The Preventive Effect of Hedge Fund Activism*

(Job Market Paper)

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Abstract

This paper examines the effectiveness of hedge fund activism in preventing corporate policy deviations. Whereas previous studies focus on policy changes in target firms after intervention, I examine proactive policy changes in all firms that face a threat of intervention. Using mutual fund fire sales as an instrument, I find that an increase in intervention likelihood leads to increases in shareholder distribution as well as decreases in CEO pay, cash, and investments. Given the reduction in managerial rent seeking, cash hoarding and empire building behaviors, it is unsurprising that operating performance as measured by ROA also improves significantly. The relationships are causal, significant, and robust to a variety of alternative model specifications and sample divisions. The results suggest the existence of a preventive effect of hedge fund activism as well as a stronger and broader impact of hedge fund activism on corporate policy than previously documented.

JEL Classification: G23, G34

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1 Introduction

As a mechanism for corporate governance, hedge fund activism should to some extent prevent policy deviations as well as correct them should they occur. The preventive effect is arguably more important as complete prevention obviates the need for correction but not vice versa. Some studies on hedge fund activism document significant post-intervention policy changes in target firms, suggesting the existence of a corrective effect. Surprisingly, however, the literature has been silent on whether hedge fund activism has a preventive effect on policy deviations as well.

In this study, I examine the preventive effect of hedge fund activism on corporate policy deviations. Previous studies on hedge fund activism generally center on the effect of intervention on shareholder wealth. Policy response is usually a secondary concern, examined only as a potential explanation for the abnormal stock returns observed around the announcement of intervention. Consequently, only post-intervention policy changes are directly examined. This narrow focus misses the preventive effect and leads to an underestimation of the overall impact of hedge fund activism on corporate policy.

To implement this study, I use a comprehensive hand-collected sample of hedge fund activism events from 1994 to 2007. I merge this sample of hedge fund interventions with three types of Compustat firms: (1) target firms, including all firms targeted at least once during the sample period; (2) match firms, including all firms never targeted during the sample period but share a set of similar characteristics with target firms; and (3) other firms, including the rest of the firms in the same industries as target firms but do not qualify as match firms.

The central task is to examine how corporate policy responds to the likelihood of hedge fund activism. This is accomplished in two steps. First, I estimate the likelihood of intervention for all firms in a probit regression, in which the dependent variable is a dummy variable valued at one if the firm is targeted for intervention and zero otherwise. In the second step, policy variables are regressed against the likelihood of intervention estimated in the first step, with one key independent variable in the probit regression, an exogenous proxy for the likelihood of intervention, excluded to guarantee the regression relationship has a causal interpretation. The corporate policies examined include: (1) financial policy, characterized by cash holdings and the leverage ratio, (2) investment policy, characterized by capital expenditures and R&D expenses, (3) compensation policy, characterized by CEO pay and CEO turnover, and (4) distribution policy, characterized by the payout ratio.

A major challenge in this investigation is identifying the causal relationship between policy changes and the likelihood of hedge fund activism. This is difficult because the likelihood of intervention is an endogenous regressor in the policy regressions. The endogeneity can arise from three sources. The first source is the reverse causality from policy variables to the likelihood of intervention. An increase in the likelihood of intervention reduces policy deviation. A reduction in policy deviation in turn reduces the likelihood of intervention. The exogenous effect of the likelihood of intervention on a policy variable is weakened by the feedback from the policy variable, making it hard to detect.

The second source of endogeneity is omitted variables correlated with both policy variables and the likelihood of intervention. For example, a change that weakens the effectiveness of the board of directors increases policy deviations as well as the likelihood of intervention, thereby attenuating the negative relationship between the two variables. I address these two concerns by instrumenting for the likelihood of intervention with mutual fund fire sales as constructed by Edmans, Goldstein and Jiang (2012). This variable is correlated with the likelihood of intervention and affects corporate policies only indirectly through its effect on the likelihood of intervention, thereby satisfying both the relevance and exclusion criteria of an instrumental variable.

Another potential source of endogeneity is measurement error. It arises here because the likelihood of intervention is a latent variable and must therefore be estimated. Relative to reverse causality and omitted variables, this problem is less of a concern in this study given the large sample size. Even though the likelihood of intervention can be estimated with error, there is no obvious reason to suspect that the estimation errors are systematically biased upward or downward. Assuming estimation errors are random, upward errors and downward errors tend to cancel each other out in a large sample and therefore have negligible effect on regression results. Nevertheless, standard errors are adjusted to allow for correct statistical inference.

Results from the two-step procedure show that the likelihood of intervention has significant effects on corporate policies. In most policy regressions, the estimated coefficients for the likelihood of intervention have the expected signs and are significant both statistically and economically. In response to a one standard deviation increase in the likelihood of intervention, CEO pay decreases by 7.42% from its sample average, suggesting material improvement in governance by reducing managerial rent-seeking. This point is further strengthened by the response in the distribution policy. A one standard deviation increase in the likelihood of intervention raises the payout ratio by 9.29% from its sample average, suggesting a transfer of wealth from managers to shareholders. This concurs with Brav et al (2008) in which the authors also find a wealth reallocation from corporate executives to shareholders. In their study, however, the wealth reallocation occurs only after actual intervention. Here, the reallocation occurs before or even without actual intervention, consistent with the existence of a preventive effect of hedge fund activism on policy deviations. Positive responses are also observed for investment policy. A one standard deviation increase in the likelihood of intervention cuts capital expenditures by 2.67% from its sample average and R&D expenses by 4.96% from its sample average. This is consistent with the threat of intervention curbing managerial empire building behavior.

The change in financial policy is somewhat complicated. In response to a one standard deviation increase in the likelihood of intervention, cash stock decreases by 2.61% from its sample average, whereas the leverage ratio is simultaneously reduced by 3.20% from its sample average. While the decrease in cash stock is consistent with a mitigation of agency problem, the simultaneous decrease in the leverage ratio seems to suggest the opposite given that under-leverage is identified in many theoretical and empirical studies as a symptom of agency problems. Further analysis reveals that this is not the case.

Under-leverage is not a typical problem for target firms in this sample. In fact, target firms on average have a slightly higher leverage than both match firms and other firms. Therefore, the decrease in the leverage ratio is not surprising. It is also consistent with previous findings that sometimes shareholders intervene on the grounds that mangers over-levered their firms. More importantly, the decrease in the leverage ratio suggests that hedge fund activists increase shareholder distribution mainly through cutting slack rather than through leveraging at the expense of bondholders. This is inconsistent with Burkart and Dasgupta (2012), in which the authors provide a theoretical model to reconcile the empirical inconsistency concerning bondholder exploitation. Their model suggests that hedge fund activists target firms with low existing leverage ratios and push them to lever up as a means of increasing shareholder distribution. Given all these considerations, I interpret the decrease in leverage as an improvement in financial policy. As a result of overall improvements in compensation, distribution, investment, as well as financial policies, accounting performance as measured by return on assets also improves.

There is a potential concern with instrumenting the likelihood of hedge fund activism with mutual fund fire sales, which facilitate not only hedge fund interventions but also corporate takeovers through their impact on stock prices. For example, Edmans et al (2012) document a positive relationship between mutual fund fire sales and the likelihood of takeover bids. In the present context, there is a possibility that firms are actually responding to the likelihood of takeover rather than that of intervention. This concern is consistent with Cyert, Kang, and Kumar (2002) in which the authors find that takeover threats are effective in constraining CEO compensation.

To isolate the preventive effect of hedge fund activism from that of corporate takeover, I collect a sample of takeover bids over the same sample period of 1994 to 2007. I cross-reference this sample with the sample of hedge fund activism events to distinguish between pure hedge fund activism events and dual events in which a firm is simultaneously targeted for intervention and takeover. By applying a similar two-stage instrumental variable regression procedure to separate samples of pure hedge fund activism events and pure takeover events, I find proactive policy and performance improvements in both samples, suggesting that the preventive effect of hedge fund activism is not a masked preventive effect of takeover. Rather, it is a standalone channel through which financial markets can discipline managers by increasing the likelihood of intervention by hedge fund activists. My results also suggest takeover likelihood as another standalone channel through which financial markets can impact corporate policy and performance. Interestingly, from the first stage likelihood estimations, I find that mutual fund fire sales correlate more strongly with the likelihood of (pure) intervention than the likelihood of (pure) takeover, suggesting that they better instrument for the former.

In summary, all corporate policies examined are at least partially proactive to hedge fund activism. This conclusion is applicable to target firms, match firms, as well as other firms. The results are qualitatively similar after eliminating the potential confounding effect of simultaneous takeover attempts. They also demonstrate robustness to various other model specifications and sample divisions.

This study contributes to the existing literature in several ways. First, it shows that as a mechanism for corporate governance, the threat of hedge fund activism is effective in preventing corporate policy deviations. Previous studies generally focus on reactive policy responses subsequent to actual interventions. Proactive responses are rarely examined and as such the impact of hedge fund activism is assessed without considering its preventive effect. Brav et al (2008), Greenwood and Schor (2009), and Klein and Zur (2009), Clifford (2008), and Huang (2009) all fit into this category. An exception to this general pattern is Fos (2011), in which the author examines proactive policy response to a shareholder activism event. But the focus of that paper is on an activism tactic, the event being proxy contests, whereas the focus of this paper is on a particular type of activist, hedge funds, who may and do choose to employ a variety of tactics from friendly communication to proxy contests.

Second, this study provides evidence that hedge fund activism affects not only target firms but also non-target firms, whether or not they are similar to target firms. This finding is important given the small incidence of hedge fund interventions. Although a broader effect of hedge fund activism is frequently speculated and there are numerous anecdotal stories supportive of this speculation, previous studies for the most part focus only on target firms. Non-target firms are included in those studies only as the benchmark against which policy responses of target firms are measured. In contrast, the policy response of non-target firms is a central concern in this study, the results of which suggest that, as a mechanism for corporate governance, hedge fund activism indeed has a broader impact than previously documented.

Third, this paper's findings suggest that the effect of hedge fund activism on target firms is also greater than previously documented. To gauge the magnitude of policy changes attributable to intervention, previous studies typically use two simple comparisons: be-and-after intervention comparisons and target-and-match firm comparisons. However, if firms proactively self-correct policy deviation when they perceive an increasing likelihood of intervention but before intervention actually materializes, then benchmarking post-intervention policy against policy immediate before or at the time of intervention would lead to an underestimation of the full impact of intervention on target firms. Similarly, if match firms, those that share similar characteristics as target firm, themselves face a high likelihood of becoming the next targets and adjust their policies to lower that likelihood, then benchmarking target firm policy against match firm policy would also result in an underestimation of the impact of intervention on target firms.

Fourth, this study provides at least a partial explanation for the differences in the existing literature concerning the policy responsiveness of target firms. Studies on hedge fund activism consistently find positive abnormal stock returns around the announcement of intervention, but not all studies link the incremental share value to subsequent policy improvements. For example, in Greenwood and Schor (2009), the abnormal stock return is attributed to a higher likelihood of acquisition following intervention. In Sunder, Sunder, and Wongsunwai (2010), Li and Xu (2010), and Klein and Zur (2011), it is simply a result of wealth redistribution at the expense of bondholders. But in the seminal study of Brav et al (2008) and Brav, Jiang, and Kim (2010), the abnormal stock returns are at least partially attributed to policy improvements in target firms following intervention.

Although the different findings are not necessarily exclusive as an explanation for the incremental share value, they have very different implications for the effectiveness of hedge fund activism as a mechanism for corporate governance. These differences across studies are likely when corporate policy is proactive, in which case the extent of post-intervention policy changes depends on the degree to which interventions are anticipated. Post-intervention changes should be small for anticipated events and big for unanticipated events. Therefore, we should control for the ex-ante likelihood of intervention when detecting policy changes in the post-intervention period. If the distribution of anticipated and unanticipated events varies across samples and the likelihood of intervention is not controlled for, empirical results on post-intervention policy changes may differ. This potentially explains why some previous studies find significant post-intervention policy changes while others do not.

Finally, this study also contributes to the long-standing debate on the real effects of financial markets. Edmans et al (2012) document a significant relationship between market valuation and takeover bids, suggesting an effect of financial markets on real corporate actions. However, earlier studies, including Palepu (1986) and Ambrose and Megginson (1992), do not find such a link. The present study extends Edmans et al (2012) by showing that financial markets can affect corporate policies, not only through its impact on the likelihood of corporate takeovers but also through its impact on the likelihood of hedge fund interventions.

The rest of the paper is organized as follows: Section 2 describes the empirical methodology, including model specification, identification and data, and shows preliminary evidence for the preventive effect of hedge fund activism. Section 3 presents the main results on the causal relationship between the likelihood of intervention and corporate policies. Section 4 performs additional analyses showing the robustness of the results to alternative model specifications and sample divisions. Section 5 concludes.

