



**HAL**  
open science

# Are Star Funds Really Shining? Cross-trading And Performance Shifting In Mutual Fund Families

Alexander Eisele, Tamara Nefedova, Gianpaolo Parise

► **To cite this version:**

Alexander Eisele, Tamara Nefedova, Gianpaolo Parise. Are Star Funds Really Shining? Cross-trading And Performance Shifting In Mutual Fund Families. 32nd International Conference of the French Finance Association - AFFI 2015, Jun 2015, Cergy, France. pp.42. hal-01458357

**HAL Id: hal-01458357**

**<https://hal.science/hal-01458357>**

Submitted on 23 Feb 2017

**HAL** is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.

# Are Star Funds Really Shining? Cross-Trading and Performance Shifting in Mutual Fund Families

Alexander Eisele, Tamara Nefedova, Gianpaolo Parise\*

April 17, 2016

## Abstract

We employ transaction data to examine trades between funds affiliated to the same institution. We show that such cross-trades exhibit an average mispricing of 0.18% compared to open market trades. This deviation is greater during times of high financial uncertainty, when the exchanged stocks are illiquid and highly volatile, and when the asset manager has weak governance and large internal markets. We use an exogenous increase in regulatory scrutiny to establish causality. Our results are consistent with theoretical models of internal capital markets in which the headquarters actively favors its ‘stars’ at the expense of its least valuable units.

---

\*Eisele is at the Swiss Finance Institute and the University of Lugano, Nefedova is at the Université Paris-Dauphine, and Parise is at the Bank for International Settlements. A part of this paper was written while Nefedova was at NYU Stern School of Business and Parise was a visitor at Harvard Business School. We are thankful for suggestions and useful comments to Giovanni Barone-Adesi, Utpal Bhattacharya, John Campbell, Hans Degryse, Ben Cohen, Truong Duong, Inh Tran Dieu, Rüdiger Fahlenbrach, Francesco Franzoni, Laurent Frésard, Rajna Gibson, Robin Greenwood, Harald Hau, Terrence Hendershott, Augustin Landier, Eric Nowak, Kim Peijnenburg, Alberto Plazzi, Sofia Ramos, Christian Upper, David Schumacher, Suresh Sundaresan, Youchang Wu and participants of seminars at Harvard Business School, the Bank for International Settlements, the Université Paris-Dauphine, the EFA 2015 meeting in Vienna, the AFA 2014 meeting in Philadelphia, the Swiss Winter Conference on Financial Intermediation, the AFFI 2013 and 2015 meetings, the EFMA 2013 meeting in Reading, the Lugano Corporate Finance 2012 workshop, the Gerzensee doctoral seminar, the Geneva conference on Liquidity and Arbitrage Trading 2012. We are also grateful to several practitioners for valuable information on industry practices. We acknowledge financial support from the Swiss National Science Foundation and the Swiss Finance Institute. Contact at: gianpaolo.parise@bis.org

The majority of financial trades takes place in open and highly regulated markets. As an alternative venue, large asset managers sometimes offset the trades of affiliated funds in an internal market, without relying on external facilities or supervision.<sup>1</sup> Rule 17a-7 of the U.S. Investment Company Act permits such “cross-trades” under conditions of fair valuation of the assets and equal treatment of both trading parties. In principle, these transactions are assumed to benefit the final investor as they limit transaction costs and commissions. Recent anecdotal evidence, however, warns that this might not always be the case. For instance, Pimco is believed to have absorbed flow-induced asset selling through in-house funds when Bill Gross left the firm, while Western Asset Management allegedly transferred \$6.2 million to top funds by systematically pricing cross-transactions at the highest bid price available.<sup>2</sup>

In this article, we explore how cross-trades are priced and what the rationale for relying on such off-exchange transactions is. The task of investigating cross-trades presents an empirical challenge: most of institutional investors are obliged to disclose their holdings at a quarterly frequency only, which makes it impossible to distinguish cross-trades from trades executed in opposite directions but with external counterparties. To the best of our knowledge, this paper represents the first attempt to correctly identify such cross-trades using transaction-level data.

We conduct four sets of empirical tests in order to gain a greater insight on cross-trades. First, we explore how cross-trades are priced compared to open market trades. The rationale of allowing cross-trades to benefit investors suggests that the spread between the execution price of the trade and the market price of the stock at the moment of the transaction (hereafter referred to as the *execution shortfall*) is lower for cross-trades since transaction costs are

---

<sup>1</sup>Around 40% of all U.S. stock trades are today executed outside of public exchanges. See, e.g., Comerton-Forde and Putniņš (2015), Degryse, De Jong, and Van Kervel (2015), and Buti, Rindi, and Werner (2015).

<sup>2</sup>See “Pimco May Have Averted Fire Sale After Gross’s Exit,” Bloomberg - June 11, 2015 and Security and Exchange Commission administrative proceeding No. 3-15688 of January 27, 2014. More generally, a recent Financial Times article reported a number of comments by industry participants on dubious cross-trading practices including the following: “I’m aware that (illegal cross-trading) happens, generally in equity funds but not always. I suspect it’s quite widespread” and “It has happened many times in the past, often in times of market pressure (...). In 2008 it was one way to ensure that prime money market funds would be protected”, see “No Surprise at Backroom Dealing Charge” Financial Times - December 16, 2012.

minimized. Counter to this intuition, we find that cross-trades exhibit an execution shortfall that is 18 basis points *higher* than that of trades executed in the open market after controlling for the size of the trade and stock, time, and fund-family fixed effects. This mispricing reallocates performance between the two parties involved in the trade (e.g., one fund buys at a discount from one of its siblings).

Second, we explore the characteristics and the timing of cross-trades. We find that cross-trades executed in times characterized by high uncertainty in the markets and in illiquid and highly volatile stocks are more severely mispriced. Additionally, we find evidence which suggests that a significant fraction of cross-trades is backdated. Lower regulatory scrutiny on cross-trading activity compared to open market trades could in principle allow institutions to arbitrarily set *ex post* the execution price of the cross-trade at the price of the day at which the greatest performance would have been reallocated among trading counterparties. Consistent with this hypothesis, we show that cross-trades are significantly more likely than open market trades to be executed exactly at the highest or lowest price of the day.

Third, we investigate how the execution shortfall correlates with fund family characteristics. Our null hypothesis is that family characteristics are irrelevant in explaining how cross-trades are priced. If, however, cross-trades were used to shift performance in an opportunistic way, we should find a higher execution shortfall within families for which agency problems are more relevant – namely, fund-families in which governance is weak and family incentives diverge from investors’ interests (Massa (2003), Nanda, Wang, and Zheng (2004), Chuprinin, Massa, and Schumacher (2015)). Exploring the cross-section of cross-trades, we find that the execution shortfall is significantly higher for cross-trades executed in families in which governance is weak, there are a high number of siblings and some funds are significantly more expensive than others.<sup>3</sup>

Finally, we explore how cross-trading activity affects the difference in performance be-

---

<sup>3</sup>A high number of affiliated funds creates incentives for tournament behavior (Brown, Harlow, and Starks (1996), Kempf and Ruenzi (2008)) and allows a fund family to transfer performance via cross-trades in a large internal market, while families with high heterogeneity in funds’ importance are those with the strongest incentives to reallocate performance (see Gaspar, Massa, and Matos (2006)).

tween “star” and “junk” funds (i.e., sibling funds of relatively high/low importance from a fund family perspective). Building on previous theoretical work on internal capital markets we can formulate two opposing hypotheses. In one scenario, mutual fund complexes may work as “socialist conglomerates” in which strong divisions end up subsidizing weak ones (Stein and Scharfstein (2000)). If this is true, powerful managers of poorly performing funds may force star funds to engage in inefficient cross-subsidization via badly priced cross-trades. The resulting outcome would be performance smoothing across different funds within the same fund family.

In the other scenario, the corporate headquarters of a multi-division company has control rights which enables it to engage in “winner-picking” – to actively shift resources to few successful projects (Stein (1997)). Similarly, fund families may use cross-trades to allocate extra performance to a number of popular or expensive funds. A large body of research on mutual funds suggests that outperformers, while attracting disproportionate inflows to themselves,<sup>4</sup> also have positive spillover effects on the other siblings in the family (Nanda, Wang, and Zheng (2004), Brown and Wu (2015)). This would make it potentially optimal from a family perspective to penalize less important funds in order to inflate the returns of their star funds. In line with the latter scenario, we show that the performance benefit to star funds is directly related to the extent of cross-trading activity and is accrued at the expense of junk funds.

In this paper, we make two main contributions to the existing literature. First, to the best of our knowledge this is the first paper providing direct evidence on the pricing and characteristics of actual cross-trades. The use of cross-trades is pervasive in the mutual fund industry, and recently regulators have decided to allow exemptions for cross-trading in other industries as well.<sup>5</sup> Therefore, a study on cross-trading activity not only improves our understanding of incentives at the fund-family level, but also serves to provide wider policy

---

<sup>4</sup>There is abundant evidence that outperformers attract greater inflows, e.g., Chevalier and Ellison (1997), Sirri and Tufano (1998), Agarwal, Gay, and Ling (2014).

<sup>5</sup>See, e.g., the cross-trading exemptions under section 408(b)(19) added to ERISA on August 17, 2006 by the U.S. Department of Labor.

implications. Our paper is the first to show that cross-trades are significantly mispriced and potentially backdated. Tying cross-trade level data to fund performance, we find that cross-trading appears to boost the risk-adjusted performance of star funds by around 1.7% per year on average (causing an equivalent loss for the least important funds). This result, in turn, casts doubt on the fraction of performance delivered by mutual funds that is truly due to investment skill.<sup>6</sup>

Second, we show that the introduction of tighter supervision in 2004 resulted in a significant decrease in both cross-trading activity and average execution shortfall. A lower deviation from benchmark prices limits, but does not necessarily exclude, that some performance redistribution is still today taking place (a number of cross-trades in our sample appears to be backdated even after 2004). Nonetheless, careful regulatory scrutiny seems to be highly effective in limiting both the extent of mispricing and the incentive to cross-trade within the mutual fund industry. Yet, other related off-exchange trading practices, such as trading in “dark pools” and “internalizers”, have become increasingly popular in recent years.<sup>7</sup> Our results provide additional evidence on the incentive of asset managers to rely on alternative trading venues and warn on the risks posed by unsupervised and less regulated trading activity.

This paper proceeds as follows: Section I briefly reviews the related literature and highlights the differences with our paper. Section II provides information on the data and describes how cross-trades are identified. Section III explores how cross-trades are priced, offers evidence from the cross-section of cross-trades, and tests the backdating hypothesis. Section IV documents the impact of cross-trading activity on fund performance. Section V provides further results and robustness checks. Section VI concludes.

---

<sup>6</sup>In general a large body of literature has been devoted to the study of mutual fund performance see, e.g., Kacperczyk, Sialm, and Zheng (2005), Kacperczyk, Sialm, and Zheng (2008), Massa and Patgiri (2009), Kempf, Ruenzi, and Thiele (2009), Huang, Sialm, and Zhang (2011), Ferreira, Keswani, Miguel, and Ramos (2012), Chen, Hong, Jiang, and Kubik (2013), and Brown and Wu (2015). Our results suggest that cross-trading activity significantly contributes to explaining the cross-section of fund returns.

<sup>7</sup>Recent estimates indicate that the total trading volume outside traditional trading venues more than doubled in the last six years, see “Dark markets may be more harmful than high-frequency trading” *Reuters* – April 7, 2014.

# I. Related Literature

Previous literature hypothesizes the presence of cross-subsidization in the money management industry. Gaspar, Massa, and Matos (2006) find that when sibling funds trade in the opposite direction, the performance of high-value funds (expensive or successful funds) is boosted and the performance of low value funds decreases. The authors posit that this pattern is consistent with performance shifting via cross-trading. Conversely, Schmidt and Goncalves-Pinto (2013) argue that fund families might systematically shift performance via cross-trades from popular funds (funds attracting positive investor flows) to distressed funds by absorbing flow-induced fire-sales. In both cases, the authors focus on opposite trades computed from quarterly snapshots of mandatory fund filings.<sup>8</sup> In this regard, Gaspar, Massa, and Matos (2006) state the following: “we should make clear from the start that we can only provide evidence that is limited by the level of information disclosure to which mutual fund activities are subject.”<sup>9</sup>

Additionally, a large literature explores incentives at the fund family level aside from the cross-trading setting. Evans (2010) finds that funds outperform while “incubated” but that such outperformance disappears after the funds are open to investors, while Chuprinin, Massa, and Schumacher (2015) find that in-house funds outperform outsourced funds by 0.85% annually consistent with the hypothesis of preferential treatment. Furthermore, Bhattacharya, Lee, and Pool (2013) show that affiliated funds of funds (i.e., funds that can only invest in other funds in the family) overweight their holdings in funds that are forced to sell<sup>10</sup> and Nanda, Wang, and Zheng (2004) show that fund families have a high incentive to start several new funds, increasing their chance of producing “star funds” (i.e., funds that

---

<sup>8</sup>E.g., if fund A buys 1,000 of a stock in January and fund B belonging to the same fund family sells 800 of the same stock in March, the two funds are assumed by related studies to cross-trade 800 shares, significantly overestimating the extent of cross-trading activity.

<sup>9</sup>Other papers looking at cross-trading activity are Chaudhuri, Ivkovich, and Trzcinka (2012) and Casavecchia and Tiwari (2015), however also those papers do not have information on pricing, timing, exact volume of the transactions and stock characteristics.

<sup>10</sup>While this result may seem apparently in contrast with our finding, the settings of the two papers are very different. We consider equity funds and not funds of funds, we focus on asset trades, while Bhattacharya, Lee, and Pool (2013) explore investments in the shares of distressed funds. We focus on all mutual fund families, while Bhattacharya, Lee, and Pool (2013) consider only the subset of mutual fund families that includes affiliated funds of funds.

outperform by chance).

While the evidence provided in previous studies is suggestive of the existence of opportunistic performance shifting via cross-trades, it does not necessary rule out three alternative explanations. First, differential skill or resources might explain why star funds are, on average, more likely to trade in the right direction (Guedj and Papastaikoudi (2008)). Second, reverse causality might be an issue as differences in performance may lead affiliated funds to trade in opposite directions. Third, other within-quarter unobserved actions (i.e., actions that cannot be directly inferred by quarterly filings), such as security lending, timing of interim trades, IPO allocations, and window-dressing behavior may contribute to explaining the gap in performance between star and junk funds (Kacperczyk, Sialm, and Zheng (2008)).

Since it is impossible to correctly identify cross-transactions from quarterly snapshots of fund holdings, the related literature is not able to provide any evidence on the characteristics, pricing, and popularity of cross-trades. Anand, Irvine, Puckett, and Venkataraman (2012) show that transaction costs have a significant effect on both fund returns and the persistence of relative fund performance. In this paper, we provide direct evidence that cross-trades are executed at a significantly different transaction cost compared to open market trades and we explore which cross-trades appear to be significantly mispriced.

Finally, tying our trade-level data to fund-level returns we provide evidence from actual trades for the favoritism hypothesis proposed in Gaspar, Massa, and Matos (2006) and Chuprinin, Massa, and Schumacher (2015) (instead of indirect evidence from quarterly holdings and returns). Our results cast doubt on the hypothesis that star funds provide insurance to distressed funds on average (see, e.g., Schmidt and Goncalves-Pinto (2013)) and on the assumption that the main reason why funds cross-trade illiquid positions is to lower transaction costs (Goncalves-Pinto and Sotes-Paladino (2015)).



## II. Data, Identification of Cross-Trades, and Summary Statistics

In our analysis, we focus on mutual fund families as a laboratory. While cross-trades are also common in other industries, the mutual fund setting allows us to obtain data from a large number of sources. This section describes the different datasets we use for our analysis.

### A. Trade-Level Data

We obtain trade-level data from Abel Noser Solutions/ANcerno, a consulting firm that works with institutional investors monitoring their trading costs. Batches of data sent by its clients include *all* executed trades for the whole period covered by the batch.<sup>11</sup> Previous research has shown that ANcerno institutional clients constitute approximately 8% of the total CRSP daily dollar volume (Anand, Irvine, Puckett, and Venkataraman (2012)) and that there is no survivorship or backfill bias in the data (see, e.g, Puckett and Yan (2011)). Despite ANcerno claims that all trades are disclosed, we cannot rule out that clients opportunistically choose which trades to submit or that they intentionally misreport execution prices. However, strategic reporting would bias the results against finding evidence for opportunistic pricing of the cross-trades. Therefore, our results are unlikely to be affected by opportunistic reporting or, in any case, under-represent the real extent of mispricing.

ANcerno provides us with several variables useful for our investigation: stock identifier (*cusip*), trade date, execution price, execution time,<sup>12</sup> number of shares executed, side of the trade (i.e., buy or sell), price of the stock at the time of the execution, commissions paid, and volume-weighted average price of the day (*VWAP*). ANcerno provides information both at the order (called *ticket* in Anand, Irvine, Puckett, and Venkataraman (2012)) and at the trade level. In particular, each order can be broken down into a number of trades executed

---

<sup>11</sup>Examples of other empirical studies using ANcerno include Chemmanur, He, and Hu (2009), Anand, Irvine, Puckett, and Venkataraman (2012).

