Governance under the Gun: Spillover Effects of Hedge Fund Activism[◆]

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March 2014

ABSTRACT

Hedge fund activism is a potent governance device associated with substantial improvements in the performance and governance of target firms. These positive changes often come at the expense of managers whose compensation and job security are threatened. Due to the activists' varying approaches and small ownership stakes, the threat of activism, unlike that of hostile takeovers in the past, is difficult to defend against with traditional tools such as poison pills. As a result, managers and directors are taking a more hands-on approach in evaluating and addressing potential vulnerabilities before an activist emerges. In this paper, we investigate the role of activism threat in motivating real policy changes at yet-to-be-targeted firms and examine whether such proactive responses are effective in fending off activists. We define threat as an abnormally high rate of recent activism in an industry and show that peers with fundamentals similar to those of previous targets are more affected by this threat. These threatened firms respond by reducing agency costs and improving operating performance in the same way as the actual targets. Such improvements lead to high abnormal returns and lower ex-post probability of becoming a target, suggesting that the proactive approach is indeed effective. Taken together, our results imply that shareholder activism, as a monitoring mechanism, reaches beyond the target firms.

Keywords: Shareholder activism, Corporate governance, Hedge funds, Institutional investors

JEL classification: G12, G23, G32, G34

[◆]We are grateful to Nicole Boyson, Alon Brav, Chris Clifford, David Dicks, Vivian Fang, Vyacheslav Fos, Paolo Fulghieri, Diego Garcia, Steven Kaplan, Camelia Kuhnen, Paige Ouimet, Urs Peyer, Raghu Rau, Jacob Sagi, Merih Sevilir, Geoffrey Tate, and seminar participants at UNC Chapel Hill and NC State for useful comments.

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1. Introduction

Hedge fund activism is an important governance mechanism consistently associated with marked improvements in the performance and governance of target firms (see Brav, Jiang, Partnoy, and Thomas, 2008; Becht, Franks, Mayer, and Rossi, 2008; Brav, Jiang, and Kim, 2013).[1] These positive effects often come at the expense of managers and directors who see a sizeable reduction in compensation and a higher likelihood of being replaced.[2] Moreover, a recent *New York Times* article suggests that shareholder activism has replaced hostile takeovers as the major disciplining force in the market for corporate control: "Today, hostile deals are on the wane, but a new threat has emerged that has put boardrooms on edge: activist investors."[3]

Unlike hostile takeovers, however, the *threat of activism* is more potent and difficult to defend against. Chris Young, head of contested situations at Credit Suisse, says, "There are no longer structural defenses. It used to be that you could set up staggered boards and put in poison pills. But there is no moat to build around your company anymore."[4] As a result, executives of yet-to-be-targeted firms are taking a more proactive approach and hiring advisors to help in evaluating potential vulnerabilities such as "whether the company's stock is trading at a discount to its peers, whether it has excess cash on the balance sheet"[5], etc. Investment banks and corporate lawyers are advising managers to monitor activism activities at their peer firms, "with a view toward minimizing vulnerabilities to attacks by activist hedge funds."[6]

Anecdotes suggest that this "activist fire drill" approach leads to real policy changes such as "spinning off divisions or instituting return of capital programs to quell dissent *before* it begins."[7] The *New York Times* article gives the example of EMC, a leading data storage provider, which started paying a dividend in part to detract activist attention from its large cash balance. These anecdotes also suggest that managers see past activist events at their peer firms as a sign of threat and proactively seek to institute policy changes that they believe will help prevent future attacks by dissident shareholders.

In this paper, we provide large-scale evidence in support of the above anecdotes. We use the full panel of U.S. firms between 2000 and 2011 to investigate the role of *activism threat* in

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[1] Recent academic work has shown that among activist investors, hedge funds achieve better success as monitors than mutual funds, pension funds, and labor unions (see Kahan and Rock, 2006; Gillan and Starks, 2007).

[2] Brav et al. (2008) show that CEO pay drops by about $1 million and CEO turnover goes up by 10% in the year following an activist intervention.

[3] See "Boardrooms Rethink Tactics to Defang Activist Investors", *The New York Times*, November 11, 2013.

[4] See footnote 3.

[5] See footnote 3.

[6] See "Key Issues for Directors in 2014" by Martin Lipton of Wachtell, Lipton Rosen and Katz, *The Harvard Law School Forum on Corporate Governance and Financial Regulation*, December 16, 2013.

[7] See footnote 3.

prompting policy changes at peers of activist targets and examine whether such proactive responses are effective in fending off activists. Our study complements previous work in hedge fund activism, which has focused mainly on documenting changes in corporate policies at actual targets. In their survey of the literature, Brav, Jiang, and Kim (2010) show that typical changes at targets include increases in dividend payout and leverage, decreases in cash and capital expenditures, and improvements in asset utilization. These changes are considered positive as they reduce agency costs and increase productivity, resulting in high abnormal returns. We provide novel evidence that policy improvements also spill over to yet-to-be-targeted but threatened peers, thereby shedding new light on hedge fund activism as a governance device. Absent these spillovers, the extant literature does not fully capture the impact of activism.

Our main findings can be summarized as follows. First, defining peers naturally as firms in the same industry, we demonstrate that an abnormally high rate of recent activism predicts future activism activity in the industry. Thus, we provide evidence that managers should rationally perceive recent activist events in their industry as a threat. Second, peers with fundamentals similar to those of previous targets experience a stronger threat and respond by reducing agency costs and improving operating performance along the same dimensions as the targets. Finally, we demonstrate that the positive policy changes at threatened peers lead to high abnormal returns and lower ex-post probability of becoming a target, suggesting that the proactive approach they take is indeed effective.

We begin by examining whether past activist events in an industry can be viewed as a sign of threat. We calculate an industry's target frequency as the number of targets divided by the total number of firms in the industry, and show that higher lagged target frequency predicts a higher probability that another firm in the industry will be targeted. This effect persists after including industry and year fixed effects and controlling for firm characteristics, which may impact the likelihood of being targeted. Henceforth, we will refer to the lagged target frequency in an industry as activism threat. Our results show that a one percent increase in threat leads to a 0.20% increase in the probability of becoming an activist target, an economically significant effect relative to the 2.7% unconditional probability.

We then establish that the threat effect is stronger for rivals whose fundamentals are more similar to those of actual targets. We capture the combined influence of firm characteristics on the targeting decision using a baseline target propensity, estimated as a probit function of a variety of firm attributes shown to be important for targeting (small size, low valuation, high institutional ownership, etc.). We find that within the same threatened industry, firms with high baseline target probability respond more strongly to activism threat than those with low baseline target probability (0.35% difference for a one percent increase in threat).

Using a difference-in-differences design, we demonstrate that peer firms in threatened industries lower their likelihood of being targeted by reducing agency costs and improving operating performance in the same way as actual targets. In response to activism threat, industry rivals increase leverage and payout, in line with the predictions of agency theory. They also reduce capital expenditures and improve asset utilization. Our triple-differences setup isolates these policy responses from those driven by (a) firm fundamentals and (b) product market competition (Aslan and Kumar, 2013). These findings are consistent with the anecdotal evidence presented earlier.

To differentiate the effects of activism threat from those of time-varying industry shocks, we instrument the threat in an industry by contemporaneous institutional trading in stocks *outside* of that industry. Similar to the use of extreme mutual fund flows to isolate valuation changes unrelated to firm fundamentals (see Coval and Stafford, 2007), we use institutional trading as a random shock that makes some marginal firms more attractive as activist targets. Our instrument allows us to strip out both firm and industry (observed and unobserved) fundamental information from activism threat and relate threat directly to the positive policy changes we document. In a similar vein, Brav, Jiang, and Kim (2013) conduct a variety of subsample analyses to establish that the performance improvements among target firms are not due to firm- or industry-level shocks that would have induced these changes absent hedge fund intervention.

We also find corroborating evidence in stock returns, suggesting that the market anticipates the positive real changes resulting from activism threat. In the year in which the threat emerges, industry peers with high baseline target probability (i.e., firms with characteristics similar to those of previous targets) experience substantially higher returns (ranging from 0.7 to 1.2% per month), compared to those with low baseline propensity. The abnormal returns we document are comparable to those observed in actual targets and decline gradually towards zero over the next two years.

In support of the "activist fire drill" approach advocated by investment bankers and corporate lawyers, we confirm that firms that proactively correct potential vulnerabilities are less likely to be targeted in the future. Our results show that the impact of threat on the probability of becoming a target is indeed weaker ex-post for peers that (a) improve more or (b) experience a larger increase in valuation, suggesting the presence of a partial feedback effect. Thus, positive policy changes seem to alleviate the need for activist monitoring or raise market valuations making it more costly for an activist to enter.

We make two important contributions to the literature. First, we contribute to the broad corporate governance literature by providing evidence of a new disciplining force in the marketplace – the threat of activism. Previous work has focused mainly on the threat of control contests (Servaes and Tamayo, 2013; Song and Walkling, 2000). However, Zhu (2013) presents evidence of substantial time variability in the threat effect of takeovers. A recent *New York Times* article makes a similar argument: "The hostile takeover is on life support, if it's not dead altogether. […] The real concern from the decline of a hostile takeover is that its disciplining effect will disappear. […] But unlike hostile takeovers, there is a real fear on Wall Street of activists."[8] We document strong industry persistence of activism, which is seen as a threat to yet-to-be-targeted firms in the industry.

Second, our results demonstrate positive real externalities of hedge fund activism, establishing that the impact of activism reaches beyond the firms being targeted and may have been underestimated in previous studies (Brav et al., 2008, among others.) These externalities have been an important but missing ingredient in the hotly contested debate on whether hedge fund activism is good or bad for the economy.[9] We show that managers rationally respond to the threat of activism in the way suggested by the anecdotal evidence – reducing agency costs and improving operating performance – resulting in positive abnormal returns. This proactive mentality advocated by investment bankers and corporate lawyers has positive real effects, which lower the need for activist intervention and therefore the ex-post probability of being targeted.

The rest of the paper proceeds as follows. Section 2 reviews the literature and develops specific hypotheses for our analysis. Section 3 describes the hedge fund activist sample. Section 4 investigates the presence of the threat channel at the industry and firm levels and establishes a causal relationship between industry-level threat and a firm's propensity of becoming a target. Section 5 presents the effects of threat on the returns and corporate policies of industry peers. Section 6 concludes.

2. Related literature and hypotheses development

In this paper, we empirically investigate the role of activism threat in motivating real policy changes at peer firms and examine whether such proactive responses are effective in preventing future attacks by dissident shareholders. Our goal is to provide evidence of the spillover effects of shareholder activism and contribute to a better understanding of activism as a governance mechanism. Spillovers from activism could result from product market competition or threat of

[8] See "With Fewer Barbarians at the Gate, Companies Face a New Pressure", *The New York Times*, July 30, 2013.
[9] For example, see "Don't Run Away from the Evidence: A Reply to Wachtell Lipton" by Lucian Bebchuk, Alon Brav, and Wei Jiang, *The Harvard Law School Forum on Corporate Governance and Financial Regulation*, September 17, 2013.

future activism. Our analysis focuses on the *threat channel* and identifies its effects from those of competition.

Previous work has examined the disciplining effects of the threat channel in proxy contests and takeovers. Fos (2013) demonstrates that the threat of a proxy contest induces real changes in firm policies. In the takeover market, Song and Walkling (2000) show that merger rivals experience high abnormal returns and that these returns are positively correlated with firm fundamentals that determine takeover probability. Servaes and Tamayo (2013) find that industry peers respond to control threats by changing certain firm policies.

Our study complements this literature by examining spillovers resulting from the threat of activism. As discussed earlier, shareholder activism has arguably replaced hostile takeovers as the major disciplining force in the market for corporate control. The anecdotal evidence presented in the introduction suggests that past activist events pose a potent threat to other firms in the industry. This threat effect may result from the fact that improvements at target firms often come at the expense of managers and directors. For example, Brav et al. (2008) show that "hedge fund activism is not kind to CEOs of target firms" (p. 1732). Following an activist intervention, CEO pay drops by about $1 million and CEO turnover goes up by 10%. Managers of yet-to-be-targeted rivals should rationally expect an increase in the probability that their firms will be targeted and that they may be fired. As a result, this threat motivates them to undertake some positive changes to fend off activists.

