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

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Abstract

We exploit a novel natural experiment to establish a clear causal relation between media attention and consumer investment behavior, independent of the conveyed information. 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 substantial extra flows as well, especially in smaller complexes. There is no increase in flows when the *Wall Street Journal* publishes similar lists absent the prominence of the Category Kings labeling. We show mutual fund managers react to the incentive created by the media effect in a strategic way predicted by theory, and present evidence for the existence of propagation mechanisms including increased fund complex advertising subsequent to having a Category King and increased efficacy of subsequent fund media mentions.

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1. Introduction

It is widely accepted that information disseminated by the media informs consumer decision making in financial markets.[1] Our goal, however, is to show that *appearance in the media* impacts financial decision making, *independently* of the information conveyed. To highlight the existence of a causal relation between media attention and financial decisions, that is independent of the information conveyed, we exploit a clean natural experiment in which the *Wall Street Journal* (WSJ) has prominently 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 $rank = 10$. 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".

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 represents a hefty 31% 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[2].

We establish that the prominence of the publication and its visibility are key to driving the media effect. Similar WSJ ranking tables, based on year-to-date return, which were published monthly on a regular basis, yet less prominently, caused no significant increase in flows following publication, as depicted in Figure 1(b). The lack of increase in flows holds even when restricting to December ranking lists, which rank based on 11 month returns, as

[1] See, e.g., Peress (2014)

[2] E.g. Gruber (1996), Carhart (1997), Chevalier and Ellison (1997).

shown in Figure 1(c). Thus, the impact of the Category Kings lists on investor decisions is mostly due to their visibility and prominence, and not to their "information distillation" role (Del Guercio and Tkac (2008)). Furthermore, even after the WSJ made all rankings readily available on its website, starting 2007, thus making the ranking information readily available for all funds and not only the top 10 in each category, there was no significant decrease in the discontinuous flows garnered by the funds prominently published in the WSJ Category Kings lists.

We further show that subsequent to publication of the Category Kings lists consumers not only "chase" published funds but rather change their attitude towards the entire brand/complex: there is a sizable spill-over effect of 1.8 percentage point increase in capital flows into the other funds of the complex in the subsequent quarter. This finding is consistent with an impact of media attention and visibility on brand name recognition at the complex level, and is less consistent with a "distillation of information" channel.

The existence of a media effect on consumer financial decision making implies that fund managers payoffs resemble a call option due to the implicit asymmetric incentives induced by the extra flows[3]. 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 funds ranked near the cutoff, only those that are unlikely to be ranked as top performing funds next quarter increase tracking error volatility.

We further establish that both subsequent fund advertising and efficacy of subsequent media mentions play a role in propagating the effect. By analyzing fund complex advertising behavior and media coverage, we are able to show mutual fund complexes increase advertis-

[3]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.

ing 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. We also show that small, young funds from small complexes, which are ex-ante less visible, enjoy a higher "bang for the buck" from being published, again consistent with the importance of visibility.

In sharp contrast to the sizable effects we identify for the highly visible Category King lists, and as additional supportive evidence of the special role these lists plays, we observe no significant discontinuities in capital flows for falsification tests in which: we only examine categories which were not published in the WSJ; the analysis is shifted in time to not coincide with the Category Kings publication; the ranking is based on most recent 11 rather than 12 month return. We also show that the discontinuity at $rank = 10$ for the Category King lists is unique and does not exist for other plausible cutoffs.

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.

2. Related Literature

The existence of a pure media effect is a natural theoretical 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).[4]

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% of U.S. households with income above \$ 50,000 owned mutual funds.[5] Finally, for 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.[6]

Several authors examine and establish the correlation between media attention and 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.[7] 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. 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

[4] See Corwin and Coughenour (2008) for a discussion on the impact of effort allocation due to limited attention in financial markets.

[5] Source: Investment Company Institute 2014 fact book - www.icifactbook.org

[6] 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.

[7] 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.

the Dow Jones Industrial Average and with future trading volumes on the New York Stock Exchange. Finally, Fang, Peress, and Zheng (2014) consider the impact of media coverage of stocks on mutual fund trades.

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 - an item is in the news if there is news to report. Our identification strategy is tailored to alleviate such endogeneity concerns, and allows us to focus on providing evidence showing causality.

Prior attempts to alleviate endogeneity concerns regarding the impact of media coverage have generally employed population splits in which different groups of agents are exposed to different media outlets. For example, in the literature concerning the effects of media on voter political leaning and behavior, 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. We are able to eliminate concerns regarding selection bias, by focusing on regularly appearing style categories in the WSJ top-10 ranking tables, and taking advantage of the fact that the WSJ uses a pre-specified fixed explicit algorithm to rank the funds. Thus, our setting eliminates the bias arising from the endogeneity of the decision to publish a specific media article regarding a specific investment vehicle.

One channel through which the media can affect consumer investment behavior is the "information distillation" channel proposed by Del Guercio and Tkac (2008), who use Morningstar ratings to explore the effect of ranking on fund flows. Del Guercio and Tkac (2008) conduct event studies of over $10,000$ Morningstar rating changes and show that these discrete rating changes lead to changes in mutual fund flows, above and beyond those predicted by a time-series benchmark regression of fund fundamentals. They conclude that repackaging of fund quality information into simple discrete ratings like Morningstar assists investors facing search costs to distill information easily. It is important to note, however, that as *all* star rankings are published simultaneously on Morningstar's website, no cogent discussion of the effect of pure media visibility, separate from information content, is possible under their setting. We aim to fill this gap, and are able to attribute a significant component of quarterly flows to a single day appearance in a WSJ category ranking table, while showing that similar ranking tables published less prominently do not garner similar investor response.

An important distinction between the WSJ rankings and the Morningstar ratings used by Del Guercio and Tkac (2008) is that the WSJ rankings are simply based on the past 12 months returns, and this fact is made explicitly clear in the publications. Morningstar ratings, on the other hand, are calculated using multiple return horizons (3, 5, 10 years) with opaque "proprietary" weights given to the different horizons (based on style drift), and the returns are also risk-adjusted[8]. As such, Morningstar ratings give a perception that there is an elaborate evaluation mechanism, beyond distillation of information, behind them, and are certified by the Morningstar brand. This is not the case with the WSJ rankings.

Our research also contributes to the literature that analyzes how implicit and explicit

[8] See Reuter and Zitzewitz (2013) for a detailed discussion of the Morningstar rating construction.

incentives impact fund managers' investment decisions. Brown et al. (1996) find that the ratio of fund volatility in the second part of the year to the first is higher for interim losers than for winners, and argue this is consistent with an annual tournament structure. Chevalier and Ellison (1997) show that risk taking behavior in the last quarter, measured by tracking error volatility relative to the market, is consistent with flow induced incentives implied by the performance in the first three quarters. Carhart, Kaniel, Musto, and Reed (2002) show winning funds trade to temporarily inflate their fund NAV on the very last day of the year, and argue this is to shift performance between years. Del Guercio and Tkac (2008) suggest that contrary to the implicit assumptions of Brown et al. (1996) and Chevalier and Ellison (1997), this tournament behavior is much more of an ongoing and high frequency tournament, which we are able to confirm in our tests.

Our evidence suggests fund managers are well aware of the impact on fund flows of making the top 10 lists. Furthermore, we show they 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. Even more striking, we show that managers understand and react to the trade-off 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 diversionary risk-shifting. Finally, we further contribute to the literature by showing that in addition to the ex-ante impact on investment decisions, ex-post family advertising is impacted by having a fund in the Category King lists, and this impact is stronger for funds unlikely to be in a similar position next quarter.

3. Data and Summary Statistics

We utilize data from the following sources: *Wall Street Journal* Category Kings tables - lists of top 10 mutual funds by category appearing in a special quarterly section of the journal, as well as monthly tables appearing during within-quarter months at the back pages of the journal ; Center for Research in Security Prices (CRSP) - returns, monthly flows, and other fund characteristics ; Trimtabs - daily flows ; Kantar Media - fund complex advertising ; Factiva - print media coverage ; Morningstar - star ratings. Below we describe how these are used.

We consider 52 quarters, from 2000Q1 to 2012Q4, in which the "Investing in funds - A quarterly analysis" section was published in the *WSJ*.[9] 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}×{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.[10] 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. We note that during within-quarter months, in which the special issue "Investing in funds" was not published, the WSJ nevertheless published similar ranking tables. These within-quarter tables were published much less prominently in the back pages of the newspaper, along with the fund quotes, and the ranking was based on year-to-date returns rather than previous 12 month returns. We use data on within-quarter months to compare high vs. low visibility publications and establish the importance of prominence. 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

[9]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.

[10]Over 200 Lipper sectors exist. 39 sectors appear at least once in the rankings.

period, category 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 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% of the time.[11] In the analysis to follow we replace the CRSP-based top 10 with the actual published top 10, though our results are nearly identical when using the CRSP-based list without a top 10 correction. This robustness helps alleviate concerns regarding replication accuracy and its effects on reported results.

One mutual fund characteristic not available on CRSP but required for some of our analysis is the *Morningstar* star rating of each fund, assigned for funds with at least 3 years of return history. We obtain the historical star ratings directly from Morningstar.