<sup>12</sup>The time variable is based on a 24-hour day and is precise to the *minute* (i.e., not the *second*) level.

at different times of the day (or in some extreme cases across different days). We find the number of trades to be more than double the number of orders in our sample which is in line with Anand, Irvine, Puckett, and Venkataraman (2012). As the relevant benchmark for cross-trades is the price at which the *trade* is executed (while the price at the placement of the order is irrelevant), we conduct most of our analysis at the trade level.

This trading information is sent to ANcerno by its different clients<sup>13</sup> and the identity of the clients is always anonymized. Importantly, while the client is anonymized, the asset manager is not. For a limited period of time in 2010–early 2011 ANcerno provided its academic subscribers with the identification table “MasterManagerXref” including unique codes (*managercodes*) with associated names of the asset manager to whom they were affiliated. It is important to mention that the set of provided identification files is subscription-specific. The sample used in this study is constructed using the fullest set of identification files provided by ANcerno, to which earlier and later subscribers do not have access. The full file includes 1,088 asset managers. The additional identification file “ManagerXref” has the necessary variables to link managing companies to the trades (see Figure 3 in the Appendix). The same identification files allow us to match ANcerno data with the Thomson Reuters database unambiguously. In particular, we match fund families from ANcerno to 13F/S12 by name. For instance, the match table provided by ANcerno includes a manager name (e.g., “XYZ Capital”)<sup>14</sup> and a managercode (e.g., 10). This allows us to match the managercode number to a number of trades in ANcerno executed by funds affiliated to XYZ Capital.

Our matched database spans the time interval from 1999 to 2010. ANcerno ceased providing the identification of asset managers in 2011. Hence, we cannot conduct our analysis in the post-2010 period. Unfortunately, while ANcerno provided us with unique asset manager (family) identifiers, it does not make available unique fund identifiers. Therefore we are able to link with certainty each cross-trade to the asset manager to which the fund is affiliated

---

<sup>13</sup>A client can either be a single fund or a fund manager managing multiple funds or, alternatively, a money manager which is managing a portfolio on behalf of the client.

<sup>14</sup>XYZ Capital is not actually a real asset manager included in our sample.

but not to the fund itself. Our contact at ANcerno explicitly stated that it is not possible to identify with certainty the specific funds using their data. Some fund/fund manager names are occasionally reported by ANcerno but they appear to be highly unreliable or incomprehensible. We are therefore able to establish that two trades from asset manager XYZ Capital are internally crossed with each other but we can only provide suggestive evidence on the exact identity of the two funds that are cross-trading. However, as our analysis is mostly conducted at the trade level, the exact identity of the funds is irrelevant as long as we are able to ensure that two trades are offset within the same fund family.

### *B. Identifying Cross-trades*

A cross-trade is a transaction in which a buy and a sell order for the same stock coming from the same fund family is conducted without going through the open market. We identify cross-trades in our database as transactions occurring i) within the same fund family, ii) in the same stock, iii) at the same time of the same day, iv) at the same price, and v) having the same volume of the trade but in opposite trading directions. For instance, a buy trade of 1,000 Apple shares executed on January 2nd, 2010, at 10:05 a.m. for \$101 is classified as a cross-trade only if we have in our sample a corresponding sell trade of 1,000 Apple shares coming from the same fund family and executed on January 2nd, 2010, at 10:05 a.m. for \$101. While it is, in theory, possible that two funds belonging to the same fund family would make exactly the same trade in opposite directions at the same time by chance, it is highly unlikely. Mutual funds do not trade at very high frequencies and usually affiliated funds rely on the same research which leads them to rarely trade in opposite directions (Elton, Gruber, and Green (2007)).

To check the reliability of our matching procedure, we compare the commission costs of open market trades with those of the trades we classify as cross-trades. In particular, commissions for cross-trades should be zero or extremely small (the broker does not need to find a counterparty for the trade, although sometimes a commission is due for bookkeeping

services). We find that the average commission (\$/share) for cross-trades is 0.0016 (the median commission is 0), while it is around 0.0245 for open market trades (see Table I, Panel A).<sup>15</sup> If we compare dollar commissions per dollar trade, the average value is 11 basis points for open market trades and 1 basis point for cross-trades. The difference is statistically significant at the 1% level. In particular, commissions are 0 for more than 90% of the trades we classify as cross-trades, suggesting that our algorithm identifies cross-trades with very high degree of precision.<sup>16</sup>

This identification procedure overcomes the main limitation of the proxies computed using quarterly or semi-annual snapshots employed by Gaspar, Massa, and Matos (2006), Schmidt and Goncalves-Pinto (2013), and Chuprinin, Massa, and Schumacher (2015). Through our approach, we ensure that opposite trades recorded in the same quarter but occurring in different days/times and having different volumes are *not* considered as cross-trades. In the latter part of the paper, we provide some suggestive evidence regarding the impact of cross-trading on fund performance. When we run regressions at the fund level, our main explanatory variable is  $CT\%_{f,t}$ : the monthly total dollar volume of cross-trades executed by family  $f$  in month  $t$  as a proportion of the total dollar volume of trades (open-market trades plus cross-trades) executed by family  $f$  in month  $t$ .

### *C. Execution Shortfall*

For the price at execution, we rely on the data provided by ANcerno. Rule 17a-7 of the U.S. Investment Company Act establishes that cross-transactions should occur at the “current market price” of the security. Therefore we focus on the market price at the moment of the execution as the main benchmark as this seems to be the closest to what Rule 17a-7 prescribes (according to our contact this is essentially the mid quote at the moment of the

---

<sup>15</sup>The average commission reported in Anand, Irvine, Puckett, and Venkataraman (2012) is slightly higher (0.028). We however find basically the same number if we limit our sample to the same time interval (1999-2008).

<sup>16</sup>Results are analogous when considering as cross-trades only transactions where no commissions have been paid. Occasionally, commissions are not charged also for normal trades. Therefore, the reporting of zero commissions is neither a necessary nor sufficient condition for a trade to be classified as a cross-trade.

execution).<sup>17</sup> In a limited number of cases ANcerno arbitrarily sets the execution time of the trade at the end/beginning of the day if the time is missing in the information provided by the institutional investor (see Anand, Irvine, Puckett, and Venkataraman (2012)). If the execution time reported is incorrect, this could potentially add significant noise to our results.<sup>18</sup> Assuming that misreporting is random there is however no reason why cross-trades should be systematically set at the highest price. Conversely, if misreporting is strategic this should limit the incidence of mispricing cases and bias the results against our main finding. As a check to ensure that such misreporting does not significantly affect our results, we replicate our analysis after dropping trades executed exactly at the opening or closing price of the day and find similar results. Additionally, we reproduce our analysis using benchmarks that do not depend on the exact time at which the trade is executed.

Cross-trades should minimize the impact of trading costs and commissions on the execution price, limiting deviations from the price quoted on the market (which is our main benchmark). Therefore, we define the execution shortfall as the absolute value of the deviation from the benchmark price scaled by the benchmark price itself. Consideration of the absolute value of the deviation from the benchmark is necessary in our setting. In fact, for each cross-trade our sample includes two twin trades with opposite execution shortfalls that would cancel each other out if signed values were considered. Formally,

$$Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}}, \quad (1)$$

where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ . The distribution of  $Execution\ Shortfall_{j,i,t}$  is plotted in the Appendix in Figure A.4. Results using alternative benchmarks are presented

---

<sup>17</sup>The time of the execution is provided at the *minute* level. However, trades can be executed at different *seconds* of the same *minute*. This would create by construction a spread between the execution price of a trade and its benchmark. Since this should affect in the same way open market trades and cross-trades (the exact execution time within the minute should be random for both), it does not compromise the validity of our results.

<sup>18</sup>It is worth noting that our ANcerno contact assured us that this problem only affects an extremely limited number of trades, mostly reported by pension funds.

in Section V.<sup>19</sup>

#### D. Fund-level Data

The main focus of our analysis is on individual trades. However, we are also able to provide some evidence at the fund level by linking our sample to CRSP mutual fund data via the asset manager identity. Specifically, we produce measures of a mutual fund’s size, fees, and flows. Following Gaspar, Massa, and Matos (2006) we compute fees as  $1/7(\text{frontload}+\text{rearload})+\text{expense ratio}$ . We also compute fund flows following the literature (see, e.g., Coval and Stafford (2007)):

$$\text{Flow}_{i,t} = \frac{TNA_{i,t} - (1 + \text{ret}_{i,t})TNA_{i,t-1}}{TNA_{i,t-1}}, \quad (2)$$

where  $TNA_{i,t}$  is the total value of net assets under management and  $\text{ret}_{i,t}$  is the monthly return of fund  $i$  in month  $t$ . At the fund family level, we compute the family size, the dispersion in intra-family returns, expense ratios, and fees. *Family Size* is defined as the log of the sum of the individual funds’ assets at the beginning of the month. We compute the intra-family *Return Dispersion* as the standard deviation of the returns of all affiliated funds in the previous month. Similarly, the *Expense Ratio Dispersion* is the standard deviation of the expense ratios, while the *Fees Gap* is the difference between the highest and lowest fee charged by funds affiliated to the asset manager in a given month.

Additionally, we compute the variable *Siblings* as the log of the number of equity funds belonging to the same family  $f$  in month  $t$  (Pollet and Wilson (2008)). We use the set of Thompson Reuters Investment Objective Codes to identify the investment style for each

---

<sup>19</sup>To verify the robustness of our results and rule out the possibility that they are driven by misreporting of the execution time, we replicate our analysis using the volume-weighted average price of the day (*VWAP*) and the open price of the day as alternative benchmarks (this does not require us to use the execution time variable at all). Some studies argue that the price at execution should be compared with the *VWAP*, which is also the most popular benchmark among practitioners (see Berkowitz, Logue, and Noser (1988), Hu (2009), and Anand, Irvine, Puckett, and Venkataraman (2012)). However, other studies warn about potential shortcomings in the use of *VWAP* as a benchmark (see, e.g., Madhavan (2002) and Hasbrouck (2007)). For instance, large trades are more likely to be executed exactly at the *VWAP*. Therefore, following Busse, Chordia, Jiang, and Tang (2015) we replicate our analysis also using the open price of the day as a benchmark (see Section V). In all cases results are qualitatively similar.

fund. Following Ferris and Yan (2009), we also build a proxy of governance based on precedent infringements. In particular, we argue that fund families investigated by the SEC for illegal practices that are potentially harmful to investors (other than cross-trading related practices) are more likely to have weak governance. Consistent with this argument, Dimmock and Gerken (2012) show that past legal violations have significant power to predict future fraud.<sup>20</sup> Therefore, we examine SEC administrative proceedings and the *Wall Street Journal Mutual Funds Scandal Scorecard* to categorize each fund family as having either weak or strong governance.<sup>21</sup>

### *E. Summary Statistics*

Sample statistics on the matched fund sample are reported in the Appendix (see Table A.I). Columns 2 through 4 show statistics from the CRSP mutual fund-Thomson Reuters match. The intersection between the two samples leaves us with 2,351 funds, organized into 452 fund families. The average mutual fund size is \$1,258 million, while the average mutual fund family size is \$39,531 million. The average fund family includes 17 equity funds.

Matching our sample of mutual funds to the ANcerno database decreases our sample size significantly. The final number of asset managers in our sample is 203 fund families managing 1,393 separate mutual funds. In particular, our matched sample contains 45% of the mutual fund families and 59% of the funds in the CRSP-TR dataset. Our sample is biased toward large institutions since the smallest families are less likely to rely on ANcerno's services (this bias has also been recognized by previous studies (see Puckett and Yan (2011)). Our final sample contains observations from 8 of the 10 largest mutual fund families in the United States.<sup>22</sup> Given that the top 10 families hold around 70% of the assets managed by the entire mutual fund industry, the bias toward larger institutions does not limit the implications of

---

<sup>20</sup>In particular, avoiding the 5% of firms with the highest ex ante predicted fraud risk would allow an investor to avoid 29% of fraud cases and over 40% of the total dollar losses from fraud.

<sup>21</sup>We focus on investigations instead of final court rulings because more than 90% of the investigations end up in out-of-court settlements.

<sup>22</sup>Given the non-disclosure agreement we signed with ANcerno we are forbidden to reveal the names of the management companies contained in our sample.

our results. Additionally, the funds in our final database perform slightly better than funds in the CRSP database.<sup>23</sup> This difference may be explained by the fact that funds belonging to large fund families perform better on average (Chen, Hong, Huan, and Kubik (2004)).

In order to limit the sample size for our empirical analysis on trade-level data, we extract three random samples, each consisting of 1% of the original ANcerno sample, and retain only those observations for which we have all control variables.<sup>24</sup> We then carry out our analysis on all 3 samples separately and report results from sample 2 as they are the weakest<sup>25</sup> and, therefore, are likely to provide a more conservative estimate of the mispricing of cross-trades. Our sample consists of 966,186 trades out of which we classify 7,368 as cross-trades and the remaining 958,818 as open market trades. Panel A of Table I reports the number of observations (Column 1), the average values for all of the main variables in the full sample (Column 2), the average values keeping open market trades only (Column 3), the average values keeping cross-trades only (Column 4), the average difference between open market trades and cross-trades (Column 5), and  $t$ -statistics for the null hypothesis of equality between the means of open market trades and cross-trades (Column 6). The summary statistics show that cross-trades are significantly bigger than normal trades both in share and dollar volume. Additionally, cross-trades generally involve stocks that present higher bid-ask spreads, are more volatile, and are bigger. In fact, they both exhibit higher average market capitalization and are more likely to be included in the S&P500 Index. The fact that most of the cross-trades occur in large market capitalization stocks is probably due to the high degree of overlap in large cap stocks across the portfolios of affiliated funds within a given family.

On average, cross-trades exhibit significantly higher execution shortfall than open market trades (0.84% versus 0.64%). Our numbers are higher than those reported in Anand, Irvine, Puckett, and Venkataraman (2012). This is due to three main differences in how we compute

---

<sup>23</sup>Average flow of 0.28% in the matched sample versus 0.09% in CRSP; average monthly raw return of 0.42% versus 0.37%; and average monthly alpha of 0.03% versus 0.00% (see Table A.I in the Appendix.)

<sup>24</sup>This procedure is not uncommon in the asset pricing literature (see, e.g., Ben-David and Hirshleifer (2012)).

<sup>25</sup>Results from samples 1 and 3 are anyway very similar and are reported in the Appendix (see Table A.II).



our shortfall measure which arise naturally from differences in the research design. First, we always compute the absolute value of *Execution Shortfall*, whereas Anand, Irvine, Puckett, and Venkataraman (2012) do not.<sup>26</sup> Second, we use the market price at execution instead of the price at placement as a benchmark (Rule 17a-7 of the U.S. Investment Company Act states that cross-trades should be executed at the prevalent price at the moment of the trade). Third, we look at trades rather than at orders (a single order can be broken down in several trades executed at different times, the relevant benchmark for our analysis will differ depending on when each single trade is executed).

In Panel B we report pairwise correlations among our main variables to make sure that stock and trade characteristics have an impact on execution shortfall that is consistent with the related theory. As with previous research, we find that the execution shortfall is positively correlated with proxies of stock illiquidity and negatively correlated with proxies of market capitalization.

### III. The Pricing of Cross-Trades

#### A. *Cross-Trades and Execution Shortfall*

Our empirical strategy uses cross-sectional variation to explore how cross-trades are priced relative to trades executed in the open market. Rule 17a-7 allows cross-trades subject to conditions of the fair valuation of assets (“independent current market price” – usually the last sale market price) and fair treatment of both parties. The Securities and Exchange Commission specifies that the adviser has a duty to, among other things, “carefully consider” its responsibilities to investors to ensure the best execution and loyalty to each fund. In particular, a cross-trade should never occur when one party could obtain a better price by going through the open market. Our null hypothesis is, therefore, that cross-trades exhibit

---

<sup>26</sup>We focus on the deviation from the benchmark irrespectively of the direction of the trade since each cross-trade is a zero-sum game in which there is a winner and a loser party, therefore for our research design it would not make sense to compute the signed deviation.

a significantly smaller execution shortfall than ordinary trades. A higher deviation from the benchmark price would, instead, suggest that one trading counterparty gets unfairly penalized in the cross-trade. In our analysis, we compare the execution shortfall of cross-trades with the execution shortfall of open market trades controlling for trade, stock, time, and family differences.

