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Preliminary Draft

The Market Penalty for Mutual Fund Scandals

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Abstract

Using fund flow data from 1994 to 2004, we examine the market response to mutual fund scandals. We identify scandals as reported in the Wall Street Journal. We report that during the 12 month period beginning with the first report of the scandal in the Wall Street Journal, scandal funds experience a significantly greater outflow of assets. The outflow is greater for funds that experienced a more severe scandal (as proxied through the size of the regulatory settlement or fine amount, the number of WSJ articles on the scandal, and the filing of formal charges). Outflows are greater where the scandal involved a penalized entity compared where no entity was penalized, indicating that the market punishes funds less where the wrongdoer is no longer associated with the fund (as is the case with individual wrongdoers). Outflows are also greater where the scandal involved harm to fund investors compared with scandals that did not harm the fund investors (but rather harmed third parties). Who initially discovered the scandal is an important determinant of the amount of outflows. Scandals first discovered by the SEC (as opposed to a non-governmental source or another governmental body) experience no significant outflows. Lastly, we report that the presence of strengthened corporate governance controls have no impact on the amount of outflows from a fund.

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1. Introduction

Unlike shareholders in ordinary public companies, holders of open-ended mutual funds have at their disposal an effective method for penalizing fund managers and for protecting themselves against continued losses resulting from a managerial wrong-doing: they can have the fund redeem their shares for net asset value. Since fund management fees are a percentage of the fund assets, any redemptions directly reduce the revenues of the fund management company. And since shares can be redeemed for their net asset value, redemptions insulate investors from expected future losses.

This paper provides an empirical analysis of the effect of managerial wrong-doing on flows in and out of open-ended mutual funds. This article examines whether fund shareholders are more likely to redeem mutual fund shares after a scandal, how shareholder reactions differ for different kinds of scandals, and how scandals affect other funds in the same family as the scandal fund. We find that investors penalize both scandal funds and, to a lesser extent, scandal fund families, by making statistically and economically significant withdrawals and that withdrawals are greater for scandals that are likely to generate greater shareholder losses.

This paper also considers the regulatory implications of the redemption rights of mutual fund shareholders. Since

shareholders can and do effectively penalize fund managers for wrong-doing on their own, there is arguably less need for regulatory enforcement actions to deter wrongdoing. This paper therefore examines the relation between SEC enforcement actions and net asset flows. Even if there is less need for regulatory enforcement actions, regulators may be useful to the extent that they assist in the detection of scandals. This paper studies the relation between whether regulators uncovered the scandal and net asset flows. Finally, the Securities and Exchange Commission, responding to the recent set of mutual fund scandals related to late trading and market timing, has recently adopted new rules on mutual fund governance requiring each fund board to have an independent board chairman and to be composed of at least three-quarters independent directors or trustees. These new rules have been criticized as both costly and ineffective. If board composition is important to investors, we expect shareholders to react differently to scandals in funds with more independent boards than to funds with less independent boards. In particular, investors would be more likely to attribute scandals in funds with independent boards to an accidental oversight, rather than to systematic governance failure, and accordingly respond less strongly. This paper also examines the relation between fund governance characteristics and net asset flows.

Section 2 of the paper provides an overview of the

literature in the area. Section 3 describes the data. Section 4 contains the empirical analysis of fund flows. Section 5 concludes.

2. Literature Overview

This is the first article to examine the effect of scandals of mutual fund flows. Prior literature in related areas falls into three categories. First, several articles examine general factors affecting fund flows. A number of articles show that past returns affect monthly fund flows (Spritz (1970), Ippolito (1992), Sirri and Tufano (1998), Patel and et. (1994)) with several finding stronger effects for positive returns than for negative returns (e.g. Ippolito (1992), Gruber (1996), Sirri and Tufano (1998)). Other factors affecting monthly flows include risk, fund size, fund complex size, media coverage, fund categories, past flows (e.g., Sirri and Tufano (1998), Patel and et. (1994)) changes in Morningstar ratings, (Del Guercio & Tkac (2001)), and name changes (Rau et al. (2004)). Goetzmann et al. (1999) find that daily flows to equity funds are negatively correlated with flows to money market and precious metal funds. Idro (2004) finds that aggregate flows into equity funds reflect investor sentiment.

A second category of studies examines various aspects of late trading and market timing (though not their effect on fund

flows). Several papers find that market timing in international funds can generate profits and propose a fair pricing mechanism to address this concern (Goetzmann et al. (2001), Chalmers et al. (2001), Greene & Hodges (2002), Zitzewitz (2003a)). Greene and Ciccotello (2004) develop a model of the impact of market timing trades on returns. Zitzewitz (2003) estimates that late trading reduces fund holder returns by 5 basis points per year in international equity funds and by .6 basis points per year in domestic equity funds.

A third category of studies relates to the regulatory changes designed to protect fund shareholders. In addition to Rouwenhorst et al., papers in that category include among others Goetzmann et al. (2001), who propose a fair pricing mechanism; Tibbets (2004), who suggests a new private cause of action to enforce independence of mutual fund directors; Ward (2005), who advocates a forward pricing rule to combat late trading and market timing; Securities and Exchange Commission (2004), which imposes new regulations on board and board chair independence in the wake of recent scandals; and Nourmand (2004), who suggests that no new rules are required because the market punishes fund wrongdoing.

3. Description of Data

We obtained data on monthly mutual fund net assets and

returns for open-end funds for the period 1994 to 2004 from the CRSP mutual fund dataset. We also collected from CRSP information on each mutual fund's objective, the fund family with which the fund is affiliated, load, total expenses, and year of organization. We include only mutual funds in the same Strategic Fund Objective Code grouping as one of the scandal funds.

We calculated monthly percentage flow data as follows:

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

yielding 298,392 monthly flow observations. As is common in studies of fund flows, this flow measure does not count reinvested fund distributions as new inflows. We exclude 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 growth not representative of fund flows after the first year. To focus on funds with substantial investor assets, we also exclude funds with a total net assets of less than or equal to $50 million on the 13th month of the fund's existence (or, in the case of a fund that was initiated before 1993, the first month of fund data in our data set). We lastly excluded as outliers the highest and lowest 0.1%

of the observations, with 297,796 fund-month flow observations remaining (spanning a total of 4072 funds). Table 1 provides a yearly breakdown of the number of scandal and non-scandal monthly flow data.

We calculated for each monthly fund period the fund's return for the prior 12 months and the average return for all funds with the same Strategic Fund Objective Code for the prior 12 months. We take the difference between a specific fund's return and the average return for all funds with the same objective as the excess return. For purposes of our tests, we divide the excess return into positive (pxret) and negative (nxret) returns. We also calculated the standard deviation in the fund's monthly returns for the prior 12 months (stdraw), the average flow into funds with the same objective for the month (objflow), and the log of the fund's net assets in the prior month (lnptna).

To identify mutual fund scandals, we conducted various searches in the Wall Street Journal database on WESTLAW to identify instances where a fund was subject to investigation or sanction by a state or federal regulatory agency. We consider each such instance as a scandal. In total, we identified 137 funds involved in scandals. For each scandal, we collected information on the fund involved. We treated the 12 months starting with the month in which the publication of the first article on the scandal takes place as the "scandal period". Due

to some missing observations, the 137 scandal funds yielded 1491 scandal-month observations.

