# Institutional Investors and Mutual Fund Governance: Evidence from Retail–Institutional Fund Twins

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Advisors often manage multiple versions of a fund. These "twins" have the same manager and similar performance but are sold to different investors with differing abilities to select and monitor managers. Comparing investor flows in retail and institutional twins, we find that institutional investors are more sensitive to high fees and poor risk-adjusted performance. Consistent with the reduction of agency problems from greater monitoring, retail funds with an institutional twin outperform other retail funds by 1.5% per year. After the institutional twin is created, expenses decrease while measures of managerial effort at the retail fund increase. (*JEL* G23, G34)

The ability of investors to vote with their feet is the principal investor safeguard in mutual funds. Because mutual fund investors can redeem their shares at net asset value, they can effectively remove the manager from the control of those assets. Fama and Jensen (1983) liken the feature of "redeemable claims" to a "partial takeover or liquidation," and argue that this market governance reduces the need for other forms of governance in mutual funds.

Whether or not redeemable claims effectively safeguard investors, however, depends on whether investors use the correct criteria to evaluate funds, and the existing evidence suggests that retail investors fail to respond to many useful

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signals.[1] In stark contrast with the evidence for retail investors, sophisticated institutional investors respond to useful measures such as expenses and risk-adjusted performance. They exercise market governance and punish poorly performing managers by withdrawing assets under management (e.g., Del Guercio and Tkac 2002; Goyal and Wahal 2008).

In this paper, we examine whether retail investors can benefit from the ability and willingness of institutional investors to exercise market governance. We first show that fund flows of sophisticated institutional investors in our sample indeed respond to useful measures such as expenses and risk-adjusted performance, while retail fund flows are much less responsive. We then examine the performance of a subset of retail mutual funds that offer a separate version of the fund for institutional investors in either mutual fund or separate account form, but where the same managers follow virtually the same strategy for both the retail and institutional assets. We find that retail mutual funds with institutional twins perform better than retail funds without, even after accounting for the endogeneity of the decision to offer an institutional twin. The risk-adjusted excess performance is an economically and statistically significant 1.5% annually.

In additional tests, we use cross-sectional differences in the creation dates of the institutional and retail mutual funds to examine whether performance for retail mutual fund investors improves after the addition of institutional assets. Specifically, we focus on the quarter of our twin matches (28.5% or 132 out of 463) where the institutional twin is created after the retail fund. We use propensity score matching techniques to compare the change in performance for retail twin funds before and after the creation of their institutional twin (treatment group) with a carefully matched sample of funds with no institutional twin (control group) but otherwise similar characteristics. The risk-adjusted performance of the retail funds improves economically and statistically significantly after the addition of the institutional twin relative to the propensity score matched control sample. The relative performance improvement of retail funds with twins is driven by a deterioration of performance of the control sample, while the retail funds with twins have stable performance. The stable performance is surprising, because the institutional assets increase total assets under management in the investment strategy (i.e., retail and institutional assets combined) by over 200%, and the past literature typically finds that performance is a decreasing function of fund size (e.g., Chen et al. 2004).

We then examine different channels through which monitoring by investors in the institutional twin fund could improve the performance of the retail

[1] For example, Sirri and Tufano (1998) and Del Guercio and Tkac (2002) find that mutual fund investors use raw return performance to evaluate funds and flock disproportionately to recent winners but do not withdraw assets from recent losers. This convexity leads to well-known problems such as mutual fund managers having incentives to alter the risk of their portfolios if they are close to being among the winners (e.g., Brown, Harlow, and Starks 1996; Chevalier and Ellison 1997).

twin funds. For reasons of both convenience and legal liability,[2] retail and institutional versions of the same fund hold virtually the same portfolio and the level of expenses in one portfolio can affect levels in the other. As a result, investors in the retail twin could potentially benefit from increased monitoring by institutional investors if that monitoring resulted in reduced fees or increased managerial effort. To test this, we use the propensity matched sample design described above to examine the change before and after the creation of an institutional twin for three channels: direct expenses, indirect expenses, and manager effort. To examine the first channel, direct expenses, we look at changes in the expense ratio and find a small but statistically significant decrease in the treatment group expense ratio relative to the control. For the second channel, indirect expenses (i.e., those costs that are not included in the expense ratio but are subtracted from fund assets including brokerage commissions, trading/implementation costs, etc.), we examine the change in the return gap measure of Kacperczyk, Sialm, and Zheng (2008). The return gap is the difference between a fund's actual return and the return on a hypothetical buy and hold portfolio of the fund's most recently disclosed holdings net of expenses. As such, the return gap abstracts from direct costs, but measures indirect costs such as brokerage commissions, trading costs, and other implementation costs. Using this measure, we find that the return gap for retail funds with institutional twins improves strongly relative to the control sample, accounting for approximately one-third of the overall increase in risk-adjusted performance. We are also able to measure two specific dimensions of these indirect costs: soft-dollar usage and brokerage commissions. We find that the use of soft dollar payments for distribution decreases after a twin fund was created, and we obtain qualitatively similar results when we analyze the brokerage commission rate.

While the third channel, manager effort, is difficult to measure directly, we examine the active share measure of Cremers and Petajisto (2009) and Petajisto (2010).[3] Active share measures the differences of a fund's holdings compared to the holdings of its closest benchmark, with more active funds having a higher active share. We find that after a retail fund adds an institutional twin, its active share increases relative to a control group. In addition, managers of these funds select stocks with lower analyst coverage relative to the control group, which is also consistent with higher managerial effort.

One potential concern with our results is that they are consistent, not only with increased monitoring, but also with the ability of institutional investors to identify superior managers. While we attempt to address this alternative explanation through the use of Heckman regressions, propensity

[2] As we discuss in more detail in Section 4, the case of Young *v*. Nationwide Life Ins. Co. established the basis of fund liability for differences in performance between twin funds.

[3] Active share represents the share of portfolio holdings that differ from the benchmark index holdings. Cremers and Petajisto (2009) show that funds with high active share outperform those with low active share.

score matching, and the identification and examination of plausible channels through which monitoring might affect fund performance, the possibility of a correlated omitted variable remains. Given the effort typically expended by institutional investors in the manager search process and the possibility that dimensions of their manager search procedures are omitted from our regression specifications, this is an important caveat to our results.

Over the past few years, the role of fund governance has attracted considerable attention in both the finance and legal literature. The focus of the empirical finance literature on fund governance has been on internal governance mechanisms such as director equity incentives or the quality of the board of directors.[4] Our study contributes to this strand of the literature, because the existence of an institutional twin fund can be interpreted as an external governance mechanism. The institutional twin funds consist of significant investments by a small group of institutional investors, comparable to large shareholders in public corporations. To the best of our knowledge, the issue of the importance of large shareholders has not been addressed in the context of mutual funds. There is ample empirical evidence, however, that institutional investors in large public corporations influence firms' corporate governance. The influence can come either indirectly via "voting (or threatening to vote) with one's feet," as the act or threat of selling shares can have disciplinary effects on companies that lead to changes in governance (e.g., Parrino, Sias, and Starks 2003), or directly via active monitoring [e.g., adoption of anti-takeover provisions (Agrawal and Mandelker 1990), CEO compensation (Hartzell and Starks 2003; Almazan, Hartzell, and Starks 2005), or proxy voting (Gillan and Starks 2000; Del Guercio, Seery, and Woidtke 2008)].

Market governance as it relates to the so-called "Gartenberg"[5] standard has been an issue of considerable interest in the legal literature as well.[6] While mutual fund regulation relies heavily on disclosure and market governance to protect investors (i.e., when a fund clearly and accurately discloses fees,

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[4] Chen, Goldstein, and Jiang (2008), Meschke (2008), and Cremers et al. (2009) all document that funds whose directors hold a larger fraction of their shares exhibit superior performance. Research on mutual fund board governance has identified an impact of boards on fees, stale pricing, fund merger activity, manager turnover, and fund performance. Tufano and Sevick (1997) find evidence that fees are lower for mutual funds whose boards are smaller and have a larger fraction of independent directors. Zitzewitz (2003) shows that the incidence of stale pricing in fund complexes is higher for funds with fewer independent directors. Khorana, Tufano, and Wedge (2007) examine a sample of fund mergers and conclude that there is a higher probability of a merger if a fund has underperformed and if it has a higher fraction of independent directors. Ding and Wermers (2005) find that funds with a larger number of outside directors are more likely to replace a poorly performing manager. Adams, Mansi, and Nishikawa (2010) find that index funds with smaller boards, boards with inside directors who are also fund sponsor officers, and boards made up exclusively of independent directors are associated with improved performance. Dann, Del Guercio, and Partch (2003) find that closed-end investment companies with smaller and more independent boards carry out value-enhancing restructurings.

[5] Gartenberg *v*. Merrill Lynch Asset Management, Inc., Merrill Lynch, Pierce, Fenner & Smith Incorporated, Merrill Lynch & Co. and Merrill Lynch Ready Assets Trust, 694 F.2d 923, 2d Cir. (1982).

[6] There is an active legal literature that has examined whether competition and arm's-length bargaining exist in the industry and influence mutual fund fees. For opposing views on the topic, see Coates and Hubbard (2007) and Morley and Curtis (2010).

risks, and performance investors are protected because they can "vote with their feet"), Gartenberg allows investors to sue for excessive fees, even if such fees are fully disclosed.[7] In the recent Jones et al. v. Harris Associates case, shareholders of an Oakmark mutual fund sued the fund's advisor, Harris Associates, claiming the fund's fees were excessive because, similar to the twin fund arrangements discussed in this paper, Harris advised a separate account for institutional investors that followed a similar investment strategy as the mutual fund, but charged much lower fees. While the lawsuit argued that the difference in fees between the retail and institutional investment products was evidence that the mutual fund fees were excessive, the Illinois District Court rejected the case, granting summary judgment to Harris Associates. The court noted that while the investment strategy for the retail mutual fund and institutional separate accounts were similar, there was a larger set of services provided to retail investors negating the fee comparison between the two investment products.[8] On appeal, not only did the Seventh Circuit Court of Appeals uphold the District Court's decision for summary judgment, but they rejected the Gartenberg standard entirely, indicating that market governance alone protected investors. While the Supreme Court eventually upheld the Gartenberg standard, at the core of the argument was the efficacy of market governance in protecting retail fund investors.[9] Our evidence contributes to this literature in two ways. First, our evidence suggests that when market governance is exercised by informed investors, it can effectively monitor and discipline managers. Second, while twin fund fee comparisons have been used to suggest that mutual fund fees are excessive, we show that retail twin fund investors actually benefit from a twin fund arrangement *relative* to non-twin retail fund investors.

Our paper is also related to two recent papers on the side-by-side management of hedge funds and mutual funds (Cici, Gibson, and Moussawi 2010; Nohel, Wang, and Zheng 2010). The two papers also exploit the fact that some

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[7] While Gartenberg enables mutual fund investors to sue for excessive fees, in its opinion, the court established a heavy burden on plaintiffs: they have to prove that the fee charged is "so disproportionately large that it bears no reasonable relationship to the services rendered and could not have been the product of arm's-length bargaining."

[8] For cases that involve fee comparisons, courts consider the nature of both investment and non-investment services rendered to different groups of investors. In the Jones *v.* Harris case, in addition to portfolio management, Harris provided unique services to the retail mutual fund clients including administrative services, shareholder communications, oversight of third-party vendors, and compliance with the regulatory regime that were not required for their separate institutional accounts. For more details on service differences for investors in retail and institutional investment products, see Collins (2003).

[9] On appeal, Chief Judge Frank Easterbrook challenged the premise behind the Gartenberg standard, suggesting that market governance was an effective mechanism to safeguard investors from excessive fees: "The trustees (and in the end investors, who vote with their feet and dollars), rather than a judge or jury, determine how much advisory services are worth." In the dissenting opinion on the petition for rehearing the case En Banc, Judge Richard Posner disagreed with this sentiment, stating: "The panel bases its rejection … mainly on an economic analysis that is ripe for reexamination … Competition in product and capital markets can't be counted on to solve the problem …"

managers manage different funds at the same time, but their focus is on the quality and retention of the manager, as well as internal transfer payments.[10]

The remainder of our paper is structured as follows. Section 1 describes the data and offers summary statistics. Section 2 examines market governance by comparing the flow-performance and flow-fee sensitivities of institutional fund investors to those of their retail counterparts. Section 3 contains the empirical analysis of fund performance of retail mutual funds with institutional twins. Section 4 examines potential channels through which monitoring by institutional investors could help the performance of twin retail mutual funds. Section 5 examines the robustness of our main results by carrying out a placebo experiment and repeating our analysis with institutional share classes separated out, and Section 6 concludes.

## 1. Data

Our sample consists of domestic U.S. equity mutual funds[11] in the Morningstar database from January 1996 to December 2009.[12] The principal sample used throughout the paper consists of retail funds, and our analysis is performed at the fund level (e.g., share classes are aggregated and all variables are value-weighted by the total net assets of the individual share classes of the fund). We classify funds with all retail or both retail and institutional share classes as retail.[13] Table 1 contains sample summary statistics. Panel A of Table 1 contains a breakdown of the number of funds and observations by year. Because our regressions control for lagged variables, the first year of data for our analysis is 1997. The number of funds is 760 in 1997 and increases in almost every year to a maximum of 1,964 in 2008. Overall, our sample contains 2,660 unique retail mutual funds.

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[10] Nohel, Wang, and Zheng (2010) examine 112 cases where the same fund manager simultaneously manages mutual funds and hedge funds. The main finding is that the best mutual fund managers would potentially leave their mutual fund families and open their own hedge funds because of a more attractive compensation package, but that the permission to run an in-house hedge fund works well as a retention device. Cici, Gibson, and Moussawi (2010) study mutual fund performance when parent firms (but not necessarily the same manager) simultaneously manage hedge funds and focus on the inherent conflicts of interest to transfer performance from mutual funds to hedge funds. They find that the mutual funds managed by these firms underperform a matched sample of other mutual funds.

[11] We use the Morningstar "U.S. Broad Asset Class," "Global Category," and "Investment Objective" variables to identify the domestic equity sample. We require all funds to have a U.S. Broad Asset Class designation of U.S. Stock and an Investment Objective classification of aggressive growth, growth, growth & income, equity income, or small company. We then remove all funds with a Morningstar "Global Category" classification of Real Estate or any domestic equity sector classification. These filters remove fixed-income, real-estate, commodity, international equity, balanced, and sector funds from the sample.

[12] The start date of January 1996 coincides with the first date of survivorship bias-free Morningstar data availability.

[13] We categorize mutual funds that have only retail share classes and mutual funds that have both institutional and retail share classes as "retail" funds both to be conservative (classifying funds with some institutional share classes as retail should bias the empirical examination against finding any difference between retail funds with and without twins) and due to the concern that institutional investors in separate accounts and purely institutional funds are fundamentally different from investors in institutional share classes, the latter being less financially sophisticated. In the robustness section, we discuss the impact of relaxing this assumption on our results.