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.

[11] Such that a typical CRSP-based top 10 list will contain approximately 9 of the 10 funds mentioned on the WSJ.

One possible propagation mechanism we consider is direct mutual fund advertising. To that end, we obtain from Kantar Media, an advertising consulting firm, 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.[12] 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 be related to mutual funds, and accounting for the fact 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 whether subsequent media coverage plays a propagation mechanism role, we construct a dataset containing the number of times each mutual fund was mentioned in 89 major US news and business publications[13]. 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 five percent of fund share classes at the beginning of the inspected period to approximately 20% 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. While the exact publication date in the WSJ varies within the

[12]A similar dataset, albeit using a shorter time period, is used by Phillips, Pukthuanthong, and Rau (2013); they provide a useful Appendix describing the dataset further.

[13]List of publications as defined by the Factiva category of the same name. Full list available from the authors upon request.

first week of the publication quarter, the flow variables constructed from CRSP consider the entire quarter. The TrimTabs daily flow data also helps us verify our results are not driven by the fact that these few pre-publication days are included in the CRSP flow calculations.

4. Empirical Strategy and Results

4.1. Media exposure causes increased investment

The foundation of our empirical strategy is a comparison between capital flows into published and unpublished mutual funds using a regression discontinuity design (RDD).[14] A significant discontinuity in capital flows during the post-publication quarter will indicate that media exposure has a causal effect on consumers' investment behavior.

Capital flows into fund i during quarter q are defined as percent increase in the fund's assets beyond asset appreciation:

$$Flow^i_{q,q+1} = \frac{TNA^i_{q+1} - TNA^i_q(1 + R^i_{q,q+1})}{TNA^i_q} \tag{1}$$

in which TNA^i_q is the total net assets of fund i at the beginning of quarter q, and $R^i_{q,q+1}$ is the return on the fund's assets between the beginning of quarter q and the beginning of quarter $q + 1$.[15] All flows are winsorized at the one percent level to decrease the effect of outliers.

The independent variable for the RDD is a 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

[14] Reuter and Zitzewitz (2013) use a methodology somewhat 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.

[15] Our results are nearly identical when using the alternative measure, $Flow^i_{q,q+1} = (TNA^i_{q+1} - TNA^i_q(1 + R^i_{q,q+1}))/(TNA^i_q(1+R^i_{q,q+1}))$. 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.

extrapolation error is to use *cutoff* = 10.5. To find the predicted value of capital flows at 10.5, we employ a local linear kernel regression (LLR) around the cutoff, as advocated by the extant RDD literature.[16]

The basic LLR equation is:

$$FlowQ_{rank,cat,q} = \alpha_0 + \alpha_1 \times D + \beta_0 \times (rank - cutoff) + \beta_1 \times D \times (cutoff - rank) + \epsilon_{rank,cat,q} \quad (2)$$

in which $D = 1$ if $rank < 10.5$ and $D = 0$ otherwise, and $FlowQ_{rank,cat,q}$ is percent capital flow during quarter q (the quarter subsequent to publication) into the fund ranked *rank* within category *cat*. The discontinuity is thus the value of α_1. It is important to note that LLR are not estimated by using OLS but by using weighted least squares (WLS), with the weights defined by the kernel and bandwidth used. We determine optimal bandwidth using the 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, as in the RDD case.[17] Using WLS ensures observations closer to the discontinuity (e.g. ranks 9-12) have a stronger effect on the resulting estimation than observations further away (e.g. ranks 4-5 and 16-17). Specifically, for next quarter flow estimation in our base setting, the optimal bandwidth is estimated to be 9, which means the observations with ranks 1 and 20 are not included in the estimation and observations with ranks 2 and 19 have about 6% of the weight of observations with ranks 10 and 11.

Formal discontinuity tests are reported in Table 2. We perform 5 types of discontinuity tests. Our main test uses Equation 2 to compute the discontinuity using a local linear kernel regression. The second test repeats this analysis but adds controls for fund size, age, expense ratio, Morningstar rating and fund return during the last quarter before publication,

[16] 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.

[17] In appendix Figure A.3, we show our discontinuity result is robust to the choice of bandwidth. In unreported results, we also verify robustness to different choices of kernel.

to control for any abnormal flows stemming from recent high returns.[18] The third test is a Z-test for difference in mean capital flow between funds ranked 10 and funds ranked 11. The fourth test compares the mean capital flow on one side of the discontinuity, at $rank = 10$, with an extrapolation of the data trend from the other side of the discontinuity (the data with $rank > 10$). The extrapolated value at $rank = 10$ using the data with $rank > 10$ is obtained by computing an LLR regression using Equation 2, in which $cutoff = 10$. In such a regression, α_0 will capture the predicted value from the right (i.e., using the data with $rank > 10$) at $rank = 10$. The fifth test is similar, but compares mean capital flow at $rank = 11$ with $\alpha_0 + \alpha_1$, the predicted value from the left (i.e. using data with $rank < 11$) at $rank = 11$ of an LLR regression using Equation 2, in which $cutoff = 11$.

All five tests yield statistically significant discontinuity in capital flows around the cutoff.[19] Our main test indicates a local average increase in fund capital flows of 2.2 percentage points, representing a 31% 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, or any significant discontinuity in mutual fund returns during the post-publication quarter that may indicate the existence of scale diseconomies.[20]

A graphical view of the main test is presented in Figure 1(a). The plot in this figure is constructed using two non-parametric kernel regressions, from the right and left, with the same kernel and bandwidth as in the tabulated result. This procedure guarantees that the tabulated result obtained using Equation 2 and the discontinuity value derived from the figure at $rank = 10.5$ are precisely the same. It is evident that capital flows into mutual funds during the post-publication quarter exhibit a clear discontinuous increase.[21]

[18] The precise method of using controls in LLR is described by Equation 4 below.

[19] We verify they remain significant when using heteroscedasticity robust s.e., jackknifed s.e., and s.e. clustered by quarter.

[20] 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.

[21] While for a convex function an RDD methodology could result in differential slopes on the two sides of the discontinuity, due to the fact that observations on one side of the cutoff are essentially ignored when computing the slope on the other side, the two slopes in the figure are statistically indistinguishable.

These results comprise clear evidence of a causal link from media attention to consumer investment behavior. Unlike previous work trying to ascertain such a causal link, these results do not depend on population splits in which different sub-groups are exposed to different media. The fact that the WSJ ranking tables are published every quarter and their content is "algorithmically" (but quasi-randomly) determined means we require no additional controls for consumer characteristics or controls capturing the decision of the media outlet to publish specific articles.

4.2. Information vs. prominence channels

The results of Section 4.1 show that appearing in the media causes increased flows into those mutual funds which were mentioned, but these results do not delineate the role of different possible channels in causing changes in investor behavior. We turn next to evidence that highlights the central role of visibility and prominence in giving rise to this effect, distinct from an informational channel.

We begin by noting that *all* the information provided in the publication was publicly available prior to publication. As mutual fund returns and their classifications into categories are widely available, any interested party could create the ranking tables in advance of publication. Hence, no new information is being provided by the publication. The "information distillation" channel proposed by Del Guercio and Tkac (2008) remains, however, a possible candidate channel. As search is costly, the role of the ranking tables in distilling information into discrete ratings and decreasing the information acquisition cost for investors is likely to be an important causality channel. However, we show that the pure media visibility channel we identify is distinct from the "information distillation" channel.

A useful feature of our empirical setting is the fact that similar ranking tables were also published by the WSJ for within-quarter months (months that do not follow the end of a calendar quarter), in which the special issue "Investing in funds - a quarterly analysis" was not published. These ranking tables were published less prominently, in the back pages of the regular section of the newspaper. This feature allows us to test whether the prominence of the publication and the media visibility it garners are the key drivers of the increased flows, by testing whether delivering a similarly distilled ranking table but with low prominence still causes changes to investment behavior.

Panel A of Table 3 presents the results of this analysis. We replicate the published within-quarter ranking tables and analyze flows into funds during the three months post publication. In the first line of Panel A, we find no significant increase in flows following the publication of the within-quarter months rankings. A distinction between the Category Kings and the within-quarter rankings is that the within-quarter rankings are based on year-to-date rather than 12 month returns. The rankings published during December are the most closely correlated to the rankings published during the subsequent January in the WSJ special issue. We also separately analyze the ranking tables published every December and based on January to November returns, in the second line of the panel. Here too, we find no evidence of discontinuity in capital flows into mentioned mutual funds during the three months post-publication. A graphical view of the within-quarter-based results is presented in Figures 1(b) and 1(c), and it is evident no discontinuity exists for both these settings, in stark contrast to the discontinuity at Figure 1(a).

A second feature of the empirical setup useful in testing the prominence hypothesis is the fact that in addition to publishing the "Category Kings" tables the WSJ made all rankings (and not just the top 10) readily available on its website starting 2007. Importantly, while one could get to these rankings with a few clicks of the mouse, they were not in the highly visible independent section "Investing in funds - A quarterly analysis", under the heading "Category Kings". Hence, this change affects information availability (e.g. WSJ rankings now have similar availability to that of Morningstar rankings), though not the information visibility (funds ranked 1-10 are still much more visible due to the publication of the special issue). We cannot reject the hypothesis that the discontinuities pre- and post- 2007 are the same (p=0.762), with the post-2007 discontinuity being less than 10% (0.19 pp) smaller than the pre-2007 one. This result shows the effect was not weakened by better availability of information regarding mutual fund rankings over time, similar to the results of Phillips et al. (2013).