In Table 2, we report summary statistics. The mean monthly percentage flow for non-scandal mutual fund months is 0.001, indicating that the average monthly fund inflow is 0.1% of its assets. In comparison, the mean flow for scandal months is -0.019, or -1.9% of the fund's assets. This difference is significant at the <1% confidence level.

4. Empirical Analysis of Fund Flows

4.1. Base Model

We construct a model to test the market response after the announcement of a scandal, using the monthly fund flow as the dependent variable. Our base model is as follows:

$$flow_t = a + ß_1 Scandal_t + \sum ß_i Control_{it} + \text{Year Effects} + \text{Fund Effects} + \epsilon_t$$

As an explanatory variable we include an indicator variable (Scandal) equal to 1 if the month flow data is from the 12 month scandal period beginning with the month in which the first announcement of the scandal is made in the Wall Street Journal. As controls, we include the positive and negative prior 12 month

fund excess return (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), 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 calculate p-values for the significance of the coefficients using Huber-White robust standard errors. The coefficient on the Scandal variable is equal to -0.017, indicating a -1.7% flow rate (significant at the <1% level). This coefficient estimate indicates that a scandal fund experiences an outflow of 19% of the pre-scandal assets in the 12 month period following the announcement of a scandal. We use this model as the base model to perform subsequent tests as described below.[1]

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[1] We perform a number of robustness tests. First, we introduce pxret^2 and nxret^2 squared terms to capture possible non-linearities in the relationship between fund flows and past returns. The coefficient on Scandal is equal to -0.017 (or -1.7%) and is significant at the <1% level. Second, we include interaction terms between Scandal and the pxret and nxret return variables. The coefficient on Scandal is equal to -0.013 (or -1.3%) and is significant at the <1% level. Third, we introduce the total load and expenses for a fund as obtained from CRSP. The coefficient on Scandal is equal to -0.018 (or -1.8%) and is significant at the <1% level. Fourth, we replace the stdraw variable with 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 Fund Objective Code group as the fund. The coefficient on Scandal is equal to -0.017 (or -1.7%) and is significant at the <1% level. Fifth, we included a dummy variable for whether the fund month was December to control for investor rebalancing of portfolios at the end of the calender year. The coefficient on Scandal is again equal to -0.017 (or -1.7%) and is significant at the <1% level.

Our construction of the scandal variable does not distinguish among the 12 months in the period beginning with the first WSJ scandal announcement. To test the separate importance of each individual month after the scandal announcement, we treat each month as a dummy variable (scan_mon1 through scan_mon12). Scan_mon12, for example, is equal to 1 if the flow-month is from the 12th month after the initial WSJ announcement.

Table 3 reports the model with scan_mon1 through scan_mon12 instead of the Scandal dummy variable. The results indicate that flows in each of the initial 11 post-scandal months are significantly negative. Flows in the 12th month are negative, but significant at only the 10% level. The results suggest that it is proper to use a 12 month post-scandal period to test for abnormal flows.

We test whether the sum of the coefficients on the 12 scandal month variables are equal to zero (Zero Hypothesis). Panel B of Table 3 reports that the Zero Hypothesis is rejected at the <1% level. Our results are consistent with the alternate hypothesis that funds involved in a scandal experienced significant fund outflows in the 12 month period immediately following the initial public announcement of the scandal.

4.2. Severity of the Scandal

We test the responsiveness of the market to the severity of

the scandal. We hypothesize that the market responds with a greater outflow from a scandal fund where the scandal is more "severe". We use 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.

Panel A of Table 4 provides summary statistics for our three proxies for scandal severity. Scandals where a fine or settlement exceeded the median size fine or settlement for the scandal funds, $110 million, 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". Finally, scandals where the SEC or some other governmental entity filed formal charges were categorized as "Enforced Scandals". As reported in Panel A, the mean flows for scandal months with a Big Scandal, Featured Scandal, or Enforced Scandal were significantly greater than the mean flows for the scandal months that did not involve the respective proxy for scandal severity (although the difference for Big Scandal flows is significant at only the 10% level).

We estimate the base model with the addition of each of our proxies for scandal severity to control for other factors that may affect fund flows. Panel B of Table 4 reports our results

for the addition of interaction terms for Scandal x Big Scandal (Model 1), Scandal x Featured Scandal (Model 2), and Scandal x Enforced Scandal (Model 3). In all 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, and statistically significant at the <1% confidence level. These figures suggest outflows for the more severe scandals of 11% to 29% greater than outflows for the other scandal funds over the 12 month period following the scandal announcement in the WSJ.[2]

We re-estimate each for the models in Panel B with the 12 months in the scandal period after the initial WSJ announcement separately included in the model (together with 12 separate monthly interaction terms for our severity proxies). For each specification we test whether the sum of the coefficients on the 12 month variables are equal to zero (Zero Hypothesis). Panel C reports that for all three proxies for severity the Zero

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[2] As a robustness check, we redefine 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 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 is possible that for the market timing scandal involving Putnam Investments, the settlement amount was determined based on asset outflows, resulting in an endogeneity problem in our regression test using Big Scandal. We were unable to find definitive evidence of this relationship however. Nonetheless, 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.

Hypothesis is rejected at the <1% confidence level.

The evidence from the regression analysis of the relationship between scandal severity and fund flows is consistent with the hypothesis that the market responds with greater fund outflows the more severe the scandal.

4.3. Fund Family Impact

A scandal involving one particular fund may impose negative reputational reputation effects on other funds in the same fund family. We test the impact of a scandal on other funds not directly affected by the scandal but in the same fund family. For the 12 month period beginning with the month in which the initial announcement of the scandal occurs in the Wall Street Journal, we define the variable “Scandal Family” as equal to 1 if a fund is a member of the same family as the scandal fund but is not directly involved in the scandal.

Panel A of Table 5 provides a summary statistic comparison of the flow for non-scandal months and Scandal Family months. The mean flow for Scandal Family months is -0.006 (or -0.6%) compared with a mean flow of 0.001 (or 0.1%) for non scandal months (difference significant at the <1% confidence level).

Panel B of Table 5 adds the Scandal Family variable in addition to interaction terms between Scandal Family and the proxies for scandal severity (Big Scandal, Featured, and

Enforced) to the base model. As reported in Model 1 of Panel B, the coefficient on Scandal Family is equal to -0.006 compared with a coefficient of -0.018 on the Scandal variable (difference significant at the <1% level). The coefficients on Scandal Family x Big Scandal, Scandal Family x Featured, and Scandal Family x Enforced in Models 2 through 4 are all negative and significant at the <1% confidence level. The coefficients on the Scandal Family severity interaction terms are less in magnitude compared with the coefficients on Scandal x Big Scandal, Scandal x Featured, and Scandal x Enforced. The difference in coefficients between the Scandal and Scandal Family interaction terms with Big Scandal is not statistically significant. The differences in coefficients for the Scandal and Scandal Family interaction terms with the Featured and Enforced variables are significant at the <1% level.