We define peer firms naturally as companies that operate in the same industry as a previous target. This industry dimension to the threat channel is supported by the theoretical literature. Jensen (1986) and Shleifer and Vishny (1988) show that the free-cash flow problem is an industry, rather than a firm, characteristic. Raff (2011) argues that knowledge spillovers could benefit the monitoring activities of pension funds and hedge funds intervening in multiple firms with common industry conditions.

Our first set of hypotheses tests for the presence of the threat channel at the industry and firm levels.

H1A. (Industry level) The rate of recent activism in an industry (activism threat) predicts a higher rate of future activism in the industry.

H1B. (Firm level) Controlling for firm fundamentals, activism threat predicts a higher likelihood of a firm in the industry being targeted.

H1C. (Cross-section) Within the same industry, firms with characteristics similar to those of previous targets respond more strongly to activism threat.

Our main variable of interest is target frequency, defined as the number of targeted firms divided by the total number of firms in an industry. We call the lagged target frequency in an industry *threat.* Hypothesis H1A focuses on the persistence of threat at the industry level. In order to control for firm characteristics that may impact the likelihood of being targeted (small size, low valuation, high institutional ownership, etc.), we confirm the presence of the threat channel at the firm level in Hypothesis H1B.

Peer firms may change their policies even in the absence of activism threat if they respond to time-varying industry conditions. For example, Mitchell and Mulherin (1996) relate clustering of acquisition activity within industries to common industry shocks and Harford (2005) shows that merger waves result from industry-level shocks, especially in liquid markets. Dong, Hirschleifer, Richardson, and Teoh (2006) establish a relationship between valuation multiples and takeover probabilities. We expect that the threat channel should affect more strongly peers whose characteristics make them attractive as activist targets (i.e., firms with high propensities of being targeted). Hypothesis H1C allows us to isolate the threat channel from time-varying industry fundamentals by examining the differential effect of threat among firms with high and low target propensities in the same threatened industry.

We expect that the presence of spillovers from hedge fund activism will also be detectable in the market returns of rivals. This prediction is supported by the literature on hedge fund activism, which shows that activists generate significant abnormal returns in their targets, both in absolute terms and in comparison to non-activist investing. Brav et al. (2008) report that the average hedge fund activist in 2001-2006 earned 14% higher return than the size-adjusted value-weighted portfolio of stocks. Clifford (2008) shows that activist hedge funds in 1998-2005 generated 22% higher annualized returns on their activist holdings than on their passive investments. Boyson and Mooradian (2011) compare aggressive activist and non-activist hedge funds and find similar results.

The share price response to the threat of activism should be positive due to the market's anticipation that (1) peers will improve their performance and governance in response to the announcement of activism at target firms, or (2) a higher likelihood that the peers which do not improve will become future activist targets.

H2: The threat of being targeted results in a positive share price response of industry peers. Within the same threatened industry, firms with characteristics shown to affect targeting experience a stronger market response.

A similar positive market reaction could be observed when activist monitoring of a target firm reveals to its peers new information about common industry conditions – *monitoring spillover* *hypothesis* (Raff, 2011). Under this alternative, any changes in firm policies at peer firms could be attributed to learning from the activist's monitoring rather than to the threat of becoming a future target. We will attempt to differentiate the threat channel from the monitoring spillover hypothesis, as well as the competitive channel (which predicts a negative market reaction), by comparing the price responses of rivals with low and high probabilities of becoming targets (based on characteristics common among targeted firms). Since these other hypotheses do not rely on the threat of activism, their effects should not differ significantly between peers with high and low propensities of becoming a target.

The threat of being targeted has a disciplining effect on peer firms, which respond by changing their corporate policies in order to mitigate this threat. These improvements should be in line with observed changes at actual targets. Previous work has shown that target firms reduce non-value maximizing behavior – increase leverage, payout and CEO turnover, and decrease capital expenditures. These findings support the theoretical literature on agency costs which argues that high leverage and payout limit a firm's ability to engage in value destroying activities (see Grossman and Hart, 1982; Easterbrook, 1984; Jensen, 1986; and Lambrecht and Myers, 2007). Empirically, Brav, Jiang, and Kim (2010) show that targets increase payout, CEO turnover, and pay-performance sensitivity. Both Clifford (2008) and Klein and Zur (2009) document increases in leverage and dividend yield, which they interpret as evidence of lower agency costs.

The literature also finds improvements in the operating performance of targets. Brav, Jiang, and Kim (2013) demonstrate that targets raise output, asset utilization, and productivity. Clifford (2008) also finds a statistically significant improvement in industry-adjusted return on assets (ROA), which he attributes to better asset utilization. Aslan and Kumar (2013) show that hedge fund activism leads to substantial increases in the market shares and cost markups of target firms.

We expect that peer firms will undertake policy changes along the same dimensions as those in target firms.

H3: Peer firms respond to the threat of activism by improving performance and reducing agency costs. Within the same industry, these changes will be positively related to firm characteristics affecting target choice.

Positive improvements of target firms are also likely to induce an intra-industry response from rivals through their competition for resources, talent or consumer demand.[10] Theoretically, Acharya and Volpin (2010) and Dicks (2012) model positive governance externalities due to

[10] A long theoretical literature relates competition to agency costs - Hart (1983), Holmstrom (1982), Nalebuff and Stiglitz (1983), Schmidt (1997), Allen and Gale (2000), Raith (2003). Empirically, Giroud and Mueller (2011) and Brav, Jiang, and Kim (2013) examine the interaction between product market competition and shareholder activism.

competition for scarce managerial talent. Empirically, Aslan and Kumar (2013) use business segment data to show that peers of activist targets experience negative abnormal returns as well as lower profitability and cash flows. Mietzner, Schweizer, and Tyrell (2011) also explore the competition hypothesis among rivals of German targets and find negative announcement returns but strongly positive one-year buy-and-hold returns.

Unlike the effects of the threat channel, those of the competitive channel critically depend on the position of each firm within an industry and the way in which the relationship between targets and peers is defined. We identify the effects of the threat channel from those of product market competition by using a *difference-in-differences* design. Specifically, we isolate the impact of the competitive channel by comparing changes in corporate policies between firms with high and low target propensities (that is, high and low activism threat) in the *same* threatened industry (and hence competing in the same product market).

Another important implication of the threat channel is that firms facing high levels of threat would respond by instituting value-enhancing changes, which in turn will reduce the effects of the threat on their likelihood of being targeted. This *feedback effect* could result from two sources: (1) The improvements at peer firms may alleviate or eliminate the problems which would have required the involvement of an activist, and/or (2) These changes would push up the peer firms' market values, which would make it more costly for an activist to initiate a campaign. This feedback effect has been shown both theoretically and empirically in different contexts.

In a survey of the literature, Bond, Edmans, and Goldstein (2012) argue that the informational role of (secondary) market prices has a feedback effect on the actions of decision makers. In the context of blockholder models, Edmans (2009) shows that a blockholder increases price informativeness by trading on private information, which affects the manager's incentives to improve long-term investment. Edmans and Manso (2011) show that a multiple blockholder structure is optimal when governing through exit because competitive blockholders' trading increases price informativeness.[11] In both papers, the threat of exit disciplines managers whose rational actions in turn eliminate the blockholders' need to carry through with the threat.

Empirically, Edmans, Goldstein, and Jiang (2012) show that the anticipation of a takeover attenuates the relationship between a target's valuation and its probability of being acquired. They also suggest that the market's anticipation of a takeover or an activist engagement raises prices, which in turn deters the intervention. Bradley, Brav, Goldstein, and Jiang (2012) examine the feedback loop between the discounts of closed-end mutual funds and open-ending attempts by arbitrageurs. They find evidence of a reduction in discounts not only through direct targeting

[11] On the other hand, Maug (1998) and Kahn and Winton (1998) show that low price efficiency facilitates blockholder formation and increases the likelihood of activist monitoring.

but also through an indirect 'anticipatory' channel which forces fund managers to take actions that lower the discount.

We expect that improvements resulting from the disciplining effect of activism threat will attenuate the need for activist involvement or raise valuations, reducing the probability of being targeted.

H4: Improvements in firm policies or the market's anticipation of such improvements reduce a firm's probability of being targeted.

It is important to note that H1 and H4 together assume that the feedback effect is only partial. If the feedback effect is complete, then we should not observe the presence of threat (H1) at all; managers will act to completely eliminate the need for an activist intervention. In reality, due to institutional and market frictions (such as agency conflicts and irrationality), it is reasonable to expect that the feedback effect will only be partial.

The above hypotheses summarize the expected spillover effects resulting from the threat of activism. A firm in an industry with a high rate of recent activism will have a higher likelihood of becoming a future target. This effect will be positively correlated with firm characteristics that affect target choice. Threatened firms will change firm policies in order to minimize the probability of being targeted, which will raise their valuations and in turn reduce their chances of becoming actual targets.

3. Hedge fund activist sample

The primary dataset used in this study is a hand-collected list of hedge fund activist campaigns between 2000 and 2011. The collection procedure combines data from regulatory filings and SharkRepellent.net and is described in more detail in Gantchev (2013). The primary data source is Schedule 13D, which must be filed with the US Securities and Exchange Commission (SEC) by any person or institution that acquires more than 5% of the voting stock of a public firm with the intention of influencing its operations or management. The list of activities requiring disclosure in Schedule 13D includes mergers and acquisitions, reorganizations, asset sales, recapitalizations, changes in dividend policies, board structure, charter or bylaws, exchange listing, and other similar actions.[12]

The full sample of activist targets with CRSP and Compustat data consists of 1,507 campaigns in

[12] An alternative filing with less stringent disclosure requirements is Schedule 13G, which is filed by large shareholders who intend to remain passive investors.

the twelve-year sample period. We exclude repeat targets within the same year, reducing the number of events to 1,397. For our industry-level analysis, we impose some additional restrictions such as requiring that each industry-year must have at least 5 firms.[13] These further reduce the sample size to 1,281 target-years, as seen in Table 1.

[Insert Table 1]

Our annual firm-level panel, which merges the activism dataset with the universe of CRSP and Compustat firms, consists of 50,957 firm-year observations. There is a significant time variation in the number of activist campaigns, which exceeds the average frequency in 2005 to 2008 but is substantially lower at the beginning and end of the sample period.[14] In most of our subsequent analysis, we control for the average rate of targeting in each year by using year fixed effects.

Table 1 also shows that about one-third of three-digit SIC industries (with at least 5 firms) get targeted each year. The cross-section of targeted industries is higher in the years with above-average number of campaigns (2005-2008). The last two columns provide some preliminary evidence that activist targeting exhibits industry persistence. For example, about two-thirds of targeted industries have target frequencies exceeding 5%. Furthermore, the number of targeted industries varies much less over the years than the number of targeted firms does, suggesting that hedge fund activists tend to scale their activities up and down within the same industries.

How does the typical target of hedge fund activism compare to the average firm? Previous work has documented that hedge funds usually target small firms whose recent stock market performance has been below their industry average. However, typical targets are not poorly performing even though they may suffer from slower sales growth than their industry peers. Hedge funds also tend to approach firms with large institutional ownership as institutional voting directly impacts a campaign's success in its more confrontational stages.[15]

Table 2 compares target and non-target firms along several valuation, performance, and governance dimensions. Similar to the findings of the previous literature, the average target in our sample is significantly smaller than the average firm in the CRSP/Compustat universe, with a mean (median) market capitalization of $1,080 ($178) million. The typical target tends to have a lower valuation, with a mean (median) *Q*-ratio of 2.00 (1.38) compared to 2.79 (1.49) for the average firm. This undervaluation is especially evident in terms of recent stock market

[13] These restrictions ensure that the main variable of interest in this study – target frequency, defined as number of targeted firms in an industry divided by total number of firms in the industry – has well-behaved cross-sectional and time-series distributions.
[14] Burkart and Dasgupta (2013) argue that increases in the net leverage of targeted firms and performance sensitivity of hedge fund investor flows generate pro-cyclicality in hedge fund activism.
[15] See Brav, Jiang, and Kim (2010) for a survey of the literature, including the general characteristics of target firms.

performance, with a median target stock return of -0.07 (or -7%) versus 0.02 for all firms. Targets also have somewhat higher stock turnover.[16]

[Insert Table 2]

In terms of their operational performance, targets have similar ROA as the median firm (0.08 for targets versus 0.09 for all firms) but lower median sales growth (0.05 versus 0.08). Targets also have substantially higher mean (median) institutional ownership – 0.51 (0.52) versus 0.44 (0.42). In terms of their governance (measured by the *G*-Index of Gompers, Ishii, and Metrick, 2003), target firms do not appear to be worse governed.

