Corportate Governance and Institutional Trading

by

Heqing Zhu

Business Administration
Duke University

Date:_____
Approved:

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Alon Brav, Supervisor

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Manuel Adelino

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John Graham

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Qi Chen

Dissertation submitted in partial fulfillment of
the requirements for the degree of Doctor
of Philosophy in Business Administration in the Graduate School
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ABSTRACT

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Abstract

This dissertation includes two empirical studies. The first examines the preventive effect of hedge fund activism against corporate policy deviations. Using stock liquidity and mutual fund fire sales as instruments, I find that when the likelihood of hedge fund activism increases, firms respond by paying shareholders more and CEOs less, stocking less cash and leveraging more, and increasing investment into research and development while cutting capital expenditures. These results imply that the impact of hedge fund activism on corporate policy is greater than previously documented, reaching beyond the handful of firms that are actually targeted to all firms that face such a threat. The second study examines the qualification of flow-induced mutual fund trading as an instrument for price pressure and by extension for corporate events whose occurrence is affected by stock price. I find that liquidity-strapped mutual finds tend to sell more widely across all portfolio holdings, which exert significant downward pressure on stock price. However, I also find evidence that mutual fund fire sales may not be totally free of information. All in all, the results advise caution in the use of mutual fund fire sales as an instrument that is exogenous to firm fundamentals.

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The Preventive Effect of Hedge Fund Activism

1.1 Introduction and literature review

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 relatively 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 against 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.

Using stock liquidity and mutual fund fire sales as instruments, I find that when the likelihood of hedge fund activism increases, firms respond by paying shareholder more and CEOs less, stocking less cash and leveraging more, and increasing investment into research and development while cutting capital expenditures. 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. These relationships are causal, significant, and robust to alternative instrument measures, model specifications and sample divisions.

The results of this study imply that hedge fund activism has a stronger impact on target firms than previous documented, as policy improvements occur not only after intervention but also in anticipation of intervention. More importantly, the impact of hedge fund activism is broader than previously documented, reaching beyond the handful of firms that actually undergo intervention to all firms that face such a threat.

To implement this study, I use a comprehensive hand-collected sample of hedge fund activism events from 1994 to 2011. 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 via a two-stage least squares regression approach with an instrument or instruments for intervention likelihood. As a robustness check, I also estimate intervention likelihood using a probit model in the first stage. The corporate policies examined include (1) shareholder distribution policy, characterized by the payout ratio, (2) financial policy, characterized by cash holdings and the leverage ratio, (3) investment policy, characterized by capital expenditures and

research and development expenses, and (4) CEO compensation policy, characterized by CEO pay and CEO turnover.

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 intervention likelihood is an endogenous regressor in the policy regressions. The endogeneity primarily arises from reverse causality. While an increase in intervention likelihood reduces policy deviation, the reduction in policy deviation in turn lowers the likelihood of intervention. The exogenous effect of intervention likelihood 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 intervention likelihood. For example, a change that weakens the effectiveness of the board of directors could simultaneously increase policy deviation as well as intervention likelihood and thereby attenuate the negative relationship between the two.

I address these two concerns by instrumenting for intervention likelihood. The primary instrument I use is a variant of the Amihud (2002) stock illiquidity ratio. (Alternative stock liquidity measures lead to similar results.) Stock liquidity facilitates hedge fund activists' acquisition of a stake in the target company. All else equal, an increase in stock liquidity should therefore lead to an increase in intervention likelihood.

Two sets of direct OLS regressions of policy variables against the lagged stock liquidity, performed over two different sample periods, provide support for the instrument's satisfaction of the exclusion criterion. First, during the "placebo" period of 1988 to 1992 when there is virtually no external intervention, regression results show no significant correlation between stock liquidity and policy variables. On the other hand, during the main sample period of the study from 1994 to 2011, the relationship between stock liquidity and policy variables is always significant and in the expected

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direction. These results suggest that stock liquidity is unlikely to be correlated with policy variables through channels other than affecting the likelihood of intervention.

Results from direct OLS regressions of policy variables against the lagged stock liquidity instrument show that a one standard deviation increase in stock liquidity (or decrease in stock illiquidity) leads to the following changes in policy variables from their respective sample averages: shareholder payout increases by 6.42%, cash stock decreases by 0.88% while the leverage ratio increases by 1.13%, and investment into physical assets decreases by 1.02% while investment into research and development increases by 3.98%. These results are consistent with the effectiveness of intervention threat in curbing managerial cash hoarding and empire building behaviors. Unsurprisingly, overall firm performance also improves: ROA increase by 24.79%.

Another potential source of endogeneity is measurement error. It arises here because intervention likelihood is an unobservable variable that must 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 intervention likelihood can be estimated with error, there is no obvious reason to suspect that estimation errors would be systematically biased upward or downward. Assuming random errors, upward-biased errors and downward-biased errors tend to cancel each other out in a large sample and therefore have negligible effects on regression results. Nevertheless, standard errors are adjusted to allow for correct statistical inference.

Results from two-stage least squares regressions affirm those from direct OLS regressions of policy variables against lagged instrumental variable. In most second stage policy regressions, the coefficient on intervention likelihood (estimated in the first stage regression) has the expected sign and is significant. In response to an increase in intervention likelihood, firms increase payout to shareholders and decrease payout to the CEO. The wealth transfer from managers to shareholder associated with hedge fund interventions is also documented by Brav, Jiang, Partnoy and Thomas

(2008). In that study, however, the wealth reallocation occurs only after actual intervention, indicative of the *corrective* effect hedge fund activism has against policy deviations. Here, the reallocation occurs before or even absent of intervention, indicative of the *preventive* effect hedge fund activism has against policy deviations.

According to the governance literature, cash encourages managers to be complacent and even destroy firm value through perks and empire-building. On the other hand, leverage imposes financial discipline on management to be efficient and consistently produce profits in order to meet interest payments. Therefore, the decrease in cash holdings and increase in leverage in response to an increase in intervention likelihood also reflect firms' proactive efforts to improve governance.

In terms of investment policy, an increase in intervention likelihood leads firms to direct investment away from fixed assets and toward research and development. On the one hand, this finding indicates that the threat of hedge fund activism is effective in curbing managers' empire-building tendencies. On the other hand, this finding also contradicts a popular criticism of hedge fund activists, that they push managers to focus on the present, jeopardizing the long term welfare of the firm. In fact, the threat of hedge fund activism pushes risk-averse managers to intensify investment into research and development, which increases firms' potential to discover new investment opportunities and remain competitive in the future.

The result for CEO turnover is relatively weak but expectedly so. An increase in intervention likelihood leads to an increase in CEO turnover but with only borderline statistical significance. This is not surprising since improving job security is likely to be the primary motivation behind managers' efforts to avoid intervention. In some cases, however, a CEO may still be pressured to resign, by the board of directors or other shareholders. Finally, given the improvement in governance as a result of these changes in firm policies, it is not surprising that the overall performance of the firm as measured by return on assets also improves significantly.

To strengthen the findings of this study, I employ a second instrument which more directly affects the cost of intervention: mutual fund fire sales. Coval and Stafford (2007) show that mutual funds are forced to liquidate holdings in response to large capital outflows. Flow-induced selling of stocks held in common by many mutual funds generates significant downward price pressure. Twelve months before the event quarter of significant mutual fund selling due to large capital outflow, the average monthly abnormal return of fire sale stocks (stock return in excess of the equal-weighted average return of all stocks held by mutual funds at the beginning of the month) is –1.19% with a t-statistic of –1.54. The monthly cumulative average abnormal return (CAAR) becomes more negative with increasing significance until the event quarter in which it reaches –14.99% with a t-statistic of –4.39. Importantly, after the event quarter, CAAR slowly reverses. Eighteen months after the event quarter, CAAR reaches –5.34% with a t-statistic of –0.40.

The temporary price drop induced by mutual fund fire sales makes it cheaper for the activist to build a meaningful stake in the target firm to carry out an intervention. All else equal, this direct reduction of intervention cost increases the likelihood of hedge fund activism. Mutual fund fire sales thus satisfy the relevance criterion of a valid instrument for intervention likelihood. Since mutual fund fire sales are by definition flow-induced, it should be relatively free of information compared to information-based sales that occur in routine portfolio adjustments. In a separate paper, I find evidence that mutual funds that experience large capital outflows sell more widely across all portfolio holdings compared to funds that face no such liquidity strain, indicating that liquidity-strapped mutual funds are forced to sell indiscriminately rather than selectively based on information.

To enhance the exogeneity of mutual fund fire sales to firm fundamentals, I construct "mechanical" mutual fund fire sales, $MFFS$, following Edmans, Goldstein and Jiang (2012). The construction rests on the assumption that mutual funds facing

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large capital outflows mechanically liquidate all portfolio holdings based on their respective weights in the portfolio. This eliminates the concern that liquidity-strapped mutual funds manage to dispose holdings based on information, concentrating sales in the "worst" stock before moving onto the second "worst" stock.

I perform the same exercise as with the stock liquidity instrument to check mechanical mutual fund fire sales' satisfaction of the exclusion criterion. I regress policy variables against lagged $MFFS$ over two different sample periods. Over the 1988 to 1992 "placebo" period of little intervention activity, there is no significant link between $MFFS$ and policy variables. On the other, during the main sample period of the study from 1994 to 2011, the relationship between $MFFS$ and policy variables is always significant and in the expected direction. I also verify that $MFFS$ has a significant and positive effect on intervention likelihood by regressing the hedge fund activism dummy, equaling one if intervention is announced and zero otherwise, against lagged $MFFS$.

One potential concern with using market conditions to instrument for the likelihood of hedge fund activism is that what is favorable to interventions is also likely to be favorable to takeovers. For example, the temporary price drop included by mutual fund fire sales facilitates not only hedge fund activists in building an intervention stake but also corporate raiders in acquiring a controlling stake. Edmans, Goldstein and Jiang (2012) find a positive link between mutual fund fire sales and takeover likelihood. This introduces additional complexity to the present study. I must verify that the preventive effect of hedge fund activism is not in fact the preventive effect of corporate takeover, a concern consistent with the finding in Cyert, Kang, and Kumar (2002) that the threat of takeover is effective in constraining CEO compensation.

I isolate the preventive effect of hedge fund activism from that of takeover by the following procedure. I collect data on takeover bids over the same sample period as the hedge fund activism data, from 1994 to 2011. I cross-reference the sample of

takeover bids with the sample of hedge fund interventions and identify three types of events: (1) pure hedge fund activism events in which a hedge fund intervention is announced but not within twelve months of a takeover bid announcement; (2) pure takeover events in which a takeover bid is announced but not within twelve months of a hedge fund intervention announcement; (3) dual events in which a hedge fund intervention and a takeover bid are announced within twelve months of each other. I then run separate sets of two stage least squares regressions, one for the sample of pure hedge fund activism events and one for the sample of pure takeover events. A comparison of results indicate that both intervention likelihood and takeover likelihood have significant effects on policy variables, in other words both hedge fund activism and corporate takeover have a preventive effect against policy deviations. Furthermore, the results show the threat of hedge fund activism as well as that of takeover are standalone channels through which financial markets can impact corporate policy.