While a spillover effect to other funds in the same fund family as a scandal fund exists, the magnitude of the market response is less than with the scandal fund. The magnitude of outflows for scandal family funds is, however, economically significant, ranging from 7% to 15% of the pre-scandal assets for the year following the scandal. While these percentage figures are lower than for scandal funds, the aggregate assets of all scandal family funds are much larger than those of scandal funds.

We re-estimate each for the models in Panel B with the 12

months in the scandal period separately included in the model for the Scandal, Scandal Family, and severity interaction term variables. For each specification we test whether the sum of the coefficients on the 12 month variables are equal to zero (Zero Hypothesis). Panel C of Table 5 reports that the Zero Hypothesis is rejected for all the 12 month variables relating to the Scandal Family variable and its interaction terms except for the Scandal Family x Enforced interaction term.

4.4. Type of Scandal

We test whether the market punishes funds that engage in scandals that directly harm fund shareholder more than scandals that harm third parties. We grouped each scandal in 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 trade, market manipulation, and IPO allocation related scandals. Of the 137 different funds that were involved in scandals in our dataset, 100 were in Timing scandals; 13 in Disclosure Scandals; and 24 in Other scandals.

Panel A of Table 6 reports summary statistics on monthly fund flows based on the type of scandal. Only the mean fund flows for Timing and Disclosure related scandals are negative and

significantly different from zero.

To provide a multivariate test, we replace the Scandal variable from the base model with interaction terms between Scandal and the type of scandal (Timing, Disclosure, and Other) in Panel B of Table 6. In Model 1, we report that only the coefficients on Scandal x Timing and Scandal x Disclosure are negative and significantly different from zero (at the <1% confidence level). The coefficient on Scandal x Other is not significant. To separate the impact of market timing from late trading, we include an interaction term for Scandal x Timing x International Fund in Model 2. Market timing is primarily an international phenomenon. Zitzewitz (2003) finds that late trading is significantly more costly to holders of international funds compared with holders of domestic equity funds. Model 2 provides a test of whether withdrawals are higher for International Timing scandals, where investors suffer greater losses. Model 2 reports that the coefficients on Scandal x Timing (-0.019) and Scandal x Timing x International Fund (-0.010) are negative and significantly different from zero. The negative and significant coefficient on Scandal x Timing x International indicates a greater outflow for International Timing scandals compared with domestic Timing scandals.

We re-estimate each of the models in Panel B with the 12 months in the scandal period separately included in the model for

the scandal type interaction term variables. For each specification we test whether the sum of the coefficients on the 12 month variables are equal to zero (Zero Hypothesis). Panel C of Table 6 reports that the Zero Hypothesis is rejected for the 12 month variables relating to Scandal x Timing, Scandal x Timing x International Fund, and Scandal x Disclosure. In contrast, the zero hypothesis is not rejected for the 12 month Scandal x Other variables.

Our results support the view that the market punishes funds involved in Timing and Disclosure scandals.[3] A puzzle, nonetheless, exists. Why doesn't the market also punish scandals that involve the Other scandals? We address this puzzle in the next section.

4.5. The Risk of Continuing Harm to Fund Investors

The market may not punish funds involved in scandals where investors that continue to keep their assets in the fund do not face an increased risk of future harm. We identify two types of

[3]Panel D of Table 6 provides a robustness test of the relationship between scandal severity and fund outflows. Because most large scandals involve market timing, our results on scandal severity presented in Table 4 may simply reflect a negative market reaction to market timing scandals. For Panel D we define 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 define timing_featured as equal to 1 if the timing scandal involved greater than the median number of WSJ articles for the group of all timing scandals (10 articles). We then add to Model 1 of Panel B of Table 6 (the model for the different scandal types) interaction terms for timing x big timing, timing x timing_featured, and timing x enforced. Panel D reports that the coefficients for the three interaction terms are 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.

scandals where investors continuing with a fund are not harmed. First, the market may distinguish among scandals based on whether the wrongdoer is still related to the fund. Where the wrongdoer is the management company of the fund itself, investors in the fund must still deal with the management company after the resolution of the scandal (leading more investors to exit the fund). Where the wrongdoer is an individual, the 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 (or 95.5% of the time). Investors that remain with the fund looking forward do not have to deal with the individual wrongdoer. Second, the market may distinguish among funds involved in a scandal where fund investors are directly harmed (such as where the fund manager embezzles money from the fund and the market timing scandals) and where fund investors are not directly harmed. For example, fund managers may engage in insider trading for the benefit of fund investors (and the detriment of third party investors). Fund managers may also obtain large allocations of IPO stock, boosting the returns for their own fund investors.

We define Pen_Entity as equal to 1 if the fund scandal results in a penalty imposed on an entity, such as the fund management company. We define No_Pen_Entity as equal to 1 if the fund scandal does not result in a penalty on an entity. We

define Harm as equal to 1 if the fund scandal results in harm directly to fund investors. We define No_Harm as equal to 1 if the fund scandal does not result in harm directly to fund investors. Panel A of Table 7 reports summary statistics on the mean monthly flow for scandals that resulted in a penalty on an entity compared with scandals that resulted in no penalty on an entity. Panel A also reports a comparison of the mean monthly flow for scandals that resulted in harm directly to fund investors against those scandals that did not do so. The mean flow for Pen_Entity scandals is equal to -0.021 (or -2.1%) while the mean flow for No_Pen_entity scandals is equal to 0.000 (or 0.0%), difference significant at the <1% confidence level. The mean flow for Harm scandals is equal to -0.020 (or -2.0%) while the mean flow for No_Harm scandals is equal to 0.005 (or 0.5%), difference significant at the <1% confidence level.

To control for other factors that may affect fund flows, we test the importance of Pen_Entity versus No_Pen_Entity and Harm versus No_Harm using the base model. Panel B of Table 7 reports the base model with the addition of interaction terms between Scandal x Pen_Entity and Scandal x No_Pen_Entity (Model 1). The coefficient on Scandal x Pen_Entity is equal to -0.019 (or -1.9%) and is significant at the <1% level. In contrast, the coefficient on Scandal x No_Pen_Entity is equal to -0.002 (or -0.2%) and is not statistically significant. The difference

between the coefficients on Scandal x Pen_Entity and Scandal x No_Pen_Entity is significant at the <1% level. The results are consistent with the hypothesis that the market response is stronger where investors expect the wrongdoers to remain with the fund after the resolution of the scandal. Panel B of Table 7 also reports the base model with the addition of interaction terms between Scandal x Harm and Scandal x No_Harm (Model 2). The coefficient on Scandal x Harm is equal to -0.018 (or -1.8%) and is significant at the <1% level. In contrast, the coefficient on Scandal x No_Harm is equal to 0.003 (or 0.3%) and is not statistically significant. The difference between the coefficients on Scandal x Harm and Scandal x No_Harm is significant at the <1% level. The results are consistent with the hypothesis that the market response is stronger where the scandal involves direct harm to fund investors and thereby a greater risk of future harm for those that stay with the fund.