Overall, Table 2 shows that the target firms in our sample share the valuation, performance, and governance characteristics that the existing literature has identified as common among firms targeted by activist hedge funds. In our empirical analysis, we will control for these attributes since they may impact an activist's targeting decision in ways that are distinct from (yet related to) the activist's industry experience. Table A.1 in Appendix A presents estimates of a baseline probit model (column (3)), which predicts a firm's propensity to become a target as a function of (lagged) firm fundamentals. In order to improve the model's predictive power, we minimally transform some of the explanatory variables. However, our results are in line with the findings of Brav, Jiang, and Kim (2010) and Edmans, Fang, and Zur (2013). A firm's market capitalization, *Q*-ratio, stock return, and sales growth are negatively correlated with target propensity whereas ROA, payout, distance to default, and institutional ownership have a positive correlation with targeting. As reported at the bottom of Table A.1, the unconditional baseline probability of becoming an activist target is 2.7% per year.

The model specification in Column (3) controls for the average rate of targeting by industry and year with the inclusion of industry and year fixed effects. We use this model, setting the estimated fixed effects to zero, to calculate the baseline propensity that a firm, conditional on its fundamental characteristics, will be targeted in a given year. We then sort all firm-year (industry-year) observations based on their estimated target propensities (mean estimated target propensities for industries) into terciles. Panel A of Table A.2 (Appendix A) reports target frequencies for these pooled probability classifications. The target frequencies increase monotonically as we move from the low to the high propensity terciles, confirming that our baseline model fits the data well. In our empirical analysis, we use these *pooled* propensity classifications to investigate whether the strength of activism threat is positively correlated with fundamental characteristics that are important for targeting (as predicted by hypothesis H1C).

[16] To the extent that turnover proxies for liquidity, this finding is consistent with Edmans, Fang, and Zur (2013).

In Panel B of Table A.2 (Appendix A), we perform a similar exercise, but now firms are sorted into terciles within each industry-year. These groupings are highly correlated with the pooled classifications (for example, over 80% of firm-year observations are in the high propensity tercile under both classifications), suggesting that much of the variation in target propensity is within industry-year. This feature is important as we use these *within-industry-year* propensity classifications (which by construction contain an equal number of firms in each industry-year) to identify the spillover effects that occur through the threat channel. The idea is that within the same threatened industry, firms with high baseline target probability should respond more strongly to activism threat than those with low baseline target probability. This research design allows us to differentiate the threat channel from the competitive channel and the common effects of time-varying industry shocks.

4. Effect of industry threat on target probability

Our first goal is to demonstrate the presence of activism threat at the industry and firm levels. First, we show that the recent activism in an industry predicts a higher rate of future activism (hypothesis H1A). Then, we confirm this positive correlation at the firm level where we control for firm characteristics shown to affect targeting (H1B). We also present evidence that the threat channel is stronger for firms with high baseline propensity of being targeted (H1C). Finally, we use instrumental variables analysis to establish a causal link between industry-level threat and a firm's probability of becoming a target.

4.1. Industry-level analysis

We start by examining the persistence of activism at the industry level. We measure the rate of activism in each industry-year by *target frequency*, calculated as the number of hedge fund activist targets divided by the total number of firms in the industry. In order to ensure that target frequency has well-behaved cross-sectional and time-series distributions, we exclude industry-years with fewer than 5 firms.

In Table 3, we estimate panel AR(1) models for target frequency. The main explanatory variable is the lagged target frequency. The models in Columns (1)-(3) do not include industry fixed effects. In Columns (4)-(6), we control for industry fixed effects using the GMM estimator of Arellano and Bond (1991). We also control for the overall level of activism in the economy by adding the lagged market-wide frequency of activism across industries (in Columns (2) and (5)) or by including year fixed effects (in Columns (3) and (6)). Standard errors are adjusted for heteroskedasticity and autocorrelation (Windmeijer, 2005).

[Insert Table 3]

Columns (1)-(3) present results of OLS regressions predicting target frequency in an industry as a function of its own first lag. We find strong persistence in target frequency at the industry level even after controlling for the market-wide level of activism in Column (2) and adding year fixed effects in Column (3). The coefficients of lagged target frequency range from 0.126 to 0.167 and are statistically significant at 1% in all three specifications. The economic significance is also high – a one percent increase in the lagged rate of activism in the industry is associated with a 0.13-0.17% increase in target frequency.

We recognize that activism tends to concentrate in some industries and therefore it is important to control for time-invariant industry characteristics that may be important for activist targeting. However, OLS estimates of our models can be severely biased when industry fixed effects are included, due to the correlation between the lags of the demeaned target frequency (dependent variable) and the error terms.[17] We address this issue using the GMM estimator of Arellano and Bond (1991) and absorbing the industry fixed effects by the forward orthogonal deviation transformation (Hayakawa, 2009b).[18] Following Hayakawa (2009a), we use two lags of the backward orthogonal deviations as instruments in the transformed equation.

Columns (4)-(6) of Table 3 report the results of the GMM estimation. The specification in Column (5) is rejected by Hansen's test of over-identifying restrictions, suggesting that some of the model's moment conditions are invalid (i.e., the instruments are not exogenous). The models in Columns (4) and (6) are not rejected. Including year fixed effects to absorb temporal variation in activism appears to perform better than adding the market-wide rate of activism as a control variable. In all three models, the statistical and economic significance of the lagged target frequency remains virtually unchanged; correcting for the average rate of targeting per industry does not affect our conclusion.

Figure 1 presents univariate evidence that the rate of activism within an industry appears to be persistent through time. We split industries into two groups – high and low lagged target frequencies – based on the residuals from the AR(1) specification in Column (6) of Table 3. This classification divides industries into those with abnormally high and abnormally low activism

[17] This problem is particularly acute in this setting since the activism sample has a very small number of time-series observations (annual frequency, 2000-2011).

[18] See Arellano and Bover (1995), Blundell and Bond (1998), and Flannery and Hankins (2013) for the performance of this estimator. Hayakawa (2009b) shows that the forward orthogonal deviation transformation performs significantly better than the first difference transformation.

frequencies. We report the average rate of activism in the current year for the two groups. The figure clearly shows that industries with high recent abnormal rate of activism (high threat) also have a high current rate of activism. Note also the significant variation in the industry-level frequency of activism during the sample period.

[Insert Figure 1]

4.2. Firm-level analysis

Our results so far show that the lagged activism frequency in an industry predicts a higher current rate of activism in that industry. One concern with the industry-level analysis is that we cannot control for firm characteristics, which may impact a firm's likelihood of being targeted and potentially the strength of the threat it is exposed to. As shown in Table 2, hedge funds typically target firms with fundamentals that are different from those of the average firm. The presence of firms with the 'right' characteristics in certain industries may explain the industry persistence of activism we document.

We introduce firm-level controls in Table 4. Our goal is to confirm hypothesis H1B that the recent rate of activism in an industry is associated with a higher probability of a firm in that industry becoming a target after controlling for its fundamentals.

[Insert Table 4]

In Panel A of Table 4, we present linear probability models (LPMs) of activist targeting. We use an LPM as opposed to probit or logit because estimates of these non-linear models with a variety of fixed effects, as in our case, are often biased in finite samples. Moreover, we include interaction terms in Column (5) and their coefficients are more straightforward to interpret in an LPM than in a non-linear model. The main explanatory variable – *Threat* – is the lagged activism target frequency in a firm's three-digit SIC industry, calculated as in Table 3.

Comparing Columns (1) to (2), we see that the inclusion of firm-level controls (defined in Table A.1 of Appendix A), significantly improves the model fit. As expected, firm fundamentals play an important role in determining the targeting decision and their effects overlap somewhat with those of activism threat. Still, *Threat* remains highly economically and statistically significant (at 1%). A one percent increase in *Threat* leads to a 0.20% increase in the probability of becoming a target. This is economically significant given that the unconditional probability of a firm being targeted is 2.7%. In Column (3), we add industry fixed effects to control for the average industry rate of activism during the sample period. This makes sure that our results are not driven by a few industries that hedge fund activists find attractive. The coefficient of *Threat* drops only slightly but it remains positive and statistically significant at 1%.

So far, the firm-level regressions in Table 4 demonstrate that a firm in an industry with a high incidence of recent activism is more likely to become a future target. However, Column (4) of Panel A shows that adding year fixed effects weakens substantially both the statistical and economic significance of the *Threat* coefficient. As suggested by the results in Table 1 and Figure 1, the overall rate of activism varies significantly over time and is highly persistent (e.g., it is highest during 2005-2008). Therefore, the reduction in the estimated effect of threat, after the year dummies are included, indicates that the threat levels for different industries tend to have the same temporal variation, going up and down together over time. We will address this issue in our subsequent analysis by resorting to the cross section of firms that are differentially threatened by the presence of hedge fund activists.

As discussed earlier, a high level of threat in an industry does not imply that all firms in the industry experience the same increase in their likelihood of being targeted. According to H1C, we expect that the effect of threat will be stronger for firms that look like typical activist targets (i.e., firms in the top tercile of their baseline target probability). We test this hypothesis in Column (5) of Table 4 Panel A. We interact *Threat* with an indicator for firms with medium and high baseline probabilities (middle and top terciles as in Panel A of Table A.2). As predicted by H1C, the effect of *Threat* is larger in magnitude and statistically significant at 1% in the high probability subsample (even after the year fixed effects have been absorbed.) A one percent increase in *Threat* leads to a 0.26% (coefficient = 0.349 – 0.094) increase in the probability of becoming a target. However, this effect is not statistically significant in the medium probability tercile and marginally significant and negative in the low probability tercile.

In Panel B of Table 4, we confirm the presence and differential strength of activism threat by splitting the full sample into high and low probability terciles. The threat channel seems to operate only in the high probability group. Note also that *Threat* remains highly statistically significant even with the inclusion of industry and year fixed effects in Column (2).

4.3. Instrumental variables analysis

The analysis in the previous subsection demonstrates that a firm in an industry with a high rate of recent activism is more likely to be targeted even after controlling for firm level characteristics. Firms with high baseline propensity of being targeted drive this relationship. These findings are also robust to the inclusion of industry and year fixed effects, which take out the average rates of activism by year and industry. One remaining concern, however, is that our results may be produced by persistent (unobserved and hence omitted) industry and firm fundamentals that attract activist hedge funds in the first place.

Activist targeting is clearly not exogenous to firm performance and governance. The ideal experiment would randomly assign a target to an activist and then track the subsequent involvement of the activist in other firms in the initial target's industry. In the absence of such an experiment, we attack this type of endogeneity by an instrumental variables analysis. We use predicted institutional trading in a firm's stock to instrument for activism threat. The intuition here is similar to the use of extreme mutual fund flows to isolate valuation changes that may drive some endogenous events but are unrelated to firm fundamentals. In the same way, keeping firm and industry characteristics fixed, a temporary market misvaluation acts as a random shock that makes some *marginal* firms more attractive as activist targets.[19] We consider these opportunistic targets as 'pseudo' randomly assigned in our context.

Our instrument helps to differentiate the threat channel from the effects of time-varying industry shocks. We predict the level of activism threat in an industry as a function of contemporaneous institutional trading in stocks *outside* of that industry. Our instrument is therefore institution-specific, rather than firm- or industry-specific, which allows us to strip out both firm and industry (observed and unobserved) fundamental information from threat. Showing any persistence resulting from this instrumented threat, or the incremental rate of activism triggered by misvaluation, would provide evidence of a threat channel that is distinct from the effects of an evolving industry structure.