2 Methodology

2.1 Identification and Model Specification

If hedge fund activism has a preventive effect on policy deviations, firms must at least be partially proactive. For a target firm, this means at least some policy changes should be observed before intervention. But a pre-intervention policy adjustment is not necessarily a proactive response unless it is aimed at preventing a potential intervention from materializing. For a non-target firm, a policy adjustment can be viewed as a proactive response whether it occurs before or after other firms are caught in an actual intervention, as long as the adjustment is initiated to reduce the chance of becoming a future target itself. Therefore, the key to detecting the preventive effect is not just looking for pre-intervention policy changes. A simple regression of policy against a future event dummy does not do the job. Instead, the key is to determine whether the observed policy changes are driven by the likelihood of intervention.

Ideally, we would want to directly regress a set of corporate policy variables on the likelihood of intervention. Unfortunately, this is infeasible. The major obstacle is the endogeneity arising from reverse causality. Threat of intervention motivates a firm to adjust its policy toward optimality. The resulting decrease in policy deviation in turn reduces the likelihood of intervention, thereby mitigating the original impact from intervention to policy variables. Endogeneity can also arise from the omission of variables correlated with both policy variables and the likelihood of intervention. For example, an exogenous change that hampers the monitoring role of the board of directors could simultaneously increase the likelihood of intervention and policy deviation, thereby muddling the true relationship between the two.

To address these problems, I instrument for the likelihood of hedge fund activism with mutual fund fire sales, as constructed by Edmans, Goldstein and Jiang (2012). Mutual fund fire sales occur when mutual funds are forced to liquidate their portfolio stocks in order to honor redemption requests by their own investors. The downward price pressure from large mutual fund sales allows hedge fund activists to buy a large equity stake in target firms at a lower price. This directly lowers the cost of intervention for hedge fund activists. Also, large mutual fund sales allow hedge fund activists to hide their purchase of a target firm's stock from public attention, thereby avoid tipping off the public to its planned intervention. This further decreases the cost of intervention. Therefore, large mutual fund sales increase the likelihood of hedge fund intervention and thereby meet the relevance criterion of an instrument variable. Gantchev and Jotikasthira (2012) document empirical evidence that hedge fund activists buy stocks from institutional investors when the latter liquidate their positions for liquidity reasons.

Different from Coval and Stafford (2007) who calculate actual mutual fund fire sales to examine the effect of market liquidity on asset price, Edmans et al (2012) construct "mechanical" mutual fund fire sales under the assumption that liquidity-strapped mutual funds dispose all portfolio stocks in proportion to their portfolio weights. The mechanical component ensures that the sales are not information-based thereby satisfying the exclusion criterion of an instrumental variable.

Specifically, mutual fund fire sales are constructed as follows:

$$MFFS_{i,t} = \frac{\sum_{j=1}^{m} MFOF_{j,t} \times \frac{SHARES_{i,j,t-1} \times PRC_{i,t-1}}{TA_{j,t-1}}}{VOL_{i,t}} ,$$

with

$$\frac{MFOF_{j,t}}{TA_{j,t-1}} > 5\% ,$$

where subscripts i, j, and t index firms, mutual funds and quarters, respectively. $MFOF$, mutual fund outflows, are extreme outflows that account for at least five percent of the total assets of the fund. $MFFS$ is mechanical in that mutual fund j's liquidation of stock i is based on stock i's weight in fund j's portfolio rather than on stock i's performance. Since the effect of a sale on a stock's price depends on the stock's liquidity, the summation across sales of stock i by all liquidity-strapped mutual funds is deflated by stock i's total trading volume.

Although mutual fund fire sales as constructed above is based on hypothetically proportional rather than actual liquidation of portfolio stocks, which reduces its correlation with stock price to some extent, this may not be a serious problem given the evidence in Coval and Stafford (2007) that actual mutual fund fire sales are in fact highly proportional to portfolio weights. This is intuitive because mutual funds must maintain a well diversified portfolio. By using the mechanical mutual fund fire sales, Edmans, Goldstein and Jiang (2012) make a small concession in terms of the relevance criterion for a big gain in terms of the exclusion criterion. Although actual mutual fund fire sales are primarily liquidity rather than information driven, mechanical mutual fund fire sales further reduces the role of information about the stock involved in the sale. Therefore, the direct link between mutual fund fire sales and firm policies is even weaker when mutual fund fire sales are calculated "mechanically". This makes it more desirable in terms of the exclusion criterion. In the next section, I report formal test results concerning the qualification of mutual fund fire sales as an instrument for the likelihood of hedge fund activism.

A potential problem in instrumenting for the likelihood of hedge fund activism with $MFFS$ is that $MFFS$ facilitates not only hedge fund interventions but also corporate takeovers, and the likelihood of both can potentially affect corporate policies. That is, $MFFS$ is a proxy for both the likelihood of corporate takeover as well as the likelihood of hedge fund activism, which is problematic given the purpose of this study is to examine the single effect of the threat of hedge fund activism on corporate policy. This potential complication, however, does not affect the eligibility of $MFFS$ as an instrument for the likelihood of hedge fund activism, as the likelihoods of the two types of events are easily separable. In the next section, after obtaining the main results on the preventive effect of hedge fund activism, I isolate the likelihood of hedge fund activism from that of corporate takeover and repeat the analysis.

With a valid instrumental variable for the likelihood of intervention, I proceed to investigate the preventive effect of hedge fund activism on corporate policy using a two-stage instrumental variable regression approach. In the first-stage, I estimate the likelihood of hedge fund activism using lagged $MFFS$ as the instrument:

$$\text{1st-stage: } HFA_{i,t} = \alpha_{10} + \alpha_{11} MFFS_{i,t-1} + \alpha_{12} X_{i,t-1} + IYFE + \epsilon_{1i,t} \ , \tag{1}$$

where HFA_{it} is an indicator variable equaling 1 if firm i is targeted for intervention in year t and 0 otherwise. $MFFS_{it-1}$ is lagged mutual fund fire sales, which affect the current likelihood of hedge fund activism directly but do not directly affect firm policies. The fitted value of HFA_{it}, $\widehat{HFA_{it}}$, is retained as the estimated likelihood of hedge fund activism. X represents a set of control variables, including natural log of market capitalization, book to market ratio, and sales. I also control for industry fixed effects and year fixed effects, represented altogether by $IYFE$. I estimate Equation (1) using probit as well as linear regressions.

The second stage policy regressions take the following form:

$$\text{2nd-stage: } P_{i,t} = \alpha_{20} + \alpha_{21} \widehat{HFA_{i,t}} + \alpha_{22} X_{i,t-1} + IFE + YFE + \epsilon_{2i,t} \ , \tag{2}$$

where P_{it} is a variable measuring firm i's distribution policy, compensation policy, financial policy, investment policy, or operating performance in year t. $\widehat{HFA_{it}}$ is the predicted value of HFA obtained from the first stage. The control variables in the second stage policy regression in equation (2) are exactly the ones in the first stage likelihood regression in equation (1). This is necessary to ensure that the variation in firm outcome variables comes only from the variation in $MFFS$, the exogenous proxy for the likelihood of hedge fund activism, which establishes the causal relationship between firm policy and performance and the likelihood of hedge fund activism. Results are reported in Section 3.

The policies I examine in this study are distribution policy, compensation policy, financial policy and investment policy. These policies reveal potential agency problems and are amenable to change, ideal for studying the effect of governance mechanisms (See for example, Shleifer and Vishny, 1997; Hartford, Mansi and Maxwell, 2008; and Nini, Smith and Sufi, 2012). The control variables affect both the likelihood of hedge fund activism and the policy variables under examination. The primary control variables used in the main regressions include market capitalization, the book to market ratio, and sales. Market capitalization directly affects the capital requirement in building a meaningful stake in the target firm. A consistent finding in empirical research is that small firms are more likely to be targeted by hedge fund activists. Market valuation is another well-documented predictor of hedge fund activism (Brav et al, 2008; Klein and Zur, 2011; and Bessler, Drobetz and Holler, 2013). Both firm size[1] and market valuation[2] play a role in shaping the corporate policies examined in this study. Sales are included here as a measure of operating performance, which affects the likelihood of hedge fund activism and corporate policies for obvious reasons (Chay and Suh, 2009; Duchin, 2010; Becht, Franks and Rossi, 2010). I also control for industry fixed effects as well as year fixed effects[3].

2.2 Data

The data on hedge fund activism is obtained from Alon Brav, who extends the sample of hedge fund activism events in Brav et al (2008) based on the same procedure. Whereas the original sample covers the period from 2001 to 2006, the extended sample used in this study covers the period from 1994 to 2007. Below is a brief description of the sampling procedure. Readers are referred to Brav et al (2008) for further details.

[1]See for example Larmou and Vefeas (2010), Ai, Kiku and Li (2012), Gabaix, Landier and Sauvagnat (2013).

[2]See for example Fresard (2010), Dennis and Sibilkov (2010), and Wang and Chiu (2012).

[3]As a robustness check, I also use firm fixed effects. The results are largely the same as the main results using industry fixed effects and are therefore not reported for the sake of brevity.

The sampling procedure starts from a complete list of Schedule 13(d) filers. To comply with Section 13(d) of the Securities and Exchange Act of 1934, investors must file a Schedule 13D with the Securities and Exchange Commission (SEC) within 10 days after acquiring 5% or more of a public company's stock with intentions to influence management. The information contained in Schedule 13D includes the filer's identity, the name of the target firm, the ownership percentage held in the target firm, and the purpose of the transaction. After the initial Schedule 13D filing, an investor must also file within 60 days each time there is a material change in its ownership stake or purpose of shareholding.

Using the information in Schedule 13D, aided by news search and telephone requests for filers' self-classification, the authors first compile a list of all hedge fund filers. They then exclude those that filed only one Schedule 13D during the period from 2001 to 2006 without indicating the explicit purpose for filing. This procedure leaves 311 hedge fund activists in the sample. Next the authors further exclude the events where the purpose of the hedge fund is distress financing or risk arbitrage or where the target is a closed-end fund or some other non-regular corporation. This screening leaves 236 hedge fund activists and 1,032 events. Finally, to avoid a small target bias that may arise from sampling based on Schedule 13D, the authors collect a sample of 27 hedge fund activism events where no Schedule 13D is required because the ownership stake of the activists in their target firms do not amount to the threshold of 5%. These events are first collected through news search in Factiva using "hedge fund" and "activism" as key words. Then, using Thomson Financial Form 13F database, only events that meet the following two requirements are retained: (1) market value of the target firm exceeds $1 billion, and (2) the ownership stake of the activist exceeds 2%. After adding these non-Schedule 13D events, the sample includes 236 hedge funds and 1,059 events. The authors track the development and resolution of each hedge fund activism intervention in the sample by searching the news and following the subsequent amendments to the initial Schedule 13D filings (Schedule 13D/As).

The extended data used in this study include all the events in the original data, plus those identified during the period from 1994 to 2000 and in 2007 using the same sampling procedure. The final sample used in this study contains 1,264 interventions launched by 330 hedge funds against 988 target firms during the period from 1994 to 2007. A comparison with the the sample in Brav et al (2008) reveals the lower incidence of hedge fund activism during the 1990s. It increases from 2000 to 2006 and then slightly declines in 2007.

The auxiliary data used in this study come from several sources. Data on firm characteristics are collected from Compustat. CEO compensation data are from Execucomp, which contains compensation information for all executives of Standard & Poor's 1500 firms. The variable in Execucomp used to measure CEO pay is tdc1. Prior to 2006, this variable represents total realized compensation, whereas 2006 and after represents total expected compensation. For consistency, I adjust the pre-2006 values following Walker (2009). I obtain mutual fund fire sales data from Alex Edmans' research webpage. Finally, I collect takeover data from the Securities Data Company.

2.3 Summary Statistics

In this section, I classify all Compustat firms into three groups: (1) target firms, including all firms targeted at least once during the sample period; (2) match firms, including all firms never targeted during the sample period and share similar characteristics with target firms; and (3) other firms, including the rest of the firms in the same industries as target firms. For all firms as well as each group of firms, I calculate summary statistics on a set of firm characteristic variables. A cross-group comparison of these statistics should be informative about why certain firms are targeted by hedge fund activists and how firms should adjust policies to lower the likelihood of being targeted. Table 1 describes these characteristic variables as well as other important variables used in this study.

[Insert Table 1 here]

Panel A of Table 1 shows the definitions of the variables. Panel B shows the summary statistics for all firms in the whole sample during the sample period of 1994 to 2007. Since variables are defined following the standard in the literature, their summary statistics are consistent with those in other studies.