This study contributes to the existing literature in several ways. First, it shows that as a mechanism for corporate governance, hedge fund activism is effective in *preventing* corporate policy deviations in addition to correcting them should they occur. Proactive response is rarely examined in studies on hedge fund activism, which means the impact of hedge fund activism is assessed without considering the preventive effect. Brav, Jiang, Partnoy and Thomas (2008), Greenwood and Schor (2009), Klein and Zur (2011), Clifford (2008) and Huang (2009) all fall into this category. The only exceptions of which I am aware are Fos (2013) and Gantchev, Gredil and Jotikasthira (2014). The first focuses on proxy contests, emphasizing intervention tactic rather than activist identity. The latter, although studies hedge fund activism, assumes that any preventive effect that exists is limited to firms in the same industry as target firms. However, many of the changes activists seek to implement in target firms are not industry-specific (Brav, Jiang, Partnoy and Thomas (2008)). And we know from anecdotal evidence that activist hedge funds

do not constrain intervention activity to any one industry. For example, activist hedge fund Pershing Square has launched intervention against companies as diverse as Target, Wendy's, and Barnes and Noble. In the current study, I do not make such an assumption, all firms whether or not in target industries could potentially become a hedge fund activism target.

Second, this study provides evidence that hedge fund activism affects not only actual targets but also potential targets. This finding is important given the small number of hedge fund interventions. Although a broader effect of hedge fund activism is frequently speculated and supported by numerous anecdotal stories, previous studies for the most part focus only on target firms. Non-target firms that share similar characteristics with target firms are included in those studies only as a benchmark against which the policy response of target firms is measured. In contrast, the effect of hedge fund activism on non-target firms, whether or not they are similar to target firms, is a central concern in this study.

Third, the results of this study suggest that hedge fund activism has a stronger effect on target firms than previously documented. In previous studies, the impact of hedge fund activism on target firms' policies is typically measured using two simple comparisons: before-and-after intervention comparison and target-and-match firm comparison. If firms proactively self-correct policy deviations when they perceive increased threat of intervention, benchmarking policy after intervention materializes against levels immediately before or at the time of intervention would lead to an underestimation of the full impact of intervention on target firms. Similarly, if firms that share similar characteristics with target firms face a high likelihood of becoming the next targets and proactively adjust their policies to lower that likelihood, benchmarking policy changes in target firms against those in match firms would also result in an underestimation of the impact of hedge fund activism on target firms.

Fourth, this study provides at least a partial explanation for the differences in the

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existing literature concerning the corrective effect of hedge fund activism on policy deviations. While studies on hedge fund activism consistently find positive abnormal stock returns around the announcement of intervention, not all of them link the incremental share value to subsequent policy improvements. Greenwood and Schor (2009) attribute it to higher post-intervention acquisition likelihood. Sunder, Sunder, and Wongsunwai (2010), Li and Xu (2010), and Klein and Zur (2011) argue that it is simply a result of wealth redistribution at the expense of bondholders. But Brav, Jiang, Partnoy and Thomas (2008) and Brav, Jiang, and Kim (2012) show that it is at least partially attributable to anticipated policy improvements. These differences are not surprising when corporate policy is proactive, in which case the extent of post-intervention policy changes depends on the degree to which interventions are anticipated. They should be be small for anticipated interventions and big for unanticipated ones. If the distribution of anticipated and unanticipated events varies across samples and intervention likelihood is not controlled for, empirical results on post-intervention policy changes may differ.

Finally, this study contributes to the long-standing debate over whether financial markets have real effects on corporate events. For example, early studies such as Palepu (1986) and Ambrose and Megginson (1992) find that market valuation has no effect on takeover probabilities. Recently however, Edmans, Goldstein and Jiang (2012) use mutual fund fire sales as an instrument for price change and find a significant and positive link with takeover likelihood. This study extends their work and show that financial markets can discipline managers by facilitating hedge fund interventions as well as corporate takeovers.

The rest of the paper is organized as follows: Section 2 describes the empirical methodology, including model specification, identification and data, and introduces preliminary evidence for the preventive effect of hedge fund activism. Section 3 presents the main results on the causal relationship between intervention likelihood

and corporate policies. Section 4 describes additional analyses showing the robustness of the main results to alternative model specifications and sample divisions. Section 5 concludes.

1.2 Methodology

1.2.1 Identification

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 will 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 intervention likelihood. Unfortunately, this is not possible. The major obstacle is the endogeneity arising from reverse causality. An increase in the likelihood 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 link from intervention likelihood to policy variables. Endogeneity can also arise from the omission of variables correlated with both policy variables and intervention likelihood. 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

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the two.

To address these problems, I instrument for the likelihood of hedge fund activism using two instruments. First, following Fos (2013), I use a variant of the Amihud (2002) illqiudity ratio. Stock liquidity should be relevant to intervention likelihood, because high stock liquidity allows activists to accumulate stakes in target companies quickly and without significantly affecting the stock price. A larger stake directly increases the influence of activists by giving them greater voting power. As a sign of activists' confidence in their plans for the target company, it could also indirectly empower activists by enhancing their credibility among other investors. This is consistent with the findings in Brav, Jiang, Partnoy and Thomas (2008) that activists tend to target companies with higher institutional ownership and trading liquidity. The authors also find that the likelihood of intervention success is positively correlated with activists' ownership stake. Collin-Dufresne and Fos (2013) show that activists purchase target firms' shares more aggressively when stock liquidity is high. Regression results presented later in this study confirm the significant and positive relationship between stock liquidity and intervention likelihood.

In addition to the widely used Amihud (2002) illiquidity ratio, I construct six other stock liquidity measures following Hasbrouck (2009) and Lou and Shu (2014): illiquidity ratio defined by yearly average of daily absolute value of returns divided by dollar trading volume; square root of the Amihud illiquidity ratio; the Amivest liquidity ratio: yearly average of daily dollar trading volume divided by absolute value of returns; square root of Amivest; volume-based illiquidity measure: yearly average of one divided by daily dollar trading volume; return and turnover-based illiquidity measure: yearly average of daily absolute value of returns divided by share turnover (shares traded divided by shares outstanding); and finally, turnover-based illiquidity measure: yearly average of one divided by daily share turnover. In the appendix I show mathematically how each measure is constructed. Using data from 1987 to 2011,

the correlation among the five illiquidity measures are between 70% to 90%, while the two Amivest liquidity measures has correlation around 90%. Not surprisingly, the qualitative results using each of these measures are largely consistent. As such, for the sake of space, in the subsequent analyses I only report results using the square root Amihud measure.

Figure 3.1 plots stock liquidity leading up to intervention year, using the Amihud illiquidity ratio and its square root. The trends are consistent with expectation: stock liquidity increases (the Amihud illiquidity measures decrease) before activists launch interventions. This suggests that stock liquidity matters for intervention likelihood.

While it is difficult to prove the exogeneity of any instrument, I perform the following exercise following Fos (2013), which suggests if not proves that stock liquidity as an instrument for intervention likelihood meets the exclusion criterion. I regress policy variables against stock liquidity and control variables over two different periods: a "placebo" period from 1988 to 1992 and the sample period of this study from 1994 to 2011. The placebo period is a time during which external interventions are virtually impossible, with the passage of antitakeover laws and firms' adoption of antitakeover provisions in the late 1980s and prior to SEC's proxy reform in 1992 which reduced the barrier of communication among shareholders. Intuitively, if the effect on intervention likelihood is the main channel through which stock liquidity impacts corporate policy and performance, we should not observe any significant relationships between stock liquidity and corporate policy and performance variables where there is little intervention activity.

The results are as expected. Although correctly signed for the most part, the coefficient on stock liquidity is never statistically significant in the policy and performance regressions during the 1988-1992 placebo period. On the other hand, the coefficient on stock liquidity always has the expected sign and is statistically significant during the 1994-2011 sample period.

Results during the early eighties period are omitted, as that was a time of rampant takeover activity the threat of which undoubtedly has an effect on corporate policy and performance. After the min-nineties, however, the incidence of hedge fund intervention grew at a much faster rate than takeovers since anti-takeover provisions were still in place. Moreover, takeover as a governance mechanism is relatively limited in reach as the enormity of the cost of taking over a large company like Microsoft or Proctor and Gamble would dissuade such an attempt. Hedge fund activists on the other hand can and do seek changes in target firms with minority stakes. Finally, the goal of a takeover may not be governance-related at all, for example often the goal is to gain access to the target firm's distribution channels, customer base, brand name, or technology. In short, hedge fund activism has become a foremost mechanism for corporate governance and the effect on its likelihood is an increasingly important channel through which stock liquidity can affect firm policy and performance.

Although the Amihud stock illiquidity ratio is a measure of general stock illiquidity rather than unexpected liquidity shock, the "placebo test" eases the concern that is correlated with firm policy through channels other than the likelihood of hedge fund activism. While there have been studies that find significant correlation between stock liquidity and corporate policy (Amihud and Mendelson 1991; Banerjee, Gatchev and Spindt 2007), they have not eliminated the possibility that intervention likelihood is the primary channel through which the correlation exists. Nevertheless, I control for firm, industry and time effects that might confound inference. I also check the results using an alternative instrument: mutual fund fire sales.

Mutual fund fire sales occur when mutual funds are forced to liquidate their portfolio stocks in order to meet redemption requests by their own investors. The downward price pressure from large mutual fund sales allows hedge fund activists to buy a large 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 instrumental variable. Gantchev and Jotikasthira (2014) 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, Goldstein and Jiang (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 eliminates the concern that mutual funds select stocks to sell based on information.

Specifically, mutual fund fire sales are constructed as follows:

$$MFFS_{i,t} = \frac{\sum_{j=1}^{m} MFOF_{j,t} \times \frac{SHARES_{i,j,t-1} \times PRC_{i,t-1}}{TA_{j,t-1}}}{VOL_{i,t}} \ , \qquad (1.1)$$

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 might not be a serious problem given the evidence in Coval and Stafford (2007) and the current study that mutual funds that experience large capital outflows in reality do sell more widely across all portfolio holdings relative to mutual funds that do not. 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 bigger gain in terms of the exclusion criterion. Although (actual) mutual fund fire sales by definition are flow-induced and therefore already relatively free of information, mechanical mutual fund fire sales further reduces the role of information. Therefore, the direct link between mutual fund fire sales and firm policies is even weaker when mutual fund fire sales are computed "mechanically". This makes it more desirable in terms of the exclusion criterion.