**Table 1**
**Retail mutual fund sample summary statistics**

Panel A: Observations by year

| Year | 1997 | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 |
|---|---|---|---|---|---|---|---|
| Observations | 8,543 | 9,597 | 10,436 | 12,704 | 14,405 | 15,910 | 16,994 |
| Funds | 760 | 847 | 963 | 1,159 | 1,285 | 1,402 | 1,569 |
| Year | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | |
| Observations | 18,497 | 20,567 | 20,598 | 21,603 | 22,155 | 22,351 | |
| Funds | 1,640 | 1,842 | 1,775 | 1,911 | 1,964 | 1,944 | |

Panel B: Sample composition

| | No. of Funds | Percent |
|---|---|---|
| No Twin | 2,197 | 82.6% |
| Twin | 463 | 17.4% |
| Separate Account Twin | 345 | 13.0% |
| Institutional MF Twin | 118 | 4.4% |

Panel C: Univariate statistics

| No Twins Sample (172,846 Obs.) | | | Percentiles | |
|---|---|---|---|---|
| Variable | Mean | Median | 25th | 75th |
| Family TNA ($BB) | 31.9 | 5.3 | 0.7 | 23.9 |
| Fund TNA ($MM) | 1,496.5 | 241.4 | 66.4 | 874.3 |
| Expense Ratio (Annual) | 1.27% | 1.25% | 0.96% | 1.54% |
| Turnover (%) | 91% | 61% | 29% | 110% |
| Quarterly Net Flow (%) | 2.35% | −0.50% | −3.82% | 4.24% |
| 4-Factor Alpha | −1.46% | −1.56% | −12.35% | 9.11% |
| Broker-Sold | 64.1% | | | |

| Twins Sample (41,514 Obs.) | | | Percentiles | |
|---|---|---|---|---|
| Variable | Mean | Median | 25th | 75th |
| Family TNA ($BB) | 24.6 | 8.8 | 2.3 | 33.7 |
| Fund TNA ($MM) | 1,423.2 | 412.4 | 126.8 | 1,297.9 |
| Expense Ratio (Annual) | 1.24% | 1.23% | 0.97% | 1.53% |
| Turnover (%) | 86% | 65% | 34% | 111% |
| Quarterly Net Flow (%) | 3.31% | −0.02% | −3.45% | 5.41% |
| 4-Factor Alpha | −0.75% | −1.00% | −11.69% | 9.95% |
| Relative Start Date (Inst. Incept. Date – Retail Incept. Date) | 0.9 | 0.0 | −3.5 | 6.1 |
| Broker-Sold | 67.3% | | | |

(*contitnued*)

Panel B of Table 1 shows the breakdown of funds into those with and without an institutional twin. To construct the sample of possible institutional twins, we combine separate account and institutional mutual fund data from Morningstar. While the separate account data come directly from Morningstar, to identify the institutional mutual funds we use an internal Morningstar share class identifier and classify a fund as institutional only if all of the fund's assets are institutional investments as designated by Morningstar. We then compare the retail fund sample to the institutional sample to identify twin matches. A twin match is identified if the retail and institutional fund have the

**Table 1**
**Continued**
Panel D: Twin start dates

| | Full Sample | | | Inception Date between 1996 and 2009 | | |
|---|---|---|---|---|---|---|
| | Retail Fund First | Institutional Fund First | Same Incept. Date | Retail Fund First | Institutional Fund First | Same Incept. Date |
| Twin Pairs | 237 | 224 | 2 | 132 | 179 | 2 |

Table 1 provides descriptive statistics for the sample of U.S. domestic equity mutual funds from 1997 to 2009. Panel A lists the number of fund-month observations in the sample by year. Panel B lists the number and percentage of funds with and without an institutional twin, dividing the twins into those with a separate account twin versus those with an institutional mutual fund twin. Panel C gives the mean, median, and 25$^{th}$ and 75$^{th}$ percentiles of fund-month observations for funds with and without institutional twins. Statistics are given for fund and family total net assets (TNA) in millions ($MM) and billions ($BB) of dollars, respectively, annual expense ratio, turnover, quarterly net flow (% TNA), the fund's annualized four-factor alpha calculated over the previous 36 months and the percentage of broker-sold fund-month observations, where broker-sold is defined as any fund that has a front load, a rear load, or charges a 12b-1 fee greater than 0.25%. For the twins sample, Panel C also provides the relative start date of the twin calculated as the inception date of the institutional twin minus the inception date of the retail twin in years. Panel D breaks the sample of retail-institutional matched twins in three groups: those where the retail fund was started first followed by the institutional twin, those where the institutional twin was started first, and those where they started at the same time. Panel D lists the number of twins in each category for the full sample and for the subsample where the inception date of the relevant fund occurs after the start of our sample period, 1996 (institutional twin inception date if it is in the retail fund first category; retail twin inception date if it is in the institutional fund first category; and either if it is in the same inception date category).

same manager(s),[14] investment objectives, fund families, and a gross return correlation of 0.95 or greater.[15] As Panel B shows, 463 out of 2,660 unique retail mutual funds or 17.4% of the sample observations have an institutional twin. Of these institutional twins, 345 are from the separate account sample and 118 are from the institutional mutual fund sample described above.

Panel C of Table 1 offers summary statistics for the retail funds in the sample, split by whether or not they ever have an institutional twin. The table lists the mean, median, and 25$^{th}$ and 75$^{th}$ percentiles of the fund family size [measured as total net assets (TNA) under management], fund size (TNA), expense ratio, turnover (the minimum of fund purchases and sales divided by fund TNA), the quarterly net flow into the fund, the fund's four-factor alpha [the three Fama and French (1993) factors combined with the momentum factor of Carhart (1997)], and the percentage of the observations coming from broker-sold funds. Unless otherwise noted, statistics are based on monthly observations. Fund family size,[16] expense ratio, turnover, and the percentage of observations from broker-sold funds are comparable across the two samples. Funds with twins have larger median TNA, higher flows, and better four-factor alpha performance. While

[14] Because fund manager information is used in identifying fund twins, those funds that are missing manager information or only classify the manager as "Management Team" are removed from the sample.

[15] While setting a lower bound on the return correlation is a logical safeguard, when matching on the first three criteria alone, the mean and median return correlations are 0.98 and 0.99.

[16] While the differences in mean and median fund family size between funds with and without an institutional twin are in the billions of dollars, these differences are small relative to the dispersion of fund family size (∼$20 or $30 billion).

the difference in performance is particularly interesting given the focus of our study, it is important to recognize that this result does not suggest causality. Indeed, we would expect that the funds that are most likely to be sold to multiple clienteles would be those with the best performance. The larger median size of these funds and the higher inflows are also consistent with this interpretation. The summary statistics for the "twins sample" also include information about the start date of the retail fund relative to its institutional twin. The second-to-last row of Panel C of Table 1 gives the mean, median, and 25$^{th}$ and 75$^{th}$ percentiles of the *Relative Start Date,* defined as the difference in years between the inception date of the institutional twin and the inception date of the retail fund. The mean *Relative Start Date* value of 0.9, for example, indicates that the average institutional twin was started 0.9 years after its retail counterpart, and the negative 25$^{th}$ percentile shows that the sample also consists of twin pairs where the institutional fund was started before its retail twin.

Panel D of Table 1 further explores this heterogeneity in twin start dates, showing the number of twins where the retail fund was started first, the institutional fund was started first, and the retail and institutional funds were started on the same date (*Same Incept. Date*). In some cases the retail or institutional twin fund was started before the sample period began (1996) so the last three columns of Panel D repeat the breakdown for the sample of 313 twin funds where the inception date for the twin fund (i.e., the second fund created) occurs during the sample period. The 132 twin pairs where the retail fund was started first and the institutional twin was created during the sample period are important for our identification strategy in Sections 3.3 and 4. A pre-twin period for the retail fund allows us to identify the incremental effect of the institutional fund's creation on retail mutual fund performance. In addition, such a sample enables us to provide estimates from propensity score matched samples. With propensity score matching, we first analyze which retail funds are the most likely to create an institutional twin, and then compare a treatment sample of twin funds with an appropriately matched control sample.

While the majority of our analysis focuses on retail funds, Table 2 characterizes the institutional twin fund sample. Panel A offers summary statistics for the sample of institutional twin fund-months. Because separate accounts do not have the same disclosure requirements as mutual funds, some variables, such as turnover, are not as well populated in the database and the flow and TNA data are only given at a quarterly frequency. As a result, in addition to the mean, median, and 25$^{th}$ and 75$^{th}$ percentiles, we also list for each variable the number of fund-month observations available to calculate the statistics. Comparing the institutional funds to their retail counterparts (twins sample in Table 1, Panel C), we see a number of differences. Consistent with an institutional clientele, expense ratios are lower and fund size is larger. Although performance is better for the institutional twins, the difference between the retail and institutional fund performance is roughly equivalent to the difference in expense ratios.

**Table 2**
**Institutional fund sample summary statistics**

Panel A: Institutional twin sample

| Variable | Mean | Median | Percentiles 25th | Percentiles 75th | Obs. |
|---|---|---|---|---|---|
| Family TNA ($BB) | 33.5 | 10.8 | 3.1 | 53.9 | 6,200 |
| Fund TNA ($MM) | 1,788.8 | 694.8 | 174.5 | 1,811.0 | 6,200 |
| Expense Ratio | 0.80% | 0.72% | 0.50% | 0.96% | 17,919 |
| Turnover (%) | 80% | 63% | 34% | 106% | 10,694 |
| Quarterly Net Flow (%) | 3.51% | −0.32% | −3.65% | 4.61% | 4,335 |
| 4-Factor Alpha | −0.56% | −0.72% | −11.46% | 10.10% | 18,205 |

Panel B: Average fund differences

| | Diff. (Inst - Retail) | | | | Diff. Test $p$-Values | | # of Twin Pairs |
|---|---|---|---|---|---|---|---|
| | | | Percentiles | | | | |
| Variable | Mean | Median | 25th | 75th | Mean | Median | |
| Fund TNA ($MM) | 547.9 | 86.8 | −209.8 | 816.4 | 0.050 | < .001 | 435 |
| Expense Ratio (Annual) | −0.42% | −0.44% | −0.73% | −0.16% | < .001 | < .001 | 437 |
| Turnover (%) | −7.3% | −0.4% | −13.7% | 6.7% | 0.004 | 0.147 | 298 |
| Quarterly Net Flow (%) | 2.04% | 0.12% | −4.76% | 6.94% | 0.079 | 0.460 | 310 |
| 4-Factor Alpha | 0.54% | 0.39% | −0.10% | 0.99% | < .001 | < .001 | 420 |
| Net Total Return | 0.62% | 0.51% | −0.02% | 1.17% | < .001 | < .001 | 463 |
| Gross Total Return | 0.13% | −0.01% | −0.43% | 0.45% | 0.047 | 0.376 | 463 |
| Pct in Common Stock (%) | 3.5% | 3.3% | 0.0% | 7.7% | < .001 | < .001 | 367 |
| MKT Factor (x100) | −0.087 | 0.039 | −1.114 | 1.051 | 0.666 | 0.354 | 420 |
| HML Factor (x100) | 0.389 | 0.025 | −0.935 | 1.177 | 0.138 | 0.884 | 420 |
| SMB Factor (x100) | −0.476 | −0.036 | −1.575 | 0.952 | 0.110 | 0.354 | 420 |
| Momentum Factor (x100) | −0.107 | −0.042 | −0.761 | 0.546 | 0.472 | 0.157 | 420 |

Table 2 provides descriptive statistics for our sample of institutional fund twins from 1997 to 2009. Panel A gives the mean, median, and 25$^{th}$ and 75$^{th}$ percentiles. Panel B contains summary statistics for the differences in characteristics between the retail mutual funds and their institutional twins. In addition to variables previously described, the table includes annualized gross and net total return and the factor loadings on the three Fama and French (1993) factors [market (*MKT*), value (*HML*), and size (*SMB*)] and the Carhart (1997) momentum factor. Panel B provides the $p$-values from a $t$-test of the difference in means and a sign test of the difference in medians. The monthly retail fund data are merged with the corresponding monthly institutional twin data. For each matched pair-month observation, the difference in the variables of interest is calculated. These differences are averaged for each of the matched pairs, and cross-sectional sample statistics from the matched pairs are given. The last column shows the number of twin pairs with complete data for each variable.

While comparing Panel C of Table 1 and Panel A of Table 2 gives an approximation of the differences between retail funds and their institutional twins, we formally test these differences in Panel B of Table 2. To construct this table, we merge monthly retail fund data with the corresponding monthly institutional twin data when they are available.[17] For each matched pair-month observation, we then calculate the difference in variables of interest. The differences are first averaged over time for each of the matched pairs. Panel B of Table 2 provides cross-sectional sample statistics from the time-series

[17] While return data for the separate accounts are available at a monthly frequency, the total net asset and flow data are only available at a quarterly frequency. For these variables, we calculate the institutional-retail differences at a quarterly frequency.

averages of the matched pairs. Columns 5 and 6 of Panel B provide $p$-values for tests of differences in means ($t$-test) and medians (sign test), and also show the number of matched pairs for which the variable of interest is available.

Panel B of Table 2 shows that the average (median) institutional fund is \$547.9 (\$86.8) million larger than its retail twin and has an expense ratio that is 0.42% (0.44%) lower. Both of these differences are statistically significantly different from zero. In each case, they are also consistent with differences in the type of investors. Because investors in separate accounts typically have much larger investments and receive a different set of services,[18] they also have lower expenses. Institutional twins have higher average quarterly net flows. Because the flows from these investors are typically more predictable,[19] managers do not need to hold as much cash on hand to meet redemptions and do not need to trade as much to account for them.

Panel B of Table 2 also compares three different annualized performance measures: four-factor alphas, net total return, and gross total return. The four-factor alphas are calculated over the previous 36 months with the standard set of factors proposed by Fama and French (1993) and Carhart (1997): market (*MKT*), market capitalization (*SMB* = small minus big), book-to-market (*HML* = high minus low), and momentum. While the institutional funds outperform their retail twins in terms of both four-factor alpha and total return (both net of fund expenses), comparing the means and medians of these differences to the means and medians of the differences in gross total return, we see that the outperformance is roughly equivalent to the difference in expense ratios ($\sim$50 bps). We do find, however, a small but marginally statistically significant difference in mean gross returns, but this is not surprising given the lower turnover and the higher percentage invested in common stock described above.

We finally examine differences in the factor loadings between matched pairs to assess differences in risk. The comparison of the factor loadings on the market, size, and value factor indicate that there are no significant differences across the institutional and retail twin funds.

Overall, the comparisons of Table 2, Panel B, suggest that while there are important economic differences between retail funds and their institutional twins, there is little or no difference in the performance other than the fee differential and little or no difference in risk between the twin pairs.

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[18] See, for example, Collins (2003) for a comparison of the different services rendered to retail and institutional investors and the resulting fee differential.

[19] The Investment Company Institute (2006) suggests that the increased predictability of institutional flows is due to fixed redemption periods (monthly, quarterly, or annually) and redemption notices or other means of advance communication between investors and managers about fund purchases and redemptions. In contrast, retail mutual fund investors can purchase or redeem on a daily basis without any notice to the manager. The additional information available to the manager of a separate account or institutional fund makes these flows easier to anticipate and therefore more predictable.

