THE MARKET PENALTY FOR MUTUAL FUND SCANDALS

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INTRODUCTION

Mutual funds have experienced enormous growth over the last forty years. In 1965, open-end mutual funds held assets of $35 billion.[1] By 2004, mutual fund assets had grown to $8.1 trillion, an increase of over 20,000 percent.[2] During this period, the percentage of all corporate stock held by mutual funds increased from 4%[3] to 21%.[4] Mutual funds as a group now constitute the largest institutional holder of corporate stock, far exceeding private pension funds (13%), public pension funds (10%), and insurance companies (7%).[5]

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[1] INVESTMENT COMPANY INSTITUTE, 2005 INVESTMENT COMPANY FACTBOOK 59 tbl.1 (45th ed. 2005). For the remainder of the article, we will use the term "mutual fund" to refer to open-end funds.

[2] *Id.*

[3] BD. OF GOVERNORS OF THE FED. RESERVE SYS., FLOW OF FUNDS ACCOUNTS OF THE UNITED STATES 1965-1974, at 82 tbl.L.213 (2006), *available at* http://www.federalreserve.gov/releases/z1/20061207/annuals/a1965-1974.pdf.

[4] BD. OF GOVERNORS OF THE FED. RESERVE SYS., FLOW OF FUNDS ACCOUNTS OF THE UNITED STATES 1995-2005, at 82 tbl.L.213 (2006), *available at* http://www.federalreserve.gov/releases/z1/20061207/annuals/a1995-2005.pdf.

[5] *Id.*

Mutual funds are typically organized as corporations or business trusts that are owned by mutual fund investors who elect the directors or trustees of the fund. The actual management of the fund, however, is performed by a fund management company rather than the elected board. The management company, in turn, is paid a management fee by the mutual fund.[6]

For example, the Janus Fund (assets of $11.9 billion)[7] is managed by Janus Capital Management L.L.C., which receives an annual advisory fee of 0.64%,[8] and an administrative fee of 0.05% of the fund's assets.[9] Janus Capital Management, in turn, is a subsidiary of Janus Capital Group, a large mutual fund management company (and itself a publicly-traded corporation).[10]

Mutual funds, in many respects, resemble ordinary publicly-traded corporations. They have a large number of dispersed shareholders/investors, and neither the fund management company nor the individual fund managers have a significant equity stake in the fund. This gives rise to the classic agency problem, where fund management companies and managers may pursue their own interest at the expense of fund investors.[11] One device to restrain agency costs – the election system for directors, which engenders the possibility of proxy contests[12] and hostile takeovers[13] – is even less effective for open-end mutual funds than it is in the context of public corporations.[14]

In one important respect, however, mutual funds differ from ordinary public corporations: holders of open-end mutual funds have the right to redeem their shares for their net asset value.[15] This right potentially gives fund investors an

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[6] *See, e.g.*, JANUS EQUITY FUNDS, PROSPECTUS 78-79 (2006) (describing the management structure of the fund).

[7] Janus.com, Fund Comparison, https://ww4.janus.com/Janus/Retail/FundHolding?fundID=1 (click on "Compare Funds" tab) (last visited Sept. 20, 2007).

[8] JANUS EQUITY FUNDS, *supra* note 6, at 8.

[9] *Id.* at 83.

[10] *See* Google Finance, Janus Capital Group Inc., http://finance.google.com/finance?q=JNS (last visited Sept. 9, 2007). In our Article, we will use the term "mutual fund" to refer to open-end fund entities corresponding to the Janus Fund, "fund management company" to refer to entities corresponding to Janus Capital Management and Janus Capital Group, and "fund manager" to refer to persons like David Corkins.

[11] *See* Michael C. Jensen & William H. Meckling, *Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure*, 3 J. FIN. ECON. 305, 312-13 (1976) (discussing the divergence between the interests of a firm's shareholders and the firm's managers).

[12] *See generally* Lucian Arye Bebchuk & Marcel Kahan, *A Framework for Analyzing Legal Policy Towards Proxy Contests*, 78 CAL. L. REV. 1071 (1990).

[13] *See generally* Ronald J. Gilson, *A Structural Approach to Corporations: The Case Against Defensive Tactics in Tender Offers*, 33 STAN. L. REV. 819, 843-48 (1981).

[14] *See, e.g.*, Lucian A. Bebchuk, *The Myth of the Shareholder Franchise*, 93 VA. L. REV. 675, 682-94 (2007).

[15] *See* Investment Company Act of 1940 § 5(a)(1), 15 U.S.C. § 80a-5(a)(1) (2000) (defining an "open-end company" as a management company with redeemable securities).

effective method for penalizing fund management for managerial wrong-doing and for protecting themselves against continued losses. Since fund management fees are a percentage of fund assets, any redemptions directly reduce the revenues of the fund management company.[16] Further, since shares can be redeemed for their net asset value, redemptions insulate investors from future losses.[17]

But investors, for a variety of reasons, may fail to redeem shares in response to managerial wrong-doing. They may be unaware of any managerial wrong-doing that has occurred because such wrong-doing has not yet been uncovered or because they do not follow the financial press. Further, investors may not be concerned about the wrong-doing. They may want to avoid the hassle of redeeming shares and reinvesting the proceeds elsewhere.[18] They may (if the funds are not held in a tax-exempt account)[19] face adverse tax consequences if they redeem shares in mutual funds.[20] Investors may also have to pay a fee when shares are redeemed.[21]

Whether and to what extent investors make redemptions, and whether they do so in response to all (or some) types of managerial wrong-doing, is thus an empirical question. From the perspective of deterrence, it is not necessary that all investors redeem their shares when wrong-doing has occurred. Rather, any significant level of withdrawals reduces the profits of the fund management company and thus provides deterrence against wrong-doing.

The actual response of mutual fund investors to managerial wrong-doing has important implications for the corporate governance of mutual funds, for regulatory policy, and for enforcement policy. If mutual fund investors

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[16] *See* Marcel Kahan & Edward B. Rock, *Hedge Funds in Corporate Governance and Corporate Control*, 155 U. PA. L. REV. 1021, 1051-52 (2007) (arguing that regulatory barriers cause most mutual funds to charge fees based on assets under management).

[17] 17 C.F.R. § 270.22c-1(a) (2007) (requiring redemption price to be based on the current net asset value). Losses that have already materialized, however, will have reduced the fund's net asset value and cannot be recouped by redeeming shares. By contrast, shareholders in regular corporations cannot insulate themselves from expected future losses by selling their shares. As long as the market anticipates future losses, the market price will reflect these losses, and shareholders who sell will receive a lower price both as a result of past losses and as a result of expected future losses.

[18] Redeeming shares from one mutual fund, deciding where to invest the proceeds, and investing these proceeds all involve transaction costs that may induce investors not to redeem such shares.

[19] For example, capital gains from the redemption of mutual funds held in a qualified pension plan or an individual retirement account are not taxable. *See* I.R.C. §§ 401(a), 408, 408A (2000) (dealing with qualified pension plans, IRA's, and Roth IRA's).

[20] *See* I.R.C. § 1001 (2000) (discussing taxation of capital gains).

[21] *See* Financial Industry Regulatory Authority, Understanding Mutual Fund Classes, http://www.nasd.com/InvestorInformation/InvestorAlerts/MutualFunds/UnderstandingMutualFundClasses/index.htm (last visited Sept. 25, 2007) ("Often Class C shares impose a small charge if you sell your shares within a short time of purchase, usually one year.").

respond to wrong-doing with substantial redemptions, and thereby both penalize fund management companies and protect the investors against future harm, alternative mechanisms to reduce agency costs – such as the voting mechanism or the market for corporate control[22] – are less necessary in the mutual fund context. This further suggests that one should explore, as some commentators have proposed,[23] the feasibility of similar redemption mechanisms in the context of regular publicly-traded companies.

Moreover, if redemption rights are an effective tool for penalizing wrong-doing, fund management companies have incentives to adopt proper governance mechanisms to reduce the *ex ante* likelihood of wrong-doing. In that case, the Securities and Exchange Commission (SEC) should exercise restraint before it mandates – as it has recently attempted to do – governance structures for mutual funds.[24]

Finally, if investors use redemptions to penalize fund management companies for only certain types of wrong-doing, the SEC should take account of these penalties in devising its enforcement policies. Specifically, the SEC may want to redirect its scarce enforcement resources from the prosecution of fund management companies already penalized by investors to the detection and prosecution of the types of wrong-doing that are not adequately penalized by investors. In addition, where the SEC does prosecute wrong-doing that elicits an investor response, the SEC should take account of the penalties exacted through redemptions in setting its own regulatory fines.

This Article provides an empirical analysis of the effect of mutual fund scandals – instances where fund management was subject to investigation by a state or federal regulatory agency – on flows in and out of open-end mutual funds involved in the scandal.[25] We find, in our baseline estimates, that

[22] *See supra* text accompanying notes 12-13 (discussing the election system of directors as a device to restrain agency costs).

[23] *See* Lucian Ayre Bebchuk, *The Case for Increasing Shareholder Power*, 118 HARV. L. REV. 833, 901-03 (2005) (discussing giving shareholders the power to make scaling-down decisions, such as forcing dividend payments); Zohar Goshen, *Shareholder Dividend Options*, 104 YALE L.J. 881, 884 (1995) (proposing to give shareholders the option to receive distribution of corporate profits in cash).

[24] *See* Investment Company Governance, 69 Fed. Reg. 46,378, 46,381 (Aug. 2, 2004) (adopting a rule to mandate that investment funds must have a board composed of at least 75% independent directors and an independent chairman), *invalidated by* Chamber of Commerce v. SEC, 443 F.3d 890, 909 (D.C. Cir. 2006); Kara Scannell & Tom Lauricella, *Cox's 'Independent' Day*, WALL ST. J., Feb. 9, 2007 at C1 (discussing rationale and history of rule and prospects for the adoption of a revised rule on mutual fund governance).

[25] This is the first article to analyze systematically the effect of scandals on mutual fund flows. Individual instances of funds experiencing outflows have been reported in the financial press in the context of market timing scandals. *See, e.g.*, Brett Arends, *On State Street*, BOSTON HERALD, Dec. 28, 2004, at 23 (reporting significant outflows from Putnam, Janus, and other funds in wake of market timing scandals). Additionally, Professors Todd Houge and Jay Wellman have reported that funds in the same family as late trading and

scandal funds on average suffer abnormal outflows of 19% of their pre-scandal assets in the year following a scandal.[26] Funds that are not themselves involved in the scandal, but are run by the same management company as a scandal fund ("scandal-family funds"), on average lose 8% of their assets.[27] These results show that the ability of investors to redeem their shares is important both as a deterrent device and as a tool for self-protection.