We construct the instrument using institutional trading data from Ancerno, which provides transaction cost analysis to mutual funds, pension plan sponsors, and brokers, representing up to 20% of total CRSP volume during 2000-2011. The data cover the trading activity of such household names as Fidelity, Vanguard, AllianceBernstein, etc. and include the execution date and time; the stock ticker and number of shares traded; the price, commission, and taxes per share; the direction of each trade and an identifier for the trading institution.[20]

Our instrument relies on the relationship between each institution's weekly trading in a firm and

[19] Gantchev and Jotikasthira (2013) show that favorable market conditions induced by institutional trading significantly influence the activist's targeting decision.
[20] Puckett and Yan (2011) argue that the Ancerno dataset suffers from no significant survivorship or selection biases.

that institution's trading in other stocks outside of the firm's industry.[21] We estimate this relationship and use it to calculate the probability that an institution will buy or sell a given stock during each trading week. For brevity, the estimates are presented in Appendix B (Figure B.1). Consistent with the findings of Gantchev and Jotikasthira (2013), Panel A shows that the fraction of other stocks sold by each institution is positively (negatively) related to the probability that the institution will also sell (buy) the firm's stock. We perform the estimation separately for each calendar quarter but our estimates appear qualitatively similar in all periods. In the last step, we multiply the predicted selling and buying probabilities for each institution-stock-week by the institution's average trade size and sum the product across all institutions holding a particular stock. This gives us the predicted weekly buy and sell volumes in each firm, which we first aggregate to the quarterly frequency using the 75th percentile function and then average across all quarters within a year.[22] Finally, we calculate the mean predicted buy and sell volumes (normalized by each firm's outstanding shares) across all firms in an industry to obtain the expected industry buy and sell volumes. Figure B.2 presents the empirical distributions of these expected volumes, which are well behaved. The variation of the expected buy and sell volumes across industry-year observations is critical for our identification.

Panels A and B of Table 5 present the second and first stages, respectively, of our instrumental variables analysis. We use GMM to obtain the estimates for both stages in one step. We instrument both the endogenous variable (*Threat*) and its interaction with an indicator (*HighProb*) for firms in the top baseline probability tercile. Here, we do not distinguish the low and medium baseline probability terciles since doing so would require more instruments and our results in Table 4 suggest that the effects of threat are essentially zero for both groups. All regressions include firm-level controls and year fixed effects. Columns (3) and (5) also include industry fixed effects.

[Insert Table 5]

Before we discuss the results, it is important to note that our instruments are valid both from the identification standpoint and from the exogeneity standpoint. All specifications, except in Column (5), pass the Kleibergen-Paap Lagrange Multiplier (LM) underidentification test, which measures whether the correlation between the instruments and the endogenous variables is

[21] As shown by Coval and Stafford (2007) among others, an institution experiencing large inflows and outflows often scales its existing stock positions up and down proportionally. Thus, if an institution trades in response to funding shocks, we should see that it trades most stocks in the same direction. Gantchev and Jotikasthira (2013) use this idea to construct an instrument for institutional trading in predictive regressions for activist targeting and for activist purchases of target shares. Here, we use a similar instrument but the instrument is not for institutional trading but for target frequency in an industry.

[22] We focus on the right tail of the distribution due to the fact that activists tend to accumulate a substantial stake in a relatively short period of time when institutional selling volume is unusually high (Gantchev and Jotikasthira, 2013).

statistically different from zero. Moreover, all models are overidentified and are not rejected by the test of overidentifying restrictions (based on Hansen's *J* statistic) at conventional significance level, indicating that our instruments satisfy the exclusion restrictions.

Column (1) of Panel A reports results for the full sample. The IV estimates establish causality between activism threat at the industry level and firm-specific target propensity; a high recent rate of activism in an industry results in a higher probability that a firm in that industry will be targeted. As shown earlier, this result is driven by firms with high baseline propensity of being targeted. The coefficient of interest – *HighProb*Threat* – is positive and statistically significant at 1%. The coefficient on *Threat*, which captures the effect for firms in the low and medium baseline probability terciles, is marginally statistically significant and positive. We interpret this result as evidence that the true effect of threat is weak for firms with low and medium target propensities and offset by their corrective responses to time-varying industry shocks (making these firms relatively less attractive as activist targets).

Columns (1a) and (1b) of Panel B present the first stages for Column (1) of Panel A. Both *Threat* and its interaction with *HighProb* are instrumented with contemporaneous institutional buy and sell volumes in *other* industries. We see that high predicted institutional selling results in high threat, i.e., more firms in the industry being targeted, whereas high predicted institutional buying has the opposite effect. Both coefficients are statistically significant at 1%. The coefficients of the interaction terms also have the expected signs and are highly statistically significant.

The remaining columns of Panel A present estimates for the split samples of firms with high and low baseline target probabilities. The results in Columns (2) confirm that the threat channel operates strongly for firms with characteristics similar to those of previous activist targets. The coefficient on *Threat* is high in magnitude and statistically significant at 1%. The inclusion of industry fixed effects in Column (3) increases slightly the magnitude of the threat effect, which is now statistically significant only at 5% due to a larger standard error. Columns (4) and (5) provide additional evidence that the threat channel is weak and statistically insignificant for firms that do not look like potential targets. Note, however, that the lack of statistical significance in the second stage does not come from a lack of instrument power, as seen in Columns (4) and (5) of Panel B.

Together, the results in Table 5 establish causality between industry-level activism threat and firm-specific target propensity. Our instrument strips out both firm and industry fundamentals from threat, allowing us to differentiate the threat channel from the effects of time-varying industry shocks. This also addresses potential omitted variable bias due to the correlation between activist targeting and persistent (unobserved) firm and industry fundamentals.

5. Peer response to activism threat

The threat of being targeted has a disciplining effect on peer firms, which respond by changing their corporate policies in order to mitigate this threat. First, we show that industry rivals experience positive abnormal returns following a heightened level of threat, suggesting that the market anticipates improvements in their valuations (hypothesis H2). Then, we relate these valuation improvements to actual corporate policy changes at rivals such as performance improvements and reduction in agency costs, similar to those typically observed in actual targets (H3). We identify both of these effects from other confounding forces (such as competitive effects and responses to time-varying industry shocks) using a *difference-in-differences* design. The identifying assumption is that the valuation and policy improvements that are a result of activism threat should be stronger for firms with high baseline target probability. As shown in Section 4, these firms experience a significantly larger increase in the probability of being targeted following recent activism activities in their industries. Finally, we show that these policy and valuation improvements do indeed lower the ex-post probability of being targeted implying the presence of a (partial) feedback effect (H4).

5.1. Returns

We start our investigation of the response of peers to the threat of activism by asking whether the market anticipates the disciplining effect of this threat. We hypothesize that the share price response will be positive due to the market's expectation that (1) rivals will improve their performance and governance in response to the announcement of activism at target firms, or (2) a higher likelihood that the peers which do not improve will become future activist targets.

Previous work has documented that targets experience significant positive returns at the announcement of activism. In their review of the literature, Brav, Jiang, and Kim (2010) report abnormal returns of 6% for the [-20, +20] window around announcement. Klein and Zur (2009) find a [-30, +30] market-adjusted return of 7.2% while Clifford (2008) estimates a [-2, +2] market-adjusted return of 3.39%. For longer horizons, Clifford (2008) reports three- and four-factor monthly alphas between 1.5% and 1.9% in the year following activism.

In Table 6, we investigate the effect of threat on the stock performance of industry rivals. We estimate the following model:

$$AR_{ij,t+h} = a_j + b_t + \beta * HighProb_{ij,t-1} + \varepsilon_{ij,t+h},$$

where *AR* is the average monthly abnormal return for firm *i* in industry *j* for year *t+h*, a_j denotes industry fixed effects, and b_t represents year fixed effects. *HighProb* is a dummy for firms in the high baseline probability tercile (see Panel A of Table A.2). The use of the *HighProb* dummy helps isolate the threat effect from other confounding forces that may also affect valuation. For example, Aslan and Kumar (2013) show that due to product market competition, a target's improvement comes at the expense of rival firms, which suffer negative abnormal returns upon the announcement of activism at the target. To the extent that these competitive effects are not correlated with the strength of activism threat, our specification is poised to pick up the threat effect as the coefficient of the *HighProb* dummy.

[Insert Table 6]

In Column (1), the dependent variable is the monthly raw return. In Columns (2)-(5), we adjust the returns using the matched Fama-French (FF) 48 value-weighted (ffi48v) and equally-weighted (ffi48e) industry portfolios and the matched FF 25 value-weighted (ff25v) and equally-weighted (ff25e) size and style portfolios. We avoid model-based adjustments due to the potential instability of factor loadings around the periods of heightened activism in an industry. Panels A, B, and C report return estimates for the year in which we identify the threat (*t-1*), the treatment year (*t*), and the subsequent year (*t+1*), respectively. To cleanly identify the effects of each event horizon, we drop the industries in which threat emerges in both years *t*-1 and *t*. In all specifications, we cluster standard errors by industry and year.

Regardless of the risk adjustment model, Panel A clearly shows that the market anticipates a positive valuation effect associated with the threat of activism. In the period, in which an industry experiences an abnormally high rate of activism (year *t*-1), industry rivals with high baseline target probability see substantially higher returns (ranging from 0.7 to 1.2% per month), compared to firms with low baseline propensity. Summary statistics further show that this difference is driven primarily by the positive abnormal returns experienced by rivals in the high baseline probability group. The low baseline probability peers experience abnormal returns that are statistically indistinguishable from zero.

Panels B and C show that the positive returns of peers with high baseline target probability shrink towards zero in the two years following the unusually high rate of industry activism (treatment year *t* and subsequent year *t*+1, in which we postulate that the threatened peers will make positive policy changes). These returns (ranging from 0.1 to 0.4% per month) are statistically insignificant and economically much smaller than those experienced when activism threat emerges. Thus, much of the valuation improvement that results from activism threat is complete within the year of the threat event, suggesting that such improvement is anticipatory and strongly related to the unfolding of activist campaigns. Finally, we do not observe any price reversals or any other indication that the abnormal returns we uncover are due to mechanical or behavioral biases.

The results so far demonstrate that the market anticipates an improvement in the valuation of those industry peers most likely to be targeted. Similar positive market reaction could be observed if the activist's monitoring reveals some new information about common industry conditions – *monitoring spillover hypothesis* (Raff, 2011). Under this alternative, any changes in firm policies at peer firms could be attributed to learning from the activist's monitoring of the target rather than to the threat of becoming a future target. Since this hypothesis does not rely on the threat of activism, its effects should be observed in peers with both high and low probability of becoming a target. Therefore, the results in Table 6, which are obtained by differencing the returns of firms with high and low baseline probabilities, should be free from the monitoring spillover effects.

5.2. Effect of threat on corporate policies

Industry rivals experience positive abnormal returns following a heightened level of threat. In this subsection, we associate this positive market reaction with improvements in corporate policies in line with those observed in actual activist targets.

Previous work has documented that hedge fund activism creates value at target firms by improving operating performance and reducing agency costs. Brav, Jiang, and Kim (2010) show that targets experience improvements in *Q*-ratio, dividend payout, and CEO turnover. They also report statistically significant changes in operating performance (ROA) after correcting for sample selection.[23] Clifford (2008) also finds a statistically significant improvement in industry-adjusted ROA in the two years following activism and attributes most of this improvement to better asset utilization. Both Clifford (2008) and Klein and Zur (2009) document post-event increases in leverage and dividend yield, which they interpret as evidence of lower agency costs.

We use a (triple) difference-in-differences research design to tease out the disciplining effects of threat. Specifically, we estimate the following model:

$$\Delta X_{ij,t+1}^{Treated} - \Delta X_{ij,t+1}^{Control} = a_j + b_t + \beta * HighProb_{ij,t-1} + \varepsilon_{ij,t+1},$$

[23] Brav, Jiang, and Kim (2010) point out that one-fifth of their sample disappears from Compustat within 2 years of intervention, which induces a negative bias in measuring post-event performance as the missing firms most likely represent successful outcomes of activism.

where ΔX is the change in a variable of interest, a_j denotes industry fixed effects, and b_t represents year fixed effects. Standard errors are clustered by industry and control firm (as we use repeated controls).

First, we define the event year as the year in which an industry experiences higher-than-expected activism frequency. We compare changes in corporate policies one year after to one year before the event (first difference). Second, in any given year, we identify an industry as being threatened if its residual from the AR(1) specification in Column (6) of Table 3 is positive in the prior year. That is, industries are considered threatened this year if they experience higher than anticipated rate of activism last year. *Within* the threatened industries, we compare changes in corporate policies between firms with high and low baseline target probabilities (top and bottom terciles in Panel B of Table A.2), which form our group of *treated* firms. Thus, the indicator *HighProb* measures the marginal change in a corporate policy between treated firms with high and low baseline target probabilities, i.e. between treated firms that are most and least threatened by recent activism activities in their industry. This second difference allows us to identify the threat channel from other confounding forces such as the competitive channel and time-varying industry shocks. These other forces should not differ in a systematic manner between the most and the least threatened firms.