## 2. Fund-Flow-Sensitivity of Retail Mutual Funds and Institutional Mutual Funds

In this section, we examine the determinants of net flows into retail and institutional funds to identify whether these two types of investors respond to different signals. Our working hypothesis is that institutional investors use more sophisticated criteria to evaluate fund managers and have a greater aptitude for market governance. We provide two sets of results to test this hypothesis. Specification 1 of Table 3 is based on the entire sample and thus allows a comparison with earlier papers (e.g., Del Guercio and Tkac 2002). Specification 2 of Table 3 uses the matched sample of retail and institutional twin funds only. Using a matched pair with the same manager, the same investment objective, and the same fund family, we are able to control for the innate ability of the manager and the influence or impact of the fund family on flows.

Specification 1 of Table 3 contains an analysis of the determinants of institutional and retail quarterly flows for all retail and institutional domestic equity funds. The dependent variable is the percentage quarterly net fund flow for the next quarter ($t=0$ to $t=3$). The independent variables include the lagged ($t=-1$) natural log of fund family TNA and fund TNA, the lagged fund expense ratio, lagged turnover, the concurrent ($t=0$ to $t=3$) percentage quarterly flow to different funds with the same investment objective, and the lagged percentage quarterly fund flow. We use 36-month total return and four-factor alpha computed from the previous 36 months of data as our raw and risk-adjusted performance measure, respectively. We include both the risk-adjusted and total return measure to see whether retail and institutional flows respond more strongly to either measure. Similar to Sirri and Tufano (1998), we allow for non-linearity in the performance measures in all specifications. We use a piecewise linear performance specification with a kink at the 20$^{th}$ and 80$^{th}$ percentiles of returns.[20] The specification has separate retail and institutional coefficients for each variable. The coefficients for retail funds are under the *Retail Coef* column and the coefficients for institutional funds are under the *Inst. Coef* column. We also test whether the coefficients on the expense ratio and performance measures for retail and institutional funds are statistically significantly different from each other. The $p$-values from these tests are listed at the bottom of the table.

The main focus of our analysis is on the coefficients of the expense ratio and performance metrics. Carhart (1997) provides evidence that past expenses negatively relate to future returns. As a result, investors should avoid high fee funds. We see from specification 1 of Table 3 that institutional investors are almost six times as sensitive to expenses ($-0.0079$ vs. $-0.0468$) as retail

[20] For the total return measure, the percentiles are calculated within date and investment objective similar to Sirri and Tufano (1998), while the four-factor alpha percentiles are calculated within date only. With these percentiles, the formula for the low return is LowRet=Min(0.2, RetPtile); the formula for the medium return is MedRet=Min(0.6, RetPtile-LowRet), and the formula for the high return is HighRet=RetPtile-MedRet-LowRet.

**Table 3**
**Determinants of institutional vs. retail flows**

| Regression | 1 | | | | 2 | | | |
|---|---|---|---|---|---|---|---|---|
| | *Retail Coef.* | | *Inst. Coef.* | | *Retail Coef.* | | *Inst. Coef.* | |
| Intercept | 0.1203 | (6.8) | 0.3701 | (4.5) | 0.1418 | (5.1) | 0.4324 | (2.8) |
| Log(Family TNA)$_{t-1}$ | 0.0051 | (9.0) | 0.0047 | (2.9) | 0.0044 | (3.6) | 0.0005 | (0.1) |
| Log(Fund TNA)$_{t-1}$ | −0.0144 | (−15.0) | −0.0283 | (−9.1) | −0.0146 | (−8.1) | −0.0254 | (−5.9) |
| Expense Ratio$_{t-1}$ | −0.0079 | (−4.3) | −0.0468 | (−3.5) | −0.0063 | (−1.5) | −0.0600 | (−3.0) |
| Turnover$_{t-1}$ | 0.0026 | (1.6) | −0.0031 | (−0.7) | 0.0008 | (0.6) | −0.0008 | (−0.3) |
| InvObj Qtrly Pct Flow$_{t,t+3}$ | 0.4145 | (8.4) | 0.9603 | (7.0) | 0.3744 | (5.1) | 0.8998 | (3.8) |
| Qtrly Pct Flow$_{t-4,t-1}$ | 0.2432 | (15.1) | 0.0130 | (1.0) | 0.3125 | (10.5) | 0.0148 | (0.4) |
| Twin Fund Qtrly Pct Flow$_{t,t+3}$ | | 0.0294 | (3.3) | | | 0.0277 | (3.2) | |
| Total Return$_{t-36,t-1}$ Low | 0.0587 | (3.0) | 0.1621 | (3.2) | 0.1368 | (3.1) | 0.0347 | (0.3) |
| Total Return$_{t-36,t-1}$ Medium | 0.0431 | (9.2) | 0.0104 | (0.6) | 0.0388 | (4.8) | −0.0384 | (−1.4) |
| Total Return$_{t-36,t-1}$ High | 0.1987 | (6.0) | 0.0444 | (0.7) | 0.1097 | (2.0) | −0.0634 | (−0.4) |
| 4-Fctr Alpha$_{t-36,t-1}$ Low | 0.0890 | (5.3) | 0.2171 | (4.4) | 0.0599 | (1.4) | 0.3095 | (2.8) |
| 4-Fctr Alpha$_{t-36,t-1}$ Medium | 0.0281 | (6.4) | 0.0826 | (4.9) | 0.0253 | (2.9) | 0.1043 | (3.5) |
| 4-Fctr Alpha$_{t-36,t-1}$ High | 0.1876 | (5.2) | 0.1945 | (3.4) | 0.1416 | (2.5) | 0.2651 | (2.0) |
| Observations | | | 108,803 | | | | 21,733 | |
| Adj. R-Squared | | | 8.25% | | | | 10.07% | |
| Coef. Difference Test $p$-Values (Retail vs. Institutional) | | | | | | | | |
| Expense Ratio | | | 0.005 | | | | 0.009 | |
| 4-Fctr Alpha Low | | | 0.014 | | | | 0.028 | |
| 4-Fctr Alpha Medium | | | 0.001 | | | | 0.011 | |
| 4-Fctr Alpha High | | | 0.916 | | | | 0.359 | |
| Total Return Low | | | 0.053 | | | | 0.349 | |
| Total Return Medium | | | 0.049 | | | | 0.008 | |
| Total Return High | | | 0.038 | | | | 0.250 | |

Table 3 presents results from pooled regressions of quarterly net fund flow on lagged fund characteristics. The first specification is estimated from the entire sample of retail and institutional domestic equity funds. The second specification is estimated from the subsample that only includes retail and institutional funds from matched twin pairs. The dependent variable is percentage quarterly fund flow ($t=0$ to $t=3$). In addition to variables previously described, the independent variables include concurrent percentage quarterly flow to funds with the same investment objective (net of own fund flow), the concurrent quarterly flow in the twin fund (equal to zero for all non-twin funds), and two different measures of performance: total return and 4-factor alpha calculated over the previous 36 months. Both performance measures use a piecewise linear performance specification with kinks at the 20$^{th}$ and 80$^{th}$ percentiles of returns, so the *Low* variables account for returns at or below the 20$^{th}$ percentile, the *Medium* variables account for returns above the 20$^{th}$ percentile and at or below the 80$^{th}$ percentile, and the *High* variables account for returns above the 80$^{th}$ percentile. For the total return measure, the percentiles are calculated within date and investment objective similar to Sirri and Tufano (1998), while the 4-factor alpha percentiles are calculated within date only. With these percentiles, the formula for the low return is LowRet=Min(0.2, RetPtile), the formula for the medium return is MedRet=Min(0.6, RetPtile-LowRet) , and the formula for the high return is HighRet=RetPtile-MedRet-LowRet. The regressions allow for separate coefficients for the retail and institutional funds. For both specifications, $p$-values from difference in coefficients tests across the retail and institutional coefficients for the expense ratio, total return, and 4-factor alpha are provided at the bottom of the table. Standard errors are clustered by fund and by date, and the total number of quarterly fund observations and the adjusted R-squared are provided.

investors and that the difference is statistically highly significant at a $p$-value of 0.005.

Carhart (1997) shows that poor risk-adjusted performance is persistent and that funds with high total returns exhibit mean reversion. As a result, investors should avoid funds with poor risk-adjusted performance and they should not chase past total returns. Retail investor flow is convex in both total return and our measure of risk-adjusted performance, four-factor alpha. These results are consistent with the results of Sirri and Tufano (1998), who show a similar non-linearity with flow responding more positively to high returns (greater

than 80$^{th}$ percentile) than to low returns (lower than 20$^{th}$ percentile). In stark contrast, institutional investor flow is unrelated to good raw performance and is more sensitive to poor risk-adjusted performance (*4-Fctr Alpha Low*) than to good (*4-Fctr Alpha High*). As the *p*-values at the bottom of the table indicate, the difference between the retail and institutional response to poor risk-adjusted performance is statistically significantly different from zero. For total return, there is no statistically significant response to good raw performance for institutional fund flow. Overall, the control variables have the expected signs and our results are consistent with Del Guercio and Tkac (2002). The coefficient on family size is positive and on fund size negative. This is consistent with larger fund family size proxying for higher visibility or lower search costs for investors. The negative sign on fund size is consistent with Berk and Green's (2004) and Chen et al.'s (2004) diseconomies of scale arguments. The positive and significant coefficients on investment objective flows and lagged fund flows are consistent with previous evidence of herding behavior (i.e., Sirri and Tufano 1998) within an investment objective and strong positive fund flow autocorrelation generated by automated investment programs such as 401(k), 403(b), 529, or other tax-deferred investment programs. We also include the concurrent flow to the fund's twin (*Twin Fund Qtrly Pct Flow*) in the regression to control for transfers from retail to institutional funds by the same investors or unobserved flow determinants common to both retail and institutional investors. The coefficient on concurrent flow to the fund's twin is positive and significant, suggesting some commonality in both retail and institutional flows that our other independent variables do not capture.

Specification 2 of Table 3 shows the results from regressions examining the determinants of fund flow in the matched sample only. Comparing the expense ratio coefficients for institutional and retail funds, we see that institutional flows are ten times more sensitive to expenses ($-0.060$ vs. $-0.0063$) as retail funds and that the difference is statistically significant, with a *p*-value of 0.009.

In the matched sample specification, retail flows are sensitive to both the high and low total return coefficients, and institutional flows are not statistically related to total returns. Looking at the four-factor alpha coefficients, we see that the institutional flows are not only more sensitive to poor risk-adjusted performance than retail flows, but also that the piecewise linear specification for institutional flows exhibits concavity in stark contrast to the observed retail flow performance convexity. In other words, institutional investors respond with greater sensitivity to poor risk-adjusted performance than they do to good past risk-adjusted performance and the difference in the retail and institutional response to poor risk-adjusted performance is strongly significant. The coefficients on the control variables are generally of the same magnitude as in the first specification, with lower statistical significance because of the lower number of observations.

Overall the evidence from flows is compelling. Institutional investors respond more sensitively to variables that predict returns, namely, expenses

and poor risk-adjusted performance. In addition, they avoid total return-chasing behavior to a greater degree than their retail counterparts. Given this evidence, it is possible that institutional investors play an important role in disciplining fund managers through market governance.

## 3. Performance Results

The previous section demonstrates that institutional fund investors indeed exhibit stronger market governance. But do retail investors benefit from the presence of institutional investors via twin fund arrangements? In this section, we estimate three performance regressions to answer the question. We first provide evidence in Section 3.1 that retail funds with institutional twins have better monthly performance using the entire sample of retail mutual funds, carefully controlling for characteristics known to affect mutual fund returns.

Mutual fund families have discretion over whether to offer a twin institutional and retail fund, and hence our baseline specification is subject to endogeneity concerns. In Section 3.2, we estimate a model that first specifies a selection equation for the probability of a fund advisor offering an institutional twin to an existing retail twin, and then includes a Heckman correction in the monthly performance regression in the second stage.

In Section 3.3, we use a particular subset of our sample of twin funds to provide additional support for an increase in performance of retail funds with twin institutional funds. In 132 out of 463 twin observations, the retail mutual fund was created before the institutional twin and the institutional twin was created during our sample period. For this sample, we can examine fund characteristics before and after the institutional twin was created. We use propensity score matching techniques to compare the change in the 36-month risk-adjusted retail fund performance for retail twin funds around the creation of the institutional twin (treatment group) with changes in performance of a carefully matched sample of funds with similar characteristics, but no institutional twin (control group).

### 3.1 Baseline performance results

We examine the relationship between the addition of an institutional twin and retail fund performance using the full set of retail funds (i.e., retail funds with and without twins). To do this, we estimate a regression of future monthly fund performance on lagged fund characteristics known to affect expected returns and on several institutional–retail twin indicator variables.

The dependent variable is the fund's one-month forward-looking four-factor alpha. This is calculated using the factor loadings estimated over the prior 36 months of data ($t-1$ to $t-36$). Using these factor loadings and the factor

realizations for time $t$, a monthly benchmark return is calculated:

$$r_t^{Benchmark} = \beta_{t-1,t-36}^{MKT} r_t^{MKT} + \beta_{t-1,t-36}^{SMB} r_t^{SMB} + \beta_{t-1,t-36}^{HML} r_t^{HML} + \beta_{t-1,t-36}^{Moment} r_t^{Moment}. \quad (1)$$

Taking the difference between the monthly fund return at time $t$ and the monthly benchmark return gives the one-month forward-looking four-factor alpha.

The independent variables include the natural log of fund TNA and fund family TNA, turnover, the expense ratio, an indicator variable equal to one if the fund is broker sold (indicated by the presence of a front or rear load or a 12b-1 fee), an indicator variable equal to one if the fund is passively indexed, lagged quarterly percentage net flows to the fund, and flow volatility as measured by the standard deviation of monthly net flows to the fund over the previous 12 months.

Our key independent variables measure the existence of an institutional twin. We split the observations of retail funds with institutional twins into three different categories. The *Inst. 1st* variable is equal to one if the retail fund has an institutional twin and that twin was created before the retail fund. The *Retail 1st–After Inst. Fund Created* indicator variable is equal to 1 if the retail fund has an institutional twin, the twin was created after the retail fund, and if the date of the observation is after the institutional fund was created (e.g., the retail fund was created on 1/1/2000, the institutional fund was created on 1/1/2004, and the date of the return observation is 1/1/2005). The *Retail 1st–Before Inst. Fund Created* indicator variable is equal to 1 if the retail fund has an institutional twin, the twin was created after the retail fund, and the date of the observation is on or before the date on which the institutional fund was created (e.g., the retail fund was created on 1/1/2000, the institutional fund was created on 1/1/2004, and the date of the return observation is 1/1/2002).

The panel regressions include time-fixed effects (specifications 1–3) and fund family fixed effects (specifications 2 and 3) as indicated at the bottom of the table. Standard errors are clustered by fund and date. The $p$-value of a difference in coefficients test comparing the before and after versions of the *Retail 1st* indicator variable coefficients is shown in the last row of Table 4 and measures whether the performance of the retail fund is statistically significantly improving after the creation of an institutional twin.

