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

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Abstract

The majority of financial trades take 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. In this paper, we employ institutional trade-level data to examine such cross-trades. We find that cross-trades used to display a spread of 46 basis points with respect to open market trades before more restrictive regulation was adopted. The introduction of tighter supervision decreased this spread by 59 basis points, bringing the execution price of cross-trades below that of open market trades. We additionally find that cross-trades presented larger deviations from benchmark prices when the exchanged stocks were illiquid and highly volatile, during high financial uncertainty times, and when the asset manager had weak governance, large internal markets, and a strong incentive for reallocating performance. Finally, we provide evidence suggesting that cross-trades are more likely than open-market trades to be executed exactly at the highest or lowest price of the day, consistent with the ex post setting of the price. Our results are consistent with theoretical models of internal capital markets in which the headquarters actively favors its "stars" at the expense of the least valuable units.

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According to recent estimates, around 40% of all U.S. stock trades takes today place outside of public exchanges, up from 16% in 2008.[1] Alternative trading practices mostly include dark pool trading, internalizers, and cross-trades. Cross-trades – trades offset internally among sibling funds affiliated to the same asset manager (or fund family) – are permitted under Rule 17a-7 of the U.S. Investment Company Act because they can in principle limit transaction costs and commissions, benefiting the final investors. Anecdotal evidence warns however that cross-trading might magnify the agency problem that arises when clients delegate the investment decisions to the asset manager. For instance, a recent Financial Times article reports a number of comments by industry participants on dubious cross-trading practices including the following: "I'm aware that it 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".[2]

The task of investigating cross-transactions presents however an empirical challenge: most 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. In this paper, we use a sample of trades executed by American asset managers from 1999 to 2010 in order to be able to identify cross-trades correctly. Specifically, in our empirical analysis we look for pairs of trades originated from the same asset manager, in the same stock, same quantity, executed exactly at the same day, time, and price, but displaying opposite trading directions. We furthermore use data on commissions to verify the quality of our identification procedure, as cross-trades are supposedly significantly cheaper than open market trades.

The exact identification of cross-trades allows us to address three so far unanswered questions. First, do cross-trades really minimize the transaction costs borne by the final investor? Second, does the pricing of the cross-trades vary with market, manager or stock characteris-

[1] "Dark markets may be more harmful than high-frequency trading" *Reuters* – April 7, 2014.
[2] "No Surprise at Backroom Dealing Charge" Financial Times - December 16, 2012.

tics? Third, how does cross-trading affect the difference in performance between "star" and "junk" funds (i.e., funds of relatively high/low importance from a family perspective)?

We conduct three sets of empirical tests to address these questions. First, we explore how cross-trades were priced. The rationale of allowing cross-trades to benefit investors suggests that the spread between the execution price and the market price of the stock at the moment of the transaction (hereafter the "*Execution Shortfall*") is low for cross-trades because transaction costs are minimized. Conversely, we find that cross-trades used to exhibit an execution shortfall 18 basis points ***higher*** than for trades executed in the open market after controlling for the size of the trade and stock, time, and family fixed effects. This extra cost involved in the cross-transaction reallocated performance between the two parties involved in the trade (e.g., one fund buys at a discount from one of its siblings.) This cost however disappeared when restrictive regulation was introduced.

Second, we explore how different stock characteristics and market conditions affect cross-trades. We find that cross-trades in illiquid and highly volatile stocks presented more significant deviations from benchmark prices. Additionally, we provide evidence suggesting that the execution price of cross-trades may sometimes have been set ex post to the highest or lowest price of the day (which we refer to in this paper as "backdating").[3] Furthermore, we investigate how the execution shortfall correlated with fund family characteristics. Our null hypothesis is that family characteristics were irrelevant in explaining how cross-trades get priced. Alternatively, if cross-trades were used to shift performance in an opportunistic way, we should find a higher execution shortfall within families for which agency problems were more relevant – namely, 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 exe-

[3]Lower regulatory scrutiny on cross-trading activity compared to open market trades could more easily 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 argument, we show that cross-trades were significantly more likely than open market trades to be executed exactly at the highest/lowest price of the day.

cution shortfall used to be significantly higher for cross-trades executed in families in which governance was weak, there was a high number of siblings and some funds were significantly more expensive than others.[4]

Finally, we explore how cross-trading activity affected the difference in performance between star and junk funds. Building on previous theoretical work on internal capital markets we can formulate two hypotheses. On the one hand, mutual fund complexes may work as conglomerates in which strong divisions end up subsidizing weak ones (Stein and Scharfstein (2000)). In this context, 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.

On the other hand, the corporate headquarters of a multi-division company has control rights that enable it to engage in "winner-picking," i.e., 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,[5] 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 to pump up the returns of their star funds. Consistent with Gaspar, Massa, and Matos (2006), we show that star funds' performance used to benefit from the extent of cross-trading activity in the fund family at the expense of junk funds.

However, both reverse causality and omitted variables may affect the validity of our results. To tackle a number of identification concerns, we use an exogenous change in fund families' internal governance to assess the impact of potential cross-trading activity on ex-

[4]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)).

[5]There is abundant evidence that outperformers attract greater inflows, e.g., Chevalier and Ellison (1997), Sirri and Tufano (1998), Agarwal, Gay, and Ling (2014).

ecution shortfall. In 2004, new regulation enforcing more effective compliance policies was introduced after an investigation uncovered widespread malpractice in industry practices. We therefore compare how the pricing of cross-trades responded to increased regulatory scrutiny with respect to open market trades (as open market trades should be unaffected by such a change in regulation), thereby providing evidence in favour of a causal interpretation of our results. Additionally, we show that cross-trading went from peaks above 6% of the total trading activity before the new regulation was introduced to below 1% afterwards, suggesting that greater supervision may significantly affect the incentive to rely on cross-trading activity. Furthermore, the execution shortfall fell below that of open market trades.

