

# WSJ Category Kings - the impact of media attention on consumer and mutual fund investment decisions

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*We exploit a novel natural experiment to establish a clear causal relation between media attention and consumer investment behavior. Our findings indicate a 31 percent local average increase in quarterly capital flows into mutual funds mentioned in a prominent Wall Street Journal “Category Kings” ranking list, compared to those funds which just missed making the list. This flow increase is about 7 times larger than extra flows due to the well documented performance-flow relation. Other funds in the same complex receive extra flows as well, especially in smaller fund complexes. We show mutual fund managers react to the incentive created by the media effect in a strategic way predicted by previous theoretical work, and present evidence for the existence of propagation mechanisms including increased advertising by fund complexes and increased efficacy of subsequent fund mentions in articles.*

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It is widely accepted that information disseminated by the media informs consumer decision making in financial markets.<sup>1</sup> Our question, however, is whether *appearance in the media* impacts financial decision making, *independently* of the information conveyed. To investigate the existence of such a causal relation, we exploit a clean natural experiment in which the *Wall Street Journal* (WSJ) has prominently<sup>2</sup> published the top 10 mutual funds, ranked within various commonly used investment style categories, every quarter since 1994. Rankings are simply based on previous 12 month returns, ensuring both minimal editorial impact and quasi-random assignment around the publication cutoff of *rank* = 10.

Figure 1(a) graphically depicts a clear discontinuity in capital flows following publication between funds which appeared in the ranking and those which did not. Using a regression discontinuity design, we find a significant local average treatment effect, between funds ranked 10 (published) and 11 (unpublished), of 2.2 percentage point increase in flow of capital into the published funds during the post-publication quarter. This increase represents a hefty 31 percent increase in capital flows during the post-publication quarter, indicating consumers strongly react to media attention directed at these funds. The publication effect on flows is roughly 7 times larger in magnitude than the effect of the well-documented performance-flow relation<sup>3</sup>.

Mutual funds are specifically useful in exploring the impact of media attention on investment choices as fund capital flows are readily available at a fairly high frequency, in contrast to other investor allocation decisions which are much harder to obtain. Moreover, mutual funds represent a significant component of many U.S. households' financial holdings. In 2013, 69 percent of U.S. households with income above \$ 50,000 owned mutual funds, and the median amount invested was \$ 100,000, mostly inside employer-sponsored retirement plans.<sup>4</sup> Finally, in many financial instruments demand forces, resulting from media appearance, change the price of the instrument in the short term, whereas mutual funds' prices are related to the performance of the underlying portfolio. This decoupling of demand and price greatly simplifies the analysis of media impact.<sup>5</sup>

<sup>1</sup>See, e.g., Peress (2014)

<sup>2</sup>The top 10 ranking lists are part of an independent section, "Investing in funds - A quarterly analysis", and have an eye-catching heading, "Category Kings".

<sup>3</sup>E.g. Gruber (1996), Carhart (1997), Chevalier and Ellison (1997).

<sup>4</sup>Source: Investment Company Institute 2014 fact book - [www.icifactbook.org](http://www.icifactbook.org)

<sup>5</sup>Admittedly, fund flows may potentially impact a fund's ability to generate subsequent returns, but for the purpose of our study this is a second order concern.

The existence of a media effect on consumer financial decision making implies that mutual fund manager payoff resembles a call option due to the implicit asymmetric incentives induced by the extra flows<sup>6</sup>. Consistent with theoretical predictions by Basak, Pavlova and Shapiro (2007) and Cuoco and Kaniel (2011), we show that funds ranked near the  $rank = 10$  cutoff at the beginning of the last ranking month, and only these funds, “diverge from the herd” by increasing tracking error volatility relative to their category in an attempt to make the list. A closer analysis reveals that funds are well aware of the trade-offs induced by this risk shifting: within the funds ranked near the cutoff, only those that are not likely to be ranked as top performing funds next quarter increase tracking error volatility.

By analyzing fund media coverage in 89 major US news and business publications and fund complex advertising behavior, we are able to show mutual fund complexes increase advertising activities (expenditure, average ad size, and number of ads) in response to appearance in the WSJ rankings, and enjoy increased efficacy for mentioning their ranking in ads or for being mentioned in news and business articles. We thus establish several possible propagation mechanisms of the media effect. These protracted propagation mechanisms are also consistent with our finding that capital flow increases are gradual throughout the quarter, implying consumers do not rush to change investment allocations following the WSJ publication, but rather are influenced by it when making allocation and re-balancing decisions throughout the quarter.

Another notable finding is the existence of a sizable spill-over effect from a published fund to other funds of the same fund complex, which also enjoy a significant increase in capital flows. This 1.8 percentage point increase in capital flows into the other funds of the complex implies consumers not only “chase” published funds but rather change their attitude towards the entire brand/complex.

In sharp contrast to the sizable effects we identify, we observe no significant discontinuities in capital flows for falsification tests in which: we only examine categories which were not published in the WSJ (Figure 1(b)); the ranking is based on 11 rather than 12 month return (Figure 1(c)); the analysis is repeated for the off-quarter months (Figure 1(c)). We also show that the discontinuity at  $rank = 10$  is unique and does not exist for other plausible cutoffs.

<sup>6</sup>And the fact that management fees are determined as a percent of fund size. See Brown, Harlow and Starks (1996) and Chevalier and Ellison (1997) for tests of fund manager risk-shifting in the presence of call-option-like payoffs.

Our main contributions are: providing a simple, clean identification strategy devoid of endogeneity concerns; showing a significant effect including a sizable spill-over to other funds in the complex; showing fund managers strategically react to the existence of the media effect; and confirming several possible propagation mechanisms of the media effect, using novel data.

Section I below discusses the related literature and puts our study in context. Section II describes the data used and provides summary statistics. Section III presents our full empirical strategy and results. Section IV concludes. The appendix presents further evidence for the validity of the RDD and the robustness of our results to empirical design choices.

## I. Related Literature

The existence of a media effect is a natural result of costly information gathering by consumers in the spirit of Grossman and Stiglitz (1980). When search is costly, the mere appearance of a financial instrument in the media leads consumers to add the instrument to their limited “consideration set”, as proposed by Merton (1987).<sup>7</sup>

Several authors examine the effects of media attention on consumer investment behavior. Sirri and Tufano (1998) consider media attention as one of three proxies for the magnitude of search costs associated with purchasing a mutual fund. They use Lexis/Nexis mentions of mutual funds in the media and correlate them with capital flows while controlling for fund characteristics, with mixed results. Similarly, Barber and Odean (2008) construct a measure based on mentions of companies in the Dow Jones News Service daily feed, as one of three proxies for media attention. They find that investors are more likely to be net buyers of stocks mentioned in the news than of those not mentioned.<sup>8</sup> Kaniel, Starks and Vasudevan (2007) correlate the existence and frequency of media coverage of mutual funds to subsequent capital flows, and Solomon, Soltes and Sosyura (2012) further correlate media mentions of fund holdings to subsequent flows into the fund. Finally, Tetlock (2007) uses textual analysis of a WSJ opinion column to create a proxy for media sentiment towards the stock market and finds that it is associated with past and future returns of the Dow Jones Industrial

<sup>7</sup>See Corwin and Coughenour (2008) for a discussion on the impact of effort allocation due to limited attention in financial markets.

<sup>8</sup>Da, Engelberg and Gao (2011) use a direct revealed investor attention measure, derived from Google search frequency of Russell 3000 stock tickers, to provide support to the hypothesis of Barber and Odean (2008) that investors are net buyers of attention grabbing stocks.

Average and with future trading volumes on the New York Stock Exchange. A limitation of these inquiries is that they are restricted in their ability to make causal claims regarding the impact of media visibility due to the endogeneity of media reporting. Our identification strategy is tailored to alleviate such endogeneity concerns, and we focus on providing evidence showing causality.

Several authors attempt to resolve the endogeneity concern regarding the impact of media coverage, mostly in the literature concerning the effects of media on voter political leaning and behavior. These attempts generally employ population splits in which different groups of agents are exposed to different media outlets. Using a population splits approach, previous researchers have shown that both television (DellaVigna and Kaplan (2007); Enikolopov, Petrova and Zhuravskaya (2011)) and newspapers (Gerber, Karlan and Bergan (2009)) have an effect on political attitudes and voting patterns (see DellaVigna and Gentzkow (2010) for a survey). In a financial context, Engelberg and Parsons (2011) use micro-level trading data to show that sub-populations exposed to different local newspapers differ in investment behavior following the publication of articles discussing earnings releases of S&P500 Index firms. Engelberg and Parsons (2011) further demonstrate how extreme weather events which may disrupt the delivery of local newspapers sever the link between local content publication and local trading.

A shortcoming of using population splits is the need to control for determinants of a media outlet's decision to publish specific content and for characteristics of the sub-populations exposed to the content, which may complicate the identification. Engelberg and Parsons (2011), for example, utilize controls for earnings, investor, and newspaper characteristics, in addition to controls aimed at capturing home bias on the part of investors and local media. Our focus on the 12 regularly appearing style categories in the WSJ top-10 ranking tables, and keeping in mind that the WSJ uses a pre-specified fixed explicit algorithm to rank the funds, eliminates concerns regarding selection bias.

Though not strictly related to media visibility, Reuter and Zitzewitz (2013) use a methodology similar to ours to test, and mostly reject, the decreasing returns to scale hypothesis of Berk and Green (2004). They exploit differences in mutual fund capital flows between funds with different Morningstar ratings, which are close to the discrete ratings cutoff points, as a source of exogenous variation in fund size. As *all* star rankings are published simultaneously on Morningstar's website, no cogent discussion of the effect of media visibility, separate from information content, is possible under

their setting. We, on the other hand, are able to attribute a significant component of quarterly flows to a single day appearance in a WSJ category ranking table.

Our research also contributes to the literature that analyzes how implicit and explicit incentives impact fund managers’ investment decisions, such as Brown, Harlow and Starks (1996), Chevalier and Ellison (1997), and Carhart et al. (2002). Our evidence suggests fund managers are well aware of the impact on fund flows of making the top 10 lists. Furthermore, fund managers seem to understand that for funds close to the publication cutoff the appropriate strategy to increase the likelihood of making it onto the list is to increase tracking error volatility relative to other funds in the same ranking category, rather than just increasing volatility. Furthermore, and even more striking, we show that managers understand the trade-offs involved with this risk-shifting behavior: among funds near the cutoff, a month before the ranking, only those unlikely to be in a similar position next quarter engage in such risk shifting.

## II. Data and Summary Statistics

We consider 52 quarters, from 2000Q1 to 2012Q4, in which the “Investing in funds - A quarterly analysis” section was published in the *Wall Street Journal*.<sup>9</sup> The publication typically contains lists of top 10 mutual funds in 22 investment style categories, each ranked based on previous 12 month return. The 12 major categories are {small cap, mid cap, large cap, multi cap}  $\times$  {growth, core, value}, and are included in the publication every quarter. The remaining are sectors (e.g. Gold, Japan), changing every quarter based on editor’s choice.<sup>10</sup> In our analysis, we concentrate on the 12 major categories to eliminate the effect of editorial bias, though including the sector categories in the analysis does not materially change the magnitude or significance of our results. Data on published funds and categories, as well as precise publication date for each issue, were collected by directly searching for the published tables in Microfiche archives of the WSJ.

The regression discontinuity analysis critically depends on the WSJ ranking procedure. This procedure starts with the assignment of each fund to a category. During our sample period, category

<sup>9</sup>This period is chosen due to data availability on mutual fund categorization used by the WSJ, which is crucial to correctly replicating the WSJ rankings. While the rankings were published in the WSJ starting 1994, CRSP does not report Lipper categories before 2000. Furthermore, Lipper changed their categorization scheme starting late 1999, after being acquired by Thomson Reuters in 1998. Using stale categories is therefore impossible. Lipper is unable to provide a dataset of the categorization of funds prior to 2000.