We re-estimate each of the models in Panel B with the 12 months in the scandal period separately included in the model for Scandal and the Scandal x Pen_Entity and Scandal x No_Pen_Entity interaction terms (for Model 1) and the Scandal x Harm and Scandal x No_Harm interaction terms (for Model 2). For each specification we test whether the sum of the coefficients on the 12 month variables are equal to zero (Zero Hypothesis). Panel C of Table 7 reports that the Zero Hypothesis is not rejected for

the 12 month Scandal x No_Harm variable. In contrast to the result in Panel B, the Zero Hypothesis is rejected for the 12 month Scandal x No_Pen_Entity variable, but at only the 10% level. Consistent with Panel B, the Zero Hypothesis is rejected for the 12 month Scandal x Pen_Entity and Scandal x Harm variables. The 12 month specification thus provides some support for the hypothesis that the market response is greater for scandals where investors expect a greater risk of future harm with the fund. Certain types of scandals resulted in a greater incidence of penalties against entities and direct harm to investors. Panel D of Table 7 reports the incidence of Pen_Entity and Harm for the three categories of scandals. Market Timing scandals resulted in the greatest incidence of penalties against entities (96.9%). Other scandals resulted in the lowest incidence of penalties against entities (43.7%). Similarly, Other scandals had the lowest incidence of Harm against fund investors at 70.9% of the scandals.

To test the interaction between Pen_Entity and Harm, we estimate the base model with the substitution of Scandal x Harm x Pen_Entity, Scandal x Harm x No_Pen_Entity, and Scandal x No_Harm in place of the Scandal variable. The use of these three interaction terms allows us to distinguish between scandals where we expect that investors will not penalize the fund (Scandal x Harm x No_Pen_Entity and Scandal x No_Harm) and where investors

will penalize the fund (Scandal x Harm x Pen_Entity). As reported in Panel E of Table 7 (Model 1), note that the coefficients on Scandal x Harm x No_Pen_Entity and Scandal x No_Harm are both statistically insignificant. In contrast, the coefficient on Scandal x Harm x Pen_Entity is equal to -0.021 (or -2.1%) and is significant at the <1% level. The market punishes funds where there is a greater likelihood of future harm facing investors that remain with the fund.

Model 2 of Panel E of Table 7 re-estimates Model 1 with the replacement of Scandal x Harm x Pen_Entity with three interaction terms for the type of scandal (Timing x Harm x Pen_Entity, Disclosure x Harm x Pen_Entity, and Other x Harm x Pen_Entity). Model 2 tests whether the market penalty differs depending on the type of scandal. The coefficients on Timing x Harm x Pen_Entity, Disclosure x Harm x Pen_Entity, and Other x Harm x Pen_Entity are negative (-0.021, -0.018, and -0.013 respectively) and significant (although the coefficient on Other x Harm x Pen_Entity is significant at only the 10% level). Evidence therefore exists that for all three types of scandals, investors penalize funds where they expect a greater risk of future harm.

4.6. Who Discovers the Scandal

We test the importance of who first uncovers the scandal using the market response as an indicator of how the market

values actions by different government actors. To the extent the Wall Street Journal coverage did not report all the required information on who initially uncovered the scandal, we conducted additional research to supplement it. Nongov is defined as 1 if any non-governmental entity or person was the initial public source for the scandal. Othergov is defined as 1 if any non-SEC governmental entity was the initial public source for the scandal (including, for example, actions on the part of state attorney generals). Secfirst is defined as 1 if the SEC was the initial public source for the scandal.

Panel A of Table 8 reports the breakdown of the mean monthly flow based for the scandal months based on who discovered the scandal first (Nongov, Othergov, or Secfirst). While scandals discovered by Nongov or Othergov experience a statistically significant outflow of funds, Secfirst scandals do not have a significant mean flow.

Panel B of Table 8 estimates the base model with interaction terms between Scandal and who discovered the scandal (Nongov, Othergov, or Secfirst) instead of the Scandal dummy variable in Model 1. The coefficients on Scandal x Nongov and Scandal x Othergov are both negative and significant in Model 1. In contrast, the coefficient on Scandal x Secfirst is not statistically different from zero.

Model 2 of Panel B adds interaction terms between Scandal x

Timing and who discovered the scandal (Nongov, Othergov, or Secfirst) to Model 1. The additional interaction terms in Model 2 test the separate importance of who discovered the scandal for purposes of late trading and market timing scandals. Anecdotal evidence suggests that Eliot Spitzer, the N.Y. State Attorney General, played an important role in uncovered the late trading and market timing scandals. While the coefficient on Scandal x Nongov remains negative and significant in Model 2, the coefficient on Scandal x Othergov is now positive (0.035) and significant. Instead, the coefficient on Scandal x Timing x Othergov is negative (-0.058) and significant, supporting the view that outside of Eliot Spitzer's actions, other governmental discovered scandals do not result in a large market response in the form of fund outflows.

In Panel C, we re-estimate each of the models with the 12 months in the scandal period separately included in the model for the interaction terms between Scandal and who discovered the scandal (as well as whether it was a Timing scandal). For each specification we test whether the sum of the coefficients on the 12 month variables are equal to zero (Zero Hypothesis). Panel C of Table 8 reports that the Zero Hypothesis is rejected for the 12 month Scandal x Nongov variables. The Zero Hypothesis is also

rejected for the 12 month Scandal x Timing x Othergov variables.[4] As with the coefficients on Scandal x Secfirst in the regression models in Panel B, Panel C reports that the Zero Hypothesis is not rejected for the 12 month Scandal x Secfirst variables.

Our results with respect to the lack of market response for SEC discovered scandals are consistent with two possible hypotheses. First, the SEC fails to focus on more important scandals and provides little value in the actions it initiates. Second, the SEC focuses more on actions that the market may not necessarily penalize after-the-fact, but nonetheless are socially worthwhile to impose regulatory sanctions. As a simple test between these hypotheses, we examine the relative focus of SEC on uncovering scandals that (a) result in no direct harm to fund investors (No_Harm) and (b) involve no penalty on an entity (No_Penalty_Entity). Panel D of Table 8 reports that no significant difference exists between the incidence of No_Harm for non-SEC discovered and SEC discovered scandals. On the other hand, 33.1% of SEC discovered scandals involved no penalty on an entity compared compared with only 6.7% of non-SEC discovered scandals (difference significant at the <1% level). This result is consistent with the possibility that the SEC focuses

[4] While the Zero Hypothesis is also rejected for the 12 month Scandal x Othergov interaction term in the same model with the 12 month Scandal x Timing x Othergov variables, the coefficients for the 12 month Scandal x Othergov interaction terms are all positive, indicating that the Scandal x Othergov observations that did not involve market timing did not experience fund outflows.

relatively more of its resources on uncovering scandals that the market would not otherwise penalize.