Table 2 characterizes firms in the year before hedge fund intervention. The first three columns report mean values of firm characteristic variables for target firms, match firms, and other firms. The next three columns respectively report the mean difference between target firms and match firms, target firms and other firms, and match firms and other firms. Parenthesized numbers are t-statistics for tests of zero mean difference between relevant groups of firms. Mean values for target firms are obtained by averaging across all target firms in the year before intervention. To obtain mean values for match firms, I first form a group of match firms for each target firm-year observation in the pre-intervention year. All firms that meet the following four requirements are included in the match group: (1) never targeted during the entire sample period; (2) belong to the same industry as the target firm, as defined by the three-digit Standard Industry Classification code; (3) fall into the same market capitalization quartile as the target firm, and (4) fall into the same book to market quartile as the target firm. I then compute an average value for each match group associated with every target- firm year observations. Finally, I obtain the overall mean value for match firms by averaging across these individual match group averages. Mean values for other firms are obtained in a similar manner, where other firms are those that meet requirements (1) and (2) for classification as a match firm but not (3) and (4).

[Insert Table 2 here]

Following the convention in the literature on hedge fund activism, I measure firm size with market capitalization. As mentioned earlier, a consistent empirical finding is that small firms are more likely to be targeted by hedge fund activists. This is because the associated capital requirement for amassing a meaning equity stake for intervention is lower. Table 2 confirms this empirical regularity. Target firms have a substantially smaller market capitalization than other firms. As market capitalization is a criterion based on which match firms are selected, it is unsurprising that the difference in market capitalization between target firms and match firms are small and statistically insignificant.

Another empirical regularity is that target firms on average have lower market valuation. This is also confirmed in Table 2. Target firms have a higher book to market ratio than other firms. As the book to market ratio is another criterion for selecting match firms, there is only a marginal difference between target firms and match firms in terms of the book to market ratio. Still, a pattern exists where target firms have a lower market valuation in terms of the book to market ratio than match firms, which in turn have a lower market valuation than firms in the rest of the industry. This highlights the importance of market valuation in determining the likelihood of hedge fund activism. This is intuitive as lower market valuation suggests that hedge fund activists have more room to make improvements and potentially more support from other shareholders.

Turning to the policy aspects, I characterize capital structure policy with two variables: cash stock and debt ratio. Cash stock is measured by the sum of cash and short-term investments divided by book assets. Debt ratio is measured by the sum of long-term debt and short-term debt divided by book assets. Table 2 suggests that under-leverage cannot be a major reason why firms are targeted for hedge fund intervention. Although agency problem between managers and shareholders is the primary cause for shareholder activism and under-leverage is identified in many theoretical and empirical studies as a manifestation of the agency problem, target firms on average actually have higher leverage and lower cash stock relative to both match firms and other firms. Similar findings are documented in Brav et al (2008) and Fos (2011).

I characterize a firm's investment policy with net capital expenditures and R&D expenses. The former is measured by the difference between capital expenditures and sale of capital assets divided by total assets. The latter is measured by expenditures in research and development divided by total assets. Table 2 shows that target firms spend more on research and development, consistent with empire building behavior. But net capital expenditures seemingly suggests the opposite: target firms invest less in capital assets than both match and other firms. These contradictory figures underscore the limitation of univariate analysis. We can see the potential over-investment problem in target firms as well as match firms once considering the capital investment in these firms along with their investment opportunities as measured by the book to market ratio. These firms have significantly higher book to market ratios than other firms while their capital investment is only slightly lower. Therefore, the data cannot exclude excessive investment as one potential reason why firms get targeted for hedge fund intervention.

Finally, the dividend payout ratio and CEO pay are used to characterize a firm's distribution policy and compensation policy, respectively. The dividend payout ratio is defined as total dividend payments divided by net income before extraordinary items. CEO pay is measured by total expected compensation as reflected in Execucomp variable $tdc1$. The message from these two policies is relatively clear. Target firms pay their CEOs more and their shareholders less relative to the other two groups of firms, suggesting managerial rent-seeking and reluctance to return cash flows to shareholders. The higher CEO turnover in target firms is hardly surprising given their inferior market as well as accounting performance.

In short, firms that are small, undervalued, over-pay their CEOs, and under-pay their shareholders are more likely to be targeted by hedge fund activists for intervention. Financial and investment policies may also play a role but the message here is not clear-cut from univariate analysis.

3 Empirical Findings

In this section I report three sets of empirical results. The first set concerns the eligibility of mutual fund fire sales as an instrument for the likelihood of hedge fund activism. The second set of results provides evidence for the preventive effect of hedge fund activism on corporate policy deviations based on two-stage instrumental variable regressions. Finally, I examine how the preventive effect of hedge fund activism changes over time.

3.1 Instrument Tests

In this subsection, I formally check the eligibility of mutual fund fire sales as an instrumental variable for the likelihood of hedge fund activism. To test whether it meets the relevance requirement, I run the probit regression given in Equation (1) as well as a linear probability regression with the same specification. In each regression, the HFA dummy is regressed against lagged $MFFS$ and a set of lagged control variables, including natural log of market capitalization, sales, book-to-market ratio, as well as industry fixed effects and year fixed effects. The results are reported in Table 3.

[Insert Table 3 here]

Column 2 of Table 3 shows results from the probit regression while column 3 gives the results from the linear probability regression. Results from both estimation procedures indicate that mutual fund fire sales positively and significantly affect the likelihood of hedge fund activism, indicating the satisfaction of the relevance requirement. Evidently, the downward price pressure induced by mutual fund fire sales creates an opportunity for hedge fund activists to accumulate stakes in target firms at a bargain. These findings are consistent with those of Gantchev and Jotikasthira (2012), who show that institutional exit triggers hedge fund activism through a causal relationship between institutional selling and activist purchases.

To check whether mutual fund fire sales meet the exclusion requirement, I run a set of regressions of policy variables against lagged mutual fund fire sales over two sample periods. The first covers the pre-study sample period from 1988 to1992. The second is the study sample period from 1994 to 2007. This test strategy exploits exogenous changes in the legal environment that significantly affects the cost of shareholder activism, thereby affecting the likelihood of hedge fund activism. In its 1992 proxy regulation reform, the SEC relaxed the restrictions on communications among shareholders of public corporations (See Bradley, Brav, Goldstein, and Jiang (2010)). This legal change resulted in a significant increase in shareholder activism events in general and hedge fund activism in particular. In the present context, we can view the period after 1992 as the period with high likelihood of hedge fund activism while the period before 1992 as the period with low likelihood of hedge fund activism. If mutual fund fire sales do not affect corporate policies other than indirectly through their effect on the likelihood of hedge fund activism, we should expect a weaker (stronger) correlation between mutual fund fire sales and corporate policy variables prior to (after) 1992. Regression results for the two sample periods are reported in Table 4.

Panel A of Table 4 shows that prior to 1992, mutual fund fire sales do not significantly affect corporate policies during the period from 1988 to 1992. The estimated slopes for payout as a measure of distribution policy, for cash and leverage as measures of financial policy, and for capital expenditures and R&D expenses as measures of investment policy are all insignificant based on any conventional standard. CEO compensation policy is not included in Table 4 due to lack of data in this sample period.

Panel B of Table 4 presents results for the sample period from 1994 to 2007, which are quite different from those reported in Panel A. The estimated slopes for mutual fund fire sales are now strongly significant in all policy regressions. The slopes are correctly signed in all regressions except the leverage regression, which is not surprising given that leverage is not a major factor attracting hedge fund activism. As indicated by the summary statistics reported in the previous section, leverage in target firms is actually higher than leverage in other, similar or non-similar, firms. The regressions explain a decent fraction of the variations in policy variables, with R-squared ranging from 14% to 45%. A comparison of Panel A and Panel B shows that mutual fund fire sales affect corporate policies during the period when likelihood of hedge fund activism is high but do not have significant effect on corporate policies during the period when likelihood of hedge fund activism is low, suggesting that they are not directly related to corporate policy and affect corporate policy only indirectly through their effect on the likelihood of hedge fund activism. Therefore, mutual fund fire sales satisfy the exclusion requirement.

A number of theoretical studies, including Edmans (2009), Admati and Pfleiderer (2009), and Edmans and Manso (2010), argue that institutional exit directly affects corporate policy rather than indirectly through an effect on hedge fund activism. There is also some empirical evidence in favor of this Wall Street Walk effect, for example Bharath, Jayaraman, and Nagar (2013). The findings that mutual fund fire sales have an effect on corporate policy only in the period from 1994 to 2007 but not in the period from 1988 to 1992 suggest that the effect of mutual fund fire sales on corporate policy observed here does not arise from Wall Street Walk, but is instead an indirect effect through its impact on the likelihood of hedge fund activism. This is not necessarily inconsistent with the Wall Street Walk effect, which comes from institutional exit that is information driven and occurs in regular portfolio adjustment. Large mutual fund fire sales, on the other hand, are primarily liquidity driven and therefore their effect on corporate policy is not direct but indirect through creating opportunities for hedge fund activism. In addition, since it is not significant in the period from 1988 to 1992, the effect of mutual fund fire sales on policy variables is unlikely to be caused by omitted variables. Altogether, the results in Tables 3 and 4 confirm the validity of mutual fund fire sale as an instrumental variable for the likelihood of hedge fund activism.

However, as mentioned previously, $MFFS$ is not a perfect instrument for the likelihood of hedge fund activism. It may not affect corporate policy only through the likelihood of hedge fund activism channel. Likelihood of takeover is another potential channel. In other words, $MFFS$ may not perfectly satisfy the exclusion requirement. But this problem does not disqualify $MFFS$ as an instrument variable for the likelihood of hedge fund activism, because the likelihood of hedge fund activism and that of takeover are easily separable.

Another possible concern in using mutual fund fires sales as an instrument is that mutual fund outflows could be directly correlated with firm performance in the first place. That is, the redemption of mutual fund shares by mutual funds' own investors could be driven by poor fund performance, which in turn could be correlated with the performance of individual stocks held by the fund. This is unlikely to be a serious concern, however. While large outflows could be driven by the performance of the fund, they are largely exogenous to the performance of individual stocks held by the fund. The purpose of requiring mutual funds to diversify is to lower their sensitivity to individual investments. Gantchev and Jotikasthira (2012) show that mutual funds that sell future hedge fund activism targets also own and widely sell non-targets, suggesting that it is unlikely that mutual fund performance is driven by a concentrated investment in the few firms that eventually get targeted by hedge fund activists. Also, the construction of $MFFS$ excludes sector mutual funds that concentrate investment in a particular industry and whose portfolio stocks could be highly correlated with one another and hence the portfolio as a whole. Considering only diversified mutual funds therefore mitigates the concern that $MFFS$ is driven by firm performance in the first place. Indeed, Edmans et al (2012) show that stocks subject to $MFFS$ do not exhibit poor performance beforehand. Nevertheless, I control for firm fundamentals as well as industry fixed effects and year fixed effects in all regressions against $MFFS$.

3.2 Main Results: The Preventive Effect of Hedge Fund Activism

This subsection presents evidence on the preventive effect of hedge fund activism. First, the preventive effect emerges from Panel B of Table 4 reported in the previous subsection, where policy variables are directly regressed against mutual fund fire sales over the sample period from 1994 to 2007. In these regressions, $MFFS$ is essentially an exogenous proxy for the likelihood of hedge fund activism and therefore its estimated slope reflects the response of corporate policy to changes in the likelihood of hedge fund activism.

The impact row in Panel B of Table 4 presents the absolute change in a policy variable given a one standard deviation increase in lagged mutual fund fire sales. The percentage row presents the percentage change in a policy variable given a one standard deviation increase in lagged mutual fund fire sales. The results are generally confirmative of the preventive effect of hedge fund activism on policy deviations. From the summary statistics reported in Section 2, we see that hedge fund activists target firms with high CEO pay, excessive investment, and low shareholder distribution. If managers have incentive to avoid being targeted by hedge fund activists, they should reduce CEO pay, cut investment, and raise shareholder distribution when they perceive an increase in the likelihood of hedge fund activism. These expectations are all corroborated by Panel B of Table 4. Changes in mutual fund fire sales affect corporate compensation, distribution, as well as investment policies in the expected directions. The effects on all policy variables are economically large and statistically significant with t-values above 5 in all cases. For compensation policy, a one standard deviation increase in lagged mutual fund fire sales reduces CEO pay by around 324,100 dollars, corresponding to a 7.42% drop from the sample average. The model explains about 41% of the total variation in CEO compensation. In contrast to its effect on CEO compensation, a one standard deviation increase in lagged mutual fund fire sales raises the payout ratio by 9.29% from its sample mean. The model explains about 22% of the total variation in shareholder distribution policy. As a proxy for the likelihood of hedge fund activism, the effect of mutual fund fire sale on CEO compensation and shareholder distribution policy as reported in Panel B of Table 4 is consistent with the existing literature. Brav et al (2008) report similar opposite movements in CEO compensation and shareholder distribution. The authors view this wealth reallocation from executives to shareholders as a partial explanation for the abnormal returns observed around the announcement of hedge fund intervention. But their results are found from target firms subsequent to hedge fund intervention. The results reported here are from all firms and represent proactive policy responses to possible hedge fund intervention.