<sup>10</sup>Over 200 Lipper sectors exist. 39 sectors appear at least once in the rankings.

definitions were supplied to the WSJ by an external data vendor, Lipper Analytical Services. At the end of every quarter, funds in each category are ranked based on previous 12 month return. However, many of these funds have several share classes with different fee structures, and consequently slightly different net 12 month returns. To ensure each fund is only ranked once, the WSJ retains the largest share class of each fund, based on total net assets (TNA).

Our method requires the complete list of ranked fund classes rather than just the top 10 published. We therefore replicate the WSJ ranking procedure using data on mutual fund returns and characteristics from the Center for Research in Security Prices (CRSP). We use category definitions and monthly return data to construct previous 12 month return for all funds in the categories examined, and total net assets data to choose the largest share class. Several of our tests require daily return data, also obtained from CRSP. As the replication process uses a different dataset than the one used for publication in the WSJ, we do not achieve full replication accuracy. Our ranking successfully matches that of the WSJ 89 percent of the time.<sup>11</sup> In the analysis to follow we replace the CRSP-based top 10 with the actual top 10 as published, though our results are nearly identical when using the CRSP-based list without top 10 correction. This robustness helps alleviate concerns regarding replication accuracy and its effects on the reported results.

Two mutual fund characteristics not available on CRSP but required for some of our analysis are the *Morningstar* rating of each fund, and the percent of mutual fund assets stemming from defined contribution pension plans. We obtain the historical ratings directly from Morningstar, and a dataset containing survey results for defined contribution assets from the *Pensions&Investments Research Center*, also used by Sialm and Starks (2012). The P&I survey data cover approximately 27 percent of our sample.

Table 1 reports summary statistics of mutual fund characteristics for a set of 111,780 fund observations on 5,334 unique funds over the sample period. Results for the full panel and 5 rank cross-sections are presented, as well as the p-value on equality of mean for each characteristic between  $rank = 10$  and  $rank = 11$ . The only characteristic for which there is a significant difference in mean between  $rank = 10$  and  $rank = 11$  is capital flows during the post-publication quarter.

One possible propagation mechanism we consider is direct mutual fund advertising. To that end, we

<sup>11</sup>Such that a typical CRSP-based top 10 list will contain approximately 9 of the 10 funds mentioned on the WSJ.

obtain a dataset on mutual fund complex advertising activity, including the advertising expenditure of the complex in print media, and a PDF copy of each magazine advertisement, from Kantar Media, an advertising consulting firm<sup>12</sup>. We manually tag approximately 6,800 ad images to extract useful ad characteristics such as the exact funds mentioned in the ad, the ad size, and whether a fund’s WSJ rank was mentioned in the advertisements. After removing ads which were found to not publish mutual funds, and considering that each ad may be published multiple times, we end up with 9,446 observations for 127 mutual fund complexes over the sample period.

To facilitate an investigation of another possible propagation mechanism, we construct a dataset containing the number of times each mutual fund was mentioned in 89 major US news and business publications<sup>13</sup>. We use the Dow Jones Factiva news collection and conduct an automated search for media mentions of almost 75,000 fund-months, searching by either fund ticker or name. We find over 13,000 articles mentioning 2,722 mutual funds which made it to the top 20 during our sample period. As we are interested in testing the effect of media mentions subsequent to the WSJ publication, we limit the search to start from the day following publication in the WSJ.

For a more in-depth analysis of capital flows during the post-publication quarter, we use data on daily capital flows purchased from TrimTabs. The TrimTabs dataset relies on voluntary disclosure by mutual funds, however, and therefore has limited coverage of the funds in our CRSP/WSJ sample. We observe daily flows for a subset ranging from 5 percent of fund share classes at the beginning of the inspected period to approximately 20 percent towards the end of the period. We use the TrimTabs data to analyze the duration and impact of the media effect during the post-publication quarter, and to test for a possible “announcement day” effect. Importantly, note that the exact publication date on the WSJ varies within the first week of the publication quarter, while the flow variables constructed from CRSP data consider the entire quarter, including a few pre-publication days. The duration analysis using TrimTabs data is used, in part, to verify our results are not driven by these few pre-publication days included in the flow calculations.

Finally, we investigate the relative importance of increases in capital flows into published funds versus decreases in capital outflows from published funds. To that end, we use data on the break-

<sup>12</sup>A similar dataset, albeit using a shorter time period, is used by Phillips, Pukthuanthong and Rau (2013), and they also provide a useful Appendix describing the dataset further.

<sup>13</sup>List of publications as defined by the Factiva category of the same name. Full list available from the authors upon request.



down of net flows into inflows and outflows obtained from the Securities and Exchange Commission’s EDGAR depository, contained in fund semiannual N-SAR reports. The information derived from N-SAR reports has several limitations: flow reporting is aggregated at the fund level rather than reporting flows into each fund-class; fund identification numbers used in EDGAR do not match those in the CRSP dataset, necessitating a manual name-matching process; approximately 8 percent of the N-SAR reports fail to download or are incomplete. These challenges to using N-SAR data, also documented by others<sup>14</sup>, result in low coverage of the funds in the CRSP/WSJ dataset. Specifically, we download 332,241 semiannual records from EDGAR, corresponding to 40,413 distinct entities, of which we match 16,986<sup>15</sup>. We are able to match 71 percent of the CRSP/WSJ funds, but only 37 percent of fund-months. Requiring funds to have three consecutive monthly observations needed to construct quarterly inflows and outflows further decreases our coverage to only 25 percent of the CRSP/WSJ fund-quarters.

### III. Empirical Strategy and Results

#### A. Discontinuity in capital flows

The foundation of our empirical strategy is a comparison between capital flows into published and unpublished mutual funds using a regression discontinuity design (RDD). A significant discontinuity in capital flows during the post-publication quarter will indicate that media exposure has a causal effect on consumers’ mutual fund purchase behavior. Capital flows into fund  $i$  during quarter  $q$  are defined as percent increase in the fund’s assets beyond asset appreciation:

$$(1) \quad Flow_{q,q+1}^i = \frac{TNA_{q+1}^i - TNA_q^i(1 + R_{q,q+1}^i)}{TNA_q^i}$$

in which  $TNA_q^i$  is the total net assets of fund  $i$  at the beginning of quarter  $q$ , and  $R_{q,q+1}^i$  is the return on the fund’s assets between the beginning of quarter  $q$  and the beginning of quarter  $q+1$ .<sup>16</sup> All flows

<sup>14</sup>see Warner and Wu (2011) and the extensive data appendix in Clifford et al. (2011)

<sup>15</sup>Our NSAR hit rate of 42 percent is in line with those of Warner and Wu (2011) and Clifford et al. (2011), who match 37 percent and 56 percent of NSAR observations, respectively, and concentrate on time periods different than ours.

<sup>16</sup>Our results are nearly identical when using the alternative measure,  $Flow_{q,q+1}^i = (TNA_{q+1}^i - TNA_q^i(1 + R_{q,q+1}^i)) / (TNA_q^i(1 + R_{q,q+1}^i))$ . The alternative measure assumes new capital flows take place at the beginning of the quarter whereas the main definition assumes new capital flows take place at the end of the quarter.

are winsorized at the 1 percent level to decrease the effect of outliers.

The main independent variable for the RDD is the fund’s rank within its style category at the end of the 12 month ranking period. Due to the discrete nature of the rank variable, the exact cutoff in the [10, 11] segment is an empirical design choice. The choice which minimizes extrapolation error is to use 10.5 as the cutoff. To find the predicted value of capital flows at 10.5, we employ local linear kernel regressions (LLR) from both sides of the cutoff, as advocated by the extant RDD literature.<sup>17</sup> The difference between the two predicted values at the cutoff is the discontinuity we aim to analyze.

We calculate two local linear kernel regressions independently for  $rank \in [1, 10]$  and  $rank \in [11, 50]$ . Bandwidth is determined using the optimal bandwidth estimator proposed by Imbens and Kalyanaraman (2012), who derive a closed-form, fully data-driven estimator for optimal RDD bandwidth. We use a triangular kernel,  $K(t) = \max\{0, 1 - |t|\}$ , shown by Cheng, Fan and Marron (1997) to have optimality properties for boundary estimation.<sup>18</sup> The basic LLR equation is:

$$(2) \quad \begin{cases} FlowQ_{rank,cat,q} = \gamma_0^L + \gamma_1^L * (10.5 - rank) + \epsilon_{rank,cat,q} & \text{if } rank \leq 10 \\ FlowQ_{rank,cat,q} = \gamma_0^R + \gamma_1^R * (rank - 10.5) + \epsilon_{rank,cat,q} & \text{if } rank \geq 11 \end{cases}$$

in which  $FlowQ_{rank,cat,q}$  is percent capital flow during quarter  $q$  into the fund ranked  $rank$  within category  $cat$ . The discontinuity is thus defined as  $\gamma_0^L - \gamma_0^R$ , the difference in one-sided predicted values at 10.5.

We further consider several falsification tests to help confirm that the discontinuity in capital flows is caused by appearance in the WSJ’s “Category Kings” tables. The first test repeats the analysis for the second and third months within a calendar quarter<sup>19</sup>, for which the WSJ did not publish the special quarterly issue, thus breaking the temporal link between publication of the special issue and subsequent flows. We note that during these off-quarter months, in which the special issue “Investing in funds” was not published, the WSJ nevertheless published the ranking tables. These off-quarter

<sup>17</sup>E.g. Hahn, Todd and Van-der Klaauw (2001), Imbens and Lemieux (2008). We also consider local quadratic regressions and high order global polynomials from the left and right. Our conclusions hold.

<sup>18</sup>In appendix Figure A3, we show our discontinuity result is robust to the choice of bandwidth. In unreported results, we also verify robustness to different choices of kernel.

<sup>19</sup>E.g. a ranking period runs from January 1<sup>st</sup> 2001 to December 31<sup>st</sup> 2001 and flows are calculated from January 1<sup>st</sup> 2002 to March 30<sup>th</sup> 2002. One of the falsification ranking periods runs from February 1<sup>st</sup> 2001 to January 31<sup>st</sup> 2002 and flows are calculated from February 1<sup>st</sup> 2002 to April 30<sup>th</sup> 2002. The second begins ranking on March 1<sup>st</sup> 2001 and is symmetrical.

tables were published in the back pages of the newspaper, along with the fund quotes, and ranking was based on year-to-date returns rather than previous 12 month returns. We find no discontinuity for the year-to-date based ranking tables either (results in appendix Figure A4). The second and third falsification tests maintain the same temporal structure as the WSJ publication but consider: categories which were *not* published in that quarter's issue and a ranking based on 11 month returns rather than 12.

A graphical view of the results is presented in Figure 1. Capital flows into mutual funds during the post-publication quarter exhibit a discontinuous increase (Panel (a)). The test using the categories *not* published in the WSJ is remarkably smooth at the discontinuity (Panel (b)), as are the tests using 11 month ranking (Panel (c)) and using the off-quarter months (Panel (d)). Pre-ranking 12 month return, the variable driving the ranking, is smooth as expected and exhibits no discontinuity around the cutoff (appendix Figure A2).