4.7. Corporate Governance

We test the importance of corporate governance in determining how the market responds to a scandal. If corporate governance worked to protect the interests of investors looking forward after a scandal then one would expect a milder market response to a scandal for funds with stronger corporate governance. In response to some of the recent scandals, the SEC adopted new governance rules requiring funds to have an independent chairman or to have a board composed of 3/4 of independent directors (except for 3 person boards which need to have only 2 independent directors). We collect data on the boards of the scandal funds for the year of the scandal to see how they correspond with the new SEC rules, i.e. whether they have an independent chairman (Indepchair = 1) and or a board with the requisite proportion of independent directors (Indep34 = 1).

Panel A of Table 9 reports summary statistics on the corporate governance of the scandal funds. Only 19.1% of the month data involved a fund with an independent chair. In contrast, 80.0% of the fund-month data involved a fund with a board consisting of at least 3/4 independent directors. Panel A reports that no statistically significant difference exists

between the flow for scandal months where Indepchair equals 1 compared with where there is not independent chair. Similarly, no statistically significant difference exists between the flow for scandal months where Indep34 equals 1 compared with where the board is not comprised of at least 3/4 independent directors.

Panel B of Table 9 reports the base model with the addition of interaction terms for Scandal x Indepchair and Scandal x Indep34. In all three models in Panel B, the coefficients on the Scandal x Indepchair and Scandal x Indep34 variables are not significantly different from zero. Our evidence is consistent with the hypothesis that corporate governance does not affect how the market perceives the impact of scandals on investors.

In Panel C, we re-estimate each of the models with the 12 months in the scandal period separately coded for Scandal, Scandal x Indepchair, and Scandal x Indep34. For each specification we test whether the sum of the coefficients on the 12 month variables are equal to zero (Zero Hypothesis). Panel C of Table 9 reports that the Zero Hypothesis is not rejected for Scandal x Indepchair and Scandal x Indep34, consistent with the hypothesis that corporate governance does not affect the market response. This result is inconsistent with the hypothesis that stronger corporate governance ameliorates the impact of a

scandal.[5]

Our results on corporate governance are not conclusive. We examine only the impact of corporate governance conditional on a fund facing a scandal. We cannot rule out the possibility that corporate governance is important in determining which funds face a scandal in the first place.

4.8. Magnitude of the Market Response

We examine the relative magnitude of the market penalty imposed on scandal funds and the regulatory penalty (a settlement or fine) imposed on the fund and associated individuals. Table 10 reports the comparison between the settlement or fine amount and market penalty for the individual scandal funds.

Because the regulatory settlement or fine is typically assessed on a group of funds in a fund family, we allocate a portion of the settlement or fine amount to each specific fund. For each scandal fund, the allocated settlement amount is equal to the settlement or fine amount multiplied by the ratio of the total net assets for the first month in the scandal period for the fund over the total net assets for all funds in the same fund family involved in the scandal. We define the asset outflow for each fund as equal to: (flow for the 12 months period from the

[5] Unreported, in both Models 1 and 3, Scandal x Chair for months 3 and 4 after the scandal announcement are significant at the <1% level. However, both coefficients are negative, indicating that funds with an independent chair experienced a *greater* outflow of funds following a scandal.

first announcement of the scandal minus the average flow for funds in the same Strategic Fund Objective Code group for the same time period) multiplied by the total net assets of the fund for the month before the first announcement of the scandal multiplied by -1.

From Table 10 note that while the average allocated settlement amount for each scandal fund is $22.1 million, the mean one year asset outflow is equal to $271.6 million. The market penalty imposed on the fund managers, however, is not equal to the total asset outflow. Rather, the fund management firm suffers a loss from the reduced fee they may charge on the reduced asset base of the fund from the scandal. The fund management firm may also experience reduced growth in their funds as fewer new investors choose to invest in the fund. To calculate the market penalty on the fund management firm, we take a conservative approach in measuring the lost fees for only the first year after the scandal due to the fund outflows. Under this measurement, the mean market penalty imposed on the fund management firm is equal to only $3.7 million. Table 10 reports that the mean market penalty increases to a maximum of $5.5 million for scandals involving a settlement or fine greater than the median settlement (Big Scandal). The market penalty for the fund family is the market penalty aggregated for all members of the fund family. Looking at the entire fund family, the one-year

market penalty is equal to $32.3 million compared to a total mean settlement or fine amount for the entire fund of $96.9 million (difference significant at the 5% level).

5. Conclusion

Using fund flow data from 1994 to 2004, we examine the market response to mutual fund scandals. We identify scandals as reported in the *Wall Street Journal*. We report that during the 12 month period beginning with the first report of the scandal in the *Wall Street Journal*, scandal funds experience a significantly greater outflow of assets. The outflow is greater for funds that experience a more severe scandal (as proxied through the size of the regulatory settlement or fine amount, the number of WSJ articles on the scandal, and the filing of formal charges). Outflows are greater where the scandal involves a penalized entity associated with the fund and harm directly imposed on fund investors, indicating that the market punishes funds less where there is less of an expectation that the scandal will translate into a greater risk of future harm for those that remain with the fund. Who initially discovered the scandal is an important determinant of the amount of outflows. Scandals first discovered by the SEC (as opposed to a non-governmental source or another governmental body) experience no significant outflows. Lastly, we report that the presence of strengthened corporate governance

controls have no impact on the amount of outflows from a fund.

Our results highlight the importance of the market as a complement to regulatory enforcement actions. A significant portion of the deterrence facing management companies of mutual funds is the market response to a scandal. The market penalty, moreover, increases with the severity of the scandal. Our research also points out where the market penalty is deficient: scandals in which, looking forward, investors do not expect to receive a greater risk of future harm. These scandals include those where the wrongdoer is not the fund management company and scandals where the harm is primarily to third parties and not to fund investors. Our results also call into question the efficacy of corporate governance reforms in protecting investors.

Table 1: Year Breakdown of Monthly Flow Data

Each observation consists of the flow for a fund-month. Scandal months are months in the scandal period defined as the 12 month period beginning from the month in which the first announcement in the WSJ of the scandal occurs. Non-Scandal months are all other fund-months.

Year	Non-Scandal	Scandal
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**

Table 2: Summary Statistics

Excess return is defined as the geometric return for the prior 12 month period minus the average geometric return for all funds in the same SI objective group.

	n	non-scandal	n	scandal	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 mean for non-scandal and scandal fund-months.

Table 3: Base Regression Model of Monthly Fund Flows and the Impact of Scandal

Panel A

Dependent variable is monthly flow. The p-values are calculated using Huber-White robust standard errors. 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*. In the model, we include the positive and negative prior 12 month fund excess return (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), 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.

Variable	**Model with Scandal Dummy**	**p-value**	**Model with Individual Scandal Month Dummies**	**p-value**
pxret	0.079	0.000	0.079	0.000
nxret	0.051	0.000	0.051	0.000
objflow	0.005	0.038	0.005	0.038
stdraw	-0.025	0.004	-0.025	0.004
lnptna	-0.007	0.000	-0.007	0.000
scandal	-0.017	0.000		
scan_mon1			-0.014	0.003
scan_mon2			-0.015	0.000
scan_mon3			-0.028	0.000
scan_mon4			-0.024	0.000
scan_mon5			-0.020	0.000
scan_mon6			-0.014	0.000
scan_mon7			-0.020	0.000
scan_mon8			-0.012	0.000
scan_mon9			-0.018	0.000
scan_mon10			-0.018	0.000
scan_mon11			-0.014	0.000
scan_mon12			-0.009	0.100
constant	0.048	0.000	0.048	0.000
Year Fixed	Yes		Yes	
Fund Fixed	Yes		Yes	
Adj R2	0.116		0.116	
N	297749		297749	

Table 3

Panel B

For each variable below, the regression model was re-estimated with the individual scandal month (ranging from 1 to 12) x the variable. Instead of scandal, for example, the model included scandal x mon1, scandal x mon2, ..., scandal x mon12. The p-value is from a F-test of the null hypothesis that the sum of the coefficients on the 12 individual month variables are equal to zero (the Zero Hypothesis).