Table 4 shows the regression results. Looking at the twin indicator results of specifications 1 and 2, the coefficient on *Inst. 1st* is positive and strongly significant in every regression. The interpretation of this result is that retail funds in the sample that were created as clones of an existing institutional fund outperform other retail funds. Because the dependent variable is four-factor risk-adjusted monthly performance, the coefficients of 0.1090 and 0.1319 indicate that the retail funds outperformed by between 10.90 and 13.19 basis points per month or between 1.31% and 1.58% per year. This is consistent

**Table 4**
**Determinants of fund performance**

| Regression | 1 | | 2 | | 3 | |
|---|---|---|---|---|---|---|
| Log(Fund TNA)$_{t-1}$ | −0.0251 | (−3.7) | −0.0439 | (−4.5) | −0.0426 | (−4.6) |
| Log(Family TNA)$_{t-1}$ | 0.0090 | (2.4) | −0.0666 | (−3.5) | −0.0605 | (−3.1) |
| Expense Ratio$_{t-1}$ | −0.0133 | (−0.4) | 0.0122 | (0.3) | 0.0103 | (0.3) |
| Turnover$_{t-1}$ | −0.0001 | (−1.2) | 0.0000 | (−0.2) | 0.0000 | (−0.1) |
| Broker-Sold ID$_{t-1}$(=Yes) | −0.0455 | (−2.7) | −0.0404 | (−2.1) | −0.0389 | (−1.9) |
| Index Fund ID$_{t-1}$(=Yes) | 0.0399 | (1.0) | 0.0800 | (2.3) | 0.0854 | (2.4) |
| Qtrly Pct Flow$_{t-4,t-1}$ | 0.1831 | (2.5) | 0.1275 | (1.7) | 0.1158 | (1.6) |
| Monthly Pct Flow Volatility$_{t-12,t-1}$ | −0.0590 | (−2.2) | −0.0410 | (−1.7) | −0.0461 | (−1.9) |
| Twin$_{t-1}$(=Yes for Retail 1st & Before Inst. Fund Created) | 0.0243 | (1.1) | 0.0308 | (1.3) | 0.0421 | (1.3) |
| Twin$_{t-1}$(=Yes for Retail 1st & After Inst. Fund Created) | 0.0932 | (4.8) | 0.1157 | (5.0) | 0.1401 | (3.7) |
| Twin$_{t-1}$(=Yes for Inst. 1st/ Inception Before Jan 1996) | 0.1090 | (4.9) | 0.1319 | (6.0) | 0.1033 | (4.3) |
| Observations | 214,360 | | 214,360 | | 192,148 | |
| R-Squared | 7.34% | | 7.86% | | 7.54% | |
| Calendar Fixed Effects | Yes | | Yes | | Yes | |
| Fund Family Fixed Effects | No | | Yes | | Yes | |
| Backfill/Survivorship Bias Filter | No | | No | | Yes | |
| Diff. Test $p$-Value | 0.012 | | 0.002 | | 0.007 | |

Table 4 contains the results from regressions of monthly fund performance on lagged fund characteristics. The dependent variable is the fund's 1-month forward-looking 4-factor alpha using factor loadings estimated over the prior 36 months of data ($t-1$ to $t-36$). In addition to variables previously described, the independent variables include an indicator variable of whether the fund is broker-sold (1=Yes) and whether it is an index fund (1=Yes), flow volatility as measured by the standard deviation of monthly net inflows over the previous 12 months, and indicator variables related to the existence of an institutional twin. *Twin$_{t-1}$(=Yes for Inst. 1st/Inception Before Jan 1996)* is an indicator variable equal to one if the institutional twin was created before the retail fund or the institutional fund was created before the sample start date of January 1996. *Twin$_{t-1}$(=Yes for Retail 1st & Before Inst. Fund Created)* is an indicator variable equal to one if the retail fund was created before the institutional fund, and if the monthly retail fund return observation predates the creation of the institutional twin fund. *Twin$_{t-1}$(=Yes for Retail 1st & After Inst. Fund Created)* is an indicator variable equal to one if the retail fund was created before the institutional fund, and if the monthly retail fund return observation is measured after the creation of the institutional twin fund. Specification 3 only includes retail mutual fund twin returns for which there is no potential bias in the corresponding institutional twin observation (see Section 3.1 for details). Standard errors are clustered by fund and date. The $p$-value of a difference in coefficients test between the before and after versions of the *Retail 1$^{st}$* indicator variables is given in the bottom row.

with a number of different interpretations, including the ability of institutional investors to select superior managers/strategies, the use of superior managers by fund families to manage institutional assets, or the additional monitoring by investors in an institutional twin resulting in improved performance.

The *Retail 1$^{st}$–Before Inst. Fund Created* indicator is positive, but not statistically different from 0. The result indicates that retail funds that will eventually create a twin do not have a statistically significantly higher monthly performance than non-twin funds over the period leading to the creation of the institutional twin, once we control for all characteristics. The coefficient on *Retail 1$^{st}$–After Inst. Fund Created* is positive, and statistically and economically significant. The coefficients of 0.0932 and 0.1157 indicate that the twin retail funds outperform non-twin funds by between 1.12% and 1.39% per year after the institutional twin is created.

The comparison between the *Retail 1$^{st}$–Before Inst. Fund Created* coefficient and the *Retail 1$^{st}$–After Inst. Fund Created* coefficient shows that retail fund

performance improves after the creation of an institutional twin. The coefficient on *Retail 1st–After Inst. Fund Created* is statistically significantly larger than the *Before Inst. Fund Created* coefficient in specifications 1 and 2, as indicated by the $p$-values comparing the difference in the "Before" and "After" indicator variable coefficients listed at the bottom of the table. This result suggests that once the institutional twin is created, the risk-adjusted performance of the twin retail fund significantly improves, despite the negative impact of increased assets under management due to the scale diseconomies documented by Chen et al. (2004) and others.

A potential concern with the performance analysis of specifications 1 and 2 in Table 4 is the possibility of a survivorship or backfill bias affecting the results. Because separate accounts are outside the scope of the Investment Company Act of 1940, they are not subject to the same reporting requirements. As a result, the separate account data in the Morningstar database are provided on a voluntary basis and may be subject to both survivorship and backfill biases. Note that the data used for the performance analysis in Table 4 come from the retail mutual fund database that does not suffer from these biases. However, if the identification of retail funds with twins is based on the separate account twin data, which are subject to biases, it may also affect our results for the twin retail fund indicators.

To alleviate survivorship and backfill concerns, we construct a bias-free sample and repeat the return regressions using this sample.[21] The bias-free sample includes observations where the institutional twin is an institutional mutual fund (these are taken from the regular Morningstar mutual fund database and are free of survivorship or backfill bias concerns) or a separate account after August 2001 and after that separate account started reporting to the Morningstar Separate Account database. Specification 3 of Table 4 shows results using the bias-free sample. After removing potentially survivorship or backfill biased observations, the results are qualitatively and quantitatively similar to what we found before.

### 3.2 Heckman correction

Khorana and Servaes (1999) show that the decision to open a new mutual fund is related to a number of variables, including fund family size, flows to the

[21] Our separate account database is constructed from annual snapshots from the Morningstar Principia separate account database. The annual snapshots eliminate the possibility of survivorship bias because funds that are removed from the database in later periods are included in earlier snapshots. Because the first snapshot of the annual database covers the period of August 2001 to September 2002, but our retail fund sample starts in 1997, any separate account performance observations from before August 2001 could potentially have a survivorship bias. We therefore classify any mutual fund observations with a separate account twin before August 2001 as a potentially biased observation. Separate from the survivorship bias concern is that of backfill bias. If investment advisors only report the returns of their separate accounts that are successful, and if they are allowed to backfill the returns of those accounts, the twin funds that are matched with these separate accounts may have significant outperformance due to an upward bias in the separate account performance. Fortunately, the Morningstar separate account database provides the year and month in which each account was added to the database, which allows us to remove backfilled observations.

family, the investment objective, and the superior past performance of other fund offerings by the same family. Because these variables may be related to future fund performance, if the decision to open an institutional twin is related to similar factors, it raises the possibility that our baseline specification is subject to endogeneity concerns. To address these concerns, we use two different methods. First, in this subsection, we employ the insights of Heckman (1979) to account for the endogeneity of the fund family decision to offer a twin fund. Second, in Section 3.3, we use a propensity score approach, matching retail funds with an institutional twin with like funds without a twin.

Since we are interested in the performance of retail mutual funds with a twin relative to retail mutual funds without a twin (and we observe the performance of both types of funds), in this subsection we use the treatment effects model of Maddala (1983) that builds on the Heckman self-selection model. The treatment effect regressions are estimated using Heckman's (1979) two-step procedure. We first estimate a probit regression in which the dependent variable is equal to one if the retail fund has an institutional twin in month $t-1$, and zero otherwise. The second-stage regression is the monthly performance regression, and includes a Heckman correction term.[22] Because we are interested in the decision to offer an institutional twin to an existing retail mutual fund, we exclude in the regressions of this subsection all sample observations in which the institutional twin was created first.

In Table 5, Panel A contains the results of the first stage selection equation; Panel B contains the results of the second-stage performance regressions. Panel A shows that the probability that a retail fund has an institutional twin fund in any given month is increasing in the past performance of the retail mutual fund as measured by the four-factor alpha over the past 36 months, and the size of the retail fund. If the fund is broker-sold, the probability increases as well.

Panel B in Table 5 shows the results of the monthly performance regressions that now includes the Heckman correction term (*Lambda*). The coefficient on the correction term is only significant in one of the three specifications. Accordingly, the indicator variable $Twin_{t-1}$ *(=Yes for Retail 1st–After Inst. Fund Created)* continues to be strongly statistically and economically significant. Depending on the specification, the annualized excess performance of a retail fund after an institutional twin was created relative to a no-twin retail fund is between 1.06% and 1.74%, which is similar to the results reported in Table 4.[23]

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[22] For details, please see Maddala (1983) and Greene (2011). If the selection and treatment equations contain the same explanatory variables, identification only comes from the nonlinearity of the inverse Mills ratio (the Heckman correction term). We include the following variables in the first-stage probit model that we do not include in the second-stage regression to help identification: past performance, past inflows, tracking error, and family institutional TNA indicator variable.

[23] We also carry out the analysis separately for the two different twin types: institutional mutual fund and separate account. The performance difference between the after and before retail twin creation indicator is a strongly statistically significant 1.06% in the case of separate accounts and a weakly significant 0.88% in the institutional mutual fund case.

**Table 5**
**Heckman correction, selection, and performance regressions**

Panel A: First stage probit regression

| | Coef. | *t*-Stat |
|---|---|---|
| Intercept | −9.404 | (−17.2) |
| 4-Factor Alpha$_{t-36,t-1}$ | 0.164 | (2.4) |
| Annual Pct Flow$_{t-12,t-1}$ | −0.015 | (−0.8) |
| Log(Fund TNA)$_{t-1}$ | 0.162 | (5.2) |
| Log(Family TNA)$_{t-1}$ | −0.026 | (−1.1) |
| Log(Family Institutional TNA)$_{t-1}$ | 0.016 | (0.8) |
| Family Institutional TNA ID$_{t-1}$ (=Yes) | −0.326 | (−0.7) |
| Expense Ratio$_{t-1}$ | −0.021 | (−0.2) |
| Turnover$_{t-12,t-1}$ | 0.000 | (0.9) |
| Tracking Error$_{t-36,t-1}$ (4-Factor Model) | −6.770 | (−1.2) |
| Broker-Sold ID$_{t-1}$(=Yes) | 0.229 | (2.2) |
| Index Fund ID$_{t-1}$(=Yes) | −0.266 | (−1.2) |
| Observations | 191,993 | |
| Pseudo R-Squared | 10.28% | |
| Calendar Fixed Effects | Yes | |
| Inv. Obj. Fixed Effects | Yes | |

Panel B: Second stage fund performance regression

| Regression | 1 | | 2 | | 3 | |
|---|---|---|---|---|---|---|
| Log(Fund TNA)$_{t-1}$ | −0.0611 | (−2.9) | −0.0556 | (−2.4) | −0.0533 | (−2.3) |
| Log(Family TNA)$_{t-1}$ | 0.0165 | (2.7) | −0.0626 | (−3.0) | −0.0611 | (−2.9) |
| Expense Ratio$_{t-1}$ | −0.0169 | (−0.5) | 0.0067 | (0.2) | 0.0057 | (0.2) |
| Turnover$_{t-1}$ | −0.0002 | (−1.5) | 0.0000 | (−0.2) | 0.0000 | (−0.3) |
| Lambda$_{t-1}$ | −0.1747 | (−2.0) | −0.0484 | (−0.5) | −0.0460 | (−0.4) |
| Broker-Sold ID$_{t-1}$(=Yes) | −0.0770 | (−3.2) | −0.0531 | (−2.0) | −0.0489 | (−1.8) |
| Index Fund ID$_{t-1}$(=Yes) | 0.1016 | (2.4) | 0.1039 | (2.3) | 0.1014 | (2.2) |
| Qtrly Pct Flow$_{t-4,t-1}$ | 0.1816 | (2.4) | 0.1271 | (1.6) | 0.1213 | (1.7) |
| Monthly Pct Flow Volatility$_{t-12,t-1}$ | −0.0510 | (−1.9) | −0.0382 | (−1.5) | −0.0418 | (−1.6) |
| Twin$_{t-1}$(=Yes for Retail 1st & Before Inst. Fund Created) | 0.0216 | (1.0) | 0.0376 | (1.6) | 0.0477 | (1.4) |
| Twin$_{t-1}$(=Yes for Retail 1st & After Inst. Fund Created) | 0.0886 | (4.6) | 0.1216 | (5.1) | 0.1446 | (3.7) |
| Observations | 191,993 | | 191,993 | | 180,512 | |
| R-Squared | 7.30% | | 7.83% | | 7.62% | |
| Calendar Fixed Effects | Yes | | Yes | | Yes | |
| Fund Family Fixed Effects | No | | Yes | | Yes | |
| Backfill/Survivorship Bias Filter | No | | No | | Yes | |
| Diff. Test *p*-Value | 0.014 | | 0.003 | | 0.007 | |

Table 5 gives results from the first and second stage of a treatment effects model. Panel A of the table shows a probit regression of whether or not a retail mutual fund has an institutional twin in any given month on characteristics of the retail fund (selection equation). In addition to variables previously described, the probit regression includes the natural log of the family's institutional TNA estimated by aggregating the TNA of all separate accounts and institutional mutual funds listed in the Morningstar database for that fund family, an indicator variable for whether or not the family has any institutional assets under management in the Morningstar database, and the annualized tracking error, defined as the root mean square of the monthly return difference between the fund and the 4-factor adjusted benchmark, calculated over the previous 36 months. Panel B presents the results from regressions of monthly fund performance on lagged fund characteristics. The dependent variable is the fund's 1-month forward-looking four-factor alpha using factor loadings estimated over the prior 36 months of data ($t-1$ to $t-36$). In addition to variables previously described, the independent variables include lambda, the Heckman correction, and is based on the inverse Mills ratio. Specification 3 only includes retail mutual fund twin returns for which there is no potential bias in the corresponding institutional twin observation (see Section 3.1 for details). Standard errors are clustered by fund and date. The *p*-value of a difference in coefficients test between the before and after versions of the *Retail 1st* indicator variables is in the bottom row.