Similarly, as a proxy for the likelihood of hedge fund activism, mutual fund fire sales have an expected impact on corporate investment policy, which is measured with capital expenditures and R&D expenses. Over-investment and cash hoarding behavior are generally viewed as symptoms of agency problems and weak governance. Managers invest in negative NPV projects for empire building. To avoid financial market discipline, they stock cash rather than return it to shareholders. From summary statistics we see that capital expenditures and R&D expenses in target firms are not significantly different from and actually even slightly less than those in match and other firms, suggesting no over-investment problem. Further examination, however, reveals that an excessive investment problem potentially exists for target firms. Erickson and Whited (2000) and Fama and French (2002) find that firms' investments should be positively related to their investment opportunities, which are measured here by the book-to-market ratio. Since target firms on average have a higher book-to-market ratio than non-target firms, theory says they should invest less. But they in fact invest about the same as non-target firms. Assuming investment of non-target firms is at an appropriate level, investment of target firms must be excessive. According to Myers (1977), firms may have an under-investment problem only near bankruptcy. For most firms in normal situations, over-investment is symptomatic of agency problems. The present analysis suggests that excessive investment is a serious problem in target firms, and as such we should expect a significant decrease in firms' investments when likelihood of hedge fund activism increases. As can be seen from Panel B of Table 4, a one standard deviation increase in mutual fund fire sale leads to 2.67% decrease in capital expenditures and 4.96% decrease in R&D expenses, respectively, suggesting that the threat of hedge fund activism is effective in curbing managerial empire building behavior.

In terms of financial policy, Panel B of Table 4 shows a simultaneous decrease in cash stock and leverage. A one standard deviation increase in $MFFS$ reduces cash holdings by 2.61% from its sample mean and the debt ratio by 3.20% from its sample mean. The decrease in cash stock is generally viewed as a sign of improvement in corporate governance. But the decrease in the debt ratio suggests to many a worsening agency problem, because debt is commonly viewed as a means to discipline managers to work hard in order to make interest payments. This is again true only on the surface. Under-leverage appears not to be a major reason for a firm getting targeted. In fact, relative to match firms and other firms, target firms have a higher debt ratio on average. This is consistent with findings in Graham (2000) in that under-leverage is more of a problem for large profitable firms than for small unsuccessful ones that are the typical hedge fund activism targets. It is also consistent with previous findings that over-leverage is a frequently cited reason for shareholder activism. Therefore, the decrease in leverage here should be interpreted as an improvement of financial policy.

Table 5 presents results from two-stage instrumental variable regressions. In the first stage, I estimate the likelihood of hedge fund activism in a probit model as well as a linear probability model. In the second stage, I estimate the preventive effect of hedge fund activism on corporate policies using the estimated likelihood obtained from the first stage. For each policy regression, Panel A reports the second stage results when the first stage results are obtained from the probit model while Panel B reports the second stage results when the first stage results are obtained from the linear probability model. The t-values are calculated based on standard errors corrected for generated regressor.

[Insert Table 5 here]

As can be seen from Table 5, results from the two-stage instrument variable regressions are generally consistent with those from the direct OLS regressions of policy variables on lagged $MFFS$. The effect of the likelihood of hedge fund activism on CEO pay is positive when the likelihood is obtained from a linear probability regression but expectedly negative when obtained from the probit model. The slope is significant only when it has the expected sign. The procedure explains 38% of the total variation in CEO pay when the likelihood of hedge fund activism is estimated from the probit model. CEO turnover is positively related to the likelihood of hedge fund activism regardless of whether the likelihood is estimated from a probit or linear probability model, suggesting the threat of hedge fund activism is effective in mitigating managerial entrenchment. But the evidence on CEO turnover is relatively weak for two reasons. First, the likelihood of hedge fund activism explains only a very small fraction of the total variation in CEO turnover. Second, the slope in the direct OLS regression is positive.

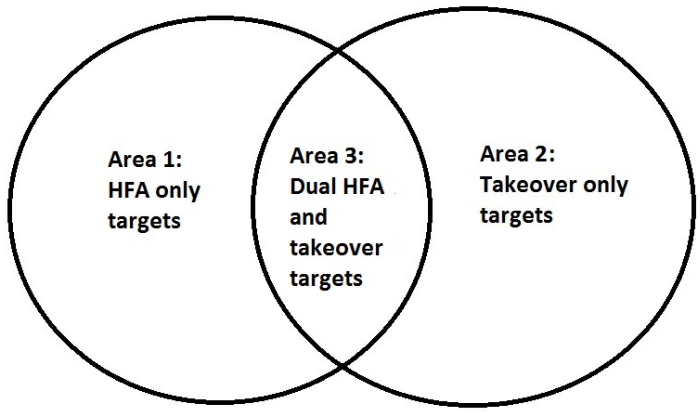
For shareholder distribution policy, the effect of the likelihood of hedge fund activism is unequivocally positive but stronger with the probit generated likelihood. The model explains 12% of the total variation in the payout ratio. In terms of the investment policy, the estimated slope on capital expenditures is expectedly negative when the likelihood is obtained from linear probability model but unexpectedly positive when the likelihood is generated from the probit model. The slope is significant only when it has the expected sign. The estimated slope on R&D expenses is significantly negative regardless of whether the likelihood is obtained from a probit or a linear probability model. Finally, results on financial policy are similar to those obtained from the OLS procedure. In response to an increase in the likelihood of hedge fund activism, cash stock declines unequivocally; leverage declines when the likelihood is estimated from the linear probability model while increases when the likelihood is estimated from the probit model. But the slope on the probit generated likelihood is not statistically significant based on any conventional criterion.

Overall the results from OLS and two-stage instrument variable regressions are supportive of the hypothesis that managers proactively self-correct policy deviations in response to a threat of hedge fund activism. These findings are intuitively appealing given the negative consequences managers usually face after hedge fund interventions. They are also consistent with anecdote evidence that some corporate advisors, for example Martin Lipton, recommend their clients to be sensitive to hedge fund investors and prepare in advance for possible hedge fund activism. Consistent with proactive policy responses, the slope is positive for ROA in all regressions.

The results reported above have several implications. First, they show that corporate policy responds not only to materialized events but also potential ones. Policy improvements occur not only after hedge fund intervention but also before or even without hedge fund intervention. As such, hedge fund activism impacts not only firms targeted for actual intervention but also other firms that respond proactively to potential intervention. The impact of hedge fund activism is widespread, despite the fact that only a small portion of public firms are actually targeted in materialized hedge fund interventions. Second, a firm may not succeed in preempting hedge fund activism if it is not sufficiently proactive, in which case actual intervention takes place and we may observe some subsequent policy changes. But post-intervention changes reflect only the corrective effect of hedge fund activism. The preventive effect is reflected only in pre-intervention changes. To assess the full effect of hedge fund activism as a mechanism for corporate governance, both the preventive effect and the corrective effect must be considered. Third, if we erroneously assume away the preventive effect, we also assume away any spillover effect. In this case, not only do we under-estimate the full effect of hedge fund activism but also the corrective effect on target firms. This is because the corrective effect on target firms are measured as the post-intervention policy changes of target firms relative to other firms. When policies of these other firms also change in the same direction as target firm, in proactive response to possible hedge fund activism, the policy changes detected for target firms are only those in excess of the policy changes in other firms. Finally, if we divide a firm's total response to an actual hedge fund intervention into two components, proactive response and reactive response, the weight of each component relative to the total response depends on the extent to which the intervention is anticipated. If an intervention is anticipated well in advance, most of the policy change would occur before the intervention. For an intervention that comes as a surprise, most of the policy changes will occur after the intervention. It is therefore important to control for the likelihood of hedge fund activism when ascertaining the extent of policy changes subsequent to intervention. If the distribution of anticipated and unanticipated interventions varies across samples and the likelihood of hedge fund activism is not controlled for, empirical results on post-intervention policy changes may differ across samples. This potentially explains why some previous studies find significant policy improvements after hedge fund interventions while others do not.

3.3 Confounding Effect: Likelihood of Takeover Bid

Liquidity-driven mutual fund fire sales create an exogenous downward price pressure, which allows hedge fund activists to accumulate a large stake in target firms at a lower cost, thereby increasing the likelihood of hedge fund activism. As such, mutual fund fire sales are a valid instrument for the likelihood of hedge fund activism. But the downward price pressure created by mutual fund fire sales also increases the likelihood of takeover. Intuitively, it is very likely that a firm targeted by hedge fund activists would also attract interest from potential acquirers and vice versa. In fact, the mutual fund fire sale variable was originally constructed in Edmans et al (2012) as an instrumental variable for price pressure to study the effect of stock price on corporate takeover. In empirical studies, it is very likely that some firms in a sample of hedge fund activism targets are also takeover targets, and likewise some firms in a sample of takeover targets are also hedge fund activism targets. The relationship is illustrated in the Venn-diagram given below:



where firms in area 1 are pure hedge fund activism targets, firms in area 2 are pure takeover targets, and firms in area 3 are dual targets. Given the overlap between the samples of hedge fund activism and takeover targets, there is a natural question: to which particular mechanism should we credit the observed policy improvements? In general, previous studies have focused separately on either takeover bids or hedge fund activism. Studies on hedge fund activism use a sample of firms in area 3 as well as those in area 1 and attribute the observed policy responses solely to hedge fund activism. Similarly, studies on corporate takeover use a sample of firms in area 3 as well as those in area 2 and attribute observed policy responses solely to takeover. This is clearly inappropriate due to the confounding effect from dual targets.

To isolate the effect of hedge fund activism, I obtain takeover data from the Securities Data Company for the period from 1994 to 2007, the same as the sample period of hedge fund activism data. After merging the two data sets, I split the sample into pure hedge fund activism targets, pure takeover targets, and dual targets. I then create two dummy variables for every firm-year observation: (1) $HFAONLY$, valued at one if a hedge fund intervention is announced but not within twelve months of a takeover bid and zero otherwise; (2) $TKONLY$, valued at one if a takeover bid is announced but not within twelve months of a hedge fund intervention and zero otherwise. Table 6 reports the descriptive statistics for the sample. Panel A shows the event distribution across the three event groups. Panel B reports separate summary statistics on a number of policy and other characteristic variables for targets of the two types of pure events. Panel C presents results from first-stage regressions of $HFAONLY$ and $TKONLY$ against lagged mutual fund fire sales.

[Insert Table 6 here]

According to Panel A, in the hedge fund activism sample, 688 events are pure hedge fund activism events whereas 417 events are dual hedge fund activism and takeover events. The dual events represent almost 40% of the whole sample, suggesting the importance of controlling for their confounding effect in the present study. Panel B shows that there is no systematic difference in the pre-intervention year between pure hedge fund activism targets and pure takeover targets, suggesting similar considerations in the selection of hedge fund activism and takeover targets. Finally, Panel C shows that as expected, mutual fund fire sales are positively correlated with the likelihood of pure hedge fund activism and pure takeover events.

To examine policy responses to the likelihood of pure hedge fund activism events, I run similar two-stage instrumental variable regressions as earlier, except replacing the HFA dummy in the first stage regression with the pure hedge fund activism dummy $HFAONLY$. As before, I estimate the likelihood of pure hedge fund activism events from both probit and linear probability models. The fitted values are used to explain policy variables in the second stage regressions. The results from the second stage policy regressions are reported in Table 7. For each policy regression, Panel A reports the second stage results when the first stage results are obtained from the probit model, while Panel B reports the second stage results when the first stage results are obtained from the linear probability model. The t-values are calculated based on the standard errors corrected for generated regressor.

[Insert Table 7 here]

According to Table 7, hedge fund activism has an independent effect on corporate policies. The threat of hedge fund activism alone, without the attendant threat of takeover, is sufficient to push managers into action. With the confounding effect of potential takeover eliminated, an increase in the likelihood of hedge fund activism still cuts capital expenditures, R&D expenses, cash stock and leverage while raising the payout ratio. The threat of pure hedge fund activism still results in a significant improvement in return on assets. Table 7 shows that the preventive effect of hedge fund activism on corporate policy documented in the previous subsection is not driven by a subsample of firms that are likely to be targeted by both hedge fund activists and corporate acquirers.