Formal regression results presented in the next section shows that mutual fund fire sales, just as stock liquidity, pass the checks for relevance and exclusion. Mutual fund fire sales has a positive and strong relationship with intervention likelihood. When policy and performance variables are regressed against mutual fund fire sales over the 1988 to 1992 "placebo" period of little intervention activity, the coefficient on mutual fund fire sales is never significant. When the same regressions are performed over the sample period of this study, from 1994 to 2011, the relation between mutual fund fire sales and all the policy and performance variables is statistically significant and in the expected direction, suggesting that the effect on intervention likelihood is the primary channel through which mutual fund fire sales affect firm policy and performance.

Inarguably, a credible threat of takeover is likely to have an effect on corporate policy. Therefore, one potential concern in instrumenting for the likelihood of hedge fund activism with mutual fund fire sales is that the price drop induced by mutual

fund fire sales also facilitates corporate bidders in acquiring a controlling stake in their target firms. Edmans, Goldstein and Jiang (2012) show that there is indeed a link between mutual fund fire sales and takeover likelihood. To my knowledge no such link has been documented between stock liquidity and the likelihood of hedge fund activism. Nevertheless, the concern remains. In the next section I detail the procedure to reduce the influence of takeover threat on policy outcomes. Readers should keep in mind that since hedge fund activists and corporate bidders are often attracted to the same set of firm characteristics, for example poor governance and performance shortfalls, it is not a given that there exists something that is significantly relevant to one type of event and is at the same time uncorrelated with the other.

1.2.2 Model specification

Equipped with instrumental variables, I proceed to investigate the preventive effect of hedge fund activism on corporate policy using a two-stage least squares approach. In the first-stage, I estimate the likelihood of hedge fund activism using the lagged instrument(s):

$$\text{1st-stage: } HFA_{i,t} = \alpha_{10} + \alpha_{11} Instrument_{i,t-1} + \alpha_{12} X_{i,t-1} + IYFE + \epsilon_{1i,t} , \quad (1.2)$$

where HFA_{it} is an indicator variable equaling 1 if firm i is targeted for intervention in year t and 0 otherwise. Its fitted value, $\widehat{HFA_{it}}$, is retained as the estimated likelihood of hedge fund activism. X represents a set of control variables, including the policy or performance variable on the left hand side of of the second stage regression, 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 (1.2) using probit as well as linear regressions.

The second stage policy regressions take the following form:

$$\text{2nd-stage: } P_{i,t} = \alpha_{20} + \alpha_{21} \widehat{HFA_{i,t}} + \alpha_{22} X_{i,t-1} + IFE + YFE + \epsilon_{2i,t} , \quad (1.3)$$

where P_{it} is a variable measuring firm i's distribution policy, financial policy, invest-ment policy, CEO compensation 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 (1.3) are exactly the ones in the first stage likelihood regression in equation (1.2). This is necessary to ensure that the variation in firm outcome variables comes only from the variation in the instrument, which establishes the causal relationship between firm policy and performance and the likelihood of hedge fund activism. Results are reported in the next section.

The policies I examine in this study are distribution policy, financial policy, investment policy and CEO compensation 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 (Brav, Jiang, Partnoy and Thomas (2008); Klen and Zur (2011)) is that small firms and firms with low market valuation are more likely to be targeted by hedge fund activists. Both firm size (Ai, Kiku and Li (2012); Gabaix, Landier and Sauvagnat (2013)) and market valuation (Wang and Chiu (2012)) 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)). I also control for year fixed effects and industry fixed effects[1]. Variable definitions are listed in the Appendix. They are standard following other studies on hedge fund

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[1] Using firm fixed effects lead to largely the same results which are therefore not reported for the sake of brevity.

activism such as Brav, Jiang, Partnoy and Thomas (2008) and empirical studies of corporate finance in general such as Leary and Roberts (2014).

1.2.3 Data

The data on hedge fund activism are generously provided by Alon Brav who has extended the sample of hedge fund activism events in Brav, Jiang, Partnoy and Thomas (2008). Whereas the original sample covers the period from 2001 to 2006, the extended sample used in this study covers the period from 1994 to 2011. Below is a brief description of the sampling procedure. Readers are referred to the original paper for further details.

The sampling procedure starts from a complete list of Schedule 13D filers. To comply with Section 13D 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, activist investors must also file within 60 days each time there is a material change in the size of ownership 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 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 sam-

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pling 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 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 for the 1994 to 2000 and 2007 to 2011 periods using the same sampling procedure. The final sample used in this study contains 2,461 interventions launched by 460 activist hedge funds against 2,025 target firms during the period from 1994 to 2011. The number of hedge fund interventions grew from just nine in 1994 to 176 in 1998. In the recession, the number of interventions declines every year to 81 in 2001. It then grew with more momentum, peaking in 2007 with 277 interventions that year. During the financial crisis, the number of interventions dropped by almost 50% to 124 in 2009. It grew to 158 in 2010 and declined slightly to 153 in 2011, the last year of the sample period. The evolution in the number of activist hedge funds per year during the 1994 to 2011 sample period mirrors that of interventions. The number of activist hedge funds in 1994 is a mere four, but by 2007 it has grown exponentially (except during the 1998-2001 recession period) to 133. During the financial crisis period, that number dropped to 78 but has grown to 84 in 2010 and 85 in 2011.

Data on stock liquidity are obtained from the CRSP daily data file. To construct mutual fund fire sales, I combine information from two mutual fund databases: from

the CRSP Mutual Fund Database, I compute mutual fund capital flows and identify extreme outflow events as outflow greater than or equal to 5% of the beginning of the period fund assets. From the Thomson Reuters Mutual Fund Holdings database , I identify the equity holdings of mutual funds. I link mutual funds' capital flows to their equity holdings by using MFLINKS, which matches the fund identifiers in CRSP with those in Thomson Reuters. For each stock, I compute mutual fund fire sales as total sales of the stock by all mutual funds holding the stock that experience extreme capital outflows. See equation (1.1) in the previous section for the construction of the variable in detail.

The auxiliary data used in this study come from several sources. Data on firm characteristics are collected from the CRSP/Compustat Merged Fundamentals Annual data file. CEO compensation data are from Compustat's Execucomp database, which contains compensation information for all executives of Standard & Poor's 1500 firms. The variable in Execucomp used to measure CEO pay is the total compensation variable "tdc1". Prior to 2006, this variable represents total realized compensation, whereas 2006 and after, it represents total expected compensation. For consistency, I adjust the pre-2006 values following Walker (2009). Finally, I collect data on takeover bids from the the Securities Data Company's SDC Platinum database.

1.2.4 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 for a set of firm characteristic variables. 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 3.1 describes these characteristic variables as well as other important variables used in this study.

Panel A of Table 3.1 shows the definitions of the variables. Panel B shows the summary statistics for the full sample of firms whole during the 1994 to 2011 sample period. Since variables are defined following the standard in the literature, their summary statistics are consistent with those in other studies.

Table 3.2 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 requirements (3) and (4).

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 smaller 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 3.2 confirms this empirical regularity. Target firms have relatively smaller market capitalization.

Another empirical regularity is that target firms on average have lower market valuation. This is also confirmed in Table 3.2. The importance of market valuation in determining the likelihood of hedge fund activism is intuitive since 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 a firm's distribution policy with the payout ratio, measured by the ratio of total dividend payments to income before extraordinary items. According to Table 3.2, the payout ratio of target firms is lower than that of other firms. This disinclination to pay shareholders is suggestive of potential governance problems, particularly in conjunction with other questionable policy practices as discussed below. Match firms pay shareholders more than target firms, but the difference is not statistically significant.

I characterize capital structure policy with two variables: cash and the leverage. Cash is measured by the sum of cash and short-term investments divided by book assets. Leverage is measured by the sum of long-term debt and short-term debt divided by book assets. Table 3.2 shows that target firms have more cash and less leverage compared to match firms and other firms. This is not surprising given that the agency problem between managers and shareholders is the primary cause for shareholder activism and under-leverage and excess cash are identified in many theoretical and empirical studies as a manifestation of that agency problem.

I characterize a firm's investment policy with its investment in physical assets and in research and development. The former is measured by capital expenditures

divided by lagged property, plant and equipment. The latter is measured by research and development expenses divided by natural log of sales. According to Erickson and Whited (2000) and Fama and French (2002), firms' investments should be positively related to their investment opportunities. Table 3.2 shows that target firms have higher book-to-market ratio or fewer investment opportunities than other firms. Theory says target firms should invest less, but they actually invest more. In particular, target firms spend more on physical assets but less on research and development than other firms, suggesting that target firm managers prefer to expand existing operations rather than look for new technology through research and development. This empire building and risk avoidance behavior are typical manifestations of the agency problem between managers and shareholders. Interestingly, match firms both spend less on physical assets and more on research and development. This may be why they are not targeted despite being in the same industry and having similar size and market valuation as target firms, which hints at the existence of some preventive effect associated with hedge fund activism.

Finally, CEO pay and CEO turnover are used to characterize a firm's CEO compensation policy. CEO pay is measured by Execucomp's total compensation variable "tdc1". An adjustment of the pre-2006 values of this variable is necessary since Execucomp changed the definition of the variable from realized to expected compensation in 2006. I do this following Walker (2009). At first glance, it does not appear that hedge fund activists are targeting firms who overpay their CEOs. According to Table 3.2, target firms pay their CEOs less than do match firms and other firms. Remember however that target firms also tend to be smaller than the other two groups of firms, and it is well-known that firm size is highly correlated with executive compensation. Later regression results show that after controlling for size, intervention likelihood has a negative effect on CEO pay, suggesting that hedge fund activists are concerned about how much firms pay their CEOs. The higher CEO turnover in target firms

relative to other firms is not surprising given inferior overall performance as measured by ROA. The difference between target firm and match firms in terms of CEO turnover and ROA is not significant.

In short, the above univariate cross-firm group comparisons suggest that firm policies are important predictors of intervention. Firms that pay shareholders less, hold more cash and spend on physical assets rather then research and development, for example, are more likely to be targeted by hedge fund activists. These preliminary observations provide a sense of how firms should adjust policies in order to lower the likelihood of being targeted. The next section presents evidence from regression analyses of the preventive effect of hedge fund activism.

1.3   Empirical findings

In this section I report three sets of empirical results. The first set concerns the eligibility of stock liquidity and mutual fund fire sales as instruments 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 least squares regressions. Finally, I examine how the preventive effect of hedge fund activism changes over time.

1.3.1   Instrument analyses

In this subsection, I check the eligibility of stock liquidity and mutual fund fire sales as an instrument for intervention likelihood. To check relevance, I run the likelihood regression given in Equation (1.2) using probit as well as linear models. In each regression, the HFA dummy is regressed against the lagged instrument candidate(s) and a set of lagged control variables, including market capitalization, sales and the book-to-market ratio, as well as industry fixed effects and year fixed effects. The results are reported in Table 3.3.