In subsequent refinements, we find that withdrawals are greater for more severe scandals,[28] and withdrawals from scandal-family funds are greater when the scandal concerns a relatively prominent fund within the fund family.[29] This result is consistent with the view that investors rationally calibrate their response to the degree of wrong-doing.

Perhaps most importantly, we find significant withdrawals only when a scandal portends that continued wrong-doing will likely result in future harm to the fund investors.[30] For scandals where the risk of future harm to the fund investors is low, however, we find no statistically or economically significant withdrawals. This indicates that regulatory sanctions are needed to deter wrong-doing to the extent that the latter scandals still generate social harm.

Finally, we examine the relationship between fund outflows and who first discovers the scandals. We find that scandals first discovered by the financial press and other non-governmental bodies are associated with economically and statistically significant outflows. By contrast, scandals discovered by the SEC generate no significant outflows, regardless of the type of scandal.[31] This result is consistent with two different explanations. The first explanation is that the SEC focuses its resources on detecting scandals that generate no investor response because the wrong-doing is insignificant. In other words, the SEC focuses on the wrong sort of scandals. The second explanation is that the SEC focuses its resources on scandals that do not portend future harm to the fund investors, but nevertheless cause social harm. In other words, the SEC focuses on the right sort of scandals – ones that ought to be deterred, but that are not adequately penalized through fund outflows. Follow-up tests suggest that the SEC focuses its resources on detecting scandals where the wrong-doing is insignificant, i.e. – the wrong sorts of scandals.

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market timing funds suffer outflows following the announcement of an investigation. Todd Houge & Jay Wellman, *Fallout from the Mutual Fund Trading Scandals*, 62 J. BUS. ETHICS 129, 133-34 (2005). Their study, however, does not control for *other* factors affecting fund flows; examine the effect of other *types* of scandals; or analyze the effect of particular scandal *features* – such as indicators of scandal severity, prominence, the likelihood of future harm and how the scandal was discovered – on fund flows as ours does. *See id.* at 130.

[26] *See infra* Part II.A.

[27] *See infra* Part III.

[28] *See infra* Part II.B.

[29] *See infra* Part III.

[30] *See infra* Part II.C.

[31] *See infra* Part II.D.

This Article is organized as follows: Part I describes the data used for our empirical analysis and the structure of the tests we performed. Part II reports and interprets the results of the empirical tests for scandal funds. Part III reports the results of funds that are not themselves involved in scandals but that were run by the same management company as a scandal fund. Finally, we conclude with a summary of both our findings and their potential implications.

I. DATA AND METHODOLOGY

To identify mutual fund scandals, we conducted searches in the *Wall Street Journal* database on Westlaw. We collected every instance between 1994 and 2004 where a fund was subject to investigation or sanction by a state or federal regulatory agency. We considered each such instance to constitute a "scandal." In total, we identified 135 funds involved in scandals.

For each scandal, we collected extensive information on the funds involved and the features of the scandal. This information included the date of the first article on the scandal, the factual allegations giving rise to the scandal, who first discovered the scandal, the number of subsequent *Wall Street Journal* articles discussing the scandal, the ultimate regulatory sanction imposed (if any), and the identity of the person sanctioned.

To illustrate, we provide examples of some scandals that are part of our study:

1. *Fidelity Magellan Fund*. In December 1995, the *Wall Street Journal* reported an SEC investigation into Fidelity's Magellan Fund and its fund manager Jeffrey Vinik.[32] The investigation was prompted by a *U.S. News & World Report* article stating that Vinik had made bullish remarks on Micron Technology at a time when the Magellan fund was heavily selling Micron Technology stock.[33] A few days later, the *Wall Street Journal* reported that Vinik and Harry Lange, another Fidelity manager, had touted two other stocks that Fidelity was selling.[34] In both articles, Fidelity was quoted as denying any wrong-doing.[35] In May 1996, the *Wall Street Journal* reported that the SEC staff was unlikely to recommend enforcement action in these matters.[36]

2. *Dreyfus Aggressive Growth Fund*. In May 2000, the *Wall Street Journal* reported that Dreyfus Corp., the company managing the Dreyfus Aggressive

[32] *See* Robert McGough & Jeffrey Taylor, *SEC Boosts Its Scrutiny of Magellan Fund*, WALL ST. J., Dec. 11, 1995, at C1. The article does not specify whether the SEC had commenced a formal investigation, or whether its review was of a less formal type, such as an examination. *See id.* Subsequent *Wall Street Journal* articles have, however, referred to the SEC conducting an examination. *See* Jeffrey Taylor & Charles Gasparino, *Why SEC Lambasted News Report on Fidelity*, WALL ST. J., Apr. 22, 1996, at C1.

[33] *See* McGough & Taylor, *supra* note 32.

[34] Robert McGough, *Fidelity Fund Managers Spoke Highly of 2 Other Stocks That Were Unloaded*, WALL ST. J., Dec. 15, 1995, at C1.

[35] *Id.*

[36] Jeffrey Taylor, *SEC Action is Unlikely on Vinik*, WALL ST. J., May 9, 1996, at C1.

Growth Fund, had agreed to pay $3 million to settle charges brought by the SEC.[37] The SEC had accused Dreyfus of misleading investors. Dreyfus had failed to disclose that former fund manager Michael Schonberg had pumped up the performance of the Aggressive Growth Fund by giving the fund a disproportionate volume of shares offered in initial public offerings ("IPOs").[38] First day gains from these "hot" IPO shares contributed 83% to the sizzling 119% return that the Aggressive Growth Fund had achieved in the eight months following its launch.[39] Schonberg, who had left Dreyfus a few days prior to the announcement, agreed to a nine-month suspension from the investment advisory business.[40]

3. *Fred Alger Management*. In October 2003, the *Wall Street Journal* reported that Fred Alger Management Inc., a mutual fund management company, had suspended three employees – including, according to an unnamed source, its Vice Chairman James Connelly – for "late trading."[41] Late trading involves permitting investors to buy or sell mutual fund shares at the 4 p.m. closing price *after* the 4 p.m. close of stock trading, which is illegal under the federal securities laws.[42] While Fred Alger Management had discovered the late trading in an internal investigation and acted ahead of any actions by regulators, the New York Attorney General's office was reported to be "looking closely" at trading activity involving the company.[43] A few days later, the *Wall Street Journal* reported that James Connelly (Alger's now-departed Vice Chairman) was being investigated for asking Fred Alger Management employees to discard e-mails related to late trading.[44] In another article, the *Wall Street Journal* reported that investment research firm Morningstar recommended that investors avoid mutual funds managed by Fred Alger Management.[45] Two months later, Connelly was reported to have been sentenced to one to three years in state prison.[46] The article noted that Fred

[37] Karen Damato, *Dreyfus Settles Charges Related to Disclosures*, WALL ST. J., May 11, 2000, at C1.

[38] *Id.* Dreyfus also agreed to pay $400,000 to the State of New York for the State Attorney General's investigation expenses and to make a $1.6 million contribution for an investor education program at the State University of New York. *Id.*

[39] *Id.* While unequal IPO distributions are not illegal, the SEC argued that the practice should have been disclosed. *Id.*

[40] *Id.*

[41] Gregory Zuckerman & Ken Brown, *Merrill Ousts 3 Fund Brokers*, WALL ST. J., Oct. 6, 2003, at C1.

[42] *See* 17 C.F.R. § 270.22c-1(a) (2007).

[43] *See* Zuckerman & Brown, *supra* note 41.

[44] Gregory Zuckerman, *Alger Executive Becomes Focus of Fund Probe*, WALL ST. J., Oct. 16, 2003, at C1.

[45] John Shipman, *Morningstar Sounds Alarm on Alger*, WALL ST. J., Oct. 21, 2003, at D13.

[46] Gregory Zuckerman, *Executives on Trial: Former Officer at Fred Alger Sentenced to 1 to 3 years for Obstructing Spitzer*, WALL ST. J., Dec. 18, 2003, at C14.

Alger Management itself had not been charged in connection with the investigation.[47]

4. *Massachusetts Financial Services Co. (MFS)*. In December 2003, the *Wall Street Journal* reported that the SEC and the New York State Attorney General's office were likely to charge MFS (a fund management company owned by Sun Life Financial Inc.) for permitting "market timing."[48] Market timing involves investors buying or selling mutual fund shares (before the 4 p.m. close of stock trading) at a net asset value that is nonetheless based on stale prices. For example, funds holding stock of foreign companies calculate their net asset value based on the last available closing price in a foreign market, which is often several hours old and will not reflect later developments known to the market timer when the timer orders a trade in the mutual fund shares.[49] Unlike late trading, market timing is not illegal. But, the regulators charged MFS with violating its own published policies regarding market timing trades, thereby misleading fund investors.[50] In January 2004, several articles appeared in the *Wall Street Journal* mentioning settlement talks.[51] In early February, MFS settled the charges brought by the SEC by paying a $225 million fine, and agreed with the New York Attorney General's office to cut its fees by $125 million over the next five years.[52] In addition, two high-level MFS executives were barred from serving as officers or directors of a fund management company for three years.[53]

To analyze the effect of scandals on mutual fund flow, we obtained data on monthly mutual fund net assets and returns for open-end funds for the period from 1994 to 2004 from the Center for Research in Security Prices (CRSP). We also collected information on each mutual fund's objective, fund family (with which the fund is affiliated), load,[54] total expenses, and year of organization. We only included mutual funds that were in the same Strategic

[47] *Id.*

[48] John Hechinger, *MFS Allowed Timing Trades, SEC Believes*, WALL ST. J., Dec. 9, 2003, at C1.

[49] *See, e.g.*, David Ward, Note, *Protecting Mutual Funds from Market-Timing Profiteers: Forward Pricing International Fund Shares*, 56 HASTINGS L.J. 585, 586 (2005). Market timing can also work for thinly-traded stock. *Id.*

[50] *See* Hechinger, *supra* note 48.

[51] *See, e.g.*, John Hechinger & Tom Lauricella, *MFS Inquiry Says Holders Lost Millions*, WALL ST. J., Jan. 23, 2004, at C1; John Hechinger & Tom Lauricella, *MFS's Wunderkind CEO Ballen May Face SEC Temporary Ban*, WALL ST. J., Jan. 29, 2004, at C1; John Hechinger & Tom Lauricella, *Sun Life Unit Reaches Pact in Fund Probe*, WALL ST. J., Jan. 27, 2004, at C1; Tom Lauricella et al., *Developments in Mutual-Fund Cases*, WALL ST. J., Jan. 15, 2004, at D7.

[52] Frederick P. Gabriel Jr. & Bruce Kelly, *Reverse Spin: MFS Not Too Proud to Settle with SEC*, INVESTMENT NEWS, Feb. 9, 2004, at 4.