In this paper, we make two main contributions to the existing literature. First, this is to the best of our knowledge 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 the regulator has decided to allow exemptions for cross-trading in other industries as well.[6] Therefore, a study on cross-trading activity provides important policy implications, while improving our understanding of incentives at the fund family level. Our paper is the first to show that cross-trades in the past seem to have been significantly mispriced and potentially backdated. Tying cross-trade level data to fund performance, we find that cross-trading potentially boosted 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 casts doubt on the fraction of performance delivered by mutual funds that is really due to skill.[7]

Second, we show that the introduction of tighter supervision in 2004 resulted in a significant decrease in both the frequency of cross-trades and the average execution shortfall.

[6] 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.

[7] 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 historical cross-section of fund returns.

A lower deviation from benchmark prices limits, but does not necessarily exclude, potential performance redistribution. However, careful regulatory scrutiny seems to be highly effective in limiting both the extent of mispricing and the incentive to cross-trade. The focus of our analysis is on cross-trading activity. Cross-trades are however only one alternative to trading in open exchanges. For instance, large asset managers increasingly rely on "dark pools" and other opaque trading venues.[8] Overall, our findings point to potential risks posed by the increasing popularity of unsupervised and less regulated trading.

This paper proceeds as follows: Section I briefly reviews the related literature and highlights the contributions of 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.

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 claim 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.[9] In this regard, Gaspar, Massa, and Matos

[8] A growing academic literature explores the implications of alternative trading venues (e.g., Zhu (2014) and Comerton-Forde and Putniņš (2015)), yet data availability is usually a major issue.

[9] E.g., if fund A buys 1000 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 to be cross-trading for 800 by related studies, significantly overestimating the number of cross-trades.

(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".[10]

Additionally, a large literature explores incentives at the fund family level aside from the cross-trading setting. 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.[11] Evans (2010) finds that funds outperform while "incubated" but such outperformance disappears after the funds are open to investors; Chuprinin, Massa, and Schumacher (2015) find that in-house funds outperform outsourced funds by 0.85% annually consistent with the hypothesis of preferential treatment; and Nanda, Wang, and Zheng (2004) shows that fund families have a high incentive to start several new funds, increasing their chance of producing "star funds" (i.e., funds that outperform by chance).

While the evidence provided in previous studies is suggestive of opportunistic performance shifting via cross-trades, it does not necessary rule out three sets of 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 difference 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)).

Additionally, previous research cannot identify cross-trades and therefore leaves necessarily unanswered questions related to how cross-trades are priced and how their pricing relates

[10] 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.

[11] 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.

to stock, market, and family characteristics. Using trade-level data, we show that i) cross-trades might have been opportunistically priced and occasionally backdated, ii) cross-trades' pricing and frequency changed drastically when regulatory scrutiny increased, and iii) deviations from benchmark prices seemed more relevant when the assets exchanged were illiquid and the cross-trades were executed in high uncertainty times. Anand, Irvine, Puckett, and Venkataraman (2012) show that trading costs have a significant effect on performance and on the persistence of relative fund performance, in this paper we find that cross-trades present substantial differences in trading costs from open market trades.

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). Conversely, our results cast doubt on the hypotheses that star funds provide insurance to distressed funds *on average* (see, e.g., Schmidt and Goncalves-Pinto (2013)) and on the assumption that funds mostly cross-trade illiquid positions 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. Cross-trades are common also in other industries, however 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.[12] 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, we conclude that our results are unlikely to be affected by opportunistic reporting or, in any case, under-represent the real extent of mispricing.

ANcerno provides us several variables useful for our investigation: stock identifier (***cusip***), trade date, execution price, execution time,[13] 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 in a number of trades executed 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 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 information is sent to ANcerno by its different clients.[14] 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*)

[12] Examples of other empirical studies using ANcerno include Chemmanur, He, and Hu (2009), Anand, Irvine, Puckett, and Venkataraman (2012).

[13] The time variable is based on a 24-hour day and is precise to the ***minute*** (i.e., not the ***second***) level.

[14] 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.

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 ANcerno subscribers do not have access to. The full file includes 1,088 asset managers. Additional identification files "ManagerXref" and "BrokerXref" have the necessary variables to link managing companies and brokers to the trades. The same identification files allow us to match ANcerno data with the Thomson Reuters database unambiguously. In particular, we hand-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"[15] 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 as ANcerno stopped to provide any identification of the asset manager after that date. 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 but not to the fund itself. Our contact in ANcerno explicitly excluded that it is possible to identify with certainty specific funds using ANcerno 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.

[15]XYZ Capital is not actually a real asset manager included in our sample.

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 having v) the same volume of the trade but in opposite trading directions. For instance, a buy trade of 1,000 Apple stocks 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 sell trade of 1,000 Apple stocks coming from the same fund family and executed on January 2nd, 2010, at 10:05 a.m. for $101. It is in theory possible, but highly unlikely, that two funds belonging to the same fund family make exactly the same trade in opposite directions, at the same time, by chance. 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 furthermore compare commission costs of open market trades with commission costs of the trades we identify 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 in our sample is 0.0016 (the median commission is 0), while it is around 0.0245 for open market trades (see Table I, Panel A). The number reported by 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). 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 high precision.[16]

[16]Results are analogous when considering as cross-trades only transactions where no commissions have

This identification procedure overcomes the main limitation of proxies computed from quarterly or semi-annual snapshots employed by Gaspar, Massa, and Matos (2006), Schmidt and Goncalves-Pinto (2013), and Chuprinin, Massa, and Schumacher (2015). Using our approach, opposite trades recorded in the same quarter but occurring in different days/times and having different volumes are *not* considered as cross-trades. Therefore, in our trade-level analysis we compare the pricing of cross-trades (***CT Dummy*** equal to one) versus open market trades (***CT Dummy*** equal to zero). In the latter part of the paper, we provide some suggestive evidence on the impact of cross-trading on fund performance. When we run regressions at the fund level, our main explanatory variable is $\boldsymbol{CT\%_{f,t}}$: 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

ANcerno provides us with a series of benchmark prices for each trade in our sample. Rule 17a-7 of the U.S. Investment Company Act establishes that cross-transactions should occur at the "current market price" of the security. We then 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.[17] In using the price at execution as a benchmark, we rely on the information provided by ANcerno. However, 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. Assuming that misreporting is random there is no reason why

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 considered a cross-trade.

