.0095)	0.0367*** (0.0096)	0.0274*** (0.0100)	0.0243*** (0.0099)
<i>NYT</i> “Investing With” (Positive)	54		0.1845*** (0.0332)	0.1507*** (0.0323)	0.1480*** (0.0322)	0.1504*** (0.0321)
<i>Money</i> 100 (Positive)	4		0.1138*** (0.0096)	0.0944*** (0.0083)	0.0849*** (0.0108)	0.0840*** (0.0086)
<i>Kiplinger’s</i> (Positive)	36		0.1214*** (0.0355)	0.1002*** (0.0346)	0.0832** (0.0361)	0.0718** (0.0361)
<i>Kiplinger’s</i> (Negative)	13		−0.0082 (0.0664)	−0.0063 (0.0711)	−0.0210 (0.0773)	−0.0107 (0.0800)
<i>SmartMoney</i> (Positive)	60		0.1055*** (0.0193)	0.0822*** (0.0183)	0.0719*** (0.0176)	0.0712*** (0.0172)
<i>SmartMoney</i> (Negative)	49		−0.0065 (0.0154)	−0.0108 (0.0150)	−0.0176 (0.0158)	−0.0212 (0.0164)
<i>Consumer Reports</i> (Positive)	6		0.1201*** (0.0147)	0.0961*** (0.0166)	0.0856*** (0.021)	0.0870*** (0.0197)
Include Investment Objective Fixed Effects?		Yes	Yes	Yes	Yes	Yes
Include Lagged Morningstar Ratings?		–	–	Yes	Yes	Yes
Include Prior Fund Mentions ( $t-12$ to $t-2$ )?		–	–	–	Yes	Yes
Include Family-Level Mentions ( $t-12$ to $t-1$ )?		–	–	–	–	Yes

Each month between January 1997 and December 2001 we estimate a separate cross-sectional regression. The dependent variable in our cross-sectional regressions is measured as the percentage change in the size of fund  $i$  between months  $t$  and  $t+11$ , minus the fund’s return between months  $t$  and  $t+11$ . All monthly regressions include investment objective fixed effects. Following Fama and MacBeth [1973], we report the time-series mean and time-series standard error for each estimated coefficient. Since many of the control variables are highly persistent, standard errors for the control variables are estimated via Newey and West [1987] with 12 lags. Standard errors for the media mention coefficients are estimated via White [1980]. (“N” indicates the number of cross-sectional regressions in which we were able to estimate the coefficient.) Prior mentions for a given publication are the sum of mentions for fund  $i$  in months  $t-12$  to  $t-1$ . Family-level mentions exclude mentions for fund  $i$ . Significance at the 10-percent, 5-percent, and 1-percent levels is denoted by \*, \*\*, and \*\*\*.

TABLE VI  
FUTURE RETURNS OF MENTIONED FUNDS RELATIVE TO THEIR PEERS, 1997-2002

Media mention: Nature of mention:	<i>WSJ</i> News	<i>NYT</i> Positive	<i>Money</i> Positive	<i>Kiplinger's</i> Positive	Negative	<i>SmartMoney</i> Positive	Negative	<i>Consumer Reports</i> Positive
<i>Panel A. Full Sample of Mutual Funds</i>								
<i>Fund-Level Analysis</i>								
Dependent Variable:	<i>Monthly return of fund i minus average return of funds with the same investment objective</i>							
Mentioned between months $t-12$ and $t-1$ ?	-0.069 (0.091)	0.216** (0.107)	0.119 (0.095)	0.093 (0.061)	-0.384 (0.275)	0.017 (0.076)	-0.211*** (0.078)	-0.055 (0.052)
Sample Size	330,674	330,674	261,202	330,674	330,674	330,674	330,674	314,978
<i>Panel B. Domestic Equity Mutual Funds Only</i>								
<i>Fund-Level Analysis</i>								
Dependent Variable:	<i>Monthly return of fund i minus average return of funds with the same investment objective</i>							
Mentioned between months $t-12$ and $t-1$ ?	-0.071 (0.076)	0.327*** (0.126)	0.144 (0.125)	0.041 (0.077)	-0.306 (0.303)	-0.080 (0.096)	-0.356*** (0.092)	-0.134** (0.063)
Sample Size	139,998	139,998	113,027	139,998	139,998	139,998	139,998	134,007
<i>Portfolio-Level Analysis</i>								
Dependent Variable:	<i>Monthly return of an equal-weighted portfolio of funds mentioned between months <math>t-12</math> and <math>t-1</math> minus equal-weighted return of all equity funds</i>							
CAPM Alpha	-0.061 (0.093)	0.141 (0.148)	0.062 (0.173)	-0.060 (0.089)	0.156 (0.498)	-0.047 (0.154)	-0.459*** (0.134)	-0.152* (0.091)
Four Factor Alpha	-0.072 (0.052)	0.133 (0.145)	0.133* (0.074)	-0.000 (0.090)	-0.092 (0.493)	-0.026 (0.106)	-0.310** (0.123)	-0.103 (0.077)
Sample Size	72	72	55	72	72	72	72	68