Formal discontinuity tests are reported in Table 2. We perform 5 types of discontinuity tests. Our main test uses Equation 2 to compute local linear kernel regressions on both sides of the cutoff and compare the intercepts. The second test repeats this analysis but adds controls for fund size, age and expense ratio. The third test is a Z-test for difference in mean capital flow between funds ranked 10 and funds ranked 11. The last two tests compare actual mean capital flow at  $rank = 10$  with LLR predicted value from the right and actual mean capital flow at  $rank = 11$  with LLR predicted value from the left. LLR predicted values at 10 and 11 are calculated using Equation 2 in which 10.5 is replaced by 10 or 11, as is the case.

All tests yield statistically significant discontinuity in capital flows around the cutoff.<sup>20</sup> Our main test indicates a local average increase in fund capital flows of 2.2 percentage points, representing a 31 percent average increase in capital flows during the post-publication quarter, relative to a predicted value of 7.1 percentage points at  $rank = 10$ . We find no indication of discontinuity in pre-ranking returns, on which the ranking is based. The tests also fail to find any significant discontinuity in mutual fund returns during the post-publication quarter that may indicate the existence of scale diseconomies. This is in line with the findings of Reuter and Zitzewitz (2013), who focus on testing the existence of scale diseconomies but find little evidence that fund size erodes returns. Finally, the last two columns

<sup>20</sup>We verify the remains significant when using heteroscedasticity robust s.e., jackknifed s.e., and s.e. clustered by quarter.

of Table 2 report the discontinuity in flows into all share classes of the same fund and into all other funds of the fund complex. We find a significant discontinuity in the aggregate capital flows into all share classes of each fund. This is to be expected, as different fund share classes typically only differ in their fee structure. Interestingly, we find a significant discontinuity of 1.8 percentage points in the aggregate capital flows into *all other* funds of the same fund complex, excluding the fund reported on the WSJ<sup>21</sup>. This significant spillover effect is consistent with an impact of publication on brand name recognition at the complex level.<sup>22</sup> We examine this spillover effect further in Section III.E.

Panel A of Table 3 reports discontinuity test results for the three falsification settings we employ. No result in the table is significant at the 10 percent level. Panel B reports the results of a falsification test for discontinuity in capital flows at cutoffs other than  $rank = 10$ . We expect to find no discontinuity at other cutoffs, and the results in Panel B confirm that the only statistically significant discontinuity is at  $rank = 10$ . In unreported results, we further verify that the discontinuity in next quarter flow is statistically different between the base setting and each of the three falsification settings, as well as between a cutoff at  $rank = 10$  and at  $rank = 9$ .

In further unreported results, we test for but fail to find a significant difference in capital flows into retail- and institutional-targeted fund classes. The magnitude of flows into all other retail classes of the same fund is similar to that of flows into all other institutional classes of the same fund as well. Though we focus our attention on increased capital flows, we examine other possible consequences of media exposure. We test and find no significant effect on fund management fees and expense ratios a quarter, and a year, after publication. Results of discontinuity tests for these possible outcomes are reported in appendix Table A2.

### B. Funds' response pre-publication

We hypothesize that the media effect causing increased capital flows into published mutual funds should affect optimal risk-shifting behavior of mutual fund managers pre-publication. Discontinuity in capital flows implies that, for funds around the  $rank = 10$  cutoff, there is a greater upside to increased rank than a downside to decreased rank. For example, a fund ranked 11 a month before

<sup>21</sup>Aggregate capital flows are computed by a weighted average of the percent flows into each fund class, with the total net assets of each class used as weight. When more than one fund of the same complex is ranked in the top 20, only one of the occurrences is kept, at random, to avoid attenuating the standard errors.

<sup>22</sup>For a discussion of information spillover between related products, see Hendricks and Sorensen (2009).

the end of a ranking period which drops from  $rank = 11$  to  $rank = 20$  by the end of the ranking period will, on average, see a 0.8 percentage point decrease in capital flows. Rising from  $rank = 11$  to  $rank = 10$  is, however, correlated with a 2.49 percentage point increase in flows.<sup>23</sup> This incentive scheme is related to, but distinct from, the one created by the flow-return relationship, discussed by Brown, Harlow and Starks (1996), Chevalier and Ellison (1997), and Sirri and Tufano (1998). Finding such a managerial response to the media effect can further increase our confidence in the validity of the discontinuity results reported in the previous section.

The theoretical models of Basak, Pavlova and Shapiro (2007) and Cuoco and Kaniel (2011) predict that managers of mutual funds around the publication cutoff will respond to this incentive specifically by increasing the fund's tracking error volatility relative to the respective category. Tracking error volatility captures how much a fund's investment policy deviates from a baseline portfolio. The tracking error volatility for fund  $i$  during month  $m$  relative to portfolio  $p$  is defined as:

$$(3) \quad TE_{i,p,m} = SD(r_t^i - r_t^p), \quad t \in m$$

where  $r_t^i$  and  $r_t^p$  are daily returns of fund  $i$  and portfolio  $p$ , respectively, during day  $t$  of month  $m$ , and SD represents standard deviation. We concentrate our analysis on tracking error volatility relative to an equal-weighted portfolio of all mutual funds in the same category as fund  $i$ . A significant increase in TE will indicate that managers are aware of the increased flows caused by media attention, and respond by risk-shifting accordingly, in an attempt to increase ranking volatility and the expected payoff of the call-like option they hold.

To observe such changes in investment policy, we demean each fund's tracking error volatility relative to the entire month-category, grouped by rank, and calculate changes in average demeaned values from month 11 to month 12 of a ranking period, based on 11 month ranking. As measures based on daily returns are inherently noisy, we aggregate funds into groups consisting of four consecutive ranks. The first row of Panel A in Table 4 lists changes in average tracking error volatility (TE) between month 11 and month 12 of a ranking period. As predicted, there is a large and statistically significant increase in TE relative to the category portfolio for funds close to the cutoff. We also report changes in tracking

<sup>23</sup>Of which 2.2 pp are due to publication, and 0.29 pp are from the return-flow relationship.

error volatility relative to the S&P500 Index (TESP) and the volatility of funds' daily returns (VOL), though theory makes no predictions regarding these metrics. We observe no statistically significant increase in either, as is evident from the second and third rows of Panel A, respectively.

An interesting feature of our empirical setting is the repeated quarterly publication based on previous 12 month return. This feature implies that a fund which enjoys exceptionally high returns in a given quarter may take advantage of these exceptional returns to get published in the WSJ during the subsequent four top 10 ranking publications, before that quarter is no longer accounted for. A fund manager considering engaging in risk-shifting behavior as described above needs to weigh the benefit of rising in the rank and making the list this quarter versus the risk of falling in the rank and losing the option to make the list next quarter. For funds with their best quarter about to “expire”<sup>24</sup>, this consideration is no longer relevant and we hypothesize fund managers will therefore engage in more risk-shifting behavior in such cases.

To test this hypothesis, Panel B splits the sample to funds above and below the median quarterly return for the first quarter of every 12 month ranking period, and repeats the analysis of Panel A independently for each sample split. Only the funds at the top group exhibit TE increase, at the rank groups [9, 12] and [13, 16]<sup>25</sup>. Panel C again splits the sample, but with the cutoff being the 25 percentile of returns as calculated only for months 4 – 10 of a ranking period. This cutoff allows us to isolate specifically the funds standing to lose the most from the loss of the first quarter's returns, and we observe a TE increase only for the bottom group around the cutoff, and of higher magnitude relative to the first split in Panel B. The striking differences between the top and bottom groups in Panels B and C strongly support our hypothesis regarding strategic risk-shifting behavior.

A second possible managerial response for mutual funds close to the cutoff is to lean for the tape, artificially increasing fund value by inflating the value of stocks they already hold using strategically placed buy orders on the very last day of the quarter. Leaning for the tape behavior by top performing funds was documented by Carhart et al. (2002). However, Duong and Meschke (2011) provide substantial evidence that the Security and Exchange Commission enacted enforcement actions against mutual funds participating in this practice following circulation of early versions of Carhart et al.

<sup>24</sup>E.g. a fund with an exceptional Q1 in 2001 may get published on the WSJ in 4/2001, 7/2001, 10/2001, 1/2002. The publication occurring on 1/2002 is the last one for which the fund returns during Q1 2001 are still relevant, and we say the quarter expires after that publication

<sup>25</sup>And the TE increase for these top groups is statistically different than that of the bottom groups



that  $CONTROLS_{rank,cat,q}$  is a vector.

Panel A of Table 5 reports the results of the independent test for each characteristic, while Panel B reports the results of tests in which several characteristics are included simultaneously, to control for their cross-effects. Considering, for example, Panel A, our results indicate that a one standard deviation *decrease* in fund size (fund age) is correlated with approximately 1.37 (0.99) percentage point *increase* in capital flows into a published fund, normalized to quarterly values.

The significant negative impact of fund size, age and complex size on mutual fund capital flows in most specifications further corroborates the search costs interpretation of the media effect, as small and young funds from smaller families are typically less visible ex-ante, leading to higher search costs for a prospective consumer. The significant effect of the Morningstar rating indicates that funds which made it into the top 10 lists also enjoy an increase to the positive effect of MS rating on flows.

The negative relation we document for the ratio of fund assets stemming from defined contribution pension plans<sup>26</sup> indicates that funds catering largely to DC pension clientele see lower increases in their flows when making it into the top 10 lists. This result is in line with the analysis of Choi et al. (2002), showing that DC investors follow “the path of least resistance” and seldom change their investment allocations. A similar negative relation for funds likely to be distributed by a broker proposes attenuated response by brokerage clientele, in line with the clientele characteristics described by Bergstresser, Chalmers and Tufano (2009).

#### D. Possible propagation mechanisms

To better understand the propagation of the top 10 effect from publication to altering a financial consumer’s behavior, we employ two datasets constructed for this purpose. The first dataset examines changes in advertising behavior by mutual fund complexes following publication using data on more than 6,600 published mutual fund ads. The dataset includes ad size and expenditure, among other features, and was extended by manually extracting features such as the names and tickers of mutual funds mentioned in the ad, or the fact that an ad mentions the ranking of a fund based on the WSJ ranking scheme, from the ad images. The second dataset was compiled by executing more than 75,000 Factiva searches, each counting the number of times a fund or its ticker were mentioned in

<sup>26</sup>We do not include the DC ratio in the multivariate test as it is based on survey data, and is only available for 27 percent of the sample.



articles published in 89 major US news and business publications during the quarters before and after publication.

These two datasets shed light on several possible propagation mechanisms. The first is propagation by increased fund or complex advertising activity, as a causal reaction to making the top ten lists. The second is an increased effectiveness of existing fund advertising efforts, for example by being able to state the ranking on the WSJ list in ads. Similarly, it is possible that existing, non-advertising, marketing efforts are more effective. We find evidence supporting all three mechanisms.

Panel A of Table 6 reports results using our main discontinuity test, described by Equation 2. The first three columns of Panel A describe discontinuity results of indicators for increase in: Ad Size - the average size in square inches of all ads published by the mutual fund complex; Amount Spent - the dollar amount the fund complex spent on advertising during the given quarter; Ads Published - the number of ads published by the fund complex. We observe a significant discontinuity for each of these indicators, showing that appearing on the top 10 publication causes a 26 percentage point increase in the probability that a fund complex will increase the average size of ads it publishes following publication, compared with pre-publication size. Similar increases in Amount Spent and Ads Published strongly indicate an increase in advertising activity by mutual funds, *caused* by appearing in the top 10 lists of the WSJ.

The fourth column of Panel A tests for discontinuity in an indicator variable indicating whether the complex has increased the number of times it mentions the fund's rank in advertising, following publication date. We find no significant discontinuous increase in rank mention activity. Similarly, we find no discontinuous increase for an indicator testing whether the fund saw an increase in mentions at newspapers and journals, based on the Factiva searches.