However, as illustrated in Table 7, treated firms in the high and low probability terciles differ along many dimensions. Changes in corporate policy that coincide with the event may be driven by these fundamental characteristics. That is, firms with characteristics similar to those of a typical activist target (and hence, having high baseline target probabilities) may change their policies anyway, regardless of their exposure to activism threat. As a result, to ensure that the changes we observe are directly attributable to threat rather than to these fundamentals, we create two separate *control* groups for the treated firms with high and low target propensities (third difference). The control firms come from the industries that are not threatened in the three years around the event, and are matched to treated firms on baseline target propensity (i.e., combined score of important fundamental characteristics), ROA, leverage, and payout. Table 7 shows that the control firms do indeed look similar to treated firms not only along the dimensions we match on but also in terms of other corporate policies that hedge fund activists often try to change. Differencing changes in firm policies between treated and control firms allows us to absorb trends in firm characteristics that may affect a firm's target probability. In order to control for the substantial difference in the market equity of firms in the high and low target probability terciles, we will also add an additional size control in the analysis of corporate policy changes.

[Insert Table 7]

Figure 2 illustrates the performance of our matching methodology. Panel A plots the density functions of baseline target propensity for the treated, non-treated (some chosen as control firms), and actual target firms. The bimodal solid line clearly shows that the baseline target probabilities differ between treated firms that are most and least threatened. The distributions of the actual targets and other non-target firms lie in the middle between the two modes, with the targets showing higher propensities as expected. Given these distributions, it is important to pick controls among non-treated firms that match most closely in propensity scores the two distinct groups of treated firms. Panel B shows that we have achieved this objective in our construction of the control groups; the two control groups match well the treated firms with high and low baseline target probabilities.

[Insert Figure 2]

Before we discuss our results, it is helpful to summarize again the rationale behind our triple difference methodology. In our context, it is difficult to isolate the changes in firm policies resulting from the presence of an activist investor from changes that firms would have implemented even without the activist's involvement. Put differently, some may argue that hedge fund activists are very good at picking targets that would benefit the most from certain industry trends. By comparing firms with high and low target probabilities in the same threatened industry, we control for overall trends in industry characteristics. By using separate control groups for each probability tercile of treated firms, we also correct for any trends in firm characteristics that are not industry-specific. The combination of these two differences allows us to control for the interaction between industry and firm fundamentals. Thus, any observed changes in firm policies should be attributed to the threat of activism.

Table 8 reports changes in corporate policies resulting from the disciplining effect of threat. We interpret the coefficient on *HighProb* as the differential abnormal change in a specific policy between treated firms with high and low baseline target probabilities (abnormal since the changes are measured against their respective controls). The difference between Panel A and Panel B is the inclusion of an additional size control in the latter.

[Insert Table 8]

The results demonstrate that the threat channel leads to a reduction in agency costs along the same dimensions observed in activist targets (see Brav, Jiang, and Kim (2010), among others). Specifically, we show statistically significant increases in leverage and payout, in line with the predictions of agency theory. We also see a reduction in capital expenditures, which becomes statistically significant after controlling for firm size. In terms of economic magnitude, the difference in leverage between the threatened and control firms is 1.3-2.1% more among firms in the top tercile of baseline target propensity than those in the bottom tercile. This magnitude is comparable to leverage changes in target firms (Brav et al., 2013) and in firms in US states that increase state tax rates (Heider and Ljungqvist, 2013). Similarly, most threatened firms also increase payout yields by 2.7-4.8% and reduce capital expenditures by 0.3-0.7% of total assets compared to the relevant benchmarks.

In terms of operational improvements, treated firms with high target propensity significantly improve their asset utilization, as measured by asset turnover, but not their profitability (ROA). This is in line with Clifford's (2008) findings for activist target firms. As for economic magnitude, the difference in asset turnover between the threatened and control firms increases by 0.034-0.041 more among firms in the top tercile of baseline target propensity than those in the bottom tercile. This is about 4-5% of the interquartile range of asset turnover for these treated and control firms. It is important to note that these changes in asset turnover, which may appear small, reflect the differential changes between the most and the least threatened firms, not the unconditional changes among all threatened firms.

Together, the results in Table 8 demonstrate that recent activist interventions have a disciplining effect on industry peers, which respond by reducing agency costs and improving operational performance. These effects are similar to those documented by Fos (2013) who shows that firms exposed to potential proxy contests increase leverage, dividends and CEO turnover and reduce capital expenditures. However, our results differ from the findings of Aslan and Kumar (2013) who demonstrate that rivals of activist targets experience significant deterioration in cash flows, ROA and EBITDA as the targets become more competitive in their product markets. Our empirical design differences out these competitive effects by comparing changes in corporate policies between firms with high and low target propensities (hence, high and low activism threat) in the same threatened industry. Thus, to the extent that the effects of product market competition are not positively correlated with our measure of activism threat, the differential improvements in corporate policy that we identify must occur through the threat channel.

5.3. Feedback effect

In this subsection, we examine whether the improvements implemented by threatened firms reduce their probability of becoming future targets. This feedback effect could result from two sources: (1) the improvements at peer firms may alleviate or eliminate the problems which would have required the involvement of an activist, and/or (2) these changes would push up the peers' market values, making it more costly for an activist to initiate a campaign. It is important to note that in a perfectly rational world with no frictions, the feedback effect should completely eliminate activism threat so that the recent rate of activism in an industry is uninformative of future activism activities in that industry. Thus, our study is based on the assumption that due to some organizational and/or market frictions, policy and valuation changes are insufficient to completely eliminate the effect of threat. We rely on the cross section of such changes among most threatened firms to measure the feedback effect.

Previous work has documented the presence of feedback effects in other settings. Empirically, Edmans, Goldstein, and Jiang (2012) show that the anticipation of a takeover attenuates the relationship between a target's valuation and its probability of being acquired. Bradley, Brav, Goldstein, and Jiang (2012) examine the feedback loop between the discounts of closed end mutual funds and open-ending attempts by arbitrageurs. More generally, Bond, Edmans, and Goldstein (2012) survey previous work on the informational role of market prices.

Table 9 investigates the presence of a feedback effect in firms with high baseline target probabilities. We have shown in Tables 4 and 5 that the threat of activism is strongest among these firms. The main explanatory variable is the interaction term between *Threat* and *Dummy(#)* where *Dummy(#)* is defined as in the heading of each column *(#)*. In Columns (1) and (2), the dummy variable equals one if the firm's baseline target propensity decreases from year t (when threat is observed) to year t+1 (when the effects of threat are measured), and 0 otherwise. We think of the baseline target propensity as a non-linear combination of a variety of corporate policies and characteristics that matter for targeting, and hence a decrease in this propensity suggests that threatened firms have changed to fend off activists. In the next four columns, the dummies are designed to capture valuation improvements, which should reflect the market's expectation of policy improvements or of an increased probability that a threatened firm will eventually be targeted. In Columns (3) and (4), the dummy equals one if the industry-adjusted average monthly return is positive. In Columns (5) and (6), the dummy is one if a firm's market-adjusted return is positive. Even-numbered columns include industry and year fixed effects as well as firm-level controls. Standard errors are clustered by industry.

[Insert Table 9]

The results in Table 9 show that activism threat (i.e., the rate of activism in a firm's industry in the prior year) raises the probability that a firm will be targeted this year. However, in all specifications, the interactions between *Threat* and measures of policy and valuation improvements have large and negative effects on the probability of being targeted. In five of the six models, the interaction term is also statistically significant. These results suggest that improvements resulting from the disciplining effects of activism threat mitigate, or in some cases, eliminate the need for activist involvement, thereby reducing the probability of being targeted. Looking at the columns with full controls (Columns (2), (4), and (6)), a one percentage point increase in an industry's target frequency last year increases by about 0.3-0.4% the probability that a firm (with already high baseline target probability) in that industry will be targeted this year. However, these incremental probabilities are virtually erased for the threatened firms that make positive changes. Note that these effects are not produced mechanically by the way we identify improving firms; the own effects of improvement (captured by the coefficient on *Dummy*) are actually small but positive.

Together, the feedback effects we show here support the overall idea that shareholder activism has a disciplinary effect on non-target firms. As activists gain experience and confidence in an industry, managers of firms in the industry rationally expect an increase in the probability that their firms will be targeted. This threat motivates them to implement some positive changes that they believe will reduce the threat. Here, we show that these improvements do indeed lower the *ex-post* frequency that the threatened firms are targeted.

6. Conclusion

This paper empirically studies the spillover effects of hedge fund activism. We show that the recent rate of activism in an industry predicts a higher rate of future activism and confirm that this positive correlation exists at the firm level after controlling for firm characteristics shown to affect targeting. Therefore, past activism activities in an industry pose a "threat" to yet-to-be-targeted firms in that industry. Firms with high baseline propensity of being targeted, i.e., those that look like a typical activist target to start with, drive this relationship. Using institutional trading in stocks outside of a firm's industry as an instrument, we identify the threat channel from other observationally equivalent phenomena, such as persistent time-varying industry shocks driving waves of activism.

The threat of being targeted has a disciplining effect. We find that industry peers experience positive abnormal returns following a heightened level of threat and we relate these valuation improvements to actual corporate policy changes such as performance improvements and reduction in agency costs, similar to those observed in actual activist targets. Finally, we show that these policy and valuation improvements indeed lower the ex-post probability of being targeted implying the presence of a feedback effect. Our results provide new evidence that shareholder activism, as a monitoring mechanism, reaches beyond the firms being targeted.

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Figure 1: Activism Target Frequency Conditional on Lagged Threat

This figure reports the average target frequency by year for industries experiencing *High Threat* versus *Low Threat*. For each industry-year, target frequency is calculated as the number of firms targeted by hedge fund activists divided by the total number of firms in the industry. For each year, the high threat group (low threat group) includes industries with positive (negative) previous-year residuals obtained from the panel AR(1) model in Column (6) of Table 3. Only industry-years with at least 5 firms are included.

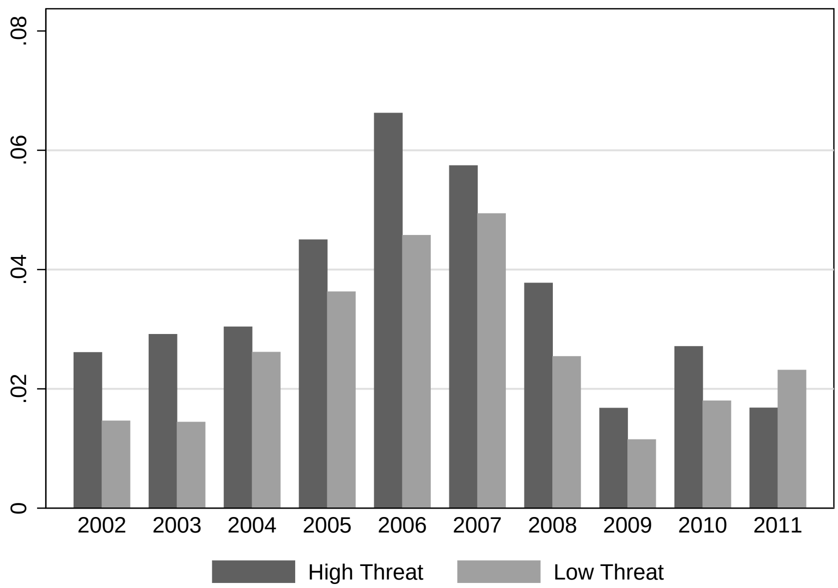
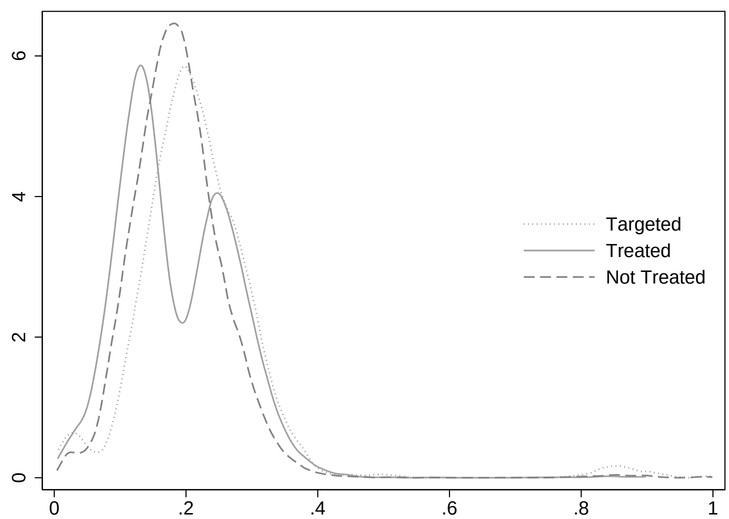


Figure 2: Control Group Selection

This figure plots density functions of baseline target probability for subsamples of firms in 2001-2011. Baseline target probability reflects the likelihood that a firm will be targeted by an activist hedge fund conditional only on firm-specific characteristics (calculated using the model in Column (3) of Appendix A). In year t, industries are considered threatened if, in the previous year (t-1), they experienced unexpectedly high number of activist campaigns, as indicated by positive residuals from the panel AR(1) model in Column (6) of Table 3. In each threatened industry-year, firms with baseline target probability in the top and bottom (within-industry-year) terciles are considered *Treated*, and are referred to as *High Prob. Group* and *Low Prob. Group*, respectively. Panel A compares density functions for *Targets*, *Treated*, and all other firms in the sample (*Not Treated*). Panel B compares density functions for *Treated* firms in the *High Prob. Group* and *Low Prob. Group* with their respective control groups. Each *Treated* firm is matched to up to 2 *Control* firms closest in baseline target probability and within the same *Return-on-Assets*-, *Book Leverage*- and *Payout*-quintiles in year t-1. The control firms are picked with replacement from industries that are not threatened in year t, and are in the non-treated group in years t-1 and t+1.