We run trade-level ordinary least square regressions of execution shortfall on *CT Dummy*, a dummy variable that takes a value of one when a trade is a cross-trade and zero otherwise. We cluster errors at the time level to account for cross-sectional heterogeneity. Formally,

$$Execution\ Shortfall_{i,f,t} = \beta(CT\ Dummy_{i,f,t}) + \Gamma'X_{i,t} + \gamma_i + \gamma_f + \gamma_\tau + \epsilon_{i,f,t}, \quad (3)$$

where  $i$  indexes the stock,  $t$  the time, and  $f$  the fund family.  $X_{i,t}$  is a vector of time-varying stock-level controls,  $\gamma_\tau$ ,  $\gamma_i$ , and  $\gamma_f$  are time,<sup>27</sup> stock, and family fixed effects, respectively. The identification of an effect for *CT Dummy* $_{i,f,t}$  on execution shortfall comes from the comparison of cross-trades with otherwise similar trades that are not crossed.

Column 1 of Table II shows that the execution shortfall is 19 basis points *higher* for cross-trades relative to open market trades, a difference that is significant at the 1% level ( $t$ -statistic of 5.44). A potential explanation for this difference is that cross-trades are on average larger in volume. To be certain that our result is not driven by trading volume, we include the volume of the trade<sup>28</sup> as a control variable in specification (2). Table II, Column 2 shows that a higher trading volume indeed affects the magnitude of the execution shortfall, as the coefficient of our *CT dummy* decreases from 19 to 18 basis points, however it still remains significant at the 1% level.<sup>29</sup>

<sup>27</sup>We use  $\tau$  (instead of  $t$ ) as the subscript of the time dummies to emphasize that our time fixed effects are at the month level, while trades can be executed at any time  $t$  of the day. We use month-level fixed effects to limit the number of dummies in our model. To include day fixed effects and cluster errors at the day level actually yields economically and statistically stronger results.

<sup>28</sup>We use the share volume of the trade instead of the dollar volume to avoid mechanical correlation with the dependent variable as the price of the stock would be included both in the dependent and independent variable. However, results using the dollar volume are similar.

<sup>29</sup>In a previous version of the paper, we reported lower estimates. The difference is due to the inclusion of family and stock fixed effects in this version of the paper.

We also consider the possibility that time-varying stock characteristics may also have an effect on the size of the execution shortfall. For instance, highly volatile and illiquid stocks usually display a higher execution shortfall than their less volatile and more liquid counterparts. For this reason, we include time-varying controls for different proxies of stock illiquidity: the Amihud Ratio (*Illiquidity*), the ratio of one over the open price of the day ( $1/Price$ ), and the bid-ask spread (*Bid-Ask Spread*). We also include proxies for stock capitalization because bigger stocks display in general lower execution shortfall. In particular, we control for both inclusion in the S&P500 index (*S&P500 Dummy*) and the stock market capitalization decile (*Market Equity Decile*). Finally, we control for the standard deviation in stock daily returns (*Volatility*). The impact of the inclusion of all controls on the magnitude and significance of the main coefficient of interest is marginal at best. Furthermore, the signs of all of the coefficients for the control variables are consistent with the findings of related research.<sup>30</sup> Results obtained from the full specification model including all control variables, indicate that the execution shortfall for cross-trades is, on average, 18 basis points higher than that of open market trades.<sup>31</sup> This result is significant at the 1% level ( $t$ -statistic of 5.37).

Our result is economically significant. Given that the average percentage bid-ask spread in our sample is 4 basis points, the marginal effect of cross-trades on the execution shortfall is 4.5 times greater than a fund would pay had it operated through market channels. Conservative back-of-the-envelope calculations suggest that cross-trading shifts, on average, performance by \$1.8 million per day in the mutual fund industry alone.<sup>32</sup> However, the exact impact of mispricing on fund performance depends on additional factors, such as, the extent of cross-trading activity and the size of the fund itself. Section IV provides estimates of the impact of

---

<sup>30</sup>With the only exemption of *Bid-Ask Spread* which turns insignificant when the other proxies of stock illiquidity are included due to the high correlation among them.

<sup>31</sup>We obtain similar results using different fixed effects, see Appendix.

<sup>32</sup>This number is obtained by multiplying \$169 billion (average daily trading dollar volume on the NYSE) times 0.30 (roughly the total US equity held by mutual funds according to *Investment Company 2015 factbook*) times 0.02 (average cross-trading activity out of total dollar trading volume of mutual funds in our sample) times 0.0018 (marginal effect of *CT Dummy* on execution shortfall). This number is likely to be a lower bound as considering only the NYSE significantly under-represents the total amount of trading activity.

mispriced cross-trades on fund performance. In conclusion, these findings provide evidence that supports the results from Kacperczyk, Sialm, and Zheng (2008) and Anand, Irvine, Puckett, and Venkataraman (2012), in suggesting that unobserved actions occurring within a quarter (therefore not captured by obligatory fund filings) and heterogeneity in transaction costs might have significant implications on performance.

## *B. Reverse Causality and Endogeneity*

### **B.1. The Natural Experiment**

One concern with our previous results is the direction of causality. A reverse causality argument suggests that instead of cross-trades explaining execution shortfall, it was high expected execution shortfall that drove the decision of fund managers to enter into cross-trades. Additionally, omitted variables may affect both execution shortfall and the choice of a fund manager to cross-trade. We address these concerns by using an exogenous increase of regulatory scrutiny.

On September 3, 2003 the New York State Attorney General Eliot Spitzer announced the issuance of a complaint claiming that several mutual fund firms had arrangements allowing trades that violated terms of their funds' prospectuses, their fiduciary duties, and general securities laws (the investigation led to what is commonly referred to as the "late trading scandal"). Subsequent investigations showed that at least twenty mutual fund management companies, including some of the industry's largest firms, had struck deals permitting improper trading (Zitzewitz (2006), McCabe (2009)). Importantly, most of the violations involved late-trading, while none of the funds under scrutiny were charged with improper cross-trading.<sup>33</sup>

As a consequence of the scandal, in 2004 new rules were introduced and adopted by the SEC requiring fund families to implement more stringent compliance policies. In particular,

---

<sup>33</sup>The late trading scandal has been used as a source of exogenous variation in other papers (see, e.g., Anton and Polk (2014)). However, in this paper we are not interested in the late trading scandal *per se*, but mainly into the regulatory framework that was implemented as a response to the scandal.

Rule 38a-1 under the Investment Company Act of 1940 forced investment companies to “have strong systems of controls in place to prevent violations of the federal securities laws and to protect the interests of shareholders and clients.” Amplifying such controls, Rule 206(4)-7 required each adviser registered with the Commission to designate a chief compliance officer empowered with the full responsibility and authority to develop and enforce appropriate policies and procedures for the firm. Additionally, compliance officers were required to report directly to the board of directors to increase their independence. We contacted a number of compliance officers at leading fund management companies to obtain more information about the actual implications of the new regulations. They pointed out that the supervision of cross-trading activity and the monitoring of the execution price of the cross-trades became one of their key responsibilities beginning in 2004.

We argue that both the increased attention to improper trading practices in the industry induced by the late trading scandal and the tightening of regulation forced fund families to, at the very least, reduce opportunistic cross-trading activity. This exogenous shock allows us, first, to improve our estimation of the impact of opportunistic cross-trading on performance. Second, it permits us to estimate what proportion of cross-trades was executed primarily for opportunistic reasons.

The new rules became effective on February 5, 2004 while the date companies were required to demonstrate compliance was October 5, 2004. We use the latter as the *treatment* date in our analysis.<sup>34</sup> Since many relevant aspects of the trading environment changed around this time as well (e.g., the liquidity of the market increased, and many new sophisticated investors entered the market) we need to compare cross-trades to a control group of trades that are at least as likely as cross-trades to be affected by increasing liquidity in the markets post-2004 but are unlikely (or significantly less likely) to be directly affected by the introduction of Rule 38a-1 and Rule 206(4)-7. Therefore, we compare the effect of the new regulation on the pricing of cross-trades (treatment group) with that on open market

---

<sup>34</sup>Using as the treatment date February 5, 2004 provides us with quantitatively weaker but qualitatively analogous results.

trades (control group). Our analysis resembles a difference-in-difference test in which only cross-trades receive the treatment in October 2004. The effect of internal governance on cross-trading activity has so far never been explored and we believe it represents an interesting result in and of itself.

## B.2. The Effect of Increased Supervision on Cross-Trades

Figure 1 shows clearly that the execution shortfall of cross-trades and that of open market trades display a parallel trend before the regulatory shock. However, the new regulation strongly affects the execution shortfall of cross-trades, while leaving the execution shortfall of open market trades unaltered. In particular, the execution shortfall of cross-trades is *higher* than that of open market trades before the compliance date (see vertical line) and *lower* afterwards. Figure 2 shows the fraction of cross-trades out of all institutional trades. In particular, the percentage of cross-trades starts to decrease at the onset of the late trading scandal and drops permanently after the funds had to comply with the new regulation. Overall, cross-trading activity went from peaks of 6% of the dollar volume traded to less than 1% on average after the new rules were introduced.

Table III shows the effect of tighter regulation on *Execution Shortfall* in a multivariate framework. Our specification includes *CT Dummy*; *Post Regulation*, i.e., a dummy variable capturing the effect of general changes in trading conditions after 2004; and the interaction between *Post Regulation* and *CT Dummy* (*Post Regulation* is not included independently in specifications (2) through (5) since it would be collinear with the time dummies). The control group consists of open market trades that should be less (or not at all) affected by the change in regulation triggered by the late trading scandal. The coefficient of the interacted variables (*CT Dummy x Post Regulation*) captures the marginal effect of the new regulation on *Execution Shortfall* for cross-trades (i.e., the effect of the treatment). *Post Regulation* and the time dummies capture the effect of a general increase in market liquidity in the last part of the sample.

Our results indicate that tight regulation had a major effect on the pricing of cross-trades: *Execution Shortfall* dropped by 59 bps almost immediately after the compliance date, falling below that of open market trades (the result is significant at the 1% level). This finding suggests that poor governance before the late trading scandal played a significant role in determining a higher *Execution Shortfall* for cross-trades. Overall, results in this section establish a causal relation between cross-trading and mispricing. Importantly, while the execution shortfall of cross-trades does not exceed that of open market trades after 2004, the remaining deviation from benchmark prices may still be enough to arbitrarily shift performance, even though probably to a lesser extent. To find execution shortfalls systematically higher than that of open market trades *in the presence of tight supervision* would be unlikely. In the following, we provide evidence consistent with the hypothesis that opportunistic pricing became weaker and is now shrouded but persists even after the regulatory change.

### *C. Stock Characteristics, Market Conditions, and Backdating*

This section examines which cross-trades are more likely to be mispriced. In our specification, we interact *CT Dummy* with a battery of stock-level characteristics and market-level conditions. The stock-level variables are *Illiquidity*,  $1/Price$ , *Bid-Ask Spread*, *Beta*, and *Volatility*. *Beta* is the beta of the stock estimated using the Capital Asset Pricing Model (CAPM), all other variables are described above (see Section III.A). The market-level variables are the volatility index (*VIX*), the NBER recession indicator (*NBER*), proxies of macroeconomic and financial uncertainty<sup>35</sup> (*Macro Uncert.* and *Fin. Uncert.*), the cross-sectional return dispersion in the day preceding the trade (*CS Vol.*), and the return of the market in the previous month (*Mkt Return*). We control in our analysis for stock characteristics non-interacted and time, stock, and family fixed effects.

---

<sup>35</sup>We use proxies of macroeconomic and financial uncertainty from Jurado, Ludvigson, and Ng (2015). The authors derive model-free measures of uncertainty aggregating the *h-step-ahead* forecast error obtained using several financial and economic series.

Results reported in Table IV indicate that the mispricing of cross-trades is more significant for highly illiquid<sup>36</sup> and volatile stocks. This is not surprising since these stocks offer more discretion on how to price transactions given that they incorporate higher asymmetric information and have lower trading volume.<sup>37</sup> One could argue that, by construction, highly illiquid and volatile securities should exhibit higher deviation from benchmark prices. Yet, including time-varying stock characteristics non-interacted in all specifications accounts for this possibility.<sup>38</sup> Goncalves-Pinto and Sotes-Paladino (2015) posit that the main reason why funds cross-trade is the reduced transaction cost when trading illiquid securities, we however show that institutions pay a significantly *higher* cost when they cross-trade illiquid securities compared to when they trade the same illiquid securities in the open market.

In columns (4) and (5), we exclude time fixed effects from our model in order to test whether market-wide conditions affect how cross-trades are priced.<sup>39</sup> We find that cross-trades are more mispriced in times of uncertainty. Interestingly, most of the mispricing of cross-trades appears to be unrelated to time-series volatility and positively related to measures of asymmetric information in the markets (i.e., Jurado, Ludvigson, and Ng (2015)’s proxy of financial uncertainty, measures of cross-sectional return dispersion, and illiquidity). This is overall consistent with the hypothesis that institutions protect their top funds in periods of high uncertainty offering additional compensation to hold illiquid/difficult to price assets.<sup>40</sup>

Furthermore, we test the hypothesis that a number of cross-trades was actually back-dated. When the main purpose of cross-trades is to reallocate performance between counter-

---

<sup>36</sup> $1/Price$  presents the “wrong” sign due to the high correlation with the other proxies of illiquidity included in our model. The sign of the coefficient reverses if the other proxies of illiquidity are excluded.

<sup>37</sup>Yet, this potentially conflicts with the requirement for the price of the cross-traded assets to be readily available and easily determinable (see 17a-7 (a) of the U.S. Investment Company Act) as prices of illiquid securities are often stale.

<sup>38</sup>Non-interacted stock and trade level variables are always included in Table IV even though the coefficients are not explicitly reported to save space.

<sup>39</sup>Also non-interacted market-wide variables and time-varying stock controls are included but the coefficients are not reported to save space.

<sup>40</sup>Interestingly, the coefficient of *CT Dummy x Macro Uncertainty* is negative, suggesting that, in this case, cross-trades benefit the investors diminishing the cost of macroeconomic uncertainty. In unreported results, we find that the coefficient of *CT Dummy x Macro Uncertainty* was positive before the 2004 regulation was introduced and turned negative afterwards. This result supports the hypothesis that careful regulatory scrutiny may change dramatically how cross-trades are used.



parties, the best strategy from a family perspective would be to arbitrarily set cross-trades *ex post* to the price of the day that would have shifted the highest performance (i.e., the highest or lowest price of the day). From our contacts with compliance officers, we understood that a common practice in the industry is to check cross-trades ex-post to assess whether the execution price was inside a reasonable interval. An execution price lower or higher than any price actually realized during the trading day would immediately flag a trade as suspicious. However, some deviation from benchmark prices is usually deemed acceptable. We test the backdating hypothesis estimating a logit model in which the dependent variable assumes a value of one if the execution price is either the highest or the lowest of the day and we regress it on our cross-trade dummy and controls. Whether the price was actually the highest or the lowest is indifferent for our purpose since the party that is expected to gain from the transaction can benefit in both cases (selling at the highest or buying at the lowest).<sup>41</sup>

Results reported in Table V suggest that some cross-trades are actually backdated.<sup>42</sup> Our estimated coefficient indicates that cross-trades are 1.7% more likely to be executed *exactly* at the highest/lowest price of the day (marginal probabilities are reported). It is however certainly possible that traders choose to cross-trade when prices in the market are extreme. Therefore, as in the previous section, we use the 2004 regulatory change as an exogenous shock to improve our identification. We interact *CT Dummy* with *Post Regulation* to assess whether cross-trades became less likely to be executed at the highest or lowest price after the new regulation was passed. Our findings are consistent with a causal interpretation of the results: after 2004 cross-trades became 1.2% less likely to be executed at extreme prices. The inclusion of open market trades rules out the possibility that market wide changes in

---

<sup>41</sup>Importantly, we cannot include stock, family, and time fixed effects contemporaneously in this specification because the estimation of a non-linear model becomes infeasible with a very large number of dummies. To limit our sample size we consider *only* families that cross-trade at least once and to simplify the computation we include *only* family fixed effects. In the Appendix, we report analogous results estimating a linear probability model keeping in our sample all observations and including time, family, and stock fixed effects (see Table A.III).

<sup>42</sup>The issue of misreporting of execution times in ANcerno is arguably more relevant for this part of the analysis since ANcerno arbitrarily sets missing time entries at the open/end of the trading day. To rule out the possibility that this could influence our result, we replicate our analysis dropping all trades executed exactly at the opening or closing price of the day. Results are analogous.

the trading environment after 2004 are driving our results (as they should also be affected). Interestingly, this result survives if we run it only on the post regulation sample, suggesting that cross-trades are still more likely than open market trades to be executed at extreme prices, even though less so than before 2004 (see Table A.VII in the Appendix). Therefore, we argue that opportunistic performance shifting via cross-trades is still likely to happen. Additional evidence on this aspect is provided in the next section.