[53] *Id.*

[54] The term "load" is used to refer to the fee charged to purchase mutual fund shares.

Insight Fund Objective Code[55] grouping as the scandal funds. We calculated monthly percentage flow data as follows:

$$\text{flow}_t = \frac{\text{Net Assets}_t - \text{Net Assets}_{t-1} * (1 + r_t)}{\text{Net Assets}_{t-1}}$$

This yielded 298,392 monthly flow observations. As is common in studies of fund flows, this measure does not count reinvested fund distributions as new inflows.[56]

We made a number of further adjustments to the data, each designed to eliminate data-points that introduce statistical noise into our estimates. First, we excluded the first 12 months of fund data for funds that were initiated at any time after the start of 1993. Flows for the first 12 months of a fund's existence experience rapid percentage growth that is not representative of fund flows after the first year. This is due to the low asset base at the start-up phase of a new fund. Second, to focus on funds with substantial investor assets, we also excluded funds with total net assets of less than or equal to $50 million on the 13th month of the fund's existence. In the case of a fund initiated before 1993, we excluded the fund if it possessed less than or equal to $50 million in total net assets on the first month of fund data in our data set. Finally, we excluded as outliers the highest and lowest 0.1% of the observations.[57] We were left with 297,796 fund-month flow observations spanning a total of 4072 funds.

We initially treated the 12 months following the month in which the first article on the scandal was published as the "scandal period." Due to some missing observations, the 135 scandal funds yielded 1491 scandal-month observations. Table 1 provides a yearly breakdown of the number of scandal month and non-scandal month observations.

[55] The Strategic Insight's Fund Objective Code is a three-digit code which identifies the fund's investment strategy. For instance, "AGG" denotes an "aggressive growth" strategy fund.

[56] *See, e.g.*, Daniel C. Indro, *Does Mutual Fund Flow Reflect Investor Sentiment?*, 5 J. BEHAV. FIN. 105, 107 (2004) (stating that reinvested fund distributions should be excluded from funds flows because they represent shareholder liability to the IRS and not incoming assets); Erik R. Sirri & Peter Tufano, *Costly Search and Mutual Fund Flows*, 53 J. FIN. 1589, 1594 (1998) (defining net flow as growth in excess of growth based on reinvested dividends).

[57] A review of some of the outliers suggests that they are due to data recording errors. For example, in some instances, fund assets were recorded as dropping in a given month by a large percentage and then increasing to the former level in the following month.

Table 1 – Yearly Breakdown of Scandal Month Observations

Year	Number of Non-Scandal Months	Number of Scandal Months
1994	17,603	87
1995	19,441	85
1996	20,807	32
1997	22,078	0
1998	25,251	7
1999	26,780	13
2000	28,606	26
2001	31,021	4
2002	33,763	29
2003	34,931	274
2004	36,024	934
Total	296,305	1,491

Prior finance literature on mutual fund flows has identified several factors affecting fund flows. In particular, a fund's past return has been shown to have a significant positive impact on future fund flows.[58] Moreover, several papers have found stronger fund flow responses for positive returns than for negative returns.[59] To control for past returns, we calculated, for each monthly fund

[58] *See, e.g.*, Jayendu Patel et al., *Investment Flows and Performance: Evidence from Mutual Funds, Cross-Border Investments, and New Issues*, *in* JAPAN, EUROPE, AND INTERNATIONAL FINANCIAL MARKETS 51, 61-62 (Ryuzo Satl et al. eds., 1994) ("A return 1% above the cross-sectional mean return in the previous period implies a $200,000 increased flow in this period."); Richard A. Ippolito, *Consumer Reaction to Measures of Poor Quality: Evidence from the Mutual Fund Industry*, 35 J.L. & ECON. 45, 61 (1992) ("For funds with a positive performance residual equal to 100 basis points over the past five years . . . the current growth rate increases by .90 percent."); Sirri & Tufano, *supra* note 56, at 1598 ("The results . . . confirm that equity mutual fund inflows are sensitive to historical performance . . . ."); A. Edward Spitz, *Mutual Fund Performance and Cash Inflows*, 2 APPLIED ECON. 141, 144 (1970) ("[I]nflows in a given period were correlated with performance in the previous period . . . .").

[59] *See, e.g.*, Martin J. Gruber, *Another Puzzle: The Growth in Actively Managed Mutual Funds*, 51 J. FIN. 783, 799 tbl.5 (1996) (showing a stronger investor reaction to above-market returns then to below-market returns); Ippolito, *supra* note 58, at 61-62 ("[F]unds that do better than the market experience a stronger response than those that do worse."); Sirri & Tufano, *supra* note 56, at 1598 ("For top performers . . . performance is associated with economically and statistically significant inflows. . . . In the lowest quintile (the poorest performers), there is virtually no relationship between historical performance and flows.").

period, the fund's return for the prior 12 months and the average return for all funds with the same Strategic Insight Fund Objective Code for the prior 12 months. We took the difference between a specific fund's return and the average return for all funds with the same objective as the excess return. For our tests purposes, we divided the excess return into positive (PXRET) and negative (NXRET) returns, and incorporated the respective variables into our tests as control variables.

As additional control variables, we calculated the standard deviation in the fund's monthly returns for the prior 12 months as a measure of the fund risk,[60] the average flow into funds with the same objective for the month, the log of the fund's net assets in the prior month,[61] and the total load and expenses for a fund.[62]

Table 2 reports summary fund flow statistics. The statistics indicate that scandal month observations are associated with statistically significant lower flows, larger fund net assets, lower returns in the preceding year (relative to other funds with the same investment objective), lower standard deviation in returns, higher load, and higher expenses.

Table 2 – Summary Statistics for Fund Flows and Related Variables

	Non-Scandal		Scandal		
	N	**Mean**	**N**	**Mean**	**p-value**
Flow	296305	0.001	1491	-0.019	0.000
Total net assets	296305	1081.4	1491	1750.7	0.000
Excess return	296304	0.005	1491	-0.013	0.000
Standard dev. of returns for prior 1 year	296258	0.040	1491	0.036	0.000
Total Load	296305	0.022	1491	0.025	0.000
Expenses	295537	0.012	1471	0.014	0.000

P-value is from a two-sided t-test of difference between the means for non-scandal and scandal fund-months.

[60] *See, e.g.*, Patel et al., *supra* note 58, at 60 n.16 (defining a risk measure as the ratio of the mean to the standard deviation of excess returns); Sirri & Tufano, *supra* note 56, at 1597 ("[R]isk . . . [is] measured by the standard deviation of monthly fund returns over the prior 12 months.").

[61] *See, e.g.*, Sirri & Tufano, *supra* note 56, at 1597 (including the log of total net assets in the preceding period as a control variable).

[62] *See* Brad M. Barber et al., *Out of Sight, Out of Mind: The Effects of Expenses on Mutual Fund Flows*, 78 J. BUS. 2095, 2114 (2005) (finding that salience of expenses affects fund flows).

II. EMPIRICAL ANALYSIS OF FUND FLOWS

In this Part, we report the results of our empirical examination of the effect of scandals on funds that were involved in the scandal. In Section A, we describe and report the results of our base regression model. Section A also discusses certain robustness checks performed through the inclusion of additional control variables and changes in the length of the scandal period. In Section B, we refine the base model by differentiating between scandals based on the severity and the type of scandal. Section C examines the impact of scandals based on whether the scandal indicates an increased risk of future harm to fund investors. In Section D, we consider the importance of who first uncovers a scandal.

A. *The Base Model*

We constructed a regression model to test the market response after the announcement of a scandal using the monthly percentage fund flow (FLOW) as the dependent variable. Our base model is as follows:

$$\text{flow}_t = a + \beta_1 \text{Scandal}_t + \sum \beta_i \text{Control}_{it} + \text{Year Effects} + \text{Fund Effect} + \varepsilon_t$$

As an explanatory variable we included an indicator variable (SCANDAL) equal to 1 if the month flow data was from the 12 month scandal period beginning with the month in which the first announcement of the scandal was made in the *Wall Street Journal*, and equal to 0 otherwise. As controls, we include the positive and negative excess return for the prior 12 month period (PXRET and NXRET), the average flow for all other funds in the same objective code (OBJFLOW), the prior 12 month standard deviation of returns (STDRAW), the total load (TOTAL_LOAD) for a fund, the expenses (EXPENSES) for a fund, and the log of the prior month's total net assets for the fund (LNPTNA). The model includes both year and individual fund fixed effects to control for year-specific and fund-specific effects that may affect the flow into a fund.

Table 3 reports the model with the SCANDAL dummy variable. We calculated p-values to determine the significance of our coefficients using Huber-White robust standard errors. The coefficient on the SCANDAL variable is equal to -0.017, indicating a -1.7% monthly flow rate (significant at the <1% level). This coefficient estimate means that scandal funds experience an average abnormal outflow of 19% of the pre-scandal assets over the 12 month period following the announcement of a scandal. We used this model as the base model to perform subsequent tests as described below.

As robustness checks, we performed a number of regressions using the base model with the following modifications: We added the squares of the excess positive or negative returns to capture a possible non-linear relationship between fund flows and past returns; we included interaction terms between SCANDAL and the excess positive and negative return variables to test whether the effect of a scandal depended on past fund performance; we replaced the

variable for the prior 12 month standard deviation of returns with a variable for the standard deviation for the past 12 month returns for a fund relative to the mean standard deviation for the past 12 month returns for all funds in the same Strategic Insight Fund Objective Code group as the fund; we included a dummy variable for whether the fund month was December to control for investor rebalancing of portfolios at the end of the calendar year; we omitted the fund fixed effects; and we computed the standard errors in the base model using clustered standard errors. In none of these robustness checks did the coefficient on SCANDAL differ substantially for the coefficient we obtained in our base estimates,[63] and in each case the coefficient remained significant at the <1% level.

[63] The coefficient varied from -0.013 to -0.018.

Table 3 – Base Regression Model of Monthly Fund Flows

Variable	Model with Scandal Dummy	Model with Individual Scandal Month Dummies
PXRET	0.079*** (20.27)	0.079*** (20.27)
NXRET	0.051*** (15.80)	0.051*** (15.80)
OBJFLOW	0.005** (2.08)	0.005** (2.08)
STDRAW	-0.025*** (-2.89)	-0.025*** (-2.91)
LNPTNA	-0.007*** (-27.87)	-0.007*** (-27.85)
TOTAL_LOAD	-0.030 (-1.57)	-0.030 (-1.56)
EXPENSES	-0.003 (-0.03)	-0.002 (-0.03)
SCANDAL	-0.018*** (-13.50)	
SCAN_MON1		-0.014*** (-3.59)
SCAN_MON2		-0.015*** (-3.86)
SCAN_MON3		-0.028*** (-6.28)
SCAN_MON4		-0.024*** (-4.39)
SCAN_MON5		-0.020*** (-4.22)
SCAN_MON6		-0.015*** (-4.57)
SCAN_MON7		-0.021*** (-5.37)
SCAN_MON8		-0.012*** (-4.31)
SCAN_MON9		-0.018*** (-5.73)
SCAN_MON10		-0.018*** (-5.63)
SCAN_MON11		-0.014*** (-5.87)
SCAN_MON12		-0.009* (-1.65)
CONSTANT	0.048*** (24.97)	0.048*** (24.96)
Year Fixed Effects	Yes	Yes
Fund Fixed Effects	Yes	Yes
Adj R^2	0.116	0.116
N	296961	296961

Significant at the *10% level; **5%; ***1% level.