The analysis so far was conducted at the fund-class level, as this is the unit of observation used in the WSJ ranking tables. The fourth column of Table 2 reports the results of the five discontinuity tests described in Section 4.1 but using capital flows into all share classes of the fund-class which appeared in the prominent quarterly "Category Kings" rankings, as the dependent variable. We find a significant discontinuity in aggregate capital flows into all share classes of the published funds. This is to be expected, as different fund share classes typically only differ in their fee structure, but acts as further evidence for the robustness of the causal effect of media visibility.

The fifth column of Table 2 uses flows into all other funds within the same complex as the fund-class which appeared in the WSJ rankings, *excluding the published fund.* Strikingly, we find a significant discontinuity of 1.8 percentage points in the aggregate capital flows into all other funds of the same fund complex during the quarter after publication of the prominent rankings tables.[22] To put our finding in perspective, we note that focusing on monthly flows Nanda, Wang, and Zheng (2004) find a 4.4% (on an annual basis) increase in flows into complexes of "star" funds. We find a 7.3% increase (on an annual basis) in flows to the rest of the complex. This significant spillover effect is consistent with an impact of media attention on brand name recognition at the complex level, and is less consistent with a "distillation of information" channel, as we explicitly exclude the fund for which such distillation occurred from the flow calculation.[23]

The analysis above indicates that media exposure (visibility) indeed has a role distinct from that of information dissemination (availability of distilled information) in creating in-

[22] Aggregate capital flows to the rest of the complex 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.

[23] For a discussion of information spillover between related products, see Hendricks and Sorensen (2009).

creased capital flows. We note that this does not mean well distilled information is unimportant to generating flows. The WSJ rankings do distill information to investors by summarizing how funds' 12 month returns stack up relative to the category into a simple discrete rank. But it is the *prominence* of the "Category Kings" discrete ranking lists, within the quarterly "Investing in funds" WSJ special issue, that is key to driving increased capital flows from investors to the published mutual funds and their complexes.

4.3. Discontinuity robustness

Panel B of Table 3 reports the results of three counter-factual settings, which aim to verify that the discontinuity reported in Section 4.1 is a unique feature of the data, caused by the publication and ensuing media attention. The first setting simply repeats the analysis in the first line of Table 2, but reports the discontinuity in capital flows when considers fund categories which were *not* published in that quarter's WSJ issue. The second setting uses the published categories, but constructs their rankings based on most recent 11 (rather than 12) month returns, ending at the end of a calendar quarter, and like our baseline test, considers flows during the subsequent quarter. The third setting combines the publication schedule of the within-quarter tables described in Section 4.2 with the ranking method of the quarterly "Category Kings", described in Section 4.1. Funds are ranked based on previous 12 month return, ending at the within-quarter month (e.g. February 2001 to January 2002 or March 2005 to February 2006), and we analyze flows in the subsequent 3 months (e.g. February to April 2002 or March to July 2006). We find no significant discontinuity in any of these counter-factual settings. A graphical view is available in Figure 2, and it is evident capital flows change smoothly around the cutoff, in contrast with Figure 1(a). Pre-ranking 12 month return, the variable driving the ranking, is also smooth as expected and exhibits no discontinuity around the cutoff (appendix Figure A.2).

Panel C of Table 3 reports the results of a falsification test for discontinuity in capital flows using the original setting of Table 2, but at cutoffs other than $rank = 10.5$. We expect to find no discontinuity at other cutoffs, and the results in Panel C confirm that the only statistically significant discontinuity is at $rank = 10.5$. In appendix Section A.1 we also verify that rankings are sufficiently fluid around the cutoff to satisfy the quasi-random assumption, that pre-ranking fund characteristics exhibit no discontinuity, and that the discontinuity is robust to different choices of kernel and bandwidth.

4.4. Funds' response pre-publication

Next, 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 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.[24] This incentive scheme is related to, but distinct from, the one created by the flow-return relationship, discussed by Brown et al. (1996), Chevalier and Ellison (1997), and Sirri and Tufano (1998). Finding such a managerial response to the media effect can also further increase our confidence in the validity of the discontinuity results reported in the previous section.

Theory predicts that managers of mutual funds around the publication cutoff will respond to this incentive by increasing the fund's tracking error volatility relative to its respective category; see, for example, Basak et al. (2007) and Cuoco and Kaniel (2011). Tracking error volatility captures how much a fund's investment policy deviates from a baseline portfolio, by analyzing the volatility of the difference in their daily returns. As measures based on daily returns are inherently noisy, our first step is to aggregate funds into portfolio groups consisting of four consecutive ranks within each category-quarter. We concentrate our anal-

[24] Of which 2.2 pp are due to publication, and 0.29 pp are from the return-flow relationship.

ysis on a 4-fund portfolio's tracking error volatility relative to an equal-weighted portfolio of all mutual funds in the same category, though we also consider S&P500 as an alternative baseline portfolio.

We denote the return on day t of an equal weighted portfolio of funds ranked $i, i+1, i+2$, and $i+3$ by $r_t^{[i,i+3]}$, for example $r_t^{[9,12]}$. The tracking error volatility for a 4-fund portfolio during month m relative to the baseline category portfolio *cat* is defined as:

$$TE_{i,cat,m} = \text{StDev}\left\{ r_t^{[i,i+3]} - r_t^{cat} \mid t \in m \right\} \tag{3}$$

where StDev represents the standard deviation operator. A significant increase in the TE measure will indicate that managers are attempting to increase ranking volatility, and the expected payoff of the call-like option they hold, by "deviating from the herd".

To uncover such a possible increase, we first rank funds within each category based on the first 11 months of a ranking period (e.g. returns from January 2000 to November 2000), and calculate the 4-fund TE measures during month 11 (pre-formation) and during month 12 (post-formation). We demean the TE measure relative to the average TE of all 4-fund portfolios within that category-month, to remove the effects of market- or portfolio-wide volatility within that month:

$$\widehat{TE}_{i,cat,m} = TE_{i,cat,m} - \text{Mean}_j(TE_{j,cat,m})$$

with Mean_j denoting the average across all 4-fund groups. We finally calculate the average difference per 4-fund group $[i, i+3]$, between the $\widehat{TE}$ during month 12 and during month 11, across all categories and ranking periods.

The first row of Panel A in Table 4 lists these average differences per 4-fund group. As predicted, there is a large and statistically significant increase in tracking error relative to the category portfolio for funds close to the cutoff. We also report changes in: tracking error volatility relative to the S&P500 Index (TESP) ; the volatility of funds' daily returns

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

An interesting feature of our empirical setting is the repeated quarterly publication based on previous 12 month returns. This feature implies that a fund which enjoys exceptionally high returns in a given quarter benefits from these exceptional returns, in terms of the likelihood of being published in the WSJ, during the subsequent four top 10 ranking publications, before that quarter is no longer accounted for. As discussed by Phillips et al. (2013), investors do not differentiate between new and stale information components of fund performance. A fund manager considering engaging in risk-shifting behavior needs to weigh the benefit of potentially rising in the rank and making the list this quarter versus the distortions induced on fund allocations. Funds close to the ranking list cutoff that have their best quarter about to "expire" have the highest incentive to engage in such risk-shifting, as they are unlikely to be in a position to make the list next quarter.[25]

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. Strikingly, only funds in the top first quarter return group exhibit TE increase, at rank groups $[9, 12]$ and $[13, 16]$.[26] The stark differences between the top and bottom groups in Panel B strongly support our hypothesis regarding strategic risk-shifting behavior.

To verify these results are not driven by a systematic difference between the funds in terms of Morningstar ratings, we also attempt to split the sample into high-rated funds (5 or 4 stars, 5,116 observations) and low-rated funds (3 or less stars, 3,754 observation). In unreported results, we find no significant difference in risk taking behavior between these

[25]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.

[26]The TE increase for each of these top rank groups is also statistically different from the corresponding bottom groups.

groups. This also holds when considering only funds with 5 Morningstar stars to be high-rated, or when considering only the sub-sample of high Q1 performers and splitting it based on Morningstar rankings.

While it may seem that changing tracking error volatility over one month will do little to help managers increase their returns as measured over 12 months and assist them in entering the rankings, it is important to note that rankings around the cutoff are extremely fluid. Even minute changes in returns can affect the probability of a fund being published, as can be seen in appendix Figure A.1. For example, a fund which started **the last day** of a 12 month ranking period with $rank = 10$ ($rank = 9$) has more than 25% (10%) chance of ending that day (and the entire 12 month ranking period) with $rank > 10$, thus not being published. A fund which started that day with $rank = 11$ ($rank = 12$) has about 20% (10%) chance of ending that day within the top 10 and being published. Our empirical strategy benefits greatly from this fact, as it ensures strong local randomization around the cutoff - a necessary feature for a strongly valid RDD. This fluidity of ranking is also the reason why tracking error changes during the last month help the managers increase the likeliness of moving their ranking measured over 12 months. For robustness, Panel C of Table 4 presents tests of TE changes during the last 2 months of a ranking period, in which funds are ranked based on the first 10 months of a ranking period, and we calculate 4-fund TE measures during months 9-10 (pre-formation) and during months 11-12 (post-formation). We again find significant changes only for the $[9, 12]$ group. Finally, we find that funds about to lose a strong first quarter, which are those shown to engage in risk-shifting the most, are also 2.3% more likely (p=0.091) to be published conditional on being within the $[9, 12]$ rank a month prior to publication, further corroborating the hypothesis that risk-shifting when close to the publication cutoff is useful in getting a fund published.