Panel A: Baseline Probability



Panel B: Propensity-matched Controls

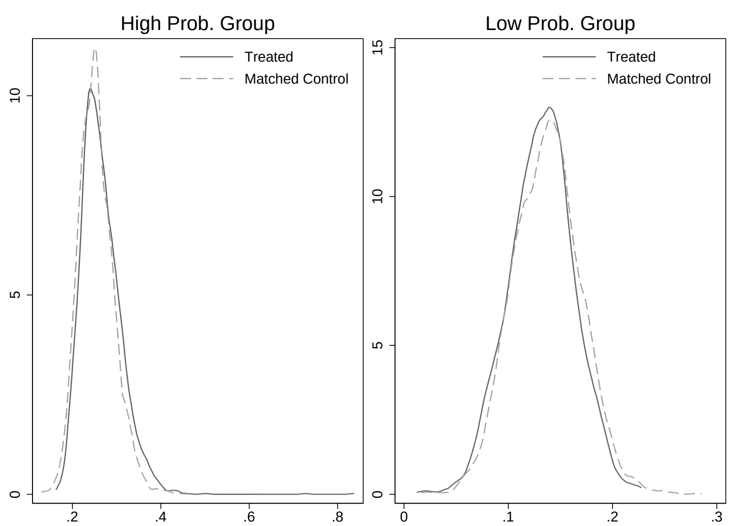


Table 1: Firm and Industry Panel Compositions by Year

This table reports annual frequency counts for targets and non-targets of hedge fund activism in 2000-2011. Industries (defined by 3-digit SIC) with fewer than 5 firms are excluded. Columns *Total* report the total numbers of firms and industries in the sample. Columns *Targeted* report the numbers of firms and industries with at least one activism campaign in the year. Column *Over 2(5)%* reports the number of industries in which at least 2(5)% percent of firms have at least one activism campaign in the year.

	# Firms		# Industries			
Years	Total	Targeted	Total	Targeted	Over 2%	Over 5%
2000	5394	57	189	42	34	24
2001	5234	69	179	44	39	18
2002	4589	89	175	46	40	25
2003	4197	82	173	48	42	22
2004	3949	83	169	54	47	34
2005	4299	145	165	70	67	46
2006	4212	182	160	82	79	67
2007	4173	209	160	80	78	65
2008	4065	140	160	54	51	38
2009	3825	69	158	35	29	17
2010	3569	84	151	43	40	22
2011	3451	72	155	38	34	25
Total	50957	1281	1994	636	580	403

Table 2: Characteristics of Target and Non-Target Firms

This table reports the characteristics of all firms in the sample (Panel A) and of target firms (Panel B). The observations are firm-year and the sample period is 2000-2011. All variables are as of the end of the prior year. *Market Cap* is the stock market capitalization in $ million (ME). *Tobin's Q* is the ratio of market value of assets (ME plus book value of debt) to book value of assets (the sum of book values of debt and equity). *Distance-to-Dafault* is the ratio of market value of assets to default threshold (max[0.1ME,book value of debt]) normalized by the firm's equity volatility. *Stock Turnover* is the ratio of average daily shares traded to shares outstanding. *Stock Return* is the total yearly stock return. *Return-on-Assets* is the ratio of EBITDA to book value of assets at previous year end. *Payout* is the sum of dividends and share repurchases divided by operating cash flows. *Sales Growth* is the yearly growth in sales from the prior year. *Inst Ownership* is the proportion of shares held by 13-F institutional investors. *G-index* is the Gompers et al. (2003) governance index where higher values represent lower shareholder rights or higher management entrenchment.

Panel A: All Firms

Variable	Count	Mean	SD	p5	p25	p50	p75	p95
Market Cap	50957	3031	15550	9	62	272	1152	10964
Tobin's Q	48917	2.785	11.983	0.625	1.029	1.486	2.610	7.478
Stock Return	50721	0.172	1.149	-0.757	-0.305	0.015	0.350	1.518
Stock Turnover $\times$100	50957	0.788	1.018	0.075	0.234	0.501	0.995	2.387
Distance-to-Dafault	50746	10.67	7.726	1.703	5.261	9.054	14.26	24.68
Return-on-Assets	50457	-0.399	66.755	-0.550	0.006	0.086	0.166	0.330
Payout	50957	0.379	2.734	0.000	0.092	0.290	0.635	1.000
Sales Growth	49436	0.113	0.398	-0.415	-0.035	0.080	0.222	0.742
Inst Ownership	46290	0.441	0.295	0.016	0.165	0.425	0.706	0.905
G-index	15842	9	3	5	7	9	11	13

Panel B: Target Firms

Variable	Count	Mean	SD	p5	p25	p50	p75	p95
Market Cap	1281	1080	3918	13	55	178	647	4475
Tobin's Q	1217	1.996	2.640	0.569	0.977	1.377	2.241	4.957
Stock Return	1277	0.050	0.889	-0.739	-0.359	-0.073	0.223	1.082
Stock Turnover $\times$100	1281	0.828	0.798	0.097	0.286	0.578	1.086	2.365
Distance-to-Dafault	1274	11.47	7.459	1.992	5.944	10.32	15.34	24.62
Return-on-Assets	1279	0.011	0.360	-0.490	-0.010	0.081	0.155	0.298
Payout	1281	0.404	0.369	0.000	0.074	0.287	0.718	1.000
Sales Growth	1251	0.072	0.372	-0.423	-0.045	0.048	0.171	0.621
Inst Ownership	1121	0.507	0.280	0.058	0.267	0.516	0.760	0.919
G-index	435	9	3	5	7	9	11	13

Table 3: Industry-Level Persistence in Activism

This table reports estimates of industry-year panel AR(1) models of activist targeting during 2001-2011. The dependent variable, $IndustryTargetFreq_t$, is calculated as the number of hedge fund activist targets in an industry in year t divided by the total number of firms in the industry. Only industry-years with at least 5 firms are included. Specifications (1)-(3) have no industry fixed effects and are estimated by OLS. Specifications (4)-(6) absorb industry fixed effects using the GMM estimator of Arellano and Bond (1991) to avoid estimation bias. Following Hayakawa (2009), the forward orthogonal deviation is used as the transformation method and two lags of backward orthogonal deviations are used as instruments for the transformed lagged dependent variable. To control for the overall level of activism in the economy, specifications (2) and (5) include the average market-wide target frequency across industries in year $t-1$, *Market-wide* $TargetFreq_{t-1}$, while specifications (3) and (6) include year dummies. Standard errors, reported in parentheses, are robust to heteroscedasticity and autocorrelation and corrected for finite-sample bias by the two-step procedure of Windmeijer (2005). */**/*** denotes significance at 10/5/1%.

	OLS			Difference-GMM		
	(1)	(2)	(3)	(4)	(5)	(6)
Industry	0.167***	0.139***	0.126***	0.154***	0.197***	0.109**
TargetFreq.$_{it-1}$	(0.045)	(0.049)	(0.047)	(0.046)	(0.067)	(0.046)
Market-wide		0.476***			0.342***	
TargetFreq.$_{t-1}$		(0.118)			(0.107)	
Year FE	No	No	Yes	No	No	Yes
Industry Effects	No	No	No	Diff	Diff	Diff
Observations	1806	1806	1806	1806	1806	1806
R^2	0.028	0.039	0.081			
Hansen J p-val				0.365	0.000	0.528

Table 4: Effect of Activism Threat on a Firm's Probability of Being Targeted

This table reports estimates of linear probability models of activist targeting. Observations are firm-year and the sample period is 2001-2011. The dependent variable is a dummy that equals one if a firm experiences at least one activist campaign in year t, and zero otherwise. The explanatory variable of interest, *Threat*, is calculated as the number of firms that were targeted in a firm's industry in year t-1 divided by the total number of firms in the industry. Panel A reports estimates for the full sample, which includes only industries with at least 5 firms and at least 3 firms in the top and bottom (pooled sorted) baseline target probability terciles. Baseline target probability reflects the likelihood that a firm will be targeted by an activist hedge fund conditional only on firm-specific characteristics (calculated using the model in Column (3) of Appendix A). The same set of firm characteristics are also included in some models as *Firm-level Controls*. *HighProb* (*MedProb*) is a dummy that equals one if a firm is in the top (middle) tercile of baseline target probability in year t, and zero otherwise. Panel B reports estimates for the subsamples of firms in the top and bottom (pooled sorted) baseline target probability terciles. *Threat* is defined based on the full sample, as in Panel A. Where indicated, some models also include year and/or industry fixed effects. Standard errors, clustered by industry, are in parentheses. */**/*** denotes significance at 10/5/1%.

Panel A: Full Sample

	(1)	(2)	(3)	(4)	(5)
Threat	0.338***	0.202***	0.181***	0.076	−0.094*
	(0.054)	(0.041)	(0.051)	(0.048)	(0.055)
MedProb*Threat					0.090
					(0.063)
HighProb*Threat					0.349***
					(0.096)
Firm-level Controls	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Industry FE	No	No	No	Yes	Yes
MedProb,HighProb	No	No	No	No	Yes
Observations	35444	32471	32471	32471	32471
Pseudo R^2	0.004	0.054	0.057	0.059	0.060

Panel B: Probability Group Subsamples

	High Pob. Group		Low Prob. Group	
	(1)	(2)	(3)	(4)
Threat	0.537***	0.192**	0.047	−0.060
	(0.097)	(0.087)	(0.039)	(0.049)
Firm-level Controls	No	Yes	No	Yes
Year FE	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes
Observations	10822	10810	10967	10941
R^2	0.006	0.101	0.000	0.012

Table 5: Effect of Activism Threat on a Firm's Probability of Being Targeted – **IV Estimates**

This table reports IV-GMM estimates of linear probability models of activist targeting. Observations are firm-year and the sample period is 2001-2011. The sample includes only industries with at least 5 firms and at least 3 firms in the top and bottom (pooled sorted) baseline target probability terciles (calculated using the model in Column (3) of Appendix A). The dependent variable is a dummy that equals one if a firm experiences at least one activist campaign in year t, and zero otherwise. The endogenous explanatory variable of interest, *Threat*, is calculated as the number of firms that were targeted in a firm's industry in year t-1 divided by the total number of firms in the industry. Estimates in Column (1) are for the full sample. Other specifications are for the subsamples of firms in the top and bottom baseline target probability terciles, as labeled in the headings. *HighProb* is a dummy that equals one if a firm is in the top tercile of baseline target probability in year t, and zero otherwise. Panel B reports first stage estimates for the correspondingly numbered models in Panel A. The instruments are the expected annual selling and buying volumes, averaged across all firms in an industry, conditional only on institutional trading in firms outside that industry and other general market conditions (see Appendix B for details). The interaction term is instrumented by the interactions between *HighProb* and the expected buy and sell volumes. Where indicated, some models also include firm-level controls, year fixed effects, and industry fixed effects. Standard errors, clustered by industry, are in parentheses. */**/*** denotes significance at 10/5/1%.