### 3.3 Propensity score matched sample

While the tests in Sections 3.1 and 3.2 use the entire sample of retail funds, we focus in our last test of fund performance on a subset of the sample. For 132 of our 463 twin funds, the institutional twin is created after the retail fund and the creation date occurs during our sample (1996 to 2009). Using the creation of an institutional twin as identification, we compare the change in performance of a given retail mutual fund before and after its institutional twin is created (treatment group) with the change in performance of an otherwise similar retail mutual fund that does not create a twin (control group). To identify the control sample, we use a propensity score matching technique that was pioneered by Rosenbaum and Rubin (1983) and has been used recently in the finance literature by, for example, Drucker and Puri (2005) and Aggarwal et al. (2009). As discussed in Section 3.1, if the decision to open a twin fund is related to past flows, the size of the fund family, or other fund characteristics that potentially play a role in the future performance of the fund, our performance result is subject to an important endogeneity concern. The propensity score approach allows us to address this concern by modeling the decision to open a twin fund and to select a control group of funds that most closely resemble our treatment group on these dimensions.

In the first stage of the analysis, we calculate each firm's propensity score, which is equal to the probability that a retail mutual fund with given characteristics creates a twin institutional fund in the next year. In the second stage of the analysis, each retail mutual fund that chooses to create an institutional twin fund (the treated group) is matched with retail mutual funds that have the closest propensity scores, but did not choose to create an institutional twin (the control group).[24] We then calculate the average change in the 36-month four-factor alpha across the event for the treated group and compare it with the average change in the 36-month four-factor alpha of the control group.

The coefficients for the first-stage probit regression are given in Panel A of Table 6. The control variables include an intercept, fund performance (*4-Factor Alpha*), fund flow, fund size, and the size of the fund family's retail and institutional assets under management, an indicator variable of whether or not the family manages any other institutional assets, expense ratio, turnover, tracking error of the fund's returns relative to the four-factor model determined benchmark, and indicator variables for the distribution channel (*Broker-Sold*) and whether or not the fund is an index fund.[25] The decision to create an

[24] Propensity scores are calculated for all fund-month observations using the coefficients from the propensity score probit model, and the control group is chosen from the set of funds with the closest matching propensity score to the treatment group from the same year and month.

[25] While we include investment objective fixed effects in the propensity score model and both treatment and control funds are domestic equity funds, we do not require the specific investment objective (i.e., aggressive growth, growth, growth & income, equity income, or small company) of the control funds to match the treatment group. To ensure that our results are robust to this concern, we repeat the propensity score matching analysis but we

**Table 6**
**Propensity score matching**

Panel A: Propensity score probit estimation

| | Coef. | | *t*-Stat |
|---|---|---|---|
| Intercept | −5.349 | | (−8.8) |
| 4-Factor Alpha$_{t-36,t-1}$ | −0.035 | | (−0.4) |
| Annual Pct Flow$_{t-12,t-1}$ | 0.070 | | (2.3) |
| Log(Fund TNA)$_{t-1}$ | 0.132 | | (5.1) |
| Log(Family TNA)$_{t-1}$ | −0.003 | | (−0.1) |
| Log(Family Institutional TNA)$_{t-1}$ | −0.002 | | (−0.1) |
| Family Institutional TNA ID$_{t-1}$ (=Yes) | 0.194 | | (0.4) |
| Expense Ratio$_{t-1}$ | 0.113 | | (1.3) |
| Turnover$_{t-12,t-1}$ (%) | 0.000 | | (0.1) |
| Tracking Error$_{t-36,t-1}$ (4-Factor Model) | −2.229 | | (−0.4) |
| Broker-Sold ID$_{t-1}$(=Yes) | 0.206 | | (2.3) |
| Index Fund ID$_{t-1}$(=Yes) | −0.082 | | (−0.5) |
| Observations | | 13,603 | |
| Pseudo R-Squared | | 6.68% | |
| Calendar Fixed Effects | | Yes | |
| Inv. Obj. Fixed Effects | | Yes | |

Panel B: Matched sample comparison

| | Treatment | | Control | | Diff. Test *p*-value | |
|---|---|---|---|---|---|---|
| | Mean | Median | Mean | Median | Mean | Median |
| 4-Factor Alpha | −0.37% | −0.86% | −0.17% | −0.77% | 0.433 | 0.581 |
| Quarterly Net Flow (%) | 10.7% | 1.0% | 8.9% | 0.2% | 0.163 | 0.221 |
| Log(Fund TNA) | 20.4 | 20.4 | 20.5 | 20.4 | 0.377 | 0.976 |
| Log(Family TNA) | 22.7 | 23.0 | 22.8 | 23.3 | 0.340 | 0.269 |
| Log(Family Institutional TNA) | 15.9 | 22.6 | 16.3 | 22.7 | 0.313 | 0.922 |
| Family Has Institutional Funds (=Yes) | 67.0% | – | 69.3% | – | 0.178 | – |
| Expense Ratio (Annual) | 1.32% | 1.26% | 1.29% | 1.26% | 0.102 | 0.220 |
| Turnover (%) | 86.2% | 70.5% | 84.5% | 62.0% | 0.594 | 0.015 |
| Tracking Error (4-Factor Model) | 5.22% | 4.60% | 5.30% | 4.66% | 0.441 | 0.324 |
| Broker-Sold (=Yes) | 78.6% | – | 76.9% | – | 0.275 | – |
| Index Fund (=Yes) | 3.6% | – | 4.4% | – | 0.318 | – |

Panel A of the table shows the results of a probit regression of whether or not an institutional twin is created for a given retail mutual fund in the following year, based on characteristics of the retail fund from the previous year. The $t$ subscripts for the variables refer to months relative to the start of the year in which the institutional twin fund is created (i.e., for a twin creation any month in 2008, $t-1$ would refer to one month before January 2008, or more simply, December 2007). Using the propensity scores from this model, we construct a control sample of the 10 funds from the same time period with the closest propensity scores to the treatment group. Panel B compares the sample statistics for the treatment group and the control group. In addition to the mean and median, the table gives the $p$-values from a $t$-test of the difference in means and a sign test of the difference in medians.

institutional twin is related to three factors. Larger past fund flows and a larger fund size increase the probability of creating an institutional twin next year. If a fund is broker-sold, the probability of creating a twin increases as well.

It is surprising that the fund families' prior experience managing institutional assets is not significantly related to the probability of offering an institutional twin. It is important to note that the probit regression is *not* estimating the probability of the fund family opening any type of institutional product but

require that the control sample is selected from the subset of funds with the same investment objective as the treatment fund with similar results.

rather it is estimating the probability of the fund family opening only a specific type of institutional product: the twin of an existing retail fund. The fund family's institutional TNA variables measure the aggregate assets of all institutional products offered by the family as captured by the separate account and institutional mutual fund databases described earlier and not just twin funds. The majority of institutional assets in the Morningstar database are invested in funds that are not twins.

Given the results of Del Guercio and Tkac (2002) and Goyal and Wahal (2008) on the role of performance as a determinant of institutional fund flows and the hiring of institutional managers, respectively, it is also surprising that the risk-adjusted past performance of the fund is unrelated to the decision to create a twin. However, both fund flows and fund size, alternative measures of market demand, are positively related to the probability of creating a twin fund. Additionally, a comparison of the average risk-adjusted performance in the summary statistics for the treatment and control groups in Panel B of Table 6 with the average risk-adjusted performance of the overall population (cf. Table 1, Panel C) shows that the risk-adjusted performance of both treatment and control group is significantly higher.

Despite the low R-squared of the propensity score model, Panel B of Table 6 shows that the matching works well for all of the fund and family characteristics examined. The differences in past performance, fund flow, expenses, fund and family size, and other characteristics between the treatment and control sample are economically small and statistically insignificant.

Table 7 shows the levels of the four-factor risk-adjusted performance of retail funds in the three years before an institutional twin is created and the three years after an institutional twin is created, as well as the changes in performance around the event. It then compares the levels of and changes in risk-adjusted performance of this treatment group with the levels of and changes in risk-adjusted performance of the propensity score matched control group. The requirement of a six-year window decreases the sample size from the original 132 to 98 retail-first funds. For the three years before the institutional twin is created, the average retail mutual fund in the treatment sample has a negative annualized four-factor alpha of $-0.373\%$. The control group has a risk-adjusted performance of $-0.170\%$ over the same period, and the two are not statistically different (since we matched on past performance in the first stage, this result is not surprising). For the three years after the institutional twin is created, the treatment sample averages risk-adjusted performance of a statistically insignificant $-0.038\%$, and the change in performance across the event is a statistically insignificant $0.335\%$. The control sample, on the other hand, has a statistically significant negative alpha of $-1.383\%$ during the three years after the twin is created, and these funds average a $1.213\%$ deterioration in performance. Comparing the increase in performance for the treatment group of $+0.335\%$ with the decrease in performance for the control groups of $-1.213\%$, the retail funds outperform the matched sample after the addition of their

**Table 7**
**Performance, propensity score matched sample**

| | | Caliper Length | Max # Matches | # Twin Funds | Total Matches | Before | After | (After-Before) |
|---|---|---|---|---|---|---|---|---|
| 4-Factor Alpha | Treatment (Inst. Twin) | None | 10 | 98 | 980 | −0.373%** | −0.038% | 0.335% |
| | Control (Matched Fund) | | | | | −0.170% | −1.383%*** | −1.213%*** |
| | (Treatment-Control) | | | | | −0.203% | 1.345%*** | 1.548%*** |
| 4-Factor Alpha | Treatment (Inst. Twin) | 0.005 | 10 | 98 | 971 | −0.413%* | 0.011% | 0.423% |
| | Control (Matched Fund) | | | | | −0.260% | −1.481%*** | −1.221%*** |
| | (Treatment-Control) | | | | | −0.153% | 1.491%*** | 1.644%*** |
| 4-Factor Alpha | Treatment (Inst. Twin) | 0.005 | 5 | 98 | 481 | −0.408% | 0.005% | 0.414% |
| | Control (Matched Fund) | | | | | −0.463% | −1.389%*** | −0.926%** |
| | (Treatment-Control) | | | | | 0.055% | 1.394%*** | 1.339%** |
| 4-Factor Alpha | Treatment (Inst. Twin) | 0.001 | 10 | 98 | 949 | −0.498%** | 0.009% | 0.507%* |
| | Control (Matched Fund) | | | | | −0.265% | −1.456%*** | −1.191%*** |
| | (Treatment-Control) | | | | | −0.233% | 1.464%*** | 1.698%*** |
| 4-Factor Alpha | Treatment (Inst. Twin) | 0.001 | 5 | 98 | 474 | −0.485% | 0.025% | 0.510% |
| | Control (Matched Fund) | | | | | −0.473% | −1.360%*** | −0.888%** |
| | (Treatment-Control) | | | | | −0.012% | 1.385%*** | 1.398%** |
| 4-Factor Alpha (Backfill/ Survivor. Bias Filtered) | Treatment (Inst. Twin) | None | 10 | 53 | 530 | −0.406% | 0.138% | 0.543%* |
| | Control (Matched Fund) | | | | | 0.132% | −1.276%*** | −1.408%*** |
| | (Treatment-Control) | | | | | −0.537%* | 1.414%*** | 1.951%*** |

Table 7 contains results of a propensity score matched sample analysis of the change in performance for retail funds before and after an institutional twin is added (treatment group). The sample consists of 98 retail funds for which a twin institutional fund is created during our sample period, and for which we have return data in the three years before and after the creation of the institutional twin. The table includes results for 4-factor alphas over the 36 months before and after the event. To identify an appropriate control group, we use propensity scores from the probit model described in Table 6, Panel A. Using the propensity scores from this model, we construct several control samples. The results we report in the first block are based on nearest neighbor matching using ten observations from the same time period with the closest propensity score to create a control group. The second (third) block shows results using a tolerance level on the maximum propensity score distance or caliper length of 0.005 to avoid the risk of bad matches. We then create a control group for each treated fund with up to 10 (5) observations that are within the maximum distance. In the fourth and fifth blocks, we further reduce the maximum propensity score distance to 0.001 and again use up to the 10 and 5 nearest observations. The last block of results is from a sample in which observations that are potentially subject to both backfill and survivorship bias have been removed and are based on nearest neighbor matching using ten observations to create a control group. For each specification, the average outperformance for both the treatment and control groups, before and after the addition of the institutional twin for the treatment group, is given. The differences between treatment and control groups before and after the event are given in the bottom (*Treatment-Control*) row. The differences in the before and after estimates for the treatment and control groups are given in the last column (*After-Before*). The intersection of the bottom row and the last column gives the difference-in-difference estimate for each variable. *** significant at 1%; ** significant at 5%; and * significant at 10%.

institutional twins by a statistically and economically significant risk-adjusted 1.548% per year.

While the econometric methods are very different across Sections 3.1– 3.3, the results from the propensity score matched sample are very similar to the results on performance improvements we observed in our tests that use the whole sample (1.18% to 1.92%, depending on the specification).

The first set of results is based on nearest neighbor matching using ten observations from the control group and excluding observations without common support. To show the robustness of our results, we repeat the analysis with different specifications. In the second (third) row of Table 7, we use a

tolerance level on the maximum propensity score distance or caliper length of 0.005 to avoid the risk of bad matches, and then match each treated fund with up to the closest 10 (5) nearest observations. In the fourth and fifth rows, we further reduce the maximum propensity score distance to 0.001. As can be seen from these alternative specifications, our propensity score matched return results are very robust.

The last row in Table 7 shows the result when the propensity score matching is repeated, but excluding all retail twin funds that are subject to potential survivorship and backfill bias. This leaves 53 twin institutional fund creations. Using this sample, we find that the retail mutual funds of the treatment group actually improve performance across the addition of an institutional twin, by a statistically significant 0.54%. Because the performance of the control group deteriorates at the same time, the treatment group outperforms the control group by a statistically and economically significant 1.95%.

The deterioration in performance of the control group in the after period relative to before is not surprising given the evidence in Carhart (1997) that above-average fund performance reverts to the mean over time. Sirri and Tufano (1998) and Chen et al. (2004) point to a likely driver of this performance decrease. Given the superior performance of both the treatment and control group funds (−0.37% and −0.17%) in the three-year "before" period relative to the retail fund population average (−1.46%), we would expect these funds to have substantial inflows and the corresponding increase in fund size[26] to negatively affect performance. While the control fund exhibits such a pattern after an institutional twin is created, the treatment group performance does not deteriorate, and in some specifications, there is evidence of a statistically significant improvement in performance.

## 4. Potential Channels for Increased Monitoring

While the evidence in Section 3 about the better performance of retail mutual funds with twins is compelling, it does not shed light on the mechanism through which the creation of an institutional twin could benefit retail investors. Using the same propensity score techniques as those used in Section 3.3, we now examine potential channels through which increased monitoring of institutional assets by institutional investors could affect retail twins.