Panel B of Table 6 reports results of affecting characteristic tests, as described by Equation 4 in the previous section. The first row of the Panel tests for influence on flows into the published fund-class, while the second row tests for influence on flows into the entire fund complex of the published fund.

For the first three columns, testing increased advertising, we observe no significant differential effect around the discontinuity, for either class or complex flows. This indicates that the *efficacy* of increasing the number of ads published by a fund which made the list is similar to that of a fund which just missed it, as is the efficacy of increasing advertising budget or the actual ad size. But while

the efficacy does not change, the increased activity observed in Panel A will still result in increased flows. Additionally, it is possible that mutual funds do gain an increased efficacy for ads following publication, but this direct effect is offset by decreasing returns to scale on the efficacy of advertising, caused by the observed increase in advertising activity.

We do observe, however, increased efficacy of mentioning funds' WSJ ranks more, as well as for having the fund mentioned more in subsequent news (non-ad) articles in the media. These subsequent news mentions for published funds may act as a reminder or reinforcement for interested consumers, whereas subsequent news mentions for unpublished funds lack such a role. For a discussion of the role of repetition in consumer persuasion see, e.g., Cacioppo and Petty (1979), Campbell and Keller (2003). The increased efficacy of Rank Mentions is evidence that mutual fund clientele react to mentions of WSJ rank more when the ranked fund is just above the publication threshold, compared to being just below it.

We further attempt to test whether funds that made it to the top 10 are mentioned more in the Kantar ad data, relative to those which did not, by extracting fund names mentioned in the ads and matching them with our CRSP-based ranking. Out of 910 mutual fund names mentioned in the ads, we match 693 (76 percent) to the CRSP rankings. While, on average, only 5 percent of funds make it to the top 20 ranking within our 12 main categories every quarter, 22 percent of funds mentioned in mutual fund ads can be matched to the top 20 funds within our main categories. This indicates mutual fund complexes indeed invest a disproportional advertising effort in these funds. Within the top 20, 22 percent of mentions are to funds ranked 1–5, while funds ranked 6–10 comprise 41 percent of mentions. Funds ranked 11–15 and 16–20 garner 22 and 15 percent of mentions, respectively. The low absolute number of fund mentions within the top 20 ranking prevents us, however, from conducting a meaningful statistical test for increase in mentions using these data.

Finally, we attempt to test whether being mentioned on the WSJ tables increases observed investor attention by considering the Google Search Volume Index (SVI), similar to Da, Engelberg and Gao (2011). Unfortunately, Google censors results with too few searches (with an undisclosed censoring threshold), such that only 4.2 percent of the fund-month observations in our dataset have non-zero SVI. We are therefore unable to test for an increase in SVI following publication in the WSJ.

*E. Attention spillovers*

As reported in Table 2, we observe a discontinuous increase in capital flows around the  $rank = 10.5$  cutoff when considering the flows to the entire fund complex, excluding the fund mentioned. These spillover flows are further corroborated by the evidence in Table 6, in which we observe a discontinuous effect for increases in rank mentions and media mentions, even when considering flows into the entire complex (second row of Panel B). The finding of spillover effect is consistent with the results of Nanda, Wang and Zheng (2004), who find a 4.4 percent (on an annual basis) increase in flows into complexes of “star” funds, somewhat smaller in magnitude than our finding of 7.5 percent increase (on an annual basis). We now explore which fund and complex characteristics have a discontinuous effect on these spillover flows.

Table 7 summarizes the results of tests for a discontinuous effect of fund and complex characteristics on spillover flows. We observe no discontinuous difference in the effect of the published fund size and age on spillover flows around the cutoff. We do find a positive effect for the published fund’s expense ratio, as well as a negative effect for complex size, measured both as the (log) aggregate TNA of the complex and as the number of funds in the complex.<sup>27</sup> These results indicate spillovers are concentrated at smaller fund complexes, with funds belonging to complexes in the upper part of size distribution gaining low or no spillover flows. This finding is consistent with attention theory, as larger complexes are likely to be more visible (i.e. possess stronger brand name recognition) ex-ante.

We further observe a discontinuous increase in the effect of having the published fund mentioned more in the media on spillover flows, indicating having the winning fund mentioned increases visibility for the entire complex, not just the fund mentioned.

*F. Time analysis of increased flows*

We next examine the duration of the media effect within the quarter, and whether there is a an observable time trend in the quarterly flow increases throughout the sample period. Duration analysis within the quarter can shed light on consumer behavior in response to media stimuli, and whether consumers have an immediate or protracted response to the media effect. Time trend analysis across

<sup>27</sup>Note that for complex spillovers, if a complex appears more than once in the rankings at the same quarter, only one of the occurrences is kept, at random, to avoid attenuating the standard errors.

quarters can test whether the magnitude of the media effect changes with time, possibly in response to a richer media environment and more accessible data regarding mutual funds.

Figure 2(a) begins this analysis by presenting differences between mean percent cumulative capital flows into funds ranked 10 and 11, for the year following publication in the WSJ, in monthly intervals. We can see that, *ex-ante*, the media effect is expected to last up to 6 months post-publication. Figure 2(b) presents the difference in flows after removing funds which were published in the following quarter. The effect diminishes and is no longer significant after the third month, suggesting the effect in months 4 – 6 may be driven by funds being republished in the WSJ in subsequent quarters. Figure 2(c) repeats the analysis of difference in flows, in daily intervals for the first 60 trading days post-publication, using the TrimTabs daily flow data. The TrimTabs data are missing flow data for 5 trading days each month, on average, making it impossible to precisely calculate cumulative flows throughout the quarter. We overcome the missing observations limitation by calculating median daily flows for post-ranking days, and then cumulating these daily medians. Standard errors on the cumulated medians are obtained using the bootstrap with 1000 repetitions.

The evidence in Figure 2 indicates a fairly smooth increase in capital flows throughout the post-publication quarter. We find no evidence of an immediate flow response following publication (no “announcement day effect”), or concentration of increased flows at the early part of the quarter. These results indicate a protracted propagation of the media effect, in which the WSJ publication is a first step in the causal chain, followed by other investor stimuli leading to purchase. This is consistent with the mechanisms suggested above.

Interestingly, the WSJ made all rankings (and not just the top 10) available on its website starting 2007. We find, however, no difference in the magnitude of discontinuity between 10 and 11 in the early (pre-2007) and late (post-2007) parts of the sample (unreported). We further find no indication of a time trend for the magnitude of the discontinuity. Both results show the effect was not weakened by better availability of information regarding mutual funds over time, similar to the results of Phillips, Pukthuanthong and Rau (2013), and indicate that it is indeed media exposure rather than information dissemination which caused the increased capital flows.

G. *Inflows vs. Outflows*

Finally, we attempt to decompose the increase in net flows into an increase in inflows component and a decrease in outflows component, to explore whether the media effect attracts new customers to the mutual fund or discourages existing customers from selling their holdings in the mutual fund. The N-SAR dataset on the breakdown of net-flows to in-flows and out-flows, downloaded from EDGAR and described in Section II, is incomplete. We lack these data for almost 75 percent of our CRSP/WSJ sample. Consequently, the results reported in this section are mixed and inconclusive.

Our analysis so far concentrated on net capital flows - the difference between incoming capital flows (consumers investing in the mutual fund) and outgoing capital flows (consumers cashing out). We now consider the contribution of increasing capital in-flows vs. decreasing capital out-flows. We begin the analysis by testing for a discontinuity independently for in-flows and out-flows. We find no significant discontinuity in either in-flows or out-flows (unreported).

Next, we use the model:

$$(5) \quad \Delta netflow_{10-11,cat,q} = \beta_0 + \beta_1 \Delta inflow_{10-11,cat,q} - \beta_2 \Delta outflow_{10-11,cat,q} + \epsilon_{cat,q}$$

which explains differences in next-quarter net-flow between funds ranked 10 and 11 using differences in next-quarter in-flow and next-quarter out-flow. By construction, we expect  $\beta_0 = 0, \beta_1 = \beta_2 = 1$ , but we are in fact interested in the analysis of explanatory power stemming from each of the components of this regression. To that end, we use the Shorrocks-Shapley factor decomposition, described by Shorrocks (1982), which decomposes the proportion of total variation of outcome explained by the model ( $R^2$ ) into the proportion explained by each component of the model. We estimate the model for ranking months and non-ranking months independently, and compare the explained variance contribution of  $\Delta inflow_{10-11}$  and  $\Delta outflow_{10-11}$  for ranking and non-ranking months. In a second specification, we combine observations from ranking and non-ranking months and estimate the model with an indicator variable for ranking months interacted with  $\Delta inflow_{10-11}$  and  $\Delta outflow_{10-11}$ .

Table 8 presents the results of the  $R^2$  decomposition along with bootstrapped standard errors.  $\Delta inflow_{10-11}$  explains significantly more of the variance in  $\Delta flow_{10-11}$  during ranking months compared to non-ranking months. The ratio of variance contribution of inflows to outflows is  $0.738/0.262 =$

2.817 during non-ranking months, but hikes to  $0.947/0.053 = 17.868$  during ranking months. When using the second (pooled) specification, the  $R^2$  contribution of both  $\Delta inflow_{10-11}$  and  $\Delta outflow_{10-11}$  during ranking months is higher than during non-ranking months. The magnitude of increase in the contribution of  $\Delta inflow_{10-11}$  to explained variance is markedly higher than that of  $\Delta outflow_{10-11}$ .

These results suggest that mutual funds enjoy both a decrease in out-flows and an increase in in-flows following appearance on the WSJ, but that increases in in-flows are likely the main effect leading to increases in net-flows following publication.

#### IV. Conclusion

We exploit a novel natural experiment to establish a clear causal relation between media attention and consumer investment behavior. Our findings indicate a 31 percent local average increase in quarterly capital flows into mutual funds caused by a mention in a prominent *Wall Street Journal* “Category Kings” ranking table, along with a sizable spillover effect to other funds of the same fund complex. We show mutual fund managers react to the incentive created by the media effect in a strategic way predicted by previous theoretical work, and present evidence for the existence of propagation mechanisms including increased advertising by fund complexes and increased efficacy of subsequent fund mentions in articles. Our findings are consistent with a search costs interpretation of the media effect, though we are unable to distinguish the relative contribution of limited attention costs vs. direct information acquisition costs.

While previous literature found it challenging to decouple the effects of media attention from those of potential information revelation due to the endogeneity of media coverage, the quasi-random assignment we utilize sidesteps this endogeneity issue. The empirical method precisely controls for the publication’s underlying information content and eliminates the need to account for other media or consumer characteristics.

For mutual funds close to the  $rank = 10$  cutoff, the increased capital flows create an incentive for mutual fund managers to engage in risk shifting, increasing the volatility of their rankings pre-publication. A back-of-the-envelope calculation suggests that the mere presence on a top 10 list in a single ranking period allows a fund to collect almost \$1.5 million in increased fees, on average.<sup>28</sup>

<sup>28</sup>The mean total net assets and expense ratio for funds ranked 10 are \$771M and 1.24 percentage points (pp), respectively.

This is in addition to, and much higher than, increased fees stemming from the well-documented return-flow relation, which amount to an estimated \$200,000.<sup>29</sup> When considering extra flows to the entire fund complex, the increased fees from appearance on the list amount to a sizable \$36 million<sup>30</sup>. As predicted by theory, we find that funds close to the cutoff that are unlikely to be top ranked funds in the subsequent quarter increase tracking error volatility relative to the respective category, in an attempt to “make the list”. The fact that only funds unlikely to be top ranked next quarter increase tracking error highlights that managers are well cognizant of trade-offs associated with this risk shifting behavior.