12 Month Variable	**Model with Scandal Dummy p-value**	**Model with Individual Scandal Month Dummies p-value**
scandal	N/A	0.000

Table 4: Importance of Severity of the Scandal: Direct Proxies

Panel A

WSJ Articles is defined as the number of WSJ 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 amount and the group of funds equal to or below the median. Mean flow comparison also between scandal funds that faced a formal governmental charge (Charges = 1) and those that did not (Charges = 0).

	Mean	**Median**	**Mean flow if greater than median**	**Mean flow if less than or equal to median**	**p-value**
WSJ Articles	10.1	8.0	-0.022	-0.016	0.005
Settlement Amt	173.0	110.0	-0.021	-0.017	0.080
	Mean	**Median**	**Mean flow if Charges = 1**	**Mean flow if Charges = 0**	**p-value**
Charges	0.9	1.0	-0.020	0.004	0.000

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 Amount. For Charges, p-value is from a two-sided t-test of the difference in mean flow where Charges = 1 versus Charges = 0.

Table 4

Panel B

Dependent variable is monthly flow. The p-values are calculated using Huber-White robust standard errors. Big scandal is defined as 1 if the scandal settled (or resulted in a fine) for an amount greater than the median settlement amount. Featured is defined as 1 if the number of WSJ articles relating to the scandal was greater than the median number. Enforced is defined as 1 if formal charges were filed by the SEC or other governmental entity relating to the scandal.

Variable	Model 1	p-value	Model 2	p-value	Model 3	p-value
pxret	0.079	0.000	0.079	0.000	0.079	0.000
nxret	0.051	0.000	0.051	0.000	0.051	0.000
objflow	0.005	0.038	0.005	0.038	0.005	0.038
stdraw	-0.025	0.004	-0.026	0.003	-0.026	0.004
lnptna	-0.007	0.000	-0.007	0.000	-0.007	0.000
scandal	-0.013	0.000	-0.013	0.000	0.009	0.005
scandal x big scandal	-0.010	0.000				
scandal x featured			-0.010	0.000		
scandal x enforced					-0.028	0.000
constant	0.048	0.000	0.048	0.000	0.048	0.000
Year Fixed Effects	Yes		Yes		Yes	
Fund Fixed Effects	Yes		Yes		Yes	
Adj R2	0.116		0.116		0.116	
N	297749		297749		297749	

Table 4

Panel C

For each scandal, the individual months in the scandal period is assigned a number from 1 to 12. For each variable below, the regression model was re-estimated with the individual scandal month (ranging from 1 to 12) x the variable. Instead of scandal, for example, the model included scandal x mon1, scandal x mon2, ..., scandal x mon12. The p-value is from a F-test of the null hypothesis that the sum of the coefficients on the 12 individual month variables are equal to zero (the Zero Hypothesis).

12 Month Variable	**Model 1 p-value**	**Model 2 p-value**	**Model 3 p-value**
scandal	0.000	0.000	0.008
scandal x big scandal	0.000		
scandal x featured		0.000	
scandal x enforced			0.000

Table 5: Family Wide Impact

Panel A: Comparison of Flow for Scandal Family v. Non-Scandal Family

Scandal Family months are months where the first announcement of a scandal for a fund with the same management company is within the time period from -11 months to +0 months. Months from funds directly involved in the scandal (Scandal) are excluded from designation as Scandal Family months. Non-Scandal Family months are all other fund-months (non Scandal Family and non Scandal months).

	n	**Non-Scandal Family**	**n**	**Scandal Family**	**p-value**
flow	284435	0.001	11870	-0.006	0.000

p-value is from a two-sided t-test of difference between the mean for non-scandal family and scandal family months.

Table 5

Panel B
Dependent variable is monthly flow. The p-values are calculated using Huber-White robust standard errors.

Variable	Model 1	p-value	Model 2	p-value	Model 3	p-value	Model 4	p-value
pxret	0.079	0.000	0.079	0.000	0.079	0.000	0.079	0.000
nxret	0.051	0.000	0.050	0.000	0.051	0.000	0.051	0.000
objflow	0.005	0.038	0.005	0.038	0.005	0.038	0.005	0.038
stdraw	-0.027	0.002	-0.029	0.000	-0.028	0.001	-0.027	0.002
lnptna	-0.007	0.000	-0.007	0.000	-0.007	0.000	-0.007	0.000
scandal	-0.018	0.000	-0.014	0.000	-0.013	0.000	0.008	0.007
scandal x big scandal			-0.011	0.000				
scandal x featured					-0.011	0.000		
scandal x enforced							-0.028	0.000
scandal family	-0.006	0.000	-0.004	0.000	-0.005	0.000	-0.004	0.000
scandal family x big scandal			-0.009	0.000				
scandal family x featured					-0.003	0.000		
scandal family x enforced							-0.002	0.080
constant	0.048	0.000	0.048	0.000	0.048	0.000	0.048	0.000
Year Fixed Effects	Yes		Yes		Yes		Yes	
Fund Fixed Effects	Yes		Yes		Yes		Yes	
Adj R2	0.116		0.116		0.116		0.116	
N	297749		297749		297749		297749	

Table 5

Panel C
For each scandal, the individual months in the scandal period is assigned a number from 1 to 12. For each variable below, the regression model was re-estimated with the individual scandal month (ranging from 1 to 12) x the variable. Instead of scandal, for example, the model included scandal x mon1, scandal x mon2, ..., scandal x mon12. The p-value is from a F-test of the null hypothesis that the sum of the coefficients on the 12 individual month variables are equal to zero (the Zero Hypothesis).

12 Month Variable	**Model 1 p-value**	**Model 2 p-value**	**Model 3 p-value**	**Model 4 p-value**
scandal	0.000	0.000	0.000	0.013
scandal x big scandal		0.000		
scandal x featured			0.000	
scandal x enforced				0.000
scandal family	0.000	0.000	0.000	0.000
scandal family x big scandal		0.000		
scandal family x featured			0.000	
scandal family x enforced				0.162

Table 6: Importance of the Type of Scandal

Panel A

Scandal Type	***n***	***Mean Flow***	***Median***	***p-value***
Market Timing	1117	-0.021	-0.017	0.000
-International Fund	229	-0.026	-0.018	0.000
-Domestic Fund	888	-0.020	-0.017	0.000
Misleading Disclosure	141	-0.031	-0.019	0.000
Other	233	0.002	0.002	0.393

p-value is from a two-sided t-test of difference between the mean flow and zero.