[17]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.

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 our results downward. Furthermore, to make sure this does not significantly affect our results we replicate our analysis dropping trades executed exactly at the opening or closing price of the day, finding analogous results. Anyway, our ANcerno contact assured us that this problem only affects an extremely limited number of trades, mostly reported by pension funds.

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 $Execution\ Shortfall$ 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. Hence, we define $Execution\ Shortfall$ as:

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

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. Results using alternative benchmarks are presented in Section V.[18]

[18] 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.

D. Fund-level Data

The main focus of our analysis is on trades. However, the exact identification of cross-trades allows us to provide some evidence at the fund level by linking our sample to CRSP mutual fund data via the asset manager identity. We therefore obtain measures of a mutual fund's size, its fees, and its flows. Following Gaspar, Massa, and Matos (2006) we compute fees as $1/7(frontload+rearload)+expense\ ratio$. We compute fund flows following the literature (see, e.g., Coval and Stafford (2007)):

$$Flow_{i,t} = \frac{TNA_{i,t} - (1 + ret_{i,t})TNA_{i,t-1}}{TNA_{i,t-1}}, \tag{2}$$

where $TNA_{i,t}$ are the total net assets under management and $ret_{i,t}$ is the monthly return of fund i in month t. At the family level, we compute the family size, the intra-family return dispersion, the intra-family expense ratio dispersion, and the intra-family dispersion in fees. ***Family Size*** is defined as the (log of the) sum of the individual funds' assets at the beginning of the month. We compute intra-family ***Return Dispersion*** as the standard deviation of the returns of affiliated funds in the previous month, ***Expense Ratio Dispersion*** as the standard deviation of the expense ratios, and ***Fees Gap*** as 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 natural log of the number of equity funds belonging to the same family f in month t (Pollet and Wilson (2008)). We use Thompson Reuters investment objective codes to identify the investment style for each 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 potentially harming investors (besides 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. Therefore, we manually 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***.[19]

E. Summary Statistics

Sample statistics on the matched fund sample are reported in the Appendix (see Table A.I). Columns 2 to 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 USD 1,258 million, while the average mutual fund family size is USD 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 mutual funds. In particular, our matched sample contains 45% of the mutual fund families in the CRSP-TR dataset. However, such families account for 59% of the funds. Our sample is biased toward large institutions because the smallest families are less likely to rely on ANcerno's services (this bias has been recognized also by previous studies (see, e.g, Puckett and Yan (2011)). Additionally, the funds in our final database perform slightly better than funds in the CRSP database.[20] 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)).

To limit the sample size in our empirical analysis on trade-level data, we extract three random samples consisting of 1% of the original ANcerno sample and retain only observations for which we have all control variables[21], we produce results for all 3 samples and report results from sample 2 since these are the weakest.[22] Therefore, results in the paper are likely

[19]We focus on investigations instead of final court rulings because more than 90% of the investigations end up in out-of-court settlements.

[20]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.)

[21]This procedure is not uncommon in the asset pricing literature (see, e.g., Ben-David and Hirshleifer (2012)).

[22]Results from samples 1 and 3 are anyway very similar and are reported in the Appendix (see Table A.II).

to provide a lower bound for the mispricing of cross-trades. Our sample consists of 966,186 trades out of which we classify 7,368 as cross-trades and 958,818 as open market trades. Table I, Panel A reports the number of observations (Column 1), and average values for all main variables in the full sample (Column 2), average values keeping open market trades only (Column 3), cross-trades only (Column 4), the difference between open market trades and cross-trades (Column 5), and t-statistics for the null hypothesis of equality in the means of open market trades and cross-trades (Column 6). The statistics show that cross-trades are significantly bigger than normal trades both in share and dollar volume. Additionally, in general cross-trades involve stocks that present higher bid-ask spreads, are more volatile, and are bigger: they exhibit on average higher market capitalization and are more likely to be included in the S&P500 index. The fact that most of the cross-trades occurs in large market capitalization stocks is probably due to the high overlap of large stocks in funds' portfolios.

Cross-trades are significantly cheaper than regular trades in terms of commissions (more than 90% of them display 0 commission) reassuring on the quality of our identification procedure. However, cross-trades exhibit significantly higher execution shortfall than open market trades (0.84% versus 0.64%). This may be due to the fact that cross-trades are used to strategically shift performance between counterparties or simply to the difference in stock characteristics. Multivariate analysis is employed in the next section to disentangle between these two hypotheses. 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 our shortfall measure that arise naturally from our different research design. First, we always compute the absolute value of $\mathit{Execution\ Shortfall}$ while Anand, Irvine, Puckett, and Venkataraman (2012) do not (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). Second, we use the market price at the 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 and not 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 trades characteristics have the impact of execution shortfall predicted by the theory. Consistent with previous research, we find that ***Execution Shortfall*** is positively correlated with proxies of stock illiquidity and negatively correlated with proxies of size.

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 compared to trades executed in the open market. Rule 17a-7 of the U.S. Investment Company Act allows cross-trades subject to conditions of fair valuation of assets ("independent current market price," usually 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 of 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 to the open market. Our null hypothesis is, therefore, that cross-trades exhibit a significantly smaller execution shortfall than ordinary trades, as a higher deviation from the benchmark price would suggest that one trading counterparty gets unfairly penalized. In our analysis, we compare the execution shortfall of cross-trades with the execution shortfall of open market trades controlling for trade/stock/time/family differences.