To examine policy responses to the likelihood of pure takeover bids, I run the two-stage instrumental variable regressions once again, except this time I replace the HFA dummy in the first stage regression with the pure takeover bid dummy $TKONLY$. Again I generate the fitted likelihood values from both probit and linear probability models and then use them to explain policy variables in a set of second stage regressions. The results from the second stage policy regressions are reported in Table 8. For each policy regression, Panel A reports the second stage results when the first stage results are obtained from the probit model, while Panel B reports the second stage results when the first stage results are obtained from the linear probability model. The t-values are calculated based on standard errors corrected for generated regressor.

[Insert Table 8 here]

Table 8 shows that takeover is another standalone channel through which exogenous price changes affecting corporate policy. When facing a takeover threat, firms respond in a similar way as facing a threat of hedge fund intervention: they reduce cash stock, leverage, capital expenditures, and R&D expenses and increase shareholder distribution. This makes sense because firms targeted by corporate acquirers share similar characteristics with firms targeted by hedge fund activists. As a result of these positive policy adjustments, ROA improves significantly. Again, the effect on CEO pay is a bit of ambiguous. Overall, the findings in Table 8 are consistent with the existing literature on corporate mergers and acquisitions. For example, Hart (1995), Holmstrom and Kaplan (2001), Bertrand and Mullainathan (2003) and Bebchuk, Cohen and Ferrell (2009) find evidence consistent with takeover as an effective mechanism for corporate governance.

An important massage to take from Table 7 and Table 8 is that hedge fund activism and takeover bid have drastically different effects on CEO turnover. While the threat of hedge fund activism significantly increases CEO turnover, the threat of takeover has a negative effect although statistically insignificant. The results suggest hedge fund activism is more effective in preventing managerial entrenchment than corporate takeover.

There are several reasons for a stronger preventive effect associated with hedge fund activism as opposed to takeovers. First, takeovers do not always leave managers at a disadvantage. The fact that sometimes hedge fund activists intervene for the purpose of preventing a takeover of target firms attests to this. Second, target managers are better able to negotiate with takeover bidders than hedge fund activists, as they typically have something the former wants, for example unique distribution channels, customer base, or technology. As such they can bargain or simply wait for better offers. On the other hand, managers have relatively little control over the timing of hedge fund interventions or the identity of the activists. Third, managers are better protected from the negative effects of takeovers than hedge fund interventions. For example, golden parachutes grant significant compensation for employment termination due to a merger or takeover but not shareholder activism. Finally, although replacing the CEO can be an effective means of preempting a shareholder activism event, it is less effective in forestalling a takeover. While removing incumbents is sometimes a central goal of shareholder activism, it is rarely that of takeovers. In most cases, it is a consequence of takeover. Given all these considerations, we should expect takeover threat to be less effective in reducing managerial entrenchment.

3.4 Preventive Effect in the Time Series

In this subsection, I examine whether the preventive effect of hedge fund activism changes over time. During the early 1980s when corporate takeover emerged as a leading external mechanism for corporate governance, firms were highly responsive to the threat of potential takeover. But over time, as more and more firms adopt various defense mechanisms, the preventive effect of corporate takeover has declined substantially. It is interesting to know whether hedge fund activism, as an alternative mechanism for corporate governance, follows the same path. For this purpose, I run the following set of policy regressions:

$$P = \alpha_0 + \alpha_1 MFFS + \sum_j \alpha_{2j} MFFS \times YEAR_j + IFE + YFE + \epsilon_3 \ . \qquad (3)$$

The third term on the right hand side represents the sum of interaction terms between mutual fund fire sales and year dummies, where j ranges from 1995 to 2007. The year dummy 1994 is dropped in the regression to avoid perfect multicollinearity. Under this specification, the preventive effect of hedge fund activism in year j is measured by the total marginal effect of mutual fund fire sales $\alpha_1 + \alpha_{2j}$.

Figure 1 plots the year-by-year preventive effect of hedge fund activism on ROA, CEO pay, shareholder payout and cash holdings during the 1994 to 2007 sample period. In each graph, the solid line represents the estimated preventive effect, the two dashed lines mark the 95% confidence interval, and the dotted line shows the linear fit.

[Insert Figure 1 here]

Figure 1 demonstrates the changing preventive effect of hedge fund activism. Over the sample period, the threat of hedge fund activism becomes increasingly effective in increasing ROA and reducing CEO pay and cash holdings. The declining trend in the shareholder payout graph does not necessarily indicate a weakening preventive effect in terms of distribution policy. It could reflect a shift in the focus of hedge fund activists away from distribution policy. We would also observe such a trend if in the beginning of the sample period, hedge fund activists target firms with the most severe under-distribution problem then subsequently move on to firms where it is successively less severe. In either case, the preventive effect of hedge fund activism remains significant toward the end of the sample period. The preventive effect remains stable in terms of CEO turnover, leverage, capital expenditures and R&D expenses, of which graphs are hence omitted for brevity. Altogether, I do not find significant evidence that the preventive effect of hedge fund activism declines over time.

Since the likelihood of takeover is another channel through which mutual fund fire sales can affect policy, I repeat the above time series analysis but control for the confounding effect of takeover likelihood. Specifically, I first exclude dual hedge fund activism and takeover events from the hedge fund activism sample and estimate the likelihood of the pure hedge fund activism. Then I perform the same second stage policy regressions as in (5) except adding interaction terms between the estimated likelihood of pure hedge fund activism and year dummies as in regression (3). Analogous analyses on the likelihood of pure takeover bid are also performed.

Figure 2 shows contrasting time series trends in the preventive effect of pure hedge fund activism events and that of pure takeover events. In terms of shareholder payout and CEO pay, the preventive effect of pure hedge fund activism events becomes increasingly stronger while that of pure takeover events becomes increasingly weaker. The opposite is true for ROA, cash holdings, capital expenditures and R&D expenses. However, the preventive effect of pure hedge fund activism events on these policy variables remains stronger than that of pure takeover events even toward the end of the sample period. Both preventive effects remain stable for CEO turnover and leverage, of which graphs are hence omitted for brevity.

These results offer some explanation for how the marginal policy effects of mutual fund fire sales change over time as shown in Figure 1. Exogenous price drops induced by mutual fund fire sales increase both the likelihood of pure hedge fund activism events and that of pure takeover events. The magnitude of the effect of the two likelihoods on some policies, however, move in opposite directions over time. As such, the overall time series trend of the effect of mutual fund fire sales on those policies could be driven by either the trend in the preventive effect of pure hedge fund activism events or that of pure takeover events, whichever is stronger. Moreover, if one increases as much as the other decreases, the overall trend could be flat.

[Insert Figure 2 here]

The time-series analysis of the preventive effect of hedge fund activism in this subsection provides valuable insights into the future viability of hedge fund activism as a governance mechanism. As hedge fund activists currently strike fear in corporate incumbents, the question is whether the preventive effect of hedge fund activism on policy deviations is likely to continue, especially given the uncertain future of the preventive effect of takeovers. According to the results in this section, while the threat of takeover remains effecting in preventing excessive cash and over-investment, the threat of hedge fund activism is becoming increasingly important in curbing CEO pay and increasing shareholder payout.

4 Additional Analyses

In this section I perform additional analyses to check the robustness of the main results with alternative model specifications and sampling. First, I repeat the analysis using discontinuous rather than continuous likelihood values. In Table 9, I use an above median likelihood indicator variable which takes a value of one if the likelihood is above the median level and zero otherwise. In Table 10, I use quartile likelihood indicator variables, where each quartile indicator takes on a value of one if the likelihood value falls within that quartile and zero otherwise. The median and quartile likelihood indicator variables have the advantage over the continuous likelihood variable of accounting for potential non-linearity in the relationship between the likelihood of hedge fund activism and policy variables. Quartile indicators have the additional benefit of identifying the source of the significance in the relationship, as linear regressions on a continuous likelihood variable could reveal a relationship that is only significant in the right tail.

[Insert Table 9,10 here]

Regressions using above median as well as quartile likelihood indicator variables confirm the main results that the likelihood of hedge fund activism leads to governance improvements, such as higher shareholder payout, lower CEO compensation and fewer value-destroying investments. In particular, results from using quartile likelihood indicator variables show increasingly stronger proactive policy responses as the likelihood of hedge fund activism rises from the first (lowest) to the fourth (highest) quartile. Moreover, for most policy variables, moving from the first to the second likelihood quartile already induces significant proactive response, suggesting a preventive effect of hedge fund activism in most firms rather than only a few firms facing the highest likelihood of hedge fund activism.

I also investigate the possibility that proactive policy response to the likelihood of external governance is driven by actual targets as opposed to the much greater number of potential targets. Specifically, I perform three sets of direct OLS regressions of policy variables on lagged mutual fund fire sales, each time excluding from the Compustat sample, hedge fund activism targets, takeover targets, and finally both hedge fund activism and takeover targets. Results are shown in Table 11.

[Insert Table 11 here]

Panels A through C in Table 11 respectively show results from the three exclusions. In panel A, firms excluding hedge fund activism targets, firm-year observations where the hedge fund activism dummy have a value of one are excluded from the overall sample of Compustat firms. In Panel B, firms excluding takeover targets, firm-year observations where the takeover dummy has a value of one are excluded. In panel C, firms excluding both targets, firm-year observations where either the hedge fund activism dummy or the takeover dummy has a value of one are excluded. We see from each panel that the proactive policy response to the likelihood of hedge fund activism as proxied by lagged mutual fund fire sales remains significant and in a governance-improving direction even after excluding realized hedge fund activism events, takeover events and both activism and takeover events. Results from this table confirm the results in Table 10 using likelihood quartile indicator variables that hedge fund activism has a wide spread preventive effect extending beyond the small number of firms that are actually targeted.

To further check the prevalence of the preventive effect of hedge fund activism, I perform direct OLS regressions of policy variables on lagged mutual fund fire sales for a subsample of Compustat firms that also appear in the Execucomp database. These are large S&P1500 firms for which hedge fund activism is more costly and therefore less likely. Results are shown in Table 12.

[Insert Table 12 here]

From Table 12, we see that the policy responses of S&P1500 firms to lagged mutual fund fire sales are significant and in the governance improving direction. In fact, the proactive policy improving adjustments here are even bigger than those from the whole Compustat sample regressions. On the one hand, these results further show the broad preventive effect of hedge fund activism. Even corporate giants are not immune to the threat of hedge fund activism. On the other hand, these results raise the possibility that big firm managers are more sensitive to exogenous drops in stock price.

I also check the sensitivity of the results to omitted variables. I perform direct OLS regressions of firm policy variables on lagged mutual fund fire sales, but with the inclusion of additional controls. Specifically, in addition to lagged market cap, book to market, sales, industry fixed effect and year fixed effects, I also include all lagged policy variables. Results are reported in Table 13.

[Insert Table 13 here]

According to Table 13, the main regression results are robust to the inclusion of more explanatory variables. Though somewhat weaker, significance is not lost. These results verify that the impact of mutual fund fire sales on firm policies do not merely reflect correlation among various policy variables. For instance, one could argue that the decrease in cash holdings is due to an increase in shareholder payout, which drains a firm's reserved liquidity but does not necessarily lead to a structural improvement in governance. Table 13 shows that even after controlling for payout, firms reduce cash holdings after experiencing mutual fund fire sales, indicating genuine attempt to improve overall governance and operations.

Finally, another concern is that the effectiveness of hedge fund activism is driven by hostile events, particularly those involving proxy contests. For example, Fos (2013) document a preventive effect associate with proxy contest. Focusing on proxy contest suggest a more critical role of activism tactic over activist identity. Intuitively, however, the effectiveness of a tool largely depends on the identify of the wielder. Analogously, proxy threat coming from liquidity strapped investors will do little to elicit a response from corporate giants. Hedge funds, on the other hand, are formidable activists. First, operating in a lax regulatory environment, hedge funds can hold onto investors' money for an extended period of time and concentrate it in intervention targets. Second, hedge fund activists repeatedly engage in activism as a profit strategy. As such, they accumulate expertise in intervening and generating shareholder value which earn them credibility and thus greater potential shareholder support against corporate incumbents.