A second possible managerial response for 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. However, Duong and

Meschke (2011) show that following circulation of early versions of Carhart et al. (2002), that uncovered the practice, the Security and Exchange Commission enacted enforcement actions against mutual funds participating in this practice, and consequently it has disappeared among mutual funds post-2001. Given that our sample starts in 2000, we lack sufficient data to distinguish any changes in increased leaning for the tape behavior for funds close to the cutoff.

4.5. Media effect propagation post-publication

In Section 4.4 we provided evidence that the presence of the media effect impacts funds' trading strategies prior to publication. We now consider the way in which the media effect propagates post publication and causes the increased flows. This is done both to provide more texture to the results so far, and to ascertain that observed propagation patterns are consistent with the visibility and prominence findings.

4.5.1. Timing of flow increase

We start by examining the duration of the media effect within the post-publication quarter to shed light on consumer behavior in response to the media stimuli, and whether consumers have an immediate or protracted response to the media stimuli. Figure 3(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 3(b) presents the difference in flows after removing funds which were published in the following quarter's Category Kings lists. 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 3(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 3 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 protracted propagation of the media effect, in which the WSJ publication either changes investor perceptions and attitudes towards the published funds and their complexes, later leading to investment, or is a first step in a causal chain, followed by other investor stimuli leading to purchase. We discuss evidence supporting both these channels below.

4.5.2. Less visible funds

Next, we examine how heterogeneity in mutual funds' sizes (in terms of assets under management) and ages (since fund inception), as well as the size of the fund complex to which the funds belong affect increased flows into published vs. unpublished funds. We hypothesize that younger and smaller funds, from smaller fund complexes, will have a higher "bang for the buck" from media visibility, as they are less visible ex-ante.

To quantify the impact of heterogeneous fund characteristics on the increase in capital flowing into the fund, we add controls to the local linear kernel regression described by Equation 2. The controlled LLR is described by:

$$\begin{aligned} FlowQ_{rank,cat,q} &= \alpha_0 + \alpha_1 \times D + \beta_0 \times (rank - cutoff) + \beta_1 \times D \times (cutoff - rank) + \\ &\quad \Gamma_0 \times \widehat{Controls}_{rank,cat,q} + \Gamma_1 \times D \times \widehat{Controls}_{rank,cat,q} + \epsilon_{rank,cat,q} \end{aligned} \tag{4}$$

with $cutoff = 10.5$, $D = 1$ if $rank < 10.5$ and $D = 0$ otherwise, and $\widehat{Controls}$ a vector of demeaned fund characteristics. The mean is calculated as a weighted average and the weights are given by the bandwidth and kernel used in the weighted regression, as described for Equation 2. This demeaning procedure is required to prevent the controls from having any intercept in the regression and skewing the results of α_0 and α_1. The differential impact of the j^{th} control variable is measured by $\Gamma_{1,j}$, and captures the difference between the effect of a characteristic (e.g. fund size) on capital flows into unpublished vs. published funds.

We consider each characteristic independently, such that $\widehat{Controls}_{rank,cat,q}$ is a scalar, and then include all characteristics simultaneously, such that $\widehat{Controls}_{rank,cat,q}$ is a vector. For ease of interpretation, all characteristics are standardized, such that the Γ_1 coefficients capture the effect of a one standard deviation change in a characteristic. Panel A of Table 5 reports the results of the independent tests for each characteristic, and indicates that a one standard deviation *decrease* in the size of a published fund yields 1.32 percentage point higher *increase* in capital flows into the published fund than a similar decrease in the size of an unpublished fund. A similar pattern holds for younger funds as well. Panel B reports the results of the test in which all three characteristics are included simultaneously, along with the interaction of fund size and complex size, and while the coefficient for fund size is no longer significant, the results in this panel indicate that smaller fund complexes also enjoy a higher "bang for the buck" from being published. We conclude that media attention amplifies the inverse relation between fund size / age / complex size and capital flows, as predicted by a visibility channel and the lower ex-ante visibility of such funds.[27]

As discussed in Section 4.2, media attention also causes an increase in complex spillover flows (capital flows into all other funds in the same complex of the published fund, excluding the one published). In Panel C of Table 5 we conduct an analysis similar to that of Panel A, but using the complex spillover flows as the dependent variable. Here too we find that ex-ante less visible complexes - smaller complexes in terms of assets under management or number of funds managed - enjoy a higher "bang for the buck" from being published. These

[27] As expected, the magnitudes of the discontinuity in both panels, measured by the α_1 coefficients, are similar to the one reported in Table 2.

facts are in line with a visibility channel in which the WSJ publication increases consumer brand awareness to the published funds and their complexes.

4.5.3. Propagation via advertising and subsequent news mentions

Finally, we investigate the impact of fund advertising post-publication, as a potential propagation mechanism of the media effect from publication to altering a financial consumer's behavior.[28] We then proceed by considering several other possible propagation mechanisms. First, increased effectiveness of fund advertising efforts, for example by being able to state the ranking on the WSJ list in ads.[29] Second, increased news coverage subsequent to appearance in a Category King list. And third, increased efficacy of subsequent news coverage in generating fund flows. We find evidence supporting most of these.

To investigate fund advertising we utilize a data set provided by Kantar Media that examines advertising behavior by mutual fund complexes using data on more than $6,600$ published mutual fund ads. The dataset includes ad size and expenditure, among other features. We extended the data 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. To analyze the frequency of post-publication news coverage and its efficacy we compiled a dataset by executing more than $75,000$ Factiva searches, each counting the number of times a fund or its ticker were mentioned in articles published in 89 major US news and business publications during the quarters before and after publication.

Panel A of Table 6 reports results using our main discontinuity test, for a specification similar to the one described by Equation 2, but with different dependent variables. 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

[28]For a discussion of mutual fund advertising behavior and effects on subsequent flows, see Jain and Wu (2000).

[29]It is possible that existing, non-advertising, marketing efforts are more intense or more effective as well.

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 27 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 a fund complex, following one of its funds appearing in a "Category Kings" ranking.

Panel B of Table 6 reports results of affecting characteristic tests, using a specification similar to the one described by Equation 4 in the previous section, but in which the dependent variable is capital flows into published mutual funds, while controlling for the fund advertising activity indicators described above. For the first three columns, testing the effect of increased advertising activity on flows, we observe no significant differential effect around the discontinuity. 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 per-ad does not change, the increase in ads published observed in Panel A will still result in increased flows. Additionally, it is possible that 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 increase in advertising activity. Results are similarly insignificant when considering the efficacy of advertising activity on spillover flows into all other funds of the fund complex, as can be seen in the first three columns of Panel C.

We further investigate whether funds that made it to the top 10 are mentioned more, relative to those which did not, by extracting fund names mentioned in complex ads and matching them with our CRSP-based ranking. Out of 910 mutual fund names mentioned in the ads, we match 693 (76%) to the CRSP rankings. Naturally, better performing funds are mentioned more: top 20 ranking funds account for only five percent of the sample but 22%

of mentions, within the regularly appearing 12 categories. More importantly, and consistent with the hypothesis that fund complexes try to utilize the appearance of their funds in the top 10 lists, within the top 10 there is a disproportionate amount of mentions to funds ranked $6-10$ relative to those ranked $1-5$: within the top 20, funds ranked $6-10$ comprise 41% of mentions, while funds ranked $1-5$ account for only 22% of mentions. Funds ranked $11-15$ and $16-20$ garner 22% and 15% of mentions, respectively. The fourth column uses an indicator variable indicating whether the complex has increased the number of times it mentions the fund's rank in its ads, following publication. We do not observe an increase in the number of times fund rank is mentioned pre- and post- publication (Panel A), but we do observe some increased efficacy of mentioning funds' WSJ ranks more, if these funds were mentioned in the WSJ rankings, as indicated by the significant coefficients on the control (Γ_1) in Panels B and C.

In the last column of Table 6 we test whether funds are mentioned more in subsequent news (non-ad) articles in the media, based on the Factiva searches, and whether such subsequent news mentions have higher efficacy in driving flows for funds which were mentioned relative to those which were not mentioned in the WSJ ranking. While we do not observe more news mentions for mentioned funds (last column of Panel A), indicating that being ranked in the WSJ does not cause more news articles to be published regarding the mentioned fund, we do observe increased efficacy of news mentions for ranked funds, both in driving fund-class flows and spillover flows into the entire complex (Panels B and C). 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) and Campbell and Keller (2003).