Table 6

Panel B
Dependent variable is monthly flow. The p-values are calculated using Huber-White robust standard errors. We grouped each scandal in one of four categories: scandals involving late trading or market timing (Timing); scandals involving false or misleading disclosures (Disclosure); and scandals involving embezzlement, theft, improper trades, manipulation, and IPO allocations (Other). In Model 2, we include an interaction term between Timing x International Fund to separate the market response to market timing (a primarily international fund related scandal) from late trading scandals.

Variable	Model 1	p-value	Model 2	p-value
pxret	0.079	0.000	0.079	0.000
nxret	0.051	0.000	0.051	0.000
objflow	0.005	0.038	0.005	0.038
stdraw	-0.026	0.003	-0.026	0.003
lnptna	-0.007	0.000	-0.007	0.000
timing	-0.021	0.000	-0.019	0.000
timing x intl			-0.010	0.005
disclosure	-0.017	0.001	-0.017	0.001
other	-0.000	0.874	-0.000	0.887
constant	0.048	0.000	0.048	0.000
Year Fixed Effects	Yes		Yes	
Fund Fixed Effects	Yes		Yes	
Adj R2	0.116		0.116	
N	297749		297749	

Table 6

Panel C

For each scandal, the individual months in the scandal period is assigned a number from 1 to 12. For each variable below, the regression model was re-estimated with the individual scandal month (ranging from 1 to 12) x the variable. Instead of scandal, for example, the model included scandal x mon1, scandal x mon2, ..., scandal x mon12. The p-value is from a F-test of the null hypothesis that the sum of the coefficients on the 12 individual month variables are equal to zero (the Zero Hypothesis).

12 Month Variable	**Model 1 p-value**	**Model 2 p-value**
timing	0.000	0.000
timing x intl		0.003
disclosure	0.001	0.001
other	0.671	0.681

Table 6

Panel D

Dependent variable is monthly flow. The p-values are calculated using Huber-White robust standard errors. We grouped scandals by type (timing, disclosure, and Other). To test the importance of more severe scandals solely for timing scandals, we included interaction terms between timing x big timing, timing x timing_featured, and timing x enforcement. We define 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 define tim_featured as equal to 1 if the timing scandal involved greater than the median number of WSJ articles for the group of all timing scandals (10 articles).

Variable	Model 1	p-value	Model 2	p-value	Model 3	p-value
pxret	0.079	0.000	0.079	0.000	0.079	0.000
nxret	0.051	0.000	0.051	0.000	0.051	0.000
objflow	0.005	0.038	0.005	0.038	0.005	0.038
stdraw	-0.026	0.003[c]	-0.026	0.003	-0.026	0.003
lnptna	-0.007	0.000	-0.007	0.000	-0.007	0.000
timing	-0.019	0.000	-0.016	0.000	-0.011	0.003
timing x big timing	-0.005	0.086				
timing x tim_featured			-0.010	0.001		
timing x enforced					-0.011	0.006
disclosure	-0.017	0.001	-0.017	0.001	-0.017	0.001
other	-0.001	0.861	-0.000	0.882	-0.000	0.872
constant	0.048	0.000	0.048	0.000	0.048	0.000
Year	Yes		Yes		Yes	
Fund	Yes		Yes		Yes	
Adj R2	0.116		0.116		0.116	
N	297749		297749		297749	

Table 7: Scandals that Affect Fund Investors Continuing with the Fund

Panel A

We use two proxies for whether a scandal will affect investors that continue with the fund. First, scandals where a penalty is assessed on an entity (e.g., the fund management company) may affect investors that stay with the fund. Second, scandals where the fund investors were harmed may lead investors to fear continued harm if they stay with the fund. The table provides a comparison of the flow for fund months involving a scandal where a penalty was assessed on an entity (pen_entity = 1) versus where a penalty was not assessed on an entity and where fund investors were harmed (harm = 1) versus where fund investors were not harmed (harm = 0)

	n	**pen_entity = 1**	**n**	**pen_entity = 0**	**p-value**
flow	1321	-0.021	170	0.000	0.000
	n	**harm = 1**	**n**	**harm = 0**	**p-value**
flow	1321	-0.020	170	0.005	0.000

p-value is from a two-sided t-test of difference between the mean flow for the penalty on an entity and penalty on an individual fund months with a scandal.

Table 7

Panel B

Dependent variable is monthly flow. The p-values are calculated using Huber-White robust standard errors. We define pen_entity as equal to 1 if the fund scandal results in a penalty imposed on an entity. We define no_pen_entity as equal to 1 if the fund scandal does not result in a penalty on an entity.

Variable	**Model 1**	**p-value**	**Model 2**	**p-value**
pxret	0.079	0.000	0.079	0.000
nxret	0.051	0.000	0.051	0.000
objflow	0.005	0.038	0.005	0.038
stdraw	-0.025	0.004	-0.025	0.004
lnptna	-0.007	0.000	-0.007	0.000
scandal x pen_entity	-0.019	0.000		
scandal x no_pen_entity	-0.002	0.241		
scandal x harm			-0.018	0.000
scandal x no_harm			0.003	0.674
constant	0.048	0.000	0.048	0.000
Year Fixed	Yes		Yes	
Fund Fixed	Yes		Yes	
Adj R2	0.116		0.116	
N	297749		297749	

Table 7

Panel C

For each scandal, the individual months in the scandal period is assigned a number from 1 to 12. For each variable below, the regression model was re-estimated with the individual scandal month (ranging from 1 to 12) x the variable. Instead of scandal, for example, the model included scandal x mon1, scandal x mon2, ..., scandal x mon12. The p-value is from a F-test of the null hypothesis that the sum of the coefficients on the 12 individual month variables are equal to zero (the Zero Hypothesis).

12 Month Variable	**Model 1 p-value**	**Model 2 p-value**
scandal x pen_entity	0.000	
scandal x no_pen_entity	0.081	
scandal x harm		0.000
scandal x no_harm		0.657

Table 7

Panel D: Fraction of scandal months involving a particular type of scandal (Timing, Disclosure, and Other) where a penalty was assessed on an entity or on an individual.

Scandal Type	**Penalty on Entity**	**Harm to fund investors**
Market Timing	0.968	1.000
Misleading Disclosure	0.936	1.000
Other	0.464	0.691

Table 7

Panel E
Dependent variable is monthly flow. The p-values are calculated using Huber-White robust standard errors. We define pen_entity as equal to 1 if the fund scandal results in a penalty imposed on an entity. We define no_pen_entity as equal to 1 if the fund scandal does not result in a penalty on an entity. We define harm as equal to 1 if the fund scandal results in harm directly to fund investors. We define no_harm as equal to 1 if the fund scandal does not result in harm directly to fund investors.