Intuition aside, I formally address the concern that the preventive effect of hedge fund activism is driven by activists' usage of proxy contests. I perform the same two-stage instrumental variable regressions with the following variations: in the first stage, I estimate the likelihood of non-proxy hedge fund activism events by regressing a *NONPROX* dummy, which takes a value of one if a hedge fund activism intervention not involving a proxy contest is announced and zero otherwise, against lagged mutual fund fire sales. In the second stage, I regress policy variables against the estimated likelihood of non-proxy hedge fund activism obtained from the first stage which now serves as my instrument for the actual likelihood. Results are shown in Panel A of Table 14:

[Insert Table 14 here]

From Panel A of Table 14, we see that the likelihood of non-proxy hedge fund activism has a similar effects on most of the policy variables as does the likelihood of hedge fund activism events in general: it increases shareholder payout and decreases cash holdings and research and development expense. It also improves firms' operating performance. Therefore, the preventive effect of hedge fund activism is unlikely to come from the usage of proxy contest.

Next I extend the above analysis by examining the importance of other hostile activism tactics in hedge fund activism's prevention of policy deviations. I perform the same two-stage instrumental variable regressions with the following variations: in the first stage, I estimate the likelihood of non-hostile hedge fund activism events by regressing a *NONHOST* dummy, which takes a value of one if a hedge fund activism intervention is non-hostile in nature and zero otherwise, against lagged mutual fund fire sales. In the second stage, I regress policy variables against the estimated likelihood of non-hostile hedge fund activism obtained from the first stage which now serves as my instrument for the actual likelihood. Hostile interventions include the usage of lawsuits and antagonistic communication requesting managerial resignation in addition to proxy contests. According to Panel B of Table 14, the proactive policy responses to the likelihood of non-hostile hedge fund activism remain strong: shareholder payout and ROA increase while CEO pay, cash holdings and R&D expenses decrease, suggesting that non-hostile hedge fund activism also has a preventive effect on policy deviation. In other words, the preventive effect of hedge fund activism does no hinge on the usage of hostile tactics.

All in all, the results from alternative model specifications and sampling procedures verify the robustness of the main result that hedge fund activism has a preventive effect on corporate policy deviations. Furthermore, this effect is widespread, observable not only in firms with the highest likelihood of hedge fund activism or smallest in size but in a significant portion of Compustat firms. Finally, the preventive effect of hedge fund activism is not limited to firms that are eventually targeted or driven by the usage of hostile tactics.

5 Conclusion

This paper provides empirical evidence that corporate policies are proactive to hedge fund activism. In addition to having a corrective effect on policy deviations as documented in previous studies, this study shows that hedge fund activism has a preventive effect as well. Specifically, in proactive response to an increase in the likelihood of hedge fund intervention, firms cut CEO pay, reduce cash holdings and leverage, limit capital investment and R&D expenses, and raise shareholder distributions and CEO turnover. As a result of these policy improvements, return on assets increases significantly.

The preventive effect of hedge fund activism remains significant after eliminating the confounding effect of concurrent takeover threat. It also survives the exclusion of hostile interventions such as those involving proxy contests. The effect is evident in the corporate world at large rather than limited to a group of "easy" targets. Time-series analysis shows that the threat of hedge fund activism becomes increasingly effective in reducing CEO pay and cash holdings as well as in improving return on assets during the period from 1994 to 2007, suggesting the future viability of hedge fund activism as a valuable mechanism for corporate governance.

This study has several important implications. First, it suggests the impact of hedge fund activism is stronger and broader than previously documented. It is stronger as target firms experience both ex ante and ex post policy improvements. It is broader as not only target firms but all firms proactively self-correct policy deviations when facing a threat of hedge fund activism. Second, the study shows that hedge fund activism provides a standalone channel through which financial markets can discipline corporate managers. Finally, by showing that hedge fund activism is an effective and viable mechanism for corporate governance, the study allows policymakers to make more informed decisions as they face heightened pressure to increase hedge fund regulations.

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Table 1 – Summary of variables

This table describes the main regression variables. Panel A lists variable definitions. Panel B reports summary statistics for all firms over the 1994 to 2007 sample period.

Panel A: Variable definitions

Variable	Definition
MFFS	Volume of stock sold by all mutual funds experiencing outflows ≥5% of assets deflated by trading volume
MC	Common shares outstanding × fiscal year end price, in $millions
B2M	Book equity / MC
SALE	Net sales / lagged assets
PAYOUT	Common dividends / income before extraordinary items
CEOPAY	(Pre 2006 adjusted) tdc1 variable from Execucomp, in $millions
TURN	One if current CEO is differs from last year's CEO and zero otherwise
CASH	Cash & short term investments / assets
LVRG	(Current + long term debt) / assets
CAPX	(Capital expenditures - sale of PP&E) / average assets
RD	Research and development expense / lagged assets
ROA	EBITDA / lagged assets
HFA	One if a hedge fund intervention is announced and zero otherwise
HFAONLY	One if a hedge fund intervention is announced but not within twelve months of a takeover bid and zero otherwise
TKONLY	One if takeover bid is announced but not within twelve months of a hedge fund intervention and zero otherwise
NONPROX	One if a hedge fund intervention involves a proxy contest and zero otherwise
NONHOST	One if a hedge fund intervention involves a hostile tactic and zero otherwise
ABVMED	One if likelihood of hedge fund activism is above median and zero otherwise
QTL2	One if likelihood of hedge fund activism falls in the second quartile and zero otherwise
QTL3	One if likelihood of hedge fund activism falls in the third quartile and zero otherwise
QTL4	One if likelihood of hedge fund activism falls in the fourth quartile and zero otherwise

Panel B: Summary statistics

Variable	Mean	Median	Std. Dev.	N
MFFS	0.0098	0.0009	0.0258	72307
LNMC	5.2965	5.1747	2.2047	71819
B2M	0.5724	0.4379	0.5261	71774
SALE	1.2325	1.0653	0.8846	65892
PAYOUT	0.0908	0.0000	0.2221	71663
CEOPAY	4.2952	2.2374	5.3209	19024
TURN	0.1138	0.0000	0.3176	19019
CASH	0.2048	0.1043	0.2323	71663
LVRG	0.2248	0.0899	0.2768	72054
CAPX	0.3770	0.2383	0.4044	64608
RD	0.0997	0.0482	0.1307	40482
ROA	0.0675	0.1138	0.2244	65544

Table 2 – Target firm characteristics

This table reports the characteristics of three groups of firms as well as cross-group comparisons in the 1994-2007 sample period: target firms, match firms, and other firms. Target firms include all firms targeted at least once during the sample period. Match firms include all firms that satisfy the following four requirements: (1) never targeted during the entire sample period; (2) belong to the same industry as the target firm, as defined by the three-digit Standard Industry Classification code; (3) fall into the same market capitalization quartile as the target firm, and (4) fall into the same book to market quartile as the target firm. Other firms include all firms that meet requirements (1) and (2) for classification as a match firm but not (3) and (4). The first three columns report mean characteristics of target, match, and other firms, respectively, in the year before hedge fund activism is announced for target firms. Column 4 presents average mean differences between target firms and match firms. Column 5 presents average mean differences between target firms and other firms. Column 6 presents average mean differences between the match firms and other firms. Parentheses enclose t-statistics. *, **, *** denote statistical significance at the ten, five, and one percent levels, respectively.

Characteristic	Target	Match	Other	Target-Match	Target-Other	Match-Other
LNMC	4·999	5·050	5·572	−0·060	−0·573***	−0·513***
				[−0·75]	[−8·50]	[−5·03]
SALE	0·1829	0·2182	0·2156	−0·0353**	−0·0327**	0·0026
				[−2·23]	[−2·13]	[0·17]
B2M	0·637	0·600	0·491	0·038	0·146***	0·108***
				[0·41]	[5·77]	[5·20]
PAYOUT	0·055	0·068	0·081	−0·013	−0·026***	−0·013*
				[−0·50]	[−4·29]	[−1·77]
CEOPAY	3·271	2·864	4·581	0·41**	−1·31***	−1·72***
				[−2·01]	[−3·75]	[−4·88]
TURN	0·203	0·202	0·197	0·0003	0·0060	0·0057
				[0·22]	[0·78]	[0·67]
CASH	0·219	0·230	0·233	−0·011	−0·015	−0·004
				[0·81]	[−0·99]	[−0·93]
LVRG	0·234	0·194	0·194	0·040***	0·040***	−0·001
				[−2·85]	[2·87]	[−0·66]
CAPX	0·359	0·390	0·404	−0·031*	−0·045***	−0·014
				[−1·72]	[−3·02]	[−1·27]
RD	0·058	0·062	0·064	−0·004	−0·006	−0·003
				[−0·87]	[−1·52]	[−0·70]
ROA	0·047	0·055	0·070	−0·009	−0·024	−0·015**
				[−0·26]	[−1·15]	[−2·55]

Table 3 – Instrument test: relevance

This table reports the results from testing the instrument's satisfaction of the relevance requirement. The sample period is 1994-2007. The dependent variable is a HFA dummy equaling one if a hedge fund intervention is announced and zero otherwise. The key explanatory variable is lagged mutual fund fire sales. Results from both probit and linear probability models are reported. The control variables are lagged natural log of market cap, sales, and the book-to-market ratio as well as industry fixed effects and year fixed effects. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. ** denotes significance at the five percent level.

	Probit	Linear
$MFFS_{-1}$	0.0120^{**}	0.5572^{**}
	[2.08]	[2.00]
$Controls_{-1}$	Yes	Yes
IYFE	Yes	Yes
n	56,614	59,633
R^2	0.0766	0.0159

Table 4 – Instrument test: exclusion

This table reports the results from testing the instrument's satisfaction of the exclusion requirement. Panel A reports direct OLS regressions of firm policy and performance variables on lagged mutual fund fire sales for the 1988 to 1992 placebo period when incidence of hedge fund activism and takeover is low. CEO pay and CEO turnover regressions are not included due to lack of data in Execucomp. Panel B reports the same set of regressions for the 1994 to 2007 sample period. The control variables are lagged natural log of market cap, sales, and the book-to-market ratio as well as firm fixed effects and year fixed effects. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. ** and *** denote statistical significance at the ten and five percent levels, respectively.

Panel A: Placebo period 1988-1992

	PAYOUT	CASH	LVRG	CAPX	RD	ROA
$MFFS_{-1}$	0.192	-0.058	0.033	0.219	-0.016	-0.058
	[1.06]	[-1.15]	[0.35]	[1.08]	[-1.39]	[-1.15]
$Controls_{-1}$	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes
n	11,166	11,192	11,190	10,860	6,153	11,135
R^2	0.2907	0.1659	0.1634	0.1079	0.3934	0.3056

Panel B: Sample period 1994-2007

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$MFFS_{-1}$	0.411**	-15.7***	-0.581***	-0.259***	-0.352***	-0.490***	-0.243***	0.659***
	[5.66]	[-6.77]	[-4.33]	[-5.37]	[-4.63]	[-7.68]	[-7.70]	[15.18]
Impact	0.0084	-0.3241	-1.19	0.0053	-0.0072	-0.0101	-0.0049	0.0135
Percentage	9.29%	-7.42%	-10.51%	-2.61%	-3.20%	-2.67%	-4.96%	20.1%
$Controls_{-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	59,357	18,394	18,596	59,623	59,615	58,427	36,682	59,221
R^2	0.2198	0.4016	0.0199	0.3702	0.2212	0.1431	0.4496	0.3722

Table 5 – Second stage IV regressions: likelihood of hedge fund activism

This table reports the results from second stage IV regressions of policy and performance variables on the likelihood of hedge fund activism, estimated from either a first stage Probit model, Panel A, or linear probability model, Panel B. In both panels, the dependent variables are shareholder payout, CEO pay, CEO turnover, cash holdings, leverage, capital expenditures, R&D expenses, and ROA. Control variables include natural log of market cap, sales, and the book-to-market ratio as well as industry fixed effects and year fixed effects. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. *, **, *** denote statistical significance at the ten, five, and one percent levels, respectively.

Panel A: Probit model

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$\widehat{HFA}$	0.482***	-7.963*	0.671*	-0.436	0.190	0.052	-0.154	0.514***
	[2.53]	[-1.83]	[1.84]	[0.64]	[0.89]	[0.22]	[-1.48]	[3.69]
Controls_{-1}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	56,652	17,340	17,749	56,466	56,458	55,465	35,296	56,212
R^2	0.1173	0.3846	0.0190	0.2978	0.2114	0.1389	0.4201	0.2685

Panel B: Linear probability model

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$\widehat{HFA}$	7.873*	176.5	6.029*	-4.932*	-6.635*	-9.325*	-2.098***	12.58**
	[1.74]	[1.53]	[1.69]	[1.82]	[-1.70]	[-1.90]	[-3.09]	[1.97]
Controls_{-1}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	59,239	18,352	18,552	59,503	59,495	58,427	36,682	59,221
R^2	0.0992	0.0204	0.0190	0.0374	0.0206	0.0118	0.0565	0.0636

Table 6 – Hedge fund activism versus takeover bid

This table describes three types of events during the 1994 to 2007 sample period: pure hedge fund activism events in which a hedge fund intervention is announced but not within twelve months of a takeover bid; pure takeover events in which a takeover bid is announced but not within twelve months of a hedge fund intervention; and dual events in which a hedge fund intervention is announced within twelve months of a takeover bid or a takeover bid is announced within twelve months of a hedge fund intervention. Panel A reports the respective sample sizes. Panel B reports pre-event year summary statistics of target firm characteristic variables for the sample of pure hedge fund activism events and the sample of pure takeover bid events. Panel C shows the results from both probit and linear regressions of the following dummy variables against lagged mutual fund fire sales: $HFAONLY$, valued at one if a hedge fund intervention is announced but not within twelve months of a takeover bid and zero otherwise; and $TKONLY$, valued at one if a takeover bid is announced but not within twelve months of a hedge fund intervention and zero otherwise. Control variables include natural log of market cap, sales and book-to-market ratio as well as industry fixed effects and year fixed effects. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. ** and *** denote statistical significance five and one percent levels, respectively.