Therefore, 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 takes a value of zero when a trade is executed in the open market. The identification strategy to pin down cross-trades is extensively explained in Section II.B. The choice of the benchmark and the potential shortcomings of our approach are carefully discussed in Section II.C. We additionally include stock, time, and fund family fixed effects to absorb time invariant differences and we cluster errors at the time level to account for cross-sectional heterogeneity. Formally,

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

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, γ_τ, γ_i, and γ_f are time,[23] 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.

Table II, Column 1 shows that the $Execution\ Shortfall$ in our sample is 19 basis points ***higher*** for cross-trades compared to open market trades. This result is significant at the 1% level (t-statistic of 5.44). A potential explanation for this difference, however, 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[24] as a control variable in specification (2). Table II, Column 2 shows that a higher trading volume indeed affects $Execution\ Shortfall$, as the coefficient of our $CT\ dummy$ decreases from 19 to 18 basis points, while still being significant at the 1% level.[25]

However, time-varying stock characteristics may also have an effect on $Execution\ Shortfall$. For instance, highly volatile and illiquid stocks usually display higher execution shortfall. Therefore, we include stock level 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***),

[23] 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.

[24] 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.

[25] 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.

and the Bid-Ask spread (***Bid-Ask Spread***). Additionally, we include proxies for stock capitalization because bigger stocks display in general lower ***Execution Shortfall***. In particular, we add a dummy for the 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 of stock daily returns (***Volatility***). However, the impact on the magnitude and significance of the main coefficient of interest is marginal at best. All signs of the control variables are consistent with related research.[26] Results obtained from the full specification model including all control variables, indicate that cross-trades have a 18 basis points higher ***Execution Shortfall*** than open market trades.[27] This result is significant at the 1% level (***t***-statistic of 5.37).

Our result is economically significant. The average percentage bid-ask spread (bid-ask spread over bid) is 4 basis points in our sample. The marginal effect of cross-trades on the execution shortfall is 4.5 times higher. Conservative back-of-the-envelope calculations suggest that cross-trading might have shifted performance for $1.8 million per day in the mutual fund industry only.[28] 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 suggestive estimates of the impact of mispriced cross-trades on fund performance. Overall, this section provides evidence that adds to the results from Kacperczyk, Sialm, and Zheng (2008) and Anand, Irvine, Puckett, and Venkataraman (2012) in suggesting that unobserved actions occurring within a quarter, and therefore not captured by obligatory fund filings, might have significant implications for performance.

[26] 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.

[27] We obtain similar results using different fixed effects, see Appendix.

[28] 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.

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 cross-trade. Additionally, omitted variables may affect both $\textit{Execution Shortfall}$ 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 in their funds' prospectuses, fiduciary duties, and 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.[29]

As a consequence of the scandal, in 2004 new rules were introduced and adopted by the Securities and Exchange Commission (SEC) requiring fund families to implement more stringent compliance policies. In particular, Rule 38a-1 under the Investment Company Act of 1940 forced investment companies to adopt and implement policies and procedures reasonably designed to prevent violations of federal security laws and designate a chief compliance officer responsible for administering such policies and procedures reporting directly to the board of directors. Rule 206(4)-7 under the Investment Advisers Act of 1940 imposed equivalent requirements on each adviser registered with the Commission. We contacted a number of compliance officers at leading management companies to obtain more information about the actual implications of the new regulations: they pointed out that the supervision of

[29] 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.

cross-trading activity and the monitoring of cross-trades pricing became one of their key responsibilities 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 led to a reduction in potentially opportunistic cross-trading activity. This exogenous shock allows us, first, to improve our estimation of the impact of potential cross-trading on performance. Second, it permits us to estimate what proportion of cross-trades was executed primarily for potentially 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.[30] 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 Rule 38a-1 and Rule 206(4)-7. Therefore, we compare the effect of the introduction of new regulation on the pricing of cross-trades (treatment group) with that on open market trades (control group). Our analysis resembles a difference-in-difference 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 itself.

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

Figure 1 shows clearly that cross-trades and 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

[30]Using as the treatment date February 5, 2004 provides us with quantitatively weaker but qualitatively analogous results.

particular, the execution shortfall of cross-trades is *higher* than 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) to (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 tighter 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 are consistent with 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 if 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.

C. Stock Characteristics, Market Conditions, and Backdating

This section examines which cross-trades are more likely to present greater deviations from benchmark prices. 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[31] (***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.

Results reported in Table IV indicate that the mispricing of cross-trades is more significant for highly illiquid[32] 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. One could argue that, by construction, highly illiquid and volatile securities exhibit higher deviation from benchmark prices. Yet, including time-varying stock characteristics non-interacted in all our specifications accounts for this possibility (non-interacted stock and trade level variables are always included in Table IV even though the coefficients are not explicitly reported to save space). 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 security in the open market.

In columns (4) and (5), we exclude time fixed effects from our model in order to test

[31] 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 of several financial and economic series.

[32] 1 over Price presents the "wrong" sign due to the high correlation with the other proxies of illiquidity.

whether market-wide conditions affect how cross-trades are priced.[33] 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. 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 turns negative afterwards. This result supports the hypothesis that careful regulatory scrutiny may change dramatically how cross-trades are used.

Additionally, we test whether cross-trades were backdated. When the main purpose of cross-trades is to reallocate performance between counterparties, 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/lowest of the day). We test this 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). 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,

[33]Also non-interacted market-wide variables and time-varying stock controls are included but the coefficients are not reported to save space.

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).

Results reported in Table V suggest that some cross-trades may in fact have been backdated.[34] 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/lowest price after the new regulation was passed. Our findings are consistent with a causal interpretation of our 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 the trading environment after 2004 are driving our results (as they should also be affected).