The dependent variable is monthly flow. SCANDAL is defined to equal 1 if the month is the same month as the initial *Wall Street Journal* article about a scandal or one of the subsequent 11 months (for a 12 month period) and 0 otherwise. For each scandal, the individual months in the scandal period, SCAN_MON1 through SCAN_MON12, is defined as 1 if the month corresponds to the specific month (from 1 to 12) beginning from the month of the initial scandal announcement in the *Wall Street Journal*. The t-statistics in parentheses are calculated using Huber-White robust standard errors.

The SCANDAL variable in the base model does not distinguish among the 12 months in the period beginning with the first *Wall Street Journal* scandal

announcement. Rather, the base model implicitly assumes that the impact of a scandal on fund flows in each month during the scandal period is the same.

To test the separate importance of each individual month after the scandal announcement, we treated each month as a dummy variable (SCAN_MON1 through SCAN_MON12).[64] The results show that flows are negative throughout the 12-month post-scandal period. The results, reported in Table 3, indicate that flows in each of the initial 11 post-scandal months are significantly negative at the <1% level; flows in the 12th month are negative and significant at the 10% level. In unreported F-tests, we rejected the hypothesis, at the <1% significance level, that the sum of the coefficients on the 12 scandal month variables is equal to zero. These results provide strong evidence that mutual funds involved in scandals experience statistically and economically significant outflows in the 12 months following the announcement of the scandal.

In order to determine whether the 12-month scandal period we used for the base was appropriate, we tested the significance of individual months outside that period. We first estimated the model with individual dummy variables for the 18-month period starting with the initial *Wall Street Journal* announcement month. The results for flows in months 1 through 12 are virtually unchanged from our 12 individual month model in Table 3. Flows in months 13 through 16 are significantly negative at the <1% significance level, and flows in months 17 and 18 are not significantly different from zero. Cumulative outflows for months 1 through 16 are 24% of pre-scandal assets (compared to outflows of 19% for months 1 through 12). We also examined the two months prior to the first report of the scandal. The coefficient on Month-1, the month immediately preceding the announcement month, is equal to -0.005 (or -0.5%) and is significant at the 10% level. The coefficient on Month-2 is equal to -0.004 (or -0.4%) and is not significant.[65] These results indicate that the estimates we obtain from our base model are, if anything, conservative estimates of the true magnitude of the outflows attributable to a scandal.

B. *Scandal Severity and Type*

We next examined the responsiveness of fund investors to the severity of the scandal. We hypothesized that investors would respond with a greater outflow from a scandal fund where the scandal appeared more severe. We used three proxies for scandal severity: the relative size of the regulatory settlement or fine amount (if any), the number of *Wall Street Journal* articles referring to the scandal, and the presence of formal charges filed by the SEC or another governmental entity.

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[64] SCAN_MON12, for example, is equal to 1 if the flow-month is from the twelfth month after the initial *Wall Street Journal* announcement.

[65] As an additional robustness check, we added a dummy for the month that a regulatory settlement or fine (if any) is first announced. The coefficient is equal to -0.007 (or -0.7%) and is significant at the 10% level.

Table 4 provides summary statistics for our three proxies for scandal severity. Scandals where a fine or settlement exceeded $110 million (the median size fine or settlement for the scandal funds) were categorized as "big scandals." Scandals where the number of *Wall Street Journal* articles exceeded eight (the median for all scandals) were categorized as "featured scandals."[66] Finally, scandals where the SEC or some other governmental entity filed formal charges were categorized as "enforced scandals."

Table 4 – Severity of the Scandal Summary Statistics

	Mean	**Median**
WSJ ARTICLES	10.1	8.0
SETTLEMENT AMT (*millions of dollars*)	173.0	110.0
CHARGES	0.9	1.0

WSJ ARTICLES is defined as the number of *Wall Street Journal* articles relating to a particular scandal (excluding articles that mention fund outflows related to the scandal). SETTLEMENT AMT is the settlement or fine amount in millions of dollars. CHARGES is defined to equal 1 if the fund relating to a fund-month faced formal governmental charges and 0 otherwise. Mean flow comparison is between the group of scandal funds with greater than the median number of WSJ ARTICLES or SETTLEMENT AMT and the group of funds equal to or below the median. Mean flow comparison is also between scandal funds that faced a formal governmental charge (CHARGES = 1) and those that did not (CHARGES = 0).

P-value is from a two-sided t-test of difference between the mean for scandal months with greater than median versus less than or equal to median WSJ ARTICLES and SETTLEMENT AMT. For CHARGES, p-value is from a two-sided t-test of the difference in mean flow where CHARGES = 1 versus CHARGES = 0.

We re-estimated the base model with the addition of each of our proxies for scandal severity. Table 5 reports our results with the addition of, respectively, interaction terms for SCANDAL x BIG SCANDAL (Model 1), SCANDAL x FEATURED SCANDAL (Model 2), and SCANDAL x ENFORCED SCANDAL (Model 3). In these regressions, the coefficients for the SCANDAL variable reflect the impact of the less severe scandals on fund flows, the coefficients for the scandal/severity interaction variable reflect the additional impact on fund flows that occurs when the scandal is more severe, and the sum of these two coefficients reflects the total impact of the more severe scandals.

[66] To avoid endogeneity problems, we excluded from our count of articles all *Wall Street Journal* articles that made explicit reference to fund flows.

Table 5 – Severity of the Scandal Regression Statistics

Variable	Model 1	Model 2	Model 3
PXRET	0.079*** (20.27)	0.079*** (20.27)	0.079*** (20.27)
NXRET	0.051*** (15.79)	0.051*** (15.79)	0.051*** (15.80)
OBJFLOW	0.005** (2.08)	0.005** (2.08)	0.005** (2.08)
STDRAW	-0.025*** (-2.89)	-0.026*** (-2.93)	-0.026*** (-2.92)
LNPTNA	-0.007*** (-27.87)	-0.007*** (-27.84)	-0.007*** (-27.90)
TOTAL_LOAD	-0.030 (-1.56)	-0.031 (-1.63)	-0.029 (-1.51)
EXPENSES	-0.002 (-0.03)	-0.003 (-0.04)	-0.003 (-0.04)
SCANDAL	-0.013*** (-7.91)	-0.013*** (-7.35)	0.009*** (2.82)
SCANDAL x BIG SCANDAL	-0.011*** (-4.20)		
SCANDAL x FEATURED		-0.010*** (-3.92)	
SCANDAL x ENFORCED			-0.028*** (-8.31)
CONSTANT	0.048*** (24.96)	0.048*** (24.96)	0.048*** (24.99)
Year Fixed Effects	Yes	Yes	Yes
Fund Fixed Effects	Yes	Yes	Yes
Adj R^2	0.116	0.116	0.116
N	296961	296961	296691

Significant at the *10% level; **5%; ***1% level.

The dependent variable is monthly flow. SCANDAL is defined to equal 1 if the month is the same month as the initial *Wall Street Journal* article about a scandal or one of the subsequent 11 months (for a 12 month period) and 0 otherwise. The t-statistics in parentheses are calculated using Huber-White robust standard errors.

In each of the three models, the coefficient on the interaction term is negative, ranging from -0.028 (or -2.8%) for ENFORCED SCANDALS to -0.010 (or -1.0%) for BIG SCANDALS. It is statistically significant at the <1% confidence level.[67] These results indicate that more severe scandals generate

[67] As a robustness check, we redefined BIG SCANDAL to include scandals that resulted in a settlement or fine that was greater than 1% of the total net assets of the fund. Using this alternate form of BIG SCANDAL we found that the coefficient on SCANDAL was equal to -0.016 (significant at the <1% level) and the coefficient on BIG SCANDAL was equal to -0.005 (significant at the 5% level), consistent with the hypothesis that more severe scandals result in a greater outflow.

It appears that the settlement amount in the market timing scandal involving Putnam Investments was based in part on the amount of fund outflows. *See* U.S. Securities and Exchange Commission, Commission Announces Completion of Independent Assessment

greater outflows than less severe ones. Depending on the measure of severity used, our estimate of outflows for the more severe scandals range from 21% to 29% of a fund's pre-scandal assets over the 12-month period following the scandal's announcement in the *Wall Street Journal*.[68]

Next, we examined the effect of scandal type on fund outflows. To do so, we grouped each scandal into one of three categories: scandals involving late trading or market timing (TIMING), scandals involving false or misleading disclosures (DISCLOSURE), and all other scandals (OTHER). The OTHER category included embezzlement, theft, improper trading, market manipulation, and IPO allocation-related scandals. Of the 135 different funds that were involved in scandals in our data set, 100 were TIMING scandals, 12 were DISCLOSURE scandals, and 23 were OTHER scandals.

To test for the importance of each scandal type, we replaced the SCANDAL variable from the base model with interaction terms between SCANDAL and the type of scandal. In these regressions, the coefficients for the scandal/type interaction variables reflect the impact on fund flows of each type of scandal respectively. These results are presented in Table 6.

In Model 1 of Table 6, we report that only the coefficients for TIMING scandals and DISCLOSURE scandals are negative and significantly different from zero (at the <1% confidence level). The coefficient for OTHER scandals is not significant. Model 1 yields estimates of outflows at 22% of a fund's pre-scandal assets for timing scandals and at 19% of a fund's pre-scandal assets for disclosure scandals, for each case over the 12 month period following the scandal announcement in the *Wall Street Journal.*

To separate the impact of market timing from late trading, we included an interaction term for timing scandals involving an international fund in Model 2. In Model 2, the coefficients for the scandal/type-interaction variables reflect the impact of the different basic types of scandals. The coefficient for the SCANDAL x TIMING x INTERNATIONAL FUND interaction variable reflects the

Consultant's Report on Losses Attributable to Market Timing and Excessive Short-term Trading by Putnam Employees (Mar. 3, 2005), http://www.sec.gov/news/press/2005-26.htm (stating that Putnam had agreed to compensate shareholders for $48.5 million in losses from the abnormal redemptions).