Columns 2 and 3 of Table 3.3 show the results for the likelihood regressions based on the Amihud (2002) stock illiquidity ratio. Results from both the probit and the linear estimation procedures indicate that stock illiquidity negatively and significantly affects intervention likelihood. Columns 4 and 5 show the regression results for mutual fund fire sales. With either estimation procedure, mutual fund fire sales positively and significantly (though not as much as stock illiquidity) affects intervention likelihood. It appears that hedge fund activists do take advantage of the temporary price drop induced by mutual fund fire sales in launching interventions. This is consistent with the finding in Gantchev and Jotikasthira (2014) that institutional exit triggers hedge fund activism through a causal relationship between institutional selling and activist purchases. Finally, columns 6 and 7 show the results from using stock illiquidity and mutual fund fire sales jointly as instruments. Both coefficients have the expected sign and are statistically significant, but stock illiquidity more so than mutual fund fire sales.

To check the exclusion of the two instrument candidates, I perform the following exercise following Fos (2013). I run a set of regressions of policy variables against the lagged instrument over two different periods of time. The first covers the pre-study sample period from 1988 to1992. The second is the study sample period from 1994 to 2007. The logic behind this exercise is that two changes to the legal environment carved out a "placebo" period of relatively no external interventions. First, anti-takeover laws and firms' wide-adoption of anti-takeover provisions in the late 80s dramatically increased the cost of external interventions. Second, SEC's proxy reform in 1992 relaxed the restrictions on communications among shareholders of public corporations (Bradley, Brav, Goldstein, and Jiang (2010)), easing interventions. During the 1988 to 1992 period, external interventions were virtually impossible. Therefore, if stock liquidity and mutual fund fire sales do not affect corporate policies other than through their effect on intervention likelihood, we should expect them to have little

effect on corporate policies during this period.

Regression results for the two sample periods are reported in Table 3.4 using stock illiquidity as the instrument and in Table 3.5 using mutual fund fire sales as the instrument. Stock illiquidity has no effect on corporate policy during the 1988 to 1992 period of virtually no hedge fund activism (Panel A) but a statistically and economically significant effect on corporate policy during the 1994 to 2011 period of heightened intervention activity (Panel B). Over the latter period, firms with more illiquid stock pay shareholders less, stock more cash and leverage less, and spend more on physical assets and less on research and development. The results in this table suggest that stock illiquidity affects corporate policies chiefly through its effect on intervention likelihood.

The results are similar for mutual fund fire sales. Panel A of Table 3.5 shows that mutual fund fire sales have no direct effect on any of the policy variables being examined during the 1988 to 1992 "placebo" period. Conversely, Panel B shows mutual fund fire sales having a significant effect on all the policy variables except research and development during the 1994 to 2011 sample period. While there are a number of theoretical and empirical studies that argue for the direct effect of institutional exit on corporate policy (Edmans (2009), Admati and Pfleiderer (2009), Edmans and Manso (2010), Bharath, Jayaraman, and Nagar (2013)), Table 3.5 suggest that mutual fund fire sales affect corporate policy only through its impact on the likelihood of hedge fund activism. These findings are not necessarily contradictory. Whereas the Wall Street Walk effect comes from institutional exit that is information-driven, mutual fund fire sales are primarily liquidity driven and as such its effect on corporate policy is not direct but indirect through creating opportunities for hedge fund activists.

The results in Tables 3.3, 3.4 and 3.5 are supportive of stock illiquidity and mutual fund fire sales as valid instruments for the likelihood of hedge fund activism. However, as noted previously, neither instrument is without flaw. In particular, stock illiquidity

and mutual fund fire sales could also have an effect on the likelihood of takeover. This is to be expected, since both variables affect the ease of acquiring shares of the target firm, which is the primary cost of takeover as well as intervention. In general, firms that are vulnerable or attractive to hedge fund activists are likely to be so to corporate raiders as well. Unsurprisingly, the 1988 to 1992 "placebo" period of virtually no hedge fund activism is also a period of little takeover activity. As such, the results in Tables 3.3, 3.4 and 3.5 cannot exclude takeover likelihood as another potential channel through which stock illiquidity and mutual fund fire sales affect corporate policy. Fortunately, it is not difficult to separate the likelihood of hedge fund activism from that of takeover (Section 1.3.3).

Another concern with using mutual fund fires sales to instrument for intervention likelihood is the possibility that mutual fund capital flows could be driven by firm performance in the first place. That is, poor performance of portfolio stocks results in poor overall fund performance which in turn leads to large capital outflows. The results in Table 3.5, which show no significant link between mutual fund fire sales and corporate policy absent of intervention, alleviate this concern.

Moreover, while capital flow could be correlated with overall fund performance, it may not be correlated with the performance of individual portfolio stocks. Mutual funds are subject to strict diversification requirements precisely to reduce their sensitivity to individual investments. Gantchev and Jotikasthira (2014) show that mutual funds selling future hedge fund activism targets also own and widely sell non-targets, suggesting that mutual fund performance is unlikely to be driven by a concentrated investment in a handful of firms that eventually undergo intervention.

The construction of $MFFS$ also excludes sector mutual funds that concentrate investment in a particular industry, in which case the stocks in the portfolio could be highly correlated with one another and hence the portfolio as a whole. Considering only diversified mutual funds mitigates the concern that $MFFS$ is ultimately driven

by firm performance. Edmans, Goldstein and Jiang (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.

1.3.2 Main results

This subsection presents the main evidence for the preventive effect of hedge fund activism. First, the preventive effect emerges from Panel B of Tables 3.4 and 3.5 reported in the previous subsection. In these policy regressions over the 1994 to 2011 sample period, stock illiquidity and mutual fund fire sales serve as exogenous proxies for the likelihood of hedge fund activism. Their estimated slopes therefore reflect the response of policy variables to changes to the likelihood of hedge fund activism.

The "percentage" row in Panel B of Tables 3.4 and 3.5 presents the percentage change in a policy variable relative to its mean given a one standard deviation increase in the instrumental. The results are generally confirmative of the preventive effect of hedge fund activism on policy deviations. From the summary statistics reported in Table 3.2, we see for example that hedge fund activists target firms with low shareholder payout, leverage, and investment into research and development and high levels of cash and capital expenditures. Therefore, if hedge fund activism has a preventive effect on policy deviations, we would expect managers to increase distribution to shareholders, leverage, and investment into research and development while reducing cash holdings and capital expenditures when faced with greater intervention threat.

These expectations are corroborated by Panel B of Tables 3.4 and 3.5. Changes in both stock illiquidity and mutual fund fire sales affect payout policy, financial policy, investment policy, and CEO compensation policy in the expected direction. According to Panel B of Table 3.4, a one standard deviation increase in the stock illiquidity measure, or alternatively a decrease in the likelihood of hedge fund activism, leads to

a decrease in shareholder payout of 6.42% from its sample mean. The disinclination to pay shareholder when threat of intervention is low is a sign of the agency problem between mangers and shareholders.

In terms of financial policy, when stock illiquidity increases (or intervention likelihood decreases), cash holdings increase by 0.88% from its sample mean while leverage decreases by 1.13% from its sample mean. These are generally taken by the governance literature to be signs of worsening governance. This is because leverage can pressure managers to work hard to consistently turn profits and meet interest payments. To avoid this financial market discipline, however, managers stock cash rather than return it to shareholders. Cash therefore can lead to managerial complacency and even wasteful spending.

In terms of investment policy, whereas investment in physical assets increases by 1.02% from its mean, investment in research and development declines by 3.98% from its mean in response to a one standard deviation increase in stock illiquidity. In other words, when intervention likelihood is low, managers have greater tendency to invest in negative NPV projects for empire building. At the same time, they have less incentive to bear the risk of searching for new investment opportunities via investment in research and development.

Using mutual fund fire sales to proxy for intervention likelihood produces similar results, as shown in Panel B of Table 3.5. A one standard deviation increase in mutual fund fire sales, or equivalently intervention likelihood, leads to decreases in cash and capital expenditures of 0.25% and 1.90% from their respective mean values and increases in shareholder payout and leverage of 6.32% and 0.34% from their respective means. Unlike stock illiquidity, mutual fund fire sales do not appear to have a strong effect on research and development investment. By and large, Tables 3.4 and 3.5 show that the likelihood of hedge fund activism, whether proxied by stock illiquidity or mutual fund fire sales, has economically and statistically significant

effects on corporate policy. In other words, in addition to the documented corrective effect, hedge fund activism also has a preventive effect on corporate policy deviations.

Table 3.6 presents results from two-stage least squares regression. In the first stage, intervention likelihood is estimated by regressing the hedge fund activism dummy, which equals one if intervention is announced and zero otherwise, against lagged instrument(s) and control variables as well as industry fixed effects and year fixed effects. In the second stage, policy and performance variables are regressed against the estimated intervention likelihood obtained from the first stage. Panel A reports the second stage results from using the stock liquidity instrument to estimate intervention likelihood. Panel B reports the second stage results from using stock illiquidity and mutual fund fire sales jointly as instruments when estimating intervention likelihood. The t-statics are calculated based on standard errors that have been corrected for the generated regressor bias. All regressions pass the over-identification test at even the 10% level.

As can be seen from Table 3.6, results from 2SLS regressions are generally consistent with those from direct OLS regressions of policy variables against lagged instrumental variable(s). Table 3.6 introduces two additional policy variables: CEO pay and CEO turnover[2]. The effect of intervention likelihood on CEO pay is expectedly negative. Statistical significance improves when intervention likelihood is estimated using stock illiquidity and mutual fire sales jointly as instruments. This result is consistent with existing literature. For example, Brav, Jiang, Partnoy and Thomas (2008) report similar movements in CEO pay and shareholder payout *after* intervention. The authors view this (anticipated) wealth reallocation from executives to shareholders as a partial explanation for the abnormal returns observed around the announcement of intervention. The results reported here pertain not only to target firms but to all firms and represent *proactive* policy responses to the possibility of

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[2] These are not included in Tables 3.4 and 3.5 due to a lack of data in the Execucomp database for the 1988 to 1992 sample period

intervention. The effect of intervention likelihood on CEO turnover is positive, also as expected. Again, statistical significance increases when intervention likelihood is estimated using both stock illiquidity and mutual fund fire sales. In short, the threat of hedge fund activism is effective in mitigating managerial rent seeking tendencies and entrenchment.

Table 3.6 shows that overall firm performance, measured by ROA, also increases significantly in response to an increase in intervention likelihood. This is not surprising in light of the policy improvements. Overall, the results from direct OLS and 2SLS regressions are supportive of the hypothesis that managers proactively "self-correct" policy and performance deviations in response to the threat of hedge fund activism. These findings are intuitively appealing given the negative consequences managers usually face after intervention. They are also consistent with a plethora of anecdotal evidence, from hedge fund activists' public acknowledgment of proactive (albeit inadequate) efforts on the part of managers to seek improvements prior to intervention[3] to corporate advisors urging firms to be on the lookout for hedge fund activists[4].