Variable	Model 1	p-value	Model 2	p-value
pxret	0.079	0.000	0.079	0.000
nxret	0.051	0.000	0.051	0.000
objflow	0.005	0.038	0.005	0.038
stdraw	-0.026	0.003	-0.026	0.003
lnptna	-0.007	0.000	-0.007	0.000
scandal x harm x pen_entity	-0.021	0.000		
timing x harm x pen_entity			-0.021	0.000
disclosure x harm x pen_entity			-0.018	0.002
other x harm x pen_entity			-0.013	0.089
scandal x harm x no_pen_entity	-0.002	0.236	-0.003	0.234
scandal x no_harm	0.003	0.684	0.003	0.687
constant	0.048	0.000	0.048	0.000
Year Fixed	Yes		Yes	
Fund Fixed	Yes		Yes	
Adj R2	0.116		0.116	
N	297749		297749	

Table 8: Market v. Regulation: Importance of Who Discovers Scandal First

Panel A

Nongov is defined as any non-governmental initial public source for the scandal. Othergov is defined as any non-SEC governmental as the initial public source for the scandal. Secfirst is defined as the SEC as the initial public source for the scandal.

	n	percent	flow	p-value
Nongov	272	18.4	-0.025	0.000
Othergov	926	62.6	-0.022	0.000
Secfirst	281	19.0	0.001	0.742

p-value is from a two-sided t-test of difference with zero.

Table 8

Panel B
Dependent variable is monthly flow. The p-values are calculated using Huber-White robust standard errors.

Variable	Model 1	p-value	Model 2	p-value
pxret	0.079	0.000	0.079	0.000
nxret	0.051	0.000	0.051	0.000
objflow	0.005	0.038	0.005	0.038
stdraw	-0.026	0.003	-0.026	0.003
lnptna	-0.007	0.000	-0.007	0.000
scandal x nongov	-0.024	0.000	-0.028	0.000
scandal x othergov	-0.021	0.000	0.035	0.001
scandal x secfirst	0.002	0.303	0.005	0.043
scandal x timing x nongov			0.009	0.131
scandal x timing x othergov			-0.058	0.000
scandal x timing x secfirst			-0.010	0.090
constant	0.047	0.000	0.048	0.000
Year Fixed Effects Eff	Yes		Yes	
Fund Fixed Effects	Yes		Yes	
Adj R2	0.116		0.116	
N	297749		297749	

Table 8

Panel C
For each scandal, the individual months in the scandal period is assigned a number from 1 to 12. For each variable below, the regression model was re-estimated with the individual scandal month (ranging from 1 to 12) x the variable. Instead of scandal, for example, the model included scandal x mon1, scandal x mon2, ..., scandal x mon12. The p-value is from a F-test of the null hypothesis that the sum of the coefficients on the 12 individual month variables are equal to zero (the Zero Hypothesis).

12 Month Variable	**Model 1 p-value**	**Model 2 p-value**
scandal x nongov	0.000	0.000
scandal x othergov	0.000	0.000
scandal x secfirst	0.509	0.262
scandal x timing x nongov		0.113
scandal x timing x othergov		0.000
scandal x timing x secfirst		0.858

Table 8

Panel D

	n	Non-SEC discovered	n	SEC Discovered	p-value
No Harm to Fund Investors	1210	0.050	281	0.043	0.628
No_Penalty on Entity	1228	0.069	302	0.306	0.000

Table 9: Market v. Regulation: Importance of Corporate Governance

Panel A

Indepchair is defined as 1 if the fund has an independent chair and 0 otherwise. Indep34 is defined as 1 if the fund's board of director consists of at least 3/4 independent directors.

	n	***Mean***
Indepchair	1491	0.191
Indep34	1491	0.800

Indepchair	**n**	**Indepchair = 0**	**n**	**Indepchair = 1**	**p-value**
flow	1206	-0.018	285	-0.022	0.111
Indep34	**n**	**Indep34 = 0**	**n**	**Indep34 = 1**	**p-value**
flow	298	-0.020	1193	-0.018	0.466

p-value is from a two-sided t-test of difference between the means.

Table 9

Panel B
Dependent variable is monthly flow. The p-values are calculated using Huber-White robust standard errors.

Variable	Model 1	p-value	Model 2	p-value	Model 3	p-value
pxret	0.079	0.000	0.079	0.000	0.079	0.000
nxret	0.051	0.000	0.051	0.000	0.051	0.000
objflow	0.005	0.038	0.005	0.038	0.005	0.038
stdraw	-0.025	0.004	-0.025	0.004	-0.025	0.004
lnptna	-0.007	0.000	-0.007	0.000	-0.007	0.000
scandal	-0.016	0.000	-0.014	0.000[b]	-0.013	0.000[b]
scandal x indepchair	-0.004	0.218			-0.004	0.310
scandal x indep34			-0.005	0.183	-0.004	0.256
constant	0.048	0.000	0.048	0.000	0.048	0.000
Year Fixed Effects	Yes		Yes		Yes	
Fund Fixed Effects	Yes		Yes		Yes	
Adj R2	0.116		0.116		0.116	
N	297749		297749		297749	

Table 9

Panel C
For each scandal, the individual months in the scandal period is assigned a number from 1 to 12. For each variable below, the regression model was re-estimated with the individual scandal month (ranging from 1 to 12) x the variable. Instead of scandal, for example, the model included scandal x mon1, scandal x mon2, ..., scandal x mon12. The p-value is from a F-test of the null hypothesis that the sum of the coefficients on the 12 individual month variables are equal to zero (the Zero Hypothesis).

	Model 1 p-value	**Model 2 p-value**	**Model 3 p-value**
Scandal Month	0.000	0.000	0.000
Scandal Month x Indepchair	0.176		0.270
Scandal Month x Indep34		0.173	0.234

Table 10: Market Penalty v. Government Penalty

Market Penalty is equal to the (flow for the 12 months period from the first announcement of the scandal – average flow for funds in the same SI objective group for the same time period) multiplied by the total net assets of the fund for the month before the first announcement of the scandal multiplied by -1. Allocated Settlement or Fine Amount is the settlement or fine amount for the scandal multiplied by the ratio of the total net assets for the first month in the scandal period for the fund over the total net assets for all funds in the same fund family involved in the scandal. Market Penalty for the fund family is the market penalty aggregated for all members of the fund family. Big scandal is defined as 1 if the scandal settled for an amount greater than the median settlement amount. Featured is defined as 1 if the number of WSJ articles relating to the scandal was greater than the median number. Enforced is defined as 1 if formal charges were filed by the SEC or other governmental entity relating to the scandal.

	n	**Allocated Settlement or Fine Amount (mill)**	**One Year Adjusted Asset Outflow**	***Expenses***	**One Year Market Penalty (mill)**	**p-value**
Overall for Scandal Funds	133	22.1	271.6	0.014	3.7	0.000
Breakdown by Severity						
Big Scandal	72	37.5	455.8	0.014	5.5	0.000
Featured	54	37.1	392.7	0.015	5.4	0.000
Enforced	122	23.7	293.8	0.014	3.6	0.000
	n	**Settlement or Fine Amount (mill)**	**One Year Adjusted Asset Outflow**	**Expenses**	**One Year Market Penalty (mill)**	**p-value**
Fund Family	33	96.9	461.4	0.012	32.3	0.036

p-value is from a two-sided t-test of difference between settlement amount and the market penalty.

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