This relationship may result in an endogeneity problem in our regression test using BIG SCANDAL. As another robustness check, we eliminated TIMING scandals involving Putnam Investments from the BIG SCANDAL regression. Once again, the coefficients on SCANDAL and SCANDAL x BIG SCANDAL were negative (-0.013 and -0.010 respectively) and significant at the <1% level.

[68] In unreported regressions, we re-estimated each of the models in Panel B with 12 separate dummy variables for each month in the scandal period for the SCANDAL variable (and 12 separate monthly interaction terms for each of our severity proxies). In each specification, the Zero Hypothesis (i.e. that the sum of the coefficients on the 12-month variables is equal to zero) was rejected at the <1% confidence level for both the 12-month SCANDAL variables and for the 12-month interaction terms between the SCANDAL variable and the severity proxies.

additional impact on fund flows for timing scandals involving an international fund.

Market timing is primarily an international phenomenon. One commentator found that market timing is significantly more costly to holders of international funds compared with holders of domestic equity funds.[69] Model 2 provides a test for whether withdrawals are higher for international timing scandals when investors suffer greater losses. Model 2 reports that the coefficients for both timing scandals and timing scandals involving an international fund are negative and significantly different from zero. The coefficient for timing scandals involving an international fund indicates a greater outflow, 30% of pre-scandal assets over a 12 month period, for international timing scandals, compared with domestic timing scandals, which lost 21% of pre-scandal assets.[70]

Table 6 – Type of Scandal

Variable	Model 1	Model 2
PXRET	0.079*** (20.24)	0.079*** (20.24)
NXRET	0.051*** (15.84)	0.051*** (15.82)
OBJFLOW	0.005** (2.08)	0.005** (2.08)
STDRAW	-0.026*** (-2.97)	-0.026*** (-2.95)
LNPTNA	-0.007*** (-27.86)	-0.007*** (-27.87)
TOTAL_LOAD	-0.030 (-1.51)	-0.029 (-1.50)
EXPENSES	-0.005 (-0.06)	-0.006 (-0.08)
TIMING	-0.021*** (-14.47)	-0.019*** (-11.66)
TIMING x INTERNATIONAL FUND		-0.010*** (-2.64)
DISCLOSURE	-0.017*** (-3.27)	-0.017*** (-3.27)
OTHER	-0.000 (-0.15)	-0.000 (-0.14)
CONSTANT	0.048*** (24.96)	0.048*** (24.96)
Year Fixed Effects	Yes	Yes
Fund Fixed Effects	Yes	Yes
Adj R^2	0.116	0.116
N	296961	296961

Significant at the *10% level; **5%; ***1% level.

The dependent variable is monthly flow. The t-statistics in parentheses are calculated using Huber-White robust standard errors.

[69] *See* Eric Zitzewitz, *How Widespread Was Late Trading in Mutual Funds?*, 96 AM. ECON. REV. 284, 287 tbl.2 (2006).

[70] In unreported regressions, we re-estimated each of the models in Panel B with 12 separate dummy variables for each month in the scandal period for each of the scandal type interaction term variables. Results are similar to those reported in Panel B.

Our results indicate that the market punishes funds involved both in TIMING scandals and in DISCLOSURE scandals.[71] Importantly, this demonstrates that the results in the base model are not purely driven by the investor reaction to the wave of market timing scandals in 2003 and 2004.

A puzzle, nonetheless, exists. Why don't investors withdraw assets from funds involved in other scandals? We address this puzzle in the next Section.

C. *The Risk of Continuing Harm to Fund Investors*

Investors may not punish a fund involved in a scandal where investors continuing to keep their assets in the fund do not face an increased risk of future harm. The fund investor has already suffered whatever past harm the scandal may have generated and, by hypothesis, the scandal does not indicate an increased likelihood of future harm. For such scandals, a withdrawal of funds would be pointless from the perspective of investor self-protection.[72]

We identify two proxies for scandals where investors continuing with a fund may face little or no increased risk of future harm. First, investors may distinguish among scandals based on whether the alleged wrongdoer is still related to the fund. Where the wrongdoer is the fund management company itself, investors in the fund must still deal with the management company after resolution of the scandal. To our knowledge, no scandal has resulted in a replacement of the management company. This should lead more investors to exit the fund.[73] By contrast, where the wrongdoer is an individual, the

[71] Because severe scandals often involve market timing (see Table 6 of Panel B), our results on scandal severity presented in Table 4 may simply reflect a negative market reaction to market timing scandals. As a robustness test, we defined BIG_TIMING as equal to 1 if the TIMING scandal had greater than the median settlement amount for the group of all TIMING scandals ($175 million). We defined TIMING_FEATURED as equal to 1 if the TIMING scandal involved greater than the median number of *Wall Street Journal* articles for the group of all TIMING scandals (10 articles). We defined TIMING_ENFORCED as equal to 1 if the SEC or some other governmental entity filed formal charges relating to a TIMING scandal. We then added to Model 1 of Panel B of Table 5 (the model for the different scandal types) interaction terms for TIMING x BIG_TIMING, TIMING x TIMING_FEATURED, and TIMING x TIMING_ENFORCED. In unreported regressions, we found that the coefficients for the three interaction terms were negative and significant at the 10%, <1%, and <1% levels respectively, indicating that more severe scandals among the group of TIMING scandals resulted in greater outflows.

[72] An investor may nonetheless withdraw funds for other reasons, such as to exact retribution for past wrong-doing or to enforce a social norm not to engage in wrong-doing, especially if withdrawing funds is not costly to the investor. *Cf.* Richard H. McAdams, *The Origin, Development, and Regulation of Norms*, 96 MICH. L. REV. 338, 355 (stating that second-order collective action problems can be solved if punishing violators is relatively costless).

[73] Indeed, even though facing a scandal is a rare event for the group of all fund families, five of the fund families in our data set were involved in repeat scandals during the time

individual is almost always removed from the fund. Out of the 110 funds where a penalty was assessed on an individual, the individual departed or was suspended from 105 of the funds. Investors that remain with the fund going forward do not have to deal with the individual wrongdoer.

Second, investors may distinguish among funds involved in a scandal where they are directly harmed and those where they are not. For, instance, where the fund manager embezzles money from the fund, or in late trading/market timing scandals where the late trader/market timer makes profits at the expense of other fund investors, the investors will be directly harmed.[74] Alternatively, fund managers may engage in insider trading for the benefit of fund investors and to the detriment of third parties, or they may obtain large allocations of IPO stock, boosting the returns for their own fund investors.[75] In these scandals, the fund investor is not directly harmed.

We defined PEN_ENTITY as equal to 1 if the fund scandal resulted in a penalty imposed on an entity, such as the fund management company. We defined NO_PEN_ENTITY as equal to 1 if the fund scandal did not result in a penalty on an entity. Where no penalty was imposed on the fund management company, it is likely that the regulatory agency concluded that the management company did not engage in significant wrong-doing, such as authorizing the

period encompassed by our study. For example, INVESCO was involved in scandals in 1994 and 2003. *See* Laurie P. Cohen & Sara Calian, *Kaweske Probed By SEC Over Personal Trades*, WALL ST. J., Jan. 7, 1994, at A3; Tom Lauricella & Susan Pulliam, *Invesco Charged in Scandal as Strong Quits*, WALL ST. J., Dec. 3, 2003, at C1. Kemper was involved in scandals in 1995 and 2000. *See* Judith Burns, *SEC Stays Tough on Mutual-Fund Ads*, WALL ST. J., May 22, 2000, at C35; Jeffrey Taylor & Robert McGough, *Kemper Unit Target of SEC Complaint, Charging Fraudulent Stock Diversion*, WALL ST. J., Mar. 3, 1995, at B16. MFS Investments was involved in scandals in 2002 and 2003. *See* John Conner & Gregory Zuckerman, *SEC Probes MFS Over Bond Issue*, WALL ST. J., April 23, 2002, at C17; Tom Lauricella & John Hechinger, *Alliance Settles Charges*, WALL ST. J., Dec. 19, 2003, at C1. Strong was involved in scandals in 1994 and 2003. *See* Sara Calian, *SEC Studies Securities Moves in Strong Funds*, WALL ST. J., Mar. 11, 1994, at C1; Tom Lauricella, *Probe Hits Strong's Chairman*, WALL ST. J., Oct. 30, 2003, at C1. Van Kampen was involved in scandals in 1995 and 1999. *See* Sara Calian, *Ex-Fund Manager at American Capital Is Barred from Industry Over Pricings*, WALL ST. J., Mar. 3, 1995, at B6; Pui-Wing Tam, *IPO's Gave Big Boost To Van Kampen Fund*, WALL ST. J., Sept. 9, 1999, at C1.

[74] *See* Jason T. Greene & Conrad S. Ciccotello, *Mutual Fund Dilution from Market Timing Trades*, 4 J. INVESTMENT MGMT. 31, 39-41 (2006) (developing a model of the impact of market timing trades on returns); Zitzewitz, *supra* note 69, at 287 tbl.2 (estimating that, between 1998 and 2003, late trading reduced fund holder returns by 3.77 basis points per year in international equity funds and by 0.88 basis points per year in domestic equity funds).

[75] For example, the alleged actions by Fidelity Magellan Fund's Jeffrey Vinik, *see supra* text accompanying notes 32-36 (discussing how Vinik may have manipulated the market by publicly touting a stock while his fund sold it), relate to a scandal where third parties, rather than fund shareholders, would have suffered harm.

wrong-doing, failing to supervise employees properly, or engaging in cover-ups.[76]

We defined HARM as equal to 1 if the fund scandal resulted in harm directly to fund investors. We defined NO_HARM as equal to 1 if the fund scandal did not result in harm directly to fund investors.[77] Of our 135 scandals, 129 directly harmed fund investors, and 118 resulted in penalties on the entity.

To test for the importance of whether the entity was penalized and whether investors were directly harmed, we substituted the following interaction terms for the SCANDAL dummy variable in the base model: SCANDAL x PEN_ENTITY and SCANDAL x NO_PEN_ENTITY (Model 1), and SCANDAL x HARM and SCANDAL x NO_HARM (Model 2). In the regression, the coefficients for the SCANDAL x HARM and SCANDAL x NO_HARM interaction terms represent the impact on fund flows of scandals involving and not involving direct harm on fund investors, respectively. Similarly, the coefficients for the SCANDAL x PEN_ENTITY and SCANDAL x NO_PEN_ENTITY interaction terms represent the impact on fund flows of scandals resulting and not resulting in a penalty on a fund entity, respectively.

As reported in Table 7, the coefficient for scandals involving a penalized entity is equal to -0.020 (or -2.0%) and is significantly different both from 0 and from the coefficient for scandals not involving a penalized entity at the <1% significance level. The coefficient for scandals resulting in direct harm to fund investors is equal to -0.019 (or -1.9%) and is significantly different both from 0 and from the coefficient for scandals not resulting in direct harm to fund investors at the <1% significance level. By contrast, the coefficients for scandals not involving a penalized entity and for scandals not resulting in direct harm to fund investors are not statistically significant.