We provide evidence that mutual funds increase advertising activity as a result of appearing on the top 10 lists, and that mentions of the fund rank in ads, as well as mentions of the fund name or ticker in media articles, have an increased efficacy in generating flows for funds which made the list relative to those which did not. These results are in line with our finding that the increase in flows is not limited to a short period of time close to publication day, but rather builds throughout the quarter and abates once a new ranking is published, which is to be expected if advertising and subsequent media exposure play key roles in the effect’s propagation. We further find that mutual funds catering to a clientele less likely to be affected by the media, such as those funds directly sold through brokers, or funds having a large portion of their assets stemming from defined contribution plans, indeed show decreased reaction to the media effect.

Our results show a sizable causal link between media attention and consumer investment choices in a simple and clear way, identify a strategic behavior by mutual fund managers in response to this media effect, propose possible propagation mechanisms to support the observed effect duration, and suggest that mutual fund investors use published lists and subsequent advertising and media mentions as substitutes to costly search.

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Sirri and Tufano (1998) find that the typical holding period for mutual fund investors is 7 years. The 2.2 pp local average increase in flows from being ranked 10 rather than 11 translates to \$1,472,302 in extra fees over the 7 year holding period.

<sup>29</sup>The LLR predicted value from the left (right) of capital flows at  $rank = 10$  ( $rank = 11$ ) is 9.56 pp (7.07 pp). Hence, being ranked 10 rather than 11 increases flows by 2.49 pp, 2.2 pp of which is due to the discontinuity.

<sup>30</sup>The mean total net assets for the complex of a fund ranked 10 is \$24,115M

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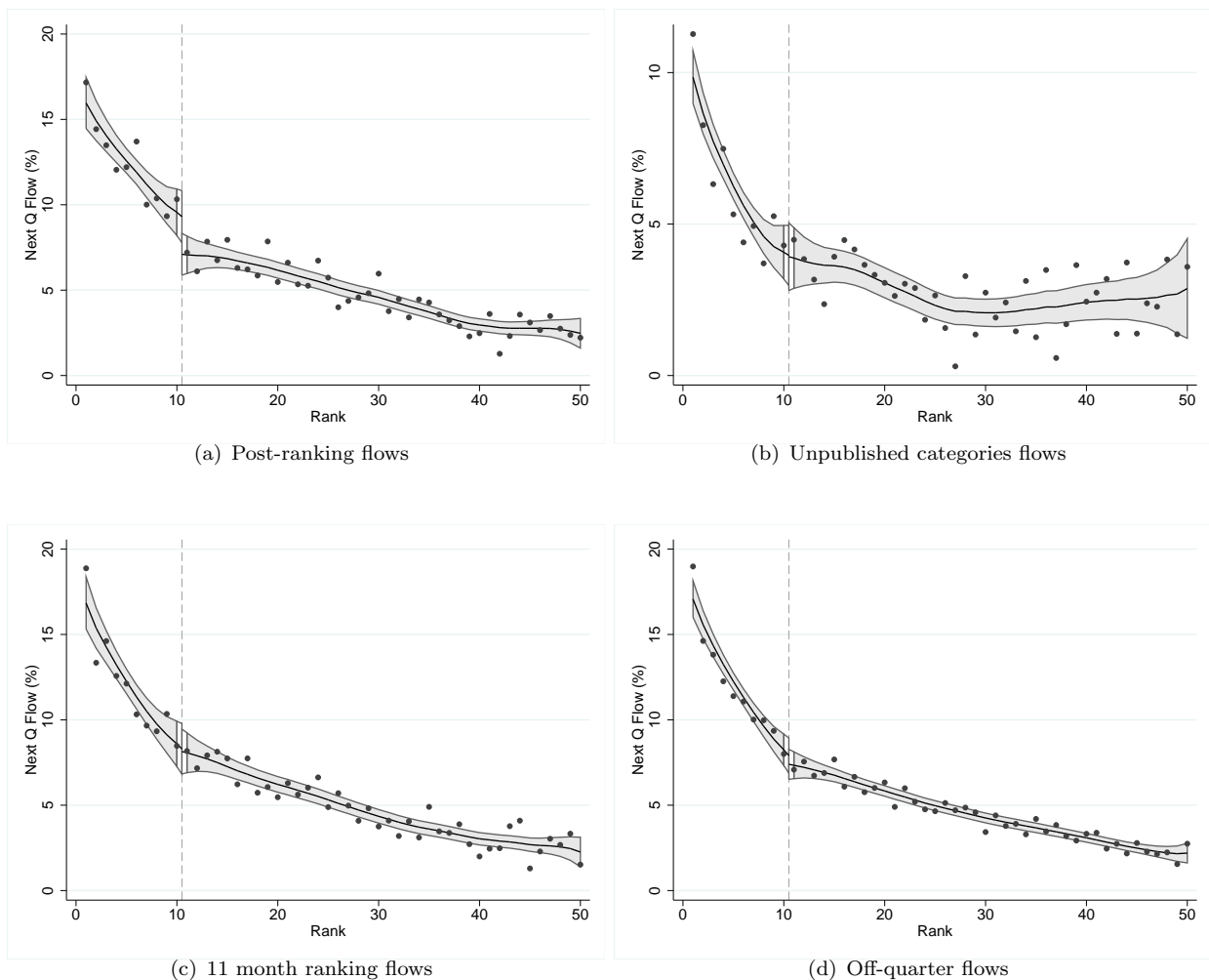


FIGURE 1. RDD ANALYSIS OF FUND FLOWS BY RANK

*Note:* Mutual funds are ranked based on previous 12 month return within their investment category at the beginning of every quarter from 2000Q1 to 2012Q4 using data from CRSP. The top 10 funds for figure (a) are replaced with the actual funds published in the Wall Street Journal. Next Q Flow is the net percentage capital flow into the fund during the quarter after publication. The figure presents mean next-quarter capital flows into funds per rank for (a) ranked funds based on the WSJ publication setting, (b) a falsification test using category-quarters *not* published on the WSJ, (c) a falsification test in which rankings are based on 11 month return and (d) a falsification test in which the analysis is shifted by 1 and 2 months from the beginning of a calendar quarter. Each dot in panels (a) and (c) represents the mean of 624 data points per rank, corresponding to 12 categories over 52 quarters. For panel (d), each dot is the mean of 1248 data points (two off-quarter months for each of 52 quarters). For the unpublished categories in panel (b), the number of data points per rank varies from 1,882 observations for  $rank = 1$  and down to 502 observations for  $rank = 50$ . The figures include local linear kernel regression lines and 95% confidence intervals for the segments  $[1,10]$  and  $[11,50]$ .

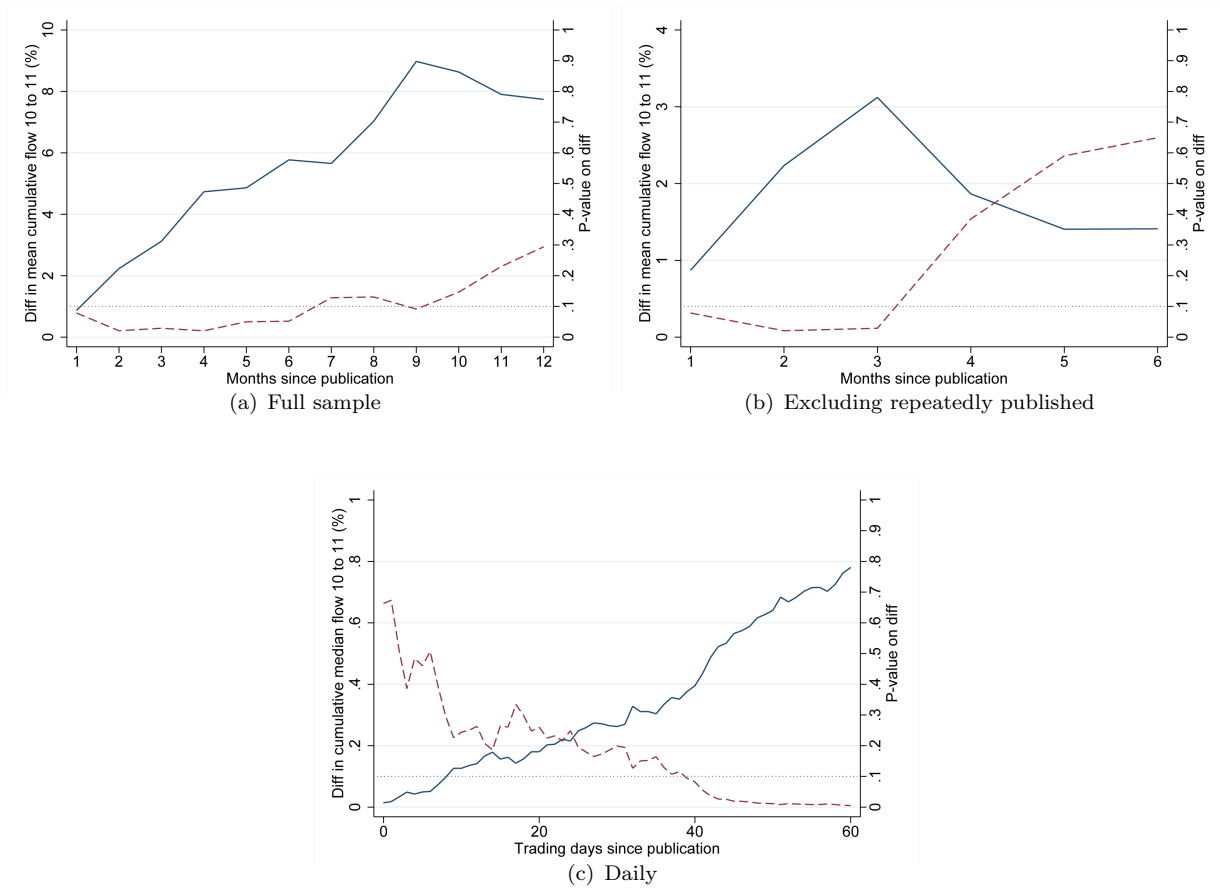


FIGURE 2. TIME TREND OF DIFFERENCE IN CAPITAL FLOWS INTO MUTUAL FUNDS RANKED 10 AND 11

*Note:* Using the CRSP dataset on monthly flows, Panel A presents the difference between mean percent cumulative capital flows into funds ranked 10 and 11, during the 12 months following publication in the WSJ, along with its p-value. Panel B repeats this analysis, but omits funds which were published on the WSJ ranking lists in the next quarterly publication (i.e. in month 4). Panel C concentrates on the first 60 trading days following exact publication day using daily flow data obtained from TrimTabs. The coverage of the TrimTabs data ranges from 5% of the funds in the early years of the sample to approximately 20% towards the end of the inspected period. Panel C therefore reports the difference, between funds ranked 10 and 11, in cumulative *median* daily flows. We obtain p-values on the differences in cumulated medians using the bootstrap with 1000 repetitions.