We also attempted to test whether being mentioned on the WSJ tables increases observed investor attention by considering the Google Search Volume Index (SVI), following Da et al. (2011). Unfortunately, Google censors results with too few searches (with an undisclosed censoring threshold), such that only 4.2% 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.

The results in this section support a channel in which the WSJ media mention is a first step in a causal chain, followed by other investor stimuli, such as exposure to mutual fund ads or exposure to news mentions, which then leads to purchase. This is again consistent with a brand-name recognition channel, but less so with a "distillation of information" channel.

5. Conclusion

We exploit a novel natural experiment to establish a clear causal link between media attention and consumer investment behavior, independent of the conveyed information. Our identification strategy precisely controls for the publication's underlying information content, overcoming the typical challenge in the literature of decoupling effects of media attention from those of potential information revelation due in part to the endogeneity of media coverage.

We show that a single mention of a fund in a prominent *Wall Street Journal* "Category Kings" ranking table, that appears once a quarter, leads to a 31% local average increase in subsequent quarterly capital flows, along with a significant spillover effect to other funds of the same complex. 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.[30] 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.[31] When also considering spillover flows to other funds in the complex, the increased fees from

[30] The mean total net assets and expense ratio for funds ranked 10 are $771M and 1.24 percentage points (pp), respectively. 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.

[31] 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.

appearance on the list amount to a sizable \$36 million[32].

Our results reveal that media attention affects flows even when it does not contribute new information, as long as it appears sufficiently prominently. We highlight the importance of prominence and visibility by considering two natural experiments, one analyzing flow response to similar WSJ rankings lists published regularly yet less visibly, and one leveraging a change in the information environment after 2007, in which all rankings, not only the top 10, were made readily available on the WSJ website. Both experiments show that prominence of the Category Kings rankings, and not the information they distill, is the key driver of increased capital flows from investors to published mutual funds: appearance on a similar yet less prominent top 10 lists does not garner additional flows; the impact of being a Category King on flows is similar pre and post 2007.

We show that the presence of extra flows, stemming from appearance in the Category Kings lists, impact fund decisions both prior to and after publication of the rankings. As predicted by theory, we find the incentive created by the extra flows due to appearance in the rankings leads funds close to the cutoff, that are unlikely to be top ranked in the subsequent quarter, to 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 volatility highlights that managers are well cognizant of trade-offs associated with this risk shifting behavior.

In investigating potential propagation mechanisms of the effect, we provide evidence showing that subsequent to a fund's appearance in the rankings the fund family increases advertising activity, where fund families with category king funds that are unlikely to be top ranked next quarter increase advertising expenditures more. Furthermore, 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

[32]The mean total net assets for the complex of a fund ranked 10 is \$24,115M

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 important roles in the effect's propagation.

Even in the 21st century, being the proclaimed King, even if only for a day, helps.

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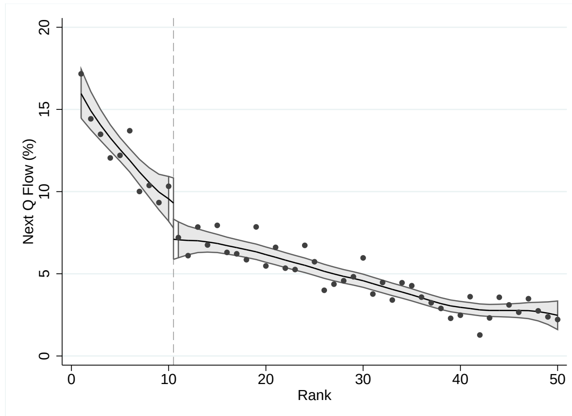
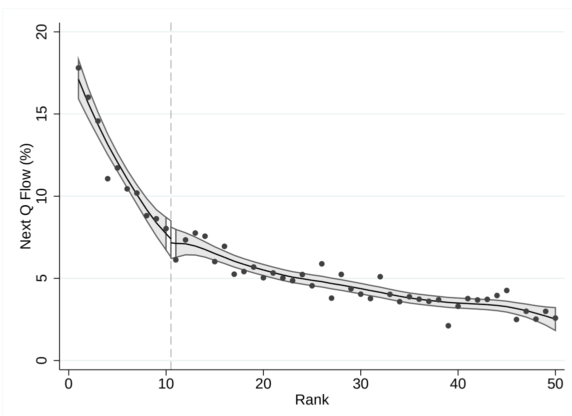
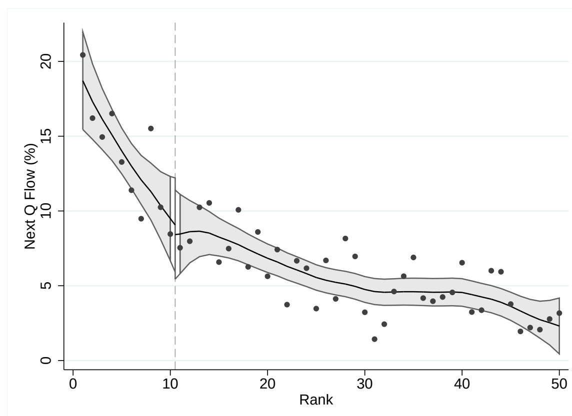


(a) Quarterly Category Kings

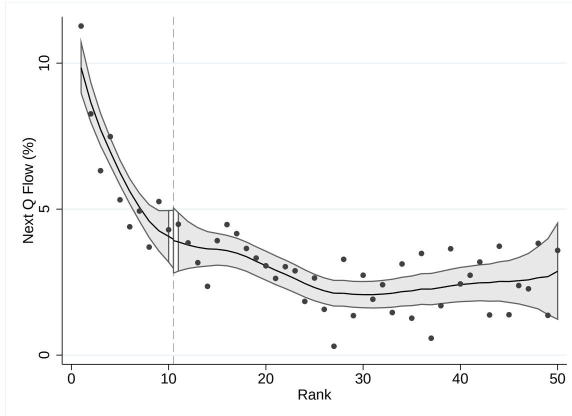


(b) All monthly YTD

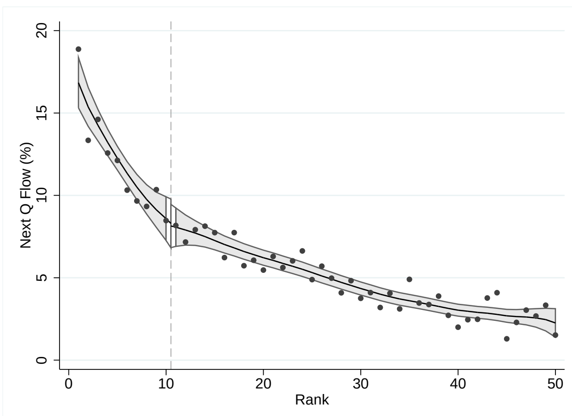


(c) December YTD

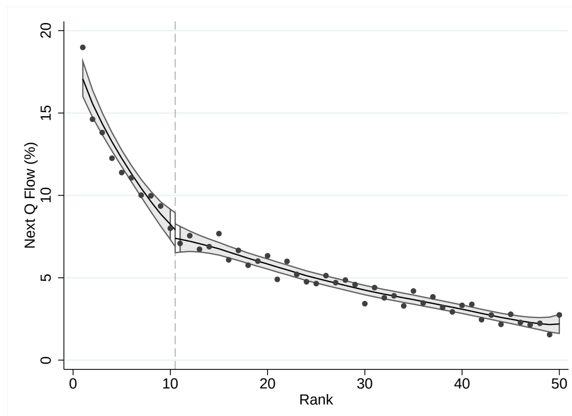
Fig. 1. RDD analysis of post-publication fund flows by rank. Each dot represents the mean net percentage capital flow into the funds at the given rank during the quarter after publication (Next Q Flow). The figures include local linear kernel regression lines and 95% confidence intervals for the segments [1,10] and [11,50]. For panel (a), funds are ranked based on most recent 12 month return within their investment category at the beginning of every quarter from 2000Q1 to 2012Q4 using data from CRSP, and each dot is the mean of 624 data points per rank, corresponding to 12 categories over 52 quarters. For panel (b), funds are ranked based on year-to-date return within their investment category at the beginning of every within-quarter month from 2000M2 to 2012M12, and each dot is the mean of 1,248 data points (two within-quarter months for each of 12 categories over 52 quarters). Panel (c) concentrates only on rankings based on January to November returns, which were published during December in the WSJ, and each dot is the mean of 156 data points. The top 10 funds are replaced with the actual funds published in the Wall Street Journal.