Panel A: Sample size

Event type	Number of events
HFA only	688
Dual HFA and takeover	417
Takeover only	10,584

Panel B: Pre-event year target firm characteristics

	HFA Only			Takeover Only		
Variable	Mean	Median	N	Mean	Median	N
MFFS	0.0118	0.0016	688	0.0110	0.0020	10584
PAYOUT	0.0703	0.0000	684	0.0957	0.0000	10550
CEOPAY	3.3020	1.9136	177	4.7544	2.4972	4027
TURN	0.1918	0.0000	172	0.1318	0.0000	3649
CASH	0.1974	0.0992	686	0.1857	0.0918	10544
LVRG	0.2133	0.1667	686	0.2009	0.1665	10544
CAPX	0.3176	0.2005	622	0.3672	0.2419	9823
RD	0.0486	0.0000	625	0.04676	0.0000	9892
ROA	0.0482	0.0806	622	0.1071	0.1371	9783
LNMC	4.9556	4.876	685	5.6030	5.4283	10495
B2M	0.7362	0.6155	685	0.5994	0.4679	10494
SALE	1.1630	1.0464	623	1.3029	1.1401	9869

Panel C: First stage regressions

	$HFAONLY$		$TKONLY$	
	Probit	Linear	Probit	Linear
$MFFS_{-1}$	1.841***	0.047***	1.245***	0.1995***
	[2.71]	[2.62]	[3.86]	[3.27]
$Controls_{-1}$	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes
n	59,012	59,940	59,376	59,940
R^2	0.0941	0.1382	0.0504	0.0702

Table 7 – Second stage IV regressions: likelihood of pure hedge fund activism

This table reports the results from second stage IV regressions of firm policy and performance variables on the likelihood of pure hedge fund activism events in which a hedge fund intervention is announced but not within twelve months of a takeover bid. The sample period is 1994-2007. The dependent variables are shareholder payout, CEO pay, CEO turnover, cash holdings, leverage ratio, capital expenditures, R&D expenses, and ROA. The key explanatory variable is the likelihood of pure hedge fund activism events estimated from either a first stage Probit model, Panel A, or linear probability model, Panel B. Control variables include natural log of market cap, sales, and the book-to-market ratio as well as industry fixed effects and year fixed effects. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. *, **, *** denote statistical significance at the ten, five, and one percent levels, respectively.

Panel A: Probit model

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$\widehat{HFAONLY}$	0.599***	-7.910	0.476	-0.327**	-0.205	-0.821	-0.009	0.368**
	[2.66]	[-1.27]	[1.17]	[-1.96]	[-1.19]	[-0.33]	[-0.21]	[2.66]
Controls_{-1}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	49,905	15,589	14,613	49,907	49,907	49,777	49,991	49,670
R^2	0.1144	0.3775	0.0167	0.3509	0.2110	0.1469	0.4829	0.3294

Panel B: Linear probability model

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$\widehat{HFAONLY}$	6.949**	159.5	7.462	-4.221**	-5.317*	-8.301**	-2.197**	9.398**
	[2.21]	[0.21]	[0.17]	[-2.22]	[-2.19]	[-2.27]	[-2.8]	[1.90]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	55,688	17,765	16,680	55,691	55,691	53,012	55,789	55,413
R^2	0.1041	0.0629	0.0139	0.2164	0.1577	0.0987	0.3206	0.2258

Table 8 – Second stage IV regressions: likelihood of pure takeover bid

This table reports the results from second stage IV regressions of firm policy and performance variables on the likelihood of pure takeover events in which a takeover bid is announced but not within twelve months of a hedge fund intervention. The sample period is 1994-2007. The dependent variables are shareholder payout, CEO pay, CEO turnover, cash holdings, leverage ratio, capital expenditures, R&D expenses, and ROA. The key explanatory variable is the likelihood of pure takeover bid events estimated from either a first stage Probit model, Panel A, or linear probability model, Panel B. Control variables include natural log of market cap, sales, and the book-to-market ratio as well as industry fixed effects and year fixed effects. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. *, **, *** denote statistical significance at the ten, five, and one percent levels, respectively.

Panel A: Probit model

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$\widehat{TKONLY}$	0.631***	-23.00*	-0.428	-0.082	-0.284***	-0.356***	0.036	0.184***
	[4.98]	[-1.65]	[-1.15]	[-1.05]	[-3.10]	[-2.80]	[1.24]	[2.58]
Controls_{-1}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	55,645	17,765	16,680	55,648	55,648	55,497	55,746	55,370
R^2	0.0561	0.0177	0.0125	0.3561	0.0391	0.1416	0.4683	0.2574

Panel B: Linear probability model

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$\widehat{TKONLY}$	1.530***	-96.51	-3.160	-0.939***	-1.182***	-1.819***	-0.471***	2.131***
	[2.92]	[-0.82]	[-0.97]	[-2.87]	[-2.95]	[-3.15]	[-3.26]	[3.29]
Controls_{-1}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	55,688	17,765	16,680	55,691	55,691	53,012	55,789	55,413
R^2	0.0381	0.0152	0.0118	0.1476	0.1882	0.1141	0.3297	0.2024

Table 9 – Direct OLS regressions: above-median likelihood of hedge fund activism

This table reports the results from OLS regressions of firm policy and performance variables on a dummy variable $ABVMED$, which equals one if the likelihood of hedge fund activism is above median and zero otherwise. The dependent variables are shareholder payout, CEO pay, CEO turnover, cash holdings, leverage ratio, capital spending, R&D expenditures, and ROA. Control variables include natural log of market capitalization, sales, and the book to market ratio as well as industry fixed effects and year fixed effects. The sample period is 1994 2007. Panel A reports the results when the likelihood of hedge fund activism is estimated from the probit model. Panel B reports the results when the likelihood of hedge fund activism is estimated from the linear probability model. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

Panel A: Probit model

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
ABVMED	0.841***	-25.57**	-0.103	0.352	-0.177	-0.137***	-0.358**	0.872***
	[2.76]	[2.34]	[-1.05]	[1.40]	[-0.45]	[-2.74]	[-2.09]	[3.44]
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	59,375	18,394	18,823	59,623	59,615	58,427	36,682	59,221
R^2	0.2186	0.3993	0.0413	0.3696	0.2205	0.1425	0.4484	0.3685

Panel B: Linear probability model

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
ABVMED	0.984***	-13.8	-0.187	-0.148	-0.264	-0.824	-0.405	0.553**
	[3.29]	[1.15]	[-1.57]	[-0.57]	[-0.01]	[-1.63]	[-0.22]	[2.13]
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	59,375	18,394	18,823	59,623	59,615	58,427	36,682	59,221
R^2	0.2186	0.3992	0.0413	0.3696	0.2205	0.1424	0.4483	0.3684

Table 10 – Second stage IV regressions: quartile likelihood of HFA

This table reports the results from second stage IV regressions of firm policy and performance variables on a set of quartile dummy variables $QTLi$ (i = 2, 3, or 4), which equals one if the likelihood of hedge fund activism falls into the ith quartile and zero otherwise. The dependent variables are shareholder payout, CEO pay, CEO turnover, cash holdings, leverage ratio, capital spending, R&D expenditures, and ROA. Control variables include natural log of market capitalization, sales, and the book to market ratio as well as industry fixed effects and year fixed effects. The sample period is 1994 2007. Panel A reports the results when the likelihood of hedge fund activism is estimated from the probit model. Panel B reports the results when the likelihood of hedge fund activism is estimated from the linear probability model. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

Panel A: Probit model

QTLDUM	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
QTL2	0.575	-41.67***	0.007	-0.074	-0.417	-1.774***	0.018	-0.016
	[1.61]	[-3.20]	[0.72]	[-0.25]	[-0.96]	[-2.97]	[0.08]	[-0.05]
QTL3	1.598***	-62.74***	0.014	-0.404	-0.574	-3.106***	-0.329	0.859**
	[3.07]	[-3.75]	[0.98]	[-1.07]	[-0.99]	[-4.10]	[-1.20]	[2.19]
QTL4	2.209***	-99.39***	0.005	-0.990**	-0.204	-1.828*	0.027	0.968*
	[3.00]	[-4.33]	[0.17]	[-2.06]	[-0.26]	[-1.81]	[0.59]	[1.90]
$\text{Controls}_{\text{-}1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	59,375	18,394	18,596	59,623	59,615	58,427	36,682	59,221
R^2	0.2187	0.3999	0.0223	0.3696	0.2205	0.1428	0.4485	0.3685

Panel B: Linear probability model

QTLDUM	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
QTL2	0.593	-8.792	0.005	-1.733***	0.028	-3.567***	-0.369*	1.509***
	[1.64]	[-0.71]	[0.56]	[-6.00]	[-0.82]	[-6.22]	[-1.81]	[5.22]
QTL3	1.397***	-3.390	0.014	-1.876***	-0.265	-4.445***	-0.380	2.115***
	[2.88]	[-0.18]	[0.46]	[-4.46]	[-0.43]	[-5.50]	[-1.30]	[5.18]
QTL4	1.784***	-55.43	0.108	-0.870	1.639*	-3.698***	0.395	2.159***
	[2.69]	[-1.62]	[0.48]	[-1.42]	[1.80]	[-3.12]	[0.90]	[3.56]
$\text{Controls}_{\text{-}1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	59,375	18,394	18,596	59,623	59,615	58,427	36,682	59,221
R^2	0.2186	0.3994	0.0221	0.3702	0.2207	0.1431	0.4485	0.3687

Table 11 – Direct OLS regressions: excluding actual targets

This table reports the results from OLS regressions of firm policy and performance variables on lagged mutual fund fire sales, excluding actual targets of hedge fund activism (Panel A), takeover (Panel B), and both (Panel C). The sample period is 1994-2007. The dependent variables are shareholder payout, CEO pay, CEO turnover, cash holdings, leverage ratio, capital expenditures, R&D expenses, and ROA. Control variables include natural log of market cap, sales, and the book-to-market ratio as well as industry fixed effects and year fixed effects. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. *, **, *** denote statistical significance at the ten, five, and one percent levels, respectively.

Panel A: Excluding HFA targets

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
MFFS$_{-1}$	0.412***	-15.42***	-0.704***	0.243***	-0.357***	-0.505***	-0.239***	0.655***
	[5.58]	[6.46]	[-4.04]	[5.01]	[-4.05]	[-7.82]	[-7.43]	[14.83]
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	57,586	17,851	18,256	57,845	57,837	56,673	35,647	57,455
R^2	0.2243	0.3967	0.0429	0.3755	0.2206	0.1436	0.4489	0.3734

Panel B: Excluding takeover targets

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
MFFS$_{-1}$	0.391***	-15.81***	0.730***	-0.252***	-0.334	-0.466***	-0.229	0.646***
	[4.72]	[5.62]	[-3.42]	[-4.70]	[-3.99]	[-6.40]	[-6.30]	[13.02]
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	43,738	11,952	12,203	59,623	43,936	43,931	27,282	43,662
R^2	0.2231	0.3961	0.0518	0.3749	0.2163	0.1442	0.4488	0.3802

Panel C: Excluding either HFA or takeover targets

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
MFFS$_{-1}$	0.396***	-15.78***	0.721***	0.235***	-0.314***	-0.481***	-0.221***	0.641***
	[4.67]	[5.46]	[-3.33]	[4.38]	[-4.05]	[-6.55]	[-6.00]	[12.71]
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	42687	11,661	11,900	42,881	42,876	42,070	26,664	42,612
R^2	0.2254	0.3967	0.0521	0.3755	0.2164	0.1450	0.4487	0.3812

Table 12 – Direct OLS regressions: Execucomp subsample

This table reports the results from OLS regressions of firm policy and performance variables on lagged mutual fund fire sales, for the subsample of firms in the Execucomp database. The sample period is 1994-2007. The dependent variables are shareholder payout, CEO pay, CEO turnover, cash holdings, leverage ratio, capital expenditures, R&D expenses, and ROA. The key explanatory variable is lagged mutual fund fire sales. The control variables include natural log of market cap, sales, and the book-to-market ratio as well as industry fixed effects and year fixed effects. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. *, **, *** denote statistical significance at the ten, five, and one percent levels, respectively.