TABLE 1—SUMMARY STATISTICS BY RANK CROSS-SECTIONS

Characteristic		Full	Rank=1	Rank=10	Rank=11	Rank=20	Rank=50	p-val(11-10)
TNA	Mean (\$M)	910.256	550.926	761.336	919.244	1100.958	1273.192	(0.330)
	SD	3678.845	1443.850	2251.147	3361.632	3934.336	5099.416	
Fund Age	Mean (Y)	11.446	9.572	10.311	11.257	11.724	11.526	(0.133)
	SD	12.115	12.073	10.747	11.473	12.163	11.609	
Yearly Return	Mean (percent)	6.056	41.667	19.682	19.097	15.683	10.012	(0.707)
	SD	24.641	50.886	27.637	27.407	25.434	22.860	
Expense Ratio	Mean (percent)	1.227	1.560	1.244	1.229	1.209	1.183	(0.594)
	SD	1.592	1.259	0.518	0.460	0.503	0.501	
Stars	Mean	3.020	3.640	3.561	3.639	3.519	3.174	(0.207)
	SD	1.067	1.394	1.119	1.069	1.037	0.982	
Beta	Mean	1.015	1.035	0.997	0.992	0.996	1.017	(0.711)
	SD	0.247	0.545	0.282	0.255	0.233	0.206	
Next Q Return	Mean (percent <sup>a</sup> )	1.188	2.170	1.273	1.221	1.407	1.279	(0.933)
	SD	10.793	12.795	10.760	10.677	10.599	10.882	
Next Q Flow	Mean (percent <sup>a</sup> )	1.373	17.166	10.325	7.203	5.478	2.217	(0.027)**
	SD	17.343	33.937	28.809	20.487	20.688	16.506	

*Note:* This table reports summary statistics for a set of 111,780 fund observations on 5,334 unique funds over the period 2000Q1-2012Q4 across several cross sections of the data by *rank*, which is the ranking of the fund based on previous 12 month return at the end of each quarter. Also reported are the p-values on the difference in mean between funds with *rank* = 10 and *rank* = 11. The characteristics reported are: TNA (total net assets held by the fund), Fund Age (years since fund inception), Yearly Return (12 month return on which the ranking is based), Expense Ratio (the percentage of fund assets claimed as expenses every year), Stars (the Morningstar star ranking of the fund), Beta (the fund's beta vs. the S&P500 portfolio), Next Q Return (the return the fund generated during the quarter following publication) and Next Q Flow (the net capital flow into the fund, during the quarter after publication).

<sup>a</sup> Next quarter return and flow are in quarterly percentage terms.

\*\* Significant at the 5 percent level.

TABLE 2—TESTS FOR DISCONTINUITY IN FLOWS AND RETURNS AROUND THE RANK=10 CUTOFF

	Next Q Flow	Prev Y Return	Next Q Return	Next Q Flow (entire fund)	Next Q Flow (complex spillover)
Fitted vs fitted at 10.5	2.203** (0.028)	-0.600 (0.599)	0.108 (0.807)	1.862* (0.053)	1.820** (0.032)
Fitted vs fitted w/ controls	1.932* (0.051)	-0.659 (0.561)	0.084 (0.850)	1.788* (0.061)	1.877** (0.030)
Actual 10 vs actual 11	3.122** (0.027)	0.585 (0.707)	0.051 (0.933)	3.127** (0.022)	2.694** (0.032)
Fitted vs actual at 10	3.223** (0.014)	0.473 (0.728)	0.033 (0.951)	2.792** (0.027)	2.706** (0.022)
Fitted vs actual at 11	2.103* (0.069)	-0.487 (0.722)	0.126 (0.812)	2.198** (0.046)	1.809* (0.055)

*Note:* This table reports the results of five discontinuity tests performed over Next Q Flow (the net capital flow during the post-publication quarter into the published fund class), Prev Y Return (12 month return on which the ranking is based), Next Q Return (the return the fund generated during the quarter after publication), Next Q Flow into all classes of the published fund, and Next Q Flow complex spillover, the flow into all the funds of a fund complex at a given rank except the published one. Our main test uses Equation 2 to compute local linear kernel regressions on both sides of the cutoff and compare the intercepts at  $rank = 10.5$ . The second test repeats this analysis but adds controls for fund size, age and expense ratio. The third test is a Z-test for difference in mean next quarter capital flow between funds ranked 10 and funds ranked 11. The last two tests compare actual mean capital flow at  $rank = 10$  with LLR predicted value from the right and actual mean capital flow at  $rank = 11$  with LLR predicted value from the left. LLR predicted values at 10 and 11 are calculated using Equation 2 in which 10.5 is replaced by 10 or 11, as is the case. Values in (brackets) are p-values on the difference being 0 (no discontinuity). For complex spillovers, if a complex appears more than once in the rankings at the same quarter, only one of the occurrences is kept, at random, to avoid biasing the standard errors.

\*\*\* Significant at the 1 percent level.

\*\* Significant at the 5 percent level.

\* Significant at the 10 percent level.

TABLE 3—FALSIFICATION TESTS FOR DISCONTINUITY IN FUND FLOWS AND RETURNS

Panel A - Perturbed settings												
	Next Q Flow	Prev Y Return	Next Q Return	Next Q Flow (entire fund)	Next Q Flow (complex spillover)							
Unpublished categories	0.043 (0.956)	-0.246 (0.687)	-0.182 (0.530)	0.162 (0.718)	-0.019 (0.843)							
Off-quarter	0.517 (0.440)	-0.639 (0.398)	-0.025 (0.936)	-0.540 (0.621)	0.217 (0.500)							
11 month ranking	0.154 (0.876)	-0.539 (0.603)	0.014 (0.974)	-0.752 (0.643)	0.011 (0.983)							
Panel B - Different cutoffs												
	6	7	8	9	10	11	12	13	14	15	20	50
LATE <sup>a</sup>	1.360	0.039	0.362	0.175	2.203**	0.791	-1.435	-0.789	-1.169	0.359	-0.332	0.275
p-value	0.253	0.972	0.735	0.869	0.028	0.391	0.110	0.382	0.199	0.694	0.720	0.682

*Note:* Panel A reports the results of discontinuity tests using perturbed settings, performed over Next Q Flow (the net capital flow during the post-publication quarter into the published fund class), Prev Y Return (12 month return on which the ranking is based), Next Q Return (the return the fund generated during the quarter after publication), Next Q Flow into all other classes of the published fund, excluding the one published, and Next Q Flow complex spillover, the flow into all the funds of a fund complex at a given rank except the published one. The test uses Equation 2 to compute local linear kernel regressions on both sides of the cutoff and compares the intercepts at  $rank = 10.5$ . “Unpublished categories” uses category-quarters *not* published on the WSJ; “Off-quarter” repeats the analysis but shifts it by 1 and 2 months from the beginning of a calendar quarter; “11 month ranking” constructs the fund rankings based on 11 (rather than 12) month return. Values in (brackets) are p-values on the difference being 0 (no discontinuity). Panel B reports the results of conducting the baseline discontinuity test in Next Q Flow at various possible cutoffs. For each cutoff  $X$ , we report the difference in fitted values at  $(X + 0.5)$  based on local linear kernel regressions from the left and right.

<sup>a</sup> LATE (local average treatment effect) - the magnitude of the discontinuity.

\*\* Significant at the 5 percent level.



TABLE 4—CHANGES IN FUND INVESTMENT BEHAVIOR DURING LAST MONTH OF A RANKING PERIOD

Panel A - Full Sample					
	[1,4]	[5,8]	[9,12]	[13,16]	[17,20]
$\Delta TE$	0.035 (0.985)	-0.716 (0.560)	1.777** (0.031)	0.194 (0.773)	0.028 (0.965)
$\Delta TESP$	-1.109 (0.572)	-2.064 (0.140)	0.237 (0.822)	0.137 (0.877)	-0.910 (0.272)
$\Delta VOL$	-1.182 (0.657)	-1.455 (0.418)	-1.251 (0.360)	-0.512 (0.653)	-0.714 (0.504)
Panel B - Top vs. Bottom Q1 performers					
	[1,4]	[5,8]	[9,12]	[13,16]	[17,20]
$\Delta TE$ - Top	0.049 (0.979)	-0.087 (0.953)	3.016*** (0.005)	2.571** (0.025)	0.645 (0.536)
$\Delta TE$ - Bottom	3.956 (0.269)	-0.283 (0.891)	0.208 (0.895)	-0.149 (0.904)	-0.724 (0.546)
Panel C - Top vs. Bottom M4-M10 performers					
	[1,4]	[5,8]	[9,12]	[13,16]	[17,20]
$\Delta TE$ - Top	-1.840 (0.254)	-1.103 (0.257)	0.801 (0.308)	-0.152 (0.835)	-0.010 (0.990)
$\Delta TE$ - Bottom	0.530 (0.949)	0.575 (0.929)	9.144** (0.024)	1.658 (0.630)	2.549 (0.392)

*Note:* Panel A reports changes, between month 11 and month 12 of a ranking period, in: average tracking error volatility w.r.t the category portfolio ( $\Delta TE$ ); average tracking error volatility w.r.t the S&P500 portfolio ( $\Delta TESP$ ); volatility of fund returns ( $\Delta VOL$ ). Funds are grouped in portfolios of 4 by rank. Tracking error is defined as the standard deviation of the difference between the daily return of the 4 fund portfolio and that of the respective baseline portfolio (fund category or S&P500). Panel B reports the results of repeating the tracking error volatility analysis separately for the top half and bottom half of funds based on their return in the first quarter of a ranking period (months 1-3). Panel C repeats the analysis but splits the sample to top three-quarters versus bottom quarter of funds based on their return in months 4 to 10 of a ranking period.

\*\*\* Significant at the 1 percent level.

\*\* Significant at the 5 percent level.

TABLE 5—CHARACTERISTICS AFFECTING INCREASED CAPITAL FLOW INTO FUNDS

Panel A - Independently controlling for each characteristic									
	Size	Age	Exp Ratio	CplxSize	MS Rating	DC Ratio	I[Broker]		
Diff $\Gamma_2$	-0.662** (0.027)	-0.093*** (0.010)	0.599 (0.592)	-0.413 (0.279)	1.301** (0.045)	-0.083** (0.017)	-0.279 (0.802)		
Panel B - Simultaneously controlling for characteristics									
	Size	Age	Exp Ratio	CplxSize	MS Rating	DC Ratio	I[Broker]	CplxSize * Size	CplxSize * Age
Diff $\Gamma_2$	-0.717* (0.053)	-0.045 (0.169)	-0.261 (0.823)	0.101 (0.787)	1.123* (0.091)		-1.788* (0.086)		
Diff $\Gamma_2$	-0.469 (0.137)	-0.063* (0.058)		-2.925* (0.055)				0.447** (0.027)	-0.026 (0.252)

*Note:* For each mutual fund characteristic we compute the two one sided local linear kernel regressions of the form  $FlowQ_{rank,cat,q} = \Gamma_0 + \Gamma_1 * abs(rank - 10.5) + \Gamma_2 * Char_{rank,cat,q} + \epsilon_{rank,cat,q}$  (see Equation 4) and report the difference between  $\Gamma_2$  from the left and right of the  $rank = 10.5$  cutoff, along with the corresponding (p-value). This difference is the differential impact of the characteristic on fund flows during the post-publication quarter for funds just above and just below the cutoff. The characteristics are: size (log of total net assets); age (years since fund inception) ; the expense ratio charged by the fund (percent) ; the size of the fund complex the fund belongs to (log of total net assets) ; the fund Morningstar rating (number of stars); the ratio of fund assets stemming from defined contribution pension plans ; and an indicator for whether the fund is broker sold, based on the method proposed by Sun (2014) . Panel A reports the results of 7 independent estimations in which  $Char_{rank,cat,q}$  includes only one characteristic each time, and Panel B reports the results of single estimations in which several characteristics are included in  $Char_{rank,cat,q}$  simultaneously, for two different specifications.

\*\*\* Significant at the 1 percent level.

\*\* Significant at the 5 percent level.

\* Significant at the 10 percent level.