(a) Unpublished categories flows

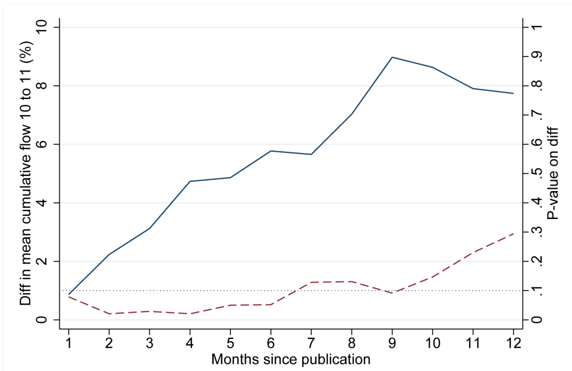


(b) 11 month ranking flows

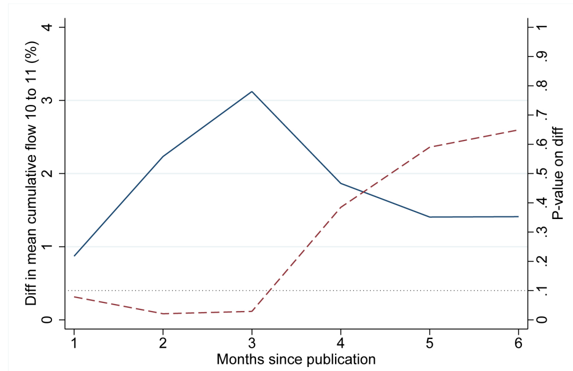


(c) Within-quarter flows

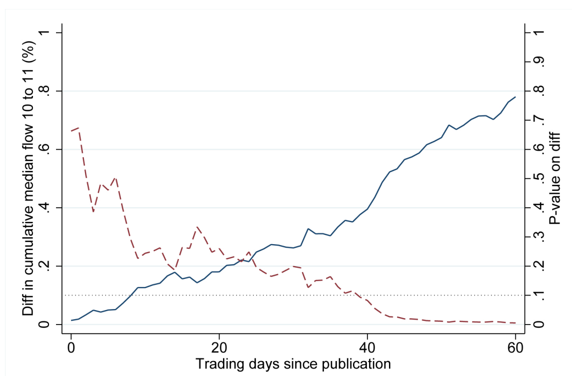
Fig. 2. RDD analysis of counter-factual settings. Each dot represents the mean net percentage capital flow into the funds at the given rank during the quarter after ranking (Next Q Flow). The figures include local linear kernel regression lines and 95% confidence intervals for the segments [1,10] and [11,50]. For panel (a), 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, but only categories which were *not* published on the WSJ that quarter are included. The number of data points per rank in panel (a) varies from $1,882$ observations for $rank = 1$ and down to 502 observations for $rank = 50$. For panel (b), funds are ranked based on the most recent *11* (rather than 12) month return within their investment category, and each dot is the mean of 624 data points per rank. For panel (c), funds are ranked based on most recent 12 month return within their investment category at the beginning of every within-quarter month from 2000M2 to 2012M12 (in which the rankings actually publishged were based on year-to-date returns), and each dot is the mean of $1,248$ data points.



(a) Full sample



(b) Excluding repeatedly published



(c) Daily

Fig. 3. Time trend of difference in capital flows into mutual funds ranked 10 and 11. 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

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, in millions), 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 percentage net capital flow into the fund, during the quarter following publication).

		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	3.047	3.717	3.636	3.635	3.544	3.168	(0.985)
	SD	1.064	1.325	1.113	1.087	1.043	0.955	
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[a])	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[a])	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	

[a] 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

This table reports the results of five discontinuity tests performed with dependant variables: Next Q Flow (percentage net capital flow into published fund class during post-publication quarter), Prev Y Return (previous 12 month return on which ranking is based), Next Q Return (return fund generated during quarter after publication), Next Q Flow entire fund (flow into all share classes of published fund), and Next Q Flow complex spillover (flow into all funds of a fund complex except published one). The first test computes a locally weighted linear regression of the dependant variable on fund rank with an indicator for $rank < 10.5$ and reports the discontinuity (coefficient on the indicator). The second test repeats this analysis but adds controls for fund size, age, expense ratio, last quarter return, and Morningstar rank. The third test is a Z-test for difference in means between funds ranked 10 and funds ranked 11. The last two tests compare actual mean at $rank = 10$ with regression predicted value from the right and actual mean at $rank = 11$ with regression predicted value from the left, using Z-tests for difference in means. Regression predicted values at 10 and 11 are calculated using locally weighted linear regressions of the dependant variable on fund rank, with the data limited to $rank > 10.5$ and $rank < 10.5$, respectively. Values in (brackets) are p-values on the coefficient 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. The last column reports N, the number of fund-quarter observations participating in the test.

	Next Q Flow	Prev Y Return	Next Q Return	Next Q Flow (entire fund)	Next Q Flow (complex spillover)	N
Discontinuity at 10.5	2.203**	-0.600	0.108	1.862*	1.820**	11,232
	(0.025)	(0.677)	(0.414)	(0.053)	(0.017)	
Discontinuity w/ controls	2.377**	0.850	0.090	1.549*	1.623**	7,880
	(0.020)	(0.259)	(0.438)	(0.092)	(0.049)	
Actual 10 vs actual 11	3.122**	0.585	0.051	3.127**	2.694**	1,248
	(0.027)	(0.707)	(0.933)	(0.022)	(0.032)	
Fitted vs actual at 10	3.223**	0.473	0.033	2.792**	2.706**	6,240
	(0.011)	(0.371)	(0.476)	(0.027)	(0.021)	
Fitted vs actual at 11	2.103*	-0.487	0.126	2.198**	1.809**	6,240
	(0.066)	(0.613)	(0.424)	(0.046)	(0.042)	

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 3

Tests for discontinuity in flows and returns in no-attention settings

Panels A and B repeat the main discontinuity test of Table 2 over the same five dependant variables for settings which lack media attention: “All YTD” analyzes flows and returns for rankings used in the monthly tables published in the WSJ during within-quarter months based on year-to-date returns; “December YTD” uses January to November returns to replicate the rankings used in the monthly tables published in the WSJ during December; “Unpublished categories” uses category-quarters *not* published on the WSJ; “Within-quarter” uses rankings based on 12 month returns ending in months which do not follow the end of a calendar quarter (and so are not published); “11 month ranking” constructs the fund rankings at the end of a quarter based on the most recent 11 (rather than 12) month return. Rankings in panel A were actually published, though with low visibility, while rankings in Panel B are counter-factual and were not published. Complex spillover correction, p-values and N are as in Table 2. Panel C reports the results of conducting the main discontinuity test for Next Q Flow in the original setting but at various possible cutoffs. For each cutoff X, we report the intercept discontinuity based on a locally weighted linear regression at $rank = (X + 0.5)$, along with its p-value.

	Next Q Flow	Prev Y Return	Next Q Return	Next Q Flow (entire fund)	Next Q Flow (complex spillover)	N
Panel A - Published YTD settings						
All YTD	0.235	-0.251	-0.041	-0.464	0.139	22,464
	(0.388)	(0.717)	(0.547)	(0.722)	(0.614)	
December YTD	0.639	-0.422	-0.079	-0.630	0.192	2,808
	(0.661)	(0.840)	(0.698)	(0.844)	(0.828)	
Panel B - Counterfactual settings						
Unpublished categories	0.043	-0.246	-0.182	0.162	-0.019	24,612
	(0.902)	(0.676)	(0.617)	(0.385)	(0.659)	
11 month ranking	0.154	-0.539	0.014	-0.752	0.011	11,232
	(0.446)	(0.676)	(0.488)	(0.661)	(0.441)	
Within-quarter	0.517	-0.639	-0.025	-0.540	0.217	22,464
	(0.252)	(0.769)	(0.529)	(0.670)	(0.619)	

Panel C - Different cutoffs

	6	7	8	9	10	11	12	13	14	15	20	50
Discontinuity	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	0.148	0.487	0.380	0.440	0.025	0.236	0.141	0.360	0.184	0.466	0.712	0.497

** Significant at the 5 percent level.

Table 4

Changes in fund investment behavior during last month of a ranking period

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 (ΔTE); average tracking error volatility w.r.t the S&P500 portfolio (ΔTESP); average volatility of fund returns (ΔVOL) ; average beta w.r.t the S&P500 portfolio (ΔBETA). Funds are ranked based on their return during the first 11 months of a ranking period, and then grouped into portfolios of 4 funds by rank (e.g. funds ranked 9 to 12 within a given category are formed into a portfolio). The tracking errors of the portfolio during the pre-formation month and the post formation month (months 11 and 12, respectively) are calculated as the standard deviations of the difference between the daily return of the 4 fund portfolio and that of the respective baseline portfolio (fund category or S&P500). The mean difference is reported, along with the respective p-value on difference from 0. Reported tracking errors are in daily percentage terms, and each value is calculated using 99,340 fund-day observations. 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 of changes in average tracking error volatility w.r.t the category portfolio (ΔTE), but forms portfolios based on 10 month return and compares tracking errors during the two months prior to portfolio formation (months 9,10) to tracking errors during the post formation months (months 11,12).