Panel A: 2nd Stage

	All	High Prob. Group		Low Prob. Group	
	(1)	(2)	(3)	(4)	(5)
Threat	0.268	1.750***	2.058**	0.037	−0.214
	(0.298)	(0.492)	(0.810)	(0.403)	(0.465)
HighProb*Threat	1.307***				
	(0.348)				
Firm-level Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	No	Yes
Prob Group FE	Yes	No	No	No	No
Observations	32482	10811	10811	10945	10945
R^2	0.037	0.051	0.038	0.003	0.002
Kleib.-Paap p-val	0.073	0.014	0.063	0.106	0.181
Hansen J p-val	0.329	0.416	0.178	0.828	0.344

Panel B: 1st Stage (OLS)

	All		High Prob. Group		Low Prob. Group	
	Threat	HP*Threat	Threat	Threat	Threat	Threat
	(1a)	(1b)	(2)	(3)	(4)	(5)
E[Selling]	4.306***	−1.207***	5.788***	3.551***	4.273**	2.798*
	(1.577)	(0.374)	(1.590)	(1.305)	(1.638)	(1.501)
E[Buying]	−3.387**	1.122***	−5.160***	−3.459***	−3.416**	−2.864**
	(1.395)	(0.372)	(1.454)	(1.264)	(1.473)	(1.424)
HighProb*E[Selling]	1.625**	9.563***				
	(0.767)	(1.963)				
HighProb*E[Buying]	−1.872**	−8.594***				
	(0.756)	(1.845)				
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Act.Prob Group FE	Yes	Yes	No	No	No	No
Industry FE	No	No	No	Yes	No	Yes
Observations	32482	32482	10811	10811	10945	10945
R^2	0.218	0.452	0.233	0.399	0.203	0.384

Table 6: Effect of Activism Threat on the Returns of Peer Firms

This table reports benchmark-adjusted average monthly stock returns for the peers of activist targets in 2001-2011. Excluded are industries with less than 5 firms or industries that are threatened in both years t and t+1. In year t, industries are considered threatened if, in the previous year (t-1), they experienced unexpectedly high number of activist campaigns, as indicated by positive residuals from the panel AR(1) model in Column (6) of Table 3. Included are only firms with baseline target probability in the top and bottom (within-industry-year) terciles, with no bankruptcy filing in year t-1, t or t+1, and equity values below the 99^{th}-percentile of the targets in the sample. Returns are estimated according to the following model:

$$AR_{ij,t+h} = a_j + b_t + \beta \cdot HighProb_{ij,t-1} + \varepsilon_{ij,t+h},$$

where AR_{ijt+1} is the average monthly abnormal return for year $t+h$, and a_j and b_t are industry and year fixed effects, respectively. *HighProb* is a dummy that equals one if a firm is in the top tercile of baseline target probability in year t, and zero otherwise. The following benchmarks are considered: *ffi48e* (*ffi48v*) - for the Fama-French 48 inudstry matched portfolios, equally (value) weighted; *ff25e* (*ff25v*) - for the Fama-French 25 size and style matched portfolios, equally (value) weighted; *raw* denotes unadjusted returns. Standard errors, clustered by industry and year, are in parentheses. */**/*** denotes significance at 10/5/1%.

Panel A: Threat Formation Year (t-1)

	(1) raw	(2) ffi48v	(3) ffi48e	(4) ff25v	(5) ff25e
HighProb	0.012***	0.012***	0.012***	0.007**	0.008**
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Observations	2207	2201	2201	2207	2207
R^2	0.223	0.085	0.021	0.062	0.036
High Prob. Mean	0.029	0.022	0.009	0.013	0.010
Low Prob. Mean	0.017	0.010	-0.002	0.005	0.003

Panel B: Treatment Year (t)

	(1) raw	(2) ffi48v	(3) ffi48e	(4) ff25v	(5) ff25e
HighProb	0.004	0.004	0.004	0.002	0.002
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Observations	2263	2253	2253	2263	2263
R^2	0.222	0.080	0.010	0.039	0.017
High Prob. Mean	0.013	0.007	0.003	0.005	0.004
Low Prob. Mean	0.011	0.005	-0.001	0.004	0.002

Panel C: Subsequent Year (t+1)

	(1) raw	(2) ffi48v	(3) ffi48e	(4) ff25v	(5) ff25e
HighProb	0.003	0.003	0.003	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	2052	2042	2042	2050	2050
R^2	0.140	0.030	0.005	0.046	0.023
High Prob. Mean	0.015	0.004	0.002	0.004	0.003
Low Prob. Mean	0.014	0.002	-0.000	0.003	0.002

Table 7: Summary Statistics for Treated and Control Firms

This table reports summary statistics for the peers of activist targets in 2001-2011 versus matched control firms. The sample includes only industries with at least 5 firms and at least 3 firms in the top and bottom (pooled sorted) baseline target probability terciles (calculated using the model in Column (3) of Appendix A). In year t, industries are considered threatened if, in the previous year (t-1), they experienced unexpectedly high number of activist campaigns, as indicated by positive residuals from the panel AR(1) model in Column (6) of Table 3. In each threatened industry-year, firms with baseline target probability in the top and bottom (within-industry-year) terciles are considered *Treated*, and are referred to as *High Prob. Group* and *Low Prob. Group*, respectively. Each *Treated* firm is matched to up to 2 *Control* firms closest in baseline target probability and within the same *Return-on-Assets*-, *Book Leverage*- and *Payout*-quintiles in year t-1. The control firms are picked with replacement from industries that are not threatened in year t, and are in the non-treated group in years t-1 and t+1. Included are only firms with no bankruptcy filing in year t-1, t or t+1 and equity values below the 99^{th}-percentile of the targets in the sample. *ActProb* refers to baseline target probability. *ME* is stock market capitalization in \$ million. *BookLev* is the ratio of book value of debt divided by the sum of book values of debt and equity. *Payout* is the ratio of dividends and share repurchases to operating cash flow. *CapEx* is the ratio of investments divided by lagged assets. *Return-on-Assets* is the ratio of EBITDA to book value of assets at previous year end. *ATurn* is the ratio of sales to average assets. All variables are as of the end of year t-1.

Panel A: High Prob. Group

	Mean		Median		Q3 minus Q1	
Variable	Treated	Control	Treated	Control	Treated	Control
ActProb	0.265	0.254	0.259	0.251	0.056	0.052
ME	326	378	117	122	329	359
BookLev	0.214	0.214	0.108	0.108	0.363	0.368
Payout	0.219	0.218	0.000	0.000	0.302	0.316
CapEx	0.037	0.034	0.016	0.014	0.039	0.042
ROA	0.019	0.024	0.056	0.054	0.181	0.155
ATurn	0.909	1.062	0.754	0.936	0.844	1.069

Panel B: Low Prob. Group

	Mean		Median		Q3 minus Q1	
Variable	Treated	Control	Treated	Control	Treated	Control
ActProb	0.132	0.137	0.133	0.137	0.041	0.043
ME	1900	2522	366	410	1596	1726
BookLev	0.240	0.241	0.110	0.127	0.411	0.427
Payout	0.217	0.209	0.005	0.006	0.331	0.325
CapEx	0.051	0.050	0.023	0.019	0.061	0.059
ROA	0.067	0.047	0.100	0.100	0.221	0.210
ATurn	0.933	0.946	0.785	0.713	0.823	1.132

Table 8: Effect of Activism Threat on the Corporate Policies of Peer Firms

This table reports changes in corporate policies for the peers of activist targets in 2001-2011 versus matched control firms. The sample includes only industries with at least 5 firms and at least 3 firms in the top and bottom (pooled sorted) baseline target probability terciles (calculated using the model in Column (3) of Appendix A). In year t, industries are considered threatened if, in the previous year (t-1), they experienced unexpectedly high number of activist campaigns, as indicated by positive residuals from the panel AR(1) model in Column (6) of Table 3. In each threatened industry-year, firms with baseline target probability in the top and bottom (within-industry-year) terciles are considered *Treated*. Each *Treated* firm is matched to up to 2 *Control* firms closest in baseline target probability and within the same *Return-on-Assets*-, *Book Leverage*- and *Payout*-quintiles in year t-1. The control firms are picked with replacement from industries that are not threatened in year t, and are in the non-treated group in years t-1 and t+1. Included are only firms with no bankruptcy filing in year t-1, t or t+1 and equity values below the 99^{th}-percentile of the targets in the sample. We estimate the following (triple) difference-in-differences model:

$$\Delta X_{ij,t+1}^{Treated} - \Delta X_{ij,t+1}^{Control} = a_j + b_t + \beta \cdot HighProb_{ij,t-1} + \varepsilon_{ij,t+1},$$

where ΔX_{ijt+1} is the change in a variable of interest from year t+1 to t-1, and a_j and b_t are industry and year fixed effects, respectively. *HighProb* is a dummy that equals one if a firm is in the top tercile of baseline target probability in year t, and zero otherwise. *BookLev* is the ratio of book value of debt divided by the sum of book values of debt and equity. *Payout* is the ratio of dividends and share repurchases to operating cash flow. *CapEx* is the ratio of investments divided by lagged assets. *Return-on-Assets* is the ratio of EBITDA to book value of assets at previous year end. *ATurn* is the ratio of sales to average assets. All X-variables are winsorized at 1%. Panel B includes each treated firms market equity value as an additional control. Standard errors, clustered by treated industry and control firm, are in parentheses. */**/*** denotes significance at 10/5/1%.

Panel A: Baseline Effects

	(1) BookLev	(2) Payout	(3) CapEx	(4) ROA	(5) ATurn
HighProb	0.013*	0.027**	−0.003	−0.012	0.034**
	(0.008)	(0.014)	(0.003)	(0.008)	(0.016)
Observations	9452	9452	9452	9452	9452
R^2	0.004	0.003	0.003	0.009	0.011

Panel B: Size-adjusted Effects

	(1) BookLev	(2) Payout	(3) CapEx	(4) ROA	(5) ATurn
HighProb	0.021**	0.048***	−0.007**	−0.001	0.041**
	(0.009)	(0.015)	(0.003)	(0.011)	(0.017)
Size Control	Yes	Yes	Yes	Yes	Yes
Observations	9452	9452	9452	9452	9452
R^2	0.005	0.006	0.006	0.012	0.011

Table 9: Feedback Effects of Activism Threat

This table reports estimates of linear probability models of activist targeting. Observations are firm-year and the sample period is 2001-2011. The dependent variable is a dummy that equals one if a firm experiences at least one activist campaign in year t, and zero otherwise. *Threat* is calculated as the number of firms that were targeted in a firm's industry in year t-1 divided by the total number of firms in the industry. Dummy is an indicator variable as defined in the heading of each column: $\Delta Prob < 0$ equals 1 if a firm's baseline target probability as of year t+1 is lower than in year t, and 0 otherwise. $AR(ind) > 0$ equals 1 if a firm's industry-adjusted average monthly return in year t is positive, and 0 otherwise. $AR(mm) > 0$ equals 1 if a firm's market-adjusted average monthly return in year t is positive, and 0 otherwise. Baseline target probability reflects the likelihood that a firm will be targeted by an activist hedge fund conditional only on firm-specific characteristics (calculated using the model in Column (3) of Appendix A). The same set of firm characteristics are also included in some models as *Firm-level Controls*. Included are only firms with high baseline target probability. Where indicated, some models also include year and/or industry fixed effects. Standard errors, clustered by industry, are in parentheses. */**/*** denotes significance at 10/5/1%.