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.

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[3] Merced, M.J. (2012). Activist fund takes aim at Office Depot. *New York Times (September 17)*. *Available at: http://nyti.ms/UOjd9V.*

[4] Milano, G.V. (2013). Keeping activist investors away. *CFO.com (November 13). Available at: ww2.cfo.com/capital-markets/2013/11/keeping-activist-investors-away/.*

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 of hedge fund activism is usually measured as post intervention policy changes relative to a group of non-target control firms. These control firms are firms that share similar characteristics as target firms and therefore are likely to be targeted for intervention themselves. If these control firms adjust policies in the same direction as target firms, in proactive response to potential intervention, the comparison of post-intervention policy changes in target firms to policy changes in control firms would lead to an underestimation of the corrective effect of hedge fund activism: only the changes in target firms that are in excess of the changes in control firms are counted.

Finally, if we divide a firm's total response to an actual intervention into two components: proactive response and reactive response, the weight of each component relative to the total depends on the extent to which intervention is anticipated. For an anticipated intervention, more policy changes would occur before intervention. For an intervention that comes as a surprise, most of the policy changes would occur after intervention. It is therefore important to control for the likelihood of hedge fund activism when quantifying post-intervention policy changes. If the distribution of anticipated and unanticipated interventions varies across samples and intervention

likelihood 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.

1.3.3   Hedge fund activism versus corporate takeover

This subsection addresses the aforementioned concern that since stock liquidity and mutual fund fire sales lower the cost of takeover as well as intervention, they could simultaneously affect the likelihood of both. Moreover, it is likely that a firm targeted by hedge fund activists would also attract interest from potential acquirers and vice versa. This leaves the possibility that the policy improvements in response to increasing intervention likelihood documented earlier could be driven by a simultaneous increase in takeover threat.

In general previous studies have focused separately on either hedge fund activism or takeover and do not address this "dual" event problem. That is, some firms in the sample of intervention targets could also be targeted for takeover. Likewise, some firms in the sample of takeover targets could simultaneously be targeted for intervention. The Venn-diagram below illustrates these relationships.
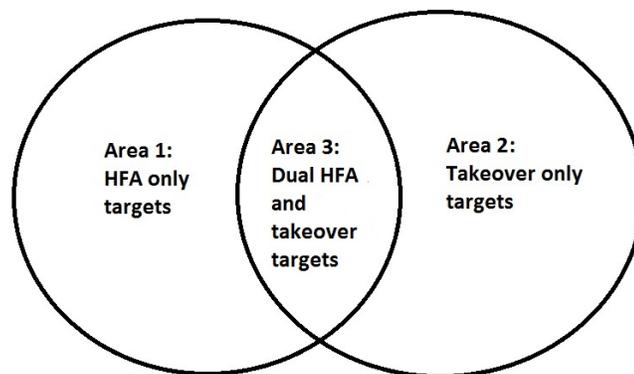
Firms in area 1 are pure hedge fund activism targets, firms in area 2 are pure takeover targets, and firms in area 3 are dual targets. Studies on hedge fund activism

use a sample of firms in area 3 as well as those in area 1 and attribute changes in firm outcome variables solely to hedge fund activism. Studies on corporate takeover use a sample of firms in area 3 as well as those in area 2 but attribute firm responses solely to takeover. This is problematic given the confounding effect from dual targets.

In the current study, to more confidently answer the natural question of which particular mechanism should be given credit for the policy improvements observed, I perform the following exercise to disentangle intervention likelihood and takeover likelihood. From the Securities Data Company, I collect a sample of all takeover bids during 1994 to 2011, the sample period of the hedge fund activism data. After merging the the two data sets, I split the whole sample into pure intervention 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 3.7 shows the size of the three subsamples.

According to Panel A, in the hedge fund activism sample, 1115 events are pure hedge fund activism events whereas 543 events are dual hedge fund activism and takeover events. The dual events represent about 30% of the whole sample, suggesting the importance of controlling for their confounding effect in the present study.

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 $HFAONLY$. I estimate the likelihood of pure hedge fund activism events using both one instrument (stock illiquidity) and two instruments (stock illiquidity and mutual fund fire sales). 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 3.8. For each policy regression, only the results using combined instruments are reported. The t-values are calculated based on standard errors that have been corrected for the generated regressor bias.

According to Table 3.8, 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. An increase in the likelihood of hedge fund activism only still cuts capital expenditures, CEO compensation, cash stock while raising the payout ratio, leverage, R&D investment and ROA. Table 3.8 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.

Admittedly, the above procedure is unlikely to perfectly eliminate the effect of takeover threat. This is because the absence of a takeover bid does not equate the absence of takeover threat. Nevertheless, using a subsample of firms that receive only hedge fund intervention but no takeover bid to estimate intervention likelihood is a step in the right direction to reduce the influence of takeover threat on policy responses. I am exploring other methods to better control for the threat of takeover.

To examine policy responses to the likelihood of pure takeover bids, I run the two-stage instrumental variable regressions once again, except I replacing the HFA dummy in the first stage regression with the pure takeover bid dummy $TKONLY$. Again I generate the fitted likelihood values from using either the stock illiquidity instrument alone or in combination with mutual fund fire sales, 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 3.9. For each policy regression, only the results using combined instruments are reported. The t-values are calculated based on standard errors that have been corrected for the generated regressor bias.

Table 3.9 shows that takeover is another standalone channel through which ex-

ogenous price changes affecting corporate policy. Firms react in similar ways when facing greater takeover threat as when facing greater intervention threat: they increase shareholder distribution, leverage and research and development investment while cutting cash, CEO compensation and capital expenditures. There is also a significant increase in ROA. These policy and performance improvements are expected as firms targeted by acquirer share similar characteristics as those targeted by hedge fund activists and all have incentive to demonstrate to shareholders a bright company future under current management. They are also consistent with studies on the governance effects of corporate mergers and acquisitions (Hart (1995), Holmstrom and Kaplan (2001), Bertrand and Mullainathan (2003) and Bebchuk, Cohen and Ferrell (2009)).

Tables 3.8 and 3.9 show that overall firm performance as measured by ROA is more responsive to intervention likelihood than to takeover likelihood. This may be because given the same likelihood of occurring, intervention poses more of a threat to managers than receiving a takeover bid. 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 attests to this. Second, because acquirers typically are interested in target firms' unique distribution channels, customer base, technology, et cetera, target firms' managers are better able to negotiate with them or solicit better offers. On the other hand, managers have relatively little control over the timing of interventions or the identity of the activist. Third, managers are better protected from the negative effects of takeovers than that of interventions. For example, golden parachutes grant significant compensation for employment termination due to a merger or takeover but not shareholder activism.

1.4 Additional analyses

In this section I perform additional analyses to check the robustness of the main results with alternative model specifications and sampling. First, in the second stage policy regressions, I use discontinuous measures of intervention likelihood rather than continuous likelihood. In Table 3.10, 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 3.11, 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 intervention likelihood 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.

Regressions using above median as well as quartile likelihood indicator variables confirm the main results that intervention likelihood leads to governance improvements, such as higher shareholder payout, less investment in physical assets but more investment in research and development. Overall firm performance as measured by ROA also improves. In particular, results from using quartile likelihood indicator variables show increasingly stronger responses for most policy and performance variables as intervention likelihood rises from the first (lowest) to the fourth (highest) quartile. Moreover, for most policy and performance variables, moving from the first to the second likelihood quartile already induces a significant response. This shows that hedge fund activism has a preventive effect on policy and performance deviations for most firms rather than the few facing the highest likelihood of intervention.

Next, I address the concern that the preventive effect of hedge fund activism documented is driven by a minority of hostile events. For example, Fos (2013) document

a preventive effect associate with proxy contests. The focus on proxy contests suggest a more critical role of activism tactic over activist identity. Intuitively, however, the effectiveness of a tool largely depends on the identity of the wielder. A 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 investment 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 potentially greater shareholder support against corporate incumbents.

Intuition aside, I formally address the concern that the preventive effect of hedge fund activism is driven by activists' use of proxy contests. I perform the same 2SLS regressions except for 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 the lagged stock illiquidity and mutual fund fire sales instruments (and the same set of control variables as in all previous regressions). In the second stage, I regress policy variables against the estimated likelihood of non-proxy hedge fund activism obtained from the first stage. Results are shown in Panel A of Table 3.12:

From Panel A of Table 3.12, we see that the likelihood of non-proxy hedge fund activism also leads to significant policy improvements. Payment to shareholders increase while payment to CEOs decrease; firms hold less cash and more leverage; investment is directed toward research and development and away from physical assets; and overall firm performance as measured by ROA improves.

I acknowledge that excluding interventions involving proxy contests is not the

same as excluding the threat of proxy contests. While it may be a costly last resort, proxy contest is nevertheless a tool available to activists and therefore the threat of a proxy battle is always present. Nevertheless, using the subsample of non-proxy interventions to estimate intervention likelihood at least reduces the influence of proxy threat on policy responses. Other methods to better control for proxy treat are being explored. Based on the results in Table 3.12, it is hard to argue that the use of proxy contest is essential to the effectiveness of hedge fund activism in preventing policy and performance deviations.

I also extend the above analysis by examining the importance of hostile activism tactics in general. I perform the same 2SLS regressions with expect for 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 instrumental variables. In the second stage, I regress policy variables against the estimated likelihood of non-hostile hedge fund activism obtained from the first stage. Hostile interventions include the usage of lawsuits and antagonistic communication requesting managerial resignation in addition to proxy contests.

According to Panel B of Table 3.12, the policy response to the likelihood of non-hostile hedge fund activism remains strong: shareholder payout increases while CEO pay decreases, cash decreases while leverage increases, investment in physical assets decrease while investment into research and development increases. Overall firm performance as measured by ROA improves. Based on these results, the preventive effect of hedge fund activism does not hinge on the usage of hostile tactics.

Results from the following robustness checks show no inconsistency with those obtained from earlier analyses. They are all confirmative of the preventive effect hedge fund activism has against corporate policy and performance deviations. I therefore omit the numbers for the sake of brevity and just describe the procedure.

I 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 the lagged instrument(s) for intervention likelihood, each time excluding from the Compustat sample, hedge fund activism targets, takeover targets, and finally both hedge fund activism and takeover targets. Results show that policy responses to the likelihood of hedge fund activism (proxied by stock illiquidity, mutual fund fire sales, or both jointly) remain significant and in governance improving directions even after excluding realized hedge fund activism events, takeover events, and activism or takeover events, respectively. This confirms the results based on quartile likelihood indicator variables, in Table 3.11, 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 the lagged instrumental(s) for a subsample of Compustat firms that also appear in the Execucomp database. These are S&P1500 firms for which hedge fund activism is more costly and therefore less likely. The policy responses of these firms to intervention likelihood (proxied by stock illiquidity, mutual fund fire sales, or both jointly) are also significant and in governance improving directions. In fact, they are greater than those of Compustat firms in general. On the one hand, these results show the broad reach of hedge fund activism, to not only small, "easy" targets. On the other hand, these results raise the possibility that big firm managers are more sensitive to the threat of intervention.