#### *D. The Cross-Section of Cross-Trades*

This section investigates how fund family characteristics affect the pricing of cross-trades. In general, family characteristics should not be correlated with the pricing of cross-trades. However, if cross-trades were used to shift performance, we may find that proxies for weak governance and high incentive to reallocate performance are correlated with the execution shortfall. Importantly, to increase the power of our tests in this section *only cross-trades are kept*. Since the number of cross-trades in the full ANcerno sample is relatively limited (we have 738,476 cross-trades), we do not need to draw a random sample to conduct our analysis but we can simply exclude all open-market trades. An alternative approach would be to interact our *CT Dummy* with family characteristics and run our regressions on a random extraction from ANcerno including both cross-trades and open market trades (as we did in the previous sections). However, this would further limit the number of fund families included in our analysis. Therefore, we decided to keep all cross-trades and test whether the cross-trades executed in some families are priced differently from the cross-trades executed in other families.

Our results reported in Table VI indicate that cross-trades executed within weak governance institutions display a 27 basis points higher execution shortfall than cross-trades executed in strong governance institutions. Additionally, a standard deviation increase in the number of siblings increases the *Execution Shortfall* by 15 basis points. Massa (2003) suggests that fund proliferation attract money inflows. Our finding indicates that a large

internal market, in which potentially multiple funds can engage in cross-transactions, besides attracting flows also incentives the opportunistic pricing of cross-trades.<sup>43</sup>

Furthermore, we show that an increase of one standard deviation in *Fees Gap* boosts the execution shortfall by 10 basis points. Results for a one standard deviation increase of the expense ratio are similar (we do not include both variables at the same time in our regressions as they are highly correlated). We include in our specifications both stock and time fixed effects but not family dummies since we are interested in estimating the effect of family-level variant and invariant characteristics on the execution shortfall. The positive effect of fees dispersion on the pricing of cross-trades strongly points in the direction of performance reallocation from junk to star funds. In fact, the incentive for conducting “winner-picking” strategies is arguably stronger when some funds are significantly more valuable than others (Stein and Scharfstein (2000), Nanda, Wang, and Zheng (2004)), while the incentive for subsidizing underperformers is probably stronger when all funds have similar importance from a family perspective.<sup>44</sup> Additional support for the “winner-picking” hypothesis is provided in the next section. Overall, our results indicate than cross-trades from weak governance institutions, with large internal markets, and high dispersion in fees among sibling funds appear mispriced with respect to the the average cross-trade in our sample.

Running our regressions only on the post regulation part of the sample (Table VI, Column

---

<sup>43</sup>We include in our regressions also the total asset size of the asset manager (*Family Size*) to distinguish the effect of large internal markets, in which many funds can cross-trade, from the effect of the size of the asset manager (as family size is a known predictor of fund returns, see Chen, Hong, Huan, and Kubik (2004).) However, the high correlation of *Family Size* and *Siblings* potentially creates problems due to multicollinearity concerns when we include both in our specification at the same time. Therefore, we also present results obtained including only one variable at the time. We also include cross-sectional return dispersion in the previous month to exclude that our result just captures heterogeneity in fund performance unrelated to cross-trading activity.

<sup>44</sup>Theoretical models of internal capital markets support the view that high heterogeneity in the importance of divisions within multi-division companies leads to the reallocation of resources either to subsidize weaker units or to support the stronger ones (Stein (1997), Stein and Scharfstein (2000)). Similarly, Gaspar, Massa, and Matos (2006) and Chaudhuri, Ivkovich, and Trzcinka (2012) argue that an asymmetry of “products” creates higher incentive to reallocate performance to successful funds and powerful clients. On the contrary, in homogenous families in which all funds have the same importance the incentive to shift performance is lower. In particular, fund complexes have a strong incentive to move performance from the cheapest funds to the funds charging the highest fees since outperformers attract disproportionate flows (Chevalier and Ellison (1997), Sirri and Tufano (1998), Agarwal, Gay, and Ling (2014)).

(8)), we find that asset managers with weak governance, a large number of affiliated funds and high heterogeneity in fees still price cross-trades at a higher deviation from benchmark prices *after* 2004 (the negative coefficient of *Family Size* is just due to the high correlation with *Siblings*). Our results therefore suggest that opportunistic pricing might still occur (even though the execution shortfall of cross-trades is significantly lower). Interestingly, in unreported regressions we find that the effect of *Fees Gap* on *Execution Shortfall* is driven by the second part of the sample. We conjecture that the dramatic growth of cheap passive funds and index trackers in the last 10 years increased the average dispersion and gap in fees, while offering a large supply of liquidity providers to star funds. Some evidence in this direction is provided in the following section. A systematic investigation on the effect of opportunistic performance reallocation on the performance of passive funds and index trackers is however left for future research.

## IV. Star Funds, Cross-trading, and Performance

### Shifting

We believe that the evidence from trades provided in the previous section constitutes the most important and novel contribution of our paper. However, exploiting our identification of cross-trades we are able to shed some additional light on the ongoing debate on the incentives at the fund family level. A necessary caveat is in order, the structure of our data allows us to identify with high certainty the fraction of cross-trading activity at the asset manager level but not the exact identity of the trading counterparties. While this was not an issue in the previous section (as our analysis was conducted at the trade level), when we explore the effect on *family*-level cross-trading on *fund* performance we are going to inevitably add significant noise to our analysis.

## A. Methodology

In this section, we investigate whether fund families use mispriced cross-trades to boost the performance of star funds (see, e.g., Gaspar, Massa, and Matos (2006)) or subsidize the junk funds (see, e.g., Schmidt and Goncalves-Pinto (2013)). Our hypotheses derive from the literature that explores the incentives of multi-division companies to allocate scarce resources to the most successful units (picking winners) versus the least successful ones (in a socialistic framework that subsidizes the worst performers).

These two alternative hypotheses have opposite empirical predictions. According to the winner-picking hypothesis, cross-trading should increase the gap in performance between star and junk funds. Conversely, the subsidization hypothesis predicts that cross-trading reduces the spread in their performance. Importantly, cross-trading could decrease trading costs and, hence, improve funds' performance even in the case of non-opportunistic transactions. However, it should not be systematically correlated with the difference in performance between star and junk funds.

Since we show that cross-trades are on average mispriced, performance must be transferred between trading counterparties and this should be reflected into fund returns (unless the party who benefits from the cross-trade is random and deviations from benchmark prices average each other out). Our empirical strategy therefore consists, first, in defining groups of funds inside a family that we hypothesize are likely to benefit or suffer from cross-trading and, second, in testing whether the difference in returns correlates with cross-trading activity within the family.

Our approach relies on the methodology introduced in Gaspar, Massa, and Matos (2006). Specifically, in our main tests we rank funds according to their monthly<sup>45</sup> flows (see, e.g., Bhattacharya, Lee, and Pool (2013)). The reason for ranking funds according to their flows is intuitive.<sup>46</sup> Funds with outflows are liquidity demanders and funds with inflows are the natural liquidity suppliers. On the one hand, under a subsidization strategy star funds can

---

<sup>45</sup>We focus on monthly observations because we cannot compute *flows* at the daily level.

<sup>46</sup>Ranking funds on net fees gives however qualitatively similar results.

buy securities at inflated prices from the liquidity-demanding funds thereby increasing the performance of the junk funds at their expenses. On the other hand, under a winner-picking strategy, star funds can buy securities at deflated prices from the liquidity-demanding funds (that are likely to be shut down anyway), increasing their own performance.<sup>47</sup>

Having ranked the funds, we then sort them into terciles for each family.<sup>48</sup> Funds in the intermediate tercile are discarded. From the two extreme terciles we construct pairwise combinations matching funds from the top tercile with funds in the bottom tercile, and we compute the spread in their style-adjusted performance (four-factor alpha). In order to control for style effects we impose as an additional restriction that the two funds operate in the same investment style.

For instance, consider a family having six funds with the same investment style and assume that in month  $t$  the funds all have different flows. This implies a ranking from 1 to 6 and two funds in each tercile. For our analysis we discard the funds ranked third and fourth and we build the return spread from the remaining funds. Specifically, the observations in our final sample would be the difference of performance between fund 5 and fund 1, fund 5 and fund 2, fund 6 and fund 1, fund 6 and fund 2.

To understand how cross-trading shifts performance across siblings, we regress the spread in performance between funds in the top tercile and bottom tercile on the percentage of cross-trading activity, controlling for family characteristics and observable differences between the two funds. Formally:

$$r_{i,t}^{Star} - r_{j,t}^{Junk} = \beta(CT\%_{f,t}) + \Gamma'X_{i,j,t} + \gamma_t + \gamma_{i,j} + \epsilon_{i,j,t}, \quad (4)$$

---

<sup>47</sup>An alternative approach would be to sort funds on gross fees (i.e., asset under management  $\times$  percentage fees), since the remuneration of mutual funds almost entirely consists of fees on asset under management (Haslem (2010)). However, it is very difficult to subsidize large funds using cross-trades since the amount of performance transferred would need to be large. Since we find most of the mispricing to be in illiquid stocks, we recognize that it would be highly unlikely to boost the performance of large funds using such transactions. Hence, we focus on the “hot” funds, i.e., funds that attract the most new money within the family. To favor such funds makes economically sense as flows respond disproportionately to positive performance and have positive spillovers to the rest of the family (Sirri and Tufano (1998), Nanda, Wang, and Zheng (2004), Basak, Pavlova, and Shapiro (2007)).

<sup>48</sup>Using quintiles yields similar results.

where  $r_{i,t}^{Star}$  is the raw performance (or four-factor alpha) of star fund  $i$  in month  $t$  and  $r_{j,t}^{Junk}$  is the raw performance (or four-factor alpha) of junk fund  $j$  in month  $t$ , provided that both funds belong to the same fund family  $f$  and have the same investment style.  $CT\%_{f,t}$  is the percentage of cross-trading activity in family  $f$  where  $(i, j) \in f$ .  $X_{i,j,t}$  is a vector of fund/family-level controls accounting for observable differences among the two funds (e.g., the difference in funds' size),  $\gamma_t$  and  $\gamma_{i,j}$  are time and fund pair fixed effects. Fund pair fixed effects capture the invariant differences among the funds, e.g., if the star fund manager is on average more skilled than the junk fund manager.<sup>49</sup> As an alternative specification we control for family (instead of fund pair) fixed effects. We do not include both because family dummies are collinear to fund pair dummies. Errors are clustered at the time level.<sup>50</sup>

The average  $spread = r_{i,t}^{Star} - r_{j,t}^{Junk}$  in our sample is positive (0.84% monthly risk-adjusted return) since on average funds with higher flows outperform those with lower flows. However, under the null hypothesis of no strategic interaction, we should expect to find a non-statistically different from zero correlation between the spread in performance and  $CT\%_{f,t}$ , i.e.,  $H_0 : \beta = 0$ . Under the winner-picking hypothesis we should expect a positive correlation between the spread in performance and  $CT\%_{f,t}$  (i.e., cross-trading increases the performance of star funds at the expense of the junk siblings), that is,  $H_1 : \beta > 0$ . Under the cross-subsidization hypothesis, we should expect a negative coefficient (i.e., families shift performance from star to junk funds, shrinking their performance gap),  $H_2 : \beta < 0$ .

### *B. Winner-picking versus Subsidization of Junk Funds*

In Table VII we investigate the effect of cross-trading activity on the performance spread between star and junk funds. We report results for the spread in style-adjusted returns<sup>51</sup>

<sup>49</sup>Skill might however be time-varying (see, e.g., Kacperczyk, Nieuwerburgh, and Veldkamp (2014) and Kacperczyk, Van Nieuwerburgh, and Veldkamp (2016)). This is not a concern under the assumption that cross-trading activity is unrelated to skill.

<sup>50</sup>The difference in performance between funds should be uncorrelated over time because there is no evidence of persistence in performance (see, e.g., Carhart (1997), Frazzini and Lamont (2008), and Lou (2012).) Consistently, we find that clustering errors at the fund pair level does not change our results.

<sup>51</sup>Subtracting the return of a fund to the return of another fund having the same investment style we "clean" our measure of performance from the effect of style.

(Columns 1-4) and for the spread in four-factor alphas (Columns 5-8). It is important to stress that our proxy of cross-trading activity,  $CT\%$ , is at the family level, therefore our measure is likely to contain significant noise. The sign and the coefficient of  $\beta$  should however provide information on the direction of performance reallocation.

We find that the relation between  $CT\%$  and the spread in returns is positive and strongly significant (see Table VII). This result suggests that cross-trading activity widens the gap in performance between star and junk funds.<sup>52</sup> Overall, this empirical finding is consistent with the winner-picking hypothesis and inconsistent with the cross-subsidization hypothesis.<sup>53</sup> We cannot however exclude that in some cases cross-subsidization of funds hit by redemptions occurs. We would actually expect this to happen in a few cases, especially when flagship funds are under significant pressure because of redemptions (some evidence pointing in this direction is provided by Schmidt and Goncalves-Pinto (2013)). However, our results indicate that this does not occur *on average*.

In Columns 3, 4, 7, and 8 we also include a number of fund-level and family controls. Specifically, to ensure that our results are not driven by differences in the characteristics between the two funds, we include their size difference ( $\Delta Size$ ), their previous month return difference ( $\Delta Past Returns$ ), their previous month flow difference ( $\Delta Past Flow$ ), and the difference in *contemporaneous* flows ( $\Delta Flow$ ).<sup>54</sup> We also include *Family Size* to account for the positive correlation between cross-trading activity and the size of the mutual fund complex, and *Return Dispersion* to make sure that our results are not driven by ex ante heterogeneity in fund returns at the family level.

---

<sup>52</sup>To make sure that our result is not driven by unobservable differences between families that do and do not cross-trade, we replicate the same analysis dropping all observations where  $CT\% = 0$ . Results are unchanged (see Appendix).

<sup>53</sup>A question that arises naturally is why the manager that gets penalized from cross-trading should engage in cross-transactions. We can conjecture three explanations that we cannot however test with our data. First, the manager of the two funds that are cross-trading may actually be the same, hence she would simply boost the performance of her top fund. Second, it is possible that a fund that is about to get closed is penalized to the benefit of its siblings. Third, the most heavily penalized funds might be passive funds and index trackers.

<sup>54</sup>We control for contemporaneous flows because when we sort funds on flows we mechanically generate a spread in performance. This should not be a problem as long as cross-trading is not affected by flows. To mitigate any potential concern we, first, control for contemporaneous flows and, second, we exploit the change in the regulatory environment described in Section IV as an exogenous shock.



Our estimates suggest that one standard deviation increase in monthly cross-trading activity increases by about 24 basis points the risk-adjusted performance gap between junk and star funds (see Table VII, Column 8). Considering families in which there is no cross-trading activity as the control group, our calculations suggest that star funds boost their risk-adjusted performance by 1.7% annually at the expense of junk funds, assuming that cross-trading funds have equal size and performance is shared equally.<sup>55</sup> Additionally, Table VIII shows that the inflow funds that benefit from cross-trading activity are *only* those that charge higher than median fees – the coefficient of *CT% x High Fees* is positive and significant, while that of *CT%* becomes statistically non-different from zero. Overall, it appears that the subset of funds that benefit from cross-trading includes only those that are most valuable from a family perspective.

Our results so far suggest that fund families use cross-trading to shift performance from junk funds to star siblings. However, reverse causality and omitted variable bias are a concern also in this setting. For instance, fund families with a higher spread in performance may cross-trade more or omitted factors may drive both cross-trading and performance. Again, we use the regulatory change that followed the late trading scandal to establish causality. In Table IX we add to our main specification an interaction variable between *CT%* and *Post*, i.e., a dummy variable taking a value of one after the compliance date was reached and taking a value of zero otherwise. *Post* captures the effect of changes in the trading environment in the post-regulation sample that are unrelated to cross-trading activity. We do not include the dummy *Post* non-interacted with *CT%* in specifications 2-4 and 6-8 because the variable is spanned by the time fixed effects. In this specification, we should expect to find  $b$ , the coefficient of the interaction between *CT%* and *Post*, to be negative in the case of winner-picking behavior ( $b < 0$ ) as the new regulation should reduce the gap in the performance between star and junk funds that was due to cross-trading. Conversely, we should find  $b > 0$  if funds used cross transactions to support junk funds, i.e., the performance gap should have

---

<sup>55</sup>The marginal effect of a cross-trading dummy is 0.28% (see Table A.IV in the Appendix). If we assume that performance is shared equally each counterparty gains or loses 0.14% per month, i.e., 1.7% annually.

been artificially low before 2004 and should now increase.  $b = 0$  should be expected in case the new regulation did not have any impact on the effect of cross-trading on performance. The inclusion of time and family dummies rules out the possibility that the effect is driven by changes in the market environment or by unaccounted time-invariant family characteristics.

Our results indicate that the new regulation was *on average* effective in eliminating the impact of cross-trading activity on the spread in performance between star and junk funds. The marginal effect of cross-trading on performance is almost completely balanced out by the negative effect of the new regulation. Overall, measuring cross-trading activity using actual cross-trades instead of opposite side transactions, we provide support for the winner picking hypothesis thereby confirming the evidence from opposite trades provided in Gaspar, Massa, and Matos (2006). Conversely, we rule out the hypothesis of systematic cross-subsidization of distressed funds (see, e.g., Schmidt and Goncalves-Pinto (2013)). As our results contain significant noise, given that cross-trades are computed at a family level, we are unable in this section to assess with certainty whether cross-trades still shift performance to some funds after the new regulation was introduced.