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$MFFS_{-1}$	0.570**	-15.77***	-0.844***	-0.644***	-0.493***	-0.617***	-0.376***	0.731***
	[3.88]	[-6.77]	[-3.35]	[-7.08]	[-3.35]	[-5.13]	[-7.22]	[9.75]
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	18,496	18,394	59,758	18,526	18,526	18,274	11,781	18,426
R^2	0.2198	0.4016	0.0414	0.4259	0.3555	0.2512	0.4729	0.3505

Table 13 – Direct OLS regressions: including additional control variables

This table reports the results from OLS regressions of firm policy and performance variables on lagged mutual fund fire sales, with the inclusion of additional control variables. The sample period is 1994-2007. The dependent variables are shareholder payout, CEO pay, CEO turnover, cash holdings, leverage ratio, capital expenditures, R&D expenses, and ROA. The key explanatory variable is lagged mutual fund fire sales. The control variables are lagged natural log of market cap, sales, and the book-to-market ratio, as well as industry fixed effects and year fixed effects. Additional control variables are lagged shareholder payout, CEO pay, cash holdings, leverage ratio, capital expenditures and R&D expenses. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. *, **, *** denote statistical significance at the ten, five, and one percent levels, respectively.

	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$MFFS_{-1}$	0.144***	-9.34***	-0.655***	0.211***	-0.139***	0.125	-0.054***	0.646***
	[2.81]	[-3.96]	[-3.10]	[-4.54]	[-1.91]	[1.02]	[-2.70]	[8.13]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additionals	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	12,904	11,486	12,270	12,251	12,270	12,183	12,133	12,199
R^2	0.5068	0.4275	0.0351	0.8082	0.7743	0.4626	0.7572	0.4069

Table 14 – Second stage IV regressions: likelihood of non-hostile hedge fund activism

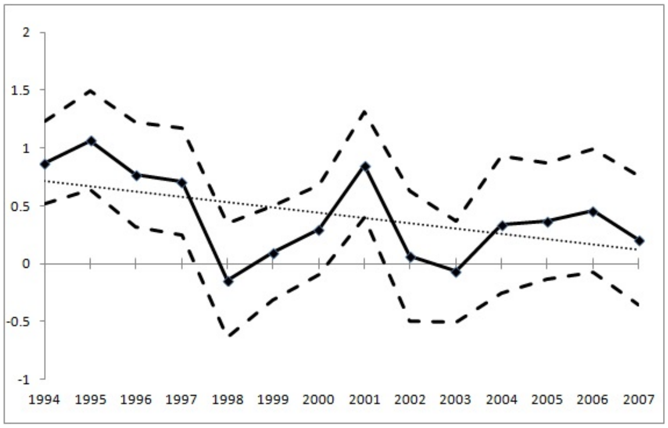
This table reports the results from the second stage IV regressions of firm policy and performance variables on the likelihood of non-proxy hedge fund activism (Panel A) and non-hostile hedge fund activism in general (Panel B). The likelihoods are estimated form first stage linear probability models. The sample period is 1994-2007. The dependent variables are shareholder payout, CEO pay, CEO turnover, cash holdings, leverage ratio, capital expenditures, R&D expenses, and ROA. Control variables include natural log of market cap, sales, and the book-to-market ratio as well as industry fixed effects and year fixed effects. Standard errors are clustered at the firm level. Parentheses enclose t-statistics. *, **, *** denote statistical significance at the ten, five, and one percent levels, respectively.

Panel A: Non-proxy HFA events

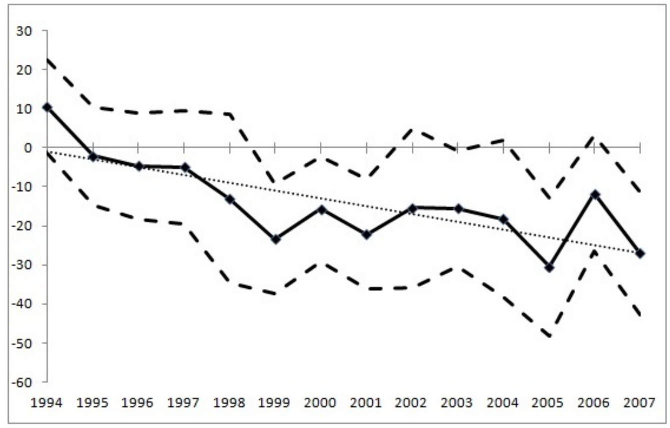
	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$\widehat{NONPROX}$	0.539***	-3.627	0.520	-0.522***	0.221	0.073	-0.322**	0.622***
	[2.57]	[-0.82]	[1.33]	[-3.13]	[0.95]	[0.27]	[-2.47]	[3.77]
$\text{Controls}_{\text{-}1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	55,697	17,148	18,332	55,939	55,931	54,841	35,109	55,568
R^2	0.0937	0.3872	0.0389	0.2660	0.2076	0.0716	0.3423	0.2239

Panel B: Non-hostile HFA events

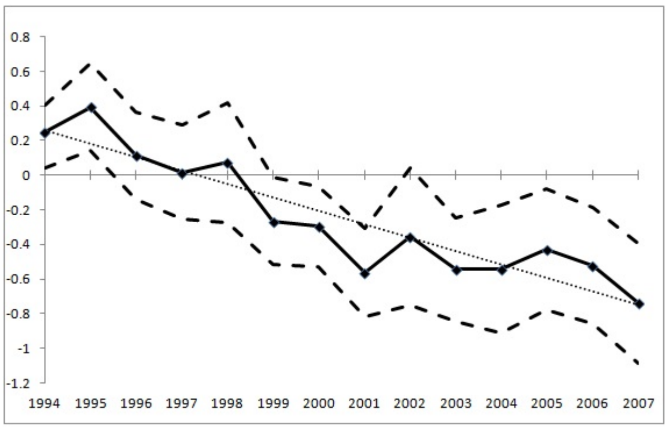
	PAYOUT	CEOPAY	TURN	CASH	LVRG	CAPX	RD	ROA
$\widehat{NONHOST}$	0.724***	-9.469*	0.469	-0.658***	0.024	-0.018	-0.442***	0.856***
	[2.87]	[-1.70]	[1.07]	[-3.36]	[0.09]	[-0.06]	[-2.72]	[4.12]
$\text{Controls}_{\text{-}1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IYFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	55,367	17,114	18,294	55,606	55,598	554515	34,983	55,238
R^2	0.0502	0.3370	0.0385	0.2064	0.2126	0.1385	0.2500	0.0951



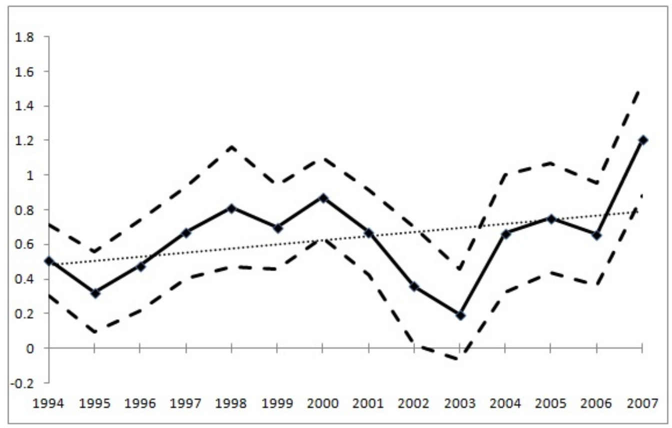
Payout



CEO Pay



Cash



ROA

Figure 1 – The preventive effect over time

This figure shows the time-series changes in the preventive effect of hedge fund activism on a set of four policy and performance variables: shareholder payout, CEO pay, cash holdings, and return on asset. The sample period is 1994 2007. The effects on other policy variables are largely stable during the sample period and therefore not included for brevity. Solid lines represent estimated slopes. Dashed lines market the 95% confidence intervals. Dotted lines are the linear fits.

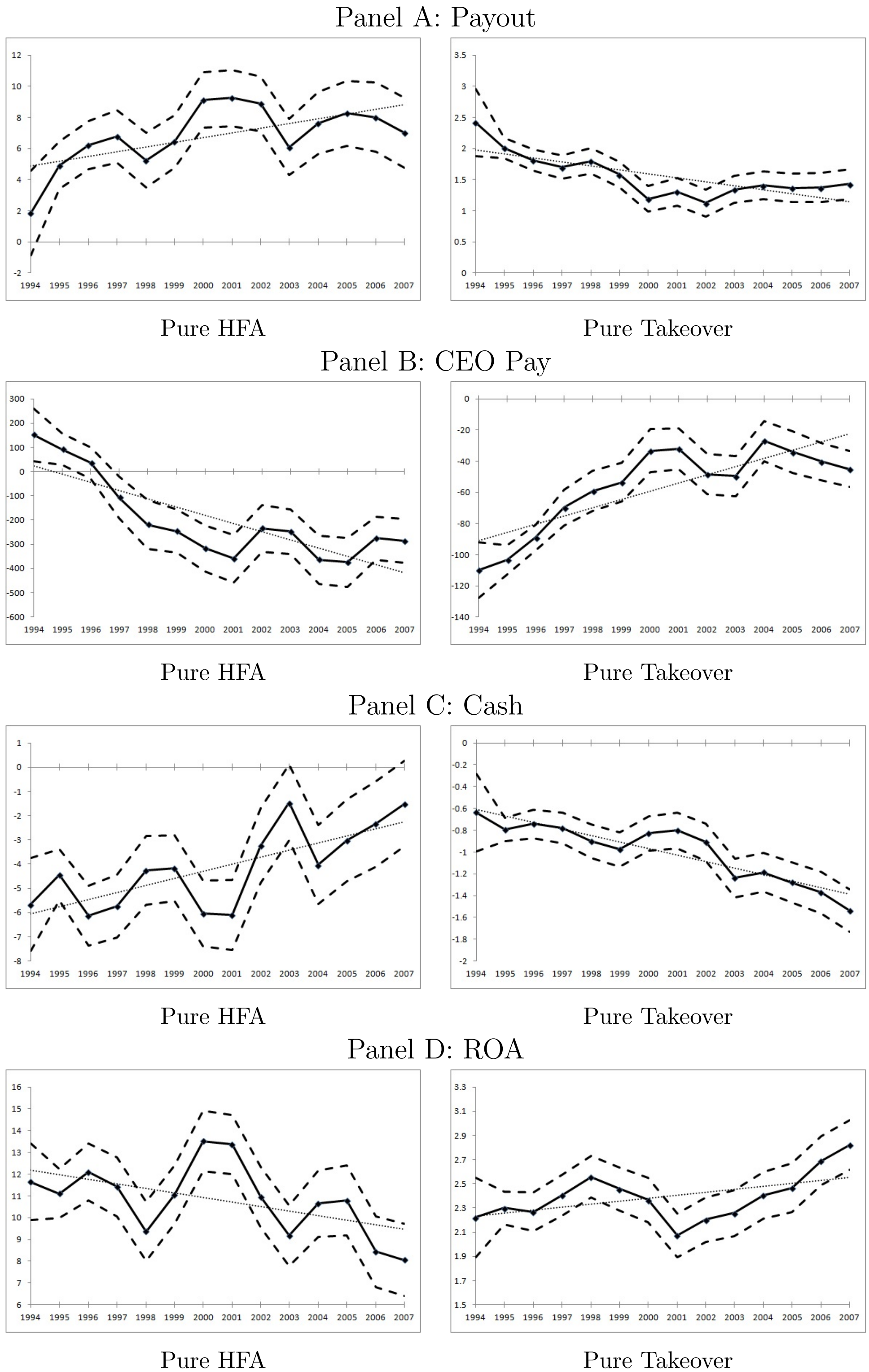


Figure 2 – The preventive effect over time: hedge fund activism versus takeover bid

This figure plots the time-series changes in the preventive effect of pure hedge fund activism as well as the preventive effect of pure takeover bids on a set of four policy and performance variables: shareholder payout, CEO pay, cash holdings, and return on asset. The sample period is 1994 2007. The effects on other policy variables are largely stable during the sample period and therefore not included for brevity. Solid lines represent estimated slopes. Dashed lines market the 95% confidence intervals. Dotted lines are the linear fits.