Although the results in Tables 3.4 and 3.5 show that the relationship between policy variables and the two instruments for intervention likelihood are unlikely to be driven by omitted variables, I perform the same set of direct OLS policy regressions but with the inclusion of additional control variables. Specifically, in addition to the

lagged left-hand-side variable; lagged market cap, book to market ratio and sales; and industry fixed effect and year fixed effects; I also include all other lagged policy variables. 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 intervention likelihood (proxied by stock illiquidity, mutual fund fire sales, or both jointly) 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. The results here show that even after controlling for payout, firms reduce cash holdings in response to an increase in intervention likelihood, indicating genuine attempt to improve overall governance and operations.

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 against corporate policy and performance deviations. Furthermore, this effect is widespread, observable not only in firms with the highest likelihood of intervention or "soft" targets 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.

1.5   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 intervention likelihood, firms return more cash to shareholders while cutting payment to CEOs, stock less cash and leverage more, and direct investment toward research and development

and away from physical assets. Overall firm performance as measured by ROA also improves significantly in response to an increase in intervention likelihood.

The preventive effect of hedge fund activism is not driven by the usage of hostile intervention tactics such as proxy contests and lawsuits. It is also evident in the corporate world at large rather than limited to a group of "soft" targets. Importantly, it remains significant after controlling for the confounding effect of concurrent takeover threat. To my knowledge, this is the first study that disentangles the effects of the two types of external threat on corporate policies. I find that both hedge fund activism and corporate takeover are standalone and effective means through which financial markets can discipline managers.

The most important takeaway from this study is that 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 an increasing threat of hedge fund activism. Accurate assessment of the impact of hedge fund activism and its merits as a governance mechanism will only become more important as policymakers face heightened pressures to increase regulation on the hedge fund industry.

2

An Examination of Flow-Induced Trading

2.1   Introduction and literature review

Endogeneity is an ubiquitous challenge in empirical studies of corporate finance. Finding a truly exogenous yet highly relevant instrument often seems an unattainable ideal. As noted by Nakamura and Nakamura (1998), "efforts to find truly exogenous instruments have the unfortunate tendency to push researchers toward the use of substantively irrelevant instruments" and "there is usually little real evidence that the instruments that are used are exogenous". In this second chapter, I investigate the qualification of flow-induced mutual fund sales as an instrument for price pressure and by extension any corporate event whose occurrence is affected by stock price.

Mutual fund "fire" sales and purchases exert temporary pressures on stock price, affecting the likelihood of corporate events such as takeovers and hedge fund interventions. Studies using mutual fund fire sales as an instrument argue that flow-induced selling is inherently different from routine portfolio adjustments where fund managers trade select stocks based on information. Large capital outflows force mutual funds to sell widely, since strict diversification requirements limit their ability to concentrate large sales in a few "worst" stocks. Therefore, mutual fund fire sales should be rela-

tively free of firm-specific information. Though intuitively appealing, this argument has not been rigorously verified.

In this chapter, I examine funds' selling behavior in response to large capital outflows, market reaction around large mutual fund fire sales, and what is responsible for the large mutual fund capital outflows. I find that liquidity-strapped mutual funds sell more widely across all portfolio holdings, which exerts significant downward pressure on stock price. On the other hand, the movement of abnormal stock returns around large mutual fund fire sales indicates the presence of information. Moreover, I find that large mutual fund capital outflows could be attributable to deteriorating fund performance and hence to the poor performance of portfolio holdings. All in all, the results advise caution in the use of mutual fund fire sales as an instrument that is exogenous to firm fundamentals.

The seminal paper by Coval and Stafford (2007) shows that when a stock is simultaneously owned by several distressed funds (that is, funds that have recently suffered a big outflow of capital), it is likely to be subject to unusually heavy selling pressure and a corresponding drop in price. The authors document "striking" patterns in the abnormal returns of fire sale stocks. Twelve months before the event quarter of significant mutual fund selling due to large capital outflow, the average monthly abnormal return of fire sale stocks (stock return in excess of the equal-weighted average return of all stocks held by mutual funds at the beginning of the month) is –1.19% with a t-statistic of –1.54. The monthly cumulative average abnormal return (CAAR) becomes more negative with increasing significance until the event quarter in which it reaches –14.99% with a t-statistic of –4.39. Importantly, after the event quarter, CAAR slowly reverses. Eighteen months after the event quarter, CAAR is a –5.34% with a t-statistic of –0.40.

The significant drop in stock price during mutual fund fire sales followed by a full reverse suggest that mutual fund fire sales are a determinant of stock price that is

exogenous to firm fundamentals. Its applicability as an instrument for price changes is immediate. Using flow-induced trading pressure as a proxy for stock mispricing, Lou and Wang (2012) demonstrate that temporary mispricing leads to a decline in investment. The authors offer reliance on equity for financing as an explanation for this effect. Firms with underpriced stocks are reluctant to issue equity and the sensitivity of investment to mispricing is stronger for more equity-dependent firms.

On the flip side, Khan, Kogan and Serafeim (2012) use flow-induced mutual fund purchases to identify overpricing. The authors argue that these purchases are exogenous since they are associated with who is buying—mutual funds with excess liquidity—rather than what specific stocks are being purchased. In addition to documenting a substantial price impact associated with flow-induced mutual fund purchases, the authors also find that insider sales and the probability of a SEO or stock-based acquisition increase significantly in the four quarters following the mutual fund buying pressure, suggesting that firm managers are able to identify and exploit overvalued equity.

By extension, mutual fund fire sales also seem to be a good instrument for corporate events whose occurrence depends on stock price. For example, it has long been argued that corporate takeover disciplines managers to act in the best interest of shareholders or risk being ousted. No study however has tried to quantify the effect of takeover likelihood on corporate policy. The primary challenge in the endeavor is overcoming endogeneity. In particular, policy deviation (from the optimal level that maximizes shareholder value) leads to takeovers in the first place. Because of this reverse causality from policy deviation to takeover, naive regressions of policy variables against takeover likelihood will not identify the disciplinary effect of takeover against policy deviation. An instrumental variable that is correlated with takeover likelihood but is not driven by firm fundamentals would do the job, and mutual fund fire sales appears a promising candidate.

Mutual fund fire sales exert significant downward pressure on stock price, which lowers the cost of acquiring a controlling stake in the target firm. Therefore, mutual fund fire sales should have a positive correlation with takeover likelihood. At the same time, if the downward pressure is temporary and stock prices reverse ex post, mutual fund fire sales are unlikely to be driven by adverse information. Edmans, Goldstein and Jiang (2012) find a significant and positive link between mutual fund fire sales and takeover likelihood, evidence that financial markets have real effects on corporate events.

In a similar spirit, the study in chapter one uses mutual fund fire sales as an instrument for the likelihood of hedge fund activism. The temporary price drop induced by liquidity-driven rather than information-based mutual fund selling creates a window of opportunity for hedge fund activists to quickly and more cheaply accumulate a meaningful stake in the target firm. Gantchev and Jotikasthira (2014) show that activist hedge funds accumulate most of their ownership in the target firms in the days right before intervention when mutual funds are heavily selling their stake. The authors identify a causal relationship between mutual fund selling and activist hedge fund buying by using mutual funds' trading of non-hedge fund activism targets as an instrument for their trading in target stocks. This is similar to mutual fund fire sales in that mutual funds' trading of non-hedge fund activism targets is driven by fundings needs rather than stock-specific information.

Studies using mutual fund fire sales to identify takeover or hedge fund activism likelihood make the implicit assumption that target shareholders are unable to disentangle price drops due to exogenous flow-induced mutual fund selling and those due to declining firm fundamentals. For example the negative link between stock price and takeover likelihood documented by Edmans, Goldstein and Jiang (2012) could be due to target shareholders learning the value of their firm from the stock price and subsequent willingness to sell their shares close to that price. Ali, Wei and Zhou (2011),

however, show that corporate insiders exploit the mispricing resulting from mutual fund fire sales (purchases) by buying (selling) their shares, suggesting that they are able to identify temporary mispricings due to large mutual fund capital outflows and trade for personal benefit based on this information. Cai, Cremer and Wei (2013) use flow-induced mutual fund trading as an exogenous shock to price informativeness and find that boards rely more on accounting performance rather than stock performance in deciding CEO compensation, particularly the short-term component such as the cash bonus, during periods of less informative stock prices. This indicates that boards on average can identify mispricing due to flow-induced mutual fund trading.

All in all, studies using flow-induced mutual fund trades as an instrument are more successful in showing the relevance of flow-induced mutual fund trading to stock price and real firm activities than providing conclusive evidence of its exogeneity to stock-specific information. While these studies consistently show a reversal in CAAR after flow-induced mutual fund trading, the reversal is slow, taking up to two years. This is opposite to the hypothesis that (non-information related) mispricings should be eliminated quickly. Another common feature of these CAAR plots is that they are cut off at the time CAAR reaches zero. However, at that time, the upward (downward) trend of CAAR after flow-induced selling (purchasing) shows no sign of slowing down. If CAAR continues to become more positive or negative, the benchmark used in computing abnormal returns becomes suspect. Moreover, these studies overlook another documented observation regarding fund flows that call into question the absence of information in subsequent "forced" trading.

While Khan, Kogan and Serafeim (2012) argue that flow-induced purchases are unlikely to be information-driven because they are related more to who is buying (mutual funds with large capital inflows) than what is bought (specific stocks), they also make the observation that fund flows exhibit a large spread, from 40.3% for the top decile to –17% for the bottom decile. Moreover, the authors find that prior-

year fund returns decrease monotonically from 16.6% for the top decile to 6.1% for the bottom decile. This is consistent with evidence in Coval and Stafford (2007) that mutual fund fire sales are predictable to some extent, because fund distress is a function of poor past performance. As a way of quantifying the predictable price pressure effect associated with mutual fund fire sales, the authors using only public information construct a hypothetical trading strategy that front runs mutual fund fire sales. Taking a short position in stocks most likely to be dumped by distressed mutual funds earns a significant abnormal return on the order of 10% per year.

Frazzini and Lamont (2008) offer evidence that alleviate the concern that fund outflows are driven by fund performance and therefore the performance of portfolio stocks. The authors use mutual fund flows as a measure of investor sentiment and show that high investor sentiment predicts *low* stock returns in the long run. In other words, by reallocating capital across different mutual funds, investors actually reduce their wealth in the long run. In all likelihood, investors are making a conscious choice to invest in low-risk value stocks rather than high-risk growth stocks. This result contradicts the concern that investors are pulling money out of funds that fail to pick the most profitable investments and putting it into those that do.