These results indicate that fund investors differentiate between these types of scandals. Scandals where the fund management company is penalized or where the scandal generated direct harm to fund investors are associated with significant outflows. Specifically, scandal funds suffered average outflows of 22% of pre-scandal assets for the 12-month scandal period (where the fund management company is penalized) and 21% of pre-scandal assets (where

[76] *See* Jennifer Arlen, *Evolution of Corporate Criminal Liability: Implications for Managers*, *in* LEADERSHIP AND GOVERNANCE FROM THE INSIDE OUT 191, 192-96 (Robert Gandossy & Jeffrey Sonnenfeld eds., 2004) (describing various ways in which management is held responsible for corporate crime).

[77] As a check on the validity of our PEN_ENTITY and HARM variables we examined a fund's market performance during the three-year period after the initial scandal announcement in the *Wall Street Journal*. Unreported, we found that the mean monthly excess return (defined as the difference in return of a fund with all other funds in the same Strategic Insight Fund Code for the same month) was negative and significant (at the <1% level) for HARM and PEN_ENTITY funds, and positive and not significant for NO_HARM and NO_PEN_ENTITY funds. The difference in excess returns for HARM versus NO_HARM funds and PEN_ENTITY versus NO_PEN_ENTITY funds was significant, respectively, at the 10% and the 5% levels.

investors suffered direct harm). In contrast, we find no statistically or economically significant outflows where no one was, or only individuals were, penalized, or where the scandal did not generate direct harm to fund investors.

Table 7 – Risk of Future Harm Regression Statistics

Variable	Model 1	Model 2	Model 3
PXRET	0.079*** (20.27)	0.079*** (20.23)	0.079*** (20.24)
NXRET	0.051*** (15.79)	0.052*** (15.84)	0.051*** (15.83)
OBJFLOW	0.005 (2.08)	0.005 (2.08)	0.005** (2.08)
STDRAW	-0.026*** (-2.91)	-0.025*** (-2.91)	-0.026*** (-2.93)
TOTAL_LOAD	-0.030 (-1.54)	-0.030 (-1.56)	-0.030 (-1.53)
EXPENSES	-0.003 (-0.03)	-0.004 (-0.05)	-0.004 (-0.05)
LNPTNA	-0.007*** (-27.87)	-0.007*** (-27.86)	-0.007*** (-27.85)
SCANDAL x PEN_ENTITY	-0.020*** (-13.66)		
SCANDAL x NO_PEN_ENTITY	-0.002 (-1.17)		
SCANDAL x HARM		-0.019*** (-14.07)	
SCANDAL x NO_HARM		0.003 (0.43)	
SCANDAL x HARM x PEN_ENTITY			-0.021*** (-14.27)
SCANDAL x (NO_HARM or NO_PEN_ENTITY)			-0.001 (-0.40)
CONSTANT	0.048*** (24.96)	0.048*** (24.98)	0.048*** (24.96)
Year Fixed	Yes	Yes	Yes
Fund Fixed	Yes	Yes	Yes
Adj R^2	0.116	0.116	0.115
N	296961	296961	296691

Significant at the *10% level; **5%; ***1% level.

The dependent variable is monthly flow. The t-statistics in parentheses are calculated using Huber-White robust standard errors.

As a further refinement, we re-estimated the base model in Model 3 distinguishing between, on the one hand, scandals where *both* the fund management company was penalized *and* the scandal generated direct harm to

fund investors,[78] and all other scandals on the other hand. "All other scandals" includes any scandal where either the fund management company is not penalized or the scandal does not generate direct harm to fund investors.[79]

This model allows us to capture the effect of scandals where the risk of future harm was the greatest, namely, when the scandal both generated direct harm to fund investors and resulted in an entity being penalized. We found that the coefficient for scandals generating direct harm to fund investors and resulting in an entity being penalized is -0.021 (or -2.1%), and this is significantly different, each at the <1% significance level, from both 0 and from the coefficient on scandals where the risk of future harm is less severe. (The latter is statistically and economically insignificant.) These results provide further evidence that the investor response is confined to scandals which entail greater risk of future harm for investors who opt to stay with the fund.[80]

With evidence suggesting that the investor response to scandals depends on whether the scandal entails greater risk of future harm, we re-examined the impact of scandal type and scandal severity. To do this, we first interacted our proxy for risk of future harm (HARM x PEN_ENTITY) with the indicator variables for different scandal types (TIMING, DISCLOSURE, and OTHER). If, within the set of scandals that generate increased risk of future harm, scandal type affected investor response, we would expect the coefficients for these interaction variables to differ from each other.

The results of this regression are reported in Model 1 of Table 8. We found that each of the coefficients for the three interaction terms are significant and negative. In unreported F-tests, we further found that no coefficient on an interaction term was significantly different from another coefficient. These results indicate that investors penalize funds where they expect a greater risk of future harm for all three types of scandals. But, within the set of scandals that generate increased risk of future harm, the investor response is statistically indistinguishable based on the type of scandal.

In contrast, we found that, even within the set of scandals that generated increased risk of future harm, scandal *severity* further affects investor response. In Models 2 through 4 of Table 8, we distinguished between more severe and less severe scandals by adding interaction terms between scandals generating direct harm to fund investors and involving a penalized entity and our three severity variables (BIG SCANDAL, FEATURED, and ENFORCED). In each of the

[78] This is represented by the interaction term SCANDAL x HARM x PEN_ENTITY, which includes 112 scandals.

[79] This is represented by the interaction terms SCANDAL x NO_HARM and SCANDAL x NO_PEN_ENTITY. In total, these interaction terms involved 23 scandals in the data set.

[80] In unreported regressions, we re-estimated each of the models in Table 7 with separate dummy variables for each month in the scandal period included in the scandal interaction terms. The results were similar to those reported in Table 7, except that the SCANDAL x NO_PEN_ENTITY variable was significant at the 10% level.

specifications, the coefficients for these interaction terms are negative and significant (at the 5% level for the interaction term with BIG SCANDAL, at the 10% level for FEATURED, and at the 1% level for ENFORCED). Moreover, in each of the specifications, the coefficient for scandals generating harm to fund investors, and involving a penalized entity, is negative and significant at the <1% level.

These results indicate that scandals generating increased risk of future harm also generate significant outflows whether or not they are severe, and, that the more severe scandals generate significantly larger outflows than less severe scandals.[81] For scandals that both generate increased risk of future harm and are severe, we estimate abnormal average outflows of 23% to 25% of the pre-scandal assets during the 12-month post-scandal period. Scandals that generate increased risk of future harm but are not severe are associated with outflows of 12% to 20%. Scandals that do not generate increased risk of future harm are not associated with significant outflows.

[81] Re-estimations of the models in Table 8 with 12 separate dummy variables for each month in the scandal period for the various scandal interaction terms yielded results similar to those in Table 8.

Table 8 – Scandal Severity and Type Regression Statistics

Variable	Model 1	Model 2	Model 3	Model 4
PXRET	0.079***	0.079***	0.079***	0.079***
	(20.25)	(20.25)	(20.25)	(20.24)
NXRET	0.051***	0.051***	0.051***	0.051***
	(15.83)	(15.82)	(15.82)	(15.83)
OBJFLOW	0.005***	0.005***	0.005***	0.005***
	(2.08)	(2.08)	(2.08)	(2.08)
STDRAW	-0.026***	0.026***	0.026***	0.026***
	(-2.95)	(-2.92)	(-2.95)	(-2.93)
TOTAL_LOAD	-0.029	-0.030	-0.030	-0.030
	(-1.52)	(-1.53)	(-1.57)	(-1.53)
EXPENSES	-0.004	-0.004	-0.004	-0.004
	(-0.05)	(-0.04)	(-0.05)	(-0.05)
LNPTNA	-0.007***	-0.007***	-0.007***	-0.007***
	(-27.86)	(-27.86)	(-27.84)	(-27.85)
SCANDAL x HARM x PEN_ENTITY		-0.018***	-0.018***	-0.011***
		(-8.70)	(-8.14)	(-2.96)
TIMING x HARM x PEN_ENTITY	-0.022***			
	(-14.24)			
DISCLOSURE x HARM x PEN_ENTITY	-0.018***			
	(-3.18)			
OTHER x HARM x PEN_ENTITY	-0.013*			
	(-1.69)			
SCANDAL x HARM x PEN_ENTITY x BIG SCANDAL		-0.006**		
		(-1.99)		
SCANDAL x HARM x PEN_ENTITY x FEATURED			-0.005*	
			(-1.73)	
SCANDAL x HARM x PEN_ENTITY x ENFORCED				-0.011***
				(-2.70)
SCANDAL x (NO_HARM or NO_PEN_ENTITY)	-0.001	-0.001	-0.001	-0.001
	(-0.40)	(-0.42)	(-0.39)	(-0.40)
CONSTANT	0.048***	0.048***	0.048***	0.048***
	(24.96)	(24.96)	(24.96)	(24.96)
Year Fixed	Yes	Yes	Yes	Yes
Fund Fixed	Yes	Yes	Yes	Yes
Adj R^2	0.115	0.115	0.115	0.115
N	296691	296961	296961	296961

Significant at the *10% level; **5%; ***1% level.

The dependent variable is monthly flow. The t-statistics in parentheses are calculated using Huber-White robust standard errors.

D. *Who Discovers the Scandal*

Finally, we examined the relationship between who first uncovers the scandal and investor response to the scandal.[82] We distinguished, through indicator variables, between scandals where the initial public source was the SEC (27 scandals in our sample); scandals where the initial public source was another governmental entity, such as a state attorney general (82 scandals); and scandals brought to light by a non-governmental entity or person, such as the financial press (25 scandals).[83] This relationship is of interest not because we hypothesize that who discovers a scandal causes a different investor response, but because it sheds light on whether the SEC and other governmental bodies detect those scandals that fund investors care about, as evidenced by a withdrawal of funds.

Model 1 in Table 9 estimates the base model with the substitution of interaction terms based on who discovered the scandal: the SEC (SECFIRST), a governmental source other than the SEC (OTHERGOV), or a non-governmental source (NONGOV). In the regression, the coefficients on the SCANDAL x NONGOV, SCANDAL x OTHERGOV, and SCANDAL x SECFIRST variables represent the impact on fund flows of scandals first discovered by a non-governmental entity or person, a non-SEC governmental source, or the SEC, respectively.