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta Prob < 0$	$\Delta Prob < 0$	$AR(ind) > 0$	$AR(ind) > 0$	$AR(mm) > 0$	$AR(mm) > 0$
Threat	0.714***	0.305***	0.770***	0.395***	0.685***	0.366***
	(0.111)	(0.088)	(0.137)	(0.118)	(0.122)	(0.121)
Dummy(#)	0.015**	0.011*	0.006	0.005	0.015***	0.018***
	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Dummy(#)*Threat	−0.371**	−0.236	−0.523***	−0.444***	−0.265*	−0.321**
	(0.152)	(0.148)	(0.156)	(0.156)	(0.154)	(0.147)
Firm-level Controls	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes
Observations	10822	10810	10822	10810	10822	10810
R^2	0.007	0.101	0.008	0.102	0.007	0.102

Appendix A. Baseline Activism Model

Table A.1: Baseline Target Probability

This table reports probit model estimates of activist targeting. Observations are firm-year and the sample period is 2000-2011. **Specification (3)**, with year and industry fixed effects set to zero, is used to compute each firm's **baseline target probability**. The dependent variable is a dummy that equals one if a firm experiences at least one activist campaign in year t, and zero otherwise. All other variables are as of the end of the prior year. *Market Cap* is the logarithm of stock market capitalization in $ million (ME). *Tobin's Q* is the logarithm of the ratio of market value of assets (ME plus book value of debt) to book value of assets (the sum of book values of debt and equity). *Distance-to-Dafault* is the logarithm of the ratio of market value of assets to default threshold (max[0.1ME,book value of debt]) normalized by the firm's equity volatility. *Stock Turnover* is the logarithm of the ratio of average daily shares traded to shares outstanding. *Stock Return* is a dummy equal to one if a firm's return exceeds the industry median. *Return-on-Assets* is a dummy equal to one if the decrease in a firm's ratio of EBITDA to book value of assets exceeds the industry median. *Payout* is a dummy if the sum of dividends and share repurchases divided by operating cash flow decreases from the prior year. *Sales Growth* is a dummy equal to one if a firm's sales growth is in the top-quartile within the industry. *Inst Ownership* is the proportion of shares held by 13-F institutional investors. *Past Campaigns* is the logarithm of the number of hedge fund activist campaigns at the firm in the past 3 years. *Ongoing Campaigns* is a dummy equal to one if a previous activist campaign at the firm is continuing in the current year. *G-index* is the Gompers et al. (2003) governance index where higher values represent lower shareholder rights or higher management entrenchment. *Mfx* denotes marginal effects evaluated at the mean for continuous variables and changes from 0 to 1 for dummies. *SE* denotes standard errors clustered by firm. */**/*** denotes significance at 10/5/1%.

	(1)		(2)		(3)		(4)	
	β / SE	Mfx	β / SE	Mfx	β / SE	Mfx	β / SE	Mfx
Market Cap	−0.176***	−0.0090	−0.157***	−0.0071	−0.141***	−0.0060	−0.164***	−0.0057
	(0.011)		(0.012)		(0.012)		(0.023)	
Tobin's Q	−0.058*	−0.0030	−0.165***	−0.0074	−0.137***	−0.0058	−0.057	−0.0020
	(0.034)		(0.039)		(0.038)		(0.095)	
Distance-to-Dafault	0.226***	0.0115	0.111***	0.0050	0.085***	0.0036	0.051	0.0018
	(0.024)		(0.027)		(0.026)		(0.054)	
Stock Turnover	0.093***	0.0048	0.077***	0.0035	0.057***	0.0024	0.076*	0.0027
	(0.018)		(0.019)		(0.018)		(0.044)	
Stock Return	−0.141***	−0.0072	−0.110***	−0.0050	−0.103***	−0.0044	−0.208***	−0.0073
	(0.028)		(0.029)		(0.029)		(0.055)	
Return-on-Assets	0.078***	0.0040	0.068**	0.0031	0.058**	0.0024	0.108**	0.0038
	(0.028)		(0.028)		(0.028)		(0.053)	
Payout	0.088**	0.0045	0.083**	0.0037	0.067	0.0028	0.024	0.0009
	(0.041)		(0.041)		(0.041)		(0.080)	
Sales Growth	−0.112***	−0.0057	−0.093**	−0.0042	−0.074*	−0.0032	−0.055	−0.0019
	(0.041)		(0.041)		(0.041)		(0.090)	
Inst Ownership	0.877***	0.0447	0.803***	0.0363	0.714***	0.0303	0.732***	0.0256
	(0.069)		(0.074)		(0.073)		(0.166)	
Past Campaigns					2.833***	0.1202	2.892***	0.1013
					(0.134)		(0.240)	
Ongoing Campaigns					−1.403***	−0.0595	−1.430***	−0.0501
					(0.285)		(0.481)	
G-index							0.011	0.0004
							(0.011)	
Industry FE	No		Yes		Yes		Yes	
Year FE	No		Yes		Yes		Yes	
Sales Growth Group	Yes		Yes		Yes		Yes	
Observations	46372		46159		46159		12945	
Pseudo R^2 / Baseline prob.	0.053	0.027	0.083	0.027	0.148	0.027	0.186	0.027

Table A.2: Across- and Within-Industry Sorts by Baseline Target Probability

This table reports firm-year and industry-year sorts by baseline target probability, which reflects the likelihood that a firm will be targeted by an activist hedge fund conditional only on firm-specific characteristics (calculated using the model in Column (3) of A.1). The sample period is 2000-2011. Industries (defined by 3-digit SIC) with fewer than 5 firms are excluded. Columns *Total* report the total numbers of firm-years and industry-years in the sample. Columns *Targeted* report the numbers of firm-years and industry-years with at least one activism campaign. Column *Over 2(5)%* reports the number of industry-years in which at least 2(5)% percent of firms have at least one activism campaign. Panel A reports sorts of firm-years (industry-years) based on their estimated target probabilities (average industry target probabilities). Panel B reports within industry-year sorts of firm-years based on the latters' estimated target probabilities and compares these within industry-year sorts with the pooled sorts in Panel A.

Panel A: *Pooled* Baseline Probability Sorts

	# Firm-Years		# Industry-Years			
Prob. Tercile	Total	Targeted	Total	Targeted	Over 2%	Over 5%
Low	15988	154	556	175	153	100
Middle	17652	390	705	219	202	144
High	17307	737	733	242	225	159
Total	50957	1281	1994	636	580	403

Panel B: *Within* Industry-year Baseline Probability Sorts

	# Firm-Years		Comparison with *Pooled*			Percent in
Prob. Tercile	Total	Targeted	Low	Middle	High	Total
Low	15477	161	12745	2616	116	30.37
Middle	17032	395	3061	11580	2391	33.42
High	18448	725	192	3456	14800	36.20
Total	50957	1281	15998	17652	17307	100.00

Appendix B. Expected Institutional Trading

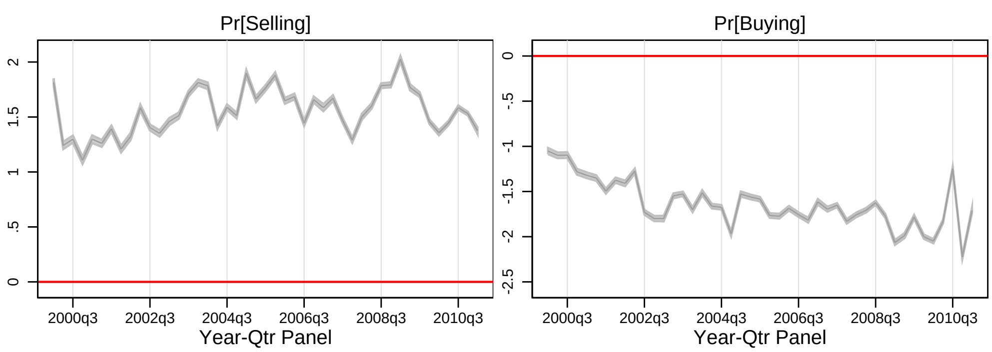
Figure B.1: Fundamentals-Free Propensity to Trade

This figure plots coefficient estimates and fit diagnostics for a generalized logistic function that captures the probability that an institution will buy, sell, or not trade a particular stock. Panel A plots 99% confidence intervals for the quarterly coefficient estimates of the main variable: an institution's total dollar selling volume as a fraction of its total dollar trading volume in other stocks outside of the stock of interest's industry (SIC3). Panel B reports the Pseudo-R^2 and number of institution-stock-week observations used in each quarter. The probability that an institution will buy or sell or do nothing in a particular stock is modeled as a generalized logistic function of trading in other stocks. Specifically, for each stock s in week t, each institution i is classified as buy/sell/do nothing if it has a positive/negative/zero net purchase in the stock. The exact specification is as follows:

$$\begin{matrix} \Pr[Selling]_{ist} \\ \Pr[Buying]_{ist} \end{matrix} = \mathcal{L}\left\{ [Trade_{i(S\notin ind(s))t} \quad \boldsymbol{Trade_{i(S\notin ind(s))t} \times FractionSold_{i(S\notin ind(s))t}} \quad (\ldots)_{i(S\notin ind(s))t-1} \quad Controls_{ist}]\boldsymbol{\beta} \right\},$$

where $FractionSold_{i(S\notin ind(s))t}$ equals institution i's total dollar selling volume divided by its total dollar trading volume in all stocks outside of stock s's industry in week t. $Trade_{i(S\notin ind(s))t}$ is a dummy variable that equals 1 if institution i trades at least one stock outside of stock s's industry in week t, and 0 otherwise. $(\ldots)_{i(S\notin ind(s))t-1}$ includes $Trade_{i(S\notin ind(s))t-1}$ and $Trade_{i(S\notin ind(s))t-1} \times FractionSold_{i(S\notin ind(s))t-1}$. $Controls_{ist}$ include the following variables: dummy variable for whether institution i buys or sells only one other stock in week t, overall fraction of days in the calendar year that institution i trades any stock, market return and VIX index for week t, stock s's abnormal turnover and Amihud ratio for week t-1. The coefficient vector β captures the effects of included variables in determining the odds of buying and selling relative to no trading (reference outcome). The model is estimated separately for each quarter (from 2000:Q1 to 2010:Q4) where the coefficient estimates for all institution-stock-weeks in the quarter are assumed to be the same.

Panel A: 99% CI for the coefficients of $Trade_{i(S\notin ind(s))t} \times FractionSold_{i(S\notin ind(s))t}$



Panel B: Model fit and observation count

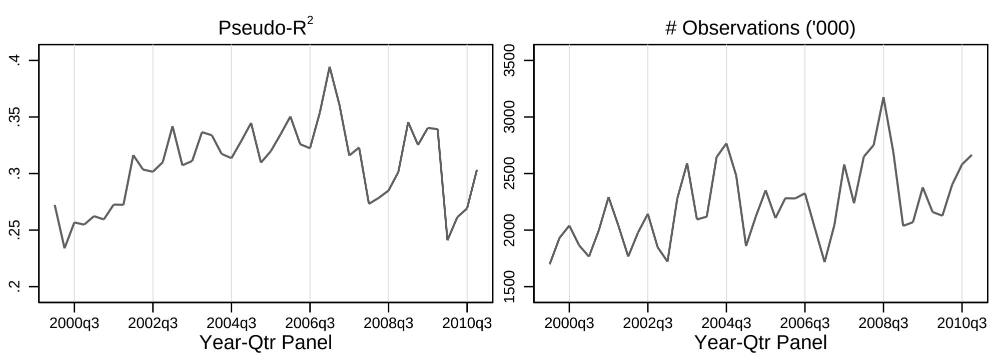


Figure B.2: Expected Industry-Level Fundamentals-Free Selling and Buying Volumes

This figure plots histograms for the expected selling and buying volumes of an average stock in each industry-year. Excluded are industry-years with fewer than 5 stocks in total and fewer than 3 stocks in each of the high and low baseline target probability terciles. The sample period is 2000-2010. The expected volumes are calculated as a function of institutional trading in stocks outside that industry and other general market conditions:

$$E\left[Selling\right]_{jy} = \frac{1}{N_j} \sum_{s \in j, \forall i} \left(I_{p75, t \in y} \left\{ \widehat{\mathsf{Pr}}[Selling]_{ist} \times \widehat{E}[Selling_{isy}] \right\} \right),$$

where N_j is the number of stocks in industry j in year y, I_{p75} is the 75^{th} percentile function, $\widehat{E}[Selling_{isy}]$ is institution i's average weekly selling volume in stock s (normalized by stock s's shares outstanding), calculated over all weeks in year y in which the institution sells stock s. $\widehat{\mathsf{Pr}}[Selling]_{ist}$ is institution i's predicted probability of selling stock s in week t estimated as described in Figure B.1. $E\left[Buying\right]_{jy}$ is calculated in the same way.