This study critically examines the assumptions and evidence underlying the exogeneity of flow-induced mutual fund sales to firm fundamentals. I focus in particular on the mutual fund fire sales measure in Edmans, Goldstein and Jiang (2012) rather than the original measure in Coval and Stafford (2007). Whereas the original measure captures real mutual fund fire sales, "mechanical" mutual fund fire sales are constructed based on the assumption that liquidity-strapped mutual funds mechanically liquidate all portfolio holdings according to portfolio weights. This is a reasonable assumption since mutual funds are subject to strict diversification requirements that limit their ability to concentrate large sales in the "worst" stocks. Hypothetical sales eliminate the concern that distressed funds manage to dispose stocks based on in-

formation to some extent and should therefore be more exogenous than real mutual fund fire sales.

I examine the validity of the assumption that liquidity-strapped mutual funds proportionally liquidate portfolio holdings by comparing the trading behavior of funds that experience a large outflow of capital with the trading behavior of those that do not. I also check whether substantial correlation exists between mechanical mutual fund fire sales and actual mutual fund fire sales. I verify the evidence for the exogeneity of mutual fund fire sales to firm fundamentals by plotting abnormal returns around mechanical fire sale events using alternative methods and risk adjustments. I also track fund performance to see whether large mutual fund capital outflows are more attributable to exogenous liquidity needs of mutual funds' investors or to deteriorating fund performance.

2.2 Data and summary statistics

The data needed to construct mutual fund fire sales come from three sources. From the CRSP Survivor-Bias-Free US Mutual Fund Database, I extract mutual fund assets and return information. Following Coval and Stafford (2007), I compute mutual fund capital flow as period-ending assets minus period-beginning assets times the return over the period. Following Edmans, Goldstein and Jiang, I define a large outflow event as outflow equal to or greater than 5% of period-beginning assets. From the Thomson Reuters Mutual Fund Holdings Database, I extract information on the equity holdings of mutual funds. MFLINKS provides an easy way of linking the two datasets, which allows the identification of stocks subject to mutual fund fire sales, that is stocks held by mutual funds that experience large capital outflows.

Table 3.13 presents key descriptive stats for the mutual funds in the sample over the period of 1993 to 2011. The sample contains about 7,000 funds and 300,000 fund-quarters. According to Panel A, most funds in the sample experience at least

one large-outflow event (capital outflow equal to or greater than 5% of the beginning of the quarter assets). Panel B indicates that in a typical quarter, funds on average experience an inflow of 10.3 million dollars or 6.7% of quarter-beginning assets. On the other hand, in a large-outflow quarter, funds on average experience an outflow of 60 million dollars or 13% of quarter-beginning assets. According to Panel C, funds that experience large capital outflows tend to be smaller and slightly less diversified.

Figure 3.2 shows year-by-year aggregate flows for the mutual fund industry as a whole as well total outflows. We see that for most years during the 1993 to 2011 sample period, the mutual fund industry as a whole experiences growth and net inflow of capital. The two notable exceptions are the early 2000s recession and the financial crisis of 2007-2008 during which aggregate flows are in decline and even turn negative. From the figure, we would expect large outflow events to concentrate in the period after 2007.

2.3   Construction of mutual fund fire sales

Mutual fund fire sales by definition are flow-induced and therefore already relatively free of information. Edmans, Goldstein and Jiang (2012) introduces a mechanical component in the construction of the mutual fund fire sales that further reduces the role of information. I therefore construct mutual fund fire sales following their definition:

$$MFFS_{i,t} = \frac{\sum_{j=1}^{m} MFOF_{j,t} \times \frac{SHARES_{i,j,t-1} \times PRC_{i,t-1}}{TA_{j,t-1}}}{VOL_{i,t}} \ , \qquad (2.1)$$

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

Subscripts i, j, and t index firms, mutual funds and quarters, respectively. $TA_{j,t-1}$ is fund j's assets at the beginning of the quarter. $MFOF_{j,t}$ is the capital out-

flow of mutual fund j in quarter t that is at least five percent of fund j's assets. $SHARES_{i,j,t-1}$ is the number of shares of stock i held by fund j. $PRC_{i,t-1}$ is the price of stock i. $\frac{SHARES_{i,j,t-1} \times PRC_{i,t-1}}{TA_{j,t-1}}$ is therefore the weight of stock i in fund j's portfolio. This is the "mechanical" component that ensures fund j's liquidation of stock i is purely a function of its capital outflow and the weight of stock i in its portfolio and not stock i's fundamentals. The total fire sales of stock i in is the sum of "mechanical" sales of stock i by all m mutual funds that experience a large outflow of capital. Finally, since the price impact of a sale depends on the liquidity of the stock, to arrive at the final $MFFS_{i,t}$ variable, the total fire sales of stock i is deflated by $VOL_{i,t}$, the dollar trading volume of stock i. In short, $MFFS$ is the portion of the dollar trading volume of a stock that is attributable to "mechanical" or flow-induced rather than information-based mutual fund selling.

The correlation between $MFFS$ constructed here and the original mechanical mutual fund fire sales variable in Edmans, Goldstein and Jiang (2012) is over 95%. Over the period of 1993 to 2011, there are a total of 460,382 stock-quarter observations of which 136,852 or 29.7% have positive $MFFS$. The rest have zero $MFFS$. Among all the observations with non-zero $MFFS$, the mean value is 6.9% while the 25th, 50th and 75th quartile are 0.1%, 0.4%, and 1.1%, respectively. Although $MFFS$ is typically small, it has considerable variation. Most firms experience no fire sales in most quarters, but a few firms experience extremely large fire sales in a few quarters.

2.4 $MFFS$ as an instrument

In this section, I investigate the assumptions and evidence underlying the use of $MFFS$ as an instrument for price changes. I examine fund trading behavior and fund performance around large outflow events. At the firms level, I examine stock returns around fire sale events.

2.4.1 Fund trading after large capital outflow

I examine changes to mutual funds' equity holdings to see how funds trade in response to large capital outflows. Tables 3.14 describes the trading behavior of mutual funds in a typical quarter, an "extreme outflow" quarter, and a "non-extreme" outflow quarter.

We see that in a typical quarter, mutual funds hold over 500 stocks and are well diversified. On average, mutual funds sell widely across their portfolio: over a quarter of the stocks. Of the stocks sold, mutual funds on average sell 22% of the shares. In an extreme outflow quarter, mutual funds hold over 400 stocks, remaining well-diversified. But now they are forced to sell 40% of these. Interestingly, of the stocks sold, mutual funds are still selling just 22% of the shares. This suggests that the large outflow of capital, rather than information, is the primary driver of the sales. Otherwise, we would observe liquidity-strapped mutual funds concentrating sales in the "worst" stock before moving onto the second worst stock, which would result in a much higher average percentage of shares sold. That said, some mutual funds are selling just 5% of the stocks in their portfolio.

2.4.2 Stock returns around mutual fund fire sales

To verify that mutual fund fire sales are free of information, I examine stock returns around mutual fund fire sales. To compute abnormal stock returns, I first follow Edmans, Goldstein and Jiang (2012) and benchmark stock returns against CRSP's value-weighted market index. Different from Edmans, Goldstein and Jiang (2012), I define a mutual fund fire sales event as $MFFS$ falling in the bottom 10% of each month rather than over the entire 1993 to 2011 sample period. This ensures that events are spread out across the sample period rather than concentrated after the financial crises in 2007.

The solid line in Figure 3.3 shows that from month t-12 to t-2, the market-adjusted

CAAR remains around zero. In the event quarter, from month t-1 to t+1, CAAR drops to about -5%. It then reverses slowly but surely over a period of fifteen months. This is consistent with the returns plot in Edmans, Goldstein and Jiang (2012) which stops when CAAR is about to reach zero. One puzzling observation is that the upward trend of CAAR shows no sign of slowing. I therefore extend the returns plot out to 36 months after the fire sale event. Sure enough, CAAR continues to rise with no sign of slowing. The monthly average abnormal return in the post-event period is only around 0.3% percent. The market appears to be crude risk adjustment.

I therefore also plot abnormal returns using the Fama-French 25 size and book-to-market portfolio return as the benchmark, represented by the dashed line in Figure 3.3. The abnormal returns pattern is starkly different. When adjusting by size and book-to-market, CAAR show some sign of decline before the fire sale quarter. From month t-12 to month t-2, CAAR declines from zero to about –4%. In the event quarter, it dips another 3%. After the fire sale event, CAAR recovers only slightly, only about 1% by month t+36.

I also compute buy-and-hold abnormal returns using the two benchmarks. Results are similar and shown in Figure 3.4. The size and book-to-market adjusted buy-and-hold abnormal returns show more sign of decline prior to the fire sale event and drop by more in the event quarter. After the fire sale, it shows little sign of reversal whereas the market-adjusted buy-and-hold abnormal returns trend upward indefinitely, crossing zero after about two years and continue to rise sharply with no sign of slowing.

Figures 3.3 and 3.4 raise the possibility that mutual fund fire sales, even mechanically constructed, may not actually be free of information. If they were, we should observe abnormal returns to turn significantly positive immediately after the fire sale. The fact that abnormal returns remain constant around zero after mutual fund fire sales suggests that stocks reach new equilibrium prices after being fire sold. This

result is somewhat surprising. As explained earlier, the mechanical construction of the mutual fund fire sales variable examined here, $MFFS$, ensures that when a fund experiences a large outflow of capital, it mechanically liquidates all holdings according to their portfolio weights. In other words, the selling "decision" by liquidity-strapped mutual funds does not introduce information into $MFFS$. This prompts investigation into the source of mutual fund capital outflows in the first place.

2.4.3 Fund performance around large capital outflow

$MFFS$ by construction eliminates the concern that liquidity-strapped funds disproportionally sell portfolio holdings based on stock-specific information, that is concentrate sales in the "worst" stocks. One question still remains: what causes large mutual fund capital outflows in the first place? One possibility is that mutual fund investors experience some liquidity need, for example to buy a house, send children to college, imminent retirement. Another, perhaps more likely, possibility is that mutual fund investors withdraw capital in response to poor fund performance. Since fund performance is determined by the performance of stocks held, if the latter is true, then mutual fund fire sales, even mechanically constructed, may not be exogenous to firm fundamentals.

Since the first possibility is impossible to verify, I focus on examining the second. I track fund performance from 12 months before a large capital outflow event to 12 months after. I define a large outflow event as outflow falling in the bottom 10% of all funds that experience an outflow of at least 5% of assets in that month.

According to Figure 3.5, over the 1993 to 2011 sample period, funds that do not experience large outflows maintain constant monthly returns around 0.5% throughout. On the other hand, the performance of funds that experience large outflows experience decline significantly ex ante. This is in line with the finding in Coval and Stafford (2007) that fund performance is a predictor of fund returns. After a large outflow of

capital, the performance of funds start to improve significantly, surpassing pre-event levels. The disposal of poorly performing holdings could be one explanation. In sum, investors withdrawing capital after poor fund performance cannot be excluded as a driver of large mutual fund capital outflows. Therefore, mutual fund fire sales, even mechanically constructed, may not be free of firm information.