The coefficients for scandals discovered by non-governmental sources or scandals discovered by governmental sources other than the SEC are both negative and significant in Model 1. In contrast, the coefficient for scandals discovered by the SEC is not statistically different from zero. This suggests that scandals discovered by non-governmental bodies and by governmental bodies other than the SEC are associated with significant outflows, but that scandals discovered by the SEC are not associated with such outflows.

Anecdotal evidence suggests that Eliot Spitzer, the former Attorney General of New York, and other state attorneys general played an important role in uncovering the late trading and market timing scandals. To test whether the result for scandals uncovered by non-SEC governmental bodies was generated primarily by late trading and market timing scandals, or whether it was more general, we added in Model 2 interaction terms between SCANDAL x TIMING and who discovered the scandal.

[82] To the extent that the *Wall Street Journal* coverage did not report all of the required information regarding who initially uncovered a scandal, we would conduct our own additional research to supplement it.

[83] For one scandal, it was not possible to determine who discovered it.

Table 9 – Who Discovered the Scandal Regression Statistics

Variable	Model 1	Model 2
PXRET	0.079*** (20.25)	0.079*** (20.25)
NXRET	0.051*** (15.84)	0.052*** (15.86)
OBJFLOW	0.005** (2.08)	0.005** (2.08)
STDRAW	-0.026*** (-2.97)	-0.026*** (-2.99)
TOTAL_LOAD	-0.030 (-1.51)	-0.029 (-1.51)
EXPENSES	-0.001 (-0.02)	-0.003 (-0.04)
LNPTNA	-0.007*** (-27.84)	-0.007*** (-27.84)
SCANDAL x NONGOV	-0.024*** (-7.78)	-0.028*** (-5.90)
SCANDAL x OTHERGOV	-0.021*** (-12.62)	0.035*** (3.44)
SCANDAL x SECFIRST	0.002 (1.02)	0.005** (2.02)
SCANDAL x TIMING x NONGOV		0.009 (1.50)
SCANDAL x TIMING x OTHERGOV		-0.058*** (-5.60)
SCANDAL x TIMING x SECFIRST		-0.010* (-1.70)
CONSTANT	0.048*** (24.91)	0.048*** (24.93)
Year Fixed Effects	Yes	Yes
Fund Fixed Effects	Yes	Yes
Adj R^2	0.116	0.116
N	296691	296691

Significant at the *10% level; **5%; ***1% level.
The dependent variable is monthly flow. The t-statistics in parentheses are calculated using Huber-White robust standard errors.

In Model 2, the coefficients for the interaction terms between SCANDAL and who discovered the scandal represent the impact on fund flows for non-timing scandals based on who discovered the scandal.[84] The coefficients for the interaction terms for SCANDAL x TIMING and who discovered the scandal represent the *additional* impact on fund flows for timing scandals.[85] The sum of the coefficients for the interaction term between SCANDAL and who

[84] For example, the interaction term SCANDAL x SECFIRST estimates the relationship between non-timing scandals discovered by the SEC and fund flows.

[85] For example, the interaction term SCANDAL x TIMING x SECFIRST estimates the additional flows for timing scandals first discovered by the SEC.

discovered the scandal and the corresponding timing interaction term estimates the overall relationship between the timing scandals and fund flows.[86]

For non-timing scandals, we found that only scandals discovered by non-governmental bodies are associated with significant outflows. For timing scandals, we found that scandals uncovered by both non-governmental bodies and by non-SEC governmental bodies are associated with significant outflows. Thus, regardless of type, scandals discovered by the SEC *are not associated with significant outflows*, and scandals discovered by the financial press and other non-governmental bodies *are* associated with significant outflows.[87]

Our results with respect to the lack of investor response to SEC-discovered scandals could be explained by two possible hypotheses. First, the SEC may fail to focus on the more important scandals and thus provide investors little value in its detection of scandals. Second, the SEC may focus more on scandals that fund investors do not penalize after-the-fact, but nonetheless merit regulatory sanctions.

As a simple test between these hypotheses, we examined the relative focus of the SEC on uncovering scandals that (a) result in no direct harm to fund investors and (b) involve no penalty on an entity. As discussed above, such scandals may not evoke a significant market response because they do not entail an increased risk of future harm to fund investors. At the same time, however, it may nonetheless still be desirable to sanction, and thus deter, such scandals to the extent that they either generated harm for third parties or generated past harm for fund shareholders without raising the risk of future harm.

Table 10 reports that no significant difference exists between SEC and non-SEC discovered scandals for the incidence of scandals with no direct harm to fund investors (as indicated by the NO_HARM variable). The lack of investor reaction to SEC-discovered scandals is not due to the fact that a disproportionate share of SEC-discovered scandals generated harm to third parties, as opposed to fund shareholders. However, 30.6% of SEC discovered scandals involved no penalty on an entity compared with only 6.9% of non-SEC discovered scandals (with the difference significant at the <1% level). This difference, however, is largely due to the fact that SEC-discovered scandals are substantially more likely to result in no penalties *to anyone* (37.0% for SEC discovered scandals versus 6.5% for non-SEC discovered scandals, difference significant at the <1% level).

[86] For example, the sum of SCANDAL x SECFIRST and SCANDAL x TIMING x SECFIRST estimates the relationship between timing scandals discovered by the SEC and fund flows.

[87] Re-estimations of the models in Table 9 with 12 separate dummy variables for each month in the scandal period for the various scandal interaction terms yielded similar results.

Table 10 – Comparison of Non-SEC and SEC Discovered Scandals

	N	Non-SEC Discovered Scandals	N	SEC Discovered Scandals	p-value
Fraction of Scandal Funds with NO_HARM	108	0.046	27	0.027	0.836
Fraction of Scandal Funds with NO_PEN_ENTITY	108	0.065	27	0.370	0.000
Fraction of Scandal Funds with Penalty on Individual Only	108	0.056	27	0.148	0.102

We define NO_HARM as equal to 1 if the fund scandal does not result in harm to fund investors and 0 otherwise. We define NO_PEN_ENTITY as equal to 1 if the fund scandal does not result in a penalty on an entity and 0 otherwise.

P-value is from a two-sided t-test of difference in the fraction of scandal funds with NO_HARM or NO_PEN_ENTITY between non-SEC discovered and SEC-discovered scandals.

By contrast, the difference in the likelihood of the discovery resulting in a penalty for only individuals is much smaller – 14.8% for SEC discovered scandals versus 5.6% for non-SEC discovered scandals – and not significant at conventional levels. Overall, these results provide tentative evidence that the SEC focuses on scandals that do not involve significant wrong-doing.

III. ANALYSIS OF FUND FAMILY FLOWS

In the previous Part, we analyzed the impact of a scandal on the specific funds involved in the scandal. But, beyond generating outflows for the fund directly involved in the scandal, a scandal may impose negative reputational effects on other funds in the same fund family. Specifically, investors may be wary that a scandal signifies an increased risk of wrong-doing associated with the fund management company more generally, and therefore an increased risk of future harm for investors in any fund managed by that company.

In this Part, we tested the impact of scandals on funds that were in the same family as a scandal fund; a total of 1214 funds. For the 12-month period beginning with the month in which the initial announcement of the scandal occurs in the *Wall Street Journal*, we defined the variable SCANDAL FAMILY to equal 1 if a fund was a member of the same family as the scandal fund but was *not* directly involved in the scandal, and equal to 0 otherwise.

We started our analysis by adding the SCANDAL FAMILY variable to the base model. As reported in Model 1 of Table 11, the coefficient on SCANDAL FAMILY is equal to -0.006 and significant at the <1% level. This result indicates that scandal family funds experienced abnormal withdrawals of 7% of their pre-scandals assets in the year following discovery of the scandal.

Though this percentage figure is substantially lower than the one for scandal funds themselves, there are many more scandal family funds than scandal funds, and their aggregate assets greatly exceed those of the scandal funds. On average, in our sample, a scandal fund had assets of $850 million, whereas the aggregate assets of all funds in the same family as the scandal fund averaged $45.7 billion. Thus, the impact of scandals on scandal family funds contributed significantly to the penalty investors exacted on fund management companies involved in the scandals, thereby contributing to deterrence of such wrong-doing in the future.

We further hypothesized that scandals generate a greater spillover effect on scandal-family funds where the scandal fund itself is more prominent than the rest of the funds in its family. That is, a scandal involving one of the flagship funds of a fund management company is likely to reflect more negatively on fund management, and will make investors in scandal-family funds more concerned about possible future harm, than a scandal involving a relatively minor fund within a family. To test this hypothesis, we calculated the ratio of total net assets of the scandal funds within the same family involved in a particular scandal divided by total net assets of all funds in the same family. We defined a new variable, PROMINENT, to be equal to 1 if the ratio was above the median ratio value for all scandal family observations, and equal to 0 otherwise.

In Model 2, we added an interaction variable, SCANDAL FAMILY x PROMINENT, to the regression from Model 1. In this regression, the coefficient for the SCANDAL FAMILY variable reflects the impact of the less prominent scandals on flows for the non-scandal funds in a fund family affected by a scandal. The coefficient for the SCANDAL FAMILY x PROMINENT interaction variable reflects the *additional* impact on fund family flows when the scandal impacts a more prominent fund within a family. The sum of these two coefficients reflects the total impact of more prominent scandals on fund family flows.

The coefficients for both the SCANDAL FAMILY variable and for the SCANDAL FAMILY x PROMINENT interaction variable are negative and significant at the <1% level. The regression shows that the spillover effect is significantly larger where the scandal fund accounted for a larger percentage of the aggregate assets of all funds in the fund family. Where the scandal fund was relatively prominent, withdrawals for scandal family funds averaged 9% of their pre-scandal assets in the year following discovery of the scandal; where it was not prominent, withdrawals averaged only 2.5%.

Table 11 – Fund Family Impact

Variable	Model 1	Model 2	Model 3	Model 4	Model 5
SCANDAL	-0.018***	-0.018***	-0.014***	-0.014***	0.008***
	(-13.96)	(-13.92)	(-8.23)	(-7.66)	(2.67)
SCANDAL x BIG SCANDAL			-0.011***		
			(-4.35)		
SCANDAL x FEATURED				-0.010***	
				(-3.96)	
SCANDAL x ENFORCED					-0.029***
					(-8.33)
SCANDAL FAMILY	-0.006***	-0.002***	-0.003***	-0.002***	-0.002***
	(-12.61)	(-3.85)	(-4.21)	(-3.83)	(-3.90)
SCANDAL FAMILY x PROMINENT		-0.006***	-0.003***	-0.004***	-0.003
		(-7.27)	(-3.60)	(-3.91)	(-1.64)
SCANDAL FAMILY x PROMINENT x BIG SCANDAL			-0.010***		
			(-7.72)		
SCANDAL FAMILY x PROMINENT x FEATURED				-0.007***	
				(-5.82)	
SCANDAL FAMILY x PROMINENT x ENFORCED					-0.004**
					(-2.41)
Fund Controls	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Fund Fixed Effects	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.116	0.116	0.116	0.116	0.116
N	296961	296961	296961	296961	296961

Significant at the *10% level; **5%; ***1% level.