Because the retail fund and the institutional twin are separate portfolios of different size and somewhat different portfolio composition, there is no mechanical reason why changes made to the institutional fund's fee structure and portfolio composition would result in changes to the retail fund's fee structure and composition. Legal and regulatory guidelines, however, help us identify why retail investors might benefit from better oversight of the institutional investors.

[26] Comparing the three-year average TNA during the before and after periods, both the treatment and control groups have fund size increases of 30 to 40%.

First, a legal precedent for performance differences between twin funds is Young *v*. Nationwide Life Insurance Company.[27] In this case, the shareholders of a variable annuity life insurance fund successfully sued the life insurance fund sponsor on the basis of differences in performance between the mutual fund and its variable annuity fund twin. The case establishes fund liability for differences in performance.

Second, in negotiating fees with the advisor, mutual fund boards can use a comparison of fees charged by other funds or fees charged to other clients such as pension funds or other institutional investors. While the SEC has required boards since 1994 to disclose the material factors used and the rationale for approving an advisory contract (such as a fee comparison),[28] the SEC modified the disclosure requirements in 2004 to specifically require that boards discuss their use of fee and service comparisons, in addition to a small list of other factors. Hence, by allowing lower fees (either direct or indirect) for the institutional fund and not discussing such lower fees when setting the fees of the retail twin fund, a board of directors may expose itself to legal liability and violate its fiduciary duties. However, such a discussion of low institutional fees in the context of retail fees may lead to downward pressure on retail twin fund fees.

Third, in their analysis of the Jones et al. *v*. Harris Associates L.P. case, the U.S. Supreme Court makes clear in their reinstatement of the Gartenberg standard that fee comparisons are relevant for excessive fee determinations assuming the services provided are comparable: "First, since the Act requires consideration of all relevant factors, §80a–35(b)(2), courts must give comparisons between the fees an investment adviser charges a captive mutual fund and the fees it charges its independent clients the weight they merit in light of the similarities and differences between the services the clients in question require."[29]

While the focus on much of the legal literature surrounding the Gartenberg standard is on the components of the expense ratio, such as advisory fees, and the difference in services provided to retail and institutional investors covered by those fees, the statute is broader and includes any "payments of a material nature," suggesting inclusion of indirect expenses as well.

In addition to establishing the legal and regulatory ramifications of differences between twin funds, these three reasons identify possible channels through which institutional investor monitoring of one portfolio could affect a twin retail portfolio, namely, direct expenses, indirect expenses, and manager effort. In Table 8, we explore these channels by analyzing changes in the net expense ratios, return gap, active share, and other relevant fund characteristics, before and after the addition of an institutional twin. The first row explores

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[27] Young *v*. Nationwide Life Ins. Co. – 2 F.Supp.2d 914 (S.D. Tex. 1998).

[28] 59 Federal Register 52689 (Oct. 19, 1994).

[29] Jones et al. *v*. Harris Associates L.P. 559 U.S. ___ (Docket No. 08-586) (2010).

**Table 8**
**Mechanisms, propensity score matched sample**

| | | Obs. | Before | After | (After-Before) |
|---|---|---|---|---|---|
| Net Expense Ratio (NSAR) | Treatment (Inst. Twin) | 82 | 1.215%*** | 1.162%*** | −0.053%*** |
| | Control (Matched Fund) | 820 | 1.268%*** | 1.271%*** | 0.003% |
| | (Treatment-Control) | | −0.053%** | −0.109%*** | −0.056%*** |
| Return Gap (Annualized) | Treatment (Inst. Twin) | 105 | 0.321%*** | 0.579%*** | 0.259% |
| | Control (Matched Fund) | 1,050 | −0.073% | −0.366%*** | −0.293%* |
| | (Treatment-Control) | | 0.394%** | 0.946%*** | 0.552%*** |
| Brokerage Commission Rate | Treatment (Inst. Twin) | 82 | 0.096%*** | 0.097%*** | 0.000% |
| | Control (Matched Fund) | 820 | 0.107%*** | 0.151%*** | 0.045% |
| | (Treatment-Control) | | −0.011%** | −0.055% | −0.044% |
| Percent of Funds Using Soft Dollars for Distribution | Treatment (Inst. Twin) | 82 | 23.26%*** | 17.44%*** | −5.81%*** |
| | Control (Matched Fund) | 820 | 21.40%*** | 19.19%*** | −2.21%*** |
| | (Treatment-Control) | | 1.86% | −1.74% | −3.60%*** |
| Active Share (36 Month) | Treatment (Inst. Twin) | 85 | 76.7%*** | 76.4%*** | −0.30% |
| | Control (Matched Fund) | 850 | 76.2%*** | 75.3%*** | −0.97%*** |
| | (Treatment-Control) | | 0.44% | 1.11% | 0.67%* |
| Avg. # Analysts Estimates per Holding | Treatment (Inst. Twin) | 112 | 13.71*** | 13.62*** | −0.088** |
| | Control (Matched Fund) | 1,120 | 12.93*** | 13.04*** | 0.113*** |
| | (Treatment-Control) | | 0.78*** | 0.57*** | −0.201*** |

Table 8 contains results of a matched sample analysis of the change in expense ratios and other fund characteristics for retail funds before and after an institutional twin is added (treatment group). To identify the matched sample, we use propensity scores from the probit model described in Table 6, Panel A. Using the propensity scores from this model, we construct a control sample of the 10 funds from the same time period with the closest propensity scores to each fund from the treatment group. For each variable, the average for both the treatment and control groups, before and after the addition of the institutional twin for the treatment group, is given. The differences between treatment and control groups, before and after the event are given in the bottom (*Treatment-Control*) row. The differences in the before and after estimates for the treatment and control groups are given in the last (*After-Before*) column. The intersection of the bottom row and the last column gives the difference-in-difference estimate for each variable. The asterisks denote statistical significance in the following manner: *** significant at 1%; ** significant at 5%; and * significant at 10%. The first row shows results for the net annual expense ratio from the funds' semi-annual SEC N-SAR filing. The second row analyzes the return gap of Kacperczyk, Sialm, and Zheng (2008) calculated as the difference between the actual fund return and the return inferred from fund holdings (less expenses). The third and fourth rows show the brokerage commission rate calculated as the total brokerage commissions paid (Q21 for the series) divided by the sum of the manager's total purchases and sales (the sum of Q71.A and Q71.B for all funds in the series from the N-SAR filing), and the percentage of funds using soft dollar or commission bundled payments to pay for fund distribution (Q26.A). The fifth row shows results for the active share measure of Petajisto (2010) and Cremers and Petajisto (2009), which is a measure of the overlap between the fund's holdings and the closest related index. The sixth row shows results for the value-weighted average number of analyst earnings estimates, where the weight is the percentage of the equity portfolio held in each stock and the analyst following data is from IBES. For active share, the average over the three years before and after the event is analyzed. For all other variables, the average is taken over the year before and after the event.

changes in a fund's direct expenses, namely the expense ratio, before and after the creation of an institutional twin. The data for this analysis are taken from a database of SEC N-SAR filings that is described in Edelen, Evans, and Kadlec (2012). The expense ratio of the treatment group decreases after the creation of the institutional twin, by 5.3 basis points on average. Because the expense ratio of the control group increases over the same time period by 0.3 basis points, we find a total control group adjusted change in the net expense ratio of 5.6 basis points.[30]

[30] We report results on the expense ratio using the N-SAR data for consistency because brokerage commissions and soft dollar distributions are calculated from N-SAR as well. Using Morningstar data to calculate the levels

Separate from the direct expenses, indirect costs that are not included in the expense ratio, such as brokerage commissions, trading/implementation costs, and the like, could also affect performance. There is evidence that indirect expenses are opaque to retail investors (e.g., Edelen, Evans, and Kadlec 2012), but if institutional investors are aware of them, monitoring of those expenses to ensure they are not excessive could benefit retail investors. To measure these indirect costs, we use the return gap measure of Kacperczyk, Sialm, and Zheng (2008). The return gap is the difference between the actual fund return and the return of the fund based on the previously disclosed holdings minus the fund's expense ratio. By comparing the actual fund return to the return on a hypothetical portfolio return constructed from a buy and hold strategy of the fund's last disclosed holdings, the return gap measures the aggregate value added (positive return gap) or destroyed (negative return gap) by a manager's actions above and beyond the fund's expense ratio. The second row of Table 8 examines the return gap of the treatment and control fund samples. It shows that the annualized return gap is positive for the treatment sample, i.e., these funds on average provide greater hidden benefits than costs, but negative for the control sample. There is also some evidence that fund advisors choose funds with a favorable return gap when they add an institutional twin: Prior to the creation of the institutional twin, the difference in return gap between treatment and control group is a positive 0.321% annually. More importantly, we see substantial improvement in the return gap (i.e., greater outperformance relative to the return on previously disclosed holdings), which almost doubles after the institutional twin creation for the treatment sample. Finally, we observe deterioration in the return gap for the control sample. The difference-in-difference effect is an economically large improvement in the return gap measure of 0.552% annualized. It is important to note that in addition to measuring indirect costs, the return gap is also a potential measure of manager effort/skill, capturing short-term performance enhancement/degradation due to trading. Consequently, the return gap result is also consistent with increased manager effort translating to improved fund performance.

The return gap is an aggregate of many unobserved managerial actions that are difficult to directly measure. However, we have data for two components of the return gap, brokerage commissions and soft dollar usage, and in Table 8 we examine how these components change before and after the addition of an institutional twin. Both brokerage commissions (Q21) and the use of soft dollars to pay for distribution (Q26.A) variables come from the N-SAR filings. Edelen, Evans, and Kadlec (2012) show that both of these costs are strongly negatively related to future performance and that they are opaque to retail investors. Recognizing their impact on performance, an institutional investor with greater awareness of these costs might discourage their use by the investment advisor,

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and changes in expense ratios increases the sample to 123 funds, and leads to a difference-in-difference of 3.3 basis points, statistically significantly different from zero at the 1% level.

and the investment advisor, for reasons outlined at the beginning of the section, might implement these changes for retail funds, too. Looking at the results in rows 3 and 4 of Table 8, we do not find a statistically significant change in brokerage commission rates. However, after the creation of a twin, we observe that the proportion of retail funds that use soft dollars decreases by 5.8%. Because the fraction of funds using soft dollar distributions decreases for the control group as well, we find an overall highly statistically significant difference-in-difference effect of −3.6%. Relative to the fraction of funds using soft-dollar distribution prior to the creation of the twin fund, the decrease is 15.5% and appears economically significant.

In addition to decreasing fees and increasing the return gap, performance could also be improved through increased managerial effort resulting in superior stock selection. Because the return gap compares the actual fund return to the buy and hold return on the previously disclosed portfolio holdings (i.e., the benchmark for the manager is the manager's own previously disclosed portfolio), it abstracts from the value added through a manager's superior stock selection.[31] While it is difficult to directly measure managerial effort, we attempt to proxy for it by examining portfolio characteristics before and after the institutional twin is added as a proxy. First, we examine active share, a measure of the overlap between the fund's holdings and the closest related index developed by Cremers and Petajisto (2009) and Petajisto (2010). Cremers and Petajisto (2009) construct active share as the percentage of portfolio holdings that differ from the benchmark index holdings. For mutual funds, active share typically varies between 0% (pure index funds) and 100%, and Cremers and Petajisto (2009) report an average active share of approximately 63% in 2002. They show that funds with the highest active share significantly outperform their benchmarks, both before and after expenses. Our second proxy is the value-weighted average number of analyst estimates. It attempts to capture whether fund managers invest in stocks with greater analyst coverage, an indication of less effort, or less analyst coverage, consistent with greater effort.

Row 5 of Table 8 shows the results for *Active Share*, measured over the 36 months before and after the creation of the institutional twin.[32] Prior to the creation of the institutional twin, the treatment and control groups both have an active share of between 76% and 77%, which are statistically indistinguishable from each other. The change in active share for the treatment group is statistically indistinguishable from zero, while the change in active share for the control group has a negative and statistically significant point

[31] It does include the value added through the manager's intra-holding period trades. In contrast, the performance measurement methodology used in Daniel et al. (1997) and Wermers (2000) uses the return on a portfolio of characteristic-based stocks as the benchmark and not the manager's portfolio itself.

[32] We measure active share over a three-year horizon, because the asset allocation of funds may be difficult to change short term. When we measure changes in Active Share annually to examine robustness of this result, we find a difference-in-difference result that is approximately half of the reported result, and statistically significantly different from zero at the 10% level.

estimate of approximately 1% (i.e., more overlap with the benchmark or less active stock selection on the part of the manager). The difference-in-difference result shows that there is a statistically significant increase in active share for the treatment group relative to the control group.[33]

Row 6 of Table 8 shows that, prior to the event, treatment firms hold, on average, stocks with more analyst coverage. After the institutional twin is added, on average the treatment group holds stocks with slightly lower analyst coverage and the control group holds stocks with greater analyst coverage, showing a significant decrease in the value-weighted number of analysts of the treatment sample relative to the control. While the changes in analyst coverage of held stocks are relatively small, the evidence is consistent with managers making a greater effort to identify stocks.

We repeat, but do not report, the propensity score analysis of the variables in Table 8 with different caliper lengths (0.001 and 0.005) and different number of maximum matches (5 and 10), as in Table 7, to ensure the robustness of our results. Our results are quantitatively and qualitatively similar to those reported in Table 8.

To summarize, while our matched sample analysis shows that adding an institutional twin is associated with decreased direct expenses for retail mutual funds, the effect is small (0.056% annually) relative to the overall impact on annual performance we measured using the same sample in Section 3.3 (1.548%). The fee analysis compares the first year before and after the addition of a twin, and the performance results compare the three years before and after, and fees might continue to decrease in years two and three. However, it seems unlikely that they would decrease enough in those additional years to account for the full 1.548% performance difference. In contrast, the return gap analysis shows a performance improvement of 0.552% after adding an institutional twin, just over a third of the performance improvement. Separate from direct expenses and the indirect expenses measured by the return gap, increased managerial effort in selecting equities in the portfolio as a result of the increased monitoring could also improve performance. While we cannot measure increased effort and any resulting performance improvement directly, our results using proxies for managerial effort are consistent with this interpretation. Overall, our analysis suggests some channels through which institutional investor monitoring could improve performance in twin retail mutual funds.

## 5. Robustness

We demonstrate in Section 3 that retail mutual fund performance improves after the creation of an institutional fund twin. Relative to a carefully chosen control

[33] Given the passive nature of index funds, including them in our analysis may weaken the difference results. In unreported tests, we repeat the active share analysis after removing index funds from the treatment and control samples and find that the improvement in active share is a statistically significant 0.89%.

group, retail fund performance increases and fees decrease after the creation of the twin. We argue in Section 4 that this effect is consistent with increased monitoring of the fund by institutional investors. To provide further support for our hypothesis, in Section 5.1 we examine the results of a placebo experiment and in Section 5.2 we repeat the flow and performance analyses separating out institutional share classes.

### 5.1 Placebo experiment

Our sample of 463 twin funds also contains 179 twins in which the retail fund was created *after* the institutional fund. Given our hypothesis of provision of greater market governance by institutional investors relative to retail investors, we would not expect to observe market governance driven increases in performance in institutional funds after the creation of retail funds. If we were to observe performance regression results for institutional funds similar to those reported in Table 5 or difference-in-difference results similar to those reported in Table 7, we would have to reject the monitoring hypothesis in favor of a hypothesis that other factors in the contractual environment (e.g., larger fund, economies of scale related to fees) are responsible for the reported changes.