D. The Cross-Section of Cross-Trades

This section investigates how fund family characteristics affected 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 from our sample. 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

[34] 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.

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 to test whether the cross-trades executed in some families are priced differently from the cross-trades executed in other families.

In our analysis, we focus on proxies of internal governance (***Weak Governance***)[35], size of internal markets (***Siblings***) and the incentive for performance redistribution (***Fees Gap*** and ***Expense Ratio Dispersion***). The choice of these variables is motivated by the previous literature. In particular, Dimmock and Gerken (2012) argue that institutions that infringed the law in the past are more likely to do it again and Massa (2003) suggests that fund proliferation might be used opportunistically to attract flows. Additionally, 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)).

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

[35] A dummy equal to one for families investigated by the SEC for practices potentially harming investors and zero otherwise, our approach follows Ferris and Yan (2009) and is motivated by Dimmock and Gerken (2012). In particular Dimmock and Gerken (2012) show that institutions that infringed the law in the past were more likely to do it again. We conjecture that having been engaged in suspicious practices, investigated families are on average more likely to lack the necessary control mechanisms to detect and avoid illegal cross-trading activity.

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.

Our results reported in Table VI indicate that ***Weak Governance*** families display a 27 basis points higher ***Execution Shortfall*** for cross-trades. Additionally, a standard deviation increase in the number of siblings increases the ***Execution Shortfall*** by 15 basis points, while an increase of one standard deviation in ***Fees Gap*** boosts ***Execution Shortfall*** by 10 basis points. Results for the standard deviation of the expense ratio are similar (we do not include both variables at the same time as they are highly correlated). We include stock and time fixed effects but not family fixed effects since we are interested in estimating the effect of family-level variant and invariant characteristics. Overall, our results indicate than *cross-trades* from weak governance institutions, with large internal markets, and high dispersion in fees among siblings present higher ***Execution Shortfall*** than the average *cross-trade* in our sample. In particular, the effect of fees dispersion on cross-trades pricing strongly points in the direction of performance reallocation toward star funds as the incentive for "winner-picking" strategies is stronger when some funds are significantly more valuable than others (Nanda, Wang, and Zheng (2004)), while the incentive for subsidizing underperformers is probably stronger when all funds have similar importance. Additional support for the "winner-picking" hypothesis is provided in the next section.

Running our regressions only on the post regulation part of the sample (Table VI, Column (8)), we find that asset managers with weak governance, numerous affiliated funds and large 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 shortall of cross-trades is significantly lower). Interestingly, in unreported regressions we find that the effect of $\textit{Fees Gap}$ on $\textit{Execution Shortfall}$ 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 used 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 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, non-opportunistic cross-trading could decrease trading costs and, hence, improve funds' performance even in the case of non-opportunistic cross-trading. 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[36] flows (see, e.g., Bhattacharya, Lee, and Pool (2013)). The reason for ranking funds according to their flows is intuitive.[37] 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 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.[38]

[36] We focus on monthly observations because we cannot compute *flows* at the daily level.

[37] Ranking funds on net fees gives however qualitatively similar results.

[38] An alternative approach would be to sort funds on gross fees (i.e., asset under management x 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,

Having ranked the funds, we then sort them into terciles for each family.[39] 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 the invariant difference in performance between the two funds, family characteristics and observable differences between the two funds. Formally:

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

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.,

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 disproportionally 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)).

[39] Using quintiles yields similar results.

the difference in funds' size), γ_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.[40] 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.

The average $spread = r^{Star}_{i,t} - r^{Junk}_{j,t}$ in our sample is positive (0.84% monthly risk-adjusted return) since on average funds with higher flows (fees) outperform funds with lower flows (fees). However, under the null hypothesis of no strategic interaction, we should expect to find a correlation non-statistically different from zero 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[41] (Columns 1-4) and for the spread in four-factor alphas (Columns 5-8). All of our regressions include time fixed effects and either fund pair or family fixed effects. Errors are clustered at the time level.[42] 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 β should however provide information on the direction of performance

[40]Skill might however be time-varying (see, e.g., Kacperczyk, Nieuwerburgh, and Veldkamp (2014)). This is not a concern under the assumption that cross-trading activity is unrelated to skill.

[41]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.

[42]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.

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.[43] Overall, this empirical finding is consistent with the winner-picking hypothesis and inconsistent with the cross-subsidization hypothesis.[44] 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. 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 ($\Delta PastReturns$), their previous month flow difference ($\Delta PastFlow$), and the difference in *contemporaneous* flows ($\Delta Flow$).[45] 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.

Our estimates suggest that one standard deviation increase in monthly cross-trading activity increased 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 back-of-the-envelope calculations suggest that star

[43] 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).

[44] A natural question that arises is why the manager that get penalized from the cross-trade should engage in it. 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.

[45] 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 endogeneity concern we therefore control for contemporaneous flow and we exploit the change in the regulatory environment described in Section IV.

funds boosted 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.[46] Additionally, Table VIII shows that the inflow funds that benefited from cross-trading activity were *only* those that charged 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 benefited from cross-trading includes only those that were most valuable from a family perspective.

Our results so far suggest that fund families used 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 would be completely spanned by the time fixed effects. Contrary to our previous specification, we should expect to find the coefficient of the interaction between ***CT%*** and ***Post*** (b) 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 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

[46] 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.

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 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 benchmark, 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:

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

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 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:

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

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 were significantly mispriced (we estimate a marginal effect of cross-trades on $Execution\ Shortfall$ of 0.18%) and likely to reallocate performance from 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 exhibit a 0.12% higher execution shortfall than open market trades *after* commissions are taken into account. Overall, our results indicate that the effect of cross-trades on performance was economically significant.