TABLE 6—SUBSEQUENT ADVERTISING AND MEDIA PUBLICATIONS

Panel A - Discontinuity tests					
	I+[Ad Size]	I+[Amount Spent]	I+[Ads Published]	I+[Rank Mentions]	I+[Media Mentions]
Fitted vs fitted at 10.5 (Diff $\Gamma_0$ )	26.461** (0.016)	20.177* (0.065)	19.522* (0.097)	4.160 (0.849)	2.625 (0.201)
Panel B - Effect of ads and media on increased flows					
	I+[Ad Size]	I+[Amount Spent]	I+[Ads Published]	I+[Rank Mentions]	I+[Media Mentions]
Diff $\Gamma_2$ - Fund-class flows	-0.023 (0.994)	-0.817 (0.827)	-0.107 (0.973)	9.328* (0.072)	3.194** (0.020)
Diff $\Gamma_2$ - Complex flows	-0.627 (0.668)	-2.764 (0.216)	-1.370 (0.320)	5.040* (0.090)	2.646** (0.012)

*Note:* Using data on actual fund advertising activity and the number of times each fund is mentioned in major U.S. news and business media outlets the quarter before and the quarter after publications, Panel A reports the results of our main discontinuity test, comparing intercepts from local linear kernel regressions from the left and right at  $rank = 10.5$ , applied to indicator variables testing whether mutual fund complexes increase advertising activity or have their funds being mentioned more in the media, comparing the pre-publication to the post-publication quarter. The advertising activity tested is: the average ad size published by the complex; the dollar amount spent on advertising by the complex; the number of ads published by the complex; the number of times a fund's rank is mentioned in ads. Panel B report results of testing the effect these activities have on the magnitude of the discontinuity in capital flows, at both the published fund-class level and the complex level. For each activity we compute the two one sided local linear kernel regressions of the form  $FlowQ_{rank,cat,q} = \Gamma_0 + \Gamma_1 * abs(rank - 10.5) + \Gamma_2 * Act_{rank,cat,q} + \epsilon_{rank,cat,q}$  (see Equation 4) and report the difference between  $\Gamma_2$  from the left and right of the  $rank = 10.5$  cutoff, along with the corresponding (p-value). This difference is the differential impact of the increased activity on fund flows during the post-publication quarter for funds just above and just below the cutoff. When using complex flows, if a complex appears more than once in the rankings at the same quarter, only one of the occurrences is kept, at random, to avoid biasing the standard errors.

\*\* Significant at the 5 percent level.

\* Significant at the 10 percent level.

TABLE 7—DETERMINANTS OF SPILLOVER FLOWS

	Fund Size	Fund Age	Fund Exp Ratio	Complex TNA	Funds in Complex	I <sup>+</sup> [Rank Mentions]
Diff $\Gamma_2$ - Spillover flows	-0.246 (0.267)	-0.042 (0.284)	1.779** (0.041)	-0.838*** (0.001)	-1.494*** (0.002)	3.170* (0.073)

*Note:* This table reports the effect of several fund and complex characteristics and activities on the magnitude of the discontinuity in spillover capital flows, into all funds of the complex other than the fund published. It reports results for: the size, age and expense ratio of the published fund, the size of the complex in terms of aggregate TNA and the number of funds in the complex (both standardized), and an indicator variable for complexes increasing the number of times funds' rank is mentioned in advertising. For each of these, we compute the two one sided local linear kernel regressions of the form  $FlowQ_{rank,cat,q} = \Gamma_0 + \Gamma_1 * abs(rank - 10.5) + \Gamma_2 * Act_{rank,cat,q} + \epsilon_{rank,cat,q}$  (see Equation 4) and report the difference between  $\Gamma_2$  from the left and right of the  $rank = 10.5$  cutoff, along with the corresponding (p-value). This difference is the differential impact of the increased characteristic or activity on spillover flows during the post-publication quarter. If a complex appears more than once in the rankings at the same quarter, only one of the occurrences is kept, at random, to avoid biasing the standard errors.

\*\*\* Significant at the 1 percent level.

\*\* Significant at the 5 percent level.

\* Significant at the 10 percent level.

TABLE 8—VARIANCE DECOMPOSITION OF  $\Delta$ IN/OUT-FLOWS

	$\Delta$ inflow	$\Delta$ outflow	I* $\Delta$ inflow	I* $\Delta$ outflow	N
Non-ranking months	0.738 [0.059]	0.262 [0.059]			77
Ranking months	0.947 [0.026]	0.053 [0.026]			60
All months	0.510 [0.033]	0.087 [0.032]	0.369 [0.053]	0.029 [0.013]	137

*Note:* Every month, we calculate the difference in next quarter inflows, outflows and netflows=inflows-outflows between mutual funds ranked 10 and 11 based on previous 12 month return. We use the regression  $\Delta netflow_{10-11,cat,q} = \beta_0 + \beta_1 \Delta inflow_{10-11,cat,q} - \beta_2 \Delta outflow_{10-11,cat,q} + \epsilon_{cat,q}$  and report the estimated variance decomposition of the regression, based on Shorrocks-Shapley factor decomposition, along with bootstrapped [standard errors]. The first row reports variance decomposition results for months in which a WSJ ranking was *not* published, while the second row reports results for months in which the WSJ published its rankings. The third row reports results of variance decomposition for a regression which includes both ranking and non-ranking months, and adds interaction terms with an indicator variable which equals 1 for ranking months. Data is from mutual fund N-SAR reports downloaded from the Security and Exchange Commission's EDGAR database. N is the number of category-quarter observations for which we have sufficient data to calculate the difference in next quarter flows between the funds ranked 10 and 11.

## APPENDIX

A1. *Validity of the RDD*

The literature discussing RDD best-practices (e.g. Hahn, Todd and Van-der Klaauw (2001), Imbens and Lemieux (2008), Lee and Lemieux (2010)) suggests several tests to verify the validity of an RDD. In our setting, a valid RDD requires quasi-random ranking around the cutoff. This requirement will be satisfied if funds' rankings are highly volatile. Additionally, a discontinuity in any of the mutual funds' observable characteristics pre-ranking may question the validity of the RDD.

Quasi-random ranking is necessary to guarantee that differences between mutual funds just above and just below the publication threshold are caused by media attention rather than reflecting a spurious correlation. Figure A1 provides evidence of high fund ranking volatility in our data. We consider the empirical ex-post probability of being in the top-10 list by publication date conditional on the rank held by the mutual fund both a month and a day before the end of a ranking period. More than 50 percent of the time, a fund ranked 10 a month before publication will not remain in the top-10 by the time of publication, and almost 40 percent of the time, a fund ranked 11 will be part of the top-10 come publication. Even when considering daily ranking volatility, a similar pattern holds. Approximately 25 percent of the time, a fund ranked 10 at the beginning of the last ranking *day* will not be in the top-10 by the end of that day, and a fund ranked 11 at the beginning of the day will cross the publication cutoff and get published. Furthermore, Figure A2 shows that previous 12 month return, the driving variable, is remarkably smooth, as expected.

This quasi-random assignment of mutual funds around the  $rank = 10$  cutoff implies there should be no discontinuity in observable fund pre-ranking characteristics around the cutoff. To verify this predication and the validity of the quasi-random assignment assumption, Table A1 reports results of tests for discontinuity in several fund pre-ranking characteristics. We find no statistically significant evidence of a discontinuity in any of the tested characteristics, or in other unreported characteristics such as Morningstar ranking, pre-publication beta, and 12b1 fees. The findings reported in Figure A1 and Table A1 ensure that the "Local Randomization" assumption of Lee and Lemieux (2010) holds.

Finally, as is common in regression discontinuity design studies, we verify the results are not driven by the choice of bandwidth. Figure A3 presents the magnitude and significance of the discontinuity

in capital flows, based on a range of possible bandwidths. The discontinuity is significant at the 5 percent level for all bandwidths between 4 and 10, and the magnitude of the discontinuity in capital flows ranges between 2 percentage points and 3.5 percentage points the quarter after publication. Bandwidth selection does not seem to drive our results. As further robustness tests, we verify our discontinuity estimates by using the robust bias-correction RDD standard errors calculation method described by Calonico, Cattaneo and Titiunik (2012), as well as by using different kernels. Our results (unreported) are unaffected. We also note that using rank as the forcing variable guarantees similar number of observations on both sides of the cutoff and so the density test of McCrary (2008) does not apply.

#### *A2. Monthly and daily publications*

While our analysis concentrates on the “Investing in Funds” special issue, published by the WSJ at the beginning of every quarter, it is important to note that the WSJ published the 12 ranking tables during off-quarter months as well. Two important differences exist between the quarterly and off-quarter publications. First, while the quarterly publications are based on previous 12 month return, the off-quarter publications are based on year-to-date returns (the returns from the beginning of the calendar year to the current month). Second, rather than being published prominently at the beginning of the “Investing in Funds” special issue, as is the case with the quarterly publications, the off-quarter tables were published towards the end of the regular investments section of the WSJ, among the “small print” of all mutual fund data and returns.

We expect a lower response to the monthly tables due to their lower ex-ante visibility relative to the quarterly publication. Figure A4 confirms our expectation, finding no discontinuity in capital flows between funds mentioned and not mentioned on these off-quarter publications.

In addition to the ranking tables published at the beginning of every month (either in the special issue or at the back pages), the WSJ also published a daily ranking table every day. This single ranking table, published at the back pages similar to the off-quarter tables, ranks the top 10 funds within a single category, using year-to-date returns. As the category chosen for publication is dependent on editorial choice, we have no way of avoiding editorial bias in the choice of category to be published every day.

*A3. Supplementary discontinuity tests*

Finally, to investigate possible outcome effects of media attention other than an increase in next quarter capital flows, we repeat the set of discontinuity tests for several post-publication characteristics, reported in Table A2. We consider management fees and expense ratios a quarter and a year after publication and find no significant difference between published and unpublished funds.



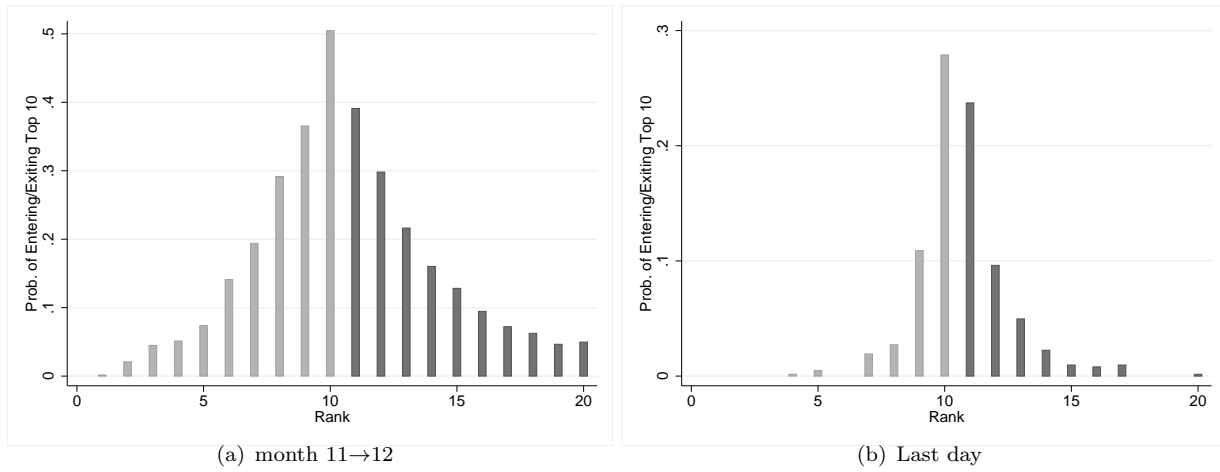


FIGURE A1. FREQUENCY OF ENTERING/EXITING TOP 10

Note: For  $rank \in [1, 10]$ , the graph depicts the empirical probability of *not appearing* in the top 10 by publication date, conditional on holding that rank a month (a) or a day (b) before the end of a ranking period. For  $rank \in [11, 20]$ , the graph depicts the probability of *appearing* in the top 10 by publication date, conditional on holding that rank a month (a) or a day (b) before the end of a ranking period.