## V. Further Results and Robustness

This section provides additional results and robustness checks.

### A. *Alternative Benchmark Prices*

Most of the results provided in the paper use the price of the stock in the market at the moment of the execution as the main benchmark as this seems to be the closest to what Rule 17a-7 of the U.S. Investment Company Act requires. However, in this section we show that our results are analogous choosing different benchmarks. As a first alternative, we replicate our trade-level analysis using the volume-weighted average price of the day instead of the price at the moment of the execution. Formally:

$$Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - VWAP_{i,d}|}{VWAP_{i,d}}, \quad (5)$$

where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$  of day  $d$ ; while  $VWAP_{i,d}$  is the volume-weighted average price for stock  $i$  in day  $d$  when trade  $j$  is executed. Results for the regression of this alternative measure of Execution Shortfall on *CT Dummy* are reported in Table X, Panel A. The results are qualitatively similar to those reported in Table II (i.e., using the market price at the moment of the execution as the benchmark price). Results obtained replicating the other tests in the paper using  $VWAP_{i,d}$  as main benchmark are also qualitatively similar and are therefore unreported. We have chosen not to present the results obtained using the volume-weighted average price benchmark as main results in the paper as the use of VWAP has potentially a few shortcomings (see Hasbrouck (2007), p. 148). For example, if a trade accounts for a large proportion of the daily volume, the weighted average execution price of the trade is likely to coincide with the VWAP.

As a second benchmark, we replicate our analysis using the opening price of the day. To make sure that our results are not driven by misreporting (some trades from ANcerno are arbitrarily set at the open price of the day), we exclude the trades executed *exactly* at the opening price. Therefore, we compute the execution shortfall as follows:

$$Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - Open_{i,d}|}{Open_{i,d}}, \quad (6)$$

where  $Open_{i,d}$  is the opening price for stock  $i$  in day  $d$ . Results are reported in Table X, Panel B and are unchanged.

### B. Cross-trades and Commissions

Our previous sections show that cross-trades are significantly mispriced (we estimate a marginal effect of cross-trades on *Execution Shortfall* of 0.18%) and likely to reallocate performance among trading counterparties. Yet we also show that commissions paid on each

dollar worth of cross-trading are significantly lower (around 10 basis points less than open market trades, see Table I). Is the difference in execution shortfall negligible after taking commissions into account? We replicate our analysis adding percentage commissions to the execution shortfall. Results reported in Table XI show that cross-trades still exhibit a 0.12% higher execution shortfall than open market trades *after* commissions are taken into account. Overall, this result confirms that the effect of cross-trades on performance is economically significant.

## VI. Conclusion

In this paper, we exploit institutional trade-level data provided by ANcerno to examine cross-trading practices. Off-exchange equity trading has reached 40% of the total in recent years.<sup>56</sup> Additionally, recent inquiries have uncovered improper use of cross-trading that allegedly penalized clients for several million of dollars and extended beyond the mutual fund industry.<sup>57</sup> To identify cross-trades, we look for pairs of trades coming from funds belonging to the same fund family that are executed in the same stock, involving the exact same quantity of shares traded, and sharing the same execution day, time, and price. Previous literature focuses on measures of cross-trading inferred by opposite side trades (often of different volumes) with the only requirement of occurring in the same quarter, thereby significantly misrepresenting real cross-trading activity.

Using our precise measure, we show that cross-trades exhibit an execution shortfall that is 0.18% higher than that of open market trades, 4.5 times the average percentage bid-ask spread in our sample. Additionally, we show that the execution price of the cross-trades appears to be sometimes set ex post to the highest or lowest price of the day. We furthermore provide evidence suggesting that cross-trading activity, when unsupervised, is mostly used as a device

---

<sup>56</sup>See “Dark markets may be more harmful than high-frequency trading” *Reuters* – April 7, 2014.

<sup>57</sup>See, e.g., administrative proceedings No. 3-15688 against Western Asset Management Co. (2014), No. 11-CV-0215 against Warren D. Nadel C. (2011), No. 2888 against Evergreen Investment Management Co., LLC (2009), No.18950 against Beacon Hill Asset Management (2004)).

to opportunistically shift performance, especially in heterogeneous fund families with large internal markets. Finally, we show that cross-trade mispricing is substantial in illiquid and highly volatile stocks, in times characterized by high uncertainty, and in the presence of weak supervision or governance.

We exploit an exogenous shock to industry regulation to rule out alternative explanations based on reverse causality, illiquidity, or changing trading conditions. We furthermore find that both the incentive to cross-trade and the severity of the mispricing diminish drastically when regulatory scrutiny increases. We show however that a number of cross-trades in our sample appears to have been backdated *after* the new regulation was introduced. Furthermore, we offer support to the hypothesis that star funds benefit from cross-trading at the expense of junk funds. This result indicates that fund alphas potentially misrepresent the real ability of fund managers to create value for their investors. Overall, our results suggest that off-exchange trading activity might have relevant implications for fund ranking, fund selection, and fund manager evaluation.

## References

- Agarwal, Vikas, Gerald D Gay, and Leng Ling, 2014, Window dressing in mutual funds, forthcoming, *Review of Financial Studies*.
- Anand, Amber, Paul Irvine, Andy Puckett, and Kumar Venkataraman, 2012, Performance of institutional trading desks: An analysis of persistence in trading costs, *Review of Financial Studies* 25, 557–598.
- Anton, Miguel, and Christopher Polk, 2014, Connected stocks, *The Journal of Finance* 69, 1099–1127.
- Basak, Suleyman, Anna Pavlova, and Alexander Shapiro, 2007, Optimal asset allocation and risk shifting in money management, *Review of Financial Studies* 20, 1583–1621.
- Ben-David, Itzhak, and David Hirshleifer, 2012, Are investors really reluctant to realize their losses? trading responses to past returns and the disposition effect, *Review of Financial Studies* 25, 2485–2532.
- Berkowitz, Stephen A, Dennis E Logue, and Eugene A Noser, 1988, The total cost of transactions on the NYSE, *The Journal of Finance* 43, 97–112.
- Bhattacharya, Utpal, Jung H Lee, and Veronika K Pool, 2013, Conflicting family values in mutual fund families, *The Journal of Finance* 68, 173–200.
- Brown, David P, and Youchang Wu, 2015, Mutual fund flows and cross-fund learning within families, *The Journal of Finance*.
- Brown, Keith C, W Van Harlow, and Laura T Starks, 1996, Of tournaments and temptations: An analysis of managerial incentives in the mutual fund industry, *The Journal of Finance* 51, 85–110.
- Busse, Jeffrey A, Tarun Chordia, Lei Jiang, and Yuehua Tang, 2015, Mutual fund trading costs and diseconomies of scale, *Working paper*.

- Buti, Sabrina, Barbara Rindi, and Ingrid M Werner, 2015, Dark pool trading strategies, market quality and welfare, *Journal of Financial Economics*.
- Carhart, Mark, 1997, Persistence in mutual fund performance, *The Journal of Finance* 57, 28–57.
- Casavecchia, Lorenzo, and Ashish Tiwari, 2015, Cross trading by investment advisers: Implications for mutual fund performance, *Journal of Financial Intermediation*.
- Chaudhuri, Ranadeb, Zoran Ivkovich, and Charles Trzcinka, 2012, Strategic performance allocation in institutional asset management firms: Behold the power of stars and dominant clients, *Working paper*.
- Chemmanur, Thomas J, Shan He, and Gang Hu, 2009, The role of institutional investors in seasoned equity offerings, *Journal of Financial Economics* 94, 384–411.
- Chen, Joseph, Harrison Hong, Ming Huan, and Jeffrey D. Kubik, 2004, Does fund size erode mutual fund performance? the role of liquidity and organization, *American Economic Review* 94, 1276–1302.
- Chen, Joseph, Harrison Hong, Wenxi Jiang, and Jeffrey D Kubik, 2013, Outsourcing mutual fund management: firm boundaries, incentives, and performance, *The Journal of Finance* 68, 523–558.
- Chevalier, Judith, and Glenn Ellison, 1997, Risk taking by mutual funds as a response to incentives, *Journal of Political Economy* 105, 1167–1200.
- Chuprinin, Oleg, Massimo Massa, and David Schumacher, 2015, Outsourcing in the international mutual fund industry: An equilibrium view, forthcoming, *The Journal of Finance*.
- Comerton-Forde, Carole, and Tālis J Putniņš, 2015, Dark trading and price discovery, *Journal of Financial Economics* 118, 70–92.

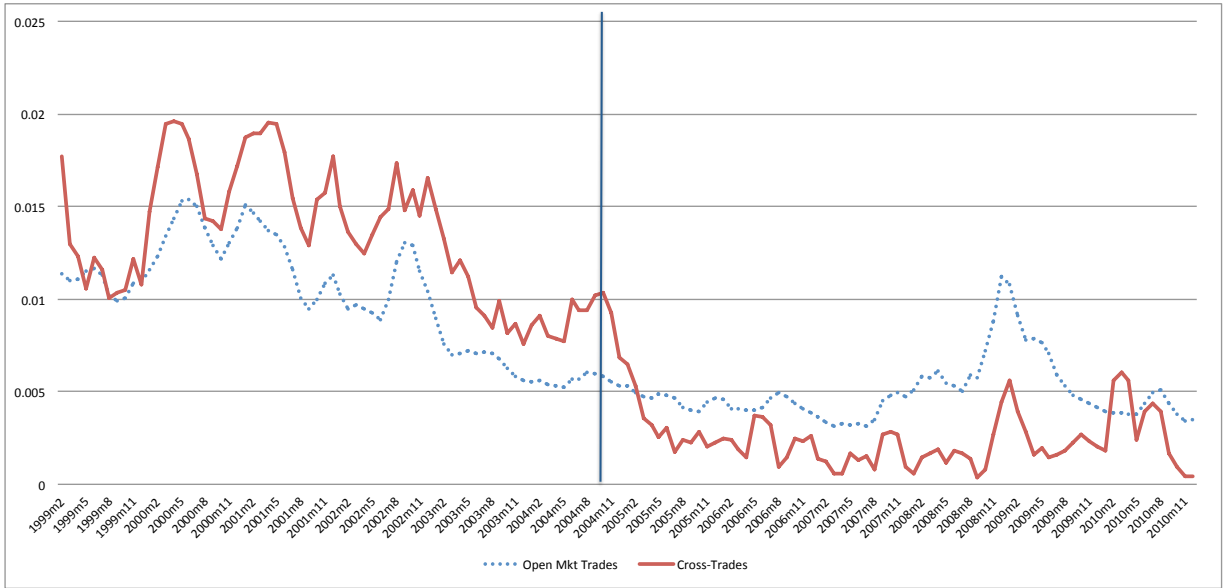
- Coval, Joshua, and Erik Stafford, 2007, Asset fire sales (and purchases) in equity markets, *Journal of Financial Economics* 86, 479–512.
- Degryse, Hans, Frank De Jong, and Vincent Van Kervel, 2015, The impact of dark trading and visible fragmentation on market quality, *Review of Finance* 19, 1587–1622.
- Dimmock, Stephen G, and William C Gerken, 2012, Predicting fraud by investment managers, *Journal of Financial Economics* 105, 153–173.
- Elton, Edwin J, Martin J Gruber, and T Clifton Green, 2007, The impact of mutual fund family membership on investor risk, *Journal of Financial and Quantitative Analysis* 42, 257.
- Evans, Richard B, 2010, Mutual fund incubation, *The Journal of Finance* 65, 1581–1611.
- Ferreira, Miguel A, Aneel Keswani, António F Miguel, and Sofia B Ramos, 2012, The determinants of mutual fund performance: A cross-country study, *Review of Finance* p. rfs013.
- Ferris, Stephen P, and Xuemin Sterling Yan, 2009, Agency costs, governance, and organizational forms: Evidence from the mutual fund industry, *Journal of Banking & Finance* 33, 619–626.
- Frazzini, Andrea, and Owen A Lamont, 2008, Dumb money: Mutual fund flows and the cross-section of stock returns, *Journal of Financial Economics* 88, 299–322.
- Gaspar, Jose-Miguel, Massimo Massa, and Pedro Matos, 2006, Favoritism in mutual fund families? evidence on strategic cross-fund subsidization, *The Journal of Finance* 61, 73–104.
- Goncalves-Pinto, Luis, and Juan Sotes-Paladino, 2015, The invisible hand of internal markets in mutual fund families, *Working Paper, National University of Singapore*.
- Guedj, Ilan, and Jannette Papastaikoudi, 2008, Can mutual fund families affect the performance of their funds, *Working paper, University of Texas at Austin*.



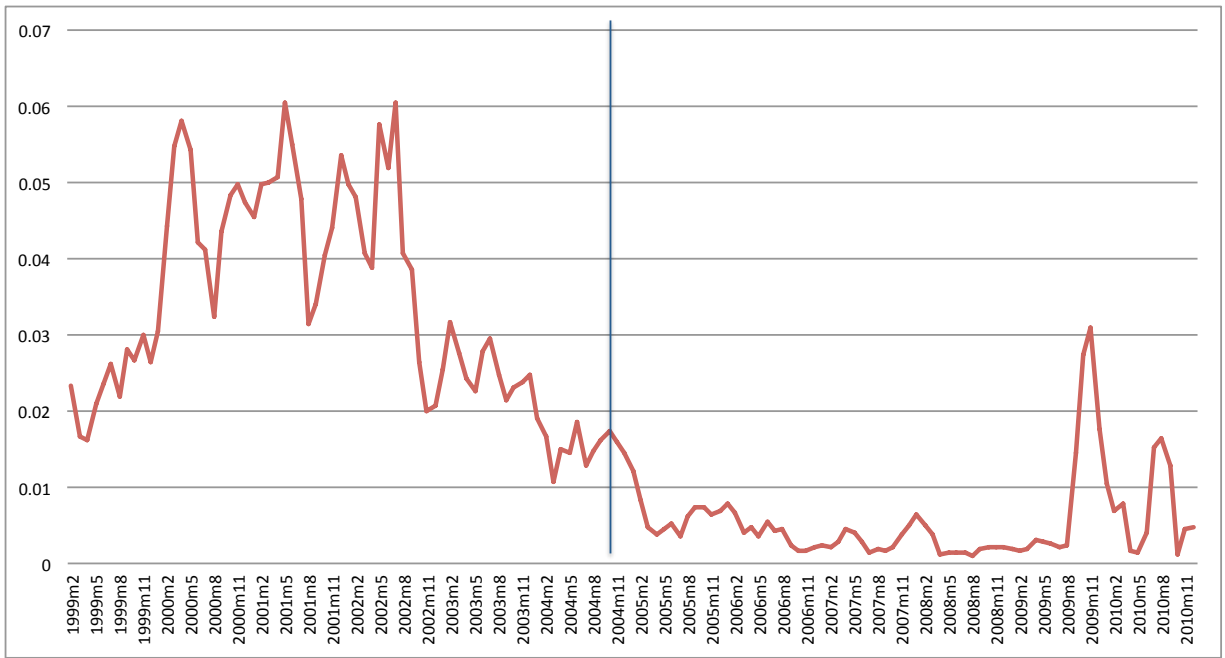
- Hasbrouck, Joel, 2007, *Empirical market microstructure: The institutions, economics, and econometrics of securities trading* (Oxford University Press).
- Haslem, John A, 2010, *Mutual Funds: Portfolio structures, analysis, management, and stewardship* (Wiley).
- Hu, Gang, 2009, Measures of implicit trading costs and buy–sell asymmetry, *Journal of Financial Markets* 12, 418–437.
- Huang, Jennifer, Clemens Sialm, and Hanjiang Zhang, 2011, Risk shifting and mutual fund performance, *Review of Financial Studies*.
- Jurado, K, S Ludvigson, and S Ng, 2015, Forthcoming. measuring uncertainty, *American Economic Review*.
- Kacperczyk, Marcin, Stijn Van Nieuwerburgh, and Laura Veldkamp, 2014, Time-varying fund manager skill, *The Journal of Finance* 69, 1455–1484.
- Kacperczyk, Marcin, Clemens Sialm, and Lu Zheng, 2005, On the industry concentration of actively managed equity mutual funds, *The Journal of Finance* 60, 1983–2011.
- , 2008, Unobserved actions of mutual funds, *Review of Financial Studies* 21, 2379–2416.
- Kacperczyk, Marcin, Stijn Van Nieuwerburgh, and Laura Veldkamp, 2016, A rational theory of mutual funds’ attention allocation, *Econometrica* 84, 571–626.
- Kempf, Alexander, and Stefan Ruenzi, 2008, Tournaments in mutual-fund families, *Review of Financial Studies* 21, 1013–1036.
- , and Tanja Thiele, 2009, Employment risk, compensation incentives, and managerial risk taking: Evidence from the mutual fund industry, *Journal of Financial Economics* 92, 92–108.