VI. Conclusion

In this paper, we exploit institutional trade-level data provided by ANcerno to shed light on cross-trading practices. In general trading in opaque and lower regulated venues has increased in recent years. To measure cross-trades, we look for pair of trades coming from funds belonging to the same fund family, 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 in a sample of trades that cover the 1999-2010 period exhibit an execution shortfall that is 0.18% higher than 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. Finally, we show that the execution shortfall of cross-trade was substantial in illiquid and highly volatile stocks, in uncertain times, 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 find that both the incentive to cross-trade and the severity of the mispricing diminished drastically when regulatory scrutiny increased. Overall, we offer support to the hypothesis that star funds benefited from cross-trading at the expense of junk funds. This strategy had relevant implications for fund ranking, fund selection, and fund manager evaluation. Our results suggest that fund alphas potentially misrepresent the real ability of fund managers to create value for their investors.

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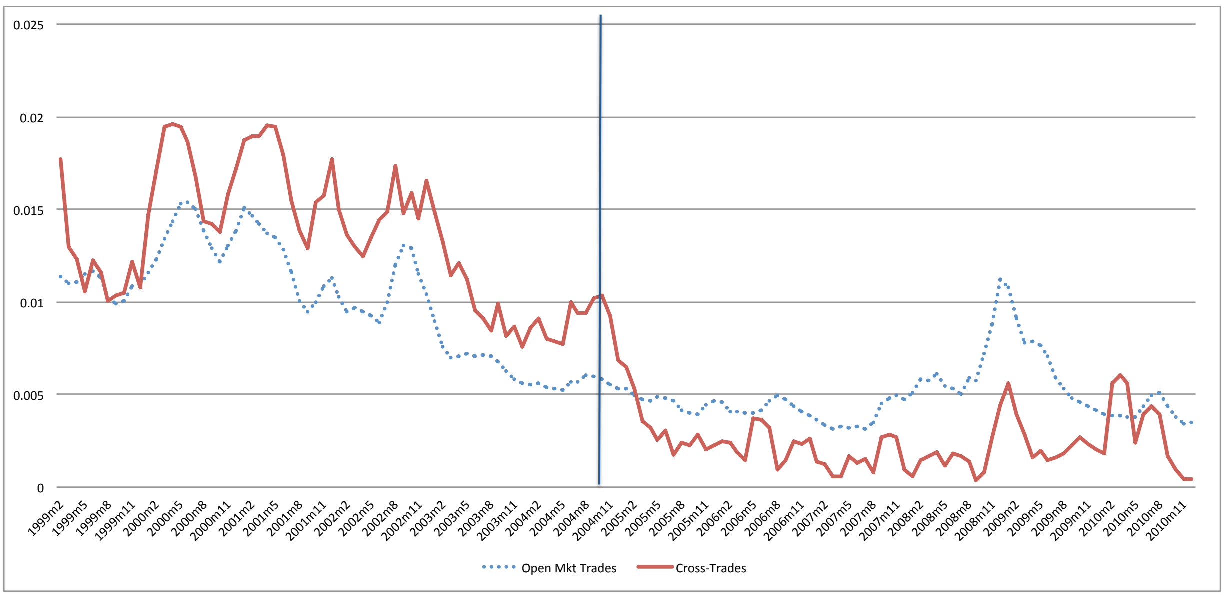

Figure 1: Execution Shortfall over time for cross-trades and open market trades. *Execution shortfall* is defined as follows: $Execution\ Shortfall_{j,i,t} = \frac{|P_{j,i,t} - P_{i,t}|}{P_{i,t}}$, where $P_{j,i,t}$ is the execution price of trade j, in stock i, at execution time t; while $P_{i,t}$ is 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. In connection with the investigation into illegal trading practices in the mutual fund industry, on September 3, 2003 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).