We analyze our placebo experiment using both the Heckman two-stage approach used in Table 5 and the propensity score matched sample approach in Tables 6 and 7. For the Heckman regression, Panel A of Table 9 gives the results from the first-stage probit regression in which the dependent variable is equal to one if the institutional fund has a retail twin in month $t-1$, and zero otherwise. Panel B contains the results from the second-stage performance regression where a Heckman correction term is included.

Panel A of Table 9 shows that the probability that a retail twin fund exists in any given month is increasing in the past performance of the institutional fund as measured by the four-factor alpha over the past 36 months and the size of the institutional fund. The indicator for whether or not the fund family has any other retail funds is also positive, indicating that families that already have retail fund offerings are more likely to create a retail twin for one of their institutional offerings.

Panel B of Table 9 shows the results of the monthly performance regressions that now include the Heckman correction term (*Lambda*). Consistent with our evidence on the monitoring value of institutional investors relative to retail investors, there is no statistically significant performance improvement after a retail twin is added. Comparing the other coefficients with the retail fund regression results in Panel B of Table 5, the biggest difference is the insignificant coefficient on institutional family size in specification 1. As we explained earlier, because the Morningstar database does not capture all institutional assets, our measure of family institutional TNA may be understating true family

**Table 9**
**Heckman correction, selection and placebo performance regressions**

Panel A: First stage probit regression

| | Coef. | *t*-Stat |
|---|---|---|
| Intercept | −3.326 | (−4.5) |
| 4-Factor Alpha$_{t-36,t-1}$ | 0.135 | (1.9) |
| Annual Pct Flow$_{t-12,t-1}$ | −0.004 | (−0.4) |
| Log(Fund TNA)$_{t-1}$ | 0.066 | (2.2) |
| Log(Family Institutional TNA)$_{t-1}$ | −0.006 | (−0.5) |
| Log(Family TNA)$_{t-1}$ | −0.031 | (−1.2) |
| Family Retail TNA ID$_{t-1}$ (=Yes) | 1.327 | (2.2) |
| Expense Ratio$_{t-1}$ | −0.175 | (−0.8) |
| Turnover$_{t-12,t-1}$ (%) | 0.001 | (1.0) |
| Tracking Error$_{t-36,t-1}$ (4-Factor Model) | −0.797 | (−0.7) |
| Index Fund ID$_{t-1}$(=Yes) | 0.272 | (1.0) |
| Observations | 98,185 | |
| Pseudo R-Squared | 7.32% | |
| Calendar Fixed Effects | Yes | |
| Inv. Obj. Fixed Effects | Yes | |

Panel B: Second stage fund performance regression

| Regression | 1 | | 2 | | 3 | |
|---|---|---|---|---|---|---|
| Log(Fund TNA)$_{t-1}$ | −0.0248 | (−3.4) | −0.0285 | (−2.4) | −0.0292 | (−3.1) |
| Log(Family Institutional TNA)$_{t-1}$ | −0.0006 | (−0.4) | −0.0235 | (−2.8) | −0.0230 | (−2.7) |
| Expense Ratio$_{t-1}$ | −0.1540 | (−4.1) | −0.0241 | (−0.4) | −0.0413 | (−0.7) |
| Turnover$_{t-1}$ | −0.0005 | (−1.3) | −0.0005 | (−1.2) | −0.0007 | (−2.1) |
| Lambda$_{t-1}$ | 0.0015 | (0.0) | 0.0504 | (0.3) | 0.0544 | (0.4) |
| Index Fund ID$_{t-1}$(=Yes) | 0.0553 | (1.3) | 0.0407 | (0.8) | 0.0197 | (0.5) |
| Twin$_{t-1}$(=Yes for Inst. 1st & Before Retail Fund Created) | 0.0271 | (0.3) | −0.0297 | (−0.3) | −0.0567 | (−0.9) |
| Twin$_{t-1}$(=Yes for Inst. 1st & After Retail Fund Created) | 0.0190 | (0.7) | 0.0087 | (0.3) | 0.0102 | (0.5) |
| Observations | 98,185 | | 98,185 | | 82,271 | |
| R-Squared | 7.28% | | 8.30% | | 6.17% | |
| Calendar Fixed Effects | Yes | | Yes | | Yes | |
| Fund Family Fixed Effects | No | | Yes | | Yes | |
| Backfill/Survivorship Bias Filter | No | | No | | Yes | |
| Diff. Test *p*-Value | 0.934 | | 0.676 | | 0.270 | |

Table 9 shows results from the first and second stage of a treatment effects model. Panel A of the table shows a probit regression of whether or not an institutional fund has a retail twin in any given month on characteristics of the institutional fund (selection equation). In addition to variables previously described, the probit regression includes the natural log of the family's retail TNA estimated by aggregating the TNA of all retail mutual funds listed in the Morningstar database for that fund family, an indicator variable for whether or not the family has any retail assets under management in the Morningstar database. Panel B presents the results from regressions of monthly fund performance on lagged fund characteristics. The dependent variable is the fund's 1-month, forward-looking, 4-factor alpha using factor loadings estimated over the prior 36 months of data ($t-1$ to $t-36$). The independent variables include indicator variables related to the existence of a retail twin. *Twin*$_{t-1}$ *(=Yes for Inst.1st - Before Retail Fund Created)* is an indicator variable equal to one if the retail fund was created before the institutional fund, and if the monthly retail fund return observation predates the creation of the institutional twin fund. *Twin*$_{t-1}$ *(=Yes for Retail 1st - After Inst. Fund Created)* is an indicator variable equal to one if the retail fund was created before the institutional fund, and if the monthly retail fund return observation is measured after the creation of the institutional twin fund. Specification 3 only includes institutional fund twin returns for which there is no potential survivorship or backfill bias (see Section 3.1 for details). The reported results include date and fund family fixed effects as indicated at the bottom of the table. Standard errors are clustered by fund and date. The *p*-value of a difference in coefficients test between the before and after versions of the *Inst. 1st* indicator variables is in the bottom row.

size and as a result it may not be capturing potential economies of scale for families with a larger institutional presence.[34]

In addition to the Heckman regressions, we also repeat our placebo analysis using a propensity score matching approach. To set up the propensity matched placebo test, we first estimate a probit model predicting which institutional funds will create a twin retail fund, using characteristics similar to those used in Panel A of Table 6.[35] The estimates from this probit model are given in Panel A of Table 10. In Panel B, we also give a comparison of the treatment and control sample characteristics to assess the quality of the matching between them. The main drivers of the decision to create a retail twin fund are high past performance, large retail family TNA, and a high expense ratio. Panel B of Table 10 shows that the matching works well. The means and medians of fund characteristics for treatment and control group are statistically indistinguishable from each other with the exception of four-factor alpha. However, given the lack of performance persistence documented by Busse, Goyal, and Wahal (2010) for institutional investment funds, this difference in the treatment and control sample should not bias our results.[36]

Using this matched sample, we examine the addition of a retail twin fund and the corresponding change in the risk-adjusted performance of the institutional fund relative to a control sample, similar to Table 7. The results of this analysis are included in Table 11. We see that the treatment sample, i.e., those institutional twin funds that add a retail twin, had risk-adjusted performance of 3.28% per year prior to the event. Given the focus of retail investors on past good performance that we examined in Table 3 and the ability of fund sponsors to use the performance of the institutional predecessor account in advertising the retail twin with certain required disclosures,[37] it is perhaps not surprising that the treatment sample outperforms the control sample by a statistically significant 1.12% per year. Consistent with diseconomies of scale documented by Chen et al. (2004), both treatment and control funds exhibit significant declines in their risk-adjusted performance in the 36 months after. In the three years after adding a retail twin, the difference in performance

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[34] In contrast to the performance regressions in Tables 4 and 13 and Panel B of Table 5, we do not include past flow and flow volatility in the performance regression in Table 9. Because the flow data are only available quarterly, adding these variables decreases the sample size significantly. However, we have repeated the analysis in Table 9 including these variables, and the twin creation results are qualitatively and quantitatively unchanged. These results are available upon request.

[35] While the probit model in Table 4 also included fund turnover and an indicator variable of whether or not the fund was sold by a broker, these variables are excluded in the model in Table 7. The institutional data do not provide information about broker distribution, and including the turnover variable does not affect the results but reduces the sample size significantly.

[36] We also repeat the analysis including only the four-factor alpha in the propensity score probit model. By only including this variable in the propensity score model, we eliminate the difference in the treatment and control sample four-factor alphas, but the difference-in-difference performance results are largely unchanged. These results are available upon request.

[37] Pierce (1998) outlines the SEC's criteria for allowing mutual fund sponsors to adopt and advertise the performance record of an unregistered predecessor account.

**Table 10**
**Propensity score matching—placebo experiment**

Panel A: Propensity score probit estimation

| | Coef. | | *t*-Stat |
|---|---|---|---|
| Intercept | −2.487 | | (−3.6) |
| 4-Factor Alpha$_{t-36,t-1}$ | 0.284 | | (3.2) |
| Quarterly Pct Flow$_{t-3,t-1}$ | 0.075 | | (1.4) |
| Log(Fund TNA)$_{t-1}$ | −0.002 | | (−0.1) |
| Log(Family Institutional TNA)$_{t-1}$ | −0.013 | | (−0.6) |
| Log(Family Retail TNA)$_{t-1}$ | 0.021 | | (4.3) |
| Family Retail TNA ID$_{t-1}$ (=Yes) | 0.108 | | (0.2) |
| Expense Ratio$_{t-1}$ | 0.328 | | (1.9) |
| Tracking Error$_{t-36,t-1}$ (4-Factor Model) | 4.410 | | (0.8) |
| Index Fund ID$_{t-1}$(=Yes) | −0.103 | | (−0.5) |
| Observations | | 8,868 | |
| Pseudo R-Squared | | 10.42% | |
| Calendar Fixed Effects | | Yes | |
| Inv. Obj. Fixed Effects | | Yes | |

Panel B: Matched sample comparison

| | Treatment | | Control | | Diff. Test *p*-value | |
|---|---|---|---|---|---|---|
| | Mean | Median | Mean | Median | Mean | Median |
| 4-Factor Alpha | 3.28% | 1.51% | 2.16% | 0.72% | <0.001 | 0.003 |
| Quarterly Net Flow (%) | 19.6% | 2.8% | 17.2% | 1.3% | 0.511 | 0.430 |
| Log(Fund TNA) | 19.8 | 20.0 | 19.7 | 20.0 | 0.508 | 0.918 |
| Log(Family Institutional TNA) | 19.2 | 22.1 | 18.9 | 21.6 | 0.450 | 0.022 |
| Log(Family Retail TNA) | 15.5 | 21.1 | 15.5 | 20.2 | 0.952 | 0.510 |
| Family Has Retail Funds (=Yes) | 85.9% | – | 85.1% | – | 0.532 | – |
| Expense Ratio | 0.27% | 0.08% | 0.30% | 0.07% | 0.086 | 0.267 |
| Tracking Error (4-Factor Model) | 5.91% | 4.83% | 5.93% | 4.85% | 0.884 | 0.560 |
| Index Fund (=Yes) | 4.7% | – | 6.0% | – | 0.228 | – |

Panel A of the table shows the results of a probit regression of whether or not a retail twin is created for a given institutional fund in the following year, based on characteristics of the institutional fund from the previous year. Using the propensity scores from this model, we construct a control sample of the 10 institutional funds from the same time period with the closest propensity scores to the treatment group. Panel B compares the sample statistics for the treatment group and the control group. In addition to the mean and median, the table gives the *p*-values from a *t*-test of the difference in means and a sign test of the difference in medians.

between the treatment and control funds is a statistically insignificant 0.112% per year. Overall, the difference-in-difference results show that the change in performance of institutional funds with added retail twins compared to the change in performance of the control group is negative. This result stands in sharp contrast to the performance improvement of retail funds observed in Table 7 and further supports the role of institutional investor monitoring as an explanation for our results.[38] As we discussed in Section 3.1, separate accounts are not subject to the same reporting requirements as mutual funds. As a result, the separate account data in the Morningstar database are provided on a voluntary basis and may be subject to both survivorship and backfill biases.

[38] Because the separate account institutional twins are not subject to the Investment Company Act of 1940 and are therefore not required to report N-SARs, we do not have access to the variables necessary to repeat the analysis of Table 8 for our placebo sample.

**Table 11**
**Performance analysis—placebo experiment**

| | | Obs. | Before | After | (After-Before) |
|---|---|---|---|---|---|
| 4-Factor Alpha | Treatment (Retail Twin) | 85 | 3.28%*** | 0.117% | −3.16%*** |
| | Control (Matched Fund) | 850 | 2.16%*** | 0.005% | −2.16%*** |
| | (Treatment-Control) | | 1.12%*** | 0.112% | −1.01%*** |
| 4-Factor Alpha | Treatment (Retail Twin) | 24 | 0.06% | −0.84%*** | −0.91%* |
| (Backfill/Survivorship | Control (Matched Fund) | 240 | −0.12% | −0.59%* | −0.47% |
| Bias Filtered) | (Treatment-Control) | | 0.18% | −0.25% | −0.44% |

Table 11 contains results of a matched sample analysis of the change in risk-adjusted performance for institutional funds before and after a retail twin is added (treatment group). To identify the matched samples, we use propensity scores from the probit model described in Table 10. Using the propensity scores from this model, we construct a control sample of the 10 institutional funds from the same time period with the closest propensity scores to each fund from the treatment group. The results we report in the first block are based on nearest neighbor matching using ten observations from the same time period with the closest propensity score to create a control group. The second block of results is from a sample in which observations that are potentially subject to both backfill and survivorship bias have been removed and are based on nearest neighbor matching using ten observations to create a control group (see Section 3.1 for details). The average for both the treatment and control groups, before and after the addition of the retail twin for the treatment group, is given. The differences between treatment and control groups before and after the event are given in the bottom (*Treatment-Control*) row. The differences in the before and after estimates for the treatment and control groups are given in the last (*After-Before*) column. The intersection of this bottom row and the last column gives the difference-in-difference estimate for each variable. The asterisks denote statistical significance in the following manner: *** significant at 1%; ** significant at 5%; and * significant at 10%. The table includes results for 4-factor alphas over the 36 months before and after the event.

Using the filters described in Section 3.1, we repeat our analysis for a backfill-free and survivorship-bias-free sample, and the results are given in row 2 of Table 11. With the filtered data, the performance in the 36 months before the retail twin creation is substantially lower and there is no difference between the treatment and control. However, consistent with the observed increase in assets under management, both the treatment and control groups experience decreased performance in the 36 months after and we find no statistically significantly difference in performance between the treatment and control groups.