The dependent variable is monthly flow. The t-statistics in parentheses are calculated using Huber-White robust standard errors.

In Models 3, 4, and 5 we added interaction variables between our measures for scandal severity and the SCANDAL and SCANDAL FAMILY x PROMINENT variables to the regression from Model 2. These additional interaction terms captured the incremental impacts on fund flows for our three proxies for scandal severity. Models 3, 4, and 5 indicate that both the relative prominence of the scandal fund and the severity of the scandal will each independently increase the spillover effect. When severity is proxied by the BIG SCANDAL or the FEATURED variable, the coefficients on both SCANDAL FAMILY x PROMINENT and the severity interaction variables are negative and significant at the <1% confidence level. When severity is proxied by the ENFORCED

variable, the coefficient on the severity interaction variable is negative and significant and the SCANDAL FAMILY x PROMINENT variable is negative (and borderline insignificant at conventional levels). For scandals that both involve a prominent fund and are severe, we estimate that scandal family funds suffer withdrawals of 15% to 18% of their pre-scandal assets in the year following discovery of the scandal. Since management fees are generally proportionate to assets, a fund management company involved in such a scandal thus lost *on average* approximately 1/6 of its *total* fund management revenues in the year following the scandal's discovery.[88]

In our final set of regressions, we examined the effect of the risk of future harm on the spillover effect of a scandal. In Model 1 of Table 12, we included the interaction terms for SCANDAL x HARM x PEN_ENTITY and SCANDAL x (NO_HARM or NO_PEN_ENTITY) to distinguish scandals more likely to pose a risk to fund investors into the future. To these interaction terms, we added the SCANDAL FAMILY variable and similar interaction terms for SCANDAL FAMILY x PROMINENT x HARM x PEN_ENTITY and SCANDAL FAMILY x PROMINENT x (NO_HARM or NO_PEN_ENTITY). These additional interaction terms capture the incremental effect scandals more likely (and less likely) to pose a risk to fund investors in the future can have on the rest of a scandal family funds where the scandal is prominent.

We reported the coefficients for the SCANDAL and SCANDAL FAMILY interaction terms (omitting the rest of the coefficients on control variables in the model). These regressions indicate that the risk of future harm is an important factor affecting the magnitude of the spillover effect. Specifically, we found that a small spillover effect on scandal family funds – corresponding to a loss of 3.5% of pre-scandal assets in the post-scandal year – is present even for scandals that did not involve a prominent fund (the coefficient on SCANDAL FAMILY is significantly negative at the <1% level in each model). For scandals that are prominent but did *not* entail significant risk of future harm (SCANDAL FAMILY x PROMINENT x (NO HARM or NO PEN_ENTITY)), we found no statistically or economically significant increase in the spillover effect (the coefficient is -0.1% and statistically insignificant).

We did, however, find substantial evidence indicating that scandals involving both a prominent fund and entailing increased risk of future harm resulted in greater outflows. In Models 2, 3, and 4 of Table 12, we added interaction terms between the SCANDAL x HARM x PEN_ENTITY and SCANDAL FAMILY x PROMINENT x HARM x PEN_ENTITY variables on the one hand, and our proxies for scandal severity (BIG SCANDAL, FEATURED, and ENFORCED) on the other. The coefficients on these additional interaction terms represent the incremental effect of a more severe scandal on fund flows for scandal and scandal family funds where the scandal is more likely to harm fund investors

[88] Re-estimations of the models in Table 11 with 12 separate dummy variables for each month in the scandal period for the SCANDAL, SCANDAL FAMILY, and related interaction terms yielded results similar to those reported here.

into the future. We find evidence that scandal severity (proxied by BIG SCANDAL) further increases this spillover effect.[89] Thus, for scandals involving prominent funds, we found that severity and risk of future harm had a qualitative effect on withdrawals from scandal family funds quite similar to the qualitative effect endured by the scandal funds themselves.[90]

[89] Although we found no significant increase in the spillover effect when severity was proxied by FEATURED or ENFORCED, in an F-test the sum of the coefficients for future harm and for severity was negative and significant at a <1% confidence level when severity was proxied by ENFORCED.

In unreported regressions, we re-estimated each of the models in Panel C with 12 separate dummy variables for each month in the scandal period for the various SCANDAL and SCANDAL FAMILY interaction terms and tested whether the sum of the coefficients on the 12 month variables was equal to zero. For each model, the results were similar to those in Panel C, with the exception that the Zero Hypothesis test (that the sum of the coefficients for the 12-month SCANDAL FAMILY x PROMINENT x HARM x PEN_ENTITY x FEATURED variables was equal to zero) was rejected at the 10% confidence level.

[90] Note from Models 2, 3, and 4 that the coefficient on SCANDAL FAMILY x PROMINENT x HARM x PEN_ENTITY was significant at the <1% level. In Model 4, the coefficient was negative and of a similar magnitude to the coefficients in Models 1, 2, and 3, but it was not statistically significant.

Table 12

Variable	Model 1	Model 2	Model 3	Model 4
SCANDAL x HARM x PEN_ENTITY	-0.022***	-0.019***	-0.019***	-0.012***
	(-14.82)	(-9.05)	(-8.52)	(-3.24)
SCANDAL x HARM x PEN_ENTITY x BIG SCANDAL		-0.006**		
		(-2.08)		
SCANDAL x HARM x PEN_ENTITY x FEATURED			-0.005*	
			(-1.71)	
SCANDAL x HARM x PEN_ENTITY x ENFORCED				-0.010***
				(-2.65)
SCANDAL x (NO_HARM or NO_PEN_ENTITY)	-0.001	-0.001	-0.001	-0.001
	(-0.52)	(-0.54)	(-0.51)	(-0.52)
SCANDAL FAMILY	-0.003***	-0.003***	-0.003***	-0.003***
	(-4.22)	(-4.31)	(-4.24)	(-4.22)
SCANDAL FAMILY x PROMINENT x HARM x PEN_ENTITY	-0.011***	-0.009***	-0.013***	-0.008
	(-11.80)	(-7.06)	(-7.47)	(–0.65)
SCANDAL FAMILY x PROMINENT x HARM x PEN_ENTITY x BIG SCANDAL		-0.004***		
		(-2.61)		
SCANDAL FAMILY x PROMINENT x HARM x PEN_ENTITY x FEATURED			0.003	
			(-1.45)	
SCANDAL FAMILY x PROMINENT x HARM x PEN_ENTITY x ENFORCED				-0.004
				(-0.30)
SCANDAL FAMILY x PROMINENT x (NO_HARM or NO_PEN_ENTITY)	-0.001	-0.001	-0.001	-0.001
	(-0.57)	(-0.52)	(-0.56)	(-0.57)
Fund Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Fund Fixed Effects	Yes	Yes	Yes	Yes
Adj R^2	0.116	0.116	0.116	0.116
N	296961	296961	296961	296961

Significant at the *10% level; **5%; ***1% level.

Dependent variable is monthly flow. The t-statistics in parentheses are calculated using Huber-White robust standard errors.

CONCLUSION

Using fund flow data from 1994 to 2004, we examined investor response to mutual fund scandals. We identified scandals through searches in the *Wall*

Street Journal database, and found instances where a mutual fund was subject to investigation or sanction by a state or federal regulatory agency.

We found that during the 12-month period beginning with the first report of the scandal in the *Wall Street Journal*, investors made substantial withdrawals from scandal funds. On average, these withdrawals amounted to 19% of the funds' pre-scandal assets. Outflows in each individual month during the 12-month scandal period, as well as for the period as whole, were statistically significant.

We next distinguished between types of scandals according to their severity. We used three different proxies for severity: the size of the regulatory settlement or the amount of fine, the number of *Wall Street Journal* articles on the scandal, and the filing of formal charges. We found that outflows for the more severe scandals were significantly larger than for less severe ones, and that the outflows amounted to 21% to 29% of a fund's pre-scandal assets (depending on the severity proxy) over the 12-month period following the scandal announcement in the *Wall Street Journal.*

Investors might not make withdrawals from funds involved in scandals if the scandal did not portend an increased risk of future harm. We identified two proxies for risk of future harm: whether the wrongdoer is the fund management company itself that continues to manage the fund post-scandal (as opposed to an individual, who is usually removed from the fund post-scandal); and whether the alleged wrongdoing harmed fund investors directly (as opposed to harming third parties). We found that outflows were greater for scandals when our proxies indicated an increased risk of future harm. Specifically, scandals that both involved the fund management company and harmed fund investors directly – where the risk of future harm was greatest – generated average outflows of 22.5% of a fund's pre-scandal assets, while other scandals generated no statistically significant outflows.

Within the set of scandals that entailed a greater risk of future harm, severity of the scandal further increased the magnitude of outflows. By contrast, we found no evidence that the type of scandal – whether it involved late trading or market timing, disclosure violations, or other issues – affected the magnitude of outflows.

We further tested the importance of who first uncovers the scandal. We found that, regardless of type of scandal, scandals discovered by the SEC are *not* associated with significant outflows, but that scandals discovered by the financial press and other non-governmental bodies *are* associated with significant outflows. We found tentative evidence indicating that this may be due to the fact that the SEC's detection resources are relatively focused on scandals which involve no significant wrong-doing.

Scandals also generated spillover effects in other funds sharing the same fund family as the scandal fund. These spillover effects were stronger when the scandal fund accounted for a greater share of the total assets of all funds in the fund family, where the scandal was severe, and where the scandal portended increased risk of future harm. We found that scandal-family funds

as a whole experienced statistically significant outflows of 7% of the pre-scandal assets and that the more aggravated scandals generated, depending on the specification used, outflows of up to 18% of the pre-scandal assets in the post-scandal year.

Our results show that the ability to redeem shares for their net asset value gives fund investors an effective method for protecting themselves against continued losses resulting from a managerial wrong-doing and, for certain scandals, for penalizing fund management. This suggests that alternative mechanisms to reduce agency costs – such as the voting mechanism or the market for corporate control – are less necessary in the mutual fund context, and that the SEC should exercise restraint before it mandates governance structures for mutual funds. Further, the SEC should take account of the penalties exacted on fund management companies through such withdrawals in determining the optimal regulatory penalty.

For some scandals, however – for example, those that do not entail an increased risk of future harm for fund investors – investors do not make significant withdrawals. For those scandals, regulatory penalties are needed to deter wrongdoing. Our results tentatively suggest that the SEC and other governmental entities should expend more resources on the detection of these scandals, where the wrong-doing harms third parties but does not harm fund investors.