- Lou, Dong, 2012, A flow-based explanation for return predictability, *Review of Financial Studies* 25, 3457–3489.
- Madhavan, Ananth N, 2002, Vwap strategies, *Trading* 2002, 32–39.
- Massa, Massimo, 2003, How do family strategies affect fund performance? when performance-maximization is not the only game in town, *Journal of Financial Economics* 67, 249–304.
- , and Rajdeep Patgiri, 2009, Incentives and mutual fund performance: higher performance or just higher risk taking?, *Review of Financial Studies* 22, 1777–1815.
- McCabe, Patrick E, 2009, The economics of the mutual fund trading scandal, *Working paper, Federal Reserve Board*.
- Nanda, Vikram, Z. Jay Wang, and Lu Zheng, 2004, Family values and the star phenomenon: Strategies of mutual fund families, *Review of Financial Studies* 17, 668–698.
- Pollet, J.M., and M. Wilson, 2008, How does size affect mutual fund behavior?, *The Journal of Finance* 63, 2941–2969.
- Puckett, Andy, and Xuemin Sterling Yan, 2011, The interim trading skills of institutional investors, *The Journal of Finance* 66, 601–633.
- Schmidt, Breno, and Luis Goncalves-Pinto, 2013, Co-insurance in mutual fund families, *Working Paper, Emory University*.
- Sirri, Erik R., and Peter Tufano, 1998, Costly search and mutual fund flows, *The Journal of Finance* 53, 1589–1622.
- Stein, Jeremy, and David S Scharfstein, 2000, The dark side of internal capital markets: divisional rent-seeking and inefficient investment, *Journal of Finance* 55.
- Stein, Jeremy C, 1997, Internal capital markets and the competition for corporate resources, *The Journal of Finance* 52, 111–133.

Zitzewitz, Eric, 2006, How widespread was late trading in mutual funds?, *American Economic Review* 96, 284–289.



**Figure 1: Execution Shortfall over time for cross-trades and open market trades.** The execution shortfall is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}}$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ . We present results obtained computing three-month moving averages in order to smooth the series. Cross-trades are defined as indicated in Section II.B. The investigation into illegal trading practices in the mutual fund industry started on September 3, 2003 when New York Attorney General Eliot Spitzer announced the issuance of a complaint against Canary Capital Partners LLC claiming that they had engaged in late trading. As a consequence rules 38a-1 and 206(4)-7 and the amendments to rule 204-2 were introduced. Industry participants had to comply to the new rules by October 5, 2004 (see vertical line).



**Figure 2: Percentage of cross-trading activity over time.** Cross-trading activity is computed as the monthly dollar amount of cross-trading over the monthly dollar amount of total trading. The three-month moving average is plotted in order to smooth the series. Cross-trades are defined as indicated in Section II.B. The investigation into illegal trading practices in the mutual fund industry started on September 3, 2003 when New York Attorney General Eliot Spitzer announced the issuance of a complaint against Canary Capital Partners LLC claiming that they had engaged in late trading. As a consequence rules 38a-1 and 206(4)-7 and the amendments to rule 204-2 were introduced. Industry participants had to comply to the new rules by October 5, 2004 (see vertical line).

**Table I: Summary Statistics**

This table provides summary statistics for our sample (Panel A) and correlations among the main variables (Panel B). The average values reported are obtained extracting a 1% random sample of trades without replacement from ANcerno. Cross-trades are defined as trades that occur in the same stock, the same quantity, the same price, on the same day and time but displaying opposite side as at least one other trade reported by the same fund family. All other trades are defined as open market trades. Column (1) reports the number of observations available for each variable, Column (2) reports the average value of the variable irrespectively on whether a trade is crossed or not, Column (3) reports averages for open market trades only, Column (4) reports averages for cross-trades only, Column (5) reports the average difference between open market trades and cross-trades (i.e., the difference between Column (3) and Column (4)), Column (6) indicates  $t$ -statistics for a two-sided test on whether the difference reported in Column (5) is statistically different from zero. *Share Volume* is the average share size of the trade; *Dollar \$Volume* is the average size of the trade in dollars; the execution shortfall is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}}$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ ; *Illiquidity* is Amihud's monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid – Ask Spread* is the difference between the bid and the ask price as reported by CRSP;  $1/Price$  is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the S&P500 index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns; *Commissions(\$/share)* is the dollar commission paid for a trade over share volume; *Commissions(\$/\$trade)* is the dollar commission paid for a trade over dollar trade volume.

<b>Panel A: Sample Statistics</b>						
	Observations (1)	Full Sample (2)	Open Trades (3)	Cross-Trades (4)	Diff. (5)	$t$ -stat. (6)
Share Volume	966,186	7,092	7,014	17,332	-10,318	-22.50
Dollar Volume	966,186	21,5581	212,707	589,603	-376,896	-28.10
Execution Price	966,186	42.58	42.57	44.17	-1.60	-0.23
Execution Shortfall	965,711	0.0065	0.0064	0.0084	-0.0019	-16.29
Illiquidity	966,186	0.0443	0.0445	0.0268	0.0177	0.090
Bid-Ask Spread	966,186	0.0031	0.0031	0.0041	-0.0011	-14.42
S&P500 Dummy	966,186	0.5153	0.5148	0.5817	-0.0668	-11.44
Volatility	966,186	0.1133	0.1132	0.1270	-0.0139	-14.93
Market Equity Decile	966,186	7.2198	7.2150	7.8377	-0.6226	-18.92
1/Price	966,186	0.0518	0.0518	0.0469	0.0049	2.88
Commissions (\$/share)	965,595	0.0243	0.0245	0.0016	0.0229	69.45
Commissions (\$/\$trade)	965,595	0.0011	0.0011	0.0001	0.0010	8.040

<b>Panel B: Correlations</b>						
	Execution S.	S Volume	B/M	ME	Bid-Ask	1/Price
Execution Shortfall	1.0000					
Share Volume	0.1194	1.0000				
B/M Dec	0.0097	0.0181	1.0000			
ME Dec	-0.1208	-0.0135	-0.2669	1.0000		
Bid-Ask Spread	0.1815	0.1373	0.0729	-0.1476	1.0000	
1/Price	0.1279	0.0743	0.1615	-0.2776	0.2822	1.0000

**Table II: Do Cross-Trades Exhibit Higher Execution Shortfall?**

This table reports OLS estimates obtained by regressing *Execution Shortfall* on *CT Dummy* and controls. The execution shortfall is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}}$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ . *CT Dummy* equals one if a trade is a cross-trade and equals zero when a trade is executed in the open market. *Volume* is the share volume of the trade; *Illiquidity* is Amihud’s monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid – Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP; *1/Price* is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the *S&P500* index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. Observations are at the trade level and are obtained by drawing a 1% random sample of trades from ANcerno without replacement. Stock, time, and family fixed effects are included and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Execution Shortfall				
	(1)	(2)	(3)	(4)	(5)
CT Dummy	0.0019*** (5.44)	0.0018*** (5.35)	0.0018*** (5.35)	0.0019*** (5.40)	0.0018*** (5.37)
Volume		0.0002*** (12.00)	0.0002*** (12.01)	0.0002*** (11.57)	0.0002*** (11.24)
Illiquidity			0.0402*** (3.83)	0.0265*** (4.63)	0.0287*** (4.55)
Bid-Ask Spread				-0.0057 (-0.40)	-0.0041 (-0.37)
1/Price				0.0037*** (4.04)	0.0027*** (3.69)
Market Equity Decile					-0.0001** (-2.00)
S&P 500 Dummy					-0.0003*** (-3.17)
Volatility					0.0195*** (17.97)
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	964,972	964,972	964,972	964,972	964,972
R-squared	0.208	0.209	0.209	0.211	0.220

**Table III: What was the Impact of Restrictive Regulation on the Pricing of the Cross-Trades?**

This table reports OLS estimates obtained by regressing *Execution Shortfall* on *CT Dummy*, *Post Regulation*, and controls. The execution shortfall is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}}$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ . *Post Regulation* equals one for trades executed from October 2004 onwards and equals zero before of that; *CT Dummy* equals one if a trade is a cross-trade and equals zero when a trade is executed in the open market. *Volume* is the share volume of the trade; *Illiquidity* is Amihud's monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid – Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP; *1/Price* is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the *S&P500* index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. Observations are at the trade level and are obtained by drawing a 1% random sample of trades from ANcerno without replacement. Stock, time, and family fixed effects are included when specified and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Execution Shortfall				
	(1)	(2)	(3)	(4)	(5)
CT Dummy x Post Regulation	-0.0066*** (-16.30)	-0.0061*** (-15.74)	-0.0059*** (-15.65)	-0.0059*** (-15.79)	-0.0059*** (-15.67)
CT Dummy	0.0049*** (15.41)	0.0048*** (15.82)	0.0047*** (15.47)	0.0047*** (15.63)	0.0046*** (15.52)
Post Regulation	-0.0030*** (-7.16)				
Volume			0.0002*** (11.89)	0.0002*** (11.44)	0.0002*** (11.11)
Illiquidity			0.0403*** (3.83)	0.0265*** (4.62)	0.0287*** (4.54)
Bid-Ask Spread				-0.0051 (-0.36)	-0.0036 (-0.33)
1/Price				0.0037*** (4.03)	0.0027*** (3.68)
Market Equity Decile					-0.0001** (-2.03)
S&P500 Dummy					-0.0003*** (-3.12)
Volatility					0.0195*** (17.96)
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	No	Yes	Yes	Yes	Yes
Observations	964,972	964,972	964,972	964,972	964,972
R-squared	0.161	0.209	0.210	0.211	0.220



**Table IV: Which/When Cross-Trades are Mispriced?**

This table reports OLS estimates obtained by regressing *Execution Shortfall* on *CT Dummy*, interactions of *CT Dummy* and stock and markets characteristics, and controls. The execution shortfall is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}}$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ . *CT Dummy* equals one if a trade is a cross-trade and equals zero when a trade is executed in the open market. *Volume* is the share volume of the trade; *Illiquidity* is Amihud's monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid – Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP;  $1/Price$  is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the S&P500 index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. *Beta* is the stock market beta estimated assuming the CAPM. *VIX* is the Volatility Index, *NBER* is a dummy variable that takes value one during crises and equals zero otherwise. Macro and Financial Uncertainty are from Jurado, Ludvigson, and Ng (2015); *CS Vol* is the cross-sectional standard deviation of daily returns in the previous day, *Mkt Return* is cumulative stock market return in the previous month. **All non-interacted variables are included but coefficients are not reported.** Observations are at the trade level and are obtained by drawing a 1% random sample of trades from ANcerno without replacement. Stock, time, and family fixed effects are included and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Execution Shortfall				
	(1)	(2)	(3)	(4)	(5)
<i>Stock Characteristics</i>					
CT Dummy x Illiquidity	0.2248*** (3.49)	0.1633** (2.38)	0.1416** (2.16)	0.1813** (2.19)	0.1930** (2.55)
CT Dummy x Bid-Ask Spread	0.1103*** (4.60)	0.1122*** (4.73)	0.0981*** (3.53)	-0.0282 (-0.83)	-0.0414 (-1.05)
CT Dummy x 1/Price	-0.0045 (-1.53)	-0.0061** (-2.07)	-0.0110*** (-3.53)	-0.0093*** (-3.03)	-0.0059** (-1.99)
CT Dummy x Beta			0.0003 (1.14)	0.0002 (0.73)	0.0001 (0.20)
CT Dummy x Volatility			0.0145*** (3.43)	0.0135*** (3.30)	0.0118*** (2.80)
<i>Market Conditions</i>					
CT Dummy x VIX				0.0000 (0.07)	-0.0000 (-0.83)
CT Dummy x NBER				0.0010 (0.93)	0.0004 (0.35)
CT Dummy x Macro Uncert.				-0.0351*** (-9.21)	-0.0318*** (-8.31)
CT Dummy x Fin. Uncert.				0.0095*** (4.50)	0.0071*** (3.08)
CT Dummy x CS Vol.					0.0668*** (2.83)
CT Dummy x Mkt Return					0.0003 (0.04)
CT Dummy	0.0016*** (4.61)	0.0017*** (4.81)	-0.0003 (-0.88)	0.0157*** (7.08)	0.0141*** (5.84)
Time-Varying Controls	No	Yes	Yes	Yes	Yes
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	No	No
Observations	964,972	964,972	951,993	951,993	951,993
R-squared	0.211	0.220	0.219	0.194	0.204

**Table V: Are Cross-Trades Backdated?**

This table reports logit estimates of the probability of a trade to be executed either at exactly the highest or at exactly the lowest price of the day (marginal probabilities are reported). *CT Dummy* equals one if a trade is a cross-trade and equals zero when the trade is executed in the open market. *Post Regulation* equals one for trades executed from October 2004 onwards and equals zero before of that; *Volume* is the share volume of the trade; *Illiquidity* is Amihud's monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid-Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP; *1/Price* is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the *S&P500* index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. Observations are at the trade level and are obtained by drawing a 1% random sample of trades from ANcerno without replacement. Only observations from families that cross-trade at least once are included. Family fixed effects are included and errors are clustered at the time level. Results using the full specification model are presented in Table A.III. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Highest/Lowest Price of the Day
	(1)
CT Dummy x Post Regulation	-0.0121*** (-5.09)
CT Dummy	0.0172*** (9.76)
Post Regulation	-0.0071*** (-8.29)
Volume	-0.0023*** (-18.97)
Illiquidity	-0.0352 (-1.56)
Bid-Ask Spread	0.4806*** (12.96)
1/Price	0.0014*** (2.13)
Market Equity Decile	-0.0029*** (-22.24)
S&P500 Dummy	0.0036*** (5.57)
Volatility	-0.0323*** (-7.26)
Family Fixed Effect	Yes
Observations	816,721
Pseudo R2	0.12

**Table VI: Do Different Fund Families Price Cross-Trades Differently?**

This table reports OLS estimates obtained by regressing *Execution Shortfall* on mutual fund family level variables and controls. Importantly, **only cross-trades are included**. The execution shortfall is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{P_{j,i,t} - P_{i,t}}{P_{i,t}}$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ ; *Weak Governance* equals one for families investigated by the SEC for practices potentially harming investors and equals zero otherwise; *Siblings* is the natural log of the number of equity funds in the fund family; *Return Dispersion* is the lagged monthly cross-sectional return standard deviation inside the family; *Exp. Ratio Dispersion* is the lagged within-family cross-sectional standard deviation of the expense ratios; *Fees Gap* is the highest fee minus the lowest fee charged within the fund family; fees are computed as the expense ratio plus 1/7th of rear and back-load fees; Column (8) reports coefficients estimated keeping in our sample only observations after rules 38a-1 and 206(4)-7 compliance date (October 5, 2004). Time and Stock fixed effects are included and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Sample:	(1) All	(2) All	(3) All	(4) All	(5) All	(6) All	(7) All	(8) Post 2004
Weak Governance	0.0052*** (10.34)						0.0027*** (3.65)	0.0019** (2.50)
Siblings		0.0016*** (4.88)					0.0015*** (3.09)	0.0016** (2.25)
Family Size			0.0001 (0.07)				-0.0046** (-2.27)	-0.0063*** (-2.88)
Returns Dispersion				0.0584 (1.51)			-0.0117 (-0.42)	-0.0304 (-0.70)
Fees Gap					0.3177*** (12.71)		0.1119*** (3.90)	0.1148*** (3.53)
Fees Dispersion						1.4014*** (13.24)		
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	738,476	738,476	738,476	738,476	738,476	738,476	738,476	309,780
R-squared	0.422	0.419	0.417	0.417	0.421	0.421	0.423	0.290