Panel A focuses on the full sample of U.S. mutual funds (excluding money market funds and mutual funds for which CRSP does not report a ticker for at least one share class during our sample period). Panel B focuses on the subset of domestic equity mutual funds. “Fund-Level Analysis” reports coefficients from an OLS regression of the return of fund  $i$  in month  $t$ , less the equal-weighted return of funds with the same investment objective, on a dummy variable that equals one if the fund received the media mention between months  $t-12$  and  $t-1$ . The dummy variable is defined using media mention data from January 1996 through December 2002. The OLS regression is restricted to January 1997 through December 2002 (and slightly shorter periods in the case of *Money* and *Consumer Reports*). Standard errors are reported below coefficients; they are robust to heteroscedasticity and cluster on investment objective-by-month pairs. “Portfolio-Level Analysis” calculates the equal-weighted return in month  $t$  of funds recommended between months  $t-12$  and  $t-1$ , subtracts the equal-weighted monthly return for the sample of domestic equity funds, and regresses these monthly portfolio returns on the market return (in excess of the risk-free rate) and other factors. Hence, for each publication, the number of observations is the number of months between January 1997 and December 2002 for which we are able to calculate portfolio returns based on our media mention data. We report the intercepts (alphas) as measures of risk-adjusted returns. Standard errors are reported below coefficients; they are robust to heteroscedasticity. Significance at the 10-percent, 5-percent, and 1-percent levels is denoted by \*, \*\*, and \*\*\*.

TABLE VII  
ESTIMATING THE IMPACT OF ADVERTISING BIAS ON MENTIONS, 1997-2002

Media mention:	<i>Money</i>	<i>Kiplinger's</i>	<i>SmartMoney</i>
Nature of mention:	Positive	Positive	Positive
<i>Panel A. Fund-Level Analysis of Relative Monthly Returns, Full Sample</i>			
(a) Predicted Mentions Including Own-Publication Advertising	-0.146* (0.086)	-0.238** (0.100)	-0.194* (0.115)
(b) Predicted Mentions Excluding Own-Publication Advertising	-0.158* (0.093)	-0.238** (0.106)	-0.225* (0.118)
(c) Predicted mentions based solely on lowest expense ratios	-0.029 (0.079)	0.145** (0.063)	0.125* (0.068)
P-value from Test: (a) = (c)	0.545	0.901	0.331
P-value from Test: (b) = (d)	0.209	0.003***	0.045**
<i>Panel B. Overlap in Predicted Mentions</i>			
Predicted Mentions (a) and (b)	91.5%	77.0%	77.9%
Predicted Mentions (a) and (c)	9.7%	1.0%	2.0%

Panel A replicates the analysis from Panel A of Table VI using (a) predicted mentions including the influence of own-publication advertising, (b) predicted mentions excluding the influence of own-publication advertising, and (c) predicted mentions based solely on which funds have the lowest expense ratio within each investment objective. Panel B measures the overlap in the sets of funds receiving predict mentions.