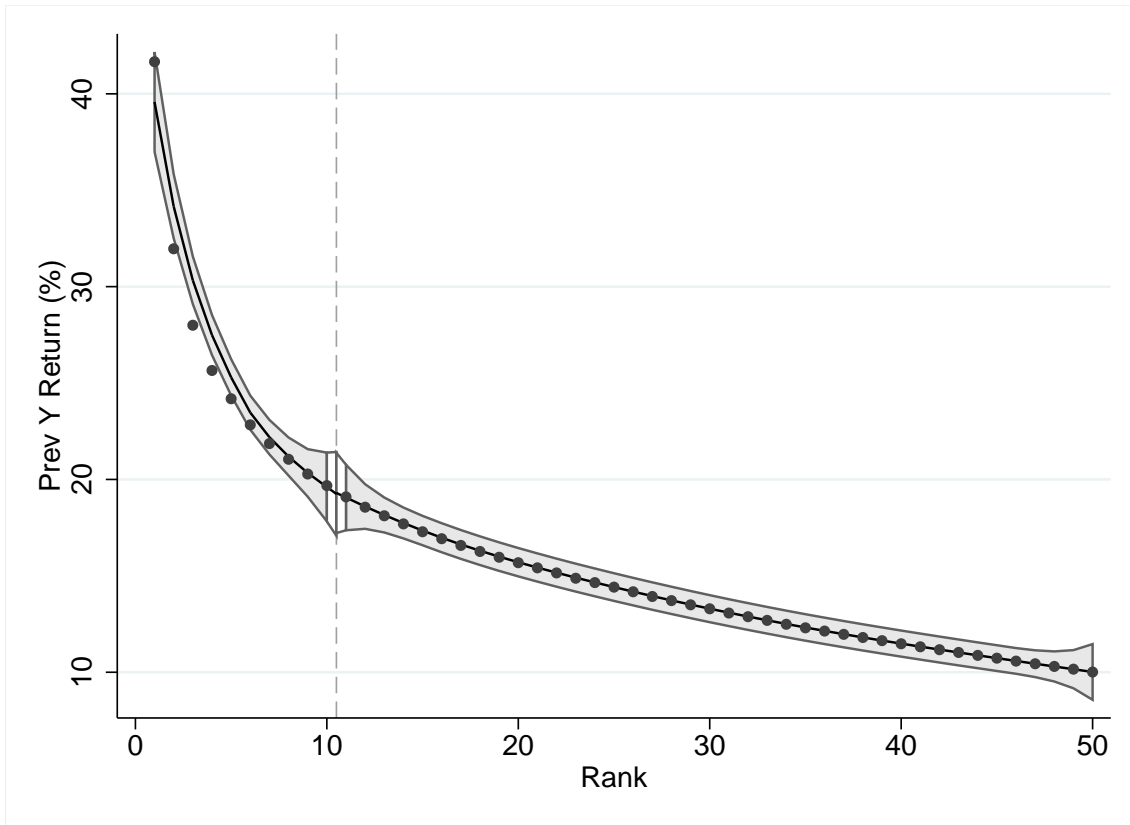


FIGURE A2. RDD ANALYSIS OF PRE-RANKING RETURNS BY RANK

*Note:* Mutual funds are ranked based on previous 12 month return within their investment category at the beginning of every quarter from 2000Q1 to 2012Q4 using data from CRSP. The figure depicts the two one-sided local linear kernel regressions of the previous 12 month returns on the funds' WSJ ranks.

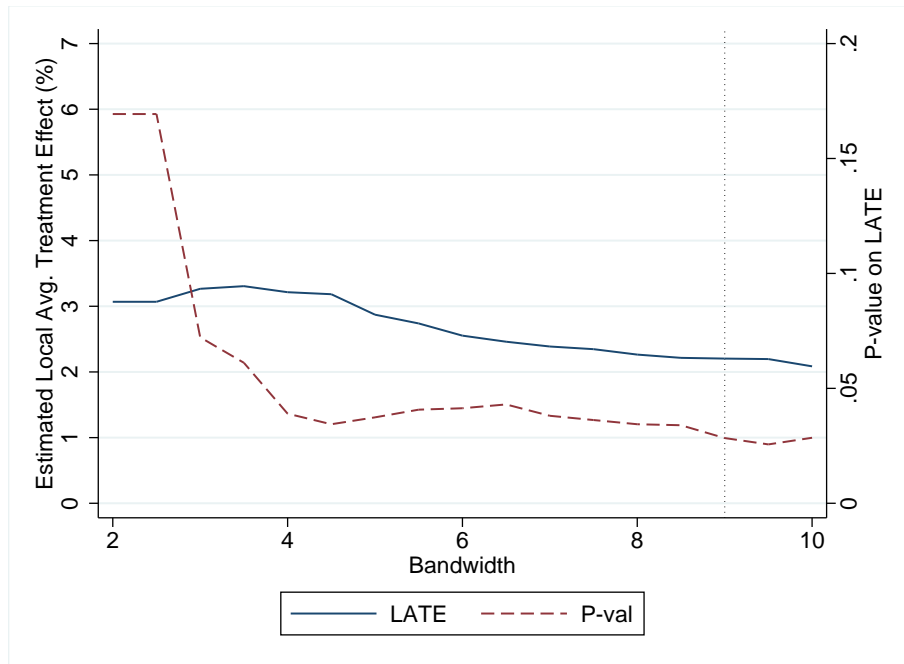


FIGURE A3. EFFECTS OF BANDWIDTH ON DISCONTINUITY ESTIMATION

*Note:* This figure presents the magnitude and significance of discontinuity in capital flows as a function of the bandwidth used in local linear kernel regressions of flows on ranks around the  $rank = 10.5$  cutoff. The vertical dotted line is at the actual bandwidth used. The actual bandwidth was chosen based on the optimal bandwidth estimator of Guido W. Imbens and Karthik Kalyanaraman (2012).

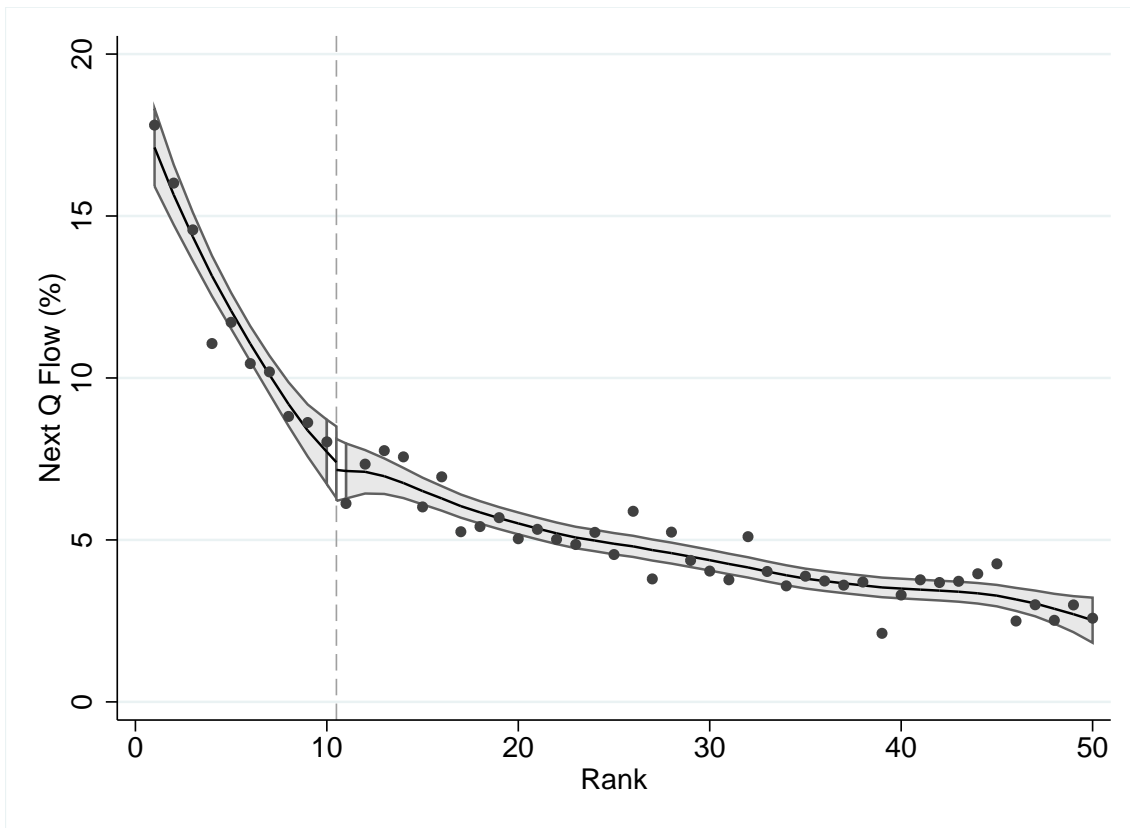


FIGURE A4. RDD ANALYSIS OF MONTHLY WSJ PUBLICATIONS

*Note:* Mutual funds are ranked based on *year-to-date* return within their investment category at the beginning of every off-quarter month from 2000M2 to 2012M12 using data from CRSP. The figure depicts the two one-sided local linear kernel regressions of the next 3 month flows on the funds' WSJ ranks.

TABLE A1—DISCONTINUITY TEST FOR FUND CHARACTERISTICS

	TNA	Fund age	Exp. ratio	Mgmt. fee	Front load
Fitted vs fitted at 10.5	-26.691 (0.822)	-0.278 (0.479)	-0.008 (0.685)	-0.003 (0.246)	-0.178 (0.228)
Fitted vs fitted w/ controls	14.100 (0.900)	-0.360 (0.334)	-0.016 (0.431)	-0.003 (0.290)	-0.179 (0.228)
Actual 10 vs actual 11	-157.909 (0.332)	-0.946 (0.133)	0.015 (0.599)	-0.001 (0.108)	-0.203 (0.396)
Fitted vs actual at 10	-100.751 (0.435)	-0.748 (0.139)	0.000 (0.994)	-0.003 (0.220)	-0.220 (0.275)
Fitted vs actual at 11	-83.849 (0.587)	-0.476 (0.380)	0.006 (0.801)	-0.001 (0.122)	-0.161 (0.410)

*Note:* We repeat the discontinuity tests of Table 2 for several observable features of mutual funds in our sample: Total Net Assets (\$M), fund age (years), expense ratio (percent), management fee (percent), and front load fee (percent). All characteristics are measured at the end of the corresponding 12 month ranking period, before publication.

TABLE A2—DISCONTINUITY TEST FOR OTHER POSSIBLE EFFECTS

	Exp. ratio+Q	Mgmt. fee+Q	Exp. ratio+Y	Mgmt. fee+Y
Fitted vs fitted at 10.5	-0.004 (0.833)	-0.060 (0.406)	0.001 (0.957)	-0.005 (0.880)
Fitted vs fitted with controls	-0.009 (0.661)	-0.038 (0.597)	-0.005 (0.787)	0.008 (0.812)
Actual 10 vs actual 11	0.008 (0.779)	-0.112 (0.113)	0.016 (0.570)	-0.046 (0.269)
Fitted vs actual at 10	-0.005 (0.847)	-0.055 (0.380)	-0.005 (0.841)	-0.027 (0.482)
Fitted vs actual at 11	0.008 (0.729)	-0.117 (0.113)	0.022 (0.367)	-0.025 (0.526)

*Note:* This table presents discontinuity tests for several possible effects of the publication in the Wall Street Journal: the expense ratio of the fund a quarter after publication; the management fee the fund charges a quarter after publication; the expense ratio of the fund a year after publication; the management fee the fund charges a year after publication.