**Table VII: Does Cross-Trading Increase the Difference in Performance between Star and Junk Funds?**

This table presents results for regressions of *Spread of Style Adj. returns (4-factor Alphas)* on *CT%* and controls. Each observation is obtained from the pairwise combinations of inflow funds and outflow funds drawn from the same family, month, and style. The hypotheses tested are no performance shifting ( $\beta = 0$ ), cross-subsidization of junk funds ( $\beta < 0$ ), winner-picking ( $\beta > 0$ ). The dependent variable is computed as the return (4-factor alpha) of inflow fund  $i$  (i.e., funds with flows in the top tercile of family  $f$  in a given month  $t$ ) minus the outflow fund  $j$ 's return (4-factor alpha), i.e., funds with flows in the bottom tercile of family  $f$  in a given month  $t$ . Funds with flows in the intermediate tercile are dropped. *CT%* is computed as the percentage of trades that are crossed between siblings for family  $f$  in month  $t$ . The other independent variables are: *Family Size*, the natural log of total assets under management at the family level in month  $t-1$ ;  $\Delta Size$ , the log difference between lagged funds'  $i$  and  $j$  total assets under management;  $\Delta Flows$ , the difference in funds'  $i$  and  $j$  flows;  $\Delta PastFlows$ , the difference in funds'  $i$  and  $j$  lagged flows;  $\Delta PastReturns$ , the difference in funds'  $i$  and  $j$  lagged returns; and *Returns Dispersion*, the monthly lagged cross-sectional standard deviation of returns inside the family. The constant is included in all specifications but the coefficient is not reported. The frequency of the observations is monthly. Time/Family/Fund Pair fixed effects are included when specified and errors are clustered at the time level. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Spread of Style Adj. returns			Spread of 4-factor Alphas				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CT%	0.1254*** (3.53)	0.1431*** (4.11)	0.1253*** (3.68)	0.1536*** (4.47)	0.0507** (2.32)	0.0619*** (2.79)	0.0509** (2.41)	0.0678*** (3.11)
Family Size			-0.0012 (-1.31)	-0.0010 (-0.99)			0.0001 (0.24)	-0.0004 (-0.62)
Return Dispersion			0.0862 (1.40)	0.0633 (1.11)			0.0885** (2.00)	0.0731* (1.67)
$\Delta Size$			-0.0002 (-0.84)	-0.0027** (-2.49)			0.0000 (0.30)	-0.0014** (-2.56)
$\Delta Flows$			0.0788*** (8.57)	0.0707*** (6.78)			0.0636*** (11.56)	0.0536*** (10.13)
$\Delta PastFlows$			-0.0306*** (-3.66)	-0.0238*** (-3.14)			-0.0254*** (-5.07)	-0.0213*** (-4.37)
$\Delta PastReturns$			0.0071 (0.11)	-0.0727 (-1.24)			-0.0084 (-0.40)	-0.0576*** (-3.00)
Fund Pair Fixed Effect	No	Yes	No	Yes	No	Yes	No	Yes
Family Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	108,720	108,086	108,332	107,706	108,720	108,086	108,332	107,706
R-squared	0.131	0.261	0.145	0.276	0.068	0.189	0.088	0.207

**Table VIII: Does Cross-Trading Boost the Performance of High-Fee Funds?**

This table presents results for regressions of *Spread of Style Adj. returns* (*4-factor Alphas*) on *CT%* and controls. Each observation is obtained from the pairwise combinations of inflow funds and outflow funds drawn from the same family, month, and style. The hypotheses tested are no performance shifting ( $\beta = 0$ ), cross-subsidization of junk funds ( $\beta < 0$ ), winner-picking ( $\beta > 0$ ). The dependent variable is computed as the return (4-factor alpha) of inflow fund *i* (i.e., funds with flows in the top tercile of family *f* in a given month *t*) minus the outflow fund *j*'s return (4-factor alpha), i.e., funds with flows in the bottom tercile of family *f* in a given month *t*. Funds with flows in the intermediate tercile are dropped. *CT%* is computed as the percentage of trades that are crossed between siblings for family *f* in month *t*. *Fees* is defined as  $Expense\ Ratio + 1/7(FrontLoad + RearLoad)$ . The other independent variables are: *High Fees* equals one if a fund charges above median fees within its family in month *t* and zero otherwise; *Family Size*, the natural log of total assets under management at the family level in month *t*-1;  $\Delta Size$ , the difference in the natural log of the lagged funds' *i* and *j* total assets under management;  $\Delta Flows$ , the difference in funds' *i* and *j* flows;  $\Delta PastFlows$ , the difference in funds' *i* and *j* lagged flows;  $\Delta PastReturns$ , the difference in funds' *i* and *j* lagged returns; and *Returns Dispersion*, the monthly lagged cross-sectional standard deviation of returns inside the family. The constant is included in all specifications but the coefficient is not reported. The frequency of the observations is monthly. Time/Family/Fund Pair fixed effects are included when specified and errors are clustered at the time level. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Spread of Style Adj. returns				Spread of 4-factor Alphas			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>CT%</i> x High-Fees	0.1769*** (4.66)	0.1005*** (2.72)	0.1460*** (4.10)	0.0871** (2.44)	0.1247*** (5.46)	0.0492* (1.97)	0.1042*** (4.60)	0.0396 (1.60)
<i>CT%</i>	0.0205 (0.45)	0.0829** (2.03)	0.0362 (0.81)	0.1000** (2.48)	-0.0232 (-0.94)	0.0316 (1.37)	-0.0129 (-0.53)	0.0424* (1.80)
High-Fees	0.0029*** (3.64)	0.0012 (0.66)	0.0022** (2.41)	0.0022 (1.28)	0.0021*** (3.39)	0.0012 (0.85)	0.0018*** (3.26)	0.0019 (1.30)
Family Size			-0.0014 (-1.48)	-0.0009 (-0.96)			0.0000 (0.02)	-0.0004 (-0.59)
Return Dispersion			0.0889 (1.45)	0.0637 (1.11)			0.0907** (2.06)	0.0736* (1.68)
$\Delta Size$			0.0001 (0.25)	-0.0027** (-2.51)			0.0003** (2.22)	-0.0015** (-2.57)
$\Delta Flows$			0.0736*** (7.94)	0.0708*** (6.77)			0.0597*** (11.05)	0.0538*** (10.07)
$\Delta PastFlows$			-0.0297*** (-3.55)	-0.0235*** (-3.09)			-0.0247*** (-4.97)	-0.0210*** (-4.29)
$\Delta PastReturns$			0.0036 (0.06)	-0.0728 (-1.24)			-0.0111 (-0.52)	-0.0577*** (-3.00)
Fund Pair Fixed Effect	No	Yes	No	Yes	No	Yes	No	Yes
Family Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	108,720	108,086	108,332	107,706	108,720	108,086	108,332	107,706
R-squared	0.138	0.261	0.150	0.277	0.075	0.190	0.092	0.208

**Table IX: What was the Impact of Restrictive Regulation on Performance Shifting?**

This table presents results for regressions of *Spread of Style Adj. returns (4-factor Alphas)* on *CT%* and controls. Each observation is obtained from the pairwise combinations of inflow funds and outflow funds drawn from the same family, month, and style. The hypotheses tested are no performance shifting ( $b = 0$ ), cross-subsidization of junk funds ( $b > 0$ ), winner-picking ( $b < 0$ ) where  $b$  is the coefficient of *CT% $\times$ Post*. The dependent variable is computed as the return (4-factor alpha) of inflow fund  $i$  (i.e., funds with flows in the top tercile of family  $f$  in a given month  $t$ ) minus the outflow fund  $j$ 's return (4-factor alpha), i.e., funds with flows in the bottom tercile of family  $f$  in a given month  $t$ . Funds with flows in the intermediate tercile are dropped. *CT%* is computed as the percentage of trades that are crossed between siblings for family  $f$  in month  $t$ . The other independent variables are: *Post* equals one after rules 38a-1 and 206(4)-7 compliance date (October 5, 2004), and zero before; *Family Size*, the natural log of total assets under management at the family level in month  $t-1$ ;  $\Delta$ *Size*, the difference in the natural log of the lagged funds'  $i$  and  $j$  total assets under management;  $\Delta$ *Flows*, the difference in funds'  $i$  and  $j$  flows;  $\Delta$ *PastFlows*, the difference in funds'  $i$  and  $j$  lagged flows;  $\Delta$ *PastReturns*, the difference in funds'  $i$  and  $j$  lagged returns; and *Returns Dispersion*, the monthly lagged cross-sectional standard deviation of returns inside the family. The constant is included in all specifications but the coefficient is not reported. The frequency of the observations is monthly. Time/Family/Fund Pair fixed effects are included when specified and errors are clustered at the time level. \*\*\*, \*\*, \*, indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Spread of Style Adj. returns				Spread of 4-factor Alphas			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>CT% x Post</i>	-0.2288*** (-3.00)	-0.1563*** (-2.99)	-0.1493*** (-3.11)	-0.1713*** (-3.61)	-0.1368*** (-3.10)	-0.0912** (-2.29)	-0.0899*** (-2.88)	-0.0996*** (-2.81)
<i>CT%</i>	0.1846*** (3.04)	0.1545*** (4.21)	0.1330*** (3.74)	0.1662*** (4.60)	0.0804*** (2.80)	0.0686*** (2.95)	0.0556*** (2.54)	0.0752*** (3.31)
<i>Post</i>	-0.0025 (-1.25)				-0.0019 (-1.35)			
<i>Family Size</i>			-0.0012 (-1.30)	-0.0010 (-1.01)			0.0002 (0.25)	-0.0004 (-0.64)
<i>Return Dispersion</i>			0.0821 (1.34)	0.0587 (1.04)			0.0860* (1.96)	0.0705 (1.62)
$\Delta$ <i>Size</i>			-0.0003 (-0.85)	-0.0027** (-2.51)			0.0000 (0.28)	-0.0015** (-2.58)
$\Delta$ <i>Flows</i>			0.0790*** (8.57)	0.0709*** (6.79)			0.0637*** (11.57)	0.0537*** (10.13)
$\Delta$ <i>PastFlows</i>			-0.0306*** (-3.66)	-0.0238*** (-3.14)			-0.0254*** (-5.07)	-0.0212*** (-4.37)
$\Delta$ <i>PastReturns</i>			0.0071 (0.11)	-0.0727 (-1.24)			-0.0084 (-0.40)	-0.0576*** (-3.00)
<i>Fund Pair Fixed Effect</i>	Yes	Yes	No	Yes	Yes	Yes	No	Yes
<i>Family Fixed Effect</i>	No	No	Yes	No	No	No	Yes	No
<i>Time Fixed Effect</i>	No	Yes	Yes	Yes	No	Yes	Yes	Yes
<i>Observations</i>	108,086	108,086	108,332	107,706	108,086	108,086	108,332	107,706
<i>R-squared</i>	0.183	0.261	0.146	0.277	0.159	0.189	0.088	0.207

**Table X: Alternative Benchmarks**

This table reports the OLS estimates obtained by regressing *Execution Shortfall* on *CT Dummy* and controls. *Execution Shortfall* in Panel A is defined as  $\frac{|P_{j,i,t} - VWAP_{i,d}|}{VWAP_{i,d}}$  where  $VWAP_{i,d}$  is the volume weighted average price of stock  $i$  in the day  $d$  when trade  $j$  is executed. *Execution Shortfall* in Panel B is defined as  $\frac{|P_{j,i,t} - Open_{i,d}|}{Open_{i,d}}$  where  $Open_{i,d}$  is the opening price of stock  $i$  in the day  $d$  when trade  $j$  is executed. *CT Dummy* equals one if a trade is a cross-trade and equals zero when a trade is executed in the open market. *Volume* is the share volume of the trade; *Illiquidity* is Amihud's monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid – Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP;  $1/Price$  is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the S&P500 index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. Observations are at the trade level and are obtained by drawing a 1% sample of trades from ANcerno without replacement. Stock, time, and family fixed effects are included and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Panel A: VWAP				
	(1)	(2)	(3)	(4)	(5)
CT Dummy	0.0021*** (11.35)	0.0021*** (11.34)	0.0021*** (11.34)	0.0021*** (11.46)	0.0021*** (11.53)
Volume		-0.0002*** (-16.95)	-0.0002*** (-16.94)	-0.0002*** (-17.64)	-0.0002*** (-17.98)
Illiquidity			0.0110* (1.84)	-0.0025 (-0.46)	0.0012 (0.28)
Bid-Ask Spread				-0.0062 (-0.60)	-0.0102 (-1.59)
1/Price				0.0037*** (4.22)	0.0025*** (3.88)
Market Equity Decile					-0.0003*** (-4.63)
S&P500 Dummy					0.0000 (0.06)
Volatility					0.0175*** (17.75)
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	965,433	965,433	965,433	965,433	965,433
R-squared	0.189	0.190	0.190	0.193	0.207

**Table X Continued:**

	Panel B: Open Price				
	(1)	(2)	(3)	(4)	(5)
CT Dummy	0.0017*** (4.97)	0.0016*** (4.72)	0.0016*** (4.72)	0.0016*** (4.94)	0.0016*** (4.96)
Volume		0.0003*** (13.56)	0.0003*** (13.55)	0.0003*** (12.20)	0.0002*** (10.00)
Illiquidity			0.0161 (0.51)	-0.0386** (-2.54)	-0.0247 (-1.65)
Bid-Ask Spread				-0.0117 (-0.30)	-0.0344 (-1.32)
1/Price				0.0163*** (4.99)	0.0121*** (5.14)
Market Equity Decile					-0.0011*** (-4.97)
S&P500 Dummy					0.0005* (1.72)
Volatility					0.0529*** (14.34)
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	949,254	949,254	949,254	949,254	949,254
R-squared	0.201	0.201	0.201	0.208	0.224



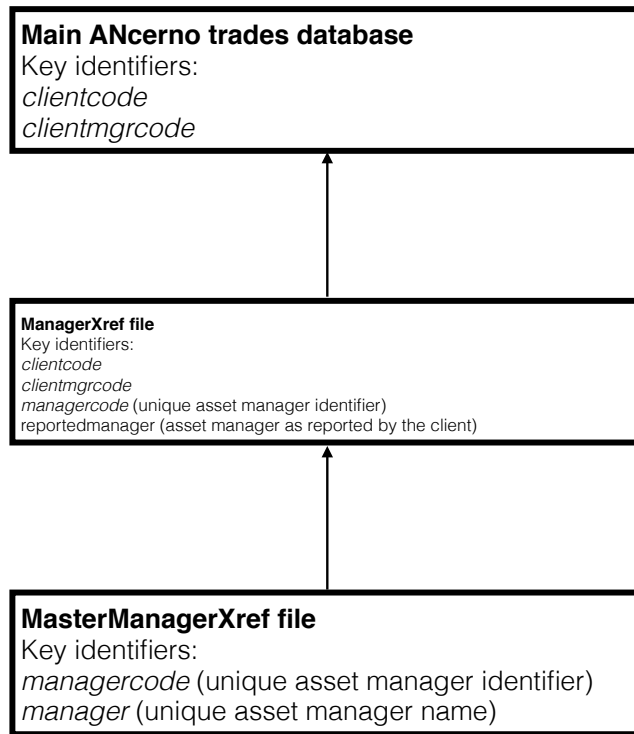
**Table XI: Is it just Commissions?**

This table reports OLS estimates obtained by regressing *Execution Shortfall* on *CT Dummy* and controls. *Execution Shortfall* is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}} + \%commissions$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ . *CT Dummy* equals one if a trade is a cross-trade and equals zero when a trade is executed in the open market. *Volume* is the share volume of the trade; *Illiquidity* is Amihud's monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid – Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP; *1/Price* is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the *S&P500* index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. Observations are at the trade level and are obtained by drawing a 1% sample of trades from ANcerno without replacement. Stock, time, and family fixed effects are included and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

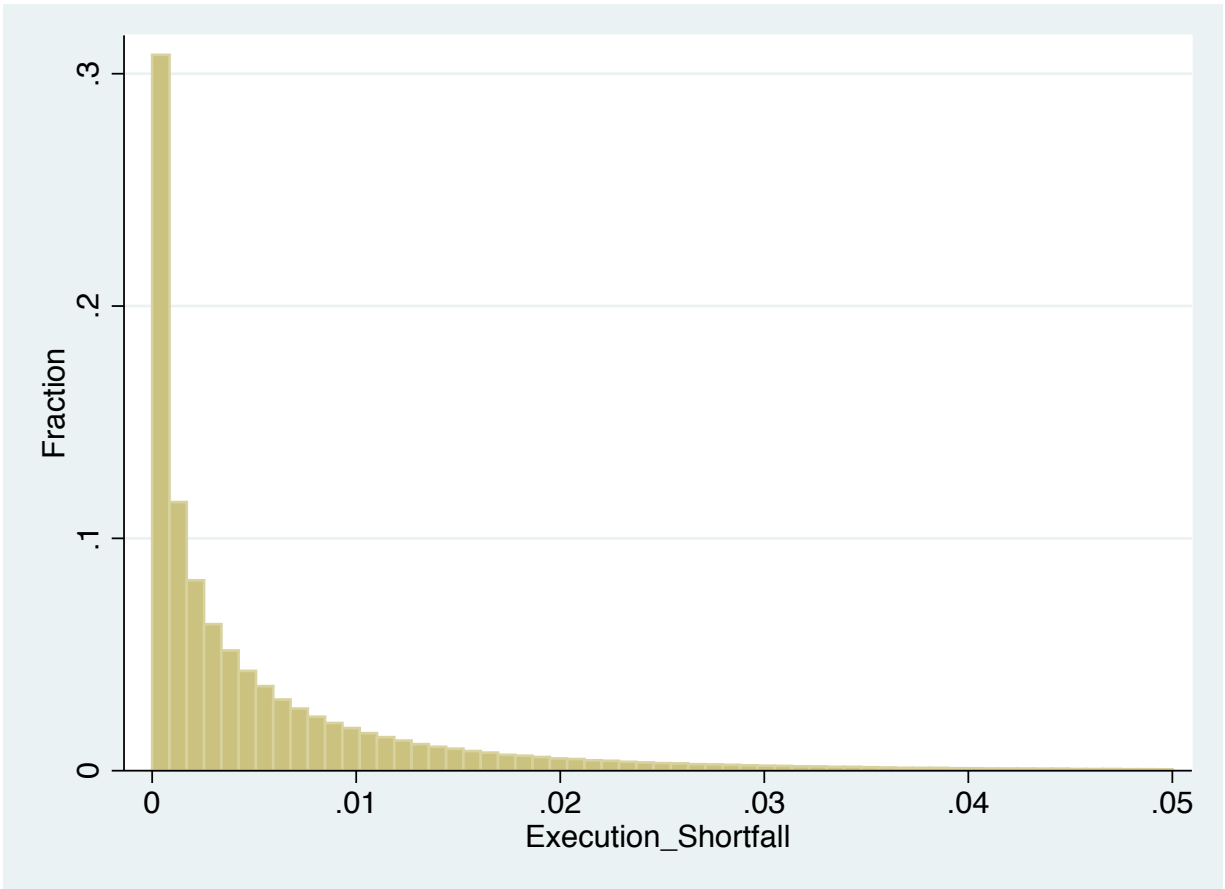
	Execution Shortfall + Commissions				
	(1)	(2)	(3)	(4)	(5)
CT Dummy	0.0013*** (3.38)	0.0011*** (2.84)	0.0011*** (2.84)	0.0012*** (3.05)	0.0012*** (2.93)
Volume		0.0005** (2.11)	0.0005** (2.11)	0.0005** (2.06)	0.0004** (1.98)
Illiquidity			0.1535*** (3.25)	0.0663* (1.94)	0.0710** (2.06)
Bid-Ask Spread				-0.0426 (-0.37)	-0.0434 (-0.40)
1/Price				0.0240* (1.89)	0.0221* (1.78)
Market Equity Decile					-0.0003 (-1.19)
S&P500 Dummy					0.0000 (0.07)
Volatility					0.0337*** (3.77)
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	964,972	964,972	964,972	964,972	964,972
R-squared	0.002	0.002	0.002	0.003	0.003

## VII. Online Appendix

The tables in the following appendix are for online publication only.