2.5   Conclusion

In this chapter, I examine whether mutual fund fire sales (or flow-induced "forced" sales) are a valid proxy for exogenous price pressure. If so, mutual fund fire sales has wide applicability as an instrument for any corporate event whose occurrence is affected by stock price, for example corporate takeover and hedge fund intervention. I find that (even hypothetical, mechanically constructed) mutual fund fire sales exert considerable pressure on stock price. I also find that after experiencing a large outflow of capital, mutual funds sell more widely across all portfolio holdings. These affirm the validity of mutual fund fire sales as an instrument for price pressure. On the other hand, I also find that following mutual fund fire sales, negative abnormal stock returns do not reverse. Moreover, I find that large mutual fund capital outflows may be attributable to deteriorating fund performance (and therefore the performance of fund holdings) in the first place. These cast doubt on the exogeneity of mutual fund fire sales to firm-specific information.

In light of the overall results, caution is warranted in the use of mutual fund fire sales as an instrument for price changes, and corporate events whose likelihood depends on stock price. That said, potential exists for alternative constructions of mutual fund fire sales that would improve its viability as such an instrument. Further research into flow-induced mutual fund trading may be fruitful in this regard and would certainly add to our understanding of the determinants of institutional trading.

3

Appendix

I construct a total seven stock liquidity measures whose formulas are given below:

The original Amihud (2002) illiquidity measure

$$\frac{1}{N}\Sigma_{i=1}^{N}\frac{|Ret|}{Price \times Volume} \tag{3.1}$$

Square root of Amihud illiquidity ratio

$$\frac{1}{N}\Sigma_{i=1}^{N}\sqrt{\frac{|Ret|}{Price \times Volume}} \tag{3.2}$$

Amivest liquidity ratio

$$\frac{1}{N}\Sigma_{i=1}^{N}\frac{Price \times Volume}{|Ret|} \tag{3.3}$$

Squared root of Amivest liquidity ratio

$$\frac{1}{N}\Sigma_{i=1}^{N}\sqrt{\frac{Price \times Volume}{|Ret|}} \tag{3.4}$$

Volumn based illiquidity ratio

$$\frac{1}{N}\Sigma_{i=1}^{N}\frac{1}{Price \times Volume} \tag{3.5}$$

Turnover based illiquidity ratio

$$\frac{1}{N}\Sigma_{i=1}^{N}\frac{1}{Turnover},\qquad(3.6)$$

where $Turnover$=share trade/share outstanding

Return and turnover based illiquidity ratio

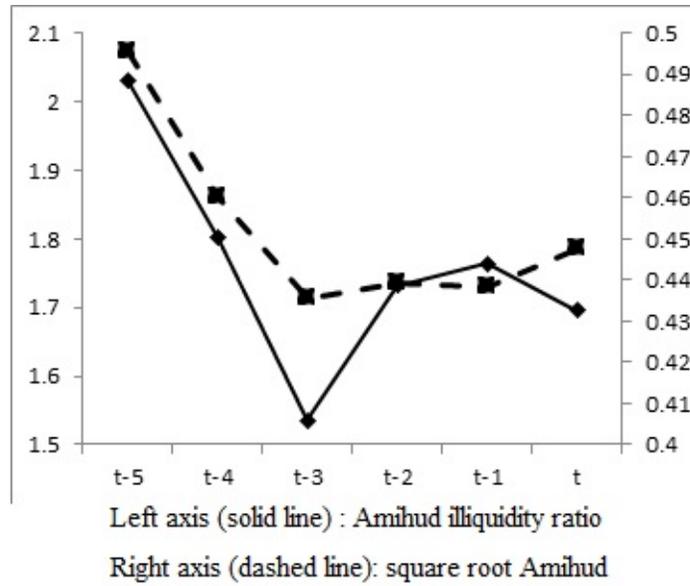$$\frac{1}{N}\Sigma_{i=1}^{N}\frac{|Ret|}{Price \times Volume}\qquad(3.7)$$

FIGURE 3.1 – **Stock liquidity before intervention**

This figures plots stock liquidity in the five years leading up to hedge fund intervention. The top panel plots stock liquidity measured by the Amihud (2002) illiquidity ratio (left axis) and its square root (right axis). The bottom panel plots stock liquidity measured by the Amivest liquidity ratio (left axis) and its square root (right aix). Year t is the year in which hedge fund intervention is announced.
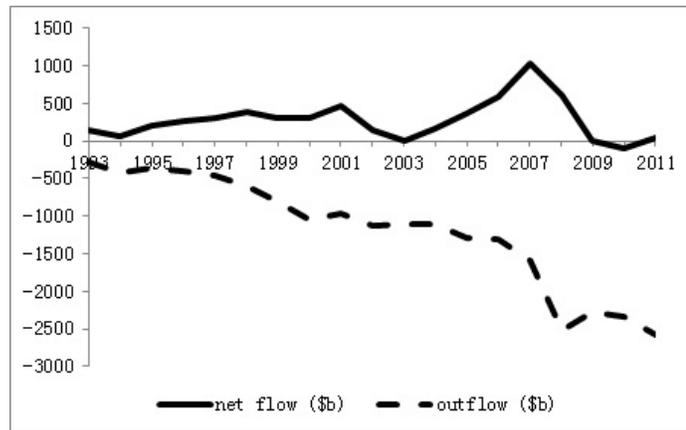
FIGURE 3.2 – **Fund flows**

This figure shows the dynamics of mutual fund capital flow, in billions of dollars, during the 1993 to 2011 sample period. The solid line represents the mutual fund industry as a whole; flows are aggregated across both funds with a net inflow of capital and those with a net outflow of capital. The dashed line represents only "outflow funds"; flows are aggregated across only funds with a net outflow of capital.
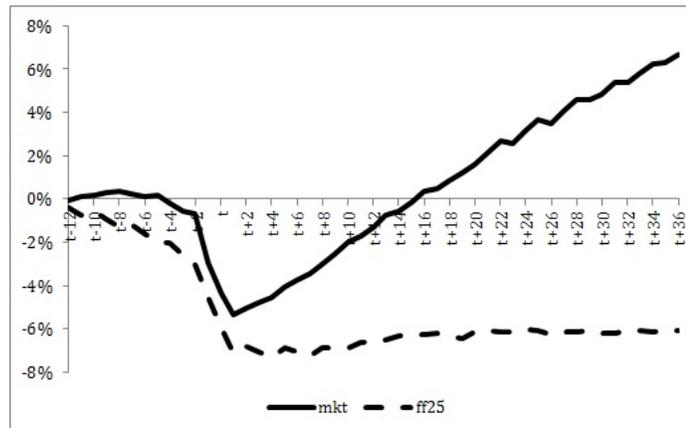

FIGURE 3.3 – **Abnormal returns around large mutual fund fire sales–CAAR**

This figure plots the monthly cumulative average abnormal returns around a large mutual fund fire sale event, defined as $MFFS$ (*mechanical* mutual fund fire sales) falling in the top 10% of all stocks with positive mutual fund fire sales that month. The sample period is 1994-2011. The solid line represents abnormal returns defined as stock returns in excess of the CRSP value-weighted market return. The dashed line represents abnormal returns defined as stock returns in excess of the Fama-French 25 size and book-to-market portfolio returns.
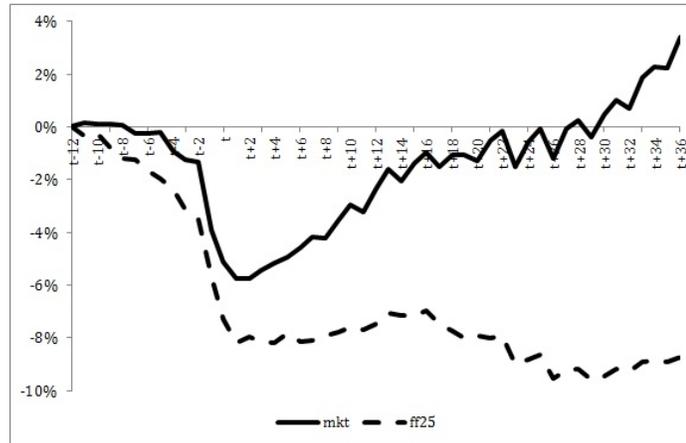
FIGURE 3.4 – **Abnormal returns around large mutual fund fire sales–B&H**

This figure plots buy-and-hold abnormal returns around a large mutual fund fire sale event, defined as $MFFS$ (*mechanical* mutual fund fire sales) falling in the top 10% of all stocks with positive mutual fund fire sales that month. The sample period is 1994-2011. The solid line represents abnormal returns defined as stock returns in excess of the CRSP value-weighted market return. The dashed line represents abnormal returns defined as stock returns in excess of the Fama-French 25 size and book-to-market portfolio returns.
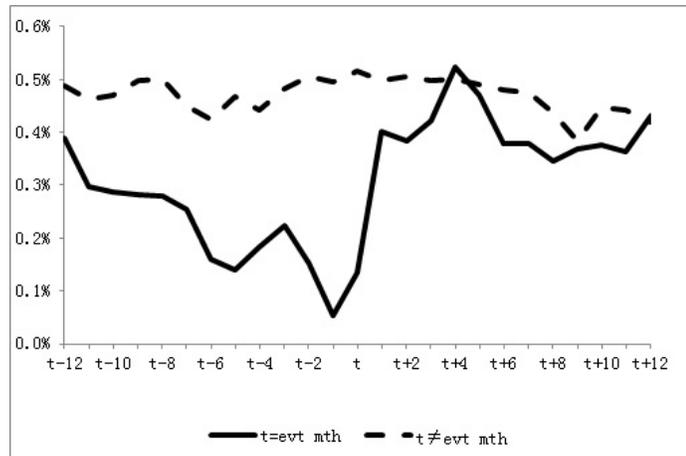

FIGURE 3.5 – **Fund performance around (non-) large outflow events**

This figure plots mutual fund returns around a large capital outflow event, defined as outflow in the top 10% of all mutual funds with an outflow at least 5% of fund assets that month, represented by the solid line. The dashed line represents mutual fund returns around a non-large outflow event. The sample period is 1994-2011.

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Biography

Heqing "Caroline" Zhu is a Doctor of Philosophy candidate in Finance at the Fuqua School of Business, Duke University. She holds a BBA from The University of Wisconsin-Madison. Her research focuses on empirical corporate finance, particularly corporate governance and institutional trading. She received the graduate Student Fellowship from Duke University, the Undergraduate Business Scholarship, the Kem- per Knapp Scholarship, and the William F. Vilas Scholarship from The University of Wisconsin-Madison and is a National Merit Scholar. She will join the Department of Finance at Price College of Business, University of Oklahoma as an assistant professor in August, 2014.