Panel A - Full Sample					
	[1,4]	[5,8]	[9,12]	[13,16]	[17,20]
ΔTE	0.035	-0.716	**1.777****	0.194	0.028
	(0.985)	(0.560)	(0.031)	(0.773)	(0.965)
$\Delta TESP$	-1.109	-2.064	0.237	0.137	-0.910
	(0.572)	(0.140)	(0.822)	(0.877)	(0.272)
ΔVOL	-1.182	-1.455	-1.251	-0.512	-0.714
	(0.657)	(0.418)	(0.360)	(0.653)	(0.504)
$\Delta BETA$	0.073	-0.024	-0.108	-0.065	0.087
	(0.898)	(0.933)	(0.756)	(0.806)	(0.753)
Panel B - Top vs. Bottom Q1 performers					
ΔTE - Top	0.049	-0.087	**3.016*****	**2.571****	0.645
	(0.979)	(0.953)	(0.005)	(0.025)	(0.536)
ΔTE - Bottom	3.956	-0.283	**0.208**	-0.149	-0.724
	(0.269)	(0.891)	(0.895)	(0.904)	(0.546)
Panel C - Two Month Change					
ΔTE	0.131	0.381	1.219*	0.716	-0.682
	(0.852)	(0.701)	(0.089)	(0.221)	(0.691)

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 5
Determinants of increased capital flows

Panel A of this table reports the results of three independant locally weighted linear regressions of Next Q Flow (percentage net capital flow into published fund class during post-publication quarter) on fund rank with an indicator for $rank < 10.5$, while controlling for: Fund Size (log total net assets of fund class), Fund Age (years since fund inception) and Complex Size (log total net assets of all funds within the complex), along with the corresponding (p-value) and N, the number of fund-quarter observations participating in the test. α_1 is the flow discontinuity (coefficient on the indicator) and Γ_1 is the differential impact of the control variable around the discontinuity (coefficient oo the interaction of the indicator and control variable). All control variables are standardized. Panel B repeats this test but controls for all control variables simultaneously withing a single regression, and adds the interaction of Size and Complex Size to the set of controls. Panel C reports the results of four independant locally weighted linear regressions of Next Q Flow complex spillover (percentage net capital flow into all funds of a published fund complex except the published one during post-publication quarter) on fund rank with an indicator for $rank < 10.5$, while controlling for Fund Size and Age, Complex Size, and the number of funds in the complex. 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.

Panel A - Next Q Flow - Independently controlled

	Fund Size	Fund Age	Complex Size
Discontinuity at 10.5 (α_1)	2.132**	1.969**	2.270**
	(0.027)	(0.038)	(0.022)
Γ_1	-1.324**	-1.134**	-0.418
	(0.036)	(0.022)	(0.282)
N	11,016	11,016	10,999

Panel B - Next Q Flow - Simultaneously controlled

α_1	Γ_1[Size]	Γ_1[Age]	Γ_1[CplxSize]	Γ_1[CplxSize*Size]	N
2.610**	-0.973	-0.810*	-1.491*	0.509*	10,999
(0.014)	(0.133)	(0.061)	(0.060)	(0.052)	

Panel C - Next Q Flow complex spillover - Independently controlled

	Fund Size	Fund Age	Complex Size	Funds in Complex
α_1	1.731**	1.683**	1.725**	1.745**
	(0.035)	(0.039)	(0.035)	(0.033)
Γ_1	-0.546	-0.374	-1.530***	-1.835***
	(0.221)	(0.347)	(0.001)	(0.002)
N	5,550	5,550	5,550	5,550

*** 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

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, Panel A reports the results of five discontinuity tests in which the dependant variables are indicator variables testing whether mutual fund complexes increase advertising activity or have their funds being mentioned more in the media, when comparing the pre-publication to the post-publication quarter (indicators on increase denoted $I^+[\ ]$), using locally weighted linear regressions of the dependant variable on fund rank with an indicator for $rank < 10.5$. Also reported are the corresponding (p-value) and N, the number of fund-quarter observations participating in the test. The advertising activities tested are: 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; the number of times the fund is mention in (non-ad) news media. The tests in Panel B use Next Q Flow (percentage net capital flow into published fund class during post-publication quarter) as the dependant variable, while independantly controlling for each of the advertising activity indicators. The tests in Panel C repeat those of Panel B but replace Next Q Flow with Next Q Flow complex spillover (percentage net capital flow into all funds of a published fund complex except the published one during 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. α_1 is the discontinuity (coefficient on the indicator) and Γ_1 is the differential impact of the control variable around the discontinuity (coefficient oo the interaction of the indicator and control variable).

	I^+[Ad Size]	I^+[Amount Spent]	I^+[# Ads Published]	I^+[# Rank Mentions]	I^+[# Media Mentions]
Panel A - Discontinuity tests					
Discontinuity at 10.5 (α_1)	0.265**	0.202*	0.195*	0.042	0.026
	(0.014)	(0.061)	(0.095)	(0.822)	(0.206)
N	1,225	567	1,165	471	10,488
Panel B - Effect of ads and media on increased fund-class flows					
α_1	2.852	1.637	3.241	2.627	2.079**
	(0.373)	(0.664)	(0.337)	(0.616)	(0.043)
Γ_1	-0.001	-0.008	-0.001	0.093*	0.032**
	(0.994)	(0.825)	(0.941)	(0.071)	(0.026)
N	1,225	567	1,165	471	10,488
Panel C - Effect of ads and media on increased complex flows					
α_1	2.158*	2.094	3.861**	1.533	1.936**
	(0.074)	(0.134)	(0.020)	(0.292)	(0.021)
Γ_1	-0.006	-0.028	-0.014	0.050*	0.026**
	(0.687)	(0.166)	(0.342)	(0.089)	(0.021)
N	384	186	357	114	6,429

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix A.

A.1. Validity of the RDD

The literature discussing RDD best-practices (e.g. Hahn et al. (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.

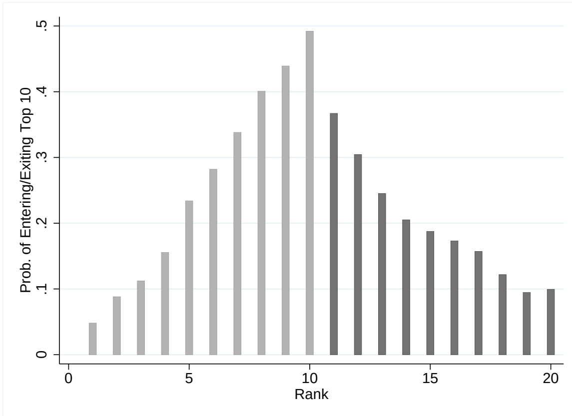
Quasi-random ranking is necessary to guarantee that differences between mutual funds just above and just below the publication threshold are caused by media attention rather than reflecting a spurious correlation. Figure A.1 provides evidence of high fund ranking volatility in our data. We consider the empirical ex-post probability of being in the top-10 list by publication date conditional on the rank held by the mutual three months, two months, 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 A.2 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 A.1 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 A.1 and Table A.1 ensure that the "Local Randomization" assumption of Lee and Lemieux (2010) holds.

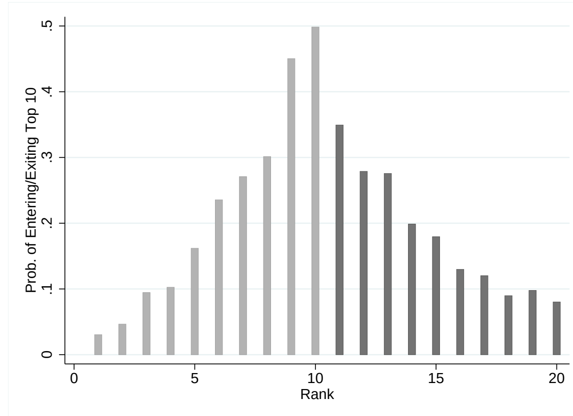
Finally, as is common in regression discontinuity design studies, we verify the results are not driven by the choice of bandwidth. Figure A.3 presents the magnitude and significance of the discontinuity in capital flows, based on a range of possible bandwidths. The discontinuity is significant at the 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.

A.2. Supplementary discontinuity tests

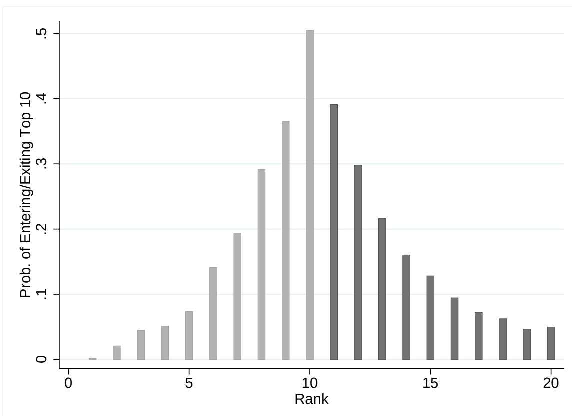
Though we focus our attention on increased capital flows, we examine other possible consequences of media exposure. We test and find no significant effect on fund management fees and expense ratios a quarter, and a year, after publication. Results of discontinuity tests for these possible outcomes are reported in Table A.2. 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.



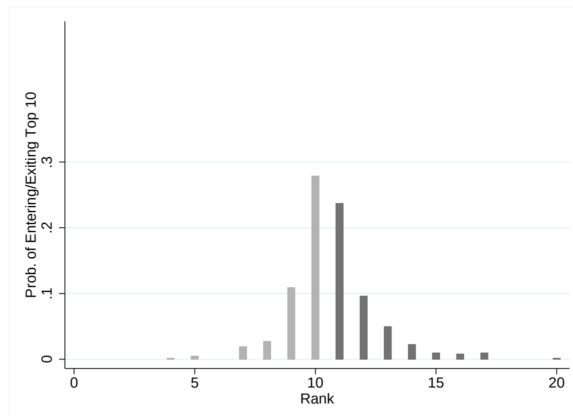
(a) month 9→12



(b) month 10→12



(c) month 11→12



(d) Last day

Fig. A.1. Frequency of entering/exiting top 10. 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) Three months, (b) Two months, (c) One month, and (d) one day 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) Three months, (b) Two months, (c) One month, and (d) One day before the end of a ranking period.