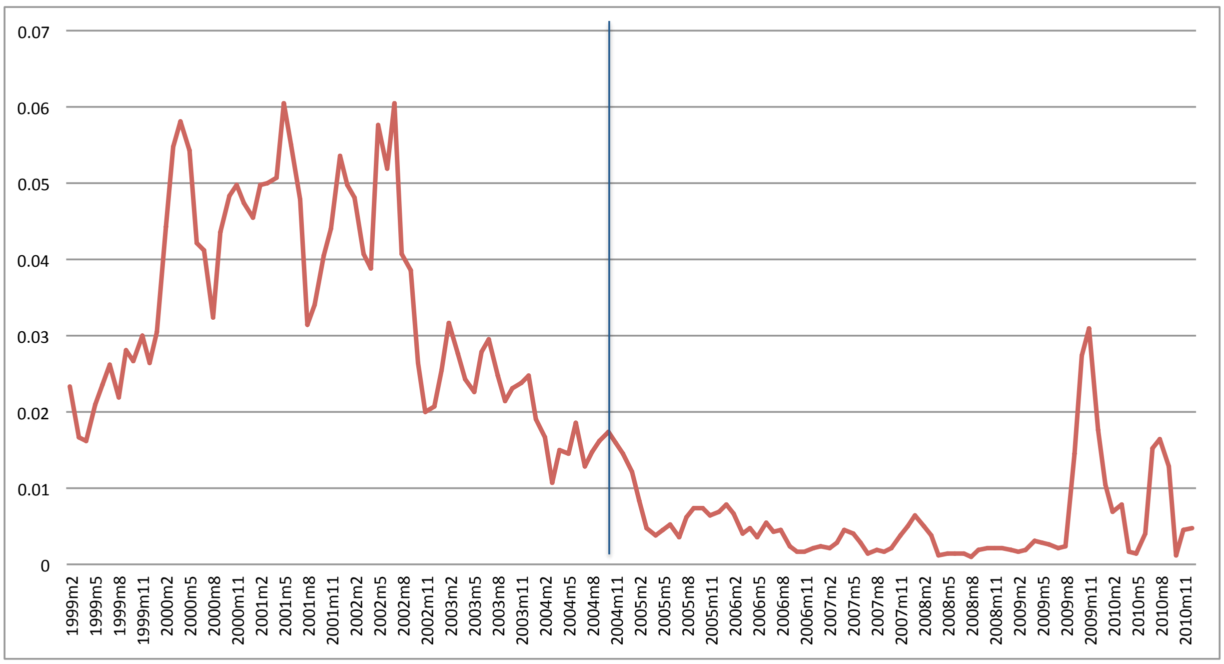

Figure 2: Percentage of cross-trading activity over time. Cross-trading activity is computed as the monthly dollar amount of cross-trading over the monthly dollar amount of total trading. The three-month moving average is plotted in order to smooth the series. Cross-trades are defined as indicated in Section II.B. In connection with the investigation into illegal trading practices in the mutual fund industry, on September 3, 2003 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 display 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 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; $Execution\ Shortfall$ 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 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; $Commissions(\$/share)$ 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.

Panel A: Sample Statistics						
	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