**Figure 3: Matching procedure.** This figure illustrates the matching procedure (conducted via the *key identifiers*) that we adopted to link asset manager identifiers/names to the financial transaction data provided by ANcerno.



**Figure 4: Execution Shortfall distribution.** This figure represents the distribution of execution shortfalls for all the trades in our sample. The execution shortfall is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}}$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ .

**Table AI: Sample**

This table provides summary statistics for the CRSP US Mutual Fund Database - Thomson Reuters match and the CRSP US Mutual Fund Database - Thomson Reuters - ANcerno match. The first sample includes 213,275 observations, the second sample includes 106,999 observations. *Fund Size* and *Family Size* are measured in millions of dollars, *Siblings* is the number of equity funds per family; *Flow* is the monthly flow defined as  $\frac{AUM(t) - AUM(t-1) * (1+ret)}{AUM(t-1)}$ , *4-factor Alpha* is the monthly fund return adjusted for exposure to risk factors (Fama and French plus Momentum); *Fees* are annual fund fees defined as  $Expense\ Ratio + 1/7(FrontLoad + RearLoad)$ ; *Return Dispersion* is within family cross-sectional standard deviation of risk-adjusted returns.

	CRSP-TR Sample				CRSP-TR-Ancerno Sample			
	Mean (1)	25th percentile (2)	Median (3)	75th percentile (4)	Mean (5)	25th percentile (6)	Median (7)	75th percentile (8)
Fund Size	1,258	38	150	641	1,679	44	210	919
Family Size	39,531	670	3,463	15,708	72,028	1951	9,099	44,796
Siblings	16.99	4	9	19	26.27	6	16	29
Flow	0.0009	-0.0173	-0.0054	0.0084	0.0028	-0.0169	-0.0047	0.0102
Raw Return	0.0037	-0.0237	0.0065	0.0340	0.0042	-0.0234	0.0070	0.0347
4-factor Alpha	0.0000	-0.0101	-0.0005	0.0093	0.0003	-0.0100	-0.0003	0.0097
Fees	0.014	0.0098	0.0135	0.018	0.0135	0.0093	0.0128	0.0177
Return Dispersion	0.0252	0.0128	0.0208	0.0324	0.0261	0.0146	0.0221	0.0329

**Table AII: Do Cross-trades Exhibit Higher Execution Shortfall? (Alternative Samples)**

This table reports OLS estimates obtained by regressing *Execution Shortfall* on *CT Dummy* and controls. Sample 2 and 3 include observations obtained by drawing without replacement two random samples of trades from ANcerno alternative to the sample used in the paper. *Execution Shortfall* is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{P_{j,i,t} - P_{i,t}}{P_{i,t}}$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ . *CT Dummy* equals one if a trade is a cross-trade and equals zero when a trade is executed in the open market. *Volume* is the share volume of the trade; *Illiquidity* is Amihud's monthly illiquidity ratio computed from CRSP; *Bid - Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP; *1/Price* is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the S&P500 index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. Observations are at the trade level and are obtained by drawing a 1% random sample of trades from ANcerno without replacement. Stock, time, and family fixed effects are included and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Random Sample 2					Random Sample 3				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CT Dummy	0.0021*** (5.53)	0.0020*** (5.42)	0.0020*** (5.42)	0.0020*** (5.43)	0.0020*** (5.36)	0.0020*** (5.53)	0.0019*** (5.43)	0.0019*** (5.43)	0.0019*** (5.47)	0.0019*** (5.43)
Volume		0.0002*** (12.38)	0.0002*** (12.38)	0.0002*** (11.94)	0.0002*** (11.55)	0.0002*** (12.23)	0.0002*** (12.23)	0.0002*** (12.23)	0.0002*** (11.84)	0.0002*** (11.45)
Illiquidity			0.0275*** (3.26)	0.0106 (0.87)	0.0132 (1.16)			0.0508*** (3.59)	0.0110 (0.87)	0.0172 (1.45)
Bid-Ask Spread				-0.0186 (-1.46)	-0.0150 (-1.43)				-0.0031 (-0.24)	-0.0003 (-0.02)
1/Price				0.0052*** (8.28)	0.0037*** (6.40)				0.0047*** (5.57)	0.0035*** (4.85)
Market Equity Decile					-0.0001** (-2.58)					-0.0001 (-1.64)
S&P500 Dummy					-0.0004*** (-3.38)					-0.0002* (-1.91)
Volatility					0.0192*** (17.41)					0.0189*** (17.23)
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	964,984	964,984	964,984	964,984	964,984	965,308	965,308	965,308	965,308	965,308
R-squared	0.211	0.212	0.212	0.215	0.223	0.211	0.212	0.212	0.214	0.223

**Table AIII: Are Cross-Trades Backdated? Linear Probability Model Estimation**

This table reports linear probability model estimates of the probability of a trade to be executed either at the highest or at the lowest price of the day conditional on being a cross-trade (marginal probabilities are reported). *CT Dummy* equals one if a trade is a cross-trade and equals zero when the trade is executed in the open market. *Post Regulation* equals one for trades executed from October 2004 onwards and equals zero before of that; *Volume* is the share volume of the trade; *Illiquidity* is Amihud's monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid – Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP; *1/Price* is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the *S&P500* index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. Observations are at the trade level and are obtained by drawing a 1% random sample of trades from ANcerno without replacement. Stock, time, and family fixed effects are included when specified and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	High/Low of the day				
	(1)	(2)	(3)	(4)	(5)
CT Dummy x Post Regulation	-0.0098** (-2.07)	-0.0100** (-2.11)	-0.0117** (-2.50)	-0.0125*** (-2.71)	-0.0129*** (-2.78)
CT Dummy	0.0185*** (5.99)	0.0191*** (6.26)	0.0206*** (6.74)	0.0214*** (7.13)	0.0217*** (7.27)
Post Regulation	-0.0057*** (-6.79)				
Volume			-0.0018*** (-20.21)	-0.0019*** (-21.03)	-0.0019*** (-21.07)
Illiquidity				-0.2041 (-0.94)	-0.1728 (-0.76)
Bid-Ask Spread				1.1846*** (7.41)	1.0750*** (7.04)
1/Price				0.0179*** (4.22)	0.0147*** (4.11)
Market Equity Decile					-0.0034*** (-10.74)
S&P500 dummy					0.0029*** (3.12)
Volatility					-0.0261*** (-7.55)
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	No	Yes	Yes	Yes	Yes
Observations	965,447	965,447	965,447	965,447	965,447
R-squared	0.055	0.057	0.057	0.060	0.060

**Table AIV: Is the Performance Spread Greater for Families that Cross-Trade?**

This table presents results for regressions of *Spread of Style Adj. returns* (4-factor *Alpha*) on  $CT\% > 0$  and controls. Each observation is obtained from the pairwise combinations of inflow funds and outflow funds drawn from the same family, month, and style. The hypotheses tested are no performance shifting ( $\beta = 0$ ), cross-substitution of junk funds ( $\beta < 0$ ), winner-picking ( $\beta > 0$ ). The dependent variable is computed as the return (4-factor alpha) of inflow fund  $i$  (i.e., funds with flows in the top tercile of family  $f$  in a given month  $t$ ) minus the outflow fund  $j$ 's return (4-factor alpha), i.e., funds with flows in the bottom tercile of family  $f$  in a given month  $t$ . Funds with flows in the intermediate tercile are dropped.  $CT\% > 0$  is a dummy variable equal to 1 if at least one cross-trade within the family occurs in month  $t$  and equal to zero otherwise. The other independent variables are: *Family Size* is the natural log of total assets under management at the family level in month  $t-1$ ;  $\Delta Size$ , the difference in the natural log of the lagged funds'  $i$  and  $j$  total assets under management;  $\Delta Flows$ , the difference in funds'  $i$  and  $j$  flows;  $\Delta PastFlows$ , the difference in funds'  $i$  and  $j$  lagged flows;  $\Delta PastReturns$ , the difference in funds'  $i$  and  $j$  lagged returns; and *Returns Dispersion*, the monthly lagged cross-sectional standard deviation of returns inside the family. The constant is included in all specifications but the coefficient is not reported. The frequency of the observations is monthly. Time/Family/Fund Pair fixed effects are included when specified and errors are clustered at the time level. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Spread of Style Adj. returns				Spread of 4-factor alpha			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$CT\% > 0$	0.0049*** (3.68)	0.0048*** (3.97)	0.0052*** (3.92)	0.0055*** (4.46)	0.0023** (2.05)	0.0024** (2.29)	0.0023** (2.13)	0.0028** (2.61)
Family Size			-0.0021** (-2.10)	-0.0018* (-1.81)			-0.0002 (-0.33)	-0.0008 (-1.15)
Return Dispersion			0.0936 (1.45)	0.0738 (1.24)			0.0914** (2.02)	0.0777* (1.74)
$\Delta Size$			-0.0002 (-0.81)	-0.0026** (-2.39)			0.0000 (0.31)	-0.0014** (-2.49)
$\Delta Flows$			0.0786*** (8.58)	0.0702*** (6.75)			0.0636*** (11.58)	0.0534*** (10.13)
$\Delta PastFlows$			-0.0309*** (-3.68)	-0.0244*** (-3.18)			-0.0255*** (-5.08)	-0.0215*** (-4.39)
$\Delta PastReturns$			0.0068 (0.11)	-0.0729 (-1.24)			-0.0085 (-0.40)	-0.0577*** (-2.99)
Fund Pair Fixed Effect	No	Yes	No	Yes	No	Yes	No	Yes
Family Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	108,720	108,086	108,332	107,706	108,720	108,086	108,332	107,706
R-squared	0.129	0.259	0.144	0.275	0.067	0.189	0.087	0.207



**Table AV: When do Cross-Trades mostly Occur?**

This table reports marginal probabilities estimated using a probit model. The dependent variable equals one when a trade is executed within the fund family (i.e., it is a cross-trade) and equals zero when it is executed in the open market. *VIX* is the volatility index level at the beginning of the month, *Market Return* is the stock market return in the previous month. *VIX* is the Volatility Index, *Nber* is a dummy variable that takes value one during crises and equals zero otherwise. Macro and Financial Uncertainty are from Jurado, Ludvigson, and Ng (2015); *CS Vol* is the cross-sectional standard deviation of daily returns in the previous day, *Mkt Return* is cumulative stock market return in the previous month. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Cross-Trade			
	(1)	(2)	(3)	(4)
VIX	0.0001*** (2.74)	-0.0000 (-0.97)	-0.0001 (-1.01)	-0.0001 (-1.44)
Market Return		0.0074 (1.09)	0.0020 (0.32)	0.0020 (0.31)
Cross-Sectional Volatility		0.0768*** (3.22)	0.0702*** (3.40)	0.0354** (2.14)
Financial Uncertainty			0.0109*** (4.06)	0.0054** (2.00)
Macro Uncertainty			-0.0343*** (-7.09)	-0.0083 (-1.12)
Nber Crisis			0.0011 (0.88)	-0.0002 (-0.13)
Post Regulation				-0.0056*** (-5.66)
Observations	966,186	767,129	767,129	767,129
Pseudo R2	0.001	0.003	0.011	0.016

**Table AVI: No Fixed Effects**

This table reports OLS estimates obtained by regressing *Execution Shortfall* on *CT Dummy* and controls. *Execution Shortfall* is defined as follows:  $Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}}$ , where  $P_{j,i,t}$  is the execution price of trade  $j$ , in stock  $i$ , at execution time  $t$ ; while  $P_{i,t}$  is the price of stock  $i$  in the market at time  $t$ . *CT Dummy* equals one if a trade is a cross-trade and equals zero when a trade is executed in the open market. *Volume* is the share volume of the trade; *Illiquidity* is Amihud's monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid – Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP; *1/Price* is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the *S&P500* index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. Observations are at the trade level and are obtained by drawing a 1% random sample of trades from ANcerno without replacement. Stock, time, and family fixed effects are NOT included and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Execution Shortfall				
	(1)	(2)	(3)	(4)	(5)
CT Dummy	0.0020*** (3.91)	0.0013*** (2.96)	0.0013*** (2.96)	0.0012*** (2.71)	0.0010** (2.44)
Volume		0.0006*** (18.53)	0.0006*** (18.53)	0.0004*** (17.79)	0.0003*** (14.60)
Illiquidity			0.0012 (0.71)	-0.0048*** (-4.34)	-0.0032*** (-6.37)
Bid-Ask Spread				0.2533*** (14.06)	0.1796*** (10.65)
1/Price				0.0046*** (4.78)	0.0016*** (3.59)
Market Equity Decile					0.0000 (0.03)
S&P 500 Dummy					-0.0008*** (-4.44)
Volatility					0.0314*** (11.80)
Stock Fixed Effect	No	No	No	No	No
Family Fixed Effect	No	No	No	No	No
Time Fixed Effect	No	No	No	No	No
Observations	965,711	965,711	965,711	965,711	965,711
R-squared	0.000	0.014	0.014	0.047	0.106

**Table AVII: Does backdating survive after 2004?**

This table reports linear probability estimates of the probability of a trade to be executed either at the highest or at the lowest price of the day conditional on being a cross-trade (marginal probabilities are reported). Only observations after the regulatory change that followed the late trading scandal was implemented are included. *CT Dummy* equals one if a trade is a cross-trade and equals zero when the trade is executed in the open market. *Volume* is the share volume of the trade; *Illiquidity* is Amihud's monthly illiquidity ratio computed from daily returns obtained from CRSP; *Bid – Ask Spread* is the difference between the bid and the ask at the beginning of the month as reported from CRSP; *1/Price* is 1 over the opening price of the day; *Market Equity Decile* is the equity decile computed using NYSE breakpoints; *S&P500 Dummy* equals one if a stock is included in the S&P500 index and zero otherwise; *Stock Volatility* is the within-month standard deviation of daily stock returns. Observations are at the trade level and are obtained by drawing a 1% random sample of trades from ANcerno without replacement. Stock, time, and family fixed effects are included when specified and errors are clustered at the time level. The constant is included in all specifications but the coefficient is not reported. \*\*\*, \*\*, \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	High/Low of the day				
	(1)	(2)	(3)	(4)	(5)
CT Dummy	0.0079** (2.25)	0.0081** (2.32)	0.0080** (2.30)	0.0080** (2.31)	0.0080** (2.32)
Volume			-0.0017*** (-15.66)	-0.0017*** (-16.02)	-0.0017*** (-16.00)
Illiquidity				-0.5676*** (-6.31)	-0.5503*** (-6.07)
Bid-Ask Spread				2.3902*** (6.57)	2.2615*** (6.34)
1/Price				0.0085*** (2.79)	0.0072*** (2.74)
Market Equity Decile					-0.0024*** (-7.28)
S&P 500 Dummy					0.0023** (2.09)
Volatility					-0.0092** (-2.10)
Stock Fixed Effect	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	No	Yes	Yes	Yes	Yes
Observations	639,404	639,404	639,404	639,404	639,404
R-squared	0.050	0.050	0.051	0.052	0.052