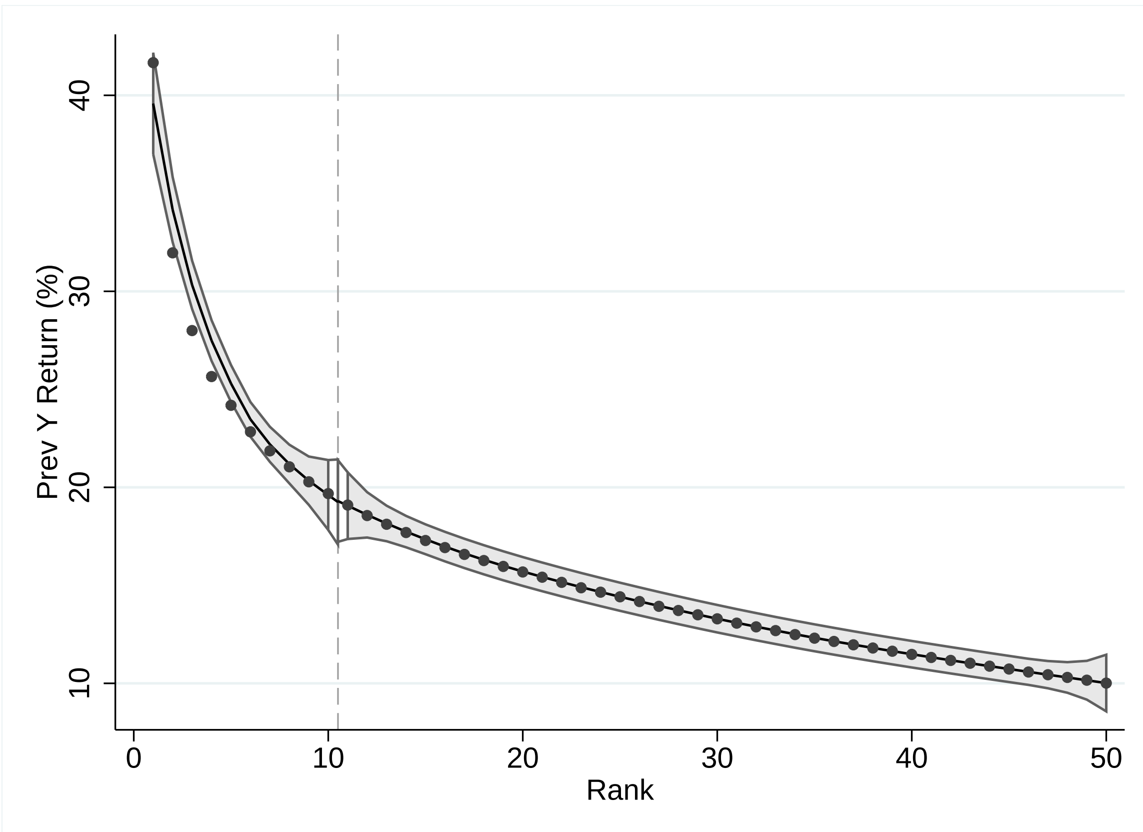


Fig. A.2. RDD analysis of pre-ranking returns by rank. Mutual funds are ranked based on previous 12 month return within their investment category at the beginning of every quarter from 2000Q1 to 2012Q4 using data from CRSP. The figure depicts the two one-sided local linear kernel regressions of the previous 12 month returns on the funds' WSJ ranks.

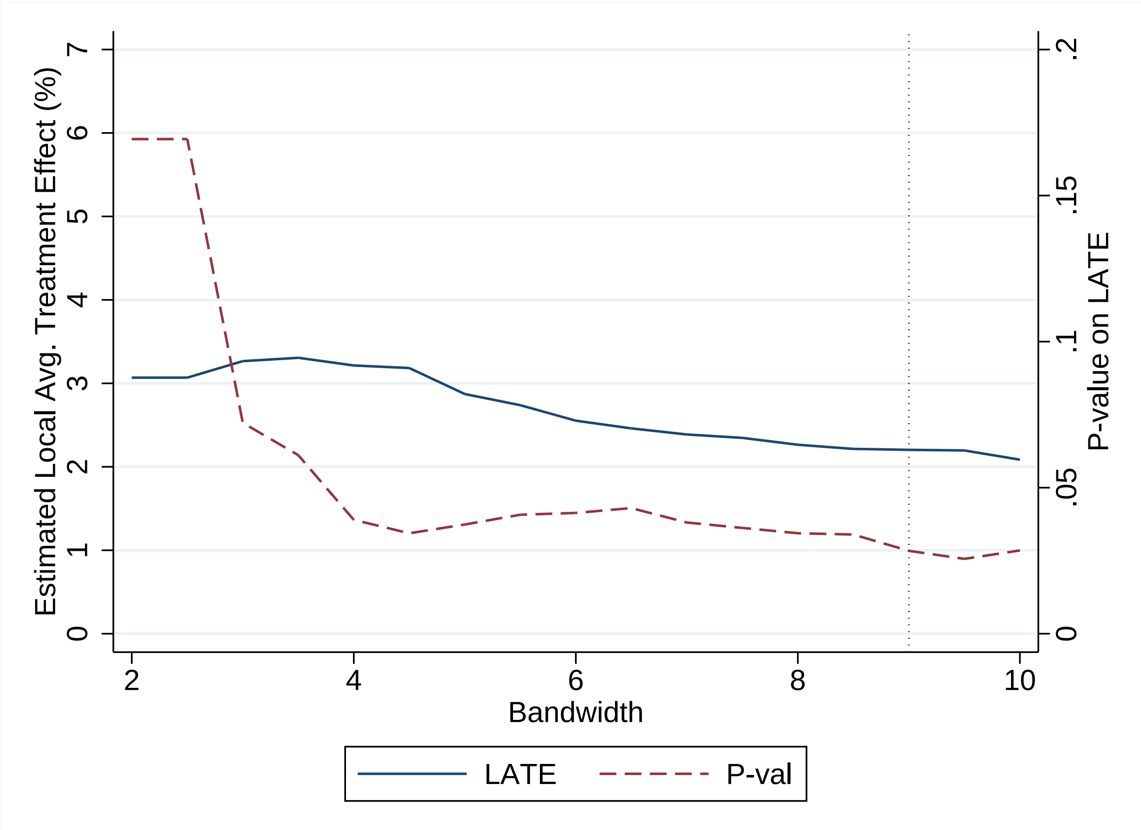


Fig. A.3. Effects of bandwidth on discontinuity estimation. This figure presents the magnitude and significance of discontinuity in capital flows as a function of the bandwidth used in local linear kernel regressions of flows on ranks around the $rank = 10.5$ cutoff. The vertical dotted line is at the actual bandwidth used. The actual bandwidth was chosen based on the optimal bandwidth estimator of Imbens and Kalyanaraman (2012).

Table A.1

Discontinuity test for fund characteristics

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.

	TNA	Fund age	Exp. ratio	Mgmt. fee	Front load
Discontinuity at 10.5	-26.691	-0.278	-0.008	-0.003	-0.178
	(0.838)	(0.488)	(0.625)	(0.237)	(0.211)
Discontinuity w/ controls	22.018	-0.891	-0.002	-0.002	-0.162
	(0.901)	(0.159)	(0.922)	(0.515)	(0.231)
Actual 10 vs actual 11	-157.909	-0.946	0.015	-0.001	-0.203
	(0.276)	(0.149)	(0.580)	(0.115)	(0.376)
Fitted vs actual at 10	-100.751	-0.748	0.000	-0.003	-0.220
	(0.419)	(0.137)	(0.989)	(0.241)	(0.321)
Fitted vs actual at 11	-83.849	-0.476	0.006	-0.001	-0.161
	(0.592)	(0.420)	(0.796)	(0.160)	(0.399)

Table A.2

Discontinuity test for other possible effects

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.

	Exp. ratio+Q	Mgmt. fee+Q	Exp. ratio+Y	Mgmt. fee+Y
Discontinuity at 10.5	-0.004	-0.060	0.001	-0.005
	(0.837)	(0.453)	(0.949)	(0.830)
Discontinuity w/ controls	0.004	-0.027	-0.004	-0.004
	(0.853)	(0.433)	(0.883)	(0.887)
Actual 10 vs actual 11	0.008	-0.112	0.016	-0.046
	(0.762)	(0.121)	(0.627)	(0.309)
Fitted vs actual at 10	-0.005	-0.055	-0.005	-0.027
	(0.797)	(0.401)	(0.890)	(0.488)
Fitted vs actual at 11	0.008	-0.117	0.022	-0.025
	(0.744)	(0.110)	(0.488)	(0.563)