Panel B: Correlations						
	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. *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***	0.0018***	0.0018***	0.0019***	0.0018***
	(5.44)	(5.35)	(5.35)	(5.40)	(5.37)
Volume		0.0002***	0.0002***	0.0002***	0.0002***
		(12.00)	(12.01)	(11.57)	(11.24)
Illiquidity			0.0402***	0.0265***	0.0287***
			(3.83)	(4.63)	(4.55)
Bid-Ask Spread				-0.0057	-0.0041
				(-0.40)	(-0.37)
1/Price				0.0037***	0.0027***
				(4.04)	(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. *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&P*500 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***	-0.0061***	-0.0059***	-0.0059***	-0.0059***
	(-16.30)	(-15.74)	(-15.65)	(-15.79)	(-15.67)
CT Dummy	0.0049***	0.0048***	0.0047***	0.0047***	0.0046***
	(15.41)	(15.82)	(15.47)	(15.63)	(15.52)
Post Regulation	-0.0030***				
	(-7.16)				
Volume			0.0002***	0.0002***	0.0002***
			(11.89)	(11.44)	(11.11)
Illiquidity			0.0403***	0.0265***	0.0287***
			(3.83)	(4.62)	(4.54)
Bid-Ask Spread				-0.0051	-0.0036
				(-0.36)	(-0.33)
1/Price				0.0037***	0.0027***
				(4.03)	(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. *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&P*500 *Dummy* equals one if a stock is included in the *S&P*500 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)
Stock Characteristics					
CT Dummy x Illiquidity	0.2248***	0.1633**	0.1416**	0.1813**	0.1930**
	(3.49)	(2.38)	(2.16)	(2.19)	(2.55)
CT Dummy x Bid-Ask Spread	0.1103***	0.1122***	0.0981***	-0.0282	-0.0414
	(4.60)	(4.73)	(3.53)	(-0.83)	(-1.05)
CT Dummy x 1/Price	-0.0045	-0.0061**	-0.0110***	-0.0093***	-0.0059**
	(-1.53)	(-2.07)	(-3.53)	(-3.03)	(-1.99)
CT Dummy x Beta			0.0003	0.0002	0.0001
			(1.14)	(0.73)	(0.20)
CT Dummy x Volatility			0.0145***	0.0135***	0.0118***
			(3.43)	(3.30)	(2.80)
Market Conditions					
CT Dummy x VIX				0.0000	-0.0000
				(0.07)	(-0.83)
CT Dummy x NBER				0.0010	0.0004
				(0.93)	(0.35)
CT Dummy x Macro Uncert.				-0.0351***	-0.0318***
				(-9.21)	(-8.31)
CT Dummy x Fin. Uncert.				0.0095***	0.0071***
				(4.50)	(3.08)
CT Dummy x CS Vol.					0.0668***
					(2.83)
CT Dummy x Mkt Return					0.0003
					(0.04)
CT Dummy	0.0016***	0.0017***	-0.0003	0.0157***	0.0141***
	(4.61)	(4.81)	(-0.88)	(7.08)	(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&P*500 *Dummy* equals one if a stock is included in the *S&P*500 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.** *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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	All	All	All	All	All	All	All	Post 2004
Weak Governance	0.0052***						0.0027***	0.0019**
	(10.34)						(3.65)	(2.50)
Siblings		0.0016***					0.0015***	0.0016**
		(4.88)					(3.09)	(2.25)
Family Size			0.0001				-0.0046**	-0.0063***
			(0.07)				(-2.27)	(-2.88)
Returns Dispersion				0.0584			-0.0117	-0.0304
				(1.51)			(-0.42)	(-0.70)
Fees Gap					0.3177***		0.1119***	0.1148***
					(12.71)		(3.90)	(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; Δ*Size*, the log difference between lagged funds' i and j total assets under management; Δ*Flows*, the difference in funds' i and j flows; Δ*PastFlows*, the difference in funds' i and j lagged flows; Δ*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***	0.1431***	0.1253***	0.1536***	0.0507**	0.0619***	0.0509**	0.0678***
	(3.53)	(4.11)	(3.68)	(4.47)	(2.32)	(2.79)	(2.41)	(3.11)
Family Size			-0.0012	-0.0010			0.0001	-0.0004
			(-1.31)	(-0.99)			(0.24)	(-0.62)
Return Dispersion			0.0862	0.0633			0.0885**	0.0731*
			(1.40)	(1.11)			(2.00)	(1.67)
Δ*Size*			-0.0002	-0.0027**			0.0000	-0.0014**
			(-0.84)	(-2.49)			(0.30)	(-2.56)
Δ*Flows*			0.0788***	0.0707***			0.0636***	0.0536***
			(8.57)	(6.78)			(11.56)	(10.13)
Δ*PastFlows*			-0.0306***	-0.0238***			-0.0254***	-0.0213***
			(-3.66)	(-3.14)			(-5.07)	(-4.37)
Δ*PastReturns*			0.0071	-0.0727			-0.0084	-0.0576***
			(0.11)	(-1.24)			(-0.40)	(-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; ***ΔSize***, the difference in the natural log of the lagged funds' i and j total assets under management; ***ΔFlows***, the difference in funds' i and j flows; ***ΔPastFlows***, the difference in funds' i and j lagged flows; ***ΔPastReturns***, 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% x High-Fees	0.1769***	0.1005***	0.1460***	0.0871**	0.1247***	0.0492*	0.1042***	0.0396
	(4.66)	(2.72)	(4.10)	(2.44)	(5.46)	(1.97)	(4.60)	(1.60)
CT%	0.0205	0.0829**	0.0362	0.1000**	-0.0232	0.0316	-0.0129	0.0424*
	(0.45)	(2.03)	(0.81)	(2.48)	(-0.94)	(1.37)	(-0.53)	(1.80)
High-Fees	0.0029***	0.0012	0.0022**	0.0022	0.0021***	0.0012	0.0018***	0.0019
	(3.64)	(0.66)	(2.41)	(1.28)	(3.39)	(0.85)	(3.26)	(1.30)
Family Size			-0.0014	-0.0009			0.0000	-0.0004
			(-1.48)	(-0.96)			(0.02)	(-0.59)
Return Dispersion			0.0889	0.0637			0.0907**	0.0736*
			(1.45)	(1.11)			(2.06)	(1.68)
ΔSize			0.0001	-0.0027**			0.0003**	-0.0015**
			(0.25)	(-2.51)			(2.22)	(-2.57)
ΔFlows			0.0736***	0.0708***			0.0597***	0.0538***
			(7.94)	(6.77)			(11.05)	(10.07)
ΔPastFlows			-0.0297***	-0.0235***			-0.0247***	-0.0210***
			(-3.55)	(-3.09)			(-4.97)	(-4.29)
ΔPastReturns			0.0036	-0.0728			-0.0111	-0.0577***
			(0.06)	(-1.24)			(-0.52)	(-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%xPost*. 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; Δ*Size*, the difference in the natural log of the lagged funds' i and j total assets under management; Δ*Flows*, the difference in funds' i and j flows; Δ*PastFlows*, the difference in funds' i and j lagged flows; Δ*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% x Post	-0.2288***	-0.1563***	-0.1493***	-0.1713***	-0.1368***	-0.0912**	-0.0899***	-0.0996***
	(-3.00)	(-2.99)	(-3.11)	(-3.61)	(-3.10)	(-2.29)	(-2.88)	(-2.81)
CT%	0.1846***	0.1545***	0.1330***	0.1662***	0.0804***	0.0686***	0.0556**	0.0752***
	(3.04)	(4.21)	(3.74)	(4.60)	(2.80)	(2.95)	(2.54)	(3.31)
Post	-0.0025				-0.0019			
	(-1.25)				(-1.35)			
Family Size			-0.0012	-0.0010			0.0002	-0.0004
			(-1.30)	(-1.01)			(0.25)	(-0.64)
Return Dispersion			0.0821	0.0587			0.0860*	0.0705
			(1.34)	(1.04)			(1.96)	(1.62)
Δ*Size*			-0.0003	-0.0027**			0.0000	-0.0015**
			(-0.85)	(-2.51)			(0.28)	(-2.58)
Δ*Flows*			0.0790***	0.0709***			0.0637***	0.0537***
			(8.57)	(6.79)			(11.57)	(10.13)
Δ*PastFlows*			-0.0306***	-0.0238***			-0.0254***	-0.0212***
			(-3.66)	(-3.14)			(-5.07)	(-4.37)
Δ*PastReturns*			0.0071	-0.0727			-0.0084	-0.0576***
			(0.11)	(-1.24)			(-0.40)	(-3.00)
Fund Pair Fixed Effect	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Family Fixed Effect	No	No	Yes	No	No	No	Yes	No
Time Fixed Effect	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	108,086	108,086	108,332	107,706	108,086	108,086	108,332	107,706
R-squared	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***	0.0021***	0.0021***	0.0021***	0.0021***
	(11.35)	(11.34)	(11.34)	(11.46)	(11.53)
Volume		-0.0002***	-0.0002***	-0.0002***	-0.0002***
		(-16.95)	(-16.94)	(-17.64)	(-17.98)
Illiquidity			0.0110*	-0.0025	0.0012
			(1.84)	(-0.46)	(0.28)
Bid-Ask Spread				-0.0062	-0.0102
				(-0.60)	(-1.59)
1/Price				0.0037***	0.0025***
				(4.22)	(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***	0.0016***	0.0016***	0.0016***	0.0016***
	(4.97)	(4.72)	(4.72)	(4.94)	(4.96)
Volume		0.0003***	0.0003***	0.0003***	0.0002***
		(13.56)	(13.55)	(12.20)	(10.00)
Illiquidity			0.0161	-0.0386**	-0.0247
			(0.51)	(-2.54)	(-1.65)
Bid-Ask Spread				-0.0117	-0.0344
				(-0.30)	(-1.32)
1/Price				0.0163***	0.0121***
				(4.99)	(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***	0.0011***	0.0011***	0.0012***	0.0012***
	(3.38)	(2.84)	(2.84)	(3.05)	(2.93)
Volume		0.0005**	0.0005**	0.0005**	0.0004**
		(2.11)	(2.11)	(2.06)	(1.98)
Illiquidity			0.1535***	0.0663*	0.0710**
			(3.25)	(1.94)	(2.06)
Bid-Ask Spread				-0.0426	-0.0434
				(-0.37)	(-0.40)
1/Price				0.0240*	0.0221*
				(1.89)	(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

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