## 5.2 Institutional share classes

In addition to the placebo test described in Section 5.1, institutional share classes provide us with another test of the potential role of institutional investors as monitors. To this point in the paper, we have categorized mutual funds that have only retail share classes and mutual funds that have both institutional share classes and retail share classes into one group that we term "retail" in our main empirical analysis. While our more cautious classification should bias us against finding any difference between retail funds with and without twins, one could argue that a mutual fund that has significant assets in institutional share classes would also benefit from institutional oversight and thus should be classified as a retail fund with an institutional twin. At the same time, anecdotal evidence[39] suggests that investors in institutional share classes may

[39] For example, a recent Harvard Business School case study on the Yale Investment Office describes: "Yale's commitment to this asset class was tested in 1998, when many hedge funds suffered in the 'flight to liquidity'

not be as sophisticated as separate account or pure institutional mutual fund investors. A separate institutional fund or a separate account has the advantage that it shields the institution's investment from potential trading inefficiencies created by retail investor flow volatility. Additionally, in their comparison of retail mutual funds and institutional accounts, the Investment Company Institute (2006) also excludes institutional share classes of mutual funds from their definition of institutional accounts, choosing instead to group them with other retail share classes because they "share many common attributes with retail share classes of mutual funds" (Investment Company Institute 2006, 1, footnote 2). Unlike institutional accounts, the ICI notes that investors in both retail and institutional share classes demand and pay for a host of additional services that would likely not be of interest to more sophisticated institutional investors, including "availability of a website, 24/7 phone contact, ability to exchange among funds in the same complex, checking account features…" (Investment Company Institute 2006, 5) in addition to other administrative activities, marketing and distribution services, transfer agency, and custody. They also point out that given the much larger size of institutional accounts at inception and the much smaller number of investors, they experience economies of scale almost immediately. A mutual fund, on the other hand, may need to be subsidized by the advisor at inception and would require investment in the infrastructure necessary to market and distribute the fund and to handle the large number of small investors required to achieve sufficient scale. Large, sophisticated institutional investors, which we think are the most likely to provide oversight, would be aware of these advantages and disadvantages, and we would expect they would opt for a separate account or a separate investment vehicle instead of the institutional share class. Hence, we would expect our results on market governance to be stronger for separate account and institutional fund twins than for institutional share class twins.

We repeat the analysis of flow determinants in Table 3, separating out flows to retail funds/share classes, institutional share classes, and separate accounts/institutional funds, to provide further evidence for this conjecture. To do this, we identify any fund that has both retail and institutional share classes. We then aggregate the variables for the retail share classes for that fund each month into a single retail fund-month observation and similarly for the institutional share classes and treat these two fund-month observations as a retail-institutional twin fund pair. The results of this flow analysis are included in Table 12. Comparing the institutional share class coefficient estimates to

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that followed Russia's August 1998 default on its debt obligations. […] Even though some of these pricing anomalies were likely to be short-lived […] a number of investors panicked and demanded the return of their capital. As a result, some funds were forced to liquidate positions at exceedingly unfavorable prices. While in most cases the university was insulated from the effects of other investors' sales because the fund managers had established separate accounts for Yale's investment, in other cases, Yale's funds were commingled with those of other investors. In these instances, Yale's returns suffered. As a result of this experience, Yale redoubled its efforts to utilize separate accounts …" (Yale University Investments Office, August 2006, Harvard Business School Case No. 807-073).

**Table 12**
**Determinants of institutional share class flows**

| Regression | 1 | | | | | |
|---|---|---|---|---|---|---|
| | *Retail Coef.* | | *SepAcct/InstMF* | | *Inst ShrClss* | |
| Intercept | 0.0908 | (0.7) | 0.4409 | (0.7) | −0.0985 | (−1.7) |
| Log(Family TNA)$_{t-1}$ | 0.0041 | (8.2) | 0.0001 | (0.0) | 0.0097 | (7.7) |
| Log(Fund TNA)$_{t-1}$ | −0.0118 | (−15.1) | −0.0252 | (−5.8) | −0.0282 | (−14.5) |
| Expense Ratio$_{t-1}$ | −0.0097 | (−3.7) | −0.0646 | (−3.1) | −0.0403 | (−6.4) |
| Turnover$_{t-1}$ | 0.0002 | (0.2) | −0.0002 | (−0.1) | −0.0083 | (−2.8) |
| InvObj Qtrly Pct Flow$_{t,t+3}$ | 0.4623 | (7.5) | 0.7874 | (2.8) | 0.6334 | (7.0) |
| Qtrly Pct Flow$_{t-4,t-1}$ | 0.4101 | (26.2) | 0.0160 | (0.5) | 0.1761 | (9.4) |
| Total Return$_{t-36,t-1}$ Low | 0.0708 | (7.9) | −0.0384 | (−0.6) | 0.1513 | (5.1) |
| Total Return$_{t-36,t-1}$ Medium | 0.0008 | (0.2) | −0.0140 | (−0.4) | 0.0141 | (1.2) |
| Total Return$_{t-36,t-1}$ High | 0.1254 | (7.4) | −0.0334 | (−0.3) | 0.1932 | (4.9) |
| 4-Fctr Alpha$_{t-36,t-1}$ Low | 0.0477 | (5.4) | 0.2469 | (3.6) | 0.0590 | (2.0) |
| 4-Fctr Alpha$_{t-36,t-1}$ Medium | 0.0188 | (2.8) | 0.0714 | (1.5) | 0.0467 | (2.2) |
| 4-Fctr Alpha$_{t-36,t-1}$ High | 0.0844 | (4.7) | 0.1621 | (3.0) | 0.1426 | (4.3) |
| Observations | | | 86,137 | | | |
| Adj. R-Squared | | | 14.91% | | | |

| Coef. Difference Test $p$-Values | Retail vs. SepAcct/InstMF | Retail vs. Inst ShrClss |
|---|---|---|
| Expense Ratio | 0.012 | $<0.001$ |
| 4-Fctr Alpha Low | 0.004 | 0.709 |
| 4-Fctr Alpha Medium | 0.255 | 0.198 |
| 4-Fctr Alpha High | 0.160 | 0.077 |
| Total Return Low | 0.087 | 0.004 |
| Total Return Medium | 0.687 | 0.258 |
| Total Return High | 0.148 | 0.084 |

Table 12 presents coefficient estimates and $t$-statistics from pooled regressions of quarterly net fund flow on lagged fund characteristics. Similar to specification 2 of Table 3, the sample includes retail fund with their corresponding separate account and institutional mutual fund twins. However, the sample also includes funds with retail and institutional share classes. For these funds, the retail share classes are aggregated and included as a separate retail fund-month observation and the institutional share classes are aggregated and included as a separate institutional fund-month observation. The regression details are the same as can be found in Table 3, except the regressions allow for separately estimated coefficients for the three different fund types: retail funds, separate accounts/institutional mutual funds, and institutional share class funds. Difference in coefficient test $p$-values across the retail and the two different institutional fund type coefficients for the expense ratio, total return, and 4-factor alpha are provided at the bottom of the table. Standard errors are clustered by fund and by date, and the total number of quarterly fund observations and the adjusted R-squared are provided.

the retail coefficient estimates, we find that while institutional share class flows are more sensitive to expense ratios than the retail flows, they exhibit little sensitivity to poor risk-adjusted performance and are not statistically or economically different from retail flows in this regard. Additionally, the institutional share class flows are actually more sensitive to total returns both for the high and low performance than the retail flows. This is consistent with institutional investors in these institutional share classes being less sophisticated than institutional investors in separate accounts or institutional funds.

Given the institutional share class flow results, we would expect that institutional share class twins do not provide as effective monitoring as the separate account and institutional mutual fund twins. As a result, we would expect the existence of an institutional share class twin to have less of an effect on performance than the other institutional twin funds. To test this, we repeat the performance regressions in Table 4, separating out twin identifiers for funds with separate account/institutional fund twins and institutional share

**Table 13**
**Determinants of fund performance and institutional share class twins**

| Regression | 1 | | 2 | |
|---|---|---|---|---|
| Log(Fund TNA)$_{t-1}$ | −0.0249 | (−4.0) | −0.0439 | (−4.9) |
| Log(Family TNA)$_{t-1}$ | 0.0065 | (1.9) | −0.0570 | (−3.1) |
| Expense Ratio$_{t-1}$ | −0.0429 | (−1.5) | −0.0304 | (−0.8) |
| Turnover$_{t-1}$ | −0.0001 | (−1.1) | 0.0000 | (−0.0) |
| Broker-Sold ID$_{t-1}$(=Yes) | −0.0431 | (−2.4) | −0.0368 | (−1.8) |
| Index Fund ID$_{t-1}$(=Yes) | 0.0276 | (0.7) | 0.0641 | (1.9) |
| Qtrly Pct Flow$_{t-4,t-1}$ | 0.2159 | (2.4) | 0.1450 | (1.6) |
| Monthly Pct Flow Volatility$_{t-12,t-1}$ | −0.0721 | (−0.8) | −0.0360 | (−0.4) |
| Inst. ShrClss | | | | |
| Twin$_{t-1}$(=Yes for Retail 1st & Before Inst. Fund Created) | 0.0827 | (3.9) | 0.0720 | (3.2) |
| Twin$_{t-1}$(=Yes for Retail 1st & After Inst. Fund Created) | 0.0225 | (1.2) | 0.0432 | (2.0) |
| Twin$_{t-1}$(=Yes for Inst. 1st/Inception Before Jan 1996) | 0.0200 | (1.2) | 0.0517 | (2.7) |
| Inst. Funds | | | | |
| Twin$_{t-1}$(=Yes for Retail 1st & Before Inst. Fund Created) | 0.0393 | (1.4) | 0.0597 | (2.1) |
| Twin$_{t-1}$(=Yes for Retail 1st & After Inst. Fund Created) | 0.1119 | (5.1) | 0.1477 | (5.7) |
| Twin$_{t-1}$(=Yes for Inst. 1st/Inception Before Jan 1996) | 0.1219 | (4.9) | 0.1627 | (6.2) |
| Observations | 208,068 | | 208,068 | |
| R-Squared | 7.32% | | 7.81% | |
| Calendar Fixed Effects | Yes | | Yes | |
| Fund Family Fixed Effects | No | | Yes | |
| Diff. Test $p$-Value (Inst. Shareclasses) | 0.007 | | 0.264 | |
| Diff. Test $p$-Value (Inst. Funds) | 0.008 | | 0.002 | |

Table 13 presents the results from regressions of monthly fund performance on lagged fund characteristics. The regression details match Table 4 except for the separate inclusion of institutional share classes. For retail funds with institutional share classes, the dependent and independent variables are calculated by value-weighting the quantities for the retail share classes only. The indicator variables related to the existence of an institutional twin are separately identified for retail funds with separate account and institutional mutual fund twins (*Inst. Funds*) and retail funds with institutional share classes (*Inst. ShrClss*). For retail funds with institutional share classes, the twin identifier is defined based on the creation date of the institutional share class in the same way as for retail funds with separate account or institutional mutual fund twins. For the two different types of institutional twins, *Twin$_{t-1}$(=Yes for Inst. 1st/Inception Before Jan 1996)* is an indicator variable equal to one if the institutional twin was created before the retail fund or the institutional fund was created before the sample start date of January 1996. *Twin$_{t-1}$(=Yes for Retail 1st - Before Inst. Fund Created)* is an indicator variable equal to one if the retail fund was created before the institutional fund, and if the monthly retail fund return observation predates the creation of the institutional twin fund. *Twin$_{t-1}$(=Yes for Retail 1st - After Inst. Fund Created)* is an indicator variable equal to one if the retail fund was created before the institutional fund, and if the monthly retail fund return observation is measured after the creation of the institutional twin fund. Funds without an institutional twin have a 0 for all six indicator variables. The reported results include date and fund family fixed effects as indicated at the bottom of the table. Standard errors are clustered by fund and date. The $p$-value of a difference in coefficients test between the before and after versions of the *Retail 1$^{st}$* indicator variables is in the bottom row.

class twins. The results of this analysis are included in Table 13. Consistent with less sophisticated investor monitoring in the institutional share classes, we find no statistically significant improvement in performance for retail funds after the creation of an institutional share class. The outperformance of retail twin funds before the creation of an institutional share class is not surprising given the flow evidence that institutional share class investors are responsive to good risk-adjusted performance. As a result, an advisor would be more likely to offer an institutional share class for retail funds with high alpha.

## 6. Conclusion

The Investment Company Act of 1940 gave investors in open-end mutual funds a unique and innovative governance mechanism — the ability to redeem. Because the decision to redeem shares and the associated loss of management's control over these assets can be undertaken independently by each investor no matter how small, investors can effectively remove the fund manager from the control of those assets. Recognizing the significant role played by "market governance," Fama and Jensen (1983) suggest that it is primary to all other fund governance mechanisms. The effectiveness of this governance mechanism in protecting shareholders, however, depends on how investors exercise their right to redeem and whether or not they respond to useful investment signals such as fees and poor past risk-adjusted performance.

Using a sample of retail mutual funds with an institutional twin, a similar but separately managed institutional fund or separate account, we examine how retail and institutional investors in similar investment products respond to these investment signals. We find that institutional investor flows are more sensitive to high fees and to poor risk-adjusted performance than retail flows. Additionally, retail investors respond more strongly to counterproductive signals like past total return than institutional investors.

We also examine the relation between the creation of an institutional twin and the performance of its retail fund counterpart. Consistent with greater monitoring on the part of the institutional twin investors, retail fund risk-adjusted performance increases by 1.5% per year if the fund manager also manages an institutional twin. Exploiting cross-sectional differences in the date the institutional and retail mutual funds were created, we examine whether institutional investors are merely better at selecting managers, or whether their presence reduces agency problems between mutual fund managers and investors. We uncover evidence consistent with the latter explanation. Fees, trading costs, and other fund expenses decrease and managerial effort increases relative to a control sample after the institutional twin is created.

Our results have important implications for recent legal and legislative developments related to excessive mutual fund fees. In 2003, Eliot Spitzer, then the New York State Attorney General, criticized investment advisors in the mutual fund industry for setting higher advisory fees for retail mutual fund investors than for clients in corresponding institutional separate accounts. More recently, a comparison of the fees between retail and institutional twin funds was at the heart of the Jones et al. *v*. Harris Associates L.P. appeal heard by the U.S. Supreme Court in 2010. Indeed, the premise behind much of the excessive fee litigation has been a breach of fiduciary duty on the part of advisors because they charge "excessively" high fees to retail clients relative to what they charge their separate account clients for the same or a very similar investment product. While the majority of these cases have been rejected by courts so far, in part on the grounds that these fee differences capture differences in the service provided

to retail and institutional clients, they have created some legal uncertainty for fund families that pursue twin arrangements. Some commentators have gone so far as to speculate whether mutual fund families will shut down and avoid such twin fund arrangements in the future to avoid litigation.

In contrast with the negative view suggested by these events, our evidence provides a positive perspective on differences in retail and institutional fund fees. If twin fund arrangements indeed provide better monitoring, it would be a justification for these fee differentials. The average expense ratio difference between institutional and retail twins in our sample is 0.42% and significantly smaller than the risk-adjusted excess performance of 1.5% of those retail funds with institutional twins. Our evidence regarding the improved performance and manager effort and the reduced direct and indirect expenses of retail twin funds suggests that monitoring by investors in institutional twins serves as an important governance mechanism for investors in retail twins.

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